STANHOPE BAYNE-JONES

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We've already talked about the papers, what I've found, and it should set spinning the years--well, the jumping jacks years, the years of going from house to house and the circumstances which gave rise to that. From what I've seen in the papers, it has had a shaping effect on you, and I think any illumination you can give to that period will be helpful to a biographer, or some researchers who want to understand you better because you're here to help explain it. If you'll take me back to those years--house to house to house and the circumstances which gave rise to that.

I'm not sure that I can do it chronologically straight--unless you tell me from your search on this where I went after my father's death and the sequence from then on. Does it matter?

No--except that I think you began this process in the home of Dr. Joseph Jones.

Yes. Well, I have very faint recollections of that, except that while I was in the house, I used to be in and out of his office which was an annex built on the side of the house and in which he saw patients. I would help him some time there. I would answer the door, sick people coming to the door. This was a big house on Washington Avenue and Camp Street, right opposite the fire engines. Are the fire engines in those papers?

Yes.

Well, I had early impressions of horrible wounds and injuries to people there. I can remember one evening a man who had his cheek missing. You could see his teeth and blood. He had been tending the back end of a lumber truck. The two-by-fours were flapping, and two of them caught the side of his face. I had a number of experiences like that in that house.

Also the house was full of the most strange things. My grandfather collected Egyptian Mummies. He had great interest in snakes, poisonous snakes, and he had a great many specimens of snakes in all sorts of containers, both in his office and outside in another building that he put up, a little wooden shack. He collected Indian remains. He had all sorts of remains that he had dug up in the Harpeth River Valley in Tennessee after the Civil War. He had cases and cases lining the halls with Indian relics, tomahawks, pipes, arrow heads, bows, everything that you could think of.

Then he got interested in models and specimens of rifles, swords, breast plates, armor. He had a full suit of armor and great, big, two-handed swords. I can remember once in the side yard I put on a heavy breast plate that must have been used by one of Cromwell's Ironsiders and got my friends to hit me with a two-handed sword. It knocked me down, I couldn't get the breast plate off and had to lie there on the ground.

He also collected Aztec things--he had great carved heads of serpents from Mexico around in the house with these snake bite things projecting above them. He collected books galore--he had a remarkable library which was later distributed. After his death it was sold for what little it would bring. It was bought by the man who was the librarian of Kings County Medical Society in Brooklyn, and it is mostly there now. They didn't identify the sources of their books. They just put them on the shelves, and you can't find Joseph Jones books in there any more.

He collected these Indian skeletons and bones of all kinds, and they went ultimately to the Museum of the American Indian in New York City. Mr. Heye bought them.

Well, now all this--I'm sure all this had an effect on me, for arousing interest in the first place in the medical things that I talked about and in the second place in archeology, or paleopathology as they call it mostly. Recently there was a conference on paleopathology at the National Research Council of the National Academy here in Washington--two years ago--organized by Dr. Saul Jarcho. He invited me to discuss a paper. I got up and said that I had no right to be there, that I had never dug up anything but maybe some dead rabbits, and that the only reason that I felt I could cite for being in this privileged position was an hereditary privilege from my grandfather. Now that conference indicated essentially a revival of interest in paleopathology in the United States, and a general society is now being formed.

I can trace a great many things back to the influence of my grandfather--more than my father influenced me. My father did influence me in medicine because he would take me into his--I would call it a sort of dispensary, or office in that home on Howard Avenue by Lee Circle. I can remember helping him wind bandages there. He had a kind of twisted, angular wire over a cigar box, and I would turn the crank on this thing, and the bandages would roll up. I got used to seeing him take care of some sick people in that place, but not very extensively.

To me it was a very happy sort of a beginning. I ran around with friends, and I was treated respectfully for a child. I can remember--I have a picture somewhere, and you may have seen it, of myself in Lord Fauntleroy's velvet clothes. Did you find it?

<u>Yes, I did. I put it over there in the corner with the rest of the photographs.</u>

Well, I fell in the gutter in that suit one day. New Orleans had no drainage system in those times. Even when I grew up there were street gutters along the side. Whenever the rain came, the streets were flooded, and we used to go out and swim in the streets. Now, they've reduced the water table by about eight feet, and they have a huge pumping system that clears the city.

Well, there were lots of interesting things to do. At that time I would occasionally go over to Biloxi, Mississippi, where Mrs. Denegre, my "Tante E", had a place called "Malua", and play on the beach, go sailing, fishing. I would see my brother and sister over there more than I would sometimes in New Orleans.

<u>Was Dr. Jones something of a chemist?</u>

Yes, he was a Professor of Chemistry. Yes, I forgot to mention that. The house was just full of his chemical things, and he did some original work in chemistry.

<u>Apparently you did too in the gold fish pond.</u>

I wasn't doing chemistry in the gold fish pond. I was a biologist at that moment, trying to see whether a fish would survive under a flame that was twelve feet in diameter. It got out of hand. It was much bigger than I thought it would be. I think it was gasoline--or coal oil that I poured on the pond.

My grandfather let me have rabbits in the back yard. I built rabbit pens, and the darn things would dig out from under all the time. I had dogs, especially a little Fox Terrier that I was very fond of called "Vixen". In that house they had to punish me a good deal because I was unruly and pretty wild. I did a great many things that I don't look back on now with any pride, but they did punish me a good deal. Two I remember--a series of frightening punishments. They told me that if I continued to be bad like that, the moon would come down with a pair of ice tongs and put them around my ribs and take me away. Do you know what ice tongs are?

Yes--the hooks.

Yes, and the other punishment I remember aroused my sense of humor very much. One of my uncles decided that he would take me up in the attic of this big house and give me a whaling with a trunk strap. I started to run around the attic, and I can remember dodging behind these upright two-by-fours that support a slightly sloping roof. He'd whale at me, and the leather thong would curl around one of these uprights, and he'd have to stop to untangle himself and the leather.

I remember all these punishments in more or less an amused fashion. My aunt, Mrs. Denegre, "Tante E", was a very modest person, and I can remember when she had to spank me, she said,"Stanhope, come over here, take down your pants, and bend over."

Then she'd drape my buttocks with a towel and then spank me.

How gentle.

Also she used to spank me with a piece of silver which made me blush every time I would look at it, and I've seen plenty of them. It's a Kirk repoussé. If you get spanked with a repoussé of roses standing out against silver, you get a lot of red. I think I had a great deal of corporal punishment.

I guess so.

But it had an amusing side. I can still laugh at it somewhat. One time one of them was paddling me with a lady's slipper and the slipper broke while I was being spanked, flew off and knocked something off a bureau. I thought it was a wonderful joke.

Another side of that life was that I had much time to myself out on the streets. The neighborhood where my grandfather lived was right on the verge of Magazine Street, a block away, which is the verge of a hoodlum district. I had a great many street fights at that time, and I developed some caution. I also had a knightly sense because one of my aunts gave me Howard Pyle's book, When Knighthood was in Flower, so I rememner this punishment that a street ruffian gave me. I went out on the street and challenged him by putting a chip on my shoulder. I'd read that he had to knock a chip off my shoulder to start something. I put a chip on my shoulder, and he swung wide. He knocked the chip off, but on the way to knock the chip off, he broke my nose and knocked me down.

Did you run free?

On the street?

Yes.

Yes. I was not a part of a gang. I was different. I was on a higher social status than the gang that was running. They would mostly persecute me.

Was there just your grandfather, Dr. Joseph Jones, in the house?

No, there was my Aunt Susie and my Aunt Mamie, Mrs. Bringier. Susie Jones was there, and my Aunt Frances was living there and Hamilton Jones. It was a great big house in New Orleans.

<u>All of them had a hand in these punishments? You said "they".</u>

Yes, I think of them as a group.

<u>"They" then includes all of them.</u>

I don't remember that there was any division among them regarding myself. Certainly I felt closest to my Aunt Susie, but I don't remember playing one off against another. My Aunt Susie was a very lovely person who, I think, cared for me a great deal.

<u>Did they also have fun with you? Can you remember pleasant times as distinct from the unpleasant?</u>

I think they had some fun chasing me around all over the place.

<u>"What will that boy do next!"</u>

That's right. I could think of all sorts of things to do.

I might as well tell you while we're on this subject that I had some gun powder and I lit some of it on a window sill. All of the smoke from the gun powder blew back in the house and frightened everybody. It was on the second floor, so they locked me in a room, but in that room I discovered some shot gun shells and a magnifying glass. I can remember to this day working all day. I unloaded these shells and made a train of powder from the window ledge along down to the fire place on which I had quite a sizable bunch of powder by the end of the day. I sat there and waited until the sun came by the window and with the magnifying glass--I didn't know that it was going to set the thing on fire. I thought it was just an interesting scientific experiment, but I got the sun on the powder and shh--you know how it goes. It went all up again.

I got another spanking for that, but that was an achievement of the scientific method.

I guess they let you out of that room.

I've forgotten what happened next.

Were fire arms part of the order of the day? Did people generally have this? Did you do a bit of hunting, or no?

Oh yes. I went hunting and so did my uncles go hunting. Firearms were around the place.

Like spoons and knives.

Yes. I don't know where I got those shells. I don't remember a shot gun, but there was a box of shells in that room. Louisiana was a great hunting country and still is.

Yes.

My brother got to be a great shot.

Was the family a family of readers--apart from Dr. Jpseph Jones who collected books to whet his curiosity?

When you say "the family", are you talking about the Joneses?

Yes, the Joneses.

Not particularly. They read a good many things, but it was Joseph Jones who collected the books, and curiously enough, my recollection is that the books that he chiefly collected are the ones he wrote himself. He published four huge volumes and did it at his own expense, and all of the upstairs, the second floor, and part of the downstairs ~~was~~ were lined with book shelves containing these volumes--maybe thousands of them. Nobody bought them, and he didn't give them all away. I had a good many sets, and there are a lot of them around still.

My Aunt Frances was a reader and an artist, and they had--all the ladies had, but particularly Frances and probably Mamie, a very literary friend in New Orleans called Grace King. Do you know of her?

Yes.

My Aunt Frances illustrated one of Grace King's books. [New Orleans, The Place and the People (New York, 1895)]. Grace King had a salon. She was a literary figure, and every week these ladies would meet, and they would discuss some very serious subject. There were a number of these literary societies among the ladies in New Orleans called the Geographics, or the Blue Stockings. Mollie Moore Davis had a salon. She was the daughter of Jefferson Davis.

Yes, that is described in one of the papers--Mollie Davis' salon and the people who frequented it.

All of that in there?

Yes. But you indicate that some members of the Jones family gave it a kind of creative, artistic flavor--Frances E. Jones.

Oh yes, Frances could draw very well with pen and ink mostly. And made pottery. She taught art at Newcomb College. Newcomb College was built just across the street from this house, a sort of huge, renaissance building with other buildings. There was an artist and historians, a potter named Mr. /Ellsworth/ Woodward--I think his name was. Well, I don't remember but Newcomb was built there. Newcomb had an art department, and Frances Jones taught in it. They had a particular kind of pottery called Newcomb Pottery that they invented, so to speak--glazed and baked themselves.

The place was full of myths and curious things. For instance, they had two iron lions on the stairway outside the steps of Newcomb College, and somebody told me that if I was lucky I would hear them roar some night. I spent many a night trying to hear those lions roar. They didn't.

There was a marvelous fire engine right across the street with great big horses that would come plunging out, and that was a great thrill.

The house was a great, big, rambling, ghost-like place, and I was very frightened at times in it--didn't know what was behind the door. Well, they lived a pretty good life there. They had Negro servants all the time. They had a cook, maids, and a choachman. They had an outhouse and a carriage and a horse. My grandfather would go to Tulane for classes in it, and to see his patients. That horse and buggy is in the inventory of his estate. It didn't amount to much.

Didn't you indicate that this house is no longer there?

It's still there. I have a beautiful picture of it, but it went to New Orleans with his papers. It's still there. It had a wonderful lot of iron work on balconies so that I could start on the ground level and put my toes in between iron roses and climb three stories.

Yes. I think there is a picture in the papers of that house with the iron on the front. I don't remember that it was three stories.

It was two long stories and an attic, a flat attic.

Yes.

Something I was going to tell you went right out of my head.

Is this the house in which you were born?

No. The house in which I was born was on Howard Avenue near Lee Circle.

185 Howard Avenue.

Yes, and my brother and sister were born there too.

Also Joseph Jones was greatly interested in Confederate records and relics. After the Civil War, he became Surgeon General of the United Confederate Veterans, and he used to wear a grey uniform, mostly part time, not a regular military cut uniform, but it looked like one, and he was collecting a great many records of the sick and wounded and the rosters of regiments and companies. A good many of them are published in the Southern Historical Journal.

He had wide interests, didn't he?

Oh yes.

Terribly wide.

He was a traveler in this country and then he went to Europe in 1871, and that's when he bought all the Jenners, three editions of Jenner's great work on the small pox vaccination with cow pox, and those originals that he bought are still in the Brooklyn Kings County library. He came back at that time, and he was Health Officer of the State of Louisiana. He got out a great big book on the management and prevention of small pox, and in that work he put the three original Jenner volumes including copies of the plates, just more or less in an order to his own fancy. Although he didn't change the text, he re-organized it somewhat, and Dr. Harvey Cushing has told me that this is the most remarkable Jenner in existence, very raer--I wouldn't say bodlerized, but it's the kind of thing Joseph Jones would do, if he wanted to.

He'd put his own gloss on it. He was also deeply interested in malaria.

Oh yes. I'm pretty sure that he saw the malaria parasite six years before the man that is credited with discovering it. It was published in 1876, in the Medical and Surgical Journal of Louisiana. It all appears in a murder case. It was his testimony that he gave in a murder trial, Narcisse Arrieux, and I'm sure that he saw it. I'm sure also that he saw the typhoid bacillus in intestinal lesions in Andersonville Prison in 1864, and that's a good many years before Eberth, the German, is credited with it.

Was the house set up with a laboratory?

Yes, he had two laboratories. One laboratory was in that office where he did urine tests and other tests. He could examine blood, and he was pretty good on blood. He was not shy about showing off his knowledge because in this trial the judge asked him,"Will you name the diameters and dimensions of the red blood cells of all the vertebrates?"

So he recites in this trial the size and shape and dimensions of alligator blood, elephant blood--any kind of blood because he had studied that with Joseph Leidy and published a Smithsonian monograph on it. Curiously enough his interest in snakes ran on to the end because the last paper he ever published appeared in the Journal of the American Medical Association, and the title was "The Ophidians". Now, people as a rule don't speak of snakes and alligators as ophidians, but he would write that word--just like his character.

When you stayed at the home, he was pretty old at the time wasn't he?

No, he was born in 1833, and he died in 1896, so that he was sixty-three years old when he died. Speaking of the influence of my grandfather, I have found that as I have gone along, I do things, have done things, just as he did them. I wasn't aware of that until I read some of his letters, or his journals, or in his books what he did about things. I worked just about the way he worked and I would do things the way he would do them without knowing it. I think it's an atavistic trait come in there.

Procedure determines the end.

Well, it had an influence on me in another way because it impressed me so much that I was doing things in the manner of my grandfather that I thought that I certainly could not outlive the life span that he had because he seemed a very old man to me. He was sixty-three. He died when he was sixty-three. I never expected to live beyond sixty-three. I'm now seventy-eight.

That could have a disastrous effect because I've had an uncle, my Uncle Charlie, who was an engineer, a geologist--what do you call a man who goes around looking for gold and minerals?

A Prospector.

Prospector, and he went to Alaska at one time--I think around the gold rush time, and he got some gold. Then he came back and settled down around

Los Angeles and was one of the early investors in the coal fields out there. Is that in the papers?

Yes, it is.

Mr. Kaiser--the Kaiser plant is out there because of this fuel. Well, my Uncle Charlie was in a very low state the last years of his life. He apparently had no money, was in a boarding house and I had a letter somewhere from him telling me that he was sorry that he was living so long, that he expected to die years before and had spent his money, and that now he had outlived his sustenance. We used to help him.

Is this the Uncle Charlie....

He was with my father in West Virginia.

You didn't see your father much after 1894?

No, I only have one recollection of him about that time, and that was down in that boarding house on St. Charles Avenue, about four blocks above us. At least that's where he was in a small little bedroom in the back ell of the house on the second floor, and I remember seeing him once there, but I don't remember anything else.

But he had been staying at your grandfather's house.

Well, if he did, I don't remember that. I suppose he was, but I don't think he was at my grandfather's house very long, was he?

No, not according to these papers.

No.

You describe a great free lunch counter that a kid can absorb in a house such as your grandfather's with arrows and the like. It's like a series of steady discoveries that you can go through.

Yes, and all of it is still with me and had an influence on me. I'm sure that my grandfather, probably more than my father, influenced me to go into medicine because I went into the study of medicine without giving it a second thought. I never had any doubts and I never had any agony such as some of my classmates at Yale had in trying to decide what profession they were going into. For me it was all settled by the stars long ago.

That's as good a way as any I know of.

Yes.

It might have been a tandem thing too because you were with your Dad in his office too.

Yes before.

So that you had the two. It was in the air you breathed.

Yes, that's certainly true.

What relationship as of 1894, did you have with the Baynes? The Baynes are the other side of the family, the legal side.

I can't pin it down to the year exactly.

I just say 1894, which is the year of your mother's death.

Yes. Well, the one that I remember best is my "Tante E", Mrs. Edith Denegre.

She married George Denegre, and she was the boss of that side of the family. She used to boast of the fact that she was known as a young woman there as "she who must be obeyed", but she was a very charming and lively person. She wasn't very well. She had some severe surgery shortly after her marriage, and she was never--well, I would say she was a very vigorous invalid--if you know what I mean. She was an invalid, but she did everything that she wanted to do, and she had people doing a lot of things for her. She had a great determining influence on where I'd go and what I did and when.

The next Bayne that I remember is my Uncle Thomas Levingston Bayne, and I remember him because he took me in to his house--he and his wife Gretchen took me in. They had three children already, so I was extra in that crowd. They lived first, as I remember it, down on 610 Royal Street which is down in the French Quarter near the Vieux Carré, in a great big house with one of these old court yards, funny creole things. My Aunt Gretchen was a Muller, no kin, of course, except through marriage. She came from a very fine family in Louisiana, the Governor Nichols family, and she was apparently a very lovely person and gay--maybe a bit of a faddist. I have a medical recollection of her. When I was down on Royal Street, she did something that has kept me from eating honey the rest of my life.

<u>Really?</u>

I was sick, and she thought I ought to have an emetic, so she made a soup, a tea out of violet leaves and put honey in it. As soon as I drank it, I threw up, and I cannot take any honey.

<u>That's a chancy thing.</u>

Isn't that a silly thing to put on the tape!

Not at all. It's the sort of thing that comes to mind.

Well, they used to go to Russellville, Tennessee, which is a little town in the mountains near Knoxville, every summer. Several summers we went there, and that was a very primitive place. Everybody seemed to get typhoid fever there. I didn't get it. That was a wonderful experience though to roam around in those marble bearing hills. I had a pretty free life because I was on the loose again.

Then they came back to New Orleans--now, I'm up to about 1898. They came back to New Orleans and built a house on 8th Street between St. Charles and Prytania, right next door to George Denegre. The yards were continuous, and I lived in that house with my "Uncle TL", as I called him, Aunt Gretchen, and their three children, T. L. Bayne, William L. Bayne, and Edith, a daughter. I guess I was a good deal of a nuisance because I can remember being locked up in rooms again and shrieking so that they could hear me all over the block, batting on the walls, trying to get out.

This is your mother's brother.

Yes, and then I used to visit with my mother's sister, Mrs. Vaught. Did you run across her?

Yes.

Mrs. Vaught had a punishment that I think conditioned my inability to keep my check book. I would go down to the Vaught's with my brother and sister, and Mrs. Vaught's children would be playing in the yard. I would do something that required my being locked up again, and this time they locked me in the back stairway. It was a long house with a long wooden stairway and a kind of closed in part going from the first floor to the second floor. It had one little cut window in the side of the wall far higher than I could stand on anything to reach. I could see out of it. They would give me a long sheet of paper, a yellow sheet, as I remember it, with figures on it and tell me to add it up and if I could add it up right, I would get out. Instead of adding it up I used to rush up and down the stairs and scream and beat on the wall. Then I would look at the figures on this yellow sheet and put down a number and stick it under the door. It would come back with my addition, my sum scratched out because I hadn't really added it up. Then I would have to go through this again. Meanwhile my brother and sister would be playing out in the yard. I could see them and this little window would let the sun come through toward afternoon. I never did get that column of figures added up. It got dark, and there wasn't any need to keep me there. They could send me somewhere else, but ever since then if I sit down with a column of figures in front of me, I just get the dithers.

<u>You want to reach for those stairs.</u>

No, I get the dithers and make so many mistakes. I'm fascinated by mathematics. I've taken course after course, and I got part way through into calculus once. I think it's a beautiful subject. The thing that thrilled me most one day was that I found that if you differentiate twice the equation of a falling body, you can come out with a value of the force of gravity--just by monkeying with the figures, but I can not work the materials because I make so many transpositions without even knowing that I have done it. I can't even see the mistakes. Are you that way?

No.

I can't see the mistakes. I get telephone numbers backwards. I don't trust myself at all when it comes to check books. To me 35 is just as valid as 53. I trace all these things back to this experience with the Vaughts. You see, the Baynes come in for disturbing my mathematical genius.

Then I can remember another experience with the TL Baynes in 1898--there were two wars about that time. One was the Spanish-American War, briefly in 1898, and the other was the Boer War, and I can remember sitting--I was climbing up on the back fence there on 8th Street and could see blue uniformed men going down to get on transports to go off to the Spanish-American War. That influenced me in a martial sense a great deal. In the Boer War they sold thousands of mules to the British, and these troops of mules with soldiers taking them down there influenced me a good deal. I always was interested in Civil War relics and Civil War Museums and would hang around them. I had almost as strong a feeling from the beginning for military things as medicine.

Well, was there any continuity with the Jones side of the family while you were with the TL Baynes?

Yes, I'd go off to see them sometimes. Oh, one period in there I actually lived--I'm not sure of the date--with my Aunt Mamie. She's Mrs. Bringier, and they had a plantation up in Ascension Parish, Louisiana, about sixty miles north of New Orleans, a plantation called "Tezcuco". I went there several summers, certainly one summer, and I had very severe malaria in that place. My Uncle Trist was a mystical sort of a figure who was in and out of the house with a shot gun, accompanied by two dogs, two pointers, or two large setters. He shot quail all through there. There was sugar. It was a sugar plantation. The Mississippi River was about a half a mile from the front door where these paddle wheel steamers, old show-boat-type things would go by against the sky. There were great, big oak trees there, so that I could build a house way up in an oak tree, and nobody could find me. The grass, flowers growing--it was a good ante-Bellum house. Now that happened in a couple of years, but not for the whole year. You can probably find the dates in some paper down there.

She's mentioned in this 1900 problem of schooling. This was a convenience in a way to allow you to get out of New Orleans for the summer time, or whenever it was.

In some of these episodes with the Joneses in ways that I never understood I would be transferred back temporarily to the Baynes. I recall this very much. I was playing with my brother and sister at Mrs. Denegre's house at Prytania and 8th Streets when a little cart drove up with a Negro on it, pulled by a horse, I guess, and on it was a little trunk and a little white iron bed, and they brought the bed and trunk into the house. I changed houses by that metamorphosis. Nobody asked me.

Just baggage.

Don't think that I'm reciting this with any particular grief because I just took it as it came.

Part of the order of existence.

I wasn't embittered. I was bad in many ways, as you would call it, but I don't think I was on the verge of juvenile delinquency which is supposed to happen in a case like this.

Well, you know, having a place where you were secure--you had a hero kind of attitude toward Dr. Joseph Jones, and by comparison everyone else must have paled just a little bit. They didn't have--you know, the accumulated things that he had around to keep you occupied, keep you intrigued.

Oh, going back to the things that Joseph Jones had--he had brass cannon, and we used to load those cannons with gun powder and nails and fire them off in the backyard--anything. A wonderful time!

Then the other thing that comes in about this is my going to Dixon Academy. Did you find anything about that?

Just a brief mention of it.

Well, Dixon Academy was a boy's school at Covington, Louisiana which is across Lake Pontchartrain, behind northwestern New Orleans. Mr. William Dixon who ran the school was the son of the President of Newcomb College. My Jones family knew the Dixons very well and somehow or other I got to Dixon Academy in the period--I would say 1900 to 1905, somewhere in there. It's hazy to me now, but that was a boy's school in which they had pretty good standards. I was there, and I played baseball. I got tremendously interested in water moccasins. I used to go out with a little leather Kodak box--you remember those square boxes?--on my side and wander around in the swamps. They had a very strange river that went back of that school. I could smell a moccasin a long time before I got there. Then I'd find this creature dozing on a log, or a branch over the stream. I had a forked stick, and I'd go put it over his neck and put him in the box--you know, these are very poisonous, big mouth snakes--and bring them back to school. I had twenty moccasins in the cellar of that school and the principal didn't know it. I was feeding those moccasins on newly hatched chicks that belonged to the principal of the school. I didn't think that it was stealing chickens. I just thought that I was being good to moccasins. These creatures didn't frighten me any, and I was fascinated with them. I caught black snakes and king snakes. They had coral snakes too, but I didn't catch many of them.

<u>They are the deadliest, aren't they?</u>

The moccasin is a pretty bad one too.

<u>What did you do with them in the cellar?</u>

They all got loose. One night they all got loose.

<u>Oh man!</u>

I had them. I'd just go down and look at them and talk to them.

<u>Was it ever discovered? I guess it was.</u>

Oh yes. I had plenty of extra tasks at Dixon because of various behavior.

Then another thing that I never doubted was that I was going to Yale. My grandfather, Thomas L. Bayne, was in the class of 1847 at Yale. My Uncle Hugh Bayne was in the class of 1892, and I had been brought up on Yale, so I was always sure that I was going there, but nobody gave me any directions for the mapping of my course of study. At Dixon Academy I took a little bit of everything. I studied history. I got pretty far in Greek. In Latin I got up as far as reading a part of Virgil. I had French. It was a pretty good schooling, but it was not very well organized.

In 1905, Mr. Denegre had a hand in sending me--no, I had gotten by that time, around 1905, back under the nominal control of the Denegres. I wasn't living with them. I was off at this boarding school, and it was about that time, 1901, or 1902, that I came back from Dixon Academy one Christmas and found that Mrs. Denegre and Mr. Denegre had hyphenated me. They didn't ask me about that either. I didn't care. It didn't seem to make much difference to me, but later I found that I had to be known as Bayne-Jones, and it had a tremendous influence immediately on my life and has ever since been influential. In the first place, when I was Jones, I sat in the middle of the alphabet, in the middle of the classroom and inconspicuously. When I got to be Bayne-Jones, I was in the Bs and sat in the front row, so that I was questioned much more than I was when I was in the comfortable mediocrity of the Js. I began to study more.

That's wonderful.

To continue with this hyphen--I became self-conscious of my name, and I don't believe people as a rule are self-conscious of their names. I had to explain, and I still do, that my name is Bayne-Jones and listed under B. It still is a nuisance because people come to Washington and look me up in the telephone book under J and think I've moved. I've missed a good many friendly calls that way. In addition, I've had an account at the American Security and Trust Company for going on twenty years, or more, and every time they change the tellers I have to explain to them that my card is under B, so I go and present a check and they look it up under J and they say,"I'm sorry, but you have no money here."

Well, I've had a deposit there for twelve years, and I have to explain my name over and over and over again.

It has the advantage--well, it's got two advantages. One is that it helps me to sort out my friends. There was a great chemist named Bence-Jones, and people who misname me and call me Dr. Bence-Jones, I know right away that they are chemists. There was a great poet and artist named Burne-Jones and when people call me Burne-Jones, I always turn on the literary parlance, or whatever you want to call it. I can tell their backgrounds by the fact that they mistake mine. The other thing is that I'm quite sure that it has been a reason why I have been elected president of at least three scientific societies because people think I'm Jones--J, and they think that the list put out by the nominating committee is in order of preference; whereas the nominating committee is putting out a list alphabetically and people tend to check off the first name on the list and being moved from the Js to the Bs I have been first on quite--well, practically all these lists, and I get all the votes. That's a fact.

<u>It's an ill wind.</u>

That's a good one.

<u>Tell me this--did they ever give you any rationale as to why this was done--the Baynes being Uncle George and "Tante E"?</u>

Yes, my opinion is that they hated the Joneses.

<u>They did?</u>

They hated the Joneses, thought it was disgraceful to be identified with the Joneses, practically told me so.

<u>That's strange because there's nothing--well, she writes you very gentle and tender letters.</u>

Who?

<u>"Tante E".</u>

Oh yes, sure. That wasn't me. It was the Jones people that they didn't think well of--my father. Also they thought that Bayne was a distinguished name, and Jones was just run of the mill.

<u>That's the feud aspects of this.</u>

Yes, that's why I say--and I love both sides of the family--I'm a missing link between two families in a feud.

<u>They did this without consulting you.</u>

Yes, I came back from Dixon Academy and found that I had been hyphenated.

Well, to finish out about this Dixon Academy and Yale. My Uncle George through Yale friends had heard about the Thacher School which is in the Ojai Valley in California. It was founded by Sherman Day Thacher in the 90s somewhere, I think. It was an excellent school, forty boys and about ten masters, and we lived in good old California ranch buildings. Every boy had a horse, and you took care of him yourself, and you roamed all over the country in parties, up in the mountains, camping--wonderful! They looked me over. I was going to Yale, and I had no orderly preparation for Yale, so I prepared really for Yale in one year out there. At the end of that year I took nineteen entrance examinations for Yale, and I failed only Advanced Algebra--again my mathematics. We played baseball and rode horses. I don't know how they did it with me, but it was all done in a year out there.

The reason I think it was easy for Mr. Denegre to send me to Thacher School in 1905, I guess, was that he was a very important railroad attorney. He was the attorney for the Louisville-Nashville Railroad, and attorney for the Southern Pacific, and all of us travelled on passes in those days. I came home from Thacher School just for Christmas week on a pass, and later I travelled to Yale on a pass, so having a pass, it was easy enough to do it that way.

<u>Tell me something more about that school--people, teachers. What about Dixon School? Was there many people that fire imagination there?</u>

Dixon didn't. Mr. Dixon--I forget his full name now--was a tall, austere man. He lived much to himself, though we saw him in classes and in the dining room, and then I think he would have certain evenings when he would read Dickens, or something, to the boys. There was a chemist there whose name will come to me after a while--a big, hearty man who influenced us a good deal in sports--basketball and baseball. My professor of Greek was a shy, little man, and he influenced my morals unfavorably because he was afraid--I being his only scholar, so that when the time for the examination came, he gave me a set of questions, and he also gave me a dictionary, a grammar, and a trot. He went out of the room, locked me in the room again, so I got a high mark.

<u>It's a great language incidentally, quite apart from your experience with it.</u>

Oh, marvelous--I like it. No, I don't remember the characters at Dixon

Academy that way very much. Curiously my recollections are mostly earthy recollections--snakes.

Yes--well, that traces back to Grandfather Jones.

In those days we had a good many fist fights in Louisiana.

In the school?

In Louisiana. It was a ritual. See that knuckle?

Yes.

Well, I broke it on the side of the baseball pitcher's head one day. He was taller than I am--he was six feet tall, and I reached up and hit him on the head. I broke my fist, and I couldn't tell him. He wouldn't stop, and he beat me for about a half an hour. The ritual was interesting. This fight began because we were both in love with the postmistress who was about three times as old as each of us, and I said to him one morning,"Did you say what I heard you had said to that lady?"

He said,"Goddam you, what is it to you?"

So I had a Saturday Evening Post in my hand, and I was still a knight, so I slapped him in the face with the Saturday Evening Post. Nothing happened then but we arranged to meet about a week later in the evening on the basket-ball field, and that's when I had this fist fight.

Another one showing you the ritual was at Biloxi a little later. I took a young lady riding. I borrowed my Uncle George's horse without asking him and took her riding. She had a horse and at Beauvoir near Jefferson Davis' house her horse stumbled, and she fell over the neck of the horse. She was riding my uncle's horse, and the horse started to go off down the bushy road. I looked at her, and I saw that I couldn't do very much, but there was my uncle's horse going off, so I dashed after and caught the horse. When I came back she was sitting up and rather angry because I had shown no chivalry at all in spite of being a southerner. She had a friend who was a powerful, young football player, and he challenged me to fight. I had to wait two weeks for that fight--worse than Cassius Clay's periods, but it was--well, we had seconds. We exchanged insults, and you had to wait and calm down, and then you had to get all mad again.

Was this a sought thing--something that you sought?

What?

Fights.

That was in the ritual of the life of the boy in Louisiana at that time. What do you mean by sought?

Was this the sort of thing that you would look for?

Oh no. I was scared to death.

I wondered.

I think the others were too, but you had to do it. That was the mores.

What about the Thacher School--since they prepared you for Yale?

They were awfully good, strong characters. Some we liked, and some we didn't. Mr. Sherman Day Thacher was the headmaster, and every evening he would read to the boys gathered after supper in a big room. He was a bit remote, but a stern disciplinarian. He had a brother, William L. Thacher, who was a big man, a national tennis champion, and we admired him no end. He taught Latin. We had a very interesting professor of physics who taught us some physics and chemistry, a Mr. Avard L. Dodge. The way they got me along, the way Mr. Thacher brought me through all this Latin--he just gave me a trot, a translation. I would read it and remember it--that is, for a while.

<u>In the light of subsequent things, how did the physics and chemistry....</u>

Physics and chemistry were elementary things at that time.

<u>Was there a laboratory available?</u>

Yes, the school had a small laboratory. I did at Yale mediocre work in chemistry and less good in physics. Physics required too much mathematics. I'll get into physics later when I finish up on telling you some more about getting ready for Hopkins.

<u>I think we've gone about as far as we ought to go today. You're getting tired?</u>

Yes.

<u>Well, that wasn't so bad, was it?</u>

Wednesday, April 13, 1966 A-54, N. L. M.

I have been wondering since the other day when we talked, whether you had any thoughts about it.

My recollection is that it seemed a little jumbled up to me, but I don't know how you ordered it.

I liked it. I transcribed it, and it's good.

Is it?

Yes, for the purpose we originally talked about, it's good--the two camps, in effect, but I want to go back today and pick up some themes which we may have overlooked. They may not figure, I don't know.

Is this thing recording now?

Yes. Religion is--you know, a strange thing, part of the inheritance that one has, and I wondered about yours in this pin ball process that you had--how much continuity there was, how important it was, how much it figured. I don't know.

Well, I think rather early in my life, perhaps because of this moving around from one doctrine to another, I got a feeling that there was a great difference between what I later learned to call religion as distinct from theology. Now, I went through a Catholic phase--as a matter of fact, I was christened a Catholic. My mother was a Catholic, and she was a Catholic because my grandfather, Thomas Levingston Bayne, embraced catholicism, as they said, on the death bed of his wife, and that was carried over to his children. My brother and sister were brought up Catholic, but the Denegre family that brought them up are very liberal Catholics. They are not oppressed by the regulations of the diocese, or the church, so I think I very early noticed a distinction between the inner deep feelings you might have about the mysteries of the universe and some controlling influence. I never believed in a living God, except when I was threatened with him like that story I told you about the moon, so I had no strict continuous theological, or religious upbringing by anybody on the Denegre, or Bayne side, but when I lived with my grandfather Joseph Jones, a very strong Presbyterian influence came to bear through Mr. Mallard, who was a minister in the Presbyterian Church, and that was a long and painful experience for me because I would have to go to church in the mornings--has it stopped?

<u>No, it's going fine.</u>

I would have to go to church in the mornings, and then come back to a big mid-day Sunday dinner. Then I can remember sitting on the side porch with the older members of my family--whoever they were, and I cannot remember now--and I was required to give the gist of the sermon. That didn't make me love those Sunday ceremonies very much as I might have done otherwise. It was a very severe sort of lesson because I was supposed to say something that fit in with what the minister had said. I couldn't just make it up.

Then I passed over into the side of the family that was more interested in the Episcopal Church--that picture in Grace King's book of St. Paul's Church downtown. I went to that church for a while and sang in the choir there, but behaved so badly that they wouldn't let me come. I got in a fist fight in the choir.

As good a place as any.

That rather put a damper on my going very far in the Episcopalian Church, but I never studied any of those things to the extent that it would lead to confirmation. I never was confirmed, so here I am a baptized Catholic without confirmation in any church, and I have had experience in the Catholic Church, the Presbyterian Church, the Episcopal Church, and the free sort of Congregational Church at Yale.

Right. I think it's typical of the times for the church to mean a great deal to families, but you had this pin ball process--you know, and Sunday with the Joneses may have been severe, a day of rest, a day of good clothes, a day sitting on the porch, not exploring the way you were wont to. That must have made it very depressing. That I can understand, and then to be passed off to liberal Catholicism, and from there a flirtation with the Episcopal Church is a variety of religious experiences that other people don't have. They have this deadening, unitary sense, most of the time. They go almost automatically and go through a process which leads to confirmation which they never really understand.

To jump ahead, I found by the time I went to visit Mr. Andrew Dickson White in 1906, I think, a very satisfying, huge, two volume treatise that he'd written called The Warfare of Science with Theology. Do you know that?

Yes.

He got his Ph D in Germany on that book, and there's a great deal in that that influenced me a good deal--satisfied things that I was wondering about.

Tied things up?

Yes, it distinguished between the scientific things that I was interested in and the church's opposition to scientific advances. I appreciated very early that the church was always taking a position--well, science would move ahead, and the church would try to catch up. One that impressed me then was the controversy over lightning rods on steeples. Lightning is a bolt of Jupiter, but it also comes out of the hand of Jehovah, God of the Jews, and Lightning rods were a profanation of a church at one time. Now they have them everywhere. The astronomical problems that were settled by Galileo and people like that interested me very much.

Was there added religious study at either Dixon, or Thacher?

No, there were prayers. There was a routine kind of prayer which, I must confess, you attended while thinking of something else. They weren't prayed with any fervor, as I remember. It was an exercise.

Part of the order of the day and not oppressive.

There is another church that I belonged to that didn't affect my religious feelings, but when I went to England to join the British battalion in World War I, they asked me when they were making out my dog tags, what church I belonged to. I said, "Episcopal Church."

They didn't know what it was, so they said, "Oh, you mean the Church of England."

I went through World War I with a C of E tag around my neck.

Marvelous! We talked yesterday, in some sense, about security.

May I say one more thing about this religion?

Surely.

I'd like to convey a sense of reverence for something, but I think it's childish, juvenile stuff--the furor that is going on now as to whether God is dead. I never thought he was living, and yet I have a respect for something, though I don't know what it is. I don't believe in any life after death, and I don't believe in all this that I used to recite about the resurrection of the body.

Those are pepsonized things that one absorbs--yes, but you at least had a variety, so that you could possibly discern distinctions and raise problems about it. It was good that you had the talk with Andrew D. White, one of the accidents of going to Thacher School in a way.

You were talking about security?

I was talking about security in terms of what we said last time. It would seem to me that the sense of security was related to the cart with the white iron bedstead and the trunk--everywhere you went that went with you. Whether it went in accordance with a request to you, or whether you knew about it--at least they are things that remained constant.

That only physically appeared once in my experience.

Oh?

The cart, the bed, and the trunk only appeared once.

I see.

But if you are making a figure of speech out of it....

I was thinking of them as your lares and panates, known things.

Probably every time I did change there was something, but this change I remember because he drove up to the front gate.

Yes. I was also thinking about eating habits being bounced around from place to place. How were you as an eater? I know about the honey, but food.

I had no fads about food. I ate what was there. Only one or two things I just don't care for, and these are beets and turnips, but I don't know why that is. They have a consistency that makes me gag. I ate anything, and as far as eating fish on Fridays, I would expect that, but I like fish anyway, and this Catholic side of my family--I don't know which member--raked up a Papal Bull that one of the Popes had issued on Lake Garda in maybe the 15th Century. He didn't have any fish that Friday so he issued a Bull and declared that teal duck was fish, so those Catholics in that group would eat teal duck, or any duck on Friday.

It was comforting to have that rationale, wasn't it?

Yes, and when I learned that it was all a matter of protein, they ate eggs on Friday--that's not too far from a chicken.

No--not too far. What about sleeping habits since you had a succession of rooms--I mean nothing that you could really call your own with any continuity.

Well, I was fortunate in most places in getting a little room, or a piece of a room--often I had a little room to myself, and I don't recall any trouble sleeping anywhere at all.

I wondered about this because you were a young kid--six to twelve, or thirteen, before going off to Dixon Academy, and the requirement imposed upon you was one of almost continual adjustment--you know, facing something new, so I wondered about sleeping habits and eating.

No, I don't recall any problems like that. I don't recall any problems of adjustment particularly. Maybe I was too shallow a person to be bothered by them.

But you did have the sense in joining the various families where there were children, of being something on the outside.

Yes.

Yes, and I wondered whether that had any carry over--like being locked in that back room, that back stairway. That's not an easy experience to absorb.

It didn't leave any long scars. It just made me mad--that's all.

All of these people--and I'm taking advantage of a certain snobbishness which comes from having looked through the war time letters--I get the impression, and I may be wrong, but I got the impression from the war time letters that as of that particular period people began to mean more to you. The choice between working in the laboratory in the rear and working with the troops was dismissed completely almost. You had to be with the troops. The letters home--the first real notions of "I'd like to be once again at Biloxi, Mississippi" when you knew that everyone was going to be gathered there. I don't know, and this may not be so, but it appears from the correspondence that people qua people began to mean more to you later than in this early period, and in this early period I wondered whether they meant anything at all--people.

As I look back on it , and I think I still have some of the same feeling about people, they were existent beings, but I was rather aloof from them. I think the kind of a life that I was leading, going from one place to the other, made me a bit secretive and want to withdraw from them. I think I avoided exposing myself to either criticism, or contentions, or to the opinion of strangers. I had a sort of life to myself, and I think it still continues.

Sure--like describing the hut you can build in a tree where you can't be found.

Yes.

Or, for example, the collection of water moccasins. You could go down and look at them. These were friends to a kid, friends that you could talk to where perhaps you either didn't, or wouldn't risk conversation with someone else. For you these were, as I say, friends.

I think that's followed me through my life because even though I know hundreds of men I don't have any particular close friends.

I wondered about that. There couldn't have been very much in terms of local neighborhood friends in this early period because you weren't there long enough to put down roots.

I was just speaking of my associates in Yale, or medical school and others. I still have long associations that are of an affectionate and frank kind, but they're not what you would call an intimacy particularly.

Yes--well, the only one I've run on to, and this was at Thacher School, was Andrew D White II who formed an attachment for and had respect for you. The letters he wrote to you are filled with humorous allusions and questions of

<u>a real, quizzical minded fellow which showed certainly respect for you, regard for you as a kind of guidein a way.</u>

Well, that always surprised me at the time, and when I have thought about it, because he having been brought up in a highly intelligent and literate family, almost an American aristocracy in a sense, that was different from the kind of aristocracy that I was talking about in my family which could match his, but he had a grandfather who had been, as Mr. White was, an Ambassador to Germany, a great collector of books, a president of a university, and a man who had travelled all over Europe and had taken this grandson, Andrew D. II with him, and so Andrew D. II to me was a highly cultivated and educated young fellow--way beyond what I was at the time, so much so that when I first got to know him, I just thought he was artifically polished. You know how a country boy would think about a city boy?

<u>Yes.</u>

But how he happened to find something congenial in me was astonishing to me because we were very different.

<u>Well, in looking toward Yale--we got you prepared at Thacher School. Yale is a great distance away from New Orleans, certainly a distance away from the Thacher School. You had people in the Bayne family who had been there and gave you, I assume, a kind of orientation toward what you might expect at Yale.</u>

They gave me more than an orientation. They gave me a feeling that that was a part of the natural life of one who was related to TL Bayne and Hugh Bayne. You see, I saw my Uncle Hugh very often during that period when he would visit New Orleans. Then he married Helen Cheney from Hartford, the Cheney Silk people, and he brought her to New Orleans to a house he built on Harmony Street, where my sister went later after my Uncle Hugh and his wife had gone. Hugh Bayne was in New Orleans, and he talked a great deal about Yale. He was a highly social person. He belonged to the Delta Kappa Epsilon fraternity, and I used to hear a great deal about that. He talked about torch light parades. He could sing. He could play the piano. My Uncle TL Bayne Jr. was a very nervy, little football player at Yale in the class of '88, but he didn't make the grade, and I learned about Yale from him. I learned a great deal from Hugh Bayne who was a very attractive person. My mother was devoted to Hugh Bayne, and I think I carried over some of her feeling without knowing it. I don't know whether you came across the last scene down there in the house when she was dying. She begged Hugh to stand by the foot of the bed and whistle tunes that she knew.

<u>Oh yes</u>.

And he did, and that's sort of a sentimental thing that is carried along in the memory that makes the Yale connection feel warm too, but Hugh Bayne was a romanticist about Yale. There was nothing else but Yale. I don't think I thought of any other college. My grandfather had been there, and I tell the story about my grandfather TL Bayne--I don't believe it's true, but he used to say that he wanted to get to Yale so bad in 1847, that he walked all the way from Mobile to New Haven to get to Yale. That's a traditional story, but it is an example of what I mean about the desire to go to the place.

Then I went to Yale without knowing anybody which also is something you might have thought about ahead of time. Going to Thacher School and going to Yale where there were only about two Thacher School boys besides myself, and knowing nobody else, you were rather isolated as compared with the people who come from Andover, Exeter, and Hotchkiss. They come in by the dozens and were the big shots right away, so again I felt that I was quite isolated at Yale. It took me a good while to find people. I got to know some of the men in my class early who were not first ranking in the social life at Yale at the start-- some of them came along. My Uncle Hugh did a good deal. He introduced me to a good many people. He had a place then in Bronxville, New York, and he used to invite me and some others down for a week end, but when you come from a small out of the way school and go to a big populous place like Yale, and you know very few people, you often walk around the streets and look at the lighted windows and hear people laugh, and you haven't got anything to laugh about.

<u>You wonder what it's all about.</u>

Yes.

<u>How did the course work go at Yale?</u>

Well, it got better as I went along. When I went there, immediately, as soon as possible, I tried to do something because Yale, they say, breeds guts, and you have to do some extracurricular work to justify the tradition of manliness at Yale, so I started to heel the <u>Yale News</u>. By "heeling" you go out and try to get copy that will be published. It was very, very strenuous because you'd go to class, and then you'd start to work and try to find things for the <u>News</u>. That would take all your evenings, and you had very little time to do any home work, so I didn't do too well in my first two years up there. In my sophomore year I remember I got a letter from Dean Henry P. Wright, and all it said was,"Sir: Your work in the following subjects is unsatisfactory", and he named everything I was taking, and he implied, I thought, that if I had taken any more, they would be equally unsatisfactory.

That's good.

I stuck at the News, and it took me three competitions. A competition lasts a half a year, so a year and a half I spent trying to get on that paper. Then when I got on the paper, I became managing editor, and things got much easier. I didn't have much else in the way of competitive things to do up there. That was enough. I was manager of the freshman track team a couple of years running, and I tried for the crew--the freshman crew, but I weighed about a hundred and forty-eight then, maybe, and I was too light for the crew.

That is one place where my name came in for advantage. The crew coach, Mr. John Kennedy, had been brought up in some kind of uncouth, muscular environment that was quite unfamilar with a hyphen. I used to get two practices. He had me on one list as Jones and on another one as Bayne, so I got two rowing practices a day. Then one day he called out for one scull for Bayne to row number 5 and Jones to row number 7. He was a humorous Irishman, I guess, because he looked at me when I stepped out and answered for both names and said, "Well, I don't know what I can do, except put both of you in a pair by yourself."

Then he dismissed me from the squad.

Well, the extracurricular activities become important--you know, it is "the thing." I don't know why, except that it seems to be a key to social things.

Well, Yale grades on a four level, and I think for my first two years I was running about two-fifty. In my senior year I think my grade was up around three-thirty, or three-forty. Then when I went to Hopkins--I don't remember working too hard, but I graduated first in my class at Hopkins and was made Phi Beta Kappa, so that wiped out some of the earlier disgrace.

<u>Was there any--Yale had a good faculty in those days.</u>

Oh yes, they had some very fine men--Wheeler in history and Sumner in economics.

<u>I guess you took whatever was required of you.</u>

It was a rather rigid curriculum in those days. They didn't have many optional courses as they do now, but I was more or less heading for medicine, and I was guided there to take physics and chemistry. I went through organic chemistry which was very hard for me and physics--oh, maybe two years or more. I studied some more Greek and some more Latin. I studied French and history and English. The English I liked very much. There were two great teachers there in my time--William Lyons Phelps and Chauncey Tinker who was the great Johnson scholar.

Well, then not far from where I lived--I was rooming near the campus--they had the old Peabody Museum. It's torn down now. It's built in a new place. The Peabody Museum was just a mass of messed up collections of dinosaurs, fish, pterodactyls. One thing they had was Marsh's Evolution of the Horse, and I remember how that just intrigued me to learn how this thing started with five toes and came out with a single hoof. I thought it was wonderful.

<u>It is. But English--this journalistic escapade that you went into was something new, wasn't it, or were you on the paper at Thacher?</u>

No.

You were not.

I think I went out for the Yale News just because it was a conspicuous thing to do. Maybe it was something that somebody told me I ought to do. I didn't do anything in any literary sense at Yale. There was a man, Professor John Berdan who had a course in what he called "minor themes". You had to write a short descriptive thing, or something, and I scored a few on those, and then Professor Phelps formed a club called "the Pundits" which had very bright people, Wayland Williams, Lawrason Riggs, and others, and somehow or other they took me into that. That club met at Mr. William Lyons Phelp's house, and they discussed Chaucer and all sorts of esoteric things.

I roomed with a very able literary young man named Howard Vincent O'Brien. I spent two years rooming with him. Did you ever hear of him?

O'Brien is mentioned somewhere.

He's dead.

Yes.

"Pat" they called him then, and he at that time had made two of the main Yale magazines, the Yale Literary Magazine, and the Courant, as they call it. He was a very clever and facile writer. After graduation he became a member of the staff of the Chicago Daily News, and he wrote for years a column called "All Things Considered." He's about as close a friend as I ever had there. We roomed together in Durfee Hall one year and a place called Haughton Hall the other year, but we went rather different ways. Well, we were in the same fraternity together. That's Zeta Psi, but he went into Wolf's Head senior year, and I went into Skull and Bones which is also something that I just thought I was going to get. I remember very well tap day. I wasn't worried at all, and lo and behold, I got tapped.

<u>Subsequently, and there's correspondence about this, your brother Bayne, his sophomore class took a jaundiced view toward what tap day had become.</u>

Not only tap day, but the senior societies. They changed tap day a great deal. The students revolted against it. For years they just thought it was a horrible, barbaric torture for these people. For instance, President Brewster, president of Yale now, Kingman Brewster--he was a brilliant student, the kind of man whom you think would be chosen by Skull and Bones. Well, when tap day came in his class, he went down in the basement and sat on the toilet during tap day. They couldn't find him. He was revolting. My brother went Skull and Bones, and my brother was in the class with Dean Acheson--1915--and Dean Acheson went Scroll and Keys. A number of others, men who have become rather prominent--I forget all their names--but they worried me and my Uncle Hugh because they didn't know what they were doing. They were revolting against the senior societies that had a good deal more to them than appears on the outside. People make up fictions about what they do.

<u>Yes. Tap day wasn't an odd day in your class.</u>

No. We took it all very naturally and expected it. I think I came through a curious kind of age up there. We were quite conservative--at least the people I was with were, but I felt on the verge of change. I've always thought that the year 1910, marked the end of an era and the beginning of some new things at Yale. I'm sure that's so, if you analyze it. I never went into it too deeply, but we didn't worry. I didn't worry. Fortunately I didn't have to. I was standing out there under that tree and one of these senior Bones men stepped up behind me. That was about five minutes before the bell rang so I didn't worry.

<u>This was, I think, important to Hugh Bayne too.</u>

Hugh Bayne, yes. Well, I can say that having been in that senior society, Skull and Bones, I know that there is no packing. There is nothing said to anybody in advance. No older person dares to say anything to the people who are making the elections. That's not true of the other societies up there to the same extent. I know it wasn't because we have the lists, but Hugh Bayne wanted me, of course, very much to be in the Bones. My grandfather was in the Bones, and Hugh Bayne was, and I'm sure he must have done a good many purposeful introductions, but he never laid a finger on the process.

<u>I only meant to indicate that he was pleased.</u>

Oh, well--he was not only pleased, but if it hadn't happened, he probably would have been grieved.

<u>That's true, but he was pleased. Did you have--how about study habits in this period, along toward junior and senior year?</u>

I studied quite hard. I wasn't very bright about it because I would study like the dickens with chemistry and physics. There was a boy in my class named Marsh Klock Powers from Cleveland, I think. He would borrow my notebooks just before the examination, and he'd get an A, and I'd get a C. He was quick.

<u>You attended class and took notes which is a new process too.</u>

Yes.

<u>Listening to a professor, getting down what is called "the meat".</u>

I changed those habits when I got to Hopkins. As I said, and this is not boasting, I was the first in my class, but I don't remember making any special effort to study. I took some notes. I read a great deal. I read much outside the textbooks. I was disgusted with one of our professors who used to take Osler's <u>System of Medicine</u> when it was one volume, and he would stand up there and read it to us. He'd say, "Now, underline this sentence."

He'd go right through the pages, and you'd underline all the sentences. This was mechanical, but in my senior year at Hopkins I came up with an intense interest in the Negro question, and I spent practically most of my class time reading books about the South and the Negroes. I'd sit there with an open notebook, but with this other book inside it, of course, turning the pages. I had no trouble in the medical school getting along, and I had some good friends--that is, of the kind of friends I had.

<u>Did you get to know many of the professors at Yale?</u>

At Yale?

<u>Yes--in the sense of teacher-student.</u>

No.

<u>They were remote?</u>

Either they were remote, or I was in a fish like stage at that time--if you know what I mean--squirming out of the way of things, but I got to know Mr. Phelps somewhat, and I got to know President Arthur T. Hadley somewhat. He was a queer man, but very nice. I don't recall going out very much to anybody's house. I had no particularly close relation to any member of the faculty.

That's almost par for the course, I think. Did any member of the faculty stir you up, kick open a window or two?

Well, I think Tinker did. Of course Tinker was a wonderful teacher of Samuel Johnson and the 18th Century, a very polished lecturer. Mr. Phelps aroused my interest in plays--Pinero, and Shaw and those things very much. Curiously enough, I didn't see much of a medical side at all when I was there, or I don't remember.

Well, medicine lay up ahead, and you said the other day when we talked that so far as chemistry and physics were concerned, there was a certain amount of preparation toward medical school that you took more seriously at Yale than you did at Thacher and Dixon.

I can add to that French and German because we were supposed to be able to read French and German. I could read German with a dictionary all right.

How did you do in chemistry and physics? You indicated that you went through organic chemistry, but how did you like laboratory work?

Very much. Oh, you recall something to me that I liked extremely well. I got along in chemistry well enough to be accepted by the great physiological chemist at Yale--Russell Chittenden.

Really?

Yes, and I took the course with Chittenden and Lafayette B. Mendel over in the old Sheffield Building. You know, that's so identified with my medical work that I hardly consider it a part of my undergraduate work, but that laboratory was extremely interesting, and Mr. Chittenden was too, and we did a lot of interesting things.

That's like getting familiar.

Yes.

Was it a well appointed laboratory?

No. Oh, it was a tumbled down place. It was the old Sheffield mansion, a living house that had been made over on Hillhouse Avenue. I suppose they put laboratory benches in the old parlor and places. It was very crowded, but that was all right. It seemed fine to me, and we did all sorts of physiological and chemical things. Lafayette Mendel was an interesting person, but Mr. Chittenden was the lively one.

Was he a critic?

What?

Was he a critic?

Yes, he was a critic, and a tremendous worker. He was Dean, not founder, but Dean of the Sheffield Scientific School, and he was very ambitoius for that school to wipe the eye of Ylae College, so that there was a conflict then in a way. Now they're all merged. They're in one university now, but Mr. Chittenden was responsible for introducing to the Sheffield Scientific School a course that he called a "select course". The men could go into that and not take any science at all, and they got through easier in their literary studies and in their studies in the humanities than the students over in Yale College across the street. This was called a "select course".

He was something of an innovator then.

Yes.

When it came time to go, or to think of medical school the indication that I have is that you studied one year at Tulane, 1910-1911.

I always wanted to go to Johns Hopkins. I never thought of any other place because it seemed so admirable to me because of the people who were there and their ideas, but as I approached the time to go I surrendered to the wishes of Mrs. Denegre. My brother was at Yale, or coming to Yale. He was still at the Thacher School then. My sister had married and had moved out of the Denegre house. Of course she was across the street. This is a true story. Mrs. Denegre had a little white poodle called "bouche troue". Do you know what that means?

No.

It means "stop gap". You see, a bouche is like a cork, and a troue is a hole, so they called the dog "bouche troue", and Mrs. Denegre wrote me that Marian had gone, that Bayne wasn't there, that she and Uncle George were lonely, and that "bouche troue" wasn't enough, so "you'd better come home."

I didn't have the nerve to stand against that. I went to New Orleans. I got into Tulane. I don't know that the financial side had much to do with it, but I got a scholarship because of my grandfather Jones having been a professor there, and I imagine that relieved them of some expense. At that time and all through Yale I was getting a thousand dollars a year from a fund I thought was inherited. I don't know who put the money up, to tell you the truth, but I got a check for eighty-three dollars and thirty-three cents a month, and I lived on that--tuition and everything else. Well, anyhow I went to New Orleans in the fall of 1910, after I graduated from Yale, and I entered Tulane.

As I look back on it now, Tulane was a rather raucous, rough time of country boys who had very little education. They didn't have to have much more than a high school education to get into Tulane at that time. For instance, the first anatomy lesson rather shocked me, as it does most students. Alan Gregg wrote a book for doctors, and he said,"Medical students certainly don't realize before they begin that their course of instruction is rather like a prize fight. They meet the cadaver first and shake hands with the adversary at the beginning."

Well, you entered medicine in those days through the dead house, and I had a different feeling about it. To go into those big dissecting rooms that smell bad and see all those bodies lying out there--for me, who had had some experience with medical things, it didn't look too well. It didn't smell very nice. It was a bit of a shock, but some of those other boys didn't mind it so much. I remember that two of us were on a cadaver to start the dissection. You were supposed to work out all the muscles and nerves very carefully, and this Georgia boy working on the lower part with me took a big knife and went around the buttocks of this cadaver and cut one side of it off like a ham and took it to the professor. He was finished with his dissection in five minutes. That was typical.

The other things I told you about.

<u>The tape recorder wasn't running then, so please put them down.</u>

I got to be president of that class pretty soon. Our histology course was quite interesting, but Professor Irving Hardesty who taught it was quite strict, and you had to draw pictures of microscopic sections, and you were supposed to have in your drawing exactly the number of nuclei that were in your section which was pretty hard to do. Then he had a course in neurology which is an extremely difficult course anyhow, but the boys thought that it was put in there to spite them, and they asked me as their president to go to the trustees to get Mr. Hardesty to take the course out of the curriculum.

The Professor of Chemistry, Mr. Abraham L. Metz, was a strong and vulgar and rather interesting chemist, but taught in a rather unorthodox manner. He--what I said to you the other day--would put Na on the board and say,"Here's Mr. Sodium. He's blonde and debonaire, and he's walking down the beach."

Then he'd write Cl, and he'd say,"This is Miss Chlorine, and she's green-eyed and avid."

Then he'd write NaCl on the board and say,"Name their baby!"

Sodium Chloride or salt.

He would come into the lecture room--once lecturing on the flash points of oil he came into the lecture room carrying an oil lamp and pretending to stumble on the threshold of the door, he threw the lamp on the floor, and it would go out. It wouldn't catch fire. It was a way that he could begin a lecture on the flash points of oil, a very dramatic way to do it.

Another time he lectured on copper, and he had all the copper ores and everything else on his long laboratory table, and he went down along that table and with his big right hand he picked up chunks of stuff weighing ten to twenty pounds and hurled them at the students sitting in the amphitheater--big copper pots too. His bête noire was Jean Jacques Rousseau because Rousseau said that you would get poisoned if you made pickles in copper pots, and he would talk about toxicology. That's a good way to teach.

He started me in the first piece of real research I did. I didn't succeed. He didn't believe that tryptophane, an amino acid, would come from casein, so he bought me many gallons of milk, and I made the casein out of the milk by precipitating it with acid. Then I tried to hydrolyze it to get tryptophane. I almost got it, but I didn't quite. This was all new.

The Professor of Physiology was an interesting person, an Englishman by the name of Gustav Mann and at the time my brother-in-law, Ralph Hopkins, my sister's husband, was an Associate Professor in Physiology, so I was pretty close to him. I was dissatisfied from the time I went there. I studied very hard--I think now in a compensatory fashion for things that I wasn't getting elsewhere--that is, I didn't care to work off energies, or anything by playing around. I didn't go to dances, if I could help it. I didn't know many people, and I can remember once at Mardi Gras that first year. I was upstairs in the attic. I had a room in the attic with a single bed, and the single bed was that Washington bed of which there is a picture in the files. I was studying anatomy up there one evening, and it was carnival time, and I should have gone to a ball, or something. Mrs. Denegre, my "Tante E", came up to the attic door and saw me--I can remember it to this day. She was about five feet one and rather plump, and she gave sort of a plunge and fell across the bed so that her little feet were sticking out over the edge. She was lying across the bed, not very comfortable, I guess, and she said,"You're the most unnatural member of this family that ever has existed!"

This was because I wouldn't go to a party. Well, I didn't go to the party. I don't remember how that ended, but at that time I began to read Emerson, and I read Emerson's essay called "Self-reliance", and he says in there that if you are in the household of your parents, or close relatives, and don't agree with them, it's better to go and tell them and get out than to try to stay there and make a go of it. I read that, and I stewed around for maybe a week, or two. I wanted all the more to go to Hopkins, so one night I went down about nine o'clock from the attic which was on the third floor to a library down stairs where Mr. George Denegre and Mrs. Denegre would sit in sort of opposite chairs and read books. I came to the door with great trepidation, and then I told them that I wanted to go to Hopkins. Well, to my amazement before I finished telling them I wanted to go to Hopkins, they agreed with my going. I didn't want to tell them that I wanted to go because I was afraid that I would hurt them, and they didn't want to tell me to go because they were afraid that it would appear that they were putting me out of the house. Well, this opened the subject up, and we were all together in no time.

Then I wrote right away to Dr. Welch, and you've seen the letter. I got an answer from Dr. Welch which is one of the achievements of anybody in the world.

Before you go on, there are two things--you told me a fascinating story the other day about a group of students who were about to tar and feather one of the professors.

That was a man named Herbert H. Bullard. Bullard was a Canadian, and he was a Professor of Osteology. His idea was like Hardesty's with the nuclei. He would teach us about the bones in the skeleton, and you had to know all the origins and insertions of the muscles, not only the big things on the bones, but all the little roughnesses where the muscles come and go. The students hated that. It was too hard really. It was just strict memorizing, and also the stuff is very variable because two bones are not exactly alike, but he would insist on this. Well, I was president of the class when I got a call from somebody in the evening, "Come on up here to Audubon Park. They've got Dr. Bullard on the levy of the Mississippi River. They've got him undressed, and they're going to put tar and feathers on him."

Well, I got up there in time and stopped them. They would have tarred and feathered him.

That shows in some ways the flavor of the times and Tulane, the attitude they would take.

Yes.

The other thing that intrigues me is that you said that even at Yale, or before going to Yale, Johns Hopkins was the place for you. How did you first hear about this place?

Well, it's hard for me to remember because it goes back into Joseph Jones and early Bayne themes. One of the admirable people at Hopkins was William Sydney Thayer, Professor of Medicine, a most charming and cultivated gentleman, who wrote one of the first papers on malaria in this country, somewhere in the 1890s. [1895-1897] Well, he was known to my family, to Mrs. Denegre, through Baltimore connections.

To digress a little. The family used to come up to Baltimore frequently because my mother went to Emmittsburg, the Catholic seminary--that's a place for monks, isn't it?

<u>She used to go to Emmettsburg, the convent.</u>

It's right up here near Frederick, Maryland. They got to know a good many people in Baltimore, including Dr. Thayer who became Professor of Medicine and also Dr. Lewellys F. Barker who became Professor of Medicine. Dr. Barker married a Halsey, and her brother was Professor of Pharmacology at Tulane. In addition, the family connections that go through Baltimore included Cardinal Gibbons. Somehow or other I identified him with Hopkins in a way as a great figure. Most of the things in Baltimore took a part of Hopkins in my imagination, but I met Cardinal Gibbons. He forgave me for being a rather gauche sort of person when my aunt, my sister, my brother, and I called on the Cardinal in his palace on Charles Street. They kneeled down and kissed his ring. When he held out his hand to me, I think I reached out and just shook hands with him. I didn't kiss his ring. I was about twelve at that time, I guess. You see, we were in and out of Baltimore a good deal, and Hopkins was there.

Now to go back to the reasons for starting with Dr. Thayer. When malaria was attributed to a parasite carried by a mosquito, that was a very exciting thing. That must have been around--well, the mosquito that carried malaria was discovered in 1897-1900, and shortly after that various people tried to confirm, or disprove this, and one of these was Thayer. He would come to New Orleans, stay at our house, and go chasing mosquitoes. Everybody thought he was a damn fool to be chasing mosquitoes. They did. I didn't think so. I thought that was a good thing. He was so charming and so interested in this biological process that I thought Hopkins must be a fine place. That's one of the things.

Then I used to pass through Baltimore and see this superb building--you know, the central dome of the administration building of the Johns Hopkins Hospital. It was very impressive.

In addition, I had an even earlier connection with Hopkins because when they were planning the hospital in the early ~~1900s~~ 1870s, the trustees invited five people in this country to submit plans for this hospital, and one of them was Joseph Jones. Another was John Shaw Billings. I forget who the others were, but there is a book in this library with those five plans in it, and my grandfather, after being prodded by the trustees, said,"I'm very busy, but here's what I've done", and he sent them drawings of the hospital. They were very like Billing's plan too, so you see, I knew about Hopkins from the plans that he had made about it, and we'd talk about the place. It's hard for me to separate my recollections of my fine experience there with the anticipations I had about the place because they have all merged as one now. I don't know where one began and the other ended.

Did you work with Thayer in New Orleans?

No. I can't say that I worked with him. I knew what he was doing.

What I meant was did you go along with him?

I may have.

Mosquito catching.

You didn't have to go far. They were all in the houses. Everybody had a cistern then.

The reason I asked about Johns Hopkins was because I know from the papers that it has had a long continuity, and I wanted you to put it down. I think we've gone just about an hour--just about enough.

I think so. I could follow up on this Tulane story with the Chicago story, if you want.

That may take a little bit more time--well, all right.

When I got through talking with Mrs. Denegre and my Uncle George about going to Hopkins, I immediately wrote to Dr. Welch. I soon got an answer saying that I had been accepted for the second year class, if I could meet the requirements. Well, the requirements for entering Hopkins were even higher than the things I would have in my head after I'd finished the year at Tulane. The requirements also included more of physics than I had had, so I found out that I could take a summer course at Chicago to make up this difference. I went to Chicago, and I have somewhere some of the records left--I lived in a little room in a side street near the University in a room so small that my bed was across the door. I had to push the bed out of the way when I opened the door and push the bed back across the door when I got in. I think I lived there--I have food bills that I saw not long ago, around six dollars a week, and my room was around two dollars a week.

I took a full course in bacteriology. I took a course in physiology with Dr. Carlson. He was a great physiologist. I took a certain amount of physics, and I read a great deal. I had good enough marks and finished all the stuff up necessary to get in Hopkins; in fact, I did more than enough, but I almost didn't get into Hopkins because after I finished at Chicago I took a little time off. Somehow or other I picked up some books on architecture and I got very interested in reading about architecture. Then I woke up one day--this had only taken me a few days--to realize that the Admissions Committee meeting which was whatever examination they'd give at Hopkins had been held and finished before I even started for Baltimore. This is a ridiculous story I'm going to tell you, but it's the truth and characteristic of the Hopkins people.

I went on down to Baltimore, and I was the only candidate that appeared that day at a meeting in the Dean's Office. The Dean then was a man we called "Bull" Williams, a great, big Professor of Obstetrics, a wonderful man, J. Whitridge Williams. Dr. Williams, Dr. William H. Welch, Dr. W. H. Howell, who became Dean, and Dr. Franklin P. Mall, the great anatomist, were sitting there, and I appeared in that room with a derby hat in my hand. One of them said, "Won't you have a seat?"

I put the hat down on a chair, and then something distracted my attention, and so they said, "Well, sit down."

I sat down, but I sat on my derby hat--right when I wanted to make a good impression. I think if you did that nowadays, they'd throw you out and say, "You're so dumb you can't come in this school," but they all acted as if I hadn't sat on my hat, and I did too, until finally Dr. Mall turned to me and said, "You can get up off your hat now. That's not a sacrifice demanded of the neophyte here."

<u>That's marvelous. Did they--this was a verbal questioning that they put you through?</u>

They didn't ask me much. They just wanted to talk to you. As a matter of fact, that system of a liberal attitude toward a candidate is chacteristic of the place, or at least it's been my fortune to have had it extended to me.

For instance, when I went before Dr. William Halsted in the end for oral examination in surgery--you know, he was a great surgeon--he talked to me about North Carolina.

When I got a license by examination, so to speak, in Connecticut after I had moved back to Yale in the 30s, they called you up to Hartford for oral examination. They even asked Harvey Cushing to come up there, but when I got in before this austere board, they just asked my opinion about things. They didn't quiz me on the facts. I could make up anything.

At Hopkins I had done so much work that it was very fortunate. I had this physiology with Carlson, so that excused me--Dr. Howell who was Professor of Physiology said that I needn't take any more. You see, I was entering the second year at Hopkins. I did this work at Chicago so that I wouldn't lose a year. Dr. Howell excused me from physiology, but permitted me to put all that time in his laboratory working on a problem, so I worked on the extraction of prothrombin from blood platelets. It's a part of the coagulation of blood in which Dr. Howell made such a great name for himself. Well, I spent a year--all of the time that I could put in--in a little room in his laboratory. It was wonderful, and he gave me an ideal which I think I've lived up to. When I got through, I wrote the paper for publication in the <u>American Journal of Physiology</u>, and I put his name first. Naturally--he outlined the problem. He guided every step, and it was really--well, he should be first as senior author. When he went over the manuscript, he changed it a great deal, or edited it, and he took his name off. Now, that is what a lot of professors don't do. When they have a student taking a degree, a Ph D, or whatnot, I know some of them just insist on putting their names on all the publications. Leo F. Rettger was one like that at Yale. I don't know whether your teachers did that--you know, before you make a thesis, you often have a number of papers. Well, Dr. Howell did this, and that paper was published. That's my first publication in 1911 [30 American Journal of Physiology 74-79 (April, 1912)].

No.

I wanted to take a course with Professor Walter Jones. He was the Professor of Biochemistry, a very brilliant and able man. I wanted to take his course in biochemistry. I went to see him, and he said, "Have you had any biochemistry?"

I said, "Yes, sir. I had course work with Dr. Chittenden."

He said, "Get the hell out of here! I can't teach you any biochemistry, if you've worked with Chittenden", and he wouldn't let me come in and audit his course.

Was this Ajax Carlson at Chicago?

Yes.

What sort of fellow was he?

He was a great big Swede, I think--or Norwegian, a big, rawbone man about six feet two, strong as the deuce, chewing a pipe stem all the time and very incisive in everything he said, firm in his opinion, opinionated, a big voice, and he was a leader.

A pied piper. How was he with students?

Oh, they liked him, admired him so much. He was so honest. There wasn't any fake about him.

But the University of Chicago is the first time that bacteriology is mentioned.

I took a course there in bacteriology with--well, his name will come to me later on [Professor Norman MacLeod Harris]. It was a nice interesting course. He was a Canadian man who was a quiet, shy sort of person, taught a routine bacteriology course, but I didn't get into bacteriology because of that. I'll take that up with you later. It's a curious thing. I went back there to Chicago in 1928, as a visiting Professor in Bacteriology, and I will tell you about how that resulted in my being offered a professorship and why I didn't take it.

Let's call a halt today--all right?

Thursday, April 14, 1966 A-54, N. L. M.

Last time we got you through the period of interest, but dissatisfaction at Tulane, the break through in communication between yourself and Uncle George and "Tante E" about Hopkins, and there was no problem--suddenly you were all singing the same song. We took you to Chicago by way of underscoring Dr. Welch's letter to you: "If you meet the requirements". Well, you did more than meet the requirements.

This was because I wanted to go into the second year without losing a year.

Right. Then there was this fascinating aside into architecture that all but eliminated the admission day meeting.

Yes, it distracted me. I forgot the date.

But at any event, you finally got to Hopkins, spoiled a derby hat, but were very gently received. I guess the preparation for going and staying at Hopkins, studying at Hopkins--as you indicated last time--had been in your mind a long time.

Oh yes.

Well, here you are. You're there. Baltimore is an old friend to you. Here you are with continuity, and I'd like you to talk about the city, the atmosphere, the general shape of things, what was available to you in terms of facilities, laboratories, people, equipment, library, and so on as of this particular time because you must have luxuriated in it all. There was more present than Marsh's Evolution of the Horse.

Well, the first thing you have to do when you go to a new town, or a new school is find a place to live. I don't remember how I went to the house where Dr. Welch lived, 807 St. Paul Street, but I did have some kind of a letter either to him, or to the housekeeper of that house that made it possible for me to get a room there. When I entered the second year class at Johns Hopkins, I got a room in the house where Dr. Welch lived, and I occupied that room on the third floor just over his bed room for the next three years. This was a great, wonderful thing for me because I place Dr. Welch with the great people of the world. I admired him enormously, and he was always very good to me in not a condescending fashion, but curiously enough with his gentility and his great knowledge of people, he treated this incoming student as a colleague.

The lady who kept the house was Miss Mary Simmons, a tall, thin, austere sort of person with a most kindly heart, but an imperious demeanor, and as soon as I got the room and she was giving me a latch key to the door, she said to me,"Young man, if you come back to this house any night and you can't get this key in the lock, if you are rich, you can go to your club. If you're poor, you go to the station!"

Pretty austere.

Miss Simmons, I think, also managed Dr. Welch who was a bachelor, in a somewhat similar kindly and strict manner. Dr. Welch was a bon vivant, and he gave elegant dinners at the Maryland Club, some of which he took me to, and I remember once seeing Miss Simmons, the day after Dr. Welch had been at one of these dinners, and she told me that Dr. Welch came home about mid-night and didn't have his key. He rang the bell and she said,"I went to the door to tell him what I thought about this, but he was so sweet. He put in my arms all the floral decorations from the center table of his dinner party and went upstairs as quickly as he could."

That shows, I think, Dr. Welch's character and his tact--and for a bachelor--how to appeal to the ladies. It also shows something of Miss Simmons' disciplinarianism.

Did you warm to him? Were you en rapport with him?

With Dr. Welch?

Yes.

Oh yes. I talked to Dr. Welch quite freely, but most respectfully all the time because he was the molder of the Hopkins medical school, the great founder of all the modern foundation support of medical education. He knew Mr. Frederick T. Gates very well who was the man who influenced Mr. Rockefeller to give the money.

Let me continue with Dr. Welch just a little on some minor anecdotes. All the people that visited the medical school from abroad, people of any consequence, would call on Dr. Welch, and he entertained most of them at the Maryland Club, wonderful dinners. He took me to several, but I happened to see one night as I went by his door how he was preparing for the dinner. He had a man from South Africa, and I noticed that Dr. Welch while putting on his tuxedo, had propped on his bureau the Enclyclopedia Britannica. I think I may have gone by enough to see what he was doing. It was turned to the article on South Africa. I went up stairs and read that article too before the dinner. At the dinner, this foreign distinguished gentleman was sitting between me and Dr. Welch, and Dr. Welch began on that article with paragraph one and said some things. I caught on to what Dr. Welch was doing, so in the next pause, or so, I picked up paragraph two, and Dr. Welch looked at me in a curious way. Well, we went on through that whole article that way.

<u>That's a marvelous technique.</u>

Well, Dr. Welch would do that. He would thoroughly prepare for anything he did, and it wasn't false. It was false on my part, but he could have done it without preparing, but that was one of his characteristics. He had a most marvelous memory, almost complete recall, and he kept on developing it by insisting on holding you while he recited in detail what he'd just read. He would read a scientific article and start to talk to you about it. You couldn't stop him until he told you everything that was in that article. He would sometimes repeat it.

I remember how long that particular quality continued. Dr. Welch died in 1934, April 30, 1934, I think. He was in his 85th year. He was born in 1850, and he was dying of a cancer in the Brady Urological Institute. I think he spent a good part of a year dying there [actually 14 months], but he was reading scientific papers all the time in the journals which we brought him, and one of them [112 <u>Proceedings Royal Society of London</u>, Series B 384-406 (March 2, 1933)], as I remember distinctly, had the first account of Elford [Gradacol] filters which are collodion filters with very fine pores, so fine that you can measure the size of viruses that pass through and those that don't. That was the first time that they began to get at the size of viruses like poliomyelitis virus and others. I remember sitting by the side of Dr. Welch's bed the day or so before he died, and he recited the whole of that article on Elford filters. That's the way he was.

He founded one of the first medical history clubs that I knew anything about. I suppose the influence of Dr. William Osler was there too, but the Johns Hopkins Medical History Club met at least once a month under the aegis of Dr. Osler, who had gone before I got there, and Dr. Howard Kelley, Dr. Thayer, and always Dr. Welch. Dr. Welch knew medical history just as he knew anything else, and of course the final accomplishment of his varied and wonderful life was when he was made the second director of what is called the Welch Library at Hopkins, the great medical, historical and general scientific medical library. When Dr. Harvey Cushing wouldn't take the job, Dr. Welch took this on--oh, in his 80th year, I believe. He went abroad and bought a lot of books in his characteristic fashion. He just bought books and greatly exceeded the provision that had been made for his purchases--he was a very mild sort of an emperor to himself. He was a very big, portly man with a very large abdominal section, very handsome, dignified, pleasant voiced, kindly--no end of energy as far as I could tell. He could go all the time.

Was he neat and tidy?

Yes, he was neat and tidy, but he didn't have anybody particularly taking care of his clothes. He was neat and tidy as far as his clothes go. He was very untidy as far as his correspondence and his books went. He had a system of allowing letters to accumulate on his desk until they got to a certain depth, and then he'd put down a layer of newspapers over those and start again. He did.

Most of his correspondence was hand written.

All of his correspondence was hand written. I talked a year or so ago to

Dr. Allan M. Chesney who was a class ahead of me and became Dean of the Johns Hopkins Medical School some years ago, and he wrote the history of the Johns Hopkins Hospital and Medical School. He went back over all these files of the Dean's Office, and they hardly found anything by Dr. Welch typewritten. He did all his Dean's work and his Foundation work in long hand.

<u>That's great organization to keep in mind an idea. That's tidy.</u>

Let me ask you if you're doing any collateral reading in connection with this?

<u>I'm trying to.</u>

Well, there's a wonderful paper by Dr. Harvey Cushing called "The Doctors Welch of Norfolk" which characterizes this wonderful Welch doctor family. Dr. Cushing was formed by Dr. Welch in a way too and admired him tremendously.

<u>Did he have humor?</u>

Yes in a way. Well, you didn't make any jokes with Dr. Welch.

<u>I understand that, but did he have humor?</u>

He had a twinkle.

<u>A twinkle. Wasn't overly serious--just well organized in terms of his interest.</u>

Yes, he wasn't frivolous.

<u>Well, the picture of him depositing the center piece with Miss Simmons coming home.</u>

He had a sense of humor--certainly.

Was he accessible to you in the house?

I never tried to associate with him in that way. To me, he was an Olympian figure. As a matter of fact, I'll confess one thing I did. I put my bed on the third floor over where I knew his bed was on the second, hoping that some dreams would come up and influence me.

You indicated that he could talk with anyone high or low without them gaining the feeling that he was condescending.

Yes.

That's a rare gift, isn't it.

Dr. Welch was very fluent in German. Of course he went to Germany from Bellevue Hospital up in New York before he went to Hopkins. Then he went to Germany almost year after year. He was a close associate of Robert Koch, of Cohnheim, of Ehrlich, Pasteur. He knew all the people who were making the bacteriological era which had begun a bit before his time, but which was coming in to what he called "the most miraculous decade in the history of medicine"--that is, the period from about 1870 to 1880, 1885, and he was a part of that. He was making discoveries of his own in bacteriology at that time. He was one of the men who introduced medical bacteriology into the United States. He had a wonderful scientific mind, great judgment. People consulted him on all sorts of things, and his colleagues at school accepted his authority very easily, even very, very individualistic people like Franklin P. Mall, Professor of Anatomy--you may have come across him in some of this stuff.

Dr. Mall was a tall, sharp spoken man of great intelligence, a Professor of Anatomy, and a man who, really before Dr. Welch, had a vision of the full time system of medicine. He preceded Dr. Welch, but Dr. Mall had everybody rather frightened because he would say such sharp things in a gentle manner. I can remember when I was trying to draw a picture of a brain that had been given to me up in his anatomy course, and it was still covered by the meninges, the dura mater which is a thick piece of fibrous stuff which was almost opaque from the fixative. I was trying to draw the convolutions of the cerebral part of the brain. Dr. Mall watched me for a while, and he said, "I think if you would remove that opaque obstruction, you'd be able to see it better."

That's the way he'd talk to you.

Like pulling the stopper.

He was a man who, as I say, frightened you somewhat, but very intelligent, and he had with him a marvelous Associate Professor named Florence R. Sabin who got to be a great expert on the formation of blood. I had the good fortune to work a little bit in her laboratory for a short time--she went while I was at Hopkins to the Rockefeller Institute. I think she was the first woman to be on the faculty of a medical school. It was in the time of Dr. Welch and Dr. Mall that women were admitted to the Hopkins Medical School. They weren't admitted at first, but the place got in financial difficulty, and Miss Mary E. Garrett said that she'd give them five hundred thousand dollars if they would admit women on the same footing as men, and they did.

How was Dr. Welch in class?

Dr. Welch in class was superb. He didn't recite the textbook on anything.

He just talked about his experiences and his knowledge. He had first hand knowledge of just about everything that he was going to talk about in pathology. His immediate successor was George H. Whipple first, and he was followed by Milton C. Winternitz--both of whom had tremendous influence on my doings. Winternitz was succeeded by William G. MacCallum who was a brilliant man, but these men all lectured from what they knew. They didn't recite the book back at you. They supposed you knew it.

How was he thought of by students generally?

Dr. Welch?

Yes.

It's the same thing. They just thought Dr. Welch was one of the seven wonders of the world. They liked him, but there was no familiarity.

They were there for business.

They were there for business, but even if there wasn't any business, there was an aura about him that influenced.

Did you have the feeling, in comparison to Tulane, that Hopkins was a professional school?

Oh yes. Of course its students were all very much more earnest than they were at Tulane, much better informed. They all had BA degrees from their colleges. They all were ambitious, wanted to live up to high standards. They didn't want to get by on any easy way. They admired their professors which is very different.

As far as the physical plant went at the time, I thought it was palatial, but I know now that it wasn't. It was built at a time when modern apparatus, modern appliances, modern fixtures had 't come in very much. There were great high ceilings everywhere, not too warm rooms in the winter. Then, you know, the whole place got into trouble when the Baltimore and Ohio Railroad failed. They lost a great deal of money at the time, but you see, it was in 1910, that Abraham Flexner wrote his great report on the medical schools of the country, and I think that Abraham Flexner thought that there was no other school in the country that could touch the Johns Hopkins.

Yes.

Dr. Welch had a good deal of influence on the Flexners. It was Abraham Flexner's brother, Simon Flexner, who became the director of the Rockefeller Institute after his work in pathology at Pennsylvania. He came from Louisville, Kentucky to work in Dr. Welch's laboratory. Dr. Welch knew both Abraham and Simon Flexner from the time they were much younger--you see, Dr. Welch lived a long time. He was born in 1850, and we're talking now for me around 1911--at that time Dr. Welch was sixty-one.

The rest of the things on the equipment side, it was a period when most of the investigators made their own apparatus. You blew your own glass and made all sorts of things, could repair instruments. You couldn't repair microscopes and instruments like that, but you could do a lot of minor stuff, and Dr. Welch's own laboratory was very simple. He wasn't doing research any more when I came there, but I could tell from the past that all he had was some bacteriological apparatus, some culture tubes. He of course presided over the pathology [the Clinical-Pathological Conferences], the great autopsy occasions where he would--

I suppose he might have introduced the system in which the pathologist and the professor of one of the clinical departments meet over a case, a body that's been autopsied, and the professor tells what he thought about the disease, what the consequences were and the processes, and then the pathologist, who has the last word and has the great advantage of having seen everything, usually tells him where he was wrong--the clinical pathological conference. It's a very lively and interesting meeting because these men don't try to put on any false front. They go at it.

How was Dr. Welch in those conferences?

He was very good. He was wise. He didn't do what some pathologists do in those conferences. He was a gentleman and dealt with the clinical professor as if he was a gentleman too. Some pathologists that I have known use that occasion to vilify and ridicule the other man. They shouldn't because, as I say, they've got the look-in on what actually was there. The other fellow was just guessing--had to guess.

It can be a little frightening. I think it could be pretty frightening with a fellow whom you mentioned and whom I met some years ago--Winternitz.

Where did you meet Winternitz?

It was here in Washington when he was down here on the Hoover Commission.

He died about five years ago.

It was some time before that. He was down here working on the Hoover Commission, I think it was, to revise the structure of government, and I saw him briefly, but he had the reputation of running a pretty brutal conference. That may be

<u>overstated.</u>

Well, I worked under Dr. Winternitz a couple of years down there. Winternitz never hurt me, but he was awful rough on some people.

<u>Yes. I guess it's one thing when you have the final say. You're--well, it's like having an ace in the hole.</u>

Talking about the apparatus--you take this thing here, the chloride determinations in the urine. That piece of apparatus that is shown in there is nothing but a graduated cylinder--just stock in the laboratory, and you put it together and make something out of it.

<u>This paper is 1913</u>. /"Simplified Methods for Quantitative Estimation of Chlorides in the Urine" 12 <u>Arch Internal Med</u> 90-111, (1913)7

This was during my third year.

<u>Yes. Who was Dr. Guthrie?</u>

He was the Professor of Clinical Microscopy which is a subject in the school where you examine blood, smears, urine, sputum--all the excretions, plus make cultures. Clinical microscopy was a favorite subject with us. It was a rather exciting sort of work--having many peculiar things that people get out of their bodies and fun trying to find out what they were and guessing.

Clyde G. Guthrie was a beau brummell, a good sized fellow with shining red hair, immaculate clothes, and I would say now, a Madison Avenue manner, but we didn't know that in those days. He had as an associate a more interesting and able person named William L. Moss, and Moss discovered the blood groups while I was there. That was a great excitement. Guthrie let me come into his shop, his laboratory, to do what I wanted to do. I usually came in after class and usually worked until late at night.

<u>The interest in this particular paper was that it appears to have been an effort to test the accuracy and simplify the tests doctors could make.</u>

That would work.

<u>A useful service. An effort to draw some relationship between what went on in a school like Johns Hopkins and practice generally, for standards.</u>

In Hopkins they encouraged you to do what they call an "arbeit". They carried that over from their German experience. Every student was supposed to be able to have an arbeit--you worked on some problem. A great many of them didn't work on a problem, but if you showed any inclination to do so, you were encouraged. You didn't have a grant to work on. There were very small funds in the departments, and you had to fidget around and get your apparatus as you could. If you had a piece of work that required the use of dogs, or other animals, you probably wouldn't get very far with the supply because it was too costly.

<u>In this particular problem--the accuracy of these tests and measurements--in setting it up did you talk to Professor Guthrie as to how it would be designed, or did he turn you loose?</u>

As I recall, they turned me loose--you see, there's no statistical study in that paper. It is not as I would do it now. I would figure out how much I had to do to get a significant result and do some other study of the problem. That and this one--I forget what this one is--but they were all episodic with me. They happened to be something that I was interested in at the time.

Yes, that's the nature of these early papers. This one is that abscess of the liver. /"Bloomfield, A., and Bayne-Jones, S.,"A Case of Abscess of the Liver due to a Streptothrix" 26 Johns Hopkins Hosp Bulletin 230-233 (1915)7.

That was because I happened to see a patient who had a most peculiar fungus-like thing--there's a picture of it there. I was partly interested in that kind of a thing then because, as I recall, Aldo Castellani was talking about those things. Was he mentioned?

No, but that doesn't mean that he was not there.

No, Castellani came to Tulane somewhere around a year or two later as a Professor, a mycologist. He became the chief surgeon for Mussolini in the invasion of Ethiopia.

This apparently was a specific case that came into the hospital--here's its history--and presented a problem.

This probably--well, Arthur Bloomfield was the resident physician then, and he was a highly cultivated doctor who went out to California--literary, intelligent, a good teacher, and he ran the laboratory. In the Hospital there was a laboratory for bacteriology, and he probably put me on that job, or else I found it and worked it up with him. It's of no consequence.

Well, it does show what comes over the transom as you grow--the same with this one. /"Pleural Eosinophilia" 27 Johns Hopkins Hosp Bulletin 12-16 (1916)7.

Well, that one I worked up myself because the eosinophile cells were supposed to have possibly an iron component. These are these cells with the red staining granules. Not only did I find them in the pleural fluid in the chest, but I studied all these cells. Then I tried to incubate those cells, I think, with hydrogen sulphide, and they got black so I thought there was some iron in them.

<u>This again is the pursuit of interest.</u>

This one on "Eventration of the Diaphragm" [17 <u>Arch Internal Med</u> 221-237 (1916)], was an episode too because I had a patient with his intestines up in his chest because they were going through a hole in his diaphragm. You read about diaphragmatic hernia. I then went to the library and looked up every case I could find anywhere, and I was greatly helped in this kind of work by the Surgeon General's <u>Index Medicus</u>. Then I got the x-rays. See these drawings. There was a very interesting man at Hopkins by the name of Max Brödel. He was brought to the Hopkins from Germany by--maybe Dr. Barker, and he founded medical illustration in this country of a special type. He was a very good draftsman. He had a technique for drawing things on a chalk board with charcoal. You rubbed it around and came out with a full brain, or something else, but Max--well, didn't Max sign this drawing?

<u>Yes, he did.</u>

Well, Max Brödel was a musician and a rather considerable artist, not medically trained, but very quick to catch on to what was under the skin. He had a place outside Baltimore which later on in my life, in the time of prohibition, was one of the great beer stubas in town because Max Brödel was a great brewer. Max Brödel's friend was Mencken--H. L. Mencken, and he was out there every Saturday night. Brödel and Mencken would play a double on the piano, and we'd get out there and sing and drink Max Brödel's beer. He drew those pictures, and he drew some more here too.

This paper called "Roentgenography of the Lungs" [Waters, A.A., Bayne-Jones, S., and Rowntree, L. G., "Roentgenography of the Lungs. Roentgenographic Studies in Living Animals after Intratracheal Injections of Iodoform Emulsion" 19 Arch Internal Med 538-549 (1917)], has a title that I know now causes a thing to be lost--you see, all that means is x-ray of the lungs, but then the subtitle says that we used injections of iodoform oil, and look at the date on that. It's 1917. I was an instructor in Pathology and Bacteriology. Waters was the x-ray man. I was the animal man, and Rowntree was the brain. Dr. Rowntree is the man who wrote that book I showed you, but nowadays to visualize the air passages, that's what they use, an opaque substance in the x-ray and this is one of the first papers ever on that subject, but the iodoform we used killed most of those dogs, so we didn't use it on patients.

Was Rowntree at Hopkins?

Rowntree was Professor of Pharmacology.

At Hopkins?

He was assistant to Dr. John J. Abel--Leonard G. Rowntree. He was a good pharmacologist, and he did a great deal with the dyes that were used to determine the hydrogen-ion concentration coming out about this time. Well, here I am just before going into the war.

Yes, 1917--though this paper might relate to work done in 1916.

One of the great satisfactions of the arbeit is that you really came in contact with an erudite professor who was kindly--in other words, it was a little like browsing in a library. You browse on personality when you're connected with this work.

<u>In another sense, it's an unplanned thing--interest has to develop, and it is interest that projects you into the midst of work like this. Suddenly you're in the midst of it--you know, and the fringe benefits are the fact that you do get a chance to talk with a first rate group of people.</u>

This first one, the beginning, with William H. Howell--he was a remarkable man.

<u>I gather you took to medicine like a duck to water.</u>

I guess so. I took to medicine with a great interest in patients, a great interest in the processes, and a great desire to make further contributions to knowledge about them, but I never did practice medicine. I always thought that I would go back to New Orleans after leaving Hopkins and be like my grandfather. I would be answering a tinkle bell like he did. The houses in those days had a pull handle on the front door with a wire going to a bell back in the hall, and that bell was a kind of cow bell on a coil spring. You'd pull that handle, and the bell jangled. I thought that's the way I would spend the rest of my life after leaving Hopkins, but the accidents--I suppose I can go into them now--of having an internship in medicine after I graduated and then being asked to be an assistant resident in pathology which I accepted, changed all that. Dr. Welch and Dr. Osler by writing and all made it perfectly plain that you couldn't have a fundamental understanding of morbid processes without having a fair experience in pathology, so I went from medicine to pathology, from an internship in medicine to an assistant residency in pathology, and that got me so interested that it began to turn my life in that direction.

Another thing happened that comes from Dr. Winternitz, and it influenced me very much. Winternitz was in charge of the Pathology Department this year of 1915--I guess it was; yes, the fall of 1915. Winternitz had built a laboratory for bacteriology on the fifth floor of the Pathology Building, and he didn't have anybody to put in there to run it, and he said to me,"Why don't you take a shot at this?"

Well, I had never had anything but an ordinary course in bacteriology up to that time, so they sent me to work with Dr. Hans Zinsser, so my life really began to change. I went to Columbia, P & S Medical School, which was over on 59th Street, back near the Roosevelt Hospital in those days. I went to Dr. Zinsser's Department and was charmed beyond resistance by his characteristics--his vivid personality and energy, ideas, a ranging mind. I worked there through 1915 and 1916, I guess, and then went back in late 1916, to take charge of that fifth floor laboratory. I got set up and started to work and then World War I came along early in 1917, but Dr. Zinsser was a most charming, fascinating person. He was a musician. When he was in trouble about a scientific problem, he'd put the problem down and go and get the fiddle and go fiddling around, fiddling in the laboratory, playing his violin. He also was a vivid lecturer who was himself writing textbooks so that he didn't have to repeat anything at all. Do you want me to go on with Zinsser?

No, I don't. I've got to turn this reel over because we're practically at the end, and when I turn it over, I want you to take me first down to Panama. All right? Where's that marvelous little drawing of the Schistosoma mansoni you made the other day?

Well, here we are. I think you're entitled to your own views. "History", whatever else it may be, is intensely subjective, and when we try to make something objective out of it, we're making something that was not before you. You know how we're called upon to make decisions on the basis of inadequate evidence all the time. So the story can't really be the way historians write about it--all nice and neat. I wanted you to go back because in 1912, there's a digression maybe, or opportunity is probably a better word--you didn't have to return to New Orleans. You received an opportunity to go to Panama and this, in part certainly, was the consequence of a recommendation from Dr. Welch and also the fact that a relative of yours was there, General Gorgas.

My going to Panama came through my relative whom I called "Uncle Willie", but he was my cousin, Colonel William Crawford Gorgas who was the Chief Sanitary Officer of the Panama Canal Zone. I don't remember when I had seen him before I went to Panama, but there was some preliminary talk with him before the letter from Dr. Welch. I probably asked Dr. Welch to write to Colonel Gorgas and told him something about it, but--let's see, Colonel Gorgas had been Sanitary Officer of Panama since 1904. He went there right after he finished cleaning up yellow fever in Cuba, after Walter Reed's work, and in two years he finished yellow fever in Panama. There wasn't any more yellow fever--indigenous yellow fever. I saw some yellow fever down there that drifted in from Guayaquil and Ecuador and in the Panama Hospital, but there was no yellow fever arising in Panama in 1912. There was lots of malaria, lots of intestinal parasites, typhoid fever and skin diseases, so I went down there in June, and

I stayed down there, I think, until nearly October.

You became an expert--and very easily when landing on the dock, and that's worth telling.

I became an expert by accidentally reading, as I usually did, anything that was printed that came in my way, and on the dock at Colon while I was waiting for the Custom's Inspectors to finish with my baggage, I saw a rather tattered pamphlet of a few pages on the floor, picked it up and read it. It was an article by Dr. Lewis B. Bates on Schistosoma mansoni which is an intestinal fluke that gets in the veins in the lower part of the bowel. The female lays its sharply spined eggs there, and they work their way through the tissue and cause a great deal of trouble. I read this paper which told about the egg and about the life history of the worm just to pass the time.

Well, I went that afternoon from Colon to Panama and was sent to a billet in the bachelor's quarters, rather in the dark, and I spent that night scratching and tossing around. The next morning I found that I had been lying on a blood streaked sheet which was the habitat of bed bugs and had been used by other people who had left an almost dark greasy pillow. I had to get up early to go to the Negro ward at Ancon which was a very big ward, about ninety patients in it, Negro laborers, and Dr. W. E. Deeks, the chief medical officer, assigned me right away to examining feces in a small room in a little out extension of the second floor of the building under a tin roof. By that time in Panama at that season the sun is pretty high and pretty hot, although it was not yet probably eight o'clock, and this was a stinking place with two hundred and fifty some specimens accumulated there. I started to examine them under the microscope. You take a little bit of this material, put it on a slide and look at it under the microscope. About the third or fourth specimen I remember that I saw, had in it a perfectly beautiful, as I thought, lateral spined egg with a miracidium in the center. I recognized it at once as fitting the picture in that pamphlet that I had read at Colon by having accidentally picked it off the floor. Thinking that as I could recognize it as the egg of the Schistosoma mansoni, it must be a common thing. If I could tell what it was, I supposed everybody would know it, so I wrote it on the diagnostic book which at that time went on down to the ward where they were having rounds, and Dr. Deeks and others saw it and came rushing up to this room and said,"What did you do with that specimen?"

I said,"I threw it out because I had finished with it."

They said,"Why do you think it's Schistosoma mansoni?"

Well, I told them, and I told them all about the worm. They were amazed and unbelieving, so I had to find the specimen. We looked at it again, and there were these eggs in anything you picked up out of it. Some of them didn't know what it was so they sent it down to Dr. Samuel T. Darling who was the Chief Pathologist of the medical establishment on the Canal Zone, and he said, "Certainly that's what it is."

It came back. It was confirmed, and so the attitude of Dr. Deeks and others changed from one of contempt to respect, and I was given a white coat and a stethoscope and allowed to come down and be a doctor.

<u>That's a marvelous story. How long was the tour?</u>

Just for the summer.

<u>Just for the summer.</u>

I couldn't spend any more time.

Was it straight ward medicine? You've indicated sanitation.

Oh, I went out with Colonel Gorgas on malaria control occasionally and saw ditching and draining and larvicidal work and some mosquito control work. It wasn't just seeing patients by any means. I saw the canal. At that time there was no water in the canal. They were still digging at Balboa, the last end of it. I saw accidents on the canal. I saw machinery working. I knew Colonel David DuBose Gaillard who built the Gatun Dam locks. I used to wander around with Colonel Gorgas at that time. He and I and a boy named G. Huntington Williams who afterwards became the health officer of Baltimore, nearly drowned in the--well, it might have been the Miraflores River. It wasn't the Chagres River. We started out--General Gorgas, Williams, and I down an old railroad embankment where the railroad had been moved off. This embankment had become covered with brambles, and the water of the Gatun Lake was rising all around us, a picturesque thing. There were iguanas up on the low branches of trees half covered with water, and there were great big tarantulas on the tops of the sticks that were up in the water. We went down through that, and the water kept getting deeper and deeper. We were going from a place called Frijoles to Miraflores, and we forget about the fact that there had been a bridge through a culvert there. This was a muddy kind of water. We got to walking with the water about breast high holding our watches and cameras overhead. Of course, we had on heavy shoes, and all of a sudden we stepped off into about forty feet of water, and we went down and up and down. I was gasping around. I was still holding on to my watch and my camera while drowning. Colonel Gorgas said, "You fool, let them go!"

I did. Then we started out to swim to what we could see were the small leaves of the top branches of trees that were then all submerged. We'd get to a tree and put down our feet, and there was nothing to rest on. We were getting pretty tired. After we did find a place to rest, we were in three separate trees. Colonel Gorgas was in one tree. I was in another, and Williams was in still another. This was muddy water, and the point was to find out where that embankment was, so we explored around and finally Colonel Gorgas was the one who found it. He apparently had been able to swim across the river and let his feet down on this embankment. He called us in, and we got back, but it was a strenuous time. We had taken off our shoes by that time, and this embankment was covered with brambles which rather tore our feet up by the time we got back.

I only mention this because it was such a close risk of a life of a man who really was a figure who contributed enormously to the welfare of the world, and that's Crawford Gorgas. He probably not only made the Panama Canal possible --they never could have dug that canal without his sanitation of it, but he had a great belief in the value of the tropics to the white man. He said that when the pilgrims discovered America, they opened up a valuable territory for the white man, but that what sanitation was doing down there was opening up not only an enormous territory, but one that was richer and could be better suited to the life of the white man. He firmly believed that the life of the white man would benefit enormously by the control of communicable diseases in the tropics which is pretty true.

He also had another belief which later got confounded, and that was the belief that once a thing is extinct, it won't come back. He would talk to me and say,"The dinosaur became extinct, and they aren't any more, and if we"--not I; he didn't talk about himself much--"make this yellow fever virus agent extinct, it will never come back."

That was his belief--eradication and finish with it, but he didn't know at that time about jungle yellow fever. The yellow fever was prevalent in the monkeys in the tree tops.

<u>There's no way to get them to come down for treatment.</u>

Well, they are getting at it now. In a case where a disease is transmitted by an insect, you have a greater power over it than you have ~~your~~ over diseases that are transmitted directly from human being to human being because you can destroy the insect, the breeding of the insect and get rid of it entirely. That would break the chain.

<u>Right.</u>

There's no such transmission in influenza, for example, and you have a horrible time. Yellow fever had been eradicated in the United States and from Panama, and there's a great eradication program to get rid of all of the <u>Aedes aegypti</u> now, and I think they'll do it.

<u>This was sort of a chance experience at preventive medicine, wasn't it?</u>

Oh yes.

<u>Was the Hopkins oriented along preventive medicine lines?</u>

Through Dr. Welch later on--I forgot to tell you that one thing that flattered me into going into bacteriology was that Winternitz couldn't find anybody very well to take the position, and it was vacant. A rather able man, William W. Ford, had been the Professor of Bacteriology there for years, and he wasn't doing so well at this time. He dropped out, or was pushed out--I don't know which, and Winternitz wanted me to take Dr. Ford's place, and here I was only two years out of school, with never any training in this subject, being flattered by being offered the position that a professor was vacating. I'm sure that appealed to my vanity in some respects and my sense of power too, though I don't know. It could. In any event, I took Dr. Ford's place.

Dr. Ford wasn't interested in preventive medicine too much. He was kind of a basic minded biologist who thought that anything that had, we'll say, a medical implication, could be applied, was not sufficiently remote to be worthy of being called basic. Well, that's not true at all. I changed that course right away. For instance, when Dr. Ford wanted to study the formation of spores in the bacteria, he'd go out and get some innocuous spore bearing organism from the soil and have the student work on that. When I took the course, I put the students right to work on anthrax. Anthrax has the same biological processes as spore formation, but it produces a fatal disease, an extraordinarily interesting disease in cattle and man. The anthrax spores in the ground were of great interest to Pasteur--all the fields got contaminated largely by the spores coming up from the buried cattle. The spores were brought up by worms digging up and pushing the dirt up--well, if you take a medical student and teach him spore formation on an organism that is medically significant, there's a burst of interest that doesn't occur when you take something remote from the soil.

It was the same thing with the capsules on bacteria--the pneumococcus has a wonderful capsule which is extraordinarily important in all its reactions and what it does. So does some slime forming organism from milk. When Dr. Ford would get around to studying capsules, he would get this thing out of milk that had no medical significance at all. I took the capsulated pneumococcus of all types, and we'd study the capsule and study something about pneumonia. Well, you can do that in medical teaching. You can teach the basic process at the same time....

<u>That you're involved in something vital to medicine.</u>

Yes, with the practical.

<u>I asked you if--you know, about the preventive medicine that you saw in the summer of 1912. You must have returned just filled with it.</u>

No, I don't recall that particularly because I had known before 1912, a lot about preventive medicine and had associations with it. I told you about Thayer coming down looking for malaria. Well, at that time malaria was being prevented. My cousin Willie Gorgas had done it in Havana. I know about Walter Reed and yellow fever. When I was at Chicago my main textbook--and other books in bacteriology and immunology later on--was George Miller Sternberg. He was the founder of preventive medicine, and all of that for years had been going through my head before I went to Panama. Panama was just sort of a field excursion into things that I was pretty well familiar with.

Also Dr. Welch had a very deep interest in preventive medicine. In 1916, I guess--April of 1916, he founded the School of Hygiene and Public Health at Hopkins. I used to sit with him in an old building over across town in Hopkins University where he had an office and talk to him about the founding of that school. I know he was deeply interested and searching. Some of the things he worked on early like the Welch bacillus were important from the point of view of preventive medicine. He didn't realize that the Welch bacillus was so important in causing gas gangrene in wounds in soldiers and what it formed. He found the Welch bacillus by--well, he did an autopsy on a Negro woman once and found the vessels in her uterus just full of air instead of clotted blood after death, and he got the organism out of that. The second time he got the organism out of dead dogs found floating in sewers in Baltimore. He actually didn't know that the Welch bacillus formed a spore. He didn't know a good deal about it, but it had great epidemiological significance with regard to wound infection.

Yes.

This he appreciated. Then Dr. Welch also knew the great founders of preventive medicine. He knew Pettenkofer, Pasteur, Robert Koch.

So it was in the air.

Yes.

In talking about Hopkins, you didn't mention the surgical side.

The surgical side was interesting to me, but not attractive. The great surgical professor that I admired very much was William Halsted. He reminded me of a basilisk--if you know what I mean. He had eyes that you couldn't see almost, and he had a very dignified and cultured manner. He was beautifully dressed. He was a man who had his shoes made in London, and he used to send his shirts to Paris to be laundered. I was once or twice to his house to dinner, and he had a superb service, servants, table linen and silver and old furniture. Halsted was the great founder of the developments after Joseph Lister in surgery. He was a very skillful operator, and he had a most delicate manner of handling tissue so that he didn't crush anything. He was also responsible for introducing rubber gloves into the operation. Up to that time they were operating barehanded.

I don't know much about surgery. Despite this long contimued interest in medicine, I went through what a good many people do, I guess. When I was in the Charity Hospital in New Orleans when I was a student at Tulane, they took me in to see an operation, and they brought a woman in on a stretcher and started to cut the bandages off her abdomen with a pair of scissors. I hit the floor in a dead faint right away, and I didn't see the operation. Then when I came up to Baltimore and was in the high stands, the sort of pipe like fabricated stands that you rolled around the autopsy table--I was up on the top shelf, and I nearly fell off because I got sick at my stomach. I remember once in Dr. Halsted's clinic, I was up in the amphitheater there, and he was operating, and I also got nauseated. I don't know whether that had anything to do with it, but I never was interested in surgery. Most medical students go through a phase like that.

Did you get any work at all on Dr. Janeway's medical service?

Dr. Theodore Janeway was another great character. When--well, there was a succession of professors who followed Osler. First there was Barker, and Dr. Barker, Lewellys F. Barker, had written papers in favor of the full time plan, but when he got down there, he decided that he didn't want to give up his practice. He had his office outside. Dr. Barker was about six feet three with long, long fingers and a long nose, very precise, and the most extraordinary teacher. He knew everything, but he knew it because he boned up before his clinics. There is a man over there now who is a professor in his eighties, and he used to think my enthusiasms for Dr. Barker indicated that I didn't have any critical sense because what I admired in Dr. Barker was that his teaching was just as precise and effective as you could want. He would draw you out in conversation too. He was good.

Now Dr. Barker was succeeded by Dr. Thayer for a while, and then we come to Janeway. Dr. Janeway was persuaded to leave a practice in New York, come down there and become a full time professor. I think he was disillusioned and didn't care too much about it, but he stuck it out, but what I liked about Dr. Janeway so much was his absolute honesty and forthrightness, although I started off in a bad way with him, and I'll tell you about that.

When I was an intern in medicine, Dr. Barker was the Chief Physician outside of his being professor, and he used to telephone me before he'd make the rounds on the ward and say,"What have you got on the ward?"

I'd tell him. Well, the next morning he'd come in, and I'd be standing at the door with all the histories in my arm. We had quite a ceremony when the professor visited--the nurse was there with a basket of pencils, knee jerk hammers and stethoscopes and whatnot. Well, Dr. Barker would come through the door of the ward and say,"What have you got?"

He'd do this as if he hadn't telephoned me, and I would tell him and then he'd think it over and say,"Well, suppose we see this case with alcaptonuria"--we'll say, some rare disease, and we'd go and see him. I'd lead him to the bed, and they always pull down the covers, make a physical examination, think about it for a while. He went through all of that, and then he gave the most superb lecture on alcaptonuria, but he boned it up the night before.

Dr. Janeway was just the opposite. He was a rather sparse, dark haired man with the kind of moustache that Hemmingway had when he was younger--you know, falling down over the sides of the mouth, and he had a way of making ward rounds that was very different. He'd come to the ward and never ask you the night before what you had. He'd ask you in the morning. He didn't ask you for a diagnosis. He'd just say,"Suppose we see that man over there."

Then he'd get his history. The intern would give a summary of the history, and then Dr. Janeway would start to examine the man, start to work out the problem. Well, you could see the agony the man was going through. He'd walk around the bed. He would just struggle. You'd see him struggling out loud to get at the mysteries and the unraveling because it's very hard just looking on the outside of a patient to know what's going on on the inside.

Darn right.

He impressed me as a very honest man who didn't want to be prompted. But again I had a bad start with him. I inherited Ward F from a big, tall fellow named Wilber G. Carlyle who had been the intern there the year before. This is 1914, I'm talking about now, and Carlyle left me thirty-five patients who were very sick on the ward and about six people in the back room. We used to put them in a little back room when they either weren't sick, or they were convalescent. I had five of those. Then I remember five very sick typhoid patients. Some were bleeding--well, they were awfully sick. I worked all night long to get up on the histories and do what I could for those people on the ward. Dr. Janeway took that ward his first round, and he came to the door--I remember it very well. He said,"I don't want to see any of those people in there. I know you've worked hard on them. I'm sure you have, and there's nothing in particular, but have you got any people in the back room?"

I said,"Yes, sir."

Well, we all trooped back there, and he said,"I can always tell whether I've got a good intern or not by the number of unrecognized pleural effusions I find."

Do you know what I mean?—fluid in the chest.

Yes.

Well, lo and behold, there was one man there in a bed, and Dr. Janeway had him sit up, and he percussed him. He listened to him for a moment, and he turned to me, and he hissed at me--the man had a pleural effusion.

I hadn't examined him even. I worked all night on those sick people. Well, he didn't bear any grudge against me. It must have taken a while for him to get over that initial experience. Maybe he was pleased that he could find a bad intern by going through his usual pattern of behavior.

Did you trade medical services, or did you....

I stayed on medicine the whole time. You trade wards. You move from ward to ward. There was the octagon ward where this fellow wkth the diaphragm was, I think. Then I was on Ward F—the male ward with all those acute patients. Most of them were West Virginian miners. I'd go and sit around by their beds and talk to them a bit, but I was very immature looking. My life suffered, or at least I've been handicapped by having practically invisible white hair and a childish looking face, so that when I'd come in, these poor fellows from West Virginia would say,"We came down here to see a doctor, not a child!"

That's good for one's morale, isn't it?

Well, they're awfully lonely people, and they exist by themselves. I think a patient sick on the ward is somebody to be nursed very carefully not in a sentimental manner, but enough to let them know that you're their friend.

You had notions of returning to New Orleans to practice. Did the experience at Hopkins--well, you had other things in front of you too. You had access to laboratories of professors who were large in their fields and who were open to suggestion and would allow you to work, and you got that attitude. You could see, happily with your trip down to Panama, something of the whole question of sanitation which is design engineering that goes way beyond just practice. You must have confronted a much more difficult choice when you see it all laid out and try to grab a small holding on the slopes of Parnassus instead of getting shooting rights over the whole of the mountain. How did you feel about the choice?

As usual I didn't go through any agony of decision. After I said to Winternitz that I would like to have this job, that was that, and when I went with Hans Zinsser that convinced me all the more that it was an interesting and fine field. Zinsser was a fine man. I came back without any question of going to New Orleans any more.

Well, let's stop for today, and we'll go back to Zinsser next time.

All right.

Friday, April 15, 1966 A-54, N. L. M.

As I indicated before we turned this on, I wanted to go back to Chicago because I bumped into the name of a man there with whom you apparently worked in the laboratory, Fred M. Drennan.

Yes, Fred Drennan was an assistant to Dr. Carlson in physiology, a very large, tall man. He was an instructor close to you in the class work, helped very much on all the animal experimentation.

His letter here--and he has a fascinating handwriting which is different every line.

Yes it is, isn't it.

He wrote you: "I have received your notebook O.K. and I'm glad you saved yourself just as much time and effort as possible on those last hundred experiments." You must have done quite a bit of work. Later in this letter he writes: "I saw Miss Farrar at a lecture the other night, but didn't get to speak to her. Perhaps by now she may have some explanation of your results with"--I can't make this word out. "I gave you my best efforts the morning we were doing the work. Don't worry too much about it. Just leave it to some research man, and he'll get a masters out of the idea and not know anything about it either." It's a witty note, but you must have been working pretty closely with him.

Yes, Drennan was very close to us in the class all the time.

Was he good from the point of view of standards?

Oh yes.

<u>That's a good thing to learn.</u>

I didn't do any research in that physiology course that I remember of any consequence, and this Farrar is a distant relative from New Orleans. She comes from a family of the Farrars, Judge Farrar remotely related to the Denegres, and one of the Farrars in this family married Dr. Joseph Goldberger who worked out all the problems of pelagra. She was in Chicago that summer. I forget which one of them was--there were about six Farrar girls.

<u>You spent apparently not a little time in the laboratory.</u>

Oh yes--long hours in the laboratory with difficult experiments.

<u>Was this Drennan's design of experiments, or were those related to the class work?</u>

I think they were related to the class work, but this is a long time ago. It's....

<u>Well, it's 1911.</u>

Fifty-five years ago.

<u>Yes. I wanted you to mention him. You mentioned a man in talking yesterday and couldn't remember his name.</u>

Drennan was Carlson's assistant. I couldn't remember the bacteriologist. His name is Harris.

<u>Later at Johns Hopkins--thinking over what we said about the school yesterday--there are some things that occurred to me that you might want to comment about.</u>

<u>One of them is the whole concept of patient care--you know, the organization of resources for patient care and how high that figured, the standards that they had with respect to it at Johns Hopkins at that time.</u>

Well, it's difficult for me to sort out the impressions I had as a student and intern at Johns Hopkins from the impressions I have from experience later on in being responsible for medical care, but I admired the medical care so much and wanted to have the benefit of it so much that I started to substitute on the hospital wards in my second year--just about the time I went down to Panama. Whenever I had the chance to substitute for an intern I would take his place and take care of patients. We thought the medical care at Johns Hopkins was superior to anything else in the country, although we didn't have experience in other places to serve as a basis for comparison. It was extremely thorough. Dr. Thayer, Dr. Barker, Dr. Adolph Meyer, Dr. Theodore Janeway--all those men were not only conscientious, but they were scholars all the time. Their patient to them was a problem, really a research problem--I'm talking about the element of medical care that begins with the study of the patient and the effort to arrive at a proper diagnosis.

<u>Yes.</u>

Nothing was spared by them either in the taking of the history, or the using of such apparatus as they had available. For the benefit of the medical care and the patient, on the laboratory side, they had excellent biochemical laboratories in the medical department, and I know only about the medical department. Although I had some experience occasionally on surgery or obstetrics, or gynecology, it's medicine I'm talking about mostly. At that time two things were developing that had a great influence on medical care--one was x-ray. They had a great man in x-ray there named Frederick H. Baetjer. He was one of the earlier x-ray men. He died of x-ray cancer of the skin later on. Hopkins was building up a fine Department of Radiology at that time. In addition, they had one of the first electro-cardiograph apparatus. A man named Douglas S. Hirschfelder--I think he wrote the first book on that subject in this country. We were able to study patients with electro-cardiography in new ways. All the specialties at the Hopkins were extremely well done--as well as the general medicine.

Dr. Hugh Hampton Young was the great urologist at the time, a fashionable urologist as so many of them get to be, and his main patient was "Diamond Jim" Brady. "Diamond Jim" Brady gave Dr. Young the money for the Urological Department, a building, hospital beds and laboratory.

The superintendent of Hopkins Hospital when I was there, Dr. Winford H. Smith, was a very careful man, a dictatorial disciplinarian with very high standards, I thought; indeed, his standards were so high that he suspected most of us of being crooks. I must admit that toward the end of an internship, every year when the interns would be changing, a certain number of thermometers, stethoscopes, knee jerk hammers, and other things disappeared from the ward baskets, and Dr. Smith practically wanted to frisk us as we went out, but that was only a part of his high ethical standards in general. He was skeptical of the characters he had to deal with, but he insisted on the best you could do for patients.

It was a very rational therapeutic school, although they did some rather severe things that we got rid of after a while. When I started, the way of treating typhoid fever was to put the patient in a tub of ice water and hold him down in it, and that was very, very severe and hard on the patient. That therapy disappeared while I was down there. The other thing that they were doing which you might not think is good patient care, but was done with the best scientific appreciation of the possibilities, was treating syphilis of the central nervous system by giving the patient salvarsan, then bleeding the patient in about a half an hour and collecting the serum which contained some salvarsan that had been in the body but perhaps got changed by the body, and then doing a puncture of the spinal canal and putting the serum in the patient's spinal canal system. That caused the most terrific reactions, headaches, and all sorts of thing. To compare what was then done at the Hopkins with the modern medical care of patients, we would find nothing different in the idea and the point of view of wanting to do the best, but, of course, they didn't have what you saw in that open heart surgery.

But the drive was there for patient care.

Oh yes.

It was oriented along those lines and severe.

Yes, it derived from the Oslerian concept of teaching at the bedside. They understood that the best element in patient care was to have medical students around because it put the doctors on their toes and kept up the interest, but that also came from the French School of Louis and other people who introduced bed-side teaching.

Incidentally, in the light of the experience, the initial experience you had with Dr. Janeway after you'd been up all night preparing those cases, Dr. Janeway later recommends you to Dr. Warfield T. Longcope for a position in

Bellevue on some medical service there, so your initial impresion was erased.

I didn't mean to imply that Dr. Janeway carried any grudge. He never did. That horrible sound he made--he was laughing through his nose, or something like that. He didn't rub it in to me.

One other item that I've been thinking about since yesterday and where infectious disease is concerned is this whole epidemiological approach and whether this was part of the presentation at Johns Hopkins?

I have to think of that to try to separate it from what I did later--I don't recall it being so strongly brought forward on the general medical service, although, as I said, I had a ward full of typhoid patients, and in considering them we tried to find out the source of their infection, and you'd read and talk about the contamination of water from privys. Where the epidemiological, or preventive medicine point of view came in most strongly was in pediatrics, and pediatrics is still the leader in clinical preventive medicine in many ways.

I think I got an impression then that I carried through later on that made me oppose the establishment of a separate department of preventive medicine at Rochester because I couldn't see how you could deal with the actual current situation in a patient without understanding or looking at the origin. Just the ordinary course of the study of a case involved knowledge of the prevention. For instance, you talk about heart conditions, heart diseases, you have to think of whether the patient has had streptococcal infections before and if so, how did he get them and how were they spread. Syphilis of the heart, for example, and this process takes you back into infections diseases that on the surface has nothing to do with the heart. I can recall that kind of preventive epidemiological mixture with the ordinary day to day observation of a patient, but there was no course that I took at that time that was a kind of epidemiological course that they would give now with statistical investigation, of dealing with communities. We had practically no experience in community health service, or the conditions in which people lived in communities, except in obstetrics.

Fourth year in obstetrics we were sent out to deliver children in the poor quarters of Baltimore, under the supervision of one of the instructors, or assistant professors, on the obstetrical service. In my case one night the instructor didn't get there in time--did I tell you this?

No.

The instructor didn't get to the place where I was in time. It was down on Wolfe Street. A Jewish woman gave birth to a baby before my supervisor got there. This was the first time I'd ever had to deliver a woman, but fortunately she had had previously a number of children, and it was quite easy. I was cleaning up and getting ready to go, and to my astonishment she gave birth to another child, so she had twins, and I didn't know it. My instructor didn't come even then, but I had these two babies and fixed everything up, tied the cord and cut it off and cleaned things. You have to make post-partum visits which I did with a little black bag, and this lady said that she wanted to name one of those children after me. One was a husky child, and the other one was a wizened little thing. I deliberately said,"Well, name this baby after me" because I--to tell you the truth--didn't think that it would survive, and I didn't want the burden of the baby, so they named this child Stanhope Bayne Saltzburg. Lo and behold, the husky baby died, and Stanhope Bayne Saltzburg survived through a very weak infancy, but was able to come along. I clothed and fed that child more or less until I got to Rochester, and then I lost track of him. His mother was one of these white women who do the ironing in a Chinese laundry--you know, you see white women ironing for a chinaman. They were very poor people down on Wolfe Street, so I learned something about the social background of childbearing at least among the poor. I used to go down there fairly often.

Patient care got an extension--I have that down here. I found that in a letter home dated 1917, when you were abroad and were concerned about the boy.

Oh did I really?

I was going to ask you about it because "patient care" does go way beyond the hospital.

Of course.

In this particular instance, she named this baby after you.

Who did I write that to?

This was to your "Tante E"--October 5, 1917--and you wanted her to send them some clothes--shoes was the recommended item.

I must admit that my motives in undertaking it were not....

These things happen--that's the interesting thing. They unfold, and you have to play it by ear. I put it down that you got involved in patient care way beyond the hospital room.

Oh yes. They had a great deal of that in New York which I'll tell you about later.

One other item at Johns Hopkins is in part a public health matter, but deeper than that. It's the sense of public service which I think was pretty strong in Dr. Welch. Maybe it's stronger in terms of that development in the Rockefeller Foundation to help through financial aid young doctors to become better and better at what they were doing. Maybe that's part of what I mean, but the sense of public service was pretty strong at Johns Hopkins, wasn't it?

Yes, I think it was, but when I was there we were so closely concerned with the individual in the faculty that I didn't think very hard, or much, about the outside relationships. Of course, Dr. Welch did, and many, many things you can ascribe to him in the way of public service. He led legislative efforts. He was head of the Board of Health. He was President of the American Medical Association. He fought the antivivisectionists. He was the guide of all sorts of good movements.

Somewhere along the line while at Hopkins you get involved in something which is available to you in terms of degree--a master's degree. There were some requirements--an application and an essay. Did you file an essay?

No, I had done work, and I forget what it was. It was about 1917, and I wanted a master of science. I think they gave me a master of arts. It's based on some of the things that are published there, and they may have been in manuscript form at the time. I remember writing to the graduate school that handles that degree and asking them. I had started this work without any thought of using it for a degree, but I asked the graduate school whether they would accept it. I didn't have to take any subsidiary courses for this.

No. This was something which I think was available for someone who had done individual work.

I think it was about 1915, 1916, wasn't it?

1915. In any event, you can begin something for one purpose, and it lends itself to another purpose, and it helps. They had this available. We talked about publications yesterday, and I know from my own experience that when you publish something, you wonder, apart from the close intimates that you may have, whether it's ever read. I pointed out to you earlier, before we turned the machine on, a letter that came to you from Saint Thomas Hospital, Dr. Leonard Dudgeon, from which it was clear that someone had read one of your articles.

Well, Dr. Dudgeon I didn't know personally, but I knew of him. He's a pathologist of distinction, and I don't take any credit for his having read my paper. It must have been on a subject in which he was interested and when you find such a subject appearing as a title on a list of publications in a catalogue or an index, you don't read the article. You turn over to see whether the man has noticed your work. This is happening still. A lot of these histories of the medical department in World War II that we have been publishing--the first thing that anybody does who gets a volume is look in the index to see if his name is mentioned.

Yes.

It's a natural human thing. I've forgotten--I know of Dudgeon--it had to do with immunology.

But if you've never had this experience before, this was a way in which you got it quite early--you know, the notion that someone has read it.

What was the year of that?

This is 1916. I mentioned to you before we turned the tape on that there were a number of societies that were social in nature, and one of them was a Medical History Club, a Dr. Nichols, and I asked you then, and ask you now, whether this was something to do, something to share. I find that you attended these meetings.

I was always interested in medical history, and so was my grandfather Joseph Jones, and he collected a lot of it. I have read a good deal of medical history, but no student could go through Hopkins without getting enthusiastic about medical history from these meetings of the Johns Hopkins Medical Society. It was a monthly affair, presided over usually by Dr, Welch, and they were very fine meetings. I don't remember who Dr. Nichols was. You say he was in Washington?

It may not have been, but this Philip Roy writes about the University Club, 15th and I Street--that's where his meeting is going to be held, but it does indicate the existence of a club that extended pretty far if you went to Washington to attend it.

What year is that?

Oh dear--it looks like 1913.

Well, I was just starting then.

Yes, 1913.

There's a Bulletin of the History of Medicine still being published at Hopkins.

Right.

I don't mean the Johns Hopkins Bulletin which had a wonderful lot of historical articles, but there was actually an Institute of Medical History there.

Yes, but they were deep in this.

Oh no, it was part of the atmosphere you breathe.

Well, you've indicated that you read outside the textbooks anyway.

Yes, I did.

So you knew your way to the library--you liked to browse.

Oh yes. The library we had at that time available to us was the collection of books in one of the ends of the main building. The Administration Building of the Hopkins. The library occupied on the first floor about four large rooms with stacks, and then down in the basement there were a series of spaces filled with stacks. Somehow or other they let me go down in there without any surveillance. I wandered down the stacks. I never have stolen any books, so maybe they didn't mind. It is a temptation sometime.

It sure is. Well, sometime in 1916, and there's a letter here from Dr. Welch, March 8, 1916: "I've written Zinsser tonight about your desire to work with him." Dr. Welch encouraged this as distinct from working with Noguchi "who is engaged on some special problem and cannot undertake to give training." Apparently this was, in part, I suspect a Rockefeller Foundation thing.

No, this was getting me ready to take over the fifth floor. I did have a fellowship--a Rockefeller Fellowship, seven hundred dollars a year.

Right.

This plus the thousand that I got from my patrimony, or wherever it came from, but I didn't work with Dr. Zinsser on the fellowship. I already had it.

You already had the fellowship.

They were arranging for me to get training--Dr. Welch was writing to help me get training to carry on the bacteriological and immunological job in the Department of Pathology at Hopkins.

There's nothing prior to 1916, about Zinsser--your desire to work with him.

This is my introduction to Zinsser, but I didn't know Dr. Zinsser before this. I knew of him. He was a great leader. He was about ten years older than I was, and before this period he had been a Professor at Stanford, and he went from Stanford to Columbia P & S School of Medicine about 1913, or 1914--I forget what the date was, but he had done work that interested me very much in immunology. He was one of the first people to introduce physical chemistry into the study of immune reactions. He himself was not a physical chemist, but he managed to have an enthusiasm for it, and he really was one that could do that. He was at that time more thought of as an immunologist than as a bacteriologist, though he was a good bacteriologist.

I wondered about that because he had been at Stanford, but in 1915, he was a member of the Red Cross Typhus Commission in Serbia.

That's right.

So it was a question of where he was going to light.

Of course, typus fever is what he became the great expert in.

Right, but then, as I understand it, you moved up to New York for purposes of either taking a course, or working with him.

It was not taking a course. I had all the courses that were necessary in the ordinary sense of taking a course. As a matter of fact, when I got there Dr. Zinsser took me to seminars, used me as an instructor and then immediately--I wouldn't say he gave me a problem, but he arranged for me to start to work on some research, and I published a paper out of that research on the coexistence of antigen and antibody in the same serum, and that is a sort of physical chemical equilibrium problem ["Equilibria in Precipitin Reactions. The Co-existence of a Single Free Antigen and its Antibody in the Same Serum" 25 J of Experimental Medicine 837-853 (June, 1917)].

I gave that paper at the Association of Immunologists in Washington, and I was taken to pieces by a very distinguished man who is still living and still working at the Rockefeller Institute, and that's Dr. Eugene L. Opie.

Oh gosh yes, but did he really take you to pieces?

As I remember, and he's right, he said,"You just think there's one antigen in egg albumen. You have crystallized that, and you think you have a totally pure thing, but as a matter of fact, there are probably three or four antigenic substances. You get rid of one, and you can do another reaction and get some more which you think is just the original one remaining in the fluid."

I think he's right about that, but I know also that there is--well, later it's been shown that there is an equilibrium as there is in most things. You get the compound form of the two substances that are present, and then there is always a little of each left that is not united, or they might be united but not in a precipitate form. I remember that meeting down there, and I was very politely put in my place by an intelligent, able, and experienced man. Do you know Dr. Opie?

He drifted by me just once, but I've heard comments about him.

Dr. Opie is getting on to be ninety years old, and he's still working in the laboratory. As a matter of fact, years later when I was the Director of the Childs Fund--we started the Childs Fund, and we'll talk about it another time--we started and got other people interested in making provision for "the elder statesmen of medical science", as we called them, and at that time we made an appropriation for a stipend for Dr. Opie at the Rockefeller Institute. The Rockefeller Institute had a policy that they wouldn't accept any outside funds, but I went to see Dr. Flexner--I think it was Flexner at that time, or it might have been Herbert S. Gasser, and they agreed to take Dr. Opie in and let this money come in for his benefit, so all those things happen, and nobody seems to carry on any grouch about them.

Par for the course. But this work--working with Zinsser on a problem like this--how clear was he? I got the impression from reading this last night....

Last night? Did you come back here?

No. I was thinking about it last night because I had read it yesterday afternoon, but this paper was an effort somehow or other to pull loose pieces of information that had yet to be really formulated into some kind of a floor, a basis upon which you could operate, that there was a lot of work that had been done that wanted a kind of rationale.

Yes.

And that this was an aim in that direction.

Yes, because the supposition up to this time was that these immune reactions were completed affairs, that equilibrium exists. We know it exists in many chemical reactions and especially colloidal reactions which are shown in that paper. That work is not far from Dr. Zinsser's type of work out at Stanford. I think that he probably talked to me about it and said that this was an unsolved problem. I don't believe that his name is on that paper.

No. The only thing that is on it is that it is aided by a grant from the Rockefeller Institute for Medical Research.

He got that. I didn't get it. It was something that he had in his department.

Yes, but it does show, I think, the fact that when you were a Rockefeller Fellow in Pathology, an assistant resident and pathologist, and instructor in pathology --it shows as of this time a foundation which was interested in sustaining young people, bright young people at the source of development, a laboratory, as distinct from letting them go out and practice. Here it's an effort to increase their educational experience, an effort to sustain scientific research. In that day you apparently had the foundation as you now have the NIH.

I don't recall any dealing with the Rockefekler Institute. I'm sure that grant came to Dr. Zinsser's Department.

How did you find him as a person? I've read his sonnets, and they're marvelous.

Isn't that last one effective?

Yes it is. You indicated that he played music.

He played the violin--he called it a fiddle.

To clarify?

Oh--it did something for him. He liked music. He liked to listen to it. He liked to play it. I don't think he played the piano, but he played the violin pretty well. In the laboratory when he played the violin, it was to relieve tensions, and I suppose he thought while he was doing it. He'd walk up and down sometimes in stressful situations and play the fiddle. I'm sure he played it at home too. I don't know whether he had two violins, or carried one back and forth.

Was he a particularly intense fellow?

Very--look at his face in his pictures. He was a romantic, intense person in his scientific work and outside of it too. His going off on expeditions was something innate in him. He was a great horseman even up to the end of his life. He rode dangerous horses--he had horses at his place up near Boston that would jump fences and throw him around against the rocks. In the Spanish-American War, 1898, when he was about twenty years old, he joined squadron A of the New York National Guard Cavalry, and he went into the Spanish-American

War as a cavalryman. He got a sabre at that time which he rattled occasionally. One very amusing but partially serious thing that happened was that he had a controversy with Dr. [Homer F.] Swift at the Rockefeller Institute. He was a great expert on rheumatic fever later on--"Speedy" Swift, naturally, we called him. I forget his full name. Well, he and Zinsser didn't agree, and Dr. Swift said so in print. Dr. Zinsser got very wrought up, and to settle the matter he challenged Dr. Swift to a duel with sabres.

He was a very sensitive man.

Then he got interested in typhus and went off on that Serbian expedition. That was a wonderful thing which was done. Dr. Harry Plotz was along too, and so was Burt S. Wolbach, and they did wonderful work. That started Dr. Zinsser on typhus, and he kept it up to the end of his life. He was a little reckless with typhus rickettsia occasionally, and I have a feeling that he might have gotten a laboratory infection at one time or another. I'm not sure, but he got sick in the course of his work.

Did he think epidemiologically?

Oh yes, he was a leader in epidemiology.

I mean even as of the time you went with him, after Serbia, after the Commission went over there and that frightful mass problem.

That was after World War I, wasn't it?

1915.

No--just before. Well, in World War I Dr. Zinsser--I know this because I got it all in my history downstairs--he was head of the Division of Laboratories and Infectious Diseases in the A. E. F. at one time. He also became what was called Sanitary Inspector of the First Corps and the Sanitary Inspector of the Second Army. He was intensely interested in sanitation and epidemiology. What he says about a sanitary inspector in essence is this--a sanitary inspector has got to know not only sanitation, but also be familiar with epidemiological methods and laboratory methods. He could use them both as ready, powerful tools, and his writings about epidemiology in World War I are great. I got to be Sanitary Inspector of the Third Army, the Army of Occupation in Germany. General Robert L. Bullard was the General in command of the Second Army, and General Joseph T. Dickman was in command of the Third Army. Somehow or other they may have got to talking, but anyhow there came up from the Second Army a manuscript of Dr. Zinsser, a rather famous paper called "Sanitation of a Field Army." They gave it to me and said,"Here's your Bible."

I read it, and I found that the field army he was writing about, the Second Army in the field, in the Meuse Argonne, was different from the Army of Occupation that I was in which was settled, living in houses, so I didn't use his paper much. Just about a few weeks ago I was putting it into a chapter in this history that I'm writing about World War II. Well, Zinsser was an epidemiologist of great note and very enthusiastic. He understood it too.

<u>Associations--the things you absorb--working with him, this kind of thinking. Did he want followers?</u>

He had devoted people with him. One of my still unsettled questions I think of usually with regret. He wanted me to come very much to Harvard on his staff and at that time--this was 1923--I was at Hopkins with the prospect that

I might become a professor there of a separate department. I had some uncertainty about going to Harvard, and I told Zinsser I wouldn't go, but he had devoted people working around him all the time. Among them is a Nobel Prize winner whom you may know, John F. Enders, who cultivated the polio virus in monkey kidney cells. He was a Ph D in English, and he came to Dr. Zinsser one day and said that he wanted to work in bacteriology. Zinsser liked him and took him in. He did that with Monroe D. Eaton who found this Eaton agent for atypical pneumonia. He had any number of people coming through his work--Reuben Ottenberg. They wrote a book together--Zinsser and Ottenberg. The Hiss and Zinsser relationship was very close. Everybody was--well, he attracted you.

Did he like independent minds?

Yes he did. He taught in a Socratic manner as well as an authoritative one. He and I had very close relations, even though I didn't go with him, and our r elations became very much more so when it came to this textbook. Do you want me to talk about the book review and the textbook now?

No, we'll come to that. You've got many more resources by that time. But what I was thinking about--in 1916, you'd never met him before--a handsome fellow, an articulate man, forceful, high pressure, and, you know, you can be warped by this kind of giant. I wondered what his attitude was toward students, or younger people who came to him. Would he look for the independence of mind?

I'm sure he did. I can't recall anything that he interposed either to studies, or ordinary conversation. A very nice thing happened right away in his laboratory, and it does in some others--instead of going out to lunch some professors in a department have a little gas stove in the back, and they fry eggs and sit around for an hour eating fried eggs, or drinking coffee. He had one of those. Very nice.

<u>Sit at the bench and eat.</u>

Sit at the bench and eat. The same thing happened to me at Hopkins. You haven't mentioned it, and that's Nu Sigma Nu. Do you want to talk about that?

<u>Yes. That's this letter.</u>

Well, it's bigger than that. Nu Sigma Nu is a national medical fraternity, and I happened to be pledged to it down at Tulane, so when I went to Hopkins I became a member of the local chapter. I've forgotten what chapter it is. They had a house at 518 North Broadway. I didn't live in that house. I lived in the house at 807 St. Paul Street. The Nu Sigma Nu house, at that time, a single brick house with marble steps that are characteristic of Baltimore houses, was located on Broadway facing the Johns Hopkins Hospital, right across from it, and it has about twenty men living in there. It got rather crowded. I didn't want to live there because it was noisy at night. I studied at night, and I would rather not live there, but I ate noon meals there and those were almost better than the classes. There were three classes, second, third, and fourth year men in each group of about ten, and they all ate together in the same dining room. The men in the fourth year class seemed to know everything so they started quizzing everybody else and quizzes lead to arguments. I think I learned more from those quizzes and arguments than I did from some of my teachers. Just self-education, or mutual education in a group like that is very valuable.

When I finished at Hopkins and was still around there, we decided to buy the house next door and join those two houses. Somehow or other I undertook to raise the money for it. I got a thousand dollars, we'll say, from Dr. Barker, and a thousand dollars from Dr. Young, some from Paul Clough and others. We took a mortgage and bought that house, and that was a burden for me and a worry until I left the place. I actually went so far as to ask those people to whom we'd given notes to tear them up because we couldn't pay it. The expense increased, and the members didn't have any money. I think that the professors who were members of that chapter who had taken those notes to start, never expected that we would be able to redeem them, so this must have been what this gentleman was talking about when he wrote about doing so much for the chapter. After I left, Dr. Paul Clough took it on, and he worried with it maybe ten years. What happened in the end I don't know.

<u>There's some question when you finished working with Zinsser, the publication of that paper--was the intent to return to Baltimore?</u>

Yes, I was going back to the fifth floor of pathology in the fall.

<u>Yes, but then in 1917, which is the following year, Dr. Zinsser wants you to return as assistant professor with him, and he writes you two letters on the same day.</u>

I'm in Hopkins at that time, aren't I?

<u>Yes, but Zinsser writes that he'd had permission from Dr. MacCallum to talk to you about getting you. Dr. MacCallum had apparently gone down to Hopkins from P & S because it's Dr. MacCallum who writes you that while you're going away to the Army, Dr. Ford will probably help. When did the Winternitz idea develop?</u>

I graduated in 1914, and I spent the year 1914-1915 in pathology and Winternitz was there then. Did you see a paper here on the "Blood Vessels of the Heart Valves"? [21 Am Journal of Anatomy 449-463 (May, 1917)].

Yes, that's here.

I did that under Winternitz. Dr. Welch was not convinced. What year is this?

Publication is 1917.

Well, they don't say when they received it.

No.

This is Brödel's work. I injected these hearts and you see the vessels around the edge of the valve and around the chordoe tendineae. Then I showed Brödel twenty or thirty of those specimens, and he made a kind of a schematic drawing putting a lot of pieces together.

Yes.

Well, that was done under Winternitz, and I think that was in the year 1915. You see, what happened while I was with Zinsser, I got called into active duty. The Pershing Mexican expedition was on then, and the 5th Maryland Regiment from the Maryland National Guard, an infantry regiment, was called out. They assembled in a field near the race track at Laurel, Maryland, and I was called down there. I forget how long I stayed. I stayed at least two weeks or so, and I got what turned out to be infectious hepatitis there. Having no pied à terre, or whatever you want to call it, to put my foot on in Baltimore, I went on back to New York to the room I had on 57th Street near 3rd Avenue, and from there I had to go over to the Hospital. I went to the Presbyterian Hospital where Dochez took me and put me in a bed.

He's worth a word here--Alphonse R. Dochez.

Alphonse Dochez was an intelligent, polished, courteous gentleman, quiet spoken, a man I rather revered for his scientific ability. He was high up on the Rockefeller staff at that time. He was a close friend of a man we called "Fess" /Oswald T./ Avery who at the moment was probably close on the discovery of what now turns out to be DNA. He called it the transforming factor, but Dochez and Avery were so close together that you'd think they were entwined. They were Damon and Pythias. I didn't know Avery very well, but I did get to know Dochez. His taking care of me at the hospital was mostly a remote sort of thing. He was giving me purgatives because they thought that maybe that would loosen the plug of mucous and let the jaundice go by, but it didn't and I was jaundiced for a long time; in fact, I got kind of green and you feel quite weak. I got over that, and I went back to Hopkins after that was over. Then it was getting on toward 1917.

Well, you joined the reserve in 1915.

Yes, I was a lieutenant--I think at the instigation of my "Uncle Willie."

Yes. Was that a time when you had two weeks on maneuvers during the summer?

Not necessarily--unless you wanted to take it. Training at Carlisle Barracks is where they sent you.

Yes, but the first time you remember being on active duty is with the 5th

Maryland Regiment.

Yes, that's the first time I remember. I may have gone to the Carlisle Barracks before then, but I don't remember it.

That's almost a normal thing in the reserve. Well, I think we've gone as far as we ought to go today. I may have a few more things about Zinsser next time. Then we ought to go into....

We ought to get to that textbook business.

We ought to get to leaving scientific work for the time being completely with the advent of war. All right?

Tuesday, April 19, 1966 A-54, N. L. M.

The documentation about the first world War is pretty good. All of the letters that you wrote home, so far as I know, have been preserved and these two diaries which are largely day by day accounts--you know, as to where you were, the time, the sort of things you fell heir to, or ran into. Now we've already gotten you into the reserve.

I have in addition to those diaries some other items I haven't brought out to you, great, big books of all the maps of our positions that we were in throughout the war with the British and the Americans and a lot of photographs of the battle fields.

You mean you've been holding out on me?

I didn't know you wanted that kind of thing.

The diaries--well, handwriting being what it is, I can't make out some the entries.

You'd like me to bring that stuff in?

I sure would. You make some drawings in the diaries, and I've been over this area.

I've got all the battlefield maps of the whole thing.

It would be very good if you would bring them in.

I don't know that I will turn those in here to the library. I don't think they want them, but you can see. They were very useful to me not long ago for a grand-nephew that I have. He borrowed them, and he kept them for three months just going over them because he'd been with his father on the battlefields last year. They went around, found old helmets and things like that.

I would understand this period better because I look upon the maps as a visual guide.

I'll bring them in then because there are pictures of some of that mud in Flanders with shell holes in it. I can remember lying there as if I was a kid swimming in a pond. You know how the water jumps up with rain drops? That's how the ground was doing.

People wouldn't believe this, but when you talk about wading around ankle deep in mud--I know what that is. Well, how did the call for active service come?

Well, as I was about to tell you--in my background there is either a military gene or a military tradition. One of my ancestors, Major John Jones was aide to Brigadier Lachlen McIntosh--I think that is his name--at the seige of Savannah during the Revolutionary War. He was killed there on October 9, 1779. Then there was my grandfather Jones in the Confederate military service wandering all over the battlefields of the South. My grandfather Bayne was a major in the Washington Artillery and was wounded at Shiloh. The father of General Gorgas was General Josiah Gorgas, Chief of Ordnance of the Confederacy. My uncle Hamilton Jones went into the Spanish-American War as a military battalion doctor, so that with the things that were in my grandfather's collection--arms and things at the house--I was brought up to be rather accustomed to the military side.

The war--the first world war affected me rather deeply. I can remember when it first broke out. I was sitting on the porch in Biloxi when the news came in, and it just disturbed us no end for a further reason. If you were brought up in New Orleans, you were brought up in a Napoleonic tradition. The Denegre family--my "Tante E" and all of those just thought that Napoleon was a second Alexander the Great and even a second Solon. He was the greatest law giver and the greatest military leader in the world. You have no idea of the vividness of the Napoleonic songs and traditions that were in my youth as I grew up. So it wasn't strange to be wanting to be with the military in some way, but to get down to 1917. I was back in the laboratory in Baltimore, having been away for that time with Zinsser and with the experience with the 5th Maryland Regiment for a short time, the Maryland Infantry. I had come back to Baltimore early in 1917, to begin work in the 5th Floor Laboratory. It seems to me that shortly after the declaration of war which I think was about April 16, or 17, 1917, I talked to Dr. Welch about the future, and Dr. Welch told me that he wanted me to stay in Baltimore on the staff on active duty and train bacteriologists for the Army. I agreed verbally to that.

What was in my heart I really don't know at this moment, but I came over to Washington to see my kinsman, "Uncle Willie", as I called him, General Gorgas , the Surgeon General. I was at that time a Captain in the Medical Reserve Corps, and I went into General Gorgas' Office which was in the old State and Navy Building down here on Pennsylvania Avenue, and before I said anything to him at all, he said,"Oh, Stan, I'm so glad to see you. Lord Balfour was in here a few minutes ago, and he said that they're desperately short of doctors for British troops and battalions. I know you've got a uniform, and if you can get it out and get packed, I'll get you on a boat in five days."

I never told him what Dr. Welch sent me over there for. Now, that's when the decision was made. I went over there intending to ask General Gorgas to put me on active duty and to assign me to teach bacteriology at the Hopkins in Baltimore, and when he talked to me that way, I didn't tell him. Now, why I didn't tell him, I don't know. Something in me may have said,"This is what I want to do", but also I would have been a little ashamed to have what the French call an "embuscade" job--tucked away safely.

Did Dr. Welch understand?

Dr. Welch never made any complaint about it. He, I'm sure, understood. He himself never had any military experience. He didn't upbraid me, or make any complaint that I remember. What his private opinion was I don't know.

You were set to go and sailed almost immediately.

Yes, I sailed early in May. I forget the date.

May 8th.

The 8th of May, and I was on a ship called the "Orduna" with the Cleveland Voluntary Hospital outfit under the command of Major Harry L. Gilchrist who later became a general and head of the Chemical Corps. On that ship were some very interesting medical people. The great surgeon Dr. George Crile was the chief medical officer of it, and there were several other men whose names I could dig out, but I don't recall them at the moment. They got to be very interesting companions, but we were--well, there were five of us unattached to that Cleveland Unit on that ship going over. We were the forerunners of a thing called the "Lost Battalion". They were altogether perhaps a hundred or more doctors like myself that were sent over to be attached to British outfits, and there is a book published that is in this library on "The Lost Battalion".

We got over there, and we went around for a day or two with the Cleveland Unit in London and then mostly on our own we met, or had a conference with Sir Alfred Keogh, the Surgeon General. They made a great deal of us. It was hard to pass anywhere without having to drink some liquor. Everybody was pressing "refreshments", as they called them, on you, and even we five with the Cleveland Unit were received by King George and Queen Mary in the garden at Buckingham. I met a captain one night there at a club, and I met him again in France. He turned out to be a captain of a British company in France that I got assigned to.

Is this Captain Lindsay who put you in the center of a room at some banquet and played the star spangled banner?

Maybe it was. It was N. E. Lindsay. Well, do you want me to go on to France?

You had a note to Sir Alfred Keogh, and I think that when you had the conference with him, he called you aside and told you that he was writing a special letter for you. Now, I don't know whether the others in the group of five had the same thought in mind about their service as you did. You wanted to be with a battalion, or at least you expressed that idea.

Well, Sir Alfred Keogh sent me first to the 69th Field Ambulance in France near Messine Ridge. At that conference—well, it didn't progress very well. We were sitting around the table, and there was present a Lieutenant Everett D. Plass who was the Chief Obstetrician from the Medical College in Iowa City, and Sir Alfred Keogh said,"Now, gentlemen, I want to place you where you can employ your specialties and do some interesting work as well as serve the soldiers in this war", and he said to Plass,"What is your specialty?"

Plass said that he was an obstetrician, and that set the Surgeon General back. He said,"We don't have much of that in the Army."

I think he stopped asking questions at that point. I really do.

Actually orders were slow in coming because you went to the Base Hospital #4 with the Cleveland Unit for a time, up around Rouen.

Yes, I went there just as a rider on the Cleveland Unit for food and clothing, and I stayed with them until the orders came for me to go to the 69th Field Ambulance. We had some maneuvering type of exercise there. I remember that we had what was called in those days a bangelore torpedo which is a very long metal rod with explosive in it. You were to shove it under the barbed wire and then blow a hole through it.

Come on--tell me about it!

I hate to say anything about a great surgeon who has done good work and has passed on, but Dr. Crile rather shocked us. He had a theory of the phylogenetic origin of shock. He meant that the more highly organized your nervous system was, the more apt you were to have shell shock, and his serious purpose was to reduce shell shock in soldiers. He began to invent double wall helmets with padding inside of them to take the shock away, ear muffs. He invented dugout doors which, of course, never could be placed and if they were placed, they wouldn't stay there. He sent us down into Rouen one day to collect all varieties of animals and plants that we could. We had to come out later leading chickens, dogs, frogs, violets, and God knows what. I think I had to pass in front of the crouched British soldiers waiting for this explosion to go off leading an eel. The explosion went off, and the creatures were blown up into the air. A lot of them didn't mind it very much, except the dogs. A few of the dogs came down out of the air and lay panting on the ground, and Dr. Crile said,"You see what I told you? There's the highest organized animal we have, and he's got typical shell shock."

Arthur B. Eisenbrey--I think is the man's name--he and I took some of those dogs behind a building and did an autopsy. We found that their mesenteric arteries had been cut by little fragments of this torpedo that went through. They were dying of hemorrhage, but the professor was satisfied that it was shock.

Well, I didn't stay there too long.

There hadn't been much work on the problem of shall shock, had there?

No.

It was a fumbling beginning--this collection of bugs, animals.

Is that in there?

Yes. There was another offer made to you about this same time from a Dr. Charles F. Hoover--I think it is Hoover--to establish a laboratory for the study of trench fever with the Cleveland Unit.

Maybe. Trench fever was just beginning about that time to be recognized. It was supposedly a typhus-like disease and probably might have been carried by lice. They never really worked it out thoroughly. They thought they had found a rickettsial-like organism which they called Wohlhynica. They thought that trench fever came from one of those Balkan states. I had it--I'm sure--at a very uncomfortable time. I'm jumping way ahead. In 1918, when I was with the Americans, we moved up to the Marne River--we'd had a little rest with the 26th Division back of Verdun--and on up the Meuse River on the right hand side of the bank. We had to march at night and rest in the day time under the trees. Well, the doctor didn't have much rest because he spent the day fixing blisters on the feet. At that time I had fever every day for a while and this characteristic red swelling in front of the shin bone, very painful legs, and a little erruption. I think it lasted about ten days.

<u>You give the temperatures in this diary, and they were quite high.</u>

Did I put all that in there?

<u>Yes. There's one comment in June of 1917--the simple statement, that you avoided being made a member of the Johns Hopkins Unit. Were they near the Cleveland Unit?</u>

Yes, those units were coming out. I was with troops, and I liked troops, and I didn't want to get sent off to the hospital laboratory. That was the only reason. All my friends were in the Johns Hopkins Unit, but if I had gone there, the same thing would have happened as happened to me when they sent me to Dijon in 1918. Did you get to that? They wanted to put me in the laboratory and do Wasserman reactions, and I didn't want that.

<u>Yes.</u>

I got on well with troops, and as I said, I didn't care very much about the outcome of my life in the war, so it didn't matter, except that I didn't want to stay in the laboratory.

You had a succession of talks with a colonel who wouldn't talk to you at first--Colonel Russell. You got a telephone call the night you finally got a chance to talk with him, and he said that he was going to move you to a battalion, but that the first thing you had to do was go to a gas school, which is a brand new thing.

Was that Colonel Frederick Russell?

I believe so.

He became a brigadier general, a very eminent man. He introduced typhoid vaccine in the United States Army about the time of the Spanish-American War, about 1900, I think it was, a brusque man. I had no real personal association with Colonel Russell.

Except that you wanted to get on to troops. You spent a good bit of time in Rouen. The Cleveland Unit really didn't have a place for you, although they were affable, as were the British who were being replaced by members of the Cleveland Unit, but you wanted to get on to the business of being a medical officer with a battalion, out with the troops, so you went in and knocked at his door. He didn't give you the time of day the first time, but late that night, you received a telephone call, and the first thing you had to do was go to a gas school.

That was reasonable. Everybody had to go to the gas school.

That's new and novel. You haven't indicated any experience with gas.

No, I didn't have any experience with gas, and I did have a new experience with the horrible type of gas mask that you had to use at that time. It was a rubber contraption and it covered your whole face with a clip on your nose like a clothes pin and a respirator that hung on your front like a knapsack on your back. I think they did expose us to some gas. Mostly it was a drill for putting on a mask and behaving. I don't know what risk we took. I doubt if we took any, although I do recall that there was some man about that time who showed that a human being could walk around in hydrocyanic acid gas that was killing dogs right around him.

But then you went on a train ride and finally a Ford car ride to the 23rd Division--a replacement camp.

The British 23rd Division--I still belong to the Officers Association of the 23rd British Division, get a letter every year to come over to London to dinner. I don't remember anything about the train ride, or where we went, but the 23rd Division is what I got into, and I think I went to the 69th Field Ambulance first. The British were at that time just about to blow up Messines Hill south of Ypres. Well, that was a very fine ambulance company, very strict discipline, and fine people--Scots. I'll get the name of the colonel in charge of it after a while, but I've forgotten it at the moment.

The only ones I have here are a Captain W. G. Johnston, a Captain H. R. Macintyre, and a Colonel Hammerton.

Colonel G. H. L. Hammerton is his name.

I said replacement camp. You did join the 69th Field Ambulance and then for a time you were sent off to the 70th or wherever you were needed. A replacement in that sense.

Yes, but those weren't very long in any one place.

No--actually until you got attached to the 11th Sherwood Foresters.

Yes, the 11th Sherwood Foresters which suited me no end because when I grew up, one of the things that we used to do at home was play Robin Hood, and I was Friar Tuck many times.

God--that's a great legacy!

It was natural.

They must have been something!

The Sherwood Foresters were Yorkshire, a Lancashire battalion with very broad accents. I soon got to know the major in charge at that time--Colonel Charles E. Hudson, and I stuck very close to him. He admitted the doctor to his headquarters, and I was with the commanding officer all the time, practically from the time I joined them, but it wasn't long before that battalion went on up into the lines at Dickebusch--I think that was the place.

Yes--where the first gas attacks began.

Dickebusch? Yes, it was--just outside of Dickebusch. I remember the first gas attack was shell gas. I remember the queer, whistling, wobbly sound of the shell coming over with very little explosion. We had working parties out just beyond a little embankment where the road went across a sort of marshy place. There must have been two hundred men working out there that night bringing up elephant iron, sand bags, all sorts of thing. This shell gas came over, and nobody knew what to do about it. It didn't seem to be especially poisonous, but it wasn't more than about ten minutes before we found that it was a lachrymatory gas. It just caused awful inflammation of the eyes, and I had many men lying out there. I made a mistake. There were no instructions as to what to do, but I had some cocaine solution, and I found if I put it in the eyes, they got rid of their pain for a while, but it came on worse when the cocaine wore off.

Then in that same place we had phosgene shell gas attacks, and in the same place at various times we had mustard gas, so I saw them all about that time, but I didn't have any trouble with gas. This lachrymatory stuff didn't seem to get in my eyes. I didn't pay much attention to the proper way to handle a gas mask because it was so suffocating and so obscuring of the vision that I used to put the clips on my nose and the respirator end in my mouth and let the rest of the mask fall down so I could see. Phosgene is horrible. Not long after the soldier breathes phosgene he collapses and starts to belled a bloody pink frothy stuff all over his face. I have that later on in 1918 with the Americans.

This was in July, and there were a succession of attacks with gas, but this is something new.

These were the first gas attacks.

You're in trench warfare, and this is the Ypres sector where the salient was, and they were changing ground all the time, swapping ground, and never with a chance to clean it up.

There were dead horses, dead men, feces everywhere. Is there an anecdote in there about how the English soft speech can accomplish what the American speech won't?

No. You told me this the other day. It's a marvelous story. I wish you'd put it in with the language, however ripe it may be from the American point of view, because it's real.

I can't use those curse words. I was walking with Colonel Hudson one night along the bank just north of Zillebeke with about seven hundred men, I think, and there were four British three inch guns firing across the road, across this embankment, and we couldn't pass. We were due up front to make the relief, and Colonel Hudson went up to the British gunner, and he said,"I say, old thing, would you mind stop firing off that silly old piece until I get my men by?"

The gunnar said,"Righto", and they stopped firing these four guns--I guess. We went by. Well, about a year later on the banks of the Meuse near Sammoneux, there were split trail, long hundred and fifty-fives firing straight from the river bank up across the road going over the neighboring hills--I mean the shells did, and the major I was walking with went up to the gunner and used the most vile profanity, called him all sorts of names and asked him how he thought he could get his 103rd Infantry by at that time with these guns firing across the road. It was a very sharp squabble, and the gunner told the infantry major that he could sit down on his behind in the mud there and wait with his men because he had orders to fire two hours, and he was going to fire two hours. They got each other so mad, and that's where the war had to stop.

There is a difference in the approach.

Oh yes--soft speech got it all the time.

What sort of resources did you have as battalion medical officer? What did you

<u>have to rely on?</u>

With the British?

<u>Yes.</u>

I started with a pretty good little medical chest, but you can't carry anything that is at all weighty, so I took out of the chest bandages, scissors, I suppose some bottles of iodine. I didn't take any anesthetic, took a few pills--sometimes we had some pills that we used to give for diarrhea--I forget what they were, a few things like that and put them in a gunny sack and carried them on my back. I had three or four very devoted aide men with me in my little battalion medical place, and they had gunny sacks on their backs, so we didn't carry much. We also carried as many Thomas splints as we could, tied up together and hanging over our shoulders. You know what a Thomas splint is for--broken legs.

The stretchers were carried by other people. We had about eight stretcher bearers most of the time, but stretcher bearers got awfully worn out in the mud, and they couldn't make more than one trip anywhere, so we had to impress all sorts of people into service. It wasn't long before the doctor persuaded the commanding officer to let him have the band. I think I broke up three bands in the course of that time because as soon as a stretcher bearer gets back with a patient he generally joins the outfit he's with in the back area. If a bandsman gets back there, the outfit behind wants a band, and they keep him.

<u>Did you operate from an aide station?</u>

I didn't do much operating at all. I just did first aide stuff. I did the best I could, cleaning out pieces and things, putting bandages on and stopping bleeding. I didn't put any heavy tourniquets on because I knew that is forgotten in time, and the limb becomes possibly gangrenous. You had so many things that you couldn't really handle, and the aide stations weren't built as aide stations. I remember one we occupied quite a while. There was a thing called the Hooge Menin Road running from Ypres to Menin and just at the beginning of the Battle of Passchendaele we went up through there, and there was an old culvert under the road and a little tunnel dug into the shoulders of the road. That was the aide station, and it was full of blood, people dying on the floor, hot and smelly, but you saw some brave things there. I remember an Australian one night who came in holding his forearm in his hand, and it was attached to his upper arm by just a few shreds. It was bleeding very much. I asked him how he got that, and he said, "I stopped a five point nine with my elbow."

Well, a five point nine shell is a good sized shell. I cut his arm off with scissors and bandaged it up and got him away. You see things that you can't do anything about. In the same Battle of Passchendaele we had our aide station in what remained of a sort of a mound on which a race track grandstand had been built. There was a tunnel in that--very narrow. The most room I had, as I remember it now, was as much space as the top of a small table. The floor was covered with wounded. I remember some wounds like this--a man brought in with the whole of the front of his skull lifted up. You could see his brain. Then it comes around four o'clock in the morning, and that's the time you could get people out with stretcher bearers. That's a very hard time for the medical officer. That's the time you've got to act like God Almighty and decide who is going to get out and maybe live and who is going to stay there and die. The rule with me and most of the medical officers was to use your stretcher bearers to take out the people who had the best chance of survival. You go through this. You can be close to a man who is moaning, in great pain, horribly wounded, and begging to be taken out, and you have to decide that you're going to take somebody else. That happens over and over again.

As a matter of fact, to jump way ahead, it is now a part of the official policy of the civil defense medical organization and the American Medical Association in this country. It's almost a revolution in medical thinking with regard to who you're going to take care of. Suppose you have an atomic bomb, the rule now is that you don't waste your time on people who are sure to die. You try to do something for those who are likely to survive, and that is in print. That's a great change in medical attitude and thought.

<u>You confronted that policy as a matter of necessity though, didn't you?</u>

In the war?

<u>Yes. There was only a limited number of people you had who could take people back.</u>

Oh yes.

<u>How close were the general hospitals to the line?</u>

Oh--it wasn't general hospitals that you took them back to. The general hospital would be back a hundred miles or more. These were the small station hospitals, little tents, maybe five miles back, but you didn't know always what it was. I had an experience one night that sounds like General Patton, but it had an effect on this man. It was also in the Battle of Passchendaele which was a horrible slaughter. We were beyond the Hooge-Menin Road, going to what's called--I think it's Polygone De Zonnebeke, an old race course, all shelled up, mud to your waist, and one of the stretcher bearers with me was named Corporal Tongue--did you run across his name?

No.

Corporal Tongue was about six feet three, a strong man, and a shell burst very close. Corporal Tongue who was a stretcher bearer lay down on the ground and started to shiver and cry and wanted to be taken back. Somehow or other I was able to pull that man up to his feet and when I let him go, he fell down again. I pulled him up again and slapped him in the face as hard as I could, and he fell down again. Then I kicked him on the bottom of his shoes, didn't kick him on his flesh. I kept badgering him until he decided that he couldn't get back. He got up slowly and put his weight and arm around my neck with his weight bearing on me--I guess a couple of hundred pounds in the mud, and we went on up to the Polygone De Zonnebeke where the battle was very thick. That man never left me from that time on. If I'd sent him back, he would have been a shell shocked psychiatric casualty. He became my batman. He'd bring me tea at four o'clock in the morning when I had to get up for sick call. He carried my latrine around, a little box. I think that that's good treatment maybe.

In Korea they tried to do a great deal of rehabilitation right up near the sound of the guns still--at the front. Well, those things in after thought enter into your general philosophy of medicine and what the principles should be, but at that moment, I was just thinking of survival. I wasn't going to send this man back with the few stretcher bearers that I had when he wasn't even hurt. Other things that happened in a battle like that were very unexpected and disturbing, but can be worked out. I remember a big captain up there in this

Zonnebeke region. I was wandering around in the night, and I found this officer separated from his men, lying on his stomach with his face in his hands, just blubbering. He'd lost control of himself, and he was no more in command of his company. I just sat down and talked to him a while. He didn't get sent back.

<u>You certainly had no experience for bombardment.</u>

Before this?

<u>I don't know that there is any way to prepare yourself for what it really is.</u>

No. Even in this Battle of Passchendaele I must have spent half to three quarters of an hour one day being shot at point blank with a cannon. Did you find that in there? It was a German cannon, a small cannon across the Gheluvelt River about four or five hundred yards from us. I was in a dugout pill box the door of which then faced the Germans. One of us was foolish enough to put some sandbags around that door, and as soon as a few sandbags were up, they started shooting at us, and these shells would whiz by. Several of them hit just above the door. Some of them hit below the door. Some hit the side of the door. This little pill box was about six feet in one dimension and about eight feet long. There were about five or six of us in there. We crowded against the back wall for a while, thinking that was a safe place to be. Somebody would say, "That's foolish! If it hits the back wall, it will spread and get us all."

We crowded up to the front wall and then they'd think they'd get it on the long back shot, but fortunately none of those shells came in. Also I was in a kind of a shelf near the roof of that heavy German pill box one night and the shell hit the corner of it and the top of the roof started coming down on me.

<u>This was an area that had just been taken from the Germans.</u>

Yes, in the Battle of Passchendaele.

<u>Yes, the door was fine so long as you were on the other side, but their door became an open door toward them when they retreated.</u>

Yes. I got a Military Cross in that battle when I was with Colonel Hudson. The Sherwood Foresters were going up to take over in the line. They had been on reserve, or at least in the second line until that time, and believe it, or not, we were having tea--about half past four with a battle going on. There was an awful bang, and the dirt fell down a little cement stairway into the dugout, and a soldier came down rather alarmed and said that a lot of men were hurt outside. Colonel Hudson looked at me and said, "Well, Doc, I guess it's up to you," so I crawled up through the dirt on the stairway and went out, and there were wounded men there. There was a lot of bombardment. I don't think I got back for a couple of days, but that's how I got a Military Cross.

<u>Just incredible things happen.</u>

I didn't believe I could be hit.

<u>Really?</u>

I stood on that mud talking to an officer and a shell came and the man next to me was gone.

<u>The same thing happened to me.</u>

The shell goes down into the ground and it bursts, and it has a kind of a full fan shaped explosion. You can be between those scatters.

Yes, you can, but that's very much of a chance. I think after a while you get to the point where you can tell from the sound where it's going to land--you know, you get a sense as to whether you should dive to the ground, or some cover.

That was a hypothetical bit of comfort because when you hear the sound of a shell, it's gone by you.

Yes, but psychologically it was helpful.

It's gone by you--unless it's a big howitzer shell that has gone way up in the air and is coming down. We used to hear--the big German naval guns were off Dunkirk, the Belgian coast, and they would fire on Dickebusch, twenty miles or more. You could see their flashes and hear the guns.

Was there much mortar fire here?

A certain amount of mortar fire, not a good deal of it. They had another thing--instead of a mortar, it was like a garbage can, a foot and a half in diameter, and it swirled up on top of the trench with two or three explosions. They didn't have the same kind of modern mortars that we have.

Those you couldn't hear. Tell me something about what you can learn from what the human body can take from the Sherwood Foresters, from all the things they were subjected to--climate, everything else.

It is incredible what the body can take--cold and wet, hungry, full of lice, dirty. Nobody is going to pat you on the head or hold your hands. There's no use weeping. Fatigue--you get so tired that you don't know you're tired.

These men--a lot of them; well, in 1917, the war had been going on since 1914; and a lot of men were approaching forty. They were that age group. They weren't the prime young fellows who had started, and they could take it. I was well and quite strong at that time. I don't remember very much. My feet didn't get sore. I got plenty of lice.

Didn't we. At some point the Sherwood Foresters were pulled out of the line, and they were directed to the Italian campaign. I'm not sure why. This wasn't after Caporetto, was it? I guess it was.

Yes, it was the Caporetto retreat that had happened and the break through on the Isonzo front. The Austrians had broken through, and they had the Italians on the run. They were stopped at the Piave River. That's as far as they got.

At this juncture--they were entrained, and you were too. You mentioned another man, another American by the name of Long.

Yes, Lester L. Long is a physician out in Seattle, I think. He was with the Koylies, the King's Own Light Infantry, and that outfit was also in the 23rd Division. The 23rd Division felt that it was a pretty spic and span outfit. It belonged in the 10th Corps, I think, had a commander named Lt. General Morland, and in one place in Flanders we were reviewed by the Crown Prince Edward.

But technically we--the United States--wasn't at war with Austria.

No, that bothered me a little technically because I wanted to go with the Sherwood Foresters. I didn't care whether they went to Italy, or Africa--I wanted to stay with them, but they did go to Italy with the British 23rd

Division to oppose the Austrians, and I found out that it wasn't proper for me to go with them because the United States had not yet declared war on Austria, as if that would make any difference in the outcome of the war, whether I was there or not. After I had gotten to Italy and everybody was quiet about this, no diplomatic episode occurred, we did declare war, so I was legalized.

This was a different kind of warfare than the flat, country type of warfare in Flanders.

Very different. The whole country was rocky. The Piave River was low. It was just a mass of boulders. The fields were hard, and after a while we were sort of in mountain valleys. In the first place, we were in softer ground, a hill called Mountebelluno, just north of Venice, but the shells that hit these rocks scattered like everything, and the Austrians apparently had huge mortars--maybe thirteen inch, something like that, and they'd fire them over the mountains, and they'd come straight down the valley and throw rocks all around.

It was getting along toward winter too.

It was getting quite cold, and the Italian habitations were quite cold. They have a little fire place in the middle of some room. It's open hearth, and you stand around, but before this Italian experience was over it snowed, and it was quite cold. They kept us on the move all the time. We kept warm at night, but we had marches, route marches, exercises and the doctor had to tag along too.

After you detrained, there must have been a march of a hundred and forty some miles.

Yes, there was a march from Mantua to the Piave River. It was a very long, hard march. The British had no--well, they apparently never had an officer like Major Munson who developed the Munson last for the American soldier's foot. The British shoe excoriated your heel in no time. It hurt the instep, the joint of the big toe. It would pinch the toes, and every one of these men had blisters in no time, so much so that almost all during that march I fixed feet most every night for most of the night. I was a mounted officer, so I was allowed to put soldiers with blistered feet on two things--I could load my medical cart with soldiers, and I could put a couple of soldiers on my horse. I could let them take turns, but the British were very strict against stragglers. The British marching discipline was wonderful, and these mounted officers would really ride a man down if he fell out. We went down these battalion roads in formation, looking good, everything shiny, and on the other side of the road coming out of the line were thousands and thousands of ragged Italians. You remember the scenes in Hemingway's Farewell to Arms? It was exactly the same thing. These men would go by and pick a field bare. They were just like locusts. They would go and take Italian houses and mess them all up, put feces on the floors. As a matter of fact, they had some way--I don't know whether they evacuated themselves into their hands, but you would find these lumps of feces sticking to the ceiling, so they'd throw it up on the ceiling. Anyhow, it was a pretty disorganized and unkempt lot.

They ought to have been good soldiers. I think they were undermined up there in the Austrian Alps by propaganda. Part of the propaganda which I picked up and which impressed me was coming from the clergy. I think that the Vatican clergy saw an opportunity to let Austria come through there and bolster the power of the Pope. That's what we supposed. Anyhow, there was a shortage of--well, at least the rations weren't given out to these men up in the mountains.

Something disorganized them a great deal. I never knew the whole thing, but there was supposedly propaganda from the clergy. The Austrians had some good generals at that time.

They did. There's one thing that we've overlooked, and that is that you went through this area in France before; in fact, you were there in 1908, when you took a tour with the family.

Yes, I'd been to France twice before this, and it seemed like the country I had seen.

That appears in the correspondence home, while you're not able to convey what the town is because of censorship, you do indicate to them that this is something that they have seen, or you described the flowers, or something familiar. You got a chance while still in France to go to Paris. What was Paris like in those days? How unreal was it with reference to the war?

I wasn't in Paris twenty-four hours.

That's right. You got recalled.

I went to Paris, and I went to the Hotel Meurice which is a spiffy place, got a room, and I took about three baths in the afternoon. Then I think I went and called on my Uncle Hugh Bayne who was in the Judge Advocate General's Office somewhere down in an office there. I think that it was this time. We went out to dinner and on the way out--I think it was the same evening--I thought I'd better go and report to the British Military Office there. I did go and report, and they said, "Oh, we're looking for you," and I had to go back the next day.

Yes, they had received orders to go to Italy.

Yes, well, I didn't know whether they were going. They made me go back to Abbeville, as I remember. Abbeville was on the Somme River somewhere.

<u>You did get a chance finally after this period in the northern part of Italy to go to Rome.</u>

To Rome, yes—ten days. Rome in that time was a very social and pleasant place, good restaurants. I was tagging around a little with Colonel Hudson who had an entrée in the British Embassy down there. We met a good many people, and I did sight seeing. I believe we got that leave extended a little bit.

<u>Yes, but then I think by the time you got back—well, you had orders to rejoin the American Forces. There's one thing about sanitation.</u>

You mean where Colonel R. J. Blackham gave me the devil?

<u>Yes.</u>

I don't remember that very well. I remember some meeting. Everybody had a lot of diarrhea, but I said something which, if I vaguely recall it, made him think that I was too careless of both the chlorination of water and sanitation.

<u>I think there was a dinner, and the comment you make in the diary is that "no one seemed friendly." This was a dinner for—I guess the 69th Field Ambulance.</u>

Not in Italy.

<u>Well, it was some organization in Italy that was over all the medical people.</u>

Yes, well, Colonel Blackham was called the ADMS, the Assistant Deputy for Medical Services, a staff officer, and he lived in a nice Italian villa. I used to see him occasionally. He lived in regal splendor, had been serving a long time in India, and he had wonderful sort of Maharaja clothes that he could sit in the evening in, but Colonel Blackham didn't carry anything after the war against me because I had a certain amount of correspondence with him; in fact, I saw him once, and he wrote several books which, by the way, reminds me that I meant to bring you some.

There is a large amount of diarrhea during this period. This is December.

Yes--on the banks of the Piave. It was more than diarrhea. It was dysentery. It was bloody dysentery.

How effective were the British laboratories? Did they have laboratories to study these problems?

Not that I know of. They were studying them, but I never saw them. A lot of good work was done in England and various places, but we didn't have any laboratory support out there. I remember one man standing up to be sent off as a replacement. When the Sherwood Foresters would get orders to send ten men to another outfit, you'd pick out the worst actors that you could, line them up. I remember that this man standing there fainted, and the Sergeant Major was pretty rough with him. I came up, and this soldier looked sick to me. I pulled his pants down, and he had bad dysentery. We all had a certain amount of diarrhea, but there actually was dysentery. I don't know what organism it was, but the British must have found out.

You make some comment about the shipping of your trunk for warmer clothes--you couldn't lug this stuff around. It was in a back area, and when you went from

France to Italy and in December in the mountains it got cold. What about access to clothing.

I don't remember any suffering about that. I had a heavy trench coat, and it seems that it was in that region where we had a Christmas party with some hot rum punch, and the padre was there. He and I left about mid-night, and we had to go around a narrow ledge of rock, and I fell off and slept in the snow all night. Is that in there too? Is it?

No, sir, it isn't.

I must have been a little mixed up.

It's surprising what you can do.

Yes, I had a heavy trench coat. Where I got it I don't know, but we had no shortage of blankets. We sent them up on quartermaster trucks.

I wondered about supplies, even medical supplies.

I had no shortages.

There is some indication in the diary that on one occasion the medical supplies were destroyed.

By shell fire.

Yes. But there was an enormous limitation on what you could do.

Oh yes. You didn't do anything, except try to keep the trenches a bit clean, try to keep the latrines covered, dig new ones, but it is no fun digging a latrine in the rocky banks of the Piave River, although the Italians could do it. When we got there, as I remember, they had full head high deep trenches right through the rock.

<u>That is commented on in the diary--that they must have been good workers, although you didn't get that impression as you watched them retreat.</u>

No, they were protected, and they were used to doing that kind of work.

<u>There were some labor battalions attached to the British in Flanders.</u>

Always labor battalions. They would dig trenches, and they would put up barbed wire. The main protection they had was what we called "elephant iron", which was a half bent corrugated iron that you'd put up and crawl under. It wouldn't stand any bullets.

<u>Then orders caught up to you.</u>

I had to wait. They said,"You can't be ordered out of here by an American order", and so they had to work that around. I think it took a couple of months to work that around, but all to the good. By that time I got up as far as the Asiago plateau.

<u>They started an attack on that.</u>

Yes they did. That's where Colonel Hudson got the VC one night. He stopped a German potato masher with his feet, and it blew off most of his feet, but you couldn't kill him. He lost his feet in that, or at least part of his feet. He had cerebro-spinal meningitis, and he had a ruptured duodenal ulcer.

<u>God!</u>

You couldn't kill him.

He was ready for duty. Then you had a fairly quick ride back to....

Dijon, I believe.

You went first to Paris, then to Chaumont, and they finally sent you to Dijon.

I must have gone from Paris right on out without any stops. Chaumont was Pershing.

Well, you went to the wrong station in Paris and missed the train.

Oh, did I?

Got out there a little bit late. I don't know that you knew, or that your orders specified that you were going to join a laboratory in France.

I got there. I don't know why. Colonel J. F. Siler was in charge.

Right.

Dr. Zinsser was around there somewhere.

Well, your orders are dated November 21st, but you didn't start to move until the following March.

Yes, there was a long wait in there.

There was a Lt. Colonel Gilchrist.

Yes, that's the same Gilchrist that brought over the Cleveland Unit, and he later became a general and the head of the Chemical Corps.

Who is General Bradley and Colonel Ireland?

In 1917, when World War I started, General Pershing wanted General Merritte W. Ireland to be his surgeon in the A. E. F. in France. Pershing started from here, and he wanted to take Ireland as his surgeon, but General Gorgas wanted Colonel Alfred E. Bradley to be the surgeon in the American Expeditionary Force because Colonel Bradley had been in London as a medical military attaché in the Embassy for some years and was actually over there. It's very interesting about policy and staff relations, and I've recently reviewed the papers, so I know. General Gorgas recommended that Bradley be made the Chief Surgeon of the American Forces in France and to have the authority over those medical establishments in France equal to what the Surgeon General had over the medical department here. That was the beginning of a very important difference in staff relationships which extended very far and interested us throughout ~~the~~ World War II also.

To answer your question about General Ireland--General Ireland succeeded General Bradley along about late 1917, and was the Chief Surgeon of the A. E. F. until, I think, March, 1918. General Bradley had to quit because he had an abscess of the lungs, and he wasn't very well most of the time, but Pershing didn't want him. General Pershing and General Ireland were both very intelligent and aggressive men, and General Pershing thought that no one stood between him and the troops over there except the President of the United States, and he had the bitter row, as you remember, with General Marsh who was the Chief of Staff, so much so that General Marsh got out an order saying that the Chief of Staff was the immediate commander of all these forces. General Pershing didn't pay any attention to it. All that is written up in General James G. Harbord's books and other things. They had a bitter time, but General Pershing was dealing constantly with the President at that time--it was Wilson, wasn't it? That set a tradition for the theater commander. Even the modern theater commander is like a viceroy, like a satrap, like an independent commander--almost independent, and that extended down to General Ireland, the Chief Surgeon, who made vast numbers of changes in medical organization, policy, administration, supply, hospitalization in France that were quite contrary to the published regulations and accepted things of the Surgeon General, so it made a split between the Surgeon General and the Theater Surgeon that existed all through World War II also. General Norman T. Kirk and General Paul Hawley--Hawley was the Chief Surgeon of the ETO. They got together pretty well, but most of the surgeons over there were quite independent of Washington--as much as they could be. Ireland then became Surgeon General in October, 1918, and was the Surgeon General of such renown--I think he held that office for twenty-three years, greatly respected, brought out the great history of World War I. Have you seen that?

Yes. What I was thinking of was that as of this time when he made the changes, you make them with reference to the scene you see in front of you.

Who?

General Ireland, when he made the changes. It's almost on the ground discretion as to what comes to you and how you're going to handle it.

That's what he said--this moving warfare in the field was so different from the static barracks, post-like things that had been going on in this country that he had to make changes, and we had over two million men over there which is terrific.

I don't know whether you saw them, or merely mentioned the fact that they were--

I think you saw a major, reported to a major, and they sent you on to Dijon where the laboratory was, where you talked with Colonel Siler. There was also a doctor there from Hopkins--Hussey. Wasn't Hussey there?

Yes, Raymond S. Hussey was there. He was a pathologist, and he goes in and out of my relations for many years. He was with us in World War II in charge of the Army Industrial Hygiene Laboratory in Baltimore.

You also met Hans Zinsser.

Oh yes. Zinsser was in the tide of glory, so to speak. Zinsser loved troops and liked movement, and he was a very important man, had charge of a division of infectious disease and laboratories, and he was the Sanitary Inspector of the 2nd Army at one time. First he was Sanitary Inspector of the 2nd Corps--well, in one of the corps. He was a very able, imaginative person who did a lot for modernizing field sanitation.

Do you remember the laboratory installation they had at Dijon?

Zinsser?

No, you--while you were there.

I was only there about four or five days, and I didn't do any work. I moped around until one morning when I was sitting on a bench by the front door, and Colonel Siler came in, and he said, "What! Are you still here?"

I said, "Yes, sir."

I couldn't say I wasn't, and he said, "Get the hell out of here! The 26th Division is on the road, and if you can find them, you can join them."

I did.

<u>This was the New England Division.</u>

Yes, that was the New England Division [the Yankee Division]--from Maine, Massachusetts, New Hampshire, Vermont, and that's all, I guess.

<u>A Captain Harry Martin is mentioned in the diary.</u>

Oh, Captain Harry Martin was, I think, a regimental surgeon of the 101st Infantry. Is that right?

<u>Yes.</u>

God, how can I remember those things!

<u>You had to report to him.</u>

Yes, I went from Dijon to Toule where the headquarters of the corps were located--whatever corps it was that was up there--just as fast as I could go. Fortunately I was picked up by a Briagadier General John M. T. Finney and carried in a car up there. They didn't know I was coming because I had no orders, and the Colonel--I forget his name at the moment--said,"All right. You want to go with troops. They're up the line at a place called Montsec"--which is a bare hill rising right out of the plain of the Woevre occupied by the Germans looking right down the throats of the Americans in horrid sodden trenches. The region I'm talking about is near a town called Seicheprey. Did you ever hear of the Battle of Seichsprey?

<u>Yes.</u>

Well, Martin was a vigorous, short statured man who was a good soldier. I didn't have to see much of him. I was under him as battalion surgeon. He was the regimental surgeon.

You arrived there on April 2nd, but your orders are dated April 11th.

That's often the way.

What's the difference between joining the 3rd Battalion, 101st Infantry as opposed to the 11th Sherwood Foresters?

Well, the difference was in the language, the habits, the food, and as far as the experiences in the war went, they were very much the same as being around Ypres. We were in mud, and we were being shelled all the time, under machine gun fire most of the time, and under the observation of the Germans who had the high ground. I didn't know anybody. I had nobody like Colonel Hudson that I admired so much to talk to and do anything with, and when you come into a thing like that you pretty well catch on as to what you're supposed to do and go do it the best you can, but again, I did the same thing I used to do with the British and I did it all the rest of the war, go out on my own and look things over.

I used to go out on patrols at night. I had an experience--I'm sure that fright made it possible for me to walk on water like one holy person did. I was out about a thousand yards in front of our lines. There was another man, and a party of Germans came in the dusk with their rifles and we lay down. They went by talking low in German, and after they went by, we lit out to get back to our trenches. There was a shell hole as big as a house, and I ran right across that without sinking, and when we got to our lines, a man stuck his head up with his rifle and said, "Who goes there?"

It was something like that, but I was over the top and on him and knocked him down before he could do anything.

<u>You know, it's surprising whatever it is will do to a person, what you can do given a set of circumstances. It's just incredible!</u>

Yes, if you get scared enough.

<u>Whether it's the adrenalin that makes you twelve feet tall, or makes you run the hundred yards in nine flat, or whatever it is, somehow you call forth far more power under certain circumstances like those that you described.</u>

Then there came this Battle of Seicheprey. The Germans about this time, I think, pushed the Americans out of Seicheprey and Seicheprey is a town lying on the lower slope of a very long hill going out into a very marshy plain over to this Montsec and to another region called Bois Menieres. They sent me in there to the 102nd Infantry because the doctor had been evacuated. Well, I went in there, and I had been there for a while under a picturesque man named "Machine Gun" Parker. He was a New Englander, a very buccaneering type of man, talking all the time of what he could do with machine guns, but I don't know that he did particularly much. Well, in that I took over a static position. Seicheprey was almost in no man's land. It was rubble at that time. We had posts all through there. I must have stayed there three weeks. We retook it from the Germans in a counterattack starting early one morning, and that's the first time I was ever fired on by our own guns. The artillery fired eight hundred yards short and killed a number of the 102nd Infantry boys who were trying to get to Seicheprey. Also while I was down in there, I used to wander around and talk to soldiers in trenches, and that was my first experience with anything homosexual. One soldier called me aside and said that he had to talk to me and please not to let them go through with the order to assign him to the battalion headquarters because he said,"The sergeant looks us over and picks us out and gets us in there and molests us."

The sergeant was a fairy. I looked a little further into it, and I found that homosexual practices were fairly common in that particular outfit and I never saw anything as demoralizing. Everybody was suspicious. Apparently there is something about according the favors of a man to a man that acquires far more for the one who gives it, a great deal of favoritism, and it was an unjust set of affairs. That got reported down to Chaumont and very strict orders came out about that, but it was a most disorganizing thing.

<u>Wasn't the 102nd overrun, in part?</u>

Yes, that was Seicheprey.

<u>Then they had to retake that area.</u>

Yes.

<u>That's when you went up in there. This was underneath the Germans. Weren't they looking down on you?</u>

Yes. Here is a place called Beaumont on a ridge, and then imagine this hill that goes down like that--Seicheprey would be here and Montsec there, but there were woods over here and deep trenches. That was the time that they were using cylinder gas. We stored a lot of it--great big gas cylinders like these carbon dioxide cylinders that you see going around on trucks here. A friend of mine, Captain Jerome P. Webster, was a gas officer, and he pulled up and left about twenty of these cylinders right outside of my aid post. If a shell had hit them, there would have been nothing left of us.

Well, the cylinder gas is what started the Germans on their chlorine attack on Ypres. They unloosed the chlorine in 1914, I guess.

Rough business, but you had a long time cleaning up this place after you had taken it.

Oh yes, it was horribly dirty. There were dead horses, dead Germans and dead Americans. It was soggy around, excrement all around.

There was a period of training that you went through. I don't know how long it lasted, but it would seem that it ran from the latter part of April on into May.

Yes, I suppose it did.

Just cleaning up the place. There is a whole series of towns--Buconville, Bernecourt.

Yes, I know that. That's down in the valley. I'll bring you those maps.

Then there was a phosgene attack.

Was that the raid we put on?

Yes.

They put on a raid against the Germans one night. I think the trenches were on Montsec, very deep trenches, and they had been used by the Germans a long time. It was our own phosgene that got us. We crawled out there before mid-night and were all lying around. We had white brassards on our arms to tell friend from foe, and I think there was a Major James F. Hickey--do I mention his name? I don't know why his name should come back to me after this time. He was in command, but he started running around in the dark saying,"Where on earth is my PC?"

He got excited, lost his way, and lost his head so somebody else had to take over and get ready for this raid which was to start around four o'clock. They had the most wonderful artillery fire. They had what they called a box barrage, a tremendous barrage of all the guns sounding down parallel to each other on the sides, across from each other on the back to keep people from coming in and in front the fire moved in while these two lines were firing down the sides. So we went in behind that barrage. You don't need to get hit if you don't get too close. I think they had more artillery in that raid than they ever had at Gettysburg, and all we caught was one little German prisoner. I remember this man. He slid down the side of one of these deep trenches and tore the seat of his trousers out. That embarrassed him more than the war, but he was the only capture we made. It gave me an idea later on which I used. We captured one night two hundred and some Germans--about twenty of us captured them. I didn't. I was with them. We had no means of guarding them all, so we took their belts and suspenders off of them, so that they had to walk through these woods and trenches holding up their trousers. They wouldn't let go of them. They were absolutely helpless!

Well, we threw phosgene over into the German side, but the wind was wrong. It was off to the right, I think, and it all blew back on us. Phosgene has a latent period. You don't know you've got it right away. It's not very irritating. There was so much dust and noise and smoke that nobody could tell that there was any gas coming, but as they all went back, some of these men began to get sick and vomit. Some of them had this frothy stuff coming from their mouths. I went back with them, and I think I must have had several hundred cases of phosgene poison--these men lying out there with no antidotes and no knowledge as to what to do. I don't think any of them died. I'm not sure. We got them all back to the hospital in ambulances.

<u>There was a decided shift in the wind.</u>

Yes--it was off to the right hand side, if I remember.

<u>I think we've gone as far as ought to go today because hereafter you get raised in position.</u>

I became Regimental Surgeon of the 103rd Infantry.

<u>Which is a different thing than being with troops.</u>

Well, I stayed with troops--is this thing still on?

<u>I'm going to turn it off.</u>

Wednesday, April 20, 1966 A-54, N. L. M.

There are a number of items which intrigue me--one of which I ran onto in the diary. I've already mentioned it to you, and you've indicated that you don't remember much about it, but I wondered what relationship you had to the sense of Army discipline as it manifested itself in Army law and the system of courts.

The British?

With the British and the Americans.

I had very little to do with the courts, except two court martials--the one that you mentioned with the British which I've forgotten, and one I'll tell you about later with an American which is quite interesting involving, as it did, a soldier in a battle and the family potentates at home who had political power greater than the power of the commanding officer. All sorts of things arose.

With the British, I admired their discipline very much even with this Sherwood Forester regiment made up of older men who had had practically no military training. By this time in the war--it was the third year for Great Britain--the discipline was good and strict. They had representatives of what we'd call MPs, Military Police, on the ground among the soldiers and there were a good many prisoners all the time in the battalion, soldiers under disciplinary punishments, or confinement.

Curiously enough I used to sleep right next to the prison, right next to the room where they put these prisoners, and I could listen to them talking and hear about some of their problems. They were not at all mutinous, but they still had a very independent spirit and would do petty things. They would steal from the local civilians in the towns where we were billeted. The British soldiers I was with were never violent like some of the Americans. In one or two of the towns where I was with an American company, or battalion, the Americans raped the French girls every now and again. I don't know of anything like that happening among the British. I think the British were older than the Americans which may have kept them a little quiet. They would--the British soldiers would steal fruit from orchards, but they didn't steal anything very valuable. Sometimes they would steal souvenirs--souvenirs were wanted by most people.

The discipline was administered in the British battalion largely by the second in command, as I remember. It might have been a personal thing in the 11th Sherwood Foresters. The commanding officer was more of a liberal, scholarly type, literary somewhat in his inclinations, whereas I think it was Colonel J. R. Halford, a big, tall man, a wonderful horseman, very stern and very strict, and he was the one we associated with disciplinary matters. He's the one who would ride up and down the line when you were on the march and ride down stragglers. That's about all I can say about it.

<u>This compares in some respect with the Americans.</u>

Yes, the Americans were not so disciplined. The Americans tend to be still civilians. These were not regular troops in the American forces. They were the National Guard regiments and the National Guard people. The 26th Division was made up of National Guard elements, the New England states. They had regular officers--some of them, but some were not regular. The 26th Division was commanded by General C. R. Edwards who was a great favorite among some people. His motto was "stout hearts and discipline", but they were quite irregular soldiers in many ways. They wandered around and didn't take proper care of their sanitation. You had to be after them all the time. They had good fighting esprit, but not good living esprit, but that is characteristic, I believe, of American youth in most places. They are not disciplined. Don't you think so?

Yes. I don't know why that is, except I think you put your finger on it when you say that they are largely civilian oriented and without the sense of tradition that the British would have. You know, the 11th Sherwood Foresters meant something quite apart from the men who were there.

Yes. The 26th Division didn't have very high standing in Pershing's Army. For instance, the Second Marines were better fighters and much better disciplined and tops. The 1st Division was a top division. The 32nd Division was also as was the "Rainbow" Division that McArthur had made up very much like the 26th Division, but it had a much better reputation at headquarters. It was felt by us that the 26th Division was being penalized because of an unfavorable reputation, penalized by being kept in the line. The 26th Division was kept in the line from March until November [1918] with practically no time out at all and with many battles.

I remember one instance--this is by hearsay--General Edwards was indirectly reprimanded by the Commander-in-Chief. As you know, in the Army you're not supposed to put two subjects in one letter. You can't file two subjects in one letter, but General Edwards wrote a letter to the headquarters commanding the 26th Division, hoping that they would get a divisional citation. In the same letter he asked for ten thousand pairs of socks. He got a reply saying that he mustn't put two subjects in the same letter, and the 26th Division didn't get any divisional citation.

At about that time they sent up to command the 26th Division a Brigadier General F. E. Bamford who was a Marine. He was a blustering disciplinarian, and we thought he tried to take it out on the soldiers in the Division. There was another Marine commander in a battle once next to us named Colonel H. J. Bearss. He was rather a law unto himself, but a strong disciplinarian that carried his men where he wanted them to go. We were in the Battle of St. Mihiel where we had to make an agnle turn to the east--starting south and turning about thirty degrees east--the whole line was supposed to do this. Well, this Marine ~~battalion~~ COMMANDER didn't turn and went right straight through the wheeling 26th Division. That's what Colonel Bearss did.

In another case Colonel Bearss went off with great bravado to the German side and got pinned down in a farm house, and we had to go and rescue him. Curious things were tied up with discipline in a way, and the disciplinarians, I thought, were not always well disciplined themselves.

<u>How representative was the court martial that you attended?</u>

That was an interesting thing. This was in the battle southeast of Verdun when the Meuse Argonne had started up north of Verdu~~j~~N. The 26th Division was on the flank and had to mkae what I thought was a very nerve-wracking diversionary attack. We attacked from the hills down into the plain of the Woevre under severe fire with barrages and everything else with the knowledge that we weren't supposed to gain anything, but were just supposed to divert the attention of the Germans--stay, if you could, and get back, if you could. Well, in that region--this is about October, I think, of 1918--there were great tunnels in the hills that we were on and from which we took off. A sergeant with me one morning when we were well into an attack was found hiding in a tunnel, so I put him under arrest for dissertion in the face of the enemy. I had no way of having him put in confinement. We were in the line and were fighting a daily sort of battle, but he became a sort of a ward of mine. I had to watch him and be more or less responsible, and whenever we moved, this man would stick along with me, with the medical section I had. That continued from that moment until after the armistace. I had no way of bringing his case to trial and no way of dismissing him on my own word. The chaplin began to intercede for him and other people. He and I got rather attached to each other in a friendly way. It was quite embarrassing and rather ridiculous.

Finally they did try him, and it was so long after the event that they just dismissed him, but it turned out that a situation existed in this man's case that probably existed in several other American units, and this situation might have conditioned this man's sense of independence sufficient to make him hide himself when the battle was beginning. He came from a small town in Maine where his father was the chief undertaker, and his father was fairly well-to-do. The commanding officer of the company in which this man was, was employed occasionally by the undertaker who had political power also, so that the officer in the company couldn't control the enlisted man because the enlisted man's parents were more powerful than the officer's people. This man came from such a situation, and I imagine that existed in a good many of those National Guard outfits where the hand reaches out from the local community into the military situation protectively.

<u>That certainly was absent in the British, wasn't it?</u>

I never saw anything like it in the British. I think also that there was a caste system with the British. The officers are much more remote from the men than American officers are.

Yes, but that can lend itself to a lot of mischief--you know, it spreads. I mentioned one other interesting item before we turned the machine on; the nature of water supply in a static situation where you have trenches, dug in positions, prepared positions as you did in Flanders and in Italy, and then the changes that are demanded because the battle becomes fluid, chase and run affair toward the end, particularly from perhaps September 1918 on, when there was this steady push. Now, purification of water seems like a simple problem, but it isn't.

Well, as I recall in the trench situation, water came up either at night in water carts close to the line, or what they called petrol tins, five gallon gasoline tins, containers. They would be brought up strung around the necks of soldiers carrying them. Most of the water, as I recall it in the trench situation, was chlorinated somewhere and brought to you. On the move when the fighting is in the open country, men will drink from streams and don't pay any attention to purification. Occasionally, I suppose, you have a Lyster Bag and set it up, or local chlorination--you try that with the American troops all the time, but you often can't use it. I don't remember much difficulty with the water supply on the open warfare part because it was a rather short period for us. We broke through--the 26th Division, or the 103rd Infantry I was with broke through Belleau Woods and went to the Vesle River, but it was rather fighting all the way. It wasn't just a march, and we had no real open country in the St. Mihiel, or other regions.

When you've picked up experience in terms of raids, no man's land, the possibility of casualties between fixed emplacements--what sort of demands did it make on your crew, the people you had with you, when you begin to have a more fluid operation? Of course, you have to play with what you have, but I wonder whether the experience led to any rethinking at some later time on the kind and quality of aid that you had.

Well, it varies very much on the terrain where you were. Let me go back to the St. Mihiel battle again. That started about two o'clock in the morning with a terrific heavy barrage going over, the shells falling on the Germans and close to Americans in a region that had been fought over since 1914, and it was all a mess of barbed wire, grass, and shell holes. When I started out and could see something by daylight, could see wounded in the middle of that mess, I really dropped my medical supplies, except for a small package of bandages. I found a German wire cutter, and I went through that barbed wire on the ground and liberated people who had fallen and got entangled in the wire. I still have those wire cutters at home and use them. They are very strong. Now, that was an open movement for about two miles or more, and then by evening we were off the hill and down into the plain, and that's where the lines sat for the rest of the time. They wiped out the salient and had lots of counterattacks, but they didn't go very far. That was a long battle line way over from Montfaucon way over nearly to Verdun, but aside from cutting out the salient, it didn't advance very much, nothing like the sweeps that Patton's Army made in World War II. They went through that region also.

Was there any problem in evacuation?

Evacuation is, I would say, easier on the move than it is in the fixed position. The fixed positions we were in were so much bombarded and had been so cut up with shelling that you really couldn't do any evacuation until toward day break, or maybe some time in the dark. The terrain was very bad. It was very hard to move anything. No vehicles could move in there, whereas when you're in the open, stretcher bearers have firm ground under feet as a rule and carry longer distances. Little ambulances can also come up very close; in fact, when we went through Belleau Woods, they had ambulances in Belleau Village which never would have been, if you had been fixed in a trench. These were Ford ambulances, little Ford vehicles--I think they carried about four men.

Did you remain at the St. Mihiel sector until the end? Quite early in Flanders you got nicked on the leg by a piece of shrapnel, spent shrapnel, which healed in a short time with no problem.

In fact, I never reported it.

Yes, except in a letter home, but you went all through this up until about eleven o'clock on that final day when again under bombardment, I think--I don't remember, but somebody discovered some casualties, or indicated that there were some out there. I don't know whether you knew that an armistice was coming.

Well, now you've jumped ahead a month.

I was just wondering in personal terms--injuries and the possibility of injury.

You go from St. Mihiel up to this region southeast of Verdun which is an indiscriminate kind of region. Then we turned and marched through Verdun, up the east bank of the Meuse River about fifteen miles past Fort Veaux and some of those Verdun battle fields and stopped at a place called Samogneux. Samogneux is up over the hills that rise from the river. We finished the war there eaxctly in the place where the French first met the Germans in 1914--you can see what a fluid war it was, but it was relatively grown over with bushes, and it was hard ground. Lately it had not been knocked to pieces too much. They had some American little tanks--"whippet tanks", they called them--that tried to come up through there, and the Germans knocked off all six of them, ~~ste~~ stopped them right there. Our line from the Germans east of Samogneux in the edge of the forest was probably thirty feet. You didn't dare stand up, but at night there was an exchange of cigarettes for some things that the Germans might have--there was a little fraternizing going on.

Then there was that rather heavy shelling from two directions--getting on now, toward Armistice time, on the 11th of November--we would get shells from behind, coming from the western side and shells coming from the eastern side because there was a curve in the line there. As far as what happened on the morning of Armistice day--yes, I knew there was going to be an armistice about a day ahead. So did everybody else, but for some reason, for morale, or to impress the Germans, orders were given that although--they didn't say this in the orders, but I'll say it--that although the high command knew that there was going to be an armistice at eleven o'clock, they ordered the battalion units to go over the top at daybreak. They started a big battle in that region early in the morning, a very foggy morning. The ground is gullies and slopes and little valleys, and you recall that I knew of some wounded in one of those places and started to go get them. I got very close to a German machine gun position and finished the war lying on my face and belly being shot at.

Then after it was over, that night we had a Fourth of July celebration. In the first place--I may have it in the diary--a German with a beautiful baritone voice started to sing. Suddenly when the guns stopped, the silence was terrific--oh, amazing silence! That night everybody who could get his hands on a Verey pistol, a Verey light pistol for barrage signals, started to shoot them off, and there were beautiful fire works. Big flares would go off, and the barrage signals were kind of a Fourth of July rocket affair that exploded in the air and then dropped a long tail of different colored burning flares, and it was a great celebration.

How did you feel about this at 11:10?

It's very difficult for me to recall how I felt about it. I felt elated that the allies had forced the Germans to stop fighting. I was glad that I was still alive, but not hilarious about it. I was as dirty as I could be, as lousy as I could be, and I guess I thought mostly of my own comfort. I had men to take care of. We had to find food, or reorganize, or get together again because we were pretty well scattered. Then we gathered up and marched all the way back, a long way back to a place called Bazoilles which--I don't know. It must be thirty, or forty miles.

All of forty miles.

I was still looking to get clean, and I hadn't had a bath since September as far as I remember. The only bath I got was on the banks of the Meuse when one night the sergeant and I went down there. We thought we'd jump in the river, and when we got out we saw bales of clothes on the banks, underclothes, and we put them on. It was an old, abandoned delousing station, and these underclothes were full of louse eggs, and in a few hours we hatched a million lice that just drove us about crazy. I didn't have any chance to change them. My uniform was out at the elbows. Although I was a captain, I had bare elbows and the cuff ends of my uniform were ragged. The inner seams of my pants were ripped. I had them held together. I wove a little willow twig through them to hold them together, and when I went back, got back with the battalion to the rear area, I found myself right in the midst of the Johns Hopkins Hospital Unit at a place called Bazoilles—"Braswillie" we called it, and my old immaculate friend, Cy Guthrie was in charge. Is that in the book?

No, it's not.

Cy Guthrie was in charge. He was a major then, and I had another friend from Hopkins named Frank Evans who was a medical man from Pittsburgh, and they wouldn't let me come into the officer's quarters I was so filthy. It was cold and moonlight. They made me undress outside the bath section of the barracks, and Frank Evans then pulled me through the window into the shower bath. I got a little clean. I bought a uniform from Dr. Guthrie. Then I was in good shape.

The battalion was reconditioned too.

Who?

The battalion you were with. This is where they received British shoes.

Oh yes. Well, they were very uncomfortable. Then from that place we went on south in France to a place called Montigny-le-Roi where the division headquarters was. From that point I got up from Regimental Surgeon to be Sanitary Inspector of the whole 23rd Division. By that time I got promoted to major. I'd been a captain when I entered the Army in 1917, and I had received no promotion all that time, although I had jobs that called for major's rank. As a matter of fact, you get lost the way we were doing. I don't think I got any pay when I was with the British—for months, but I had a letter of credit that my Uncle George took out, and every time I got near a French bank I would draw out money from it with this letter of credit much to his surprise, I think, and maybe some inconvenience.

The 26th Division Sanitary Inspector was a very interesting and enlarging position because you had the power to go all through the division to see what was going on, to try to see that the billets were clean and sanitary measures were being properly observed.

At one time down there, we were visited by President Woodrow Wilson, General Pershing, and Chief of Staff Tasker Bliss. That visit was very hard in some ways and very amusing in others. To get ready for it they hauled us out in the road about daybreak. It was some cold, wet morning, and we stood along the roadside that the President was coming down from about daybreak until he came, around 11:30. He had dinner, a noon dinner, in the big dining room in a French hotel in this resort town. What I'm leading to, I think, is indicative of the lack of tact, or a political sense on the part of the President. We were all in the dining room, and we had taken a lot of trouble to get this dinner ready for him with the best food we could find. He went through the dinner, and at the end of it we thought that the President would get up and say that we were to get home soon. There was a long sort of pause after dinner. The President and General Bliss got up, walked across the dining room to the main door, and as Mr. Wilson went through that door, not having said a word to us, he turned around and in a rather high voice said, "Good bye."

n That's all that he did. It was extraordinary that he should do that.

<u>It's just not understandable in a political figure.</u>

It was understandable to me afterwards. He was, I suppose, an academic sort of man who may not have known how, or may have been too proud to condescend to jolly people along, but all of us were disappointed, except one reporter woman who went around after the President had left and collected his spoon and fork—we dined off of mess kits, and she was getting souvenirs of the utensils that the great had used.

Were you bothered much by reporters during the war?

No, I don't recall any except at this big party of high government officials coming with the President.

He came off pretty low that day.

Yes, we didn't feel much affection for him.

This becomes a waiting period too, as to what is going to happen next. There are notes home fearing being made part of the Army of Occupation, hoping that it would not happen.

I have that in my writings?

Letters to "Tante E".

Well, we didn't know whether the 26th Division would be in the Army of Occupation. As a matter of fact, they formed another new army--the 3rd Army was established and organized very late--oh, early in November [1918], about two weeks before the Armistice, under General Dickman. They had no training, no experience with civil affairs, or occupation procedures. I think we knew the 3rd Army had been put together, and I suppose we were afraid that they would put the 26th Division in the 3rd Army. They did put a division in the 3rd Army—

no new ones, but the 42nd, for instance, was put into the 3rd Army and stationed in the Ahr Valley. When I was Sanitary Inspector of the 26th Division, some high officer came and called on me one day when I was making an inspection and intimated that there was a position that they would like me to take at some headquarters--I've forgotten. I didn't. I wanted to stay with the 26th Division. Then orders came for me to go and become Sanitary Inspector of the Army of Occupation which I did in January of 1919. I think that's the date--early in January.

<u>January 7th in the diary,"Ordered to the 3rd Army on the Rhine as Sanitary Inspector of the Army" but this is a brand new thing, a brand new setting. I don't know that any existing regulations, or manuals related to the kinds of problems you could get into. You make certain changes in the organization--particularly with reference to the army epidemiologists, one of which you were allowed, but you thought that you needed two--one with the statistical branch, and one assigned to the laboratory in the event an epidemiologist was needed. This is the first time that this comes out--that is, the use of an epidemiologist, the need for two in the kind of organization you had with the static occupation troops. It makes a lot of sense.</u>

I think that the idea of needing the two in addition to the Sanitary Inspecter is not to be credited to me so much as to that very wise Colonel who was the Surgeon of the Army of Occupation, Colonel J. W. Grissinger. We set up an office there in a fine building in Coblenz. The Surgeon's Office was at headquarters, and lo and behold, the epidemiologist they sent to work with the Sanitary Inspector was my senior, so to speak, from Johns Hopkins, my admired friend Alan Chesney, a much better man than I was. We set up an office in a big room--two rooms maybe, next to the Surgeon's Office, covered the wall with the huge map of the occupied area. We had daily, often hourly reports of what was happening in the occupied area. We got around a bit ourselves in cars, and we pinpointed every case of infectious disease that was going on there. They had two outbreaks of influenza, and they had a considerable amount of typhoid fever among the divisions in the Ahr Valley. The influenza was among the soldiers too, but the typhoid was mostly among the civilians. We knew that, and we knew about scarlet fever and the things happening in the Army.

I think there were about two hundred and fifty thousand men in that Army of Occupation, occupying a great strip of land from the border of Luxembourg to the south bank of the Rhine and then half of a bridge head extending over across the Rhine. The other half was occupied by the French. Well, Alan Chesney and I had all these statistics and locations, and Colonel Grissinger just thought that was very fine. All the visiting brass hats and everybody that came to Coblenz he'd bring into this map room of disease and show how much he knew about what was going on in the troops and civilians in the region. We had a good control over things. There was a wonderful liaison with the Civil Affairs. There was a Civil Affairs Branch Headquarters separate from the Army to deal with the civilian health, and the man who was in charge of that had been the Health Officer of the City of New York. I forget his name, but I knew him.

I stayed there from January until some day in June, 1919, and during the course of that time I had two personal experiences with Colonel Grissinger. One of them was somewhere along in April, I suppose. Telegrams began to come in from Hopkins, one from Dr. Welch saying that I was needed back in Hopkins to teach. I don't know whether there is any of that in the book or not.

<u>No, you got this in the mail somewhere along the line.</u>

That's just my promotion. Well, Colonel Grissinger was a very honest, straight-forward man, and this telegram came in from Dr. Welch saying that I was needed at Hopkins to teach and to please let me out. Colonel Grissinger called me in his office, and he said,"Major, do you think you're really needed at Hopkins to teach?"

I said,"No, sir."

He said,"All right. Go back to your outfit."

I couldn't tell him that I thought I was needed to teach. Then at the end in June, when I did get released I went in to say goodbye to Colonel Grissinger, and I got the greatest accolade I ever received. After all those months of hard work he said,"Goodbye, major. You haven't given me any trouble."

<u>That's wonderful. This area in terms of its own sanitation, existing installations, went from more refined in the larger cities down to almost crude in the little villages.</u>

In the occupation area--yes.

<u>And while a certain percentage of the troops were housed in barracks, a great many of them were housed in various levels of concentration of the population.</u>

Yes, in civilian houses.

<u>Yes and with the added burden, the tax that they're being there would be on existing installations. What sort of help did you have--Army Engineers, and so on?</u>

The Army of Occupation had an engineer battalion to take care of the water supply, and that was under laboratory control. This other laboratory man you mentioned came up there. I forget his name at the moment. He was in charge of the laboratory and most of their work was diagnostic work on some diseases like typhoid and constant supervision of water supply, but in that region the vegetables and fresh foods like that were a source of great anxiety because the Germans fertilized that region with what we called "honey carts". They would pump out the cesspools into these wagons--they looked like great big barrels, and they would drive them out in the fields and would spray the fields with all this decomposing excrement, get it all over the cabbages and the low vegetables.

The other difficulty in Coblenz and other places was venereal disease, and the policy was still, and it always is, under debate as to whether you try to control houses of prostitution and by examination say that certain women are not infected, or whether you try to prohibit it altogether. In Coblenz the authorities authorized certain houses to be accessible to the men, and you'd see lines, hundreds of men standing outside the doors of these houses of prostitution waiting to get in. They had a fair amount of venereal disease, but not too much.

<u>What did you do for influenza?</u>

You can't do anything for influenza. Influenza--the 1918 epidemic of influenza was very much worse in the United States and in the camps of the recruits over here than it was in the more seasoned troops in France. There were two waves of influenza, one in March and one in the winter extending over to 1919. What you tried to do was separate the man, and to sleeping quarters you would try to get as much air in the place as possible. There was no drug that you could use. There was no vaccination at that time, but I've recently looked over the statistics of the influenza in that region, and it was not very great.

No, except--you know, it was spotted on the horizon. Given the experience of 1918, and its possibilities.

Oh yes.

Whether it was the more settled life, the more frequent baths, and bathing, incidentlaly, was set up too, again I suspect with the Army Engineers and their mobile bath.

Yes, also these troops were getting seasoned by now. They were stronger and more resistent than the recruits.

There were a number of telegrams--you mentioned telegrams. There was a telegram--I guess this is in one of the letters--that indicated General Ireland through General Gorgas asking your release as soon as possible, and to that Colonel Grissinger replied not available. Another telegram came back to him asking when you would be available, and it was here that it was indicated that you were urgently needed by Johns Hopkins. You're in the Army Of Occupation, the war is behind you, you're probably at sixes and sevens. You have a job to do to be sure, and it has its interest. You're going to work anyway at it, but--you want to pick up older things. I guess this is symptomatic of all troops, a desire to get home.

Yes, I wanted to get home, although Coblenz is a delightful city. We went to the opera almost every night. We had the officer's club.

There's some marvelous poetry in these records by "Speedy" Swift.

Oh yes, that's Dr. Swift of the Rockefeller Institute who is a great man on rheumatic fever. I lived in a billet that was a palace on the top floor of the house of one of the members of the firm of Kuhn, Loeb and Seligman, big bankers--a very plush life.

A far cry from dugout days.

Yes. That was the bank president. We were close to the bank, so we had American soldiers guarding the place. They were polite to us in there, but the family didn't associate with us.

The job, in effect, had been done.

Yes. There always was a great deal of boredom in the war just as much as at any time. I don't know of any situation where you're so bored day after day--even in the line.

It's a deadening thing, except when you get moving, and there's sound. That somehow excited adrenalin, and you can live through it, or at least you can move, but there are days and days of boredom. There is some indication of going back to Hopkins--you did get some indication from them that they had held your position for you.

They told me when I left that they would hold it open, and I had no anxiety about it at all. I took it for granted.

This was reaffirmed. I don't think you planned on being gone for such a long time perhaps, though I don't know.

I don't remember that they ever calculated on how long I'd be.

In any event, your return from war to Hopkins in terms of the correspondence is like the silence of November 11th--certain doubts about enthusiasm, feeling strange and distant--in a sense, the laboratory itself wasn't the same laboratory. Faces had changed and moved on. You moved into 712 St. Paul Street which was pretty close to where you lived when you started in Hopkins. Do you remember anything of those days, those initial days, trying to find your way back?

Yes, I remember that it was a rather slow process getting out of the Army. I landed--I forget where I was. Newport News?

Yes, Newport News, Virginia.

Yes, and then I had to go to Fort Dix to get dismissed from the Army which is a long way from Newport News, and that took some time. Everybody is in a rush to get out, and nobody seems to care very much about anybody else. Then I went back to Baltimore. It seemed very natural to get back to work gradually. I didn't have to wait, as I recall it, for anybody to get out because I don't believe that anybody was in my place in the laboratory at that moment. I believe I had been promoted during the last part--what--June, 1919, while I was still over there, to what they called their position of associate which is just under associate professor. The laboratory that I went into, I think, was in the Hunterian Building across the street from pathology where I had been before--no, it wasn't. Was it?

This I don't know.

No, no. It was back in the pathology building, and then Dr. Lloyd Felton and I started to do work there. We built a hydrogen-ion generator, and that generating hydrogen set that building on fire. I think it was in 1919 that that happened, and then we moved across the street.

Initially this period--well, you lived with a Dr. David M. Davis for a while.

Yes, I know him still. He was a urologist. He's still living.

He was waiting for a house to be built, or was going to move into a house where, I believe, he was going to live with his sister, but he joined you at 712 St. Paul Street. Then you met a Dr. P. A. Schule who back in the Army of Occupation was very much interested in refuse and left certain instructions to collect it, and you not infrequently gave instructions to burn it.

I don't remember that.

But he was at the Hopkins apparently. Then you moved into the Baltimore Club--you seemed to have been at odd's end.

The Baltimore Club was a very nice place to be in. I think I must have stayed in the Baltimore Club for a year or so. I had a room there.

How did you feel about the laboratory? Can you pick up things? Do you have to start fresh? There's evidence in the letters that you encountered difficulties making a pure culture again.

I don't remember.

Well, you had in effect gone at right angles with laboratory work, straight bench work, and had gotten into administration, sanitation.

All I recall is that when I got back there, we just went back to work very busily. We were studying cultures of all kinds and had a collection of influenza bacilli to work on. I'm a little uncertain now. Did Davison come in at this point?

<u>Yes, sir.</u>

Wilburt C. Davison. I was in the fifth floor again, and he came up one day and asked if he could work in the laboratory. He'd been a great friend of Dr. Osler in Oxford, became a good pediatrician in the Harriet Lane Home, and then he became the Dean of Duke University Medical School and was Dean there for twenty years or more. He worked with me for a while. Felton worked with me. Dr. Thomas Rivers came.

<u>I want to go into that next time--Felton and Rivers, but MacCallum was the head.</u>

Dr. MacCallum was the Professor of Pathology.

<u>Yes.</u>

At Hopkins they had never given bacteriology independent status in spite of the fact that Dr. Welch was one of the greatest bacteriologists in the country. He was also Professor of Pathology, and I think it was his feeling that bacteriology and pathology were so closely bound together intellectually and by methods and interests that he preferred not to separate them. Dr. MacCallum was head of the combined bacteriology and pathology--pathology predominated.

<u>His wasn't an unfamiliar face, but it wasn't a familiar face.</u>

MacCallum?

<u>Yes.</u>

No, I didn't know him until--well, I got to know him during the course of that year.

<u>There's--well, this may be related to the whole business of coming out of the war and getting back to civilian things, but your correspondence during this time indicates that there is a certain quality about MacCallum that makes you feel like an outsider in your own laboratory. It's just possible that in 1919, this was so.</u>

I think Dr. MacCallum would have done that even if there hadn't been any war.

<u>Really?</u>

Dr. MacCallum was a rather sharp, critical man who was deeply interested in original ideas of his own. He was a top ranking pathologist, wrote the best book in pathology in the country, still a classic in its way. I don't think he was very much interested in bacteriology. I remember in this same building that he decided that he would take a lecture I was going to give on typhoid bacillus. I was awfully glad to have him meet my class and talk, but he got up there--I remember very well, and he said,"Typhoid bacillus is a gram-negative, motile organism that ferments glucose without producing gas", and then he suddenly said,"Well, all that's perfectly dull! Let's talk about something else."

Well, you'll feel a little strange with a man who will treat your subject that way. Dr. MacCallum had traveled, and in India he had gotten interested in leprosy, I think he got some flayed skin of cadavers that he brought back that were peculiar. He was a brilliant person.

<u>Dr. Felton wasn't the easiest person to work with.</u>

No. Felton was a high strung, enthusiastic man of strong opinion, very well informed and rather an original investigator. We got to knew each other pretty well, but then we rather separated when I loaned him some money. I got to needing money and asked for it, and that was thought to be pressing things too much, so that rather separated us. That was several years after this.

I get the impression from the corrspodnence that he had the kind of personality that blows hot and cold--you know, explosive on occasion, and that there were not a few rows in the laboratory with one person or another, and you'd hear it from both sides. That's not a very good position to be in, or to find yourself.

Well, Felton was very able, and he went on to Harvard and did some fine work.

Very good work--yes. During this period there is mention of two offers you received; one is on Dr. Richard Strong's staff at Geneva, the International League of Red Cross Societies. Do you remember that?

No.

That's strange because it figures on the correspondence.

He wanted me to come on his staff?

Then there was a big land and oil company that required someone in Central Africa which you talked about not a little in the correspondence.

Curiously enough I was not interested enough to remember them.

You say in the letter that you're pulled in that direction partly because of the difficulties in the laboratory, but that you're going to stay, so opening year, the unsettled nature of 1919, into 1920, was apparently a difficult transition year all the way around--people coming back either overworked because they had remained at Hopkins and hadn't been part of the war, or overworked because they had been in the war and were returning to Hopkins, and all the clash of temperament that you get out of a collectivity that is trying to find its way into a momentum again. I think that's probably a fair summary of what transpired, and it is reflected in differing ways in the derrespondence. Let's pick up the Bence-Jones protein studies next time--that brings in H. B. Cross, D. W. Wilson, Tom Rivers, and I wondered, in your thinking about it, whether you go back and pick up any of the items and ideas that you had with Zinsser to pursue. Rivers has influenza, and there were some experiments with cats--just where you got the supply....

We had cats up in the animal room on the top floor. That was over at the Hunetrian Laboratory. We moved out of the other building which had burned.

Yes, there's the whole business of the fire iN that laboratory.

You want that new?

We've gone just about an hour. You were tired when you stareted.

I was.

Well, you won't be here tomorrow.

I won't be here until Monday.

Well, we'll put it off until Monday.

<u>Monday, April 25, 1966 A-54, N. L. M.</u>

<u>The first thing I'd like you to tell me something about today is this little book that you brought in with you this morning on the Eclat Club, a kind of continuation of the commraderie, the spirit that some medical people had as a consequence of their war experience.</u>

The little book I brought you this morning was one of the original copies of the history of the Eclat Club, meaning--a French word for shell burst--<u>éclat d'obus</u> was the burst of a shell. Eclat here doesn't mean any high society, or artificiality. The club was formed in France by a group of surgeons who wanted to continue after they got home their personal relationship and really, seriously, to do something about keeping alive the new knowledge they'd gained from advances in surgery and the treatment of wounds that they had been able to make during the war. They wanted to keep up their personal relationship which had rapidly become cemented into lasting and unbreakable friendships, so in 1919, I believe, they had an organization meeting, and as they say in their preface to this book, this was perhaps the only society of its kind in America, that it stands for something choicer and finer than any other group, or organization of medical men in the country. They add also that the key note of the club must be comradeship,"The rare type of fellowship that was born abroad and is living always at the home and hearts of the members" of this club. Well, that is said by men who had been through very tough experiences, and it is not just a soft sentimental touch.

My impression was that they had elected within the first few years of their formation forty men. You counted the names in this book and have gotten the dates which have gone out of my head, but most of them--as I remember it--were surgeons, except four, and those four were what we call medical men--Dr. Hans Zinsser, Dr. Russel Wilder from the Mayo Clinic, and an extraordinarily fine man named Dr. Maurice Pincoffs, the Professor of Medicine at the University of Maryland, and myself.

The club had several meetings a year--as I recall it, usually two, but as you say, sometimes three, and those meetings were divided into two parts. The morning session on the day of the meeting was usually a clinic in a hospital followed by discussion and talks, not set speeches, but people really telling about their experiences and discussing cases, conditions and ideas of medical education and training. In addition they discussed the possibilities chiefly among surgeons for high appointments in medical schools. They didn't take any part in the politics of the appointment of professors, but these men who were members of this club were in high positions in their schools and could influence the selections of deans and particularly professors of surgery. I think for a number of years they were probably the most influential body in the country in exerting a disinterested influence on getting the best men available for these positions.

The second phase of the meeting was a jamberee sort of thing done in high style. Limousines would pick us up at the hospitals, or medical schools and take us off to some place for an afternoon and evening meeting. One that I remember particularly was going from Albany to the Battlefield of Saratoga, passing on the way the house where General Grant had died of cancer of the throat. We went thoroughly over the Battlefield of Saratoga where Johnny Burgoyne was playing his part as "Gentleman Johnny", where he got really badly licked by the new sort of Army that Washington had at that time. I think that the battle was in the fall of 1777. Is that right? I don't know. I have forgotten the date exactly, but anyhow, it was probably the crucial battle of the revolution because if Burgoyne had been able to link up with Clinton in that part of New York, there would not have been much for Washington to do. Also Washington's Army at that time was down to 7,000 or 8,000 men. The worst thing had been the small pox in Continental Troops in the early part of 1777, so important that when Washington was at Morristown he decided to have the whole Army inneculated. Jennerian vaccination by innoculation with ~~small~~ cow pox hadn't come in so, as you know, they had a way of immunizing actively. It was a very interesting thing to have been done on a mass scale--the first introduction of immunization on a mass scale into the United States Army. After that time, Washington had practically a small pox free Army, and he had small pox free soldiers when he fought Burgoyne. I have later learned that a good many serious historians of those times say that the doctors won the revolution. I can't make any such claim as that in the face of the character of George Washington, but they had an enormous effect on the protection of the troops.

Well, after visiting the battlefield and learning something about the history of the times, we went on from there to the race track at Saratoga and spent an evening in the gaming rooms, a very good time. A very good dinner usually ended the meeting.

Another time I recall was when the New York World's Fair was just beginning--not the last one, of course, but the one before that--when Grover--

<u>Whelan?</u>

Whelan met us and was our guide. It was an example of how these men could get anything they seemed to indicate they would want, and the afternoon of that meeting was spent at Theodore Roosevelt's old house at....

<u>Cold Spring Harbor?</u>

It's near Cold Spring Harbor, but it has a name. The house--The Roosevelt house is on the hill up there [Sagamore Hill], and he's buried there, but one of the members of the Eclat Club was Dr. Richard Derby who by marriage was related to Theodore Roosevelt. We had a good talk at that time about affairs of the government, visited the grave and then afterwards spent time in a very plush club there at the place at Oyster Bay.

That's the kind of thing that went on until about three years ago. The club decided that after it had elected its forty some members, it wouldn't elect any more, and as the years went by, they would generally spend more time counting the ones that had passed on than looking for anything new. It got rather dismal, and so a few years ago they decided not to hold any more meetings. They gave their silver cup to the Smithsonian Institution, and they turned over the records of the club to their most renegade and defective member, myself, and I am now surviving as the Secretary of the Eclat Club.

<u>In personal terms, coming back from the war, did you have any thoughts about the experience at all? Let me dilute what I've just said. This is a period in which society somehow sanctioned patterns of behavior that would appear to be, or seemed to be, illogical in a stable society--the destruction. Flanders Field, for example, is a staggering experience. But in personal terms, looking back on it, what do you carry away with you on the nature of warfare?</u>

I carried away a very distinct dichotomy, or schizophrenia, or whatever you want to call being able to live with two ideas about the same thing. Warfare and destruction strictly related to military effort seemed to me to be a very natural sort of a thing. I knew you couldn't get on without it, and I know that what I'm going to say is not peculiar to myself, but whereas you could see soldiers blown up, churches knocked down, the productive fields all plowed to pieces by shells, water supplies ruined, roads cut to pieces, communications broken--all the things that would be catastrophes in civil life--you could see all that without regarding them as catastrophes because they seemed--unless you got licked--a part of the expected events; whereas anything that touched a civilian close to you was very distressing. That happened frequently. I have seen soldiers go almost to pieces because a girl, or a child was hurt in the street by falling debris, or something like that. That's the way I felt about civilian damage before I went to the war, and it was no different afterwards. I don't think I more than any others got any special callous, hardened feeling about the destructiveness of war for as long as it seemed to be connected with, as I say, military effort, but when it touched on ordinary civilians it was very painful. Certainly I can't recall anything after I got back in the United States that I did, or any of my friends did, because they had been brutalized by war. Is there such in the letters?

No. In the letters while the war itself is in process, there is sudden, enormous expenditure of energy in its service, expected, anticipated, unquestioned. The letters are filled with comments on the really heroic attitude of men who have to withstand fantastic ordeals and do it without complaining--that's a tremendous lesson to learn, if you've never had that experience.

Do it without complaining--it's expected, and you expect it of yourself.

But the end result is to leave the scene the moment it's over. The energy that is put forth because of the requirements imposed upon a group of men, when that requirement is no longer imposed....

The energy is turned to getting back home.

Right and immediately.

Yes.

Well, when you get back to Baltimore, as we indicated, I think, to some extent last time, the scene had changed, and you were going back to work and I think almost for the first time with some continuity, in the Laboratory of Bacteriology in the Department of Pathology at Hopkins. Some old faces that had been instrumental in establishing this had disappeared from the scene--Milton C. Winternitz is one. Dr. Welch with whom you had had associations for a period of years had assumed the burden of the School of Hygiene and Public Health and was removed from the scene.

He was organizing at that time the School of Hygiene and Public Health.

Yes, but insofar as the Department of Pathology was concerned, the person you had to deal with directly was Dr. MacCallum with whom you had had some correspondence and I suspect some small association. Getting back to work was--I don't know, how do you feel about it? What was available to you? What did you find?

I got back to work with a great deal of ease and comfort because they had saved my job for me, and I had nothing to worry about. Actually I had been promoted in June of 1919, I think made an associate. I was made an Associate in Pathology because bacteriology was included in the Department of Pathology. It had no separate budget that I recall and no separate status. The reasons--well, there were two kinds of reasons. One was a philosophical reason, and the other one was the will of the person in charge. Dr. Welch always thought that bacteriology and pathology should be together with pathology dominant, and Dr. Winternitz at that time, and George H. Whipple before him, had the same idea. They didn't make any effort to change it, and Dr. MacCallum, I think had a stronger conviction than any of the others that the two should not be separated, so it didn't bother me any. They gave me everything I needed. I had a place to work in on the fifth floor again in the old Department of Pathology. I was able to get new apparatus. We were able to start some new things.

At that time Mansfield Clark over in the Department of Agriculture in Washington--William Mansfield Clark had written very important papers on the biological effect of hydrogen-ion concentration, and that was an eye opener. That's what they call a "parameter of reactions" that determines what's going to happen in so many of these chemical reactions, as well as reactions of living things like bacteria. So Dr. Lloyd Felton and I built a hydrogen generator which had grave consequences for the future of bacteriology at Johns Hopkins because one night it exploded and burned down the old pathology building. I think that was probably January 1920, or somewhere around that time.

<u>Quite early.</u>

I hadn't been back very long. I got back from the war in July, [1919] the end of June, and I think I got to Hopkins about a month later, somewhere in there.

As far as what you said a while ago, that my work before this time had been episodic and after my return it seemed to settle down with more constancy, it did as far as the general field went--bacteriology and immunology from then on were my main interests and concern, but it was extremely episodic work within the field, picking around one place, or another that seemed to be interesting and being guided by theory only for short distances. I never was good enough, or intelligent enough, to develop any particular theory that guided my experimental work. It was always very secondary. I can say the same thing about Dr. Zinsser. You notice his papers--he's one minute on physical chemistry and the next minute on rickettsial diseases, and then he'll read another article and start on something else. A lot of people work that way.

<u>The department--the bacteriology laboratory had functions to fulfill within the total context of the hospital and the school.</u>

Yes.

<u>So, selection--you get some interesting material from sources you didn't anticipate sometimes.</u>

Yes, bacteriology had a heavy obligation of teaching. The first class I had to teach was ninety students, and that's more than I ever had thereafter, and it came at a time when I knew far less than I did later on, but ninety students to be taught in bacteriology is an enormous load of cookery. You have to make--we had to make all the media in which we grew the organisms, make all the solutions that we used, make up stains and do hundreds of things in those days that you don't nowadays when you can buy the stuff. Then all of that was hard work. Let me go back to this fire.

What we went through is characteristic of the poverty of the Johns Hopkins and the willingness of the people on the staff to do any grade of manual labor to keep it going. After the fire was put out, we had to move across the street into a portion of the original administrative building of the medical school in which there was an auditorium and the Dean's Office. On another floor above was the Department of Physiology, and on the top floor was the Department of Biochemistry under Walter Jones. We carried--I remember how we took the residue of the unburned laboratory equipment out of the old building on Monument Street and across to the new. The elevators were out, so we had to really throw it down the stairway from one floor to the other, one landing to another, and then carry it across the street. Then we started to put together a bacteriology laboratory in the auditorium. Some old benches were brought in, and they had to be fixed up in a hurry with plumbing, little sinks with gas lines. This seems trivial, but it did affect my relations to the students. I was lying on my back in a dirty laboratory gown underneath one of those benches with a stillson wrench screwing up a gas line connection when I felt somebody kick me gently more or less in the ribs. A student well dressed was standing over me, and he said to me, if I remember, "What kind of a guy is this fellow Jones? What do you have to do to get in this department good specimens and good media, good stains?"

In other words, he took me for a janitor, or as we called them at Hopkins a "diener", and he was ready to give me a tip so that he could get a pick of culture media and things. That indicated to me that the academic position carried no recognizable aura.

<u>Did you identify yourself?</u>

I didn't identify myself until the next day when I appeared to give the introductory lecture.

<u>He must have just died.</u>

I remember kidding him about it, but it was very difficult to try to do any work in the midst of a disorganized place like that, but curiously enough, it didn't stop. We had our laboratory on the third floor of a building called the Hunterian Building. It was a relatively new brick building down on ~~Washington~~ N. Wolfe Street about a block and across the street from where we had been burned out. On the third floor, I think it was, above the Hunterian Laboratory for Embryology where some very noted people, Warren Lewis and others, were working, we set up in the room up there, and I had some very interesting associates at that time. Felton was there, and then Dr. Thomas Rivers joined us, who became famous as the greatest authority on viruses in the United States later on. Howard Cross was an assistant, and we had a media kitchen and about four rooms for our laboratory up there.

Rivers was the most noted one of the lot. He was a graduate of Johns Hopkins, was in a class originally ahead of me, but graduated in the class after I did because he had developed what they thought was progressive muscular atrophy, and he went down to Panama taking a year off when people thought he wouldn't survive. He lost most of the muscles in his hands and in between the bones of his forearm, but he was alert, mentally very able, and he had had an opportunity during the war to be on a commission known as the Pneumonia Commission that went through all the recruitment centers and camps studying influenza. He collected several hundred strains of influenza bacilli, what was called Pfeiffer's bacillus at that time and what was thought to be the cause of influenza. This was years before the virus of influenza was discovered, and he also had good histories of the conditions of patients from which the strains had come, and we were able--mostly Tom Rivers--to develop much new information about the influenza bacillus in a good many studies.

I learned a great deal from him about bacteriology. He knew much more than I did. I also learned something about personal dealings between men. Rivers was a very opinionated, high strung man of extraordinary independence who would just not do anything, if anybody told him to do it. I don't mean he would combat it actively. He just couldn't take a suggestion. He got into a state with his work where he was not making any progress. This was along about February, somewhere in 1920. I discussed this problem with him because I was head of the so-called department, or section, and I outlined some experiments I thought he might do to help to solve his questions, but he didn't pay any attention to that. Finally he disappeared out of the laboratory. I knew where he was. He was in the Green Spring Valley, but I didn't communicate with him. I respected his privacy, but he came back of his own accord in March, and he came up to me and said,"I was sitting by the fire last night and I had some of the best damn ideas I ever had in my life", and he told me almost verbatim what I had told him the month before. He's the kind of man--and I've met a good many and I have learned how to deal with them as a Dean and in other positions--who will not act on anything until it comes into his consciousness as something of his own. As long as it was somebody else's suggestion, he wouldn't pay any attention to it, and after this--it did solve some of his problems, and he went on.

It was also at that time that I had a further influence on River's life. I was chairman of the program committee of the Society of American Bacteriologists --this is getting on, I think, to 1922--and we decided that it was time to have a symposium on viruses, filterable viruses. Up to that time these filterable viruses had been known. They had actually been known since Beijerinck's work in 1898, on the tobacco masaic, and then foot and mouth disease had been found by Löffler to be due to a virus, but little was known. At the Philadelphia meeting of the Society of American Bacteriologists the program committee arranged for a big symposium and the main speaker was Thomas M. Rivers. Tom wrote me and told me afterwards that all the rest of his life his interest was in viruses. That symposium was the turning point. I suppose giving him the honor and job of being the main speaker on the subject made him go rather deeply into it.

<u>His correspondence is very enlightening on that talk.</u>

Is that so?

<u>He has some things that he wants to say about the field of a general nature. He didn't want to get trapped in specifics. He wanted to fence in a field, or fence in an area.</u>

He did--yes, it was wonderful. I admired him so much for doing a thing like that. I never could have done it myself.

<u>How painstaking and fussy was he at the bench?</u>

He was very accurate--always, the rest of his life. I would call it painstaking, but not fussy. In bacteriology one of the great problems was to avoid contamination of your material with organisms. You can see how important that is now when they're worrying about putting bacteria on the moon, or Mars. The same thing was just as serious for us in those days. If you get a contaminated culture, you don't know which of the things in it are doing what you think one of them might be doing. Handling viruses is even more difficult than bacteria because you can't see anything. Until recently with tissue cultures you don't know, except by animal passage as a rule, what you're dealing with. Now with tissue culture you can tell whether you have a contamination. Even with all the care that Tom Rivers and other virologists exercised, they found out later that without their knowing it they were dealing with mixtures of viruses.

<u>That's one of the studies that you had with D. Wright Wilson on the Bence-Jones proteins.</u> /"Specific immunological reactions of Bence-Jones Proteins" 18 <u>Proceedings,</u> Society for Experimental Biology and Medicine 220-222 (1921)/.

That's not a virus. That's a chemical.

<u>But the question was dealing with mixtures.</u>

Oh yes. Well, that--I want to refresh my memory about something in there.

<u>Do you want me to turn this machine off?</u>

You might because I want to look in one of these books.

<u>O.K.</u>

Well, the Bence-Jones proteins are peculiar proteins that are in urine and in blood. They are soluble in warm conditions and precipitate out in cold. That's almost contrary to what other proteins do which flocculate when they're heated and stay in solution when they're down to room temperature, or body temperature. That characteristic later, after my time, is found to be extremely important in certain diseases of people. Well, I was always interested, even from the work with Dr. Zinsser, in the fractions of antigenic substances, and I had in Baltimore access to several specimens of Bence-Jones protein. Some had been isolated from the urine of a peculiar disease called multiple myeloma and some had been found in other conditions. I think Dr. Edwin O. Jordan had a crystaline Bence-Jones protein, so I collected these and used all the methods I could think about to see whether they were the same thing, or whether there were differences among them. I did this by immunological means because the immunological tests were far more sensitive than any chemical tests at that time. I had D. Wright Wilson, who was a biochemist with Walter Jones, and he helped me on the chemical side of it. We did the immunological work in my laboratory in the Hunterian.

Most of that work was specific precipitin reactions with absorbed serum and very interesting immunological reactions with the uterine horn of a guinea pig that had been made anaphlyactic hypersensitive several weeks ahead with a small amount of protein. When you suspend one of the smooth muscle portions of the uterus of a sensitized guinea pig in salt solution and put in a very little amount of the protein, it causes a violent contraction. You saw those graphs in there.

<u>Yes.</u>

You can do that, and if you have the guinea pig sensitized to two Bence-Jones proteins, you can get a reaction from the second one after the reaction of the first one has been exhausted, and so on, so we published these papers on Bence-Jones protein much to the confusion of the world, so to speak, because it has allowed me to separate my friend and emphasize my hyphen. When Bayne-Jones writes a scientific paper on Bence-Jones, then it gets all mixed up. I've known since then when people call me Bence-Jones that they have a scientific, probably immunological, or biochemical background, but there was a great artist and painter named Burne-Jones and when people call me Burne-Jones I immediately try to fish out from my past inadequate education the cultural aspects of the humanities of Burne-Jones.

<u>The impression I get from these studies is that it was an effort to fix more carefully the nature of that with which you are dealing.</u>

The general philosophical point of view is very common to bacteriology because almost the whole advancement of the science depends on discrimination between microorganisms that look alike--I mean the fermentation reactions, the biochemical effects of them. I carried this a little further with some papers about the same time on the composition of the nucleic acid of the colon bacillus, ["On the Presence of Nucleic Acid in Bacteria" 33 Johns Hopkins Hospital Bulletin 151 (April,1922)]. At that time it was believed that the pentose component and one of the nucleotides of plant nucleic acid were different from those components in animal nucleic acid, and I wanted to see whether the colon bacillus was animal or plant. Most bacteria had been called plants. It turned out that they are a little of both, but that was the time when the marvelous developments of nucleic acid chemistry had not really begun. The great experts on Nucleic acid in this country, Walter Jones and Phoebus A. T. Levene at the Rockefeller Institute, had a very definite schematic representation of the molecule of nucleic acid, and it got so fixed in dogma that it really stopped the advance in the subject until Avery and other people began to find out about DNA.

One feature of the papers on Bence-Jones protein was some communication from Ludvig Hektoen who was at work on the same problem.

Well, Dr. Ludvig Hektoen was almost comparable to Dr. Welch in pathology. He was the head of pathology out at the Univerisity of Chicago and the head of the Journal of Infectious Diseases, I think. He helped in the Archives of Pathology and had an institute, The McCormick Institute. Dr. Hektoen was a very wise and able man that I knew until his death. Do you want to go on with Dr. Hektoen?

The only thing that I have for him in this period is the Society of Bacteriologists.

Yes, he was president, but I'm thinking about personal relations with Dr. Hektoen which rather clouded the relationship in the latter years of his life with me--say, from 1932 on. In 1932, I left the University of Rochester School of Medicine to go to Yale via the National Research Council, and at that time the Associate Professor of Bacteriology when I was there, was a man named Konrad Birkhaug. You've probably run across him in these papers. Konrad Birkhaug was a Norwegian and so was Dr. Hektoen which may have had something to do with the event, but anyhow, when I left, Dr. Birkhaug thought that he would succeed me, but there was much opposition to him for various reasons in the faculty. He didn't get the appointment, and he rather held me responsible for the failure of his appointment to such a degree that it broke up our relationship. I have never had any connection with him since then except one or two letters.

Dr. Hektoen at that time was concerned in the Union of University Professors, a group of professors, an organization that is very jealous of tenure. Birkhaug was an Associate Professor, and there wasn't any job ahead of him when he wasn't appointed at Rochester, so Dr. Hektoen took a hand trying to bring pressure on the faaculty at Rochester and me too, that Birkhaug be appointed. That was upsetting, but then Dr. Hektoen and I knew each other at the National Research Council and at scientific meetings, and to my astonishment about 1941, he invited me to give the Hektoen lecture in Chicago in early 1942. Well, the war was getting hot about that time. I was slow in getting my manuscript done, and I was about to go off to the war, disturbing factors which bothered Dr. Hektoen, so that we were never quite the same again, which I regretted very much.

<u>I want to come back to that whole problem after we get to Rochester because it's an interesting one in terms of management--it really is.</u>

What?

<u>The problem of the associate professor, but in this period--well, here's a letter from Leonard Colebrook, England in reply to a request that you made of him on the bacteriology of actinomyces.</u>

I published a paper on that, ["Club Formation of Actinomyces Hominis in Glucose Broth with a Note on B. actinomycetum-Comitans" 10 <u>Journal of Bacteriology</u> 569-576 (December,1925)].

<u>Yes, and also a letter to Colonel Siler on the same problem. I thought that they might have had something to do with class work, an example.</u>

No actinomyces in the lesion of the jaw of horses has the most beautiful fan-like clubs, a little sprig of the growth ends in a yellowish bulbous, microscopic cluster of clubs--they call it, and I just got interested to see whether the club would grow, or the stem would grow. I fixed up some little culture dishes, little culture cells, and I watched them for days--very slow growth, but the clubs didn't grow at all. As I remember, the little filament at the stem grew some. I think I have a picture of it here, but I didn't produce any disease with this specimen of actinomyces, but we always got them to show the class because they are very beautiful things.

<u>There was an unanticipated event in the cat colony which led to an interesting paper,</u> ["Respiratory Infection and Septicemia of Cats due to the Hemolytic

Streptococcus" 31 Journal of Infectious Diseases 3-8 (December,1922)7.

They developed hemolytic septicemia probably transmitted through the respiratory tract, but I think that was an episodic description of something that happened. Did we go deeply into that?

To study twenty-five--they died within four days. There was another colony of fifty that were also studied at the time on which you and Tom Rivers wrote a paper, taking swabs from their throats,/"Influenza-like Bacilli Isolated from Cats" 37 Journal of Experimental Medicine 131-138 (February,1923)7.

We ought to get the credit for taking the swabs from the throats of the cats--it was pretty tough going. You can get chewed up.

Yes, but this was something that happened which lent itself to a study. I would have thought in terms of the publication that the Society of the Prevention of Cruelty to Animals might have been on your back.

We didn't hurt the cats.

No, but the fact that you had twenty-five of them in a warm room--you know.

Well, the epidemic got started, or the epizootic, as they call them among animals. They are enormous. Great flocks of chickens die all of a sudden of leukemia and fowl pox. In the early days when they had Texas Fever in the cattle, thousands of cattle died, and in foot and mouth disease, the only way they could control that disease in this country was to kill all the cattle and bury them. That's still the chief way of handling that. The antivivisection people are mostly concerned with dogs and surgery, experiments that require some sort of an operation on the animal, or dismal, solitary confinements, or unsanitary cages, neglect, but I don't recall that the antivivisectionists ever made any objection to the study of naturally occurring infections in great groups of animals.

This must have been a blow since animal resources were hard to come by.

Oh yes. We didn't have much money for that.

Is there any more comment about Wilson and the need for biochemistry--that is, in your thinking about bacteria, the need for a broader team almost--I won't say "team", but another eye and competence?

Oh yes. Bacteriology and biochemistry go hand in hand just the way biochemistry and physiology go along with the study of any living organisms. That's a basic thing. Bacteriology in a sense is a living biochemistry in which you can manipulate the living, so to speak. You can dish up life with a platinum loop. It thrills you very much to think of that and to wonder what it is you're dealing with, so I was always interested in trying to link biochemistry, provide for biochemistry in the place where I was working. I did at Rochester, had people--I think the first Ph D degree given in my department at Rochester was a biochemist. I had close relations with Dr. John R. Murlin in nutrition and Dr. Walter R. Bloor in biochemistry particularly about fats. It seems very natural that the two, bacteriology and biochemistry, go together like building a house.

Here's a letter from D. J. Davies from the University of Illinois with a lot of samples that he sent you. Was this for course work?

Let me see--1921. These are all very interesting fungi--Sporothrix, one of them named after Schenck and Hektoen, Blastomyces, and Streptothrix. These are yeast-like and fungus-like organisms. I apparently didn't have any, and I put things like these in the collection. Some of them I would study, and sometimes publish something about, but usually not.

I notice in the correspondence, and this is general, that there is enormous sharing of strains you may have for purposes of class work.

Yes, everybody did it, and it grew--well, each person collected a lot of strains, and you had a culture collection in your own laboratory. Of course they had enormous duplication all over the country. Out of that grew what is called the National Type Culture Collection which is now a collection of thousands of strains of viruses, fungi, bacteria, and yeast. It's housed here in Washington. After a long, desperate struggle to get funds for keeping it up, it's now richly supported by the National Institutes of Health, and it's called the National Culture Collection. There was one in England. There's one in the Pasteur Institute in Paris. Collecting in these days was a characteristic thing. You kept your own rather extensive culture collection because you never knew when you'd want something, and you didn't have time to wait for the mail. These things that Davies sent me I would study, and I had a notebook on my collection. I knew what to expect of them because variability--and this is something I'd like to say something about when we talk about Dr. Zinsser--in all these organisms is one of the features of their lives, and you have to know them.

This preparation for class work--later on at Rochester we'll get to it--preparation for classwork is the collection of samples?

Oh yes. Collecting specimens even took me into the end of Cuba from which Castro came later.

Yes, that's a special trip you take that I want to go into by itself. Somewhere along in I guess it's June of 1922, Dr. Whipple comes into your life, and there's an exchange of views about a new department. Do you remember your talks with him?

Whipple became a great Dean--what is the date of the founding of the Rochester School of Medicine by Mr. George Eastman and the Rockefeller Foundation. It's about this time--1922.

Yes, they had time to plan ahead before the school would be ready.

Yes, long before. There wasn't anything out there for a school of medicine, except a field that was on quick sand near a cemetery out on the bank of the Genesee River. The story of the founding and the negotiations between Mr. George Eastman and Mr. Abraham Flexner have been well written up, particularly in books about Dr. Whipple. The University of Rochester had a very intelligent and able president at that time, Rush Rhees, and at that time Dr. Whipple, just before he went to the University of Rochester, was the head of the Hooper Foundation of the University of California Medical School in San Francisco. Dr. Rhees went out there and saw Whipple who didn't want to leave his job out there right away, if ever, and Dr. Rhees persuaded him to take on the deanship of the University of Rochester School of Medicine for an indefinite term. Dr. Whipple went to Rochester about 1922, to plan for this school and to get a faculty. He's still there in the laboratory. He's now about 85 years old, and he's been there a long time--forty some years. Dr. Rhees was very broadminded and so was Dr. Whipple, and Dr. Whipple started about the time that he wrote to me--does this have anything to do with the professorship?

Yes.

Well, he started to write to me about the professorship. I think it came about, or what helped to bring it to a head about this time was that Dr. Whipple happened to come into the laboratory one day when I was working on the bactericidal effect of ultra-violet light.

That's another man--Van der Lingen.

Yes, J. S. Van der Lingen.

South Africa. I forgot to mention him.

Yes--well, Van der Lingen was there visiting, and he and I worked together, but I think I have more papers than that one. Is there just one with Van der Lingen?

This is the one in 1922,/"The Bactericidal Action of Ultra-violet Light" 34 The Johns Hopkins Hospital Bulletin 11-18 (January,1925)7.

Well, I had--Van der Lingen was rather a physicist, I think, and he helped me to get spark gaps, cadmium spark gaps, and other things that had a different spectrum in the ultra-violet, and I could, imitating other people who had done it before, spread bacteria on a culture plate, hold them up in front of the spark gap, and then incubate the plate. The organisms would grew except where they had been killed, so that you'd get the spectrum of the arc in the growth of the bacteria. Well, I was working on that when Dr. Whipple came into the laboratory in Baltimore, and he was deeply interested for reasons I didn't understand very well at that time, but I think it had a rather influential effect on his opinion of me because he had by then become very close to Mr. Eastman, George Eastman, a most remarkable man who was interested, of course, in spectrographs and the spectra of various sources of light, studying his films. He had a fine physical department, the Eastman Kodak Company, and Dr. Whipple said, "Certainly Mr. Eastman would be interested in this" and told him about it. I think that helped me to get an appointment.

The appointment dragged on for quite a while. I think that it--I forget when he offered it to me.

There may have been an earlier contact than these papers, but here's the letter in June.

Well, this is soon after his being in Baltimore.

But then decision was delayed until October.

Yes.

They were going to give you a department--that is, when they constructed the school brand new, it was going to have not only the medical school, medical hospital, but the city health board too.

Dr. Whipple apparently wrote me June 22, giving a budget of the department, saying that it would be a department, and telling me about the professorship and other conditions, but I didn't accept this until later in the year--I forget just when.

October.

Yes--October of 1922. As I recall it, I asked Dr. Whipple to give me more time. He wanted the question of appointment settled sooner than October, and there may be a letter in here from him agreeing to giving me more time, but saying that he doesn't do it with any pleasure. Is that in here?

Yes--not to you, but to Dean Williams.

I remember that expression somewhere in here. Well, now the reason for that delay has nothing to do with Rochester. It had to do with Dr. MacCallum. Dr. MacCallum practically told me that if I didn't accept this Rochester appointment, he would get the trustees and the faculty at Hopkins to make a separate department of bacteriology and make me the professor and head of it. I have never known what happened, but the upshot after his consultation with Dr. Simon Flexner and I don't know who else--well, it just sort of died out. I remember I had a final talk with him in which he said that he had not succeeded in arranging this. I think if I had been better, or a more able person, that they would undoubtedly have made a department at that time and made me the head of it.

I think it was the timing. This came in June, and there was to be one more meeting of the trustees in June. That didn't give them sufficient time to canvas the possibility of developing a Department of Bacteriology.

At Hopkins?

Right. They wanted to meet this offer because they didn't want to lose you.

That might have been so, but my impression was from Dr. MacCallum--I don't know that I have any evidence of it--that if I had really been good enough, they would have taken some extraordinary action to hold me there.

<u>Nonetheless this is really a fantastic offer from the University of Rochester.</u>

I finally accepted the University of Rochester at that time and started to work on the plans. Dr. Whipple sent us the plans, the blueprints and sketches that had been made up to that point, and all of us--the young faculty that was being appointed--each man had a chance to draw his own floor plans within reason, and I worked on that the latter part of 1922, and 1923, until I went abroad. They thought that it would be good for me to go abroad in 1923, for several months and come back and go to Rochester early in 1924. I was quite pleased with their ideas to include in the department provision for doing the work of the Rochester City Bureau of Health--that is, the diagnostic bacteriology from specimens from sick people, or the whole of the immunology, the Wasserman tests, the pneumococcus type tests, everything that required immunological work and also the bacteriology of water and milk which was a very large job.

This was a conception of a very great man in public health whose friendship I enjoyed all the time I was there and that is George W. Goler, a remarkable man, and one of the early public health figures in this country. He believed earnestly that the welfare of Rochester and the good of the citizens would be advanced by having the Strong Memorial Hospital, the University Hospital to be, connected very organically with the Municipal Hospital, and they were built side by side, although they had separate administrative management to some extent. The work of the Rochester Health Bureau Laboratories had been down in some old buildings in the center of Rochester by Professor Charles W. Dodge and somewhat under the guidance of Dr. Joseph Roby, but we planned to take them into my department, the Department of Bacteriology at the school, and in doing so we drew plans for it, practically provided a common media kitchen for the city work as well as for our school work and research. We had enough certainly to start two wings of this huge building, and the wings were about thirty-five feet wide and a hundred feet long, as I remember. I had plenty of space.

Dr. Goler managed to do something with his city financial people which was quite unusual. He gave me authority to hire and fire, so to speak. I could engage anybody and put them on the payroll without having the approval of the city politicians in any sense. The salaries didn't come under city review, except that they were reported to Dr. Goler and the supplies they used. Of course they used thousands and thousands of dollars worth of supplies of glass-ware, culture media and whatnot, but these were all purchased by the medical school. A certain proportion of the total, based on the work without actual count of test tubes and petri dishes, was paid for by reimbursement to the school from the city. That arrangement continued all the time. There never was any trouble at all over the finances, or the administration. They let me alone in that respect, although I saw plenty of Dr. Goler and Dr. Edward W. Mulligan, a remarkable man, Mr. Eastman's physician, Dr. Joseph Roby, a number of men that are characteristic of Rochester. It was what you'd call a civic-minded place. They really did things in a highly idealistic, but very practical manner for the good of the community which impressed me very much.

I liked to work with them all, but I'm sure it had an influence on my future that rather took me out of line for advancement in research. I think that is my own fault, or a characteristic of my disposition. It goes back maybe to that childhood time when I told you I was being inadvertently trained to be a Dean by my "Tante E". You can't do this work in a population of a good many tens of thousands of people with all sorts of conditions requiring laboratory examination without having to spend a lot of time on it and without being willing to put aside your own work to talk with people who call on the telephone about things of interest to them; namely, the doctors who send in the specimen and the people sometimes from whom the specimen originated, to talk incessantly with the members of your staff who have thousands of problems that you can help them solve, to watch the details of administration, to be responsible for proper supply, equipment, and materials, and to have a general public relations activity; whereas if you want to do any really good solid research, you better be the kind of person who says that he has no concern with those affairs and sticks to his knitting of research. Well, to me, I may have felt unconsciously that my ability in investigation wasn't particularly good, and that the satisfactions of doing the work that I have outlined were so great that I probably succumbed to the lure.

We've gone over an hour. Perhaps we'd better stop today.

We've gone an hour and twenty-five minutes.

Perhaps we'd better stop today. Goler is important, but there's a period from 1922 to 1924, which involves a trip abroad, the examination of other laboratories, meeting doctors and other people in Paris and Belgium which is important.

Yes, I went to the Pasteur Institute in Paris and saw the laboratory in Brussels.

Let's do that then. This biophysicist that you worked with. That was wholly new.

Van der Lingen?

Yes. That was wholly new. Initially the nature--I can see some traces back to the work with Zinsser, that kind of continuum, but some of the things were accidental, episodic. There are more special cases that come up to pathology, but that's the nature of the job. You're there, and they excite interest at Hopkins. They require study and the publication of a paper. They happen, but they might be off a line of development. Then the accident with the cats. Who could have foretold that one? Then Van der Lingen, South African background, a biophysicist.

What year was that?

This was 1922. He was a lecturer in biophysics at Johns Hopkins in the winter of 1921-1922--"The Bacteriacidal Action of Ultra-violet Light."

I used to correspond with Van der Lingen after he went back to South Africa. Itwas called a Department of Pathology and Bacteriology at that time. I had forgotten that they had given it that name. It says:

> The work was undertaken under the direction of Dr. Van der Lingen, who was Lecturer of Biophysics in the Johns Hopkins Medical School during the winter of 1921-1922. After his return to the University of Capetown, South Africa to take up his duties as Senior Lecturer in Applied Mathematics, I obtained the data on the absorption spectrum of a bacterial emulsion, the action of the inner ultra-violet light (390-300Kw) the temperature coefficient of the bactericidal action of light, and the effect of hydrogen-ion concentration."

I see I did this after he'd gone.

You went on with the work.

As I remember it, he was interested in this, talked to me about it, and he was there for a while, but the note says that I went on with it after he left, and those things are the important things in this paper. I think I have a picture in here of the spark spectrum. There's the killing region. This is the culture--see. There's the killing region around 200-300 mm. That's the same thing--the spectral lines come out.

I may be wrong, but as I read this paper, my understanding was that it was an effort to fix more precisely where the killing limits were.

Yes. I forgot how that began. Van der Lingen was not in my department. He was over, I think, with Dr. John J. Abel, pharmacology, but I've forgotten. There's the cat study. All right?

Then matters get episodic again. For example, there's work there with the Department of Pediatrics and Dr. Lawson Wilkins ["Indurated Ulcer of the Tongue due to Odium Lactis" 26 American Journal of Diseases of Children 77-82 (July, 1923)], work with Dr. Isaac R. Pels in the Department of Dermatology ["Elastic Tissue Simulating Mycelial Filaments in Skin Scrapings" 8 Archives of Dermatology and Syphilology 37-43 (July, 1923)].

Some of that work would come about this way. I'd be working, and these people would get interested in something, and they'd bring the stuff up to my room. Sometimes you'd go through thousands of things like that. Is this still running?

Yes.

Sometimes you don't do anything. Sometimes it makes a great difference.

In each instance, I think, there was material that could be worked on, a specific case which is mentioned.

That one with Pels is just clearing up a false observation. A curious thing --the elastic tissue fibers in the skin branch and twist like fungus growth, and to make a diagnosis of a fungus lesion, or a crusted fungus lesion on the skin, you scrape it, and it shows that you can get things that look like fungi out of it. We had to do a differential stain, I think, to make it.

Does Davison appear in there yet--Wilburt C. Davison?

Yes, this book contains his paper, "Divisions of the So Called Flexner Group of Dysentery Bacilli", [32 Journal of Experimental Medicine 651-663 (December, 1920)].

Davison brought that in. What year is that?

1920.

Well, Davison had been a Rhodes Scholar, a great, huge man, and while he was at Oxford, he learned some new ways to differentiate dysentery and typhoid organisms. This was largely an outgrowth of the war, and he brought the material to my laboratory with a note from Dr. Osler, I think. That's not in this correspondence, is it? I think it probably was in his own pocket. This leads into bacteriophage, but that's what happened to me all the time. Bacteriology is related somehow to nearly everything that sick people, doctors, and students are concerned with, and all the time people were bringing things to the laboratory, getting me to look at them. Sometimes it is easy to say what it is and what it will do. Sometimes it is not easy. It's new, and then you study a little. That's rather scattery work.

Let's call a halt today, and I'll see you tomorrow.

Are you finished with this Eclat book?

Tuesday, April 26, 1966 A-54, N. L. M.

Since you have to go at three o'clock, we'd best get at it. I wanted to go back to some observations that you may have with respect to this initial experience you had with the teaching of students. A lot of other things were at hand about which we commented somewhat yesterday, but how did you find teaching?

Well, the formal teaching of bacteriology to medical students was the beginning of teaching of bacteriology in a formal manner, but at Hopkins you're teaching all the time--even before you graduate. I had , as I told you, a substitute position as an intern almost from my second year on, and every intern has under him a number of people called clinical clerks who work right along beside him and do a certain amount of the routine work. The clinical clerk examines the patient the same as the intern does, and the intern discusses his patient with his clinical clerk. He is teaching right from the start. The method is there. You imitate your chief. As a matter of fact, some of the imitations got to be so ingrained that you couldn't tell the resident from the chief surgeon sometimes because of his manner and the way they emphasized one thing, or another.

Teaching as such was no new experience to me when I went into the bacteriology after the war. You see, after being an intern in medicine, I was an Assistant Resident in Pathology, and you're responsible for groups of the class. I was teaching in pathology at that time. The only difference was the size of the class and the administration of the program for teaching. The subject really wasn't a new subject to me. You speak of efforts to keep ahead of the youngsters--well, I never felt that particularly in any competitive sense. It's very stimulating to find some student who has picked up something and is ahead of you. My difficulty with the subject of bacteriology at that time, to tell you the truth, was that I never had any real,broad basic training. I went for a while with Dr, Zinsser and worked on a research problem, as you know, and then that was broken up. Then the war broke up the next beginning, so I always felt that, unlike a good many other men that were in the line of teaching and research, I had a lack of basic training which made it rather difficult, but perhaps a little more fun.

Teaching also involved the teaching of nurses. I taught a course in bacteriology for nurses right from the start which means--well, you don't talk down to them, but they don't have the basic information that the medical student had had from a college course. Most of those young people, young women, were from high school, or else first, or second year in some college. They were an interesting group to teach. You had to simplify a good deal, but I didn't simplify too much. You give them a fairly sound course, and then I had occasional teaching experiences with visiting groups of people, or as a visiting lecturer, or I had a seminar at some other medical school.

<u>Was the fact that you had ninety students after the war a unique thing?</u>

Yes, ninety students is much too much--too numerous. You can hardly get around to give much individual attention to a company that size. About fifty or sixty is a manageable lot, and besides with bacteriology culture media is so varied and the containers of it so numerous that you deal with things by the gross rather than in smaller lots, but teaching ninety students was a very severe sort of burden. Fortunately I only had one year of that.

<u>It's a combined lecture-laboratory course?</u>

Yes, it was a combined lecture-laboratory course. Usually I think we lectured too much. Mostly we had some lecture perhaps every morning. I've forgotten what the schedule was, but I carried that through at Rochester too. We had a great many lectures, but after a while, at least in my experience, being a teacher for a little while teaches the teacher how to get the students to give their own lectures. It's a very interesting little socratic method perhaps that you can apply. If you know what you want to bring out and face a class, you can ask questions and then ask another student what he thinks about that, and with a little suggestion and a hint every now and then, you bring out the whole lecture without having delivered it. As a matter of fact, the students give the lecture, if you manage the class with tact and know when to put in the right word.

<u>Were you a--this may sound like a curve ball and I don't mean it to, but it may help you to amplify the statement that you've just made--were you a book man with the course? Did you have a book, a text on which you relied?</u>

Yes, we used Hiss and Zinsser. That was the basic text, but there was a famous old book before that by Sternberg, the great masterpiece of early bacteriology, a huge volume with a thousand or more pages, but we tried always to direct the attention of students to current literature and basic things that were not in textbooks. We tried to get them to read some of the originals. If they could read French or German, so much the better. The spirit of the teaching at Hopkins was to avoid the didactic textbook in most things, if possible, but I think I told you once before that we had one resident in medicine that taught us by having us sit down and open Osler's <u>Principles and Practice of Medicine</u>, and he would read it and say, "Now, underline this sentence."

That didn't arouse much enthusiasm.

You have an aversion to that approach even at this late date, so I would assume in talking about teaching, you would have avoided that sort of thing as one does the plague.

Yes. In some places I think the teachers are so respectful of the textbook and so timid that they don't dare go much beyond the covers of that book, but at the Hopkins right from the start a spirit of independent inquiry is encouraged in the entering students as well as in the teachers themselves.

Did you get much challenge from the students?

It's hard for me to recall, but you always feel a challenge. You want to see them light up with excitement of the knowledge that study and discussion brought to their view, and very often you find highly thoughtful, original minded young men that are able to sustain substantial discussion, but they vary all the way from people who don't do anything at all to people who are very bright and run ahead.

I think this is also a period--I mentioned this to you before we turned the machine on--when you got married.

Yes, I got married June 25, 1921. Curiously enough, I always get the 21 and the 25 mixed up, so that I have to go back and think about it.

The lady I was fortunate enough to marry was from an old Baltimore family. Among her ancestors were Robert Smith who was the Second Secretary of the Navy and General Samuel Smith who won the battle of Baltimore. She was living at that time with her great uncle, Mr. John Donnell Smith, who had become a world renowned--where did you pick up his name?

He was a botanist.

Do you know of him? He was a botanist. I'll come back to that.

She was in Baltimore all the time I was in medical school, but I never called on her. She was living in the house with Mr. John Donnell Smith, a great big house at 505 Park Avenue. She was living there. I never called at that house until after World War I, because I had heard so many fine things about Mr. John Donnell Smith as I grew up that I thought he must be dead. I never heard anybody praised by the members of my family in such a manner unless he had passed on.

The connection in here is curious. Mr. Johns Donnell Smith was in the class of 1847, at Yale with my grandfather, Thomas Levingston Bayne. They were classmates. They went to the Civil War at the same time. Mr. Bayne was in the Louisiana Washington Artillery and got wounded at Shiloh, and Mr. John Donnell Smith, although coming from a town that was divided between the South and the North--Baltimore was a double allegiance town--went into the Army of Northern Virginia and served under Jackson and General Lee as an Artillery Captain. Richmond fell in 1865, and my grandfather was there connected with the Ordnance Department under General Josiah Gorgas--that's my grandfather Bayne--and his wife was about to have a baby. When Richmond fell, S. W. Smith, the father of John Donnell Smith, brought her to his house in Baltimore, and she had her baby in Baltimore. The family connection was very close, and it was just kind of stupidity on my part, or some impression I had made, that I didn't call there.

Nannie Moore Smith, as the future Mrs. Bayne-Jones was named, had been trained as an expert x-ray technician and had worked with Dr. Lewellys F. Barker.

She was drawn into service in the war, went abroad, not with the Hopkins Unit, but with another hospital unit which, curiously enough, was at Bazoilles, so I passed very close there in 1918, but didn't know that she was in that place. This went on. It was only in 1921, that I happened to meet her on a little sail boat in the Baltimore harbor. No, that must have been 1920, or 1919--I forget which. In any event, we were married in 1921, and soon set up house keeping in a little divided house on Park Avenue with very little to live on at that time.

Did you meet and get to know the uncle?

Yes, I got to know Mr. Smith. I'll tell you how he got to be a botanist. It's very remarkable that he did so much. After the Civil War he did not know quite what to do with himself. He felt unhappy in Baltimore because Maryland had not seceded. Finally he took himself off to Guatemala, Nicaragua and Honduras, and he made a fundamental, basic, systematic collection of the flora of these countries. He had his house when I saw it full of cases of his herbarium. Later on I saw lots of the folders of his herbarium at Kew Gardens in London. He described all these plants in Latin and published several small books on them and very accurate observations. They told me at Kew Gardens that his herbarium folders never contained mixtures of plants. They were all parts of the same thing, and that's very unusual because the immature stages of them are hard to recognize, so the basic descriptions of the flora of guatemala, Nicaragua, and Honduras are those of John Donnell Smith in the 1870s, early 1880s. I think he named two hundred and fifty species, but I'm not sure. It's a terrific lot of things. I never could talk to him much about botany because I was interested in function and what the plants did, how they developed and propagated themselves. He was altogether interested in the vination of leaves and the way the leaf came off the stem and all the morphological details. He was not interested in function at all.

That's interesting.

Yes, I went through the Victorian standards and requirements of his domain. When we got around to it, I remember going to see Captain John Donnell Smith in his study, and I asked him if I could have the honor of the hand of his grand-neice in marriage--a very solomn occasion. He thought it over for a while and said all right.

He appears occasionally in the correspondence.

Yes, he used to come up from Park Avenue--they lived on Park Avenue and Hamilton Street, I think--anyway one of the cross streets, and we lived at eight hundred and some number further up /821½ Park Avenue/. He'd walk up to our little apartment and sometimes have supper with us, sit and talk. Mrs. Bayne-Jones was very, very fond of him.

We talked about the offer from the University of Rochester last time and the effort made to see whether Johns Hopkins would break through and create a separate department for bacteriology as had already been done at P & S and, I think, at California. In any event, they didn't do so, so you had this Rochester possibility before you of really growing up with a brand new department, to shape and mold it the way you wanted to without any vested interests standing in your way, but before going there, I think from July, 1923--though, I think, you accepted the offer in October of 1922--there was no plant in being, as I understand it, at Rochester at the time, and they were very much interested in gathering a faculty that would sit in judgment on its growth.

Well, plan for its growth, not a judgment on its growth.

In short, they were looking ahead, but from July, 1923, on you take a trip abroad. As I think I told you, there's one letter from England. Your only comment is a general comment, "I have been seeing lots of doctors, some sick people and many bacteria." The letter doesn't allow you to risk any specifics. I wonder whether any occur to you now. This is a safari in a field of which you'd grown enamored.

Well, the arrangement for going abroad at that time originated with Dr. Whipple and President Rhees at Rochester. They saw a period where there would not be any need for me as an active member of the faculty. There wasn't any school then, and there wasn't any teaching. I worked hard on my section of the plans in the early part of 1923, and they thought that it would be very good for me, as they did for other oncoming members of the faculty, to go abroad and see what I could see in that time. That, of course, was in the Hopkins tradition also because Dr. Welch went abroad nearly every year. Dr. Halsted and Dr. Osler went abroad. That was the thing to do. As a matter of fact, it was quite natural for one coming from New Orleans to go abroad, because the young men of New Orleans went by sea to Paris and London a great deal before the Civil War, and the New Orleans Medical and Surgical Journal is full of interesting observations of these visitors on the European clinics and laboratories, so much so that trips abroad continued after the war. During the Reconstruction when the iron curtain came down between the North and the South, men were still going out of New Orleans, so that some of the members of my family--my grandfather Jones went to London and Paris in 1871, and so it was natural to want to go abroad to see these people and the things that they were doing.

It is astonishing to me how I had entrée without being known. I must have had some wonderful letters that don't show up. I probably delivered them to the authorities, but I saw the chief people in the bacteriological laboratories at least in London, Paris, and Brussels--those are the chief ones--Oxford, and good people in Edinburgh, so I must have had good introductions from Dr. Welch and others because I was a totally unknown person at that time.

The expenses of the trip were paid for by Rochester which began to provide for my expenses even before my formal appointment which was around about October, wasn't it? I couldn't have afforded that trip on my own small income, but we went over on a--I think it was a second class passage we may have taken to get over on, one of the big boats, and in Paris--we went straight to Paris, and Nan and I got a place in a pension in Passy where they spoke only French which helped us to learn a little French.

I should know more French than I do, but I prevented myself from learning French, I think, because I soon found out--my Uncle George Denegre who spoke French fluently and my "Tante E", his wife, spoke French, and one day at the dinner table when the three of us were together, they started talking French. I understood every word of it, and they were talking about me, so I didn't let on that I understood. I never showed any capacity in the French language, but I learned a great deal about myself. I could read French, and I acquired a sufficient accent so that it wasn't altogether neglected, but I had no facility with it. It's curious though, that if you have a little basic material at that age or earlier, you can rather develop it in the situation.

I talked French with these people in the pension and on the Metro, and I had introductions that admitted me to the Pasteur Institute. I saw Emile Roux who was the great man early on diptheria, a great, tall, ascetic figure like a prelate of the inquisition almost, and with him was A. Besredka. I met Besredka in the laboratory. Besredka was an immunologist of considerable interest to

Dr. Zinsser and others.

The chief friend I made there was at Garsche outside of Paris where Gaston Ramon was the man in charge. Ramon then was about in his fifties, and he had charge of the making of diptheria toxin and antitoxin at Garsche. He discovered a method of titrating diptheria toxin by mixing the toxin and the antitoxic sera in a proper proportion. There was a flocculation, and you could tell the strength of the toxin against the known strength of the antitoxin, or you could reverse that and titrate the antitoxin, if you knew what the strength was of the toxin you had. This method had to go in and out between animal testing and test tube testing. Well, I learned the method, and when I came back to this country, I went up to the State Serum Laboratory in Albany, a year or so, going from Rochester to Albany, and I got Dr. A. B. Wadsworth, the chief, and his assistant, Mary Kirkbride, to set me up in a laboratory and give me unknowns from their big supply of toxins and antitoxins because they were making diptheria toxin and antitoxin for New York State at that time. This method worked out so well that I published a paper or so on the titration of toxin and antitoxin by the flocculation method, ["The Titration of Diphtheria Toxin and Antitoxin by Ramon's Flocculation Method" 9 Journal of Immunology 481-504 (November,1924); and "The Titration of Toxins and Antitoxins by the Flocculation Method" Chapter 54 The Newer Knowledge of Bacteriology and Immunology (Chicago, 1928) 759-771]. The point of it of particular value to me was that this remarkable Dr. Gaston Ramon became my close friend, and he credited me with introducing his method in the United States. There was nothing original on my part, except that I had good facilities and a lot of things to work with. Ramon was coming into international notice at that time so that ever since 1923, until he died a few years ago--- I had some papers of his. I don't know whether I left them in the file here, but he kept sending me his publications, particularly after he got a prize or two from Rome, or Paris, and he began to write a great deal about his own importance.

I found one of those--a history of his work.

He was a most interesting man, and of course that stable-like laboratory at Garches has a room on the second floor where Pasteur died, and it was a moving thing for me to go out there and see it.

How were they on the development of equipment?

They were low on the development of equipment. A laboratory in those days was like a poorly equipped kitchen compared with what you find in laboratories nowadays with all the high speed centrifuges, the refrigerators, the extraordinary microscopes, and the many things that you can have. They were using their wits and their fingers more than they used their high class laboratory glassware and so forth.

Well, I think I stayed in Paris longest of any place on this trip. I got a great deal out of it that still stays with me. Particularly the association with men of original, scientific integrity and imagination is an influence you crave, and you can never escape it if you wanted to. It is bound to affect your life.

I forgot where we went next. I think the next thing I did was to go up myself from Paris to the North. I went to Brussels first. Oh yes--Mrs. Bayne-Jones was with me at Brussels. I had a letter, two letters, a letter I think from Zinsser to Dr. Jules Bordet who was the great immunologist, about in the same rank as Erlich who founded immunology, and I met Dr. Bordet in his laboratory.

He had written some great books which I think influenced Dr. Zinsser as much as any writings of the time. Of course Erlich's books were also influential. I happened to know a brilliant scientist and an able writer named Paul de Kruif. Do you know who I mean?

Yes.

I think that de Kruif had written *Microbe Hunters* at that time. Also he had gotten in trouble at the Rockefeller Institute by saying things that Dr. Flexner didn't like. De Kruif was in Paris, and he gave me a letter to Jules Bordet's assistant, Andre Gratia, which I kept for a long time because it was the best opener of confidence that I have ever had. De Kruif just told him, "This is B-J. Give him some *civet de lapin*, and you can tell him anything."

Do you know what *civet de lapin* is?

No.

It's rabbit cooked up in a brown gravy with chives and wine. I got to know this man quite well, and he took me around. Mrs. Bayne-Jones and I took a trip out into the battle fields of Ypres before we departed from that region, and they were still very rough--shell holes and barbed wire and quite interesting.

Unbelievable.

Then I went to--well, she went somewhere, and I went on North to Berlin on a train. From Berlin, as I recall it, I went on to Warnemünde where I took a ferry to Copenhagen, Denmark. There was a very great man there named Theodore Madsen, a pupil of the great physical chemist and immunolgoist, Arrhenius, who was equal to Bordet and Erlich. Madsen was the head of the Serum Institute at

Copenhagen. I visited there, saw him and a number of his assistants, and got to learn about various fungi and serological reactions, particularly of toxin and antitoxin, complement fixation, and so forth.

That was a time that I saw some amazing financial changes. The German financial situation had gone to pieces, so that when I got off the train on the platform in Berlin, the newspaper was offered to me for seven million marks--one little sheet. You can see what the debasement of the currency was. My dinner in the dining car was a billion and something marks. I was sitting in a compartment with three Germans on the way up that night, and we got to talking a little about these things--fortunately they could speak English, and somehow or other it came out that I had a few dollars in my pocket. One of these men gave me four billion marks for a one dollar bill. That was equal to two hundred and sixty million dollars in the old style, so I learned some economics from that part of the trip. That certainly was a way to cancel all your debts by paying off in this debased paper money.

<u>I'm interested again in equipment. How was the Copenhagen laboratory?</u>

That was fine, one of the better ones. It wasn't any better than what we had in the United States, but it was well equipped so far as glassware is concerned and incubators. In these days bacteriological equipment was neither very ornate nor intricate. You could make most of it yourself.

From Copenhagen I went across over into Sweden, to Stockholm, because there I had a friend--oh shucks, I was going to say his name, but it will come back to me in a minute--who had visited me in Baltimore and also was well known as a bacteriologist. He took me around to the Karolinsky Institute which really had an effect on me because there were several men there of importance who were working on the variations of bacteria. They were getting away from the monomorphic ideas of Robert Koch which Dr. Zinsser held to very closely--so did Dr. Welch--and since seeing that these organisms had a life cycle maybe, and a great series of forms where one was as valid as the other, but Koch and the doctrinaire people said that those were either contaminants, or degenerative phases, and they threw them out. That--well, I saw enough there to be convinced and get interested in the subject, and I met more people who had interest in bacterial variation. One of those was de Kruif who described what are called rough and smooth colonies of bacteria.

At that time, if read from the textbook, the ordinary description was that a colony of a certain organisms would be round, smooth, and glistening. On the same plate, very often, you saw crinkly little colonies that were rough and wrinkled, and often you didn't see them at all, even though they were there. I don't know--you can be totally blind to what's in front of you, if you don't think it belongs, but these rough colonies were a variant of the organism. The smooth was a variant too. They did have different properties of pathogenicity and different serological effects. At that time the rough and smooth variations of bacteriology were coming on, and I'll tell you more about it when I get to London. I saw men who had been at this kind of work for years, and it opened my eyes a great deal, so much so that later on in Rochester in 1932, when I went to work with Dr. Zinsser on the revised edition of the textbook which I rewrote practically, I introduced a great deal of bacterial variation in that revision that Dr. Zinsser never countenanced before. Now, it's the common, ordinary thing, but I was impressed psychologically that actually those colony forms could be right in front of me, and I didn't even see them, and when I did see them, I discarded them for a number of years without looking any further. I had a good and interesting time in Stockholm. Can you think of the man in Stockholm?

<u>I think it's in the correspondence. Let me turn this reel over and take a look.</u>

All right? My bacteriologist friend at Hopkins, whom I had seen before, was Hildring Bergstrand who also was a man who was interested in those early days in bacterial variation. I'm leaving out all the cultural side of this trip, like Swedish antiquities and a place called Skansing where they reconstructed the old habitations of the Swedes in the early Christian era, cabins, and people dressed in the costumes of the people of the time. Sweden at that time (1923) was in the stage of modern architecture. Their City Hall was a most startling place--violent colors, ebony columns against yellow backgrounds, and distorted images on the walls. What they call "the modern phase" was coming in then.

<u>Did you have a guide for this sort of thing?</u>

No, I didn't have a guide. I had one guide for the first visit, but that was long after a long dinner party, and Bergstrand took me there at eleven o'clock at night to the Karolinsky Institute. These Swedish dinner parties are remarkable things. The one I'm thinking about now started at four o'clock in the afternoon, and this is not an exaggeration. I think we ate a meter of eel in centimeter slices, and between each slice of eel you had a drink of aquavit and you had to make a speech, or somebody made a speech, so it lasted a long time. All the time I was collecting in my head information about these things. I don't believe I have any diaries, or notebooks of the trip.

<u>I haven't found any.</u>

I don't think I kept notes. Let's go on to England. I forget how I got there, but in England Nan and I were together, and we traveled around a little. The chief trips we made were one to Edinburgh and one to Oxford. The one to Edinburgh was partly sight seeing and partly because I heard that a famous doctor named Dr. Byrom Bramwell had specimens of Bence-Jones protein. I went to call on him, and I found him in this beautiful Adams house. He gave me a vial of this protein which I brought back and worked on at some time or another. Edinburgh was a charming and interesting city, but rather grim--gray stones, high buildings, more or less crowded. There was one good walk up on to the hill called Arthur's Seat, and there's a good view from up there. I think it was getting on toward fall when we got there because it was getting kind of chilly, and the hotel was cold.

Then we came on back--I think we came back that time; I'll say we did go to Oxford first--yes, and at Oxford I ran across a man whose name I have forgotten, but who was an officer in one of the British Ambulance Companies I served with during the war. He was working in the laboratory on new methods of agglutination reactions for the identification of typhoid and paratyphoid which is a great group of variable bacteria that was being worked out chiefly by Dr. Joseph A. Arkwright. Arkwright was famous for that new serological work where you could do something with this group that had not been done very much before--that is type them. They gave them letters--type A, B, C, and so forth and V-1--there are lots of different names, but that's important because you could link the type to particular infections and outbreaks. A lot of basic information was coming out about the nature of the constitution of these organisms.

Well, I'm sorry I don't remember the name of this friend of mine. I will later probably. He had a motorcycle with a side car, and he took me on one trip to Blenheim Castle of the Marlboroughs which is a marvelous big thing. Nan and

I wandered around and saw beautiful things at Oxford, Bodlean, the theater, the stained glass windows, Christ Church, Magdelin College, Balliol, lots of things. It was satisfying having read a great deal about it. It was a romantic structure that fit in with romantic notions of early reading of those times. Then we went on to London, or got to London at one time or another, or maybe several times. How I knew where people were, or whether I went to Institutions just so, I don't know, but I went to St. Bartholomew's Hospital I remember, and there I met another gentleman who was very far advanced on bacterial variation. Have you got St. Bartholomew's down there?

Was it a Dr. T. Joekes?

It may have been Joekes--no. This was a tall--I've forgotten his name. I could have looked up some of this. They were very kind and showed me their laboratories which were like all the other laboratories, but there was a good deal of interesting talk. Then I met Leonard Colebrook who was also interested in the typhoid group together with some of the anaerobic spore bearing organisms that had caused a lot of trouble in the war--tetanus and gas gangrene type Bacillus welchi. Then over at another hospital across the river I met Dr. P. Fildes.

I didn't run across his name.

Fildes was there, and he was very far advanced in the bacteriology of tetanus. He was separating tetanus bacilli into toxin forming and non-toxin forming, and studying antitoxin, but Fildes comes back into my life again with biological warfare because later on he was head of the Portland Laboratory where the British went so far with bacteriological warfare, particularly with anthrax.

Have you run across any of that yet?

No, not yet. That's up ahead of me.

To anticipate it--I won't go into the story--as World War II was coming along in 1939, Secretary of War Henry L. Stimson got aroused by newspaper accounts that the Germans were using bacteriological warfare and that a good deal had been published on the subject, so he called an advisory committee, and even before the war he invited me down here to sit with General James S. Simmons, Merrit P. Sarles, E. F. Fred, and Captain Charles S. Stephenson who was later an Admiral, and we were the first biological warfare committee for the War Department. We collected a lot of information, but I'll come to that later because that amounts to something in World War II.

Was Dr. Fildes thinking in terms of variability at that time too?

Oh yes, they all were. It was new and exciting, and it was something that you had to pay attention to. Who else have you got on that list?

I had Arkwright, but at the Lister Institute.

Arkwright was at the Lister Institute, and I think he had an influence on the Oxford Laboratory. I went to the Lister Institute and got some cultures out of their collection to bring home too.

I think that accounts for the correspondence. Then there's Professor George Nuttal at Cambridge.

You pronounce him Nuttal. He was the great parasitologist.

Yes.

He had been on the staff of Dr. Welch's department at Hopkins in the 1880s, and Nuttal wrote great books on parasitology, the protozoa, and other things. He was a very learned man. I didn't get to know him, but parasitology always fascinated me. We used the word "beautiful" about these creatures, and they are. They're larger than bacteria. You can see much more about them and converse with them in a different manner than you can a little obscure microorganism. They have complicated life cycles, and they do all sorts of extraordinary things. I didn't see much of Dr. Nuttal. I know he was close to Osler. He was at Cambridge, I think, and I didn't get much to Cambridge, and curiously I don't know much about Cambridge. Cambridge I associate with atomic physics, all the new things of quanta, and quite a difference from the Oxford development. Oxford I associaetd with literature and biology, and Cambridge I associate with physics and mathematics.

<u>There is correspondence with Clifford G. Dobell.</u>

Clifford Dobell was at the hospital of the British Medical Research Council at Hampstead, something like that, and Dobell was a most perspicacious, precise and exquisite literary scientist as well as being a parasitologist of unequalled facility in handling these organisms and separating them. He wrote a great treatise on the amebae. He was able to separate them. He had five varieties, and his books were the authoritative books on <u>Ameba histolytica</u> which was the cause of dysentery and the other endamoebae and the varieties that exist there, but his great enthusiasm in life was the study of the writings of the man who discovered the bacteria and the protozoa and that is Anton van Leeuwenhoek--do you know him?

<u>Yes.</u>

Debell wrote a great book called <u>Leeuwenhoek and His Little Animals</u>. He had published that. Clifford Bobell with all his impatience had learned to read 16th Century Latin and, more astonishing yet, he could read the Dutch script of Leeuwenhoek who wrote tremendous letters to the Secretary of the Royal Society. That's where his original descriptions in the late 1600s and early 1700s came out. Leeuwenhoek drew the first picture of bacteria which I copied, and everybody else has copied in books they write. Leeuwenhoek developed his own lenses and his own microscope, but he didn't have a tubular microscope. He'd take a little glass bead, a spherical bead, and mount it, and then he would put his little drop of water on the point of a pin, and somehow or other focus on it, see what he could see, and through magnification--you know, you can get a magnification through a spherical thing. It was quite wonderful what he did. He discovered the bacteria. He discovered fungi. He scraped teeth and, of course, if you scrape your teeth, you can find anything. He drew muscle fibers that no one had seen and really, I think he put in the link that was missing when Harvey described the circulation of the blood because Leeuwenhoek discovered capillaries, the little, fine vessels that exist between the veins and the arteries. That discovery is attributed to Malpighius, but I think that Leeuwenhoek--and his pictures are about the same time in the 1600s--is just as important as Malpighius, so Leeuwenhoek exists on the cover of the <u>Journal of Bacteriology</u>. I'm looking to see if you have one here. There he is--he's our great saint.

<u>Debell was collecting and studying this man.</u>

Yes, Debell collected all his original things, translated them from the original Dutch, great books.

<u>He was associated with a hospital under this Medical Research Council.</u>

Yes, but Dobell wasn't working on patients at the hospital; he was working on the ameba.

<u>This National Institute for Medical Research that I suspect....</u>

The British National Institute for Medical Research? I don't know whether it's called "National"--British Medical Research Institute. I don't know, but that was a great center of their national effort in medicine. I went there particularly. It was concerned with my own field, bacteriology, mycology and immunology. I went out to Mill Hill and saw people working on various things, and they were all very nice. I came back to it again in World War II in connection with typhus.

<u>The British Empire--India, Singapore, Australia--there was an Institute of Hygiene and Tropical Medicine in London, I think.</u>

There had been an Institute of Hygiene and Tropical Medicine in London for years and years. It had been a great center of interest in that subject.

<u>Right.</u>

And there are divisions of teaching in the London Medical School devoted to this--a School of Tropical Medicine.

<u>I know of your own interest in tropical medicine, but it's also a spur for the development of the Academy of Tropical Medicine in 1934, after this trip. I thought it might have helped, though I don't know.</u>

Oh well, when that came up, it was a natural interest and advance there.

<u>Sure.</u>

I was a founding member of the American Academy of Tropical Medicine that started right here in Washington, started through the efforts of the Dean of George Washington School of Medicine, Earl B. McKinley, but you don't want that now.

<u>No. As you think about this whole trip--this lunch counter, where you had a chance to sample the experience of other men--it must have stretched you.</u>

Oh yes, it was enormously interesting and always a part of the whole piece. I didn't feel any--well, if it were a lunch counter, it was kind of a Lazy Susan lunch counter, that just went around. There wasn't any beginning, or end.

<u>I was thinking of later developments at the University of Rochester for the Department of Bacteriology.</u>

Oh yes, it had an enormous effect because it opened your mind, and you had a vision of what people were doing.

<u>I want to talk to you about the University of Rochester, but I think we'd better stop now.</u>

Yes, don't get into it because that's a long story, and I've got to go.

Thursday, April 28, 1966 B-9, N. L. M.

Before I turned this on, I gave you something of my interest today. I guess we can break it down into two topics. One is the town of Rochester itself, 1923-1924—what was in the air? How would you characterize this town? This has a lot to do with what happened. The other topic is this group of men pictured here that were brought together even while the school of medicine was being developed and who participated, in terms of whatever it is they brought with them in experience and insight and ability, in its development, so that there's really the town and this new mutation, a new medical school.

Rochester is one of the older towns in Northern New York. Of course, having been interested in Indian remains from my Grandfather Jones' time, I found when I got to Rochester that it was the home land of the Seneca Indians. It was full of Indian relics and Indian lore. Rochester began in the 17th Century when the French moved in from Canada and began to settle. Then along about—maybe the middle of the 18th Century, or toward the closing of that century it began to get into the hands of American people—English people first who began to build up a town as so many of those places were originally started on the sites, or on the locations of waterfalls. They used right away the power from three waterfalls that are in the city. The Genesee River passes through Rochester, and they built mills. They raised grain in that region, and they began to grind wheat and other things. It began as rather a center of ingenious economical exploitation of the resources of the country both in power and in products.

Then perhaps in the early 19th Century, 1812, or thereabouts, or even later, Germans began to move in there. They had a tremendous effect on the development of the industries in Rochester and on the cultural outlook of the people. I'm sure they did. Rochester became not only a great milling center, it also became a great transportation center when the Erie Canal went through there and the railroads began to go through, but there also developed from the early times, through these Germans particularly specialized industries. Bosch and Lomb is an example. Rochester became a great center for the manufacture of lenses and microscopes, eye glasses, all sorts of special things, and when I say in this letter that you have here, that I found Rochester right away a fine place in which to live, the fact that it was a cluster of specialized industries with extremely interesting people of a scholarly bent at the heads of these industries made it very pleasant and easy to get along with scientific work in the midst of a community that appreciated not only the immediate scientific applications, but also the humanitarian and cultural side. Rochester developed, and still had when I was there, a big clothing industry, lots of specialized machinery, relatively small machinery, but there was a wonderful plant for making sterilizers, the Castle Company.

Then in the early 1880s, Mr. Eastman moved into that region. About the same year I was born, I think he either invented the Kodak, or began to build the Kodak plant, and that became the main industry of Rochester under Mr. Eastman who was there certainly from the early 1880s until his death in 1932. You can't understand the development of Rochester unless you understand the effect Mr. Eastman had on it, not only by his enormous philanthropical gifts, but his interests in the cultural life of the city. He gave large sums of money to institutions in Rochester and to the Massachusetts Institute of Technology where for years he was known as "Mr. Smith" before his name was disclosed as the giver of huge sums to M.I.T. It is said that he gave away altogether about a hundred million dollars, and in doing so, he built the Eastman School of Music, as it was called, to which he brought as a director, Howard Hanson, an original composer of modern music, and Mr. Eastman had visiting conductors there--Eugene Goosens, I think, was one name, and there were a number of others. Mr. Eastman fostered a Technological Institute in Rochester and built there also one of his early dental clinics. He somehow or other got interested in dentistry through a very remarkable practicing dentist and a political dentist of great power named Harvey J. Burkhart. I think one of Mr. Eastman's first dental clinics was the Dental Clinic of Rochester. The fact that the Dental Clinic of Rochester was there before the medical school was thought of had a great deal of influence on the medical school which was called from the start the Universit of Rochester School of Medicine and Dentistry. I'll come back to that side of it later because a very double jointed name like that was the basis of a split in ideas between Dr. Whipple and Dr. Burkhart and even people in the city. It was also the occasion of some division within our faculty.

Also in Rochester there were very fine men of broad interest. Mr. Eastman's physician was Dr. Edward W. Mulligan, a big, gaunt man who rather terrified people until you got to know him. He was a great power in the town and watched over what went on at the hospitals, what the City Health Department was doing. He was the medical conscience of the place most inconspicuously. He was supported by a close friend of his named Joseph Roby. There were other physician in the town that I got to know who were high minded men of considerable ability.

Dr. Albert Kaiser.

Albert Kaiser was one of the best of the lot. He was a pediatrician with a great public spirit that so many pediatricians have, and I think he became Health

Officer of Rochester after Dr. Goler's death, though I'm not sure about that.

Then a layman in the trustees was Mr. Edward Minor, a very wealthy man, who was a book collector and whose chief interest was in all the original and early works on yellow fever. He collected not only in that field but other books. For instance, he gave me an original edition of Beaumont's classic on the gastric juices, the Alexis St. Martin experiments, and Mr. Minor and Mrs. Minor had groups of friends around them and Dr....

Sawyer?

No. I mentioned his name a moment ago. Mulligan--Mrs. Mulligan had a great big house, and they used to have a real, old, Victorian type of salon. Once a week people would come there for tea about four O'clock, and you'd have a talk on some sensible subject, or some significant subject. It was not just chit-chat. They really put on a kind of salon that sometimes I had seen in New Orleans with Grace King and some of the others. Always there were good things going on in Rochester in music and lectures, and the town was visited by a great many people. I suppose Mr. Eastman's connections were all over the world, so it was surprising to me, and to all of us, I think, to find in the upper border of New York, a rather isolated..../the phone rang/

<u>You were commenting on the way the town was organized--a multiplicity of turn vereins in terms of interests, clubs and so on.</u>

It wasn't exactly an organization of the town. It was something natural and indigenous in the spirit that they had and which brought them together. They had a great many clubs. The sense I got was that they were seriously interested, without being mushy about it, in community welfare, and perhaps my appreciation of that element in the population probably derived, in the first instance, from Dr. Goler, the health officer, and Dr. Rhees, the President of the University, and then Mr. Eastman, but I only got to know Mr. Eastman somewhat much later.

Dr. Goler is worth a vignette.

Dr. Goler was one of the first vigorous health officers in the United States, in my opinion, and he belongs in the categories of George C. Shattuck and Charles V. Chapin---these men who were innovators and absolutely independent of politics, courageous, thoughtful, energetic, tireless in doing what they thought was the right thing to do and willing to both accept and suggest new ideas and new undertakings. Dr. Goler survived practically all the political changes in Rochester over a period of maybe forty years before I got there. They couldn't get rid of him because he always had support for anything that was going on. He had some very severe times to deal with in the earlier days--an epidemic of small pox, an epidemic of fatal scarlet fever. He had to make all sorts of improvisations to build up his health department and under small appropriations. I don't think he ever got from the city very much money for his work. When I got there, he had a small laboratory, but big ideas about that laboratory and about hospitalization. It was his conception that the new medical school, if it could and wanted to, could combine with the City in activities of medical care so far as hospitalization is concerned, by having a Municipal Hospital on the grounds with the Strong Memorial Hospital. They had different administrative supervision, or different superintendents. In the case of the Rochester Health Bureau Laboratories, he was glad to turn it all over to the medical school, and it came into my Laboratory of Bacteriology right from the start. Did I talk about this once before?

Well, Dr. Goler just gave me his complete confidence and made such arrangements that it was possible for me to engage people to work in the laboratories and to discharge them, if I thought it necessary. As a matter of fact, I don't recall any changes in personnel. Some of those people who came in to work with me remained my friends after I left Rochester. I saw one of them not long ago who used to make media--Mrs. Josephine Mikletisch. The qualifications of these people were left to my judgment and in the assessment I was never bothered with Civil Service forms and the rigamarole that often goes with a municipal enterprise, and that Laboratory grew to be very large. It took in all the health examinations of a laboratory nature from excretions, blood cultures, and throat cultures, surveys of all kinds for bacterial infection, the water and milk supervision and lots of miscellaneous examinations.

How did you take to the laboratory largely staffed with women technicians? This was novel.

Yes, they were women technicians. Miss Hester A. Austin was head of serological work where we were doing Wasserman tests by the thousands. A man was in charge of the water and milk, and his wife worked with him. We had many women on the staff all the way through. They were good.

There's just this comment in one of the letters--"What do you think of women workers in the laboratory?" You inherited a health bureau--a four female health bureau.

I took them on, and Miss Austin was one of them, I'm sure. Who's that letter to?

This is to your sister.

They worked just the way men would work, and they worked just the way a physician would work. We didn't have--as I recall it, we never had a time punch card system. They came early and stayed late.

Did the possibility of increasing the Public Health Laboratory occur not to by-pass Albany, but so that you wouldn't have to send specimens on to Albany? Miss Austin, I believe, was sent down to the Laboratory at Albany.

For training.

So that she could do this work locally.

That was the policy of the Albany State Laboratory. They had no ambition to take over the jobs for the municpalities. The only difficulty that I remember in developing that work from a public relations side was that some doctors with vested interests in laboratories, either their own, or group laboratories, had a feeling that the school was doing this free for the city and not charging the patient anything, and that this was interfering with the emoluments that physicians had a right to expect. That didn't bother us very much because the Monroe County Medical Society was one of the best, most liberal minded of any that I've known about, except perhaps the Medical Society of Maryland in Baltimore. They didn't interfere with us the way the New York County Medical Society tried to interfere with the New York Hospital and the Cornell Medical School when I went down there. At Rochester, I think the physicians had the same general point of view that I've been trying to attribute to the citizens of the town who were not in medicine. This grew to be a very large practice, so to speak, because there were many thousands of specimens, and behind every specimen there is a patient, as a rule, and that patient is anxious to know what it is all about, what is found, and the doctors are pressing to get answers because their actions in treating the patient depended a good deal on what the laboratory found. In reporting the findings to the doctors, and sometimes to the people, occasionally you have to do a little missionary education because they don't always understand. For example, I told a doctor one day that the specimen of sputum that he'd sent to me contained a pneumococcus. He said, "It can't be. My patient hasn't got pneumonia."

Well, his patient had pneumococcal laryngitis, and of course that comes from the larynx up into the sputum. Just because there wasn't any pneumonia he thought we must be wrong. That sort of thing happened over and over again. Every now and then they had an outbreak of a disease that was puzzling. One time I remember scarlet fever was troublesome, but what put the great burden on us, which I did not work out properly, was an outbreak of what we now know as psittacosis. It was called parrot fever at that time. The people who had these little love birds and kissed them got infected with what we know now is a virus, but when I worked on it, and these people in pet shops were getting this disease and the people in the town were getting it, I found what had been reported before, a paratyphoid organism. I didn't go any further than that, but my friend, Dr. Rivers at the Rockefeller Institute, really worked out psittacosis virus.

<u>This letter indicates that you no sooner brought the Public Health Laboratories into the general bacteriology laboratories, than you had a diphtheria outbreak in one school--this is the sort of thing that would come in--and a typhoid outbreak in a state hospital. This was the kind of thing that would come in--sudden like.</u>

Oh yes, you had to be ready to do almost anything. At that time tularemia was a common disease in rabbits in the region. Tularemia is communicable from the rabbit pus, if you get it on your hands. Every Saturday I can recall hunters would go out, and then they'd hear about tularemia, and they knew that I was around, or that my laboratory would tell them whether these rabbits were safe to handle. Sometimes by late Saturday afternoon my laboratory table was just stacked with dead rabbits--a lot of work to do.

Another area that the Health Bureau Laboratory took me into was medicolegal work. I got to know the Chief of Police.

Andrew J. Kavanaugh.

Kavanaugh was a fair haired, big Irishman with a tenor type of a voice. He had no means at all for identifying spots that might be blood, and if they were blood, whether human blood or animal blood, so I used to work with him and work out his cases for him. I did a good many until I had some experiences with the legal frenzy and to me reprehensible behavior of district attorneys prosecuting some suspect and who in my opinion were not after the truth, but were after winning a case. They would take me on the stand, and both the prosecutor and the defense would make life so miserable that I didn't want to do it after a while. Do you want an annecdote about that?

I sure do.

Once at Canandaigua I was an expert witness on the examination of some blood and hair that had been scraped off the fender of an automobile that had killed a man. The district attorney got very excited about it. I was sitting at a little table underneath the judge's stand almost, and this lawyer gave me the best example of legal frenzy that could be turned on and off that I've ever seen. He started pacing around the room shaking his hand at the jury saying that "this cruel bootlegger here"--he had no business saying "bootlegger" because he didn't know it--"has killed a boy who is the only support of an aging mother."

He kept getting closer and closer to me. We had been talking about these blood stains, and the defense witness was on the stand. I was sitting below. He kept getting closer and closer to me, crying before the jury and of course poisoning their minds. As he passed my table, he said to me,"What is that word that I'm trying to think about?"

It was all of a sudden--like that.

As he came around again, I said,"Hemosiderin crystals."

He shook his finger at the witness on the stand for the accused and said, "What are hemosiderin crystals?", and then he made a big fuss about that.

After a while at that, I told Chief Kavanaugh I didn't want to do any more, but that I would help him in any way I could. I found out then another thing about lawyers; they have a pattern--that is, these criminal lawyers have a pattern, so one day the chief asked me to help him out. He'd had a lot of sooty clothes that had been pulled out from a chimney flue in a room where a man had been killed, and there was a question as to whether there was human blood on them or not. I told the chief that I was not going to do the work, but that if he would send somebody up from his laboratories, I would try to help him find out, so he sent up to me an expert on petroleum products. This man knew nothing about examining blood stains, but I knew the lawyer who was going to defend this accused person, and I'd been--excuse me again. [the phone rang again]

<u>We were in the midst of that</u>....

Turn this off a minute.

We were in the midst of a trial.

He sent this petroleum chemist up, and, as I say, I knew the pattern that the defending lawyer would take in cross-examining this expert witness. So we piled the dirty and bloody clothes on a table in the middle of my laboratory, and I walked all around the room for two or three hours telling this man what to do in the order in which I thought he'd be quizzed. It turued out to be human blood, and there are lots of things you have to do to be sure that you're not getting contaminations as you handle it. About a week later this man went on the stand and qualified as an expert, and then the defense lawyer took after him. He came off with very great success, and he told me afterwards,"Doctor, it was just like turning over the leaves of my notebook."

This was an utterly untutored immunologist who only had to be told how to answer the questions of a lawyer, if you could guess what he was going to ask ahead of time. Well, I didn't do that any more, but I would help by indirect ways.

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Did you meet Calvin Goddard?

Oh yes, I know Calvin Goddard from a long time. How does he come into this at Rochester?

The Scientific Crime Detection Laboratory.

He was in Chicago.

That's right.

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I knew Goddard from a long time. He was a great ballistics expert. He's the one who solved the Valentine murders. He was a colonel in the Army; as a matter of fact, he was the head of the Army Historical Unit just after Colonel Joseph H. McNinch. Calvin Goddard died a few years ago. I knew him before. He didn't come into this particular situation.

<u>They offered--well, you didn't want to participate any more, but there was an Association of the Chiefs of Police.</u>

I was a member of the International Association of the Chiefs of Police. I carried a ticket for a long time.

<u>When they explained to you at Johns Hopkins that you would have at Rochester a rich variety of source material, you never anticipated this sort of thing, did you?</u>

No, I never anticipated that I would be into this, be doing work in this for the police, but it's a routine immunological exercise to be able to distinguish--as a matter of fact, I go back to Joseph Jones again because his description of the malarial parasite, 1876, is published in the account of a murder trial in Louisiana, near Donaldsonville, a man named Narcisse Arrieux was killed by being hit on the head by those big weights that are used in a grocery store, and he bled on the mahogany table there and on his clothes. My grandfather, the Health Officer for Louisiana, Joseph Jones, was sent pieces of slivers of bloody wood and pieces of wool cloth cut from the shirt of a Negro who had been suspected of the murder, and he said in his report that this was not only human blood, and he described why he knew that it was human blood, not from immunological tests, but the blood corpuscles were alike in size and shape as those he had seen in Philadelphia. He had written a big monograph on the mammalian corpuscles. He said further that it was also the blood of a man suffering from intermittent fever, and he described what undoubtedly, in my opinion, are malaria parasites in those red cells, so again I go back to my grandparental influence. This kind of thing was all in the same line.

How did Goler feel about the service--the Public Health Laboratory in this kind of operation?

Dr. Goler was in favor of doing this kind of thing. In fact, he left me alone. He not only didn't question it, but he didn't try to protect me from myself which I wish somebody had done. You get into all sorts of things. You see, the populace call bacteria "bugs", and one of the jobs I had to do was go into an apartment, where for some reason or other, there was a sudden development of little bits of beetles, little bits of things, the size of pepper grains, thousands and thousands of them that came out of the woodwork and dropped into the bath tub and on the bed. There was a young couple in there. So knowing that I was a bacteriologist I must know all about "bugs" and they called me in.

Another was a case of a woman who didn't care very much for her husband so she got two horned toads--you know, these little, dry frogs from Arizona, and she had one on each shoulder all the time so that she could not be embraced. I was called in to take those monsters away. I kept them in the laboratory for a while.

There is more than meets the eye in bacteriology.

Indeed there is. One of the consequences of the introduction of the Public Health Laboratory was the development of the New York State Association of Public Health Laboratories.

Yes. Well, I knew Dr. A. B. Wadsworth, the head of the laboratory, and I knew Miss Mary Kirkbride, his first assistant, and I told you that after I came back from abroad in 1924, the first thing I did was to go over and work on the Ramon test in that laboratory. I knew that Dr. Wadsworth had been thinking for some time that they ought to have some meeting or organization of the people who had laboratories in Syracuse, Albany, aside from the state laboratory, Buffalo, Rochester, and a good many hospitals had good laboratories. This grew up into an organization and they had at least one meeting a year in Albany. This association is still going after all that time. I had an invitation to the meeting that went on last week. I'm an emeritus member of it now.

Didn't they have periodic difficulties before the state legislature--the "Battle of Albany of 1932"--the medical committee.

You mean, the antivivisectionists? Yes, they had a very violent period of antivivisection agitation in New York. Was it 1932? I thought it was a little earlier. Somehow or other I was made chairman of the medical committee to oppose these people. They had a big hearing in Albany to which I went. I went through the usual effort as Dr. Welch did. He led opposition to the antivivisection group in Washington in the early 1900s. There was much more than the hearing in Albany because it was necessary to find out who were important in controlling the votes of the members of the legislature, the Senate and the House. That was the first time I had ever seen the operation of a local political ring under a man who was a good citizen, but not really very much educated, and this was the Republican Party of Monroe County. The head of that local political organization was a plumber, and I remember going down to the Genesee Hotel, I think, to have a meeting to consider this vivisection problem. The room was all draped--a big room draped with heavy brown curtains. You could see the people who were moving behind the arras. Here was this plumber sitting at a big desk, and the legislators were sitting around on little, flimsy, picnic-type chairs. I came in to that, and somehow or other explained to this man that guinea pigs were needed, for example, to titrate, or find the strength of diphtheria toxin. He said--I didn't know that he was going to say this, but he said,"Oh, I understand that. My child was saved by antitoxin. You say that it was measured against guinea pigs?"

I said,"Yes," and told him a little about it, so he turned to these legislators, and he practically said,"Well, boys, we're going to support this Doc!"

That's what they did. Then the next one--the movement got into a little trouble in the Senate, I think, in Albany, and we found that there was a man in Buffalo who was a fancier of certain kinds of dogs, and through that there was an approach to him. He influenced a vote or two, so we came out all right, but that agitation, as you know, is going on at present, and fortunately I haven't had to bother with it any more.

Developing an association in a sense puts a floor of standards for the operation of public health laboratories.

The Public Health Laboratories weren't in this antivivisectionist thing. This was state wide medical schools and everything else.

The correspondence shows that it comes from all directions--yes, but part of the growing knowledge in the state is through the development of associations--from Wadsworth to William H. Park to J. K. Wilson on soil bacteriology--all these people and others were very much interested in a floor for standards in their laboratory for laboratory service, and as a corollary to that the necessity for experimental animals. I know that the volume of work in the Public Health Laboratories was just tremendous.

Yes, the volume grew a great deal—went up into the thousands and thousands of tests. What the test is is still to be determined because you say that you're going to do one test for syphilis and one blood specimen, but you might have to make six, or eight manipulations of the specimen. Some people call this specimen tests, and some people put down all the things they do, but we gave them specimens and then when it was a very difficult procedure, you could separate the tests. The work is far more than the thousands of instances that are in the reports.

Yes, did the immunological manual come out of this kind of work?

The one I showed you?

The one published by a committee--eh, I don't have it here which is one of the bad reasons for not leaving that room, but a small publication, a manual for tests, I guess.

Everybody has a manual. I brought you the one I used for my class. There are published manuals, but the published manuals are rather compromise editions. They try to suit everybody, whereas most of the cooks have some way of doing it that they prefer, so you generally write your own manual with the minor variations either changed to fit a situation, or to fit in with supply, or to fit in with money, or to fit in with your preferences. There are always a couple of ways of doing something.

I was thinking of a small publication which I'll show you tomorrow. I don't remember its precise name, but later Dr. James A. Kennedy....

Jim Kennedy?

Jim Kennedy tried to get a revision accepted, but it was an effort to standardize tests, and share them generally.

That was going on all the time too because new tests wear being devised and new variations were coming out. It's customary to shed around specimens that are unknown and let another man try to see what answer he gets. Standardization goes on all the time. I want to tell you about Mr. Eastman's health service for the employees at Kodak, but I don't know whether you want to go into that now or not.

There may be other things about the Public Health Laboratory--there are a lot of people who figure there. I hope I've brought it along. Here's the original staff and the way in which it grew.

This is 1932--this is near the end.

Yes, the original staff is listed at the top.

Oh yes. Miss Myers married the man who got to be in charge of the water and the milk. Some of these were visitors. I called myself the "Ziegfield of Laboratory Technicians" because I looked for intelligence, health, strength and affability in these people. They were all good looking girls. They were strong and well, and there were a great many marriages in this group. They don't show up here as Mrs, but marriage became so common in our amiable surroundings that of the ladies who visited for a few weeks, one of them wrote me she had been there for two weeks and hadn't been married yet.

Did any puzzles that led to scientific exploration grow out of the Public Health Laboratories?

Oh yes, we published papers occasionally. Mrs. Priscilla Cummings got to be an expert on streptococci in connection with pediatric troubles. I think that there were a good many minor papers--I forget them now. I don't remember titles, but I know that there were a good many publications, and these people would give papers--a lot of them weren't published--at the meetings of the laboratory association.

<u>Were you able to intrigue, invite, otherwise corral doctors in the community to come in and work in the laboratory?</u>

I don't think I cared to try to do that. I had lots of relations with doctors, but--well, probably if I told the truth, I didn't want them to be around in that capacity.

<u>It was running and approved service for them. I thought maybe</u>....

I used to go to their meetings in the Monroe County Medical Society. I knew practically all of them....

<u>Let's turn to</u>....

Are you watching the time, because they told me that Dyer /Dr. Rolla Dyer/ would be there until three o'clock.

<u>You want to go upstairs? It's ten minutes to three. We'll stop and come back to this tomorrow.</u>

Friday, April 29, 1966 B-9, N. L. M.

Well....

I want to get you an ash try. That was on the machine.

In a speech you gave at a meeting of the City Club in Rochester in January 23, 1960, you indicated that soon after you arrived in Rochester, you became connected informally with the development under Dr. William A. Sawyer of the medical service for the employees of the Eastman Kodak Company, and that this service was more than a routine establishment for industrial medicine. As another illustration of the public-minded nature of the people in Rochester, this is a good example. I thought that perhaps you'd tell me something of this development under Dr. Sawyer.

It stemmed really from the influence of Mr. Eastman because Mr. Eastman early set up two things of unusual character at that time for the benefit of his employees. He gave them a share in the profits of the company, and they would get sort of dividends on the increase in the value of the products, and he had a very loyal group of some ten thousand employees out there at the plant. I don't think he ever had a strike. That's part of his social mindedness, so to speak, and at the same time, he set up on the advice possibly of Dr. Goler, possibly of Dr. Roby, or Dr. Kaiser, a medical health service for his employees. He had in charge of that medical service a Dr. Sawyer whose first name I have forgotten.

William Sawyer.

William Sawyer was about my age, and we had many common interests in the infections that were prevalent, the respiratory infections and whatnot among those people. They were cared for by Sawyer in dispensaries and some home visits in the manner of, say, the British Health Service, or later day health service for populations of people. They didn't have to pay, and they were watched very carefully not only when they were sick, but in applications of preventive medicine, in sanitation. The conditions under which they were working were carefully regulated so that their health wouldn't be damaged any more than was maybe unavoidable in some cases, when they were exposed to either fumes, or temperatures, or conditions of confinement which I'll mention in a minute which were part of the process of manufacture. This was a very enlightened medical service. It included also preventive, prophylactic dentistry and was quite in line with Mr. Eastman's ideas of a dental clinic, of doing things for groups of people.

Mr. Eastman didn't seem to concern himself directly with individuals. He was an aloof man, and his philanthropy was institutional, or adjusted to groups and populations rather than for individuals, but he understood that the individuals composed the groups, and, of course, he cared for them. This was a broad-minded, social undertaking and very well done, I thought.

When I mentioned confinement, I was thinking at that moment of the men and women who had to work all day long, or at least an eight hour day, in practically total darkness in rooms that were not noisy and rather oppressively quiet. I used to think it was a sort of solitary confinement, except that there were many people there. I'm talking about the rooms in which, for instance, all the sensitized photographic printing paper was made. They handle that in the open to package it, but it was in rooms dimly lighted with deep red colored lamps. They managed that so that those people didn't become annoyed by their isolation and were not hurt by it in any way that I could see. I think Dr. Sawyer used to remove them if he saw anything like claustrophobia, or the sense of isolation evertaking them. That was part of the work , and of course Eastman photographic products involved the use of many poisonous things like the silver salts that they used in the emulsions. At the same time out there at the Kodak plant a great many people were developing the Eastman organic chemical production. They had an enormous laboratory for making and synthesizing organic chemicals, indeed, one of the men, Dr. Hans Clark, who was in charge of that work for a while was the greatest organic chemist in the country. He became Professor of Organic Chemistry at Columbia and later at Yale, so that it was a very interesting manufacturing plant full of experiments in social undertakings and a concern for people.

<u>This is the first time you met Marion Folsom.</u>

I suppose so. I don't remember the first time I met Mr. Folsom. He was a Director of the Eastman Kodak Company, not a trustee of Rochester University at that time, I don't think. I got to like Mr. Marion Folsom from the start. He's very quiet and reserved. Although born in Georgia he was rather a New Englander type. He was absolutely forthright and thoughtful, didn't say much, but what he said meant a great deal. I knew Mr. Folsom in his office. I didn't have any social, or personal contact with him to speak of at that time. As a matter of fact, even when he was Secretary of Health, Education, and Welfare in Washington, I never saw him outside of the office. For some reason, or other I don't know, he rather lived a secluded life both in Rochester and here.

<u>This was again another example of what forward thinking men would do in that town.</u>

Yes.

Symptomatic of the place.

Yes, this was characteristic of it.

Well, so much of what a school is and becomes is related to the man who heads it up. He gives it somehow, or in some way an atmosphere, a tone, and this brings us to the President, Dr. Rush Rhees. I don't know how you want to talk about him. He was the leader here, a somewhat older man than those in this group, but with a large experience. The book you let me read [John R. Slater, Rhees of Rochester (New York, 1946) 304] is a good one in the sense that it explores the essence of a man without coming to a conclusion, and so any illumination you can give about him would be helpful because after all, he did largely set the tone.

I think that's the way to express it. Dr. Rhees was one of the wisest educational administraters that I ever met. He'd been president of the University of Rochester for perhaps fifteen, or twenty years when this school began, and he had liberalized a rather narrow institution. Rochester was founded, I think, in about 1850, and the University had some rather restrictive Methodist type of influence on it. Dr. Rhees had much broader experiences and uplifted the place and liberalized it. In addition, he had very close association with Mr. George Eastman. He was proud of the Eastman Kodak Company and was no doubt influenced by Mr. Eastman's large ideas. Typically, like a good many in Rochester, he was devoted to his city and the best things, and when you spoke of his being a leader in the medical school's development, you used the expression "set the tone" which I think is a very good one.

Dr. Rhees did not pretend to know anything specifically scientific about medicine, or pathology, as I recall it. He could talk with you about what you wanted to do and how you were going to teach, the philosophical aspects of it, but he didn't bother at the start to have much to say about the actual effects of actions that were done. However, he was responsible for taking fire at the idea suggested perhaps by Mr. Eastman and perhaps by himself--I don't really know the origin of the project for the school, but Dr. Rhees took the leadership, had the first dealing with Mr. Abraham Flexner and was a sharp bargainer because he got out of Abraham Flexner and the Rockefeller Foundation a million or so more than Mr. Flexner intended to give. Mr. Eastman wanted his money matched also, so that they had a pretty strong line up against any withholding.

They had to overcome parochialism in medical educational ideas at the time. There was a medical school at Buffalo, one at Syracuse, and all the great medical centers at New York, and why on earth start another medical school in Rochester in the middle of that? Cornell had a medical school with a campus in New York, two hundred or more miles away from the campus in Ithaca. That was a point that Dr. Rhees, Mr. Eastman, and even the rest of us had to examine, and either accept, or oppose. I wondered myself--why put another medical school up in that place where there was no particular outstanding clinical facilities? It was not a medical center. It had a population which looked to us at first so well paid from their employment that there would not be occasion for a great deal of indigent patronage of the hospital and school. Indigent patronage is a very important thing for material, if I may use a crude term, from which young medical students and young doctors are trained. I don't mean any careless, loose experimentation but until later, as it was at New York, it was not possible to examine the pay patient, so to speak, in the presence of groups of people as you could examine the indigent. Rochester looked to some of us as a place where there wouldn't be the usual amount of opportunity for seeing disease among people who could be studied very thoroughly without any objection on their part.

Dr. Rhees settled that to his own satisfaction. He must have been very persuasive because he persuaded Dr. Whipple to come on as Dean at a time when Dr. Whipple was well set at the University of California as a Dean and as the head of the Hooper Foundation and didn't want to touch this Rochester job at first. Dr. Rhees went out there and determined to stay until he succeeded in persuading Dr. Whipple to come to Rochester as Dean, and in persuading him he gave him life tenure in the job, so to speak, and imperial power over his faculty and school that few deans have enjoyed. Dr. Whipple exercised that power in a very thoughtful, but undoubtedly firm manner. Dr. Rhees was very close to the school. He had a part in the building plans. He sanctioned what was pleasing to Mr. Eastman as it was to the parsimonious New Hampshire product—where was Whipple from? Vermont or New Hampshire?

New Hampshire.

New Hampshire product that Dr. Whipple was in approving the plans of the building, the style of the building and all of which it was made, so that it turned out to be a structure that we called "early penitentiary style" of architecture. It was very bare. There was hardly any plaster anywhere in it, but sanded brick walls inside. There were bare cement floors which were finally oiled with linseed oil. All the money was spent on equipment and room to work in which was wise, but aesthetics were not respected especially. Mr. Eastman liked that. The rest of us thought it was a little bit crude at first, but you soon appreciate the advantages you have from working in a place where there was really money for the work. Well, Dr. Rhees kept in touch with all of that. We used to talk with him about plans, floor plans even--not in relation to the scientific work going on in them, but in relation to living and working in the quarters.

Then Dr. Rhees was the presiding officer most of the time at the faculty meetings. The faculty was called "The Advisory Board" which is a term which I think Dr. Whipple gave it, instead of giving it a more executive type of title. We got along very well in the Advisory Board, talked about the plans of the place, the educational outlook and the independent projects that we were going to undertake. I think that most of us knew more medical schools than we had come from. All of us had been to scientific meetings. I don't recall that we ever went around the country looking at other medical schools to see what could be done. We knew enough, and we knew more or less what we wanted to try to do. Dr. Rhees encouraged the individual expressions of the members of this Advisory Board.

He was a man of great tact, and I remember--well, I think the best example of his tactfulness came up about the second year of our progress in the school. We all had budgets for our departments. I think mine was about twenty thousand dollars a year. Some were more, but Dr. Whipple didn't let the heads of the departments spend that budgeted money without his approval. We used to have to go down to his office and talk to his secretary about our wish to buy, we'll say, a hundred test tubes. Every little amount had to be passed on by the Dean's Office. It got so tightly controlled that several members of the faculty including myself used to meet on the enclosed fire stairs and talk these things over. We worked ourselves almost to a revolutionary pitch. We said that if we couldn't have our budgets, the school couldn't have the Dean almost. So we were going to have a show down faculty meeting in front of Dr. Whipple and Dr. Rhees. Dr. Rhees had talked to me and others about it. He knew what was afoot, so that day he said,"Now, gentlemen, we are met to see whether we can devise some plan by which the Dean's Office can be relieved of a great deal of administrative work in dealing with the purchase of supplies, animals, and things like that for the different departments. Have you any suggestions?"

Somebody said immediately,"Yes, give us control of our budgets."

Dr. Whipple probably knew that he had to give way too. Dr. Rhees said, "That's a very interesting idea. Let's see if it's acceptable to the Dean."

He asked him, and so we got our budgets through that business, but we were ready to blow the place up almost. Dr Rhees did a hundred things like that. He was a very approachable man--not exactly jocular, or familiar, but dignified.

<u>He knew what was going on.</u>

Yes, he knew what was going on in the school. He knew what was going on in the politics of the town. He knew what was going on in the field of education in the country. He was, I think, a very wise man.

<u>That's a very nice annecdote--I was going to say shrewd, but that was the man, to get the best out of a group.</u>

Yes, his timing was good. I don't know that any of us put him wise to it, but he knew something was brewing.

Well, another kind of thing that he and Whipple and the rest of them did is illustrated by a very small point. There were firm and rigid, convincing opinions on large matters of education, we'll say, or of activities of one kind or another, but not too much concern with the smaller matters. Dr. Whipple was quite willing to watch things and take actions according to the way they were turning out of their own accord. An example of this, and I've always thought a good one, was when that place was built there were grass plots between big wings of the building and around the wings and the corners. A natural desire of a builder would be to go and lay pavement walks by some arbitrary scheme right away, but Dr. Whipple didn't put down those walks right away. He let it go to see which paths would be stomped down by students and others, and then he laid a walk there. That's a good way to do.

<u>Very nice.</u>

That went on through the school a good deal in our relations with the students and others. Instead of trying to force them to do something that would have been silly, it was so much more interesting and permanent to let them have some expression and then build on that.

<u>What role, if any, did Dr. Rhees play in the development of the Department of Bacteriology?</u>

Dr. Rhees was happy to have the Health Bureau Laboratory work done there. He could have made that more difficult by saying that one of his professors ought not to give half or more of his time to the service of the City because Dr. Rhees was like a good many other university officials who very wisely say that ordinary service, either in the care of patients, or in the laboratory services, is not a function of the university, not a predominant function, but he didn't oppose at all the developments that went on in the Department of Bacteriology with the Health Bureau Laboratory.

He was interested in the sides of bacteriology that had to do with people rather than in the biology of bacteria. I used to make vaccines to try to prevent Dr. Rhees and Mr. Eastman from having colds; make swabs of their throats, grow the organisms and make a vaccine out of it. Dr. Rhees was interested in the human applications of the subject. He was interested in the quality of the work of the students, and we discussed the progress the classes were making at times. He was not a man who was just impressed by ABCD marks. He was after intellectual content. It was a very happy, pleasant, easy relation.

On the subject of colds, I have an exchange of correspondence you had with Dr. Rhees about a chlorine applicator, a Mr. Turner, I believe.

Turner?

You used this apparatus in the laboratory for the treatment of dogs suffering with distemper, a small group of puppies. There was in existence this apparatus, and there was the question of what to do about it, but in writing to you Dr. Rhees asked you to investigate Dr. H. S. Diehl and his experiments at the University of Minnesota on the student body with reference to colds.

Wallace and Tiernan were the manufacturers of chlorinating apparatus for the water supply by which they put anhydrous chlorine gas into the water and had flow meters and controls. There was a phase--some time in the middle 1920s, I think--of preventing respiratory infections by the inhalation of chlorine gas. Dr. Diehl was the Dean of the University of Minnesota Medical School at Minneapolis. I don't know whether he published, but he was the one who seemed to give a boost to the subject. Then there were some extraordinary claims for it--a miraculous cure of colds, so much so that Mr. Eastman became interested and so did Dr. Rhees. I tried it out on those dogs with distemper. Distemper is now known to be a virus disease with a secondary bacterial infection, and I don't think that apparatus did those dogs any good. They got an apparatus to try on Mr. Eastman, but I don't know that we ever went that far. What you were to do was to put your head in a sack and breathe chlorine, but I don't think that I had to go through that. It didn't last very long.

No.

I was never convinced of it.

Your letter was to that effect, that you were not convinced. Diehl did publish an article in the AMA Journal. You reviewed that article and sent Dr. Rhees a copy of it. My reason for bringing it up was to find out from you something of his scientific interests, and this episode would disclose interest.

He had scientific interests, but this was a popular thing. Mr. Eastman was interested, and Wallace and Tiernan were a Rochester firm, and they were exploiting it. A number of things that people want to exploit have bacteriological elements in them, and the problem in the laboratory is not just to be doing service work all the time for manufacturers. For instance, the Castle Company would like endless experiments on sterilization pressures and temperatures, some of which I did. Eastman Kodak Company every now and then got hold of a compound that was claimed to have strong bacteriacidal properties, and I made some tests for them. An anology is the Chief of Police—they all want help on something, and you scatter yourself to pieces on it, but Dr. Rhees in this case, was, as I recall, not interested in it from a scientific view point, but from the public hurrah about it. It was a situation in a minor way like this krebiozen cancer cure that has been raising the devil with people the last few years.

Even Tiernan in his letter, and Dr. Rhees quotes his letter to you, said to Dr. Rhees,"I would much rather have the truth about the treatment and therefore not be led into undue expense and waste of time in pursuing a matter which did not hold out any promise." So Dr. Rhees wanted your judgment to explore the Minnesota experience which you did, so that he was interested enough to climb on top of that article and ask your judgment about it, to be in a position to be more helpful to Mr. Tiernan. I gather that was what the correspondence was about. It does show interest. I asked about the role he played in the development of the Department of Bacteriology. There are some--I don't know exactly. Well, let me say that these things are ripped out of context. They all happened at the same time, and dealing with them individually is wholly unreal, but in 1929, there is an offer to you from Chicago, and it has certain consequences in the development of the department.

Shook the tree.

Didn't it! Well, let's go to Chicago because this will bait, in part, Rhees' response. Do you remember this?

Yes. I think in 1928, I was invited to be a visiting professor at Chicago. Wasn't it 1928?

Either the invitation was in the spring of 1929, or the fall of 1928--I'm not sure.

It was a summer semester at Chicago.

<u>It must have been 1929. Yes.</u>

This is it.

<u>The correspondence begins in October of 1929.</u>

Yes, it was the summer of 1929, that I was a visiting professor at the University of Chicago. I went out there, lived near the university, and lectured on basic, non-medical bacteriology. I ran the class. It wasn't medical bacteriology, but general bacteriology, bacteriology of soil, water, atmosphere, and metabolism of bacteria, variations, all the basic scientific side of bacteria without particular relation to pathogenic action. I carried that on all that summer and returned to Rochester at the end of that summer in 1929.

As a side issue before I forget this—I would like to put in here something about another activity I had out there. I had been working in my own laboratory in Rochester on the heat production by bacteria. I built a differential micro-calorimeter, a very sensitive thing. I got it so I could measure heat, I think, from one microorganism, and I published a paper on that heat production by bacteria, /"Bacterial Calorimetry II Relation of Heat Production to Phases of Growth of Bacteria" 17 <u>Journal of Bacteriology</u> 123-140 (February,1929)7. I did the experiment in the course of a day, we'll say, and then I made an empirical equation and extrapolated to what the expected result would be in forty-eight hours and so forth. Then I did experiments at those times, and they came out very close to the expected result from calculations, and so I wrote the paper.

A man replied in the <u>Journal of Bacteriology</u> and said that I was all wrong, /N. C. Wetzel,"A Note on the Application of Buchanan's Formula to Heat Production in Bacterial Cultures" 18 <u>Journal of Bacteriology</u> 117 (1929)7. The way he explained my error was to have his paper be composed of about three pages of integral equations. Well, I had had a little calculus, but I've already explained that I cannot understand profound mathematics, so on the side at Chicago I used to study mathematics from Friday evening until the next Monday morning, like old King David, locked up in my apartment--I had a little two room apartment--trying to get enough mathematics to understand what this man had said about my being wrong.

I had made the mistake--I got an instructor in mathematics from a section that was dealing with imaginary kinds of things. They would give the equation for a circle, say that it represented a square, and go on from there. This mathematics instructor couldn't see what I wanted exactly. I think maybe mathematicians now do it better. I wanted him to tell me how to take experimental data and put it into equations. How to handle that sort of thing was the problem, but he gave me a great deal of work to do. The thing that broke me down was that he gave me--I remember this problem. "At half past four in the afternoon when the sun is thirty-five degrees above the horizon, you throw a baseball into the air at two hundred feet a second"--that's too fast, but take that--"and the shadow of the baseball falls on the hemispherical dome of a nearby observatory. Plot the path of the shadow when the ball has fallen for one half second. Plot the path of the shadow and the rate of the progress of the shadow across this hemispherical surface."

That didn't help me.

That's staggering!

Isn't it? That was no compliment to me that he did that. It was ridiculing me. This mathematician that I had could solve that problem in a few moments because all the verbiage covers up a few well known physical equations that he has. He knew just what to apply.

I didn't get to ever understand the paper that said I was wrong.

I kept on working on that subject. Dr. Whipple didn't interfere, but I'm sure very often he would wonder, and so did some of my friends, why I would be interested in heat production by cultures. Well, it's a fundamental thing. Curiously it turned out to be of interest in other ways. Another professor in the Rochester group was studying the heat generated by impulses in the nerve. Take a sciatic nerve out of an animal, use very delicate measurements and see what heat is produced, and then you get some idea of the energy involved. He was not a bacteriologist, and he didn't take any care to keep his preparations sterile, so I showed him that all he was measuring was the heat produced by the contaminating organisms that were growing on the surface of the nerve. Bacteriology entered into a whole lot of things like that.

Well, I'm at Chicago. I finished the course, and I'm back in Rochester. I hadn't been there very long before President Robert M. Hutchins who used to be the Secretary of Yale University, a very vivid sort of a person, invited me to come back to Chicago to see him and Mr. Benton, I think, who was the Vice Chancellor, to talk about a possible position in bacteriology. So when I went to Chicago, I went to see Chancellor Hutchins and Mr. Frederic Woodward. In the course of a few hours they offered me the Professorship of Bacteriology because Professor E. O. Jordan was about to reach the age of retirement. Well, I knew a good many of the younger men in Dr. Jordan's department like I. S. Falk and my friend L. G. Taliaferro, who was the head of parasitology, and others. They assured me that they would welcome me if I came there as Professor of Bacteriology, and I was on the point of saying yes, that I would be honored—well, I was honored anyhow--and that I would be pleased to have them consider me for appointment by submitting my name to their trustees.

About five o'clock--I remember now--I went to see Dr. Jordan again in his backroom in the Ricketts Laboratory. We ralked in a friendly manner because Dr. Jordan, a most respected man, had taught me bacteriology way back when I was getting ready to go to Hopkins. He had written a famous textbook, and he was known the world over for his work on the preservation of food products--particularly meats, and in the course of the talk, he suddenly said that he wanted me to know that the authorities of the University of Chicago wanted to keep him on as the titular head of the department, even though he'd have another professor with all the responsibilities "because", he said,"of my extensive and great influence with the food packing industry."

He was a great consultant for Armour, Libby, and a whole lot of others who had had problems in sterilizing hams, saussages and canned stuff of all kinds. Well, I knew right away that that wouldn't be well for him or for me. I was very respectful and fond of Dr. Jordan, but I could see only trouble in an arrangement like that where men of my--I won't say aggressive, but pressing qualities, would get to work under his superior status and have the responsibility for the teaching and running of the department.

That would be a very unpleasant thing for both of us.

<u>There's one thing that you might clarify. This offer wasn't in connection with a medical school.</u>

No. It was the University Department of Bacteriology, but it was near the Chicago University Medical School. They're all out there in a cluster, and there are subsidiary laboratories in the hospital, and medical students were taught some of it in Jordan's department. The whole parasitology was there too. As a matter of fact, the laboratory in which Dr. Jordan's laboratory department was housed was the Ricketts Laboratory. That's named after Dr. Howard Taylor Ricketts who discovered the minute microorganism that causes typhus fever. The microorganisms got to be named after him. That's where the term "rickettsia" comes in.

<u>Well, as you saw it at the time would this have afforded you greater opportunity for your own research?</u>

Yes, I would be head of a bigger department. I would have a much bigger budget. I would have a different position in the academic world, so to speak, by being the head of a famous well supported department in a famous university that was far older than the few years of Rochester. I was very tempted to do it until this came up. The offer to me, I forget the amount, but it was considerably greater--I think I was getting eight thousand dollars a year from Rochester, and this would have given me a great deal more. I came back and told Dr. Whipple and Dr. Rhees about it. Not much was said, but they were glad that I did not take it.

As I said, it shook the tree in two ways. I needed additional staff members notably a protozoologist and parasitologist, and I asked for Dr. Oliver R. McCoy. I got him right away with a good salary, equipment and everything, but the thing that amazed me, and I found out without asking for it, is that my salary was increased. I found that out by noticing first that my bank book wouldn't balance. There was more money in the bank than I could account for. I didn't say anything about it because I know I'm not good at arithmetic. I thought that I would wait another month. The next month the balance was even larger. The bank had accumulated this excess. What they'd done at the school was increase my salary without telling me--that is, the university paid it into my bank account.

<u>They also offered you an assistant to take care of the Health Bureau activities--Dr. Kennedy.</u>

Kennedy was a pupil of Zinsser. The two positions I wanted, I guess, were the parasitologist and the assistant in the Health Bureau and in teaching. Jim Kennedy was enormously helpful.

<u>Then there was--well, I'm looking at a memorandum concerning the proposal made to you by the University of Chicago, and this is on University of Rochester stationery, their efforts to....</u>

What part of Rochester?

<u>This is titled Intramural Correspondence. Apparently you went back and talked to President Rhees.</u>

Is that my memorandum?

<u>No. I think this was sent to you. It says at the bottom, "This statement to be reviewed by President Rhees." There are three items listed and one of them is a fellowship fund which will pull you into greater contact with the University of Rochester. Earlier, when I asked about Dr. Rhees, he raised a question with you in 1928, about curriculum changes for the undergraduate school, and you replied about studies in biology, and in 1930, this matter comes to a head.</u>

I gave courses in bacteriology to the undergraduates at the University.

Right. But you set up a whole separate section under Ralph P. Titsler.

Yes, Titsler is in the Department of Agriculture here in Washington now. He was their bacteriologist.

This proposal from Chicago made this particular idea jell plus these other two. Another person with whom you had to deal directly, and this may bring us to the people in this photograph with greater particularity, was the Dean, Dean Whipple. You've indicated something about the Dean. He bargained pretty hard with President Rhees to shield himself from unnecessary intrusions by civic functions.

He actually said that he would have little to do with them, and President Rhees said,"We'll let the public relations of the University take care of it."

I wondered in choosing a group of young people--and he was a young fellow himself--this faculty that sat in on the growth and development of the medical school, the hospitals, the relationships between the medical school and the various hospitals which were there. This is worth a word. They've had some continuity in your own life. I'm thinking of some in particular who may not be important, I don't know, but in surgery--John Morton.

John Morton is still up there. He's an extraordinarily good surgeon. In the influences on the establishment of the school, the influence of Johns Hopkins was possibly the predominant one. They used to say that we were starting up another little Johns Hopkins there because Whipple and I, George W. Corner--was Morton Hopkins?

I think so.

Yes, and Karl M. Wilson in obstetrics were all from Hopkins, but we didn't consciously imitate the Johns Hopkins. It was bred in us by that time, and you didn't have to think about it. It was like walking. Dr. Whipple was the leader of the administration of the school. There wasn't any question of that, but there was quite a good deal of independence in the heads of departments. We used to work together a good deal. There was perfectly wonderful teaching you could do in the beginning with a group like this. In a small class and with time, I can remember things like this--I had opportunities that I never had anywhere else. In taking the subject of diphtheria--I believe it's in the second year--I could take the students to see a case, a child with diphtheria and a membrane in the throat. We could study that bacteriologically and study diphtheria toxin which causes certain kinds of paralysis. There were people in that hospital who had post diphtheritic paralysis of the uvula, the swallowing apparatus, and some other things. At the same time there was a death from diphtheria, and Dr. Whipple did the autopsy, so right there in the place there was unequalled opportunity because we were all working together and had common interests in subjects, presentation, and students to take everything from the bacteriology to the final anatomical autopsy in diphtheria.

The same thing was done in tuberculosis. Right near the hospital there was the Monroe County Tuberculosis Sanitarium or Institution. There was a good deal of tuberculosis among people in the dispensary, people who were sick with various forms of tuberculosis, and there were the autopsies of tuberculosis. We could take tuberculosis from prevention of tuberculosis right on through to the final obsequy.

That didn't happen after a while. The place got too big, classes got too big, work in departments too big. Everybody was too much occupied, although we never lost fellowship among the faculty. The only difficulty I ever had was with one of the professors who was at Rochester before we came in, Professor John R. Murlin, who is a biochemist, not a medical man, a nutritionist. Dr. Walter R. Bloor was a great biochemist that they brought in, and he took a position that perhaps Dr. Murlin wanted to have. Dr. Murlin came from a different origin than the rest of us and had different points of view. There was around him at that nutrition department sort of a fence, difference of views and difference of behavior.

How did they react to the general title of the school--School of Medicine and Dentistry?

That was a compromise, but very interestingly managed by Dr. Whipple. Dentistry was put in the title obviously because of Mr. Eastman's long interest in dentistry. It caused a separation in the faculty until we saw that they weren't going to run a dental school out there, a fact which made a more sharp difference with Mr. Eastman's great philanthropical, dental adviser, the man who ran the Eastman Dental Clinic, Dr. Harvey J. Burkhart. Dr. Whipple's idea was that the medical school would take a few highly qualified dentists and let them work for M.D.s, or even let a man work for a Ph. D. in the dental field, even though he wasn't a dentist. At Yale later there was such a scheme, and sometimes we tried it at Rochester by which a dentist would go back into say the second year of the medical school and work for an M.D., so that he could be a dentist and an M.D. There was never even any effort made at Rochester to set up a school of practicing dentistry with chairs, drills and fillings. That disturbed Dr. Burkhart, and he never was reconciled to it, but again this bacteriological link which fits in with my hyphen, as I told you in the beginning, let me be pretty close to Dr. Burkhart and the dental clinic. I had a little section of a laboratory down there at the Eastman Dental Clinic. I taught dental hygienists, these women who scrape teeth, and I taught some of the dental interns that were there. I used to make a great many cultures and things, and I got on very well with them.

Dr. Philip Jay.

Jay came to work with me. He was a dentist from Detroit, and he did very good work on what's called *Bacillus acidophilus* which is an organism that gets into the cracks in your teeth and produces an acid from sugars and things and was supposed to have caused dental caries. Philip Jay worked with me, and I had other people in the laboratory who were interested in various sides of dentistry, but Whipple's wisdom in setting up this dental fellowship program and the post graduate work was enormously successful. I think I gave you a book on the careers of these people who have been there. They have become deans of dental schools, leaders in dental education, and they represent a great contribution to both medicine and dentistry.

So far as students in the school, the requirements for entrance were so high that you never got ~~a~~ dental students in these early days after the school was open.

I don't blame them because there ~~wasn't~~ weren't any offerings in dentistry in the school. They would have had to go through as medical students straight. The requirements were fairly high, and one of the best requirements was the required interviews with all the applicants. At least three of us would interview every applicant.

So far as developments in the Department of Bacteriology--as work increased, provision for an assistant to do the autopsy bacteriology was made quite early--

1927—Dr. William L. Bradford. Do you remember that?

Bradford was attached to the laboratory, and he worked in autopsy bacteriology for a while. After that, he began to be the main assistant to Dr. Samuel W. Clausen in pediatrics, and he finally became Professor in Pediatrics. That was another phase of collaborative work that I was able to help a little bit. In one of these wings toward the end of the late 1920s and early 1930s, I was able to provide a separate laboratory room—one for medicine, one for surgery, and one for pediatrics. They were all together. We were all together, and the Health Bureau Laboratory was around there, so that it got to be quite a varied and interesting place. Bradford was working on rheumatic fever at times and endocarditis, curious organisms, but the good thing was to have all the people interested in bacteriology on the same floor. You couldn't walk around without meeting another bacteriologist and that extended also to radiology with the physical side—Dr. Warren.

Dr. Stafford Warren.

Yes, he later became Dean of the University of California Medical School at Los Angeles and medical director of the Atomic Energy Commission during the building of the bomb. At Rochester he was interested in the heat treatment of gonococcal infections. The idea was that if they knew what temperature would kill the gonococcus and artifically by electrical short waves, probably raise the temperature of the body to an abnormal fever, the idea was that you would kill the gonococcus in that way. The same thing was used for syphilis infections. That required bacteriological backing, so I developed a relation with the radiology department in that way. They had a person who became quite distinguished in that line—in the study of the gonococcus. I think his name was Carpenter.

C. M. Carpenter, isn't it? But as the work increased this provision for assistantships allowed the Department of Bacteriology to have contact with medicine, surgery, pathology and so on which was good. You didn't run into any difficulties in establishing the assistantships?

No, the salaries weren't very high, and the place was attractive. There were lots of other benefits from the life there, the character of the young faculty that attracted people. I don't recall anybody declining anything.

No. You had a succession of people who really have gone on--Dr. Johns S. Cunningham, Dr. Richard P. Howard, Dr. Donald S. Martin. These were assistants for a year in autopsy bacteriology.

They've all had interesting careers.

When they set up the Dental Research Fellowship Program was that attached to the Department of Bacteriology?

No.

Basil B. Bibby--did he come in?

Yes, Basil Bibby is still highly influential in the dental educational field. I think Bibby was attached--well, that was centered in Dr. Bloor's department, biochemistry. That didn't matter.

No, except that there are publications of papers by Dr. Philip Jay.

Jay was on my staff, and Bibby worked with me too. He did a good deal of bacteriology. He was an Australian.

Yes. What was the burgeoning interest in parasitology that you had?

That was a life-long interest with me. I told you about my grandfather and the malarial parasites. I had seen worms and parasites in my earlier days in New Orleans, and I was very much interested in them through my medical course. I had the thrilling experiences in parasitology clinically in Panama when I was down there with General Gorgas, and it was a subject which at that time was not well developed in medical schools. I think that one had to do a good deal with parasites, protozoan parasites, but no formal provision had been made for it, and I didn't do it at first when I went to Rochester. After Chicago I saw the lack of it because I had a close association in Chicago with a great parasitologist, Taliaferro, and it seemed very natural to have a department that I soon didn't call bacteriology any more. I called it Microbiology. That's a term that has taken over a good deal. Now they have more Professors of Microbiology than they do of bacteriology, but that isn't broad enough. It is, in a way, but it embraces immunology also. They all go together.

Also having Dr. Oliver R. McCoy set up continuing relationship with the Gorgas Laboratory in Panama.

Yes. I don't know how long I've been a director of the Gorgas Memorial Institute, but it's been a long time. The Gorgas Memorial Institute it's called. I wasn't a director of the laboratory, but the Gorgas Memorial Institute is a going concern still.

I think McCoy was a consultant to the laboratory.

Yes, and McCoy then went over into the Rockefeller Foundation which had great interest in tropical medicine. Now, parasitology takes you into tropical medicine more rapidly than bacteriology does, because of malaria, filariasis, and all the worms.

The other day off the tape we talked about the Academy of Tropical Medicine, and this is on the way toward it. This is 1930, and its development is in 1934, so by 1934, there is pressure for a group, and this is partly due to interest and also possibly how to finance research in tropical medicine.

It's still the trouble right now.

So far as the department itself is concerned, I asked you yesterday, and I may have confused the issue about the relationship between the Department of Bacteriology and local physicians as to whether they possibly were invited in. We were talking at the time about the Public Health Laboratories, and I don't think that was possible, but does the name Dr. Paul W. Beaven....

Yes, Dr. Paul Beaven was a pediatrician in town, and he came to work there some time. He and I published a paper together on a peculiar acid fast organism that wasn't a tubercle bacillus, but it produced a sort of pneumonia in animals, ["Mycobacterium (Sp.?), Ryan Strain, Isolated from Pleural Exudate" 49 Journal of Infectious Diseases 399-419 (1931)]. It was in the sputum of a child, and I don't think I've ever found them again. He used to come in and actually do work on animals and cultures, but with all friendliness, his life at that time just showed how hard it would be for a practicing physician to do this work. He would come in, get everything out, start to work, the phone would ring, and he'd have to go. They didn't have any time for it. Some physicians can do it.

The man who could adjust life to let his research go on without interference from anything is George Whipple. By George, I never saw anybody--maybe Albert

Sabin is the same kind, but Dr. Whipple laid it down at the start that he was going ahead with his work on the blood forming factors, the work that led to his getting a Nobel Prize, though he never had that in his mind in the beginning. All that work on pigments, hemoglobin, and blood formation went right on through with the most laborious quantitative experiments with every little factor, and yet he had imagination in it too, but it seemed to me just like grinding, grinding, grinding. He was not diverted.

On the other hand, Dr. Whipple had plenty of play in him. He knew how to enjoy some of the finest things in life, aside from his very interesting family. Do you want to talk about him?

Yes.

Dr. Whipple managed to take holidays into the wilds. He was a great fisherman. He'd go up fishing for salmon in Nova Scotia. Every year he'd go out in the West. He was a great trout fisherman, and he'd fish in the trout streams in the West. In addition, he was a hunter better than Nimrod and being a parsimonious person, he made every pellet in his shot gun count. He never missed. He'd kill all the clay pigeons, and he never missed a bird. He formed a hunt club of Morton, myself, William S. McCann, and Warren. Dr. Whipple and Staff Warren, men about the same size, over six feet, never could miss anything. They could shoot pheasants and shoot doubles, hit one going North and turn around and hit another one going South. I had the privilege of hunting with these people, I think, largely because the law allowed three birds per person, and if they had somebody in the crowd who couldn't hit a bird, those three birds would be divided. That would let Warran and Whipple kill a bird and a half more.

Did you see among my medals the medal they gave me? They gave me a brass medal at the end for faithful attendance and ability in finding game, and this medal is stenciled with the letters Dr. Whipple used for his anemia dogs.

Well, we also had a little poker club which met once a week. We'd play for small stakes and drink a little beer which was very pleasant. Whipple enjoyed those things. He was a great photographer. He had an enormous collection of pictures. He was always snapping something, and although he seemed austere and strict in his administrative things, he was quite a natural human being.

Do you remember Leroy Garnsey? You had exclusive right to hunt on his patch of ground near Cayuga Lake about ten miles from Seneca Falls. It was apparently through Garnsey. Incidentally you wrote a marveleus letter about this. May I read it?

> The arduous shooting days are upon us, and I am stiff and sore from walking all day in snow and rain carrying a shot gun which never hits anything. We go pheasant hunting on four days of each year, the last two Thursdays in October and the first two Saturdays in November. One forced march is therefore, still ahead of me. I like it in spite of the pain that comes from unaccustomed walking from daylight to sunset, and in spite of the poor showing I make. At least I think I like it, though my hunting instinct is better developed for bugs than for birds.

Who is that to—Marian?

Yes, your sister.

Well, those were strenuous days. We'd get up before daybreak, drive about thirty miles and tramp around in the wet all day, and as I say, they put me down in the bushes to scare up the birds. Then you'd get home in the evening and you could hardly move.

There were a number of, I guess Eastman Kodak Company people who may have gone

<u>along--Frank Lovejoy.</u>

Mr. Lovejoy was a director of Kodak, but I never saw much of him.

<u>In the hunt club?</u>

I don't think so, but my bacteriology came to the fore in that connection too. It was prohibition in those times, and years before I had started to collect yeast. I pushed it along a little further then. I thought it might have some practical benefit. I collected all the beer yeast of the world--Löwenbrau, Pabst--all the yeast, so I became the faculty brewer, and in the basement of my house I brewed beer in forty gallon kegs and made wine. I discovered that there was more to beer than just the fermentation. It never got high alcoholic content, but it was easy to make. I got on a side issue to study hops, and people don't realize what hops are. Hops contain golden yellow, resinous, little globules in among the dried flowers. They contain an alkaloid called lupulin, and lupulin belongs in the morphine family. I didn't know that, but I thought that this gave the beer a flavor. I went and got the hops, and just picked hops that had the most of these golden droplets in them. I remember the first brew that I had of that. At our little poker party, it put everybody to sleep, particularly Nathaniel W. Faxon. He went first.

<u>I'm surprised that the brewing industry doesn't make more of that. Back in the Department there were fellows. There were the Fleischman fellows--Harold W. Pierce and Dr. Sara E. Branham. This is a relationship that goes outside the University?</u>

These fellowships were not settled in my department, but those people worked there. Pierce was a biochemist very much interested in the intestinal flora, particularly the bacterial flora of the large intestine. It was the smelliest piece of work that ever came into the laboratory. Poor Harold Pierce developed an extreme degree of arthritis, but he got his degree doing that, and his degree was largely based on a bacteriological study of the intestinal contents. He became a professor at Vermont.

Sara Branham was in the department for a while as a visitor, I think, and she was interested in the meningococcus. She became a great authority on the meningococcus and went from Rochester down here to the laboratories of the National Institute of Health where she stayed until she died, still working on the meningococcus.

<u>I indicated that these were Fleischman Fellows, and you indicated that that was some relationship that sprang up between the medical school. I think this was a grant.</u>

I don't remember that it even had that name attached.

<u>Well, there was an A.M.A. Fellow--Dr. Georges Knaysi.</u>

Well, Georges Knaysi was really in the Department of Agricultural Bacteriology at Cornell, and his son is still an eminent person down there, but he was very interested, and convinced me that a bacterium had a nucleus. It was not supposed to, because it was so small that you couldn't see it. Now, with the electron microscope they see the nucleus of the bacterium. Knaysi was not very close to us. It seems to me that he was a commuter. His real place of work was Cornell.

<u>Though provision was made for him in the laboratory for a period of time.</u>

Yes.

Maybe he was on part sabbatical--wanted to pick up some other information.

I think so.

In any event, these people at one time, or another were at work in the laboratory. Then there were a series of Rockefeller Fellows who came, I guess, under their international program--Dr. Istvan Bezi from Budapest.

Yes.

Dr. Alfredo Reda from the University of Philippines.

Yes, he worked on the cholera organism.

Dr. Armand Frappier from Montreal and Dr. Masao Nishio from Tokyo.

Nishio was a good friend, a rather polite man. We invited him to dinner one night at our house. He was a bit late, and I found out afterwards that he had forgotten about it, had eaten a full dinner out at the Strong Memorial Hospital and then hurriedly came in and ate another dinner with us without letting on.

Strange.

A very polite Japanese.

I gather initially that Mr. Flexner's notion was to build a new beacom for a new concept in medicine by the University of Rochester School of Medicine and Dentistry--you know, there were no vested interests in the way, and you could shape it from the beginning. Then I guess their international program--maybe Alan Gregg.

No, Gregg was just head of the medical section in the Rockefeller Foundation. Russell might have been the head of it.

F. F. Russell. In any event, you had a succession of these who came into the laboratory. Then there was a special student. Do you remember R. Gordon Douglas?

Yes, indeed, I do. Douglas was sent up to work with me from Cornell University New York Hospital Medical Center in 1931, I think, or 1932, somewhere like that because he was to be Assistant Professor of Obstetrics down there, and they weren't ready for him, so he spent a year with me, and he did a nice piece of work. Was it mercurochrome that he worked on? In any event, that started a friendship that still exists. Douglas succeeded Dr. Henricus J. Stander as the Professor of Obstetrics and made a marvelous record at the New York Hospital, reduced infant, newborn mortality down to almost nothing. If he lost one woman a year from obstetrical reasons, he was shocked. He had a beautiful record. They lost some women who had strokes, and things like that.

I don't think they opened the Strong Memorial Hospital until April 1, 1928, but you'd been in permanent quarters from 1925.

Yes, we started in the dog house up there. Whipple built a little structure two stories high, a hundred feet by a hundred feet, in which there was storage of all sorts of things, the heating plant, a room for me, a room for Bloor, a room for Whipple, and many rooms for his dogs. I don't remember whether McCann, or the others, worked in there or not, but that's where I worked for a couple of years.

Then you moved over into the....

The Main Building.

<u>The main building, but by the time they opened the hospital, you must have had quite a large development. You had--what was it?</u>

I had finally four wings of that building on the second floor on the Northeast side, and those wings were about thirty-five feet wide and a hundred feet long, so there was plenty of space.

<u>I think we've probably gone as long as we ought to go today.</u>

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Will it pick up your voices?

Yes, our voices will be all right. I'm just afraid that there may be an obligato--sort of a choir outside. As I indicated, I want you to go back. While we've sketched in the variety of experience that you had at Rochester, we didn't deal directly with the department, its teaching aspects and research aspects as they relate to yourself, Dr. Birkhaug, and Dr. Kennedy. I know that this is highly arbitrary. The papers indicate that Dr. Kennedy came by way of Dr. Zinsser, and I know that as of this time, as of 1931, you either had just reviewed the 7th edition, or there was correspondence coming on about the 8th edition. We're going to set these purge as though they had no other contaminants. I wondered about the Department of Bacteriology, where you had a chance to develop without any existing vested interest in the department. You could build it the way you wanted to. The search, I suspect, initially was for an assistant in the department when you think in terms of students. This takes us all the way back again to 1925. In thinking about your department, fresh, brand spanking new, and you wanted to continue with your own work, the search was for aid and assistants with respect to that laboratory and students. What--how do you search for an assistant? What was the process in 1925?

It's hard to recall all the details, but I can say that my attitude toward the department was formed by the actual obligations to the school and to the city. That determined a great deal--the obligations to the school and the hospital are to provide teaching for a second year class in bacteriology, to offer some support to the department of pathology in getting at the causes of the lesions that are disclosed by autopsy, and to give support to the clinical departments, either advisory support or actual facilities and assistance in working on their bacteriological problems. Then, of course, there is the large obligation to serve the Health Bureau of the City of Rochester.

Those were very large and pressing obligations which took precedence over at least my own work because, to tell you the truth, I have always put my efforts on my own investigations on a secondary relationship to these institutional obligations. That might be due to lack of comprehension of the subject, but I think it also was due to the fact that I did two things. One is that I found a great satisfaction in administrative work that led to results of a practical nature in the disclosure of the causes of conditions and in the disclosure of the nature of microorganisms and provided a satisfaction that repeated failures with the research undertakings denied me. I think that's been true of what I've done right along. It's far easier to be a Dean, I think, than it is to be a crackerjack investigator. Some men could do both like Dr. Whipple. Nothing deterred him from his investigations. A man like Albert Sabin, who is famous now for oral vaccination against poliomyelitis, is undeterred by any outside events from his laboratory and investigative work. Well, I never had either that courage, or that drive, or that confidence in myself to put that kind of work ahead of the administrative work.

Then another thing I did was that I felt toward the people that I was able to attract to the department that they should have quite a wide range of freedom in what they wanted to do. Some people who are heads of departments make all their people work on the same general line of problems. I suppose if I had a real clear conception of the general problem that ought to have been worked on, or that I would want to work on, I would have in the first place sought people who were interested in that subject and were willing to do the work without any compunction, or simply say, as some heads of departments say,"No, I will not support you, if you don't work on this phase of the problem."

There were two men who were important in this for me--Konrad Birkhaug and later James Kennedy. Birkhaug was a prima donna in investigation who worked very hard and with a good deal of imagination. He got into a little difficulty at the Hopkins because his imagination persuaded him sometimes that things that had not happened actually had happened. He was working on a serum for streptococcal infection by producing abscesses in a donkey, as I remember, and the result of that in relation to erysipelas. He had some difficulty having his work accepted at the Hopkins. I don't know whether that comes out in these papers or not. I thought that he was correct in his observations mostly, and although I didn't know him very well, when he came up for a position with me in the laboratory I was very glad to have him because he was an extremely interesting and able person, a very attractive man, a raconteur of the first order, a pianist, enormously energetic.

Artist and sculptor.

Yes, he had many--he was a sort of Benvenuto Cellini in a way. Well, when he came, he had a laboratory room quite as large as mine, a big room, and all the supplies, animals, and anything that we could give him, and he was allowed to go his own way. He and I talked over his problems all the time, and I could make some suggestions that helped along, but he was an investigator in his own right and pursued his problems very earnestly and got some very interesting results.

He was an excellent teacher, had great clarity of expression, and worked very hard on lectures and the preparation for his class. Laboratory preparation for bacteriology is an arduous task for each day's work, and Konrad would do it extremely well. He would arouse the interests of the students. He gave very finished and very fine lectures, almost too finished, not just repeating text-books, but they were precise and orderly and not as exciting as some disorderly presentations are where more argument can come about. Well, he worked there and had a great many outside interests. He got along among the bacteriologists.

Well, the time came when I was about to go in 1932, when they began to talk about my successor. I was not on any committee that had to do with choosing my successor, but I felt that I could not recommend Birkhaug, to the degree that he wanted to be recommended. His name, of course, was on the list, but I didn't push his candidacy any. That caused him to have a grievance against me. Dr. Hektoen also took part in that sort of personal controversy, and so did the university professors union, as we called it, the Association of American University Professors. It went on in a rather tense atmosphere of uncertainty for weeks and weeks, and finally they chose Dr. George Packer Berry to be my successor.

What was the nature of the misgivings? Was it the relationship to other departments?

Let me say I'm not so sure, and I'm dealing with a record here now that will be heard in the case of the character of another man, and I'd rather be careful about it. You can just say that he didn't impress them sufficiently favorably to be appointed the professor. I can think of many possible explanations, but I think it would be better not to put it on the tape, if you would agree.

The record is fairly extensive, but when the successor was finally chosen, the presentation was made that that successor ought to have a free field in which to operate. It seemed reasonable from the school's point of view.

That was reasonable from many points of view. Most of the faculty in that grade turn in their resignations when a new chief is coming on. Usually they're not accepted, but everybody feels that he ought to have a free hand.

Also the papers indicate that it was quite a problem in the school, dealing with this--that is, I think it was good that President Rush Rhees was there, and he handled it from the President's Office on a basis which could have been acceptable, except for the range of characteristics that Birkhaug had where he could misread a phrase, or convert the meaning of a phrase into that certainly not intended by Rush Rhees, although--let's face it. The problem was there, and it became a diplomatic thing, an inside public relations problem which was very difficult.

Well, it got so unhappy in our relationship that the friendship broke off then and never was renewed. He went abroad after a while. I think he was a prisoner of the Germans in Norway. He came back to this country after the war. In 1947, he came to work in New York City on BCG, and he wrote me a letter about that time wanting to let bygones be bygones, and I replied, and it may be in the folder there, that I just didn't see any way to do it. I have forgotten what I said.

Well, in 1939, at the Waldorf Astoria at the Congress of Microbiologists....

Yes, that's right.

There was apparently a scene down in the basement, or somewhere, and he apologizes for that in a letter in which he is quite contrite, not in keeping with the usual flavor of his correspondence.

Is the scene with me?

Yes. He comments to the effect that he had just been looking through a package of personal correspondence which the Gestapo had returned to him and that he came across some notes that you sent him in 1932--copies of which are in here, and these had to do with the action of the president, very good letters--and he says that they moved him deeply, that "I decided to write you a few words to express my sincere regrets about my bad behavior towards you both at the parting of our ways in 1932, and still worse toward you, B.J., at our meeting in the basement of the Waldorf Astoria Hotel on that Thursday evening, September 7, 1939, at the official banquet of the Microbiological Congress. It was stupid, very stupid of me, and I fail to find any excuse for my bad manners."

I have no recollection of what he's talking about there.

The comment I made before we turned this on was that the characteristics of the man were such that injuries were almost imagined, and an effort to steer carefully through his characteristics was a very difficult one for the University of Rochester, and it became more difficult for you because you were his chief. You understood the Rochester point of view, and also you bent over backwards--that's wrong; you didn't bend over backwards. You did it normally because that's the way you are--you wrote to I don't know how many places for suitable positions for him--four anyway that I can think of, so you recognized the difficulties from his point of view, but there was also the school to consider, and if he didn't have the flavor which in the judgment of his peers would carry on, or develop the Department of Bacteriology in relation to the other parts of the school, there wasn't any alternative. The difficulty did continue for a long time as a continuing troublesome spot, I suspect, from his point of view. He went to the Pasteur Institute. There are long letters not to you, but to Miss Creegan.

From Konrad.

Yes, comments on his expenditures, what it is he has left. This can be the sop to a personal cerberus as distinct from what is factually correct. This I don't know. I don't have any comment on it, except that these are the papers, but this is the first time you'd been in a ticklish spot like this that I can think of. I haven't run across another one like this in the files.

No, I never have had a similar one. I've had fist fights, arguments, but nothing like this, and this was very prolonged.

Did this effect his work while you were there?

No, as I recall, this was just sort of before I was going away--maybe from January to June, 1932, something like that.

It increased in intensity from the moment he received the letter from the president. He sort of read into it more than was intended. The students' reaction to him was favorable I gather.

Yes, he was very attractive to students, a lively person, essentially friendly, but apparently this cut him so deep that he suffered from it.

The other fellow who was in the department came as an assistant. He came, as I understand the correspondence, through Dr. Zinsser's Office--Dr. Kennedy.

As I remember, I wrote to Dr. Zinsser and asked him if he could help me find somebody who would be willing to be a very hard working assistant, and as I recall it, Dr. Zinsser said that most of the men who were trained by him had already been placed and that they were much in demand which is the truth. His was a popular laboratory for training people and a great source of supply of young men who were wanted at other places, but he recommended Jim Kennedy to me, and I liked him when I saw him. We had a frank talk about the kind of work that he would be asked to do, and as I look back on him now, he was most unselfish. There wasn't anything he wouldn't do, if you asked him to. I won't say that he was a slave, but he made himself so helpful and anticipated so many tiresome tasks that he used up all his time in that. I don't believe that he did any original research work the time that he was there. How long was he with me--two years? You said 1931.

<u>1931</u>.

Then when the successor was appointed, Dr. Berry, he did not ask him to stay, and Jim Kennedy then went down to be head of the Health Department Laboratory at Louisville, Kentucky.

<u>First he went to Georgia</u>.

Well, I've forgotten, but he's still down at Louisville. That was an ending of an association. We have corresponded a little bit since then, but I haven't seen him since those Rochester days. I hear about him, but we haven't exchanged any letters for a long time.

<u>There's one study that he had been doing in Zinsser's Laboratory with someone else up there at Harvard, and they were publishing a paper which was quite</u>

critical of a Dr. Coca.

Dr. Arthur F. Coca--yes, I knew him very well. He was an allergist and had some very peculiar notions about leukocyte counts and different conditions. Coca himself was a pianist subject to migraine headaches and dizzy spells, and I remember once he came to see me at my house and immediately had to lie down on the sofa. Nothing could be done until the next day almost because the poor man was suffering so much. Coca was connected, I think, too with the Lederle Company at Pearl River, just North of New York and was a man of rather strange characteristics and peculiar ideas, but a bright person who had a respectable standing among immunologists, but rather a crank.

I think the subject was the differentiation in the types of blood, or blood groups.

Yes, well he was interested in blood groups. He was early in blood typing. He was one of the original investigators on studies of transference of sensitivity by serum from one individual into another, and he put them--he called them atopins--put that serum into the other individual and the spot where you put the serum becomes sensitive to the thing that the man was sensitive to, whatever it be, pollen, or something else.

Dr. Kennedy and for the moment I can't remember who the other person was, the senior member.

Reuben Ottenburg.

No.

Well, probably the senior member would be responsible for that because I can't think of Jim's attacking anybody.

In the writing objections were raised. You know, you've said before that when you write a paper, someone else will read it and what they will look for is the manner in which their own work is treated, or whether it is treated.

That's right.

Apparently Dr. Coca's work was treated in this study and not treated in a manner in which he thought it deserved, and it raised again a kind of peripheral negotiating problem in order to get words that would convey what the authors intended and not to be too....

I have a vague recollection that Dr. Coca and Dr. Zinsser were at outs with one another in New York. Before Zinsser went to Harvard, he knew Coca in New York, but Coca was important. He was one of the early men who started blood banks which are very good.

After you removed yourself from Rochester, I think on your recommendation, Dr. Kennedy was placed in charge of the public health aspects of the laboratory as its administrator, and he continued there, but apparently couldn't--somehow, or someway--Dr. Berry was wholly different, and you know how those things start.

I don't believe that I'm doing any injustice to Jim Kennedy by saying that he didn't have the capacity to deal with public and general problems of the city Health Bureau Laboratories. He could do technical work of a high order, but the rest of the relationships were beyond him.

You know, when you leave a post--you saw Rochester from baby up through swaddling clothes into a burgeoning thing. You had more material to deal with

<u>and the relationships that you had were open both ways. I can't think of anything in the papers that would indicate that you had any difficulties along the line so far as the management of that laboratory is concerned vis-a-vis all the needs and demands that were placed on you whether they came from the school, or the hospital, or other laboratories, or other departments. You apparently had time to do this and it's in keeping with your views that this is the service, but any views with respect to your successor--I don't know that you expressed any. By this time the world had changed, and new things were on, but on the nomination of a successor which is going to alter the nature of this laboratory--it removes one variable and substitutes another, and everything and everybody is changed in the process.</u>

I didn't go to Washington for a year before I resigned from Rochester.

<u>You were there in 1932.</u>

That was a leave of absence from Yale University. I had already accepted the Yale position, and I went down and had been appointed chairman of the Division of Medical Science of the National Research Council.

<u>Let me put on another reel.</u>

All I can say is that I cannot recall any disagreement, or disfavor, or disorder in the relations I had in the school that caused me to leave it. As I remember, I had a vague sense that it was time to move on. It wasn't that this particular move in 1932, was financially advantageous to me. That wasn't the reason that I left at that time. Although I had a prospect at Yale of getting a larger salary than I had at Rochester, I was sorry to go. You always have mixed feelings about things like that, and when you try to think them over later on, you may make up your mind that one thing was the cause of it and another thing was a cause of it another time.

Do you want me to go on with the Research Council?

No. This is roughly 1931, and we ought to go back to June of 1930, when you had a visit from a classmate--Professor French. Isn't he a classmate?

Yes, Robert French--sure.

There's enough in here to indicate that the original suggestion with reference, I believe, to Trumbull College....

To be a Master at Yale.

Yes, dates back to June of 1930.

Robert French was Master of Jonathan Edwards College when the residential colleges were being built and opened up at Yale. What do you want me to say about that phase of it?

He seems to have been acting on his own behalf to interest you in coming as a Master of a college in the new educational developments at Yale.

I forget whether he talked to Winternitz first, or whether Dr. Winternitz talked to him first. Robert French was a Professor of English in Yale College and probably hardly knew that there was a Yale Medical School in existence there because Yale College was very self-centered and didn't care too much, or very much, for its professional schools, and indeed, to Robert French the very language of medicine was a jargon that offended his ears. I used to sit down with him and make him work out these words etymologically, and he had to admit that they were the most beautiful explicit words that he could find. He was a student of Chaucer, and these medical words are much better than Chaucer's words, some of them. He was a dear friend of mine, a member of the Bones Club I was in, and we were close together all the rest of the time we were at Yale and before I came to Yale--I mean, before I came there as a Master and then right on up to the time of his death.

Professor French seems to have been very much interested in having you come there as a Master representing science. Perhaps some notion as to the new educational system which was being installed at Yale is worth a word because when President James R. Angell writes to you, and I think at the behest of French, seldom have I seen such carte blanche in a letter.

Is that Angell's letter?

This is a copy of his letter--yes. Do you remember this one?

Yes.

The residential colleges were a graft of the Oxford-Cambridge system on to American Institutions, a model of these buldings even, and the system was what they thought the English colleges were. Mr. Charles Seymour who was provost at Yale, had been abroad and was a fellow of a college at Cambridge, I think, and had admired them very much. Actually the college plan was started because money was available. Mr. Edward S. Harkness offered Yale the money to set up colleges, and Mr. Angell and Seymour turned it down. The money went to Harvard and the Harvard residential houses as they were called, were built with a gift of about twenty million and were in operation when the Yale college plan was coming along, so that the Harvard example helped Yale to accept the gift when it was renewed. Yale set about in that time to build these extraordinary Gothic college house buildings. Several of the colleges are built in good Georgian brick, but one of them has Georgian brick on the inside of the court while the whole frontage on the street is Gothic--the same building. It was a synthetic thing, and the new buildings--the new things that have been done by Saarinen and others for the new colleges--show that they weren't permanently enamored of that kind of Gothic obscurity.

The plan of the colleges was to have about seventy, eighty, a hundred students living there in the college quadrangles and taking their meals there, but the colleges had no money and no faculty except attached fellows, had no set administrative duties, except to keep watch over the behavior of the students living within the confines of that particular college. These colleges were not like the English colleges in that respect because all the English colleges have endowments and financial management of their own and actually have curricular matters that they supervise. These colleges were to have consultative arrangements between student and faculty. It's been changed a good deal since then and in recent years, which I won't go into, except to say they've got assistant deans living in the colleges now, and the colleges are taking on more and more formal activities in the education of students.

Each college had a fine common room, fine dining hall, and all of us tried to build up libraries. We had some money to buy books, and each college developed a library along the lines of interest mostly of the Master. One phase of money that came in there which was very useful and excellent for both student and faculty to support was what was called a Bursary System. Mr. Harkness and some others had left money to give about 1900 dollars a year to each college and with that the college could employ, or arrange for the employment of students, provided they did not assign them to menial tasks like waiting on table, things like that, so we developed a system by which students became research assistants and literary assistants to members of the faculty mostly. It worked very well.

Now to go back to myself--you say I have a "carte blanche" there from Mr. Angell. With all due respect to Mr. Angell who was a friend of mine and who passed on some time ago, I doubt if he knew what he was writing. I think he said in that letter that he wanted me there to represent science in the colleges and that I would find--I think he says this--conditions for my work as favorable as I would find anywhere else. Doesn't he say that?

"We feel equally certain that we can promise you opportunities for fruitful work in your own line that you would recognize as fully meeting your requirements." That's pretty vague.

That's pretty vague, and that's pretty big.

Yes.

I don't believe I took that too seriously; in fact, I never pressed it very far after I got there. I'll have to go back to the background of that to explain my own behavior and his. In the first place I had no illusions about my representing science in the colleges. I was a non-mathematical biologist, as I have explained before, and Yale was famous for people like Benjamin Silliman in chemistry, and Willard Gibbs with the phase rule and physics on a high plane, mathematics of great intricacy and power, so I had no false ideas about my representing science in the place. I could represent a point of view of respect for the experimental approach to problems and, in essence, the so-called experimental method. The scientific point of view was probably common to all those other disciplines. Although you might not have a capacity to work in astronomy because you happen to be a bacteriologist, you can have a high regard for astronomy. They talk the same language when it comes to assessing observations and looking for the things you would undertake to test a theory and maybe alter your hypothesis according to the new findings. All that was common to both.

Before I got to Yale and before I talked with Dr. Winternitz, I had been dealing with the bacteriophage claims of Dr. Felix d'Herelle, the great discoverer of bacteriophage, who was brought to Yale from some place in the region of the Caspian Sea--I think he had a laboratory out there through the Pasteur Institute. He not only made this extraordinary observation of the ability of this virus like material to get inside of a bacterial cell and reproduce itself in enormous numbers and destroy the cell, a most amazing phenomenon that turned out to be one of the most important discoveries biologically of the present time because it takes into the study of self-replicating material--such as DNA. That's what the bacteriophage puts out into the cell.

Well, anyhow, the Council on Pharmacy and Chemistry of the American Medical Association of which council I was a member, spent a lot of time examining the claims of Dr. d'Herelle--so-called bacteriophage therapeutic claims and preventive claims. He had a rather extraordinary notion that health could be contageous because if bacteriophage destroys microorganisms and you can put the bacteriophage in the air and get it in your body, and it is in your body, occurs in the intestine all the time and in secretions, you could infect people with something that would preserve their health. Then he thought it was an extraordinarily valuable agent for the treatment of the urinary tract infections because it dissolves colon bacilli and proteus bacilli. He made some extraordinary claims for its ability to cure disease. I used to talk to Dr. Winternitz about that before I had any notions of going to Yale and before I had a full appreciation of the position of Dr. d'Herelle at Yale.

Dr. d'Herelle had an Associate Professor's position in the Department of Bacteriology in the Yale Medical School, and there were two full professors there also--Leo Rettger and George H. Smith. George Smith, the head of the department on the medical side, was the great admirer of d'Herelle and had him close in his laboratory in close association. Dr. Leo Rettger was a general bacteriologist, more interested in the production of sour milk because he thought he had an organism that would produce long life like the Bulgarians had--*Bacillus acidophilus* it was called. He made acidophilus milk and drank it and sold it. Then he was interested in the biological characteristics of bacteria, all the sides of bacteriology that do not necessarily have any connection with medicine. Well, to fit me into that laboratory they had to find space which had not been provided when Mr. Angell wrote his letter, and I managed, with Dr. Winternitz's help, or Dr. Winternitz's influence, to get three fair sized rooms and an office in a wing across from Dr. Smith and Dr. Rettger in the same building. These rooms were unfitted for bacteriology, and no provision had been made for equipment. I was given some equipment in the place, and at that time Dr. Winternitz secured the resignation of Dr. d'Herelle, and I inherited a good deal of his bacteriological equipment. Also I was able to buy some for myself, but I was off there, rather isolated in a sense ostracized, and I had no assistant at that time. Later Dr. Monroe Eaton came from Dr. Zinsser's place--Monroe D. Eaton is a very able, original investigator, and he did a great deal of good work there on diphtheria toxin and tetanus toxin. Both Monroe Eaton and I taught graduate students.

Well, that was not like what Mr. Angell had written me. I really suffered from having been in the position, or been put in the position, or maybe I worked myself into the position, of displacing Dr. d'Herelle from a place where he was congenially located and admired. Anyhow, I got to work there and started some research of my own on tetanus toxin, its method of transmission by nerves, and some general metabolic studies on bacteria. Then I began doing the odd sort of things that I had always been doing, working with the department to study organisms in various lesions--I did surgical bacteriology, autopsy bacteriology, and I assisted the pediatrics department in some things. I didn't have anything much to do with medicine because the head of medicine, Dr. Francis G. Blake, was himself an expert bacteriologist and interested deeply in infectious diseases.

Now, going back on the college side, I have written this in a report long ago to Yale--not to their liking--but I found soon that this plan of having a Master of a college subject to academic committee meetings and so forth and subject to consultation at any time by any student in the place, and in a place that was run with a sort of boy scout type of idea of association between young men and the professor, so to speak, wouldn't work for a man who had to work in the laboratory. This is actually what I went through. I would plan an experiment that would take from eight to ten hours--that's rather a normal working day in a laboratory. I would no sooner get over in the laboratory than I'd get a call to come over to a meeting in the President's Office, or some faculty, or you'd have all sorts of events in the college for which you were responsible, social events, athletic events. Each college had teams. Each college had a recreation program for which the Master was responsible, and our house was full of students all the time. Mrs. Bayne-Jones was extraordinary in the grace and constancy with which she watched after the needs of the students and provided all sorts of entertainment and company for them. She had a very great ability for remembering their names on first hearing them which I never could do. Sometimes there would be fifty of them in the Master's parlor, and she'd introduce everybody. I don't see how she did it, but it was wonderful.

Well, that went on for some time, and this I can tell truthfully--I won't say any man, but particularly one like myself, who has always been impressed by the institutional obligations, will go down under that arrangement that I have just described. You soon realize that you can't work ten hours in a laboratory on a set of experiments. You cut the protocols and the plans down to eight hours, and then you cut them down, or at least I cut them down to six hours. Then I tried to devise things which I could work in a few hours a day and pick them up on another day, and sometimes you wouldn't get back to them on the next day. The laboratory was clear across town for one thing. Ultimately the system defeated itself as far as having science in the college goes. I talked to them about this, but I think you can understand what I'm trying to say--particularly a man constructed as I am would give way under that arrangement. I have always thought that if I really had had the faith in my ability to do important research, I wouldn't have gone down under it, I'm sure. The combination of these circumstances resulted in my almost doing nothing in the laboratory of my own, but I did everything I could to see that Monroe Eaton had all the supplies that he wanted, and he could work all day and all night. That's the way it went from the time I was there until 1935, when I became a Dean.

<u>The correspondence about the original position is quite protracted--that is, you were still in Rochester, and Professor French who headed one of the colleges was very much interested in having you come as a head of a college, but you raised a whole series of--well, quoting a sentence from Mr. Angell to Provost Seymour, "The nature of requirements". One of these was appointment as Professor</u>

of Bacteriology in the Yale Medical School.

That they did.

A laboratory consisting of...., appointment on the faculty of the medical school.

That they did.

Yes, the initial equipment of the laboratory to be provided outside of the budget of the laboratory, and then some budgetary secretarial service. The teaching and administrative duties were left pretty hazy because they had to wait until facts hit you in the face, but apparently they were very much interested for their purposes to meet your requirements as best they could because they wanted science to be represented in the new educational system which overlooked, as you've pointed out, your necessities for doing some work. This is long and protracted.

Did it start in 1931?

It started, as I understand it from the correspondence--the first idea is broached to you on a visit by Professor French in June, 1930--he says later that your coming to Yale as Master goes back to a conversation he had with you in June of 1930.

It all seemed very attractive and very exciting and new.

But then--I gather that the college wasn't ready to receive you in 1931.

The college buildings in which Trumbull College is located are reorganized and renovated buildings of the Sterling Memorial Quadrangle. The money from Trumbull College came from Mr. Sterling's gift of land that was down by Greenwich, Connecticut. It was not as richly and easily set up as Pierson, Jonqthan

Edwards, and all those new colleges. Mrs. Bayne-Jones and I worked at Rochester even with the help of Mr. Eastman on revising the plans for the Master's House at Trumbull College which was sandwiched in between three great, tall, five story, stone dormitories. We did set a house in there that was a very fine house, but it wasn't ready in 1932, and I accepted this appointment as chairman of the Division of Medical Science of the National Research Council for one year. Shall I go on with that?

Yes, because you leave Rochester--you continued there for a period of time until they settled the replacement, maybe through June of 1931. Does that sound right?

Yes.

Then you went off to Washington, though your appointment begins at Yale as of that period.

Yes.

You were on whatever it is--sabbatical, or leave of absence. In any event, there was agreement with Yale that you do this. The Research Council has to do with Ludwig Hektoen, doesn't it?

Yes. The National Research Council is a division, or a subordinate part of the National Academy of Sciences, and it was set up in 1915, by the National Academy and modified in 1919, in accordance with an order of President Wilson who wanted to bring to the aid of the government special scientific research on problems of importance to the government in any way at all, and the National Research Council was set up as such, and it contained a number of sub-divisions, like a Division of Biology and Chemistry, a Division of Engineering, a Division of Medical Sciences, and I was chairman of the Division of Medical Sciences. Dr. Hektoen was the chairman of the National Research Council as a whole for a while. Then Dr. Hewell succeeded him--William H. Howell. At that time, the council suddenly got poor, so that the salary they had agreed to pay me was cut in half.

That's a fine memory!

Is that right?

Yes.

So we came down here and got a little apartment over on Foggy Bottom, and I had, I thought, a very busy time with important things, but when I saw what happened in World War II, when the National Research Council and the Medical Division were dealing in millions of dollars, the small budgets that were considered at the time I was the chairman were just "chicken feed" as Dr. Winternitz told me later. Although it was "chicken feed", I like to tell what happened with me and Dr. Hewell. A remarkable Russian-American scientist named Selman Waksman--do you know who he was?

New Jersey--agricultural, soil....

Streptomycin. This is the Division of Medical Sciences. One day when I was sitting in my office a short, dark, bushy headed man with rather heavy features and a kindled eye came in and said that he wanted a grant of twetny-five hundred dollars to help him find out what kept the streptococci in the soil in balance with the fungus elements. He was interested in the ecology of the organism in the soil which is a profound ecological problem. The relationships of organisms in the soil, in the air, in bodies is intricate and extremely interesting. Any one of them has an effect on the others, and they generally seem to be in balance. If they aren't, then out goes the other, but what had that to do with medicine?

Well, it did interest me because I could see from some past interests that this was in a biological line that I would like to see worked on, so I took him down the hall to see Dr. Howell, and Dr. Howell was a very broad-minded man. He had been the Professor of Physiology with whom I had worked at Hopkins in the early times, and he thought that Waksman's idea was very interesting too, so he approved giving Selman Waksman a grant to study why the streptococci and the fungus forms were in balance. Out of that came streptomycin because streptomycin is a product of the streptothryx, a fungus that grows in the soil. You never know what will happen some times. Waksman out of that developed a great remedy for tuberculosis, a drug that is also important in the treatment of typhoid fever. Enormous royalties from it built his microbiological research laboratory at Rutgers.

<u>Was this the nature of the job--to sift possible support?</u>

Yes, we had a grant-in-aid program. We had a number of things, but not much money, not like nowadays.

<u>This is an earlier day, and in terms of your own experience it is related, in part, to how to support research, the Leprosy Foundation and later the Child's Fund. This is the first experience that you'd had.</u>

Yes, with either giving a grant, or refusing one.

<u>Right.</u>

We had a little money in the Research Council for making grants.

Did you also work at this time with Lafayette Mendel in the American Medical Association Committee--chemistry and pharmacy.

Not Lafayette Mendel so much. He was professor of biochemistry at Yale, but the head of the Committee on Pharmacy and Chemistry was Dr. Torwald Sollmon, the pharmacologist from Cleveland, from Western Reserve. I knew Dr. Mendel, and I worked under him as a student when I was an undergraduate at Yale, and I had known him off and on all the time.

Did the National Research Council task and the American Medical Association--were they parallel?

No.

They're not.

They had no connection.

Did you wear two caps?

I was a member of a committee--a review Committee on Pharmacy and Chemistry, and I was a member of other committees, but it had no organic connection with the National Research Council.

Were efforts made for legislation in terms of further support in this period on the National Research Council?

No, the legislation that changed and was responsible for the modern development of the National Research Council didn't come about until after World War II, when President Roosevelt and Vannevar Bush set up the national defense research agencies.

<u>The National Research Council in this period did have some legislative base, didn't it?</u>

Yes. By actions of the National Academy of Sciences in response to letters and an Executive Order of President Woodrow Wilson, during the period 1918-1919. It didn't have any new legislation. It got its funds very largely from granting organizations. For instance, one of the things we were studying while I was there was under Dr. William Charles White, a search for a substitute for morphine. It was supported by pharmaceutical people and by some philanthropic foundations. Another thing of very great importance was a Committee on Sex--all the biology of endocrines and hormones that come from organs of internal secretion, sex organs and so forth. Another big project that we had something to do with, and that I carried forward again at Yale, was Dr. Yerkes' study of the behavior of big apes, the chimpanzee in Florida. The studies on looking for a substitute for morphine were quite interesting, but turned into a whole lot of routine testing--the chemists could make compounds so much faster than the pharmacologists could test them out in animals that it just built up on the shelves an enormous supply of material. I enjoyed that year. That is also the year I helped Dr. Zinsser revise his book.

<u>Yes, I think it's probably time to go back and pick him up too. We said initially that so many of these things operate at the same time that it is hard to get the flavor of reaction as between one and another.</u>

Do you want to start on the book?

<u>You wrote a review of the 7th edition which is an interesting review. I don't know how you read, but your notes indicate that you did almost an atuopsy on that 7th edition.</u>

I told Dr. Zinsser that I was going to do this because Morris Fishbein invited me to do this. I gave Dr. Zinsser an outline of what I was going to say. The substance of what I was going to say about the 7th edition was that it was the best book for teaching that I had ever put my hands on because, as I wrote in the review, it had an overt error on each page, a slightly concealed error on each page, and a very subtle error on each page. You give the book to students, and a lot of them hand you back just what's on the page. You know that man is not thinking, is not bothered by the error. Another student is a little bit upset and confused, and he asks some questions. A third student will see all the errors, will work out the answers himself, and will have nothing more to do with the book. He is then emancipated from the tyranny of the printed page, and he's a good man. I put all that in the review and told Dr. Zinsser that it was going to be about like that, but he had forgotten that I was going to write the review. He had gone to Algiers to be with his friend Nicolle to work on typhus.

<u>Nicolle?</u>

Yes, Charles Jules Henri Nicolle--he was very fond of Nicolle. Nicolle's picture and profile and Rocketts are on the Typhus Commission Medal now.

Well, this review was published while Dr. Zinsser was away. It was published in the <u>Journal of Infectious Diseases</u>, I think, which was put out by the American Medical Association, and when Dr. Zinsser got back to this country, he got furious and thought his enemies in Chicago were after him. He wrote an indignant letter to Dr. Morris Fishbein, the editor of the Journal of the American Medical Association and other publications, and Fishbein referred it to Dr. Hektoen, and Dr. Hektoen told Dr. Zinsser that I had written it, so when Dr. Zinsser heard that from Dr. Hektoen he wrote me,"Oh, since you did it, it doesn't matter. It's too trivial."

He knew his book ought to be revised, and he tried over and over again to get somebody to help him. He asked me two or three times, and I said that I had too much to do and that I couldn't. I succumbed as usual. I was in his laboratory in Boston one day up at Harvard, and he got a telephone call--it was from somebody down somewhere in the South, I think, saying that he was sorry that he couldn't undertake to be co-author with him in revising the book, so Dr. Zinsser started walking around the room, saying,"What on earth am I going to do? How am I going to do this? If you don't help me, I don't know what I'm going to do."

So I said,"All right, I would."

I really undertook a piece of work then. I was then chairman of the Division of Medical Sciences of the National Research Council, and I suppose I didn't know what a labor this would be. I had a full time job at the National Research Council building, and I persuaded the librarian of the Surgeon General's Library, which was the forerunner of the National Library of Medicine, the old building down on 7th and Independence Avenue, to give me a corner back in the stacks, a table, and stack privileges, so I could run down there at all sorts of odd moments and in the evenings sometimes and work on the notes and things for this book. I was living right across the street from the Navy Hospital. Back of that was the Hygienic Laboratory, the forerunner of the National Institutes of Health, and Dr. George W. McCoy was the head of that and a friend of mine, and he gave me free run of this library, so with those two libraries and putting in all the time I could find anywhere, I managed to write in long hand in that year, I think, more words than are in the Bible. I think I wrote seven hundred thousand words in long hand which was a great labor.

I found that the book really needed a thorough overhauling to bring in the modern ideas about bacterial variation, the morphological variations as well as the cultural variations, and the host variations. That had not been in Dr. Zinsser's thinking very much to that time because he was strictly a monomorphist, as we call them, a pupil of Robert Koch who thought that a bacillus was a rod, and a coccus was a sphere, and that a sphere could not be a rod and a rod could not be a sphere. Well, there are all sorts of transitions between those two. One could be one one time and another time the other one. The power of the language is so great, and I'll give you an example at that time. There were two important organisms. One is called Bacillus abortus which causes abortion in cattle and undulent fever in humans. It was discovered by B. Bang in Norway, or Denmark. It's a very serious infection for humans as well as cattle, a long chronic thing, abscesses and troubles. A very similar kind of a disease, Malta fever, is produced by an organism known as Micrococcus melitensis, discovered in the Island of Malta, and these two diseases were treated separately and described separately. The organisms were described separately because a coccus couldn't be a bacillus, and a bacillus couldn't be a coccus. They are as close together as brother and sister. They have cross immune reactions. They have many cultural reactions in common. They belong on the same stem and after they were put together by Alice Evans, Karl Meyer and others about this time [in the 1930s] it was a great clarification. Well, that was going on, and it runs through the revision of this book in many ways. The same with diphtheria bacillus, tetanus--everyone of them varies just like human beings.

Well, Dr. Zinsser worked hard on this revision too, and I worked as hard as I could with very little sleep, aided in wakefulness by giving myself light potations of stuff that caused intense indigestion, and that disturbed me so much that I could sit up and work--mostly hard boiled eggs. Then I didn't want to have any difference with my dear friend, Dr. Zinsser, who was high strung. I told you this anecdote. Shall I put it in here?

Yes.

About the carbon copies?

Put it in.

I invented some fictitious bacteriologists and would type off a letter with a carbon copy and send it to Dr. Zinsser. I would say that this man had written to me and said,"You're revising this book, and I want you to notice that the definition of the diphtheria antitoxin unit, for example, is entirely incorrect."

I would send that to Dr. Zinsser, things like that, and Dr. Zinsser had a great big black pencil. I signed these letter like J. P. Squill, Keukuk, Iowa, and it would come back to me with a big pencil mark on it,"Squill is an ass."

I'd write him another one and finally break him down. We never had any disagreements, or fuss about it.

Was he open to conviction on variability?

Oh yes, he took it up strongly after that, and he worked on variable organisms, the Rickettsia of typhus. He contributed, I think, the best chapters in that book. A chapter on tuberculosis, a chapter on typhus fever, and most of the chapter on encephalitis have a touch of the master that Zinsser was in that phase of it.

Most of this is carried on by correspondence.

Yes, we didn't see each other very much. I would take manuscript to meet him either in Boston, or New York some week-ends, and he'd drop in.

Was he a person who dispatched business this way?

Oh yes, he was a very facile person, clear headed. This was done in a year, and I think that 8th edition--what is the date on that? 1933.

1933.

It was about a thousand pages, and I added a chapter on the history of bacteriology that hadn't been very good in the previous work. As far as using the nom de plume, Dr. Zinsser could understand that because he wrote sonnets and other things and signed it Rudolph Schmidt. Nobody knew who that was. He never told. I'm not sure I know yet, but I think his mother was Schmidt. I don't know.

I don't know either.

But you saw it in these sonnets, or in As I Remember.

Yes.

Every now and then he'd fall into that third person, that fictitious character.

The papers are filled with clarity about the nature of the contract, due regard to the Hiss family. He's very gentle about this in not wanting to....

You mean, when he divided royalties with me?

Yes.

We did all right on that 8th edition. I don't know whether the accounts are in these papers or not. Maybe I had them in another book. That edition sold for eight dollars a copy, and we sold forty thousand copies, I think, but I don't believe the royalties were fifty-fifty. Were they?

No.

He had two-thirds.

Whatever obtained in terms of the contract with you was fifty-fifty. There was an initial period in which someone else had written a section.

Ottenberg--no.

No, someone up there at Harvard, and initially he was sharing something with him, [Ernest E. Tyzzer]. The initial person was Philip H. Hiss.

Yes. Hiss. The time came when I wanted to turn it over to David T. Smith and Norman F. Conant. Did you notice the dedication in that?

No.

Look at that! You'd think I was dead. It puts me along with Hiss and Zinsser both of whom are dead, and I'm next. Is this going on there too?

Yes--"To the memory of." Some of the comments received after publication are quite illuminating. A lot of people noticed that the book had been really revised, and you were revising this right up to the moment of publication, as the galleys came in and new work came in. You wanted to make sure. People would send you this, and you wanted to make sure that this new viewpoint was incorporated in the textbook. That's an endless kind of thing, isn't it?

Yes it is. You start to revise in one place and the domino falls down somewhere else. We had to watch that. In the war this book was sold to the Army. There are copies in all the laboratories in the Army. I was on the book selection committee for the Surgeon General—General Hugh Morgan and I and several others, and after a while—I don't think I was really afraid of "conflict of interest" because there wasn't any book like it. They could not have avoided it. I'm not saying that boastfully, but there wasn't any other textbook like this at that time—full of practical directions as well as philosophical discussion. Just toward the end of the war, I got Appleton and Company to figure out just how much of my royalties had come from the books that were purchased by the Army, and I sent then a sizable check to the Treasurer of the United States. It didn't do the Surgeon General any good. It goes into the Treasury when you pay back the government. I felt good about it.

The other moral problem I had with the book during the war came through my administration of the Army Epidemiological Board. I was in closest contact with all the original main investigators of infectious disease and bacteriologists in the United States for four years. I knew what Dr. Sabin, Dr. Rivers, Dochez, Avery—all the new things that were in their minds and what they were doing. Toward the end of the war Appleton wanted me to put out another revision, a book, and I didn't see how I could do that without disclosing what I knew from the unpublished reports of these other workers, my colleagues, so I decided I wouldn't do that. I could have had a scoop. I had the run of that

It's all in the files. I told Mrs. Zinsser about it, and she didn't object particularly, and Dr. Smith and Dr. Conant at Duke took it on, and they put out a good edition. Since then it's now reached the 12th edition.

<u>This is a period also for purposes of understanding and writing, of really climbing on top of the field. When you put pencil to paper, you begin to discover what it is you don't know.</u>

Oh yes.

<u>And roaming through the Surgeon General's Library and the Hygienic Laboratory may have afforded you your first over all view.</u>

By that time I didn't have to roam too much. I knew pretty well who was doing what and where they were being published. In that field there were excellent indexes, journals, abstracts of bacteriology, chemical abstracts and <u>Index Medicus</u>.

<u>But it had been a fast moving field from about the early 1920s.</u>

Oh yes--all the virus work came in. I want to tell you something about Berry and the biological slant that we were talking about a few minutes ago. I hope in your transcription of this you can bring it in in the proper place. Berry had discovered something very close to what Avery, Colin MacLeod, and Maclyn McCarty had called a transforming factor--that is, they had found that they could grow a pneumococcus type 2 in a pneumococcus type 3 medium where the type 3 had grown, and the quality of the type 3 would go over into the type 2 form and remain--acquired characteristics. Well, now we know that that is a transference from these nucleic acids. Berry had done the same thing with a virus that causes papillomas and a viscous kind of degeneration of cells. He transformed the viruses by letting them work together in an infected animal. He was interested in that fundamental thing too, so he wasn't just clinical.

Oh no--anything that I said that would imply that, and this was off the tape, was in error. Although he assumed the burden of the administration of the Department of Bacteriology at Rochester, he continued with his researches in the virus field.

Oh yes, and he's been a very fine Dean at Harvard.

Well, I think that we've gone as far as we ought to go today.

Wednesday, May 4, 1966 A-60, N. L. M.

I've already indicated that my interest today is to go back from the vantage point of Rochester all the way into the 1920s and deal with your work in the field of bacteriology. I see certain things which when I mentioned them, I'm sure, weren't in your mind at the time you did them, but taken together they lend themselves to a kind of organization which may be wholly unreal, but it looks that way to someone like myself who goes through the papers. Some of them we have already talked about--the early episodic days, but there is a growing awareness on your part for the need for a basis in parasitology among other things. I don't know its origins. Maybe you'd care to comment on that, and there is this trip to Cuba which I think you ought to put in.

Well, in Rochester there was very little "material", as we called it, in helminthology and protozology. Infections with protozoa and helminths occur more in the southern tropical regions than they do in a salubrious, somewhat northern region like Rochester, but it was necessary, even without that current supply of material from the inhabitants in the district, to teach students about the existence and characteristics of these organisms. Students need to be broadly educated no matter where they are and they need to be specifically educated if they go into certain regions where these organisms occur in greater abundance. There was no collection of slides of protozoa and the eggs of intestinal worms in Rochester, and it's necessary to have a collection of slides that can be used over and over again that contain stained malarial parasites, stained amebae, stained babesia, stained spirilla. You need to have bottles and jars of feces suspended in formaldehyde solution into which you can dip and and take out a drop and look at the eggs of a parasite, or a worm. These didn't exist in Rochester.

I think through my friend Dr. Harbert Charles Clark, Director of the Gorgas Memorial Laboratory who roamed around the Caribbean and knew that part of the country very well from Panama to all the Islands, I got an invitation to go down to Cuba in the region under the control of the United Fruit Company. Dr. Clark was the scientific adviser and medical manager, really, of the United Fruit Company in the tropics. The United Fruit Company was liberally interested in supporting studies on parasites because it kept their own laborers well, and it added to the increase in knowledge. They had some very fine people on their staff. I was invited to go to the United Fruit Company establishments in the eastern end of Cuba, in Oriente Province, which has the mountains in it from which Castro later came.

To get to Oriente Province I went through Havana because Havana still remembered Walter Reed and William Crawford Gorgas. There were still living people in Havana who had worked with the Reed Yellow Fever Commission in various ways, notably Aristides Agramonte who was very cordial and very nice to me. There was also, as I recall it, a Dr. W. H. Hoffmann in the Carlos Finlay Laboratory. Carlos Finlay was the great old man of the region who long before the discovery, or the proof that the Aedes aegypti mosquito--or the Culex fasciatus, as Finlay called it--was the insect vector of yellow fever virus, Finlay felt quite sure that that was the mosquito that carried the disease, but he never could prove it. He couldn't prove it because there were certain things that had to be observed before you could prove a successful experiment. There's always a period of about twelve days between the time when a mosquito bites a patient and when it's infectious. That period was noticed by Henry R. Carter in the South of the United States. He thought that there was some important biological reason for the lag between the first case of yellow fever in a community and the other cases, and that period of lag until the next case is the time for the virus to multiply in the mosquito and get up into the salivary glands where it can be injected into the next person. Also Finlay didn't know that there were certain periods in the infected individuals when the virus can be obtained from the blood and other periods when the individual is still very sick, that you can't get any virus at all. I don't think you can get it before the third day, and it only lasts for a short time.

Those things Walter Reed and his associates proved. Walter Reed was spurred on to do this work because in a sort of casual remark Walter Reed's attention to the virus possibility had been aroused by Dr. Welch who mentioned the work of Löffler on foot and mouth disease at that time. He told Walter Reed in essence that the reason you don't find anything that you can see in the yellow fever blood is that the agent may be a filterable virus, and that was what really turned the trick.

Well, I went to Cuba with a great load of paraphernalia for the collection of specimens to bring them back for the teaching of my class. This paraphernalia was largely some metal boxes full of bottles containing ten percent formaldehyde solution, so attractively put up that the Custom's Officer in Havana thought I was trying to smuggle drugs into the country, and I had a hard time getting through. He finally let me through. He didn't take the paraphernalia away from me. I saw Agramonte and some other people in Havana for the few days I was there. One of the advantages of that meeting was that Dr. Hoffmann gave me some eggs of the <u>Aedes</u> mosquito that they had carried on from the days of Carlos Finlay. I was interested in having these eggs, though I didn't know what to do with them. I dried them on a piece of paper, and I put them in an envelope and carried them back to Rochester. After these mosquito eggs had been for some months in the ice box in Rochester, I thought I'd bring them out, put them in some water, and see what would happen. Lo and behold, they developed into full fledged mosquitoes. I could watch the whole stage from the larva to the pupa to the adult. I kept a colony of these mosquitoes going for three years, or mo: studying the bionomics in an amateurish way. It was quite interesting.

At that time I had this stop motion picture camera with a microscope going and I photographed the whole life cycle of that mosquito from the egg, the hatching, the larval stage and the like; in fact, I got so that I could tell whe the changes were going to occur, when the larva was going to change into a pupa. I could dip him out with a little dropper--or when a pupa was going to give off a nymph. It changes color and activity. That film, I think, was the first motion picture of a full life cycle of an insect of this microscopic size. I didn't do anything more with it, except publish it, and it got into the Eastman Kodak teaching film series with a little descriptive manual that I drew up for it.

These were colored films?

No, black and white.

I fed those mosquitoes on myself for about three years. I had them in long tall jars with water and grass in there, and I covered the jar with a silk stocking provided by my secretary. I would cut the feet off of the stocking and fifty or sixty mosquitoes would light and have a good feed. I could draw my arm out without killing them against the silk.

There is in the teacher's manual that went with the film, slides or pictures that indicated when they were feeding. Was this you?

Yes. I don't believe that anybody else cared to do that. I don't believe

I tried to get anybody. It's not comfortable, but it doesn't hurt too much. Well, to go back to Cuba.

<u>There's just one thing--Dr. Hoffmann speaks here in his letter of transmission experiments. You must have talked to him about it. There's some question about Africa. F. F. Russell had made a collection of American and African eggs about this time also. That's the International Rockefeller Foundation group. Dr. Hoffmann writes: "I always planned to make transmission experiments...."</u>

Hoffmann was planning this?

<u>Yes.</u>

He's rather mixed up in here because he's trying to do transmission experiments with the different spirochetes and to observe the development in the mosquito. He says that it is easy to do. This is in the period when yellow fever got off the track with Hideyo Noguchi. Do you want to go into that?

<u>Yes.</u>

Well, Noguchi, the supposedly great genius, was at the Rockefeller Institute about this time--or a little sooner, sometime before. I forget the dates, but it was earlier than this date in Hoffmann's letter. Noguchi had gone into Guayaquil after having studied the disease known as Weil's Disease, called hemorrhagic jaundice, and in Guayaquil he innoculated into guinea pigs some blood from a person supposed to have yellow fever, and he found this organism, a <u>Leptospira</u> which he called <u>Leptospira icterohemorrhagiae</u> in the blood of the guinea pig. This spiral, a spirochetal organism, is an extraordinaryly lovely thing to look at, bright, shining under the dark field, and it spins. It has two nicely curved ends, curved in opposite directions. Noguchi thought that was surely the cause of yellow fever, and it was heralded all over the world as the cause of yellow fever. Hoffmann here is speaking of transmitting *Leptospira icteroides* by mosquitoes, but he says, "I must say that I do not believe that the *Leptospira icteroides* is a specific germ."

There was a lot of skepticism of Noguchi's findings, but Noguchi had such strong backing from Flexner and was such a world renowned figure! He was in the height of fame--based very largely on incorrect work and curiously unrepeatable observations--and was the great authority on yellow fever, on polio, and sand-fly fevers. People began to attack him. Russell may have been looking for some of these things in Africa, but the other men--Adrian Stokes was one of the important ones opposing Noguchi and I, curiously enough, although I didn't know much about it, never believed in Noguchi's doing this. Noguchi made a vaccine with *leptospira* against yellow fever. My dear associate and assistant, Howard Cross, was drafted from the laboratory in Baltimore to the Rockefeller group studying yellow fever in Vera Cruz. They vaccinated him, and he hadn't been in Vera Cruz for two weeks--well, in a little over two weeks he died of yellow fever. That happened before I went to Rochester. There was much skepticism of Noguchi's work, but Noguchi didn't give in, although the criticism mounted, and people were not able to confirm his work. Some wise doctor in Ecquador said that Noguchi was dealing only with Weil's disease, this hemmorrhagic jaundice, but that comment made no impression on people.

Then Noguchi went back to the Gold Coast in Africa, and there he tried to repeat his own work. He couldn't do it, and there he died of yellow fever. The supposition is that he killed himself. Everything toppled down about that time on Noguchi's head. This happened a little earlier than this work of Hoffmann, but Hoffmann is naturally skeptical, as a lot of us were.

You didn't intend to obtain eggs this way?

This was an accident. Hoffmann said, "Do you want some eggs?"

I just said, "Yes."

I was interested in everything that would grow. I had never cultivated a mosquito, so to speak, and I was very interested in seeing all the forms and transitions.

You kept the colony alive for three years.

Yes, at least three years. Now do you want to go on to Oriente Province?

Yes.

I went out in Oriente Province, and they gave me a laboratory in a building. They let me roam the area to collect what I could, and I studied them as I collected them. There was a very interesting man there named Dr. Eugene R. Whitmore who was studying Black Water Fever which is a disease I got interested in in Panama, a complication of malaria. I had a very interesting sort of a laboratory in a building--only a table and a microscope. A friendly chameleon used to come and sit on the window screen and stay with me while I was working at the laboratory--good companionship. I would then go out in the country with my little bottles of formaldehyde and collect feces. Then with slides I would collect blood. I got all types of malaria parasite--the quartan form was to be found in the laborers there from Canton, and they have curiously enough very abundant infection with an intestinal worm called Clonorchis sinensis. I got a lot of rare specimens, hook worm, Trichuris, etc. All the worms were there and very nice collecting.

I think I stayed there about a month. I came back with my tin boxes and all my bottles full. I didn't want to risk any more custom's trouble. When I went through Santiago on the way back I was offered no end of very good rum, but I wouldn't take any because if I brought it in, the custom's fellow in New York would confiscate all my stuff if I tried to bring in any rum. That was a great disappointment because the Customs Officer in New York--I remember now I had a these boxes and bottles, and the Custom Inspector said, "I'm not going to look at those things. I know how hard you boys have to work down there in that heat I think you can go right through without any inspection."

I was held up for possible drug peddling going in, and I innocently kept myself from bringing back any rum on my return, but I had a fine collection for the teaching of a class.

<u>I wondered whether there was in existence at this time any regulations governing the introduction of this material.</u>

No, they were all dead. There are very strict regulations now. We had a great deal to do with it in the Army. We brought in infectious material from the Pacific region. There are rules. You have to get permission. The Public Health Service has rules about bringing in those things. There are some items you can't bring in. You can't bring in foot and mouth disease, or some of those other violent diseases of animals, but jumping ahead when we worked on biological warfare, we had some very secret stations where they had special permission, like an island in the St. Lawrence River, or an island off Montauk Point, where you could do some experimental work with live material that they wouldn't allow in the continental United States.

<u>This also illustrates the field trip approach too--that is, the relationship of the field to the laboratory and the necessity for going to the field.</u>

Oh yes--the whole thing is one piece of fabric. It's got different designs on it, but it's all connected with the strands.

I don't know that field work today is done to the same extent that it was then--like your trip to Cuba.

Oh yes. Field work is--well, with the Typhus Commission we had an enormous amount of field work, even epidemic and murine typhus and--do you mean for a professor to go to a place?

Yes.

Unless he has a project, he doesn't do much. These collections--well, everybody was collecting, and I suppose every decent school has a good collection. You have to have boxes and boxes of the same thing so that you can--well, the ideal is provide every student with a box of maybe thirty, or forty slides, and then you have an endless amount of material in jugs for worms and eggs, but they're all dead.

The results of this trip--I know it aided you in Rochester ultimately in terms of parasitology.

I was always interested in parasitology, as I told you before, through my grandfather and his early observations in malaria, the fact that malaria was common in New Orleans. I had seen parasites back as early as I can remember, so it seemed a natural part. This was an effort to increase the resources of my department, teaching a vital subject.

Ultimately you brought in a man to work in this field.

Oliver R. McCoy.

Where did you find him?

I'll have to look up the papers for the origin of that. I may have met McCoy at a meeting, or--wasn't McCoy connected with the Hopkins before he came to Rochester?

I think so.

I think that was the connection--the School of Hygiene, or something like that. He was a very bright young man, a very nice person, and we were very glad that he was willing to come. He had a deep interest in trichinosis at one time and the general infection of animals. Trichinosis occurs in the United States--people eating raw pork. It occurs very often in places where meat inspection was not sufficient to prevent the consumption of infected pig meat, but I don't remember the details of how McCoy came, unless the papers are in the departmental files.

It's not in the papers, and that's why I asked the question, save that he did a lot of work once he was set up and established in the laboratory. He published a lot of papers.

Oh yes.

And I think it was also a way whereby your department maintained continuing contact with the Gorgas Memorial Institute--through McCoy who was made a consultant.

Through McCoy too, but as I say, I was always connected with the Gorgas Memorial Institute.

Yes, surely. I wonder whether this Cuban trip and subsequent developments in parasitology are, in part, some of the seeds for the development of the Academy of Tropical Diseases too.

I'm sure it was. As I say, I have always been interested in tropical diseases because of my origins in New Orleans, my grandfather's interest and th occurrence there of both malaria and yellow fever. I was in the last yellow fever epidemic in New Orleans. In 1905, I was on my way to the Thacher School when the yellow fever broke out that summer. That was, I think the last yellow fever epidemic in New Orleans, but to get to the Thacher School I had to go through a very rigorous series of quarantine procedures because people were as alarmed of yellow fever then as they were back in the 1870s. Then /in 1905-190 the Texans set up a guard on the Louisiana border so that no trains, nor anybod could get through there. They'd stone them, or shoot them. I was put on a train in New Orleans and the doors of the day coach on which I was on my way to North Carolina were nailed shut, and screens nailed over the windows. I didn't get out of that coach until I got up to Sapphire, or somewhere in North Carolina where I had to wait a period before I could go on. I had to wait in quarantine to see if I had yellow fever. Then to get on to the Thacher School I had to go around by Chicago and down through Sante Fe, New Mexico and Pasadena and on to the town on the coast north of Pasadena--I've forgotten what it is now /Santa Barbara/, to Ojai. It was a long, round about trip.

Well, I never forgot that sort of live experience with yellow fever. Then too, it was very natural for me to talk to people about tropical diseases wherever I met anybody interested. When I came here in the National Research Council in 1932, I met Dr. Earl B. McKinley who was the Dean of the George Washington University School of Medicine. He had been in Puerto Rico, and he ha definite interests in tropical medicine. He started a move which became the Academy of Tropical Medicine. The idea was that we were going to collect twenty million dollars and have a great tropical medicine establishment in the United States, but it never got to that point. We got a charter, and I think I was a charter member of the organization--about 1933, wasn't it? I am an emeritus member still of the American Association of Tropical Medicine, and I know a good many people living--and many others who have gone on too--who are experts in that field.

I think, if I'm not mistaken, that the National Research Council sponsored a survey of tropical diseases back in those days.

Maybe so, but more recently still the National Research Council--I have a big book downstairs--had a commission through the Army Epidemiological Board and others to do something about tropical medicine in this country. After World War II, most of the medical schools and the universities dropped their courses in tropical medicine and weren't producing people who knew about it. It looked as if Tropical Medicine was in a parlous state. I don't think you want to go into that now.

No, except that one--well, in part, the basis for McKinley's drive for an association was this survey that he had made as of that time.

The one we made was in the late 1940s, in the 1950s. I have a huge copy of it downstairs [*Tropical Health, A Report on a Study of Needs and Resources* (Washington, 1962), 540.] I'll bring it up tomorrow, or I'll go get it now, if you want me to.

No, I was just thinking of tropical medicine in those times. Your own interest dates back quite deeply, but other people were becoming interested too, not merely for collections; in part, for collections of material--you know. Groups met--the American Association of Bacteriologists.

The American Association of Tropical Medicine was existing then too, and nearly all medical societies in the South had papers presented on some disease of a tropical, parasitic nature, and the American Medical Association, I think, had a section on it. They do now.

This is a phase of your own interest that is constant. Let's go back to your own laboratory, some overviews as to what you were up to. One is this thermochemical investigation of immunological reactions, a study you did 1920-1922, but you didn't publish this paper until you got to Rochester ["Heat from Reactions Between Antigens and Antibodies: Special Reference to Diphtheria Toxin and Antitoxin" 22 Proceedings Society for Experimental Biology and Medicine 246-248 (1925)].

I finished the work at Rochester that I dared to publish. I finished it in about 1923, 1924, I think, in the laboratory, in the animal house that was there. As I recall, it was tetanus toxin and antitoxin that I was mixing to see whether heat was produced. It did produce heat. The deeper interest in it was not simply observation of the production of the heat, but it was partly to see whether a differential microcalorimeter would work in such a situation. The deeper scientific part was that I had an idea that if I could measure the heat of the toxin-antitoxin reaction I could determine the molecular weight of tetanus toxin. I wrote a paper on it and gave a molecular weight that is much too low. I think that was the deep interest that I had in the problem.

This is adapting a piece of equipment to measurement--that is, Hill's.

Yes, A. V. Hill's differential microcalorimeter, and the interesting thing about that piece of equipment was that you had a means of measuring the heat in undeterminable mixtures in such a way that everything balances out, except the reaction that you're interested in, the reaction of the growth of bacteria, or the reaction between the toxin and antitoxin. You have two flasks. Everything in them is the same, except one of them will have the reaction which you're interested in. The differential part of the process simply means that whatever happens in these two flasks is related to the reaction in which you're interested. Everything else balances out.

Well, all sorts of things happened with that apparatus. I built the one I used which was rather difficult, and I learned so much that I can't call bacteriology. I learned all about specific heat. I learned about conductivity of heat, about temperature measurements, but the most fundamental thing I observed, I didn't pay any attention to--I found out that Faraday and others had done it. I had a very sensitive galvanometer which is an apparatus to measure electrical currents, a very fine instrument, and I had it attached to a long thin wire. It was up on the wall, and I had a wire coming down to the calorimeter on my laboratory table. I just casually picked up one of these wires one day, and I walked around the table where it was, twirling the wire a little bit, like a rope that you skip. I walked from North to South--or East to West, it doesn't matter--and while I did that, I happened to notice the mirror ~~of~~ of this galvanometer was deflecting a little bit to one direction as I walked around, we'll say, from North to South. When I came back, it was deflecting in the other direction. I was quite excited about that because I thought maybe my galvanometer was wrong, or else I was electrical, or something. It turned out that what I was actually doing was cutting the lines of magnetic force on the earth with a copper wire. This is an electrical motor, so I discovered, incidentally to a study of bacteria, the very fundamental thing that Faraday and others had discovered; that by whirling a coil in a magnetic field, you get a current, a modern generator. You never know when these things will turn up. I was a good many years too late on that one.

Part of the joy, I suspect, of working in a laboratory are the surprises that you bump into--aren't they?

Yes.

But you changed from this tetanus toxin antitoxin study to the study of growing bacteria itself. Part of the rationale in the article is the way in which physiologists generally have been using this approach and have been overlooking the bacterial aspects, ["Bacterial Calorimetry. I. General Considerations. Description of Differential Microcalorimeter" 17 Journal of Bacteriology 105-122 (February, 1929)].

I don't think that was any major thing in my case. I saw the application of that. The physiologists had been overlooking bacteria in everything they did to such an extent that my friend in Baltimore, Admont Clark, thought he had discovered what turned out to be insulin, until I showed him that he had a colon bacillus in his perfusion fluid. He had a big perfusion apparatus there in the laboratory, with a heart, a pumping heart, a perfused heart attached to a pancreas in this machine. He would perfuse it, pump a salt solution through there. He put some sugar, glucose, in salt solution, and it went through the pancreas and into the heart, and the glucose got used up. Well, of all the foods that are delectable to the colon bacillus is glucose. Admont Clark didn't realize that he had a contaminated, wonderfully vigorous culture of colon bacillus in his material that was using its glucose up. He thought that it was the internal secretion of the pancreas working through the heart muscle that was using the sugar. Actually though, he was ahead of Sir Frederick G. Banting and Charles H. Best, and if he could have carried that through clean before his death, which occurred about that time, he would have been ahead of Banting and Best in the discovery of insulin.

Is that going in here--all those voices?

I don't think this will pick it up.

Well, physiologists didn't know much about bacteria. They couldn't see them even when they're there by the billions, when the material they were working with got turpid. I was interested in the heat production of bacteria because there had been many observations on heat production. A great law of what's called thermogenesis is that bales of hay ferment and catch fire from the heat produced by the bacteria. Everybody knows that fermentation in jars of preserves is what makes a thing warm to the touch. These are very old observations. As I recall it, Otto Meyerhof, who was a great physiologist theorized on the heat production of bacteria as a measure of the metabolism of the organism. This is the same as human being heat production which is going on all the time, keeps our body temperature what it is, and is related to your metabolism. Nobody had really studied the relationship of heat production to the growth curve of bacteria, so I thought that I'd try to do it by getting a very sensitive method of measuring temperature, ["Bacterial Calorimetry. II. Relationship of Heat Production to Phases of Growth of Bacteria" 17 Journal of Bacteriology 123-140 (February, 1929)]. At the same time I had to devise a means of counting the

the number of bacteria present and relating the heat production to the growth curve. I think I had charts in that paper. I haven't seen them for a long time. That paper shows, I think, that the younger cultures of a bacteria--when they're in a very active growth, logarithmic phase--produce more heat than afterwards, though they continue to produce heat. I studied heat production in yeast. The yeast has a budding phase, and the cycle of heat production is more variable in yeast. It waxes and wanes, goes up and down. Well, that worked--that differential microcalorimeter. I did that work when--1929?

A little bit earlier.

I should have gone on with it, if I had been a good chemist, or a good biological chemist. I've often thought of the things I ought to have done.

This is a whole series of papers, one in particular with a woman--Rhees.

That's Dr. Rush Rhees's daughter. [Henrietta Rhees].

She got a master's degree.

She got a master's degree out of rat bite fever.

But she worked on this paper. You had general considerations as to what you were going to do and the first publication was this growth curve business. Later there appeared a note on the application of Buchanan's formula, a formula that you used in the paper.

Calculating growth rate.

Right--this note was on the application of that formula to heat production in bacterial culture by N. C. Wetzel who didn't particularly care for the mathematics of your paper.

Well, I had to get some help. Buchan's formula has an integral in it, as I remember, and I wasn't any good at calculus. I've forgotten who helped me.

His criticism was helpful, illuminating in terms of the formula used, but there were three aspects of this problem which you set aside in the original study with Henrietta Rhees, and they are the measurement of the sizes of bacteria, ["Growth in Size of Micro-Organisms Measured from Motion Pictures. I. Yeast, Saccharomyces cerevisiae" 1 Journal of Cellular and Comparative Physiology 387-407 (June, 1932); "Growth in Size of Micro-Organisms Measured from Motion Pictures. II. Bacillus megatherium" Ibid. 409-427 (June, 1932); "Growth in Size of Micro-Organisms Measured from Motion Pictures. III. Bacterium coli" 2 Journal of Cellular and Comparative Physiology 329-348 (December, 1932)], the determination of their surface area, and chemical studies of their metabolism. These were subsequently developed. They show a deeper concern for bacteria as bacteria quite apart from their application, clinical utility—a growing concern with bacteria.

Well, just the measurement of the heat of bacteria—I should have carried it further. For instance, the amount of the calories liberated by, say, the decomposition of a gram of glucose by yeast might be different from the same thing done by bacteria, but I didn't get down to it. If the heat liberated is different, it means that the basic chemical reaction is different. It would mean that the bacteria would have a different cycle going through the decomposition of glucose. If I had tackled it at that time, I would have come out with some simple business that would have been entirely wrong because now the cycle of carbohydrate metabolism in a body is more complicated than the orbits of the heavenly spheres. It's terrific!

The last paper you publish is on carbohydrates, and I think it's in this stream. I mentioned the factors that you set aside, measurement of the sizes. The problem is how do you measure the size, and that brings in another machine.

Yes, the sizes were measured with this time lapse photographic motion picture apparatus that Clifton Tuttle and I devised, ["An Apparatus for Motion Photomicrography of the Growth of Bacteria" 14 Journal of Bacteriology 157-170 (September, 1927)]. It really was a delicate apparatus because you could work at very high magnification. I think we let this machine go all day and all night with an oil immersion lens and with very little change in the focus. I had the whole apparatus enclosed in a carefully regulated temperature-controlled box so that the arms and metals of the microscope didn't expand, or contract and move the focus. I had five, or six thousand feet of film that all got burned up in my Master's House fire at Yale in 1933. I lost them all--all my books and everything else was burned up in that fire--just before we moved into the Master's House.

Before that, fortunately, Dr. Edward F. Adolph, the Associate Professor of Physiology, at Rochester, was a biologist, a mathematician, and interested in the growth of human beings as well as all growing things. He saw these motion pictures of growing bacteria and yeast, and we set about to measure them. What we did was to project the image on a screen and just measure the length and the breadth with a caliper. We had long tables of those measurements. Then there's the formula--from the length and the breadth you can calculate the volume and the surface. We did it with the bacteria, the yeast, the spore formation, ["Cytological Changes During the Formation of the Endospore in Bacillus megatherium" 25 Journal of Bacteriology 261-274 (March, 1933)_7, with bacteriophage and megatherium /"Changes in the Shape and Size of Bacterium coli and Bacillus megatherium under the influence of Bacteriophage - A Motion Photomicrographic Analysis of the Mechanism of Lysis" 57 Journal of Experimental Medicine 279-304 (February, 1933)_7. That's where, if I hadn't stopped about that time, I might have gone on to genetics because this paper on megatherium with bacteriophage was accepted finally by the Journal of Experimental Medicine just before I was going down to Washington with the National Research Council. Dr. Peyton Rous was the editor, and he saw these peculiar forms in the pictures and asked me why I hadn't gone on to follow those forms to see whether there was a genetic factor that was determining the variation. Well, I was finished with it by then, and had no more chance.

Yes, life got a little complex. There was a series of papers on the growth and the size--bacteria, yeast, megatherium, colon, endospore. That's a very interesting series because the publications contained slides to illustrate, and a visual representation is so much better.

Yes. These papers are still being noticed. Every now and then I find somebody reviewing the growth of microorganisms that cites these papers.

The last one I bumped into, a copy of which I don't have here--I couldn't find it. I'm not saying that it isn't here. It might be in some file I have yet to come to--and this was the effect of carbohydrates on bacterial growth and the development of infection--1936, /"The Effect of Carbohydrates on Bacterial Growth, and the Development of Infections" 12 Bulletin of the New York Academy of Medicine 278-284 (1936)_7.

That's a very small thing. That was at the New York Academy of Medicine. Dr. Herman Mosenthal invited me to do that. He was interested in diabetes, and people with diabetes and high blood sugar have endless boils. They can beat Job with their boils. The question presented was is high blood sugar related to susceptibility to bacterial, particularly staphylococcal, infection? My conclusion was that it wasn't, but I don't know that I ever did any serious work on the problem.

<u>I know that your own time was cut to pieces in terms of other work. As you put it, one time, you were sucked into the abyss of administration and away from the laboratory, but it seems to me that this series of papers does lend itself to a kind of continuum.</u>

I don't know that I used the word "sucked into". I think I just consciously stepped into it because, as I told you, I got satisfactions out of that kind of work that I thought I'd never get out of research work.

<u>I wondered what relationship there was between the work that you did at the bench and the judgments that you exercised at the National Research Council--any at all?</u>

Oh constantly. Otherwise, you couldn't deal with the problems of research projects, as we call them, when you're a director of a fund granting organization. You either have to know what the man is talking about from the first, or you have to know how to find out about it. The more basic experience you have, the less you have to look up.

<u>I wondered also about the time you spent as referee on the Council of Pharmacy and Chemistry of the American Medical Association.</u>

That was all educational. You don't get paid in a job like that, but you get a lot of information that is bound to be useful in similar situations.

<u>I wonder if it added anything in terms of your thinking about bacteria?</u>

It did because the kinds of things I did on the Council of Pharmacy and Chemistry often had to do with chemotherapy, like the chemotherapy of urinary tract infections, like bacteriophage, vaccines, things like that. It was all together. The Council on Pharmacy and Chemistry didn't put me on the problem of the chemistry of drugs because I wasn't a chemist.

<u>The letter I had reference to before we turned this machine on--I can't put my hands on it at the moment--is an indication that future development is in chemotherapy--that is, as of the time of the latter in your thinking.</u>

Yes, but, curiously enough, chemotherapy of bacteria at that time into the 1920s was thought to be a hopeless business. You wouldn't be able to find any drug that could kill a bacterium without killing the host. All of this is before antibiotics, before the sulphonamides. It was almost a dead end. The great therapeutic advance came at the time when Erlich developed salvarsan, the silver bullet, where he put the arsenic in combination with an organic radical. That looked like a great burst, the chemical cure of syphilis, and the thought was that a great many other things were going to be cured by newly discovered drugs, but the hopes were dashed. There was quite a period in there--from 1910 until later in the 1920s, or maybe even later in the 1930s--when the sulphonamides and penicillin came in, antibiotics and the sulphur drugs, much to the amazement of everybody.

I did have a connection at the Council of Pharmacy and Chemistry with claims with antiseptic substances. One of them was mercurochrome from the Abbott Laboratories. Mercurochrome was of sufficient interest to persuade us to work on it at Rochester. I got a grant from the Abbott Laboratories, and Birkhaug worked on mercurochrome. Douglas also worked on mercurochrome. Everybody was always testing things to see whether they would have any effect, but we didn't have anything like the huge screening programs that are being used now for cancer chemotherapy, testing everything, whether there is any rhyme, or reason for testing it or not, to see whether it will do anything.

<u>You make comments in letters I have read to other professors at Yale about the difficulties inherent in the whole process of purification, the kind of problem Eaton bumped into in an effort to purify toxins.</u>

Monroe Eaton came there with some interest in purification of toxins, but I was interested too, and I did everything I could to favor and support his work on attempts to purify the diphtheria toxin and tetanus toxin. The influence on Eaton, and on me too, came from a remarkable man who was on Dr. Zinsser's staff, J. Howard Mueller. Howard Mueller was a Ph D chemist in Zinsser's medical, bacteriological laboratory, and his line of work had been to purify the factors in peptone solutions and bacterial media that were responsible for stiumlating growth. He found copper compounds of great importance for stimulating growth. He was up in Zinsser's laboratory trying to purify growth factors in bacteriological media. Eaton was there, and I was in and out of that laboratory, so it was a subject in which we were interested. Eaton did very good work on the diphtheria toxin and the tetanus toxin.

<u>There are two other items about this time--one of them is this research study; Gonococcus and Gonococcal Infections.</u>

This was undertaken as a result of connections formed during the war time with preventive medicine in the Office of the Surgeon General, the Public Health Service, and the American Social Hygiene Association. We'd been talking about gonococcal infections very seriously ever since the venereal disease problem came into our field of preventive medicine during the war. At that time, the gonococcal infections were very prevalent. Later in World War II they found penicillin and the sulphonamides would take care of venereal diseases. There had always been a relation between the Social Hygiene Association and the Army Medical Service with venereal disease, and this published study is a part of the result of that association. Who supported this? Don't they acknowledge a grant here?

Well, we had a grant to do this work. It was necessary to bring together the knowledge of gonococcal infection, and this is not a laboratory study in any sense. This is a literary study.

<u>Right--a research study into the literature.</u>

Yes. We tried to assess the statements we made, and they were full of controversial points. This report has got a big bibliography.

If I could close that off. If I do that, will it hear? It won't cut off?

<u>No, it won't cut it off. Do you want me to turn it off?</u>

I was going to say something improper.

<u>In the 1920s--Tom Parran....</u>

Is that off?

<u>No. Tom Parran</u> [Dr. Thomas Parran, Surgeon General, U.S. Public Health Service]

was very much interested in venereal disease. He took several trips to Denmark to assess their experience and approach toward the problem. I don't remember whether he went further than Denmark--England, I believe--and then he set up a Cooperative Clinical Group outside of the Public Health Service with support from the Mayo Clinic, or some friend of the Mayo Clinic, to fund the collation of materials, studies. One of the laboratories was at Johns Hopkins. There were others. I think there were five or six other laboratories involved in this cooperative study working in this field, with case histories and the like. It was a continuing problem, and it may well have been the Social Hygiene Association that supported this pulling together of information.

Yes, the Social Hygiene Association--we had most of our meetings down in the office. Walter Clarke was medical director, or director of the medical activities of the American Social Hygiene Association and secretary of this committee. I see here that the National Research Council gave a thousand dollars for the support of this work. I think that was a big sum in those days. We started to meet in New York in 1933, and we had most of our meetings in the offices of the American Social Hygiene Association. It had very high purposes. I never cared very much for the point of view of either Dr. William F. Snow, or Dr. Walter Clarke in these matters because they were sentimental, emotional and not really scientific, and this was an attempt to bring scientific thinking into their affairs, and if you'll turn that off, I'll tell you what I said.

What interested me was that with all the other things you were doing, like the revision of Zinsser's book....

I'd finished the revision. This is 1933.

But the galleys were coming in.

Yes, and I was Master of Trumbull College. I became Dean of the School of Medicine at Yale in 1935, so this overlapped a good many things. Putting this report together was hard work and took a lot of time. In the course of it I got German measles, and I didn't know I had it. I was working late at night and didn't observe my body particularly even in the shower bath. I developed this rash which I hadn't seen. I only noticed that I wasn't quite right when I first put my head on the pillow, and I had the usual enlarged posterior auricular glands. When you have German measles, the lymphnodes enlarge back of your ears. I remember I had one meeting with this committee in New York. I had to attend it, and my eyes--I had a sort of conjunctivitis. I had no particular rash on my face. I went down to the American Social Hygiene Association's Offices and sat way over in a corner by a window while we had this meeting. After the meeting some one said to me, "You must have had an awful party last night! You look as if you'd been on a binge for a week."

All my high purpose and the sacrifice of my comfort to attend a meeting, and I was accused of having a hangover.

<u>There's one other piece of business I want to get in here that occurs about this time--this book and the circumstances under which it was done.</u>

This book you're showing me is the subject and author <u>Index</u> of the volumes of the <u>Journal of Bacteriology</u> from volume 1, 1916, to volume 30, in 1935. Interesting to note on the title page is not only my name, but it acknowledges the assistance of Mr. Austin E. Andersen who was a bursary student. I told you about that bursary aid program in the colleges. This work was done in 1936 and 1937, published in 1937, and it's all the listings of everything, a very useful index. I don't really know now why I did it, except that it's the kind of a chore I would do. It was extremely hard. I'd never made an index before, and I told you I got in trouble because although I knew that you should have only one entry on a three by five card, I got so tired making these entries--I think there are ten thousand of them in here, or something like that. I made the index of the revised 8th edition of the textbook which is a very long index. I got so tired doing this index that I put several entries on a single card and gave myself more trouble than I would have had, if I hadn't done that foolish thing.

You indicated that....

May I say one thing about this? You make an index of a number of volumes of a scientific journal like this, and you think a great deal about the subjects that you put on the paper. It's educational to a great extent.

You indicated to me, I think, walking through the stacks downstairs that this was done evenings at Trumbull College.

This was done mostly in the evenings. I think I worked on it, as I would a thing like this, at odd moments in the day time, Sundays, but it was done much at night in my office at Trumbull College. The Master's Office was in a little stone extension of the main Master's House, and students could see me working there. The Master was available by the mores of the system, as well as in my case by my own personal interest in these young men, to the student who wanted to talk. They were able to tap on my window pane, come in and sit for hours 'til one or two in the morning. I learned some very astonishing case histories of young people growing up. Some of them you could help.

I think we've gone as far as we ought to go today.

Thursday, May 5, 1966 A-60, N. L. M.

Yesterday we tried to put your scientific work in some perspective, some organization. It was quite arbitrary from my point of view, except that looking back on it, it seemed to lend itself to organization. There's one other problem which appeared in a little study you published on rat bite fever which was kind of rare, or was, in fact, rare. It had certain continuing vitality subsequently at Yale, and this is the early work with Dr. Edward G. Nugent's patient at Rochester. I thought we might take that up first today /"Rat Bite Fever, Report of a Case with Demonstration of the Causitive Organism and Its Uses in the Treatment of Paresis" 27 New York State Journal of Medicine 1113-1116 (October, 1927)7.

This is another problem that I got into because something happened to turn the organism up in my laboratory. It wasn't a deliberate search for the organism of rat bite fever. It began with the study of the blood of a patient of Dr. Edward G. Nugent in 1926. I think Dr. Nugent had a suspicion that it was rat bite fever, but he didn't find it. We didn't find the organism in the patient's blood either. It's difficult to find in the blood of a human being. I innoculated, injected blood into a guinea pig, and ten days, or so, later when the pig was sick, a drop of its blood under the dark field of the microscope showed this little bit of a spiral organism of very active motion with three or four coils of spirals in its body characteristic of the form of an organism known as Spirillum minus; in other words Spirochaeta morsus muris, the organism of rate bite fever.

Naturally I tried to cultivate it and study it, but I never was able to cultivate it, or grow it in artificial medium. The question was how to prove that this organism was the cause of rat bite fever in human beings. At that time the treatment of syphilis of the central nervous system by inducing another fever, a febrile disease, in the patient was a common practice. They did this by the injection of organisms into human beings. They did it by giving such things as emulsions of typhoid bacilli to produce a fever. We didn't undertake to do that in a patient, unless you had some means of curing the patient of the artificially produced disease. Well, rat bite fever in the human being is easily cured by injections of the antisyphilitic drug arsphenamine; as a matter of fact, patients develop a rash all over the body. They get sore eyes. They develop a huge chancre-like ulcer at the point of innoculation, but when you give arsphenamine, it wipes it all off the patient just as if you did it with a sponge, so we felt perfectly safe with this man to whom we gave artificially induced rat bite fever. He developed all the symptoms and all the pathological conditions.

That was an experiment which fulfilled Koch's postulates. We had an organism that you could get out of a patient, and when you put it back into a patient, you reproduced the disease and got the organism back. That was the subject of the paper that was published on it first. Later I published a long review of all the literature I could find on rat bite fever, ["Rat Bite Fever in the United States" 3 International Clinics (41st. Series) 235-253 (September, 1931)]. It's known as "Sodoku" in Japan, or India. It occurs more frequently in Japan than it did in the United States, and I'm surprised that it doesn't occur more among the Negroes who live in rat infested slums.

This organism had an interesting connection with my beginning at Yale. When a new professor comes in, they have a meeting of the faculty--I suppose in many places they do the same--and the incoming professor gives a lecture, or a talk, or a demonstration on something in which he is currently interested. I put on a demonstration on rat bite spirochetes, spirillum in the blood in the ampitheater at Yale so people could look at it in the dark field. I talked about it, and in the audience was the powerful Professor of Medicine, my friend, Francis G. Blake, who had already published a paper on a very variable streptococcus, Streptobacillus moniliformis, as the cause of rat bite fever.

Mention of it is in here, I think.

Blake thought it was a streptothryx, but this is a very pleomorphic streptococcus. Have I got the name of it in there?

Number 7--this is the larger study. He published, I think, in 1924, a case which had come up in Boston, or which appeared to have come up in Boston.

He published in the Journal of Experimental Medicine a paper on the "Etiology of Rat Bite Fever" in 1916.

Yes, in 1916.

The organism which he thought caused rat bite fever was this pleomorphic-like streptococcus-like organism. Well, how could the same disease be produced by a spirillum and also by a steptococcus? These organisms are not at all related, but the disease of rat bite fever has some similarities with a type of arthritis that Dr. Blake and others were studying. The organism that he had never produced a skin eruption, or the ulcer, chancre-like lesion, and the disease was not subject to cure by arsphenamine, so it obviously was different. It was very interesting that two different organisms were obtainable from people, different people, who had the story of being bitten by a rat.

There I was just entering the school and having something that was contrary to the finding of the much more distinguished Professor of Medicine. It was a controversial point, but we never bothered to argue about it too much, and it has turned out that both are right. The Spirillum minus that I had produces the typical rat bite fever, and this other organism, streptothryx, or Streptobacillus moniliformis produces a somewhat similar disease, but different, and the two are running along now just about in that state. I was carried away by interests in showing this organism. I can't remember that I intentionally put on a show that would be critical of the finding of the Professor of Medicine.

What was the problem connected with its isolation and cultivation?

It just won't grow. We tried everything. Henrietta Rhees has a summary of all that in her Master's thesis. Nothing we could do would make it grow, and nobody yet has grown it outside the body. I suppose it would grow in the embryonated chicken egg--the one that they grow viruses and other things in with cells, but I didn't carry it over into that medium. I didn't go on with the work in rate bite fever after those demonstrations.

You went to Yale by way of Washington, D.C., and while we said some things about Trumbull College and being Master of a college, and you have paid tribute to the enormous sensitivity with which Mrs. B-J handled working with youngsters, being a Master and a scientist must have pulled you in two directions. What is there about being Master of a college, the work of the college, the work of the Council of Masters--that kind of association with the Masters of the different colleges presents a varied and interesting lot certainly in terms of their letters.

Well, the colleges, of course, are under the administration of the

President and the Corporation of Yale in the usual way, and they had attached to them members of the faculty. All the Masters were members of the faculty of professional rank, and they had very greatly differing interests--Professor French in English, a Chaucer expert; Arnold Whitridge, Professor of English too; the Professor of Classics Clare Mendel; an engineering socialogist Eliott Dunlap Smith; a very attractive Rhodes scholar, athlete, an amateur writer, and English teacher, Allan Valentine; and a Professor of Art and a noted etcher in his own right, Emerson Tuttle. I think that's about all the Masters. They were authorized to form a Council of Masters which met and discussed many of the problems of the colleges. We met once a month anyhow and gave advice on appointments, the development of the bursary system, rules for behavior in the colleges, the fostering of the dining halls of the colleges in competition with the dining rooms of the fraternities; in fact, some of the fraternities had to go out of business because of the college growth. We had hundreds of problems that were discussed, and to me it was extremely interesting because I hadn't, as a rule, talked with profesors of English, or history, or the classics. I envied them very much--these men in classics and literature--in the relation to their work and behavior in the college plan. I had to get across town to the medical school and bang around on a cement floor in a laboratory all day as best I could, while the Professor of Greek, we'll say, could lie on the couch in his college and read Homer and be doing the work of the college. He didn't have to go out. He was always available. I was available to the students at very difficult hours, either early in the morning, or when I got back after five o'clock from work, or after dinner.

I told about the building up of the labraries. I had a special problem in Trumbull College because this was not a Harkness college. It was a poorer college. It didn't have as much money back of it--the residue of the Sterling Fund. It was housed in rather cramped quarters in a way, in older dormitories next to the huge Yale Library on Elm Street. The people who had set it up had had to fit the college into existing student rooms and buildings; whereas the newer colleges could be planned by the Masters, and they had much better common rooms, dining rooms and living quarters than were available at Trumbull College. For instance, there were a number of fellows attached to each college--fifteen of them, or about that, from economics, history, classics, chemistry, covering the whole academic field, and these fellows, members of the faculty, were supposed to have offices, or did have offices in the college to which students could come for counseling on their courses, or anything they wanted to talk about. In the newer colleges the Masters had an opportunity to include in the new building the study rooms and the comfortable quarters for the fellows, attractive quarters, whereas when I got to Trumbull College there was no such room for anyone. I had to go and persuade Mr. Farnum, the all-powerful treasurer of Yale University, to put up some University money to fit about twelve student bedrooms for fellows' studies. Of course, that took away dormitory income and didn't add to my popularity with the financial managers of the institution, but we did get them after a while. Always Trumbull College, I think, suffered somewhat from a lack of financial support and suffered from the kind of quarters it had as compared with the other colleges, but we got on pretty well.

I enjoyed it, and I didn't enjoy it. It was very hard work, and I was not in sympathy with the general, what I call, "boy scout" notion of many of the Masters and the ladies, the wives of the Masters, who had plenty of time to engage in personal, social and amusing, or serious, relationships with students in their colleges. Some of them developed, really, such a motherly complex that they would remind you of female birds, or hens taking a flock of chickens around. I thought the colleges ought to be more like the English colleges and let these men find their own way through life at that time--talk to them about their problems, but not to have too paternalistic an attitude toward them which is what I did.

Well, fortunately the entering class that was assigned to Trumbull College really contained many brilliant men--the class of 1936. There were people in that class that have made distinct names for themselves in literature and in politics. They were great in athletics in Yale. The great Larry Kelly was a member. I admired them very much. I had rather personal experiences with all of them. Somehow or other they would find me working in my office in the back of the college and would come in and tell me the most remarkable stories about their parents, their love affairs, their financial situation, and the pressure they were under. I saw some very fine things. For instance, a great tackle on the Yale team lost ~~his mother and~~ his father at one time in his sophomore year, I think. They were poor, and he came to me once and said,"Some Yale alumni have visited me. They wanted to give me an alumni scholarship. What do you think about it?"

I said,"It sounds to me like a football scholarship because they want you to play football on the Yale team. I don't think a self-respecting man would take a thing like that."

That fitted in with his thoughts about it, and this huge fellow spent his summer on scaffolding on his back, on ladders, or whatnot, with a drill drilling holes through concrete floors in buildings to put in electrical wiring and stuff. In college he was a bursary student, and he did this kind of drilling work during the summer. You saw things like that among the students that were very affecting.

I remember one man whose mother lost her mind, as we'd say in common parlance. She had psychiatric trouble and was put in an institution down in Maryland. This boy would go down to that town and stand on a corner for hours waiting until his mother passed in a company with one of these custodial officials of the psychiatric hospital, just to see her. She didn't recognize him, but he would do that.

You found that they'd come in and tell you about curious situations that had occurred--a boy who had grown up in a small town and had come to Yale was engaged to a girl who in a small town was his equal, but he soon outgrew his fiance, and this kind of student would have a great problem. His eyes were open, and he saw a scope of the universe that he never would have seen in a country town. The girl didn't satisfy him intellectually anymore. This kind of man would feel under a great obligation to marry this girl that he'd promised to marry. He'd been engaged to her, and he'd cut her off from other opportunities to get married because she was loyal to him. The problem you discuss with a man like that was how injudicious it is to marry anybody out of pity for them. They'd understand that it wouldn't last.

That's the kind of thing that came to me a great deal as the Master, probably because I was a doctor. They'd tell me lots of things about their physical troubles, or their family troubles. They wanted advice, or information. I suppose if I took the time, I could think of a hundred very personal stories that were told me in all seriousness and resulted in rather useful action some time. Some times you couldn't do anything.

Well, there was also a great intellectual interest in the affairs of the students in the college. They wrote and put on some plays of their own. At Trumbull, they established a magazine called the Trumbullian--do you have any copies of that?

There are a couple of issues in the files.

I liked Trumbull College because of its patron saint--Jonathan Trumbull of Lebanon, Connecticut, who was a very steadfast man in the revolution, furnished the iron that George Washington could use to make cannon. The Trumbull home was up in Lebanon, and we used to have a pilgrimage up there with the fellows, sometimes the students. He was sturdy and forthright--"Brother Jonathan", as he was called in the old days--and one of his sons was a great artist, John Trumbull, which was a sport development in the family, and one of the students at Trumbull College wrote a tremendous thesis on John Trumbull the artist. All of it went along as part of a whole life of different people, different ages gathered together, and I don't recall any serious difficulties either among the students, or in the relations between my fellows and the students.

Our house was a very fine Master's House. It was built especially by alteration and addition for our occupancy of it. We had help on the plans from Mr. Eastman and others. It was a very expensive house, but it was a place that I entered toward the end of my being there with a shudder because I knew that as soon as I got inside I would see it swarming with students sitting on the floor, sitting everywhere with no regard for the privacy of myself, or Mrs. Bayne-Jones. Whenever they had a dance, or student festival of some kind, she would take in as many of the girls as the boys brought there as it was possible for her to house, and we had to take care of the parents. We had a huge kitchen and ice box, and I think the students in these days drank a great deal of milk, quarts and quarts of milk. Sometimes they--is that book of hers in the files with the names of the people she saw? I didn't know I brought that over.

<u>These were the dinners.</u>

It isn't complete. It stops after a while. She tore out a great many pages, but you can see that we had dinners and dinners and dinners. Entertainment by the Masters was part of the obligations. We had an academic guest suite in the college Master's House, separated a little, in a wing, and you could lock the door, and this academic suite was always occupied by visiting members of the Corporation, the monthly meeting of the Yale Corporation. One of the members of the Corporation was attached to each college, but in addition the Trumbull academic guest suite was used as a sort of residence occasionally by a professor who had no other place to go like Mr. Allerdyce Nichols, the head of the School of Drama, who stayed for months. They didn't take their meals in the Master's House. They went elsewhere--the college dining room usually, but the burden of taking care of the linen and keeping the place clean fell on the lady of the Master's House.

To revert to this milk business--I'll tell you a trivial episode that I saw. As I said, the students were all drinking milk and especially when their parents were around. One boy wanted some milk in our parlor, and we brought in a bottle of milk and a glass, and he tilted his glass and let the milk run down slowly into it. I said, "This don't foam like beer."

He did that in front of his mother. His habitual way of pouring beer into a glass caught up to him on the milk, but they were not rowdy, and there wasn't much drinking. As a matter of fact, I went to student parties occasionally at the Waldorf Astoria, or some other place, a coming-out party, or engagement party, and they had always two bars. They had a milk bar at one end of the dance floor and a liquor bar at the other. All the young people were at the milk bar, and their parents were at the liquor.

I got interested in a phase of those students at that time enough to talk with them about it, telling them that they seemed to be ashamed of their best instincts. There was a move, a beginning of civil disobedience, a beginning socialistic point of view. There was Mrs. Lindberg's <u>Wave of the Future</u>. Do you know that book Mrs. Anne Morrow Lindberg wrote? It influenced them a great deal, and they wouldn't admit they were patriotic. They wouldn't countenance any talk about service to their country. They seemed to be hard and disenchanted on things of that sort, but I knew underneath that they had a great sense of integrity and devotion to the country and to good things, and it all came out in World War II. These were the men who fought the war--gloriously. I still have close connections with a number of students who were in Trumbull College, and they will be life long connections.

Well, I went through this as long as I thought I could take it, but I was getting into other things. At the same time I got to be Dean of the Medical School, and I got to be Director of the Board of Scientific Advisers of the Childs Fund, and I was still trying to work in my laboratory. I had no end of committee connections outside of Yale, so in 1938, I resigned as the Master of Trumbull College. Fortunately we were able to move to Trumbull Street, and the University rented me the house they owned of great historic interest, the house of Benjamin Silliman, and we enjoyed that very much until I went to the war in 1942.

<u>Do these entries reflect dinners in the Master's house?</u>

Yes.

<u>Formal dinners?</u>

Formal and informal.

<u>There was a dining room in Trumbull College for the students, wasn't there?</u>

Yes, but these were in our house. We had a dining room that could seat comfortably twelve people, well-furnished. Some of it was the fine old furniture that Mrs. Bayne-Jones had from her family, and some of the furnishings of the Master's house were taken from the great Garvin Collections of furniture in the Yale Art Museum.

<u>There are some fascinating names that appear in here, but unrelated to science--John Hersey.</u>

John Hersey was a student in the college. We tried very hard to keep Trumbull College from being known as a pre-medical college.

<u>That must have been hard.</u>

Yes, they always thought--the students who elected that college thought that they would be close to the Dean, and that this would help them get along in medicine--I mean that you could give them advice. You see, they were undergraduates in the college. They weren't in medicine, and it was hard to get into medical schools, and they thought that electing Trumbull College would be a good thing. Also I had a good many medical people in the fellow's group. Dr. Harvey Cushing was a fellow of great renown. Dr. Winternitz was a fellow. The one who raised the greatest problem was a most interesting psychiatrists named Clements C. Fry. Dr. Fry was the psychiatrist for the Yale student health organization, and he had his fellow's room up on the fourth floor of the same building that housed the Master's House, a five story building. He used his sofa as a therapeutic, psychiatric couch, and all the time disturbed and queer students were going up to Clem Fry's place to be treated, and it began to give the place a rather shoddy name.

Trumbull College didn't get to be known as a pre-medical college. That was the trouble. Having a medical man supposed to represent science, or be broadly interested in science, and yet working in the medical school tended to restrict the interpretation, but I had excellent scientific people in the fellows--the great theoretical physicist named Henry Margenau, a botanist-biologist, John S. Nicholas, and Charles H. Warren who was the Dean of the Sheffield Scientific School and who succeeded me. Nicholas succeeded Warren. There have been four Masters since they started. They had a fellow's meeting every Thursday evening--all colleges did because that's the evening when all the servants go out of the house. We would meet and go down to dinner in a body and sit at the head table, the Master's table, come back and have a discussion about something.

How did you enjoy the association with the other Masters?

Very much. Robert French, the Master of Jonathan Edwards, was a long-time friend of mine from classmate days at Yale. Alan Valentine and I used to play tennis together. Arnold Whitridge was not so approachable. He comes from a group of Victorian socialites of New York--rather snooty. Elliot Dunlap Smith was a very interesting person, and he was an engineer with social ideas. At that time, engineering was being urged and taking steps itself to be a broad social subject, not just applied work. The engineering courses were being broadened. Clare Mendel, the Professor of Classics, was still a close friend. He was a Navy captain, a rather vigorous, opinionated man who married twice. He could get mad, but it didn't matter. Emerson Tuttle, the artist, was a highly cultivated man in literature and other things, and he had a very fine college in Davenport College. Then a friend that I still have who is here in

Washington is Arnold and Mrs. Wolfers. Arnold Wolfers was a great authority on international affairs, and he's still here with the Johns Hopkins International Affairs. We met in the Council of Master's meetings, but we saw each other all of the time someway, or another.

I think in a way one gains insight into a place like Yale University just by this contact. I don't know how much contact you had at the University of Rochester apart from the medical school and the Department of Bacteriology?

Well, we had contact with a great many people in Rochester outside of the medical school because Mrs. Bayne-Jones got to be soon one of Mr. Eastman's rather constant associates. He had four ladies that used to go with him and sit with him, talk with him always at his musicals and things--Mrs. Whipple, and one or two others, and these ladies would bring their husbands into the association they had with Mr. Eastman. You met a great many people. Mr. Eastman had a musicale at his house every Sunday, and you'd meet all sorts of people there. I knew a few people outside of the school in the University faculty--Dr. Rhees, Professor Charles W. Dodge was a close friend of mine, the Professor of Biology, and a long time friend of Dr. Goler; in fact, Professor Dodge and Dr. Goler were among the early people who made diphtheria antitoxin. They got a horse and immunized him under the steps of Edison Hall in the late 1890s, I guess.

Professor Dodge was in the Health Bureau Laboratory and his specialty was to watch for the appearance of diatoms in the drinking water. Diatoms are these beautiful, little, microscopic organisms of extraordinary color, shape, and size. They vary a great deal in the water. Some of them come at certain times and give the water not only a curious color, but a most awful taste. One of the problems in water supply was to watch what happens in the reservoirs--follow the diatoms. What I'm getting at is how precise Professor Dodge could be. He had a little bottle of black magnetic sand. It was sand that could be picked up by a magnet--little fine grains. He used to use this sand as the filter bed for his diatoms--made a little filter out of paper. The diatoms would go through the paper, but if you sprinkled a certain amount of this sand in there, they would be caught in the sand. Then you could wash them out. He concentrated them in that way--put a quart of water through this thing. When he got through the filtration, he would take out his paper and let it dry. Then he would start picking up the sand grains with a magnet. He used this little bottle full of sand for thirty years, and I doubt if he lost two grains. I inherited the bottle from him when I took over that part of the work, but the bottle got empty.

<u>The other Masters on the occasion of your retirement from Trumbull College, and incidentally it hasn't been pointed out, and I think you ought to, that the nature of the appointment was life time.</u>

Yes.

<u>The original Masters were to be there.</u>

Yes. French stayed until he died. There was no term, except the retiring age--not life time. You had to retire at age sixty-four optionally. Yes, I don't remember any time limit at all on the appointment. Nicholas stayed there until retirement for age. Whitridge left. Emerson Tuttle died as Master. Wolfers left. Vallentine went to the University of Rochester to be president. Mendel stayed until he retired.

<u>On the occasion of your retirement as Master of Trumbull College, there was quite a good bit of poetry read by some of the other Masters.</u>

They gave me a fine dinner party at Whitridge's house, and some of this poetry is very clever.

Yes, it is.

I don't know whether any of it is in these files, or not?

Yes, it's here--I guess there were six pieces from six different sources.

They were so much cleverer than I was--it's just amazing.

It was a good experience. Don't you think in terms of subsequent things that happened to you at Yale, that it was good to have this initial experience with a variety of youngsters in the college setting?

Oh yes, I think it was a very humanizing and soul enlarging experience. Anything that is as intricate and extensive as that is bound to increase the comprehension of even a dope.

You couldn't help but pick up some insight into the nature--some of your early reports.

I want to take that dinner book back to her. You don't want that.

Except that it does indicate who was present.

Well, you keep it, but put it back with my papers.

Why don't we leave it here in the files, and when we come to go through them we can decide then whether it's wise to leave it, or not.

Well, it belongs to her. I didn't know that I had brought it up.

Maybe you'd better take it then. I think before we can get into scientific work and changes at Yale, we ought to get into the conditions that you found in the medical school, its growth and development, the spur behind this growth and development, and some of the difficulties that led to the deanship problem in 1935.

That's a large order.

Let me turn this over--we've got about ten minutes left on this side, and I hate to get started on something and then have to stop and change reels.

I believe we've had enough for today.

Monday, May 9, 1966 A-60, N. L. M.

I want to take advantage of your experience at Johns Hopkins and your experience at the University of Rochester to give some comparative insight for its own sake into the developments at Yale Medical School as background, in a way, to understand the rebirth and development of the 1920s and into the 1930s which would set some dimension for what it is you fell heir to, the kinds of problems you fell heir to, so if you could draw on experience in three places because they are different in some respects, and tell me, so far as you can recall, or have thought about it, or read about it, something of the background of the emergence of Yale in the 20th Century, particularly after 1918.

Well, in my recollection and my feeling about these places, they are not so distinct as they would appear to you. I have had some connections with Yale, as I brought out earlier in our talk, since at least 1843, when my grandfather went there and was in the same class with Mr. John Donnell Smith who was a great uncle of the future Mrs. Bayne-Jones. In the intervening years of the last half of the 19th Century I had some relatives in Yale most of the time--my uncle Hugh Bayne, my uncle T. L. Bayne, a whole lot of cousins by marriage in the Cheney family and a number of people from the South who continued to let their children go to Yale, although they were still remembering the Civil War and Reconstruction, and Yale was not so popular in the South any more after the Civil War. Yale was a place I was familiar with from very early times, but the side of Yale with which I was familiar was the academic and social side.

I was there as an undergraduate from 1906 to 1910, taking preparatory work for medical school, but to tell you the truth, I paid very little attention to Yale Medical School. At the time when I was in college, Yale Medical School was in a little old tumbled down building on York Street—one of the original buildings almost, going back to the middle of the century. Yale Medical School had been founded with some éclat in 1831, by Nathan R. Smith and some very fine physicians of that time. It had a respectable position in the medical world and history of the country, but no particular distinction. There's a great pamphlet on Yale —no, that's not it; that's later—by Dr. William H. Welch, and you might want to get this and see it. It must be in the library here.

In 1901, Yale University celebrated its bicentennial. It was founded in 1701, and one of the chief speakers at the bicentennial celebration was Dr. William Welch. He wrote a history of the Yale Medical School which was published as a separate booklet in connection with that bicentennial celebration. I know from talking to Dr. Welch and others, that he had to work very hard to fill the fifty or sixty pages of this pamphlet. Yale Medical School was a good school, but not in a class with Hopkins, or Harvard, or Chicago, or Pennsylvania at this time we're talking about, and I had never had much connection with the Yale Medical School. I had no thought of going there. I wanted to go to Hopkins as I have told you before, but I got sidetracked to New Orleans for one year. Nevertheless, I knew that the Dean at the Yale Medical School through the period from about 1910, a little after 1910, up to 1919, and that's Dr. George Blumer, was a very fine, solid, upstanding man, conservative, careful, unbreakable integrity and dull, but Dr. Blumer was a great professor of medicine. He wrote good textbooks, ~~but~~ AND he was very much respected. I used to see Dr. Blumer occasionally when I went back to New Haven for one thing or another.

As I say, Yale Medical School was a place outside my ordinary ramblings. I was very much more familiar with the academic and the social side of Yale College, and I make a distinction here between Yale College and Yale University. Although Timothy Dwight in the 1870s, or thereabouts, is supposed to have made the first steps toward creating a university at Yale, Yale College was then, and still is, the rather dominant element in the place. One, who grew up as I did, thought about Yale College more than he did about Yale University, so when I went back there I had the same feeling about having been there before that I had when I went to Hopkins.

Hopkins was not a new place for me to go to as a student. I knew Dr. Welch Dr. Thayer, Dr. Barker, and a good many other people in the Hopkins and in Baltimore before I was a student, and I had an interest in the things that they were doing.

Starting at Rochester was a new venture, but as a matter of fact, it also seemed rather like being in familiar surroundings because most of the men on the new faculty were old friends of mine. It wasn't strange at all. We thought alike in a great many ways. We had common experiences. At Rochester we had to build up this medical school within a university that was then about seventy years old, I think, and a conservative university, coeducational, placed in the middle of the town of Rochester, whereas the medical school was built out on the banks of the Genessee River, about five miles out from the center of the city. Again, we were in an isolated location, just as I felt Yale Medical School was in an isolated location because it was quite across town from the University. Our problems in starting the medical school at Rochester were surrounded by an inheritance of the traditions of medicine and the ideals of medicine and the usual thoughts that we all shared about the policies and procedures for conducting medical education. All of these things were in our minds in common, and I didn't feel as if it were a new building in the desert, or anything like that. For my part, at least at Rochester, I began to feel very much at home among the people. I suppose this was because Dr. Goler was so cordial and enthusiastically supportive of the effort and so wise that it made the transition quite easy.

almost coming into a respective part of your family that you hadn't seen for a long time. I don't know how to characterize the situation much more, but I would emphasize that I had a great continuity through them all. There wasn't any great break in either one, or the other.

At Yale--to take it up at the time I went back there in the thirties. Is that when you want to get into now?

Some insight into its development since 1918, because I think it had taken a turn for the better, had it not?

Yes, Dr. Winternitz, who I think graduated from the Johns Hopkins in 1907, went to Yale University in 1917, I think, as the Professor of Bacteriology and Pathology. He didn't go there straight as the dean. I think it was for nearly two years that he was in the Department of Bacteriology and Pathology, and Dr. Blumer was still Dean. Dr. Blumer continued as Dean until 1919, and Dr. Winternitz, if I remember correctly, became Dean in 1920, and he was Dean of Yale Medical School until 1935, fifteen years of the most extraordinary development of any established medical school in this country. It was a complete renovation of ideas, aspirations, methods of teaching, research undertakings, and all in the social cast that was characteristic of Dr. Winternitz and characteristic of Mr. Hutchins who was Secretary of the University and other supporting people who gave Dr. Winternitz unlimited, loyal support, although you would think that they would not have had much interest in the deliberately socialistic phase of medical education and medical care that he sponsored, and one of these men was the all powerful Treasurer of the University, a tall, gaunt, firm man named Mr. Thomas Farnam.

Dr. Winternitz seems to have set about right from the start to revolutionize the teaching and service plans in the Yale Medical School even before he became Dean, but when he became Dean, he had a chance to bring this huge and sweeping ideas into practical effect. He was able to get an enormous building program started. He built the Sterling Hall of Medicine which is this huge building covering the better part of a large block. He built a new powerhouse, built new animal quarters, built a better Department of Pathology, added wings to the old, tumbling down New Haven Hospital, and set up very soon what he called the Institute of Human Relations. The best way I can characterize that is to recall a diagram that Dr. Winternitz gave me which convinced me that what he was doing with human relations was just to take the whole university into his bailiwick and give it another name. The diagram he drew for me about the time I had gone back there as a Professor and Master of Trumbull College was a five pointed star. The five pointed star represented the nucleus of the basic structure of the Institute of Human Relations, and at the point of each point of the star, he had a little circle, a little sphere, and one of them was labeled Yale Law School and another was labeled Yale Divinity School, and another was labeled Yale College, and the Yale Medical School came in there—in other words, what he did was to take all the big departments of the university and make them little appendages of the Institute of Human Relations. He nearly succeeded in getting that grouping established, until the people who had not only vested, but perfectly legitimate rights in the Divinity School and the Law School as autonomous organizations almost with special functions and special obligations, turned against the plan, and enormous opposition was aroused to the Institute of Human Relations in the university community—also this opposition was aroused outside—who rather ridiculed this idea of having a thing called the Institute of Human Relations when it really was a power play to bring all the main division of the university under one jurisdiction headed by Dr. Winternitz and maybe Hutchins.

Well, by the time I got there, the furor of the Institute of Human Relations was rather quieting down. It quieted down rather quickly because the Institute of Human Relations idea was promulgated largely by 1931, and I got there in 1933, and what it had become by that time was a Department chiefly of Psychiatry and Psychology. They had in one of the large wings of the Sterling Hall of Medicine the Department of Psychiatry with patients in all stages of manic excitement to great and dangerous depression showing minor abnormalities of behavior to very serious abberations.

Dr. Winternitz also had planned to introduce into the Yale Medical School foreign professors, and the Professor of Psychiatry was Eugen Kahn whom he brought over from Vienna, I think. Another importation was Dusser de Barenne a neurophysiologist, a great, big strong dutchman. I don't think either of those men took root here. Dusser de Barenne died of a heart attack just shortly after I finished being Dean. Kahn left, and he is now a Professor of Psychiatry somewhere in the South. In my opinion, when they transplant a foreign professor in new soil, he carries on a rather wilted existence. Nevertheless, Dr. Winternitz started a vogue of having foreign professors come in, and they've had one recently at Yale who didn't settle down too soon and probably won't stay very long. The other members of the faculty that were in the medical school--I forget when they were attracted there, but I think Dr. Winternitz had much to do with getting them. They were very promising young men and had developed into excellent authorities in their fields, like Dr. Grover Powers in pediatrics and Dr. Francis G. Blake in medicine. An indigenous Yankee of the country, Dr. Samuel Harvey, was there as Professor of Surgery, and he was a remarkably stable philosopher in the medical school and a man of general rekown at that time. The other men were good too--Arthur H. Morse in obstetrics.

The school, however, was suffering from lack of funds. It had no endowment to speak of. Its financial problems somehow Dr. Winternitz had managed temporarily to solve, or at least he managed to get money to do the things he wanted to do, but it wasn't resulting in much permanent resources in money. For example, a very great man who was on the faculty was Robert M. Yerkes, a great student of higher primates, chimpanzees at Orange Park, Florida, where he had his colony of these animals. He was a psychologist, and he carried a huge undertaking by yearly pleading with the Rockefeller Foundation to give him whatever he needed per year--36, or 40 thousand dollars. That seems ridiculous now it's so small. I had a little problem in connection with that work when I was the Dean, to get it refinanced for the last time, I think, on that same source, the Rockefeller Foundation through Yale.

Dr. Winternitz had succeeded in getting from the university and from outside sources enough to build these new buildings, to bring in new people, and by persuasion and eloquence to revise the educational program to a considerable extent.

<u>How effective was the Flexner Report in this rebirth and through the Flexner Report the Rockefeller Foundation in the 1920s?</u>

The Flexner Report was in 1910. Everybody was familiar with it. The report indicated that Johns Hopkins was so far ahead of everything else, that all anybody could do was try to catch up a little bit. I don't think the Flexner Report was of any particular benefit to Yale. Yale, as I said, ranked among the good schools, and the Rockefeller Foundation was interested in supporting medical education to a great extent at that time. They don't do it any more to that same extent, but they did support a good deal at Yale--just at the present I forget how much, or when. There were several other foundations that contributed to work going on in the place. The Anna Fuller Fund giving money for growth studies, the basic development of cancer research at Yale. Mr. Frederick P. Keppel was interested in having his foundation--what was that?

Carnegie.

Yes, Carnegie gave some money for dental education at Yale which we can talk about now or later. One of Dr. Winternitz's ideas, for example, was a very broad idea that affected his efforts in connection with specialties and general education. He had two bête noires, or whatever you call the kind of beast that you want to fight the most, and one of these was the tenure of professors. He thought that life tenure for a professor was one of the most inhibitory things that a school could have. "You can't get rid of them", he'd say, and he wanted to get rid of some of them. That's been true of most administraters of educational institutions. They have to obey the traditions of tenure, but they don't want to always. A man is finished, and he stays on.

The other thing that Dr. Winternitz wanted to break down was the walls between departments. The departmentalization in a school is extraordinary. The people in surgery want to build up surgery as a complete little school with the emphasis on surgery. The same in medicine, and the same in neurology, or something else, and they all go about their work with very little communication between these sections. The one thing that was different at Yale, at the time I went there, as compared with Rochester at the start, was that at Rochester we were all free to associate and didn't have too many other burdens or interests.

We could pool our efforts in education and research, but at Yale it was a series of rooms, so to speak, or compartments in which one department would live and not go through the door into the other. Winternitz tried to break that up by forming what he called study units. For instance, he set up the Atypical Growth Study Unit which means that it would be broadly interested in the functions of growth and particularly in the growth that is not typical. That's a long lot of words to mean cancer, or neoplastic growth. If you study growth as they do now everywhere, you can't just study the change in the length and breadth of an organism. You've got to study the biological metabolic processes, the whole physical chemical situation, the enzymes, even the effects of cosmic radiation on them. They are all a part of the ecology of the universe.

Well, Winternitz saw that, and he tried to bring together in the Atypical Growth Study Unit the pathologists who were seeing cancer in the autopsy, the surgeons who were seeing cancer on the operating table, and the medical people who were seeing cancer that wasn't being treated surgically, or cancer diagnostically in the patients as they came along. The physicist could help with apparatus, and Winternitz even brought a physicist over from the University Department of Physics and gave him an office in the medical school so that this man was always available to discuss the physical aspects of problems, whether it be a question of apparatus, or whether it be cellular processes. Radiation was rather well developed at the Yale Medical School mostly from the diagnostic point of view. Radiation in relation to Atypical growth was well known ever since anybody saw the cancers that developed on the hands of the men who did the first x-ray work. It was quite interesting at that time to see radiation coming into the field. The same thing was happening in chemistry. It was a period when the sex hormones were very much to the fore, and, for example, the Professor of Anatomy, Dr. Edgar Allen, was more of an expert on some of the internal secretions, particularly sex estrogens, than he was on structural anatomy, and anatomy at about that time was doing what Dr. Winternitz would like very much to have it do--that is, go over into fields of biochemistry, or physiology, to study processes as well as structure. Well, the Atypical Growth Study Unit was formed and functioned, and, as I say, it brought into Yale a lot of competence in the field of cancer. I can remember the impression that group made upon Mr. Starling W. Childs when he and his son came to visit the place. I think it turned their attention to Yale in a way they hadn't appreciated before.

The trouble with the study units is that they go along very well for a few years, and they become departments, and you have to break their walls down. They get a little money, and they begin to have vested interests, have interest in positions, interests in physical laboratory, space, and whatnot.

Winternitz had also another group called the Neurological Study Unit. A characteristic of Dr. Winternitz's idea was that this should bring together the psychiatrist, the psychologist, the neurologist, the anatomist, an expert on nerve structure, the sub-department that Dr. Harold S. Burr had on neuroanatomy --you saw his letter there--that kind of aim is fine, but the only thing about it is that they flourish in a broad way, have a flourishing time in broad and liberal activity for a few years, and then they get set. I suppose all living things do something like that.

This is great excitement though in the generation of new ideas and novel approaches.

Oh yes, but it's extraordinary how divisions in a school just occur either because of geographical accidents, or the inclinations of the people. Yale Medical School is divided structurally by Cedar Street which runs between the hospital and the Department of Pathology on the one side and the so-called basic science departments on the other--anatomy, physiology chiefly--and then the Institute of Human Relations. Now, people don't cross Cedar Street, although it's about thirty feet wide. That's a fact. It was a greater chasm in that school than the Colorado canyon.

<u>That's incredible isn't it.</u>

Yes, but it happens everywhere. It happened at Hopkins. There is something about the geographical separations that are very powerful. To cross Cedar Street you had to put on a coat, or a hat, something like that. It took time, and it came up--I mean this idea about having accessibility as a prime element in a plan came up when Dr. Cushing, Dr. Fulton and I were talking about a structure for the Yale Medical Library. We'll get to that in more detail later, but let me put it in here. I'll say this--at one time Mr. Grosvenor Atterbury, who was an architect classmate of Mr. Childs, had an idea that this library ought to be a beautiful marble, mausoleum--as I called it--set up on a piece of land just beyond the Institute of Human Relations. Dr. Cushing and I felt very strongly the other way, that the library ought to be a place into which the students would fall unavoidably, so we put the library on the end of one of the stems of the middle wing of the Sterling Hall of Medicine, a beautiful little corridor going into a Y shaped extension, the historical medical library on one side and the working library, or current library, on the other, and it was so easy passing by there to walk in through the door that thousands went in that never would have gone up otherwise, I'm sure. We fixed it like a trap.

<u>In the late 1920s, I think from the Yale Faculty, C-E A. Winslow and from the Yale Law School, Walton Hamilton, were involved in a study, the Committee on</u>

the Cost of Medical Care. Did that have its ramifications at Yale, or was that one of the new ideas that were abroad in the land also?

Yes, it was a new idea in the land. Dr. Winslow, Charles-Edward Amory Winslow--curiously I haven't mentioned him so far. He was the head of the Department of Public Health, and Dr. Winslow was a noted man, very eloquent, a prolific writer, and a great power in public health in the country. He was not a clinician, and he didn't have anything particularly to do with medical students. His budget came through the Dean's Office in the medical school because he didn't have a school. He had a department, although it was called the School of Public Health. Dr. Winslow was a man apart in a way, not on his own inclination because he was a perfectly charming, hospitable person, but because, I think, the other members of the faculty were either too busy, or too disinterested to have much association with the Department of Public Health.

That Department of Public Health was an enlightened department. Dr. Winslow was a prophet of the new ideas of social medicine and very important in all sorts of civic and other activities that are outside of the ordinary medical field; as a matter of fact though, he was a fellow of Trumbull College, and I got to know him well. For instance, when he was in Trumbull College, he was very much interested in city planning, and with him we got a Russian or two over. At that time it was a little bit bold for a member of the faculty to have such guests, but they came over and talked about city planning. Dr. Winslow knew the chief city planners in England, and he and Ira Hiscock studied the city planning of New Haven, but what good it did for that I don't know because New Haven is an unplanned situation, if there ever was one. Yale University occupies most of the center of the taxable property of New Haven and makes a great trouble with the city officials and town and gown battles.

<u>And especially when you headed into the early 1930s and the depression.</u>

I don't know much about that because I was down here in Washington then.

This study of the cost of medical care was fought by the regular medical profession as they did so many things of that kind. They--the one I have up there on the cost of medical care, the financing of medical care, even as late as that, the late 1940s, was opposed by a good MANY of the medical people in the organized medical profession. The school had to make its way through innovation into innovations by a constant sort of a battle with the offices of the Connecticut State Medical Society and the Bridgeport Medical Society, although Dr. Winternitz had managed to make good friends of the main powers there, Creighton Barker, the Secretary of the Connecticut State Medical Society, was a strict AMA type of man, but he was mostly friendly.

Then Dr. Winternitz set up an advisory, consultative committee composed of the Dean, several members of the faculty, and the chief medical officers of the state--I mean physician officers; not politically appointed officers--members of the State Medical Society, the Bridgeport Medical Society, and so forth. They would meet three times a year and talk over the problems of the medical school as they bore on the practice of medicine in the State of Connecticut. It's rather sharp at times because they want to protect the financial emoluments of the practicing physician, and they are rabid on what the physician calls "the corporate practice of medicine." Yale and other places I've been do get into the corporate practice of medicine, if you want to strictly define it, and that's anathema to the AMA type of person.

<u>Was Winternitz behind the development of the full time system at Yale?</u>

No, the full time--yes, he was back of it, but the full time system at Yale was started at the Hopkins. The full time is in the Flexner conception of things. I was at the Hopkins when, of course, that was going on. Dr. Osler had left. After he had left, he expressed indignation that any professor would venture to talk about medicine who didn't have to fight in the arena in the battles with the patients and their families and all that sort of thing. Dr. Welch, however, was thoroughly full time from the start, and Dr. Franklin P. Mall, Professor of Anatomy at the Hopkins was probably ahead of Dr. Welch in the concept of the full time system.

I was on a committee which Dr. Welch had set up to periodically review things in the full time system. I can't remember exactly the last meeting, but the substance of it was that Dr. Welch called us down to Hopkins to discuss the full time system and at that time--and I think I was at Yale then; I was representing Yale--the full time system was not working well for two reasons. One was that it was set up on a shoe string financially. It offered salaries to these great professors of medicine and surgery that couldn't compare with the salaries that they could get elsewhere, or couldn't compare with the salaries they should have to live the kind of life they were supposed to live. That was one thing.

The other thing bad about the full time system was that they early, and maybe they do still to a certain extent, selected young men who were in the beginning of their productive research period and had great promise in research. They brought them in to be heads of departments and loaded them so with administration that they couldn't do any more research. Administration, teaching, public relations, faculty committee meetings, committee meetings all over the country in connection with educational and research foundations are incessant interruptions and take away, or exhaust the energies, or the thoughts of the people who have to do it. The full time system was really breaking down because it was ruining the people they thought were just the right ones to come into it.

Was this true of your experience at Rochester? How did Whipple regard the full time system?

He was in favor of it, but again Rochester at the time I'm thinking about it, in the 1920s, was smaller, and I don't know that the outside calls on the people were anything like what developed later in the 1930s when more and more organizations had formed and more and more people were asked to be advisers. Well, an example is the growth of the National Institutes of Health. Their Councils are composed of teachers everywhere, and every day there are meetings of great length, and once you get on to one of these things, one of these organizations, and either do something helpful, or don't do anything disreputable, you're asked to go on again. It just builds up. Nowadays--well, I read a paper the other day that members of the faculty must at least spend a third of their time away from the school to be of any good to the school.

That's incredible. At Yale then, the full time system was in operation only partly.

Oh, it was in operation for the heads of departments--surgery, medicine, pediatrics, obstetrics, psychiatry, physiology, anatomy, pathology--all those. At Rochester we began to break down the full time system as far as salaries go. We had to for the clinical members. We made what we called the modified full time system which I think is a good plan, and I carried it out at Yale somewhat and much at Cornell in New York. The modified full time system simply is this--a man is allowed in private practice to collect fees equal to his salary. If he gets ten thousand dollars a year, he can make another ten thousand. If he collects more than ten thousand, the excess over ten thousand goes into some fund. Some places put that money in a fund to support their departments, some places turn it into the school, and some places turn it into the university, if the central university manages to get hold of it first. The best plan, I think, is to turn it into a general fund for the whole benefit of the school. Some departments make so much money that they themselves become granting institutions within the school. I know of a department of surgery that supports a good many of the departments of medicine and biochemistry, and other parts of a medical school, but if it's pooled and is under the control of a committee, or some one person, as it was in New York, it can be used for the supplementation of salaries, to buy books, to have a research fund for venture investigations of people who haven't yet reached the stage where they can get money from a foundation, to carry malpractice insurance for the interns and residents, and a whole lot of things.

I think at Cornell we called it the "full time fees fund", but at Yale when I was the Dean there, and I don't remember any particular difficulty with this, I'm sure that professors, some of them, collected money in addition to their salaries.

By the time you become Dean the emphasis is toward the modified full time system. Well, you know, great changes take place in the nation as a whole in 1929--the collapse, and I wonder what effect this had. It may not have been felt initially, but Winternitz certainly felt a kind of retrenchment, even in the midst of re-birth, development and re-thinking, and it's hard to balance that kind of act--certainly to the satisfaction of everyone. It's impossible. I would think that impossibility would be part of the atmosphere, hostile or otherwise, to which you fell heir in terms of 1936. Part of this nation-wide calamity was to spur

<u>new and exotic thought--you know, how wild they were from one extreme to another as to what to do.</u>

Well, you see, I was at Rochester when the depression began, and I felt it personally because I had trouble selling a house for half what I paid for it. I don't think I could have sold it then, unless I threw in twenty-five gallons of the best wine I ever made and an elm tree in my celler that had blown down and I cut up and stacked.

<u>You had to sweeten the pot with the best wine you ever made.</u>

Yes, it was good, and I persuaded somebody to pay half of what I paid for the house. Then I went to Washington shortly after that, and I was not in Yale at all during those pinching times. I didn't get to Yale until 1932, and when I got to Yale in 1932, I was much concerned with getting Trumbull College started and my own laboratory, so I was not in the know as to what was going on in the administration of the school very much. When I was there at that time I had no responsibilities as a department head. I taught some graduates who were registered under Dr. Leo F. Rettger and worked with some of the clinical departments, surgery and pediatrics. Medicine had a great department of bacteriology of its own. It was a distinguished department. James Trask was a great expert on streptococci in the country, and they were getting good grants from the National Research Council and other places.

I don't recall any particular hardship showing on the surface at the time when I first went there in 1932. When I became Dean I had the pain of cutting fifty thousand dollars out of the first budget I had to handle, and that was a good big slice out of a small budget. I don't know whether that is in my first report or not.

<u>Yes—there's a thing called the "fluid research fund."</u>

The fluid research fund was about twenty-five thousand dollars a year which had been given by the Rockefeller Foundation. That was modeled to what I used later on in this full time fees fund at Cornell. This money was put out in small grants to professors, or instructors, or even a student maybe, in a venture research. It was administered by our so-called Prudential Committee, and that's part of the Board of Permanent Officers, professors. It was about twenty-five thousand dollars a year which, of course, doesn't go very far in a big place. The difficulty there again is the tendency of people to consider something done experimentally for support for a short time as permanent. It becomes an expected resource. The repeated requests for practically the same thing year after year shows that they are counting on this as budgetary money. As a matter of fact, some of it did get into budgetary situations. It's very easy to do that, to employ a research technician who turns out to be doing a lot of the departmental work.

<u>What was the occasion of the offer to you of the deanship? Out of what does that grow?</u>

That's going to be a hard question for me to answer to you because I haven't answered it to myself yet. Now, I really think the attention of the faculty was focused on me because I was a new person and had no commitments of any kind. I will leave out of consideration that they must have thought I was capable of being Dean. I took that for granted by their asking me without putting on any side about it. As they looked over the people who were on the faculty who might be considered for the deanship, they would find that all of them had been there long enough to have made a few enemies perhaps, or have made a number of special friends and perhaps had expressed themselves about policies and procedures that they couldn't turn away from, and one quality a Dean should have without being weak about it is not only the quality of understanding all sorts of points of view, but a willingness to work out many, many problems that come to the Dean from members of the faculty. Some who had already been members of the faculty for a number of years might not have been so impatient with some of the thoughts of their colleagues. What I'm meaning to say is that I was rather foot free. I imagine that's why I came up to their notice.

Now, they were determined to make a change, and if I hadn't been Dean they would have attempted to have another member of the faculty made Dean, but at Yale the process of appointing, or making a Dean as outlined in the bylaws at that time, was that the Board of Permanent Officers of the school every five years could make a nomination to the President of the Yale Corporation for somebody to be Dean. That's rather unusual in some places because many deans, once appointed, contimue--well, Winternitz continued fifteen years, but he came up every five years. At Yale, the deanship comes up every five years, or it did until lately. I think President Griswold broke that down. Well, Dr. Winternitz's term as Dean was to end in 1935, so in 1934, the Board of Permanent Officers began to think about it, and they should have the nomination in, I think, to the President's Office in probably December of 1934.

Well, to go back to my relation to this situation. Suppose the faculty had not wished to consider me as a candidate for the Dean, or their nomination, they would have had to pick somebody else among their own ranks. I feel quite sure that the President and the Corporation would not have accepted any other one there, and I know definitely that Dr. Winternitz would have fought another nomination very bitterly. You can see what I mean by that because when I called on him to tell him that I was letting my name go in, he said, "B-J, if it's you, it's all right. If it had been somebody else, I would have fought them."

Then I think there's a letter from President Angell to me somewhere in these papers not only asking me what my point of view was about certain things in medical education, but expressing the opinion also that the President and the Corporation at Yale admired Dr. Winternitz greatly and did not particularly care to see a change. Mr. Angell and the Corporation didn't press that point. I didn't have to meet any unfavorable criticism from Woodbridge Hall.

<u>And there was no problem at all with reference to Dean Winternitz--none.</u>

Oh no, we had a mutually respectful relationship--more respect on my part, I'm sure, because I regarded him as a great man--and also an affectionate relationship because he was never cruel to me as he was cruel and harsh to some people. He never hurt me.

<u>It was a sticky situation that came up in 1934.</u>

Over this deanship?

<u>Yes--long and continuing, I'm sure, and it came to a head at that time.</u>

Well, the transition occurred without any break in the school. It didn't cause a ripple.

<u>I think the problem was complicated somewhat by problems that had nothing to do with the deanship--the whole atmosphere of the nation.</u>

That may have been.

<u>Well, we've gone a little over an hour. Suppose we stop and pick it up again tomorrow.</u>

Tuesday, May 10, 1966 A-60, N. L. M.

Yesterday we got you into the deanship. I think that it's proper to record that this period of the emergence of this as a possibility and its final settlement is only a matter of nine days at best.

The date of that is January, 1935. The date of the resolution of the faculty when they recommended me.

I think the deciding question was the fact that they would recommend you, and that comes between December 12, and December 19, 1934, a series of meetings with the committee, certain notices and interviews with Dr. Winternitz, a substitution of Dr. Cushing in your place on the Committee on the Appointment of the Dean--all this takes place in seven days, from December 12 to December 19, 1934. Now, you do see President Angell on December 26th for a long afternoon's discussion of general policy. I presented to you, gave it to you to read, the questions which he raised on January 2, 1935, and your reply of January 7th which is an overall effort to sustain what he believed to be the essential policies of the school. I think it's important to fence this in somewhat because it is a point of departure and gives us some insight into President Angell and your relationship to him. Do you remember that?

Yes. President Angell wrote me on January 2, and I have his letter before me now. He addressed it to me as the Master of Trumbull College, and he said that he wanted to know my point of view about a number of policies of Dr. Winternitz in the medical school because the Corporation had greatly admired Dr. Winternitz, as many others had of course, and generally would not like to see any radical change in his policies. Then he lists a number of matters that he considered as especially important, and he asked me questions about them. I know now from what this letter recalls and my handwritten notes in the margins of it that I found that I was really honestly in agreement with all of the policies that he was bringing up for consideration.

I pointed out in my reply that Dr. Winternitz and I were both trained at Johns Hopkins, that we had an impress on us that would not be rubbed down in any kind of association with other schools, that he in his way was carrying out ideas that he had absorbed and breathed in at Hopkins, and that I had done somewhat the same things at Rochester but only less vigorously and in a different manner from his. I told President Angell that I would naturally go ahead with the kinds of things that were going on in general like attempting to deal individually with students. That was something that Dr. Winternitz cared for a great deal, and that was something that was very characteristic of the early days at Rochester--teaching was individualized, the students were individualized. We had to keep some marking system in Rochester and at Yale too, but it was not anything that we paid much attention to. We didn't try to grade students by fractional points of grades such as some people think are important. We looked for the man's characteristics and his manner of work. Actually Dr. Winternitz had been successful in not requiring attendance at classes; in fact, he rather urged the students, if they didn't think well of a professor, not to boycott his classes but to make protests. I felt the same way about attending classes. I thought it would be a pity if they didn't go to their classes because you never know when some very useful remark will be made. Perhaps in a long hour, or two of tiresome and not very intelligent teacher student relationship something will happen that will be of the greatest importance, and it's better for students to be there and not miss it. I would have gone ahead just about as

Dr. Winternitz had been doing in the cultivation of the development of the individual's responsibility and his control of his own fate more or less. That was easy to answer in the same line of thought that Dr. Winternitz had had.

Mr. Angell asked about my ideas on the integration of the several departments in order to foster the best teaching of the students and to foster, as I could see it, a great many other advantageous operations in a medical school in addition to those of teaching and patient service. It was a perfectly familiar operation to me. I had some of it at Hopkins, although I had relatively little opportunity to put it into effect there when I was beginning in bacteriology, and again after the war I was off in a side building, so to speak, after a fire, but at Rochester, as I think I said the other day, we had integrated teaching and joint departmental work right from the start, and we could do it delightfully because we were small and free and not too plagued by other responsibilities. I had thought the same at Yale, although up to this moment of being made a Dean I hadn't had any special opportunity to do anything in the school along these lines.

In setting up Trumbull College I had done more than some of the masters in bringing to the group of fellows representatives of very diverse departments--we had physicists, mathematicians, artists, doctors, historians, and literary people. They were all sitting together. We met and knew about our separate and joint endeavors and a good deal about the departments that each man represented. That would have been a natural development, and I think that after I became Dean we went along these lines of helping to integrate departments. You can't integrate departments by doing what I saw a lady do in a street car with her two children. She plunked them down in the front seat of the car and coordinated their pleasure by bumping their heads together and commanding them to enjoy it.

That doesn't work, and it certainly doesn't work with professors. It's a matter of the spirit. Mr. Angell said that he was interested in the effort to integrate the medical school as fully as possible into the general scientific and intellectual life of the university. The medical school yearned for that sort of thing. It was Dr. Winternitz's great desire to knit it in so closely that the university would be covered by the mesh of the knitting and woven into the medical school. I explained his ideas on the centralized Institute of Human Relations which would take the whole university under its aegis, but the medical school at Yale had always been a little bit out of the line of the intellectual activities of the university and yearned to be a part of them more than the university would permit, or would have the information to allow, or invite. A good example of that--and it could come up later, the time of Hugh Long, when he was Dean in the 1950s, the corporation actually passed a vote saying that the Yale Medical School was an integral part of the university. That's between 1830 and 1950--it took over a hundred years to do that.

I don't believe I took up Mr. Angell on this point. The desire of the medical school was there, but the integration with the university depended on something forthcoming from the university departments. Dr. Winternitz had done this as I perhaps mentioned before in the case of physics. He brought a Professor of Physics over and gave him an office where he could consult with the heads of departments in its school of medicine, and it was a very helpful arrangement.

Mr. Angell speaks of the development through the Institute of Human Relations of opportunities for voluntary group attack on basic human problems. That I didn't know very much about. I couldn't answer that very well because I didn't know what the point of view of the Institute of Human Relations was. I regarded it largely as a center for psychology and psychiatry. Mr. Angell was a psychologist and naturally interested in that line, but I have forgotten what I answered about this. I probably dodged it by saying that I really didn't know, and in reviewing it now I'm not sure that Mr. Angell indicates a genuine support of the movement in that rather vague question that he asked.

I wondered how knowledgeable he was about medical school affairs.

Mr. Angell knew a great deal about it. You can't be a good psychologist the way he was without knowing about orders and disorders in the central and peripheral nervous system. He'd been in a medical environment through psychology a good deal. He was favorable to medical school activities in teaching and attended some of the faculty meetings, though not as many as Mr. Seymour did, because when I got to be Dean, I made it a point to go over and escort the President from Woodbridge Hall to our faculty meetings. After a while he came on his own.

Mr. Angell speaks of the conception of the hospital and the institute as agencies through which the university immediately touches and serves the community. Well, that's a difficult thing to answer in the specific, though it's easy to answer in general. Certainly the university should be in close, harmonious contact with its community and the government of its community. Of course, I was very familiar with that sort of thing from experience in Rochester with people like Dr. Goler, Mr. Eastman, the Rochester Health Bureau, and the Municipal Hospital. I would have answered that, and I hope I answered it with reference to the hospital and the medical school without saying what I thought the Institute of Human Relations might do in this connection. I wasn't knowledgeable enough with the Institute to say.

I don't believe it had much of a purpose in that connection, except that one very remarkable professor in the Institute of Human Relations was Arnold

Gesell. He was a great student of child development and with a few people he set up a clinic in which he could watch child development. He wrote those great books on the development of the child at different ages, and he did a good deal with community relations through child guidance and things like that. So did another psychologist over there named Walter R. Miles who was a practical psychologist and got interested in aviation psychology and the psychological physiology of work effort and different activities. Also in the Institute of Human Relations there was a remarkable man named Mark A. May who was a sociologist and economist; as a matter of fact, Mark May succeeded to the head of the Institute after some years, after I had become Dean. He was a fellow of Trumbull College too. Well, Mark May was responsible in the years after 1935, of drawing in people from the Rockefeller Foundation enterprises, and one product of it, and I can't remember the auther of it at the moment, [John Dollard] was a rather famous book called Class and Caste in a Southern Town. That was back in the middle 1930s and this book, I think, brings out in a very clear way the kinds of things that William Faulkner spoke and wrote about later and William Hodding Carter--just describing conditions under which Negroes lived and white people on the wrong side of the railroad tracks as compared to the others--that kind of thing. The Institute was in sociological studies outside of New Haven as well as inside of New Haven.

All of this is very good that Mr. Angell had in mind, and as I say, it didn't embarrass me at all to answer it, and I don't think I made any excessive promises, or said anything that wasn't natural from experience.

Your reply indicates that this was like jumping off the dock into a swollen stream the nature of which you weren't quite sure of, but that you could agree in principle with the principles he announced.

Well, the jumping off the spring board was that nine day sprint. I hadn't thought at all about being Dean of that school. I hadn't any ambition for it, and I hadn't thought of myself as being an administrative officer of a great concern like that.

<u>You indicated to me a couple of days ago that you made some immediate changes in the Dean's Office, the physical office, a less elaborate place in which you could function which, I gather, has continued.</u>

Yes, Dr. Winternitz had constructed in the Sterling Hall of Medicine a long room with two smaller rooms, one at each end, lavishly furnished with oriental rugs and beautiful furniture from the Garvin Collection at the Art School. He set himself up with a sort of regal potentate's environment. It seemed unnatural to have a Dean of a hard working place like that in such rich and gorgeous surroundings. None of us ever felt comfortable going in to see him in this Mussolini-like habitation, and he knew that from the ordinary chit-chat that goes on with people from time to time--someone would ask him how he was in his palace and whatnot, but when I came to be Dean I couldn't bear to go into those quarters, and I didn't use them at all. I immediately got a room across the hall, a small room that could be fixed up with some plywood bookcases. I got a good desk and got some pictures in there. It put me right next to the secretary and Miss Miriam K. Dasey the head of Admissions and right next to a remarkable and able person in the medical school, Miss Lottie G. Bishop who really ran the place, finances, knew everything and was very sound in judgment. She must appear somewhere in these records. She was a great person to help the Corporation, help Woodbridge Hall. She was good on publications. There was nothing she couldn't do well.

Weren't you lucky.

Yes. She was strong-minded, had her opinions, and they were always good.

In terms of the school....

This room I'm talking about is still the Dean's Office. They haven't gone back. Mark May took Dr. Winternitz's old office, but I think he let it get a little shabby so that he would feel better.

That's the sunshine of the place in a way, but you found in the administration of the school certain committees in existence which seemed to make for multiplicity of talk about subjects, extension of time in which subjects would be taken up, and there is a certain streamlining that comes about so far as affairs of the school are concerned, and I think the adjustment was in keeping with the feeling that you had toward the chiefs of the departments. I think you ought to explain that.

I've forgotten the names of the committees, but we'll get to them in a minute. Committees get appointed and tend to perpetuate themselves. Every now and then you have to sweep them out. You do this in the government and anywhere else. Committees are appointed for a specific purpose, a job, and when that's over they are reluctant to disband, but in an educational institution it's natural to be changing committees all the time. For instance, I imagine in the early days that they had committees on the immaculate conception, but that's hardly in keeping with modern notions of reproduction, and that has to go out. How long a committee of that kind stayed in depended on the parochial nature of the institution. There are committees that deal with admissions, curriculum, libraries, and various parts of the school. Some are useful, and some are not.

Some ought to be combined, and some ought to be disbanded. The attitude of the person who is in a position to change the committees determines a great deal their continuation, or their stopping, although, as I remember, I don't think that I disbanded any committees just by a flash emotion about it. You generally talk it over with the chairman of the committee, or some of the members. Which ones were changed?

The school was run, I think, by its Board of Permanent Officers.

It was run by the Prudential Committee. Nominally it's run by the Board of Permanent Officers.

This was the large committee.

Yes, but the Prudential Committee is an old Connecticut name for an executive committee, and it could do some things that didn't have to go to the board, but most all things having to do with appointments, promotions, educational policy went to the Board of Permanent Officers. Budgets didn't. As I remember it, budgets went through the Prudential Committee. No--I'm not sure of that. Budgets were too confidential, and they went to the Dean and from the Dean....

To the Treasurer.

Yes, to the Treasurer. The reason I'm a little confused is that in 1955, I made a survey of Tulane University Medical School, and I put the budget of every department in my report. When the report was done, the dean down there, when I asked him what to do about copies, said, "Send me a few."

I asked him if I couldn't send one to every head of a department, and he reluctantly consented, so at Tulane in 1955, every head of a department got a look at the budget of every other department, and it never caused any trouble at all. Usually they think, "Well, if that man sees that I'm favored by something, he'll crowd the Dean to favor him likewise", but it didn't happen. At Yale the budgets were not shared among the faculty members. They didn't have anything to say.

There had been two standing committees that were discontinued--one was the Standing Committee on the School of Medicine and the other was a Standing Committee on the Biological Sciences which did not meet and therefore was discontinued.

That was revived I think later on.

Yes, as a university matter as distinct from a school of medicine matter.

I don't remember any activity of the Committee on the School of Medicine.

It didn't meet either, but it was put up as something to which the Prudential Committee had to report, and it made for the delay and discussion of matters.

We got rid of that.

There were two other committees underneath the Prudential Committees--one on Clinical Subjects and one on Pre-clinical Subjects which was an arbitrary division that you didn't find very satisfying, and I wondered whether there was any effort to build a bridge.

Did we keep those?

Yes.

Well, there's a lot of interest in these fields, but I don't know that any of those departments worked closely enough together to deserve a committee on common problems. They are all so disparate--clinical departments go their own way, have very different problems, different personnel and different points of view. Same about the pre-clinical.

<u>The only other committee that is mentioned is the one on Public Health.</u>

Well, that was so because that was practically a school under the medical school. Public Health was kept at a departmental status, although Dr. Winslow would have liked to have it recognized as a school, but it had no endowment to speak of. It had the Lauder Fund which was a smaller fund, but its budget came through the Office of the Dean of the school, and its appointments went through the Prudential Committee and the Board of Permanent Officers. It had certain problems which didn't need to concern the other pre-clinical and clinical departments, and therefore a Committee on the Department of Public Health was a good thing.

<u>The others were the study units that we mentioned the other day, centers of intellectual activity, and your view expressed here in the annual report is that they are serviceable so long as they spark intellectual activity, but if they become fixed entities, you're for their disbandment.</u>

Well, they fostered intellectual activity by two or three mechanisms. One was to hold meetings of their groups at which members of the group would present their current work, or have outside investigators invited into the meeting. The other part of the work that the group did was supervise, or they could act in an advisory capacity over the activities of units within the group. The study group might have people from the Departments of Medicine and Pathology in it, and it's good to have some central place where they could pool their interests and not duplicate, or lose track of each other. Then they got some money after a while, these study units, and they began to make grants.

<u>One thing happened, I think, to which you fell heir by way of a yardstick, or an insight into the conditions of the school, and this was the rating by the AMA's Council on Medical Education and Hospitals which was as of conditions in 1934. This indicated, I think, the condition of the library, which is a subject I want to come To subsequently, but it was in pretty bad shape. There was some limitation on this Council's access to sufficient material to warrant some of the judgments they reached with reference to pediatrics, although clinically that was bad, and that involved the hospital, a subject I would like to come to in a minute.</u>

That was in Dr. Winternitz's time. I don't think I paid much attention to that.

<u>Except that it is a guide, a yardstick as of that moment, and I think in the Dean's Report you do indicate that they didn't have access to sufficient material to warrant some of the judgments they made.</u>

I think I probably left out of the report that they didn't have access to enough intelligence to deal with some of them.

<u>It's a signpost on the road.</u>

Those committees of the AMA are composed, as a rule, of very conservative practitioners.

<u>Yes, but they do issue scraps of paper, and one is compared with reference to all</u>

schools in the country, even if there are limitations on the basis of their comparisons. Then there is a development which shows that some thought about the needs of the school and the hospital was taken by the Dean's Office, and this is the "John Schoolcraft Preliminary Survey of the Needs of the School and the Hospital", and he comes from Hamlin and Browne, an organization that does this kind of work. Do you remember that?

Yes. You touch me on a tender spot.

Really?

Yes, when you bring that up. I had forgotten all about it because one tends to forget things that were unpleasant.

Mr. Schoolcraft was a professional investigator working for a firm, Hamlin and Browne, that did that all over the country in schools. There was another one in New York called John Price Jones, and we gave it the name "What Price Jones" because they would charge a great deal, come around the school, talk to all the heads of the departments and put in a report to the president that has already been given to him by his dean. They don't know anything about the school and they have to get whatever they know from the lucubrations of the people they interview, so they make these hurried surveys. This process is pushed on to the administrator of a place by the financial side, usually, of the institutions. I think John Schoolcraft came through Mr. Farnam's Office, but again, to go to this Tulane survey, or before that one, I was at Cornell-New York Hospital. There were three strong moves by members of the Board of Governors to import an outside investigator, a management consultant to go over things. They can go over supply problems and the housekeeping, but they don't know enough to touch any intellectual problems.

I'd better get a little water. Hold it a minute.

There are some of Schoolcraft's productions in these papers somewhere.

I haven't found that yet.

One little pamphlet. Well, those people are good in certain fields of management, but they are not good when it comes to the accomplishments of a member of the faculty, what he should be paid and how he should work.

In terms of structure--increases in the school structure, what is needed, in the reports here there is some comment about the increased need for facilities--library, clinical beds and so on, so that with all its limitations--you know, it is at least again a peg which shows that there is some effort to survey the needs.

Well, responsible administrators are doing that all the time. This man's second hand stuff that he puts in his reports is very impressive.

I was thinking of it in terms of what we do all the time--make a survey of manpower, and policy with respect to manpower is an effort to make some basis for the exercise of judgment. We have increased the use of this process, made it better disciplined, and this may be its beginning.

Those in my experience have been prompted by some narrow-minded official who wants to reduce expenses.

Oh boy! The very opposite comes out.

I soon had experience in this Dean's Office with one of the members of the Yale Corporation who would read my reports and read other reports and write me long letters about everything that was wrong. I don't know whether any of those are in the files, probably not. They're probably at Yale. Then another member of the Yale Corporation who was sure he knew all about medical education and hospitals was my friend Bishop Henry Knox Sherrill of Boston. Bishop Sherrill was on the Board of Managers of the Massachusetts General Hospital, and he was a guide, or influanced the opinion of members of the Corporation. The third one was Dr. Fred T. Murphy. He was on the Ford Hospital Board in Detroit. Dr. Murphy had been a surgeon in World War I, a very well-to-do man with relatively little practice and contact with medical schools, but he was the advisor of the Yale Corporation on medical school affairs, much to the detriment, I think, of the school in some respects because he didn't try to keep up. He was friendly, but he didn't know the modern things that were going on that had passed him. He was one of the self-perpetuating members of the Corporation. Yale has two sets of Corporation members--one are the original trustees, if you want to call them that, members appointed originally, and they have elected their successors ever since 1701, and they are elected to retirement age at sixty-eight. The others are elected for terms of three to six years by the alumni, but Dr. Murphy was one of the self-perpetuating, permanent members of the Corporation, so I learned from that a rule I hoped to promote at other places, and that is to have a succession of trustees more responsible for the affairs of an institution that is progressing through the years, all washed by changing waves, than this man who sits on the inside and doesn't know what is happening.

It didn't take you long to run into difficulties in floating an idea then.

No. You have to have an idea to run against the difficulties.

This matter of internal work in New Haven is made more complex by developments way beyond the control of the medical school, the implication for medical education and research in the conceptions, if not the plans, for participation of the federal government and the states in making provision for medical care which comes out at this time. There was a committee of physicians led, in part, by Dr. John P. Peters that sponsored a development against entrenched interests in the medical associations, an effort to alter thinking, or to make some new assessment. This led to a National Health Conference, the first of its kind, under Miss Josephine Roache in 1938. There was the first National Health Survey by the Public Health Service sponsored by funds from the WPA, so we had more to deal with. These were in the air. There was the President's Interdepartmental Committee which fell heir to that body of material which had earlier come before the President's Committee on Economic Security, but which had not been worked into the Social Security Act, particularly health insurance schemes which trace back to Professor Winslow's Committee on the Cost of Medical Care. All this was creating problems to which one would have to adjust and which were way beyond the power of a medical school, or a university, to control, and certainly indicated that the future was going to require something in the way of development within the school. Well, if you put those ideas in the context of 1935, as revealed by these reports, the emphasis is upon retrenchment. The first task you had to do was to cut fifty thousand dollars out of the budget which is the reverse of what seems to be needed. As I've indicated to you, there's a sudden increase in gifts of all kinds, from institutions, from foundations, from individuals, anonymous and otherwise, which seems to stem from revisions of the tax law in 1934, and 1935, so that for research purposes there are funds coming in greater sums to the school, but the school itself is retrenching which makes for problems in certain areas I want to come to at some other time like nutrition, or the library, or the animal quarters, and so on. Today I think we may be able to see locally the problems that medical schools were going to face nationally by dealing with the New Haven Hospital and the New Haven Dispensary and its problems. This is the center of the medical school for educational and research purposes, and the problems are just fantastic--incredible. It does have a role in the educational process.

Well, you can't conduct a school without it.

You give a very good account of this in this report which I'd like to put in because it sums up the problem. Let me read this:

> It is impossible to describe in a few paragraphs the most important of the intricate relations that bind together the University, the School of Medicine, at the New Haven Hospital and the New Haven Dispensary. A brief outline will have to suffice here. The attendant problems can only be mentioned, without full discussion or more than an indication of possible solutions.
>
> In the School-Hospital arrangements the two strongly directing influences are educational ideals and the conception of service to the community. The University is primarily interested in educational facilities gained by its alliance with the Hospital, but having undertaken to contribute to the support of a general hospital, particularly one which is in fact the municipal hospital of New Haven, cannot escape the obligation to provide a certain amount of medical care for the indigent sick of the district. The New Haven Hospital and New Haven Dispensary, having agreed to put their facilities at the disposal of the University for educational purposes, cannot conduct their affairs as a strictly business proposition....

It's open ended--you know.

> As institutions needing and deserving larger municipal support they suffer in the battle over taxes and charges which is going on between the University and the City, as is plainly shown in the previously quoted account of hearings before the Board of Finance.

You put in earlier the mayor's comment about the tax business, and I think the request to him was for twenty thousand dollars, an increased appropriation for the Dispensary. This is just twenty thousand dollars.

He wouldn't do it.

No, he wouldn't do it, but this is the basis for the school.

There is no dividing line between School and Hospital and Dispensary.

> This is a highly advantageous arrangement for the educational program of the School. If this coordination of affiliated institutions did not exist, the University could not have a first class four-year medical school. Without it the University would retain its superb departments in the general field of biological sciences, but the residue would not be worth considering as a two-year medical school.

That puts it on the line. Now, the University had problems. Economic fact discloses an annual appropriation from University funds somewhere in the neighborhood of four hundred thousand dollars a year, and a good bit of this is into this hospital. Where do you go?

The medical school had a relatively small endowment, and expenses were going up all the time. There was always a deficit, and the University made up that deficit by taking from its general income, and that general income was largely the income derived from dining halls and dormitories. These were situated in the Sheffield Scientific School, or the Divinity School, or the Yale College, and to take that money for the medical school prevented the Department of Classics, or history, or something else from doing something else with it. They wanted it. The University was, I would say, generous to the medical school considering the financial stringency of the time.

It finally reached a point where the Corporation had to put a limit on what they could do. They put a limit of a hundred thousand dollars. They wouldn't go beyond that. They appointed—meaning no disrespect to the previous superintendent; I think he retired—James A. Hamilton.

He was the director of the New Haven Hospital, and we brought him there. Hamilton was an able man.

Yes, he disclosed that the hospital was being run in an efficient manner. I think you were invited to sit with the committee on the hospital.

The Medical Board.

To oversee its affairs. They even went so far as to raise the ward prices to five dollars a day--do you remember that?

Yes.

And the mayor's reply encouraged the town, saying that unless it was a vital emergency, patients should go to the other two hospitals in the town.

They would go under too.

But here you are. You run a medical school which requires this hospital for its educational purposes, and this is a big drain financially. It puts a premium on efforts to get funds, some sort of endowment. There is a fund raising committee with Mr. Spenser Burger. Do you remember that?

Mr. Spenser Burger was a very public spirited citizen of New Haven, and the President of the New Haven Hospital. He was a rather well-to-do man, and he did succeed in raising some money, and he gave a good deal of his own. He was a manufacturer of corsets and some other things, but as an aside on your microphone here, one might mention the transitory essentiality of corsets. People thought at one time that that was to be a permanently safe investment. Brassieres had not come in when they put up the corset bonds. The same thing happened when they put up the canal bonds. People right here in Washington thought that canals would be a permanent form of transportation in this country, and they sold the bonds on a long term basis. All these things change.

I think there was an effort to extend the service of the school to an annual postgraduate clinic for doctors, to inform the Alumni and physicians generally in the State of Connecticut that there were services available at this medical center to which they could have access. This worked pretty well--that is, it was a beginning of a kind of public relations as to what was available at Yale, either to inform physicians generally, or to help them in the new things which may have been coming on.

There are two sides to that relationship--the practitioners in a state around the medical school--and one is good, and the other is discouraging. There are a number of enlightened physicians who support a modern progressive medical school and hospital and are glad to use its facilities. There were also a great many physicians, and there were in the State of Connecticut, who didn't approve of the Yale program for medical education. They wanted students to be turned out who could do minor medical practice right away and certainly would have glib answers to all sorts of questions derivable from manuals, one thing and another--memory, people who are turned out of school with nothing but memories of their work. Our ideal at Yale, Rochester and at Hopkins too was to teach a man method and to let him understand that every sick person is a problem and not to care so much whether he learned what the leukocyte count in German measles is on the fifth day of the disease, but to know that it is important to study the leukocytes. He can look up what the figures would be, but he must KNOW the principle of the thing. If you bring up students who have a research point of view on everything connected with medicine, they become concerned with methods, procedures of investigation, and they don't depend on a fallible memory so much. Lots of physicians in Connecticut thought that we ought to be turning out people who were very slick to pull out of their memories all of the facts that they might need.

In addition I found that there was a desire on the part of a lot of the physicians in the state to have their sons admitted to the medical school. They put all sorts of pressure on the Dean--all the way from policemen. The judges of the Supreme Court would come to the Dean's Office to get a son in, or help him get in. That was all right, if the man's son was an able candidate, but tragically enough, a lot of these young men had been inhibited by the behavior of their father's medical practice. They had seen their fathers practicing successfully with this money-type approach that I mentioned. They were pragmatic, practical physicians who had little insight into the processes they were dealing with, so that a student seeing a successful father with so little mental equipment, so to speak, going ahead would be disinclined to do the hard work necessary to study medicine. Some students we admitted from such a parental environment, and they didn't do well because they had an example of success without effort.

That's grim. Since you brought up the students--there was a complete overhaul of the admissions procedure. There were any number of tests that were discarded. I think one of the things you did was to make admissions a function of a sub-committee of the Prudential Committee.

The Committee on Admissions. We interviewed every student admitted there. I saw them all, and we divided them among the rest of the members of the Committee on Admissions. There are very good tests that a student can take as far as aptitude is concerned, but you can't be guided by them too closely. I think the problem of the admission of students is solved by an intricate synthesis of a lot of information about the person, and in my case I wouldn't put a value on each part of it. I did notice one thing soon, and I think Dr. Blake and others had the same feeling, that the man who comes to you--well, you always ask him why he wants to study medicine. A great many of them say that they want to serve humanity and do something for their fellow man. Usually you find such a student hasn't the motivation to survive the hardships of a medical course. He soon wearies of his efforts when he finds that the only thing that is supporting him is his humanitarian, or sentimental point of view; whereas the ones who come in and say that they want to study medicine because when I was a youngster I collected butterflies, or shells, or got interested in frogs--in other words, had a biological approach--they have some permanent scientific, or at least some permanent biological interest, and this carries them along. Through this interest they increase their own internal resources.

You never can tell for sure. They have pre-medical courses in chemistry and physics that were touched on, so to speak. The student as a rule had to do well in ordinary chemistry, organic chemistry, and at least the first course in physics, but the Committee on Admissions got sorry for the poor Dean who saw all the students and began to let him have a couple of what they called "Dean's choices." I would be able to take one or two men that I thought ought to come, although they didn't have any competence in organic chemistry, or physics, or something else. One particular one that I admitted on that basis had failed in organic chemistry and practically failed in physics, but he was a productive writer and scholar in Greek and the Classics, a very cultivated youngster from Charleston, South Carolina, and from a distinguished family of physicians. He graduated first in his class after four years in the medical school, although he would have been thrown out on his preparation.

The other thing about students that we used as a guage for their capacity was their National Boards--what's called the National Board of Medical Examiners.

They give two sets of examinations--one at the end of the second year, and the other at the end of the fourth year. These are nationally accepted examinations so that it is advantageous for the students to take them and pass them because if he passes the national boards, it's easy for him to get reciprocity from the different states. He doesn't have to take State Board Examinations in every state. Once he has passed these, he's eligible to pay the license fee in any other state that he wants to go to. Everywhere he goes he has to pay the license fee. They care more about that than they do about giving the examinations sometimes. These examinations by the National Board took the place of some examinations in the Yale School of Medicine, but not the promotion system. Some schools promote their students--I mean by promotion from first to second years and so forth--on the basis of their standing on the National Board Examinations, but it's fatal for an independent educational institution to turn over its promoting system to an outside agency, so we kept it in our own hands and often advanced students who did not do well on the National Boards and held some back who did. It's a useful thing to have. I used to be a member of the National Board of Medical Examiners. They've been at it so long that somebody has codified all the questions that have ever been asked, so it's easy to bone up for it and prepare.

Once having admitted a class, and the reports that you write are very good on the classes--a general over view--it's surprising the percentage of them that require assistance in going through school. NYA [National Youth Administration] is established in this period to help them. There are efforts toward a kind of bursary fund in the medical school to help them.

A loan fund.

Yes, but the statistics that show this are quite revealing about the content of the classes.

Well, we paid no attention to the student's economic situation, if we had any means of helping him by loans, or scholarships, or youth funds, of one thing or another. The Dean is in some very privileged positions in those relationships. I know at least two or three who were admitted while I was there, and they had no money at all. I knew one or two well-to-do men in the school, students, and told them about these economically poor students, and they paid their way through, provided I didn't disclose the donor's name, and I never have. We had all sorts of ways of trying to help them--even jobs, and these loan funds were very useful.

A continuing source of difficulty that runs through these annual reports are the poor faculities for housing and dining that they had.

Oh, they had squalid quarters around the New Haven slums, and the eating arrangements were poor, usually in a basement of a tumbled down house. Dr. Winternitz wasn't able to do anything about it. I wasn't able to do anything about it, but it's been much improved in later years. Mr. Harkness built a dormitory there. The men in the colleges across town lived much better, ate much better, but they didn't allow medical students to go into Yale College accomodations. On the whole though, they were very hard working, quite well disciplined students. Medical students go on a rampage nearly every spring somehow or other, and they did their bit, but it didn't amount to much.

I think what is of interest when you fall heir to the deanship are the ramifications of the problems you confront.

I was already familiar with a good many of those things. I had been observing Deans--knew Dr. Welch way back, and I had seen the other side of the Dean's Office handling similar problems, and then handling a big department at Rochester--they were all there.

I wanted you to put this in because next time, and we've gone about an hour, I'd like to show and illustrate with your help, the need for a library and its development, even confronting retrenchment and subsequently the appearance of the war so far as the building and its design is concerned. Another side, I think, because of retrenchment--there is attrition, not to say emasculation, of nutrition within the faculty, made necessary by the departure of some people to more favorable climes--Food and Drug Administration is one I remember, but this opens up a whole area that is not being covered by the school as such. The desire to meet that problem is in this Nutrition Institute which is a very interesting story. The other story which we will come to subsequently, which is a successful one, but not within the school, is the Childs Fund. I think they will tend to illustrate that the problem, in part, as a Dean was to beat the bushes for support somehow someway.

I don't think "beat the bushes" is the best phrase that you can use. Is that on?

Yes, it's on. Let me turn it off.

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As I indicated to you earlier, going through the records of Yale and your deanship, I'm impressed by the number of problems that come up for solution, and these, I suspect, can be boiled down to who to obtain, personnel, the student body, and then the actual needs, plant needs, ideas needs for the school and the funds to sustain them. Some of these are succesful. Some of the ideas are novel, new in terms of the time. One that you confronted, the Nutrition Institute, is a complex problem because it involves your continuation in the deanship. It's arbitrarily selected because it is complex and because it does involve a personal story. The records disclose that you didn't operate alone, but operated with knowledge and approval--which I would expect, and this is another thing you do. You operate with due regard to the sensibilities of the institution. You have that institutional sense. This is a strange story, and I wanted you to include it as an example of the sort of thing which, given this period of time, the Dean was confronted with--what to do with an idea, how to make it serve the needs of the school, and ultimately decided on grounds that have nothing really to do with the idea, or how to slip the clutch and make it move. I don't know how far you want to go, but in terms of the records that remain some indication of your personal views with respect to this problem ought to be in the record. Then we can parallel this with that other story which runs right along with this one--the development of the Childs Fund. This happened all at the same time. There's only twenty-four hours in a day, and I don't know how you did it all. There is attention to detail here which is also symptomatic of you--ferreting out the detail to be in a position to exercise your judgment, and it's disclosed in these papers. The nutrition story is an idea burgeons somehow within the University itself, the treasurer's department perhaps, in December of 1938, and you're very shortly involved in an exchange of views with the principals. Why don't you tell me what you remember of these events?

My recollection of the beginning of this is very hazy. I'm not sure where the original idea about exploring a possible partnership with the food manufacturers came from, but about the latter time of 1938, somehow or other I met with John Wesley Dunn who was the lawyer for what are called the Food Manufacturers. They don't manufacture food, but manufacture containers, the processing of foods, cans. The President of American Can and groups of people who were concerned with packaging meat, vegetables, and all sorts of things. We called them "food manufacturers", but as I say, they manufacture the containers and process the food. They don't make the food.

There was abroad in the realm of ideas some perception of the current importance of nutrition, nutritional research, and the need for further developments in teaching and training chemical nutrition people, dietitians, all sorts of people who could help improve the provision for and dispensing of foods. Nutrition seemed to be a proper thing to include among the activities of a school of medicine. It seems hardly arguable since the universities were much concerned with agriculture and the process of agriculture, notably Cornell. Most universities had agricultural connections. Personally nutrition was interesting to me from my connections with Joseph Goldberger and pellagra through family relationships.

As I say, I have forgotten how this all started, but it moved along very fast. Mr. Dunn who was a very reticent, thoughtful lawyer, very precise, pushed it along through his personal connections with Mr. Clarence Francis, the President of one of the large food manufacturing companies, a Mr. James A. Adams, and some others, and out of these preliminary talks, into which I soon brought the Treasurer of the University, Mr. George Day, we developed a proposal that would be submitted to the University.

The essential features of this proposal were that the food manufacturers would agree to provide, under conditions always subject to the approval of the University, funds for a building, income for operation for a certain length of time, and funds for use for grants-in-aid. They would also agree to a governing board composed of members of the University, distinguished public figures, and representatives of the industry. Certain officers would be appointed by this board and policies would be determined by the board all subject to the final approval of the University. The function of the Institute of Nutrition, as I recall it, was to be educational on a broad basis--for training in nutrition medical students, dietitians, nurses and actual food technologists. The service functions of the Institute would be those connected with making tests and special studies for the contributing industrial group. They would have an opportunity to have the work done at the Institute on problems of concern to their particular manufacturing process, and they would pay for the cost of that, for materials and for some of the salaries of some of the people who would be doing the work.

In the proposed agreement was provision for a Professorship of Nutrition at Yale with the salary to come out of this industrially supplied fund. The proposal also contemplated the erection of a building, an extension to the south of the Sterling Hall of Medicine in Cedar Street in which the Institute of Nutrition would be housed, just adjacent to the existing Department of Physiological Chemistry, and that building would be paid for through the industrial contributions. Talk about this proposal between Mr. Dunn, Mr. Francis and myself went on for quite a while --Mr. Day in and out of the talks--and got to a point where I was able to put down on a piece of paper a summary of the proposal. This summary of the proposal was submitted after preliminary talks to the President of the University for presentation to the Corporation, and I have a record of that in these papers here. I have given a summary, but I can say that as early as on May 29, 1939, President Seymour wrote to Mr. Charles Wesley Dunn referring to this outline of a proposal which Mr. Dunn and I had made and which I had approved, and saying for his part that he could assure Mr. Dunn that the University would be happy to enter into this type of arrangement, that he would be glad to proceed from this point with legal examination of the phraseology of a contract, so to speak, and with the final drawing up of papers for submission to the Yale Corporation and the Food Manufacturers for adoption.

I went ahead all through the spring of 1939, and into the summer and fall with a good many more talks and increasing enthusiasm for the fundamental ideas in the conception; namely, the partnership between Industry and a University, between Yale University and the Food Manufacturers, in an educational and research undertaking of considerable magnitude in a field that was obviously very important and easily predictable as one of the developing areas of scientific medical and public health work not only in the United States, but in the world. It seemed to me to have great possibilities for the advancement of the University's renown and the University's contribution to knowledge and service, and it seemed to me to be practically of benefit to the University by the funds that would be brought in to erect a building that was needed, to provide a professorship that was needed, and to provide opportunities for the University's participation in an educational program. Most of those, in fact, all of those major issues that were of primary concern to the University could not be undertaken at this time because funds were not available in the University treasury. The food manufacturers were enthusiastic about the plan because they saw benefits to their own industries properly, and they saw an opportunity for them to make--how can I say it--a patriotic contribution to the welfare of the country, and I think they had the highest motives.

Well, this carried along in the medical school with occasional reports by me to the Prudential Committee of the Yale Medical School and some rather long talks with some of the professors about it. It led in the late fall of 1939, to the drafting by Mr. Frederick H. "Fritz" Wiggin, the legal adviser of Yale University, of a draft of a contract, and that was done, I think, in December of 1939. After having worked over this with Mr. Day and Mr. Wiggin it was submitted to Mr. Charles Seymour the President of the University for consideration at an oncoming meeting in December of the Yale Corporation. Mr. Seymour was prepared to submit this proposed agreement to the Yale Corporation at the mid-December meeting, but as I understood it--I wasn't present at the meeting, but I was told that Mr. Seymour hardly began to speak about it when there was a very strong attack on the whole thing led by Mr. Dean Acheson. I never knew his [Mr. Acheson's] reasons for doing that, so I would only be making surmises, if I should guess them now, but I have a written statement which he approved as a member of a subsequently appointed committee to consider this matter with me and in which he put the reason on the very high ground of the preservation of the integrity and the independence of the University against possible corroding commercial interests.

Well, Mr. Seymour, I understand, did not tell the Corporation about the letters of approval that he had given me in May for the activity I was carrying on, the work I was doing under letters in which he said that the University and he would be happy to have this undertaking prosper, so the Corporation at this meeting--somewhere in mid-December, on a Saturday--immediately appointed a committee to consider the matter with me as Dean. They very courteously came over that Saturday afternoon to the Dean's Office which was in the School of Medicine. Considering the dignity and the eminence of the committee members their coming over to meet with me was a great compliment and a respectful action. The committee was composed of Mr. Acheson, Mr. George Day, Dr. Murphy, Bishop Sherrill, and Judge Thomas D. Thacher. They came into the Dean's Office, and we had a long meeting of about three hours or so. Their opinion was expressed really at the start and not changed by the long discussion, and that opinion was essentially as I've outlined, that while Yale would be interested in this field as an academic undertaking, it was not interested in having a relationship of the type proposed with the commercial manufacturers. They didn't draw up a final report at this meeting, but they adopted some phraseology which was largely produced by Bishop Sherrill, saying exactly about what I've already said, saying that the Corporation members could not allow the University, or any officer of it, to put the University in the position of having this kind of relationship with the commercial food manufacturers.

That's the way it was left at the end of that meeting. The committee then reported, I think, verbally to the President and maybe gave him some memorandum, but on December 10--well, the Corporation meeting was on December 9, 1939, so it was earlier than I indicated by my earlier remarks. Right after the committee had met with me, I wrote the President a note about the meeting and gave him not only a summary of what had taken place, how Mr. Day and I had explained not only the proposed mission of the institute, the proposed governing of it, the proposed financing of it and including a frank expression of the plan that provided for a temporary annual gift rather than an outright gift, but we were talking in terms of over a million dollars for the whole thing, but I also wrote

President Seymour that while I should deeply regret to see this undertaking fail, I had a feeling that if the University adopted the point of view of this special committee, it was probable that I would not be able to go on as Dean.

That's the way it was left for a number of days, until later on in the month when I had had one or two talks with Mr. Seymour. He went away for a Christmas vacation and came back--and at the end of that time I told President Seymour that the action of the special committee, which obviously had the approval of the Corporation, and his failure to support the approval that he had given me previously, earlier in the spring, for proceeding with the negotiations made it impossible for me to go on with the deanship for another term. The reason I could say that I was in a position to speak of another term was because the Corporation had already appointed me Dean for another term, beginning July 1st, 1940, to run until 1945. I felt that if I took this licking, I would be no good for my school and no good as an officer in the administration under Mr. Seymour because I can't conceive of a person, any person, so defeated after thinking that he had the approval of the highest officer in the University, going ahead and pretending to be a loyal supporter of his chief when he couldn't possibly be a loyal supporter under those conditions. Well, I did tell him that I would resign, and I thought I wrote a letter of resignation, but apparently it's not found in these papers. They may be in the Dean's papers up at New Haven.

Mr. Seymour was apparently surprised at the action, and I know that he was very much upset. Everything that happened after that, asking me to reconsider, or expressing any opinion on my resignation was all highly favorable. It was a very unhappy situation in spite of the fact that these commendations were coming in on account of my past activities, including a commendation from

Mr. Seymour, but I went ahead after that and finished out my term as Dean, ending June 30, 1940.

The timing of this is important--in the sense that this matter of nutrition jells at a time when the committee is appointed to seek the reappointment of the Dean in December of 1939. You did have some misgivings about the Dean's Office in terms of the load, the amount of work, the need for assistants--well, at the same time you were trying to balance other work on which you were engaged; namely, the Childs Fund.

I didn't in the Dean's situation involve any consideration of the Childs Fund at this time because I think that had been more or less concluded before all this happened, but, you see, they start to consider the next Dean in December of the last academic year of his tenure as Dean. They had the usual meetings in December, and they came up with a recommendation for my reappointment --I mean, the faculty of the medical school, the permanent officers.

I wrote to the President expressing very great appreciation for that and for the indication that he had given me that the Corporation would approve the faculty's recommendation, but I had some things in mind that were needed for the Dean's Office and for the school that I wanted to talk over with him and try to get settled before accepting the deanship. There was a group of questions that concerned administration, finance, and personnel. I wanted an assistant dean to help with the administration of the school. I hoped to get a salaried physician as a health officer for the students, an associate dean, and more help for the executive secretary of the school. The executive secretary was Miss Bishop, and she was terifically overworked with what had to be done. There was always a need for supervision and maintenance of the buildings. Those big buildings were very hard to keep up to Yale standards. The Yale standards were very high on building maintenance, but they couldn't be carried out even, I should say, two-thirds with the then existing allowances for the Yale Medical School. It was too much, and yet they needed repair, and they needed alteration. Those were the things, and I had other questions of policy. When one is working in the position of a Dean he sees thousand of problems that he can't solve, either because he doesn't know what the answers could be, or should be, and usually in the case of a burgeoning medical school like Yale at this time, the solutions depended upon the availability of funds, and they were short of funds. About those things I wanted to talk with the President, and I did have an opportunity to speak with him around the middle of December. Most of those points he agreed to. He was quite willing to make those allowances, add administrative assistance, and a paid position for the care of the health of the students. Those were settled, at least they were settled by saying that they would be granted.

I think the top paragraph here is what broke the camel's back.

Well, the paragraph--I said in the letter of December 10th to Mr. Seymour that the most serious problems were those related to the plans for the Institute of Nutrition which had been discussed last week wuth the Corporation's special committee, and I did tell the President--I read it here now--"The decision reached involves questions of my judgment and regard for the welfare of the University. It involves also the relation of the dean to the president and other officers of the central administration and my colleagues in the school and the outcome of further discussion of the committee's action will have a profound effect on my standing and my fitness to serve the University usefully."

Well, the outcome was even worse than the committee's formulation of their ideas because it was an absolutely closed issue after that.

What intrigues me, and I don't know that you have anything to add as to how they got to this final vote.

You've found in a file a handwritten note in my handwriting marked "A copy of the Yale Corporation's records of a vote taken on May 11, 1940, voted on a recommendation of the Committee on Educational Policy"--that was a new committee that had been formed--"that the establishment of a National Institute of Nutrition be approved in principle, it being understood that Yale's participation in the institute should be conditioned upon approval of the Yale Corporation of the personnel of the institute's board of directors and advisory committees and the plans for its organization and functioning."

This is a general statement of procedures and authorizations that were in the original planning, that all of this be approved by the University. This vote approved in principle and leaves out all the details that I have mentioned--the financing and methods of governing the institute and its functions and its mission, the building. This is a broad statement.

But the sub-committee of the Corporation set limits for the discussion which had already been rejected as between the University and the food manufacturers.

Yes, and they wanted me to go back and see the same manufacturers that had told me originally that those limits would not be acceptable. I couldn't do that.

What I have thought since this happened was that it did come as a new subject to the Corporation at that time, although the President and the Treasurer knew what was going on, and Mr. Seymour in his letter to Mr. Dunn said that he had not been able to bring this to the Corporation, that he had saved time by speaking to some of the members personally, and that he could now write to Mr. Dunn that we would be glad to do it. I think that was less impressive than if it had been reviewed earlier by the Yale Corporation, and I have a feeling that Mr. Acheson didn't know much about it before he came over to the Dean's Office. It was a new thing.

<u>A great deal was lost in terms of its potential.</u>

I think a great deal was lost because nutrition has forged ahead as one of the most important subjects in world economy--food for the population that is overwhelming the space of the earth is a major consideration now. There have been great advances in nutrition, and there are still great advances coming on. My interest continued because from 1950 on, or 1951 on, I've been a member of an extraordinary committee called the Interdepartmental Committee on Nutrition for National Defense composed of members of the Army, the Navy, the Air Force, the State Department, the National Institutes of Health, the Public Health Service, the big agencies like AID, and with a far ranging program and studies of nutrition practically of thirty-five countries in the world so far, and it's done a lot of good, but I'll tell you about that later when you get around to it.

<u>The letters to you--they don't reach the question of the institute because the reasons given by the school--I said "by the school." I don't know. In any event, what is reported in the press is the overall pressure of work and the desire you have to do certain specific things--like continue with the efforts to seek endowment for the school, to continue to devote attention to the Childs Fund, and to get back to work in bacteriology which you'd left some time ago, but the</u>

expression of support--there's a collection of letters here that are just incredible--to you personally. Yerkes, for example, all the way from Florida. We haven't talked yet about his desperate plight, one of the problems, but I showed you Emerson Tuttle's letter.

They were wonderful letters. They came from people who might have written quite contrarily, if you just take disagreements of opinion, but these are very personal letters.

I don't know the extent to which the school understood the basis for this. Let me say that I would expect that you would not disclose the deep personal questions of reliability and integrity which were involved in the non support of a view which had all the appearances of being supported up to a point. You had the rug pulled out from underneath you.

I don't think after this happened I went around and talked about it at all. I don't think I made any defense. I don't think I made any defense with Mr. Seymour. I think I just said that this has been done, and I can't take it.

Right, and then I think it was dropped and you went on. Incredible--you know, what was the state of nutrition at the school?

The nutrition had been a subject of interest at Yale for a long time. Professor Chittenden shortly after the Civil War became a leader in physiological chemistry in America. He had studied abroad in Germany on the composition of proteins, food stuff, and peptones, and he was himself a dietary faddist because he ate a little bit, but he almost fletcherized, chewed a certain number of times. Mr. Chittenden was a small man, slight, not tall, with a pointed beard, glittering eyes, springy step, full of energy and ambitions, led the Sheffield

Scientific School into the rank of an almost independent university and greatly admired him. He had with him men that moved over to the medical school--two men with very deep interest in nutrition, Lafayette B. Mendel and Arthur H. Smith. Then there was George R. Cowgill there also, so that there was a nucleus of nutritionists at Yale. Yale had a School of Nursing that was on a high grade--we'll talk about that later too, but the School of Nursing required a bachelor's degree for entrance, and the School of Nursing had very intelligent women concerned with the nutritional state of patients, and they just weren't merely dietitians serving plates of soup. They wanted to know what was in the soup and how it was metabolized. Public Health naturally was interested in nutrition because of all the deficiencies that are consequent upon defective nutrition and the supply of food and types of food. The social conditions of people in relation to their diseases, or deficiencies were of considerable interest to Dr. Winslow and to others who were studying the cost of medical care and surveying the health of the people, so there was a very good tradition of nutrition at Yale, but not a well endowed, or opulent effort.

<u>There was something to build on and much interest.</u>

As far as informing the faculty of the stages of these negotiations, I must tell you truthfully that I didn't because the conversations between Mr. Day, Mr. Wiggin, Mr. Dunn, Mr. Francis, and myself were fluid conversations. They weren't fixed. They weren't ready. They probably wouldn't be ready for disclosure until a contract was agreed upon, and you find out in life, or when you're Dean, or anything else, that when you broadly discuss uncertainties, you add to the uncertainties. I didn't feel at liberty to talk about some of these things in general, but they did know, the Prudential Committee did know, and I had one or two, as I recall it, very serious, sort of controversial talks about it with Dr. John P. Peters who himself might be considered in the nutritional field. His great study on body water was, in a sense, a nutritional study, and he was interested in nutritional metabolism, a very brilliant man. I would like to tell you about Jack Peters, but I really don't want it on this record.

On the part of people who didn't understand the real basis for your disinclination to continue with the deanship how many brought pressure on you to continue? They were pleased with your deanship, I'm sure, because their letters....

Yes, you showed me one from Emerson Tuttle, if I can immodestly refer to it myself. President Seymour said somewhere in one of these letters that he thinks that my deanship was the happiest five--oh, he said,"I cannot remember any five years of a Dean's administration here more successful than yours."

That's an accolade.

Well, when the school faced a change abrupt as it was, without a kind of forewarning--I don't know that you can have a forewarning. This is the normal course of events, and if you decide for grounds that are reasonable to you not to continue I suppose they have to accept it.

I was chosen, considered, and appointed in about nine days, or ten days, and the Lord giveth and the Lord taketh away in about the same time.

I guess you're right.

This didn't take much more than nine days.

No, but the search for a new Dean....

Yes, they started right away on that. They discussed it with Alan Gregg who would have been a wonderful Dean, but Alan Gregg was not interested in school administration. He was the administrator of a great section of medical education and medical research of the Rockefeller Foundation, and Alan Gregg, tall, eloquent speaker and writer and it--he was very wise and very attractive, and he didn't want to be Dean, and I think he told them so. What they did--they compromised and made Dr. Francis Blake, the Professor of Medicine, also a very able man, the acting Dean for the year, and he remained Dean until--this was in 1940, and he was Dean at least until 1950, at least ten years. Then Hugh Long took on as Dean.

That's an interesting period. Do you have any sense of criticism of your deanship? You indicated to me the other day that in the development of overall plans that you weren't a planner. You said this. I don't know that you had any indication of this because I've bumped into all kinds of plans that were going on, maybe not an overall plan.

I told you that there was criticism of me for being not the kind of planner that Winternitz was, for example, broad conceptions of a whole mission, a whole school. I had many plans for individual situations, and most of these came to me for dicussion from the heads of departments. I didn't feel that I was a messiah of medicine.

That's a good way to put it, but then I think also the times did their bit to condition this because the emphasis was on retrenchment.

Yes, the times--well, it was rather dismal in some respects.

We'll get to tomorrow, if we can, the positive exploration of funding, the Childs Fund. It ran parallel to the nutrition study, and it involved planning

<u>both with reference to possibilities inherent in the school and beyond the school's control as an idea which is new and novel too.</u>

Let me say it before I forget it. You say it was "planning." It was accidental.

<u>To you--it came over the transome to you.</u>

Yes.

<u>But the manner it was established once it functioned was a plan of great significance so far as the school was concerned--it eventuated in a positive way, though the detail of its beginning may be quite accidental and indeed was.</u>

There are two ways that I think about planning. One is to sit down with a blank sheet of paper and draw out a scheme of urban development or whatnot. That's the architectural type of planning. That applies to intellectual planning too. You could just say, as you would of a house, that you want so many rooms for this, or that and say of a medical school that you want so many departments of this and that and sit down and draw them up from nothing, but the kind of planning I was engaged in, and you have to in an institution like this, was to deal with the sections of it that are already in existence. Mr. Angell told me one day when I was talking about the future of the school,"B-J, you know, these great institutions have a momentum of their own, so it matters very little what anybody in a position of authority does with them."

I think that's true.

<u>It is a wise comment.</u>

He didn't mean for me to lie down on the job.

Oh no.

But there's something mysterious about an instituion. It has a life and vitality. His word was "momentum." It does continue in its direction, unless it's acted on by some external force.

But in that period when you had to decide whether to be or not to be Dean--it was like jumping into a huge swollen stream,

Yes.

And a good bit of the atmosphere and the problems you confronted as Dean related to a crisis over which you didn't have any control--the whole sense of retrenchment, the depression, and the new and novel ideas that were popping out of the ground, new organizations to challenge the older, staid, and more conservative organizations.

Ideas of medical care and social elements in medicine were in a state of flux and argument at that time.

I guess we've exhausted this.

Let me read Gladys's letter. [Gladys Seymour], I see here that Charlie was....

Thursday, May 12, 1966 A-60, N. L. M.

I've already indicated that I would like you to deal with the developments in the medical library at Yale today. Again, putting this in a context--while it may be a little too logical--I'm aware of the fascinating people who are involved in the development of the library, but nonetheless there is a crying need in the Dean's Reports about this facility, its role and function in a medical school, and its condition is described as wholly inadequate. That's not only the judgment of the school itself, but also of the AMA Committee which came around to assess the school--well, there's just this enormous need viewed against this period of retrenchment in the 1930s. What to do about it, how to get it started? It's a human story too, quite apart from the economics, and there are a number of very fascinating people involved--Dr. Cushing with whom you'd had some relationship all the way back, I believe, to Hopkins.

Yes, I was there when he was there.

Amd Dr. John F. Fulton, a strange fellow in his own way, and any light you can shed on the marriage between the times, the need and the personalities here to develop what I have pictorially represented here as the historical library and the working library.

What date do you want to take off from?

The earliest date you have is in the Report of 1935-1936, and it's that year....

That early 1935-1936 Report, my first Dean's Report, points out the inadequacies of the library. It was a very small collection housed in inadequate quarters, and it lacked money for acquisitions for new books and subscriptions to journals. It had very little prospect at that time of getting any increased resources either in funds, or collections. Everybody knew that something ought to be done about it, but they didn't know how to do it, or when it should be done, and, as I recall, the building up of the medical library was not in any high priority in any scheme of fund raising. There were so many more urgent needs for professor's salaries, staff salaries, or for the support of the working departments, that the library appeared a little bit off on the side and didn't arouse enough interest.

You asked me where the library fit in to the functions of a medical school, and I take it you mean in the course of medical education. It has a bearing on every phase of the life and activities of a school of medicine that has any educational ideals, in the first place, and any service ideals, in the second. It is a store house of all the knowledge of the past, a place where you can meet, or come in contact with, the thoughts of the past. It is also a meeting place for the recent past as well as for the far past, or the distant past. It's a place in which doctors and students can come into contact through the perusal of journals with the current thought of all sorts of physicians and investigators of the time. Not only is it a repository of ideas and thoughts, but the very physical nature of books, illustrations, portraits, and diagrams and all the appurtenances of publication have in medicine, I think, as they do in any other professional activity, a stimulating value just of themselves. It's a wonderful thing to have a copy of Vesalius, we'll say, the great anatomy of Vesalius in your hand.

That book had an enormous influence on one of the founders of the Yale Medical Historical Library; namely, Dr. Cushing. He had a copy of the Fabrica. For me, thoughts about a library in a medical school went back as far--I link it again to my grandfather Joseph Jones who filled the house with books, mostly his own compositions, but they were there. Then at the Johns Hopkins visiting the library, studying in the library, and going to original sources was almost as natural as going to the current lectures. It was in the air. Dr. Welch did it. Of course, the great bibliophile there from the very beginning was Dr. Osler, and the Osler Library in Toronto now is one of the greatest collections of medical books, and Dr. Osler influenced very much the man who was probably the main actor in raising the curtain for the drama of the Yale Library, Dr. Cushing. Dr. Cushing was very much influenced by Dr. Osler, probably was lured into the collection of books by the example of Dr. Osler. Dr. Cushing had been an Associate Professor of Surgery, a great experimenter at the Hopkins, and he carried that same impress with him to the Brigham Hospital in Boston and brought it with increased richness and power to Yale when he was given a place there through the activity of Dr. Winternitz after Dr. Cushing's retirement from Harvard Medical School. I think that must have happened early in the 1930s, when he came down to New Haven.

Well, it seems axiomatic that the library should be the heart of a great institution of learning, and so it was at Yale. The Sterling Library is one of the greatest libraries in the country, and in addition at Yale they had very rich departmental libraries which was one thing I and some others worked against when we began to build up the Yale Medical Library. Lots of books that ought to have been available easily to the medical students were in the departmental libraries of chemistry, physics, or biology--all the way across town--and since this Yale Library was built up, many of those departmental collections have been put in the Yale Medical Library.

The time was ripe. The need was there, and the time was ripe for doing something, and the doing of it, I think, came about I really feel miraculously and accidentally through the presence on the faculty of two great book collectors; namely, Dr. John Fulton and Dr. Harvey Cushing. They knew and trusted each other, and they had friends also among other collectors; namely, Dr. George M. Smith who had a great collection of books, and Dr. Edward Streeter who had the most famous collection of pharmacies, measures and weights. They had another friend in Switzerland, Arnold Klebs, the son of the great bacteriologist Klebs, who had much to do with early studies on tuberculosis and diphtheria, was a man of culture with an interest in history which he passed on to his son. Dr. Arnold Klebs in Switzerland had specialized in the collection of incunabula, and he had probably all the incunabula of medicine. He had a huge collection.

Drs. Cushing, Fulton, and Klebs slowly formed I won't say a partnership, but a very close relationship bound together by their interest in books. They were influenced by the need of Yale for a great library in the medical school, and they were, or at least Dr. Cushing and Dr. Klebs were reaching the point in their lives when they were beginning to think of the disposal of their rich collections. Dr. Fulton's enormous collection which he had been making since he was a student at Harvard was in the possession of himself as a young and vigorous man who expected to live quite a lot longer. He was not yet ready to give it outright to Yale until he knew what these other men were going to do.

Now this takes us up to about 1936--about that time. There was much talk and negotiation, letter writing, as to when Dr. Cushing would make a move, Dr. Klebs make a move, and Dr. Fulton make a move. Somehow or other they managed to move along as a congenial trio. The first real step to the library project--If I could call it that--was Dr. Cushing. He made it very plain that if provision could be made for the housing of his books, he would give them.

Once he did that, his friend Klebs said that he'd do the same, and in due time Dr. Fulton promised to give his books, but there was no place for a building, and there was no money for a building.

That's where the Dean came in mostly because he had a little say about calling meetings of people and dealing with the authorities in the University. Very important in the influence upon the University was a close friendship between Dr. Cushing, Dr. Wilmarth Lewis, and the Treasurer Mr. Tom Farnam, and it was really through them, after they'd convinced Mr. Farnam, that the Corporation at Yale voted to make available a sum of money from the Sterling bequest, the same source of money that had built the Sterling Hall of Medicine. There was a lot of land still around Greenwich, Connecticut, in the Sterling estate, that could be sold for purposes like this, and they said that they would build a building for the historical library particularly, and, of course, nobody wanted just the historical library. The ideal was a whole modern library as well as a historical library. They decided then that they would be able to put up the money for a building in part from Sterling funds, for the historical library chiefly, and in part from regular University sources.

There was much dispute as to where the library should be. The lot of land where the Sterling Hall of Medicine was situated was crowded with buildings, but there was a vacant space on Davenport Avenue near Cedar Street, where Mr. Atterbury, the architect employed for this plan, thought it would be fine to build a beautiful marble--what I would call a mausoleum for the housing of this library. Dr. Cushing and I felt opposed to that and so did Dr. Fulton because it put the library out of the normal traffic of the students and the faculty members. They might visit it as a curiosity, but we wanted something for them to fall into, so they decided that the building should be on the lot of the Sterling Hall of Medicine, but where?

At that time one wing of that building extending from the center ended in a very large animal house. If you'll turn those plans around, you'll see how you'd enter. We're talking about a plan of a building. This is the entrance way, and right about here was the animal house--full of dogs and monkeys. It served as the animal quarters of the whole school, and yet this was the very location that was most favorable for the library. It was difficult to get any agreement from departmental heads as to where the animals might be housed. Finally some decision had to be made, and I took the liberty of issuing a suggestion that the animal house be abandoned, torn down, and the animals moved over into the basement of the Brady Building. That was done through the good will of the people concerned and to the great discomfort of the people over in the Brady Building. The stench from the animals went all through the building, through Dr. Winternitz's department, through the Nursing School which was housed on the first floor, through bacteriology up on the third floor and teaching on the second floor. Of course that was on the land where the New Haven Hospital was situated, and the noise of barking dogs and mewing cats was disturbing to the patients, I'm sure. But that's the only place we could find to put the animals. We knew it was temporary, and, as a matter of fact, since that time they have a modern, air-conditioned, soundproof animal quarters that are beautiful and very salubrious.

Well, having decided on the place for the library, the plan then developed in the form of a Y, a wing extending out from the center of the building and extending out in two wings in a Y shaped manner into the back yard of the Sterling Hall of Medicine property. It was a scheme that gave plenty of light, plenty of ventilation, and quite a lot of room. The building was planned there to go down below the ground about three stories, to be thoroughly air-conditioned, rising above the ground about three stories, and that's the way it came out finally.

There was one period of great uncertainty and that, as I remember, was 1939, just after the outbreak of World War II in Europe. The occasion of the outbreak of the war was taken by certain officers of the University as an excuse for proposing an abandonment of this plan at this moment. Their argument was that if the war was coming on, steel would be in very short supply, costs would go up greatly, and it would be difficult to get labor. Every possible objection was brought in, even in 1939, when the United States was not in the war, but as a matter of fact I know later from the things that I'm studying for the history I'm writing of Preventive Medicine, that there was a very early perception of the possibility, or the probability that the United States would join in the war.

President Roosevelt declared a "limited national emergency" in September 8, 1939, one week after Germany had invaded Poland, so it was reasonable that the authorities at Yale, who knew all of that, would think that this library project would become involved in wartime stringencies, that it wouldn't be a favorable moment to undertake the construction and equipment of a library. There were some very emotional meetings and much worry. Some of these conferences took place at my house on Trumbull Street. Others took pace at Dr. Fulton's house, and at Dr. Cushing's house. Mr. Wilmarth S. Lewis was a moderator of a good deal of this and brought to bear some very good sense and power because always as a member of the Corporation he supported the libraries and museums at Yale. The central University Library, the art school, the Peabody Museum all benefited from Mr. Lewis's effort. Fortunately the authorities of the University saw the opportunity to attract these irreplaceable collections of Dr.Cushing, Dr. Fulton, Dr. Klebs, Dr. Streeter, and possibly something from Dr. Smith, although he didn't put much into it, to the University if there were suitable housing for them on the medical school grounds, and so that prevailed.

It's been a very great success--renown in the United States as one of the most valuable medical libraries in the country. It's integrally woven into the fabric of libraries at Yale. It has relations with all the libraries in the country. It's extensively used not only by the students and faculty, both the historical and the modern medical library, but also by the physicians of Connecticut. They have free access to it, and there is a loan service for them to it, so in many, many ways from the human side as well as the intellectual side, it has been a most valuable addition to the school and to the University.

Now, what kind of thing should I say more about?

This plan--just to fix it in time--was approved both as to site and general lay-out by the Corporation on May 8, 1937, and the appropriation, I think, of $600,000 was voted by the Yale Corporation on June 21, 1939.

You see, they approved the plan two years before they approved the support of it, and, as I say, the plan existed there, but when the war came on they were just going to let it stay.

It just came on the eve--June 21, 1939. I'm interested not only in the collection of books as a form of--I collect books.

So did I.

Yes, so did you. I think ultimately you deposited....

I gave Yale about four thousand books, but the ones I gave to the Yale Medical Library were the books I had collected in my own field. I had practically all the original publications in bacteriology. Now, bacteriological publications took the form of books not in the textbook line, but in the monographic line, they were what you'd call pamphlet type books. They were specialized monographs on smaller subjects, parts of it. Most of the bacteriological literature was spread in the journals. Pasteur, however, published considerable books on the diseases of beer and wine, his molecular dissymmetry studies, and I had all of Pasteur's books. Robert Koch, the other great founder of bacteriology, published mostly in journals. His work has been collected, but the original works are in journals. I gave all that to Yale. It was in the air to do things for this library.

It sure was.

Mr. Starling W. Childs and Miss Alice Coffin got interested in it, and in addition to the Child's Fund for Medical and Cancer Research, the Childs Estate and Miss Alice Coffin, who was Mr. Childs' sister, gave money directly for the support of the library. There's a Childs Fund for the library separate from the medical research. In addition, on the advice of somebody--I don't know--very wise advice, the Dean set up the Yale Medical Library Associates and got quite a great group of fine people to pay ten dollars or more a year, sometimes more, to help give funds for buying books. One of the persons I was fortunate to meet in this effort to get books and associates for the library was a lady in Pittsburgh [Mrs. Rachel McMasters Hunt] who had a great collection of herbals. She gave only a few of those books to Yale, but she was a great help. She was a relative of Mr. Childs too. Students got interested in it, and people gave money for subscriptions. It came right along. Now, I don't know how many journals they take, but it's really an up to date place as well as a fine historical laboratory.

One person we've mentioned who has had some continuity with the library since its opening, its dedication--Dr. Fulton, and the records that are here covering those early days after it opened, show the difficulty of funding the actual work of the medical library which required some help--the Starling Childs Fund. It's a beautiful place--as you can see from the representation here.

Dr. Cushing provided for support in his will, and Dr. Fulton's wife, Lucia, had a fortune of her own, and I think that John, a rather impecunious student, purchased most of his books through gifts from her , and she's very modest. You'd hardly know how much good she does. I'm sure she must have contributed to the support of this library. Certainly she did for John's collection. The library is a beautiful building. It's tastefully designed. It's an inspiring place--good for exhibitions, good for meetings.

Has it served in terms of its original design as a catch-all?

Oh yes, it's crowded all the time. They walk in there and spend a lot of time--so much so that it's opened 'till midnight. The medical historical side is built in an ornate Tudor combination, great huge beams, pendants, pendentives, galleries and carvings, whereas the working, or at least the current medical library is a stark, industrial type of building. You can see it there in that picture. There's no ornamentation to speak of, but bright and very pleasant. All the wall spaces of the entrance hall are hung with pictures and prints which are changed constantly, and there are beautiful cases in which exhibits of periods, publications, and men, biographical things, are put on from time to time by the devoted Miss Stanton and other people on the staff.

It's interesting that two birds were killed with one stone--the rumor or the thought that Dr. Cushing would give his books, if a suitable place could be maintained, was made to serve the interests of the school also in its general working library. Solved both problems.

I like the way you say solving both problems, rather than a mortuary metaphor--killing two birds with one stone.

I was thinking of the Dean's Report. They're pretty bleak in the early days about the condition of the library. There's no question about the need. The design just lent itself to accessibility, whereas that other site was on Davenport Avenue, and it would have been a wholly different place.

In addition in that library building it was possible to use an upper story for the headquarters of the Childs Fund which you'll notice later on.

That's a subsequent development.

For which they paid Yale a nice little rent.

I think it's also true, isn't it, that the Childs Fund itself found an absence of materials, books, references relevant to the cancer field and established a fund whereby books were purchased and stored in this library, still under the....

We had a plate for the Childs Fund--ownership, and we bought lots of books on cancer and subscribed to journals slowly because, as we'll point out later, this 1937 period was known as "cancer the great darkness". It was a pitifully inadequately supported field of research.

People have continuity here also--George M. Smith. He not only has it with the library, but later on in the Childs Fund. He's quite a fellow. I don't know that there is anything that you'd care to say about these people as people--

<u>quite apart from their interest in books which might help to illuminate this library.</u>

Well, Dr. Cushing was a productive scholar. He was an eloquent speaker, cogent, and a writer of grace and facility on many subjects, notably his biography of Sir William Osler and his writing on Vesalius. He was always interested in books before this library, as I said before, and carried that forward into it. Dr. Fulton was Professor of Physiology and had come to Yale from Harvard, a very vigorous and wide ranging man, could do almost anything, a great, energetic, facile person. He had collected pretty nearly all the 18th Century scientific work in England. Robert Boyle was one of his specialties, one of his special interests. Harvey on the circulation of the blood earlier than Boyle was one of Fulton's special interests, and he had--I don't know how many thousands of books. He housed them in his Department of Physiology. Dr. Fulton attracted a great many students, graduate students and young members on his faculty. His main field of interest in physiology was primate physiology, the nervous system of the higher apes and also dogs. He brought up a line of investigators who have been eminent men in that field in the country. Dr. Fulton was able to go abroad for studies and social meetings, and scientific meetings about every year, so he personally knew everyone from Sir William Osler to Charles Scott Sherrington and all the great neurophysiologists of the time. He brought all this back to Yale in a stimulating manner.

Dr. Fulton wrote incessantly--at least he dictated because as I told you, he had a dictaphone in his office, in his automobile, in his bedroom, and in his study in his house, and he was carrying around these records to his secretaries all the time. He had a great sense of fellowship with the people in his department. They were devoted to him, and he and Mrs. Fulton issued yearly not an encyclical, but something of that type, printed and telling what had happened in the laboratory during that year and giving personal news about members of the staff who had been there, or were there.

I didn't know Dr. Klebs very well--I wouldn't say that I knew him at all. I met him, but he was over in Switzerland. I knew of his reputation, and I knew the value of his books. I knew that Dr. Fulton and Dr. Cushing had to be very persuasive to get Dr. Klebs to join in with the plan, but they did, and that's the reason for this three leaf clover on the front of that pamphlet. That's their motto. It is a three leaf clover.

Dr. Edward Streeter was a friend of Dr. Cushing's more than he was of Fulton's, or mine, and he lived in Stonington, Connecticut where we saw him occasionally. For years he had been collecting weights and measures, buying them wherever he found them all over the world. He was a well-to-do man, could travel, and he had the greatest collection of weights and measures going back to Phoenician times and all the way through. They persuaded him to give them to the library, provided they would be properly housed, and they are housed there and were right away in two adequate rooms. I met Dr. Streeter at his house a few times when we went to talk about these things, but I can't say I knew him at all well.

George M. Smith had been very influential at Yale in the field of cancer research. He was the medical director of what was known as the Anna Fuller Fund, a fund given by Mr. Fuller, a businessman in New Haven which was the first endowment for cancer research--well, it wasn't an endowment for cancer research at Yale. It was the first collaborating, or cooperating foundation that gave prior consideration to Yale's needs. It could make its grants anywhere it wanted, but it gave most of them to Yale. It didn't amount to a great sum, but it was a seed, and it was a seed that put out plants that attracted Mr. Childs when he came up to New Haven on a visit just before he made his great foundation. Dr. Smith was housed in the Department of Anatomy. He was a great friend of Dr. Edgar Allen who was Professor of Anatomy, and his special interests in experimental work were fish--particularly pigmented fish--fish with big black spots because they developed melanotic tumors, and his collection of books was largely in ichthyology. He was a mysterious man, kept his own counsel pretty closely, and one was never quite sure what he was thinking, or what he was going to do after he said something. You liked him, but he aroused caution. I could say more about Dr. George Smith, but I don't think I'll do that.

He'll come in again in the Childs Fund where his experience was tapped, his knowledge on a consultant basis. I think he was ultimately made a member of the advisory board, so his knowledge was made servicable.

Turn it off for a minute, and I'll tell you something.

This is terrible. [The recorder was turned off] There's one other person who has no special connection with medicine, but who was quite instrumental as a member of the Yale Corporation. His interests were in literature, particularly the Horace Walpole collection, but as a person. This is Wilmarth Lewis.

Wilmarth Lewis was the class of 1918 at Yale and has been devoted to Yale ever since he was an undergraduate. He went over in the Artillery in World War I, came back and was soon elected as one of the self-perpetuating members of the Yale Corporation. By that time he had alteady begun to be a book collector in his own right. He had married Annie Burr Auchincloss, and both the Auchincloss family and the Lewis family had considerable fortunes. She had the larger fortune, and she was interested in building up the book collection of his. His interests turned to Horace Walpole, and he became a great authority on Horace Walpole. His efforts to publish and annotate the letters of Horace Walpole turned in to an enterprise to reconstruct the 18th Century. He had planned forty volumes in his series. That will give you an idea of the scope.

He soon became interested in all the collections at Yale--archeological collections, the Peabody collections of animals and artifacts of all kinds, the art museum, the school of music, and most of all the central University Library. I don't remember how I got to know Mr. Lewis, except it must have been through Dr. Cushing because they were great friends from the beginning.

<u>I think the first letter is an acknowledgement and generous thanks for being able to stop off at your house at Trumbull College.</u>

From Mr. Lewis.

<u>Yes, he had some engagement with Mr. Atterbury, Dr. Cushing and others. That's the first instance. His own book--I think he published a little book called Collector's Progress.</u>

I have a copy of that.

<u>It's a witty, subtle thing.</u>

He's a most attractive man, full of interest, literary interest, historical interest, and interest in people, but he was the power behind this library move. He supported Dr. Cushing, Dr. Fulton, and had a close relation with the Treasurer of the Yale University, Mr. Farnam--could influence him. I don't remember what his relation to Dr. Winternitz was. Dr. Winternitz was, of course, in the background of a great deal of this--didn't oppose it, but didn't appear too much on the promotional side. Mr. Wilmarth Lewis has done wonderful things during this period as a Corporation member, which ended two years ago, for the building up of the central library, the appointment of the librarian and staff.

About this time, an example of what he could do--there was a change in the librarian at Yale. Mr. Andrew Keogh, the chief librarian, reached the retirement age. He was a thoroughly trained guild librarian, and he wanted to have the next librarian chosen from among the guild trained librarians, some of whom, to tell you the truth, seemed to me to be more interested in whether a catalogue card should be three by five, or two and three quarters by six, or something like that. They spend enormous amounts of time on cards for catalogues. That ran through the kind of thing that people thought Mr. Keogh was mainly interested in. On the other hand, Mr. Keogh took that library through a very important stage. From the time that it was moved from the Yale campus to the superb new building, it had come from small to great, but Mr. Lewis in the Coproration, and elsewhere, showed even at that early date what he could bring about. He was largely responsible for the appointment of Mr. Bernhard Knollenberg as the librarian. Mr. Knollenberg was a lawyer and a financier in Wall Street, but he was also a scholar. He published a book on George Washington and other things. His wife was a sculptor. Knollenberg was an interesting and vigorous librarian but the particularly interesting thing was that Mr. Lewis was able to break a tradition of vested interest in the appointment in the guild and bring in somebody that wasn't trained as a librarian at all.

I had the pleasure of meeting him just once out at his home in Farmington, a warm person who made you readily at home--no fuss, but then the detail into which he could go in terms of indexing--it was like an education for me.

Oh yes. Out at Farmington, he bought an old house, an early 17th Century house, I think, and then built on to it additions to house the Walpole Collection. The house is filled with Walpoleana, portraits, objects from the Walpole "Strawberry Hill" mansion, if you call it that. He set out, among other things, to get all the books that had been owned by Walpole in his library, and he got most of them. He had the shelf list from Horace Walpole, and the books are all in the places where they were in "Strawberry Hill". He has a most enormous collection of political cartoons and prints of 18th Century England which became the field of work for Mrs. Wilmarth Lewis. She became a great expert and cataloguer of these thousands of prints. She was a very lovable person, quiet, unassuming, and yet absolutely constant in her support of their joint enterprise in the Walpole Library. I got to know Mr. Lewis fairly well. Mrs. Bayne-Jones and I have been up to see him at his house in Newport year after year for a good many years. We stay a week.

Well, I think we've gone perhaps as far as we ought to today. The next subject I'd like to bring up is the Childs Fund, and that's going to take us a little time.

Monday, May 16, 1966 A-60, N. L. M.

Many of the things we've been talking about the last three or four sessions all happened at the same time. We've been focusing really on the deanship, the variety of experience a man can have as Dean. Some of the things are indicated long before you ever become Dean--they have a past, a development and you fall heir to something in motion. Another part of the task is dealing with something that comes in wholly unannounced, an accidental thing, which during the time of your deanship, burgeoned into a means of support for research, not alone at Yale, but beyond Yale. This is novel in terms of its time. You had had previous experience, I think, on the National Research Council in the support of research. You had some contact through Hans Zinsser with the Leonard Wood Leprosy Foundation and its problems of getting a foothold in the field, not in any direct, official way, but through Hans Zinsser....

I was a member of the board. From the start, my connection with the Leonard Wood Memorial was as a member of the Central Advisory Medical Board.

I guess you were--I'm thinking of correspondence with Hans Zinsser where you wrote that you had been asked to sit with them. There wasn't then any indication that it was a formal relationship, but simply because you were in Washington.

In those photographs there are dozens of photographs of the board--Zinsser, Frederick Gay, a lot of people.

I mention it to indicate that you'd had experience--what to do with a problem.

Yes, what to do with grants-in-aid. The same in Rochester--we had a fluid research fund at Rochester, and we had grants from other places.

The whole development of the Academy of Tropical Medicine had grants-in-aid in view as a possibility. Well, here's—well, I don't know how you got into this, except that the correspondence shows it comes via Dr. Sam Harvey. Anything you can remember by way of background, certainly the negotiations which led to the formal gift to Yale of the Childs Fund and the establishment of the Childs Fund will be helpful because it is rather novel, given 1937.

I think it was Pasteur who said that phenomena occur and mean nothing to some people, but in time mean a great deal to the prepared mind, and I think that comment would apply also to the prepared institution. In this case, interest in cancer had been nourished for a long time at Yale. It goes back— well, it goes back a long time, to tell you the truth, to the missionary work in Canton, China. Peter Parker sent to the Yale Pathological Department about 1840, a great series of oil paintings of tumors and cancer in the Chinese people in Canton. They were exhibited around there keeping up a Yale interest in the natural history of cancer. Then in later times at Yale there was the influence of the Anna Fuller Fund which must have begun in the 1920s. I haven't a history of the Anna Fuller Fund here, but I know that Mr. Fuller, a businessman in New Haven got interested in cancer probably, as so often happens, because of the death of some member of his family from the disease and through his association with George M. Smith. He founded a fund for the support of cancer research which gave most of its money to Yale, although it didn't need to confine it to New Haven and Yale. It could and did give funds elsewhere. That was the basis for what had been existing for perhaps five years at least before 1937; namely, the Atypical Growth Study Unit which is a name Dr. Winternitz gave to this group of scientists and medical people interested in neoplastic disease. It was prophetic that he called it "atypical growth", focusing on the problem of growth because the fundamental problem in cancer, as all recognize, is a problem of the mystery of growth, and fortunately, if you have the conception that it is a mystery of growth that you're studying, you can be very broad in your activity because there is hardly a biological, chemical, or physiological process that is not somewhere involved in the problem of growth, and that allowed the cancer investigators to roam very widely over the field of all the elements that had to do with growth. Those things existed at Yale sometime before this accidental thing we're going to talk about now happened.

Cushing's tumor clinic was there too.

Yes, Dr. Samuel Harvey was interested in growth. There were tumor clinics in the hospitals elsewhere. Of course, in New York Dr. James Ewing who was the great man in cancer work at the Memorial Hospital and at Columbia and related hospitals there, was much interested in cancer research. There was a good journal published in this country for a number of years on cancer which failed about this time, but what happened that brought me into connection with the cancer field was an accidental happening arising from sources which I still do not fully comprehend. One day in 1937, early in 1937, Dr. Samuel Harvey, the Professor of Surgery, sent me a copy of a letter that he had received from Dr. Dye.

John Dye.

Yes, John Dye, a physician in Waterbury, Connecticut, in which Dr. Dye told Dr. Harvey that he knew of a wealthy man in New York who had shown some interest in setting up an endowment for cancer research, or a foundation for cancer research, but had not yet cleared his mind whether it would be for cancer, or something else, and had not made any commitment to any other institution and probably had not given much thought to settling this foundation at Yale. Well, Dr. Harvey with his usual wisdom, his gift of thought and expression, wrote me a long letter analyzing the advantages and some disadvantages in having such a foundation which he thought might come to Yale, and if so, should be part of the medical school. His idea was that this would not be a separate foundation, but something like a department in the medical school with its own funds.

This put me in touch with Dr. Dye. We arranged to meet, and he invited me to accompany him to New York to see this mysterious person whose name was given to me only on the train going down from New Haven to New York and only given to me in a vague way, Mr. Childs. His first name wasn't given, so I thought that he meant Mr. Eversley Childs who had put up a good deal of money for the work on leprosy that the Leonard Wood Memorial Fund had used and started a long time connection. Last year on the Philippine Island of Cebu the Leonard Wood Memorial dedicated a brand new laboratory known as the Eversley Childs Laboratory.

Dr Dye didn't tell me anything about the collegiate connections of this gentleman that he was taking me to see, nor did he tell me that he was deaf--quite deaf. We got off at Grand Central Station, went up Park Avenue to an apartment house, and we were admitted to a duplex apartment on the fourth, or fifth floor of a fine building there. Mr. Childs came in, greeted me in a friendly way, and we sat down in an alcove window overlooking Park Avenue. Mr. Childs then told me about his plans and his connections and early, fortunately--it could come out possibly in answer to questions--that he was a Yale man man in the class of 1891, and a great friend of Dr. Havey Cushing. That made me feel at home right away because 1892, was my Uncle Hugh Bayne's class at Yale, and I knew people in 1891, and I was an admirer and fortunately admitted to the friendship of Dr. Cushing, so we got off on a good start.

It soon developed that Mr. Childs was seriously thinking about starting a foundation. He didn't tell me how much money he was going to put into it, but he did indicate that he was interested in having a foundation on which his three sons could serve as trustees, Richard Childs, Edward Childs, and Winston Childs, possibly as a motivatedly idealistic thing for them to be doing. They had very different interests at that time. Winston Childs was a plunging financier on Wall Street with very original ideas--for example, of cornering all the dogfish and getting the vitamin-containing liver oil rights out of their livers. That went on for some time, but his enterprises were not too successful. I think at one time he was a big owner of Newsweek. Richard Childs was a more intellectual, visionary type, not so much in business, although I think he did have a publishing company which didn't turn out too well, but he was on the side of liberalism and had different opinions sometimes from his more conservative father and his other two brothers. Edward Childs, a big handsome man, was a bachelor whereas these other two brothers were married, and Edward's main interest was in the Yale Forestry School. The Childs family had a great tract of land over the beautiful hills of Norfolk, Connecticut, where they had what some called "a summer home"--it really was an all-round patriarchical mansion with stables, horses, land, lakes, canoes, and was very close, next door, to Mr. Childs' classmate, Senator Frederick C. Walcott.

All of this more or less came out at the talk, but I'm adding to it from things that happened later on just to put them together here. It turned out in this talk that Mr. Childs was primarily interested at the moment in setting up a foundation for the study of problems of poliomyelitis at the Rockefeller Institute because his son Winston had had an attack of polio and had a little residual paralysis. I didn't argue much against the Institute. It didn't seem necessary. I didn't believe that the Institute would be interested in doing the kind of thing that Mr. Childs wanted to do because at that time they were rigidly defending their independence and would not accept grants from outside. Maybe they thought that would keep Mr. Rockefeller's munificence more in their own control, more in their own interest and not allow it to be diverted by comparison with gifts from the outside.

I did tell Mr. Childs the wonderful work that was being done at New Haven by this Atypical Growth Study Unit as representative of the work going on chiefly in the Departments of Anatomy and Pathology. The Professor of Anatomy, Edgar Allen, was greatly interested in the estrogens and the female sex hormones, and he had with him Dr. William U. Gardner who was also a brilliant worker in the field of hormones and the production of neoplastic growth. Gardner has succeeeded Allen. Also in that department there was Strong--I've forgotten Strong's first name for the moment--but Strong was a great geneticist.

Is this L. C. Strong?

Leonell C. Strong. He was a geneticist, had been breeding mice for years and years and years. Mrs. Strong worked with him too, and he was able to bring up certain strains of mice that would have mammary cancer in a certain percentage and were very valuable material; as a matter of fact, as good material as was beginning to come out from C. C. Little's genetic station at Bar Harbor, Maine. Well, fortunately Mr. Childs did come to New Haven. He was accompanied--I remember that day--by his son Edward, and he went all through the Department of Anatomy and into the Department of Pathology where Dr. Winternitz had some interesting things to show--tumors from human autopsies, and various things. We introduced them to workers in the immediate field and from the periphery of the field like the biochemists and others who were interested in cancer research, not to Dr. Harvey. As I recall it, we didn't go very far into the clinical side of cancer because it was determined that Mr. Childs was interested in basic research rather than setting up anything for the care of cancer patients. From the start a principle which came into our conversation was that if he did set up anything in cancer for research, it would not exclude certain clinical studies, provided those clinical studies offered some opportunity to understand better the etiology of the disease.

Mr. Childs appreciated what he saw very quickly. He was very much impressed and talked with intelligence about cancer as being a group of diseases, not just one thing. Cancer is not one thing. It may be hundreds of things, but he also was interested in two other phases--one main phase that comes out in his speech of dedication of the foundation later, that somewhere behind the door, or stairway there was a Banting who would discover for cancer what Banting and Best found in insulin for diabetes. He had that entrepreneur's hope--I suppose a good old American pioneering outlook that somebody, a genius is hiding, or lost somewhere, and all he needs is a little help to get out and do some world astounding thing.

The other thing that interested me in this highly intelligent man at the start was that he showed that he'd had some contacts with people who were quacks in cancer. He not only thought that somebody would discover the cause of cancer, or some cancer certainly with a little help, but also he was beginning to be pestered more and more, by letters from quacks, cracks and cranks who were trying to get him to do all sorts of curious things for the support of their work. I don't know any field in which there are more wild notions of cure. We had to deal with such problems as a Swiss Institute that cured cancer by mistletoe extract. It was that kind of thing, although when you look into it, it's not so foolish as it seems. In medicine there is a great doctrine of "signatures", as they call them--things that look like something and are good for something, like hepatica is good for the liver, and there are mandrakes that are good for male disorders because they've got two legs in the roots. There is a great cancer institute in Switzerland that made all its extracts from mistletoe, and that does sound very foolish. It didn't have any good effect on cancer, but it's a very old notion. Mistletoe is a parasite--and it suddenly grows on a foreign tree just like a cancer grows, a foreign growth, on a human being. Mr. Childs was already bothered by some of those things. I think they came to him because he was a well-to-do, public-spirited man, and people, even before he had a cancer institute in his mind, were beseeching him for gifts.

I think you ought to go into his background in Norfolk--his tie with Senator Walcott and with Dr. Welch.

Mr. Childs comes from an old family out in the Pittsburgh region and after he went to Yale and graduated in 1891, he developed even more his Connecticut origins, and I think they reach back rather far in some lines. The family place, as I have said, is this beautiful old house on the hillside, on the lake in Norfolk, Connecticut. Norfolk, Connecticut was famous long ago as the birthplace of Dr. William Henry Welch who was born in 1850. Dr. Welch's father was a great physician in the region. Dr. Cushing has written a very moving paper called "The Doctors Welch of Norfolk". The Childs family knew Dr. Welch and admired him, and they knew his tradition. Senator Walcott knew Dr. Welch and supported Mr. Childs in his loyalty, as he always did when Mr. Childs wished to do something as fine as he had visioned this cancer research foundation--when it got in his mind that it would be cancer.

Mr. Childs--most of the money, I think, which Mr. Childs intended to put into this foundation came from the estate of his wife, Jane Coffin Childs. She was a Coffin, and she had come from the Coffin Family who had made its money in General Electric. She had died of cancer. She must have been a most charming and wonderful person. I never met her. She was dead before I met them, but her sister was still living, Miss Alice S. Coffin, a friendly, but very shy lady, living on one of the floors in this duplex apartment, or part of a floor, and she also became a contributor to the Yale Medical Library Fund--what I told about last time. There's a Coffin Fund there for the support of the library--salaries and books. Miss Coffin died a few years ago after having led a very secluded sort of life for all her years. As I say, she was very shy, but very nice and friendly.

I think Mr. Childs was interested in what he saw going on in these laboratories quite as much as he was by the sentimental attachment that he felt toward the Rockefeller Institute where work was being done. He and Edward both appreciated that they were talking to earnest men who intelligently were working very hard at difficult problems.

Also this was a critical time in the examination of support for cancer research. There were in this country two foundations--the Anna Fuller Fund which was small, very small, and the International Cancer Research Foundation established by Mr. William H. Donner in Philadelphia under the guidance of Dr. Mildred W. S. Schram, who was a very able woman and director of their affairs. They had an advisory board, and the foundation made grants all over the world. They were in the field ahead of the Childs Fund, but not with the same breadth of concept, nor the same generous outlook toward the investigators and the work, though they were doing very good work. At this time Fortune magazine published a great article on "cancer the great darkness", and cancer was a great darkness. It was a mystery. Nobody knew, and they don't know yet what actually brings cancer about, what keeps it going, how it kills people and what to do to cure it--I mean really substantial cures. Surgery and radiation are still the best means of treating cancer, although they have hundreds of drugs now, some of which are beneficial and palliative.

I will now talk briefly about what happened after Mr. Childs made this first visit which I think was in January, 1937.

January 28, 1937.

Things moved very swiftly after that. Mr. Childs made up his mind quite soon that he wanted the foundation to be in cancer research, wanted it to be called the Jane Coffin Childs Memorial Fund for Medical Research--broadly termed, but he expressed its main interest in cancer, as you'll see from his dedication. He had plans for a Board of Scientific Advisers and plans for a management board composed largely of people that he knew very well, mostly members of his family and close associates. There were Mr. Childs, the three sons that I mentioned, Mr. Walcott, and Mr. Christie P. Hamilton, who had dealt with the General Electric accounts and management for a long time. Mr. Albert H. Barclay was the legal counsel and also a member of this Board of Managers. Mr. Barclay was a classmate of Mr. Childs in 1891. Then Mr. Childs wanted on his board at least two representatives from Yale. They would be ex-officio--Mr. Seymour, the President, and George Parmly Day, the Treasurer. All that moved very fast. I think Mr. Childs was used to formulating broad conceptions and administrative management without any loss of time and without any real uncertainties. We soon began to talk about those details. He would come--I think he came to my house in Trumbull College over and over again for long afternoon talks, and he stayed there over night some times.

One of the first things that had to be settled after he had decided that he wanted his foundation to be located at Yale was on the side of personnel, the question of a director of the scientific activities of the foundation. At the start, it seemed that the man who would be chosen would be Dr. Winternitz. There was a great deal of inquiry and soul searching talk about the question of a director. I had no real part in what went on privately on much of that. I was asked my opinion. It came out very soon when Mr. Childs asked me if I would be the director of the Board of Scientific Advisers and if I could do it, as I thought I would have to, in addition to being the Dean of the Medical School. That would have the advantage of keeping the fund in very close contact with the medical school. I had already agreed, or about this time agreed with Mr. Childs that the Foundation would be set up almost with autonomy of its own. It was to be at Yale, but not under the control of Yale, a very extraordinary arrangement. The securities were set aside and held in a bank in New York under Mr. Hamilton, Mr. Childs, and the Board of Managers. Yale, however, was called the "custodian", and Yale handled the checks. Money passed through Yale, but Yale had no rights to say anything about the sale, or purchase of the securities. Yale agreed to that. I don't think the Yale Corporation had anything like it in the place before. It's still working that way now, although the University has made a number of efforts to get control of this fund as part of the University and under the control of the administration of the University. The Childs Fund, as is said in Mr. Childs' dedication, is not to be exclusively used at Yale, but Mr. Childs expressed the hope that the Board of Scientific Advisers would find at Yale opportunities to use most of the money. Isn't that about the way it's said?

<u>Yes</u>.

I haven't read that document in quite a while.

You can imagine what a long series of difficulties had to be faced after these general principles were agreed upon. I saved all the drafts of the Charter of the foundation and of the bylaws. All of the governing regulations are in these documents, and they were worked over and over again. If you look at them, you'll see hundreds of changes from time to time. It was very severe work for Mr. Barclay who had a big legal practice and had much else to do, but Mr. Childs had a way of pressing business associates so that they did what he had in mind as being desirable. He did not use the same methods in any sense with the scientific advisers. He admitted into the bylaws a statement that the Board of Managers would never make a grant for research in the field of the foundation, unless the Board of Scientific Advisers recommended it first. I had some instrumentality in getting that into the bylaws as a regulation. I had to answer many of the wild letters which Mr. Childs was receiving from people that had all these queer, crack notions about cancer and its cure. In addition the newspaper publicity that emanated, I think, from the Yale offices was very unfortunate at tne first. It says in the early accounts that came out in the papers that the Childs Fund would be twenty million dollars. It was nothing like that. It was several million dollars, and its principle has gone up a great deal since, but all the world was informed that here were twenty million dollars going into cancer research and that would be receptive to any ideas as to how the money might be used. That had to be dispelled by practice rather than words--I don't think anybody ever put out any definite contradiction of the original publicity, but it was soon apparent that the Fund wasn't operating at twenty million dollars a year.

The Fund also had an interesting financial angle at the first which the director was able to turn, although some of the businessmen on the board were very hard to convince that it was the wise and proper thing to do. The first budget I brought in after the meetings of the Board of Scientific Advisers covered a period of about three to five years. We realized that cancer research was a long, time-consuming process. Most of the problems undertaken in cancer research are heart breaking. They turn out to be no useful results at all, or a negative result. Many a man has broken his heart working on cancer research, although many a man has stayed at it. You couldn't get projects under way, or good people devoting themselves to work in the field, unless they had some promise of support for at least three to five years. Well, when that process started, I brought in a budget covering from three to five years, and the total budget for that period was about three to five times the total annual income of the Fund. These men didn't want to commit money it didn't have as current income. Of course, it could sell securities. Say you make a grant of two thousand dollars for one year. If you make that grant for five years, you've got a commitment for ten thousand dollars, and you've only got the current income that would meet five. I hadn't expected that kind of a problem to arise. I had been used to foundations that had money in the bank already. The first time we went off on this venture, we pledged more money than the income would support, although we knew that the Board of Managers could sell securities, if they had to.

<u>I think there was a provision which allowed the Managers to sell part of the principle, if need be.</u>

That's what I mean by selling securities—yes, a provision empowered the managers to do it. Also there was a wise provision in the bylaws, that if in the opinion of the Board of Scientific Advisers approved by the Board of Managers it appeared that there was nothing further to be learned, or gained by cancer research, the money could be devoted to research on any other problem in the whole field of medicine without disturbing the organic relationship with Yale. They never mentioned the Fund being moved out of Yale. They never mentioned the Fund being moved out of Yale, but the Fund could be shifted from cancer to some other work.

The other thing that influenced the judgment of the Board of Managers was that they didn't want to put Fund money in bricks and mortar which is a horrible conception to my mind because you've got to have a building in which to work. There was much effort at Yale to get a cancer institute built, and the managers were all afraid that the money would go into a building and leave nothing to continue with the research. The only building that the fund ever built was a mouse house for Leonell Streng. We built about a fifteen thousand dollar mouse house which is still there, using, economically, part of a brick wall that had been set up to see what the new wall of the library would look like. It was still standing so we built the mouse house around that.

<u>Wasn't the intention of the drafters--that includes yourself, Mr. Day, Mr. Barclay, and Mr. Childs--to write an enabling act with the least restrictions possible, although the bylaws, enacted after the Fund was established, created the process--that is, the Board of Managers couldn't make a grant without it being initiated, or sanctioned by the Board of Scientific Advisers built into the process once the Fund was established.</u>

It could be initiated anywhere, but it had to come through the Board of Scientific Advisers.

<u>So far as the Board of Managers was concerned, they had no separate power to</u>

initiate their own ideas. They had to come through the Board of Scientific Advisers.

Maybe I'm getting into semantics now, but any manager could say, "I think this is a good field to go into" and then we would work on it and see what it looked like.

Mr. Childs seems to have been a very fruitful man with ideas. One of the early notions he had--I think there were at least three. One was an independent foundation by itself. Then there was a mobile unit of investigators where they could go anywhere. Then there was the endowment to some specific institution and under this last--he had talked to Dr. Rufus Cole at the Rockefeller Institute --was a university medical school with an attached hospital. Apparently he had these pretty well thought out as possibilities.

He did. He worked hard on it, and evidently before I met him, he had probably been a year or two thinking it over and talking to people. His main supporter and adviser was his son, Edward. I think in the files there is a little sheet of paper where Edward draws up the institute for polio investigation. Did you find it?

Yes. As a matter of fact, polio as an interest, doesn't drop out of these series of drafts until June 1, 1937. Reference to polio is finally omitted. I think this is because they agreed to the broadest shift in emphasis in the event that something happens in cancer and it's no longer a problem. The Board of Managers is empowered to change the sights to some other problem. How was Mr. Childs to negotiate with? He must have been pretty easy because he had such interest.

Mr. Childs was a very forthright man, and the only difficulty negotiating with him was that he was quite deaf. You had to speak pretty loudly, and sometimes you'd have to say it over and over again. He was a big man, about six feet two, broad features, easily smiling, friendly, not noisy, but a warm person. From the time I began to be a director of this foundation, I began to be almost a member of his family, and so did Mrs. Bayne-Jones. Mrs. Bayne-Jones and Mr. Childs formed a lasting friendship. We still see the members--just a few weeks ago his daughter Barbara was down here with her husband, and it was Richard, largely, who did quite a little in having my portrait painted for the Dean's collection, painted for the medical school a few years ago.

In a letter that he writes quite early on March 4, 1937, Mr. Childs is thinking of you as the chairman. There's some problem, some confusion in its name--chairman, or director, whatever it was, but apparently....

I think it's chairman of the board. I don't like the word "director". I don't like to consider myself directing people. I'd rather be their servant.

This is quite early, but you indicated in comments you made that in view of the proposed development at Yale, in the light of his visit, and the interest and background already existing at Yale, it was easier to have the Dean's Office represented because that was where the various parts of the medical school met, but then Mr. Childs chose you as a person, quite apart from being Dean.

That must have been very fortunate because I never acted in relation to the Childs family as if I was representing the Dean's Office. In fact, I would tend to oppose things that would seem to come like pressure from the University, or the Dean's Office. The Dean's Office in the medical school procured for the

Childs Fund two sizable rooms above the library which they still use as an office. The Childs Fund fixed it up, decorated it, and they held their meetings up there. I used other rooms in the Sterling Hall of Medicine, a wonderful big reception room where they would have lunches and social meetings. The Dean could probably get this place more easily than an outsider could have gotten it, but such things as that were in the ordinary protocol of the day. As Dean you'd do it for other organizations too. The Childs Fund paid Yale rent for that room, the office, and still does.

You used--I say "you used" but you had access to Edgar Allen. I think you sent a draft proposal over to him for his comments once.

We weren't limited to where we could go for advice. The Board of Scientific Advisers would receive from the chairman of the board in advance of a meeting dockets which gave the whole of the application, the references, letters of inquiry--I could write to anybody who knew anything about the kind of question that came to us. When those things would come in, we'd have long discussions at our meetings, make up our recommendations, or else put it over to another meeting to work out something else, but there was no limitation on what consultation you could seek.

This was even before the Fund was established, when the process of negotiation was on.

I was not put under any bind of secrecy in the preliminary negotiations. I don't think I talked very much about it.

No. There was something of a blanket so far as the outside world was concerned, but Dr. Harvey was close to you, and he had views and expressed them well, was thinking in terms of Yale as the beginning and end for this Fund. I don't know what Dr. Allen's views were, but he had already in being in the Department of Anatomy these various studies that were ongoing, so he would be a logical source to consult with too.

Yes, and another source, and I know I must have talked to him, was Dr. Francis Blake who was equal to Sam Harvey in wisdom and experience in the school. He wasn't interested in cancer much. There was little going on in the cancer field in the medical department. Surgery saw a lot. Obstetrics saw a good deal with the gynecological cases. The Department of Urology under Dr. Clyde L. Deming saw plenty of prostate cancer and other things. These were the chief ones with pathology.

How much of a role did Seymour play in this--he's hardly mentioned at all.

When did Seymour become president? Let me look at that Yale Catalogue there.

Mr. Angell was still President--Mr. Angell made this announcement because it was ready in June of 1937.

For the academic year 1937-1938, and that academic year is July 1st to June 30, so we overlap parts of two calendar years. Mr. Angell would have been President the first part of 1937. Mr. Seymour comes in after July.

This was ready for announcement at the meeting of the Corporation on June 19, 1937, and Mr. Angell made the announcement. I don't know that you talked much with him about this. George Day.

Oh yes, George Day and I saw a great deal of each other--this was right in

George's field because he was a great fund raiser for the Yale University Press and all sorts of things.

He was very much interested in the successful pursuit of this.

I think Mr. George Day really thought that Yale University would control this fund and the resources; in fact, I have evidence that he did.

The search for scientific advisers gets somewhat involved--a survey of people on the scene. Any number of names are mentioned--Warren Lewis, James Ewing, Francis Carter Wood, Burton T. Simpson, James B. Murphy. There's a period when you go to Atlantic City for a meeting, for example, and part of the meeting is represented by handwritten notes. At meetings you can talk with people as to who would be fruitful, and the names that finally come out of this are a good group of people. It isn't long before that original list is extended, increased. Ross G. Harrison is one.

Harrison was on from the start--right on through.

He'd had a long continuity in this picture.

Dr. Harrison was one of the wise, original people in the biological world; and he was the first one to cultivate nerve tissue, grow tissue outside the human body. That tissue culture was a fundamental method of cancer research--very important nowadays everywhere.

Then there was Rudolph J. Anderson.

Anderson was the head of the Department of Chemistry--you see, we had a chemist, a biologist, a pathologist--Winternitz, a virologist--Peyton Rous. That's about all there was at first, I think.

You talked to Simon Flexner and a lot of people for information.

Yes, I was trying to get a balance on this board.

Picking someone's brains with respect to people. There's a lot of commentary on them in these notes that bear on how long one hangs on. Some of it isn't very fruitful with reference to some--like James Ewing who was pretty pessimistic.

Dr. Ewing was a great figure in cancer pathology and cancer treatment. Dr. Ewing didn't discourage me, although his ideas were against what we were trying to do. I went to see Dr. Ewing one day in New York and told him that we were interested in studies bearing on the causes of cancer. There were two theories on the causes of cancer as there are in many other things. One is a proximate cause, and the other is an ultimate cause. Lots of things can be called "a cause". If you expose your hand to radiation, x-ray, you'll get a cancer of the skin and you'll say that radiation is the cause of the cancer, but what does the radiation do to the cells that twenty years later develop a cancer in that place? Methylcholanthrene causes cancer in rather the same kind of way. A great many other things are carcinogenic, cancer producing, but that doesn't tell you what the real process is, and Dr. Ewing told me one day. He said, "B-J, if you do anything to have this Fund try to work out the ultimate causes of cancer, you'll just be persuading them to waste their money", and he wouldn't have anything to do with that.

That's strange.

He was a strong minded man of enormous influence.

Carter Wood was another, but he was finishing. He was head of a failing journal which we took over later and helped to run a bit. Carter Wood--I used to go and see him. He was in the hospital at Morningside Heights behind the Cathedral there--Roosevelt? No. [St. Luke's]

Wasn't he with the Croker Institute for Cancer Research?

Yes, the Croker Institute was transferred later to Columbia. Carter Wood was out of--well, there's coming into my mind a story as to why I knew these people. Before the Childs Fund came about there was a thing called the American Cancer Society. It was an old cancer society, and it was under the presidency of C. C. Little. We used to meet in New York, at the Harvard Club and various places, and these meetings would last all night sometimes. Simpson would be there from Buffalo, Carter Wood, Ewing sometimes, George Smith, all the people in cancer. I had a preliminary acquaintance with them without being connected with them. I kept it up for a long time.

Burt Simpson was a very interesting man, devoted to the building up of the New York State Cancer Institute at Buffalo which is now quite famous and able, but he was beyond the fieldmof interest in age that we were after. He had no special ideas about cancer research, but he was a good man.

I think we've gone as far as we ought to go today. Next time, I'd like to get into the state of the art and how Peyton Rous fits into that state because there's long continuity with him.

Yes, he just got a thirty-five thousand dollar prize for proving that virus causes some cancers.

Tuesday, May 17, 1966 A-60, N. L. M.

I'm not too clear. I may just ball it all up for you.

I ran into this note, as I told you, in looking through the record. I find that the establishment of the advisory committee and the way in which it functioned....

We called it a Board.

Yes, the Advisory Board, and the way in which it functioned is really something new and novel.

The Scientific Advisory Board.

Right, and the bylaws created for the Board, I suspect, came at the first meeting of the governing board, or the Board of Managers. There's a note here about Mr. Barclay having to draft these bylaws in a very quick time. Do you remember the circumstances at all?

I remember that we consulted a great many people about the organization of a scientific advisory body because we'd had no experience of any extent in it either in my life at Yale, or in some of the others, and I'm quite sure that we must have gone through a good many preliminary formulations as to what we wanted to do and naturally brought them, in time, to the legal counsel of the Childs Fund; namely, Mr. Albert Barclay. Now, this note that you read said--what did it say?

The note said, in substance, the bylaws which he was forced to write in about four minutes in 1937, when he felt that he could well have spent four years on them.

I imagine that's an exaggeration for some kind of a rhetorical effect that I may have had in mind at the celebration--the 10th Anniversary. Well, Mr. Barclay would not have been able to write these bylaws in four minutes without preliminary drafts, and I'm sure that's what he did. The bylaws of the Childs Fund have never been altered. I don't think they've been amended since they were adopted in 1937, and the bylaws and the phraseology of the Deed of Gift became quite influential throughout the country. There were many requests for copies, people imitating it. Where this wisdom came from I hardly know at this time, but it must have been distilled from the advice of many, many people that we consulted. For me, I know there was a great deal of traveling around to see people that were experienced in cancer research and in administration, so that what we put together passed through many hands and through many minds and stuck.

The beauty of the bylaws lies in the fact that they enabled this Scientific Advisory Board to function and to have that degree of control over idea and program which without the bylaws it might not have had.

We'd known other cases where the Board of Managers, financial managers and administrative managers, of the non-scientific affairs of a foundation would by intention, or by accident, or by ignorance do things that were not consonant with the scientific aims of the foundation. Also, as I said last time, Mr. Childs and other members of his family and members of the Board of Managers were under pressure from quacks, reputable and disreputable quacks, to do things that were not scientific in the investigation of cancer, or towards the treatment of cancer and part of the provisions in our bylaws was to protect ourselves, protect the scientific advisers from ill conceived actions on the part of the Board of Managers without imputing to them any base motives. We were afraid that they might do something out of enthusiastic ignorance.

A feature of human life. Once you've got the Scientific Advisory Board established, you confront this whole new field, really, as to where its limits are and what to do in the development of a program--do you wait for applications, or do you design a program?

That search for a program began, of course, from the very start. I see here from one of these early minute book records that we talked with Dr. Gye for one. Dr. Rous, who was a member of our Board of Scientific Advisers, had ideas about a program; in fact, I think we asked a lot of people to give us suggestions as to what the program would be. It was perfectly obvious that the program of the Fund with relation to cancer research could be divided at the start into two main subdivisions--one a program initiated by the Fund and carried on for purposes that the Fund, the advisers with the managers, selected, the sort of program based on ideas that were initiated and brought out by the people constituting the scientific advisers of the Fund. That would be a program sui generis, a program particularly as the Childs program. The second subdivision was to carry out the ordinary affairs of acting as an agency making grants-in-aid of research. That we could do quite easily because we were in contact with very fine investigators of imagination and ability who needed funds for the support of their research and who knew how to put in lucid and convincing applications. We also included in that group of projects supported through applications requests for aid that had been invited. We'd see a man who needed additional support for his work, or some support to start work, and we'd suggest to him that he put in an application, and if it was good enough, the board would recommend its support and adoption.

We did start later on in Dr. Cyril N. Hugh Long's Department an invited program which taught me a lesson in not a bad sense, but in a rather human natural one. I had a notion that the central problem of cancer was the fabrication of protein by the cell--protein synthesis--that cancer cells can't grow without synthesizing protein, that no cell can grow without synthesizing protein. It seems that the synthesis of protein is a fundamental biological, biochemical process common to all cells, and it probably differs in different cells, and it might differ in malignant cells quite considerably from normal cells because the malignant cell in a body can continue to capture the substances that are built into proteins, while the rest of the body is starving. You see animals and human beings with enormous tumors which continuously enlarge while the body bearing that tumor melts away as if it were starving, so certainly there's something peculiar about the synthesis of protein by malignant cells. We got Dr. Hugh Long, the Professor of Physiological Chemistry, interested in this program, and he brought into his laboratory a worker who knew some particular transaminase reactions, or some reactions that were particularly important in the building of protein and who started to work on that subject. We hoped that he would use malignant tissue and keep his bearing on cancer, but it was not more than a year before he was way off in a field of enzyme chemistry that had no real connection. I'm talking not of a connection in the sense of applied research connection; I'm talking about its ideological connection with the synthesis of protein. Here was something that had been started on an invitation welcomed by a chief of a department, initiated by a brilliant young worker, and then abandoned because some other phase of his investigation had greater appeal.

That experience, I imagine, would happen with a good many attempts to impose a program, or to start a program somewhere even by invitation. You can't control what the investigators will think, or what they'll do, so at least we got on better by supporting ongoing work, or work that somebody really wanted to do, and not something we wanted done. I don't know that any foundation in cancer research has really succeeded in getting an overall program. There are big programs at the National Cancer Institute, but they're all divided into subparts, not one cancer research program. The Royal Cancer Hospital in London is interested very much in carcinogenic studies, the actions of carcinogens, but is working on a program of carcinogenesis that is not as broad as the field of cancer by any means. Well, it's extremely difficult to be broad enough--or it's difficult to devise a program. Maybe none of us were bright enough to think of where the central problems lay and how to attack them, and that is still going on--a foundation in search of a program is a common wandering sort of a creature.

Initially, certainly, the major emphasis was assisting local ideas at Yale and the build up there, or the continuing grants for work already in progress.

Yes, as you recall, Mr. Childs in his speech opening the foundation, his dedication speech, said that he hoped that although the Fund was not primarily an affair of Yale, the Board of Scientific Advisers would find enough worthy work at Yale to make Yale a great center, an outstanding, or leading center for cancer research in the country. There was much going on at Yale, as he well knew from the impression it had made on him when he came up for his first visit, to justify a considerable investment, so to speak, in the cancer research going on at Yale with the hope that it would be extended, that new people would be added to the Departments of Anatomy, Pathology and Biochemistry, particularly, and that this would develop a broadening and stronger interest in the subject. Our grants at Yale from the start were all thoroughly well considered and were not influenced too much by the fact that they came from Yale.

That's true--the minutes show that some grants are pared down from a request because the work really can't be shown to be necessarily related, or something in which the Scientific Advisory Board wishes at that moment to proceed with.

To jump ahead--I'll show you that you never can really tell. We granted a fellowship to a man named Joshua Lederberg later on--this is in the 1940s, or a little later. He was over in the Sheffield Scientific School working with a noted scientist named E. L. Tatum who found a variation, genetically determined, in a mold called neurosphora. Lederberg tried to look for the same thing in bacteria, and he was then able to transform bacteria genetically. He proved--nobody knew at the time--that there was a conjugation between bacteria. He made all sorts of variants that he could distinguish metabolically. Some could use, we'll say, leucine, and some could not use leucine. Different amino acids had different effects on their growth. Well, that seemed so interesting to the basic problem of growth in relation to the normal cell and the malignant cell, that we gave Lederberg one of the first important fellowships of the Childs Fund. We were very eager to have a Fellowship Fund which would support promising young men. Although that seemed to be stretching the concept of the limitations of the fund a bit, the Board of Managers readily understood the importance of this work as promise and made the fellowship. Lederberg from then on became more and more competent in genetics, bacterial genetics, and a few years ago he got a Nobel Prize for this work. Now, this work hasn't solved the problem of cancer, but it has opened up a lot of good issues particularly in the modern field of DNA and the modern chemistry of genetics. Well, all of that is important for cancer, but you couldn't have been sure of it when you started this boy on a fellowship.

We did a good many things like that. There's a great scientist in the country working with non-pathogenic protozoa named Tracy M. Sonneborn, a man out in the West. He found in paramecium a lethal factor called the kappa factor which is transmitted genetically and causes the death of the PARAMECIA that receive this paramecium. That's a very interesting--the possibility that you could get something going in a cancer cell that was lethal for that cell, so we supported Dr. Sonneborn's work for quite a while. He was a famous geneticist and became even more important.

Those things have a sort of programmatic--if I can use such a word--connotation. They started small and isolated, and if they're good enough, they grow into quite sizable projects.

<u>This is the first time since we've talked, I believe, when geneticists have appeared, but I gather this indicates the wider ramifications of the particular problem you're confronted with and the need for picking brains from many sources.</u>

This isn't the first time that geneticists have appeared because Leonell Strong was a geneticist, and we supported him from the start in one way or another.

<u>Oh yes.</u>

Here's his first thing--Leonell Strong, "The differential effect of methyl salicylate on the growth of spontaneous tumors on two strains of inbred mice", [30 Journal of Heredity 85-86 (1939)]. One of the first things supported was a genetic study. I don't know that there's much more to say on the eubject.

<u>Also quite early the fund got involved in the purchase of equipment where it was related. It hadn't anticipated the need to put funds into equipment, but the problem was beginning to get more complex, and it required more complex equipment.</u>

I think there was an intimation coming from the Board of Managers that they wouldn't be particularly interested in spending large sums of money on physical equipment, but there was no prohibition of thoughts about it, or what might be done. As a matter of fact, the Board of Scientific Advisers found out, or learned from Dr. Long, or Dr. Kurt Stern, that some quite expensive physical chemical apparatus was needed to carry on the kind of work he was doing on glycolysis and other metabolic processes. One of these pieces of apparatus was an ultracentrifuge which we had to have made. We put that in a basement room and enclosed it in a bomb proof cement column because it rotated at fantastic speeds, and when and if the rotor broke from the centrifugal forces, it would go right through anything almost. It had to be protected, and it cost a good deal. We had the optical apparatus for studying diffusion of protein, migration in electrical fields and solution--quite a lot of expensive apparatus in Dr. Long's department.

The others were not expensive. We bought no end of cages for Dr. Strong's mice, fitted up rooms in which they could be housed, and occasionally we bought an ordinary microscope for some one. These were the days before the thirty thousand dollar electron microscope came in. If it were in then at the start, the Fund certainly would have had to go into electron microscopes at considerable length because that is a tool that opens the lid of mystery.

Later on we spent a fair amount of money to aid the Chester Beatty Cancer Research Institute in London, and that goes into the World War II period. The British were not able to provide the apparatus needed for the rehabilitation of this laboratory, and yet Sir Stafford Cripps said that they must buy British. We defeated Sir Stafford Cripps by making the grant and saying that the money would remain in this country, so we bought the apparatus over here and shipped it to London. That went on for several years. I don't recall, aside from the equipment for Dr. Long's Laboratory, any expensive outlay for apparatus.

By and large where equipment was involved from outside sources, the thinking of the Scientific Advisory Board was not to get involved--the cyclotron, for example, while it may hold some promise for the future, they didn't see tying up the fund either to California, or to MIT.

Yale was stupid about cyclotrons--if I can use such an adjective with my Alma Mater. There was in the Department of Physics before this time the man who invented and built the cyclotron, Professor Ernest Lawrence, but he moved out to California. Then his brother Dr. John Lawrence got interested in radioactive substances, radioactive phosphorus, beginning studies of the treatment of leukemia with radioactive substances. We helped to buy radioactive isotopes, but we didn't go into the machines to make them.

I think the program at Yale--there was no work being done in the field of viruses, and as part of making Yale a center, or a drive in the direction of research in the field of cancer origins, provision was made for studies in the virus field at Yale in your laboratory without the man being mentioned until some time later. You pick areas, and that's important.

What date is that?

The actual vote is January 15, 1938--some time later than this publication appears, but nonetheless, the thinking goes back a good bit for the inclusion at Yale of this kind of study.

Yes, the man who interested and influenced us very much in thoughts about the possibility that viruses have a relation to neoplastic growth was Peyton Rous and, as I recall it, we invited Peyton Rous to be a member of the Board of Scientific Advisers from the very start. In doing so, we had to oppose the candidacy of Dr. Rous's main antagonist, Dr. James B. Murphy, who at the Rockefeller Institute did some excellent work on cancer, transplanted cancer and other growths in animals, but who was fanatically opposed to the idea that a virus could either initiate the cancer, or continue its malignant progress after it started. Murphy was a close friend of some important people at Yale--President Angell and some others, so that he was in a position to put a good deal of pressure on the Board of Scientific Advisers to nominate him for appointment to the board. This illustrates the faith put around the advisers by the bylaws because they not only could deal with the scientific program and projects, but they dealt with matters of personnel. They nominated all the men who became members of the Board of Scientific Advisers. They didn't attempt to nominate the members of the Board of Managers. All these people--both of the boards--were appointed by Yale University only on the nomination of the managers. The way it worked was that the Board of Scientific Advisers would recommend to the managers, and the managers, if they approved, would pass it on to Yale and Yale would appoint. When it came to the appointment of managers, the Board of Managers could among themselves decide who they wanted. Yale could not initiate a nomination. They could turn a nomination down, but they never did.

<u>You also talked to Dr. Gye from England.</u>

Gye was over here a few times. He was interested in bacteriophage and other viruses, and he made such extraordinary statements that could not be confirmed that he lost standing for a bit, but when he returned to England, he kept on with his work on viruses. I think his work was at Mill Hill Cancer Research Laboratory outside London. He was a friend of Dr. Hans Zinsser. Dr. Gye was a prolific scientific writer in the field of viruses and was a very interesting and able man. I see by this book you show me here--the minutes--that I have a quotation under the program of advice that we got from Gye as well as Rous, but I don't recall that we actually followed anything that Dr. Gye recommended specifically. It was mostly a point of view and an attitude.

Yes--he had certainly genuine enthusiasm for the virus field.

Oh yes.

This development takes place in 1937 and 1938, and in an earlier paper. This period is referred to as "the physiological year" because of the developments that were taking place on the national scene. I don't know to what extent the developments on the national scene are related to changes in the American Cancer Association, or the spur which C. C. Little may have provided. I'm not sure at the moment now--I guess you were on that board.

Yes, I was on the board when Little was the director of the American Cancer Society. He revived the people out of the doldrums and apathy. He formed what was called "the Women's Field Army". He attacked cancer with Amazons, and this women's army that he developed became a very enthusiastic and powerful body all over the country. Groups of them had a great effect on the administration of the American Cancer Society in the New York headquarters. They raised a great deal of money for those times and did good work. I forget when I first got in connection with that society. It might have been around 1937, about the same time that the Childs Fund was forming. We used to have long, long evening meetings--the scientific adviser's body--in New York at one place or another. One of the members was Mr. Frank B. Jewett, President of the Bell Telephone Laboratories. Do you know him?

<u>I know the name.</u>

He was on the Board of the American Cancer Society. He opened my eyes to the industrial support of research at one meeting. He said that the Bell Telephone Laboratories in its communication research had a research program that would double, or triple anything that was available in the cancer field for research on cancer. He hardly knew how much was going in to research. I think he was talking in terms of maybe twenty million dollars a year, or more, but he said one of the things he did in relation to the management of their investigators was to leave them pretty much alone--"don't try to force them to put up a certain number of ideas, or inventions a week and be sympathetic and helpful with their vagueries. If we have an investigator who suddenly gets a blonde secretary and wants his office furniture changed from mahogony to bird's eye maple, we refitted his office, gave him the pale carpets he wanted and the light colored furniture and let him be happy."

<u>That's wise.</u>

I think that's wise. The investigators are not really too temperamental, but they are high strung, and they work very hard. I suppose no group of men are subject to so many frustrations that have spiritual pains with them, so what difference did it make to Mr. Jewett. If he could get a coaxial cable that would take twenty-five messages in both directions by letting a fellow have a bird's eye maple desk, he could be well satisfied.

<u>This is also the year in which the National Cancer Institute Act is passed.</u>

That was passed in--was it August of 1937?

<u>It was signed in August, 1937.</u>

That was the first separate institute under the National Institute of Health. It used to be just the National Institute of Health. Then when they started the National Cancer Institute, the first subdivision, they had to put on a plural, and it became the National Institutes of Health. The Cancer Institute was founded at the instigation of a very influential man living in Texas. I've forgotten his name, although I have written a biographical sketch of him.

Is this Dr. Dudley Jackson?

I've forgotten his name. I wrote a sketch of him in the 1950s somewhere, published in the Journal of the National Cancer Institute. It's too bad that my memory has gone at the moment--his name and what he did--but he had an influence on the Congress, and he had an influence on Dr. Dyer who was head of the National Institute of Health, Rolla Dyer. They got this legislation, and they started an enterprise for cancer research out at Bethesda where the NIH was then settled.

It didn't have any program to begin with. It was just a vague idea, so Dr. Parran, Tom Parran, who was the Surgeon General of the Public Health Service under the jurisdiction of which these institutes function, called together a committee of advisers to draw up guide lines, or plans for a program for the National Cancer Institute. The members of the committee which was appointed, probably in the fall of 1937, were myself--listed in their report of this meeting as Professor of Bacteriology and Dean of the School of Medicine at Yale, Dr. Ross G. Harrison, who was the chairman of the National Research Council and Sterling Professor of Biology at Yale and who had been the first man to cultivate tissue outside the animal body. He did it when he grew nerve cells. The other members were Dr. Clarence Little, who was then director of the Roscoe B. Jackson

Memorial Laboratory at Bar Harbor, and Dr. John Northrup, a member of the Rockefeller Institute for Medical Research, who was a very distinguished physicist and mathematician, a devoted friend to Dr. Hans Zinsser, a physicist with biological interests and inclinations, and some of his work was in physical chemistry. The other member of this group of five was Dr. James B. Murphy who is here listed as a member of the Rockefeller Institute for Medical Research where he worked in the laboratory concerned chiefly with cancer.

We had a number of meetings and supplied Dr. Parran with a report which Dr. John R. Heller has since called the guide lines for the National Cancer Institute and which some of us called the "charter" of the scientific work of the National Cancer Institute. We gave a report on our thoughts about the biology of the cancer cell and what was known about that, the then present status of the study of the carcinogenic agents, a discussion of the hereditary factors in malignancy, and a discussion of research objectives. In the consideration of neoplasia and the field to be investigated, we pointed out that there was a difference between what you might call causal genesis, the things that set in progress the immediate neoplastic change from a normal cell to a cancerous cell. That's called a causal genesis, the immediate effect, but once a cell has acquired neoplastic properties, it goes on as an uncontrolled type of multiplication and growth without any necessary further connection with the agent that started it. A good example of that is the cancers that arise from radiation. People exposed to radiation at some time or other have some change made in their skin cells, we'll say, that brings about a cancerous growth twenty years later, and, of course, there's no more radiation going on to effect that malignant growth. That seemed to be the case in many of these neoplastic changes. I'd like to know how much technical discussion of this is pertinent?

<u>This report we can read now, but it's a consequence of a process, and I'm interested in the process in 1937 as between these individuals.</u>

They all knew each other. I was the youngest and the least experienced of the lot. I believe Dr. Murphy was the chairman, but it doesn't say--does it?

<u>No.</u>

Well, anyhow Dr. Murphy was the one I had the sharpest controversy with--was least successful in convincing because he was so strongly opposed to the notion of the virus etiology of cancer. I remember that he was responsible for one or two drafts of this report that absolutely denied any significant influence to virus. I had been for some time much impressed by Dr. Rous's brilliant studies on the virus cause of sarcoma, neoplasms in chickens, fowls. He had a filterable virus that produced a fatal malignant disease in chickens. I thought that was rather convincing evidence of the virus etiology of some forms of cancer, but Dr. Murphy said, and truly at that time, that no one had ever proved that there was a virus involved in the cancers in mammalian species, although staring him in the face was this remarkable work done by Dr. Richard E. Shope, who was also at the Rockefeller Institute, on the virus that was gotten from the papillomas, wart like tumors, of cotton tail rabbits. Those wart like growths sometimes would turn into cancer. As long as they were in the ordinary wart form, you could get the virus back out of them. When they became cancerous, you couldn't get the virus anymore. You could, however, know that it must be somewhere around because you could get serological reactions--a complement-fixation test indicating that the virus, or the antigen of the virus, was still in the tissue, but that wasn't a proof that the virus was in the cancer.

The proof seemed to me to be unnecessary because we knew from other conditions that once malignancy started, it continued without any further contact with the thing that produced it, even with the carcinogenic compounds like methylcholanthrene. You can produce a fatal sarcoma in the rat, we'll say, by planting a little pellet of methlycholanthrene under the skin. You take that out, excise it after the growth is started, or even before the growth has started, and you cannot detect any loss of methylcholanthrene. The amount is so small that you can't find it by weighing it. If you remove the methylcholanthrene pellet from the tissue of the animal, the sarcomatous growth goes on, and there never is any more carcinogen in that particular thing. The same thing might be true of the virus.

Well, my controversy was mostly with Murphy. I don't remember that either Little, or Harrison, or Northrup joined into that part of the talk very much, and so when the report was published in Public Health Reports in December, 1938, it contained phrases which I now see on review of the text that I got into that in spite of the opposition of Murphy. Some people regard the statements in this report as denying that viruses are of any importance in producing malignant changes in cells. In reading over this report today, I found that which I hadn't really noticed before; Dr. Heller's quotation of a part of a sentence, saying that the many organisms which have been described as specific etiological agents of cancer may be disregarded and included viruses among these specific organisms. The text of the report doesn't bear that out. There are two sentences in this report that I'm referring to--one of them deals with animal parasites and bacteria, and the other refers to virus, ~~another~~ AND later on in the Report it says that the possibility that viruses do exist in mammalian tumors is certainly worthy of further consideration. That was meant to encourage further work on viruses.

The interpretation of this Report was so unfavorable to notions of the virus etiology of cancer that relatively little work was done on it for a while.

A few people worked on it, but nowadays it's the whole field--well, I don't say the whole field. That's a foolish remark, but there's an enormous amount of investigation going on in viruses in tumors, viruses in cancer and lo and behold, they have viruses now that produce all sorts of cancers in hamsters and in guinea pigs. They haven't tried them on human beings yet, and I don't know that they've--yes, they have some viruses out of human neoplasia, chiefly out of leukemia and some out of warts that become malignant.

This is a very difficult subject because it's so easy for tissues to become contaminated with viruses. The extraordinary pains that you have to go through to prove that if you get a virus, say, out of a muscle, or a gland, that it isn't just there in a carrier. It just might be drifting through. It's all present everywhere. Viruses contaminate most everything, so it's a very difficult field, but to go back to this report. I've always taken it as an encouragement for the investigation of viruses. Let me go ahead and say what I helped to do in the Childs Fund in this field at Yale particularly on the advice and under the influence of Dr. Rous. There were two things--I helped to secure first Rous's appointment to the Board of Scientific Advisers. He was the one we looked to for advice on viruses in cancer. The second thing was to bring to Yale a very brilliant Spaniard who had been at the Rockefeller Institute, after some remarkable work on the spreading factors so-called in vaccine, small pox vaccine material, which he had done at Barcelona. We were fortunate to be able to bring Dr. F. Duran-Reynals to Yale. I could give him a place in the space allotted to me in bacteriology. I think we had to take some rooms from somebody else, but Reynals was in there and did very remarkable work on viruses, Rous sarcoma, a whole series of virus studies in neoplasis until his death about two or three years ago.

This is a side issue. An administrative problem arose in his case which teaches a lesson. Yale University had no funds for a salary for Dr. Duran-Reynals, although they gave him an academic appointment. His salary was paid by the Childs Fund year after year. Well, after--oh, some time in the 1940s, after the war, some member of the newer board didn't think so well of the progress and productivity of his work. They began to question it, and reasonably so, because Duran-Reynals had a most wide ranging imagination and equally pictorial language. It was very difficult sometimes to grasp what he was trying to say or do, and it became a question whether he should continue to get the salary from the Childs Fund. If his salary were withdrawn, he'd lose his position at Yale. What would have happened if he had not died about the time when this problem arose, I'm not able to say. It's a disastrous situation when a foundation puts up the salary of a man who advances into, we'll say, the 50s of his years and has no footing on any other academic ladder.

The same thing happened in the case of Leonell Strong. We paid his salary, and there was a strong, deep conviction in some members of the board that his work had come to an end and that the fund wasn't justified in supporting him any further. He had to leave. He went to Buffalo. That question is whether a fund morally should support a man that is in a position of tenure and depends on the fund. It's bound to come to an end. Usually the salaries aren't large. I think the lesson drawn from it influenced the National Institutes of Health to make what they call career appointments. They provide salaries, life time salaries, for promising investigators, and some of the other organizations are now doing that too, realizing that something like cancer research is really a life long job with no assurance of a productive issue.

Well now, the relations with the others were very friendly. Dr. Little was a vigorous person who contributed to the genetical discussions of our part in here on genetics very effectively. I can't remember very much what the contribution of Dr. Northrup was. I've spoken about Dr. Murphy. Dr. Ross Harrison was the wise patriarch of research in this field, slow of speech but very cogent, absolutely honest, the one on whose opinion you depended very much.

To what extent did Dr. Parran figure in this?

Not conspicuously. I think Dr. Parran felt that he had appointed a committee of advisers, and he was wise enough to let them alone. Dr. Parran was a vigorous Surgeon General of the Public Health Service and was interested in this.

This would indicate that the Cancer Institute Act came, in part, as something of a surprise to the Public Health Service, so that faced with an institute, they had to devise a program, or some guide lines within which to function.

Yes, I'm sure the institute was set up before they had any program, and that's why I'm regretful right now that I don't have the manuscript of the thing I wrote about the man who really did it.

And yet I suspect that drafts of this report may be up at the Childs Fund—that you may have had.

I doubt it. This was something I did as Dean.

I haven't found them in the papers here.

This is what I did as Dean. Although I was thinking about the Childs Fund all the time, I don't think I mingled these two.

Once the institution on a national scale is established it brings up the relationship between the Childs Fund and other funds in the field of cancer generally For example, Yale is just on the edge, early in 1938, of the Hubbard McCormick Fund, so that there is an increase in money interest in the whole field. This may have been , in part, stampeded through these Amazons---I don't know, but the climate was created.

Yes, there was a burst of support for cancer. The National Cancer Institute was to have a large budget, but I don't recall how much. It hadn't yet set up its procedures for its council. I became a member of the Council of the National Cancer Institute fairly early and have had connections with it off and on until a few years ago which I'll talk about later. Lots of money began to come into the field of cancer research, and an enormous amount has come in later years, so that a private foundation like the Childs Fund now has to think not only about a large program in cancer research, but whether it has any opportunity. The fellowship funds that come from national sources now are so much more ample than the private foundation sets up that it's very difficult to attract able, young men to the fellowships that the foundation gives, though a lot of them prefer it to a national, governmental grant. In the field of support of research, thousands of projects are now carried and supported by the National Cancer Institute and the other institutes, whereas the private foundations have much less opportunity to function in the support. Nowadays support is so generously supplied by the government and federal agencies that even a great foundation like the Rockefeller Foundation has altered its objectives. It's not any more concerned to the same extent that it was in medical research and medical education. It's gone into population studies, agricultural studies, learning studies, communication, music.

The Public Health Service had at Harvard a group composed of Harold Stewart, Murray Shear, H. B. Andervont--I guess dating back to J. W. Schereschewsky--working on cancer.

Shields Warren.

So that they had some program.

Yes, they had a cancer group up there around the pathology department, in anatomy, biochemistry--any good medical institution is bound to have fields of interest, and they include cancer.

Had you known Voegtlin, the Director of the National Cancer Institute?

Yes, I knew Carl Voegtlin at Hopkins. He was an assistant to Dr. Abel in pharmacology. He came over from Switzerland, I think, about that time. He was a tall, very handsome, slow moving, dignified man. My first impression was that he was amusing as to his language. He wanted to be very emphatic one day, talking about a drug, and he said, "This substance is useless to say the least doubtless!"

That's marvelous.

That was very convincing.

I knew Dr. Voegtlin in connection with this cancer report because he was the director of the new National Cancer Institute. He was not a member of this committee. We were reporting to him directly and to Dr. Parran. I saw Dr. Voegtlin repeatedly and had only one difficulty with him which came about through the activity of Dr. George M. Smith who was always full of ideas about enormous projects that would come about through his stimulation. One of them was to set up all over the country prolonged longitudinal studies of specific kinds of cancer, like gastric cancer, or mammary cancer in places. He drew Dr. Voegtlin into this plan a good deal, so that Dr. Voegtlin and Dr. Smith came up particularly to see the chairman of the Board of Scientific Advisers--myself. They wanted to see if they could persuade the board through me to put a large sum of money in this project. To me it was a survey type of thing that had interest, but no particular scientific value. I didn't have any real difference about it with Dr. Voegtlin, but I was a disappointment to Dr. Smith, that he didn't have his way on this thing. Is that in the records somewhere?

Yes.

Another one like that--I felt that some of those things had a piratical motivation to capture the fund. To jump ahead again and along that same line, my friend, Mr. Reginald Coombe who was the head of the Board of the Memorial Hospital one time when Dr. C. P. Rhoads was the head of the new Sloan Kettering Research Institute, they actually--those two--came up and arranged for a meeting of the Board of Scientific Advisers with them. They practically made a plain proposal to transfer the Childs Fund to the Sloan Kettering Institute. That must be in the records too. I think Mr. Coombe was the instigator of that. Dr. Rhoads was embarrassed by being brought up there to set forward the great advantages that would accrue to the world in general, so to speak, by a transfer of this money to the Sloan Kettering. It turns out that Sloan Kettering is so munificently supported by Mr. Alfred P. Sloan that it hardly needed what would have come from the Childs Fund, but anyhow, it put this proposal before a board that had already some practice in this piratical kind of operation by having to face the authorities of Yale University on matters of similar nature. The board didn't regard this as an entirely new idea.

<u>Wasn't it lucky that it was set up initially as an independent entity?</u>

Yes. A curious thing as I look back on it--Reggie Coombe, a Yale man, was on some important committees at Yale, the Education Committee, the Committee on Development. He was an attractive person, and this Childs Fund episode came near upsetting our relationship because I ventured in one discussion with him in New York to say that I didn't think it was right for him to be doing. That cooled our relationship for a while. It came back in an almost extremely friendly, easy way afterwards. Reggie Coombe was devoted to cancer research like so many who have had the tragedy of it in their family--his daughter died of sarcoma of the bone in her leg.

We certainly have jumped around. I don't know what this will make any sense, or not.

<u>We've got the year fairly well covered. I think we ought to stop now. Tomorrow I'd like to go back to some other things at Yale and pick up the Childs Fund somewhat later.</u>

What kind of things at Yale?

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It may have mystified you on occasion, but we've been discussing the deanship for about a week. We talked about problems of the general hospital and its need in medical education and for educational purposes, the budgetary problems about that and means to help it. We talked about research in terms of the pursuit of two ideas--one, the Nutrition Institute, a marvelous idea, but unsuccessful, and the other, the Childs Fund. Today I'd like to come to what is an administrative problem, I believe, inside the University and a human problem as well, involving Dr. Robert M. Yerkes. I think to get into it we ought to take up the connection Dr. Yerkes had with Yale because it had long continuity. As a matter of fact, his first publication,"Provision for the Study of Monkeys and Apes", appears in Science in 1916, so he'd had long continuity with the subject by the time the problem emerges as a real problem in 1936-1937. In 1924, at Yale there was established an Institute of Psychology. In 1925, an affiliate of that Institute was the Primate Laboratory with Dr. Yerkes housed in temporary headquarters in a barn with four chimpanzees.

In New Haven, wasn't it?

Yes, in New Haven. A good deal of work went on, but most of it seems to have been of the field research variety. For example, in 1929, there were two in particular--one Dr. Harold C. Bingham who pursued a naturalistic study of the mountain gorilla in the Albert National Park in the Belgian Congo supported by Yale, in part, and the Carnegie Institution in Washington. That same year saw Dr. Henry Nissen pursuing a naturalistic study of the chimpanzee in cooperation with the staff of the Pasteur Institute Laboratory which was located in Kindia, French Guiana. The report of the latter study which I read last night was a marvelous report. But the first step taken by the University for the extension and improvement of the facilities for anthropoid research is in 1930, with the construction at Orange Park, Florida, of the Anthropoid Experiment Station. Its purpose was to establish a breeding colony of dated animals of known life history. I think the following year, in 1931, the old Primate Laboratory founded in 1925, in New Haven was abandoned, and a new laboratory was created in the Sterling Hall of Medicine called the Laboratories of Comparative Psychobiology. For administrative purposes the laboratory was transferred from the Institute of Psychology to Physiology under Dr. John F. Fulton. By a vote of the Yale Corporation on June 18, 1934, they established a University Department of Physiological Sciences, and the co-chairmen of that department were Professors Lafayette B. Mendel and John F. Fulton. In the school of medicine the Department of Physiology was changed to Laboratories of Physiology and under that term were grouped six laboratories as a conceptual group--physiology, primate biology, neurophysiology, pharmacology and toxicology, applied physiology and physiological chemistry. Only three of these were directly integrated with and responsible to the school of medicine--the laboratories of physiology, pharmacology and toxicology, and neurophysiology. Applied physiology was across town, and physiological chemistry, while it was integrated in the work of the medical school, its budget went elsewhere. The Yale Laboratory of Primate Biology while their relations were intimate with, but again, for budget and administrative purposes, they were not part of the school of medicine. Once this University Department of Physiological Sciences was established, Professor Mendel dies, and President Seymour set up a committee to study and report on the problem created by Professor Mendel's death as to what to do. As Dean, this for you was a period of retrenchment--University policy. You were asked to cut out of the budget of the medical school over a two year period fifty thousand dollars, and there's a statement in your report,"the budgetary restrictions...were particularly regretted as they wiped out not only a hoped for increase, discussed when Dr. Fulton was considering the candidacy for the professorship of Physiology at Oxford, but also cut into previous allocations", so whatever arrangements the University may have made with Dr. Fulton, the fact of retrenchment as University policy made it, I gather, impossible to perform. This is the administrative problem that emerges. In 1936 and 1937, and I gather by this time Dr. Fulton is chairman of the University Department of Physiological Sciences, he writes in his report to the president as follows, and this shows a difference in policy which is the administrative problem:

> The chairman of such a group, therefore, is bound in decisions and in the adoption of general policy to think in terms of the University, as well as the immediate needs of the School of Medicine. Consequently when pressure is brought to bear upon him to curtail things not immediately useful to the School of Medicine, decision must inevitably depend not upon actual requirements of the School of Medicine, but upon the broader needs of a great University. The School of Medicine as such might not be justified in maintaining a section of protein chemistry or a laboratory of biophysics, but, if these disciplines are not supported in any other parts of the University, it is obvious that the School of Medicine is not only justified, but it is clearly obligated to maintain them.

That's a statement by Fulton.

Yes--I have no idea as to what the University may have agreed in their bargaining with him with respect to the Oxford offer, but, you know--you hang on to a rare orchid somehow, someway, but nonetheless the University policy which he talks about here was precisely this--"to bring pressure to curtail things not immediately useful to the School of Medicine" given this particular time. You have no quarrel with his view as you state: "With this statement of the ideal of University organization I am in entire agreement", but then later you write:

> When limited funds and facilities must be distributed among numerous departments, to meet urgent needs, practical considerations take precedence over ideal arrangements. It then becomes necessary to compare obligations and to make selections according to the best judgment of the value to science and education to be anticipated from any single part of the whole organization....There is no occasion for a conflict here, but the problem of distributing support...bristles with difficulties.

Well, one of the difficulties is what to do about the Laboratories of Primate Biology because in 1936 and 1937, they are approaching termination of their supporting grants. They are without any definite assurance of continued financial support. This crisis has caused uneasiness in the staff. Members have resigned to seek other more secure positions, and this has meant a consequent curtailment of investigations. Some are abandoned, or otherwise cut back, and while it never is really clear what its relations are to the school of medicine, they are intimate, but from a budgetary point of view and administrative point of view this laboratory has nothing to do with the school of medicine, so you are compelled in this annual report to say:

> Deeply as I feel concerned in the fate of the Laboratories, I have had to tell the President, as I have intimated to Dr. Yerkes, that, in my opinion the obligation for the future support of the Laboratories of Primate Biology does not rest primarily upon the School of Medicine.

Well, the fat's in the fire. Do you remember anything about this internal problem?

I'm hazy in my recollections as to how this problem came up to me for complete solution, so to speak, because about this time I was well aware of the crisis ~~article~~ in the laboratory and that the Rockefeller Foundation had decreased its support and intended to decrease it further. I saw the value of the scientific work on behavior with these higher apes and primates. I knew that they had value as research subjects and as subjects about which teaching could be developed, but I think that the problem got so acute, and I think that as there was no one else to whom Dr. Yerkes could turn, he naturally would come into the Dean of the medical school. In addition, I always had good personal relations with Dr. Yerkes. I admired him very much. As I said to you before, he had a Jovian head, a very fine head and an appearance, a dignified man, reserved, perfectly honest, very able, and working in a field which interested me, in the sense of an amateur psychiatrist, but I knew I had no competence with it. Probably I began in a small way with this to act as an intermediate negotiator between Dr. Yerkes and the Rockefeller Foundation because I knew very well the man in whose division at the foundation this matter rested, and that was Dr. Alan Gregg. It wasn't very long before I had to prepare, really, a lawyer's brief on behalf of the argument for the continued support of Dr. Yerkes for presentation to the Rockefeller Foundation, and after visits and talks, the Foundation agreed to renew its support for a year or two more. It relieved the tension and anxiety in this case.

I don't remember what the exact next step was, but it appeared to all of us that it was necessary to have a Scientific Advisory Board connected with it, outside of the primate laboratory staff, and with that in mind, I can recall consulting Dr. Edgar Allen because an endocrinologist was needed, and I think we brought in Dr. Carl G. Hartman as chairman at that time. He was one of the first.

Yes.

Dr. Leonard Carmichael came in as a consultant about that time, but I forget who the rest of the people were.

Dr. W. H. Taliaferro from the University of Chicago.

You pronounced it wrong.

I'll be darned.

Yes, Taliaferro is an old Virginian name, and it's spelled the way you pronounce it, but it's pronounced "Toliver." He was a man I knew very well when I was out there teaching in Chicago in 1929. We became fast friends, and we still are friends. He was a great parasitologist, and you needed a parasitologist in dealing with the possible parasitic infections of these animals from jungles. Also he was a very intelligent man and knew a great deal about administration of a department, and he was a biologist basically, a fine biologist. Who else was there?

Yerkes was the director, and then you were ex-officio on this board which included Hartman, Allen, Carmichael, and Taliaferro. The problem got pretty severe in some ways because there was a special committee appointed by the Yale Corporation, a joint committee was to report their recommendation as to what to do in April of 1937.

I don't remember either what the Yale Laboratory of Primate Biology Incorporated signified, except that it did own property in Florida and wanted to be in a position to conduct business of its own. It had probably land in Orange Park. They were close to Jacksonville and had political and business connections to watch out for, and maybe one of the reasons for incorporating was to be in a position to receive gifts.

Then I think in keeping with the view that you had, that the medical school's interest should not be paramount, in the sense of being responsible, the Corporation committee may have been established so that the University committee could be called the custodian of this and while it would be administered in the Dean's

Office, it required a committee that went way beyond the Dean's Office--Hartman, Carmichael, Taliaferro, and so on.

That's a group of scientific advisers more than administrative people. Hartman was administratively inclined, and so was Leonard Carmichael. Actually that group, or one like it, had to do with the selection of Dr. Yerkes's successor in due time--I think it was Jacobson who was in the group there; Nissen was too--and determined a program for the use of the animals and the future research. My recollection of these events that we are now talking about is that I took it as if it were wholly an obligation of the Dean to work it out. I worked a good deal on this. I don't remember separating it in my mind from an affair of the medical school. You don't have the papers?

No. Just this one here. The administrative porblem is an interesting one and in a sense it's loaded with the possibilities for mischief, even for the most disciplined of people--to have a group of laboratories called a University Department with a direct line to the president and three of those laboratories reporting directly to the Dean--you know, it takes a very strong president to sustain and support the position of the University with reference to his administrative heads as Deans and have another person who is volatile, creative, impatient, like John Fulton, and I mean no disrespect to him, although it does take a degree of discipline not to deal with the three laboratories also in the direct line to the president. It places a great premium on the quality of the man who is president. I thought in reading this annual report--that these were a built in succession of straws from an administrative point of view. The Nutrition Institute was the final one because it was a bald refusal to approve publicly what had been designed and developed with approval. I can't see why

<u>you would include in your annual report the general propositions, except to clarify what you yourself had done, and had been compelled to do under the University rule, and make the record. I don't know whether this is so, or not.</u>

My recollection of the University Departments at Yale is that we paid little attention to them, either as administrative units, or as educational units. The main function of some of them was to deal with advanced degrees outside of the medical degree--the Ph D degree in the sciences. There was a University Department of Medicine which Dr. Blake and Dr. Harvey representing medicine and surgery rarely used. It appeared all the time in the catalogue as something that was there. I think Dr. Fulton and Dr. Long did have some direct association with the president at this time in connection with their university departments, or in connection with this University Department of Physiological Sciences, and associations between them and the president were troublesome to the Dean, but I've forgotten the details about it. I don't know that I worried very much about it.

<u>No, but if you were a battalion commander and had a rifle company that was assigned to regiment and assigned to you for rations....</u>

Well, Dr. Phillips, the analogy is quite proper, but the practical situation is very different. I've drawn up many organizational charts, but I've never been confined to the channels in that chart--very much like, for example, what General Somervell did when he took over the Army Service Forces. He said, "Avoid these channels. Pick up the phone and talk to your opposite numbers so you get the business done."

I have never been confined by an organizational chart, so what you say about a battalion having different commands coming to it from different sources is troublesome to a battalion in a military situation, but it wouldn't be so troublesome to a school and its parent university and departments that are in both jurisdictions. You can always reach people by easy conversation in those places, and you're in a place where even a whisper is heard, so very little goes on that you don't know about.

One thing I didn't understand about this is that the major field of their concern in the Department of Physiology was the physiology of the central nervous system of primates. Why weren't they more interested in what happened to Dr. Yerkes?

I think that was a personal matter--Dr. Yerkes and Dr. Fulton. Dr. Fulton was a neurophysiologist, had been trained by Sherrington, and he had an experimental method that required him to perform all sorts of operations on the brains of dogs, chimpanzees, and other creatures to study the pathway of nerve transmission, and what happened when you ablated certain parts of the hemisphere of the brain, but Dr. Yerkes never did and work like that, probably would not be able to carry on the kind of study he did in behavior, if he began to mutilate the animals. Yerkes was very reserved, and Fulton was very ebullient. Yerkes, I think, got on all right with Dr. Fulton, but I can't recall that they were fast friends, or easy companions. The objectives of their two departments were entirely different. Dr. Yerkes always throughout the whole period that I saw him was interested in the behavior of these animals without any more restraint on them than was necessary to keep them in cages. I went to Orange Park and got pretty close to some of these animals. It was interesting to see how at that time they were bringing some of them along by addicting them to morphine, so that the chimpanzee when it came to need a dose would go and lie down on the table and prepare itself to be injected--practically get the syringe for the doctor just like human beings, I think.

<u>I thought the Orange Park station was a source of supply. I gather from you that it was not.</u>

No. They had some animals born down there, but they kept them for their own purposes largely. They didn't supply them freely to other institutions in the country--I don't mean "freely" in not getting paid for them. They may have passed around some animals, but that was not a part of their task.

<u>Dr. Fulton did not rely on this as a source.</u>

No, Dr. Fulton bought his mostly, and Dr. Fulton didn't have many primates. He had a few on the top floor of the Sterling Hall of Medicine, but a great deal of his work was done on the higher primates. He used monkeys, and he used dogs.

<u>That would explain--I was going to say disinterest, but I don't mean it as it sounds.</u>

Separate interests.

<u>I understand, I think, why the problem fell on you. There just wasn't any other place.</u>

I think that's right and being a Dean--you have to be concerned with everything that concerns your school, and when you began to get into a problem of administration and support, such as this was, you can't let go and say, "Well, only a part of this I can do. You do the other part."

Right, but in personal terms in 1936 and 1937, it was sufficiently serious for Dr. Yerkes who saw the fruit of a life time of labor go.

Yes, he was about to be wiped out. He'd lose his colony, and he'd lose forty years of work? At least he'd lose the fruits of forty years of continuing work, but he had a great deal of notes and material to be written up, so during this period of stress, he had an outlet in his writing, working at his desk.

It's only been until recently that we've now got primate centers. This was the only source of its kind as of its day.

Yes. Now they have large establishments that have recently been built and supported by the National Institutes of Health called Primate Research Centers. They are intended to breed animals, to have animals for the study of behavior, have animals with similar susceptibilities almost as human beings have to viruses. They plan to use them for cancer research. They plan to use them, and are using them, for example, for research on leprosy which can not be transmitted to any mammalian, including the primates so far. They've innoculated a lot of them at the primate station at Covington, Louisiana at a station built and supported by the National Institutes of Health. There's still a good deal of dispute as to whether these very expensive installations, expensive places to maintain and expensive animals to study, are going to justify themselves, but they probably will in time.

Does this Dean's Report tell the outcome of this? I know it got refinanced, and I know it got helpful advice from its advisory committee.

Very much so.

The director was changed. Dr. Yerkes went out as director in 1941. I forget how long it remained down in Florida, but I know that in the past two or three years it has been moved up to Emory University at Atlanta under a totally different management--scientific and otherwise.

I think the Rockefeller refinancing, in a sense, was a temporary thing. There was an initial burst to develop a new laboratory at Orange Park for physiological studies, and then successively dwindling contributions from them. This was offset, in part, by a study of infant behavior under the Pels Fund, so that they were able to keep alive through this Dean's Report and the first report I have here by Dr. Blake because of these infant studies, although his report indicates that the financial problems are as yet really unsolved. It's a rare kind of thing.

Yes, and very courageous people stick with it.

This is just one of the many things that came to you as Dean--the maintenance of a resource, as distinct from creating the means for new research support, or new ideas to develop a program, or how to keep the local community reasonably happy and working. This is wholly different. It's personal in terms of a man's whole life work. It's also administrative because there really was no tie anywhere for this man.

We've forgotten another phase of neurophysiology that came almost separately, and that's the laboratory of neurophysiology that Dr. Dusser de Barenne had. He was in another part of the Sterling Hall of Medicine. He was a great big Hollander who had been brought over by Dr. Winternitz. He was a brilliant man who did excellent work in neurophysiology, especially the physiology of muscle. He died shortly after the period we're talking about. I don't think that

Dr. Dusser de Barenne was concerned in this primate problem.

Not at all. The story in a way was that the termination date was just shoved off again, but they did some great work. I read that field report with great fascination last night.

This is an account of the laboratory.

Then there's a special report on Dr. Nissen's field study. Maybe people better educated than we are now can make more use of the accumulated wisdom they find just by this quiet observation they have.

Probably—well, it's better if you can study these creatures in their natural environment.

In talking about the Nutrition Foundation we got you separated from the deanship, but you're entitled to some views with respect to the experience as a whole. It kept you away from the bench, but it didn't keep you from thinking about the possible financing of research and the need and necessity for such financing. In some ways, you got a better overview of the total needs because you had to deal with them than you would have, had you not had this experience as Dean, but there may be other things too—the line is certainly developed toward increasing administrative responsibility plus the search for men and ideas. Do you have any views on the deanship?

Well, as far as the sources in my case are concerned, sources of information, they were constantly increasing in number from the variety and interest. In other words, I became a member of a number of scientific advisory boards, or committees of foundations, and even of societies. I always had an interest in the bacteriological side, the Society of American Bacteriologists and Immunologists.

I became a member for a while about this time, I think, of the Advisory Board of the International Division of the Rockefeller Foundation. I was invited to be on that board. I served a year and was not reappointed, although most people were, because I had a little bit of an independent line, a line different from that which the directer, Dr. Sawyer, cared for. As a matter of fact, Dr. Sawyer, Dr. Parran, and Dr. Joseph Mountin ran that division with a closely knit management of their own, so that the meetings really were rehearsed in advance by Dr. Sawyer and his close advisors and were characterized by plans being laid before the rest of the membership and approved rather routinely, but it was full of interest. You'd listen to people of great knowledge and skill discuss problems of yellow fever, dengue, malaria, tropical diseases as well as diseases of the people of this country--all kinds of things were coming in all the time. To me, it was all part of a whole continuum from the Dean's Office to these foundation and committee meetings and back to the Dean's Office. It was just like going around your own home, so to speak. It doesn't seem to be a departure to go from one to the other. In the course of that kind of round of associations, you meet many people who might be suitable for appointment in the faculty of your own school should vacancies become available for new appointments. I imagine every Dean has a sort of a recruitment center in his brain that works all the time.

<u>A ready card index. But you do get stretched, in a way, at the International Division because of its team studies out in the field. I don't mean to delimit your interest in any way prior to your joining this, but it is a continuum in areas which the Dean's Office as a Dean's Office didn't allow. It was like salt</u>

on the steak, some condiment where you could go back to tropical medicine, deepen your interest, because you'd had that interest with much continuity. Looking from this point back on it, meeting with the International Division certainly lent itself to thinking about the kind of problems which became important with 1939, and the advent of war, thinking in broader ways rather than whether we can meet the budget for the hospital, or whether we can do this, or that. I was thinking expressly about your views on the deanship when you leave the Dean's Office. I gather you have no particular regrets with respect to it after the five year period you put in there. You'd had concern with students, with multitudinous problems.

I think it would be incorrect to say that I didn't have regrets in leaving it. First, perhaps my feelings are mixed up because the manner of the separation was a great shock to me--I mean, it depressed me for weeks and weeks and in addition that happened in December--I mean the decision to go out of the Dean's Office happened in December of 1939. My resignation was in in early January, and I had six more months of a lame duck existence in the Dean's Office in which the problems were just as pressing, as if I were going to be there all the rest of the time, and yet I didn't have much of a heart to tackle some of them. I had no association particularly with my successor at that time. It was some time before they got around to asking Dr. Blake to be an acting Dean, and he knew the place so well there wasn't any necessity for him to come and find out anything about it from me.

I regretted leaving the Dean's Office because it meant separation from my admired friends. If you go out from Cedar Street over to Trumbull Street, you've got three miles of different country between you and your own old friends. You regret too, if you're honest about it, hanging up the robes of power, because you have great power as Dean. It's there, and people respect the office, if they don't respect the man. I'm quite sure that I was natural enough and maybe vain enough to enjoy the authority and was sorry to have to put it aside.

Then many other benefits come to a Dean. He's on a great many desirable invitation lists. He has dozens of interesting and important visitors so that going out of the Dean's Office at Yale is cutting off quite a bit of life. In my case, the Childs Fund was continuing me as Chairman of the Board of Scientific Advisers, and while that appointment had no organic connection with the deanship, it gave me a lot of continuing interest in scientific enterprises and association with delightful people and interesting people, but life was not so full as if the Dean's work was going on along side of that. It was good to have the ease of freedom from some of these difficult problems, but I think I really was sorry to go.

But the times change too, and they were about to make even greater demands on you.

The war.

We ought to stop now and consider tomorrow the changes in atmosphere. One of the happy things about these reports is that you include insight into the times in which these events occur. Of course they end in 1940, and the last report is as you describe the lame duck year, less of you because it is the last report--that is, more or less a summary of reports made to you with less of you in it, as distinct from this one where you use the report at once as a summary and also an argument for policy. The period of 1939 and 1940, brings a new context in which we're going to have to operate for a while, and if Yale Medical School, or Yale University was anything like Amherst College, the friction of ideas, possibilities, and lines of cleavage developed along wholly new and unexpected routes, fight or not fight, war or no war, the America First Committee and the Committee to Aid America by aiding the allies, the Destroyer Bases Exchange--all these things began to turn the world in which we had lived in the 1930s almost upside down. I don't know that it disturbed you in those terms, but we can go back next time and find out.

I want to say one more thing about leaving the Dean's Office to have it on the record--the Office of the Childs Fund was in the Sterling Hall of Medicine, so that every day I was back in the medical school environment. The office was there, and many of our grants were at Yale. It was not a complete cut off.

I think the new offices were above the library.

Yes, from the beginning.

So there was continuity.

Yes with the people, the work, and the location.

Thursday, May 19, 1966 A-60, N. L. M.

I want to--oh, try something today in a way. I guess it's true that any period of time is a collection of variables which keep changing. I guess that is its history, and as it changes it creates new scenes that you have to confront both as a person, an individual, and as Dean. As the 1930s deepen toward the 1940s our whole involvement in self concern, which perhaps characterized the early part of the 1930s certainly up to 1937, begins to shift. There are new voices abroad in the world across the sea. We face a Johnson Act which enjoins a studied indifference--neutrality. There's the invasion of Poland, Dunkirk--successive things which are disturbing. I think this is paralleled by a political campaign between FDR and Wendell Willkie, the third term issue--all of it must have had some impact, and I'm basing this on what transpired in the atmosphere at Amherst College. It sort of churned things up. We had debates--between America Firsters and Walter White's Committee. This seemed to be the great debate that went throughout the land terminating with the blast at Pearl Harbor. This is an upsetting period in a way. I don't know how you see it from the Dean's Office. I haven't any idea, which is why I raise the question as a preliminary to the actual action you do get involved in as a consequence of war. I wonder in this period, say 1938 through 1940, with increasing severity in 1939, as reflected in our own government's actions--the limited emergency, the efforts to do two things at the same time--build a fire proof house and take out fire insurance. I don't know how you saw this, but this is a feature of the period. I don't know whether there's any response you'd care to make, or not. I hope there is. It did have implications for education--unknown ones at the moment, as to what was going to be expected of medical schools, medical personnel, scientific personnel. It just loomed suddenly. I don't know whether you anticipated it,

<u>or whether you had any thoughts about it.</u>

Yes, I had thoughts, but it's difficult for me to separate my personal emotions and thoughts from official emotions and thoughts as the Dean. The Dean's efforts were always very personal affairs for me, and I don't believe that I had the mental stature to generalize thoughts about the situation as broadly as you seem to indicate might have been the case. I was sympathetically on the side of France and Great Britain from the start. My roots are the traditions of Napoleon and French people in New Orleans, and it's natural. The British are my relatives, I've always felt, and I despised the Germans even medically before the war because we had a few of them, Prussian doctors, as visiting residents of Johns Hopkins when I was down there, and they were the most arrogant--James W. Fulbright to the contrary notwithstanding--overbearing people that I ever saw, and I didn't like them; in fact, I think--well, I have a feeling now that I expected we'd go to war in our allegiance to Anglo-Saxon traditions and ties. I expected it so much that it wasn't a surprise to me when it did occur.

In the medical school even in 1939, after the invasion of Poland, we didn't make any particular moves to get the school in a position to take part in the national effort for war. As I said before, we were effected immediately in September of 1939, when the plans for the great medical and general library of the medical school were in danger of being pushed aside by the crises of the war in Europe. That was one operation of the school that was immediately involved in the wartime situation.

I remember practically nothing about 1938, disturbing the people. I don't think it affected the school. I don't remember any particular debate. I was still in 1938, the Master of Trumbull College, and I did have arguments with the students about their aprticipation in military activities, should it be necessary for them to do so. I can remember distinctly talking to men like John Hersey and others along the lines of telling them that they seemed to me to be ashamed of their best patriotic and devotional attributes. They wouldn't admit that they would fight on the side of this country, and yet they turned out to be the unsung heroes of Guadalcanal and all sorts of places. At that time--you correct me if I'm wrong--there was a book in favor, called the <u>Wave of the Future</u> by Mrs. Anne Lindberg. They were all reading that. It has a lovely title, but it is a book that seemed to me to advise a selfish withdrawal from the affairs of your own country, and it had a little tinge of the Nazis in it, as I think her husband [Charles A.] Lindberg at that time was somewhat influenced by [Hermann Wilhelm] Göring and the air force people there in Germany. This book, the <u>Wave of the Future</u> took the students of Trumbull College by storm, so to speak, and was the basis of arguments that I'm telling you about, which shows that I did feel a concern about the attitude of the generation that had to bear the burden of a war, if it came along, but the behavior, the ordinary behavior of the medical school population, the people in Trumbull College, and those outside in the University were very little, if any, altered.

I was close to some people who were engaged in larger affairs, and I'm sure they affected my notions about medical relations with communities at that time, and one of these was Rex [Rexford Guy] Tugwell. He was in the Yale Law School, in and out of it at that time, and would come over to Trumbull College. As a matter of fact, somewhere in that time I went to Puerto Rico where Mrs. Roosevelt had been. She cleaned up San Juan, or helped to clean it up because in San Juan the people were living in hovels on sticks over the mud flats. She did something, or she and Mr. Tugwell were responsible for economic improvement that was connected almost immediately with a sanitary disaster which influenced me in thinking about whether you ought to do everything that appears to be good before you know all the consequences. What they did was to electrify the hills and farms, the backlands of Puerto Rico. When they did that, the people got electric refrigerators. Up to that time they had been boiling the milk from their cows, and when they got the electric refrigerators, they put the unboiled milk in to keep it cool and fresh. It was not many months before they had the largest epidemic of undulant fever--I mean Brucella abortus type of fever that they had ever had. It was all because they had done all this good to give them electrification in the back area farms. Someway or other that set up a principle for me in sanitary work and in thinking about public works that look as if they are going to be all to the good, that unless you thoroughly consider all the possible consequences you'd better not undertake some of them. That sort of thing was happening.

The Willkie campaign--was that 1940?

Yes.

That campaign influenced me politically because my classmate was licked in getting the nomination by Willkie, and Robert Taft was a man I admired very much, but I frankly must say that I would not have wanted to see him be president. While this convention was going on and Willkie was coming forward and got the nomination, naturally my thoughts went a good deal to the national political situation. Willkie's nomination didn't have any effect that I could recognize in the medical school, except that he had been identified with the Tennessee Valley Authority. He had been legal counsel for the public utility company.

The Tennessee Valley Authority comes into the history of the Yale Medical School, joining the school in a national, sociological experiment and undertaking because Dr. C.-E. A. Winslow fostered the ideas of the Tenneessee Valley

Authority, supported them, and knew one of the Morgans. Morgan [Arthur Ernest] was a commissioner there, I think. Well, Dr. Winslow would have Mr. Morgan at his house. He had me there too, and we'd talk about the problems of the Tennessee Valley Authority in relation to the sanitation of the area, the health of the country, and its people. I got rather aroused in listening to Mr. Morgan because he said all the time that all he was engaged in was a water conservation problem, to make these dams and to keep flood control when, in effect, he really was thinking about free electricity almost, schooling, and a socialistic-type of state which at that time was beyond my active sympathies. I feel very different now. I'm a little more educated.

Well, that's the kind of thing that would happen on the national scene and would affect the medical school to some extent.

Did you have any thoughts about the Johnson Act?

I don't know about it.

The Johnson Act was the act that enjoined neutrality.

Oh yes, I thought neutrality was humbug. You couldn't be neutral. Technically you could be neutral, but of all the things that will tie your hands is to be required by the Congress to refrain from doing certain things. At the same time we were stirred by Mr. Roosevelt's talking about the arsenal of democracy--about that time, certainly by 1940, and shockingly saying--I think in one of his speeches when he was talking about quarantining Hitler, he assured the nation that he would not send any American youth to fight on foreign soil. Didn't he say that?

Yes.

Well, those things at the age that I was then--I wasn't a kid, but I still had a few ideals about truthfulness and the meaning of words--it seemed that it wasn't a wise thing to say, an honest thing to say, and saying it showed he really didn't believe it.

When the talk of politicians, the talk of the President of the United States, did involve matters of whether or not young men would be sent abroad to fight, the welfare of your students, their future lives both in medicine and in the college became rather prominant in your daily thoughts, and again this provoked arguments with the studnts, but I didn't take part in any movement, or any organized discussions of the war; in fact, Yale didn't have any as far as I know at that time.

Did it get into in any way the debate that ran between Lindberg and the America First group and William Allen White--because some Yale graduates, Dean Acheson, Judge Thacher, and two others who may not have been Yale graduates, but whose names escape me at the moment, published a statement in the New York Times, trying to find a way around the Johnson Act in order to aid Lord Lothian who was then in Washington, D. C. and was much interested in trying to sustain England and hoping that some means could be found--I don't know whether that argument--well, I know it occasioned a good bit of discussion at Amherst, but then maybe we read the New York Times.

I was not on the governmental plane that Dean Acheson had achieved by that time. He was a public figure and had served in the Roosevelt Administration. He had already been an Assistant Secretary of the Treasury, and he is a very articulate, intelligent person who was destined--even then you could see that he was destined for high achievement.

<u>This publication in the New York Times was as a private citizen then. I think Roosevelt had sort of eliminated him from the New Deal.</u>

Yes, he had his polite farewell with Franklin Roosevelt.

<u>But this in a way was the basis for later things--like Lend Lease which is a new concept certainly and the Destroyer Bases Exchange which came in 1940.</u>

I'm very familiar with both the actual operation of the Destroyer-Bases Exchange and Lend Lease from the point of view of its effect upon the Army Medical Department, the huge problems of preventive Medicine that came about through our having a responsibility for the civil population as well as a place for troops on the islands in the Caribbean. Lend Lease presented the Army with the greatest logistical problem that anyone had ever faced. Again, it didn't appear to influence things in the time we're talking about. It was later that I came into contact with Lend Lease through its effect on medical supplies, competition between the British and the United States for material of all kinds.

<u>But as an idea in terms of making effective the arsenal of democracy.</u>

I was all for it personally.

<u>Yes, although its full implications as to how you go about doing this is a different problem entirely.</u>

I think I was cynical enough to think that the term itself was a misleading one. I can remember talking to friends as to whether anybody really believed that the lend would be repaid, or the lease honored. I thought it was a give-away which it largely was, especially to the Russians.

<u>Yes, but in general terms there is a shift in the scene.</u>

Yes. It's sort of pervasive and huge. I must confess that the scene was shifting, but that it was like great objects moving in the dark to me. That sounds like a stupid thing to say, but I'm telling you the truth.

Well, when you come to each day, though we're taking advantage of hindsight now--when you come to each day, the thing which I suspect was important, certainly at Amherst, was self-concern in the sense that I wanted to continue with my education. In 1942, I won a fellowship to Harvard. I wanted to take that fellowship so hard I could taste it, and two days later I was in the Army.

Yes.

It was that kind of thing. I don't think you ever really--I'm talking about myself--that you ever really come to understand the nature of the forces that were alive until you're practically through the experience. You walk in and take Dachau and you have a visual image of what this was all about, this inhumanity--incredible things. Then you get a kind of footnote, a rationale, a visual picture which you don't get in 1939, or 1940. It's just beyond you.

Yes.

I suspect that happened to a lot of kids. It was a fumbling period. They wanted to continue with the warm and familiar.

That's right.

And this despite the fact that a great discussion raged and certainly sympathies went toward London--you know, and how do you share self-sympathy with concern for London?

That was inspired and kept ablaze by Ed Morrow. It used to move me a great deal to hear "This is London." Also I had the recollections of my close attachment to Brigadier Charles Hudson from World War I. I almost claim him as kin. It wasn't long before the Battle of Britain disturbed us a great deal. That was in 1940, I think, after Dunkirk. Dunkirk was a moving thing that we understood, and we wondered how that nation could stand without any arms, without any fortifications, without any ships to interpose--they had ships, but they couldn't get them in there. The Battle of Britain started me and a good many of my friends on definite plans of how the school might do something about aiding the British and largely though it took a personal turn as to how many British children we could take out of London and the bombed area. Dr. Fulton and Lucia Fulton were leaders in that. They brought over the two Florey children. [Sir (later Lord) Howard Walter Florey] I tried to bring over Charlie Hudson's son--I had an enormous amount of papers to fill out, statements as to what my income had been for the previous five years in detail, and by the time I got it all together, young Charlie Hudson had gone into the British Air Force. He didn't come over.

Another stirring thing about that time was the arrival in New Haven of a gentleman now known as Lord Howard Walter Florey. He came over here to get some money to promote the production of penicillin. He came to see John Fulton, and he came to see me. This was in the 1940s, and he wanted about twenty-five hundred dollars. We got it for him--largely John got it from the Rockefeller Foundation. That was the beginning of the practical penicillin development in England. Penicillin had been discovered by Dr. Alexander Fleming about 1928, and actually crystalized in the preceeding year, or close to it, because Dr. Gye whom we mentioned here not long ago, a few days ago, told me that his son had been wounded and had an open wound in his thorax, his chest, which was purulent, and he managed to get a few crystals of penicillin and put them in to this boy's chest with his own hands. He had a marvelous result, but that's the way it was doled out by crystals, not as they made it later.

Professor George R. Cowgill in the Yale Medical School Department of Physiological Chemistry, a man we mentioned before and who had some thoughts about cancer research which were not supported by the Childs Fund, was one of our friends and consultants. He moved out to the fermentation laboratory of the government in Peoria, Illinois, where they made penicillin according to new formulas that he brought in for the fermentation by the mold Penicillium, a new strain of the mold. He did splendid work out there--he could get it out by the ton. Those things were going on. ~~One~~

One thought about what you do to bring back the opportunities for training such as Dr. Winternitz had set up in the Brady Laboratory around World War I, and we did have some training courses in prospect, but I was out of the deanship when that developed. I suppose that there are a great many things, if you sat down long enough and laid a piece of paper out you could realize that there were involvements in events that were the product of the European war.

How much can you tell me about this little episode? [Biological Warfare] I don't want to get into these others yet. [Army Epidemiological Board] Can I use those papers that you have downstairs? Can I look at them?

Yes.

The ones that you showed me when we went to try to find the papers on the development of the board?

Yes, you can look at those. They're very disorderly. There's a report from every commission in there.

I'd like to see them.

All right.

I have a feeling about scraps of paper. But this one on biological warfare--there isn't very much on this.

It's tremendous. In my Army file down there, my declassified file on biological warfare--it's probably got two hundred and fifty pages in one binding. Do you want to get into this now?

Yes. Let me tell you what I've seen. There was an interview with René Dubos who wouldn't talk about this.

You interviewed him?

No, a friend of mine did. He also talked with Alphonse Dochez who also talked mainly about influenza and colds.

What year was this?

This was--I'm guessing, maybe five years ago, but this in terms of the way in which it emerged as a problem, given what was going on in the world, is an interesting story, and I don't know that there's anything anywhere on it.

How far do you want to go in talking about biological warfare now? It begins for me in 1941.

Right--in November.

In 1941, there were rumors of the Germans using bacteriological warfare, as we called it at that time. These rumors were very strong, and they were brought to the attention of Mr. Stimson [Henry L. Stimson, Secretary of War].

You may have to go over--parenthetically--all this when I get that file because I may get it mixed up now.

<u>Do you want me to go and get the file?</u>

I'll go get it.

<u>Let's do that because then we'll have the contemporary items.</u>

I have a great many papers on biological warfare because I was asked in 1941, by Mr. Stimson to come to Washington and meet with General [James Stevens] Simmons, Captain Charles Stephenson, Dr. William B. Sarles, and Dr. E. B. Fred chiefly, to discuss what we knew about biological warfare, to help Mr. Stimson evaluate certain rumors and to consider what might be done for the future in this country in relation to preparations for defense against bacteriological warfare and possible use of it. This began for me, I think, in the summer of 1941. I was called in probably because I was a bacteriologist. I had no previous experience with the subject, and I couldn't find anything, as I remember now, in the general scientific literature about it, but I did by accident find in the index of the <u>New York Times</u> a number of references to the previous use of tests of bacteria spread artificially by Germans. I went to the University Club in New York, and they had a collection of the <u>New York Times</u>. I spent several days going over those references that I picked up in the index. There was enough to show that possibly the Germans had used it a little bit in World War I on a very small scale in sabotage efforts in this country. There was a good account of how some Germans had liberated <u>Bacillus prodigiosus</u>, a test organism. That's a miracle producing bacterium of great historical interest. It produces a red pigment, and it's called prodigiosus because they got it out of one of the early miracles in a cathedral. In the cathedrals, moist and damp, they put the wafers of the host out in front of the alter, and this bacterium would settle on them, invisibly at first, and then a red spot would occur, and it was the blood of Christ, so they would go out and kill a lot of Jews. That's that organism. It's one that the bacteriologists use for teaching--the beautiful color it produces. The color is soluble, and I once had a necktie dyed with the pigment of the prodigiosus, so it was a test organism. They have several test organisms that are nonpathogenic, can be sprayed around and followed and were used in many of these things.

We collected what we knew about biological warfare at that time in 1941. This group that Mr. Stimson called together was quite an informal committee. There had been some papers published by medical officers, chiefly General Leon A. Fox. He was a captain then. He published his first paper in 1938, or 1939, about biological warfare and came to the conclusion that the technical difficulties would be so great that you couldn't use those biological agents for offensive purposes. General Simmons also as far back as 1934, had written about the vulnerability of the Panama Canal to biological warfare attack, and General Simmons was so impressed that when he came in preventive medicine in 1940, he determined to his own satisfaction, that there was a great danger of the spread of yellow fever by liberation of infected mosquitoes. He used this scare to persuade the general staff and others to sanction vaccination of the American soldiers against yellow fever. That had an enormous consequence for the Army, and we'll talk about it later, because the vaccine, unknown to the people making it even, contained homologous serum hepatitis virus, and we'll come back to that later. It produced perhaps two hundred thousand cases of hepatitis in 1942.

To go back to the Stimson connection and skipping a lot of other things in talking about what we knew about this in 1941, there was enough afoot to make you take it very seriously, and we told Mr. Stimson that. This is before the Japanese had attacked Pearl Harbor, the period I'm dealing with now, and I'm trying to get the date of a very alarming episode that occurred with the International Health Division Laboratories of the Rockefeller Foundation in New York. Japanese appeared there, and tried to obtain the yellow fever virus strains that were being used in experiments toward making a vaccine. It looked as if we had afoot some things that would confirm General Simmons' fear that yellow fever might be used by an enemy country to infect our soldiers. There were other rumors that made it seem apparent that it might be used, or it had been used, and this brings us to the time in August of 1941--as I said, the summer of 1941--when Mr. Stimson and Mr. Bundy arranged to have an advisory committee formed. This did become an official committee when Mr. Stimson asked the National Research Council to set up a committee. The Secretary of War didn't appoint the committee at the time. The National Research Council did, and I was still a member of it. I was a member of that committee and of all the changes of the committee that occurred during the war. General Simmons was a member. E. B. Fred stayed as a member. Sarles stayed as a member, and a great many other people.

It had a series of names mostly to try to disguise it--the WB Committee. B. W. was too disclosive, so they just reversed the letters. Then it was called the ABC committee at one time. Those names don't mean anything, except they thought they were being very clever in concealing what was going on. My connection with it was largely because the Surgeon General's Office was involved from the start. General Simmons persuaded Major General James C. Magee right away that there was such a danger, or supposed danger, from biological material liberated by the enemy that it was absolutely obligatory upon the Surgeon General to devise protective measures, to find out what the Germans were going to use and immunize troops against the organisms. Anthrax was one they talked about immunizing against. Even at one point we went so far as to persuade German prisoners of war to be bled so that we could have the serum to test to see whether they had any antibodies in their own troops to unusual organisms.

Biological warfare had been outlawed by the Geneva Convention, but people felt that the Germans would disregard that, and later on when the Japanese came in, it was felt that they would disregard it. The Surgeon General had to be informed about all this and had to take a part of it. The organizations that really handled the problem for the Army were the Office of the Surgeon General and the Chemical Warfare Service. Naturally it falls in techniques of toxic gas use. The National Research Council had definite advisory obligations to the Surgeon General, and relations were fine. I do not believe that the Office of Scientific Research and Development had much to do with this problem, but the Committee on Medical Research was under the Office of Scientific Research and Development [OSRD], and the Committee on Medical Research [CMR], was housed in the National Research Council, Division of Medical Sciences, and so there was a close relation between all these organizations.

It was a difficult floundering time. Nobody knew what would be used, or how it would be dispersed, or put into the air, or put in water. There were all sorts of vague positions, or situations--for instance, serious consideration was given to the possibility that the Germans would load a great big airplane full of culture fuild, gallons and gallons and put typhoid bacillus in there. Then they would fly it over a reservoir, and the United States would shoot down the plane and would bring down typhoid into their water supplies. There was great worry about sabotage.

When--now if we can pass on to Pearl Harbor in December of 1941, the situation became much more tense and serious because actually Lt. General Robert C. Richardson was very fearful that the Japanese would loose the material, or distribute it in a fashion of sabotage in Hawaii, so we sent from the Preventive Medicine Office, Lt. Colonel Joseph F. Sadusk, a long time good friend of mine, out to Hawaii, and he took charge of the antibiological warfare service in Honolulu and was busy at it for more than a year, set up things that had to be done--set up testing laboratories, collect samples, watch people, control them as much as you could. There was a certain amount of that in this country too for the watch over possible sabotage of food supplies. For instance, one time a great deal of canned meat was found to be full of powdered glass, and it hadn't been seen until the cans were open after the meat had been sterilized. That sent Brigadier General Raymond A. Kelser to great effort on the part of the veterinary service to sample enough cans of meat to be sure that they were all right.

I think this was an accidental thing--this glass in the food.

Bacteriolgoical warfare got to be extremely interesting and extremely important scientifically because money poured in to conduct new experiments on the study of metabolism of organisms, of toxins, and the study was for two reasons. One was to get to know as much as you could about large scale production of organisms that might be used by an enemy and might be used by ourselves. The organisms particularly were the anthrax bacillus, some of the viruses like the Q fever virus which had been worked on very extensively and has been used to produce infection in volunteer American troops in fields out in Dugway Proving Ground, Utah. Toxins were studied like Staphylococcus enterotoxin. A very interesting possibility was botulinus toxin which is so extremely poisonous that a billionth of a billionth of a little bit will kill.

There was no facility, as they call it, for doing this work in the Army at this time. The Surgeon General had the ordinary bacteriological laboratories, but no big facility and didn't want one, so the big facility for the mass production of cultures and toxins was put in the hands of the Chemical Corps and an enormous plant was built at Fort Detrich, Maryland, to carry on their investigations--both of the metabolism of the organisms and the means of dispersal. They built all sorts of bombs out there that when dropped from airplanes burst into little round packages that flutter away and scatter the stuff, bombs that produce smoke-like emulsions in the air that drift over fields, over acres, all very interesting and as alarming as you want to make it, and as foolish as you want to make it. Nobody knew what really was the extent of the threat. Nobody knew what actually was practical, what could be done, and nobody knew what would be the best means of protection against these things. One part of the protection was detection--how are you going to know that these invisible microorganisms are in the air? They float around. You can detect them by sedimenting samples of air in an impinging apparatus, but that takes time and you have to wait for them to grow too.

The philosophical and moral problems were very difficult of solution. I'm sure General Magee and Major General Norman T. Kirk, his successor, couldn't bear the thought of the medical department going into biological warfare and killing people in what they thought was a very dirty and sneaky method. They would say the same about poison gas. Well, as a matter of fact, you can make a fine argument for the fact that biological warfare is a humane method, even more so than gas. The idea soon became prevalent that you probably wouldn't kill many people with these things, but you could make them sick, and if you could make a town sick, or a garrison sick, you could take it with ease, and most of the work has gone on that way now--even in gas warfare where there is quite as much concern with incapacitating agents. Incapacitating agents nowadays include everything from LSD on to all sorts of behavior-affecting compounds, and they don't necessarily kill. The same may be said about most of these infections that would be produced experimentally, artifically in biological warfare. They might be uncomfortable, but not lethal.

The Surgeon General centered the biological warfare interests of his department in Preventive Medicine, and that's how it fell to us in Preventive Medicine to stand for the Surgeon General in these things. It was called by some other name that was quite secret and nondisclosive ~~unit~~ [the Special Protection Unit] in the Surgeon General's Office, and it was handled in the Laboratory Division in the Preventive Medicine Service. We had a liaison with the Chemical Warfare Service which worked very well for a while because we commissioned Dr. Cornelius P. Rhoads, the head of the Memorial Hospital and Sloan Kettering Institute as a colonel with the thought that Dr. Rhoads, being a medical colonel and the liaison officer with the Surgeon General, would be more devoted to the Surgeon General than he would be to the chief of the Chemical Corps, but just the opposite happened. Colonel Rhoads was fascinated by the Chemical Corps's scientific interest in the subject, and he became a close friend of General Alden H. Waitt, and not only did we not see so much of him in the liaison position with the Surgeon General, but he actually made no end of difficulties. This whole story is just personal difficulties and conflicts of personalities and opinions.

Colonel Rhoads stayed in this a couple of years--not quite to the end of war, practically to the end of the war, about a year from the end of the war. He had been brought in by Mr. Stimson on a pledge that he would be allowed to go back to Sloan Kettering in about a year or so. He stayed a year, but he then insisted that he be released and it fell to me working with Major General George Lull, the Deputy Surgeon General, to go over the papers in Mr. Stimson's office and to arrange for this discharge of Colonel Rhoads, although we at that time wanted to send him to Japan. We were getting ready for an invasion of Japan at the end of 1944, or the middle of 1944, and the Surgeon General took actions, and Preventive Medicine helped to carry the actions through for the preparation of and storage for antitoxins in large amounts, botulinus antitoxins, what antibodies we could get against anthrax, and a good one was found out after a while. There was a good deal of attention paid to an organism called Bacterium tularense, the organisms that cause tularemia, and to the rickettsia of Q fever. It was a curious mixture of sort of frantic preparation to protect the troops against something which could not be weighed or measured in any exact manner, a mixture between that and some very important scientific advances because it was like so many things that happened in the war--they result in scientific advances. Great advances in Preventive Medicine have occurred in every war and medicine and surgery of course, and most of that goes over into the civilian so that the product from a war is not all death and destruction.

I have here a complete lost of all these committees and technical things--who, what, and where. Look at all this--here's a whole page of names of people we drew in. When the committee--I don't think I want to put those in the record. It isn't necessary. When the committee got really set up the Secretary found that the field was too big just to be carried by his usual staff, so he brought in Mr. George Merck, who was the president of Merck and Company to be the head of biological warfare in the Office of the Secretary of War, and

Mr. Merck stayed there until the end of the war. He wrote this report that I showed you called the Merck Report. He was a very valuable, level-headed man in that position. I think he helped Mr. Stimson who had great confidence in him. We all did, and it helped very much to have George Merck remain the responsible officer or representative--he wasn't commissioned--in the Secretary's Office.

I have here--I wish I'd had this when I was talking about yellow fever vaccine. This is a page from a publication of the War Department called Technical Manual TM 30-480, called "Hand Book on Japanese Military Forces" dated May 14, 1941. It tells about the Japanese, but in small print at the bottom of page 40, in this edition, is this sentence "The existence of bacterial warfare battalions has been reported"--that is, among the Japanese--"and organizations and strength are unknown. It is believed that yellow fever would be the most likely virus that may be used."

General Simmons got that inserted in there, and that appeared only in one printing of that manual. The next sets of prints leave it out. Meanwhile the vaccine had been sanctioned by the Army. That's pretty clever. It scared them.

Yes.

Well, the biological warfare effort and interest still continues. It's being supported by the Chemical Warfare Service largely at a rate perhaps of ten million dollars a year, I suppose. Does that give that enough?

In terms of our relationship to our allies?

Yes--the British were intensely interested, and we had liaison with them all the time. They had a biological warfare station of their own at Porton. I knew the bacteriology professor who was in charge of that. They were particularly interested in producing anthrax in sheep. They would be tethered in a field and a shell containing anthrax exploded, and anthrax would be produced in animals that were wounded by fragments contaminated with the organism. We had a close connection with the Canadians in exchange of all information and in addition secured in the St. Lawrence River an uninhabited island on which it was possible for some of our medical officers and other people from the Chemical Warfare Service to work on foot and mouth disease and rinderpest. You couldn't import that. That's too dangerous to bring into the country, but the Canadians let the Americans put on experiments on this island somewhere in the St. Lawrence River.

The other big establishment for testing biological warfare agents is what is known as Dugway Proving Ground. Have you ever been there?

No.

Dugway Proving Ground is just to the west of Salt Lake City. It's a government reserve that is bigger than the state of Rhode Island, in the hills and rugged mountains. It's practically a desert; in fact, it is the basin of a great ancient lake called Bonneville. It is so dry that the tracks of the Donner Expedition, or Donner covered wagons going through to the west are still there--so they say. I've seen them. I don't know whether they freshen them up all the time. Dugway Proving Ground is so big that you can fire artillery at targets against the hills and see how you can spread biological agents from the shelling. Dugway Proving Grounds is where the test with Q fever virus was put on with about a thousand volunteer American soldiers in the field. The material was liberated on a carefully measured wind, and samples taken everywhere, a beautiful piece of work by Colonel William D. Tigertt.

Bacteriological warfare was kept very secret. Nobody could talk about it. There was nothing said to people. Public Relations weren't involved because it was so secret. It came to rather interesting notice however in 1944, when the Japanese sent fire ballons over here. The Japanese made a great many paper balloons, and they liberated them in Japan. They went way up and got into the jet stream. They crossed the Pacific and fell in the mountains of Oregon, California--oh, there must have been twenty, or thirty of them. We had to go and have people watching and get those balloons, the fragments. An enormous amount of work had to be done to see whether they were carrying biological agents. We thought they were. It was an extraordinary thing that these things came over. Fortunately they didn't carry biological agents--they would burn, but they didn't make any serious forest fires. They could have--"Japanese Fire Balloons" they called them. Those jet streams are the ones that have so much to do with our weather, and they're very constant, pretty fast.

<u>On the positive side--you mentioned metabolic studies on bacteria. I remember the ones that you did, that we talked about back in Hopkins and Rochester. The positive side with reference to medicine now--you know. Are there publications?</u>

Oh yes, publications on anthrax toxin and antitoxin. Most of this scientific work has been published, and the publications will tell about how you make improvements in the production of diphtheria toxin, for example, or botulinus toxin by altering the medium. There are studies on the nature of these toxic substances that were very much like those that were carried forward by Eaton who worked on diphtheria and tetanus toxins in my laboratory at Yale. The thing I tried to do with tetanus has not been picked up, or carried forward--the mystery of how tetanus toxin gets around in the body. It's still unsolved. It's supposed to be picked up by peripheral nerves and travel along nerve trunks to the central nervous system. That's in there.

Also while this is going on there's great improvement in physical apparatus and precision apparatus. You can measure--these are called aerosols, and they found out a great deal about how large a particle in an aerosol must be, say, down below 10 microns. It won't go down into your lungs, if it's even a little bit bigger. The method of measuring the number of particles in an aerosol in a cloud all have been beautifully worked out--all sorts of optical apparatus has been developed. Manufacturers have been aided a great deal by their collaboration in these big projects. I think a great deal of positive contributions have been made.

It is a very old thing. Here is a picture from Esquire Magazine of 1965, showing a catapult throwing a dead man into a fortress. In the old days, they found people dead of small pox and threw them with catapults over the walls of the city. Your friend, Lord Jeffery Amherst, was an early practitioner of....

This particular art?

Yes, he was with the British and would have arrows dipped in small pox lesions in human beings and shoot the Indians with them, and he produced out-breaks of small pox among the Indians before the revolution.

It's an old art.

It's an old art. That was small pox. They also did that with plague. When people died of plague they threw them over the wall, or into the fortresses of cities.

It did represent a potential danger which had to be met.

Oh yes.

I only knew about its early beginnings but from what you said this had continuity right through the war and continues--that studies are being made.

There's enormous investment in them, and I think their research program would run into the millions. After I finished with these committees in war time, I came back to be Technical Director of Research in the Office of the Surgeon General and immediately got back into bacteriological warfare activities which involved both the Chemical Corps and the Surgeon General's Office at that time. Then I became a member not as a representative of the Surgeon General, but just as a member of what was called the Army Scientific Advisory Panel, [ASAP]. That Scientific Advisory Panel had a separate sub-panel on biological warfare which is still in operation in the Office of the Chief of Research in the Department of the Army.

I think I'll turn you loose.

O.K.

Monday, May 23, 1966 A-60, N. L. M.

Last time we talked in largely general terms about 1939 and 1940, the coming of the war, the shift in the wind, and its consequent changes in thinking about what to do. In the end that became the preoccupation of everyone who had some responsibility for some continuing program--what would its effect be? The Germans were loose. Britain was being bombed, and we were trying to do two things at the same time, one consistent with the Johnson Act, neutrality, and the other realizing that we were involved, really, and all that we were was involved--Western European Civilization. It forced reconsideration of events, and it was in that context that I noted certain positions you held. I don't know how important they were, or whether they were important. I'm not sure even when they begin, but there is a relationship, a continuing relationship, with the National Research Council on two particular committees, one on scientific personnel as consultant, seeking men and ideas, and the other in the field of pathology which would indicate lines to Washington and the center of things. The National Research Council was open to you, as it had been through acquaintanceship picked up over the years. Out of this context there emerge certainly two men in particular, initially, who were responsible for focusing movement, or their anxieties into the special board, the Army Epidemiological Board, though it has a larger title. I want you to think in terms of the emergence of this board, and more particularly of the two people I mentioned, Colonel James Stevens Simmons and Dr. Francis G. Blake, though there are others too, but its background, the piling, the reference, or key points that helped to float and sustain it. This is quite important in the light of what happens.

I have the opinion that the connection with the National Research Council in 1940, was a relatively minor affair from their point of view as it was from mine. It was habitual with Deans and members of faculties of medical schools to serve on all sorts of committees of organizations at that time, and it still is. You've named two committees of the National Research Council, Division of Medical Sciences which apparently I belonged to, but I do not think either one of them was of any particular importance. The manpower, scientific manpower personnel committee was one that dealt with questionaires, circulated people to find out what their commitments were and make priority lists that might be useful if it became necessary for the council, or any of the agencies to call in men to serve voluntarily on some scientific administrative project. The Committee on Pathology was a broadly interested committee. It was not limited to anatomical pathology, but was one concerned with the real basic meaning of pathology which is scientific knowledge of disease, and this dealt with pathological physiology. It dealt with infectious diseases and quite a number of things that are outside of anatomical pathology. Those committees were pleasant and interesting bodies to be united with. They had two or three meetings a year, sometimes more, and when called together, this caused a group of men from around the country to meet in Washington for a day, or two. It was a mechanism for keeping the members currently informed, and the members could make some contribution too, but as I say, neither one of those committees in my opinion, was important.

In addition the National Research Council Division of Medical Sciences had at least a two day annual meeting in Washington at which summary reports were made on large fields of activity within the Division of Medical Sciences. That was an occasion when former officers of the division would appear, and it was also a social as well as a scientific occasion. What other things brought me to Washington at that time, I don't know, but I seem to think there were some things like the activities of the Leonard Wood Memorial, a Foundation for Leprosy studies and control, on which I was a member of the Scientific Advisory Board for a good many years. It met usually in New York, but sometimes it met in Washington and was another continuing connection for me with the people concerned with the control of infectious diseases, people who did not live in New Haven only, or live in Washington only, but were representative people from around the country.

I think I've already told about the disturbances that were coming into our lives through the overwhelming march of the Germans through the Low Countries, the fall of France, Dunkirk for the British, the bombing of London, the Battle of Britain, and the concern we felt to do something to bring some of the British children to this country. That kept us personally in contact with the progress of the war in Europe. I had no real close connection with the preventive medicine developments in the Office of the Surgeon General, although I knew a bit about what was going on through my connections with Dr. Blake, whom you've mentioned, and Lt. Colonel, as he then was, Simmons. I really haven't very much more that I can add that I can drag up out of my recollection about activities of 1940.

<u>We mentioned one last time, not 1940, this special study that you did for the Secretary of War which....</u>

On biological warfare?

<u>Yes.</u>

That was in 1941. That was all covered in that other talk.

<u>In so far as you know, even if it's second hand, how did this emerge and what is it related to in the way of background?</u>

You mean the Army Epidemiological Board?

Yes.

The story should really go back to the activities and ideas of General George M. Sternberg who was the Surgeon General of the United States Army from about 1893 until 1912, somewhere along in there. Part of his activities that can be said to be related directly to the genesis of the idea of the Army Epidemiological Board were those activities through which he established research boards that had in their objectives, their procedures, many of the elements that reappear in the Army Epidemiological Board set up. These were research boards that General Sternberg established in the Medical Department of the Army about 1900, when the United States overcame Spain in 1898, and sailed out into the world as a great, coming naval power and for the first time an international influence and force. It was soon realized that the United States was responsible for the welfare and health of millions and millions of people living in a tropical environment, and that the health of these people was properly a matter for consideration by the Medical Department of the Army which was the only professional, medically equipped and trained unit in the War Department that could undertake the necessary work.

In 1900, General Sternberg established the Walter Reed Commission for the study of yellow fever in Cuba and within a year—that story is so well known that you don't want to rehearse it here, but within a year Walter Reed and his associates, Jesse W. Lazear, Aristide Agramonte, and JAMES CARROLL, had shown that the Aedes egypti mosquito was the arthropod vector of yellow fever and that yellow fever was caused by a filterable virus. That was all done within a year. By 1901, yellow fever had been stopped in Havana, and the whole of the modern era of the control of yellow fever began to develop. Walter Reed had a long and possibly disorganizing field service experience with the Army, and at one period he did go off to Johns Hopkins and was able to study under Dr. Welch for a while which was very important, and Walter Reed hoped to get back there some day which he did after the Cuban experience.

I mention this because Walter Reed's association with Dr. Welch is not sufficiently well known. A few people know that it was Dr. Welch who said to Walter Reed once and I think wrote him, that since he could not find any visible bacteria, any microorganisms in the blood of yellow fever patients, he really should look for a filterable virus. About that time the work on foot and mouth disease, a viral infection, was first coming out [Löffler and Frosch, 1897-1898], and Dr. Welch was alert to those things. For example, I heard it said— well, an example that makes me want to make this point is that I heard today a major general high in the Medical Department, speaking at the 25th Anniversary of the founding establishment of the Army Epidemiological Board, say that Walter Reed like Sternberg was a lone worker without much help from advisers and consultants as the investigator can get nowadays from the wide association. Well, that's not true. Walter Reed had advice from Dr. Welch, and Walter Reed had great advice from Carlos Finlay who believed that this mosquito carried yellow fever, but wasn't able to prove it, and Walter Reed had the extraordinary and important advice from Major Carter who noticed that after a case of yellow fever appeared in a town, say, in Louisiana, or somewhere, there was about a period of two weeks before the next case appeared, and that observation of Carter's....

<u>Made all the difference.</u>

Yes, made all the difference, so to Walter Reed from the outside came the definite, positive suggestion, almost an opinion, of Carlos Finlay that the mosquito was the carrier, Carter's observation of the period of time that was required for the development of the virus in an infected person and transferring it to the mosquito, and then Dr. Welch's suggestion that it was a virus. Walter Reed had fairly good advice, and that was used by him with great power and skill.

Now, the type of operation of the Yellow Fever Commission in Havana had these main elements. At first, it involved the calling together of a group of able, well informed men capable of conducting experiments under difficult conditions with very scientific, critical protocols. That was one element. The other element was the association of civilians with the military in such an undertaking which occurs again, as you will see. Walter Reed was associated with Carlos Finlay who was a civilian, and Dr. Agramonte in Havana and several other people whose names I don't remember at the moment were civilians and knowledgeable civilians. The third element of importance in the arrangements for any such board is the financing. Walter Reed probably had relatively little money for the conduct of the work that he was doing on yellow fever, and he didn't need much money. He had a hospital that had been taken over from the Spaniards. He had volunteer soldiers as subjects, and he needed practically very little equipment besides a few tubes and a microscope. The financing, however, had to be provided from the Medical Department of the Army. That required travel expenses and other things, so all of those elements were in that original Yellow Fever Commission's charter, so to speak, or set up.

At the same time in 1900, or about the same time, General Sternberg established the Tropical Disease Research Board in the Philippines which was a very important one. It lasted from 1900, really, until about 1933, through about two or three phases. There was one board that lasted a while, and then lapsed a bit, I think, under Surgeon General Robert M. O'Reilly and then another board was appointed, and then a third one. The people on those Philippine Boards were given the freest charter, so to speak, or the finest choice of problems they would work on so long as they were problems that they saw were important to the people in the Philippine Islands, for example, and to the troops in these islands, and they were thought to be important from the point of view of their threat to the health of soldiers and of civilians and to the economy of the country. The purpose of undertaking scientific research in the Philippine Islands was to make it possible for the Tropical Disease Research Board to study these diseases in the land in which they occurred. They had a principle which reappeared again in the Army Epidemiological Board objectives; to study diseases where they occur and also in laboratories that were rather backup laboratories, you might say, or where the basic work could be carried on after the specimens were first examined in the field.

Well, examples of what was done out in the Philippines was the discovery of beriberi by Captain Charles B. Vedder, a whole lot of nutritional studies on the deficiencies of vitamins, or from eating polished rice which causes neuritis from the lack of vitamins, edema, paralysis. There were excellent entomological studies on the mosquitoes and arthropod vectors of the islands. There were studies on malaria of all kinds. Colonel Joseph F. Siler and later Colonel James Stevens Simmons discovered the virus of dengue fever out there and its transmission by an <u>Aedes</u> mosquito. The work was carried on in totally modern methods and conceptions that almost--as I've often thought, the Army Epidemiological Board was just a continuation of that kind of thing.

Another research board was set up in Panama, and to that Colonel Simmons, or Major Simmons then, was transferred, and he served on this board in Panama for a number of years. He got deeply interested in malaria and had some extraordinary field experiences when the troops were taken out of their barracks in Panama City, or the Canal Zone and were sent on maneuvers through the jungle down there. In neither case did they come across yellow fever, as far as I remember. Of course, in the Philippines there is no yellow fever. It's one of the mysteries of the world--why there is no ~~malaria~~ yellow fever in the Philippines, or in Southeast Asia, or India. Those countries including New Guinea are the natural habitat of the very mosquito that carries dengue fever and carries yellow fever, but for some reason, known only to God so far, yellow fever has never broken out in those regions, and there's no immunity of the people.

This fact will come up later when we talk about quarantine regulations worked out by Preventive Medicine in World War II. The fear of the introduction of yellow fever into India was so great that the quarantine regulations of the most restrictive type were instituted by the British and the Indian governments, and at one time these regulations were so difficult to live up to and so obstructive that our troops could not be landed. They had to stay two weeks on a ship in a harbor in Bombey, and air flights were impeded. I'll tell more about that later.

To go back to these boards--they did excellent research in the Philippines and in Panama, and it is important to note that Lt. Colonel Simmons, as he's now becoming, finally Brigadier General Simmons, who was the founder, in my opinion, of the Army Epidemiological Board, had basic experience as a member of these research boards in the Philippines and in Panama, and actually as the President of the Board in Panama for a while.

Another group of boards came about in World War I, appointed by General William Crawford Gorgas, the Surgeon General, and these are called the Pneumonia Commission. There were dreadful streptococcal infections and empyema--

highly fatal--and pneumonias in the induction centers and in the big camps in the United States in 1918-1919, which is the same time also as the fierce pandemic of influenza that swept the world. You couldn't do anything about influenza. You can't do much about it at present, except to vaccinate against it, but they didn't even have a vaccine then. The mortality from sickness in that period from 1918 to 1919, was due to streptococcal pneumonia, lobar pneumonia of the pneumococcus type, empyemas, septicemia, and very violent infections. This Pneumonia Board that was appointed at that time had a very distinguished composition. Its leader, I think, was Dr. Rufus Cole who was the head of the Rockefeller Institute Hospital. Associated with him was Dr. Tom Rivers, the brilliant Dr. O. T. Avery, another brilliant and thoughtful worker in infectious diseases, Dr. Alphonse R. Dochez, and a very substantial man who becomes important again in the Army Epidemiological Board, Dr. Francis Gilman Blake. That group worked just as the Army Epidemiological Board Commissions worked by going out into the field and studying the diseases among the troops in their living environment, the ecology in which they existed, and making observations in the field and in the military laboratories, and bringing back to their own laboratories specimens for further study. Dr. William G. MacCallum who was a member of the Pneumonia Commission brought back a great deal of material from penumonic lungs and other lesions to Johns Hopkins Department of Pathology. The bacterial and serological studies were conducted mostly at the Rockefeller Institute with Dr. Cole, Dr. Avery, and Dr. Dochez.

Now that is, I think, a sufficient background of the Army Epidemiological Board because the same general ideas prevailed in the establishment of the board and many of the same people that I've mentioned were connected with the Board. In the Surgeon General's Office, General Simmons, as I said, was brought first from Panama in 1939, to be in the Office of the Surgeon of the 1st Corps

Command in Boston. He then was transferred to the Surgeon General's Office in Washington on February 24, 1940. The records aren't entirely clear, but I think he was chosen for this move through the knowledge that the then Surgeon General, Major General Charles R. Reynolds, had of General Simmons. General Reynolds was distinctly interested in Preventive Medicine. Nevertheless, in the Surgeon General's Office at this time Preventive Medicine had no standing as a specialty. The activities in Preventive Medicine were diffused in a nameless fashion through the Professional Service Division in the Office of the Surgeon General. The Surgeon General explained in his Annual Report for that year, that the demands on the Professional Service Division were so varied, as he put it, that it had been found impractical to bracket any of the activities in any special subdivision. Professional service, for example, was concerned with military sanitation, with reports of sanitary inspectors on which action had to be taken by the Surgeon General. It was concerned with statistics of morbidity and mortality. It was concerned with venereal disease control. It was concerned with a whole range of preventive medicine and public health, but under no name as such.

Well, it didn't take General Simmons very long to persuade Surgeon General Magee who I think already was of the same opinion as General Simmons, that re-organization was necessary, that increased power and increased personnel were necessary if preventive medicine were to do anything worth while, so it was on the 7th of May, 1940, very shortly--Simmons there in February 24, 1940--in about six weeks, they issued an order setting up the Preventive Medicine Sub-division still under the chief of Professional Service.

That was a distressing thing for preventive medicine and for General Simmons too because the chief of Professional Service was and continued for some time to be an officer who thought that the chief important duties of a medical officer were only the care of the sick and the wounded. He didn't encourage preventive medicine, and he got in the way of a lot of things, this time and later, that were desirable. Now it was in this period between May and the middle of December, 1940, that the plans for what became the Army Epidemiological Board were formulated and finally approved. The formulation seemed to have occurred, as I have been told, in conversation between Lt. Colonel Simmons, Dr. Francis Blake, Dr. Dochez and perhaps Dr. Avery, but Blake and Simmons were the chief ones that made the moves, and it was Blake who was then Professor of Medicine and Dean at Yale, having succeeded me in the deanship, at this time--no, he hadn't come in at this time, had he?

<u>It was just at this time--December.</u>

Oh yes--December, but I'm thinking of Blake getting in before June, to talk about the Board--perhaps he did. It's the period after June to December. Curiously enough at this 25th Anniversary of the Army Epidemiological Board, celebrated today, Dr. Colin MacLeod, who was one of the first members of a commission under the board, recalled that Dr. Blake had spoken to him somewhere in the summer of 1940, so persuasively, that he cancelled his application for a commission in the Navy and decided to be the head of the Pneumonia Commission under this new Board, preferring that, as he said, as a service to any other available at that time because it fitted in with his interests, although it meant he would have to remain as a civilian which he did all during the war. We tried to get Dr. MacLeod to come in to the Army, but we never could get a uniform on him. Dr. MacLeod was natively a Canadian, and I think he'd become a naturalized citizen at that time. He was in a very promising academic, medical career at New York University.

Dr. Blake and General Simmons worked very hard with such consultation as they could find, largely, I think, from Dochez, Avery, and Rufus Cole on plans for this board. Essentially it was to be a central board composed of, I think, nine members and a number of other groups which were called commissions composed altogether of civilians at this time. The military persons connected with the board were the servants of the board, but not members of it. General Simmons had this board placed in the Office of the Preventive Medicine Service in the Office of the Surgeon General of the Army. I was under him as deputy chief of Preventive Medicine Service in the Surgeon General's Office. I became in time the administrator of this board, and at the start the Board had one or two other military assistants, notably two who haven't been recognized sufficiently. One was Major William S. Stone, head of sanitation in the newly set up preventive medicine organization and the other was Major Carl Lundeberg, an epidemiologist really.

Well, after preliminary drafts, I think, of the plan for the organization of the board, the board was established and at that time was called the Board for the Control and Investigation of Influenza and other Epidemic Diseases in the Army. That long title didn't survive very long, but it was an extremely valuable sales tag. The recollection of the horrors of the 1918-1919 pandemic of influenza and the deaths among the soldiers and the population of this country was bright in the minds of all the officials that had anything to do with the approval of this plan. General Simmons knew the power of that group of words very well, and he used them intentionally.

<u>He built on it.</u>

Yes--after formulation of the plans General Simmons in December of 1940--what's the date?

<u>December 27, 1940.</u>

On December 27, 1940, General Simmons addressed a letter to the Chief of the Planning and Training Division through the Chief of Professional Service Division recommending that the Surgeon send the attached letter after signing it, a letter which had been written by General Simmons, to the Adjutant General asking for the establishment of the board with the long name with the approval of the Secretary of War. This was acted upon with astonishing speed. The letter from Surgeon General Magee which had been written, as I said, by Colonel Simmons, dated December 27, 1940, was approved by order of the Secretary of War on January 11, 1941. That is exactly fifteen days—astonishing!

<u>Wasn't Selective Service in operation at this time? Weren't we collecting young men in camps? The opening phrases and paragraphs of this letter relate—you know....</u>

To the expansion of the Army.

<u>Yes.</u>

Oh yes. We weren't in the war, but there were many things of a war-like nature going on. As a matter of fact—this is a side issue, but the Army began to expand late in 1939, and by this time in 1940, or shortly thereafter, there were a million six hundred thousand men in an Army which at the beginning in 1939, was around two hundred thousand, maybe—not quite that much. In 1940, the Army was large enough for the War Department and the Operations Division of the War Department to conduct maneuvers in New York State, in Wisconsin, in South Carolina, and in other places in which divisions and actual Armies were put in the field, so that by the time we really got into the war, our generals had had field experience in handling corps, army, divisions--all sorts of logistical problems. This expansion had gone pretty far when the Board was approved, and it set this country in position to enter World War II in 1941, after Pearl Harbor, December 7, 1941, in better position than it ever was at the beginning of any war in which the country had been engaged. It still needed plenty to be done, but it was in pretty good shape.

Well, this is an excellent letter from General Simmons. It deals with organization and personnel, the status of civilian personnel, the procedures to be followed and how they would function. The board was to consist of a central body of scientists and technicians, such as would be required, and would meet at the call of the Surgeon General. The Surgeon General's call was in this sense, as in all others, issued by the Surgeon General's representative--that is, the Surgeon General calling the meeting here would be General Simmons. That's a stock phrase. Then there would be an additional group of expert scientists and technicians--"technists" as he called them.

When called on by the President of the Board, who was a civilian, the investigative terms would go out in the field and do work there, and this was called "fire fighting." An investigative team would come out right away, go into a place, look it over, make a study, see what needed to be done, make recommendations, and often they would continue work on the problems at their home laboratories in universities when they got through with the field work. This was done with great generosity by the civilians who composed these commissions and by the universities to which they were attached. They were faculty, and the universities contributed laboratory space, the salaries of their faculty members who were members of the commissions. The twenty dollars a day per diem which was paid to these men while they were on duty would barely feed them. The universities paid their salaries and never asked for reimbursement.

It was quite a wonderful thing; the way they were moved by patriotic sentiments even before the United States entered the war. After the United States entered the war, there was nothing asked of these commission members that they would not do. In a year there were upwards of two hundred civilians, experts connected with the board in this manner.

As I said, the long name of the board--"Board for the Investigation and Control of Influenza and other Epidemic Diseases in the Army"--took too long to say, and after it made its impression General Simmons didn't object to the informal substitution of Army Epidemiological Board for the long name. He himself called it on his organization charts and in his reports anything that happened to come into his head. Sometimes it would be the "civilian epidemics board." Sometimes it would be the "civilian epidemic control board" and those--well, he might have had some reason to emphasize the word "civilian" in those cases. That's the only time that came in, but he was very proud of this civilian connection. He was connected with, or had contact with, or knew all the foremost public health, preventive medicine authorities in the country. He traveled a great deal to scientific meetings. He gave a great many papers about preventive medicine, and he had a great host of friends and scientific colleagues.

This Board was something quite new in Army procedure. The newness of it, aside from the analogies to the Sternberg boards which were chiefly composed of military people, was that this one put a greater emphasis on the civilian component. This was rather new, and the fact that they would be paid a <u>per diem</u> of twenty dollars a day when on duty was something that had rarely been done. I can remember dealing with some of these questions at a time when the budget officer of the War Department was so opposed to this that he would exaggerate the possible dangers and disadvantages of letting the Surgeon General have such power over the pocket book of the War Department and the professional manpower of the nation. He said that there was nothing im this plan that would prevent the Surgeon General from hiring every doctor in the country which was never the intention. These were carefully selected and thoroughly, broadly trained specialists.

The Board was officially approved in January 11, 1941, and it began to work by first establishing different commissions. They had all sorts of names--ten commissions and according to this memorandum here, the first ones were appointed on February 6, 1941, and the 10th was appointed in May, 1942. The names of the commissions are what you would expect in general, but you'd have to know more about what they did to tell what they were--the commission on influenza, on epidemiological survey, on pneumonia, on measles and mumps, on hemolytic streptococcal infection, on meningococcal meningitis, on neurotropic virus disease, on cross infections in hospitals, which became the Commission on Airborne Infections, on tropical diseases, and on acute respiratory diseases. Those names are simply the names of the most important groups of diseases that the military had to contend with.

The Commission on Epidemiological Survey, for example, was an ill-defined affair of which I happened to be the director. Its main program was outlined by Dr. Dochez on the supposition that if you made a constant bacteriological examination from cultures from throat swabs and looked for influenza bacilli, pneumococci, and streptococci and found the normal day to day proportion of the distribution of the organisms that appeared in cultures you would be able when that distribution was changed to predict that something was going to happen. For instance, if you had a normal, proportional distribution in which five percent of the colonies would be meningococci, and all of a sudden you'd get seventy percent meningococci, you'd be frightened.

<u>You'd know that something was up--right.</u>

It was a very tedious study though, and it was hard to work out. We set up our groups to help us in the First Corps area around Boston, the Fourth Corps area in Durham, North Carolina, and the Ninth Corps area in San Francisco, and we had another group under Morales Otero in San Juan, Puerto Rico. All of them did some of this routine bacteriological survey that I've told about. Some of them became quite limited and specialized from the start. For instance, the Ninth Corps area, in California, had a group on which Dr. Charles E. Smith served with distinction. This group got deeply interested in dust borne coccidioidomycosis infections in the Army Air Force Training Bases in the Desert Training Center, wherever there were dry, dusty regions that carried these spores.

I don't know how much technical stuff you care to have put in here, but I should make it limited, unless there's some principle involved.

<u>You're primary source material for this, and I'd rather not have you limit such comment as you care to make. The interesting thing on the establishment of this commission is on February 22, 1941--this plan....</u>

Yes, but the commission was appointed on February 26, 1941.

<u>The program was almost immediate--work began.</u>

Yes--well, I came down here many times and met with a good many of these men. J. Howard Mueller in Boston, David T. Smith in Durham, and I were on the telephone for hours--most of that was set up by telephone--there was a great pressure to get it done.

Up to this point, aside from the twenty dollars <u>per diem</u> allowed for men who would come on duty and aside from reimbursement for travel expenses, there was no money in the Treasury of the Board--no treasury, no money. It was very hard to get the money. The Director of the Bureau of the Budget of the War Department couldn't understand this. He just thought that this was almost a criminal raid upon the funds of the War Department. I think Major Lundeberg and a Colonel Francis C. Tyng in Supply in the Surgeon General's Office carried on most of the negotiation toward this end, representing Colonel Simmons who even at that time knew very well how to navigate in these troubled waters in such a way that he was never in danger of getting sunk personally, and he could therefore sort of stand on the deck and drive his associates into dealing with the hard-boiled War Department budget people. I don't think we got any money set aside for this until late in the year, but I left those papers downstairs.

I think the Board in a series of meetings during the spring of 1941--one in February, one in May, and a later one in June developed a yearly program with a budget and submitted it. By the time it received consideration, the request was for a proportion of the year, because I don't think funds were made available until October of 1941. There's a parallel development going on that seemed to suggest to Mr. Bundy who was Assistant to the Secretary of War that there might be duplication involved. The Committee on Medical Research under OSRD had been floated, I think, some time in 1941, and --you know, who was going to do what.

Well, the difficulty with words is that they easily appear to be duplicates, but it's the substance. For instance, at one time somebody in the Defense Department said, "We're going to cut out duplication. Look they have a Department of Physics in the Army and a Department of Physics in the Navy, and that's duplication! We're not going to have any duplication."

To go back to the names of these commissions--there's much duplication underneath these names, far more than the general gentlemen running the funds of the War Department possibly appreciated, or asked questions about. You have a Commission on Pneumonia, for example. Well, one of the common complications of measles is pneumonia, but we have a Commission on Measles. We have a Commission on Influenza. Influenza is a respiratory disease, and pneumonia is a constant complication of it. The Commission on Hemolytic Streptococcal Infections takes in everything from a sore throat to bronchial pneumonia, and there's pneumonia again. Cross-Infections in Hospitals is another one in which the Commission on Cross-Infections in Hospitals would be dealing with the pneumonias that arise from infected sprays coming from the mouths of patients, sneezes and whatnot. It became the Airborne Infection Commission in 1942, but it was still interested in the results of airborne infections which were often pneumonic. Tropical diseases--there are many pneumonia-like diseases, pneumonitis and pneumonias connected with tropical diseases. The whole thing looks as though it's separate, but it could be said to be a duplication, although each one of those has its own characteristics as pneumonia and a certain ecological and associational individuality that sets it apart.

In addition, neither General Simmons nor I, nor the Directors of these commissions felt bound by these names. In 1943, there was this huge outbreak of hepatitis caused by the introduction of the virus of hepatitis into soldiers with the vaccine against yellow fever. There may have been two hundred thousand cases. The mortality was not high--two percent of hospitalized cases. Not two percent of the thousands. Many of them never went to the hospital. We threw everything we had into this critical investigation of hepatitis which was frightening. This outbreak was frightening to General George C. Marshall and to the Secretary of War. It was incapacitating great sections of the Air Force particularly at the time of the Battle of Midway. It was a great worry. It was investigated in greatest secrecy too. To attack that problem we used the virologists and epidemiologists from the Commission on Influenza and the Commission on Pneumonia, and the Commission on Measles and Mumps did some of the best work on hepatitis as did the Commission on Tropical Diseases under Wilbur Sawyer. Later on the Commission on Neurotropic Virus Diseases, working in the Middle west and in Sicily, got deeply interested in normal infectious hepatitis and its relations to post vaccinal jaundice. Well, that continued throughout the rest of the war.

One of the very delightful things about the Board was that we could understand each other even if we put meanings in words that were not apparent from consultation with a dictionary. That same thing, I think, struck me in the organization in the Preventive Medicine Service in the Office of the Surgeon General--the names of the divisions and branches. The Preventive Medicine Service got to be a huge organization, and it was a puzzle to give names to all the sections and branches. General Simmons was constantly changing them, and yet they were often just names to subdivisions of one group that had done the whole thing at one time, say laboratories, or enteric infections. Those names didn't trouble us too much. You had to be very precise when you sent the organization chart forward to be approved in the Office of the Surgeon General's Executive Officer, but among ourselves, certainly so far as I was concerned, the name didn't mean too much; in fact, at one time I thought that the creation and the naming of subdivisions was a method of securing a slot for a promotion. Create a new branch, put a captain in, and make him a major in a week. That was done.

Now, we've finished enough with the finances, I think. This board--I'd like when I get that sheet back to put in the amounts because it's pitifully small.

I think when you bring those papers back, it will shed more light on the process--the internal struggle for recognition within the Army, quite apart from Preventive Medicine, plus this relationship outside, again for recognition, to the new idea that was floated under the Committee on Medical Research of OSRD. Somewhere along the line, I guess, Dr. Blake was persuasive enough to obtain from Dr. Lewis H. Weed a letter to Mr. Harvey Bundy sustaining the necessity for this board and its financing. This early period became what was referred to in some notes that I have seen as the fight for the recognition of the entire program and a battle for the appropriation.

That went on all the time.

We'd better come back to that because I'm just about at the end here. We've gone on for over an hour.

Yes, it's twenty after three.

Tuesday, May 24, 1966 A-60, N. L. M.

Yesterday we got into--oh, the historical background of the Army Epidemiological Board with some insight into then Lt. Colonel Simmons.

Simmons was a full colonel by that time.

We traced something of his background and interest.

See if that isn't signed Colonel?

He wrote that for General Magee--Lt. Colonel--this letter which came out representing his thinking, or the thinking within the Preventive Medicine Section....

It's the combined thinking of Colonel Simmons, Dr. Blake, and Dr. Dochez, I think.

This letter was accepted--I'm not using proper Army terms--by the Secretary of War on January 11, 1941.

The letter was submitted over the signature of the Surgeon General to the Adjutant General, and the Adjutant General laid it before somebody in the Office of the Secretary of War who was able to put an endorsement on it as of January 11, 1941, "approved" [with minor exceptions] "by order of the Secretary of War."

Which gives it a very good parenthood, and I think that is important in subsequent developments.

That approval by the Secretary of War was a very high and important approval. It overrides lesser approvals, and it was very important further when these consultants were appointed. The first Board was appointed by the Secretary of War. Most of the consultants were appointed by the Secretary of War; in fact, we called them "consultants to the Secretary of War", and they were very proud of that. It had a valuable leverage in many places that they went.

This letter approved embodies an idea which is activated as something real in a series of meetings in February through June, 1941. A program and a suggested budget comes out of these meetings. There's work afoot, but insufficient funds. I think as of that year, or for taht year, the Board was authorized not to exceed fifteen thousand dollars.

Very small.

The Beard seemed to require not only the approval of its program, but funds with which to implement that program, and this takes us pretty much through 1941--October.

The first thing that was done in the organization of the Board was the decision on the numbers of commissions to be appointed first and the directors of those commissions. If you'll give me that penciled note, I'll refer to them.

We talked--where is that? Here. We talked about the commissions.

I'll just put in again for the record that by February 6, 1941, seven commissions had been appointed, and by November, 1914, the eighth commission had been appointed, and by December, 1941, the ninth commission. Altogether there were ten commissions, the last in May of 1942. Once those commissions were appointed, or were decided upon, directors were appointed, and the directors were asked by Dr. Blake who had been appointed the President of the Board, to formulate plans and to suggest names of men who would be suitable as members of the commissions. That took a lot of hard work and time in the early part of 1941. It was rather rapidly completed. At the same time a commission director, after he had gathered around himself some of the specialists in the field of a particular commission's interests, formulated a program for the work of the commission. Field work was implicit--if I may use such a word-- in the "fire fighting" conception of the Board. The commission without formulating a program would just say that it was ready to undertake investigations in the field, in camps, or in recruitment centers, or wherever troops might be, and wherever troops might be faced with a problem that this particular commission was competent to attack. Everyone had the same idea. You didn't have to formulate a research program to attack events that had not yet occurred. At the same time each commission could make a program for the continued, long term studies that they would carry out in their own university laboratories. That was easily done.

As you say, the next important thing, and this was simultaneously worked on with the other plans, were the efforts to obtain funds. The funds were needed for three purposes--travel funds for members of the commission, assurance that the per diem of twenty dollars a day while on active duty in the field would be paid, and the money that could be used for the support of research carried on under the auspices of the commission, or by the commission. That was quite a new problem, a new maneuver at least in the War Department's-- I won't say that it was entirely new in the War Department's support of research, but it was in larger volume and under more urgent circumstances at this time than it had been for some time in the past. The people who were dealing with this subject of the financing of the Board didn't understand it. They didn't understand the aims of the Board very well and were anxious about federal money coming through the War Department for the support of these medical investigative projects. One group of people in the Budget Office of the War Department, especially the director, were suspicious that the arrangements for a per diem of twenty dollars a day and no limitations set yet on the amount that might be made available would, as he told me, possibly give the Surgeon General the power to call in all the doctors in the United States. It was ridiculous. He was a very difficult man to deal with when it came to getting money for the activities of the Board, and as a matter of fact, he wasn't persuaded to approve this from the War Department Budget Bureau point of view until into 1942.

The Office of the Secretary of War wished to assure itself that this undertaking by the Army Epidemiological Board was not something that would duplicate other things that the War Department was doing, or was being done by agencies close to the War Department. The agencies close to the War Department in the medical field were at this time the Office of Scientific Research and Development which had come along and also the National Academy of Science's National Research Council, and there are records in these papers we have here before us of the application of the War Department to, we'll say, the National Research Council, to determine whether or not the Council regarded this program and proposals for the Army Epidemiological Board as a duplication of effort in medical research. The War Department addressed letters to Dr. Lewis Weed who was chairman of the Division of Medical Sciences--I think Mr. Bundy was the one who approached him and in a very natural way because the National Research Council had been set up by the National Academy of Sciences in 1919, in accordance with suggestions in an Executive Order of President Wilson to furnish scientific advice and services to government departments. That was its function. In essence that was the function of the original National Academy of Sciences, but the National Research Council was more of an operating agency in its relationship to government departments than the National Academy of Sciences. It was perfectly natural and proper that the Secretary of War would call on the National Research Council for advice on the validity of the program of the Board, on its capabilities, and particularly as to whether they thought it was an effort in duplication that might be wasteful of money.

The replies, as I remember them and I've seen them recently, both from Dr. Weed and also Dr. A. N. Richards, who was the Chairman of the Committee on Medical Research of the Office of Scientific Research and Development, were that the Board was not a duplicating mechanism, that it had a militarily oriented program that the National Research Council did not have and that it was set right in the midst of the military affairs in a way that the National Research Council could never attain. Although they didn't say it, they showed, and everyone knew this to be true, that Dr. Richards and Dr. Weed were very close and admiring friends of General Simmons; as a matter of fact, the composition of the Board and of the commissions was enriched by drawing into it a great many members of the boards and committees of the National Research Council and the members of the National Academy of Sciences. Dr. Blake—well, I could name dozens of them that had positions in both organizations. Furthermore, they were—well, the first authorization of the Surgeon General to set up a separate subdivision of Preventive Medicine in his office contained a requirement that that Preventive Medicine Division should have a liaison relationship not only with the Public Health Service, but with all civilian agencies that were concerned in the field of Preventive Medicine, and it was assumed—General Simmons was the official liaison officer to the National Research Council and to the Committee on Medical Research; in fact, he was a member of the Committee n Medical Research appointed by the Secretary of War on the recommendation of the Surgeon General, so this was all very natural--what we called S. O. P. It isn't going outside of the War Department to have such relationships.

There were some trips outside the War Department by individuals such as General Simmons and myself, but that was done to bring influence to bear on officials in the government by laying problems before important civilians outside the government who could reach, we'll say, the Secretary of War, or reach some of the members of the General Staff which I will tell about more plainly when it comes to the story of the allotments and the control of the personnel for the Board.

Maybe I'm again being too logical, for which please forgive me, but this letter--this letter of December 27, 1940, in view of expansion, had in it a note of urgency. The action from the first and second meetings of the Board related to a sense of urgency--that is, young men are coming into the Army, and time is important, and this appeared to be something of a road block.

What was a road block?

Whether the program was going to be a duplication, or whether it should be supported with funds seems to have been a duplication itself in the face of this urgency.

Well, General Simmons and Dr. Blake certainly had the sense of urgency, but the guardians of the check books have a sense of conservatism. The country was not yet at war and a few of them appreciated the semi-war like conditions of recruitment centers, mobilization centers, training centers. At the same time the Army was increasing, getting up by the end of 1941, to above a million men, and it had engaged by that time in the extensive maneuvers that we talked about. Those who were concerned in the composition of that letter knew more than some of the people who were mostly interested in financing.

Right--and I think it's interesting to point out that Dr. Weed's letter builds on this sense of urgency--the early experience with influenza and its potential explosion at any time.

Yes. Well, that was General Simmons' cleverness and wisdom at putting the word "influenza" in the title of the organization. It was an alarm, sounding an alarm at the start, and influenza--you don't need to have a war to have a devastating influenza epidemic among people in the land. That would attack both civilians and military at the same time, as happened in 1918-1919.

Also from my experience I'm sure that a sense of urgency, or an exhibition of urgency, adds emphasis to the points of persuasion. If you just flatly put that out, nobody would pay any attention to it. I showed you in the Manual about Japanese medical affairs a footnote saying that the Japanese might use yellow fever for biological warfare. That puts urgency in a sort of threatening manner. I think the letter of December 27, 1940, is not at all overdone. I thought it was urgent and urgent too, in the sense that anyone knows that these things take time, so the faster you can get them going the better.

I--coming to it fresh as I do--in reading this I don't get the sense of salesmanship here. This is real.

It's a sincere thing--of course it is.

What is it--whatever aggravation comes in terms of delay doesn't subtract from the seriousness of it and the sincerity of it, although I know human problems in a big organization like the Army take time. It's interesting, I think, to know that in one of the memoranda I read this morning that even the conservative forces within the War Department who had to do with finance in the very act of putting a limitation on the funds which could be used in 1941, also included in that order, in the event that something like influenza might occur, a provision which empowered the Board to do whatever was necessary, and you can't get looser language than that.

No. Also the letter had to be fairly strongly stated to overcome reluctance of the stand pat Medical Department officers.

I wondered about that.

They didn't care for this Board. They thought it was an intrusion--a lot of them did. They were--I won't say jealous. They didn't have the capacity to be jealous--I mean they weren't jealous because they were better; they were jealous because they had a system with which they were satisfied. They'd had years of peace.

The warm and familiar.

Yes. We ran across that all the time as will come out in some other things here. Efforts of the Board to conduct its work with some independence and originality came up all the time against the habituated rule--governed medical officers.

This accumulated pressure and clarity of the program and its need from the National Research Council and OSRD aided not a little....

Oh yes.

In educating personnel that had to deal with finance to release funds, though while the budget was designed as of June 26, I believe, and papers were forwarded, the actual grant didn't come until some time in October 6.

1941?

Yes, so funds were proportioned for the year.

Just to go outside of the Board-National Research Council-Medical Department relationship--I could show you in books that I have downstairs how policy of the Surgeon General was based on recommendations from Preventive Medicine that got approved by special committies of the National Research Council. The whole immunization program was submitted--I was going to say dose by dose, but I mean material by material, to the National Research Council. It would come back from the National Research Council, after deliberate consideration in a committee, approved, and then the Preventive Medicine Division would lay the plan before the Surgeon General for approval. As these things in Preventive Medicine that affected the whole Army could not be put into force by the Surgeon General, he then had to get the approval usually of the Assistant Chief of Staff G-4 of the War Department General Staff. If you'll look at the Army Regulations, we'll say, on everything on sanitation, hygiene, immunization, everything for the preservation of the health of troops that concerned the behavior of the troops, they all are over the signature of General Marshall who was Chief of Staff. The Surgeon General can't give any orders in the line. He can order things for his own people in the Medical Department, but he can't issue any commands for line troops. There are whole books of orders that are just signed by order of the Chief of Staff signed George Marshall--I mean by order of the Secretary of War signed George Marshall. That binds the National Research Council by its action in approving the recommendation that comes to it from Preventive Medicine, we'll say, for the adoption of the yellow fever vaccine. It puts the National Research Council in a chain of what becomes command.

Right.

Well, those relations were very harmonious. We knew each other, and some man from Preventive Medicine attended every committee meeting in the National Research Council that had anything to do with the work of prevention of disease all through the years of the war. We knew not only the officers, but the members of their advisory committees, and we had free access to much special information that they gathered, or was reported to them for information. Well, I think it might look on the surface of this as if it were a strictly Medical Department-War Department undertaking, but it was done in thorough collaboration with important civilian agencies.

Well, is there any more there?

I think we've clarified the financing--that is, it was available in October, but by that time some of the commissions had started working and I think introduced new problems which the War Department hadn't confronted--the whole matter of contract with the universities, the special problem--I guess it's in Camp Clayborne which led to....

Atypical pneumonia.

Yes, which was unforeseen at the time, but a commission laboratory emerged as necessary in terms of the continuity which I think you ought to go into.

Well, the contact--the War Department contracts--or "research contracts" as we called them, contracts for the conduct of research, were full of problems that the War Department financial staff had never met before like this. They were used to contracts for tangible objects--"hardware" it is called. They were used to contracting with people that could make something according to specifications in the contract and deliver it on a certain day, or suffer a penalty. They had to learn that you could not do that with ideas and biological materials. With the latter you can't be sure how you're going to come out, and you can't be sure that the problem remains the same while you're working on it. Contracts had to be adjusted to intellectual flexibility and a degree of uncertainty that doesn't exist in the ordinary commercial field. These contracting officers tried at first to just adapt the ordinary commercial type of hardware contract to the intellectually, and I say spiritually, variable factors that they hadn't had much to do with before; namely, the problems and the scientific investigative methods on the one hand and the universities as organizations and the people of the universities on the other--a group of people they hadn't dealt with before and a group of subjects that they hadn't dealt with. It took a good deal of time to reach understandings and work out satisfactory contracts.

Those early contracts were submitted by universities to their legal counsel. Strong objections were made, and drafts went back and forth while the work was already under way. It has always been very generous of the universities to take on these costly jobs for the government without immediate reimbursement. It happened in two ways that they weren't reimbursed immediately--one was that some of this work began before the contracts were actually signed, and the university paid with the prospect of being repaid, and the second was the lapse of time that occurs between the beginning of a fiscal year and the time when an appropriation becomes available. There were periods in all of this work when there was no money--for maybe two months--to be paid to the universities to meet additional expenses. Often the Congress had not acted by the end of the fiscal year and the beginning of the next fiscal year. Sometimes they hadn't acted until the session was either over, or about to begin. The rule was that the authorities in the budget sections of these university departments could continue to make money available at the rate at which it had been made available in the previous part of the year, even though no new appropriation had been made. Often that wasn't enough. There was more work going on and need for money than the older rate of pay could support, so the universities went rather far into--I won't say real debt, but they advanced sums of money to keep this work going out of a sense of patriotism.

The same things happened to investigators who were sent abroad and moved around, or were lost track of, so to speak, and didn't receive any pay for several months sometimes. They were working in the back woods of Okinawa, way off in Iceland, or Newfoundland, or North Africa, and they didn't get any pay because this pay didn't come through ordinary Army channels. The paymaster with the troops never paid this money. This money came out of a central Washington pay system with very complicated vouchers to fill out and all sorts of scrutiny of the items by the accountants. Finally it got paid. I have here records I was going to use, the actual budgets by commissions for every year from 1941 until 1946. It's interesting that the total expenditure for the ten commissions over that period was $1,498,044. The present rate of expenditure for this Board is about three and a half million dollars a year. But some of these things look ridiculous if you look back over them. One year's expenditure on Tropical Disease Research here is $540. Big contracts of $20,000 with Chicago, or New York University, or Columbia were eye catching and startling to the people who dealt with them, but they're small now. I think we were what I used to call--and do in present talks with directors of commissions--

thousand dollar agonizers in these days, and now they're million dollar agonizers.

You initially had this Commission on Epidemiological Survey, and I have that contract here--I think I do--yes. The complication was a contract directly to Yale with the power to subcontract to other institutions.

That wasn't--what's the date of that?

Let's see--1942.

This didn't start that way exactly. It practically did though. The Commission on Epidemiological Survey was, as I think I have explained, primarily designed to keep watch over the bacterial flora in the throats and noses of soldiers because it's useful. If you found in a camp when you started that ten percent of the soldiers were carriers of meningococcus, and the next week there would be thirty percent, you'd get alarmed. That would mean that there are that many more hundreds of people carrying the organism. That was what we were looking for.

I now remember what you're driving at because this is early 1942. The plan was approved by finance officers as well as by the Board management that this commission--or it might have been any commission--could be used as a sort of a banker for others that needed money and didn't have it in their budgets, or had overspent; as a matter of fact, after I went out as director of that commission in 1942, it was taken over by Dr. Blake, and under his direction bacteriological survey of throat and nose cultures practically dropped out, and this became a commission which could finance unexpected things that were occurring in the fields of other commissions. It never had a very large budget.

No. Here in the four areas--1st Corps, 4th Corps, and 9th Corps and one other--oh, Puerto Rico.

Yes--Puerto Rico.

The total for this, and this is for all purposes, is $31,000.

In 1942? Actualy expenditure was $13,000. I'm not sure that the amount you gave there as being the amount allocated for this commission isn't too large. Is that a final contract form that you have?

I don't know.

Yes, this is an approved contract. That's about the way it ran, but the expenditures were much less than that. It ran about $18,000--I have it in here. Sometimes it was more. It got up in 1944, to $76,000, and in 1945, that contract was $136,000 with expenditures of $127,000, but it did a lot of interesting things.

I'll say it did.

It was very useful when the outbreak of post vaccinal jaundice came on--hepatitis. There was much extra work to be done then.

You've indicated that there was cross reference among all the commissions so that when something did happen, you could draw upon three, four, or five commissions for the particular problem.

As a matter of fact, we drew on the capacities of the men who were attached to these commissions. Of course they were listed under the commission heads.

It was a convenient rubric in the early days, but when you got functioning, they lent themselves to a wider use. Besides, you couldn't anticipate what was going to walk over the horizon, and when something did, you had the wherewithall to move. The original design was field work and supporting interim work in the home laboratory. There are two things here--was there any relation with the Army Medical School in Washington and were there any relationships at all with the Army Corps Area Laboratories?

Yes. The Army Medical School in Washington had in the past a Division, or a Department of Preventive Medicine of which General Simmons was the chief. It also had a large laboratory section of which General Simmons had been the chief shortly before he came back to the 1st Service Command. There had been a long standing relation between the Surgeon General's Office, of course, and the Army Medical School, and it got a little closer in the development of measures of preventive medicine and materials. A noted example of that is the typhoid vaccine. In the years going back to before Colonel Siler's time to General Russell's time, 1904, or thereabouts when he introduced typhoid vaccine in to the Army, the Army Medical School made all the typhoid vaccine that the Army used. It wasn't put out for commercial production, and they were continuing to make it during the beginning of World War II, during the period we're discussing. An example of how they worked with Preventive Medicine on this is found in the composition of the typhoid vaccine. It used to be called triple typhoid vaccine in the earlier days because it was composed of typhoid bacillus, paratyphoid A and paratyphoid B. They decided that paratyphoid A was not sufficiently prevalent to justify its addition to the vaccine, and that it ought to be eliminated to reduce the reactions. Typhoid gives you quite a red spot on the arm where it's injected, a little fever and malaise for a couple of days. If you could remove some component that had the capacity of causing these symptoms, you could reduce the discomforts of the vaccination, so at the beginning of the war paratyphoid A was not in the vaccine, but the National

Research Council in answering a question from Preventive Medicine dealing with the literature thought that paratyphoid A would be met sufficiently often in the regions to which troops were going to be sent to justify its return to the vaccine, so triple typhoid vaccine was adopted almost--well, in 1941 anyhow. I've got it downstairs in a book, but that is one example of the relation of Preventive Medicine to the Army Medical School.

The other thing--the Board is not so much concerned in that. This is mostly Preventive Medicine to the Army Medical School. Your question was what the Board had to do with the medical school.

Yes. There were in existence Corps Area Laboratories.

Well, General Simmons fostered laboratories all the time.

Are we talking about the Board, or are we talking about Preventive Medicine?

We're talking about the relationship that the Board might have had with an existing medical school in the Army and the existing Corps Area Laboratories.

They didn't have any official connection with them, but they worked with them and were aided by the facilities there. When the Board investigators would go out to a region, they often used the Corps Area Laboratory as a base. They would use the Army Medical School as a place for scientific communion, for exchange of ideas, and for certain materials. There WERE very fine people at the Army Medical School who could give them advice on the problems they were meeting in the field, but they had no organic connection.

I asked this because there is a phrase in the contract with respect to supplies where you could draw whatever possible from medical supply which would imply installations, or laboratories.

The commissions when they went out?

<u>Yes, and yet anything else that you couldn't obtain through existing supplies, you could buy directly.</u>

Yes, that was put in, I imagine--I don't remember how it was. That's an actual thing for government purchasing agents to require that they use the stocks they have. The people who handle supplies in the military don't like nonstandard items. They don't like purchases outside when the material can be drawn from stock piles or supplies. This is quite reasonable, of course, because standardization is very important in the Army to reduce multiple types of the same thing. Take the standardization of scissors. There's an endless amount of different scissors that surgeons want. The same thing is true about drugs, but the great standardization process is going on all the time. The Board wouldn't care particularly about standardization. These men on the commission were used to buying drugs and things from their local drug stores, or pharmaceutical houses. In addition, the government--buying in such huge quantities as it has had to--even before the war could get these materials and drugs much cheaper than the commissions could buy them.

<u>But to have an item in a contract which enabled civilians in the field to draw supplies from existing medical supplies, I think, is a novel wrinkle--civilians they were.</u>

Yes, they were civilians with a kind of a cloak on. They didn't have to account for this material. They drew very large amounts of it in some places, as in this Respiratory Diseases Commission Laboratory in Fort Bragg. I can't recall any difficulties. These people were sensible people. Nobody overdid. I never had any indication of graft, or black market work, or anything of that type.

<u>This was set up to enable them to function. It had that in view.</u>

Yes. Those laboratories were very helpful. For instance, the 4th Corps Area Laboratory under a very able commander was a beautiful place. They developed a mobile laboratory--big trucks, which our commission people used. It wasn't very satisfactory, but they'd take this truck out of there, and it was fully equipped with media, stains, chemical reagents, standardized reagents--all right there at the hands of the commission.

The commission you speak of at Claiborne started in a relatively informal way. A peculiar pneumonia broke out down there in 1941, I think, and the people who went there first to look at it were Dr. A. R. Dochez, Dr. C. M. MacLeod, and Dr. Yale Kneeland. They came back and said that they had found a peculiar pneumonia which they called primary atypical pneumonia. It's now proved to be due to a virus-like organism that was discovered by Monroe Eaton. He discovered it after he'd gone out of my laboratory in New Haven. A subsequent investigative team, composed of Dr. John H. Dingle, Dr. Barry Wood, and Dr. Gerrit J. Buddingh didn't need a great deal of material. They had unlimited facilities for x-rays. Their studies were largely x-ray studies. This peculiar infiltration in the lungs had a characteristic x-ray appearance, and its distribution is hard to recognize by physical diagnostic means, but the supply of x-ray films and x-ray service was like what you've been talking about. There was no charge against the group of investigators, or the commission they represented.

<u>Out of that experience the Board is confronted with the establishment of a continuing laboratory....</u>

For respiratory diseases.

Which was not anticipated in the original program. There's no particular reason why it should have been. What were the considerations that weighed in Dingle's mind. He's the one who presented this as a possibility.

Well, what weighs in the mind of the Commission on Respiratory Diseases is before them in the title of the board--"influenza and other epidemic diseases." Respiratory diseases in new recruits are predominant, the most prevalent of all the conditions they meet. The dangerous epidemic diseases in recruits are well known as respiratory diseases, including influenza and now we know a lot of other virus diseases. The pneumonias of several types--pneumococcus pneumonia, streptococcus pneumonia, bronchial pneumonia, lobar pneumonia--the pneumonias belong in the respiratory disease group, and the third main condition they meet is the gastrointestinal infections. Typhoid fever was no longer any problem, or no special problem to the Army and the soldiers of the Second World War, but diarrhea and dysenteries were and still are, and I can say right now that we're going through very difficult times with respiratory diseases in the Army in the United States with the increasing recruitments.

Dingle was interested in respiratory diseases. He primarily was interested in Boston in meningococcus infections, but he saw the scope of the problem and the scope fitted his sense of capacity to devise and design and carry out faithfully and constantly and over a long period the most difficult clinical and scientific investigations that existed almost. He couldn't do that by periodic appearance at a camp. He had to be in a place where he had a big laboratory, a large staff of competent people, a hospital in which there were apt to be many cases of repiratory disease. You had to be in a place where they occur, and in the Army you don't send from hospital to hospital very much the people who have sore throats, or upper nasal pharyngitis, or pneumonias--

they stay at the hospital on the post. He could have chosen Fort Dix, if he had wanted to, because Fort Dix had plenty of that kind of trouble. Fort Bragg was chosen partly because it was a very large post. I think Bragg had a strength of eighty thousand, or more some times. It had a big hospital. It had a very advanced staff of clinical chiefs who had come into the Army out of private practice, or a university practice like Dr. Worth B. Daniels, a leading physician of Washington, and a number of other men who were glad to cooperate with a group of investigators who would come in.

In addition, the Medical Chief at Fort Bragg was then Brigadier General H. C. Coburn Jr. He had set up one of the best medical establishments in the Army, equal in many respects to anything I think we had seen in civilian life. He had a large medical hospital there. He had a large medical dispensary, and he even went extensively into the prenatal care of babies expected to come to the dependents around the camp because there were many people in Fort Bragg. He had a very intelligent venereal disease program. He had a commanding general Major General D. C. Cubbison, who was in command of the Field Artillery Replacement Training Center. The acronym of that doesn't sound too well, if you put FARTC--so we were careful.

I guess you were.

Field Artillery Replacement Training Center--C instead of S at the end.

Major General D. C. Cubbison was receiving thousands of recruits. Bragg was a Field Artillery Center. It was an unrivaled opportunity for Dingle to get at what he wanted of the appearance of respiratory diseases in men just brought into the Army. He wasn't depending on the hospital until these men got sick, of course. There was no other similar place that had so many incoming susceptibles that were sure to come down with something, and Dingle is a very remarkable investigator, did superb work down there, and in addition we were able to supplement his subjects with hundreds of conscientious objectors. Dingle's laboratory was extended by a hotel, a near-by hotel that we took over under contract--we had money for it--in which were housed groups of conscientious objectors who allowed themselves to be sprayed with material that was thought to contain the viruses of these infections.

Dingle produced experimentally atypical pneumonia for the first time. He produced other things. One extraordinary thing about Dr. Dingle was that he saved in frozen state samples of practically everything he got from patients, or was using from patients. He had thousands of specimens frozen, specimens obtained in the period from 1942 until 1945. He then went to Cleveland as the Professor of Epidemiology and started a study of respiratory diseases occurring in the families in Cleveland. He was at Western Reserve, but about that time there began to appear at Fort Dix and elsewhere people suffering with what he called undifferentiated upper respiratory tract infection. He'd never been able to find the cause down at Bragg. Coincidentally, though, in the latter period Robert J. Huebner at the National Institute of Health and Maurice R. Hilleman at the Army Medical School had discovered several new respiratory tract viruses, adenoviruses, so using those new viruses Dingle could go back into his collection of frozen sera and do complement fixation tests. Ten years or more after the war he began to work out the pattern of undisclosed respiratory diseases that he couldn't do any other time because he had saved the specimens and these new viruses had been discovered. That has been one of the fine examples of how military findings can go over into civilian practice because the importance of this was seen, and it was possible to make proper vaccines for some of these special virus infections which was beneficial for the troops as well as civilians. The trouble is that there are so many of these special virus infections. So many varieties are occurring all the time that it's difficult to keep up.

The creation of this laboratory raised, again, a financial question.

It raised an administrative question because there was opposition to it, and it was difficult to get exactly the authority that was wanted. Among these papers is a letter signed by General Magee under the line by command of Major General Brehon Somervell. Major General Brehon Somervell was the commanding general of the Army Service Forces which by the reorganization of the War Department in March 9, 1942, was the new controlling element over the Surgeon General. The Surgeon General by this reorganization lost his staff position. He had a whole layer of command put between him and the General Staff, and he was relegated to a very inferior and uncomfortable position. He couldn't do anything really without the approval of General Somervell, but it was possible in ways that I don't really recall entirely now, for Colonel Paul Robinson, head of supply at this moment in the Surgeon General's Office, in consultation with some people in the Army Service Forces to devise a letter--what is it? Here it is. A letter on August 12, 1942, addressed to the Commanding General of the 4th Service Command, and it was then S. O. S.--Service of Supply; it wasn't the Army Service Forces yet. It became Army Service Forces in a very short time, but this reorganization had put the Surgeon General under S. O. S., under General Somervell's jurisdiction, so this letter was addressed to the Commanding General, 4th Service Command, S. O. S., Atlanta, Georgia, for the attention of the Surgeon. The Surgeon was Colonel French. Colonel Sanford W. French was not particularly involved in the implementation of the requirements in this letter. He merely passed it on to General Coburn at Fort Bragg because the letter asked for the establishment--well, really directed the establishment of this respiratory disease laboratory in Fort Bragg. Somehow or other it was possible for me, General Simmons, and Paul Robinson to get the approval of the Surgeon General to sign this letter and the substantiating signature also of Colonel John Rogers who was the Surgeon General's Medical Department Executive Officer practically telling the 4th Service Command Medical Establishment to set up this laboratory, give a building for it, or put aside a building for it, enlarge some quarters that the commission laboratories already occupied, provide cooperation, provide certain supplies, transportation, security, assignment of additional personnel and technical assistants, and really put Dr. Dingle and the Respiratory Disease Laboratory in business.

When General H. C. Coburn got this letter he thought, I suppose, it was somebody's dream that had little substance, but after a week or so, I remember he drove hurriedly up to Washington and came in to see me--I don't think General Simmons was there at the moment--and what he said was, "My God, I just realized that this was a command letter."

He took it as a command letter, and so did everybody else. From that moment on this laboratory did superb work.

It brings in by way of support--the Army finance group were what they were, but there were efforts, and successful efforts, to accumulate a fund, establish a fund to sustain this laboratory from outside government sources--foundations.

Not much. It's to be noticed too, that this was August, 1942--the war was on. We were deep in the war then. Patriotic feeling was high, and you could do things in this period that you couldn't possibly do in slack time, though Simmons did interest the Markle Foundation and the Commonwealth Fund too, I think. He even tried to interest the Rockefeller Foundation to supplement Army support with grants, but that never really amounted to much. He had a much larger project to set up tropical education with a grant from the Markle Foundation along the Central American Highway in Nicaragua on through Panama.

Initially I think the problem arose as to how to accept whatever grants were going to be made, and they set up a separate fund, I believe, in New Haven.

I think we found out that you couldn't make grants to the Army.

Yes.

Yes, so they set this up under the Dean at New Haven.

The President of this Board.

Yes. I have one of these folders down here--Foundation Grants--where the policies and all are discussed. I put that aside because I knew that was an interesting development.

Certainly novel for this time. Here it is, but there's nothing in the folder. I must have pulled that out.

Well, it was just a mechanism to....

Here it is.

It was just a mechanism to make money available. What you've just shown me is a handwritten copy of mine of a memorandum to General Magee from me June 22, 1942, in which I told of the interest of the Commonwealth Fund, the

Rockefeller Foundation, and the Markle Foundation in making grants for the support of research on acute respiratory diseases, the grants to be administered under the Army Epidemiological Board. I said in this memorandum that the amount may be as much as a hundred and fifty thousand to two hundred thousand dollars, that after consultation, the best way to proceed would be to permit the foundations to make the grants to Dr. Blake for use of the Commission on Acute Respiratory Diseases under the supervision of the Board, and that Dr. Blake is ready to accept the responsibility. He did so. This was approved by Brigadier General Larry B. McAfee who was Acting Surgeon General at the time. He got the memorandum on June 22nd, and he approved it on June 23rd, and the same day I wrote Dr. Blake to go ahead. There are all sorts of background notes on how to set this up. This is all right. Dr. Blake was the agent.

<u>I think you pointed out the scene had shifted by 1942, and it was possible to do a lot of things....</u>

This is June, 1942 here.

<u>Yes which you could not have done in January of 1940.</u>

I'll give you an example of very modern times. The Armed Forces Epidemiological Board has just celebrated its 25th anniversary. The total cost of this ceremony and celebration is probably more than two thousand dollars--I don't know how much it is. The payment of that sum partly comes from available appropriations for travel, or whatnot in the Medical Department, but most of it has come from outside. The way that's been handled for this function of the Board was to set up a bank account in Boston in the name of the President of the Board at present, Dr. Gustav Dammin, and he's paying the bills out of this money set up for the Board. I don't suppose the government would care very much where an account was set up to help one of its agencies as long as it didn't have to administer the fund and be bondu by any policies of the fund.

<u>The only distinction I'd make is that there is a differense between a ceremony and a full scale laboratory investigation by Dingle. Out of this comes the need for equipment too, which is a complicating thing because they are novel things that are needed--not nuts and bolts that the War Department is used to and this foundation money may have facilitated that. Having this fund available may have facilitated the reach for machinery.</u>

Sure--quicker and with greater freedom. I've seen some requisitions for equipment batted around and batted around. One man will put an endorsement on it saying use this, and another one saying use that.

<u>Also, the foundations must have been aware of the nature of the program, the validity of it, and the possible general use of the findings.</u>

Yes, well each one of these discussions there with the foundations are not with the Board of Directors of the foundation, but with the scientific directors. The Discussion with the Markle Foundation were with F. S. Russell, the Commonwealth Fund with Lester Evans and the Rockefeller Foundation with Wilbur Sawyer.

<u>You were talking a common language.</u>

Yes.

<u>And it was a matter of expediting matters. Dingle was a--I don't know. I get</u>

this from some of the correspondence, a real impatient fellow to get started.

Dingle was always impatient. He wanted the most extraordinary amount of everything all the time. He'd come see me about once a month with enormous requests. Dingle was a man of imagination. He wasn't selfish in any of this.

I wasn't implying that, but he was champing at the bit, and this enabled him to at least get started. Well, we've gone longer than an hour. I guess we'd better stop. We can come back to this tomorrow.

We haven't finished this financing because I think you ought to dictate yourself in there something about Robert Taft coming to our help.

We can come back to that tomorrow. All right?

Leave that material somewhere.

Wednesday, May 25, 1966 A-60, N.L.M.

Several days ago you asked me my recollections of the work and significance of the Committee on Scientific Personnel of the Division of Medical Sciences of the National Research Council in 1940. I was not very clear in my recollections when I spoke about it to the microphone, but since then I have reviewed some material that I am in the process of using for the writing of the first volume of the History of Preventive Medicine in World War II.

In the first World War there was no system of classification of scientific and medical personnel. They didn't know what a man's qualifications were. They didn't know how to assign them. They had all sorts of misfits. They didn't know how to transfer people from one job to another because they hadn't this information on qualifications. That applied to many surgeons, many physicians, and many people in lower grades in technical services. The outstanding men like Dr. Zinsser, Dr. Siler, and people who rose to be Sanitary Inspectors of Armies, or Corps were easily placed because they were well known--like Haven Emerson who was great in public health. They knew where to place him, but there were many, many other men whom General Merritte W. Ireland as the Chief Surgeon of the A. E. F. in 1917-1918, couldn't very well place because he didn't have any systematic measurement of their training, their capacities, and their experiences.

That was the reason why with the sense of the possible oncoming war in 1940, the National Research Council set up a Committee on Scientific Personnel with a view to devising systems of classification that would be of value to the war effort, not just the Surgeon General. It was for the War Manpower Boards that were forming about that time.

You asked me also a day or so ago what the relation of the National Research

Council was to the Office of the Surgeon General in technical and policy matters, and you seemed to think that the liaison and consultation between the Office of the Surgeon General and the Division of Medical Sciences of the National Research Council and the consultation that the War Department, the Office of the Secretary, had with the NRC was some possible indication that the War Department and its agencies were going outside the War Department to get influential advice. Well, as I explained, that was very natural because the National Research Council had been set up in 1919, by the National Academy of Sciences in response to a suggestion in an Executive Order of President Wilson as an agency to render advisory service in the fields of science and medicine to the War Department and other agencies.

Well, in 1940, with the immanent appearance of involvement in the European war, the National Research Council began to study the classification of medically trained people, people in the field of medicine and public health, and that is acknowledged very clearly in the publication about medical personnel in World War II where it states plainly that this classification system suggested by the National Research Council for the designation of the proficiency of men and their specialties was the first one adopted by the Medical Department. They adopted the NRC system of specialty classification and the rating of officers, and that was the basis of the much more extended study by the Committee on the National Roster of Scientific and Specialized Personnel which was under the chairmanship of Leonard Carmichael working chiefly under the War Manpower Commission, I think it was called. That took in an enormous amount of area for study and classification.

out in his report, it wasn't a Civil Service object that they had in mind. It was a complete classification of the talent in science and medicine in the country. He cites examples; that they actually supplied in answer to requests the names of two men whose income was over two hundred thousand dollars a year. They weren't seeking a job, but they had high qualifications for scientific work. The same thing applied to the classification of the physicians in the country. The Surgeon General's classification was coupled with the American Medical Association's attempt at classification, and the Surgeon General's interest, of course, was the classification of the specialties and abilities of people who were in the Army and people who were in the Medical Reserve Corps.

I mention this in so much detail to make up for what I left out the last time and to indicate two things. One is that in 1940, the first definite, invaluable classification of specialties and competence in the broad medical field came about. This was rather slowly utilized and developed in the Office of the Surgeon General. As a matter of fact, it didn't come into exactly the orderly manner that it needed until October, 1943, when a Training Manual of the War Department published in October, 1943, carried over the classifications that had been worked out jointly by the NRC and the Surgeon General's Office and had in addition a code attached to it, a system of numbers--a four digit number with a letter on it. The letters A. B. C. and D. indicated grades of competence. The numerals indicated specialty and sub-specialty experience. These were known thereafter as MOS--meaning Military Occupational Specialty. I think it's a good example of the cooperation of the military establishment, both general and medical, with a great civilian agency like the National Research Council.

<u>I think I may have misconveyed in a sense. One can look at the book published by James Phinney Baxter and get a wholly wrong impression as to what cooperation</u>

<u>was.</u>

Baxter had nothing to do with this. He was OSRD.

<u>Yes, but in writing that up, he so concentrates on that agency that one can gain an impression that somehow or other there were walls.</u>

My impression of the Baxter book is that it is biased, and that the two volumes--you know that there are two volumes--are inadequate. I don't think that President Baxter knew what I'm talking about.

<u>Yes, and this begins in 1940.</u>

Long before his time. There's the documentation of it in some of these books. I want to go and get that.

<u>Yesterday I was asking about the relationship between the Board and the Army Medical School and the various Corps Area Laboratories, and you had responded. Then it occurred to you to go on into another relationship about which I didn't know and hadn't asked you--the relationship between the Preventive Medicine Service as it developed and the Quartermaster Corps.</u>

Well, the relationship between the Medical Department and the Quartermaster goes back at least to 1775, when these two offices were more or less started because the Quartermaster was responsible always for subsistence, clothing, shoes, tentage, shelter, and many of the physical things that a soldier had to use to care for his health. In addition, the Quartermaster has always been responsible for the provision of rations. He has so much concern for the preservation of the health of troops that it is impossible to draw a line between medical preventive medicine and Quartermaster preventive medicine, and that has been over the years, since the Republic started, a source of both discord and cooperation because the Surgeon General, the head of the Medical Department, has the opinion that anything that affects the human individual, anything done in the Army affecting the physiology of the human individual, is in the field of medicine and that the Surgeon General ought to have the main say. The Quartermaster, on the other hand, has a broader point of view, saying that these are physiological functions that don't have to be studied and managed by medical people.

I personally had this conflict before me in the Preventive Medicine Service and even in parts of the work of the Army Epidemiological Board, but having been at medical schools where the Professor of Physiological Chemistry was a Ph D doctor and not necessarily medically trained, I could well understand that you didn't have to have an MD to be able to be competent and very valuable in the field of health and medical care. The point I wanted to bring out in mentioning the Quartermaster was the relationship specified in 1940, when General Simmons came into Preventive Medicine, and succeeded in May of 1940, in having Preventive Medicine set up as a separate subdivision within the Division of Professional Services. Preventive Medicine functions had been diffused through that Professional Service Division in a pretty nearly unrecognizable fashion. When General Simmons got the Pieces together and drafted the statement of functions that the Surgeon General approved and issued in one of his office orders, he included in the statement of functions that the sub-division of Preventive Medicine would have liaison with the Quartermaster Department regarding food supplies, water supplies, waste disposal, insect control, housing sites, sanitary appliances and bathing pools, swimming pools. In other words, the first charter of the sub-division of Preventive Medicine reiterated functions that had been performed over the past century or more, and they were very specifically stated here as a main function and obligation of this new sub-division.

It was quite natural that this liaison should occur, and it did occur with great profit to both sides. It was harmoniously developed by General Simmons who was a friend of the colonel who became a general, Brigadier General Georges F. Doriot, in the Research and Development Division of the Quartermaster Corps at their experimental location at Lawrence, Massachusetts, and later at Natick, Massachusetts—just outside Boston. At that time the Quartermaster and his Research and Development people had established fine working relationships with the Fatigue Laboratory at Harvard and with the nutrition departments in various parts of Harvard University Medical School. They had wide interest in fabrics for clothing. They began to get interested more and more in climatology, the acclimatization of soldiers, the study of the responses of soldiers to heat and cold. They had a continuing interest in the footwear, the footgear of soldiers, as the Surgeon General had also, dating back particularly to the time of General Munson who developed the Munson last and made an Army shoe that made it possible for the soldier to walk fifteen miles one day and walk fifteen miles another day. Up to that time they had shoes that cramped their feet, deformed their feet, caused blisters, and the Munson study at Fort Leavenworth in the 1880s, or 1890s, revolutionized ideas about the footgear of soldiers.

The Quartermaster was doing somewhat the same thing as was being done in Preventive Medicine. Parallel with the development of, we'll say, sanitary engineering in the Preventive Medicine Division of the Surgeon General's Office, there was also sanitary engineering in the Quartermaster Department. Interests were common. The lines of endeavor were parallel, but not coincident all the time. What I'm trying to say is that there was no real duplication of effort, although we were working in the same field and the same words were used. I think that says about all I wanted to bring out in that connection.

The Chief of Medical History has had Quartermaster authors for two or three volumes on the Quartermaster Corps in World War II which I have seen but do not have before me now. They emphasize what I've said; that the Quartermaster Corps in World War II, as before, had the principal business of providing for housing, food, clothes, personal equipment, fuels for camps, laundries, baths and other services, and many, many things could have been considered to be in the field of public health and preventive medicine.

<u>Quite early there was a study of the amount of space--overcrowding of men in barracks.</u>

That was done by the Preventive Medicine Service which called upon the Army Epidemiological Board. In 1942, I think, we were asked by the Surgeon General to make a survey of the conditions in barracks with special relation to the amount of floor space, or the amount of cubic feet space allotted to a soldier in barracks. What was done illustrates the value of the Army Epidemiological Board to the Surgeon General in making available to him expert consultants in the field, men very broadly informed and men of eminence. It indicates also that the Board could undertake a study of this kind without being restricted to the use of a named commission. A group was appointed headed by Dr. Perry Pepper who was a great physician from the University of Pennsylvania, from the very distinguished Pepper family, a man whose speech had been influenced by his patronym. He was very witty and peppery. He wrote a book on medical etymology which is very witty. Well, this group of people assembled around Dr. Pepper and formed a sort of Commission on the Housing of Troops.

At that time the Surgeon General's protest at actions taken by the War Department, which resulted in the crowding of men into barracks, had been futile. They were futile because General Marshall was not at all convinced that it was necessary to provide more space, considering the then great shortage of building material and the shortage of labor. Floor space---I forget the required amount. I think it was around fifty square feet per man. I forget what the regulations were, but they were avoided by the billeting people, and as a result soldiers were actually crowded into about half the space that Army Regulations said they should have per man. This Commission on the Housing of Troops went all around the country, visited a number of posts, and made observations and measurements---observations on how the men were living regarding the space available in the barracks. They made a report and strongly urged the Surgeon General to forward this report to the War Department for the consideration of the General Staff, asking that steps be taken to provide more square footage per man in barracks.

General Marshall was interested in that, but he disapproved of the recommendation, and it turned out that the General was quite right. We never had an epidemic in World War II of any serious consequences, and the crowding continued right on through the war, but consider the crowding on troopships like the Queen Elizabeth, or one of those boats which carried fifteen thousand men on one trip.

<u>Four bunks high.</u>

The bunks were never empty. They slept a group for eight hours and then let them go somewhere else and had another group come in and sleep. They had three shifts in these bunks per day--beyond belief. They didn't have any severe outbreaks of disease on these ships. Most of the outbreaks occurred in transports and places that had poor latrines, poor "heads" as the Navy called them. They had much diarrhea and dysentery attributed to fecal contamination in these overcrowded and dirty latrines, especially on transports. I knew something about this because with the aid of some people from the Board--I forget, but perhaps the Commission on Cross Infections, Airborne Infections, we made a study of the air systems in the Queen Mary and the Queen Elizabeth, as examples, at the Port of New York. This Commission on Airborne Infections had discovered that spraying propylene glycol vapor in the air would sterilize it. We tried to prevent respiratory disease in barracks by spraying glycol vapors in the air, and they seemed to be effective in some cases. The idea was to see whether we could do this on these big transports like the Queen Mary. The one or two visits I made to the Brooklyn Yards where these ships were docked, I found out, for example, that the Queen Mary had seventeen separate air ventilating systems in that huge boat, and that the air was changed every few hours. Well, I don't remember how much. It was calculated that even to get the small concentration of glycol in the air that was needed, you'd have to fill half the ship with barrels of glycol in order to have some to spray because as soon as you put the spray in one of these places, it would be sucked out and blown away. In addition, the vaporizing machinery and the added plumbing that would go with it to disperse the spray would take up another lot of room, so we didn't do it.

But General Marshall--well, I don't want to put this in here/. What's that?

That's by order of General Marshall. What I was going to say is this problem of space in barracks and in hospitals is complicated by the double bunking. This was studied by Dingle and the Airborne Infectuon.

Well, it isn't only the ordinary respiratory infections that are concerned in this, but also the airborne meningococcal infections. Epidemics of meningitis occurred rather disastrously in World War I. A group of English investigators, who saw they couldn't do anything about getting more space for men in crowded barracks in England, developed a system of sleeping head to feet, so that a man wouldn't cough right into the face of the other man, but you could watch them and see a man sleeping with his head to the North and another man with his head to the South, and if the man with his head to the North starts to sneeze and cough, he involuntary rises to a sitting position and is over the man next to him and coughing right toward him. He's not coughing up into the air. It didn't work very well.

As far as double bunking, our commissions as well as the sanitary officers of Preventive Medicine were of two minds. It looked as if double bunking would add to the problems caused by crowding because a double bunk was put in place where one bunk was before, but as a matter of fact, my impression and the impression of some of the commission members was that the double bunking in barracks was protective. It separated the men, and it really made it possible to take a space allocated for two men and occupy it by what you might call a Siamese Twin-type of arrangement, where the two men became one in the double bunk, sleeping one over the other. They had actually more space around them than if they had separate beds.

The whole problem is complicated by other things in camps for troops--do you want all this stuff?

Yes.

Consider what happens in the wash rooms and latrines. You have fifty men in a barracks, or fifty men on a floor, we'll say, and they all have to go to the bathroom--to use the polite expression now--about the same time in the morning. They're all coughing and spitting at the same time. They're cleaning their teeth at the same time. They're washing themselves at the same time, so that these "facilities", as they're called, became occupied by people shoulder to shoulder, back to back, all sorts of mingling, so that if you had plenty of separation and space in the sleeping part of the barracks, it's all crowded out by the ablutions.

The other place where crowding and infections occur is in mess halls. The soldiers sit crowded side by side in mess halls, and in addition, if you follow down a mess line, which usually has a rail on it, and make cultures off that rail, you can get streptococci, meningicocci because the soldiers going down the rail, sneeze, put their hand over their mouths while they sneeze and cough, and then put their hand on the rail, and the next soldier comes and puts his hand on some wet mucous that has organisms in it.

The other crowded region that gave us really definite indication of respiratory infection transmitted through the air and dangerous in a crowded place were the Army Air Force training schools--like the Patterson Air Force Base. These men had plenty of sleeping room quarters, but they were always having epidemics of lobar pneumonia out there. They had high carrier rates. Dr. MacLeod and some others studied them and traced it down to the groups that were taken in to small rooms for lectures and movies for training, so that it isn't as simple as it seems by just saying that they must have a certain amount of space in barracks.

As a matter of fact, you'd think that putting men in tents would be a healthier thing for them, but it's not always so, because conditions of chilling and wetness and even ventilation in tents is not so good sometimes. General Marshall was interested in these things, and as you said this one here--this

AR on meningococcus prevention, as I told you, was signed by the Chief of Staff by order of the Secretary of War. It comes out from the Surgeon General's Office, and this particular one dealing with meningococcus meningitis is the experience and thought of members of the Commission on Meningococcal Infections of the Army Epidemiological Board.

<u>Do you want to tell me something with this off?</u>

Yes.

<u>You mentioned the Air Force. The Commission on Epidemiological Survey, out in the 9th Corps Area had not a little contact with the Air Force. What was the relationship between the Surgeon General and the Air Force?</u>

That would take a book to answer that.

<u>It doesn't figure.</u>

Are you talking about the Board?

<u>The Board went where it was called.</u>

Yes, but it didn't have anything to do with the relations. The Board didn't take up the problem of the relations between the Air Force and the Surgeon General.

<u>Was its functioning shaped in any way by the relations?</u>

Yes, the--this is incomprehensable unless you go into the relations between the Air Force Medical Department and the Medical Department of the Surgeon General.

<u>I guess that's important then.</u>

Well, I'll do it briefly. From my personal experience, when I went down to the Surgeon General's Office in 1942, I found the Army Air Force Surgeon with his group in the office on 1818 H Street being an entirely separate medical service and with very hard words said about the Surgeon General. This was a colonel I got to know named Grant who became Major General David N. W. Grant, the Air Surgeon. I thought that there was an internecine war between the medical departments of the Air Force and the Surgeon General. It's a fact that the Surgeon General, General Reynolds, before General Magee, wanted to consOlidate under one direction all the medical activities of the Army. The medical activities of the Air Forces were part of the Army because it was called the Army Air Forces and was under a division of the War Department General Staff. I soon learned and soon got a sympathetic outlook on this division of interest and jurisdictions. I got a rather sympathetic feeling for the Air Forces when I knew more about the history because even in World War I there had been an effort for the Air Force to get autonomy, and for the Air Forces to get autonomy was a continuous thing from World War I until it actually got its autonomy in 1942, but in 1941 and 1942, it was struggling very acutely to be an autonomous arm of the military establishment. It practically had it in practice, but not in name. General Henry H. "Hap" Arnold, I think it was, first Commanding General of the Army Air Forces, had direct access to the Army Chief of Staff. General Marshall made a number of steps to reconcile the differences and give the Air Forces as much autonomy as possible, and it gradually worked to that. What we saw down in the lower level of the Medical Department were the chips coming from this battle for autonomy.

General Arnold had an unfortunate experience that pushed him to make a report and inquiry that brought this to a head in 1942. He was coming back from the Philippines, and his pilot was medically under the control of the flight medical officers who were under the Surgeon General, and one of these flight medical officers, flight surgeons as they were called, in Honolulu disqualified General Arnold's pilot for some minor medical reason. It made General Arnold so mad that he started a vendetta. Of course, now when you look at that experience, it is just superficial stuff because the great, deep thing was the struggle of the Air Force to become an autonomous service in the military establishment, and it was not long before the Army Air Forces became the strongest arm in the whole organization. The Air Force strength rose to above two million. It was bigger than the Army ground forces, far bigger than Artillery. It was a very important and justifiably an individualistic service.

The cry of the Air Force officers, both medical and line, was that the problems of the selection, training, management, care, and service of an airman was entirely different from a foot soldier, that they had to have, therefore, their own medical establishment to care for these men in a special way--psychologically, physically, and so forth. They found the Surgeon General wasn't competent and didn't seem to be interested very much, although Surgeon General Magee and other people just kept on to keep the Air Force Medical Department under the Surgeon General. It extended very much further though, when the Air Force, General Grant and his associates, began to insist that they should have all their own hospitals, and they succeeded in getting their own hospitals. I have a memorandum of General Magee saying, in effect,"Look, we have an Armored Force, men in the tanks subject to noise, heat, and incineration in this tank when it gets hit. The wounds are very peculiar to the tank. The physiological and environmental conditions are peculiar. The Armored Force is not asking for special hospitals, or special medical service. Why should the flier have it?"

That, I think, is an argument with analogies that are not very suitable, but the Air Force did succeed in getting general hospitals, station hospitals--well, they actually succeeded in getting a separate medical service, including everything except certain supplies that were made available through the general medical supply. They also were very successful in recruiting civilian doctors that the Surgeon General wanted very much. They had some wonderful recruiting people like Colonel Russell Lee who is a friend of mine. They got the cream in many ways--very fine people. They needed, of course, a promotion system and a pay system that was different from the ground medical officers. The problems ought to have been seen in my opinion and adjusted by the Surgeon General to a comfortable arrangement without the blood letting that went on during these very severe arguments.

The upshot of this was that when the War Department and the Army was reorganized in March, 1942, three coordinated services, three branches of the military establishment were listed. One was the Army Ground Forces. Another was the Army Air Forces, and the third was S. O. S.--Services of Supply, which by August became Army Service Forces. The Army Air Force had a commanding general who soon became a member of the Joint Chiefs of Staff and soon became a member of the combined Chiefs of Staff, so that the commanding general of the Air Force went right on up to the top of the staff level. In these Joint Chiefs and Combined Chiefs of Staff positions, he was not even the servant of General Marshall. He was equal with General Marshall, so they had a *de facto* autonomy even in 1942. Another thing that helped them from the beginning of the war was the fact that the British and the French had separate, autonomous air forces, so that when Mr. Churchill and Mr. Roosevelt met on the ship and the Atlantic Charter was discussed in these early days, the British Air Force Marshalls were there on the same level as General Marshall himself, and our

Air Force people came in on very high policy making levels.

When the reorganization of the Army in 1942, was developed a little further between March and August, when A. S. F. was set up, the whole Medical Department was placed under Army Service Forces--S. O. S. at first. The Surgeon General then came under the Commanding General of the Army Service Forces. Before that time, he'd had a staff position that made it possible for him to speak directly to the Chief of Staff and to the Secretary of War. He lost all that. He had a whole command level placed between him and the War Department--I mean the General Staff of the War Department. It was a bitter and rather crushing time, full of extreme difficulties and led to more things than concern this Board, but I will say that Preventive Medicine was relatively little affected by these changes than other divisions of the Surgeon General's Office. There was an investigation that took place, and Preventive Medicine was complimented by the people who were attacking the Surgeon General's Office, and we had very good relations all the time between Preventive Medicine and its agent, the Board. For example, I happened to know the colonels in charge respectively of the Preventive Medicine Section of the Office of the Chief Surgeon of the Army Ground Forces and the same with the Chief Surgeon of the Army Air Forces--I knew their preventive medicine officers who were rather new to the game, so new that they were constantly needing advice. We used to have almost a daily telephone conference--at least I did--with these officers, talking about problems coming up in their services, so our Preventive Medicine Service helped them as much as possible. If I didn't know what to say, I could find out and tell them at another time.

There were problems in the Army Air Forces that fell right into the interests of a number of Commissions of the Board. The Army Air Forces before the war, before the establishment of the Board, was very much interested in coccidioidomycosis, a fungus infection of the lungs that occurred in the dusty air training centers in California. It was called "San Joaquin Valley Fever", but we had to change that because that was not good public relations. It didn't work well with the Senators and Representatives of California to name a disease after a region of their country. Did we talk about this last time?

I just pointed out the 9th Corps Area because the Air Force is involved.

Work had been started in California by General Charles R. Glenn who has remained a friend--Brigadier General Glenn in the Air Force. We soon linked them up with one of our commission on which there was an expert in coccidioidomycosis; namely, Dr. Charles E. Smith. They worked for a while with Preventive Medicine only with the Air Force medical officers in the dusty airfields in California, but after I came down here, we could take that problem into the Board and bring Dr. Smith into the picture. That continued throughout the war, and they did extraordinary and valuable things for the Air Force, for science in general, and for the relations between the Air Force and at least Preventive Medicine in the Surgeon General's Office.

The other fields were the fields in respiratory diseases, particularly streptococcal and meningococcal infections. Our commissions worked at Chanute Field, Wright Patterson, and in fields out on the West Coast, also in some of the fields in Maine and Massachusetts, as if the commissions were a part of the Air Force. The commission would be called in by them, and there was no particular red tape in getting in. You didn't have to use much red tape to tie up a bundle to go out to one of these places. We had commissions in Air Force installations in San Antonio, Fort Sam Houston, and many other places.

The relationship of that kind continued throughout the war with many problems in common, and they got very close with Preventive Medicine of the

Surgeon General's Office and the Board when it came to foreign quarantine. The air travel between, we'll say, Africa and South America, between the western coast and the Pacific Islands raised hundreds of problems that required investigation. We had a whole branch in Preventive Medicine called Foreign Quarantine that did wonderful work. Lt Colonel Philip T. Nies's work tied in with the interests of the Commission on Tropical Diseases of the Army Epidemiological Board. It was largely administrative. It involved liaison with the Public Health Service because they're responsible for quarantine. It involved relations with the Typhus Commission and a far reaching intellectual and scientific association that was fostered without much attention paid to the administrative differences of the people that were working in it.

I think that's its sunshine. No matter--let me say this--no matter what was going on between the top ranking people, it didn't effect the interests of the investigators in a given area whether it was Air Force, Ground Force, Field Artillery, Tanks, or what.

Yes. With the tanks, the Armored Corps, we had the most interesting relationship at Fort Knox, where General Simmons was largely instrumental in setting up the Armored Force Medical Research Laboratory. The personnel was largely selected by him, and the people in Preventive Medicine and some of our Board commission members worked down there on atabrine, fatigue, climate, exposure.

You're going to have a time straightening this out between Preventive Medicine and the Board. It doesn't matter because it was all part of the same thing.

We were talking in part yesterday about the problem of gaining finances, or support for the Board's work, and out of that problem came what the relationship was between the Preventive Medicine Division and NRC, and CMR of OSRD. You said that you wanted to make some mention of this as also a function of the question of finances, or appropriations.

What we're looking at here now is a letter to me from my friend, Senator Robert Taft, August 12, 1941, in which he says that he has written to General Magee regarding the Board for the Investigation and Control of Epidemics in the Army, asking whether its appropriations were included in the current appropriations bill, and offering to present an appropriation resolution as an amendment to the bill on the floor of the Senate. Bob Taft encloses for me a copy of the letter which he had received in reply and makes the comment,"I take it that the matter is not proceeding very quickly."

General Magee's reply which was probably written for him by someone in his office is an unenthusiastic, rather weak acceptance of the status quo and indicating that he didn't see that he could do very much about it in a hurry in getting funds for the Board. Well, now this is August, 1941, and the Board is much in need of funds. Commissions are already at work probably supported largely by universities. I cite this as evidence that one can go around the higher officers of a great establishment and bring to bear some personal interest. There's no concealment about it because General Magee knew that this was going on and probably was glad to have it go on.

The state of affairs discouraged Francis Blake because about the same time in 1941, he wrote to General Simmons that if the appropriation wasn't coming along, perhaps the whole thing ought to be dropped, and he adds to that,"It's too bad" which is kind of a strong expression for a New Englander, an inhibited man. We got the money.

Did I give you the amusing story of General Simmons and the budget officer?

No.

The budget officer of the War Department--the acronym of whose title was BOWD. General Simmons from North Carolina pronounced Board "Bowd", and he pronounced the acronym of the Budget Officer of the War Department as BOWD, so it was not always possible to know what the general was referring to. The BOWD--meaning at this time the Budget Officer of the War Department--was either suspicious, or too cautious about making these monies available. He said at one time that it looked to him as if the Surgeon General was trying to get authorization to employ at twenty dollars a day all the physicians in the United States. It was difficult to get them to understand--I can see now that it was difficult for them to understand something quite so new as War Department support of intangible research. That's what I think.

It wasn't a lack of communication because it was supplied with cogent statements such as General Simmons' and the Surgeon General's letter about the establishment of the Board, and that even went further with a mimeograph from General Magee to the Commanding Officers of the Corps Areas and to the War Department too, which excerpted a lot of that letter, so it took, I suppose, a process that kept us poor.

When you get to talking about the procurement and allottment and the holding of officers for the Board, this type of maneuver, where a civilian member connected with the Board, or even an officer connected with the Board was able to have a direct association and contact with the Secretary of War and one of the Assistant Chiefs of Staff, resulted in actions that overrode the Medical Department and the Surgeon General with benefit.

I think once the problem of financing was clear, the questions of contractual arrangements for something other than hardware were more easy. The contract itself I find a fascinating document in terms of the ability to function under it. In our talks we didn't get into the difficulties in contracting directly and subcontracting from a general contractor which was part of the setup, but which later was set aside, and individual contracts were made with each individual university where work was being done.

But they always could make subcontracts.

But the practice, I think, later winnowed out the process of subcontracting because you dealt directly with that university where work was in progress, but under the contracts, certainly in your own case, there emerged this whole question of conflict of interest, a very neat problem, and the manner in which it was handled by the legal representatives of the War Department is superb. There is a marvelous memorandum here as to the approach to take. Now, in your own case--from what was generally known, I suppose, or going on, and to which you may have had access--Vannevar Bush was concerned about conflict of interest. He had a different scale of operation in OSRD than you had. Dr. Blake raises the question, and you raised the question in your own case because of your connection with the Childs Fund and Yale, and you found conflicting answers within the War Department depending on how you presented the case and to what officer, whether it was the Adjutant General, or the Judge Advocate General, or the Surgeon General's Office. This is, and was then a neat problem.

It arises from a statute that makes it a criminal offense for an agent of the government, or an officer in the Army, or in one of the positions we were in, to have financial dealings with organizations, or people, outside the War Department when the matters concerned were work that the War Department desired to have done. I suppose it's called a conflict of interest. It is a conflict of interest, and it's still continuing at the present and at an intensified rate. It's very severe now. President John F. Kennedy made it very much more difficult as far as reports go, but going back to 1942, when I came in to the Office of the Surgeon General and was charged with administration of the affairs of the Army Epidemiological Board, I looked into this subject because I soon realized that I was dealing with contracts with the university of considerable amounts. The amounts were increasing, and my advice as a representative of the War Department would influence what the universities would do. It might appear that I might be favoring Yale, for example, in this case. The Childs Fund wasn't in the picture at all. It was my connection with Yale that made the conflict of interest. The conflict of interest didn't arise when I was a civilian at Yale and was the director of the Commission on Epidemiological Survey. It arose....

<u>When you joined the Army</u>.

Yes, when I put on the uniform and went in the office in Washington--the Surgeon General's Office. It appeared to me that I was in danger of being accused of doing things that might be prejudicial to the policies of the management of the funds of the government and that I was in danger of the application of the statute with regard to conflict of interest, so I asked a lot of questions about it from different agencies and, as you said from different people I could get different answers depending on how I presented the problem.

I should have put in earlier that in my existing arrangements at this time, Yale University, acting as a fiscal agent for the Childs Fund, was paying in to my salary an amount that made my salary as a Lt. Colonel on active duty equal to what I had at Yale, and the salary that I had at Yale at that time came entirely from the Childs Fund. That's where the Childs Fund might have been involved, although they had no projects. After a while I just wrote to Yale and said I didn't think it proper for me to continue to receive that salary, and Yale didn't object to cutting it off.

Yes--that's how it emerged.

New, Dr. Blake was in a similar position when Dr. Blake was President of this Board and a consultant to the Secretary of War, but he was not under the restrictive orders and control of the War Department that I was under as an officer. On the other hand, civilian administrators and civilian officials in the government are subject to the statutes of conflict of interest. We looked into Dr. Blake's position as carefully as possible and got him advice. We didn't make any changes in financial arrangements. We then introduced procedures in handling the contract recommendations and budgets in the affairs of the Army Epidemiological Board which were pro forma correct, but maybe substantially deceptive. The rule was that when a proposal for a contract came up for consideration by the Board, the director of the commission would present it and argue for it, and then he would leave the room when the Board considered taking action. The minutes always showed that the director of the commission had left the room. I don't believe that made the Board feel any freer because they were never under any pressure, recognized pressure, to do anything special for any one of these men. What I mean is that not one of the directors of these commissions were any more favored than another, but they left the room. Dr. Blake would leave the room when anything came up about Yale, or any financial thing in which his position outside the Board might involve him.

The alternative was to go to Congress for a law. If the application of the criminal code was to be made here, the work of the Board really hung in the balance. The question was do we devise a procedure which will enable us to convey disinterest with respect to decision making and make the action consistant with the criminal code, or go to Congress for a special law excepting this.

Well, it's impractical to go to the Congress and get a law passed in any reasonable time. Just think if this proposal would have gone to Congress what it would have opened up--all the contractors that were making cannon, tanks, and airplanes. It's still a matter of argument as to how these people function.

Dr. Bush went to Congress, or thought he would go to Congress and get a law, but the application of the very law which he obtained was quite severe also.

I don't recall that we had any contact with Dr. Bush over this, or OSRD at all. This was something settled. As far as I knew, this was the only division of the Surgeon General's Office in which the matter came up. I don't really know about the rest. Procurement and Purchasing Officers of the Surgeon General's Office soon became involved in expenditures of more than a billion dollars a year. The Surgeon General's budget rose from a few hundred thousand to more than a billion so fast that they couldn't keep up with it. The men in these jobs couldn't think in these terms and didn't know what to do. The difficulties in obtaining the willingness of manufacturers to turn aside from work they primarily intended to do and adapt their machinery and their staff to making new things needed by the Medical Department was a constant difficulty and a temptation to, I won't say, bribery, but to make some fringe benefits available. I don't know that these purchasing officers in New York, or St. Louis, or any place ever had any problems over conflict of interest.

We've gone more than an hour. Are you kind of tired?

Yes. I might as well go.

I've got a few more items under contracts which I want to put to you.

Go ahead.

One of them involved the question of patents in the event that something was discovered that was marketable. Now, so far as I can see in reading the reports, the only place where this might apply was in these aerosol, anti-insect sprays.

The DDT spray.

Yes--particularly Air Sterilization at the University of Chicago.

Yes, that was the Commission on Airborne Infections.

Right.

The glycol vapors.

Right. How were patents handled? That's a pretty rough matter. For example, here's something that was designed, I suspect, at Yale.

The simple thing about the patent was that the investigator whose work had been aided by service funds, War Department, or anywhere else, was required to notify some responsible officer of his discovery and the possibility of patenting it, and if it were patented, it would--well, it had to be patented by the individual. You can't--an institution can't patent a thing, and that individual was bound in some way--I've forgotten whether he was bound by agreement. Maybe this agreement here in Yale is a paraphrase of the general rule that the investigator, the individual, would take out the patent and dedicate it to the public. That was understood. We had no case in this. Some of it might have been patentable, but nobody wanted to. We all hated the patent question. It is foreign to the spirit of anybody trained in medicine to patent something that is valuable to the health of the people. The ethical physician is brought up in the belief that only quacks and the selfish, people out to make money, improperly patent medical discoveries. It's not in the oath of Hippocrates, but it's just as binding in the mores as if it were.

This has emerged in correspondence all through the Rochester years and even before that--the question of patent in the event that....

Yes.

The other thing under contracts was the final disposition of property purchased under the contract.

Yes. The property belonged to the United States Government, and the property officer, however, could look it over and declare whether it was surplus. He could then fix a price and give the institution in which the property had been used and located a chance to buy it first--first chance to buy it. We had no difficulty over that. As a matter of fact, we didn't have a great deal of expensive property bought. Most of the property under these contracts was disposable--drugs, animals. They were things that would wear out.

Right, except the Harriet Lane Home, the property of the Airborne Commission was transferred to the United States Public Health Service for use at Bellevue--it says in the records.

We had to put blowers and things in there, and it costs something to do that. It wasn't much.

I think the contract itself expressly indicated that final disposition of the property gave an option to the university to buy.

Yes.

Once declared as surplus. The other thing that comes under contracts and maybe it's a little more complicated question was insurance. Let me turn this off, and I'll see you tomorrow.

What do you mean--malpractice....

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We've exhausted some of the substance of this very interesting facet of continuing growth of the Army Epidemiological Board and the Surgeon General's Office. The question of allotments emerges. There were certain efforts made by other agencies--Navy, Selective Service, from time to time to intrigue, or otherwise cajole, or corral the civilian members of the Board. The question arose quite early that if it continued to any considerable extent, it would perhaps be the end of the effective working of the Board and its work. This is a real problem as it emerged in terms of sustaining effort, and any comment that you'd care to make as to how this particular problem was handled....

Well, it soon became apparent after the commissions got to work in military situations that a certain number of officers were needed to make it possible for the work to be accomplished, and to have these officers of the capacity desired, they had to be selected according to specialties, their investigative interests, not clinical specialties, but their scientific and investigative specialties. They had to have also some promise of continuation, or security in the positions, or the jobs which they were asked to fill. There was no such thing at that time as an assured research career in the Army. They would put a man in a position and then change him to something else, even though he was a special, excellent investigator. There were of course many other men who had stayed for a long time during peace years in their research positions, but when the war was on, the demands for officers were so great that the authorities in the Surgeon General's Office couldn't promise a man that he would be kept in one position for any particular length of time. Furthermore, there were people within the Surgeon General's Office who thought that the assignment of officers to this investigative Board would be wasting a military person in a situation where probably a civilian would do as well. Well, civilians would do as well, but as I say, they would do as well episodically, but they wouldn't do as well over a long period and do as well in a military situation where there was a continuation of work.

This situation also involved certain future considerations of the activities of the Board all over the world in years after the war where certain officers with experience with the Board's procedures and methods might be needed, but that wasn't paramount. The practical situation of needing to get and to hold officers with certain qualifications suited for the work of the commissions was the guiding and stimulating influence on our behavior. Well, as you mention, the Navy somehow or other could offer these men more attractive placement than the Army could. The Navy actually could take a young investigator, or middle aged investigators, commission them, and keep them in the laboratory, or in some place, but the Navy didn't have at this time any special labpratories, or boards working in this country and overseas of the type of the Army Epidemiological Board. The Navy did get some special installations later on--one in Cairo and one on Taiwan, for example.

Well, we were anxious about getting and holding officers, and the matter came to a head in the case of Dr. Aims C. McGuinness, a pediatrician of Philadelphia who was on the staff of the Children's Hospital under Dr. Joseph Stokes who was the director of the Commission on Measles and Mumps. McGuinness was offered an appointment in the Army Medical Corps as a major at the time when the Navy was also offering him a position of similar, or equivalent rank, with an added lure in the Navy offer of some considerable permanence in the assignment, whereas the Army Medical Corps could not offer the man they wanted; namely, McGuinness, any assurance of continuation in the work for which he was being brought into the service.

This went on so far that almost an ultimatum was sent. Dr. Stokes had to send almost an ultimatum to Preventive Medicine, to General Simmons and me, saying that unless something were done soon, he would have to let McGuinness go to the Navy. Well, it so happened that Dr. Stokes had known pretty well another pediatrician in New York named Philip Stimson, the nephew of Henry L. Stimson, who was the Secretary of War, and Philip Stimson had introduced Dr. Stokes to the Secretary of War, and in the course of the conversation, a year or so maybe before the McGuinness episode arose, the Secretary of War had told Dr. Stokes that if there ever is anything you think I can do for you, let me know. I think that was an unguarded statement, without any expectation on the part of the Secretary of War that Stokes would ask him to do something that would affect the administration of one of the agencies of the War Department.

Stokes then, as I remember, brought this up with Dr. Blake and with me, and it was decided that he make another effort to see Secretary Stimson, which he did, and Secretary Stimson put him in touch with Brigadier General Miller White who was G-1, Assistant Chief of Staff, the head of personnel in the War Department, a very understanding and intelligent man. I say that not only because he agreed with our proposed procedure, but because it was a fact that he was. Then it was arranged that Dr. Stokes and Dr. Blake first should write to General White, and that took place on November 13, 1942, according to this memorandum. Dr. Blake followed that with a letter summarizing the discussions and putting forward definitely a request which General Simmons had approved that an allotment, a special allotment of twenty-five officers be made to the Office of the Surgeon General, with the ranks specified from Lt. Colonel down to Captain among the twenty-five, for assignment to commissions of the Army Epidemiological Board, or to the headquarters office of the Army

Epidemiological Board. We had in mind bringing in McGuinness and commissioning him as a major and assigning him to be an assistant to me as administrator of the Board.

From that time on we had a series of unexpected difficulties. Although General White turned over this letter of Dr. Blake to one of his assistants with a view to putting the allotment through, nothing happened for quite a while. Finally a letter did go from General White's Office to the Office of the Surgeon General, and it fell into the hands of Colonel John Rogers, the Executive Assistant to the Surgeon General, Executive Officer in the Surgeon General's Office, and General George Lull who was the Deputy Surgeon General. These papers were sent to the Surgeon General's Office on December 5, 1942. However, Colonel Rogers at about that time had to tell General White that the papers could not be found--that's a paraphrase of his statement.

I had no knowledge how personnel matters were handled. General Lull did because he had been Chief of Personnel in the Surgeon General's Office, and the regular military people would know what was necessary. I think the first paper simply gave the Surgeon General a procurement objective of twenty-five officers without specifying an allotment. So I went back to General White's Office, and he turned me over to a colonel [Reuben L. Jenkins] who knew the ropes, and he wrote a statement by order of G-1 and the General Staff that the Surgeon General should have a procurement objective of twenty-five officers, and a special allotment for use by the Army Epidemiological Board. That came back over to the Surgeon General's Office and somehow or other disappeared again. It was said that it had been misplaced, or that it was lost.

One Sunday mornimg, a few days after this, I went down to the office at 1818 H Street and wandered down to the lower floor to the personnel office and found Colonel J. R. Hudnall, who was then Chief of the Personnel Service in the Surgeon General's Office. I was told by him that he hadn't any recollection of the papers, so I sat down by him and asked him if he would kindly look through the drawer of his desk to see if it hadn't been perchance attached to other papers and somehow or other gotten in the drawer. He did, and he found the paper in the second drawer of his desk, and this was the paper for the authorization of the allotment of officers--2 Lt. Colonels, 23 Majors. It was given by direction of the Secretary of War which established a special procurement objective and a special allotment of officers for the Board and from that time on we began to accumulate officers. McGuinness came in shortly after, and we filled our quota which was necessary because several of these officers were sent right away to Fort Bragg to work for Dingle. Some were sent to other and different places. It was all finished in this stage by December 15, 1942, but what I used to call "picking at" our people continued.

There were no orders issued to take these officers off their assignment with the Board, but there were inducements presented to them from other divisions, and many times one or another of the men on the staff of the Surgeon General's Office would take it up with me to see if it wouldn't be proper to release one of these men for what he considered a higher calling. It got so bad through events that I don't remember very well now and it seemed so risky to go on as we were going, that we transferred the whole allotment to the Respiratory Disease Commission Laboratory at Fort Bragg which by that time had been set up as a Class IV Installation under the Surgeon General. The reason for doing that was that it relieved the Surgeon General of a charge for these officers. They weren't counted against him, so to speak, after they were removed to the Class IV Installation. In other words, when he reported on his strength, he had to list twenty-five officers which were not working on what he considered his primary purposes.

Right, and I think there were some areas where all the officers allowed were n completed, and the Surgeon General's Office wanted to make certain promotions and they wouldn't put these through because "You have vacancies. Use them."

Also they liked to take some of these slots, put somebody in, promote him and then have him doing work that really wasn't for the Board. Well, it worked pretty well with the officers carried on the list of the Class IV Installation of the Respiratory Disease Commission at Fort Bragg, but it was very complicated and difficult to handle the papers and their pay and all that sort of thing. It was a little bit insecure, but about this time through the stature that the Typhus Commission had attained, or at least had from the start under the Executive Order of the President with high military authority as a special agency of the War Department, we could use the same protective method for this Board by getting the Board classified in a group called "Miscellaneous War Department Activities." I have somewhere a list of them--I forget, but there were five or six. The personnel and affairs of a Miscellane Activity of the War Department were handled at the headquarters of the War Dep ment in the General Staff region, so the allotment of officers was put in to the Miscellaneous Activities of the War Department; namely, by name, the Army Epidemiological Board. It made no difference in the handling of them and thei activities, as far as I was concerned. As administrator of the Board, I found out how the staff officers over there wanted things reported, what they cared about, and what they had to do, and they wanted to see this thing go forward for the honor of the Secretary of War by that time. So it was taken completel out of the reach of the Surgeon General and stayed that way to the end.

The original order that came down coupled two subjects--procurement objective

~~AND~~ allotment.

That was the trick. That was not two subjects exactly. It sounds like it, but that Colonel told me that just procurement objective wouldn't do me any good. The allotment has a purpose. Where's the order? Doesn't it say for the work of the Board?

Oh yes, but later on, this order is attacked and split in half by some other colonel in the Adjutant General's Office who wanted to refine it a bit.

I've forgotten that, but it doesn't make any difference.

No. It was true with the objective and allotment as one.

Where does it say that they shall work for the Board? That ought to be on a piece of paper in that file. I'll tell you what it looks like. It's on a pink carbon copy sheet with ruled lines between parts of it. Is that my copy of it there?

No. All you have here are these two.

Yes, that's it. You see, the procurement objective is separate. The allotment here is a special allotment of officers to be provided the Board for the Investigation and Control.... You'd have the procurement objective and this states the purpose as to what they are to be used for.

This went through.

Yes, this went through by order of the Secretary of War.

Here's a note also where he says a procurement objective will be a matter of a--what is it?

A matter for a separate communication, but that's December 11, 1942. What's the date of this one? December 2, 1942--but no, that's the Surgeon General's business. Sometimes they left off the date, and they get stamped--oh, there it is.

11/23/42--November.

Those are enclosures. The memorandum for record attached to this paper was that the above action was taken to provide officers for the above project in accordance with a request of Dr. Francis Blake president of the above mentioned Board, dated November 16, 1942.

I don't care about this--this is technical.

Yes, but they began to snipe away at it. For example, even down in Fort Bragg Coburn declared some of the men in the laboratory as surplus so that it was necessary to take the officers for the morning reports and attach them to a procurement section in New York City.

Oh yes.

In order to protect them before the discovery of the Class IV Installation.

Yes, they were attached to the purchasing office in New York City for a while--any place to run for cover. That didn't last long.

Then when I think the reorganization of the Army itself took place, it was further complicated by the attitude of the Army Service Forces. They had different views. They treated the allotment as part of the bulk allotment for the Surgeon General's Office which again, a special allotment suddenly became part of a bulk allotment.

That was a very grievous time--that reorganization period, the behavior of the Commanding General and his officers in the A. S. F. They just made all the possible troubles that they could, and I can show you in writing the statement that it probably was an effort to get General Magee.

The way out of this was the Miscellaneous Activity because that put the Board under the General Staff.

Yes.

And gave it their protective cover, and also that particular process shortened the name of the Board.

The name Army Epidemiological Board--we did that without an official action of any kind. It just got so that in conversation we were calling it that. There was no specific action that changed the name given it by the War Department.

This piece of paper here--there's nothing sacrosanct in a piece of paper, but there is one here where the Board is referred to. Here.

It looks as if I had done it.

Yes.

But as I say, it was changed to Army Epidemiological Board without any particular action. The original name had official sanction because it had been adopted with approval of the Secretary of War, but we didn't go back over that trail.

No. Most of this sniping is a normal part of living, I suspect, and when you operate, when you're trying to function wherever problems emerged, it was a little difficult to keep the kitchen stabile sometimes.

Well, it was all done with the courtesy of a duel.

It was expected.

Yes. I think all of us remained friends through all this. Nobody said any harsh words. They might have thought them. I was having an advantageous double experience at this time. Iwas the director--well, soon after this began, I was the director of the United States of America Typhus Commission, and I learned a lot about higher ranks and higher procedures in that because I was acting under an Executive Order of the President, had a Lt. General--General LeRoy Lutes, a ~~griend~~ GREAT friend, an extraordinary man, as a member of our executive committee in the Typhus Commission which I'll tell you about later, if you're going to take that up.

I want to, but not now.

But what I'm saying is that this is not an isolated thing. I had three jobs at this time. Iwas Deputy Chief of Preventive Medicine, Director of the Tyhpus Commission, and Administrator of the Army Epidemiological Board. It put me in contact conveniently with a lot of officers.

What was true of settling this was to keep peace with the workers too, to keep them working, keep them operating, and Dr. Blake's representations to the Board had a vivid sense of urgency--that in the event something wasn't done.... I think they operated initially under Selective Service, that you could designate certain people as necessary civilians, but this overlooked the fact that they might be inducted into the armed forces, and there wasn't any power

<u>in the Board to protect its own personnel, and there was no particular interest shown in the Surgeon General's Office generally to grant that protection. That had to be carved out to sustain the work of the Board. It's part of breathing, but an interesting part of the story as to how it sustains itself.</u>

They weren't interested in the Board particularly, but they were equally disinterested in Preventive Medicine. That was a part of the unromantic, dry performance that Preventive Medicine has to put on to preserve the health of the troops. It also is sort of inherent in Preventive Medicine that it has no spectacular, or dramatic accomplishments to show because the better you are in Preventive Medicine, the less you have to show. If you prevent it all, there's nothing. They want you to stop. It happens in civilian life. If you stop small pox by vaccination, the people stop being vaccinated, and another generation comes on that's not immune, and they have an epidemic. It's a fact--the better you are in Preventive Medicine, the less you have to show for it.

<u>One of its consequent problems.</u>

It hasn't got the appeal that surgery has.

<u>Even the problem of trying to get a certain officer from the Medical Army Corps, the Surgical Corps....</u>

There's no Surgical Corps--Sanitary Corps, and M. A. C.--Medical Administrative Corps. We got later on some M. A. C. and Sanitary Corps officers.

<u>Yes--this is that quiet negotiation trying to work out the words that would be acceptable to people who were pretty sticky, I think, in some cases in the reading of the regulations, but it made your task interesting in human terms</u>

within the Surgeon General's Office to get this idea floating and keep it alive.

What I'm going to say sounds bragging, or foolish maybe, but I always had in the Army sort of a rule that when you got an order, wait for the countermand, and I did that over and over again.

Well, having read these papers, I'm impressed by the way in which you navigated these shark infested waters, because I know in the Infantry when we got things like this--we didn't have half the expertise that you had in keeping alive. Well, we're practically at the end of this tape, but there's one other....

I'd already been a Dean.

Yes--I know, but there's one other....

Wednesday, June 1, 1966 A-60 N. L. M.

Let's see--in the past we've talked about the Childs Fund that you continued to serve on after you got out of the Deanship. We talked about the change in atmosphere in late 1939 and 1940, with the coming of the war. We talked about the several committees of which you were a member, where you had continuing association in Washington, D. C., and we've also talked about the development of this Army Epidemiological Board as an idea and then as acceptable to the Secretary of War, but we never got you to the point where you actually left civilian life and entered military life and what you really joined. What did the Office of the Surgeon General and its Preventive Medicine Section consist of? That's what I'd like to know today--get you into the Army because there were changes to meet different problems both in an organizational setup and, I suspect, in terms of the intelligence you received from the field.

I came on active duty in the Office of the Surgeon General of the Army with an assignment to the Preventive Medicine Subdivision on February 12, 1942. It didn't take very long for me to make up my mind that this is what I should do and what I wanted to do because I'd had a reserve commission for a long time. I'd been through World War I, and I had known the new chief of the Preventive Medicine Subdivision; namely, Lt. Colonel James Stevens Simmons from the early days and admired him, liked him very much. He called me up on the telephone--I think it was on, we'll say it was on a Thursday--and said if I'd come down to Washington, he wanted to talk to me about assisting him in preventive medicine administration in the Office of the Surgeon General. I came down to Washington and stayed a night with Colonel Simmons, went back to New Haven to think it over, and by the end of the next day, I telephoned him that I would be very happy to come down and be on his staff, which I did. I was down in Washington and on active duty in a relatively few weeks, or less than a week.

Less than a few weeks.

Yes, less than a few weeks. I packed up in a hurry and left an enormous amount of books and furniture and things to be packed by Mrs. Bayne-Jones, but she managed to do it with greatability and much cost to herself from the fatigue of it. She didn't come down to Washington until about a month after I was here. I entered into the Preventive Medicine Division at the time when it was a Subdivision. I keep calling it a Division, but it was only a Subdivision, or at least it was just about to emerge, or had just emerged from being a Subdivision.

To go back a little bit. In 1939, as this organization chart shows, it was an inconspicuous section in what's called the Professional Service Division. Toward the end of that year the Surgeon General reported that the demands on the Professional Service Division were so great and varied that they hadn't been able to set up separate organizations for the separate functions. In other words, there was no formal preventive medicine organization in that Professional Service Division. As I say, it was diffused in a kind of nameless aggregation. General Simmons was brought from the headquarters of the Surgeon of the 1st Service Command in Boston on February 24, 1940, and was given charge of the affairs of preventive medicine that were mingled in the Professional Service Division.

When he talked to you the night you came down here in answer to his telephone call, what did he sketch to you, or what kind of pictures did he have in his head?

He was a preventive medicine evangelist--very enthusiastic and a keen sense of the world wide significance of the work, a great and burning faith in the ability of a well applied lot of knowledge to keep the soldier well and fit to fight. That was his slogan at that time. He was very friendly and kind, a lovable person, but high-strung and impatient. He was impatient with stupdiity, and he'd be impatient with blocks that were put in the way by people who couldn't grasp the whole scope of the preventive medicine effort as he did. I think he talked to me in terms about like that. He didn't have to spell things out to me because we'd been thinking alike so much of the time. I had already seen him occasionally on National Research Council work, or work at the Army Medical School. I used to come down in these days and visit him in the laboratory when he was working on dengue and some viruses, and typhoid vaccine, so we'd exchanged ideas over the years. It didn't take much talk, and I don't really remember that he made me any speech.

I was just thinking--well, I would assume that this particular existing scheme was unsatisfactory to a man of thought and action.

It was unsatisfactory to anybody.

But he happened to have been fingered for the responsibility.

Yes. Colonel Simmons was made responsible for these activities in February. By May 7, 1940, a set of plans and visions for the future that he had in his head had gone before the Surgeon General and had been approved, and they carved out of that Professional Service Division a Subdivision of Preventive Medicine which was soon organized. It was under the single-handed direction of Simmons because he didn't have more than a couple of officers to help him, but he got his organization out under a separate name, though still a subdivision of the Professional Service Division, and he divided this Subdivision up into branches. I happened to be recently just looking at the functions which were given to this Preventive Medicine Subdivision in May, 1940, under the directorship of Colonel Simmons, and briefly it was given the function of having an advisory supervision over military sanitation and control of communicable diseases--a very broad statement--except among animals. It had the function of having liaison with the Quartermaster Department regarding food supplies, water supplies, waste disposal, insect control, housing sites, sanitary appliances, and bathing pools. It had supervisory functions of an advisory nature over the medical department laboratories, except the central dental laboratories. It had a function specified to have a liaison with the United States Public Health Service and other government and civilian agencies and should take office action on sanitary reports from sanitary inspectors.

Well, that compresses in a few words a number of things that sound like abstractions, but they aren't. To any of us who were working to get recognition of the functions of preventive medicine, this says a great deal because you know that these abstract terms are susceptible to imaginative interpretations. You can--as Simmons would--see a world wide activity just at the mention of food supplies, or insects, or any of these things. They just are flare words--you see far into them. This also is a basis for recommending expansion because when you start to do any of this work in abstract terms, you find that you get more than was written into the text at the moment, and you go and put up to the Surgeon General, as Simmons did, plans for expansion over and over again.

Also this is the basis for requesting personnel. He had one or two officers with him, but by 1940, he had four other remarkable men on temporary assignments one of which remained on a permanent assignment. Three of these were sanitarians--

two sanitary engineers, Lt Colonel A. W. Sweet and Lt. Colonel William A. Hardenbergh, capable sanitary engineers. They came on in June of 1940, and these two men made surveys immediately of Greenland and Newfoundland in connection with what was going on with the British in the Atlantic, these matters which were involved in the discussion of the Destroyer deals of that day and were involved in the defense of the United States against submarines, airplanes, and anything that the Nazis might think up. These surveys that Colonel Hardenbergh and Colonel Sweet made were the first area surveys made from a sanitary viewpoint that the Army had possessed. They had area surveys in the United States, area surveys in the Philippines, area surveys in our possessions of Hawaii and Panama, but nothing like this--a survey of a foreign country's possessions. They were so good that they were immediately of enormous value to the planning in Preventive Medicine because the conditions were described very well. They passed immediately to the War Department. The General Staff, Operations Division had nothing like them, and they could use them for strategic considerations. That was the basis--that was a start of a very large and valuable activity in preventive medicine called medical intelligence. We had a Division of Medical Intelligence represented by that book on Global Epidemiology /Philadelphia, 1944 504./, that collected information about health, disease, food, railroads, and water supplies in all the countries all over the world.

The other sanitary engineer, or trained sanitarian in the office was Colonel Ira V. Hiscock who was the assistant to Dr. Winslow at the Yale Department of Public Health. Colonel Hiscock came down in June, stayed a few weeks, and was soon transferred to the School of Military Government at Charlottesville. From there, after a while, he went over to be with Major General John Henry Hilldring in the Division of the Special Staff of the War Department that handled Civil Affairs and Military Government all over the world, and General Hilldring called Colonel Hiscock his Surgeon General. It was a wonderful liaison he maintained with General Simmons and the rest of us, going back and forth between preventive medicine in the Surgeon General's Office and preventive medicine in all the occupied and liberated countries. It was just the global fulfillment that General Simmons would prize very highly. With his imagination and ability, he could see what should be done.

A remarkable thing, I think, in the work of these two men--Colonel Sweet and Colonel Hiscock--was that as early as the 26th of June, 1940, at the instigation of Colonel Simmons, they produced about a ten page, typed report on planning for civil affairs-military government in occupied territory. Now we hadn't any occupied territory at that time, but they just sat down and imagined what would be done. This was transmitted to the Surgeon General by Colonel Simmons as it had been signed by Hiscock and Sweet. That's the first document of thought in the field of civil affairs and military government. It antedated anything that General Eisenhower was thinking about. He hadn't even got the command in Africa by that time. This is 1940. Major General Allen W. Gullion, the Provost Marshall General , who had charge of the Military Government School at Charlottesville, Virginia, the University of Virginia, had no conception of these things and had no preparations for training, or teaching people anything about the sanitary, or medical aspects of civil affairs. This developed into an enormous cooperative effort between preventive medicine and civil affairs because not only was there a Civil Affairs Division in the General Staff, but in due time the Civil Affairs Division was established in the Preventive Medicine Service under the direction of Colonel Thomas B. Turner.

Well, then the other remarkable man that Colonel Simmons brought into his office in the latter half of 1940, was Lt. Colonel Leon A Fox who stayed in the Preventive Medicine Subdivision until January, 1941, when he was put in charge of a remarkable, new type of thing, a medical service run by the Chief of Engineers. The Chief of Engineers set up a medical service under General Fox to supervise the sanitation, hospitalization and care of all the employed construction laborers who were building bases in the Caribbean, in Newfoundland, Greenland--on the Atlantic side of all these things, so Fox went out into that, roamed the country vigorously as he would, and just poured in reports of great novelty and value on Trinidad, on the Virgin Islands, Puerto Rico. He covered the whole thing--down in Surinam, in Aruba. All places that you can think of that you didn't know anything about, Fox would go there and make a survey and report. Fox would come into the Preventive Medicine Office every time he was back in the country and see us, and all copies of his reports were sent in.

This is 1941--Fox left the office in January of 1941, and went on this medical service for the Chief of Engineers and then went further in 1942, and into 1943, through Africa. Rommel was making trouble up along the Mediteranean shores, and it looked like we had to build an alternative route for getting supplies to the British in Egypt, and the route picked was from Accra--well, down across the North of Brazil to Ascension Island, to Accra and then up, as I remember it, to Maiduguri, El Fasher, Khartoum, and down the Nile to Cairo. In other words, it went across Central Africa just about at the bottom of the Sahara Desert. I knew it because I've flown it. That took us into the necessity of making surveys in all sorts of new countries. Ascension Island had to be surveyed, Natal had to be surveyed in Brazil, Leopoldville, Accra--all these places, and that's where this enormous accumulation of medical intelligence just grew up for the benefit of the Strategic Operations Service of the Army and for the health and medical affairs for the Surgeon General.

It struck me as very remarkable that Colonel Simmons who of course knew most everybody, could draw these able men in right away, and he never missed a bet. He was his own best personnel officer. This was before—what they called SFN numbers, Specialty Field Numbers, and the MOS numbers, Code for Military Occupational Specialty, were only just being talked about. It was three years before MOS numbers came in, and besides you can't size up a man from a dry little paragraph about his specialty, his education, his proficiency, and his experience. There are qualities that you should look for in these men--qualities of imagination, spirit, to say nothing of honesty, that don't show up on those MOS numbers. Colonel Simmons could see whether a man had such qualities, or whether he didn't, in a moment, and as I say I don't think he ever made a mistake on a major officer--present company excepted, of course.

Don't be bashful.

This is happening in 1940. This is over a year before Pearl Harbor. This is May and June and the rest of 1940. In 1942, when I went there the Preventive Medicine Subdivision had become the Preventive Medicine Division, and here is the organization chart of May 15, 1941. I have a copy. You can look at that. Now you see that two things have happened from this chart that weren't shown on the previous one. The Preventive Medicine Division is up there on the top line as a coordinate division with the Professional Service Division. It's no longer lost in that professional care part. The little pick under the Divisions above mean that they have a lot of subdivisions and branches underneath them, but I didn't want to clutter the chart up by putting them down there. This chart is copied from one that General Simmons had drawn up, and as you'll see on the right hand side coming off in a position that is about equal with the subdivisions, there is one thing called the Board for the Control of Epidemics. That was a shortening of that long name that General Simmons put in there, and when I did this chart, I put Army Epidemiological Board in parenthesis, although it hadn't been so called at that time. Under that, the commissions that had been appointed by May of 1941, are listed, and that includes tropical diseases too--ten of them, I think.

What you see on the left hand side of that line in the middle is that the Preventive Medicine Division contains an Epidemiology, Disease Prevention, and Industrial Hygiene Subdivision. Now, I'll say something about these names in a minute. It seems like a mishmash to put epidemiology, disease prevention, and industrial hygiene in one block. That was done over again and over again because you had to put a name on these blocks--General Simmons had to put a name on these blocks, and it didn't bother him to mix things in the same block. It doesn't mean that that subdivision had to be all under one man. For instance, industrial hygiene by this time had to come in because the Army was beginning to have industrial mobilization bear upon it. It had enormous contracts--the government had contracts under way for munitions, food, clothing, all sorts of things, that were wrapped up in Lend Lease. Lend Lease had come in in 1940, I think. Well, there were thousands of people involved in this, and they were working in plants that were sometimes owned by contractors, and sometimes they were operated by the government. They were government owned and operated and contractor owned and operated plants, but the government had a responsibility. This Industrial Hygiene Division developed because General Simmons saw the need of it, and he got right away from a very able man named Anthony Lanzer, Professor of Industrial Hygiene at New York University, the advice he needed. Also preventive medicine had a very fine relation with the

National Research Council in whose Division of Medical Sciences there was an Industrial Hygiene Subdivision under Abel Wolman, but this was in the air. It was approved right away as soon as it was mentioned. It was put on this piece of paper with epidemiology and disease prevention, but I don't think that bothered Lanzer. It didn't bother Karl R. Lundeberg who was in the epidemiology side of it.

Venereal Disease Control Subdivision was a branch of the Preventive Medicine Subdivision even from the early 1940 days. It was enormously important and had extraordinary relations with the government. The function of the Preventive Medicine Subdivision that I read where we were instructed to have liaison with the United States Public Health Service and other civilian government agencies--these were those who had a background in the control of venereal diseases. There was a very large organization in the government under Secretary--not MCNamara.

Paul McNutt.

Yes, Paul McNutt, and in that organization was Charles Taft. They developed a great system of attempting to control venereal disease in the civilians in areas around military establishments. It was a huge organization. This was work that General Parran, the head of the Public Health Service, was very much interested in and was the source of rather bitter criticism--I mean that work was a source of criticism of the Army because General Parran thought at one time that the Army wasn't doing its duty. This liaison with the Public Health Service took on government-wide activity and significance right away, and that Venereal Disease Control Subdivision right here on the chart had most to do with it.

Sanitation, Hygiene, and Laboratories was another rather mixed up sort of a thing because the Laboratories Division developed later on its own into a huge scientific and medical affair. It was an aid to sanitation, but didn't have any responsibility for sanitation. The Sanitary Engineering Subdivision was under Hardenbergh, did superb work on water supplies, filtration plants, sewage disposal plants, ditching and draining, mosquito control, garbage disposal--all sorts of things that the sanitary engineers would do. Then you see down at the bottom we've got Medical Intelligence coming out, but they mingled the Tropical Medicine Subdivision with it because you could only have a certain number of these blocks, and nobody cared whether you mixed the babies. That's the way it looked about the middle of 1941.

Shall I go on with this?

Yes.

The next big change that you see is in this organization chart of February of 1942. This organization chart is just before the big reorganization of the War Department and the Army. Suddenly on March 2nd, effective March 9th, the War Department issued a circular and some general orders which did away with the old War Department General Staff Operations Division. It consisted --rather it constituted and established three main services; Army Air Forces, Army Ground Forces, and the S. O. S., Services of Supply, as it was called at that time, later the Army Service Forces, A. S. F. This was the Air Forces coming out toward their autonomy. The Army Ground Forces were the competent foot troops under General Lesley J. McNair. The S. O. S. which two months later became known as the Army Services Forces was the whole supply and material section of the Army under General Somervell that fed and clothed the Army, bought munitions, all sorts of enormous things, probably the biggest business in the world, legistically speaking, but it hit the Surgeon General a terrible blow. Up to this time--that is up to March, 1942--the Surgeon General was a staff officer. He had direct access to the Chief of Staff and to the Secretary of War. By this reorganization and the implementations that went on, the Medical Department was reduced to a very low order of one of the branches of the supply service. In staff relationship it was way below the administration of the Ground Forces, or the Air Forces. The Surgeon General found a whole level of command had been put between him and the Chief of Staff.

Then the A. S. F. began to take things away from him. They took his control of many hospitals in the Corps Areas away from him. They took away from the Surgeon General his authority over selection and appointment of officers in certain ways. It made it extremely difficult for the Surgeon General to function. It was humiliating, and it aroused the profession of the country. It was a great blow to the medical service of the Army. Very bitter times, and it led finally to a very serious investigation of the Office of the Surgeon General in 1943. There were, in my opinion, doctrinaire views of the matter by people in logistics that would lump medicine simply with the supply services. They didn't understand the scope and intellectual content of medicine, but I think there also was pique and, what is already in print, some obvious feeling that the medical officers were old, unprogressive, incompetent people. The medical budget had risen from a few hundred thousand dollars to more than a billion, and the old supply officers in charge of finance and others who had been promoted during this expansion just because they had to have slots for colonels to be filled, were incompetent to handle all this new business. It was very severe, so we're talking now--what have you got?

<u>I have the chart of February 27, 1942.</u>

1942 and 1943--you see, this antedates all this I've been talking about by a few weeks, and if you'll notice at the top there are fifteen divisions--large divisions in the office reporting to the Surgeon General. General Somervell and his staff said that no man can have that many divisions reporting to him, and part of his plan was to cut them down. Well, you see Preventive Medicine on this one has now made some clearances in its own organization. It's been able to take occupational and military hygiene out of the Sanitation Division to stand separately. The Laboratory Division has come out separately. Infectious Disease Control has a clear separate thing. The Sanitary Engineering Division, while it was separate before, it's clearer there, and Medical Intelligence stands out by itself. This is evidence of the evolution of this organizational pattern which shows Preventive Medicine growing up and developing some muscles. The other part--the Army Epidemiological Board and the Typhus Commission are still carried there as if they belonged in, and they did belong in the Office of the Chief of Preventive Medicine. They are not under one of these subdivisions. They are right coming off from the Preventive Medicine Division at the top.

Well, the philosophy of the change you'll see on the next chart, July, 1943, which is about a year or so after the reorganization of the Army had taken place, and you'll see what the chart makers have done here. There were too many people reporting to the Surgeon General, and they have changed that now. They have made them services--six things they call services, but look what it did to the Chief of the Professional Service. It made seven division report to him. We've got thirteen, and that's about as many as we had topside on the previous chart, but it doesn't look the same. The Chief of Professional Service was not competent to handle all of that, and I suppose he had to let these divisions under his service go their own way. For instance, the Medical Division was under the command of Brigadier General Hugh Morgan, one of the most noted physicians in the United States. He was the medical consultant for the whole Army, continental United States and abroad. The Surgical Division was under General Fred W. Rankin, and he was one of the great surgeons of the country. He had charge of all the surgery everywhere in the Army. He was the Chief Surgical Consultant. The same with the Dental Division which got to be very independent. Veterinary Division got to be very independent, and here comes the Reconditioning Division because the soldiers are coming back now. This chart is drawn again leaving out the subdivisions of these other Divisions, but it shows Preventive Medicine with a division and seven branches with the new established Typhus Commission and the Army Epidemiological Board coming off from the Office of the Chief, but it wasn't clearly under the Office of the Chief. It was really a Miscellaneous Activity of the War Department which we'll talk more about, but everybody agreed that it belonged in Preventive Medicine. The U. S. A. Typhus Commission began with an idea of General Simmons who was to be the first director of it, but he didn't want to take it. All the planning for it had been in Preventive Medicine; as a matter of fact, it was planned for a visiting commission to see what the conditions of typhus in North Africa and the Middle East were, but it got placed in this independent state which we'll tell about later. There is the Army Epidemiological Board still attached to the Office of the Chief of the Division, and there is no change in the commissions in numbers.

I will now pass to the chart of February, 1944, which illustrates the maximum growth of the Preventive Medicine Service. General Simmons and all of us were very sad when we weren't a Service. In 1943, we were a Division again--

Preventive Medicine, and it was something to be a Service because that was tops, and as you see on this chart, dated February 3, 1944, I had it drawn so that the Preventive Medicine Service is in the center here--coming straight off from the Surgeon General, not under any of the other Services. This is the maximum development. Preventive Medicine Service has a deputy chief--I was the deputy chief by that time, Director of the United States of America Typhus Commission, and Administrator of the Army Epidemiological Board, but General Simmons called me in one day and said that he thought it advisable for him to have other assistants, and he then appointed an assistant chief for sanitation, hygiene, epidemiology and one for tropical disease control. That partly came about because both of us needed help. I had about all I could do with the Typhus Commission and the Army Epidemiological Board. Besides there were personal matters in these arrangements. I found myself over regular officers who had a seniority in time of service, but not in rank to me. I was a Brigadier General shortly after this--in March of 1944.

Now we have under this Service ten divisions each one with from three to four to five branches, and that is the maximum expansion of the Preventive Medicine Service. I think at that time the Preventive Medicine Service had fifty-three officers and over a hundred clerks, starting from one man and a couple of clerks in 1940. The branches on these charts are indicative of special parts of the work of a division, but I think in all of these charting exercises there was some private thought of making a slot for a man who ought to be made a colonel. If he could be made a head of a division, he would be a colonel. If he could be made a head of a branch, he was apt to get a promotion to a major. This kind of a thing in the Army is a promotion scheme as well as administratively necessary. Well, that's as big as we get.

Now passing over across the Rhine into 1945, here is the chart for January, 1945, where you can see it is a chart of only the Preventive Medicine Service. I left all the rest of the things out because I didn't want to crowd the page too much, but two new things are interesting on there. Coming off from the Office of the Chief and the Deputy Chief and the Executive Officer is a thing called a Special Protection Unit. That's BW--Biological Warfare, and on the other side is a new addition called Health Education Unit which developed a pretty active role in providing audio visual aids, as they call them now--for educating the soldier in matters of health. It's still a quite large and active division with lots of branches and very interesting people and work going on. Civil Public Health Division came in 1944, and it's still on the 1945 chart. That was under Colonel Turner. It had a very important influence on events in SHAEF and Civil Affairs Military Government in Europe.

Then the war having finished in June of 1945, demobilization took place rapidly and the Preventive Medicine Service dwindled to four divisions and eleven branches. The work was still pretty heavy, but the organization was cut down in offices and officers. I stayed in this Preventive Medicine Service, or as it was called after a while, Division until September, 1946, when I got separated from the Army.

<u>Did the Office of the Surgeon General regain at any time the lines to the Chief of Staff?</u>

Yes, it did in a curious way. General Kirk had succeded General Magee on June 1, 1943, I think. I'll look that up to be sure. He had a great deal of respect from the authorities in the War Department and from the President. He tried to regain the staff status that the Surgeon General had had, but he made no progress for a while. There was a very valuable man in the Army through these years named Tracy S. Voorhees, a lawyer from New York, a businessman, who came in as a Colonel and was attached to the LEGAL Division of the Surgeon General's Office at one time. Tracy Voorhees was rather like General LeRoy Lutes. You could send him all over the world, and he'd straighten out something that was bad. You could send him to India on medical supplies, I think, to the Philippines, a very persuasive, thoughtful man close to Mr. Robert P. Patterson who was Assistant Secretary of War, and Tracy Voorhees knew Mr. Stimson too, so he exerted his influence to bring about a restoration of the Surgeon General's staff status and managed to get a letter from Mr. Stimson saying to the Surgeon General, that I regard you as my chief medical adviser on matters of health and care of the soldiers, and I want you to speak to me whenever you feel like it, and I want other people to know that you are my doctor, so to speak. I have that letter downstairs. That wasn't organizational, wasn't charted, so to speak. This was a letter from the Secretary of War as to how he regards the Surgeon General, but it was effective.

How he would function, but Somervell was....

Somervell was still over him.

That makes it a bit sticky at times. But didn't--well, you indicated quite early that the information that was collected by the intelligence unit found usefulness in the ground forces too?

Oh yes--all that material went over to the Central Intelligence--CIA. Well, it's not that, but there was always a staff officer concerned with intelligence at G-2. G-2 always had a staff officer with which we used to battle sometimes because they wanted to take over this very successful medical intelligence enterprise. They used all the material we collected, and they used it as bases for campaign plans and all these war studies. In addition, they collected an enormous amount of material on their own and had a big reading room. Our officers used to go over there and spend days in their reading rooms getting their material. They used our material, and they put out a number of intelligence publications. Then with the collapse of Germany, they began to send all sorts of missions abroad to collect German material, German drugs, German clothing, German books, German weapons. One of these missions had a very secret purpose--to look after, we'll say, the rocket situation. The Germans were firing the V-2s toward the last of the war.

Peenemünde.

Yes, Peenemünde, and German fuels. All of that was bound up with medically interesting things some of which were collected by medically interested people. One of the missions was called Alsos Mission. Do you know what that means?

Alsace?

No, Alsos. It's a greek word for Groves. Remember Major General Groves?

Yes.

Who made the bomb? Well, Alsos is just a greek word for his name. Well, they had enormous intelligence from the Army Service, and our Surgeon General put out a series of mimeographed, legal-length sheets called "Medical and Sanitary Data", notes on Java, Indonesia, Central Africa--stacks of them.

In short, what began quite early in Greenland was another way for ground troops to look at what they might anticipate.

That was the intention. They collected rainfall data, climatic data--food supplies, water supplies--I mean information about them. What I've been saying--I don't know whether you want all those words typed out, but what I've been saying is that the concept of preventive medicine in the mind of General Simmons and the sympathetic minds of those who worked with him was a world wide concept.

It's hard, I think, to beat an imaginative man who is successful. It just is.

Also something came from him that made for effective congenialty. All the officers as heads of these divisions and branches often had desires for the same thing at the same time, but I never recall any bad tempered situations except possibly on my part when one of them would try to take officers from the Army Epidemiological Board.

I can understand that, but again the leader makes a difference.

Another thing about General Simmons is that his influence persisted whether he was around or not. I think I was deputy chief and in the chair pretty nearly half the war, but it all went on just as if he'd been there. He was sick for a while, and then he'd go off on long trips to India, Europe, Panama, Haiti.

Enough problems sprang up with reference to the health of troops to make preventive medicine even in the eyes of non-medical personnel very important.

What I want to emphasize again is that this is not, as some of the medical officers in the Surgeon General's Office thought, empire building. This organization chart represents even less than we might have done, if we'd had more money and more men, but it must be understood that Preventive Medicine is concerned with the preservation of the health of the whole Army. It is larger than the Surgeon General's field because his jurisdiction extends only to the people under his command; namely, the medical corps, enlisted men and officers and hospitals, and he's interested in the cure. People are brought to him and if he can cure them, he cures them. If he can't cure them, he rehabilitates some of them, and he buries some of them. Preventive Medicine is all the time concerned with everybody in the Army including the Surgeon General to try to keep them all well and healthy and fit to fight. It works through--Preventive Medicine works through the line. Ever since the days of George Washington and the Continental Army it was worked that way, and all the Army Regulations having to do with immunization, sanitation of posts, certain measures for the health of troops are issued by order of the Secretary of War and signed by the Chief of Staff, so they don't come out from the Surgeon General, although the texts were prepared in the Surgeon General's Office.

Who was it that fingered General Simmons for this position way back--put him in charge?

I have a very shrewd notion who did it, but I never had it from him, or I have never seen it in writings, but I think Surgeon General Reynolds, the predecessor of General Magee, who was very much aware of preventive medicine and wrote a fine introduction to George C Dunham's book on Military Preventive Medicine, was the man who did it. He is the one who issued the order that brought Simmons from the Office of the 1st Corps Area in Boston into his office. General Reynolds knew Simmons in Panama, made him President of the Board down there and had known him in Washington. General Reynolds was a very intelligent man. In World War I, he was under General Ireland and later succeeded General Ireland as Surgeon General.

This is like bringing in a fellow with horse power who is going to stir up the animals, but who is going to get the job done and--you know, it just takes that drive.

He had it, but I would not put him in the class of practicing administrators in the same sense that I would be. He didn't have to fool with details.

He already knew their significance?

No, he would just say,"We're going to do this."

The work that the division chiefs did and the deputy chiefs did was infinitely detailed--no end to it. We were encouraged to go about things with some originality by even so hard a disciplinarian as General Somervell.

Were you?

He was a curious mixture of contradictions of small type things and broad ideas. I think he's much broader than we realized at the time, and that some of the small things that were done WERE DONE by his assistants, but he said early that he didn't want time wasted over exchange of ordinary memoranda and routing through channels. He actually said,"Pick up the phone and call your opposite number."

For me that worked extraordinarily well in two ways--two cases that I know about. One of them was in 1945, when we crossed the Rhine, and there was all this typhus uncovered in the Rhineland, thousands of cases straying out from these concentration camps. A cordon sanitaire was set up on the Rhine so that all the people, the thousands of refugees and others that were heading for the Netherlands and places like that, had to cross the river, and they were not allowed to cross the river unless they had been dusted with DDT. The cordon sanitaire were inspection posts at the Rhine crossings, ferry crossings, bridges. Well, DDT was hard to get--a little bit, but Lt. Colonel Leonard A. Scheele came over one day from London and talked to me and said that he was desperately in need of more DDT to dust these people. Believe it or not--this is what actually happened--he wanted eight million pounds, I think, of a ten percent DDT powder. Colonel Lundeberg was Assistant Chief of Service at that time--March, or February, 1945. Colonel Scheele and I went to see Lundeberg. Colonel Scheele was in the Public Health Division in SHAEF in London and told Colonel Lundeberg what the situation was. We then and there drew up a little short memorandum addressed to the Surgeon General saying that we recommended that this be procured and sent over by air as soon as possible. Surgeon General Kirk, whom we went to see, immediately approved it, and I took it over to Major General Glen E. Edgerton who was the Chief of Foreign International Health Division in A. S. F. He approved it right away, and in two days we had that material over there by air. The only piece of paper exchanged was a little bit of a piece. I was at that time Director of the United States of America Typhus Commission standing on an Executive Order of the President, so I didn't feel too shy about asking for it.

The other time that interests me, a happening outside of channels, resulted in the revocation of an order that General Douglas MacArthur had given when he was Supreme Commander in Tokyo. There was an International Division of A. S. F. There was a Jewish captain dealing with typhus supplies, and this captain would come over and show me a picture of his latest son, or tell me about his wife, family, this and that, and one day he called me up and he said, "General, did you see those top secret telegrams from General Hildring to

General MacArthur?"

General Hildring's telegram said to General MacArthur, in effect,"What do you mean by saying you won't have any typhus commission control supplies in Japan, or any medical supplies sent to the Japanese when I going to the Congress to get money for this supply of drugs and medical things?"

In the meantime we had thirteen ships, parts of them loaded with Typhus Commission supplies in several ports in the country, and they had been unloaded by orders that came from SCAP over General MacArthur's signature. I telephoned to Lt. General Lutes who was the Deputy Chief of A. S. F. and chairman of the executive committee of the Typhus Commission and gave him the numbers of those top secret telegrams and he sent for them and read them. He took them to the Office of the Secretary of War, Chief of Operations, and said what I told them, that refugees were pouring into Osaka from Korea--and this is now late September, or October, 1945--and there was some typhus there, and we got very much worried. General Lutes got a telegram sent back to Tokyo saying that these ships would be reloaded, single hatch top loaded, and that the material would be accepted over there. My executive officer [Colonel Joseph F. Sadusk] was in Tokyo at the time, and when that telegram came in, there was consternation, he tells me, in the office because words like that hadn't come from the War Department to the imperial general.

This is in keeping with Somervell's notion to function.

Those anti-typhus supplies got there in October, or late in November. Typhus broke out in Osaka and spread through Japan. I have a chart of the outbreak in another publication, and it's a most spectacular thing. It was in the winter and cold and by delousing with DDT millions of people that curve went straight up to 31,000 cases and down to the base line by the end of

January--just like a spike, but it could have been real bad.

Had they not had the materials.

Yes, if the materials hadn't gotten there. What interests me--as I say about going through channels--it all started because I happened to know this boy and talked to him about his children.

Well, I imagine whatever was done within the division had the design and support from General Simmons too, and good support within a division helps it to function.

Yes--when I talk about the Typhus Commission, I didn't always bother General Simmons with all these things. Some of them happened when he wasn't there. He was on the executive committee, and he, Dr. Dyer and Captain T. J. Carter of the Navy were constantly consulted on the executive committee. The executive committee had General Lutes, the Surgeon General of the Navy, and the Surgeon General of the Army also on it, but the affairs were more or less routine, and I did not bother them.

This executive committee--you mentioned Dr. Dyer--ties in public health.

Yes, because the Executive Order not only made the Typhus Commission rather independent of the Surgeon General, but part of the Executive Order says that one of its duties--what is it?--he says, "By virtue of the authority vested in me as President of the United States and as Commander in Chief of the Army and Navy of the United States, and for the purposes of protecting the members of the Armed Forces from typhus fever and preventing its introduction into the United States, it is hereby ordered...." That last is an obligation of the Public Health Service. I never tried to do anything under that authorization, except once, and that roused up the executive committee, so I didn't go through with it. That once was when we were getting Mexican laborers from Chihuahua. They were coming in, and they had occasional cases of typhus among those Mexicans working in the middle of the country--Iowa and places like that. They were lousy, and the problem was to get DDT down there and to get it used. There was some hitch, so I started as Director of the Typhus Commission to get up a team to send down into Mexico with dusters and DDT, usurping a function of the Public Health Service. Dr. Dyer was quite aroused by that. We had a meeting and decided that it would be more politic not to do it that way.

There's--these boxes don't show the relationship to the Department of Agriculture and the scientific work there.

No, there's nothing there on these charts that is not organically connected with the Surgeon General, but we had--well, the National Research Council doesn't show there, the International Health Division, the Agriculture Department. In fact, I don't show the War Department.

No--I think it's a lot easier to see some of the ramifications of change, and the Army Epidemiological Board work that we talked about. Maybe we'd better stop because we've gone longer than an hour.

Well, I'd like some of that to get in there because just to drop the Board in and to talk about tropical diseases, hepatitis, to me is without a head.

Well, it illustrates that in the office you weren't jealous of title--that if problems did emerge, you could use skills available, or skills you had access to--the commission designation didn't matter. I got somewhat confused after going through all these papers.

You're too logical.

I guess it's your function to pull me out of the confused state.

Is that still on?

Yes, and we can take up next time hepatitis as it goes through. Perhaps we'd better set the Board aside for the time being and bring in that Commission here--this fantastic order.

Don't you think these charts will give a little picture of the Office?

Thursday, June 2, 1966 A-60, N. L. M.

Will you make it down there [the Cosmos Club meeting of the Washington Academy of Medicine] all right tonight?

Yes--6:30. Yesterday we talked about the development of the Preventive Medicine Division, in part in terms of its guiding light, General Simmons--we got something of his substance and quality and the way matters happened--illogically, but they happened to aid the growth and development of preventive medicine as one of the big items, certainly, with concern for nine million troops, shifting populations, the possibilities and potentialities inherent in this. I find in the scheme--an item you've given me--the emergence of another agency, different from the Army Epidemiological Board and sustained by an Executive Order of rather far reaching power. This had to do with the Typhus Commission. As we've indicated, words by themselves are not self-activating, and to put substance into this Executive Order requires the push, the idea of men. I wondered out of what kind of context, thinking, this emerged? Since this involved any number of agencies; not just the Army, but other agencies as well, how this was really managed--I guess that's the word--so that it came into being and acted as effectively as it did in the area of typhus which has a long history of being the great scourge of Armies in the field, civilian populations overrun. Typhus was held in check so well by this commission, in part by the discovery of DDT, and by the accumulation of evidence to condition judgment. I suppose there are two things--how it emerged and some internal criticism as to how it operated in terms of the vastness of this power which is conveyed to this Commission.

Well, I'll give you a short historical summary, and then I'll say frankly what I think about the Executive Order as an expression of policy and as an instrument of power. The uncertainty as to the amount of typhus, so to speak, in North Africa at the time of the invasion had General Simmons, the Surgeon General, and other people quite worried. The amount of typhus was not known-- as Colonel Perrin H. Long on the staff in London when the North African invasion was being set up said, "They didn't know what they were going to face in the way of typhus in North Africa," and as a matter of fact, looking at the records later--such as G. Grenoilleau's study of typhus in Algeria and Morocco showing that there were about forty thousand cases in that population, among the Arabs and the people the Army was going to mingle with. It was not known, but there was a suspicion that something was going on because typhus was increasing in Egypt, and it was likely that it would increase in North Africa in the winter too, so General Simmons and Dr. Dyer first began to consider getting authority to send over a commission, if you want to call it that, to make inquiry, come back, and make a report, and on the basis of that report, it was supposed that a deliberative plan would be made as to how to deal with the situation through the normal channels of sanitation and preventive medicine.

At that time there was, I'm sure, no thought of the kind of instrument represented by the Executive Order 9285 of December 24, 1942. There was no thought of an instrument like that, but the commission was never sent. They began to get some information that typhus was occurring among the natives of North Africa and that obviously something more than ordinary exertion against typhus should be put in operation for the protection of troops. They at first asked General Simmons if he would be the director of a commission even before the order was drawn up. I'm talking about the period maybe in September of 1942, certainly in October. They asked General Simmons if he would accept the appointment as Director of a Typhus Commission. He couldn't do it. He couldn't leave his directorship of the Preventive Medicine Service to take on this one thing. They asked next Dr. Rolla Dyer who was Director of the National Institute of Health. Dr. Dyer, a Public Health Service Officer, had the rank of Brigadier General then, and he didn't want to leave his important work on public health of the United States in the time of war to go abroad on a mission of this type. The other man they asked to be the director was the Chief of Preventive Medicine Service in the Bureau of Medicine and Surgery of the Navy and that was Admiral--he was Captain Charles S. Stephenson at that time, and then things began to change. Stephenson accepted, and from that time on the contact of the Army representatives with the developing plans, as well as the personnel plans and operational plans for going overseas to study typhus, were more or less under the control of Admiral Stephenson as a Navy operation.

Admiral Ross T. McIntire, the Surgeon General of the Navy, backed up Admiral Stephenson, and now I'm talking from hearsay--I was told by Admiral Stephenson that he knew very high ranking people in the State Department, and that one of them had suggested to him that if he went abroad on a mission of this type that required extraordinary powers to be successful, he should have an ambassadorial rank. It turned out that those who had charge of recommendations for ambassadorships didn't want to make one for Stephenson, so someway or other he, with the advice of lawyers, I think, in the State Department and perhaps in the Executive Mansion, for all I know, drew up this extraordinary Executive Order 9285. I do not recall ever having seen a draft of this before it was published. I didn't, or I don't believe that either General Simmons, or Dr. Dyer saw it because I saw most of the papers that came in. Being Deputy Chief of Preventive Medicine at this time, I saw most of the papers that came in to General Simmons' Office, and I have no recollection of having seen this order until it was published.

As you said, it is an extraordinarily powerful order. The President says that he is appointing a Typhus Commission to serve directly under the Secretary of War. The language after that makes the Secretary of War and the Director of the commission empowered and responsible for doing most of the things. The Director is directly referred to in such a way that the Director later felt that all he needed to do was to report to the Secretary of War, that the Secretary of War need not be involved and was not involved in the local decisions that were made. The beginning of the Executive Order says that this commission is appointed for the protection of the troops against typhus wherever it occurs,"or may become a threat" to the troops. That opening of what "may become a threat" to the troops is a very extending term because one place may be a threat, or what starts an epidemic? Who knows. One louse might do it. I don't believe that's so, but it could start somewhere. The Executive Order also says in the very first paragraph that there is not only this protective function, but the commission must do something, or the commission is empowered to do something, to prevent the introduction of typhus fever into the United States.

Well, the introduction of typhus fever, or any communicable disease into the United States is an obligation by law on the Public Health Service with their quarantine system. I always thought that whoever wrote this Executive Order did not coordinate it with the Public Health Service because I don't see how the Surgeon General of the Public Health Service, or any of the chiefes there could accept infringement of their jurisdiction to that extent, and I got into a situation only once as Director of the Typhus Commission when I started to do something that involved an infringement upon the Public Health Service, and that was to set up a plan for delousing Mexican laborers coming into this country--the lousy Mexican laborers coming into this country from a typhus infected district in Chihuahua. Dr. Dyer opposed that, and he had the backing of General Simmons and Captain T. J. Carter, who was then acting for the Navy, and we dropped it.

The other main points about the Executive Order are these paragraphs that begin over and over again,"The director of the commission is authorized and directed to formulate and effectuate a program for the study" of typhus and its control everywhere it exists, or may be a threat--"The director is authorized and directed" to call on all agencies in the government that might be able to help, and these agencies that are under the control of the government are told to supply the commission with funds, personnel, and supplies, and provide for their housing and feeding. That was told to the commanding generals of the theaters in a special letter from the Secretary of War to General Dwight D. Eisenhower in the European Theater of Operations, to General Douglas MacArthur in the Southwest Pacific, to the commanding general of the United States Army Forces in the Middle East.

That letter accompanied by the Executive Order had a very persuasive effect on these high ranking officers. They never questioned it. They took it as an order and never, except in one case, tried to say that as they had all these responsibilities for funding, transport, provision and caring for the commission, they should have control, and that was late in the war when an Air Force Major General in command on the forces in Egypt, Cairo, the Middle East, without consulting the commission notified our executive officer that the organization chart showed that the commission was thereafter attached for command purposes to the headquarters of the commanding general of the theater. Before that time, it had not appeared on organization charts of the headquarters of the theater. It had just been listed as something "attached for administrative purposes" which is an Army phrase for feeding, clothing and transporting and doesn't involve command. That notice didn't go into effect. A protest was made right away. The general saw it, and in a friendly way let it go.

Well, on the basis of this Executive Order for singling out one disease, the Typhus Commission Office was set up in the Office of the Surgeon General of the United States Army and was carried as an appendage of the Office of the Director of the Preventive Medicine Service. The Chief of Preventive Medicine Service was also the superior officer of the Director of the Typhus Commission because I was the Director of the commission and was also General Simmons' deputy. I also was a medical officer under the Surgeon General, but I've always had a hyphenated existence, and I seem to be able to function amicably, I would say, with the Baynes and the Jones just the same--that seems like a childish way to put it, but that's the way it was.

Things were done without any argument that I recall. I got approval all the time from General Kirk, the Surgeon General, General Simmons sufficient to do what the Executive Order says the commission can do, and it set up practically its own channels of communication. The Office of the Director of the commission in the Surgeon General's Office in Washington had its own message center and could send cables directly overseas, but the wording of these cables was like this--from Kirk to MacArthur inform Sadusk. Sadusk was the executive officer over there. The cable would never get to General MacArthur. It would go somewhere in his staff, and some colonel, or major would pass it on down, but General Kirk's name was always in those radiograms going overseas to commanders, or to anywhere--anywhere they went out of the Surgeon General's Office they had General Kirk's name in there as the sender, and when they came back, as a rule they were addressed to the Surgeon General for

Bayne-Jones. I think practically all those radiograms were to me, a few for Simmons, but they'd all go that way. That was an example, but when those radiograms were prepared by me they would go down to the classified message center to be looked over for proper phrasing, the kind of thing that you'd have to do for cryptographic security. You're not allowed to repeat a whole lot of words over and over again because that gives the code away. If a policy of any consequence was involved, I always cleared it with General Simmons and General Kirk. I don't think I ever did anything that they weren't fully aware of, except minor matters about sending a thousand pounds of DDT for some job, or something like that.

This Executive Order is so powerful that it could have broken offices to pieces and you could have raised hell, if you wanted to. If you wanted to rear back on this and assert--as some rough officers might want to do--authority, it would have made trouble. Don't you think so?

Oh yes.

But it never did. After I got settled in it, I had in the epidemic typhus group in Cairo, North Africa, and Italy, and in the group in the Southwest Pacific working on scrub typhus through New Guinea up through the Philippines, even to Japan, about thirty medical officers of the Medical Department and Sanitary Corps officers. They were assigned by order to the commission. I had all their records. I arranged for their pay. In other words, I had a contingent. I had about fifteen Navy officers, about fifteen, or eighteen Public Health Service officers and perhaps fifty civilians from Navy, non-enlisted men. All those records are still in a group in the Historical Unit, AMEDS. I ought to have counted before I tried to do this. I'll correct it, if you need it. It ran as a separate sort of installation, so to speak, although it was right in the house with the Surgeon General and Preventive Medicine, in the next room to General Simmons, in the same room as the Army Epidemiological Board, the same room in which I did all the work I had to do as the Dupty Chief of Preventive Medicine.

If it had been under a Navy officer, I don't think it would have worked as well as it did because they had a separatist tendency. It was not only apparent in Admiral Stephenson who developed coronary trouble in Jerusalem, and I will say frankly, provided an opportunity to make a change in the directorship. He was succeeded by a Navy Captain named E. Harvey Cushing, nephew of Harvey Cushing. He's a long time friend of mine, so what I have to say is objectively an assessment of a streak of arrogance. It was in the man, I think, but he didn't last very long as Executive Officer. When--well, he was brought back from Cairo and at that moment, about March, 1943, General Simmons took a very positive action and brought in his long time, life time friend, Colonel Leon A. Fox who hda a splendid record in sanitation and preventive medicine, who had been in charge of the Corps of Engineer's health service for the bases in the Caribbean and the Atlantic, and who had surveyed the air routes from Georgetown, Natal, Ascension right across Africa. Colonel Fox was a voluble, vinegour, picturque, profane man of great ability. He was willing to accept the directorship, and he was promoted to Brigadier General in about a week or so and sent to Cairo. Do you want me to go on with this kind of stuff?

Yes.

When he got to Cairo, he found the group at Cairo, which was the only group functioning at that time in the field, in a disorganized state. There were seeds of dissension in the very formation that Admiral Stephenson engineered. In other words, Admiral Stephenson brought in the renowned Dr. Fred L. Soper from the International Health Division of the Rockefeller Foundation as a member of the commission with the understanding that the Rockefeller Foundation would pay his entire salary and that he would still be more under the control of Dr. Sawyer than he was under General Fox, or General Simmons, or anybody else. Dr. Soper is a very strong minded man. There were two other Rockefeller Foundation people, International Health Division--Major John C. Snyder and Major Charles M. Wheeler. There was difficulty in agreeing on plans and policies among these men. I wasn't there at that time, but I know from the correspondence that it was a bitter and heart breaking business.

It broke up so that Dr. Soper went on off into Africa, North Africa, and joined up with Colonel William S. Stone, the Preventive Medicine Officer of the North African Theater just at a time when a curious thing had happened. General Fox, Dr. Soper, and others in Egypt had been working on dusting clothes with DDT to kill the lice and control typhus. Something went wrong. One experiment failed, and the rumor went around that the DDT was defective, and samples were sent home to be tested. They were all right. There was a lot of name calling and cross talk about the efficacy of dusting clothing with DDT. Dr. Soper went on to North Africa and immediately, without any further question, put on a large program of dusting lousy Arab prisoners in North Africa using DDT, using Rose Dusters, Admiral Flower Dusters--you puff it under the clothes, a remarkable thing. It killed the lice. It had a long lasting effect. It would stay in the clothing for weeks. They were far better than delousing by heat and bathing, because after you delouse by heat, or bathing, and you put on clean clothes, you get lousy in fifteen minites again. With DDT you get protection lasting for weeks.

Also DDT soon got on the black market because it was thought to be an opiate. These people could sleep after they got deloused. They thought that this was the best sleep producing drug that they had ever come across. It was so good that it was possible to delouse Moslem women with their clothes on, poking the dusters up under their clothes and down their bodies, so it was very successful, and it was already to go when the Naples epidemic started.

Now to go back to the commission in Cairo. As I say, that was the only functioning unit at the time, aside from the central office in Washington, and in that central office in Washington I had by this time come into formal assistant relationship with some authority in the Typhus Commission. Up to this time, I had just been handling the papers without any authority at all; took them as the routine of the deputy's job--that is, the Preventive Medicine job. General Fox was so disturbed and worried by what he saw that he sent a cablegram back to General Simmons and me and said that he intended to bring the commission home. It only took us a few minutes to realize that that would be a fatal thing to do. Actually we sent a radiogram to General Fox in the usual way, Kirk to the Commanding General for General Fox, saying that you will not do that--about as plain as that.

General Fox then came home. He could get orders from the Commanding General, and he made many trips back and forth to the United States on a moment's notice. He came back to talk it over, and he understood what it was. He said--well, as I remember, he really didn't like the administrative paper work so much. He was a field man. So he was. He'd been commander of the Medical School at Carlisle, and he made all these extraordinary sanitary surveys from Newfoundland down to Surinam and everything in between, and all the other routes, Air Force routes across Africa. He loved field work, and he was able to do it, and he could take any kind of a beating in the work, got cooperation wherever he went, and he just enjoyed it far more than he did office work of any kind, so he was satisfied to be Field Director, and I was satisfied to have him be Field Director.

They made me Director. I never raised the question as to who was boss, but Fox handled it very nicely. I went overseas twice in this time--in 1943, in December, when he was Field Director as a Brigadier General, and I was a Director in the rank of Colonel. We had a tremendous Typhus Conference in London with all the leading officers of the Royal Army Medical Corps concerned with typhus--Major General Sir Alexander H. Biggam, Major General D. T. Richardson, a few from the British Medical Research Council, people from all down the line. Sitting in this conference room talking about typhus, going through a very complete agenda, the session would go on, and the British officers would want an authoritative answer, and they turned to General Fox there and asked him what the General's opinion was, and he would deferentially bow to the Colonel, and I had to answer. He was playing a game, I think.

Then later on, when I went to Egypt in March of 1944, I was then a General and Director, and I went on a long trip on the air route across Africa to Khartoum and on to Cairo, and when I arrived at Ciaro, General Fox was there at the airport with a big car and some aides. and he bowed this general into the car. He was pulling my leg a little bit, but it worked out all right.

While I was over there in Cairo, I went around with General Fox to all the laboratories--we had a superb laboratory--and we had the whole fever hospital. Fox had made great friends with the Minister of Health of Egypt and a lot of Beys and high ranking officials--the Typhus Commission was the thing in Cairo at the moment. Fox had rehabilitated it, and they were doing excellent work in the laboratory and on the ward. I stayed there about ten days and saw these things.

I had another job at the same time talking to the Ministry of Health about yellow fever vaccination. The rules--the Egyptians were being governed by the Indian rule which had not yet caught up with the new knowledge, and they were unnecessarily severe in requiring revaccination at more frequent intervals than was necessary against yellow fever. They didn't know that the immunity would last a long time. The Egyptians were playing, I think, some kind of a game with the Indians and the British. It resulted in holding up air travel through Cairo, military air travel, and kept our troops from getting off ships in Bombay. Sometimes they were kept on board for two weeks. It was serious enough that I went to see the Egyptian Ministry of Health and got the rules changed because I had plenty of evidence along with me, that I had gotten from Dr. Sawyer largely, but that was a side issue. The fact that I was Director of the Typhus Commission and had this Executive Order and was well received by the minister--American minister in Egypt and by general officers didn't do any harm.

Now we're getting toward Naples, and I'd rather not go on with Naples right now. It will take a long time.

Let me just ask you this--what part in the development did the desire to vaccinate the civilian population play? Did the commission license public health associations in countries?

No.

Didn't they?

No. We gave it away.

But as agents.

The vaccination of civilians could have been done by any doctors anywhere, where the doctors could get their hands on the typhus vaccine. Anybody in this country could buy it and give it, but the United States Government had millions of doses made. Here even the commission came in on the supply of typhus vaccine. At one point, the pharmaceutical manufacturers in this country, who were making the vaccine, didn't have enough bona fide firm orders to justify their continuing production. This was critical, because the staff making typhus vaccine were immune. Some of them had had typhus--it was dangerous work. Some caught typhus and died while they were making the vaccine. Most of them had been immunized by injections of the vaccine. Some of them got doses that they couldn't handle, and it was a danger at that time that those expert, immune staffs of the manufacturers would be dispersed, so I went down to see the supply officer in the Surgeon General's Office and waved this Executive Order at him, and he said,"All right. You write me a piece of paper and say that we must order ten million doses", or something like that, and I did. It was a little piece of paper that kept these staffs at work.

Well, so far as civilian vaccination abroad there were two things in there. A certain amount of vaccine was just given out free, we'll say, in Naples and administered by the United States people who were working on the epidemic in Naples. They did that in other places too. We had a very good vaccine, a better vaccine than the British had, and we used it. It began to be distributed by General Fox in some of his trips, notably to Teheran in 1943. He then got terribly outraged about it because the vaccine found the black market in Iran. He took it up to Iran to be given to the poor people, but it got in the hands of the bankers and the lawyers, and they kept it for themselves, and the poor people didn't get much which disturbed General Fox a great deal.

In addition, when General Fox and Colonel Edward S. Murray were sent in civilian clothes in to Turkey, they took the vaccine along with them to give to Turkey--we had a project to study the effect of vaccination in producing immunity by using the prisoners in the Zondulak Prison, I think, but the great distribution to civilians came after the Cairo Conference. The Cairo Conference was in....

<u>Late 1943, wasn't it?</u>

Yes, late 1943 /November 22-25, 1943/, and General Fox was at the Cairo Conference, staying out at the Mena House, and he had some access to the President's party. Along on that party were Admiral McIntyre, who was President Roosevelt's physician and a member of the Executive Committee of the Typhus Commission, and Lt. General LeRoy Lutes, who was the chairman of our Executive Committee, and all these gentlemen were in the Presidential family, so to speak, at Cairo. General Fox had the very bright idea that it would be wonderful for American prestige and good will if there were a free distribution of typhus vaccine to the citizens of the Middle East countries. He got that approved. Admiral McIntire took that to the President, and the President said all right, and immediately a cablegram came over saying that the President approved the distribution of typhus vaccine to the Middle East free. We had millions of doses in storage in Cairo at that time. I think another thing that might have influenced General Fox, and this is a matter of record anyhow, that it would be better to put it out through American agencies rather than through the British, because the British had a habit of taking the American label off and putting a British label on a good will package. I think maybe we distributed ten million doses of typhus vaccine on this presidential approval.

This was in an area by and large with which we'd had little experience--Iran, Egypt--nomadic tribes in Iran that are on the move all the time.

Iran's typhus came down from Russia, from Poland. At the very beginning of the war there were Polish refugees that came down the Persian corridor, as it is called, and they brought typhus there. It was a British medical officer named Brigadier A. Sachs who wrote a paper on typhus and got worried about typhus in Iran right from the start, and of course through Iran was going all the Lend Lease supplies to Russian from Abadan on the Persian Gulf, and the bottom of the Caspian Sea, through there, so that it was a worry. We were more familiar with Iran than we were with some other places. I forget the name of the commission /Millspaugh Commission--Arthur Chester Millspaugh/, but there were missions under American people who were making health surveys and economic surveys in Iran. I don't remember the name of the Commission that had been functioning there before, but it came close to me toward the beginning of the war. Former Major General Charles Reynolds, who had been the Surgeon General, was offered the position as the head of a health commission to go over in Iran, and we gave him a lot of information. We did know a lot about Iran, more than we did about Egypt.

There was a fellow named James M. Landis.

In Iran.

He was in the whole middle east in economics.

Well, the Middle East Theater started as an economic affair, supporting the Lend Lease operation for the Russians. There weren't many troops there for a while. There never were very many.

<u>Did the laboratory work just on questions of typhus--or did it also do these intelligence surveys, other diseases that might be in the area?</u>

Only incidentally. The only other disease that it surveyed was relapsing fever in the British Sudan. Relapsing fever is carried by a louse. The big diseases, schistosomiasis, bilharziosis--they didn't bother with that. They saw cases of it. I will bring you the whole bound volume of the Typhus Commission scientific reports. You haven't see that, have you?

<u>No.</u>

Well, come down with me after this, and I'll give them to you. They made studies on immunization--K. S. Ecke and J. C. Snyder. They studied the susceptability of a rodent called Gerbillus to infection with typhus organisms and scrub typhus organisms. They got material from India, Burma. They made fine clinical studies on typhus, and the big basic study was on the pathology of typhus by Commander William B. McAllister, Jr.

<u>Back in the States--you said something yesterday about an Executive Committee that functioned.</u>

The Executive Committee is not in the Executive Order.

<u>No, I knew it.</u>

The Executive Committee was set up by General Simmons, Dyer and Stephenson at the start because they wanted to do two things--they wanted to have high ranking advisers, and it was a good political move to have the

Surgeons General--Navy, Army, and the Public Health Service--and the Deputy Commander of the A. S. F. as the so-called Executive Committee. I think the name was badly chosen because it never executed much.

<u>It may have been part of the channels of communication to keep everyone apprised of what was going on.</u>

It didn't meet very often, and maybe a good deal was done on the telephone to General Lutes. Of course, I used to report to Surgeon General Kirk at least once a week about all this, and I suppose somebody reported to the Surgeon General of the Navy and somebody to Dr. Parran at the Public Health Service. This Executive Committee, I believe was looked on as something that would come to your rescue, if you got into trouble.

<u>Like a buffer.</u>

Yes.

<u>What about the questions of priority--you had no funds.</u>

We had no funds at all. I have some correspondence where I applied to the Director of the Bureau of the Budget for a budget, and this is the answer that I got. The Director of the Bureau of the Budget and the Director of the Bureau of War Department Budget--he had a say because the Typhus Commission was set up as a Miscellaneous Agency of the War Department, personnel matters, and really its line of communication passed straight to the War Department. These other services couldn't touch it, so its finances would also be of interest to somebody in the War Department, as well as the Director of the Bureau of the Budget. They studied this request of mine for a while, and actually the reply I got was "With this Executive Order, you don't need a budget.

All you have to do is just tell the Commanding General, or ask the Commanding General for funds."

What about travel--it says that you can go anywhere.

Yes, travel--I had relatively little to do with the travel orders, except those I would request. For instance, if a man was in Cairo, and I wanted him to come back to headquarters, you'd send the Commanding General a radiogram, saying,"Request that Colonel Synder be sent back to Washington on Temporary Duty for ten days." All the travel orders would be written in the theater. As a matter of fact, I don't think travel was any particular consideration to anybody in the war. There were no limitations to speak of on the amount that was spent. I'm sure that the travel I had under the commission was charged to the Surgeon General as a regular order. When we sent officers over on travel orders which turned out to be very big and complicated things, I would work with a Colonel J. A. Grotenrath in the War Department. All our travel orders going out for people who weren't on the staff, we'll say, of the Surgeon General, and couldn't get them some place else, we'd get them through Colonel J. A. Grotenrath. I remember one case where we wanted to send a group to Burma to the scrub typhus laboratory we set up on the Irrawaddy River. For example, Colonel Thomas T. Mackie was the head of that, and I can recall working out Colonel Mackie's order with Colonel Grotenrath in the War Department because Colonel Mackie at that time was on the staff of the Army Medical School, and they had no concern with traveling him over there.

You moved public health personnel too--A. G. Gilliam.

Yes--well, you'd get the theater to write the order. I forget when we sent Gilliam out of here. He was on the first group, wasn't he?

Yes, and he went to China.

Gilliam got scrub typhus out of that.

When it came to making action effective, this was a pretty good rub--this Executive Order.

Oh yes--it's broad enough, and you had to do it. As I say, I don't ever remember it being used as a stick in the sense of beating anybody.

No, but we can come to it tomorrow where certain offers of aid and assistance were made in the Mediterranean area which were, in fact, refused--you know, Naples, but that's too long, and you said earlier that you had to go, and it's a quarter after three.

Is it quarter after three? Seems like I've been here an hour--I came a little ahead of time. Well, it varies.

When Lt General Ronald Mackenzie Scobie went into Greece, the Typhus Commission in Cairo was then in working relations with the Middle East Supply Unit, and I've got great files of minutes on the Middle East Medical Commission, British officers mostly, but we were able to attach Major C. J. D. Zarafonetis to an outfit going into Greece at the time when the Greek Civil War was on. The British were just entering the country, and the Germans were being pushed out. I don't remember the details on that, but we had no trouble traveling Zarafonetis into Greece. It was handled out of the theater, the Middle East Theater, and he was brought back.

There is some delay mentioned in here, owing to discussions between Churchill

<u>and Stalin with reference to the Balkan areas.</u>

Not only that--it involved Churchill's desire to attack the soft under belly of the Axis, and I think Churchill, if he'd had his way, would have gotten through ~~the~~ To Vienna and stopped the Russians before they got down to the Danube, but there is a record somewhere of Stalin talking to Churchill about this, and they practically decided,"You let me alone. I'll let you alone."

They gave it up, but while that argument was going on, General Fox was very urgent to get into Yugoslavia, maybe to Roumania, but certainly Yugoslavia, the Balkans, and I tried my best to fix it up for him to get permission to go there. I even went so far as to go and see the Chief of Operations in the War Department General Staff, close to Secretary Stimson's Office. It dragged on that way, and nobody would tell me what was holding it up. I didn't know there was this difference of policy opinion between Stalin and Churchill, and that the American war plans were somewhat affected by that detent, or whatever you want to call it. In the middle of that, Fox got a little tired of waiting, and he sent a radiogram over here saying, "Unless I hear in twenty-four hours whether or not I can go into Yugoslavia and the Balkans, I'm going."

Well, all those radiograms are read in the War Department too, and General Styer /Lt General Wilhelm D Styer, Chief of Staff, A. S. F./ was acting for General Somervell, was the commanding general at that moment. He called me up, and he told me to radio General Fox that the War Department was not taking orders from Fox.

I devised a polite radiogram to Fox saying about that. It was a Saturday, as I remember, and I went over to Baltimore to the christening of a Liberty Ship which was being named for my friend Jim Trask /Dr. James D. Trask/ who had been on the Streptococcus Commission. I left the draft of my radiogram to Fox which had to go back out through A. S. F. with Colonel Sadusk to take over to the Pentagon, to get it cleared and sent, because this involved General Styer. Colonel Sadusk took it there. They took it away from him and rewrote it in General Styer's Office in the most insulting terms to General Fox and sent it over my name--that's right. Sadusk called me up in Baltimore and said that I'd better come back,"This is pretty bad."

I did come back. I went over there, and I talked to a Brigadier General--this sounds foolish, but I got into a state where I practically invited him to meet me out back of the Pentagon, and his excuse was that he really didn't agree with the rewritten message over my name, but General Styer had told him to do it. That was the end of that.

Fox didn't get into the Balkans at that time. Later Fox did get into Yugoslavia and the Balkans, and we had one of the most successful typhus control operations of the commission's history then because Fox is built like Tito and acts like Tito, and they struck it off together right away. General Fox told me that he met Tito on an island in the Adriatic and told him that he wanted to get into Yugoslavia, and after listening to him, Tito said,"Dubro" which means O. K. Fox had his own plane by that time. General Fox had a personal plane given to him for his use in Cairo, and he flew back to Cairo. He got three or four of the members of the commission and flew back to Zagreb. We worked on typhus control in Yugoslavia for the rest of the war and even afterwards. E. S. Murray made such friends over there that he'd go back year after year after 1945, and do typhus studies, and in the course of those studies they made a great confirmation of Dr. Zinsser's idea that Brill's Disease in the United States was a latent typhus infection, and they proved it.

The arrangement which saw the development of a field director with a reserve rear eschelon headquarters was a good working arrangement.

You see, there's no field director in the order, and the order still said that the director is authorized and directed to do this, but he didn't swing around on the field director.

I would have thought that it would have been impossible somewhat for Stephenson as the director and in the Eastern Mediterranean to really run the shop.

Of course, he wasn't. He didn't foresee it at all. He couldn't run it from out there. He was just thinking of kudos--ambassador.

Were there continuing relations with the State Department as this Executive Order implies?

Right through--I have a great file of correspondence with Dean Acheson and people like Kirk, the Minister at Cairo, Ambassador Laurence A. Steinhardt in Turkey, and John G. Winant in London.

Earlier you mentioned a problem that developed as you put it, with DDT, but that was MYL at that time.

Yes, MYL was the louse powder that started the thing. MYL had pyrethrum in it, and MYL was probably first used at Naples, and got the Naples epidemic under control before DDT. It was called Powder Body Crawling Insect.

One each.

In shaker tins. MYL was a pretty good powder, not as good as DDT.

I think there was some seasonal problem in Kenya which was the source of

<u>the material....</u>

Made MYL short.

<u>Yes.</u>

Yes. It involved the Typhus Commission representative, myself, and the Surgeon General in a little controversy with the British Purchasing Commission over here which was under the control of Dr. John R. Mote who was an American, but he was acting more British than a cockney. They wanted tons of DDT at a time when we didn't have enough for ourselves. I've got here from Simmons' diary a statement--curiously enough where Simmons says that he's willing to help the British but that he is not going to penalize the American needy forces by giving away DDT at that moment. Well, we tried to get pyrethrum plants over to this country to plant in Arizona to start a plantation. The typography of the country was rather like that where they grow in Kenya. It's a kind of chrysanthemum-like flower. They did get some plants, but they didn't succeed in growing them.

To go back to the British Purchasing Commission. Dr. Mote, after he didn't get the DDT because we couldn't give it to him, said to me, "General, I just want to tell you this, that if you don't arrange to have DDT released to us, the Empire will squeeze you."

Imagine an American talking like that!

<u>DDT as a compound had been in existence for seventy years.</u>

DDT was a dichlorophenol--it's got tw~~o~~ phenol rings with a couple of chlorine atoms between them. It's like so many things in medicine. It was made for some other purpose. It was made by a Swiss chemist who was simply studying for his Ph.D. chlorinated phenols. This had been put on a shelf in some laboratory in Zurich--I believe it was Zurich, until H. Mooser began to try different things in a screening process on insects. They didn't know that this was good to kill insects. They didn't know anything about the pharmacology of DDT, and they tried it on flies at first, and it was lethal for them. I have a reprint from Mooser about his work on DDT, and he signed it to me and signs himself as "The enemy of lice and sometimes the friend to man."

Somehow, or other they sent you a sample of this, and you shipped it down to Orlando, I guess it was.

Not to me--the man who is important in that is Colonel William S. Stone who was the head of the Sanitation Division....

In the European Theater?

No, he was in the office in Washington. General Simmons didn't send him to North Africa until January, 1943, but the Geigy Company sent DDT over with some information, and Colonel Stone got the first sample and sent it down to Orlando.

That certainly revolutionized things, didn't it.

It certainly did. I did the usual infantile act on it. As soon as I saw it, I ate a piece of it.

How did it taste?

It burned my tongue. You know how children put things in their mouth.

Well, you'd better go because it's twenty-eight minutes after three.

Stone is a case too. That's on there though. Take it off.

Monday, June 6, 1966 A-60, N. L. M.

Last time we talked, we talked about the developments which led up to the execution of this Executive Order 9285, and it's a fantastic order, conveys quite a bit of power on paper, but again the agony of making paper walk is part of the problem. In a sense, the first opportunity one gets to make it walk is in the Mediterranean Theater, European Theater generally, and perhaps more particularly North Africa. You indicated last time that you established laboratories in Cairo and got set up, that there was some development in refining the administrative process within the Typhus Commission--establishing the field director and a home base with deeper roots than had existed before, and on the basis of some experience. But North Africa had gone off as an invasion, had taken troops into an area which was quite a problem from the point of view of typhus and unknown virtually at the time of the invasion. You showed me a letter the other day from General Simmons, a memorandum requesting from those in command of troops, or those in command of operations like this, much more in the way of information than you'd had theretofore, so that you could make available the expertise in unknown areas through surveys and so on. Getting this Executive Order implemented as a moving thing appears in the Mediterranean, and more particularly Naples. In the absence of clarity, in the absence of communication desired or otherwise, certain operations are set in motion to which you fall heir by invitation toward the end of December in 1943, in Naples, with a full blown, desperate situation on your hands and the problem of giving it shape, dimension and organization to a conclusion. It's in making this paper walk in that area--this is the effective thing as distinct from whatever human foibles were on the scene that are in some of the documents I've seen. I'd like you to comment on the Naples situation as to what you had by way of aid, what you had by way of productive facility, material, people--who was on the scene that you could rely on, what the relations were with the British, French--it's a complicated picture, the Fifth Army in Italy, complicated even more by the introduction of enemy territory that had to have military government superimposed on top of it--not an easy picture even on a clear day. I don't know what sort of comment you want to make, but it's an arresting problem in a lot of ways.

Well, the Executive Order, as you say, was not a self-executing document. It had first to be accepted by any Theater to which it was to be applied, although the acceptance came usually in response to a letter from the Secretary of War to the Commanding General of the Theater. As I recall it, that was not done in the case--perhaps a letter was not sent in the case of the North African Theater, and why it wasn't sent I don't know. There were a good many things that were not done in relation to that theater that were done in others. The lack of information about what was going on in the theater to which you referred, in citing that memorandum of General Simmons of, I think, sometime in the summer of 1942, was that memorandum requested that Preventive Medicine be informed as fully as possible on impending campaigns, or movements of the Army and informed on what the possible needs might be, numbers of troops, dates and so forth. That memorandum was addressed to the Plans and Operations Division of the Surgeon General's Office. The plans and Operations Division did get most of that information, but they didn't pass it on, so that Preventive Medicine was in the dark a good part of the time--at least at this stage.

I would like to leave out the discussion of personalities, but let it be understood that personalities and ways of thinking had a lot to do with the troubles that arose in the administration of typhus control in the North African Theater. I've spoken of that in the chapter in the book, Volume VII of this Preventive Medicine Series and have said in reference to the dissension and the rather bitter situations that developed that there were questions that never could be really answered clearly and to the satisfaction of all concerned, that there was a great accomplishment to be shared by all concerned and that there was glory enough to pass around to each person some of it, and that's the way I'd like to leave it.

As for the organizations that you talk about, there were in that Theater a very complicated series of interlocking organizations. In the first place, because it was a British American Theater and double command throughout and not always a clear jurisdiction--not clear jurisdiction between offices and officers in many cases. There were newly formed organizations operating there such as the Civil Affairs group which had never had any experience before in the field. They were people hurriedly trained at the Provost Marshal's School at Charlottesville and were sent out to North Africa to work with Civil Affairs. Now some of these were Public Health Service officers who knew about public health matters in the United States, but that was different from public health administration under military rule.

There were similar sets of British civil affairs persons who had more experience because the British got interested in civil affairs for occupied and liberated countries as early as 1940. They saw the needs right away and had been building up civil affairs; in fact, when the campaigns were being organized in London, when they began to think about moving across the channel and all the things that preceded the Normandy Invasion, the British had what they called "country houses." Each country that might be liberated, or occupied had an office staffed by British officers assisted by some officers from those countries. There was a country house for the Netherlands, one for Denmark, one for Norway, a big one for France and another one for Italy, and these "country houses" as they were called, studied the conditions in the countries they represented and made plans for the care of the civilians that would be put in effect when the occupation, or liberation took place. They were inhibited in their communications with each other and with the Americans because of the great secrecy that had to be imposed on any of these cross channel plans. It's inconceivable how you could talk sensibly about what you were going to do in Holland for the care of the liberated people, unless you could say that we're going to be in Holland on Wednesday, the 10th of March, or something like that, but you couldn't give away that kind of information, and so they planned in the dark. They got along.

The same sort of thing was affecting us in relation to North Africa. You can understand perfectly well why they wouldn't broadcast their campaign plans among all sorts of people, but there were some major bits of information that could be imparted that would be a basis for preventive medicine planning that wouldn't give away the campaign secrets. We could talk in terms of letters or codes or something else of that nature.

The local government first in North Africa under Admiral Jean Francois Darlan and the residue of the French had relatively little to do with the control of communicable disease in the areas. All of the MAIN ports, Algiers, on up to Tripoli, Bizerte were under, from our point of view, total American control. We had laborers from Morocco and Algeria, but no authority, no police, nor medical authorities of any consequence, so that I thought of it at the time as a strictly American affair which, of course, it wasn't because the British were in there. We went pretty much our own way cooperating and collaborating with the British, or American unit until after they had gone through Sicily and had moved into southern Italy. They entered Naples on October 1st, 1943.

Until that time there was no really operating Allied Civil Affairs organization. They had formed one in North Africa before they got across the sea, but it hadn't any practical experience. They had collected some fine men to work with them both on the British side, Colonel G. S. Parkinson, and on the American side--men like Colonel Wilson C. Williams. They set up an Allied Control Commission with a Public Health Section which had extensive meetings dealing with problems of the accumulation of medical supplies, food, things that they would need for the rehabilitation of the country. They had some of that in readiness when they arrived in Italy, but the Allied Control Commission, whose records are voluminous, itself had difficulty unifying its functions. They were at cross purposes very often, so much so that early in 1944, General Simmons sent Colonel Thomas B. Turner, who was the new head of our Civil Public Health Division in the Preventive Medicine Service, over to North Africa and in to Italy for a review, a first hand look, at what they were actually doing in the Allied Control work for civil affairs public health activities under military government. Turner made very valuable reports. Then he went on to London in time to have a good deal of influence on the Civil Affairs Public Health Sections of the Supreme Headquarters of the Allied Expeditionary Forces, civil affairs then being under British Lt. General Sir Arthur Edward Grassett, so while there was great need for improvement, there was a great need for haste and improvization, and much was happening while these men were trying to get their organization in shape.

In the medical headquarters of the North African Theater of operations there were some strong people. Brigadier General Frederick A. Blesse was the

Theater Surgeon following General Kenner, and he was a very good man. One time he thought there might be a serious affair with typhus in Italy, as anybody would say--a million people crowded into a bombed and wrecked city, water supplies gone, food supplies inadequate, no soap, no cleanliness, no sanitation--anybody could sit down and write a memorandum saying that this is the fertile soil of typhus and that it will be bad if it breaks out there. He didn't take any immediate steps, and I don't think he became alarmed about the typhus situation in Naples until much later.

After they had gotten into Naples, information about typhus that might have been available somewhere else not only was utilized by the BBC in that broadcast, but could have been utilized by American medical authorities because typhus had been increasing in Italy from about January, 1943. It came in across the Adriatic with the prisoners of war from the Balkans. There had been no typhus in Italy for the previous twenty years, so that it was a non-immune population, and the few cases that were coming along are all set forth here.

In the absence of a letter to the commanding general of the theater pointing out that the Typhus Commission was set up by this order and would be ready to assist in any way it could, and with the feeling that the Theater medical authorities had, or appeared to have, that they were quite self-sufficient, and with the strong moves made by the International Health Division of the Rockefeller Foundation to take charge of typhus control largely at its own expense in North Africa--with all those things, no call was made on the Typhus Commission to render any assistance. As a matter of fact, one time when the question was asked if the Typhus Commission was wanted in there, the answer was no, and yet typhus was increasing in Naples. The louse powders were at that time effective, but in rather short supply, so far as DDT was concerned. MYL was plentiful enough, but DDT was just coming into production in any quantity. The theater had asked for large shipments of DDT, had obtained some in the period August to October, but was turned down on a request for a rather large amount in September of 1943. I was able to put the case to the Quartermaster in Washington about December 1st, 1943, and get a shipment of DDT over by plane and boat pretty soon. They used that as well as MYL in dusting the people of Naples, but most of us think that the MYL cut down the lice sufficiently to reduce typhus before they had enough DDT.

Well, all sorts of people were involved--Wilson Williams, Brigadier Gallaway, Brigadier G. S. Parkinson, Colonel H. D. Chalke, the British Typhus Commission sent out from London which included at least two Americans, Dr. Joseph E. Smadel, and I forget who the other one was. There was much confusion and many units and many individuals trying to do the work. Finally, it looked as if it would be getting out of hand, so the authorities in the Naples area called General Fox who was in Cairo to come and to bring such help as the Typhus Commission could give them, and that's about the time, in the middle of December, December 20th, that the Typhus Commission went in to Naples.

Fox had supplies of vaccine which were not thought to be particularly useful in the situation because it takes a while to produce a state of immunity after vaccination, and the conflagration was rolling on. Fox had DDT in Cairo and could get more, and he had a number of able workers with him that he took in. He enlisted--General Fox enlisted the aid of Colonel Harry A. Bishop who was of enormous help in managing the work of the groups on typhus under General Fox after December 20th [1943] into January, 1944. The program was the usual one--a great dusting of all the people, a million or so dusted in a short time. There was isolation of patients in hospitals. There were follow-up contacts--delousing all the contacts in a typhus patient's house. His bed clothes were deloused, dusted with DDT. So were the rooms, and there was a fairly widespread delousing of the citizens of Naples rationally guided with a possible contact with typhus and guided by the fact that they nearly all were infested with lice. Also the Typhus Commission organized what were called "flying squadrons" to send groups out in the environs of Naples to look for sparks of typhus that had blown out there, and they did find some secondary foci. Then in addition the work involved a Navy epidemiological unit under Lt. H. M. Gezon, Epidemiological Unit #23--I see here. It came in and did some very nice work toward the end. The epidemic reached its peak and started down shortly after the Commission got in there, and there's a dispute as to whether the efforts of the Commission brought about that turn, or whether it was going to turn already. That's hard to answer.

The other things that General Fox was able to do was to get much needed transportation that the Rockefeller Group had not been able to get. They had not been furnished any transportation by the theater itself. There was a division or so in the Foggia area near the heel of Naples under command of a friend of General Fox named Major General Arthur W. Pence. He understood the situation from the way General Fox described it to him. He put Naples out of bounds right away and turned over not all the transportation of a division, but he turned over jeeps, two and a half ton trucks, and motorcycles and side cars, and the boys were able to get around after that.

Yes.

Bearing on the remark I made the other day, the better you are in preventive medicine, the less you have to show for it, I would like to tell you what a representative of the press in Washington said when he came to interview me about the Typhus Epidemic in Naples. About the second question was, "How many cases did you have?"

I said, "Nineteen hundred."

The reporter closed his book and said, "That's nothing."

Well, having nineteen hundred cases in this susceptible group of a million and a half--they weren't all susceptible--the accomplishment of cutting that infection down to that size in that short time was something the reporter ought to have hailed with fanfare, but he wanted thousands and thousands of cases to make it worthwhile to write it up.

Typhus control in Naples was continued largely with dusting DDT from the various machines, power dusters and hand dusters. Naples continued to be a point of collection of supply and for forwarding of material--oh, even up toward the side of Poland. We had some people go up through the Balkans up to Poland and into Austria toward the end of the war.

Well, I think that the cooperation with the British was very harmonious and effective. There was a time when the British typhus group under Colonel Chalke seemed to think that the American typhus group was trying to take too much credit, but Chalke in the end wrote nice polite papers of thanks and so did General Parkinson.

The British American typhus collaboration was dangerously near an unhappy complication in 1943, in November when a proposal was made by Dr. John R. Mote, head of the British Purchasing Commission in Washington, and it came up for discussion at the Typhus Conference in London in November, 1943. He proposed that there be a joint British American Typhus Commission with powers and privileges and obligations more or less as outlined in this Executive Order

9285. General Simmons and I opposed it in our talks about it in Washington, and I was sent over as a delegate, or at least a representative of the Surgeon General, on the one hand, and as Director of the Typhus Commission on the other to attend that conference in London in November. It was apparent that the British authorities didn't want a joint commission really, and they saw that we didn't want it either, and they dropped it. Had it gone through, I'm sure that there would have been much trouble. I'm sure there would have been ineffective rivalry and ineffective fights for dominance had the British made a joint arrangement with us. We had all the material, all the productive capacity and machinery, and more people available, but the British--this man Dr. Mote, representing what he thought was a British opinion, would have desired control of all these resources, I'm sure.

The outbreak of typhus in Naples was the most serious outbreak in modern times for the American Army in Europe and North Africa, and the next ones occurred in Japan in 1945.

This had to do largely with concern with the civilian population which apparently didn't meet the test of the Army commanders who were thinking in limited terms, didn't understand the nature of preventive medicine a lot of times.

We had no typhus to speak of in the American Army--103 cases, I think, and no deaths at all, whereas the British in Algiers had more than a hundred cases and about twenty deaths. They didn't have as good a vaccine. At that time our troops were not lousy, and we were thoroughly immunized with the vaccine. It is hard to get line officers to see in a quiet moment that the environmental civilians can represent a considerable menace to their campaigns.

They ought to have learned that from maneuvers in the United States. There were big maneuvers in 1940, in Wisconsin and in Louisiana, in Texas, larger ones even in 1941, and in each of those maneuvers areas, the extra military area sanitation became very important, and the system was built up by which the Army medical establishment cooperated with the state establishments and the Public Health Service. They actually set up three cornered directorates, so to speak--state, Public Health Service and Army medical inspectors as a rule under a surgeon. Now, the chief diseases that these groups combatted were venereal disease in extramilitary areas and malaria. In the system of combatting those diseases among the populations in the environment surrounding military posts, or in areas that were occupied by maneuvering troops--in those situations there were the same elements that were appearing again in the typhus control of civilians. That's why I say the line officer ought to have had some appreciation of the importance of sanitation control in civilians because they represent a source of infection of troops.

Well, you have to explain that all the time.

This opens up a whole area that I want to come to--part of the development out of this is a whole educational program, publication program, sign painting programs--endless to try to influence awareness--whether it's in the South Pacific, or wherever it is. This is a development, that the need for instruction is part of Preventive Medicine.

General Simmons in 1944, when he got his organization into this high state of being a Service added to it a Health Education Division which was to educate the civilians and educate the soldiers and the line officers, the enlisted men and the line officers. We were educating them against and trying to tell them about the diseases occuring among the civilians that would be harmful to their military activities, and we published a book about mosquitoes called "Ann" from Anopheles. We published a thing called "Snail Fever" which is the name the soldiers gave to Schistosomiasis Japonica. We put out a lot of audio visual aids which in my opinion were not too effective. They were cartoonish and on a low level. My own feeling is that you can talk straighter to people. Don't you think so?

Yes. Remember the opening lines of the film on venereal disease--I guess I saw that about nine times the first month, "Most men know more about their automobile than they do their own bodies." In that Fort Eustis heat in a big theater--that was some picture to see. That was hitting straight, I thought. The need for this is a continuing thing. There is nothing inconsistent with this Executive Order and continuing Army Regulations at all. This is a broader base on which to operate than the Army Regulations.

The Executive Order never was interpreted to weaken, or detract from the medical service sanitary arrangements that were required by Army Regulations and by the stated mission of the Medical Department, but the Executive Order 9285 does something that the Army Regulations couldn't possibly do. It tells the Public Health Service, and it tells the Navy to join with the Army in this undertaking to control typhus, study, control, prevent typhus wherever it is, or may be a threat to the troops, and when that Executive Order was written we knew nothing about scrub typhus as a disease. Scrub typhus just came into the field because of its last name, and it was far more important as a cause of sickness, disability, and death than epidemic typhus. When we took in scrub typhus, no one stopped to ask whether the Executive Order applied or not.

Yes, and I think there was a full blown bona fide request from the field which was loud and clear in terms of their experience.

From Australia--from MacArthur. That looked like a full blown request, but it was prompted. The thing which is done routinely in a case like that, and I think that I could get the telegram--you learn in Preventive Medicine about the outbreak of a disease, and you immediately get in touch with the commanding general and say, "We can help you. Ready to send so and so", but you cannot send these people into a theater unless the commanding general asks for it, so his reply is not a reply of obedience to a warning flag that is hung out before him, but his reply is couched in the terms of a commanding general. He says, "Come over."

Had the British been working with dusting powders the way we had?

To some extent. They had some knowledge of DDT; in fact, I think the Geigy people had given some to the British too--yes, they were like anybody that was intelligent enough to see what comes from heat and steam meant--anybody would try to get some chemical poison for lice. It's much simpler to use, and of course the spraying of chemical poisons on arthropods, insects is old. Paris Green was sprayed to kill mosquito larvae a long time ago.

They didn't have anything comparable to the Board that you had. They had a Typhus Commission, but how about their scientific approach to their troops? Did they have laboratories in the field?

Yes, they had laboratories in the field, but not as extensive as the American laboratory system that General Simmons built up. A good example of a laboratory in the field is the big laboratory that was set up at Salisbury, England, by Dr. John E. Gordon before we got in the war. This was the Harvard Unit sent over at the request of Sir Wilson Jameson who was a very fine Chief Medical Officer in the Ministry of Health of Great Britain. They had this big laboratory at Salisbury which became General Laboratory #1 for the Americans when Normandy was coming on. The British field experimental work was very early, for example, in typhoid fever. Their work on typhoid vaccine was done by Sir Almoth Wright in the Boer War. We sent General Frederick F. Russell over to see what it was, and he brought it back to this country. The British were well ahead in their tropical studies of parasitic diseases. Patrick Manson discovered the transmission of filaria in the mosquito, and Ronald Ross discovered the transmission of malaria by anophelene mosquitoes. The romantic stories of--oh, this cattle disease in Africa, kind of a sleeping sickness, Trypanosomiasis. It's called a sleeping sickness. David Bruce knew about the parasite. They knew about the chemicals to fight these flies and other chemicals to fight the insects. The British tropical medicine was far ahead of tropical medicine in this country, but their actual establishments in the field were not as numerous nor as well equipped as ours.

<u>Continuing with this liaison with the British--how was this handled in Australia, China, Burma, India?</u>

In Australia relations were very good, but rather confused because General MacArthur had some medical advisors that were more, I should say, his acquaintances than people picked for their special capacity. He did, however, have on his staff in Australia one of the finest men in the American medical service, and that was Colonel Maurice C. Pincoffs from Maryland. He became the great preventive officer for the Southwest Pacific, especially after they got into New Guinea and up through Manila. The British in Australia cooperated with the Americans very thoroughly in their atabrine studies on the control, or suppression of malarial infection. A very fine man named Brigadier Hamilton Fairley studied the dosage and the time intervals in atabrine and worked with our people thoroughly. In addition when it got around to scrub typhus the great expert on the mite was R. Lewthwaite in Australia, and we had fine relations with him, especially through Colonel C. B. Philip. We had relatively few troops in Australia. There were a good many there in 1942, on their way, but they got off into New Guinea before very long, and the ones that were in Australia were up toward the Northern point opposite Port Moresby in New Guinea. The relations were harmonious, helpful and valuable.

In India and Burma, the relations with the British that I know about concern scrub typhus again. We had a large contingent of the Typhus Commission on the Irrawaddy River where we built a big laboratory and put it under the command of Colonel Mackie. That group studied scrub typhus largely with the aid of Indian authorities and sometimes the British authorities. We had one British officer attached to the commission--at Myitkyina, and we had some relations with Lord Mountbatten, but the India-Burma Theater to start with was a poor little orphan thing. It was the India-China-Burma Theater in the beginning, and then it got separated to two theaters, and it never was properly supported. It was always sort of a motherless child in a way.

<u>I'm almost at the end here, so let me put on some more ammunition.</u>

Wednesday, June 8, 1966 A-60, N. L. M.

We have established the Division of Preventive Medicine. It's in being, and we've added to it the Typhus Commission, and this Executive Order sustaining it. Even earlier than that we set up the Army Epidemiological Board with its functions to continue laboratory work and be on call in the event that something unforeseen happens to which answers are required, or some answer is required. In the course of this, you gave me this map which you're going to use in your book, but it shows certainly the interest, the range and extent of concern in Preventive Medicine. It's pretty far reaching with all lines on this map leading back to Washington, D. C. and headquarters. We've talked about men. I showed you some things I found in General Simmons' folder about manpower. In talking about Naples, the subject of the production of DDT came up, the problem of how to expedite it as of that time. I want to see the office, and more particularly, your office--your concern in Washington--function and how it met three different problems--hepatitis, both here and overseas, and from 1941 through, I think, 1945; scrub typhus which was a brand new problem that came on the scene; and then schistosomiasis, the tropical disease which also occurred. I want to see how one organizes to meet these problems, what he has at his command, what the limitations are in personnel, supply, whatever, and I think these matters are seen best from where you were sitting in Washington.

Well, you have to have this centered in Washington when you're talking about the Preventive Medicine Service of the Office of the Surgeon General because that's where it was physically located, and in that central place, I'm there as the Deputy of the Chief of Preventive Medicine, General Simmons, with whom I have had a long time, perfectly free and easy association, where you can talk about anything that you might want to talk about. General Simmons had a ranging mind and a vision, and he actually foresaw a great many of the problems that were coming up. There were new problems that were appearing that he understood right from the first emergence of the problem. An example of that I can give you is the sudden development of the Occupational Hygiene Industrial Health program in the Surgeon General's Office. Industrial Hygiene was almost as big--well, quite as big as the Typhus Commission in a sense, although it was limited largely to Washington, but the program that General Simmons developed for Industrial Hygiene under Colonel Lanza took in the health, care, and supervision of, I suppose, more than a million workers in the defense plants, the chemical plants and all the plants that were engaged in war production.

Now that ordinarily had not been in Preventive Medicine before, so what do you do when you meet a situation like that? You get a man who knows something about industrial hygiene, and that was Colonel Anthony J Lanza, brought in from New York University where he was a professor in this very subject. Inspite of being rather deaf, he was an outgoing, aggressive person who soon made his way with the higher officers of the War Department. He was taken into confidence by them, and in a few months he had the responsibility for the health care of all the workers employed in contractor owned and operated plants and government owned and operated plants. That program extended very far outside of the production line because you can regard soldiering as an occupation. Some peculiar hygienic aspects of a soldier's life come under what the Industrial Hygiene Division had to deal with. For example, toxicology was very important in these plants that were making munitions and poisonous gases and poisonous compounds--well, the soldiers were exposed to the same thing. The soldier cleans his rifle with carbon tetrachloride. It will knock the devil out of his liver, if he isn't careful, so toxicology goes over from soldiering into civilian industrial hygiene without our being bothered by the transition at all because it has an intellectual continuity.

The same thing applied, we'll say, to the relation between the Quartermaster, the Chemical Corps, and our Preventive Medicine Service on the impregnation of clothing. Clothing, tentage--these materials were impregnated with compounds to make them waterproof, or to make them more ventilated, or to keep insects away, or sometimes to have an antidotal effect on surrounding noxious agents. Now, impregnation--the substances that you put into clothing to impregnate them are often things to which the human body is allergic. Skin eruptions break out--irritations of the skin. You have to do thousands of tests on normal individuals before you can O.K. a piece of impregnated clothing for a man to wear. That fell into the Industrial Hygiene Laboratory which was established by Colonel Lanza, first at Walter Reed and then it moved over to Baltimore. Now I've gone away from these subjects that you spoke about because this industrial hygiene health program might not be visible in its scope, might not be visible from the charts you have, and unless you knew what I'm telling you about toxicology, about the impregnation of clothing, you wouldn't see the ramifications of that to all sorts of phases of medicine, of Army administration, and of Army production and utilization of materials.

Well, you could do the same thing if you looked at the other Divisions of the Surgeon General's Office, or the Preventive Medicine Office. For example, the Laboratories Division had under its control not only the supervision of laboratories, but actually the mission for which they were constructed. There were huge Army laboratories. There were Corps Area laboratories. There were hospital laboratories. There were diagnostic laboratories.

There were test laboratories. There were laboratories for all sorts of purposes which fell into General Simmons' ideal that he derived from Sternberg. His own book is dedicated to Sternberg, to the great man who established laboratories in the Army, and hundreds of other Army officers are indebted to him too for the standards he set and the plans. The Laboratories Division like some of the other divisions--they had a large personnel office. Colonel Elliott S. Robinson and the other officers travelled all around the country, get names, records of people, had a big file on possible laboratory officers.

Now, I'm mentioning these activities because, as I say, they don't show up in the charts very well, but they're very like--they don't differ in spirit, or in activity; they don't differ much from the Commissions of the Army Epidemiological Board. They do both things. They go out into the field to deal with a situation. They bring the material and the thoughts back to their offices, and even some of it is brought back to laboratories like Walter Reed Medical Center, or the Army Industrial Hygiene Laboratory, or the Armored Medical Research Laboratory. All these are comparable to University Laboratories. Now, these things you might have mentioned, just as you did the hepatitis, or the schistosomiasis.

You want to get at how these things originate and start to cause a stir. You have to appreciate the fact that at least our section--or I'll say, at least the Surgeon General's Medical Department with its Divisions is more close to what is happening all through the Army, closer to events happening all through the Army than, for instance, the Artillery, or some Signal Corps. They don't have the same human relations that medicine does, nor do they have the same conversational relations--as I might say it. Take our office, the Preventive Medicine Office. Right from the start, telephonic and telegraphic communication was authorized so that the heads of these divisions, Epidemiology Division, Laboratory Division, Industrial Hygiene Division, Sanitation Division--these heads were talking all the time with the people in the main camps in the country and often overseas, but I'm limiting it to the United States at the moment, so that the Preventive Medicine organization in the Surgeon General's Office was never isolated from the field, or from the activities of sanitarians, or from preventive medicine officers out in units of the Army.

It seemed very natural, for instance--and I'll go to hepatitis now--that we should know about what was happening as soon as it happened. Hepatitis started early in March, 1942, to be noticed because surgeons at posts, medical officers in the field called in and said, "I've got a strange lot of jaundice out here. It seems peculiar to me, but there's so many of them, and they're coming right along. What is it?"

We didn't know what it was at that time. Call it jaundice for the moment before we try to differentiate. There was a well known disease called catarrhal jaundice, and that's the name that goes over into infectious hepatitis, naturally occurring infectious hepatitis. Catarrhal jaundice got its name given to it by a great German pathologist, Rudolf Virchow. He thought that a mucous condition of the bile duct took place and backed up the bile and caused jaundice. That's what everybody thought this was at first, although some people knew enough about the literature to know that jaundice had been associated WITH artificial--I was going to say infection, but I'm not sure; I'm going to say infection at the moment--infection coming from the use of syringes. People knew about salversan jaundice. If you use the same syringe to inject salversan in a series of individuals, there were well known little outbreaks of jaundice among these people for whom the same syringe was used because a little bit of blood stayed in the base where the needle is. There also was known to a good many people, but not as clearly as it should have been, that in Africa a number of British workers--maybe five or ten years before this war, or five years anyhow--had noticed the occurrence of hepatitis and jaundice in people who received yellow fever vaccine that contained human serum, that puts human serum in the vaccine. The same observations were made on some convalescent serum jaundice infection when used to prevent measles or mumps. These things were known.

It was known, I think clearly to General Simmons--it was not known to me at the start--that it was possible that jaundice would be associated with injections of human blood into human beings. It was known to occur after blood transfusions. That was called homologous serum jaundice. The confusion at the first was that it wasn't realized in the first few days by enough people that two diseases, two jaundice producing diseases were taking place at the same time--naturally occurring infectious hepatitis was occurring, and then this post vaccinal yellow fever jaundice, vaccine induced jaundice, was occurring.

As I say, the reports began to come in early in March [1942]--it was probably by the first week, or ten days, in March, and we were getting reports over the phone and telegraphs of as many as a hundred cases in a day which is very extraordinary for infectious hepatitis. It came--I'm trying to answer your question as to how you get started on this in Washington--just as naturally as if you were talking to the man in California as if he were in the room with you, so to speak. You get it very quick. It was obvious that we had a very serious situation arising. General Simmons and the rest of us naturally followed the proper course--to attempt to arrive at the cause before you knew what to do to prevent it. To arrive at the cause there were obviously two ways of doing it, and one was to accept the hunch of a very wise man like Karl Meyer who from the start, when he first saw these occurrances of jaundice in California, said that it was related to the vaccination against yellow fever.

The vaccination against yellow fever had been started in, I think, November of 1941, somewhere along there, with Air Force people. General Simmons got approval for this vaccination, recommended by the National Research Council, approved by the Surgeon General, and approved by the Adjutant General also for administration. The source of the vaccine was the International Division Laboratories of the Rockefeller Foundation where for about five years Max Theiler and others had been making a yellow fever virus vaccine from an attenuated yellow fever virus strain called the 17-D virus. This vaccine that they made was a Berkefeld filtrate of an extract of the chicken embryo on which the virus had been grown, filtered, stabilized--as they thought--by the addition of a small amount of supposedly inactivated human serum that had been heated at 56° C degrees for half an hour, or an hour. It didn't inactivate the virus. It's heat resistant.

Immediately General Simmons began to ask the members of the Army Epidemiological Board to help investigate this situation. There's nothing new in that principle. That was what you would do. That was what the Board was created for--to be ready, so he got--well, one of the first groups he called up was Dr. Thomas Francis, the Commission on Influenza in Michigan. Francis was available and could get out. He got a good epidemiologist, Dr. Kenneth F. Maxcy from Hopkins, to join Francis, and then he appealed to Dr. Wilbur A. Sawyer as chairman of the Commission on Tropical Diseases to go out to California and start the investigation. The Commission on Tropical

Diseases was brought in not because this was thought to be especially a problem in tropical medicine, but because Sawyer was the head of the International Health Division and knew the vaccine problem from the beginning.

I imagine that perhaps he and General Simmons had had some conversations about this possibility even before it happened. I don't know that for a fact. I've been told that, but it seems natural that as the International Health Division had known about post-vaccinal jaundice occurring with their work on yellow fever in Brazil--Dr. Soper had made reports on it--there was some knowledge of the possibility. It was regarded as a remote possibility because, as I say, the serum had been heated, and we thought we were dealing with an innocuous stuff.

Well, the epidemiological problem to be worked out was very difficult because, as I say, these two diseases were running along at the same time. One very sharp differentiation I noticed at the start--the incubation period of the naturally occurring hepatitis was about twenty-three to twenty-five days, or thirty days--somewhere in there. The incubation period from the time the vaccine was given until the time the hepatitis appeared was usually ninety days. We didn't know that until we were just beginning to collect the information. Fortunately they did start to collect the information very early as to what lot numbers of vaccine these soldiers had been injected with, and it appeared very soon that certain lot numbers were what we called icterogenic.

There was great anxiety because many of the health officers in the country thought that it was actually yellow fever. Some of our advisers on the Epidemiological Board thought it was yellow fever and were frightened. In the Surgeon General's Office, and in Preventive Medicine, it was not known at first as to whether it was yellow fever. Very few people had seen yellow fever in this country--hardly anybody. Somehow or other the situation got settled in my office in the Preventive Medicine Service, and I became almost totally absorbed in this hepatitis problem for the next six months or more. We collected a great deal of data which has been used in some of these splendid reports like the Persis Putnam report, and there are others. By the end of March, 1942, certainly early in April, I who had thought the disease was catarrhal jaundice believed that it was related somehow to the yellow fever vaccine. I'm sure that General Simmons got that point of view about the same time. I knew that Dr. Francis and Dr. Maxcy after having first rather suspected that yellow fever was being produced, noticed that it was possibly more related to the vaccine. They were studying a fine group to bring that out. They were studying the Air Force people who had been vaccinated with yellow fever vaccine at Jefferson Barracks. Everywhere these groups went--Chanute field, or up into an airfield in Maine, or somewhere else, they all got jaundice about the same time, and that pointed to something from their origin.

By the 15th of April, the decision had been made to discontinue the Rockefeller International Health Division vaccine and get vaccine from the Public Health Service. Thanks to a recommendation of the National Research Council in 1941, the Public Health Service at the Rocky Mountain Laboratories had set out to equip itself and get a capacity for, a capability for making yellow fever vaccine both as a reserve in case of an emergency and as a means of supplying vaccine to the civilian populations in yellow fever areas, if necessary, like the Virgin Islands, or somewhere. They had a serum vaccine at the start, and they had a little trouble with it. The Public Health Service vaccination in the Virgin Islands was associated with hepatitis. It got very clear from studies in the literature and consultations that we had with everybody that knew anything about it, that the serum was probably the noxious agent in the vaccine. It seemed wise to get rid of it, so we persuaded the

Public Health Service to make what we called aqueous vaccine. They put aqueous salt solution in place of the serum, so by the end of April, we had an aqueous vaccine from the Public Health Service, and it was used thereafter all the time in the Army.

The Rockefeller group changed to an aqueous vaccine also. There were hardly any new cases after April 15th attributable to the vaccine, except those that were already on their way to come down sick--except one group who had not either gotten the word, or disregarded an order from the Surgeon General's Office. We called in all the lots of yellow fever vaccine that were all around in different medical supply depots, in hospitals, in all sorts of places--dispensaries, and we thought we got it all in. I got a letter one day from a surgeon in Alaska, and he said, "I think I'm going to have outbreaks of jaundice here in August."

It's on one of these charts, isn't it?

Yes.

He said that for some reason he hadn't turned in the vaccine that he had and had given it. He dated when the outbreaks would start, and it did start--I think it's illustrated in the chart in that article.

The problem was solved by the epidemiologists in various ways. At first the Commission on Tropical Diseases was not too helpful--well, you can understand that when it is headed by a man who is responsible for the great organization that makes the vaccine, he might tend just naturally, as he did, to emphasize the importance of the element of naturally occurring hepatitis in this situation, while a man like Karl Meyer went straight to the vaccine and followed it right through. There was no way--well, there were no serological tests for it. You could test for liver function, and the tests for liver function were improved by the need for them in all the material that was available, but that test was a non-specific thing. It showed that the liver was damaged, but it didn't show whether it was damaged from naturally occurring hepatitis, or vaccine. Nothing could be seen. No organisms could be seen, nor cultured from the blood, or from the tissues, and no animal was found to be susceptible either to the post vaccinal jaundice, or plasma infusions, or the naturally occurring hepatitis.

It was necessary to use human beings volunteering for tests. This was done with great enlightenment because it showed that this material was filterable, that it was highly resistant to heat and chlorination, that the agent, or whatever it was, occurred in contaminated water supplies as in that water supply at Akiba, in Pennsylvania that Dr. Joseph Stokes studied which was water polluted from a nearby privy. Further, the experimental work on human beings showed that the disease could be produced by putting a very small amount of fecal material in a glass of milk, or something like that. You'd get the disease. Much scientific information came out of this work, but as yet they have not yet proved that it is a virus disease. They are supposed to have found a virus in recent years, but the work has not yet been confirmed, so it's still a mystery.

The number of cases that occurred in the Army will never be known. I think my chart had about twenty thousand, or twenty-two thousand cases on it in this period, but there must have been ten times that many. I've been to posts where everybody was sick at the same time. They had a little pain in the joints of their hands, or swelling lips and face, or a little fever, but no jaundice. It was more shocking to the commander of the Army, I think, than any bombardment at the moment that was going on because so many people were sick, and there were so many critical things in the offing and going forward.

For example, the Battle of Midway was won by air pilots some of whom were suffering from hepatitis, jaundice, and great secrecy was imposed on all working in this situation.

As I pointed out to you, we were not able to tell Dr. Gordon, Preventive Medicine Officer, European Theater of Operations, that the Second Armored Division elements that were being sent over to North Ireland, contained people who had been vaccinated with yellow fever vaccine and who were coming down with the jaundice. They did have it. It frightened the British at first, but they--apparently Sir Arthur S. McNalty knew the African work of the British on jaundice following yellow fever vaccination and Dr. Gordon, very intelligent, worked out the epidemiology of the association.

The other phase of it that nobody knew anything about at the start was that it was not transmissible from one patient to another. I never saw a secondary case. That was a comfort. In Dr. Gordon's big *History of Preventive Medicine in the European Theater*, it's indicated that when the British people saw that this was post vaccinal jaundice, they just passed it up and said that they weren't afraid of an epidemic starting. No epidemic has ever started from it. There were no secondary cases, as far as I know. Well, fortunately it ended.

How did it differ from the later naturally occurring hepatitis?

Hardly at all. It differs in the incubation period. The incubation period is longer. You couldn't tell the damaged livers one from the others. The livers were little damaged in some cases, terrifically damaged in others. The regenerative process goes along with the necrosis. The length of the sickness is very variable. In both cases there are very light cases, and in both instances there are very prolonged sicknesses with nerve degeneration, brain damage, and all sorts of prolonged illnesses. They were studied in the 15th Air Force a good deal in Italy, and it was shown, as it was in North Africa, that the normally occurring jaundice--infectious hepatitis that I'm talking about--is responsible for incapacitating for a lengthened hospital stay that far exceeds anything that venereal disease was producing--four or five times as much, but very much fewer cases. The naturally occurring infectious hepatitis became quite prominent in North Africa in 1942, and 1943, and stayed so all through the war. It was a seasonal affair. It came up mostly in summer, late summer and fall. It occurred in Italy in the fall and winter. At the same time, as we know now, it was occurring the same way in Denmark--it's a world-wide affair, I'm sure, though I don't know about the Asian side very much.

Subsequently 1942, 1943, North Africa, I think, and 1944 in Italy--what was the nature of communication? Was it as immediate? Did you have the same contact with field officers that you had with base operation people here in this country?

No, there wasn't. There was close communication, or easy communication between the central theater organizations and their field installations. Dr. Gordon knew everything all the time, but we didn't know what was going on all the time in the European Theater because, as I have pointed out, the Surgeon General lost his staff status. He was under A. S. F. He had a block there. He lost the authority in many cases to appoint medical officers in the Corps Areas. He had trouble in getting acceptance, or even notice of the material that he was sending abroad for information, and so to improve communications they arranged for the Essential Technical Medical Data [ETMD] letters. Did I put that on the tape last time?

These are ETMD. It was a report to the Surgeon General from the commanding general of a theater of operations written, of course, by the theater surgeon and his assistants, but coming through the commanding general without the surgeon's name, signed for the commanding general by the adjutant and also beginning "In accordance with" some number of the Adjutant General of the War Department, let's say, Letter AG 350.05 (28 Dec 42) OB-S-SPOPH-M, "this report is submitted." They were voluminius reports. They had sections on sanitation, on hygiene, on preventive medicine, surgery, medicine, supply. They were books almost--once a month. That didn't happen--well, I think ETMDs came about in 1944 probably. They made up for this lack of communication, but it was officially nothing like as easy as the communication between the Surgeon General's Office and the people in the field in the United States.

On the other hand, voluminous personal letters passed between the Surgeon General and the Theater Chief Surgeon. When I speak of the Surgeon General, I'm taking the embodiment in the Surgeon General of all the chiefs of services. For instance, the Consultant in Surgery and the Consultant in Medicine, General Rankin and General Morgan, traveled all around to the theaters and saw what the surgery was like, saw what medicine was like, had conferences with the surgeons in the field and with medical men. I knew General Simmons had a great many letters from different places, and he himself made long trips--I think he covered every theater.

<u>I was thinking of Dr. Stokes who went abroad to the 15th Air Force to pick up samples and bring them back.</u>

Joe Stokes?

<u>Yes.</u>

Well, good work was being done in that area by the very fine 15th General Medical Laboratory under Dr. Ross L. Gauld who is now in the Walter Reed Army Institute of Research--splendid work. Also they worked on Q fever. Well, on the same subject in answer to your question--hepatitis, schistosomiasis--it doesn't differ. It didn't differ in my opinion from what went on in the Dean's Office. You're talked to by anybody who has anything to say. You hear things. You hear things at dinner parties. You hear things in the latrines.

<u>Everywhere.</u>

Everywhere, but you get a sense about what may be important, I suppose, and what might not be important. Do you want to go on with schistosomiasis?

<u>What you said to me off the tape some days ago was the fact that the rubrics that you had, whether it was the Typhus Commission, or the Army Epidemiological Board, meant something, but when a specific thing like hepatitis came up, where you had the possibility either to examine something in the field, or bring it back to the laboratory, the rubric didn't mean anything. You could take--what you knew had been going on with continuity, like Dr. Stoke's studies in Philadelphia, and the chance happening at that Camp.</u>

The Akiba Camp.

<u>Yes. He could right away check his local results against something that was occurring in the field and also study it epidemiologically.</u>

Yes, Stokes was the Director of the Commission on Measles and Mumps, but he did his best work on hepatitis.

Right.

Dr. John Paul became a great authority on hepatitis. He was chairman of the Commission on Neurotropic Virus Diseases, and somehow or other he got out in the Middle East with General Crawford F. Sams and the Army out there, studying sandfly fever. Sandfly fever is caused by a little bit of a gnat called a Phlebotomus papatasii. They were working on studying this little gnat on the banks of the Dead Sea and out in the Negev Region, various places. They even got a little hospital section for it, but in the course of that, John Paul was testing convalescent serum from people who had had sandfly fever, doing neutralization tests, and he gave himself homologous serum hepatitis. Thereafter, he did extremely good work on hepatitis, but that was the Commission on Neurotropic Virus Diseases. Of course, his sandfly fever work extended in just the way you've outlined to a very large undertaking without any inhibition because of the names but rather a great boost because of the knowledge of the capacities of the people and their interests.

In Sicily it looked as if for a while they had the largest outbreak of malaria that the Army had witnessed, or suffered from. Thousands of cases were sent down to hospitals with fever of unknown origin, small fevers, intermittent fevers, peculiar fevers, and they were all diagnosed "malaria" after a while. One of the doctors thought that they'd found malaria parasites in the blood, but they were probably making a mistake of misinterpreting the appearance of blood platelets riding on a blood cell. That went on for several weeks, and all of a sudden malaria stopped in Sicily, and we could tell from the reports coming in that an epidemic of sandfly fever had come on. Well, now, in Preventive Medicine in the Army you have two kinds of epidemics. One is an administrative epidemic, and the other is a really naturally occurring thing. By an "administrative epidemic" I mean the sudden change in term and classification. Some people will have common colds. All of a sudden the officers in charge will decide that that's influenza, and you get a report of two or three hundred cases of influenza breaking out, whereas up to that time they had just been common colds and the sniffles.

The Navy had the same thing. They have a thing called cat fever which is catarrhal fever, and every now and then somebody decides that it is probably influenza, or it might be one of these arboviruses, or some kind of viruses. They suddenly change the name, and you've got a new epidemic. That was a little bit the explanation of the Sicilian situation. When they discovered that it was sandfly fever and not malaria, malaria disappeared practically. The people who worked that out were three people mostly--John Paul coming out of the Middle East on sandfly fever, and he had with him Lt. Colonel Cornelius B. Philip, who became one of the chief men on scrub typhus in New Guinea and through the Philippines and who wrote that superb article in here, and the other person who was sent in there was a remarkable man, Albert B. Sabin. Albert Sabin was on the Commission on Neurotropic Viruses Diseases, but he got interested in sandfly fever as an offshoot from Dr. Paul's work. Sandfly fever didn't really have much of a neurotropic significance. It sounds very loose, I'm sure, to you, but it's practical. It works.

<u>I know it works. That's the beauty of it, but sitting here in Washington and reading these statistics, you have to have some sense as to where to put your emphasis--how to read them.</u>

Yes--of course, we all start with a fair amount of training, or experience. Simmons had been a great deal in the tropics and around the world, and I had been around a bit, and most of these people are very intelligent, able people, and they think things through.

<u>Contrast this with the scrub typhus that came up--that particular problem--you knew, as seen from Washington as it first emerged because it was new, or it turned out to be new.</u>

The scrub typhus began with a bang. It began on Good Enough Island in Milne Bay off the western coast of New Guinea near Dobadura. On Good Enough Island, a hospital moved in there, and instead of going into an area where troops had been and which was burned off, that hospital moved into fresh kunai grass. Kunai grass is the home of the mite--this little beast that carries the rickettsia--Tsutsugamushi, they call it. I'll bet you can't spell that!

<u>TS--all right.</u>

Well, that first outbreak--excuse me, this hospital had the first outbreak over there and, as I say, they moved into this virgin grass lands, so to speak. It hadn't been burned over, and it hadn't been cut. It was full of vigorous mites, and that was probably the most fatal outbreak of scrub typhus that we had. Some outbreaks of scrub typhus had thousands of cases and only a couple of deaths. This Good Enough Island had about twenty-five cases and maybe ten deaths. One of these people who died was a protege, or a relative, a psychiatrist, who was a relative of a Congressman, and that aroused the Congressman very much--why should a psychiatrist be sent into the kunai grass on the firing line like that? Well, he wasn't aware of the fact that psychiatry like surgery was moving right up to the front. They were rehabilitating men on the battle field almost which is good practice. That was the first stir.

As soon as we began to look into it, we began to get reports that put you wise to the conditions--Australians had known something about it and had a bit in the literature, and one of the great experts on mites, Dr. R. Lewthwaite was from Australia. About the same time, as I remember it, we got a report of a British Division which had gone ashore on Ceylon for a rest, and they had a bad outbreak of scrub typhus there. Scrub typhus had been known in the rubber plantations. For some time it had been known--it had occurred in the region around Formosa, in the islands between Formosa and China. As I say, it had this long name--Tsutsugamushi disease. The Japanese called it that. They discovered the rickettsia in it and the mite transmission probably, but not too much was known about it at that time.

It caused a great fright too, because the bite of the mite is a little bit itchy at first. The itch mites are worse, but they don't cause scrub typhus. The scrub typhus injection by the mite soon turns into what they call an eschar. It gets a little black necrotic area on the skin about the size of the end of your thumb, and when the soldiers saw that happen they thought that they had the finger of death on them and a lot of them did die--not too many died, but that frightened them very much. You can see the pictures. This is in 1942, when Good Enough happened, I think.

In 1943, after talking with General Simmons, it seemed that we should send a commission out there, if General MacArthur would accept them, to study the disease on the ground, and it wasn't occurring anywhere else. We had no idea what the extent of it would be, so we persuaded Dr. Blake and Dr. Maxcy to head up a joint commission of the Typhus Commission and the Army Epidemiological Board to go out--well, first landing in Australia and then on up to New Guinea to investigate this disease. They did that. They made superb investigations, isolated the organism in animals, brought these animals all the way back to this country; in fact, studying these far away diseases which could be carried back to the laboratory only in an animal made an infected monkey, or a mouse, one of the best passports for a quick flight across the ocean, as you can imagine. Lots of officers escorted monkeys and mice back. That was a valuable thing because it gave opportunity to study the disease in the laboratory in this country. Fortunately the Department of Agriculture and the United States Public Health Service were very liberal in granting permission for bringing infectious material through quarantine to be studied for the benefit of the war effort. Some of these things they didn't want to get in this country.

Well, scrub typhus extended through the jungles of New Guinea. We had to deal with it all the way up. When Dr. Blake, Dr. Maxcy, and Colonel Sadusk returned, they left over there Major Glen M. Kohls and several others, including Cornelius B. Philip and Raymond C. Bushland, who studied the problem mostly from a preventive aspect. They devised an impregnation of clothing with dimethylphthalate, a soap chemical mixture, and taught the soldiers how to dip their clothes in this solution and impregnate their clothes in the field. The trouble with that was that it rained most of the time in New Guinea, and the stuff leaked out of the clothing pretty fast, but it made the men conscious of it.

They also began to understand the value of burning off this kunai grass in certain areas. We soon noticed a very interesting thing about scrub typhus. It occurred in patches. There would be an area about, say, half an acre or so from which cases would be coming and adjacent areas where no cases would be coming. The reason undoubtedly is that the rickettsia of scrub typhus is transovarially transmitted in the mites. It goes through the egg, so a female laying eggs would lay the rickettsia, and that would develop into the next generation and so forth. It's just like patches of the Japanese Beetle.

They occur in patches where they live. If the mites were infected, the patches were infected. Scrub typhus occurred in Burma, and I have already said that it occurred in Ceylon, and some of these people came up from Ceylon into Burma. It occurred a good deal in the Ledo Road which had been built, put through by General Joseph W. Stilwell. The Typhus Commission studied the disease in that area and helped to control it locally. We put up a very fine station laboratory on the Brahmaputra River, near Myitkyina. We had a little to do in China, but nothing of any concern with scrub typhus. There was nothing, as I recall, in Formosa. Scrub typhus did occur occasionally in the Philippine Islands--Mindanao right on up to Luzon and, of course, we were afraid that if we were going to invade Japan, we would have to meet it again.

It was so clearly variable. In some outbreaks the mortality would be five percent, and in other outbreaks the mortality would be less than one percent. At Sansapor, near what is called the Vogelkop in New Guinea, there were probably two thousand cases. As soon as these troops got ashore in an old abandoned plantation, scrub typhus practically stopped the war for them for a while, but they didn't die from it. They were incapacitated. The same on the Island of Biak. They not only had the Japs to contend with, but this disease also.

Much has been learned from these investigations. It resulted finally that people who had experience in this disease went back and worked on it in Malaya, and they found--this is after the war, but it was based on the war experience--an antibiotic, chloramphenicol that stops it. It's good. Chloramphenicol is also a cure for typhoid fever. Like so many things that the Army was doing, the results went right over into civilian medical practice and public health. None of this was held secret, except for the stations and numbers of troops involved, something like that.

On the basis of what you discovered you enter into a period of training, education, directives--I don't like the word, but circulars which would indicate what to do.

Yes, and training not only medical personnel, but line officers as well. In fact, the line officers took change of Bushland's impregnation methods very quickly.

New we come to an old friend of yours--schistosomiasis. The North African campaign--very little.

No there wasn't. There was hardly any schistosomiasis in Algiers and Morocco. Schistosomiasis is largely in Egypt, in the delta of the Nile, and that's largely caused by Schistosoma mansoni which is a horrible disease. This is a fluke, or flat worm for the female, and the male is like a little thread. In the human body they live in the veins around the lower part of the intestines, for example, and the flat female forms a nesting place for the thread-like male, and he spends his life in what's called a gynecopheric canal. She carries him around in this embrace. In the Philippines where we first met schistosomiasis, it was due to a fluke of the same family, but of a different genus called Schistosoma japanica, and then there's still another variety, Schistosoma haematobium--maybe more than that, though japanica and mansoni are the chief ones.

The organism, the worm, the fluke has an intermediate host--a snail. It lives in little bits of snails called Oncomelania, very small snails that are attached to the reeds along fresh water streams, river banks, lake banks and pools. At a certain stage the parasite migrates out of the snail--and this is a little, invisible thing--swims in the water and bores through the skin of a man who happens to be in the water. You'd think that it would be easy to control--just keep people out of the water, but they needed a bath over there. You can well imagine that in that hot sweating place. You couldn't keep them out of the water. In addition, where the outbreaks started to be bad was on Leyte when the Engineers were sent in to repair bridges over fresh water streams. They just waded right in the streams, and a lot of them got schistosomiasis. In addition, some of our doctors would be on a sand spit and didn't want to get wet with salt water, but they could see, as in the case of so many of these sand spits, there would be fresh water trickling behind them, and they'd go into the fresh water and they got infected.

This was a serious disease. It was studied by two groups of people. One was a straight forward laboratory survey group that came out of the Army itself--no relation to the Board. At the same time the Army Epidemiological Board, the Commission on Tropical Diseases organized a special subcommission on Schistosomiasis, headed up by Dr. [Ernest Carroll] Faust who was head of tropical medicine at Tulane University, and I'll have to look in that book because his name is gone again.

<u>Laurence, isn't it?</u>

No. It's right on the first page. I underlined it somewhere. Once this is found, let's try to remember it--Willard H. Wright, Colonel Willard Wright. Sounds like Laurence, doesn't it.

<u>I thought you meant Faust.</u>

Wright took over from Faust when Faust came back. Both these groups did a lot of good work, and they didn't meet enough to tangle. Although the A. U. S. people weren't connected with the Commission, they didn't raise any particular criticism as to what the Commission of the Board was doing, and the Board

Commission had all the backing we could give it from Washington--not only in the personnel and equipment that was sent out with them and the equipment that was renewed for them, but for their work on snail poisons. Trying to get rid of these snails in the streams, we sent them what's called a malicologist. He's an expert on snails. Then they wanted a great many expensive compounds that would be made for us by du Pont, or anybody that knew, or had some chemistry along the lines we were interested in. About once or twice a week, or more, I would send twenty-five pounds of some chemical to these people on Leyte, air mail, and that's about two hundred and fifty dollars a shot, but the war was on, and there were no questions asked. They did find snail poisons, and they're stull looking for better ones.

Were they able to break the cycle?

In certain places, but it's so huge a problem. It would take tons and tons of this material to get the snails out of this fresh water river, say, even in Leyte. The disease occurs in Japan, and Willard Wright was ready and after the occupation of Japan did some studies in the Fujiyama region. That also resulted in a great increase in knowledge of the biology of this worm, the snails, the parasite which is all to the value of tropical medicine for civilians. Also it alerted the people of this country to a danger because these men coming back were excreting the eggs of the worm in their feces. That raised another problem that Preventive Medicine hadn't visualized at the start, but took it in its stride, and that is the sanitation of a railroad bed with troop trains moving over it. Think of the excreta that is dumped on the railroad bed. You can't contain it all in buckets in the cars, and the cars were overcrowded, and when they would be passing over a trestle, or a stream, the stream would be infected. They stopped in stations. We had a great problem.

The book about the sanitation of the railroad bed has not yet been written, I guess. It illustrated the part of another problem that we got into. We got very much concerned toward the end of the war with two groups of people coming to this country--prisoners of war. We had about four hundred thousand, maybe, in this country. What diseases were they bringing in? Then when our own troops returned from malarious regions, or schistosome regions, what would they bring into the country?

I remember one instance when just sitting tight and figuring out the probabilities and acting on them turned out all right, though I wonder why. In Grottaglie Air Base in the bottom of Italy, a very peculiar fever broke out among the troops. It was Q fever, as we know now. We didn't know it at the time. That troops ship bringing these soldiers over to this country had a few sick when they went abroad, but they had maybe two hundred cases on that ship coming in. They all got into New York and Brooklyn and got dispersed before anybody was alert to what they were doing. Here we were confronted with, say, two hundred carriers of some infection going to Maine, to Louisiana--well, we had a conference, a talk as to whether the Preventive Medicine Office should immediately set out to locate all these people and herd them in to some kind of observational detention. We decided that we wouldn't do it, and nothing happened.

The same thing happened during the war when I happened to be in charge. At one time poliomyelitis broke out in a recruit training center in California. Several hundred of the people, young soldiers that had been associated, were being sent on troops trains to Indianapolis, to Chicago, clear across the country. The health officers got worried. There was quite a pressure brought to bear to stop these shipments--even stop trains. The sensible thing was to know that poliomyelitis is not too contagious among some of those age groups, that the peple were scattered, and that it would be very difficult to catch them. We let it alone, and I don't know of any secondary case. Maybe the Lord's with you some time.

How about the treatment of....

Schistosomiasis?

Yes.

They probably try to kill these flat worms that are in there, but once the parasitic worm lies in a blood vessel in the rectal region and around the bladder and excretes these sharply spined eggs--some have a lateral spine and others have a terminal spine--these eggs work their way through the tissue and cause a great deal of inflammation. They all get buried there, and they're practically inaccessible. Sometimes they have big operations to take some of that infected material out. One of the dangers in it is that cancer develops on top of it after some time.

In this particular letter there is indication of a shipment of the host.

Yes. Dr. F. J. Brady who wrote that letter is a malacologist, a very good man. Again, we brought back into this country many of these snails, packed in wet grass, seaweed. Several colonies were built up. Dr. Henry E. Meleney had one in New York, cultivating snails and infections passing from one to another without any harm to the community because you knew how to take care of it, but again, here's another case where military effort and research directly enriches the knowledge of the civilian. I can show you that it isn't half as difficult as it seems to be from an organizational point of view--this is daily conversation.

You knew--you have to know where the people are and have the power to send them someplace, and they have to have the desire to go. You can't awaken interest. It has to be there, and it makes its own.

Bear in mind that this is in the war time. People would do things at their greatest inconvenience to serve their country. It didn't take any persuading. The persuasion that had to be exercised was directed often against the authorities of the institution, not the professor that you wanted. The professor was willing to go, but the dean didn't want him to go because he'd have to find somebody else to run his classes.

What I meant was that this relationship that Preventive Medicine Service had with skill, with able men with interest, didn't direct them. It just opened up opportunity for them which is a different thing, and this is a war time situation--it was like peace time, really. The skilled man could merely pursue an interest.

That was the guiding principle in assembling this Army Epidemiological Board, or the Typhus Commission. Commissions were organized and set up in line with the interest of the chief. As a rule, he assembled his colleagues.

My experience in the Army, particularly in the Infantry, didn't allow for discretion, but here it did. In the field of science where scientists were concerned, it was action as a function of interest on their part, and you had to know who--well, just the story you told about Dr. Paul. It just unfolded.

This is different from the construction of the atomic bomb. That was--the people working in that had to work strictly in nuclear physics, mathematics. They couldn't go off on sandfly fever while they were trying to make a bomb

The sandfly fever people could go off into Japanese encephalitis, if they wanted to.

I don't think that is sufficiently reflected--that an Army can be organized with this Preventive Medicine Service to allow for this kind of freedom--that's what you're really dealing with--skill, men, opportunity. I showed you the other day--and this is perhaps on a working level--one of the problems that you confront in looking for people, stand by people for malaria control--ditching, draining and so on. While you had applications, those who are fitted both by training and some experience for it were small. It showed that you needed far more in the way of trained personnel than was available.

That's right. We had huge training programs and screening of the people. Many of those who applied, so to speak, for positions were not capable of carrying on the work. There was always a shortage, particulatly in the malaria control units composed of officers and enlisted men. Both got to be very valuable and desired by the overseas theaters in malaria regions, and it was hard work to find enough and to keep up their training.

Where was the control of malaria centered? I don't find it--who had charge of it?

Preventive Medicine.

That I haven't found in any of the papers. I knew that there were lots of volumes published.

Volume VI.

Well, I don't have Volume VI.

A thousand pages--it's downstairs.

I know that there were a lot of malaria studies going on--all through the war, National Research Council and OSRD.

Yes, the Committee on Medical Research--studying antimalarials, looking for a drug, but malaria control is part of one of the sections of the Preventive Medicine Service. Oliver McCoy was mostly in charge of that. We had a great malariologist with us named Paul Russel, and then he went to North Africa. He also went everywhere--in the Pacific. Sometimes malaria control would be in what was called the Epidemiology Division, sometimes in the Tropical Disease Control Division. That's where it usually was--Tropical Disease Control.

Well, thank you very much.

Are you finished with these books?

Yes, I'll bring them down with me.

Thursday, June 9, 1966 A-60, N. L. M.

Yesterday we talked about three different areas--hepatitis, scrub typhus, and the schistosome problem, and the way in which the Typhus Commission and the Board functioned when you received reports, or information that something was up in the field that needed to be covered, the way in which these matters were expedited irrespective of title where interest was allowed to function--indicated just in the study of Dr. Paul himself, how the free curiosity of a scientific spirit functioned within this overall Army cover, discretion in the field which was preserved. There's another aspect of the scientific community and that's the necessity for the publication of findings. This varied in the National Research Council, OSRD, some of the more prohibitive matters in which they were involved, but to help keep scientists happy, or be the Dean of a medical school, there were certain procedures which you do establish here which allow publication in both areas, both in the Typhus Commission and the Board. I don't know what the considerations were which led to this, or whether there was any problem connected with it. To me, this process is a unique expression--the grafting of a civilian understanding onto the Army. I think it's important and worth a word. Certainly you were inundated with papers--there were a lot here in the files from the Dingle group which I've seen, but more than that, there were scads of publications that came out. How did you handle this when you thought about it before publications began to come in to you?

I didn't have to give much thought to the desirability of scientific publications among the group that were in Preventive Medicine because it had been habitual with them in their university connections and even for General

Simmons in his Army connections. General Simmons was publishing papers about dengue, typhoid, mosquito transmission of disease, malaria, ever since he was a lieutenant in the Army almost. He had an academic sense about publication. The members of the Board and the Commission were used to free publication, the policy of the medical schools to which they belonged. The only difference between publication in civil life in a medical school and those that were desired from scientific work in the Army was the need to preserve security. Obviously there was certain information that could not be reported in the papers that the writer would have desired to have and that had to do with the number of troops involved, their locations, and any plans that would indicate an oncoming campaign, or another move in a campaign. There had to be set up over these publications some supervisory function of the Surgeon General which is not fair to call censorship, but is to be looked upon broadly as an aspect of the protection of security.

In the Surgeon General's Office, or at least the Preventive Medicine Office, the method of handling these manuscripts that were destined for publication was essentially this--the author would write up the work, or a piece of the work as he saw fit. He picked the subject. The subject was not handed to him from the Surgeon General's Office, or the Preventive Medicine Office. He wrote a paper as he would if he he had been in a faculty of a school when his research reached a point where it was desirable to publish it. He thought it was desirable to publish it for the usual motive that influenced the investigator in the direction of publication; namely, the desire to make new knowledge available for the advancement of research, or the improvement of practice, or for the benefit of the public health, or anything that would come of the work either in relation to basic research, or applied research, or applied activities. These were the normal motives, and these men concerned with the work in the Army behaved in the normal manner to which they were already accustomed.

When the manuscript was received at the office, we'll say, in Preventive Medicine as an example, it was read by several people in Preventive Medicine for its scientific content, its validity, and its relation to security. If it was decided that the manuscript was acceptable, if it was somewhat edited at the time of its first reception, it was then referred to an overall office in the Office of the Surgeon General, usually called the Office of Technical Information. That was composed of a staff of people who read these papers without knowing anything about the scientific sides of them, or the clinical sides--they were interested in security, and they read the manuscripts from the point of view of security.

They had an added element in the security procedure over which they were guardians, and that was, you might say, a political element. Not only was it not permitted to disclose military information that might be of comfort and aid to the enemy, but it was not permitted the author to say derogatory things about other officers, or about other services. If it should creep into an article written by a medical officer in the Army that the Navy was no good in the situation, they wouldn't allow that to remain in the manuscript. That's understood, and there were situations when that might have been said openly--I mean, it might have been said and have been removed.

After these papers were scrutinized from the point of view of security and were approved, they came back to the Office of submission; namely, we'll say again, Preventive Medicine, or one of the branches, or divisions of Preventive Medicine, with a stamp on it "No objection to publication"--stamped and signed by the Office of Technical Information. That's as far as they went at that time. They didn't go to the War Department Offices, or the

General Staff Offices for advice on a paper unless some high policy, or general staff matter was involved. There was no Department of Defense at that time. The General Staff and the War Department didn't have the same kind of public relations, or technical information service, in effect, that now exists under the Department of Defense, so that once it was cleared by the service, the Medical Department, for example, it was finished unless some high policy was involved.

Then it was returned to the branch, or division, or service, Preventive Medicine again, and it was submitted to the journal selected by the author. Sometimes General Simmons would like to see a paper put in a program of a meeting of a scientific society, and that scientific society would determine where its proceedings were published as in the case of the hepatitis paper by Douglass W. Walker that we looked at. That was published in the American Journal of Hygiene after its presentation at some public health meeting in which General Simmons was interested. Usually the papers were sent from our office to the journal selected by the author. The correspondence was handled that way and proof sheets, galley proof and page proof, sent in the usual way, either through Preventive Medicine and to the author, or sometimes it was just the journal dealing with the author. Once it was cleared and on its way, nobody wanted to put any obstacles in the way of prompt publication.

The outlook on Preventive Medicine toward these matters was that there should be the greatest freedom of publication and, as a matter of fact, publications from General Simmons were coming out as early as the early 1940s. He published some things as soon as he got to the office almost about the preventive medicine program of the Army and what was in his mind. The Board's scientific publications began after the Army Epidemiological Board had been functioning about a year. Publications from that Board began to appear, I think, in 1942. They were published continuously after that as manuscripts became available. I don't recall--I think I saw them all--and I don't recall any ever being stopped because it was thought that the material ought to be classified.

We tried to avoid classification as much as possible of reports and manuscripts. That was not true of other organizations like the Committee on Medical Research which was composed largely of civilians with some military officers, Navy and Army, on as liaison members. That Committee on Medical Research classified almost everything. So did the National Research Council. The Navy had a very strict classification policy which made things very difficult at one time. How irrational they can seem to get is illustrated by what happened in 1944, with regard to malaria. I was told that Admiral Chester W. Nimitz said to General Marshall one day in the Southwest Pacific that there's too damn much talk about malaria, and General Marshall said, "All right, we'll shut it up."

I'm not quoting him. I'm making up what might have been said. There was a very small piece of paper that came out of that saying that everything about malaria will be regarded as secret from this time on. That never was the attitude of Preventive Medicine, so we started to oppose that from the start. The enforcement of that policy adopted through the action of General Marshall and Admiral Nimitz was so strict that it began to prevent our circulating ancient documents on malaria--for instance, spraying the jungles with DDT came up, almost routine procedures in malaria control for killing mosquitoes. All the textbooks used for years show airplanes dusting tree tops with Paris Green, and insecticides were listed all the time.

It was said that nothing could be said about atabrine, an antimalarial drug, because the Japanese might benefit by it. We have Japanese papers translated showing that they had atabrine in New Guinea and were using it for the suppression of malaria and for the treatment of malaria. This got so bad that they had to form a Joint Army Navy Committee at a high level, general staff level, and they happened to have appointed me as sort of a responsible member for the--I forget the name that it was called. [Joint Steering Committee] It's in some of these papers here. I attended many meetings in the Pentagon facing a stern Admiral who was upholding the secrecy of the Navy. Finally we won because I went over one day loaded for this Admiral with a suit case full of all the standard textbooks I could get, old circulars and stuff about malaria, full of pictures, all the material that had been used in the open for years. I told the Admiral that this was what had been going on, and he said, "Sir, I do not mean to be offensive in any way, but I just don't believe you."

I had this material all around my feet at the table, and I put them out and passed them around among this committee, this Joint Committee of Army, Navy and Staff, and that ended that. It tied the thing up for, I guess, nearly a year. Fortunately nothing like that happened in the Preventive Medicine Service with regard to publication either of members of the staff, or General Simmons, or the Army Epidemiological Board, or the Typhus Commission. By the end of 1945, upwards of--oh, I'd say upwards of two hundred papers had been published by members of the Board, and it was a system that continues through all the life of the Board since its founding, for twenty-five years, through Korea and all the special Army missions it's performed with the resultant publications that were shown in an exhibit at the 25th Anniversary of the Board about two weeks ago--about 1450 papers have been published on its work. That is the essence of the publication system with regard to scientific papers of current work.

The other--I might mention it here--the other big publication program IN which Preventive Medicine was involved and rather deeply involved is the historical program. Do you want to mention that?

Yes.

From the very beginning of the war always there were bold, large plans made for the collection of material for the history. This is ingained in the Army since the Civil War. In 1862, General Hammond, the Surgeon General, wrote a very famous circular that started the collection of material for the famous Army Medical Museum and at the same time for the publication of this enormous six volume, almost folio-size volumes, called the Medical and Surgical History of the War of the Rebellion, a title I hate to recite because in my part of the South they don't use the word "Rebellion" in connection with a difference of opinion which occurred at that time. The collection of materials for the publication of the history of the medical department in a war was traditional from the Civil War on.

As soon as it looked as if the Army was going into World War II--oh, before that, the historical program for the Medical Department in World War I resulted in the publication of about 14, or 15 huge heavy volumes under General Ireland, and that's a superb history of the war. Even before that, a model was the history of the Crimean War which was really the first good medical history that has ever been put out, so we had plenty of historical and literary tradition in the military services, particularly the Medical Department, that fostered the idea of the collection of material and its subsequent publication in an historical manner.

At the beginning of World War II an Historical Division was got up in the Surgeon General's Office and Colonel, later Brigadier General, Albert Love, a very fine scholarly officer was put in charge of it. He was tall and thin and quiet spoken, very polite, but a very firm and able man with a very fine <u>History of the Development of Statistical Method in the Army</u>--classifications and handling of historical materials. General Love set up the Surgeon General's history branch, as it was called, and began to collect materials, but about the time we got into World War II, very powerful civilians began to take over the idea that they might write the history of the war. These powerful civilians were housed, we'll say, centered, or nested in the National Research Council. They took it upon themselves to say that they, representing the civilian interests in the country, would publish the medical history of the war. On their board were two very vigorous people; namely, Dr. Morris Fishbein, the editor of the <u>Journal of the American Medical Association</u>, and Dr. John Fulton who was Professor of Neurophysiology at Yale about whom we've talked before--very active people. They began to set up a system for the collection of material for the history of the war and caused General Love to wring his hands in despair because the material was being diverted from his office. For example, Dr. Fulton, probably unconsciously, or at least without clear knowledge of what he was doing, put himself in contact with the surgeons of some of the theaters and told them to send their reports to him and not to the Surgeon General. I met with that civilian group, and I think that there are some pictures in the files here that show our meetings.

It was obvious that they couldn't live up to this obligation that they had assumed. There was a great deal of bickering and trouble, and finally--getting around to General Kirk's time--General Love was supported by the Surgeon General to the extent that recognition of his Historical Division was official, and he had no further dealings with the civilian group of the National Research Council with regard to history. This civilian group was then disbanded. General Love tried very hard to get all of us in the division and branches to collect material and to be sure that copies got to his office for the history, but it was obvious pretty soon that a secondary group, a secondary agency, so to speak, couldn't carry the spirit of the thing. The history has to be written by those concerned in it, we felt in Preventive Medicine, although I must say that I don't believe that Preventive Medicine cooperated very well with General Love's Office, although we had plenty of memoranda about how necssary it was to begin to write history and to give him drafts and copies, and especially documentary material, archives.

About 1944, or 1945, the idea developed that Preventive Medicine, for example, should write its own history as a part of the medical history, or the history of the medical service. A Board was formed of which General Simmons was a member. I was a member. About six or eight people were members from the Preventive Medicine Service. The control of the board aside from being under the chairmanship of General Simmons was pretty much under the sway of the head of the historical service in the Surgeon General's Office. One of these was Colonel Joseph H. McNinch who was at the same time head of the Surgeon General's library. He was succeeded by Colonel Calvin Goddard. A great deal of material was collected. A great deal of floundering took place. It was very difficult to know what they had and how to make a program for it. The accumulation continued, and our Board on the History of Preventive Medicine in World War II as a part of the history of the medical department did not get well organized, or at all productive until about the 1950s. Its first publication in a volume form came out in 1955, and it had to do with Personal Hygiene and Immunization. In the same year a volume was put out on Environmental Hygiene which is a mixture of sanitation and quarantine accounts. These two volumes came out about 1955, fully ten years after the Board had begun to function. The program after many, many changes, finally contemplated publication of nine volumes in this series.

To make this short, let me say that as of this date, there have been six volumes published in Preventive Medicine. Two more are in complete manuscript form, but not yet edited and the third one; namely, Volume I is being written by me. I took it on a few years ago after a manuscript by a paid lay writer was found to be entirely unsatisfactory. The Board wouldn't accept it. It was atarted again. Shall I tell about the meeting when it started?

It came about 1961. I became Chairman of the Advisory Board on the History of Preventive Medicine, a board appointed by the Surgeon General annually. I became chairman in 1954, just after the death of General Simmons. He had been chairman from the start in 1945. When this big manuscript by this lay writer was sent in for review by all members of this board, it was found by all members of the Board to be so unsatisfactory that they didn't want to have anything to do with it, or with the Surgeon General's Office, if it were published. It was decided that to say that it was so unsatisfactory that it was not acceptable, and in that meeting the members of the Board were sitting around the room, looking down at the table in a glum way, wondering what to do next. One of them--I won't say his name, though it was probably Colonel Hardenburgh, turned to me and said, "You certainly can do this from having been mixed up in all of the affairs of preventive medicine during the war. You're an old man. You have plenty of time. You take it on."

I told them that I would take it on, provided they would let me do it in my own way; namely, that they wouldn't harass me every month and ask me where I was with it, or put any kind of a whip over me, and that they wouldn't pay me anything for it. As a result, I have been slaving at this thousand page book since about 1962. I find it very severe and very interesting. I have reviewed the whole of preventive medicine of the war in many aspects during this past few years. I mention here my connection with this project of the publication of the history in connection with the things that Dr. Phillips has been extracting from me to show that the current feat of memory is not much more than a reciting exercise in class from either recent, or previous reference to a book where I know the information lies.

<u>How did you find the material preserved?</u>

In the archives?

<u>Yes, covering this war period.</u>

Well, the material--we call them archives--are preserved first in the Surgeon General's Office. He had a whole floor full--a lower floor full in one of these wings of the so-called Main Navy Building. Half of it at least was his record room, filled with steel drawer files, some locked and some not. It was a rule of the Surgeon General's Office that any time any letter, or report was handled, it had to be sent down to the record room for filing, or else a copy of it sent down. That built up a huge collection. That was a system that was followed too in the Offices of the Surgeons overseas. They collected their records currently. So did hospitals. The amount of records that were preserved was enormous. The letters and reports of Preventive Medicine didn't always go down to the record room because I, for example, kept all the Typhus Commission records together in a unit and all the Army Epidemiological Board records together as a unit. General Simmons kept his office records as a unit, and he sent down to the record room of the Surgeon General's Office, if I may tell the truth, as little as possible because it got lost. It got lost during the war because it was filed in some arbitrary way that would be changed at another time, and you couldn't relocate it.

The second trouble which began during the war and developed later, came from the decision of somebody who knew about archives, that rubrics should be different, that the unit filing system, like keeping everything of the Board together, or keeping the Typhus Commission together, or Preventive Medicine together was not logical. The system that came in was that these unit files would be broken up, subdivided, and refiled under such rubrics as "public health", for example. Public health is broad enough to cover much in preventive medicine, but not all of it, much in the Army Epidemiological Board, but not all of it. For instance, all the basic and general work that Dingle's group did on acute respiratory diseases has to do with public health, but it also has to do with virology and pathology and moreover, it belongs to that unit laboratory. Some people couldn't see that.

In addition, as the material came back from Europe, and it did come back from Africa, the Southwest Pacific in great crates of files and records. Some didn't come back, and a great example of that is the files of General MacArthur. They have only recently come in to the files of the War Department--trunks of them. You knew about that.

Yes.

Well, imagine what is happening in the warehouses, or storehouses, when these millions of pages come back to be preserved. They come back. They were put in warehouses that are used by the National Archives chiefly--in different parts of the country. There are great storage files in the St. Louis Medical Depot, for instance, and medicine is only one thing. Ordnance probably has as much, or Chemical Warfare just as much, but I'll talk about the medical. We have great storage depot files in Kansas City. There's an enormous World War II collection of records in Alexandria. There's a whole huge building there with high ceilings, so that files extending from the floor to the ceiling, twelve or fifteen feet high in this old building, can be approached only on step ladders. It's pretty hard to search a file on a wobbly step ladder ten feet off the floor.

The Surgeon General has been trying to collect archival material that belongs to him--this storage place, the Research and Reference Branch of the Historical Unit in the Forest Glen Section is a big building, and some big rooms in other parts are full of files. They have an easy access to Kansas City storage, St. Louis, and I think that St. Louis has all of what we call the 201 files. Every officer in the Army has a 201 file in which every order and everything he's thought about almost is kept. They're all kept. You can get at them. It takes two or three weeks to get it out. The material is there. It gets disturbed every now and then by somebody in the Adjutant General's Office who thinks it is not being handled properly and ought to be refiled. The staffs that are handling it are not adequate. They're not particularly trained for this work. Some of them are historians. Others are not. After a while they get pretty good at finding things. They are not productive historians. They are filing people. You'd be astonished to see how much material is available, and if you work hard on it, you can dig it up.

Another thing that has happened to the files is that there was one time when a promotion system was instituted by which a person got advancement according to the number of linear feet of material he destroyed in a year. If you destroyed in a year fourteen feet, you got a better position than one who destroyed only twelve feet. It's all authentic material. It's astonishing to see the original copies, carbon copies of everything, orders, endorsements, opinions very freely expressed and very discoursively put forward in these things.

What was their criteria for destruction?

That was a mystery to me. Often they are destroyed by people who don't know their value. The criterion is that they were too crowded and that they'd better get rid of some of this stuff. That is a pressing problem. Have you ever been in the National Archives Building? That's very much crowded, but in these Alexandria Buildings, it's even worse.

Are all the war office files of the Preventive Medicine Service available? Have they all been preserved?

No--not all of them. I wouldn't be able to tell you that. I didn't know how many they had to start with. I know that there are some things I can't find because, as I say, they were broken up and filed under other headings. They haven't had staff enough to sort them all out. They tried for a while to catalogue them, but they haven't catalogued all the documents. They have a big steel cabinet with three by five cards, or three by seven cards, and I noticed that they are not using that any more because they have changed them, changed the filing cabinets. The filing system, of course, if it is to be any good, has to show the location of the document as well as what's in the document-- at least some one little line, and occasionally they do that. Then they go and change all the filing cabinets, and when they do that, they change the locations and don't enter them on cards, and so they finally threw the card file away.

Do they still have your files intact?

I don't let them touch them. I took them over there intact, and they're practically all intact. Some of the Army Epidemiological Board material got out of my hand when I went from the Army back up to New York--to New Haven and New York. The Army Epidemiological Board was then still functioning as it is now, as a part of the Preventive Medicine Service. I had no right to take the files. I begged them to keep them in a unit, and they did as much as possible, but they are scattered, some of them. Sometimes I can't find what I know is there.

The Typhus Commission's files are in six five drawer filing cabinets, and they have not been disturbed since I put them there. I had it written in to Mr. Truman's Executive Order disbanding the Typhus Commission that the former director be responsible for the disposal of the records of the Commission. I had a covering letter on that from Mr. Patterson who was the succeeding Secretary of War. He was very kind in what he said about my service with the Commission and confirmed the fact that while I should have due regard to the interests of the Navy and the Public Health Service, I should have the say on the distribution, or at least the handling of the files, so when I went from the Surgeon General's Office back to New Haven in 1946, I took these six filing cabinets locked, combination locks mostly, and bars in combination on a truck with a guard to New Haven. I put them in a locked room in the historical library--Dr. Fulton's library. There was room up there. I kept them there about a year. I moved them in the same way down to first one and then another apartment that I had in New York always locked because they hadn't been declassified at that time. I didn't declassify them until I was back here in the late 1950s. In 1953, when I came down here, I brought them down again and got a carrell in the Library of Congress and put them in a locked room up there. They let me work on them there. I brought them all down again, meticulously, with an armed guard and fanfare. From there I moved them out to my office in the Historical Unit. While I was at the Library of Congress, I got authority to declassify them, and I spent about a year putting a declassified stamp on everything, perhaps exceeded the authority, but it was a reasonable thing to have done, and I wasn't letting them out of sight, and they are now not in locked files. They've been so much associated with me that nobody touches them without asking me, and the reason they ask me is that I know them all by heart mostly, but it would be wonderful if all the unit files could have been kept that way.

We've gone about an hour, and I'll have to get to these peculiar days.

What are the days?

It would just disrupt what we've done. We've been pursuing a theme--records, history, publications.

We've finished that. There's nothing more to say.

Friday, June 10, 1966 A-60, N. L. M.

I've been talking off the top of my head on the question of foreign quarantine and the efforts of the Army. I wanted you to explain the relationship that the Army, and more particularly, the Preventive Medicine Service in the Army, had with existing civilian agencies in the United States where troops might go either for purposes of maneuver, or be stationed, and we had a lot of troops stationed where none had been before. Big camps were being created, and there are these relationships between existing civil institutions and the Army that had to be taken care of. Then, no one anticipated that we would send the enormous number of troops overseas which we ultimately did, nor the fantastic amount of supplies and equipment in their support which involved transport, ships--all the basic things, potential plague, cholera--the works. Some awareness of how this problem emerges as a problem and occupied the attention of of the Preventive Medicine Service in the Army and some of the problems that emerged which I think in some ways had to do with immunization--you were in Egypt to work out some arrangement with them. India was another area where relations with a nation state made for stickiness. There is running through this the Army's basic impulse for secrecy so far as transporting troops and supplies are concerned, ingrown, basic, and yet the attendant possibility of insect infection in areas where they want to exclude them. You have the possibility of human carriers of disease also that arises. It's a sticky problem--reading this summary version by Dr.--let's see what was his name?

Lt. Colonel Philip T. Knies.

Any comment you'd care to make would prove helpful--in terms of Preventive Medicine.

It is in the Army Regulations Manuals and many statements that the United States Army has to obey the laws of the locality in which it might be operating, and these laws in our today's talk are the ones that have to do with quarantine. Quarantine by statute is a national matter and a state matter in the United States, and the enforcement of quarantine of human beings is in the hands of the Public Health Service, but it is not to be forgotten that there is also the necessity to apply quarantine regulations to animals moving across either state lines, or from foreign countries into this country, or out of this country into some other country. That was usually handled by the Department of Agriculture, although the Public Health Service was also involved and interested in the diseases of cattle and swine. Some of these diseases are transmissible to human beings too. Furthermore, there is another side of quarantine that every one is familiar with, a state matter and a local matter, and that is the plant quarantine. The soldier can't drive his automobile across state lines with, we'll say, certain plants in his trunk without being held up and having the plants taken away from him because very serious economic losses have been produced by infection--virus infection, fungus infection carried by plants and even citrus fruits, potatoes, even things like that, so it's a very broad field of activity and one with which the citizen is familiar before he gets into the Army.

When he gets into the Army he finds that he's faced with the same situations that he would have as a traveler in civil life, and the differences are those brought about by the Army specialized, characteristic activities of movement in the open, of troops in masses, of troops mingling in all sorts of regions. It becomes a mass proposition rather than the individual one I was speaking of at first. Quarantine applied to masses is a large undertaking, although, of course, in the end it comes down to the individual.

However, to deal with large masses and in a way to prevent carriage of disease, carriage of infection by these masses as such, you have to have mass methods like immunization which starts as an immunization of an individual, but you soon have thousands of individuals immunized against something. You lose sight of the individual, but you've gained the protection of the mass.

Quarantine also requires attention to the vehicles, the transports that transport the masses or the individuals. An example is the transport of mosquitoes by airplanes. A mosquito can get into the covers of a retracted landing gear and be carried for miles and miles rapidly--say a mosquito infected with malarial parasites in West Africa will be on the northern coast of Brazil over night almost. That's what happened. Probably the Anopheles gambiae mosquito was brought over from the region of Dakar, and it carried a malarial parasite which looked no different from the other parasites in the infections but it had gained an unusual virulence in the gambiae mosquito. This situation amounts to--I think it's just like so many other things in Preventive Medicine and in the Army; it's the enlargement of well known civilian public health and preventive measures and the adaptation of these measures to the Army situations. The principles are all well known, and I do not believe that in the war any new principles of protective quarantine were developed, but they could be applied very vigorously and with a lot of force.

For example, in a sense it's quarantine if you remove from a locality in which soldiers are quartered, or operating, the local population to some other area. In some of the islands of the Caribbean the only available good place for stationing soldiers, we'll say, would be on a beach with a bit of a harbor, and a native village would be right there too. What was done then--they just transplanted the whole population to an area some miles from where the troops were, beyond the flight range of mosquitoes. This in a sense is imposing a quarantine in reverse, and the reverse of it is that a type of protection of a post containing soldiers is to locate it someplace away from native populations. That was not done, for instance, in the region of Dakar. One time the Air Force units had their quarters right in the middle of a valley, a nice piece of land, in which the natives were living, and the natives going back and forth brought infection into the troops that were there because the insect vectors were the same in both cases.

Obeying the regulations of states and countries is an intricate matter requiring a great deal of knowledge about people and about situations. The obedience is not just a routine affair, automatically exercised, or automatically observed. The local quarantine laws are administered by local health officials, and even they come under the purview of a governor of a state, we'll say. That brings the military group into contact with the local civil government almost on every level from food handling, water supplies, on up to policing an area, or traffic regulations. This sounds very confused and mixed up the way I'm telling it, but it's rather true to life that the complexities must be appreciated and variation must be made to meet these complexities.

Some of the quarantine regulations, however, affecting the movement of our troops were very severe and inflexible. The one that is the best example of that is the Indian quarantine against the possibility of the introduction of yellow fever into the great over-populated subcontinent of India. Yellow fever has never been in that region. There are no immunes. At the same time the Aedes aegypti mosquito is prevalent, and we know that it is carrying a virus rather like the yellow fever virus; namely, the dengue virus. Dengue occurs in India, and it occurs in Asia, but there has never been any yellow fever in that region. Why that is so, nobody knows, but the Indian Government so dreaded the possibility of the introduction of yellow fever into the country and could so vividly see the great loss of life that would occur if yellow fever were introduced into this non-immune population that they adopted quarantine rules involving immunization against yellow fever that were hindrances to troop movements. These Indian quarantine regulations for yellow fever were imposed perhaps ten or twelve years before the war, and we were beginning to know something about immunization against yellow fever, but the Indian quarantine regulations didn't change with the advancing knowledge. At the time of the war the Indian quarantine regulations, specifying when the soldiers should be immunized, how often the immunizing injections should be repeated, and what interval should be allowed to pass after the injections had been made, were antiquated and were much more stringent than was necessary. The application of their rules sometimes resulted in holding up troop ships in Bombay Harbor for two weeks longer than they needed to stay there.

In addition, air travel across Africa, through Egypt to Karachi and Northern India were impeded by these same regulations. I became involved in it when General Simmons was not able by correspondence to persuade the British and the Indian authorities to make these ameliorating changes in their regulations. I went to Europe in 1943, on sort of a mission to do several things. I was instructed to add to my duties on a visit to London, to try to have conferences with what was called the Interdepartmental Quarantine Commission of the British. It was a combined Indian, African, British committee. My orders sounded as if they had come from on high. I was representing the Surgeon General, and in the letters and things I carried, it was stated that the General Staff of the War Department was interested in my having a chance to lay this new knowledge before their Interdepartmental Quarantine Commission.

I was in London for nearly a week being told everywhere that it would be impossible for that commission to meet because everybody was away somewhere. All of a sudden the members of the committee turned up because a British Air Marshall--I think it might have been Air Marshall Sir Arthur William Tedder who might have been in the United States--heard about this problem and cabled over to the authorities in London, telling them that I was over there and that they should see me. I had meetings and got along very well, particularly with the aid of Colonel Tom F. Whayne who was Military Medical Attaché in the American Embassy. They were very interesting people to meet, and the conversations indicated what I have said at the beginning, that the relations between the Army and the local civil authorities is important and has to be nurtured. I returned to the United States with the feeling that we would get along further with the Indian Government after this meeting, and that was a fact because Dr. Sawyer of the International Health Division gave me all the latest information on the yellow fever vaccine and the immunization processes which the Indian Government didn't have.

It wasn't all over that year, 1943, so that in March, 1944, when I went to Cairo, I had a chance to talk to the Egyptian Minister of Health and people in his government who were still adhering to the Indian strict quarantine rules, not so much because they were afraid that yellow fever would occur in Egypt, but I think they were having some arrangements with the Indian and British Governments that were politically advanced by their taking the position that they did. This again shows the ramifications of what looks like a mere regulation of quarantine into the high policies of a country. Well, the Egyptian Minister of Health listened to all of this and after a while made the necessary changes in their regulations, and that cleared the air passages, so to speak.

Quarantine was of interest to the Preventive Medicine Service in the Surgeon General's Office from the very start although a quarantine branch, or a quarantine division didn't appear in the organization chart until about 1944, I think. That didn't matter because quarantine was handled in the Division of Epidemiology--it would have something to say. The Division of Tropical Diseases naturally had a good deal to say in its relation to malaria. We had a branch called the Control of Communicable Disease which naturally would take in quarantine. In the manner characteristic of the Preventive Medicine Service, the jobs were done by different people in different parts of the organization without having to have their activities fall in just one mold, or be molded by just one set of affairs. They could work throughout the Service in different divisions on the problems as it pointed up the interest of that division.

Quarantine matters naturally didn't confine themselves to the Army because the Navy had people in about all the same places as the Army had people. The Air Force had people and vehicles in the same places as the Army had, so that quarantine problems tended, really, to unite the separate services even before there was the unified Department of Defense. There was an organization called an Interdepartmental Quarantine Commission, I think, composed of Army, Navy, and Public Health which was supposed to do some joint work in adjusting quarantine matters, or helping to enforce them, or pointing out the need for the application of quarantine methods, but my recollection is that that committee didn't function very well. I don't recall anything that it was known for, but as usual, General Simmons saw plainly the scope and importance of having some central quarantine office, so to speak, for the Army, and he brought into the Preventive Medicine Service Lt. Colonel Philip T. Knies who turned out to be an extraordinarily able quarantine officer.

Colonel Knies was sent all around the world and met people in the Army, the Navy--he didn't meet the Public Health Service overseas because the Public Health Service was not involved in foreign affairs very much at that time. Colonel Knies sent in extremely valuable reports and had great success in getting people to understand the problems, or in persuading people to take actions that would support measures to prevent the introduction of diseases from one place to another, or the spreading of diseases from one place to foreign places. Knies was very much interested in quarantine applied to control of insects. I think he was more concerned with mosquitoes than he was with men because a mosquito they were interested in was pleased to travel on any vehicle that was moving whether it was an airplane, or a destroyer, or a truck. The methods of controlling the vector arthropod were the same in each case. The problem was the same, and that was another enlightening and unifying bit of information that I think neither Army, nor Navy, nor Air Force people had at the beginning of this joint effort. Knies developed a lore--call it that--of foreign quarantine. It was very effective and useful.

He was not so much concerned with the quarantine matters of prisoners of war that were being admitted by several thousands into this country. These people came from malarious districts of North Africa, or Italy--we had several thousand Italians, and they had malaria. The German prisoners of war didn't bring in any particularly serious infections, unless they happened to have served in malarious areas when they were captured. They were brought in when they still were infected. There was rather good control of these imported individuals by careful examinations and by housing in screened barracks in areas where there were vector mosquitoes. For the other parasites that they might bring with them, they were screened by fecal examinations and blood examinations, applying to a prisoner of war routines that you would apply at a port of entry in peace time, but applying further measures at the places where these prisoners were settled, the prisoner of war camps, and when the prisoners of war were sent out on labor duties, like cutting timber, they were handled just as you would handle troops in the field in a malarious region, or in a healthy region.

The other group--the third main group to which quarantine rules had to be applied were our own troops returning from foreign areas from which parasites could be brought into this country by the infected individual. That possibility gave us a great deal of anxiety and caused a lot of difficult work. Examples of the parasitic diseases against which a guard was erected were malaria, filariasis, amebic dysentery, ordinary bacillary dysentery--typhus fever also. Typhus fever really meant that you were more concerned with the elimination of lice from lousy individuals and their clothing than you were in eliminating the disease. It is easy to recognize typhus and quarantine the person by isolating him in an isolated disease ward. The possibility of the introduction of diseases into the country by returning soldiers was kept in mind constantly not only toward the end of the war, when the big demobilization and redeployment was in effect, but even in the first part of the war when people were returned even the first year. Some would go and come back sick within a few months after they had been abroad.

Fortunately no outbreak of disease that I knew about in the war was traceable to a returned soldier, or a prisoner of war that had been brought in through these quarantine barriers. Why that freedom of disease occurred is hard to say. It's stretching it too far to say that quarantine preventive measures did it. They might have. There may be some other rules about starting infections that we don't understand yet. Of course, starting infections is one of the problems of biological warfare, and it hasn't been solved yet. Perhaps there is some natural protective condition that we don't quite recognize that helped in this case to keep from starting diseases in this country. Malaria was the one most dreaded, and it could be easily spread in the South, but all this time the malarial rate in the Army and the civilians was going down to the lowest level in history--almost. There was a scare of the introduction of small pox from Korea later, at one time there was a scare of the introduction of filariasis from Okinawa because these Japanese prisoners on their way over were found in Hawaii to be largely infected with this blood worm. The Hawaiian authorities got quite excited about it, and these Okinawans and prisoners from Okinawa were herded together, isolated and not allowed to circulate in Hawaii and not allowed to come into this country. Where they were sent I don't remember--I suppose back to Okinawa. I don't know that there's very much more to say about this particular thing.

<u>Food--like the importation of fresh pork into Australia.</u>

In Australia people were afraid of trichinosis--they would be in any country. It's remarkable why these things don't spread when you think they would. In New Zealand the veterinary people, our veterinary people were horrified to see how dirty and dangerous looking were the abatoirs and the meat handling plants in New Zealand. Something was slowly accomplished by example and persuasion to improve these conditions, but it was a condition they never did fully improve. We don't know how much it costs in health--the conditions affecting the milk supply, especially with relation to tuberculosis. They didn't have pasteurized milk, and they had plenty of tuberculosis in the cattle. I've been recently working with an officer who is writing a chapter on New Zealand, and we can't find hardly any evidence that these conditions were detrimental to the troops that were stationed there, and quite a few troops were in New Zealand for a rest.

There are also naturally occurring areas in the Southwest Pacific, around the Admiralty Islands where they have no malaria at all. This area of the ocean and the islands is shaped like a great big polar bear. General Simmons used to have this polar bear well outlined on his wall map. Nobody knows why malaria isn't there, although the vector mosquitoes are in the region.

Another disease that has been subject to frantic quarantine in times past is leprosy. Leprosy was very prevalent on some of the islands in the Southwest Pacific. Ninety percent of the populations would be leprose. Leprosy in the Philippines was so prominent that the Leonard Wood Memorial for the Eradication of Leprosy in honor of General Leonard Wood was established. They have that word "eradication" in the title of the original thing, thinking of eradication which is a concept that is not accepted by some in this country at present, but it certainly is a valid concept. Well, leprosy was frightening, and it was thought that leprosy would be spread in this country by people returning to this country particularly from the Southwest and the mid-Pacific too, as in Hawaii, but leprosy develops so slowly that we may not have the answer to that problem yet. There's no real evidence that anything bad happened about leprosy. Two cases of great interest occurred in this period that made you think that leprosy might be transmissible in this region, and these were the cases of two men who developed leprosy in tattoos. They were tattooed probably by a native who was putting the needle in his mouth, or somehow or other carried infection because these two men did develop leprosy in a relatively short time in the places where they were tattooed. It was a remarkable experiment that one wishes might be tried more often by the scientifically inclined because the lepra bacillus has never been cultivated. Nobody has ever been able to grow it. Nobody has produced artificial infections with leprosy, except some peculiar lesions that are now being seen in the last few years in the feet pads of guinea pigs and rats where they inject the lepra bacillus, or lepra material into the feet pads and it looks like a multiplication has taken place.

Why leprosy disappeared from PARTS OF the world, as it did in the last hundred and fifty years or so, or less, nobody knows. It was very prevalent in Scandinavia at one time. The lepra bacillus was discovered by G. A. Hansen. Unless the improvement is due to better hygiene in general, better nutrition, you have no real explanation as to why leprosy dwindled.

Quarantine is an extremely interesting subject historically, legally, demographically, and militarily. It's a very broad field of preventive medicine, and it brings in everything.

It's certainly a surprise in terms of the quantities of men and material that we shipped out, some of which we brought back, salvage, troops--that we didn't stir up more on the way.

Yes, it is. For instance, scrub typhus has never occurred in this country, and yet we were afraid that it "might." I'm not making a pun, though I was thinking of mites at the moment. They were brought into this country because the salvage people sent tanks back. If they didn't send the tanks, very often they would send the treads, and these tanks had been plowing through the fields in New Guinea, through scrub typhus areas. The treads would dig up the mud, and the mud would have kunai grass in it, and the mites also live in the ground at one time, so we had a flurry of excitement once when tanks and tank parts like this from a salvage area were found in this country to have mites in the material that was clinging to the parts. That was immediately examined and found not to be significant. It was not a trombicula type of mite. It was like an itch mite. Mites are a very large family, and they differ in many minute ways, but yet important ways.

Of course, quarantine also involved some consideration of biological warfare because it was felt that the disease might be brought into a country by an enemy sending in infected individuals. It was very important in the field of veterinary interests, the possibility that foot and mouth disease, or swine plague, would be shipped into the country without its being recognized either by overt enemy action, or by accident. I don't recall anything about plants in this. I don't know that Knies even mentions plants.

No. He does indicate that there was some exposure to plague, but no cases of plague, no rats were discovered that were plague infested, that there was exposure to cholera, but again, no tunnel, no channel.

We were lucky in that war. There was no influenza epidemic. Conditions were all right for it, but we didn't have it.

His general view is that the program based upon preventive medicine, a complete program, a normal day by day affair, which given the size of the Army we had and the amount of material we shipped abroad, covering embarkation and debarkation overseas and then in reverse, that as long as one adhered to that program, everything went fine, although certain isolated cases came up--like the fellow who was flown, I think, back from Japan with small pox, discovered on the plane. All others on the plane were revaccinated. You could take steps like that.

Yes. My Executive Officer in the Typhus Commission got typhus on the island of Hokkaido from louse infected blankets of the coal miners up there. He wasn't lousy, but he did get some louse feces, I suppose, in his eyes and his nose, and he began to get sick in about a week. He came home, and he was sick on the plane. He had a typical case of epidemic typhus in Alexandria. Nobody told.

You could take steps. Things didn't come in waves, but by individuals.

I've often thought of that. Of course, nobody told. I had some responsibility in that case. You have to report by law communicable diseases, but I donot believe that his case was ever reported to the health authorities.

It's just fantastic that more didn't occur.

It is--even so, it seems so risky.

Even the redeployment of troops from the European theater, or the Far East. You know, that's a simple statement to make, but the process of redeployment is a bit more complex.

In redeployment, the troops went mostly through the United States--most of them. Some of them went out through Suez. I don't know how many thousands were actually redeployed--you see, it didn't last very long. V-E Day was May 8, 1945, and V-J Day was August 15, 1945, so there were but three months in between, and you couldn't redeploy many troops.

<u>Where were you on V-E Day?</u>

I was in the Surgeon General's Office, and I think we brought a radio down and put it on the desk. V-E Day came quite gradually. We were following the troops up through Germany, across the Rhine into Germany, into Austria with Patton. It was obvious that it was on the way--I think everybody expected to have a V-E Day as soon as the Armies got across the Remagen Bridge. Every day was just another German Army captured, another German Corps, another German Division knocked out. Victory was expected momentarily, whereas V-J Day came rather suddenly. But on V-E Day I think I heard it over the radio. We had gone to work on an ordinary day, and I don't recall that the office was closed, or that anything special was done to break up the routine in the Surgeon General's Office. I can't remember for sure where we were. We were at 1818 H Street.

Let me ask you--when did President Roosevelt die?

<u>I think it was early in May.</u>

V-E Day was May 8th.

<u>The President's death preceded V-E Day. It may have been in the latter part of April, but I think early May--maybe his death preceded V-E Day by a week, or ten days, maybe more. I knew we could hardly wait to get the Stars and Stripes when it came out from December, 1944, on. By that time whenever the Russians took off, they seemed to move a hundred miles at a time. We were all expectant, hopeful that we'd meet them at the Rhine.</u>

In the evening Mrs. Bayne-Jones and I walked down Connecticut Avenue from about the bridge to Jackson Square, and everybody was milling around down there--

blowing horns, swirling around. I think we stayed down there a couple of hours in the crowd.

What about the changes that this would make--that this part of the war problem had been settled?

There were lots of anticipation of the changes in the Surgeon General's Office. We had been working on it for months--a whole series of demobilization actions that had to be planned; what are you going to do with these troops when they came back. In addition, there were lots of hard studies of the reorganizations that were to come about immediately following the surrender in Europe and hastening on with the expected surrender in Japan. These I thought were very tedious paper exercises. They were awfully hard too. You had to make all sorts of calculations. Some men in the Preventive Medicine Service were extremely good at these things, and some like myself were not any good. Colonel Robinson was excellent, but all through the office they were planning what they called "post war planning." They had to. Immediately Preventive Medicine began to get cut down as you can see it in these charts. It dwindles down fairly rapidly in 1945. All the Surgeon General's Office was busy--I can remember that General Kirk was concerned greatly in all of the future things that were going to happen, and we had many conferences.

Well, the growth, in part, had been in response to a need plus the internal structure of the Army itself and its reorganization. The growth of Preventive Medicine Service had been in response to a need clarified by that department and to suddenly face the necessity....

You hadn't much freedom as to what you might do. The Medical Department was really demobilized by a very bright young man named Eli Ginzberg. He was brought down to A. S. F. and under General Somervell--one of his sections had charge of this, and it was Eli Ginzberg's job to figure out the procedures--I mean the calculated numbers and the type of officers that should be let out at certain times--try to keep a balanced group while it was a diminishing group. It took a lot of savvy to do that.

I want to come back next time because we're just about at the end here--come back to V-E Day and the whole of 1945, and place it against the implications in the first chapter that you've written about Preventive Medicine which shows the cyclical development of the medical department in response to a war and the difficulties between the war periods--to all intents and purposes it becomes no more than a holding force. Now different problems emerge with V-E Day--that is, the necessity of continuing the Army in the field, occupation, which is a much larger base than the one you originally had over there in the Army of Occupation. That required some planning for medical things. Here in 1945, you anticipate the withdrawal of effective numbers but not the nature of the problem, and how you planned to meet that problem in the period of 1945 because you're still there through 1946 up through....

September of 1946 I was separated.

Let's come back to that on Monday. All right?

All right.

Monday, June 13, 1966 A-60, N. L. M.

Have you got it on? Turn it off--I want to show you something.

We've been treating changes in organization and growth of Preventive Medicine to meet the demands of war time activity in the Army, but the summer of 1945, beginning in May with V-E Day followed by the explosion of the atomic bomb in August /Hiroshima and Nagasaki/ and the surrender of Japan in August, forced reconsideration of reorganization so far as the Medical Department is concerned. Something you wrote earlier which I have read, the 1st chapter in your History of Preventive Medicine shows the growth of the Army Medical Department given stressful times and then almost the abandonment of its war time position. The world had changed in 1945, and thoughts about it may have changed in 1945, since we were falling heir, in a sense, to areas of the world which had to be policed with continued concern for medicine and health plus the work that had been going on of a research nature during the war. I don't know when thinking of this kind starts--post war developments. I have before me testimony before Senator Pepper's Committee early in 1944, so far as the research aspects are concerned, but there are at least two topics--what happens to the Medical Department of the Army given the summer of 1945, and its needed reorganization, and out of that reorganization how do we provide for continuing interest in research as well as support for research as far as the Army is concerned after the termination of hostilities. 1945 and 1946 is a pretty critical time.

I think you ought to go back further than that.

All right. Be my guide.

I'll first mention—I'll take the subject of research and deal with it in terms of the Board's research in the Surgeon General's Office. The first Research and Development Board—aside from those I have already mentioned which had to do with research in tropical countries, the Philippines, Panama—in Washington of any consequence in relation to war was connected with a Research and Development Division which was established in the Office of the Surgeon General as early as May 1, 1942. That was called a Research and Development Division. It was in the Administrative Service, and it was charged with the planning and execution of all research and development activities dealing with the Medical Department—professional and field equipment and supplies.

We who were concerned with the Army Epidemiological Board and its really intellectual interests in the causes of disease and controls, really blessed the language of this limiting directive to the Medical Research and Development Division, confining them to concern with professional and field equipment and supplies. That left us entirely free to go on with the Army Epidemiological Board as an academic university type of organization interested in profound problems, not supplies.

Colonel Roger G. Prentiss Jr., was made the chief of that division, and he was also liaison officer of the Division of Medical Sciences. His name appears over and over again from 1942, with various research boards of the Surgeon General's Office. There were a number of them, and they increased their scope so that they at one time practically—about 1945—began to take over the Amery Epidemiological Board. We managed to avoid coming under the authority of a new group of officers that didn't understand the traditions of the Board, or knew the people. That was not limited only to the Office of the Surgeon General, but also to the War Department because they set up in the War Department about the end of 1944, beginning in 1945, a Research and Development Board under Brigadier General William A. Borden who tried to have very extensive control. His group didn't function as long as half a year, and the Army Epidemiological Board could go on in its traditional manner.

In August, 1945, there was a very definite establishment of an Army Medical Research and Development Board in the Office of the Surgeon General with broad scope and interests sufficient to cause a liaison with the Army Epidemiological Board, but the officer commanding it, the chairman of that Medical Research and Development Board in the Office of the Surgeon General was Roger Prentiss, Colonel by now, who allowed the Board to proceed in its normal way and was satisfied with summary reports. He didn't interfere in any way, but you can see from this--well, I'll say from 1942, 1943, 1944, right on through, there was continuing interest of the Office of the Surgeon General in planning, coordination and prosecution of medical research.

The planning and coordination from war time activities merged without any perceptible change into post war planning. They began certainly in 1944, to begin to make post war plans--estimates of personnel, estimates of organization. It was perfectly plain that the war was going to end and that these good things would have to be carried on some way, or other, and people began to think about it.

If I can--if it's all right to go from medical affairs to very important civilian affairs, I would like to refer to the correspondence between Vannevar Bush and President Roosevelt in 1944. I have here the famous book by Dr. Vannevar Bush who was head of the Office of Scientific Research and Development and of the National Defense Research Agency, I think, and this book is entitled Science: the Endless Frontier. It consists of a group of papers that Vannevar Bush has written at one time or another since the early 1940s while he was head of SORD. They were gathered by the chairman of the National Academy of Science, Dr. Frank B. Jewett--do you know this book? Briefly in chapter 3, Vannevar Bush refers to a letter that he received on November 17, 1944, from President Roosevelt asking a number of important questions; what can be done consistent with military security to make known to the world as soon as possible the contributions which have been made during our war effort to scientific knowledge. Fortunately the Army Epidemiological Board had been publishing right on through the war, as we brought out last time. This was a benediction upon our last efforts.

The second question the President asks had particular reference to the science against disease; what can be done now to organize a program continuing in the future the work which has been done in medicine and related sciences. You have seen in General Simmons' papers, the collected papers of General Simmons, that that is a constant theme on which he played ever since the beginning--the union between the military effort in medical research and education with the civilian effort in medical research and education which was perfectly plain and agreeable to him; in fact, he was one of the chief proponents in fostering such relationship. I think all of us who had responsibilities in Preventive Medicine were constantly aware of that and proud and pleased to have any opportunity to point out where military discoveries under war conditions went right over into civilian life and were of benefit to public welfare.

The President then asked what can the government do now and in the future to aid research activities by private and public organizations. Can an effective program be proposed for discovering and developing scientific talent in American youth so that continuing future scientific research in this country may be assured on a level comparable to what it has been during the war.

These questions answer themselves almost. Any enlightened man would know how to answer these. I bring them out now to tell you that a good many of us were aware of this exchange between the President and Vannevar Bush. I knew Mr. Bush slightly, but I knew better one of his closest associates, President James B. Conant of Harvard. President Conant was close to General Simmons because about 1945, President Conant began to think of bringing General Simmons to Harvard as Dean of the Harvard School of Public Health. It was that time that President Conant got a million dollars from the Rockefeller Foundation, a hundred thousand dollars a year for ten years, provided General Simmons would go there as Dean. It's a fact that we used to see Mr. Conant occasionally on times when it was not possible to talk with him in any detail because at this period, he was deeply concerned in the construction of the atomic bomb. He was down here living at Dumbarton Oaks which is a house that Mrs. Bliss gave to Harvard. I used to meet Mr. Conant on the street and at various places. These ideas somehow or other were so commonly discussed that any episodic paper like a report to this committee of Mr. Pepper's--what is it on?

Subcommittee on war-time health and education.

Yes, that statement contained ideas which are in these questions and were in the minds of all of us who were concerned, not only with the daily operations of research, training, and procurement, or nurture of scientific personnel, not only in the minds with respect to current operations, but natural thought for the future. About the same time, as I have said before, all offices in the Surgeon General's Office were concerned in 1944, with drawing up plans for the post-war activities--demobilization, refitment of organizations, reorganizations and reductions. Reductions most obviously would come about for two reasons--one is that it is ingrained in the Anglo-Saxon people to disband their armies as soon as the fighting is over. We've done it over and over again as you see from the chart there, where the Army drops from eight million to two million in a half a year or so. It was very drastic.

Also we used to say, "What are we going to substitute for patriotism when this is over?"--how to hold these people who have been devoted to the service of the Army and the government when it was a matter of strengthening the country and doing what you could to protect it? What are you going to do to hold them when they were more keen to get back to their homes and their own jobs than they were to stay on something that was practically applied in the military? Fortunately the Army Epidemiological Board was able to continue without any jar, or rupture, its traditional course. All the divisions of it remained the same. The personnel remained the same, and it has continued from that time to the present with some ups and downs, I must say, but it has grown a great deal. It has gone on as it was intended to.

Now, to go back to these statements that General Simmons and I made to the Pepper Committee in December, 1944, I would say that they were a little bit late, in a way. They say what we had been thinking about for fully a year, and they say essentially that the nation has grown great through its scientific achievements in the war which put it ahead of practically every country in the world, and that research is absolutely essential for national security. It's brought out very plainly that the results of scientific research helped in a way, a very primary way to the winning of the war; that it was perfectly obvious that steps must be taken to preserve the stature that had been achieved and to provide for future growth; and that provision required attention not only to resources and ideas, but attention to personnel. It was necessary to have very good registers of people who were specializing in various kinds of sciences. It was necessary to strengthen the institutions where they were being educated, and it was necessary to make this system such that the universities were not trammelled by the officialdom of the government. All I can say again is that while I was not in the upper councils of any of these chief agencies of the government at which policy was determined, I think I could have used the language that the policy makers did because I was familiar with it through associations and natural ideas both at Yale and in the Army.

Were ideas like this expressed by people like--just citing them as examples--Dr. Paul, Dr. Dingle?

Oh yes. They expected--well, they knew about our actions. It was--well, I should say that it was almost like, "What are you going to have for breakfast?"

It didn't take much talk. It was something in your heart and soul, an appetite--you might say. Scientific meetings were carried on all during the war by scientific organizations--like bacteriologists, pathologists, immunologists. They'd meet. Usually the general ideas are expressed more openly in a presidential address at the time its laid down, but many, many of these papers of the period begin with some generalized statement of setting. All the heads of the commissions--Paul, Dingle, Francis, Shope--the leaders in there and all the members of the Board were enthusiastically and often vocally supportive of the things that are brought out in these papers. Nothing in the post war period changed the basic concepts that are in General Simmons' letter of December 27, 1941, about this Board, except there were some practical changes. This is rather prophetic--this letter of

December 27, 1941.

The great consequence that arose from this book /Bush's Endless Horizons/ was the establishment of the National Science Foundation to which I would like to return after speaking of another organization. I've mentioned these Army boards, the Surgeon General's Research and Development Board under General Prentiss, the War Department Research and Development Board under General Borden and a rising growth of research and development of enormous proportions within the War Department. That kept right on.

Very interesting problems came up after the cessation of hostilities-- I'll say after August, 1945, and made acute in 1946. Would it be appropriate to speak of them now?

Yes.

The question was asked what provision would be made for the continuation of all the desirable contract research that was being carried on by universities under contract with the government agencies, particularly the War Department, or the Navy Department, and even the Public Health Service? Were all those contracts to be cancelled with the termination of hostilities, or what provision could be made for financing them and holding the people at work and comfortably in the post war period? Fortunately the Navy quietly had built up a research organization just fitted for this that few of us understood, or knew anything about. There had been for years a branch of the Navy Department that was concerned with basic physical, mathematical and biological research as well as the applied research needed to build ships, submarines, guns and ammunition, and that in 1946, became the United States Office of Naval Research. Do you know about that?

<u>Yes, but I didn't know that it had that deep a background.</u>

See what it says there--"superseded"--I found that this afternoon. /"Office of Naval Research: 20 Years Bring Changes" 153 <u>Science</u> 397-400 (July 22, 1966)./ The title of the organization that it supersedes is on that paper.

<u>Office of Research and Investigation.</u>

That's what it had been called, and it had been going on for years. This now becomes ONR--we called it--and it was established on August 1, 1946. It was established therefore, after V-E Day which was May 8, 1945 and before V-J Day which was August 15, 1945. It was taking over things that this previous organization was not fitted to manage. They got large and new appropriations. They stepped into the field of supporting contracts on basic research that was a blessing beyond belief. The Army didn't have the means to do this, and the National Science Foundation hadn't yet been established. The National Institutes of Health were in a relatively small stage at this time and had their own programs so THAT ~~this~~ this ONR did a wonderful thing to back up the choice and fine projects in basic biological research, medical research, and medical research applied as well as basic, mathematics, ordance--all sorts of things. They have continued to the present, but with diminishing activity in this field because the National Science Foundation has gotten so big.

<u>Is Dr. River's hand in this?</u>

No. Rivers at that time was out in the Pacific. Ore Reynolds was, I think--he was the chief one, but I don't know who led it. I have never located a history of that organization, but I know Ore Reynolds--he stayed in research and went way up top in the Defense Department's Research and

Development Section. He still is a director of some foundation now--I don't know what, and I do not know the naval officers who had the wisdom to do this. I'm sure that Dr. Rivers if he had been consulted, would have been back of it, and maybe he was, but I don't know that for a fact. Well, now ONR eased the situation very much because instead of these contractor research projects falling down because they had no financial props, the Navy could take them over, if they were good enough to be taken over, and they did. The Army Epidemiological Board was able to continue on about the same financial support that it had FROM THE ARMY. It never was great--it had two or three hundred thousand dollars a year, something like that. I have the figures. I don't know whether I showed them to you.

Yes.

The total for the expenditures on the Army Epidemiological Board from 1941 to 1946, was about a million and a half dollars, as I remember. I'll check that later, but it's low.

The other big thing coming up at this time was the momentum of the research effort and the need for continuing research on nuclear physics--well, there was a revolution in physics going on, but that was the practical push to it. All sorts of things were just beginning to be talked about--solid state physics we knew nothing about until about this time, transisters, and the atomic bomb research was immense beyond the knowledge of any but a few people, so revolutionary and important that it seems trite now to say so. It had been going on in this country since the 1930s more or less, and very intensely carried on after Enrico Fermi and Karl T. Compton got a chain reaction under the grandstand at Chicago. Albert Einstein wrote the President a letter saying that this energy released from this reaction indicates that you could make an explosion of enormous power, practically suggesting a bomb to President Roosevelt. I think that was in 1939, or 1940---somewhere around that time. That work became known as the Manhattan Project.

Now we talked about the Manhattan Project but we were not sure what it was. It turned out later that it was conducted by the Army at Oak Ridge and out in western places under the direction of General Grove--what was his first name? I've forgotten his first name. [Major General Leslie R. Grove] He didn't take the Surgeon General into his confidence at all. I suppose nobody talked very much about it. The management of the Manhattan Project was centered in the Office of the Secretary of War in the hands of a Yale classmate of mine named Mr. George L. Harrison. He was down here a great deal of the time. Years before he had helped Senator Carter Glass draw up the legislation for the Federal Reserve System, and he was president of the Federal Reserve Bank in New York, a great expert on money. He dealt a great deal with the Chancellor of the Exchequer in England and the monetary policy of the time. He was brought down by Mr. Stimson to take charge of the matters of the atomic bomb in his office, just as Mr. Stimson had Mr. Harvey Bundy as a special assistant for other matters. Mr. Harrison didn't talk, or tell me anything. I didn't know about this until afterwards; in fact, I knew very little about it. The Surgeon General knew very little about it until it was about over. I had an inkling once when Dr. Stafford Warren, who was one of the medical directors of the Manhattan Project, came to the Surgeon General's Office, up to Preventive Medicine to get a lot of the kind of vaccine you have to give people going into the Pacific area. This was for the crew of the SS Indianapolis which carried the bomb. We supplied him with the vaccine, probably in July of 1945.

To repeat the dates of the bombing--the 1st atomic bomb was dropped on

Hiroshima on August 6, 1945, and the one of Nagasaki on August 9, 1945. We knew about that very soon. I can't remember the publicity in the paper, but it was known. They couldn't conceal it.

No--not very well. I think there was a White House announcement.

We got anxious--General Simmons and I, although he wasn't there all the time, but we thought the Surgeon General ought to begin at least to do something to protect the soldiers against blast and burn and radiation. Without talking too much to the Surgeon General about those dangers at that time, we talked to him--this is General Kirk as Surgeon General--about his need to know. One day when General Simmons happened to be away--the fact that he was away had nothing to do with what we did--the matter came up casually. Brigadier General Hugh Morgan and I were talking this over, and we thought we'd best go down and see the Surgeon General at 1818 H Street, a floor or two below, and we did. General Kirk said that he would be willing to go with us to see General Groves. General Groves had an office in the War Department building which is now the present State Department Building, 22nd and K Street. We walked over there after getting an appointment with General Groves. General Groves was just as frank and nice as he could be--just as if nothing had ever been concealed. He showed us pictures--one of the first pictures of the bomb bursting over Hiroshima, the mushroom cloud and the damage that it had done. He agreed with us that it would be advisable to get a group of medical officers and biophysicists into that region as soon as possible to see what could be done by a study. We knew that some Japanese studies had been made, that there had been a temporary look in by I think--some American officers, perhaps Shields Warren, and perhaps Dr. Stafford Warren from Rochester too. General Groves said that he thought that was a good idea.

Morgan and I went back to the office and drafted a radiogram to General MacArthur's headquarters for General MacArthur through General Guy B. Denit and took--it was signed Kirk--but we took it back to General Groves, and he said,"This is fine. Would you please ask the Surgeon General that he not limit me to saying merely that I concur with this. Ask him if he would allow me to be co-author?"

That message went out signed Kirk and Groves--I guess General Kirk was senior, a Major General by that time, and I think that that is the basis for Colonel Oughterson's commission.

Ashley W. Oughterson was a Yale man, and we knew that he was out there in the Pacific. He'd been interested in radiology. He was a wonderful man, a good surgeon but he himself, according to this story in ways that we didn't know anything about, had already made a proposal that is described in here that they send a commission in to study it. This is not an attempt to say who did it. I'm just putting this in to show that in several places in the world people were thinking about the same sort of thing. Colonel Oughterson and Major Averill A. Lebow, a pathologist, Colonel Shields Warren, a biophysicist and a medical man of distinction, and others went into Hiroshima in September 18, 1945, and worked there until late December, 1945.

Their being there led to the establishment of a commission still going on called the Atomic Bomb Casualty Commission, the ABCC, ADMINISTERED BY THE NATIONAL RESEARCH COUNCIL. It's now under the charge of Dr. George Darling. After years of difficult times, it is coming along. It is supported, I think, about at the rate of a million dollars a year, by money from Navy, Army and the National Science Foundation. It's done superb work. It's got a big laboratory and a big hospital and great records of cases of people they've been able to trace. They're looking for late effects of the radiation. The chief late effect is the occurrence of an unusual amount of leukemia among some of the younger people who were exposed and keloid, big scar tissue, among some of the women. The bomb has been a tossed ball of propaganda by the Japanese and the communists and we've had trouble with our medical people working in that region because the Japanese hate the people who burst the bomb on Hiroshima, and they attach the guilt to all Americans they see. Fortunately the relations between the Japanese medical profession and medical officers there have been good, and good things are coming out of it.

Well, we have ONR, we have the Army continuing. We have the National Institutes of Health growing up in this period right after the war, the Atomic Bomb Commission, the continued studies on nuclear physics, continued enormous studies on the physiological effects of radiation at Oak Ridge and other places--at Rochester, an enormous development of laboratories in the medical school at Rochester to study the effects of radiation.

The final development, I think we should mention here, is the foundation in 1950, by an Act of Congress of the National Science Foundation. It was delayed a long time. Vannevar Bush proposed it essentially in this book which was published in 1945. Yes, this book, Science: the Endless Frontier, was published in 1945, and it had the germ of a science foundation in it, but the people who were putting up the National Science Foundation from the start said that it should be an agency independent of the government. They didn't want any government officials on their board of directors, or any hand of the government put on free, scientific inquiry in the country. They were afraid that applied research would take altogether precedence over basic research on which you couldn't put a price tag at the moment. There was much bickering and otherwise back and forth for nearly four years. Then when the National Science Foundation started, its appropriation was far less than was asked for. They had trouble, but they've grown and now have an appropriation of about five hundred million. I don't know what it is--five or six hundred million, and the National Science Foundation under a friend of mine named Alan Waterman--do you know him too?

<u>Yes.</u>

He's a good man.

<u>Yes, he is a good man.</u>

He's retired lately as the director. I should say that they came out of the war with a good, strong organization for the future development of science and training in this country.

<u>What was the experience with OSRD that saw OSRD go out of existence? Was that just a statutory thing?</u>

Yes--originally it was the Office of Emergency Management. Then it was National Security Research Agency for a while. Office of Scientific Research and Development was one echelon under that, and when the war was over, appropriations stopped. Other agencies carried on, and a lot of the contracts of OSRD were picked up by ONR, especially in the malaria field. They were spending about three millions to four millions a year looking for drugs, but the unfortunate thing about that was that when the war was over they thought that they had all the drugs they needed to control malaria and mosquitoes. Well, mosquitoes got so they could chew DDT and enjoy it. They were resistant to it. The malarial parasites, as we now know from the Viet Nam experience, the <u>falciparum</u> type is resistant to chloroquin and antimalarial drugs that were thought to be more powerful than quinine. Quinine in large doses will still get this malarial parasite, but the dose is so large that it makes the taker sick. The whole emergence of resistance to insecticides, and pesticides and parasite resistant to antiparasitic substances was rather new. It came out after the war with a bang. We have a paper in front of us entitled "How Magic is DDT?" We couldn't tell how magic DDT was. General Simmons thought it was so magic that it was something that was created by the Lord. It probably was. It was endowed with powers of divinity in General Simmons' mind and in the minds of all of us, but cockroaches, mosquitoes, and flies--all sorts of things are resistant to DDT now.

A new form of variability.

You can see that and the Army's interest in basic research--this is much later--now, I'm getting, for the moment, into the 1950s. I was still connected with research in the Surgeon General's Office and with the Board, and we set up as huge a plan to study resistance in cockroaches, flies, insects that you could manage, and that took us into the basic physiology of the liver of the cockroach and how it detoxified DDT--lots of basic problems in chemistry and physiology of the insects worked out under government supplied funds.

What position did the National Research Council have after the war? It had been certainly a channel through which the Army....

I happen to know about the history of the National Research Council from having written something about it recently. I was the chairman of the Division of Medical Sciences in 1933, and I know that its history is largely one of an advisory body lacking funds and real authority. The first body formed as an advisory body in science to the President was the National Academy of Sciences, founded in 1863, with the approval of President Lincoln and an act of the

Congress in response to a group of scientists proposing that such a thing be done. The National Academy of Sciences with great distinction went on its way from 1863, living up as best it could to its directive from President Lincoln which said that it should experiment, investigate and report in the fields of mathematical, biological and physical sciences, including agriculture, and all things that are important to the security of the country, and it did a fairly good job on a whole lot of things.

It was the main adviser to the President in a statutory manner, but when World War II came on, President Roosevelt for reasons that I can guess at, and I won't talk about them too much because it's largely gossip, decided to bypass the National Academy of Sciences and form the National Science Research Committee, NSRC under which was the OSRD. OSRD for the medical field set up a special Committee on Medical Research.

Now there is a step in here that I have left out--the National Research Council which was created in 1915, not by the Congress, but by the National Academy of Sciences. The National Academy of Sciences in 1915, feeling that we were going to get involved in World War I in Europe, wrote to President Woodrow Wilson and said that they would like to do anything they could to help the government, and he replied, possibly in words that had been furnished him, that he would like to see the National Academy establish a research council with rather special interests and objectives to investigate and report and advise on things of importance to the government and having to do with the war, or possible war, and that's where the National Research Council came in. It was not established by an Act of Congress. It is a creature of the National Academy without statutory basis.

In 1919, when the war was over another exchange took place between the President of the United States and the President of the National Academy of

Sciences by which clarification was sought. They wanted the National Research Council to continue, but they'd like to have some defining statements from the President as to his outlook on it, a reorganization of its own organization— in other words, more divisions created, more people brought in, more authority to raise funds. They had no money. They wanted more authority to raise funds for grants, get it by foundation grants, individual grants and even some appropriations from the government. They even went so far as to get a ruling from the Attorney General as to whether it was legal for the Academy to have an organization like the National Research Council acting for it in these matters. The Attorney General approved everything they wanted to do, so in 1919, the National Research Council was reorganized by the National Academy of Sciences in the terms of an Executive Order of May 18, 1919, issued by President Woodrow Wilson in which he stated what he'd like to see done, but he didn't order that it should be done. It's called an Executive Order. I have a copy of it. This strengthened the National Research Council very much, and it's gone on generally in that way ever since.

In the war, [WORLD WAR II], it turned over its quarters and much of its staff to the Committee on Medical Research of OSRD. For instance, the chairman of the Division of Medical Sciences of the National Research Council was Dr. Lewis Weed who had been dean at Johns Hopkins. He kept an office over here, but the chairman of the Committee on Medical Research of OSRD was Professor A. Newton Richards, a very distinguished man of the University of Pennsylvania. He set up an office down here, and he was really superior to Dr. Weed, but again, among fair minded men and general people who were patriotic, who wanted to do the right thing for the country, that didn't make much difference. We had a very great respect for the National Research Council for two reasons. It had the approval of the President, the so-called Executive Order. It was an official advisory body, and as I think I have said before, it practically became an element in the chain of command. The Surgeon General did not adopt the vaccine for, say, tetanus, or yellow fever without first consulting the National Research Council, and it continued to consult the National Research Council all through the war, even though there was a Committee on Medical Research, and with great help to the Surgeon General.

The National Research Council appointed a great many advisory committees. I was talking to Dr. R. Keith Cannan the other day about these matters--the present chairman of the Division of Medical Sciences. I was asking him where I would find the recommendations and the rulings in the adoption of tetanus toxoid as the immunizing agent used in the war, immunizing against tetanus. He said,"B-J, we've never written any of these things up, but you'll find some of it in the Minutes of the Committee on Pathology, some in the Minutes of the Committee on Surgery, some in the Minutes of the Committee on Immunology, and some in the Minutes of the Committee on Bacteriology."

In other words, the National Research Council was doing things the way we did them on the Board very much without forcing one subject to go under one heading. It's too bad that they haven't written up all these things. They have piles and piles of records down in that NRC office--their deliberations. They had a hand in almost everything that was done. For the malaria studies we were most closely related to the Committee on Medical Research of OSRD. General Simmons was a liaison officer to both the National Research Council and the OSRD Committee on Medical Research; in fact, he was a member of the Committee on Medical Research and held an appointment as liaison officer direct from the Secretary of War, but that got so busy for him that he got Dr. Thomas Turner appointed as an assistant for this.

In terms of that summer of 1945, with the nuclear problems, you were going to need even more in the way of research for something which had been unknown and had just burst on the scene in two instances. It changed the whole nature of the game in a lot of ways.

Yes. Nobody knew how to protect against radiation, or to protect against fall out. It was a great stimulus to Civil Defense, but Civil Defense people were at a loss to know what to do.

They had insufficient continuity.

No, but they don't know yet.

No, and in terms of that early chapter that you wrote, the likelihood of a medical department being submerged in the light of the atomic bomb and subsequent events in terms of the station of American troops abroad with continuity seemed to foreclose--you know, the diminution of the importance of the Surgeon General's Office, as had happened in that early chapter.

I know from my contact with the Surgeon General that his line of research has been on the treatment of burns, a great deal on the search for some drug that could be taken in advance that would protect against radiation damage, or if the body had been irradiated, the drug would be a palliative, maybe a cure. Enormous work has been done on that. The Surgeon General--this is jumping way ahead--put a reactor in Walter Reed so that they can get direct radiation of animals. I don't think they're making any isotopes.

Did the Typhus Commission continue?

No. The Typhus Commission continued to about September, 1946. I closed up as fast as I could after the war was over by bringing officers home and getting them off the Commission and either reassigned, or separated from the service by maybe July. Certaonly by August of 1946, all of the people who had been serving on the Commission, except myself, were separated from it and probably separated from the Army. I was separated from the Army in September of 1946. I drafted the Executive Order, and Colonel Tracy S. Voorhees helped me, that President Truman signed disbanding the Typhus Commission--I think I spoke about this the other day, and I have further amplification of it in letters from Mr. Patterson who was then Secretary of War which put me in charge of the records and the final settlement of the affairs of the Commission. Fortunately the Commission had never had a budget so I didn't have a nickel to account for.

The Army Epidemiological Board continued....

Pretty much the same way, though it has grown a great deal since then. The Army Epidemiological Board has twelve commissions now and a far larger budget than it ever had before, but it is really--it became in 1953, or thereabouts, the Armed Forces Epidemiological Board. It got a charter from the Defense Department which was supposed to bring together the research interests in this field of the Army, Navy, and Air Force. In this charter the Army is made the managing agent, so the Board's headquarters are located in the Office of the Surgeon General of the Army, and the Army being the agent puts up practically all the money. The Navy and Air Force have put in very little money, though they get plenty of benefit from it. They are very appreciative in all they say, and they furnish a lot of information to the Board.

The Board has continued, but it has come under a fairly close surveillance of the newly formed Research and Development Command in the Office of the

Surgeon General. There used to be a Division for Research and Development, but now they've got what they call a Command which has taken under its control, so to speak, all the research laboratories of the Surgeon General and all the research that is done in those laboratories--Fort Knox Laboratory, some in Quartermaster, some at Edgewood, and the Armed Forces Epidemiological Board has to process everything through that command--all the research done in Germany and a great deal of effort to have research done in all the general hospitals of the Medical Department is under the Research and Development Command. It's a very large affair now. It has about thirty-five million dollars a year for research and development.

<u>During the war was there much liaison with the Navy? We've seen it in the Typhus Commission, but ONR bursts on the scene.</u>

We must have known a little about that, but there was not a great deal of cooperation with the Navy. Again, I know a personal thing that was done from long friendship with Dr. Rivers. Dr. Rivers was put in command of the splendid Naval Research Laboratory called NAMRU #2, I think, first on Saipan. About 1942, or 1943, Rivers was made a Commodore. In New York he collected material, fabricated buildings, and had them all shipped to Saipan. He set up a large research establishment out there. The reports, however, that got to the Surgeon General were carbon copies that Commodore Rivers gave to a woman lieutenant. She was going back and forth, and when she came to Washington, she would give me a sheaf of these reports. They didn't send copies to the Surgeon General of the Navy to be forwarded to the Surgeon General of the Army, but he and I exchanged material, I gave him a lot of Typhus Commission stuff.

He was certainly operating out in the South Pacific, but there's no basis so far as you're able to recall at the moment, for the emergence of ONR, except Reynolds.

Reynolds is the one we had connection with. I say I don't know the background of the higher command that brought on ONR, but it superseded a Navy research organization that had been in effect.

Does that date jibe with the termination of OSRD? It does, doesn't it? 1946?

Yes.

They had continuing grants.

They were taken over by ONR mostly.

What begins in 1940 mushrooms.

Oh, it was a big time.

The planning, sampling ideas about organization for post war years must have been pretty rough--you know, given this curve in terms of people, the paper requirements of reorganizing the office, and to know that you were going to have to deal with the problems, even if you had two thousand men in the Army.

Oh yes.

The chart here from January 1945, to October of 1945, shows a streamlining.

It was greatly decreased. I think these charts are quite graphic. You save a lot of words.

Yes, and some agony too--I should think.

Oh yes.

What was the attitude on the part of the Army people toward Dr. Bush?

I wouldn't be able to say.

Wouldn't have any idea?

I could tell you about my own. I have--I said that I admired him very much. He was a fascinating man, most original, vigorous, tangy.

Tough. His thinking went way beyond matters medical into basic research in the physical sciences.

Yes, physical sciences, but applied stuff. He envisaged an encyclopedia of it that you could just throw into the desk console of his called "Mimex", and he could retrieve anything from it that he wanted. He was all into computers--way ahead of things.

That's another thing the fact of war demonstrated--the increased equipment in the signal corps, radio equipment, measuring devises, and so on.

The great thing in the war was radar.

The application of this new electronic knowledge subsequently in research where you needed new and more complex devises. I don't know, but I suspect that the Surgeon General or yourself, as deputy chief of Preventive Medicine, stood with reference to the new problems in 1946, pretty much where you stood as chaiman of the Scientific Advisory Board of the Childs Funds, with the knowledge that there was a lot of basic work to be done where the connections between the basic work and the field may not have been clear, but it had to be done. I suspect that was probably in the thinking anyway--that we needed to scratch the surface in a lot of things even before we understood their possible relevance.

Oh yes--sometimes the possible relevance was perfectly clear, but sometimes the investigator was just driven by curiosity--he was going to find out.

Then too, when you read the reports of Dr. Dingle's group and the kind of detail into which they went--they had the opportunity to function.

All that of Dingle's has been carried on ever since he left Fort Bragg out in Western Reserve. Last year he and his collaborators published a great book on health in the home. The Chief of the United States Air Force announced the other day that he had copies of that sent to all his officers. It has to do with respiratory diseases in closely living communities which is easily applied to a situation on a ship with people altogether. The Navy has been very careful with it. On shipboard they are horrified if there is the slightest suspicion in a man of tuberculosis. There have been some secondary cases on a ship. Then they have, like the Army, a problem in diarrheal diseases on shipboard when latrines get infected.

I find that the war set a premium on certain kinds and qualities of scientists, gave them a chance to really run with the ball, in effect. There's relatively little in terms of the institutional leadership--universities, colleges, who, in terms of the research development during the war, were the recipients of funds, grants which enabled continuing work to go on. Yet they seem unrepresented, without a voice. I don't know. I don't understand it, but the the scientists really, I think, came alive during the war time period. They were the fair-haired boys--I don't mean any disrespect in that term. I don't know whether Conant talked and acted as a scientist first, or as an administrator.

I'm sure the universities from the start were worried about the possible loss of their independence. They couldn't avoid serving their country, and they couldn't impose their conditions on it. They couldn't say that you must not do this because it isn't in the spirit of the free academic life, but after the war they could, and some of them actually refused to have any more classified work done on the campus. They wouldn't do that. Now, they really have the problem--they're so overwhelmed by the largess of the National Institutes of Health in this field that half of their academic budget, or more, comes from outside in the sciences.

In 1946, was there any inkling that you knew of, or was it in the air--this growth of NIH. Rolla Dyer--his experience had been in the Hygienic Laboratory....

The National Cancer Institute was founded in 1937, so that they already got the plural on the institutes. The other institutes were coming along. I think it's clear from this letter of President Roosevelt to Mr. Vannevar Bush that he's inquiring about how the government can operate with the private agencies, what can the government do now and in the future to aid research activities by public and private agencies. He's still thinking that it's aid they're giving--not control.

I go back to the Dean's Office time at Yale when not a little of your energies were channeled into these areas of raising funds to support research. I remember that the Dean's reports show a steady increase in outside funds for the support of research.

I never was much good at it.

<u>But it grew and helped sustain research at Yale in the 1930s, and it certainly pointed to the fact that outside funds were necessary.</u>

Oh yes. Pitiful academic budgets. They didn't have enough money to raise decent salaries.

<u>Right--you know, with the war it was a kind of continuation. To be sure with a single interest--the government. I don't know that any foundation would be comparable to what has happened in government support.</u>

No.

<u>Look at it from the point of view of the scientist. He's had the opportunity to go back to the pre-war order of things, or to do something to maintain opportunity. That must have been a simple choice for many of them.</u>

We got it all right.

<u>Let's call a halt--all right?</u>

Tuesday, June 14, 1966 A-60, N. L. M.

This is the first time that we've met in the morning. We should both be just sparkling.

Yes--that's right.

We've talked briefly before we turned this machine on in two areas really--the termination of the war, and I wondered the extent to which any comparative studies were made with respect to Preventive Medicine in the enemy forces--Japan and Germany. Also whether there were any responsibilities imposed on the Preventive Medicine Service for American prisoners of war--where continuing medical problems might present themselves both as sources of care and studies for information--whatnot. I don't know. You indicated that there wasn't much, but it might be an area in which you might care to comment.

After the end of the war with Germany there were tremendous problems of handling refugees, displaced persons, and a group that we called RAMPS--Recovered Allied Military Prisoners of War. All of that involved application of preventive medicine for the care, feeding, and control of communicable disease among the millions of refugees and displaced persons in Germany; in fact, it was one of the main problems of Civil Affairs-Military Government, taken over from the Army in Germany and was an arm of General Clay, and in General Clay's book he has a paragraph saying that the medical service rendered an extraordinarily useful service during this period in the handling of all these people. It was an enormous problem because there were far more refugees and displaced persons than there were American troops, and yet they were still handling some of these situations on the basis of troop strength for issue, and there were places where, we'll say, there were more German prisoners of war to be fed than there were American troops in the area, but the supplies coming from the Quartermaster were based on the troop strength so that there wasn't enough to go around. Problems of that nature involving Preventive Medicine continued right on through until after 1946--I had something to do with the handling of the problems from our office up to, say, September of 1946.

They weren't insoluble problems. They were largely matters of supply, organization, and personnel. It took a good many people to come in through the Allied Military Government to manage these affairs and nearly all these Allied Military Government groups; notably the Americans, had fairly strong Preventive Medicine Sections staffed by officers who had some preventive medicine experience in the Armies that captured these areas and in the corps and divisions that were involved in the overcoming of Germany. They left behind through this organization of Civil Affairs - Military Government, a public health establishment that continued for some time and was related more or less to the Preventive Medicine Section of the Surgeon General's Office, but also had other connections because Civil Affairs had been a part of the Special Staff of the War Department, and they had other avenues of communication and supply. The people were in great distress in those areas and much had to be done to safeguard the people themselves and our troops in their midst from outbreaks of disease that might have been bad. For example, water supply systems were all destroyed--Berlin and most of the regions from Berlin to the west, that part of Germany--there was great destruction of water supplies, depletion of food supplies, depletion of clothing, lack of soap, lack of bathing facilities, and lack of food. There was a great deal of undernourishment which would make people susceptible to disease. All of that was understood and remarkably quickly conditions were improved. There were some excellent men in charge like Colonel William L. Wilson of American Civil Affairs Public Health, and the British had a somewhat similar organization, but they were not so much involved as we were with the masses of the people, except for the British in the enclave around Hamburg. What's not realized also, not realized until you read the records of the Air Force strategic bombing--have you ever seen those records? The strategic bombing of Berlin, Hamburg, Peenemünde, Dusseldorf--all those cities were the objects of very destructive raids which left people in destitute conditions, and they left considerable problems, but fortunately great shipments of food were available from Army stores and additional things, and it was less than a year before conditions of health in occupied Germany were in pretty good shape.

The syetem applied was the system of utilizing the German public health organizations and regulations as much as possible and to use also the German public health personnel as much as possible. A hitch in the use of the German personnel was the denazification policy--anybody that had Nazi sympathies, or had been in the Nazi party, couldn't be used for this work. Many times the work was impeded by the fact that there was a rambunctious Nazi in charge of it who didn't want to cooperate. It was impeded also because it was hard to find people to work in all these subdivisions of Germany, but that cleared up, and I think the problem of refugees and displaced persons, while enormous, passed fairly soon out of the ken of the ordinary military preventive medicne that we've been talking about in the war because other organizations were formed to take care of the relief necessary.

The story of the American prisoners of war who had been in German camps is complicated and contradictory. I forget how many thousands of Americans were captured, but it ran to a large figure--a hundred thousand, or more, and they were scattered in camps in the mountains. You probably met some of them in the Austrian region. In some of these places they had been badly treated--mostly by a limitation of their food supply. There was actual starvation, malnutrition among American prisoners of war. They were liberated as rapidly as possible and taken care of through a dispensary Army system, or through hospitalization, if necessary.

Not much was known at that time about the proper way of feeding a starved person, or a depleted person. Sometimes they got these liberated prisoners and filled them too full of chocolate malted milk, all sorts of meat and stuff, and they weren't quite ready to digest them. They got sick. On the other hand, the limitation of food for a starved person need not be as strict, it was found out a little later, as they thought at that time. The best examples of studies of this were the liberated people in Holland. In northern Holland the Germans under Arthur Seyss-Inquart really set about to starve these people to death. When we liberated the Dutch, the famine discovered was almost unbelievable. Well, they began to feed these people with pre-digested protein material obtained from England which they could inject. That was intravenous feeding material that is used in surgery everywhere. It was found, however, that a judicious, slow increase of the ordinary food taken by mouth was a perfectly feasible and wise thing to do. The people had digestive enzymes that were still evokable, I'll say, because although they had not been in use much, nothing much to work on in an empty intestine, or stomach, the machinery for making them was there, so it only needed recall. They found out that they could feed them by mouth fairly well, and they tried that on our returned prisoners also.

The returned prisoners on the whole, in my opinion, were not in too bad a condition. It was starvation chiefly. They hadn't been treated with any particular brutality. So far as I know, they were not treated as the Japanese treated our prisoners. I happened to be involved in a study of the after effects of the imprisonment on civilian and military personnel both by the Germans and the Japanese. There's an organization in the United States set up to present to the Conbress claims for lasting injury. That's it.

You have given me this report that this committee made on this subject. It contains a section--I can't put my hands on it at the moment--of the American prisoners of war that were in German prison camps. It was about as I have said. It was not as bad as one expected, and this study showed that there were not many recognizable after effects ten years or more after these people had been released and back into work. I represented the Department of Defense on a committee composed of people from the Navy, the Veterans Administration, and the Public Health Service, and we made a report to the President which he presented to the Congress. You might want to make a reference in the margins to that report. /"Effects of Malnutrition and other Hardships on the Mortality and Morbidity of Former United States Prisoners of War and Civilian Internees of World War II: An Appraisal of Current Information" House Document No. 296 84th Congress, 2d Session (Washington, 1956) 69 pp./ People use that report as a basic document because there's nothing quite like it.

For the study of the prisoners of war recovered from the Japanese, the Surgeon General sent to Japan Brigadier General Hugh Mortan. He went to Japan and through the country and studied these prisoners just at the end of the war, and he got a great deal of physiological and medical information about them, published one or two papers on the subject, and found that the main trouble was malnutrition, starvation, some effects of exposure, and psychological depressions because the Japanese were brutal. There are well authenticated cases of their handling prisoners of war--astonishingly brutal. One group of our prisoners of war were sent from Manila up to Formosa, I think, crowded in a little, hardly ventilated cabin in very hot weather, and many of them died just as they did in the black hole of Calcutta. Some of the prisoners were beheaded with Samuri swords. Some of them had their legs cut off while they were standing up by big swords hitting them. Several of them were fastened with wire, or ropes on the bow of a submarine, and the submarine would submerge for a while and then come up, get a breath, and go down again. Nothing like that was done to our people by the Germans, and we didn't do anything like that to the German prisoners we caught, except for one instance of their being put in a hermetically sealed box-car--I told you about that, but that was an accident.

Now, that's about all I think I have to say about the refugees and the prisoners of war. You can see that there were ordinary preventive medicine problems involved. There were no new principles.

<u>No. This would indicate that there was continuing concern with the effect of malnutrition and brutality on prisoners of war.</u>

This report indicates that the concern naturally was by the government, but the chief agitators were a group of representatives of these people who were continually introducing claims for indemnification and getting representatives and senators from their district to present bills. It was something to be exploited rather than the fact that they had any particular reason, medically, to do this. I'm right. Right here--"approximately a hundred thousand members of the United States military establishment were captured in the European and Mediterranean Theater, about twenty seven thousand by the Japanese." The survivors living here had this organization defending their own interests and pushing for a bonus type of thing.

Naturally the Medical Department, the divisions of medicine in the Surgeon General's Office were interested in the knowledge of the nutritional state of these prisoners returned, and Preventive Medicine had a Division of Nutrition in our service. In 1944, it was established, but it had been partly there before, and even before that there was a Division of Nutrition in Professional Services under Colonel Paul E. Howe who went over to England and helped in the European Theater. After 1945, whenever it was heard in the European Theater that prisoners were released, or were in an unhealthy state, the Surgeon of the Theater, General Hawley, would send a group of investigators from his office. One group, I know, went out under Colonel Wendell H. Griffith who was the chief nutrition officer. Another group under Colonel Herbert Pollack went to camps in France where American returned prisoners were. They made studies of vitamin deficiencies, hemoglobin, blood anemia, weight studies--just the way nutritional studies would be made, and they found some vitamin B deficiencies.

<u>This would indicate that there was some effort to take what might be used as a scare headline and convert it into something knowledgeable.</u>

Yes.

<u>Yes, and as of this time, I wonder if there were any efforts made to assess the condition of the enemy Army in preventive medicine in the field as compared to what the Americans had done. I'm thinking specially of a book I read the other day, though it is not limited to that subject--the Cold Injury book. This would be whether the Germans in their own thinking and development--they're not an unimaginative people--whether there were efforts made to assess</u>

their experience. You indicated before we turned this machine on that there were a number of teams investigating German records and so on which were kept, but I don't know--you indicated that there wasn't much done, but just thinking about it and with specific reference to cold injury which was a--well, it's a horrible book, but a very interesting one.

I'd like you to get in the record the whole title--it's Cold Injury Ground Type by Colonels Tom F. Whayne and Michael E. DeBakey. Now cold injury ground type means that it occurs in the foot soldier, as a rule, and it is synonymous with immersion foot. The reason it is put in there as ground type was because it occurred on the ground about sea level, or maybe up a little bit, and it differed a good deal from the cold injury air plane type where they actually froze from exposure to the cold without being wet.

High altitude--yes.

Yes--high altitude, but this cold injury ground type that Colonel Whayne and Colonel DeBakey studied and about which they wrote such an extraordinary book was the injury that followed exposure to wet and cold on the lower extremities chiefly. These men didn't have proper shoes, and they didn't have proper socks to keep out the wet in trenches, or fox holes where they were. They didn't have enough socks to change as they should. There was a great--did you read about the Quartermaster, Major General Robert M. Littlejohn, in England who neglected, or he didn't listen to the medical opinion on the need for socks to be stockpiled in advance. That's why our men didn't have them.

Apparently in Italy at least, the Germans didn't have among their troops cold injury. I have seen records where German medical officers in hospitals that had been overrun by our troops would ask to be taken to American hospitals so that they could see this condition, said that they didn't have it among themselves, and I think that's because they had better foot discipline, better socks and more of them, and probably maybe better boots. It's a very difficult problem to handle, except by foot discipline and a chance to change socks and rub the skin because if you make an impervious boot, as we did, shoe packs, as they called them, the foot stews in there. The skin peels and comes off--just like boiled fish. It was very serious--this cold injury ground type was very serious in Italy and in the Luxemburg bulge region. Preventive Medicine people on the ground over there--Colonel Stone notably in Italy, Colonel Gordon in the ETO, and the teams that were sent out--were all aware of what should be done and made urgent recommendations. The Preventive Medicine Service in Washington was informed of these conditions and tried to help and went so far, looking ahead, as to send Colonel Gordon and Major William L. Hawley to the Philippines in probably June of 1945, July. As soon as Colonel Gordon could come back from Europe, he was sent to the Philippines to try to begin to prepare the medical service and the line officers to combat, or prevent cold injury ground type among troops that were going to be sent into the invasion of Japan. It was expected that the terrain in Japan would be conducive to the recurrence of this type of disability unless something were done. They had the thorough cooperation of General Denit, the Surgeon of the Army which was destined to go into Japan, and he helped organize a team and make plans that would be ready to go with the invasion forces. It wasn't necessary.

<u>Your view is that there either wasn't time, or interest, or necessity to assess German preventive medicine.</u>

Well, speaking for myself--I don't recall any special effort except this incessant demand of the Medical Intelligence Sections of Army Service Forces chiefly, to send collecting missions over to Germany to get any kind of information that we could--they collected drugs, dressings, medical kits, manuals, mimeographed sheets of paper which by the time of my separation from the Army amounted to a great mass of materaal which, as I recall, none of us really studied. The push on the study came through the desire of the War Department and the Army Service Forces to make available to commercial people in this country German manufacturing processes, German patents, German discoveries of new materials not in the medical field especially, but in the field of commercial interest and economics. These missions were very demanding and scoured all over Germany, and, as I say, sent back a lot of material which was examined sometimes and sometimes not. I don't recall any special contribution that either the Germans, or the Japanese made to the control of communicable disease, or preservation of the care and health of soldiers in the war.

<u>A view you expressed before we turned this on was to the effect that German medicine at one time had been a leader, but the organization of the state, or the position of medicine within the state, these reasons or something else again, had reduced it.</u>

Yes, it deteriorated--possibly as a result of the policy of the central government, or possibly because good medical people were used up in the Army which was very large in Germany and had huge losses. Also the Germans were cut off by their own actions, or perhaps by the natural situations of the censorship of countries from the literature, or the reports of medical progress in the United States.

<u>What about their research activities during the war?</u>

I know very little about that. I don't know of any special contributions that the Germans made during the war, except for their attempts in biological warfare--their experiments, their cruel experiments on Jewish prisoners where they were studying such things as exposure of the human being to heat and cold, putting a human being in water and bringing it almost to a boil, seeing how he could stand it, or submerging him in very cold water. The cold water experiments were motivated by the German's knowledge of what happened to a sailor, or an aviator who fell into the cold North Sea. The temperature of the waters there is down below forty, I think.

Rather cold.

Our people were interested in that too. The Germans experiments on exposure to cold, particularly, were examined after the war, and as far as I know, they didn't add anything special to physiological knowledge. The other experimental work in Germany that proceeded at this time was their effort to make typhus vaccine, improve their methods of typhus vaccine. They didn't have typhus vaccine made with rickettsia cultivated in chick embryos as was the source of the Cox vaccine that we and the British used. They used the old method of infecting lice and grinding up the lice and making a vaccine out of that. The usual way that they infected the louse was to strap him down with a little fine wire under a microscope and with a very fine glass pipet put typhus blood into his rectum. That was done. I have a great series of pictures of Hitler's fine laboratory in which that type of vaccine was made--I forget the name that is given on it [Weigl's vaccine]. Where is the Rickettsial Diseases in Man?

Right there.

Are we wasting tape--are we?

No.

Well, it doesn't give an account of that in here. I'll put it in when I read the proof of this.

The Germans knew about atabrine; in fact, they were responsible for the earlier introduction of atabrine, and they used it for malaria control. They did not have DDT, although it was right over in Switzerland near them. The German preventive medicine procedures had been laid down pretty well in the Franco-Prussian War in the 1870s. They knew how to prevent small pox by good vaccination, and they used that in this war, and there was relatively little small pox in the Germans. As a matter of fact, I'm quite sure that I'm not just being careless, or deficient in memory when I say that there was no special thing that seemed to come out of German preventive medicine during the war.

This has been the generalization, but scientific personnel had always counted pretty highly in the German state in the sense of the contribution they could make--I don't mean to say that they had position, but they were needed and were necessary. They certainly had a highly developed chemical and pharmaceutical industry, but again bombing and the ravages of war--their installations had been pretty well knocked out.

And also probably raw materials were short--things from which things are made were not so readily available.

And when demands begin to be made on a dwindling competence--first things come first, and apparently they went into Peenemünde and rocketry rather than other areas. What position did Japanese scientists hold in this war?

Relatively little. Japanese scientists that I know anything about didn't contribute anything during the war. They were probably engaged like the German scientists in reduced activities along these lines. Before the war they were in scientific strides of advancement both in the fields of gastro-intestinal infections and rickettsial diseases. They discovered the rickettsia of scrub typhus before the war. They had some scrub typhus in the course of the war, but I don't know that they did anything in particular about it. Their own conditions were very miserable--all that I have read about. Japanese detachments in New Guinea, in Burma, in the islands in the Pacific were more or less left to fend for themselves. It was a discipline of penury. It seems to me that they didn't have any real feeling for the welfare of the soldiers. He was there to die for his country, and it was hard enough for them to bring in enough supplies after a while. The Japanese by the time New Guinea had been turned, had lost about eight thousand of their pilots--all their best people. There was nobody left that was any good, and they reduced their pilot training, for example, down to about seventy hours. I happened to have recently had to answer the inquiry; what was the state of mind of the Japanese pilot? Well, fortunately there wasn't much written about it, except a section of a strategic bombing survey of the Air Force on the strategic bombing of Japan. There's a good section in there on the kamikaze pilots. They were volunteers. They didn't want to be called suicide pilots, and they didn't want to be called murderers. They thought it was the highest calling to sit in a plane and go diving down on a ship and to lose your life for your emperor and your country. They could use a low type of pilot as they did by the hundreds in that case because a man didn't need to know much about flying. He followed a guide war plane over to the region where the battleships were and then took off at an angle and swooped down. There wasn't any particular finesse that he needed, but they caused a great deal of havoc at Okinawa. For two or three days there they sank many a ship and killed many an American. They were not the best pilots.

I had the same impression about the Japanese Medical Services. Either they didn't have good men, or the good men were used up. Nothing shows up very much. I've read translations of Japanese accounts of malaria control in New Guinea. They had some interesting ideas like giving atabrine intravenously. They did do a certain amount to treat malaria, and they used atabrine a certain amount to control malaria. I think their quinine supply was even lower than ours. Quinine from Java and through there was pretty well cut off, and I don't know about the Japanese capacity to take the raw material and make relatively pure quinine sulphate out of it. Their sanitation in their camps was relatively poor from what I've seen in the records. I don't know what they did in the burned up cities such as Tokyo. They must have had an awful mess.

The Japanese talent was there. It came back fast once the war was over. As soon as the war was over in 1945, General MacArthur became Supreme Commander of the Allied Powers--SCAP I'm trying to say. He brought over, or Surgeon General Kirk sent over first Colonel, later Brigadier General Crawford Sams, who had been the Surgeon of the forces in the Middle East, one of the most remarkable public health officers that the Army military has produced, and in this supreme headquarters set up in Tokyo there was a Public Health Section under General Sams which did the most remarkable things for the benefit of the Japanese. For instance, they vaccinated the whole Japanese population--millions of people--with typhoid vaccine. They vaccinated the whole population with small pox vaccine. With relation to the Typhus Commission they practically deloused the whole population. This is in the period shortly after the surrender, and the period continued for several years.

The cooperation of the Japanese was wonderful in all of this. They furnished teams. They wanted to learn. They soon caught on how to make DDT themselves from what we taught them. They began to make their own vaccines, showed much enterprise and much interest in these scientific things. There never was any serious outbreak of disease even in devastated Japan. It seems to have been characteristic of situations all over the world all during the war--there was far less spread of disease than we would have expected. I don't believe that there was any serious cholera, or any serious plague in Japan at that time. There were good Japanese institutions that cooperated with Americans in preventive medicine work. It was not long before the Army Medical Service established in Tokyo and later in Camp Zama the 406 General Medical Laboratory which is a splendid laboratory--big, capable of doing all sorts of things from the study of air pollution to the study of water pollution to the study of infectious diseases. It was under the control of very able men most of whom had preventive Medicine in the Surgeon General's Office and in North Africa, and Colonel William D. Tigertt who is now the head of the Walter Reed Army Institute of Research. They had excellent staffs and people to work with them. It was continuous. You would have thought--well, the transitions were not sharp.

Then I suspect this reexamination of what the enemy had done gave way because so many new problems emerged that summer.

Yes.

<u>There was difficulty enough.</u>

Some of the difficulty in examining these enemy records was the difficulty to find enough translators. We were always looking for people who could read and translate German and Japanese.

<u>The only way I came into some records of German "scientists" was some of the records of the Nuremberg Tribunal where they used involuntary prisoners in fantastic ways. Whether there was a stated goal or aim, it didn't appear.</u>

They had aims, but they put it under curious, lay medical control some times. That famous paper by one of the special guard type of Nazi soldier for experimental testing of typhus vaccine--they gave vaccine to a lot of these Jewish internees, and then innoculated them and others that didn't have the vaccine with typhus rickettsia, typhus material. They had a high mortality, I think, from the unvaccinated, maybe twenty-some percent and very little in the vaccinated, but that was under one of those special guard type military policemen.

<u>I think maybe we've gone as far as we ought to go today, and we won't get into a personal assessment. I'll leave you free to think about that.</u>

Personal assessment? Is this thing running still?

Thursday, June 16, 1966 A-60, N. L. M.

I guess in some ways if we're conscious and we have an experience, we pick up some wisdom because of it. You've been in positions successively now--all the way back to the laboratory at Johns Hopkins, the University of Rochester, a much larger domain, to Yale--you know, with problems as untidy as human affairs usually are--and then this sudden expenditure of energy on a twenty-four a day basis, really, because you think, eat, drink, sleep, talk all about these problems during the war.

You've got that out of chronological sequence because of the war--oh, I see. You're talking about World War II.

Yes, and working always on a larger setting where problems are greater. I know when you go through them, you work from day to day. You have an idea where you're going, and what it is you want to attain, and you do whatever you can within the given time to achieve it. Human problems take time--sometimes more time than you want to put on them, or that you see the necessity for. Many things we have by way of experience we learn from, and we don't articulate what it is we pick up from the experience. We never do. We just carry whatever it is on to the next task--a broader way of looking at things perhaps, a greater sensitivity to people, a better realization of this elusive, magnetic thing called America--whatever. I wondered in personal terms looking back on the experience, what it all added up to in personal terms, working with men, working with ideas, working with reference to supplies, needs, pressure--you know, the location in which this particular human variable had to function. This is kind of arbitrary because there's no break in thinking about these things, really, but arbitrarily isolating the experience you had during the war, viewed against all that other accumulated wisdom that you'd picked up along the route--take them; institutional obligation, men and men, ideas, means--you use some very interesting words in the manuscript that I've seen. You haven't seen it yet--but questions of "leverage." That's indicative of what I mean. You get to the point where--assault. It's almost like the infantry in attack, I think. That's subjective on my part. I don't know that it accords with what you think about it, this period, or whether you've ever thought about it in these terms.

I doubt if I have ever thought about it in those terms because the terms that you use there imply a consciously conceived progression. My life as I think of it mostly is episodic. In other words, as I think back on it, I can say that I didn't regard any of these experiences, however complicated or long drawn out, as a preparation for something else. In other words, it was not a course of planned development. I hda no thought, for example, during the deanship that I was being fitted for the kind of work that I had to do in Preventive Medicine Service in the Army which, however, was very much like the occupation of the deanship. As we have said before, most of the problems of a medical school are very much like operating in the existing organizational activities of a Preventive Medicine Service in the Army. In a military setting the people, the names, and the titles are different, but the principles are the same. As I say, I can't think of any of this as being regarded by me as a training for something next to come. I never knew what was coming next except in general. I suspected after the war that I would go back to Yale which didn't turn out too well and was only a temporary staging, but I never expected after I had been through World War I to go back into uniform in World War II at my age, or that such an opportunity as I had in World War II would have been available for me. I think if you look back on these things as affairs in passing without trying to read into them any depth of planning, or any measurable future, you understand what I mean when I say that I think of my life as rather episodic.

Even granting the episodic nature--that is, the unexpected role that is thrown your way. To live is to function within what you find, and I would suspect that you'd sharpen both insight, pencil, wit, human--you know, knowledge, whatever the episode.

I think I had a good heredity, a good education, and I had a great advantage of being associated with, I think, noble people most of my life and able people. I accumulated a sort of culture as I was accumulating experiences--if you use "culture" in the sense that I'm trying to use it now; that it's not just a foible of wit, but a way of life, so to speak. Those qualities that I acquired through association, heredity, and education were reapplied each time with enough variation to meet the situation.

Another element I think you acquire is this--you don't just meet situations. You foresee them, and that's a little different type of mental activity--foresight and really reaching a solution of a problem before the problem has come across your path, or before you see it, or before it has occurred. Now some people can devise intricate philosophical and behavioral conceptions and methods and without actually being involved in an experience. I take it the philosophers, economists, creative artists do that kind of thing all the time. I think I went from one thing to another dealing with events as they occurred, but dealing with some events that were sure to occur because similiar events had occurred before. In other words, I was telling you that it's like going into a familiar room. As long as the door is shut, you don't see what's in that room, but you know pretty well before you open that door and go through it what you're going to find. You might find the chair and the table in a different place--well, then you just sit around, or move it around and put the chair where you think it ought to be, or leave it where it is. That's very poorly said, but that's the kind of thing I mean where you come across situations that you have not foreseen, can deal with very easily on the basis of past experience.

In all of these things I suppose I had the usual fits of temper that people have ~~in~~ which, in my case, sometimes make me think faster and better, and sometimes they are just confusing and distracting. As you go on through more and more frustrating, or difficult, or vexing experiences, you stop getting mad about them. General Simmons used to say,"There's a war going on. There's no time to get mad about these things."

That was good principle to acquire. For the medical side of it, I got no additional proficiency either in laboratory work, or clinical work, or in sanitary work as an operator, but I did have the advantage of having an immense amount of new information provided for me by my associates and the people I was working with like all these brilliant and able scientists on the Army Epidemiological Board. Being with them was like being in the midst of an active and productive faculty of original investigators all the time. The administrators on the Army side were sometimes very capable and enlightening men. Some of them were stupid and not any good. I look back on it as being very fortunately situated and having some very fine teachers around me all the time, people that contributed to my increasing knowledge even though they were not intentionally, or consciously carrying on such work as the stated purpose of their lives. In other words, they didn't care to train me, but they couldn't help but train me.

<u>I gather that you look upon mots of the things through which you go as a form of training--like a room. You've been in it before, but not quite this way.</u>

That's just what I think--that you pass through these different rooms. I don't think that anything is just as it was before. Everything is in the process of change--the Pythagorean notion. He said that you can't step into a stream of running water twice in the same place, but it's still running water.

<u>That's a very wise saying--it's all altered.</u>

Yes, it's all altered, and I could understand some of the early talk about relativity from being brought up on that Pythagorean notion. One of the early observations of the people who developed relativity and modern uncertainties of nuclear physics showed that just merely by looking at an electron, you can't observe these little things without altering their position, or their size, or their velocities. Well, that's true about these other things. Your running water is changed by your going into it, but it is still running water, if you want to abide by it.

Naturally, I'm sure I was very ambitious and always have been more or less competitive to the annoyance of some of my friends. I can still remember what my classmate at Hopkins, Dr. Frank Evans--he became a doctor--said. He said that the trouble with me when I was at Hopkins was that I always wanted to be ahead of somebody. I told him I couldn't help it. He thought I meant that I couldn't help being ahead. That didn't endear me to him.

<u>That's marvelous.</u>

I don't think I've ever done any particular dirt just to get ahead of somebody.

That would be, I think, unlike you--that is, unlike the you I've come to know through these papers. I can cite just the example of the length to which you went to aid the Associate Professor of Bacteriology at the University of Rochester after President Rhees's letter. You wrote--at least four different places--the nature of interets and concern.

Birkhaug?

Yes, and you have at least expressed in your feeling about microorganisms and rickettsia, respect for them, and to step on a man, someone you could see--I just don't associate it with you.

I don't know--I have shot birds, shot animals.

Oh sure--that's part of breathing too, but I think the general flavor is one of respect and regard for living things, but--you know, you chafe at delay. I would think that a huge organization in which you're attempting to effectuate a given aim, trying, in effect, to grease the ways in which that ship will float more easily and readily--the whole sense of delay in a military situation is something you have to anticpate and come to understand for what it is. It's not a preventive thing, not really--it's just that there's more to move--"leverage." That's why I picked up that word.

Well, in the military situation you meet delay and frustrations all the time. The reaction that I had, as I recall it now, to delaying influences was of two kinds. One was to be bored to death with the situation and to wait it out. Very often in active service, say , in the trenches, you'd wonder how you were going to get through the day when nothing was happening. You probably know what I mean by being in the infantry, and there are many other states where you're bored by the situation and can't do anything about it, and you have to wait it out. The other reaction is to attack it as furiously as you can and as wisely as you can with whatever weapons you may have, or whatever tools you may have. That is the one reaction that you would characterize by leverage. You can have wonderful leverage in your body, and if you know how to use it, you can lift huge weights, shoving it up on your thighs and your back as levers, where you can't use the lever of the forearm to lift a great many pounds. One is experienced with levers from the physiology and structure of the body, but in administrative work, leverage has to be devised. In the first place a fulcrum is very necessary. If you don't know the kind of orders, or whose they are, depending on whether the fulcrum is in the middle or behind, or above--the position of the fulcrum determines the way the leverage is applied. When you get out of this pictorial language and take a practical situation, you may find that you have to push somebody else into a move. As I think you recall, I asked Colonel Hudnall if he would kindly look in the drawer of his desk to find the authorization for the officers for the Army Epidemiological Board. That was a kind of gentle leverage to the colonel in charge of the Section on Personnel. There are other kinds of leverage that you use. As an example, the leverage to get the typhus supplies over into Japan through the mediation of a strong man named Lt. General Lutes. You inform your lever, and then the lever begins to act by itself.

<u>Yes--General Lutes.</u>

He did. He went straight to the Secretary of War about that and cleared it up. Another kind of leverage is through clandestine trickery, I might say, but I don't think we did much of that in preventive medicine. I certainly don't remember any particular episode where a result was accomplished by trickery in our group. It's said to occur in the universities. As one very famous scientist told me once--he came to see me and told me about the diversion of money intended for a herd of Arabian horses in his university to other purposes. This man wanted the Arabian horses project carried forward, but the president had diverted the funds, and this scientist came storming around talking about the inherent dishonesty of university presidents which occurred a little bit, I think, in the case I described of my own experience with the Institute of Nutrition. That kind of thing you don't descend to if you've really got good genes. Also it's the kind of experience that can make a man bitter. The only time the word was used as I recall it, in my having a reaction of bitterness is what Mr. Dean Acheson told me when once he referred to the Institute of Nutrition episode some years ago. He said,"Yes, I understand that you left the deanship on account of that with not a little bitterness."

That was the way he put it, but I don't think so--at least I didn't carry it too long.

You ought to put his other comment in--after the fact.

Yes, the one he told me the other day,"Yes, that was a mistake that we made"--to do away with that project, establishing an Institute of Nutrition.

It's in the correspondence and also in talks I've had with you--there's a direct quality about you which would preclude participation in a kind of

<u>trickery.</u>

I'm interested in your saying that, but I take it with a grain of salt because I may be having to restrain something that would lead to trickery, or tricky behavior, and that is the way I had to scrounge to survive, so to speak, in my early childhood.

<u>I wondered about that.</u>

I don't understand myself why I didn't became a juvenile delinquent for one thing, or how--I was obviously very deceptive and had a fair amount of castigation on account of it when found out, but it didn't last long enough to cause me any great pain, or put me in any great jeopardy, either in college, or afterwards.

<u>I think that whatever you did in those days was also done directly--that is it was done where it could be discovered. For example, the window and the explosive. This wasn't any clandestine thing. It was something that was done, and it exploded. There is this quality of directness. I think you also put your finger on another thing--that if your genes go deep enough, there are somethings you just don't do. There's another thing that runs and I wondered whether it had anything to do with life in the Army. Pictured all through what we've done so far is regard for not just older men, but older, able men--Joseph Jones, a great hero, Dr. Welch, Dr. Goler, Dr. Rhees--people you can single out either because of their quality, their own directness, and their ability. I wonder if this had anything in the--well, it's hard in the military establishment. You were all pretty much in the same age group.</u>

I think it's easier in the military establishment because you r evere your commanding officer. I revered the Office of the Surgeon General not only because my relative, William Crawford Gorgas, held that position, but because I had respect for authority perhaps beaten into me, or perhaps again from association with peopel who attracted respect, but in the Army you start with a high regard for your commanding officer. I don't know how it is in the infantry.

The infantry tends, I suspect--certainly in the elements I know anything about --to be an all civilian agency. I mentioned this before in the distinction drawn between the 11th Sherwood Foresters and American units--there is something to the 11th Sherwood Foresters quite apart from the men who were there.

They had a tradition, a history. Yours was just an outfit put together at the last moment.

But you had good and sufficient reason--if nothing else up there in those two bound volumes of General Simmons. He worked--at the bench.

Yes, he was quite a fellow. He was an intricate and puzzling man at times. He had--very attractive manners, was very friendly, very intelligent and imaginative and vigorous, but also I think he had some of the limitations acquired, or imposed by Army officers, or a regular Army officer's life. He'd been used so much to being fed and clothed and getting handouts that I think the moral distinction over some things were dulled, not that he did anything that I know os that was particularly bad, but you see that kind of thing among officers.

In the Surgeon General's Office I admired General Magee, liked him, and I see General Magee all the time. He lives in the Army-Navy Club now. General

Kirk was a violent and most attractive man respected for his honesty and ability and his frankness. Simmons we talked about. The rest of us that were colleagues in the same flight of age, so to speak, chiefs of divisions in the Preventive Medicine Service and the deputy chief who was really a chief of a division as I was as deputy chief were like--I'd say members of a faculty again, so much so that the work seemed to me more like seminar work all the time than anything that was prescribed by orders. We used to talk very freely at staff meetings and all sorts of meetings. I got an impression that the military service was more democratic than some of the things I had seen on the faculty at Yale. I recall several times when problems were under discussion, there'd be a master sergeant, or a sergeant in the group. He'd have an opinion to express, and he expressed it. People would listen to it, and it would be debated. Sometimes it had an effect on the group and on the decision that was made. It was just like you were talking over a thing as a group in the classroom.

<u>That's an interesting observation--the seminar approach. I know in the infantry as you come from all walks of life and you're there for a while, suddenly you take on the coloration of a team in ways that are not entirely clear to you, but you begin to think in different terms. I would suspect that a good many Preventive Medicine Service people came to it both from the regular Army and from civilian life--that it was a shake down cruise for a while until you found, without ever articulating it, that means whereby you get the task done, and it is a team.</u>

The regular Army officers, of which there werea very few really in the Preventive Medicine Service, were different from the reserve officers, or the civilians that had been brought in by General Simmons from universities, and it might be that they were different because they had had a confining sort of training. They had come up through the Army Medical School and had served under orders from lieutenants on up to when they were Majors, or Colonels when we saw them. Therefore, they were living more according to Army regulations than the rest of us who didn't give a damn about Army regulations in the same sense that they did. Another thing--and I say this just to be truthful without meaning to be offensive to the memories of these Regular Army Officers who were with us, and that is that the Army Medical Officer's life is a very protected life. He's hardly had to meet the competitions, or the situations that the civilian had to meet coming up through a faculty in a school or in the practice of medicine, or making a living by his own wits in laboratories that he may run, or some other work; whereas the Army officer has a protected and safe life, a career that is before him that he can achieve just by being a good boy. As long as he obeys the rules and has enough sense to make a little contribution every now and then and keep out of trouble, he can go on up and get promoted regularly when the time comes, whereas men who go out in medical fields as practitioners, or as teachers, or as investigators have selected a life of uncertainty and jeopardy, different from the protected life of an Army officer. Those who come through that with some measure of success tend to look down a little on regular officers--don't you think so?

Yes. But you're right--the demands imposed on each group are different in kind and quality.

Yes.

How much of whatever it was Secretary Stimson was filtered down? This is a large organization. We indicated I think before, certainly with President Rhees, that whatever it is he was created a tone for the University of Rochester. That was equally true of the Presidents of Yale under whom you served as indicated in the files, but here--this is a huge thing. I don't know. I know something of Mr. Stimson through talks with Justice Frankfurter. He was a considerable man, but how much of whatever it was he was filtered down?

I'm not a fair sample of the filter bed because Philip Stimson, Mr. Stimson's nephew, was a classmate of mine at Yale, made a great name for himself as a pediatrician and a great expert on poliomyelitis, so through that I knew a little of Mr. Stimson before I had any Army connection. Then Mr. Stimson's two main assistants down here were close to me. One was Harvey Bundy who was just a class ahead of me at Yale. I knew him very well and that kept me closer to Mr. Stimson than some of the other people in our group who just knew this great figure somewhere down town, or in the Pentagon. The other one was George Harrison, my classmate at Yale too, so I had a sense of Mr. Stimson's qualities, the way he thought, the vigorous honesty, steadfastness coming to me because of these personal associations that other men didn't have, but I think he was a very remote figure in the Army. They hardly knew that there was anything but a Secretary of War. He wasn't Mr. Stimson to anybody. He was a Secretary of War all surrounded by generals and rules. He had relations with his assistant generals and others that were admirable. General Fox wanted to get into Yugoslavia. I went so far as to speak to Mr. Bundy about it and get him to speak to Mr. Stimson because Mr. Stimson by Executive Order 9285 was the head of the Typhus Commission--"The Secretary of War and the director" are mentioned equally in many of the provisions in that Executive Order, so Mr. Bundy thought that this ought to be brought to Mr. Stimson's attention, since I'd urged Mr. Bundy that General Fox was quite restless and probably right and wanted to do what he was going to do. Mr. Bundy went and talked to Mr. Stimson and brought me back the message that Mr. Stimson would have no hand in it at all because--to quote Mr. Bundy, "Mr. Stimson said that he's not going to tell these generals how to run this war."

Well, as I know now, that wasn't quite the answer. He wasn't going to tell the generals to do this NOT out of so much respect for the generals, but because he was prevented from taking a hand in it by the conversations between Roosevelt, Churchill and Stalin. I think the British were the ones who didn't want Fox to go into Yugoslavia, or the British representative. That kind of thing which displays some characteristics of Mr. Stimson came to me on account of my position with the Typhus Commission and my intimacy with Mr. Bundy, whereas it wouldn't have gone to anybody else in the Division, so as I say, I'm not a good sample of the filter bed.

Even assuming that so far as the Preventive Medicine Service was concerned he was a remote figure, what about General Marshall--different entirely?

General Marshall was, of course, even more remote, but I think as I have already told you we came in contact with General Marshall because he had some definite ideas of preventive medicine. He opposed the introduction of the finest agent to immunize people against tetanus; namely, tetanus toxoid, because it took him about a year to understand that tetanus toxoid was not tetanus anti-toxin. He'd had tetanus anti-toxin, horse serum, injected and got the hives and was very uncomfortable in the earlier years, and he thought that this tetanus toxoid would make the soldiers sick in the same way, and he held it up.

Then I think I told you about his letter to the Surgeon General on the subject of the vaccination of the people in the Southwest Pacific. He wrote General Kirk that everywhere he went he saw soldiers losing time because they were getting one vaccine injected after another, and General Marshall said to General Kirk,"It doesn't seem to me that the Surgeon General needs to get a hundred percent record in these matters", and he advised the Surgeon General to look at the policies and procedures of the General Staff, as General Marshall said,"We take a calculated risk. Why can't the Surgeon General take a calculated risk?"

I have that letter in my file and am keeping it as an example of what a contrary thing a man might do, because if the Surgeon General took a calculated risk and there was a bad outbreak of disease, General Marshall would have peeled off the head of the Surgeon General.

I never met General Marshall. I have seen him in action talking. One of the customary things during the war was for General Marshall to brief colonels and generals of all grades who were in Washington. They would be called over to a great big assembly room on the top floor of the Pentagon, and General Marshall would speak, and he spoke usually very sharply and in a manner that no argument, or contradiction was to be tolerated. It was clear in his mind. He was sure of what he was saying. He was very positive.

I have read the Stimson....

<u>The diary</u>?

I read the diary and the book--<u>On Active Service</u>, and I've gotten to know a lot about Mr. Stimson from that, and I've read another book about Mr. Stimson a while ago.

<u>Elting Morison's book.</u>

Yes--that's right, very good. I liked that better. Well, as I say, I have so many sources of knowledge of Mr. Stimson that I don't know what I got out of his influence during the war and what I got out since the war.

<u>When the time came in September of 1946--wasn't it?--for you to leave that office, that office had already been virtually shut down.</u>

Yes--well, it never was shut down, but it was greatly reduced down to a division again. Most of its previous divisions and branches were cut off and all greatly reduced.

<u>How did you feel about leaving the office then? Put it this way--it isn't every day that a fellow becomes a general. I had to wait a long time to become a captain--so I know.</u>

Well, it's very nice to be a general. It's a long step up from being a colonel to being a general. You get all sorts of unexpected pleasures and privileges. When I came back from Cairo in 1944, I had two examples of courtesies paid a general on my trip back. One of them first came when I was sitting on the porch of the Shepheard's Hotel in Cairo. We were talking to General Fox and having a little drink in the afternoon, waiting for the passage of time until the plane was to take me out to Casablanca in the evening. A staff car drove up, and this officer said to me,"Ig the general can get ready right away, we'll take you out to Payne Field because there's sand storm coming, and we want to get that plane off the ground."

I had a little bag of stuff upstairs. I went and got it. I was driven out there. I was taken to Casablanca in the belly of a hgue four engine plane.

I was the only one there. They had about six or eight mattresses in that belly. I piled them together and had a good sleep. Then when I got to Casablanca, they gave me as a billet the most beautiful three, or four story house that had been General Patton's billet. I was the only one in there, had a great big bed less than a foot off the floor in a big room. On the roof the windows went out on to tiled terraces up top overlooking Casablanca and the harbor--living very high and being a general.

That's the kind of thing that happens to you. I think when you get to be a general, you have a greatly increased sense of responsibility. If you have any sense of responsibility to begin with, it gets sharpened and extended by the experience of being in a position of trust and some authority.

It's easy to forget you're a general. After a while the novelty wears off, and you find that you're just the same person--almost. It was difficult for me to forget being a general when I took the uniform off. I was walking down Elm Street in Yale about the last part of September, 1946, and four soldiers in khaki shirts and pants came down the side walk. I was in civilian clothes. I forgot that I was in civilian clothes. I didn't think about it until I walked right square into them, I think. I was used to having soldiers in khaki give way, and that's one of the characteristics of the life of a general--he finds that things give way.

<u>They sure do.</u>

You begin to expect it automatically--well, I bumped right into those boys. They thought I was an impertinent civilian.

When I got separated, I had an experience that indicated how some generals behave on that occasion. I was in the Pentagon, and I telephoned down to the dispensary that I would like to make an appointment to have a physical examination for separation--did I tell you that?

No.

The girl said,"Yes sir, General", and she gave me an appointment. I went down at the stated time, and she pulled out a card and said to somebody standing by,"Here is General Bayne-Jones--physical examination for retirement for disability."

I said,"I didn't ask you for that. I have no disability. I want just a physical exmination for separation."

"Oh," she said,"you're the only general officer who has been through here that hasn't asked for retirement for disability."

You see--generals not only cause things to give way in front of them, but they really take the props out from under some things, and it's easy to push them down. That always struck me as a curious bit of medical doings, I would say, that so many of these general officers who retired for complete, or serious disability, went into even more taxing jobs than they were under at the time when they were in service. General Simmons is one. He got practically total disability, and he went immediately into this strenuous work of being the Dean of the Harvard School of Public Health.

He was separated before you.

Yes--in July. Retirement for disability has monetary value too because the retirement pay is exempt from income tax.

That's a nice thing to know--that is, if chance ever strikes me. Well, you know, when you close the door for the last time on this period and go back to Yale--it's a change.

Yes, it is a greta let down.

<u>I suspect that the pressure was less in 1946, than it had been theretofore in the office--wasn't it?</u>

Yes, it quieted down a great deal; in fact, there was nothing going on. Demobilization was taking place, and there was great activity at the ports and at camps, but nothing that stretched the Preventive Medicine Unit in any place that I know of. They'd had so much harder work in the war that this was all routine and easy. I recall the greatest bsuiness of the last month or so for me in the office was writing citations for people to get medals. We tried to get medals for everybody, so much so that one day I went into General Kirk with a list, and he flew into a rage, "I won't look at it! I won't have anything to do with it. Preventive Medicine is so grasping that they just want all the medals we've got."

<u>That's par for the course. Well, I don't know--a catalogued day in the midst of this had no hour, did it?</u>

In the war?

<u>Yes.</u>

I didn't have any hours. I worked all day and way into the night. Got up early in the morning. Did a great deal of writing in long hand, and fortunately my secretary could take a long hand letter and type it. Some of them regard that as an insult. They'd rather take dictation. On the other hand, in the Army, some secretaries are penalized because they take dictation. Some of the promotions, I found, are based on the number of people one of these clerks supervised, whereas one who took a lot of dictation from an officer was regarded by the personnel management people as being a slave-like, subsidiary person, although that person taking dictation also contributed to the composition of the letter. You can often look at the secretary and see whether what you're dictating has got any force or validity. It's easy for them to raise their eye brow, or look bored. They'll tell you. I used to spend a lot of time writing. Practically all of the letters I wrote long hand at night, or during the day when I had time, but to go back to this supervising system, I remember one woman who got up to a GS 6, or 7 or more who was handling the disability and discharge records. She had a room with about forty girls--about that many were under her supervision, and she'd get an armful of officer's 201 files, records, and walk around and distribute these things to the clerks, but she was the supervisor of a platoon.

Well, we're practically at the end of this tape, and I think next Monday we'll go back to the pin ball process again.

You mean going back to Yale. That didn't last very long. Is that on still?

There is some continuity all during this period of the war--the Childs Fund which we abruptly terminated--which we want to go back and pick up, and then there's that marvelous magazine and that print shop that failed.

I had nothing to do with that.

No--but it was an agonizing thing to get the journal published. That has to do with the cancer field, but we can go back and pick that up on Monday. All Right?

You'd better put down a note that you're going to talk about the Childs Fund so that you'll be sure to remember it.

<u>Tuesday, June 21, 1966 A-60, N. L. M.</u>

I've made some notes while you were away.

<u>Oh! Oh!</u>

No, I just wanted to--I'm sick of this rambling stuff.

<u>Don't be.</u>

What?

<u>It's good. Last time in personal terms we gave some summary of the experience in the Army. One now is faced with the necessity of returning to what might be referred to as a normal operation. I don't know what "normal" is certainly so far as the Childs Fund and Yale are concerned. You'd just been through an exacting period, and I suspect all institutions are altered in the process also--they can't escape it. I wonder what you found when you returned to Yale. We can pick up traces back into the period just passed. We haven't mentioned Dr. Meader, the administrative changes, the search for younger men in this period which is implicit throughout all these records here, fencing in this field where the limits are still very elusive, how one deals with it. The new attitudes that one has toward it, I suspect, are tied to people-- I don't know, but I get that in reading the minutes of the meetings of the Board of Scientific Advisers. You had continuity with the Childs Fund all during the war, and that's important, something which we haven't brought out--that is, it was not something that was dropped. Continuity of interest continued plus dealing with the substance, the business of the office.</u>

Yes, I think that's important to put in, but I would like to go back of what you just mentioned to, say, 1940, or thereabouts, to bring in the relation of the Childs Fund to the journal called Cancer Research, the problems that were involved in that; the shifts and whatnot that journal had to go through, the war, and then go through the war time management to my return to New Haven in 1946.

All right.

Shall we deal with the journal as a separate thing?

Yes.

All right. I'll carry that right on through then until 1947.

Yes.

In 1940, the only American scientific publication of value in the field of cancer research was called the American Journal of Cancer. It was edited by Francis Carter Wood. It was supported, in part, by the Chemical Foundation through the interest of Francis O. Garvin and at that time, in 1940, it was running a deficit, a very large deficit. There was much anxiety about the future of this important journal and much dissatisfaction with the way in which it was being managed by Dr. Wood. One of the first ventures in support of publications by the Childs Fund was the generous response to a recommendation of the Board of Scientific Advisers by the Board of Managers in authorizing in 1940, about 1940, a thousand dollars a year for five years to help support the American Journal of Cancer. This was continued right on through; as a matter of fact, the Childs Fund is still supporting the successor journal to the American Journal of Cancer, a journal called Cancer Research. It's unusual for a foundation to do a thing like that, but I think that set a pattern. Other foundations followed. Notably the next one who followed was the International Cancer Research Foundation called the Donner Foundation--Mr. Donner being the donor and president of it and having with him a very able Victorian lady named Mrs. Mildred W. S. Schram who was the secretary.

The American Journal of Cancer accepted this thousand dollar a year grant for five years, and I was appointed to the editorial board. As a matter of fact, I said this was 1940--it goes back actually to 1938. I was appointed to that editorial board in 1938. It was done within the first year of the Childs Fund's existence.

Yes, I think there had been considerable dissatisfaction--maybe that isn't the correct word--with Francis Carter Wood's editorial management of the journal, so that something had to be done, and it was a continuing source of uneasiness.

Right through 1939, the difficulties with the journal increased, and it was perfectly apparent in January, 1940, that a reorganization would be needed, and in the course of the reorganization there was a question of who would take the ownership of the journal, if it were reorganized. The Crocker Institute for Cancer Research at Columbia University Medical School was thought of, but they wouldn't take it. The three main problems then were the management, editorial and financial of the American Journal of Cancer, the question of ownership of it, and the question of its financial support for the future.

The upshot of all this consideration, many conferences, was the decision in 1940, to start a new journal to be called Cancer Research, and this new journal was guaranteed support from the Childs Fund, the International Cancer

Research Foundation, and other organizations amounting to some twelve thousand dollars a year. That was considered sufficient with the additional expectation of subscriptions. It turned out to be a satisfactory estimate. The Board of Managers of the Childs Fund did not wish to be responsible as owners of this new journal, Cancer Research, but the International Cancer Research Foundation agreed to be the owner and sponsor. They had the business office of the new journal, Cancer Research, at the offices of the International Cancer Research Foundation in Philadelphia, largely under the management of Dr. Mildred Schram. That was done by the fall of 1940.

Under the new management the first issue of Cancer Research came out in 1941, from the Lord Baltimore Press which was the publishing agency, and this arrangement worked all right for a little while. The editorial board of the new journal Cancer Research was curiously set up as an advisory editorial board in a way. The chairman of it was James B. Murphy of the Rockefeller Institute, my friend and enemy, or at least my friend and my superior opponent for so long, and I was called the secretary of this board of editors; as a matter of fact, the secretary did all the work. All the manuscripts came to me, all the abstract manuscripts as well as the articles, and it was too heavy and too time consuming to send them all to Dr. Murphy and let him decide what was good and what was bad, so the secretary became in fact the editor. We got on all right because sense enough was used to send Dr. Murphy the kinds of things that he would be sure to condemn, and that made him feel that he was serving with special value. He continued in that position all the way through.

When I went into the service of the Medical Department of the Army on February 12, 1942, I had by that time gotten a considerable experience in my first round of editing a scientific journal, and I brought down with me to

Washington in February of that year, 1942, manuscripts that were in the process of being edited for that journal. I remember for the first couple of weeks down here that I had a little back room in the Raleigh Hotel, and I edited this journal on the top of a steamer trunk, or foot locker type of trunk. I don't know why I had such a little room, except the town was very criwded at that time. Mrs. Bayne-Jones was remaining in New Haven to close up our house and put away the very large collection of books I had in the house, put away by having them transferred to the basement of the Yale Sterling Library for storage. Well, I edited the journal down here until about maybe April, 1942, when we persuaded Dr. William H. Woglum who was Associate Professor of Cancer Research at Columbia University School of Medicine, to take it on, and he took over the editorial job for the new journal Cancer Research and carried it through until the early fall of 1946, when I got out of the Army and took it back to New Haven.

In the meantime, the Childs Board had appointed Dr. Ralph G. Meader as assistant to the Director of the Board of Scientific Advisers, and Dr. Meader's work began on February 1, 1942. Dr. Meader is a very careful--I say is because he still is as he was then a very careful, thoroughgoing, reliable student, reliable administrator, but not a man of great originality, or special originality, or productiveness in research. This was so much so that he had come to the third term as an Assistant Professor of Anatomy at Yale, and at that time if you came through a third term and hadn't been promoted to Associate Professor, you'd have to go look for a job somewhere else. Well, this was true in his case. Nevertheless, the Board appointed him as Assistant to the Director at first, and then he was called Assistant Director. Thta was discussed with the Dean, and it was pointed out to the

Dean, Dr. Blake, who had succeeded me, how valuable Meader would be in this situation for the Board, how he could serve the school as well as the Board because he was allowed to continue some teaching. His salary would pass over into the Board's Administrative Budget. Altogether it was a mutually beneficial arrangement when they agreed to let Dr. Meader remain Assistant Professor of Anatomy. He was enormously helpful to me, to the Board, and has been to the National Institutes of Health to which he went as an assistant in the Research Grants Program about 1946, or somewhere like that.

One difficulty with Ralph Meader's methods is that he didn't know when to stop being complete. He began to review for the Board of Managers and the Board of Scientific Advisers everything going on in cancer research in the world, so the reports of the Assistant Director became voluminous abstracts, sort of news bulletins of cancer research everywhere. It got so big that nobody could read them much, or read them thoroughly, so that he made a separate sort of publication of them, as I remember, in the board's affairs, but he worked very diligently through all this period on the Childs Fund records, but he didn't have anything to do with the journal, Cancer Research.

Now, let me say one more thing about Ralph Meader and myself--all during the war he would come down with an enormous brief case filled with applications and papers about the Childs Fund. We had at least one long meeting every month. I would go to New Haven and attend as many meetings as possible of the Board of Scientific Advisers and the Board of Managers during the war, so I kept in very close contact with the Fund all during the war in addition to the other things going on in the Office of Preventive Medicine Service.

Dr. Woglum managed the journal very well as editor during this period. Publication was not interrupted. He had a good assistant, Miss Elizabeth B. Barber, and continued to put out a very good journal. To finish with my part of the journal, I will carry it through now until the last year and a half, 1946-1947. When I came out of active duty, the editorship passed to me again right away, and I moved the material and the Assistant Editor-Secretary, Miss Barber, from the Crocker Institute at Columbia to New Haven. I was given rooms for the work of the journal by Dr. Fulton in the new medical library and was comfortably set up there. I also had the records of the Typhus Commission.

It was not hard to get back into editing the journal. It had been carried on in the same tradition, the same line of activities that we had adopted at the first--the policies were the same, and the people were all the same, but the work was very heavy, exceptionally so because people were putting in a great many manuscripts since their publication had been somewhat held up during the war, and we became involved in a series of difficulties with publishers. It seemed best for me to take the publication of the journal to a printer in New Haven--Tuttle, Morehouse, and Taylor, a fine old firm of publishers in New Haven, that had been publishing learned works for members of the Yale faculty for years and years. It had quite a reputation, published poetical productions--very good work, almost a traditional thing in New Haven and Yale going back into the 1870s, or,1880s, or somewhere there. It was almost a part of the university. They published a satisfactory journal, but the editor had to do a great deal of footwork with that. The cuts were made in a little shop down an alley, and the editor had to carry the pictures down there; as a matter of fact, the editor mounted the pictures that had to be made up for half tones, and sometimes color, and it was footwork, a personal job to see that this little photographic plant did its work in time for the use of the plates by the printers. It was always hard to get ahead of the game. Material was used up about as fast as it came in, and the schedules were tight.

The printer was satisfactory, and the printer printed and mailed the journal--the lists of subscriptions and addresses all came to us from Dr. Schram's Office in Philadelphia. I didn't have to deal with subscriptions, or lists, but a tragedy happened about late 1946, or maybe it's in 1947. A Jewish group in New York suddenly bought out the ownership of the Tuttle, Morehouse, and Taylor publication firm because they discovered they could sell the old linotype machines in Brazil at several times the original amount they would pay Tuttle, Morehouse, and Taylor for them, so they immediately started to dismantle that plant. They discharged fine old artisans, printers who had been there for years, devoted, almost scholars, although they were employees of the printing firm. They carted away the machinery. The place began to go down, but our own journal material was there because we had a close partnership almost with this publication firm. An issue of the journal was in process of being printed at this time, so that manuscripts and cuts and incomplete frames of type were carted off to New York by the purchasers. In addition we'd rented from Tuttle, Morehouse, and Taylor storage for all the back issues of the journal. They were up in the garret. They were carted off, so that I had to find out where they were in New York. They were over on the Eastside, down near the Holland Tunnel, somewhere in that region, and I went down there and practically had to threaten these people to retrieve our own material. They weren't clear as to whether this new firm wanted to publish the journal; as a matter of fact, they were not a publishing firm. They were intermediary people. I don't recall that I had a great deal of help from Dr. Schram through this episode, although she was greatly disturbed and greatly anxious, as we all were. Finally, however, the issue for that year was completed.

Another arrangement was to come about in 1947. My editorship ended in

June, 1947, and my friend, Dr. Balduin Lucke, a pathologist at the University of Pennsylvania School of Medicine, the man whose monograph on the pathology of hepatitis, epidemic and postvaccinal hepatitis, was examined by us not long ago. Dr. Lucké was a charming gentleman, a scholar, scientist, antiquarian. He loved old things. For instance, he was able to rescue from furniture thrown out from Walter Reed Army Medical Center the desk chair of Walter Reed himself, and Balduin Lucké would sit in that chair and think about Walter Reed. It was a nice thing to do. Lucké wasn't very well. I don't think he lived much longer after this year. He was editor until December of 1947, and somehow or other I had to fill in again as editor from January to June of 1948.

At that time the journal was being printed a little while in one place, and Dr. Schram would make an arrangement for it to be printed in another place. We had very great difficulty with printers. I thought authors were difficult, but publishers, printers are worse. Finally in 1949, the journal of Cancer Research was transferred for editing to the University of Chicago, and a doctor friend of mine, Dr. Paul Steiner, became editor-in-chief, and he continued in that for a number of years. The journal then was transferred for publication sometime toward the 1960s, I think, to the William and Wilkins Company in Baltimore, and Michael Shimkin who was in cancer research at NIH, a scholar, an epidemiologist, quite a thinker in cancer research, took over as editor-in-chief. He still is, and the journal is still going, flourishing with good subscriptions and good support from the National Institutes of Health.

I think that's all I want to say about the journal. It's a very interesting episode, and unexpected episode in my life. I didn't expect to fall heir to such difficult work, and I didn't expect to have such an educational experience. I learned a great deal from it.

The files are interesting in one respect--strangely, a lawyer's interpretation of what constituted a contract--you know, it's a technical thing. This was Pepper from Philadelphia. We don't have to go into it, but it is very revealing of the kind of thing you can fall heir to when it was claimed that a reply by you constituted a contract, when, in fact, if you read carefully the reply, it isn't so. I don't know how Pepper figured in this.

I didn't have any power to make any contract.

Exactly--that's the first point. The second point is that what you wrote didn't constitute a contract. The papers are all here. Pepper is not an inconsiderable attorney.

The Pepper family is a great family--Perry Pepper in medicine.

The lawyer certainly didn't read this very carefully.

Well, in this whole period--you mention the search for young men through this whole period. The Childs Fund was searching not only for young men, but for older men. Young men came through a fellowship plan which we adopted early in the direction of the Fund, and a number of men appointed as fellows of the Childs Fund have become quite eminent--one was Joshua Lederberg, a Nobel Prize winner, and another one is my friend Robert Stowell who is now over in the Armed Force Institute of Pathology as the Director of Research. You could name quite a few others, but the Board of Managers liked that program.

The only difficulty that I recall with it was the dispute with Mr. Hamilton, the treasurer, as to what the Board had meant by one time putting aside the sum of sixty-five, or seventy thousand dollars for the fellowship program. Dr. Meader and I took it as an allocation of a fund to be used more or less under the discretion of the Board of Scientific Advisers, subject as always, to approval by the Board of Managers. We went along and recommended some fellows and wanted to keep this fund showing in the budget, but Mr. Hamilton took it, as I recall it, that the submission of the name of a candidate for a fellowship to the Board of Managers was an admission that the managers had made a mistake, or didn't know what they were doing when they set aside the total sum. Did you see that correspondence?

Yes.

That was a troublesome point. It was the kind of thing Mr. Hamilton would do, but it didn't worry us too much. That was a good successful venture, and it's still going on.

The other thing the fund did intellectually outside of its own affairs was to hold conferences, many conferences. In these days from the beginning until I went into the war, and even during the war, I recall meeting a great many people through the fund who came over as visitors, or came in to see me about grants, or one thing or another. The Childs Fund from the beginning had in its policies the rescue and support of a journal, the holding of conferences, the making of grants, the support of a fellowship program for young people, and the search for the elder men, the so-called statesmen of science, the elder statesmen of science who needed some money for research. More particularly one of the latter was Dr. Eugene L. Opie. Opie was working at that time in the Rockefeller Institute which had a policy of not accepting outside grants, but in a year or so they softened up and did take the money from the Childs Fund offered for Dr. Opie's support, and that continued quite a while.

In the course of this experience with the fund during the war--we mentioned this before--I became involved with the interpretation of the statute on conflict of interest. Have we had this in the record already enough?

<u>We talked about the conflict of interest as it applied to contracts under the Army Epidemiological Board and the termination of funds from the Childs Fund which were paid you to maintain a level of income--that arrangement that you had had with the Childs Fund was finally terminated.</u>

Yes--that was a gradual process. It wasn't terminated at first, but I saw more and more that there was a risk of a conflict of interest that would be embarrassing for the fund and to the university because I was handling contracts for research not only at Yale, but at a good many places where the Childs Fund had some interest. I found I could get any kind of interpretation of the law that I wanted, so to speak, by the manner of asking the question, so I decided finally myself to ask the university to cut off my salary that they were paying me from the Childs Fund through Yale and that amount was to make my salary--Army salary and what the Fund paid me--equal to what I had before I went on active duty. For several years there I wasn't on the budget at all, but I did go back on the administrative budget of the fund in 1946, when I got out of the Army and back in the work.

<u>What effect did this period of warfare have on the continuity of investigation in cancer?</u>

It decreased it because it took men out of the cancer research laboratories and called on them to do other work--sanitation, or bacteriology, or pathology. The men remaining in the cancer laboratories were inelligible for service in the Army. They were older, or ill. The war decreased cancer research abroad also as well as here. Abroad they suffered, or at least in England they suffered from a lack of equipment and instruments. After the war the Childs Fund could come in andhelp them to rehabilitate their laboratories; in fact, I was in London in 1943--I mentioned that before--on other business, but I saw Dr. Kennaway--E. L. Kennaway, I think it is. Dr. Kennaway was a big, tall man with a beginning Parkinson's Disease at that time. This had nothing to do with Parkinson's Law. Dr. Kennaway was the head of the Chester Beatty Research Institute in the Royal Cancer Hospital in London. He isolated benzpyrine and the carcinogenic material among the yellow dyes that the Japanese used to produce cancer. Benzpyrine is a powerful carcinogene, and it occurs in soot--it's a coal tar derivative.

Chimney sweep.

Yes, chimney sweep cancer, and Kennaway was a friend of George Smith, and he came to New Haven very often. We liked him. Alexander Haddow was also, whom I met in London in this time during the war. I also went to see Gye at the Cancer Hospital at Mill Hill. My visit to London as a Colonel on a mission to do with quarantine against yellow fever and some consultation with the Surgeon of the European Theater of Operations was combined with looking around the cancer work as much as possible. The same thing was happening over here. I attended some cancer meetings, and I was in touch with the National Cancer Institute.

The war in Europe--with the transfer of some regugee doctors and I gather scientists to this country, disrupted the continuity of effort in Europe. I don't know whether the international congresses were held during the war, or not.

No.

They were not.

We put them on after the war.

By invitation people would come over. I suspect that certain emphasis was placed on the need for people working in the cancer field to rub shoulders.

Yes, and they needed help. The only case that I remember--I should look up his name--there was a friend of Mr. Winston Childs who landed from Poland in this country, or he was from some middle European place. He needed help and quite a pressure was put on Mr. Childs to get something for him from the fund, and we had to protect Mr. Childs.

Mr. Childs....

I'm talking about young Winston.

Mr. Childs himself, the father, died in 1946, didn't he?

Yes.

I wonder when you subtract the quality he had, or the quality he represented--variables--when you remove them, some people are altered in the process.

Mr. Winston Childs Sr. died on December 20, 1946. He was the patriarch of the family. He was the gentle, but all powerful controller of his children. He had three sons who were members of the Board of Managers and a daughter, Barbara. Barbara intellectually was just as able as the sons, but being the sister, they didn't take her into the men's Board until a good many years afterwards. When Mr. Childs died, I think the family probably was at sea for leadership, and perhaps the sons who were of very different temperament and capacities may have grown a little further apart than they would have if their father had still been alive. They didn't separate. It was a closely knit clan, both living fairly close together in New York City--if you can say that you live close together with anybody in New York City, even though you're in the same house, and in Norfolk, Connecticut, where Mr. Childs and his son Edward had an enormous estate, forestry, artifical lakes, horse riding fields, boats and everything for a vigorous outdoor life that Mr. Starling W. Childs Sr. had had.

During this period also--up 'till the beginning of the war and afterwards--they were building the Yale Historical Medical Library, the Childs Fund iffices in it, and the Cushing Memorial, so to speak, to which Mr. Childs was devoted and this kept him and his sister, Miss Coffin, closely interested in the medical school, in the university, and in the library. He really put his mind on the cancer problems that were brought to him. He read the report of the director submitting and recommending applications to the Board and giving the list of applications recommended for disapproval, or declination with great care--underlined the words, would ask questions. He studies his lessons, and in the Board meetings usually he was the one who in the Board of Managers made the primary resolutions all the time. They wanted for him.

<u>How much and to what extent was leadership in the field exercised during the war years by the National Cancer Institute? I don't want this to sound like a curve ball--let me say that--I can't remember his name, but Dr. Roscoe Spenser became the director. Who was the one before him--the pharmacologist?</u>

Voegtlin.

<u>Voegtlin. With promising beginnings in a field which is as uncharted as cancer was, I suspect that during the war in the National Cancer Institute, while it continued to function and they did their own work there, leadership was wanting. I guess Dr. Spenser with whom I have talked and for whom I have great affection, really, was put in the position of speaking a language in which he hadn't been instructed really.</u>

No--Spenser was a bacteriologist and his work over in his laboratory in the National Cancer Institute was dealing with variations in bacteria, largely, and they had transplanted people from Dr. Little's genetic laboratory at Bar Harbour, set up mouse strains, brought Andervont and other men down. As I recall it, the National Cancer Institute when Voegtlin was there working with George Smith started as leaders in a program of clinical cancer investigations. They set up groups to study cancer of the stomach, cancer of organs in different places--I mean the study at different places. That dropped down in the war, and they were very eager to get the Childs Fund deeply involved. We took the position that that was a bit outside the line that the Childs Fund wanted to do. The Childs Fund had supported the tumor clinic at Yale largely by providing for its equipment and adhering to the policy of the fund; that the fund was not to be involved in the care of cancer patients, or the treatment of the disease unless the case, the situation, and the treatment would throw some light on the etiology. The Childs Fund did not help any other clinic. This was a case too in which Mr. Childs' remark at the opening of the fund, that he felt Yale ought to be aided to become the great cancer research center, justified some favorite treatment of Yale requests, so we gave considerable money to the Yale Tumor Clinic. I think that groups to study cancer of organs was a design by Dr. Voegtlin and George Smith for other places under more or less the guidance of the National Cancer Institute.

I do not recall that the National Cancer Institute was the real leader in these matters at this time. It was overshadowed in activities in the up-gfothw of the Memorial Hospital for Cancer and Allied Diseases in New York when Dr. C. P. Rheads became its director after Dr. Ewing resigned. That became the great and vigorous center for cancer research, but that was impeded during the war because we brought Dr. Rheads into the Army as a Colonel hoping that he would be a liaison between the Surgein General's Office and the Chemical Warfare Service. He became so enamored of the Chemical Warfare Service and so valuable to Major General William M. Porter, the chief chemical officer, that the liaison function for the Surgeon General was reduced to a rather routine, ineffective, and troublesome relationship. Dr. Rheads remained in this liaison position until 1945, a time when we were wanting to send Dr. Rheads to Japan. The Memorial Hospital people recalled that Secretary Stimson had asked to have Dr. Rheads' services down here only a year, and that he would be realeased when the Memorial Hospital said that they wanted him to return. Well, he served more than a year, but they were demanding his release in 1945, and that was brought about, but the Memorial Hospital was going ahead at that time in new things. It was the basis on which the Sloan Kettering Institute for Cancer Research was founded.

Another leader in the field going through the war was C. C. Little's laboratory at Bar Harbor--the Jackson Memorial Laboratory, a great genetic laboratory with deep interest in cancer, and Dr. Little had brought up there with him Dr. Bittner.

Johnny Bittner--yes.

John J. Bittner who really had discovered what was called the milk factor in the cause of mammary cancer in mice, something in the lactation in the milk of certain strains of mice that carried an agent, probably a virus, that causes the production of cancer in mice that are suckled. Bittner's work was going on in the war. At Yale much of the work continued. Strong continued in his laboratory as a geneticist in mice studying carcinogenesis. Gardner was doing excellent work on endocrines in relation to cancer. Gardner was Associate Professor of Anatomy then--W. U. Gardner, I think.

It strikes me, and I may be completely wrong, but unlike the American character which is interested in certain amount of drum beating, pyrotechnics, and fantastic effort to achieve a goal, that the cancer field in terms of its management, or the design of experimentation was digging in for a very long haul.

That's true--people in cancer research enter it with the knowledge that more hearts have been broken by the failure of experiments to disclose the real nature of the process, or what to do about cancer. That's one thing--the people who enter it know that they are facing a problem that has been insoluble for years and years and years. In addition, all the cancer experiments are very long drawn out things. The cancers by transplantation take and grow rapidly, but transplanted cancer is a rather unnatural thing. Cancer produced by carcinogens, chemical substances, or by radiation may not appear for three hundred days after you've done something, so you just take a hundred mice and go and sit under a tree somewhere for three hundred days. Of course they don't do that, but they have that in their minds.

The war gave every indication that there would be more in the way of technological development--measurement, tools with which to work. I wonder if this was becoming apparent to managers, people who were heads of funds. I remember that early in the Childs Fund they had not particularly wanted to get into the business of providing equipment although it did--an ultracentrifuge at Yale and some other things in the BIOCHEMICAL Department for a specific purpose, but with the electronic refinements during the war for war purposes, their utility in the field of science--whether this was becoming apparent or not. I don't know.

The great instrument that has disclosed the characteristics of a cancer cell is the electron microscope. It was beginning at that time, and a whole new field called molecular biology was beginning--at least it was getting to be recognized that there was a biology of molecules as well as of cells. It goes beyond the cellular theory. I'm sure that molecular biology had started and that the electron microscope work was well enough along so that the people at the Rockefeller Institute--I'm pretty sure Jordi Casals and others were trained by Scandinavians who were ahead in these fields--were putting their knowledge to use in cancer research.

Of course the computing techniques were coming in. They were used for data processing. Actually cancer research does not require extraordinary equipment at all--ultracentrifuges, ultramicroscopes, all the modern glassware in the separatory tubes, the things that turn things over automatically and save thousands of manual manipulations were coming in. Also there was the beginning of the knowledge of isotopes. Radioactive isotopes were known from the beginning of attempts at fission; in fact, they were known before that, from radium studies, but the knowledge of the value of radioactive isotopes in cancer research, I think, was just coming into the field particularly through the relationship of people who knew about radioactive phosphorus, materials of that kind, that could be used not only for treatment but for labeling. By "labeling" I mean putting them in compounds and getting the cells so that they could be followed through a metabolic process, but I'm shaky on the dates when these things actually occurred.

To finish with the Fund--I've finished with the journal. To finish with the Fund--I came back to it in September of 1946, after I had come back into the reserve corps after active duty, and I functioned as Director of the Board of Scientific Advisers from 1946, into early 1947, and it was around that January that the people in the New York Hospital-Cornell Medical Center began to talk to me about the possibility of my coming to New York as President of the Joint Administrative Board of the New York Hospital-Cornell Medical Center. I had no such idea in mind when I went back to the Childs Fund and to New Haven after the war, and it seemed to me a rather fortuitous event.

One of the things I was doing in the medical school at that time was to make a survey of all the laboratories to see if we could get together some central laboratory. There were laboratories of chemistry and medicine in pediatrics, laboratories in pathology under Winternitz, laboratories in bacteriology, in medicine, elsewhere, and there was much duplication--apparent duplication, but not real duplication. In other words, those departments couldn't well function unless theh had a certain amount of laboratory, but I studied that material, that situation during the early part of 1947. I wanted to see what was happening in other places, and I knew that the New York Hospital-Cornell Medical Center had a central laboratory.

I'm getting off now into the oncoming New York Hospital-Cornell Medical Center.

<u>That's right--you can go a little bit longer on that.</u>

This will--I will go only so far as to tell you that it led to my informing the Board of Managers and the Board of Scientific Advisers of the Childs Fund somewhere early in 1947, that I had decided to accept the offer of the appointment as President of the Joint Administrative Board of the New York Hospital-Cornell Medical Center.

To return however to the opening gun, it was fired as far as I knew when I went down to New York to see their central laboratory. The man in charge of that was a most notable medical man--I think he was one of the most notable of his time, Professor of Medicine, David P. Barr who had been Professor of Medicine at St. Louis and was a very distinguished Professor of Medicine at Cornell-New York Hospital. I was talking to Dr. Barr about the central laboratory, and he said,"Excuse me, I want to make a telephone call."

He came back, and we talked some more about the laboratory, and finally he said that Mr. William Harding Jackson, the President of the Board of Governors of the New York Hospital was downstairs in the Governor's Room and would like to see me, so I went downstairs to see Mr. William Harding Jackson in the Governor's Room which is a most beautiful, smoked oak panelled room of Eighteenth Century design, splendid table, portraits, heavy rugs, fire place--just suited for a hospital that claimed that it had been established by a word of George III of England. The King did allow his name to be used in the Charter of the New York Hospital, but I doubt if the King of England knew anything at all about the New York Hospital, as I brought out in this pamphlet that I wrote on the Charter.

Anyhow, I went down and met Mr. Jackson who is a vivacious, intelligent, eager person. He was a partner of Mr. John Hay Whitney. John Hay Whitney was the son of the late Payne Whitney who gave most of the money for the New York Hospital-Cornell Medical Center in 1932. Mr. Jackson asked me if I would be interested enough in the presidency of the Joint Administrative Board to come down occasionally in the next few weeks, or months, to meet with groups of the Board of Governors. I did that from sometime in January continuing on to about April, somewhere like that. It finally had to come to a conclusion, and I told the Board of Scientific Advisers of the Childs Fund and the Board of Managers of the Childs Fund that I submitted my resignation as Director of the Board of Scientific Advisers to take effect 1st of July, 1947. They accepted that, appointed a Committee on the Directorship of the Board of Scientific Advisers and nominated Dr. Winternitz as my successor.

I left the Childs Fund with much regret and with a feeling that I was leaving a part of my own family because I had gotten very close in my relationships to Mr. Childs, Winston, Edward, Richard, and Barbara Childs that, as I say, it was almost like a family relationship. In addition, I was deeply interested to see the fund go ahead, particularly as it was to face new problems as it was coming into an era in which it was no longer of very conspicuous, financial importance. When the Childs Fund was set up, the total amount going into cancer research by 1939, or 1940, estimated all over the country was around seven hundred thousand dollars. By the end of the war the National Cancer Institute had gone up into the millions in support of cancer research, and that was the beginning of the government agencies entering the research support field with great riches so that the probbem was what an independent fund could do.

Yes, and where it could make its mark.

Yes.

In personal terms, after a period of war in which you were involved in so many things--were you at all disenchanted with climbing back into the Childs Fund?

No, I wasn't disenchanted coming back into the Childs Fund, as you put it, but I was very unhappy and distressed about the situation I was returning to in New Haven. Neither Mrs. Bayne-Jones nor I wanted to go back to live in New Haven enough to settle the question then--we took it slowly. When I went back to New Haven after the war, so far as the medical school was concerned I was quite an outsider. My laboratory that I had used was still partly being used by Dr. F. Duran-Reynals who had remained there, but he was then attached to the regular bacteriological laboratory of Dr. George H. Smith, although Duran-Reynals' salary was still being paid by the Childs Fund. I had no encouragement, or welcome to return to work in the laboratory at Yale; in fact, I felt very much like an outsider who had not much to look forward to in the place. Naturally after having been busy to the limit in very large enterprises in the war, it was quite a come down in activity to return to the very small and quiet, semi-interment. I had study room space allowed me by Dr. Fulton in a wing on the side of the Yale Medical Library where I took all the records of the Typhus Commission, intending to write a history of the Typhus Commission. I had some help and got a little way with it, but that history has never been written, except as I included it in a chapter in Volume VII of the Preventive Medicine Service Serves ["Typhus Fevers" Chapter X, pp 175 - 274.] I was still a Professor on the Yale faculty, but I had no calls upon me to take part in professorial activities. It was a very welcome offer that came to me from New York. I don't know what I would have done without it. I don't think anything that was ahead of me at Yale could have equalled what came to me in the New York situation.

From the point of view of content taught at school, was there any carryover from the things that you had learned--say, tropical medicine in the war time, in the Yale Medical School?

After I went back?

Yes.

No, I wasn't called on to do that. I tried once or tweice to talk about some of these things, but I wasn't invited to do it.

Was tropical medicine part of their curriculum?

Not a formal part. It was scattered through. I tried when I was there to set it up a little bit.

The burgeoning interest of Preventive Medicine from the study of tropical diseases in the war period fell apparently on barren soil.

Not only at Ylae, but all through the country it went down. The reasons are easily understood because there was no impirtance of tropical medicine in the country at the time. Malaria had been practically eradicated, or at least there was very little malaria in the country. The scare of the introduction of tropical diseases by returning troops and prisoners of war had passed because nothing had happened there. The sanitation in Panama was in good shape. We were getting free of the Philippines. There were no special tropical medicine interests in Hawaii, except leprosy out there. Things like the developments of the late 1940s and 1950s in the Middle East had not yet come about--I'm talking now about the Anglo-Iranian Oil Company, a huge development in Arabia, and trachoma and the things that the Harvard School of Tropical Medicine has put forward. General Simmons had a great opportunity to develop tropical medicine at the Harvard School of Public Health where he became Dean, and he used that to the limit. He had--that was under his start, and it was carried on by Dr. John C. Synder--that's become a great international school with interests in tropical medicine and many other diseases of other countries and population problems.

Back of the offer that came to me from New York Hospital-Cornell Medical Center, as I learned after I began to talk about it with the people there, was the unexpected cordial support by Mrs. Harvey Cushing and her daughters. I don't know this for a fact, but I think they are the ones who suggested my name to Mr. Jock Whitney and Mr. Jackson. I didn't know Mr. Jackson, and I didn't know Mr. Jock Whitney, but I knew Mrs. Harvey Cushing and her daughters. There are three daughters--one of them married Jock Whitney, and one of them married Mr. Vincent Astor. Jock Whitney was the partner of Mr. Jackson and President of the Board of Governors of New York Hospital. Vincent was one of the most important governors of that hospital. Mrs. Bayne-Jones and I were close to the Cushing family. All the time when they were living in New Haven we saw much of them when Dr. Cushing came down after his retirement from Harvard. They had a big house on Whitney Avenue up there, and we played croquet in the back under the elm trees looking aside while Dr. Cushing skillfully pushed his ball. We were in his house, sitting with him and looking over his books because he had a lot of his rare books out there at his house. We had very interesting dinner parties with the Cushings, his daughters, so that relationship had existed a good while, and I rather think that they--in fact, I'm quite sure that they were important in at least having my name brought forward to this Board of Governors of the New York Hospital. I don't think the Board of Governors would make an appointment without inquiring into quallifications. They were important.

The other daughter of the Cushings had married the Preisdent of CBS.

The radio station--Paley.

Bill Paley [William S. Paley]--we used to go out and see them on Long Island, not far from where you live, where Mr. Whitney's house was. [Manhasset]

You indicated earlier you studied the central laboratory at Yale. How did that come about? I haven't found anything out about that. Was this an interest of your own?

No--as I say, when I went back there after the war, I was interested in--well, we had great laboratory aggregations in the Preventive Medicine Service, and I knew something about central laboratory work, how they were managed, and I forget how the question came up at Yale at this time. There was a move always for reorganization, consilidation, economies--this was a study that ought to have been done. Perhaps I had nothing else to do.

I was thinking of it as to whether the post war period--certainly the war time period disclosed the need for group effort. Dr. Winternitz's old idea--study units. You had a whole series of study units during the war under convenient headings.

All the commissions, and there were plenty of others.

I wondered whether in the post war period you saw from the Childs Fund point of view an increase in the development of study units, group attacks as distinct from studying on an individual basis?

I can't say I did. We always were betting on individuals and hoping to find a brilliant young person as Mr. Childs said, the Banting behind the attic doorway, or the cellar door. The Childs Fund continued to foster conferences and occasionally called together groups of consultants. That runs through the whole story, but no definite program of organizing study groups; as a matter of fact, I don't think an outside foundation can organize a continuing study group, unless it comes out of the will and wish of the people concerned in the place where it is. You can have large conferences that last four or five days, and then they break up and go away. We had some of these.

I'm just fishing really--but given the field of cancer as elusive as it is and the number of illuminating views you can take with respect to it, whether chemical, genetics, or whatnot, while you still support individuals, doesn't it take a fantastic synthesis now to climb on the various materials that have been produced within these areas?

Certainly.

This isn't a generalist in any way, but it would seem to be either a man who has combined within himself many disciplines beyond the normal one where you'd say that he is a bacteriologist. That's not fair either because you disclosed in the early work that you did, developments in terms of variation which required you to learn about heat, measurement, size, and ultimately metabolism, to learn something about carbohydrates, but the way that has gone since--how complex it has become. It makes me wonder where one draws the line between support of an individual, or support of a kind of seminar approach., I don't know. As I say, I'm just fishing.

Most modern research is team research, led usually by some one man, but he's got very able associates. It's too big and complicated--the attack is to big and complicated to be carried on against one block house as it used to be in the early days. The research, however, can be narrowed to as narrow a field as testing out a group of viruses like adenoviruses, or some of these peculiar viruses that produce cancer in mammalian species, or it can be as broad as the whole deoxyribonucleic acid genetic problem--chemistry and genetics. I think anybody working in the field to be any good has to have broad interests and broad comprehensions, and how much, however, he applies at any one particular stage of his investigation depends on his judgment.

I'm making a judgment on the basis of no experience whatsoever, except reading, some of the things we've talked about, some of the stuff I've read in preparation for our talks, and that is that in dealing with infectious diseases and the approach one takes toward them, or can take toward them, from the Preventive Medicine point of view you had at your call, or at the pleasure of the President of the Board, groups of experts--you know, where they could go and see--like scrub typhus, where they could follow it right down, so you could create for yourself a manual on how to avoid it. This isn't so in the cancer field. Cancer doesn't lend itself apparently to that kind of operation. I may be as mad as a March hare, but it seems to me that it doesn't, and the volume of material that is published today in the field of cancer--I don't see how anybody climbs on top of it for the synthesis. Maybe they don't get enough time to put their feet up and dream and think away the way the individual might--I don't know.

I think they do. I think there are analogies in scrub typhus to cancer.

One of the analogies--take any form of leukemia. You can run that right down from the start to the virus cause of it, maybe, and the treatment--put it all in a package. You can take cancer of an organ, like cancer of the stomach and relate that to genetics, food habits, chemical effects. I don't think it's so different.

Are you going to be able to interupt the cycle as you can with malaria?

No, malaria is transmitted by a vector arthropod, and you interrupt the cycle by getting rid of that mosquito. Cancer is not transmitted in that way. There probably are elements that you can interrupt, and one of those is radiation. There used to be plenty of cancer of radiologists who were exposing their hands in the early days to x-ray. They broke that cycle by protecting their hands, or changing the methods of handling the material so that the exposure wouldn't be there. There's an attempt to do that now with fluoroscopic radiation, radiation from taking x-ray pictures, and radiation from x-ray treatment--attempts to cut down the amount of radiation an individual has with a view to possibly decreasing cancer. They're trying to break the cycle by early diagnosis like the George Papanicolaou smear work in cancer of the uterus. They are doing the same with cellular studies of material sucked out of the stomach. I think there must be some--well, in any process there's some place where you can break it.

That would seem to be a reasonable expectation. This was one of the reasons why I thought you might have been somewhat disenchanted with a much longer haul.

It didn't last very long. I was back in 1946, and out of it by the middle of 1947.

Then the personal association which is real and vivid for you, as it has been in any number of places with older people like Mr. Childs. Once that radiation, or whatever it is he was, is removed, it's a different gambit, and chance being what it is--we'll go to the Joint Administrative Board next time.

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I've gone through piles of paper on the Joint Administrative Board. It's kind of difficult to know just where to begin in terms of what you fall heir to. You ought to give some retrospective view of the development of the hospital and its relations to the medical school before you arrive, the circumstances which led, after a period of years, to filling this office of President of the Joint Administrative Board--I'm not sure it was referred to as president at the time--and, as you saw it, the need for revision in the Charter of arrangement which would give more precise scope and dimension to the functions of the Board. There also is a climate to which you fall heir and which I think you can detail. It makes a difference in understanding the minutes of the Board as to where the interests of various groups that are present are located. That's rather vague, but within these vague contours....

I'm going to start with my acceptance of the extraordinary offer which came to me, or my eager acceptance of the extraordinary offer which came to me, to be what was called at that time the President of the Joint Administrative Board. I had for some months been visiting the hospital and the medical school, and I had a number of conferences with groups of members of the Board of Governors. I became fairly familiar with the magnificent structure and building and the very inspiring history of the hospital particularly. The Society of the New York Hospital was established by a Charter issued over the name of George III in 1771. It came about that that Charter was drawn up by the Council of the Province of New York and didn't get to the Privy Council in London. The King never saw it. They put the King's name on it that allowed the incoming governor, the Earl of Dunmore who was the representative of the King, a "beloved lieutenant", to act for the King in these matters.

The establishment of this hospital came about through an address given by a great physician who had been trained abroad, and that's Samuel Bard. I brought a biography of Samuel Bard this morning for you to look at in addition to one of the histories of the New York Hospital. In his address of May of 1769, Samuel Bard spoke of the usefulness and the necessity of a public hospital with excellent ideas of its functions. One that interested me particularly was what he had to say about teaching. He said the hospital should be an educational institution. I'm reading a quotation from Samuel Bard's discourse on the duties of a physician in which he said:

> Another argument and that by no means the least for an Institution of this Nature, is, that it affords the best and only means of properly instructing Pupils in the Practice of Medicine; as far therefore, as the breeding of good and able Physicians, which in all Countries and at all Times has been thought an object of the highest Importance, deserves the Consideration of the Public, this Institution must likewise claim its Protection and Encouragement.

Therefore, right from the very planting of the idea of the New York Hospital, an educational, teaching hospital was envisoned. We like to think that the Johns Hopkins Hospital was one of the first of its kind; as a matter of fact, the New York Hospital was the first of its kind, a teaching hospital in the United States. Pennsylvania Hospital had been founded by William Penn in 1751. It was twenty years older than the New York Hospital, but it did not have the teaching ideal, or a program like the New York Hospital. I thought these were wonderful sentiments expressed by Samuel Bard and the people who were associated with him--John Jones and other fine men, Cadwallader Colden--all had high ideals for education as well as for the care of the sick. I found out later since I studied this matter at the New

York Hospital that as early as perhaps 1714, a Scotsman had laid out a plan for a teaching hospital almost in the same words, so the idea was in the minds probably of all intelligent physicians.

New York Hospital's opening was delayed by the revolution. The British occupied it during the revolution. There was a fire in the place, and they had troubles of one kind and another, so the hospital really didn't get going until 1791. After that, it served its community and state in the most high-minded, generous, and intelligent manner and seen became an internationally famous institution. Everything about it to me was very attractive both from the esthetic point of view, the beauty of the building--a beautiful building which I'd better explain now. It was a building I came to dislike a good deal because it was frozen beauty, so to speak, because it impeded medical activities. The central building is twenty-eight stories covered with limestone, with corridors many faced with marble, and with rather Gothic types of windows, with many, many angular panes in them, small panes. The building was built on the principle of the segregation of diseases according to the sex of the sufferer, so that women with brain tumors, or needing neurosurgery, were on the female part of the surgical section, and perhaps two hundred feet away would be males with brain tumors and needing neurosurgery. That went all through.

You could see from the beginning that you would have to revise a system like that to be modern, and when I talked to Mr. Henry R. Shepley, the architect from the firm of Bullfinch in Boston who planned this marvelous building, about the rigidity of this frozen, beautiful structure, he said that I really didn't appreciate the fact that this building was built with hundred year material. I told him that I thought he didn't appreciate the fact that it was also built with what they supposed were hundred years ideas. The situation illustrated to me that if you are going to build a hospital in these modern times with great and rapid changes in medicine, you'd best build something you could push down in twenty years or so, not just keep it because it's "hundred year material." That you could see was an oncoming problem at the start.

New York Hospital was a very populous place. At the time I was brought there the bed capacity was upwards of a thousand beds; whereas in the original conception when the joint Cornell-New York Hospital arrangements were made they talked in terms of four hundred beds. A thousand beds were in that place at that time. In addition, the elevators--I think there were eighteen elevators in the building--were antiquated before they were putting in the first hoist. They are hand operated elevators--too small and not sufficiently arranged in usable groups. The base population of the New York Hospital-Cornell Medical College, and the School of Nursing, and the employees was about fifty-five hundred. We counted the visitors added to that one year to be hauled by these hand-operated, slow elevators, and we found that about 750,000 people were coming into that place. The traffic problem was enormous, but these same hand-operated elevators are still there because it is enormously expensive to change these elevators going up twenty-eight floors and requiring new electronic button control in place of some girl standing in there slamming the doors by hand and pushing a crank to start and stop the elevator which rarely stopped level at a floor.

Other things that you could see would be needing rearrangement for the sake of the people in the hospital were, for instance, the toilets. If you broke away from this segregation of diseases according to the sex of the patient and put males and females in the same s ection, ~~we~~ As we did later in the same ward where the water closets and the bathrooms, w ash rooms, had been male or female, you had to make a new set. To run flush toilets down twenty-some stories also adds a great d eal to the cost of alterations.

Also at that time there was coming into modern hospital construction the piping of oxygen to beds where patients had to be put in oxygen tents and oxygen had to be all through the hospital. In New York Hospital it was still wheeled around in cylinders on hand manipulated trucks; whereas in modern hospitals they pipe the oxygen in from some tanks buried in the outside and brought up through pipe. You could see these things when you first went there, but you knew that it would take a long time and a lot of money to get any changes.

I w as thrilled by the appointment. It s eemed miraculous to me that I should have such an opportunity put before me. I entered into it with the greatest enthsuiasm and still think of the experience, the six years I was there with admiration for most of the people with whom I w as associated, with great respect for the institutions and all they accomplished and all that they were planning to accomplish, and deep affection for many of my associates.

It had a bad start which I will tell about briefly and without any particular hard feelings now. I found out soon that I was not expected, or at least nobody paid much attention to my coming there. I was aware of that so that I actually telephoned Mr. William Harding Ja~~k~~ckson and asked if somebody would meet me the day I arrived. He s aid that he would meet me at the door of the hospital which he kindly did, and he showed me to the office that they had set aside for me. If I hadn't called him, I think I would have arrived without knowing where to go. I never had any notice at all of my appointment from the Trustees of Cornell who took part in making the appointment with the Governors of the hospital. That was managed by Chancellor Rufus Day. I don't believe Rufus is his proper name.

Edward Ezra.

Edward Ezra Day. We called him Rufus because he had a red tinge in his hair. He put the appointment through the Trustees, and naturally they had to take action in the Trustees for two reasons--one, to give me some authority in the position in which I was being placed, and twe, for financial reasons because my salary was equally divided between the Society of the New York Hospital and Cornell University. In looking over these papers with you, neither of us has found any indication that the Trustees sent me a notifcation of my appointment.

You ought to explain why....

I'm going to get around to that. I'm talking now a little bit about my reception in the place. I'll give you an explanation of the origins of some of these actions in a few minutes.

The Dean of the Medical College, Joseph Hinsey, had been told--I found out later--that my coming there would have no effect upon the method of arrangements by which he'd been conducting his part in the center and the medical school. He was obviously hostile and regarded my coming as an intrusion, and rightfully so. He was right in doing so in view of what I knew he had been told by Chancellor Day and others.

The background of these curious difficulties really is to be found in the events from 1928 to 1935. In 1928, a rather loose agreement between the Society of New York Hospital and Cornell University had been in effect by which there was an association between the Cornell University Medical College and the New York Hospital. The college was then down on 1st Avenue near Bellevue, and the New York Hospital at about that time was down on 18th Street near 5th Avenue. Well, obviously in thinking about the further strengthening of this union and increasing the facilities for both the school and the hospital, new lands had to be acquired, new buildings put up, and about 1928, they began this joint building project on funds that had come to the Society of New York Hospital from Mr. Harry Payne Whitney, who actually though, had a very deep interest in the Cornell University Medical College so that in his contribution to this enterprise, the development of facilities, the development of programs for medical teaching, research and care was in his mind naturally to be carried forward by both the medical college and the hospital. I don't remember all the details, but some of his gift was intended for the medical college and some of it did go to it. They appointed as director of the New York Hospital-Cornell University Medical College Association Dr. George Canby Robinson who in 1928, when this was done, was....

Tulane?

No. I'm trying to do with arithmetic. He was born in 1878, and it's now 1928. He was fifty years old and had had a brilliant career as Professor of Medicine in St. Louis, Acting Professor of Medicine at Hopkins, Associate in Medicine at the Rockefeller Institute, Dean of the Washington University Medical School in St. Louis, and Dean of Vanderbilt Medical School in Nashville. He was a productive investigator and a very vigorous man. He came into this project at the time that they were beginning to build what was later called the New York Hospital-Cornell Medical Center, on a tract of land, a good many city blocks on 68th Street and what was then called Avenue A, now York Avenue, extending from 68th Street up to about 71st and from the East River on one side to Avenue A on the West.

Cabny Robinson accepted his responsibilities and enlarged them rapidly. He was a great power in the building of the place. He was given authority to alter plans, to make commitments of money, and he had real authority to make some of the appointments on the hospital staff and on the faculty. He was, in effect, the director of the whole center, and he was carrying out his duties without sufficient authorization from the standing agreement. The agreement had been outgrown, and it was mutually agreeable to both the Trustees and the Governors that these things should be done. I don't mean that Dr. Robinson was doing anything that was out of order with respect to his superior authority. His manner, however, became more and more the manner of a commander, and he began to get in trouble with the faculty.

After the building was open in 1932, and the educational process and the care of patients were transferred from the downtown locations to this new place, different problems came about that required a different manner of handling them. He was used to--I don't know what the explanation of all of this is, but the point I'm getting to is that in 1935, the faculty declared that they no longer had confidence in Dr. Robinson, and he left. It was a great shock to him, broke his life up, I think, although he continued to do various pieces of work like carrying on with the Red Cross Blood Program and was for many years Director of the Maryland Tuberculosis Society. He moved, however, down to his home in Baltimore, stayed there, became progressively arthritic, progressively sick and progressively concerned with what had happened to him. I wanted to get at the story of those events from him, so after I had gone to the New York Hospital-Cornell Medical Center, I went down to see him. He had prepared for this meeting by setting out on the table all of his papers and correspondence justifying his actions plus a considerable number of pages of a manuscript he was writing to give the correct story of his life and his relationships. Fortunately after a long talk, he was able to say that it really didn't seem to matter so much now. As a matter of fact, he wrote me when I came back after that meeting to please tell Mr. Jackson, Day and the others that he had decided not to go on with working his case up, so to speak, but would drop it. He said in his letter that it was all right since I was going into the position there and, to use his own words, he had torn the damn thing up. I think he was happier after that, and so were we in New York.

After Canby Robinson left the New York Hospital-Cornell Medical Center, they made no appointment. The authorities of the hospital and those of Cornell did not make any appointment of a director of the center, so that the medical school from 1935 to 1947--twelve years--went on its own, so to speak, within the center, the hospital more or less on its own within the center, but neither one could go its own way because it's obvious that they met on many points and many associations, so that somebody had to be in the middle to help them make up their minds, to settle questions, and the person who emerged into this important, commanding, or advisory and influential position was Dr. Henricus J. Stander.

Dr. Stander was a square built, slow thinking, able, opinionated Boer from South Africa. He was Professor of Obstetrics. He had built his obstetrical clinic up in the fashion of the Frauen-Klinik of the Germans which was almost a complete and separate institution of its own. He had his own library, his own pathology section, his own obstetrical surgery, his own obstetric nursing group, a separate anesthesia unit--he had just practically a separate obstetrical hospital. He had real reason to do that historically because the section of obstetrics in the Center was the old Lying-In Hospital. The Lying-In Hospital had a long history going way back to the early 1800s. It had been a pet project of J. P. Morgan, had enormous support, and had affiliated with the New York Hospital and allowed its integrity to be preserved. Actually it had its own Board of Managers, so to speak. It had a strong committee, and Stander inherited these traditions, and you can still see it in the present catalogue of the New York Hospital, the Lying-In Hospital stands out almost as a separate institution.

I wish you'd explain something that you told me about your approach with Grover Powers.

I said that Stander was very opinionated, and the example I gave you was his resistance to the adoption to what is called "Living in", a system by which the baby is put in a room with its mother right from birth and not shut up in a little nursery somewhere separate. The father is allowed to come in to the room with the mother, and the father and the mother get early acquaintance with their baby so that there's really no break when they get home together. It's still all in the family. It's very valuable psychologically both for the child and for the parents, but Stander didn't see it because it upset routines. I showed him that it was rather unnatural to do what they were doing in obstetrics in New York Hospital by separating the baby from the mother right away and letting her see it only when she nursed it. Sometimes she didn't nurse it, and she didn't see her baby very much. His reply, explaining why he didn't want to make any change, was, "You know, this professional management requires you to do unnatural things."

He was like that in a good many things. Stander was extremely able, and he developed into a powerful figure. He was at one time President of the

Medical Board. He was a patriarch. People in both the medical school and the hospital brought him their problems. He had much to say about these things. I think he explained to this medical faculty that my coming to the Joint Administrative Board would relieve the department heads of a good deal of tedious, administrative work--that was his conception of my function. In spite of all that, Stander and I had been friends at Hopkins, and we were still friends up there. There never was any break.

My relations with the medical school--I came in as an imposition over their habits and the authority and behavior of the Dean. Dean Hinsey had been assured that my coming would make no difference, and the faculty didn't welcome me in any outspoken manner. The Dean of the School of Nursing, who became a dear and extraordinarily interesting friend, Virginia Dunbar, didn't know where I fitted in because, as I remember, she said that all she knew about me was what she read in the papers. I got to be very close to the School of Nursing, got a very deep interest in nursing education, which gave me the opportunity to satisfy this interest to a considerable extent.

When I got there, there were several things afoot which I had to take hold of immediately without knowing really much about them. I'll speak of three. One was the beginning consideration of the affiliation of the Hospital for Special Surgery, which was an orthopedic hospital down on 42nd Street near the East River, with the New York Hospital. That hospital had a tradition and a pride, and it didn't want to have its identity smothered if it came up and joined the New York Hospital-Cornell group. It was very difficult. Negotiations had been going on between some of the Trustees of the hospital and some of the Board of the Hospital for Special Surgery, and the first thing I had to do after I arrived there, was to preside over a meeting of a group, a joint committee, that was considering the negotiations. I found from that meeting that the hospital had one way of thinking of things and that the Hospital for Special Surgery representatives had another, and neither one was telling the rest of their colleagues just what was afoot. That even affected the characteristics of some of the minutes of this first meeting, a draft of which was sent to me when I happened to be out in Colorado and was written by a representative of the New York Hospital. It was so different from what I recalled of the meeting that I had to do it over. That was one problem that was stewing, so to speak, at the time I arrived.

Another one I soon found out had to be tackled was an understanding that had been reached between representatives of the hospital, not the Board of Governors of the Hospital, but I think it was in the directorate of the hospital, Murray Sargent, the director, the Dean of the medical school, the Chancellor of Cornell University, and the treasurer, the executive treasurer, executive officer of the medical school, Mr. Edward K. Taylor, to take or to receive from the hospital as a gift, a piece of land between the power house and the East River, bounded on the South by 71st Street and on the North by 72nd Street, unoccupied by any building at the moment. This piece of land was to be put to use as a site for a dormitory for the medical students. Medical students were living in very poor quarters around Avenue A, 69th Street and a dormitory was desperately needed. They needed proper housing. The plan to build them a club like dormitory right over the banks of the East River facing the sunrise was most attractive, of course.

Deeply concerned in that was the most famous and influential alumna of the Cornell Medical College, Dr. Connie M. Guion. I was told that I should be very careful not to do anything that was contrary to the wishes of Dr. Guion. My conception of Dr. Guion before I met her was that she was probably a termagant and got her way by frightening people. I couldn't have been more wrong. She's a most gentle and charming lady, a North Carolinian--very idealistic, quick sensibilities, understanding, a beloved physician who took care of many, many important people in New York including Mr. Whitney, Mr. Vincent Astor, and others. She's the most trusted physician on the staff. She was in private practice and had a clinical appointment in the hospital. She was a good teacher. She was vigorous, lively, full of ideals, and perfectly honest. We soon talked about this plan, and she saw right away what I saw and which I had confirmed before I talked to her, that that was the last bit of unoccupied land in the immediate vacinity of the New York Hospital on the site on which it was built, and indispensable for future hospital expansion. I was able to get from Mr. Shepley a record of the fact that the Governors had told him, or put down somewhere, that they looked forward to preserving that land for hospital building later on. I was able to get Dean Hinsey to reverse his point of view. Dr. Guion first accepted the view that the land ought to be preserved for the hospital and began to think about a medical dormitory on York Avenue which has since been built and is a very fine building. This was urgent problem number 2.

The third problem, of course, was my relation with the school and the hospital because, as a matter of fact, there was nothing in the old joint agreement of June 14, 1927, that really covered the then present situation. They needed to revise that agreement to provide for the Office of the President, to define his powers and their scope, and it turned out in a short time to set the President in the Medical Center in such a position that he had a normal and easy relation to all the components. This was particularly acute with relation to the Medical College because Dean Hinsey had been told by Chancellor Day that my coming there would make no change in his relations. One of the arrangements I found needed to be changed was that much to my surprise, I was not expected, or invited to sit with the faculty in its executive sessions. The faculty was a large faculty, and like all faculties, it had to divide into an executive group, the heads of the departments--at Yale it was called the Board of Permanent Officers, at other places by other names. It was called the Executive Faculty, I think, at Cornell. Actually when I told the Dean I was coming to the next meeting, he stood in the door and said,"You don't enter this room."

He was not aggressively opposed to me, but he wasn't cordial at the start. An example is that we had a meeting of the Board of Trustees of Cornell in the room of the Board of Governors of the New York Hospital. They met there once a year routinely, had a luncheon at that time. I was placed at a little side table along the wall with a secretary and was hardly spoken to by anybody. As I say, when I look back on it now, I don't think I regarded it then as intentional hostility as much as an evidence of a reaction against a stranger coming into the midst of groups that had been living together in what they supposed was harmony and productiveness.

These experiences with the school and the need for definitions caused us to get a revision of the Joint Agreement, as it was called, and I think that was accomplished within a year.

<u>I think that's quite early--November, isn't it?</u>

It's within the year anywhow.

<u>Yes.</u>

It was within the first year of my being there. I recall that in this effort to get the Joint Agreement revised, Mr. William Harding Jackson was enormously helpful to all concerned. He took this problem, a legal problem, and he was a lawyer, and he had meetings of his own with the Trustees of Cornell, with me, and with others, and it was done by December, 1947.

Yes, the amendments to take effect as of January 1, 1948.

Even that amended agreement was not entirely satisfactory because changes were occurring all the time, and toward the end of my term down there, in response to Mr. Neal D. Becker, as I remember, one of the Cornell Trustees, was a plan to review the whole thing and revise--review with no thought of breaking anything down, but revise to fit a situation that had developed.

The powers of the President were extended, and it gave you....

You mean in 1948?

Yes, and it gave you access to the various components.

It really recognized that by this time the New York Hospital-Cornell Medical Center had four main components. One was the Cornell Medical College. One, we can say, was the Joint Administrative Board field. The third would be the hospital itself, and its satellite hospitals--Payne Whitney Clinic and the Westchester Division, and the School of Nursing. Each one of these was represented by a budget. Therefore, it was easier to keep their interests in mind. The medical school had a budget. The joint operation had a budget. The hospital had a huge budget. Payne Whitney had a budget. Westchester had a budget, and the School of Nursing operated on a budget which was entirely provided by the New York Hospital.

Let me say here--while I'm thinking about the School of Nursing, let me explain its name and how it came about. It used to be called the New York Hospital School of Nursing. It was a hospital school of nursing. They had some difficulty in attracting high class nurses who wanted to have superior education and a university type of degree so that shortly before I came there, Chancellor Day and the Trustees at Cornell had agreed to take the School of Nursing under the aegis of the university, provided it was called the Cornell University-New York Hospital School of Nursing and provided the university would not be asked for a cent to meet its expenses. They adhered to the latter with great fidelity.

Curiously, there was something apparently very canny in the upstate New York Trustee group even though the men themselves may have come from more liberal communities in the South and East, or the middle of the country. Their attitude toward the income and expenses of the medical college as such was that the medical college had to live on the income from its endowment, which was only about a million or so dollars--the endowment was about a million--and the fees from students. The Trustees would not countenance a deficit no matter if the income was remaining constant and the costs of medical education, the costs of running the school, were going up all the time. The medical school made a contribution to the clinical departments which were regarded as joint departments. That's what surgery, medicine, pediatrics, obstetrics and some of the specialties were--joint departments. The budget of the joint departments was shared equally at the start of my coming, between the medical school and the hospital. After a while, the hospital had to put up a bit more than the medical school and the supplementation of salaries was made for the benefit of both institutions by using money from what we called a "full time fees fund", the money collected by the full time doctors from seeing patients which they had to collect. They had to make charges, but the money collected went into a general fund which was used under the direction of the President of the Joint Board and the Joint Board for the benefit of the combined institutions. That was a considerable expense to the hospital, but the Cornell Trustees regarded it as--I was going to say a Shylock type of....

<u>It was a deal.</u>

A deal--yes.

<u>Right.</u>

I took some time to learn about these expenses and budgeting because it was very intricate, but it did work out that under this revised plan, I saw all the budgets. I sat in the meetings of the Board of Governors. I was invited, on the hospital side, to attend the meetings of the Finance Committee all the time. I sat in the medical faculty and talked to the Dean of the medical school about his budget, both the medical school and the joint budget, and I had much conversation with Miss Dunbar about her budget. In addition, Payne Whitney had a separate endowment and budget, and that came through the hospital, but the Joint Board also saw that. I was taken in to the Governing Committee, so to speak, of the Westchester Psychiatric Division which ran on its own, income and expenses. I saw its budget, but its budget didn't come in through the joint arrangement. It's a curious thing how much the passing around of a piece of money, no matter how small it is, is coupled with intricate political and personal, emotional problems, a token that is very valuable beyond the money, and the money exchanged in these manners opens up slots and doors that probably wouldn't have been open unless there was a transaction going forward.

I think my relations with the hospital were fairly easier than those with the medical college at the start, but after a year or so, it was for me all one family. The separation that disturbed me very much and probably disturbed the other person was the separation that soon developed between myself and Mr. Jackson. I can see his point of view very well. I think I was a great disappointment to Mr. William Harding Jackson, the President of the hospital because he used to talk about balancing the budget of the New York Hospital, the deficit in which was then running around two million dollars a year. The total budget was about twenty million. To balance that, or to wipe it out rather, would cut off a tenth of the expenditures, and when you start to do that, you cut down services. You'll have to stop making salary increases. You'll have a small staff. You'll lose staff. Your morale goes to pieces. I really didn't do much about trying to balance the budget, except studying everything very carefully and soon realizing that little businesses of petty savings really didn't amount to very much when all of a sudden, for example, you'd have to replace the great oil burners in the heating plant--two hundred and fifty thousand dollars in a week, or when the elevators needed replacing--very expensive, all those things, and the costs of medical care going up and up and up.

I was amazed to see that even though the deficit was there, the gifts and legacies wiped it out. They didn't want to count on those as fixed income, but almost every year the huge deficit dwindled to a rather small figure when these gifts and legacies were taken into consideration. For instance, annual giving--just the simple annual giving in those days to the New York Hospital was about six hundred thousand dollars a year. It just flowed in, and then enormous legacies came in all the time. People had willed their money in the past, and it was coming due so you got a feeling that it wasn't too dangerous.

The other thing with Mr. Jackson you'll see in these letters he writes before I came, that my ideas about the social aspects of medicine were close to his and that he looked forward to some developments. I don't recall those conversations as being very specific. It would be perfectly honest of me to say that I can see the value of group practice and institutional medicine, and I did, and when I was in the Army, I spoke at Rochester and at Yale of the excellent impression that had been made on me by the practice of medicine in Army hospitals--a great big organization, but as good medicine as you want to see anywhere. The things in this field which concerned me, Mr. Jackson, the hospital, and the school were first, a plan that had been debated before I came there to establish in the hospital a so-called diagnostic clinic. Some of the Governors had been working on this, particularly Mr. Jackson. He wasn't clear about it, and he still had doubts as to the legality of the proposed arrangement. The arrangement was to be a clinic that offered a complete diagnostic service with a referral to some private physician, if necessary, or admission to the hospital, if there was no private physician to be had. They were to admit to this clinic people of any income level--rich, middle, not indigents though. The latter could be taken care of in the ordinary outpatient department. Those admitted to the clinic would be charged a fair sum in a sort of modified group practice, except that the element of prepayment was not in there. These would be payments made for services of the clinic at the time when rendered, but there was not in the New York Plan a plan for prepayment group practice, comprehensive service.

Mr. Jackson had been working on that for some time, and I think he was very much influenced by the success in 1947, of the Henry Kaiser Permanente Hospitals and group health system and service in California, Oregon, and

Washington. This had been started in the war in the desert in California by a very remarkable doctor named Sidney Garfield. At the time, when they were putting in the great irrigation system for Los Angeles, there were about five thousand laborers in the desert, and they had no way of taking care of them. Garfield's original plan was that they would contribute five cents a day. They started on that, and they soon paid off the debt for the hospital he'd made, and he made money. He was able to pay the salaries of a group of physicians, and that was a big success right away. Mr. Kaiser got in it, particularly when his ship building enterprise was developing in San Francisco after Pearl Harbor. He, however, had to limit it to the men who were employed because he had contributions coming in to this plan from the subcontractors and insurance. That has succeeded enormously, as you'll see from the books that I brought up here. It's a great success.

My feeling was that perhaps some of the Governors and possibly Mr. Jackson were looking on this plan of a diagnostic clinic--in fact, they say so openly--as a means of making money to reduce the deficit of the hospital. At the same time in New York a very extraordinary comprehensive, group medical care and health program was developing under the direction of Dr. George Baehr. It was called a Health Insurance Plan for New York City--abbreviated and always called HIP. Its affairs and what was happening influenced me considerably to go slow. HIP activities of furnishing pre-paid medical care by physicians paid for from income derived from those who subscribed to the plan was violently attacked by the New York profession in medicine. All the important people there in the New York County Medical Society--Dr. William B. Rawls, Dr. B. Wallace Hamilton, and a good many other were more set in their ways than even the American Medical Association. They accused HIP of all sorts of skulduggery and unethical conduct and fought them just as hard as they could. If the New York Hospital had attempted to enter right away into a plan for comprehensive service on the prepaid health insurance plan, they would have had even more reason to talk about the corporate practice of medicine because the hospital would be doing it, and the hospital was a corporation.

They actually made such an accusation later on which I might as well go on and tell now. It could hardly be said lack of courage in not going forward with this development of either a diagnostic clinic at the moment, or a prepaid health insurance plan because it wouldn't have affected my fate very much whether the battle had been won by the hospital, or the battle had been won by the set practitioners in New York. I must admit that I was frightened by the severity of some of these people--Dr. Rawls was a very hard man. Dr. Hamilton tried to put me in my place immediately on my arrival. He was the all powerful secretary of the New York County Medical Society, a very dapper, incisive, well dressed, thoroughly reactionary man. I applied for admission to the County Medical Society because I always thought that a physician ought to be a member of his professional societies like the American Medical Association and the County Medical societies. I was such in Maryland, in Rochester, and in New Haven, and I wanted to do the same thing down here [New York]~~, but they~~ had a requirement, which could have been waived, that an applicant for admission to the New York County Medical Society had to be interviewed by a member. They usually waived that in the case of a man who had been a dean of a medical school, or had been a general in the Army--things like that. Not Dr. Hamilton. Dr. Hamilton fixed an appointment for me with a Negro physician in the basement of a tubled down house in Harlem, and

I had to go there and stand out on the street with other youngsters just out of school waiting to be interviewed also. Hamilton did that just to be offensive, I think.

Well, I went and I got to be friends with this fellow who interviewed me. We were a little embarrassed at first. No sooner was I in the Medical Society of the County of New York than they made me chairman of the Committee on Public Health and Education whose immediate problem at the moment was an intense difference and difficulty over segregation. New York was beginning to adopt some laws in the state against discrimination--they didn't call it segregation; they called it discrimination. They accused the New York Hospital-Cornell Medical School of having a quota with regard to Jews. Whether or not there's a quota in the schools, it's very curious though that the Jewish proportion of the classes of most of the medical schools is around fifteen percent. It just happened that way, but this applied to Negroes too. Cornell took Negroes when they got one that was good enough, but they weren't good enough as a rule. It's very tragic to take a Negro into a school of higher education like that at that time and have him fail. That breaks his life up, and it's a loss of time for the other people. Well, this was a very severe affair in the county, and they made me chairman of this committee right away. I had to learn all about it and make a report about which I have forgotten all the details, but it had to be given at a very large, open meeting at which the debate was acrimonious among the members of the two sides of the questions--whatever they were.

Those were signals to me that there was trouble in this town.

I guess they were.

They slowed me up, or frightened me because I didn't do too much at first, in the first years, except to learn about the place and learn about the people, and I'm sure that Mr. Jackson got impatient. I think he said to me once,"When are you going to do something important?", or words to that effect. I remember that. Also there was a Murray Sanders.

You mean Sargent.

Yes, I mean Sargent. I knew Murray Sanders, but he has nothing to do with this. He was on the Army Epidemiological Board, one of the workers at least. Murray Sargent was to be replaced, and there really was no director of the hospital after he left. Mr. Laurence G Payson was made a sort of deputy president in Mr. Jackson's footsteps, and Payson, as delightful a man as he was, had no experience in running a hospital, so for months I used to spend most of the time over in the other side of the building with Payson, Wheeler and other people, doing the management of the hospital, which I didn't know anything about either, but which was very time consuming and probably diverted me from some other things that Mr. Jackson would rather have seen me do.

During that time--to show the lack of entente cordiale--the search for this director was conducted by me and unbeknownst to me by Mr. Jackson. He invited a candidate, a very fine man up to be looked over, but he didn't tell me about it. Finally with a great deal of force and foresight, Mr. Jackson decided to get the Governors to appoint Dr. Harry N. Pratt, who was then Director of the Memorial Hospital across the street, as Director of the New York Hospital. This was done, and for the face saving, it was done on the recommendation made to the Board by me, but it had been accomplished before that. Dr. Pratt is still there. He turned out to be a good director, though there was a period in his life as a Colonel in the Army when he seemed a bit too dictatorial, and he was sent home for a little relaxation for a while. Those are the immediately inherited problems which were pretty intricate and difficult to solve.

Did anyone ever convey to you why at this moment they thought they would fill this presidency? They carried on the way they had been carrying on for twelve years. I don't expect them to alter their spots, but some reason had to be offered for filling this post--what: to give a broad general view to the center which hadn't been done for twelve years.

I never inquired into it. I can guess. I think that some of the Governors thought that Dr. Stander had too much unauthorized power. They thought also that the Governors had much less access to the Trustees at Cornell than they wanted. There was no common meeting ground. Whatever--well, they didn't have anything like the meetings of the Joint Board in this period very much. I think that with a basic agreement which only needed a few changes to make it more effective and make it an instrument for unity in the combined organizations, it would be natural for them, thoughtful men that they were, to think of building on that basis. Of course, it was the end of the war too, and people were having an idea of a brave new world after the war.

This wasn't an unimaginative place by a long shot. There were good men here.

Extraordinary--the thoughtfulness and superior work that had been done in medicine and surgery, scholarly work of professors like Eugene F. DuBois and Charles R. Stockard in physiology, anatomy, and medicine. Whether David

Barr, the Professor of Medicine, worked on getting this job filled or not, I don't know. He'd been there only two of three years after coming from St. Louis. He had a great mind and a great concern for a whole unified operation.

It doesn't--maybe they looked for immediate miracles, and this was a very complex institution--just the budgets alone, and to get time to fence it in, they were perhaps not appreciative of that. For example, even in the area of the diagnostic clinic there was opposition to the very unfolding of the plan within the Center. You mentioned earlier that a private consultation service had existed there for some time. This had all been discussed before you arrived, and the decision was that they were going to put the diagnostic clinic through anyway. This was Jackson--that they were going to push through this diagnostic clinic irrespective of what the opposition thought.

The private consultation system had been more or less developed by Dean Hinsey out of regard for the young doctors who were coming out of the war. They were not only given special privileges by being allowed to practice within the hospital, but they had good offices fitted up. They had their own clientele, and there was a group of some twenty or more surgeons and medical men with offices in the hospitals and privileges to see what patients they wanted, and they built up quite an extensive medical enterprise without much of an organization. They kept their own fees. They didn't put it into a pot and divide it afterwards. They were all on their own. That became a vested interest that had to be divested later on. They regarded the plans--some of those regarded the plans for the diagnostic clinic as an infringement, as an activity that would decrease their opportunities for lucrative practice.

Without a head, without someone vested with the power of an overall view for twelve years--it's not surprising that the pursuit of personal interest had created little principalities within the larger organization, so that the ramifications of that only come up in time as problems emerge, and the initial one here with reference to the diagnostic clinic. It's so overshadowed by the outrage abroad in New York City, the fact that there were misgivings within the Center is overlooked, swept aside as not really important because the other was so important.

Well, the interests of members of the faculty like Dr. Barr and certain members of the Governors in the social aspects of medicine and comprehensive care was very deep and constant, and it resulted in the end in the actual establishment of what was called the Vincent Astor Diagnostic Clinic along the lines that we've been discussing, and, in addition, the development of a very fine system of comprehensive care based on the dispensary clinic contacts, particularly with medicine. A plan developed finally according to which a student who was assigned, we'll say, to a patient--they called it a case--would do examinations and carry on the study under the guidance and with the association of the doctor, or resident, or member of the faculty who was in charge of that particular branch of the clinic. That patient and that patient's relatives then became the family in which this young medical student carried on a supervised practice. These were outpatients, and the student would go from the Outpatient Department to their homes in the Yorkville District, which is a middle class district between 56th Street and 90th Street, East River and Central Park, and see these people in their homes and become very friendly and helpful to them. The students would watch the course of their illnesses, or their recoveries. They would keep thoroughly in contact with the hospital, arrange for revisits, arrange for consultations when necessary, a very sensible plan for the indigent. They were poor people. They could pay something, but not much. The physicians of New York didn't care, or didn't bother. That was the type of corporate practice which had no revenue that they could get.

That Outpatient Department is a striking thing because the system had to be worked out whereby the relationship between the Outpatient Department and the teaching mission of the hospital and the school was made manifest--that is, some basis whereby, and I may use the wrong word here, the selection of patients for illustrative purposes with reference to a student was the paramount thing.

That was a matter debated in the Medical Board and the Governors--a very serious thing. A number of Governors thought that the doors should be open to anybody who came--no selection. It was a democratic institution, and New York Hospital belonged to the people. It had that tradition, that there ought not to be any selection on any basis at all. The teachers, professors, and faculty members wanted a selection for several reasons. They wanted variety. They wanted to get what was called "teaching material" of great value, novelty and interest. In addition, this was the very sensible basis for selection--to keep your Outpatient Department from being altogether filled up with chronically ill people for whom you can do nothing. If you have, say, a capacity to deal with a thousand patients a day, or something like that, as we did, and if you have started the year with that number and you have a thousand patients coming, if ten percent of them have chronic arthritis, or chronic alchoholism, or some chronic illness that is not going to change very much and about which you can do relatively little, that increment of chronic invalids increases time after time so that you have no space left with which to take new patients.

You can always take emergency patients, if you want to, and by the way the emergency service at the New York Hospital which was relatively small when I was there, has grown to be a very big thing and is very well done.

This hospital has always been greatly interested in new ideas, new processes. In its early history, which you'll see in that book over there, it developed the first liberal psychiatric service in this country. It knocked the chains off the maniacs. They had a section hospital at Bloomingdale on the very site of Columbia where you've been studying. That's the Morningside Heights Bloomingdale Hospital of New York Hospital, and the Bloomingdale Hospital, the psychiatric Hospital of New York Hospital, is now out at Westchester, at White Plains. Well, they've done many things that were new in surgery and medicine, and they were always intellectually interested, generously interested in the broad things of the community and the country.

The problem of how to spend man hour time in the Outpatient Department on cases that would be helpful and illustrative to the student was part of the mission and not have them spend most of their time on patients that were called "returnees", people for whom nothing could be done. It was a matter of the expenditure of time and energy on the part of both faculty and students and no necessary learning. It raised quite a problem.

The Governors understood that. A certain amount of selection came in.

Well, let's stop for today. All right?

All right.

Friday, June 24, 1966 A-60, N. L. M.

Yesterday in talking about the past history of the developments of the Cornell Medical College-New York Hospital Association we may have, in a way, misconveyed that in the absence of Canby Robinson, or with the departure of Canby Robinson, a void was created. You did say that certain powerful figures emerged one of whom was Dr. Stander, and he seemed to have had matters pretty well in his grasp, but today we wanted to correct the impression that there was an absence of a central kind of feeling, or power used somewhere in this period before you arrived. For me, the considerations which led to their offering you this post still lurk in inference. I don't know why--why they would suddenly want to do this, or what the forces in the existing situation were that would lead to the recognition, or the isolation of a person especially in this post when one had not really been there to all intents and purposes since 1935, but there were some impressions that you wanted to correct.

In thinking over what I said yesterday, I felt that I had given the impression that the Joint Agreement between Cornell University and the New York Hospital in 1927, had been allowed to lapse and that there was no real, central organization to attend to the affairs of the Medical Center as a whole. That wasn't the impression that I should have given. The agreement had not lapsed. It remained in effect. There was a Joint Administrative Board. It held periodic meetings. It however did not have a director as required by the original agreement. The person who emerged as the unofficial director--unofficial as far as Trustee action on an appointment was concerned--was Henricus Stander. Evidently he was recognized as an authoritative person in the Center, and he handled much of the common business of the two institutions. I remembered after I had talked here, that during part of this period, anyhow, he was paid a considerable supplement to his regular professorial salary for his services as administrator.

At present we're not able to find among these papers a copy of the Joint Agreement of June, 1927, and yesterday we were referring to a paper here, a pamphlet called an "Account of the Agreement...." which really deals with the modifications and amendments of the agreement that became effective on January 1, 1948. I recall having written this pamphlet, and I know that the first two pages of it, while being an abstract of the main sections and clauses of the original Joint Agreement, are almost in the words of that agreement, so that this document, if taken carefully, can give the substance of the agreement under which the Center was operating from 1935 to 1947, and will show then the amendments that came in through events that arose after my being there.

Dr. Stander had been chairman of the Medical Board.

President of the Medical Board.

He had been President of the Medical Board for a long time--fourteen years.

Yes.

Which is a source of great influence.

Certainly.

And with continuity. I think in 1948, very shortly after you come there, within the year, Stander died.

Stander died in 1950, or 1949, somewhere in there./May 2, 1948/

We got you in the place yesterday with some views as to the problems that emerged the way they would come in. I indicated to you that I had been reading the Minutes of the Joint Administrative Board with respect to the problem of affiliation between Cornell Medical School and the Sloan Kettering Institute-Memorial Hospital. These notes begin, so far as the Joint Administrative Board is concerned, in September 23, 1949, but they disclose that something in the way of negotiation had been going on for some period of time. I thought myself that there was something else lacking in our presentation yesterday, so far as you are concerned, and that's the connection that you'd had for a long period of time--1942, I believe, with Memorial Hospital, its Board. You'd had something in the way of awareness with continuity so far as that complex is concerned for some period of time.

That is implicit or explicitly stated in some of the things that I told about the Childs Fund. I had frequent conferences with Dr. Ewing in the days of the formation of the Childs Fund when Ewing was the great scientific director and pathologist at Memorial Hospital. He was succeeded by Dr. Rhoads who was a close friend of mine and with whom I had frequent meetings. I also mentioned the Memorial Hospital developments when I told about the time when Mr. Reginald G. Coombe and Dr. C. P. Rhoads came to the Childs Fund, really I thought, on a piratical expedition, wanting to have the fund transferred to Memorial and Sloan Kettering. Soon after the Childs Fund had been established, in 1937, I was appointed a member of the Board of Managers of Memorial Hospital--not of the Sloan Kettering, but of the hospital. Sloan Kettering Institute for Cancer Research at that time had not been established. It came on later. It had its separate management and boards and went through, as is usual with a big institution, a complex coordinated situation like that, many, many changes of administration, policy, procedure, and operation. They had to feel their way a good deal, but I never was on the administrative board of the Sloan Kettering for Cancer Research Institute, although I attended a good many of their meetings. After I had left New Haven, after I left the Childs Fund, I was still a member of the Board of Managers of the Memorial Hospital, and I still was invited in to some of the Sloan Kettering-Memorial Hospital meetings. I knew practically all the main people at Memorial Hospital, but I do not recall being brought in to talks about this proposed association between the Memorial Hospital-Sloan Kettering Institute and Cornell Medical School and possibly New York Hospital before rather definite plans came up for consideration.

There must have been a great deal of discussion between Chancellor Day, Dr. Rhoads, Mr. Frank Howard, possibly Mr. Laurance Rockefeller, Reginald Coombe, and maybe Dr. Hinsey about these things, but I didn't hear much about them until they came to me as rather a finished product. As I think I mentioned, Mr. Poteat....

Thank you--that's good for me.

You can scratch that spelling out later. Mr. Poteat [J. Douglass Poteat] was the manager, or administrator of the Sloan Kettering Memorial Hospital complex, an assistant administrator to Dr. Rhoads. He came to see me in my office in the New York Hospital, the Joint Board Office, and presented me with a set of papers which outlined the proposed arrangement between, we'll say, Cornell University and the Sloan Kettering-Memorial group. This was the first time I'd seen them, and I told him after glancing over them, that I had never given any concurrence to such suggestions. He took the papers back to Dr. Rhoads, and that cut the matter down to where it could be examined in the Joint Board meetings, and from thereon consideration of this proposed arrangement comes in pretty definite in the subsequent meetings of the Joint Board, beginning--when did you say?

September 23, 1949.

Well, of course, a good deal had gone on before that. It was nearly a finished plan which, as I saw, was so much finished that the very day that the paper came over to my place, Mrs. Bayne-Jones and I had accepted an invitation to dinner with Dr. and Mrs. Rhoads and Mr. and Mrs Howard in Dr. Rhoads' apartment on the top of Sloan Kettering Institute to celebrate his elevation to a semi-dean's like position. The proposed plan, in essence, was for the establishment of the Sloan Kettering Institute for Cancer Research-Memorial Hospital complex as a subdivision of Cornell University co-equal with the medical college, and in my opinion, equipped by the plan to take predominance.

Dr. Rhoads was stirred to move in this direction by his intelligent appreciation that the Sloan Kettering Institute needed an educational program with a power to recommend the awarding of degrees, masters and Ph.D. degrees in science and biology, biological sciences largely, both for the improvement of the morale of the members of the staff who yearned for educational associations and for the recruitment for the staff. He could obviously get better men if he could give them a better opportunity for improving their minds and academic reward. That was the basis of Dr. Rhoads' concern--very high ideals of the value of the educational opportunity for specialists, biologists and other scientists, working on cancer in the Sloan Kettering Institute. The plan excluded recommendation by Sloan Kettering for medical degrees, or granting medical degrees; as a matter of fact, the granting of degrees was so well provided for in the interests of Sloan Kettering that, in my opinion, the graduate school, through which these papers would process, would have little to say about it at Cornell. The same thing might have happened at the medical school.

The discussion after this came out in the Joint Board--I should say after it was dragged out--was very prolonged, but not acrimonious. Although there were strong feelings behind the opinions that were expressed, people kept on talking to each other. My personal feeling was that Chancellor Day was so much impressed by the possibility of financial gain by the University through such an arrangement that he would have done harm to the Cornell Medical College's interest. I think that's in the minutes.

<u>Had there been any relationship between Memorial Hospital and Cornell Medical School prior to this?</u>

They'd had--Cornell Medical School, I think, had Dr. Ewing as a Professor of Pathology for many years. In the earlier days Cornell Medical College was way down town on 1st Avenue and 26th Street, somewhere like that, and Memorial was up on the Westside in the 90's, just off Central Park, so geographical separation made association quite difficult. They had intellectual interests, and of course New York Hospital had plenty of patients with cancer, so that there must have been a good deal of consultation going on. I don't know how much overlapping staff there was on the clinical side. Many of the surgeons that operated on cancer in the cancer hospital also had appointments in surgery in New York Hospital, I'm sure.

In addition, about this time, or through this time, this very great and fine personality, Dr. Papanicolaou, who had been brought to Cornell University Medical College by Dean Hinsey and supported there through grants, and one way or another, through many years of hard work, was discovering what's known as exfoliative cytology--the shedding off of cells from the uterus, or the vagina, or different orifices of the body which would disclose their nature when properly stained and studied. These are the Papanicolaou smears that you may have heard about, a whole basis for the early diagnosis of cancer and rather more certain a basis than inspection, not as certain however as biopsy, but you don't need to do any operation to get these cells--just wash them out.

That was of interest to the Memorial Hospital group. They were beginning to get interested in the prevention of cancer. They had a clinic supported by Mrs. Strang who was interested in cancer prevention, and the Papanicolaou early diagnosis would lead to a joint set of interests between Cornell and the preventive sides, or early diagnostic sides of Memorial Hospital's work. I imagine that there were other connections.

You have mentioned the Douglas Deeds of Trust. The Douglas Deeds of Trust had to do with a supply of radium which Mr. Douglas brought over and gave Memorial Hospital on some conditions, which I have forgotten, but which provided for common interest in this radium to be shared between Cornell Medical School and Memorial Hospital. The Douglas Deeds of Trust came up in connection with the study of the plans to make an association that we're talking about here between Memorial Hospital-Sloan Kettering Institute on the one hand and Cornell Medical College and the New York Hospital-Cornell Medical Center on the other. My recollection is that there wasn't much in the Douglas Fund at this time, and that the lawyers found the means for satisfying any of the questions that came up. It wasn't a point of great importance.

I just raised it as a footpath between Memorial Hospital and Cornell Medical School.

Well, the Douglas Deeds of Trust, I'm sure, had never operated to bring the two together. It was there and had to be taken care of.

The new organization in this picture then is the Sloan Kettering people. I think you ought to point out--certainly from my reading of all those minutes here of the Joint Administrative Board that there was no disenchantment with this as a possibility. Everyone wanted this affiliation.

Certainly.

The only problem is on what basis is the affiliation to be made and how that is to be worked out. There were any number of problems that were not thought of and were best thought of by delaying the affiliation until such time as they can bubble to the surface and gain expression. I think that's the process we're going through here.

In my opinion the medical school had to be defended against its own officers and that the forum in which the matter could be viewed from all sides was the platform of the Joint Administrative Board, and I think that it was, although prolonged, a profitable period of deliberation. What was done then is still continuing with harmony now. What it amounted to finally was--it came out much to the surprise of Dr. Rhoads and others--that Sloan Kettering Institute is now known as the Sloan Kettering Division of the Cornell Medical College--just the opposite from what they started out to do.

It worked to the advantage of Cornell. You had to take some trips which are of interest in terms of the variety of experience to which you feel heir--the Board of Trustees at Ithaca. You had to go there, or felt that you had to go there.

Yes, I had a good association, a frank association with Mr. Arthur Dean, who was chairman of the Executive Committee of the Board of Trustees of Cornell University. He was also on the Joint Administrative Board. I think I was able to tell him that we needed to give more information to the Board of Trustees than they were receiving from the Chancellor. He himself, Mr. Dean, thought that probably I, who was more familiar with this than he was, should be a good one to try to tell them about it, and he invited me to a meeting of the Executive Committee of the Board of Trustees at Ithaca. I went with him. I went up to that meeting. It was a rather brief affair, but I told them about these things. The outcome was that they took a position opposite to what Mr. Day had recommended. It disturbed him, and he--as I remember--got up and left the meeting saying words to this effect; that he'd been chief administrator of Cornell University for the past fifteen years and that this was the first time that the Trustees had taken a position contrary to some recommendation that he'd made. He left the room. I left the room with him. We went and sat down and had a good talk. Mr. Dean was a valuable person. He almost became president of Cornell, but I don't know whether I ought to put that down here.

He was the one responsible for ultimately putting into draft form the nature of hte agreement.

I should think he would--he wasin the great firm of Sullivan and Cromwell, which is the international law firm of which John Foster Dulles was a member at one time, I think. Mr Dean had a great head. He also, after this, went through a terrible experience as the United Nation's Representative in the talks at Panmunjom with Kim, the North Korean Communist. It lasted for two or three years, didn't it?

Yes.

It was a wonder that it didn't kill him.

In a way this illustrates the difficulties in managing a plant as complex as the Cornell Medical Center was becoming, let alone existing, where parts of the machine still continued to function within their own limits as they understood themand drew them, and meaning no disrespect to them, they pursued their interests, but didn't see a much larger view which, in this instance, modified their position to their own advantage.

As I recall it, there were two phases of it, as far as I'm concerned. One was the principles and policies of medical education that you cannot pass on to some non-medical institution. The second is that I think I had a feeling that academic freedom as a way of life, not just as a manner of speaking, was in danger of being sold out, and that oughtn't to happen. That you can decide very easily.

The large matters, as they are so often, are simpler to understand and follow than the ramifications of the personal relationships and the wishes of individuals. Take the distance from Ithaca to New York and the problems which confronted their own Trustees with respect to this plan--where they didn't have the detail.

It never got further in the Trustees than the Executive Committee of the Trustees, and three of them, of course, were on the Joint Board--President Day, Mr. Becker, and Mr. Dean. The distance between Cornell and New York is about two hundred and fifty miles which has been a severe--it's too long, and causes a severe isolation of its own medical college. It's too long an umbilical cord. Communication was always hard. I suppose Dr. Hinsey had to go up there every month perhaps, maybe. I went just a few times.

It was good in the sense that they had someone present who was going to assert a view representing the joint interests of everyone;otherwise....

That was the function. It was not a personal matter with me. That was the function of the office.

Yes, but old habits--confronting a void were hard to drop, but you created a forum in which views were exchanged with respect to this proposal, the manifold problems in any kind of merger like this, or affiliation like this, which were unthought of initially, and the more talk you have, the more opportunity you give people to voice either objections, or variations on a theme until somehow the affiliation emerges. I think myself this kind of problem asserts the reason why Mr. Jackson and Mr. Day in the first place thought it important to find someone to fill that spot, even though their actions might have belied

<u>the fact that they wanted someone like that. For example, this seems to have been a problem in which New York Hospital had no real interest, although how the new affiliation would affect the existing agreement was never thought about. This was one of the questions you raised--if you're going to change the nature of the machine, how does it affect the existing relationships between New York Hospital and Cornell, and nobody had thought about those problems.</u>

I'm sure they thought about it, but they weren't pertinent--I mean the records don't show much talk about saying this is something of no immediate concern to the New York Hospital any more than the New York Hospital power plant was of concern to Memorial. There are plenty of things in the combined joint operation that were not joint--there were elements in there, but they didn't have to be dealt with in a combined fashion. New York Hospital, I'm sure, probably regarded Memorial Hospital as a part of this affair that would concern it most and its stake in the joint clinical departments. For the rest they were remote from it, and the rest was rather basic biological research on the chemistry, biochemistry, cellular nature of cancer which the hospital participated in only through clinical connections. Sloan Kettering was a basic research laboratory. I don't think this even went to the Board of Governors of the New York Hospital. I suppose I talked to them about this at some time, but I don't remember. I used to report to them, and of course there are three members of the Board of Governors on this Joint Board. Mr. Jackson, Mr. Whitney, and Mr. Henry S. Sturgis sitting there, or Mr. Hamilton Hadley, as Governors would see some implication in this for the hospital and shrug it off perhaps. In addition, as I told you, Dr. Pratt, the Director of the Hospital, was sitting in this meeting.

Philosophically it's best to have people voice opinions than to have them foreclosed.

We had another great institution in the same region, and that's the Rockefeller Institute for Medical Research. It's right across the street and we constantly noted the efforts to have informal association with them. We actually did have people come over from the Hospital of the Rockefeller Institute and do work on some patients that had conditions which were of interest to them. That never developed into an affiliation, but the Rockefeller Institute for Medical Research yearned to have an educational opportunity and an educational course, and later, now, since Dr. Detlev W. Bronk has gone there, it is an incorporated university. It's no longer the Rockefeller Institute. It's the Rockefeller University, and it grants degrees. It was an easy give and take between the Rockefeller Institute and New York Hospital in an informal way--they could go back and forth with ease, but there was nothing involved like this proposal that came in of Memorial Hospital and Sloan Kettering wanting to be a division of Cornell University on the same level with the medical school.

Once the agreement is signed, and the agreement is signed--this newspaper clipping photograph of it....

Yes, there is an official agreement signed by Mr. Coombe, Dean Hinsey. No representative of the New York Hospital there. I'm there, I think--looking on.

Yes--I wonder how--this is hard for you to assess since you were sitting there at the desk, but with something like this, what effect does a successful pursuit of this have on the attitude of others in New York Hospital and the Cornell Medical School toward the Joint Administrative Board?

Just a minute--to go away from that. Mr. Hamilton Hadley is a Governor, and he's in that picture. He was later the President of the Hospital, so he's sitting in signing this agreement too. I have no copy of that agreement. There's none in the files.

No. There's none in the files. There is a series of amendments to the original agreement as signed, and they are incorporated in the Joint Administrative Board Minutes. The agreement allowed for provision to work out greater detail after the original agreement was signed. I just wondered how the office, the Joint Administrative Board--whether it gained. It had been pretty much of a void for some time, but through an experience like this, it must have gained.

Are you talking about the year?

I know human problems take time, and this is 1950.

Yes, this is 1950, but the Board had gained a plenty between 1947 and 1950. There were lots of things going on like this that hadn't gone on before--I mean things in which the Board was interested--educational things, budgets, appointments.

The Board did assert itself in terms of these Minutes, but having even made made that position, for a member--Chancellor Day to try to bypass it. Well, it's an interesting story.

I think that if the Joint Board hadn't been in good operation at that time, they would have succeeded in having this Sloan Kettering come in as an equal of the medical school. It's quite possible that instead of it turning out that the Sloan Kettering is the Sloan Kettering Division of Cornell University Medical School, it would have been the Cornell University Medical School Division of Sloan Kettering.

Yes.

A number of people won't agree with that. It's probably too pessimistic.

This is related in a way, but there's the problem of beds and bed space, and this, in turn, is related to the scarcity of nurses. I really don't want to get in to the nurses problem, because that becomes a continuing one, an examination of the whole field of nursing, but there were areas in the hospital that couldn't be opened because of the insufficiency of the staff--somehow. There are hints and suggestions in these Minutes that the problem of the supply of nurses prevents the opening up of new areas, beds. How to use a bed makes a big change in this period, 1947 on--the whole nature of ward and ward patients begins to shift to semi-private. This is a skillfully managed thing from the point of view of a teaching institution. I don't know that you'd met it before in just these terms. This is worth a word as to how you float, or how the idea is floated to use semi-private bed space and patients in a teaching institution. I don't know that it was done anywhere before.

It was done informally before. Many, many bedside teaching sessions have been held on private patients with the consent of the patient. Dr. Osler used to do it. Dr. Barker used to do it, and any good teaching physician probably could get the consent of his patient to use the patient for a teaching session. The patients are interested. They're awfully interested in hearing about themselves for one thing, and the second thing they sense what the rest of us know; that better medicine is practiced in the presence of students than in their absence--not entirely always so, but usually so. Take it as a general thing.

It became apparent in New York Hospital that something had to be done to improve the income of the hospital. That was probably the basic motivating force to change ward beds into private, or semi-private beds, but the other great influence was the hospital insurance plans--Blue Cross. Blue Cross would make it possible for a patient who without Blue Cross would have to come in as a ward patient, to have his hospital expenses paid, if he comes in with the Blue Cross support, so the hospital had to move to make the accomodations available that justified putting in a charge allowable by Blue Cross. Blue Shield, the first of the doctor's fees--we'll leave that out. Blue Cross, the vast hospitalization insurance plans all over the country, pay a certain amount per day for a patient in a hospital, provided the accomodations are of such and such quality. Ward patients don't qualify for that, although the ward patient costs sometimes even more than one who is on a private service.

At this time, really, the hospital costs were beginning to mount very, very much. They were getting up to the amount, as I recall it, of twenty-two, twenty-three dollars a day. Now it's very much higher than that, so naturally the managers of this hospital, all hospitals, were thinking of what they could do to put themselves in a position to get the income that would be available from Blue Cross, if accomodations were satisfactory, so we changed hundreds of beds from ward beds into semi-private beds in the hospital. The semi-private accomodation was usually a walled room, but sometimes you could get semi-private type accomodations by having sliding heavy screens. When a patient then is in a semi-private accomodation, the patient has certain rights that wouldn't have been regarded if he'd been on the ward, and one of those rights is whether or not he'll be a subject for teaching and demonstration. That immediately affects the educational program of the institution.

You can readily see how changing a ward bed into a semi-private bed is a matter of great concern to the Joint Administrative Board. It is of immediate concern to the Cornell University Medical School whose students need the opportunity to see these patients. It is of immediate concern to the Cornell University Medical School because the members of the faculty who have clinical appointments in the hospital are apt to be affected very much by the type of patient on whom they are carrying out their professional duties. It is of great importance to the clinical members of the hospital staff as well as of the medical school to have what you call adequate and varied "teaching material", if I can use such a phrase when I apply it to a living patient for whom I have a tender regard, but "teaching material" is a stock phrase without any connotations of hardheartedness in it.

We went through this at great length and difficulty at the New York Hospital. I was on the side of getting as many semi-private beds added as possible and altering the situation so that the beds could contain patients willing to be used in the teaching program. One of those teaching programs very seriously affected by this were the surgical patients. How are you going to train an intern, or a resident, and by the way the training of the interns and residents in this Center, as in all good centers, is quite equal to the burden and interest in the training of students. We train third and fourth year students, perhaps about fifty in a class, on out-patients, in-patients, patients, but you have maybe two hundred interns and residents in this whole complex that are also in training. People tend to forget that. These poor interns and residents when I got there received a mere pittance in a salary, and they were mostly getting married and having more and more children, and that was another problem involving the educational joint interests of the place, but to go back to this semi-private bed business.

You can see how it would fall to the Joint Board to have some major say in whatever arrangements were made, and I was on the side of more and more semi-private beds, if possible, seeing the advantages of this to the hospital. Naturally, the hospital representatives would be on this side too. The representatives in the joint center from the medical school would have an interest and probably tend to be on the side of not decreasing the ward beds because they felt that would decrease the opportunity for teaching medical students. This went on for months with very strong people like Dr. Guion at one time against doing it and some of the surgeons, particularly the surgeons. This is a problem that comes up with the surgeon. The surgeon is going to, say, train an intern, or a resident, chiefly residents, on operations of either minor or great difficulty, and he has to be really responsible for the outcome of the operation, but he allows the resident, or assistant resident, to perform the operation supposedly under his eye. Sometimes they don't stay there, but they usually do. Well, that's more easily arranged with a ward patient who rather expects to be handled by some of the lesser people on the staff under supervision; whereas a person who is paying for it is going to see that he gets a proper serving.

Well, you can see what a complicated thing that would be. That ran further, and this involved the joint situation also, because the Blue Shield Insurance paid the fee, the surgeon's fee, we'll say, or even the medical fee. What are you going to do about that fee? Collect it? Who collects it? The man who does the work is an unlicensed physician probably by this time, or at least unregistered in the place. The Blue Shield refused to pay unless there was a surgeon's name on it. They wouldn't pay unregistered surgeons. For a while they refused to pay a bill submitted by the hospital because that would appear as a hospital practicing medicine. The fees that came in from private patients like this went to a fund called the "full time fees fund" which was under the administration of the Joint Administrative Board which was used not for the benefit specifically of the department from which the fees came because they would all have gone mostly to surgery, but to the whole institution. That full time fees fund was used by the Joint Administrative Board for supplementation of salaries, for the payment of premiums on malpractice insurance for the interns and residents because there were some very severe and successful suits against the interns and residents in various hospitals.

Well, this is an example of a ramifying problem that starts with a good idea of making a ward bed into a semi-private bed that will yield ten dollars more a day, or something like that, and you've got the whole institutional arrangements involved in that simple move. It also required structural alterations in the hospital both as to the walls and the number of people who could be on a floor, the character of the bathrooms, the nursing, the food service, and the recreational areas on porches. None of it, in my opinion, is separated at all from anything else. It all weaves into the educational philosophy of the place, the spirit of the people, and what it says in the original Joint Agreement; to make the best possible teaching opportunities in the midst of the best possible medical service.

Was it ever accepted that semi-private patients per se would be subjects for examination?

Yes, we had great success with that. Isn't there a report on that?

Most of the reports that are referred to are absent, just the pages where they appear in the original copy, but the reports themselves, except for a finance report, or a Center Survey Report are not here.

Well, we studied it at great length from a logistic point of view, a financial point of view, a psychological point of view. We had many beds set apart for this. They were set apart as teaching beds. That involved getting the consent of the faculty member and the hospital clinical member to consent to the arrangements. Some men didn't want to do it. For those that did, it worked out very well.

Another influence that involved the policy of the hospital, the policy of the joint institution--the policy on the use of beds came out with a great outbreak of poliomyelitis that took place in New York--I forget in 1950, 1949-1950. New York Hospital was reluctant to take severely ill poliomyelitis patients because it was perfectly obvious that if they don't die, they are going to be sick a long time. It requires expensive respirators, expensive nursing, and it requires the filling of a bed which might be occupied for months. That problem came before the Joint Board as to whether facilities or beds should be put to the use of the care of polio patients because they knew that they would be tied up for a long time. As a matter of fact, the Joint

Board and the hospital were very generous--probably inspired by the excellent work that Philip Stimson was doing on this subject--to take in polio patients, and we had practically a ward set apart for them. I think that lasted for nearly two years.

<u>That was in response to a problem that appeared in the community.</u>

Yes. Of course, students haven't seen much of poliomyelitis, and that was of some value in teaching.

<u>We've gone just about an hour, and I think I'll turn you loose.</u>

O.K. I'll go see Watson Davis. Didn't I mention Watson in one of these talks?

Friday, July 1, 1966 A-60, N. L. M.

We've talked a while--bascially about the overriding, continuing problem in this Cornell Medical School-New York Hospital complex--the question of finances, the increasing deficit in the hospital, existing arrangements that had to be revised as in Urology, not streamlining, but to eliminate certain duplication and save some funds which were in a sense, a drop in the bucket when compared to the defucit. This is a continuing question and clashes in the atmosphere from day one. Out of this there are some problems that I'd like you to follow up today--the revision fo bed space from ward space to semi-private which did, in fact, increase income for the New York Hospital. Another side of this finance pucture is the continuing struggle over the sharing of the costs of the psychiatric department which has its own history. The hospital had paid for its full budget, although by this time the budget had been made a joint affair. It came under the joint budget, and out of that discussion a much wider process emerges, the Center Survey which, I suspect, was an attempt to educate all participants as to what, in fact, they were dealing with when it came to the question of what the hospital does, how it functions, how the parts fit. This is spread in the Minutes of January and February of 1953, but the problem runs pretty much from before the time you arrive and all during the time when you were there. This is a real ACUTE problem from the point of view of management, administration, human relations--the works, because the whole human story is in every one of these issues. I don't know what you'd care to say about them, but it struck me as a kind of later handling on a different level of the kind and quality of problems that you had on a much smaller basis when you were at Yale. Here you're now in a different position--

you're dealing with Deans, Presidents of Universities, and Presidents ofthe Society and getting them to chew a common diet with reference to knowledge about their own institution which is the important thing.

I think you've seen what is the central poitn at issue, and that is the financial situation existing between Cornell University and the Society for the New York Hospital. The problem of financing the joint combined institutions was there when they first combined. They didn't have enough money then to do what they wanted to do. They had to raise a lot of money to build these buildings and both for the medical school and the hospital. They got millions of dollars by gifts at that time from Mr. Harry Payne Whitney and from the Rockefeller Foundation some of which went into the Society of New York Hospital and some of which went into the Cornell University Medical Colelge, particularly the Rockefeller gift of something like a million dollars which at that time had a string tied to it; that it must be used to support the fill time system.

They argued about expenses from the beginning--I should say in the late 1920s when they first began to get together. These questions became more acute and difficult to settle when they completed the structures on 68th Street to 70th Street and moved the hospital and the medical school up to that location. At the same time they were doing that, the expenses were increasing very much, and the income was not. The deficits were customary over all the years of this combined institution. They had made previous attempts to find out what they might do to reduce those deficits, and the characteristic behavior of Trustees and Governors to appoint someone to make a survey was exercised even before I came to the place, and that was Basil C. MacLean. Bail MacLean had been the Director of the Strong Memorial Hospital in Rochester, and he was supposed to have been very sharp in those matters of administration. I remember reading Dr. MacLean's report, but it didn't seem to me to have anything in it that was very good for the place. It dealt with details, and I don't believe that he understood either the spirit, or the scope of the school and the hospital.

I was brought there by Mr. Jackson largely under the banner of being the deficit eradicator which, I quite honestly believe, would be a proper calling for an administrator. I had had experience reducing deficits at Yale and knew what it meant when you started to cut the salaries, maintenance expenses, and service expenses in a great teaching institution which was coupled with the care of sick people. I felt at Yale to do too much of that would destroy the place, and I felt at the New York Hospital-Cornell Medical Center that to cut as much as even ten percent, as I told some of the people, would cause the institutions to fall in the gutter and be objects of disgust rather than of charity, that nobody would pick them up, that they would lose their staff, that they would lose their morale, and that they would lose their opportunity to make the contribitions that were responsible for their being a great hospital and a great medical school. I had these two thoughts in mind; that it would be good if the deficit could be cut, and that it would be disastrous if the cutting harmed the institutions.

I don't think that's ambitendency, or something that would make you entirely ineffectual, unless you think a very ruthless, forceful, compelling administrator-type of behavior was proper. I'm sure Mr. Jackson and the Governors knew from my past that that wasn't the kind of person I was. I didn't have any capabilities of doing a thing like that. The story of the finances and the deficits are all laid out in this report here which I think would be good to list as you do some of these documents in the part where we're talking about them. /Report and Recommendations Relative to Reduction of the Deficit of the New York Hospital, October 30, 1951/

There's no use rehearsing the whole detailed history that's in this report of the Center Survey Committee of May 8, 1953, a committee composed of myself, Dean Hinsey, and Dr. Harry Pratt, the Director of the hospital, to go over the whole center's history, arrangements and mutual affairs. It's all in there, so there's not much use, in my opinion, to try to summarize the details.

The deficit shows up as being very large and of a size that can be manipulated. When Mr. Langbourne M. Williams Jr. wanted to throw in all the depreciation of buildings as well as equipment, you could make a perfectly huge deficit that exceeds any prospect of being wiped out. I never believed in putting in those depreciation figures. Yale University never depreciated. The buildings were given to them in the first place, and they would be renewed by gifts. They didn't depreciate equipment because they considered the wearing out of the equipment as a part of operating expense, but Mr. Williams used to call it the "vacuum cleaner point of view." Vacuum cleaners depreciated pretty quickly in his mind. I thought of them as just wearing out from use; in fact, I never understood depreciation in the sense that big word would imply. I think that it would be some wise setting aside of reserves for replacement of things that wear out, what one does ordinarily, that that would have been a better way to look at it than to throw a big figure of depreciation in a budget, when actually it didn't depreciate. There was no money set aside. They would depreciate one percent a year, we'll say. They didn't set aside one percent a year to meet that cost of wearing out.

They just put it in the budget as something they ought to have done and couldn't do. I never was buffaloed by depreciation in the sense that some people want to use it, but still it's a talking point, and it can make a set of figures look terrorizing.

These were difficult matters to handle, and, as I often have thought, it seems rather remarkable that I was allowed to stay there during these periods. When I left after all this talk and work, the deficit was very much larger than when I went there. It hardly sounds like successful administration. On the other hand, from what I know from what people have written me and evidences I have from relationships, the administration was successful to the degree that it brought the place a unity that it didn't have before. The medical school and university Trustees were given by the Joint Board more information about the whole center than they had ever had before, and I can say that same thing about the Governors, and when I speak of the Joint Board, I am depersonalizing myself because I was the mouthpiece, the instrument of bringing these things in verbal form to the Governors and to the Trustees and supplying them also with a great deal of written information.

The two questions that you refer to; namely, the reduction of the ward beds by a hundred and the attempt to bring out a sharing of expense of the Payne Whitney Clinic and the Department of Psychiatry between the medical school and the hospital involved a great deal of conflict from which, to tell you the truth, I didn't suffer in the usual sense of being battered by a battle. I did suffer because of the implications of the actions contemplated. I couldn't bear to think of that place going down in any way. It had a wonderful tradition and history, and it had a great capacity and productivety at the time both in the medical school and in the hospital. The hospital was undoubtedly the more renowned of the two partners in the joint operation, so much so that I can recall some of the Governors asking me whether the Cornell Medical School was any good. I never heard the Trustees, or anybody in the medical school ask whether the New York Hospital was any good. It was notably good. The Cornell Medical School was founded in 1898, and it had to struggle all the time and was relatively poor, but it had a distinguished faculty, and it was a good school. I didn't think until the end of my time up there that the Governors really knew much about the medical school and its accomplishment.

The same might be said of the Trustees at Ithaca, although they did show more concern with the Society of the New York Hospital periodically. One of the stated meetings every year of the Trustees at Cornell was held in the Board of Governors' room in New York Hospital in New York City. I think that that was superficially a gesture to indicate a relationship between the two institutions, but I think it was also a measure of convenience because these Trustees had a great deal of business to conduct in New York, and they would have had to come down to New York City at least once a year to handle the affairs of the university properly. Anyhow, there were Governors present for parts of those meetings--not the serious business part, but the social part, and it was a good thing to do.

As it went on toward 1951 to 1953, the Governors appointed me to handle the--not the Governors. Excuse me. The Joint Administrative Board appointed me to handle the task which we have mentioned; namely, the reduction of the ward beds in order to decrease the deficit by bringing in more income for the hospital from the semi-private service that would be created by a change in the status of beds. The process, as you call it, by which that appointment comes about is this. The Governors notice that there is a great deficit, and they talk it over with officers of administration who clearly point out that the major factor in the deficit of the New York Hospital is the free care and the underpaid services that they render to the sick poor of the city. The Governors are horrified to think that they are going to have to reduce the gratuitous services that their institution renders, but they msut know that it must be looked into, so the Governors asked the Joint Administrative Board to direct the President of that Board--President so-called--to look into the matter and come in with some recommendations as to how the income of the hospital can be increased by some three hundred thousand dollars a year.

The President of the Board, myself, looks in to a good many things, and he knows from his experience and from what he knows about other hospitals, which he sharpens up by visits during the summer, or some time during the year, that there are two ways that you can reduce expenses of free, or under-recompensed costs of the sick poor. You can close parts of the hospital, or change the status of beds from non-paying beds to paying beds. At Yale I went through that when I was required, as Dean, to cut fifty thousand dollars out of the first budget that I had to deal with. One thing I did with the approval of the Board of Permanent Officers up there and after much consultation was to close a whole ward in the New Haven Hospital. I think it remained closed for a better part of the year. Whether that did anything to reduce the deficit of the New Haven Hospital I don't know. Costs are mounting all the time in other ways, and when you close the ward, income falls off. It just seemed like a vicious cycle--if you're losing money on an operation and you cut off what money you have from that operation by discontinuing it, you're just going to lose more money. Doesn't that seem so?

Yes, it does.

Well, we decided at New York that closing wasn't the thing to do. The next rational thing for which you could find a paradigm and other examples was to change the bed from a non-paying bed to a paying bed by changing ward beds to semi-private beds. The change--aside from the involvement of structural matters such as plumbing, bathrooms, partitions, things like that--involved the educational program for undergraduate students and particularly the training programs for residents and assistant residents, and particularly in surgery because the people on a pay status would not submit so readily to being, as we say, worked up and then operated upon by anybody, by the residents, if they could help it, or assistant residents. They wanted to be operated upon by the chief men in the specialties, or the services. That was studied for a long time, and the details are in a printed report which I wish you would list when you get to that--this one of October 30, 1951, on the Reduction of the Deficit in New York Hospital.

I presented the decision I had reached to the Medical Board first because the proposal affected the clinical departments most. The proposal was to reduce by a hundred beds--to take about twenty out of medicine, about sixty some out of surgery, and perhaps twenty more out of obstetrics and gynecology. The proposal didn't touch pediatrics--poor little pediatrics had things listed as beds that were bassinets, and they had relatively few beds. The Medical Board gave prolonged consideration to this proposal. Members of the Board and I discussed it separately, and then in a long, long session, it was presented to the Board, discussed by the Board, and most of the full Minutes of that discussion are in this pamphlet here.

<u>To what extent is this process involving the change in designation of beds typical of the time?</u> <u>In my judgment, it's the hospital responding to currents</u>

outside the hospital.

You could see that because at the same time we were considering means by which the hospital would be paid more by the City of New York, for one source. The City of New York, we'll say, set a per diem on the care of indigent patients--we'll say it was twelve dollars a day when it was costing thirty dollars. We were all the time trying to up that amount from the City, and in addition, Blue Cross was paying a certain amount of medical insurance for a patient who had a policy with Blue Cross to meet the cost of this bed care, or his stay in the hospital. We had incessant arguments with the directors, managers, and financial people in Blue Cross to get them to see what it was costing the hospital and to get them to raise their pay to the hospital for their subscribers. These figures were so large that Blue Cross didn't dare raise the premiums on their subscriber contracts to give them enough money to give to the hospital what it said it needed, so it wouldn't be solved. There wasn't enough money, but it was increased by fifty cents a day, or a dollar a day. Dr. Pratt worked at that all the time, and I used to work at it. Those were indications of movements in the population, or in the civil community outside the hospital, in relation to the hospital to meet the costs of medical care and the more nearly they could meet that cost, the less the deficit would be, so that there was a constant effort to do that.

That was aside from the change of the status of beds, except by changing the beds from ward beds to semi-private beds, you had more beds available for subscribers to the plans.

I'd only like to make the comment about this report; that it was fairly prophetic as of 1951, in this comment here--the indication that the trend is going to be toward voluntary, or governmental plans.

Yes, it says in there, which I wrote, that it looks as if in the future there wouldn't be any ward patients and of course, now--this is July 1, 1966, and medicare begins today, an enormous government outlay on medical care which is interesting to me in another way because the Kaiser Plan in which Mr. Jackson was interested at the beginning and which has been so successful, was promoted by Mr. Kaiser as something very typical of the American Way of Life, that they would rather support themselves than to have the government support them. That was said just the other day, expressed by Mr.--well, I read it in the paper....

Someone commenting on medicare?

Yes, this public figure said that he would rather pay his doctor than to have his doctor paid by the government because he felt that the doctor then would be working for him, and he had a sense of controlling the doctor in a way, or at least being nearer to him. That also was a sentiment by such fine doctors as Dr. Francis W. Peabody and others in the earlier days. They had said that they would rather have a man pay them two bits because the exchange of money between the doctor and the patient rather united them in a more respectable relationship than if a man didn't pay anything for his care.

With all due respect to philosophical things, that can become a fixed idea--you know, the need for that relationship, or the need for ward beds in educational matters for the school can become such a fixed idea that the management of the hospital can't see its way to opening up a new process to educate all parts of the complex to something wholly new which might also help the deficit which is what I think that report did.

This report was discussed very thoroughly in my presence and among the members of the Medical Board first with strong statements of opposition, and very plain statements that the chiefs of clinical departments feared that it would reduce the opportunity for teaching, training, and research. It was presented by me to the medical faculty and explained, and they had the same sentiments. Though they weren't so deeply concerned in the clinical research, yet they had much concern with the medical education of third and fourth year students, and even earlier the students are trained in the hospital too, and it's not so commonly known that these so-called pre-clinical departments are constantly making contributions that are of great value to the clinician. For instance, in biochemistry at the New York Hospital-Cornell Medical Center Vincent du Vigneaud, a great professor there who won a Nobel Prize, discovered the hormone that has to do with the contraction of the uterus and was of very great importance in the obstetrical practice of the Lying-In Hospital.

They all expressed the same anxieties and the same opposition, but curiously enough the Medical Board ended its discussion with the recommendation that if no other way could be found to reduce the deficits than to change these hundred beds from ward beds to semi-private, the Medical Board as heads of departments would do all they could to cooperate with the Governors to see that it went along well. The medical school faculty adopted the same resolution in the same words. I look back on it as a very remarkable sort of ending. I was present when those votes were taken, and I don't recall anybody looking at me in an ugly fashion, and we continued to go along and working for the whole institution as ever. The beds were changed to semi-private from ward beds, and the next report shows that two hundred and fifty thousand dollars had come in, so it was all right.

Was it possible for the hospital and the medical school to gain access to semi-

<u>private beds for teaching purposes? This was another educational gambit in the sense that a patient will have more available to him if he has more people around who are considering his case.</u>

Well, the way that was managed was to solicit the interests of certain surgeons, we'll say, who would be willing to try to work under that scheme. There were some surgeons there who wouldn't have anything to do with it, so we didn't try to have their patients put in to these situations. It was an adjustment--like Dr. Preston Wade who was very much opposed to this, a marvelous, fine surgeon, but he said that he would work with it, and he did, as did Connie Guion naturally--she's so public spirited and understood this, and although opposed, worked with it. Many other doctors in the place wouldn't have anything to do with it, and there was no attempt made to force them at this time to cooperate and collaborate. There was a time when we had to offer a yes, or no choice to the doctors who had private practices and offices in the hospital, but that's a different matter.

<u>There was experience afforded which enabled them to revise their notion of what a ward patient did for education when they can get it on semi-private. Once the idea was floated, it put in being something wholly new.</u>

Yes, the patients were well treated. They were studied well, and a great many of them liked it. They felt actually sometimes that they had been skipped on the rounds, or some study, and they complained.

<u>You know--the introduction of something new, or something not entertained and it becomes new and novel, it takes a little time for it to become warm and familiar, and then it is never a problem.</u>

No, it was never a problem. That school, that combined joitn operation was willing to try new things. We probably won't talk about the comprehensive medical care program that was develope d by Dr. Barr and Dr. George Reader in the dispensary and other places--the Joint Board was behind that, and it came on rather late, but it involved a great many readjustments and changes on an educational basis.

One of the implications of that is that the deficits, in part, were chargeable to outpatient care, and the dispensary--you know, and some means had to be found to make the Outpatient Department more selective in terms of the cases taken for educational purposes than to simply have the continuation of the same peoplel.

We spoke of that last time.

Yes, but this was going on too--a new way of thinking about it, functioning with an Outpatient Department and dispensary. It parallels this other development. I like that report and the documents that are appended to it, the way all parties were granted a hearing to give vent to their philosophical point of view and once those are aired, they see the necessity of doing something about the deficit even if it meant swallowing their philosophical views, or learning something new, and this is great.

I think this report is far more than an administrative document. It's a history of thought, a history of feeling, and a history of relationships.

Right. This was quite successful, and the year's experience did show an increase in income of a quarter of a million dollars. While it didn't erase the deficit--no, it didn't. The other, in a sense, wrangle is chargeable to a relationship that sprang up initially between the Payne Whitney Psychiatric Clinic and the Medical School and the Hospital whereby the head of the clinic, Psychiatrist-in-Chief, and I guess the Professor of Psychiatry in the Medical School were the same person.

Oskar Diethelm.

A very good fellow, but the point is that the assumption was that there were sufficient funds available to sustain it and therefore it became a problem of some disinterest to the Dean of the medical school because this fellow had three hats that he wore. I've put it in a crude way--perhaps, but the budget didn't go through the medical school, the medical part of it, and the question that ultimately comes up is the sharing of funds again for this particular clinic. It too runs into deficits which appear on the Joint Board budget, and the problem is further stimulated by the committee, the Payne Whitney Psychiatric Committee, who also, I guess, were [illegible] hard-headed people, who didn't like to look at red ink and sought means whereby they could escape the stain which comes from it. In any event, it becomes a pretty severe problem, though I suspect it's net effect is to merge in the study of the whole center.

The endowments for the Payne Whitney Clinic came from the Payne Whitney family, Mr. Payne Whitney, in two allotments. One was 11/300ths of the estate, and the other was 8/300ths of the estate--I think they were something like that. One was plainly for the Payne Whitney Clinic, and the other one was a little ambiguously stated. The smaller one had a possibility of being interpreted as being a gift to the medical school for psychiatry in the medical school, but it was kept by the Payne Whitney Clinic, so it looked as if the funds for the Payne Whitney Clinic were all there from the gifts and that the Trustees would never be called upon to put any money up for them, and yet the Payne Whitney Clinic was a functioning, teaching part of the medical school right from the start. They began to introduce pretty soon psychiatry into the first year and into the second year too, and there were plenty of people from this group in psychiatry in the third and fourth year.

The administrative setup was complicated as you have indicated by the existence of the Payne Whitney Clinic Committee of which Mr. Edward W. Bourne was chairman. That interposed a strong legislative and authoritative body between the Joint Administrative Board, between the President of the Joint Administrative Board and the Payne Whitney Clinic and the Dean of the school and the Payne Whitney Clinic because the budget went first to the Payne Whitney Committee, and it was decided then. It did get over into the Joint Budget, but the medical school had hardly any look at it at all. No other department was so fenced off by a controlling board as was the Payne Whitney Clinc, except, of course, the Westchester Division which we won't consider here--it was psychiatric, and it did some teaching, but it ran on its own.

The discussion of the Payne Whitney Clinic finances from the point of view of the sharing of expenses between the university and the hospital, meaning the Payne Whitney Clinic, got very acrimonious, and I think I say in that report on this Center Survey that there was more harm being done to the joint enterprise by the arguments and name calling over the Payne Whitney Clinic situation than by anything else and that something ought to be done to clear it up. We had no difficulties about it, as I recall it, in the Joint Board, except one time when Mr. Bourne tried to get the President of the Board to get rid of Dr. Diethelm. I don't know whether that shows in the Minutes or not. He thought Dr. Diethelm ought to resign. In addition, he wouldn't raise Dr. Diethelm's salary, and Dr. Diethelm's salary was much lower than that of medicine and surgery. We couldn't get it raised for a while. In addition, the Payne Whitney Clinic Committee declined to approve a budget for one year, but the Governors of the Hospital thought that was rather drastic, and they disapproved it, although the committee hadn't recommended it. It was a pretty hard slugging match for a while. I think it more or less quieted down. When I brought it up for final settlement I was pretty nearly finishing my term, and I don't remember what happened.

I infer from the notes that I have read that it did reach a point where it got pretty hot and in the discussion, so far as these Minutes convey, a suggestion is made that the whole center be surveyed, so that they were able to deal with this situation as part of a much larger study because of the views expressed by certain members who looked upon the medical school as, in effect, free loading. It was easier for them to make that comment than to scratch the surface and see whether that, in fact, was so, so they were able to avoid dealing expressly with this psychiatry problem and deal with the whole center, as you did in this survey. /Report of the Center Survey Committee to the Joint Administrative Board, New York Hospital-Cornell Medical Center, May 8, 1953/

That led to the appointment of this committee on the Center Survey--Dean Hinsey, Dr. Pratt, and myself.

It's one thing in the quiet of one's own closet to express a view which may soothe one's anxiety by making the comment like "the medical school isn't carrying its own load, and that's the cause of all our problems." It's quite a different thing to set up a process whereby one can express this and express it openly and publicly and then have the direction toward, "Let's find out." It doesn't figure any more because once this report is completed, they can close their eyes if they want to, but if they want to talk about the center as a center, they've got to go to this report.

I think so.

In this sense, maybe, you were raising the level of discussion and argument by informing the various members who had the ultimate judgment to exercise.

I suppose that's the outcome. To tell you the truth, I don't remember being motivated that way. I think what I have here in this report is what any scholarly person, if I can call myself a scholarly person, would do. It has a bit of history. It tells current events. It tells where the problems are and it suggests solutions.

When I read this, I got the impression that what you had done for the center as a whole was kind of an autopsy.

No, the center was alive.

I'm sure.

You can't do an autopsy.

I use the phrase as an intellectual one--you dug in the papers. You talked to everyone you could. You held thirty odd meetings or so. You discussed it with two other parties.

We collected an enormous amount of reports. We had great help from

Mr. John H. Keig, the financial....

<u>The comptroller.</u>

Yes, the comptroller. I think that I was the one who had the most opportunity to go in and look at those things because both Hinsey and Pratt were inhibited by their official position. Hinsey had a curious appointment as an Alumni Trustee of Cornell which bothered Mr. Day. He [Mr. Day] said at one time that he forgot to tell the medical faculty that they mustn't do that. Hinsey was an Alumni Trustee of Cornell, or at least a faculty Trustee of Cornell. Pratt felt that he had so many stated obligations as Director of the Hospital with special interests that he couldn't be impartial about some of those things.

<u>Yes.</u>

Which is probably true.

<u>In the end here under recommendations both of them disqualify themselves from some parts of the recommendations.</u>

I think that we said in the beginning of this--attention is called to that in the beginning, and it does appear at the end too.

<u>But I think that what the process disclosed is that there isn't any solution to it.</u>

The outcome of this report was general acceptance of the report as an informative document, and I think that people like Mr. Hadley and Mr. Whitney on the hospital side, probably Mr. Neal D. Becker and Mr. Dean on the university side appreciated it. In the time remaining for my tenure there, there was nothing brought up for action on it. The opposition came from Mr. Bourne, or at least the criticism came from Mr. Bourne. He gave me a note saying that I was putting myself in an impossible position by this report, and he was disturbed by what I wrote about psychiatry in there.

I think the Governors were surprised by the first statement in the conclusions, and I remember writing that, that the medical school was paying its way. I really think so. They didn't argue with me about that. They probably laughed.

I don't think so because the traces were available for them to turn to another recommendation ultimately; that the problem isn't trying to decide who is responsible for what deficit where, or cutting up the total portion into little compartments--you know, and aiding one to the disadvantage, or at the expense of another.

That wouldn't reduce anything.

Right, but the important thing was that both the medical school and the hospital ought to indulge, for certain purposes, in a joint fund collecting scheme. The whole trend toward expenses is getting higher and higher all the time--the course of expenses is going up all the time, and the deficits will doubtless do the same, and the need was for funds. It would be easier, instead of wrangling as to how we got there, to go out and somehow establish some means whereby we can have a joint claim, or appeal for funds which will aid us. I think that does come out of this.

In the last two or three years, the Society of the New York Hospital has gone out fund raising, and they have raised about fifty million dollars. As far as I know, Cornell University didnot join these fund raising efforts, aside from some special efforts of Dean Hinsey, and the new dean didn't do much in that. They did some though. Dean Hinsey toward the end began to get quite substantial grants and gifts from the Olin Foundation. He knew the Olin manufacturers, and he got money for a new dormitory, money for the new library and for a new building in the place, and on his retirement now, five hundred thousand dollars has come in to make a Professorship of Anatomy in his name. Also in the last year a gentleman named Mr. Israel Rogarsin all of a sudden out of the blue gave five million dollars to the Society of the New York Hospital because, as he explained it, he met the new dean who took him through the place and pleased and impressed him so much that he gave five million dollars, but he gave it to the New York Hospital. Now I'm sure--I don't know what's going on, but I imagine in the generous way that these things are done up there that that will spread over to the benefit of the clinical departments, the so-called joint departments.

This process of being President of this Board in a way is establishing a process which they can all follow, an open ended process. There isn't an answer anywhere. There's a continual look, a peek at what exists and how best we can adjust to meet this, or that, and if everyone will sit down and talk up, out of the chaos perhaps of their own petty views, something broader in suppprt of the Center as an idea can emerge. That's what's in these notes, what's in these reports. I don't know how the President and the Joint Board functions now, but you must have put in some pretty deep piling for that very process, and that's good--like educating your own rulers.

I don't know how these things happened. I remember one period of rather hopelessness in the process of carrying this Center Survey out to some conclusion. It came to me when I was in Chicago. I wandered into the Chicago Art Museum down on the lake front, and I sat down in a room in front of a picture by Van Gogh. This is a gorgeous, golden yellow and pale green, light green picture of part of an apple orchard with yellow flowers and things in the field under it. It seems foolish to talk about it, but some solutions came out of the musing in front of that picture that I connected with some of these stages--I still remember it.

You know--it puts a tremendous premium on discipline.

Discipline of what?

Self-discipline to keep your eye on the bird on the wing; namely, what to do about the problem and not get involved in the baronial conflicts between people who are otherwise decent people.

I don't remember anything like that. I didn't have any operative function, except maybe in connection with the full time fees and operations carrying out some of the resolutions of the Joint Board, the change off of the doctors from their private practice, operations in connection with the Professor of Radiology whose resignation had to be obtained--that's different from the operation of running a hospital and running a department. Perhaps the saving thing in the situation was my early appreciation of the so-called title of "the President of the Joint Board," sitting at the head of the table between the President of the University and the President of the Board of Governors of New York Hospital both of whom had enormous power in contrast to the meager trappings of the so-called President of the joint affair. Perhaps this aroused a sense of humor that was salutary.

The analogy that I got when I read some of the early notes, having learned to know you real well--it was Daniel entering the lion's den--really.

Daniel knew he was going in the lion's den. He could see what they were doing. He saw the lions. I didn't see the lion's den.

No, but it didn't take you long to dig in along certain lines, and that was the approach you had--if it was no more than creating means whereby people can give vent to what it is they're feeling and thinking.

They used to do that. I had a big desk in the room that was assigned to me--as I say, a big portrait of Valentine Seaman behind me on the wall, an admirable surgeon in the early days of the New York Hospital, and on the left hand side of that table was a pretty comfortable chair. I think it was occupied most all the time by somebody from one of the departments. The doors were always open, and they could come in and sit down and talk. You ran an appointment book--you had to attend meetings. You had to see people at certain times, but nobody had to call up first before they could come in.

I think we've gone as far as we ought to go today. Next time I'd like to go beyond the hospital as a hospital to this Commission on the Financing of Hospital Care and then take....

I've got the other two. Is this on still?

Yes, and then take another look at....

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Are you finished with these books?

Yes, though I want to look at this one again.

That's a deep one, isn't it.

Yes. Today we want to take a look at the nursing profession and its problems as they were revealed to you as President of the Joint Administrative Board. We have not had a chance to speak of nursing. We talked about the laboratory, the need for a hospital in medical education, and we've sort of not avoided it, but nursing never came up before. As President of the Joint Administrative Board one of the segments of that total complex was the nursing school, and while you've given me some books to look at which tell something of its background, its history, the problems before the Joint Administrative Board were largely financial, somewhat administrative, an effort to define more clearly what the nursing service for the hospital was and what the school of nursing had become. This is also a period shortly after the war when there's this tremendous increase in the scope and complexity of medical care. There's a great demand put on the number of nurses that are available, and decreasing numbers of nurses are being educated, and this shortage takes you into surveying the field as you did in this study. This is a period in which you got deeply involved in nursing, talked not a little with some of the figures who were instrumental at this time in thinking about the field, and you even wrote an essay here on the need for research in nursing ["The Role of the Nurse in Medical Progress" 50 American Journal of Nursing 601-604 (1950).] This may open up a vein of experience that you did have which is still of vital interest to you. I was interested in the comment that you made before turning this machine on with respect to the whole concept of the development of a hierarchy in the profession. As of this time this was still a developing process. It had gone from apprenticeship toward a profession at New York Hospital. I don't know where it was on that highway. Where nursing fits in the total scheme, its relationship to research, and it has a history of people too for whom you've had a great regard--all of them bending an oar, I think, in the direction of more and better education for nurses, more clinical concern, the use of their powers of observation, dwvelopment and use of those powers--particularly Dr. Welch and Dr. Winslow, but not limited to them. Maybe what I have said will help to set the scene.

I would like to go back of even the things you've mentioned with regard to my connections with nursing. I vividly recall that when I was a medical student substituting on the wards at the Johns Hopkins and as an intern that I had the good fortune to have two head nurses who were remarkable women and who taught me a great deal in my own medical, educational experience. They taught as bedside teachers almost as the professors did--they were acute in their observations. They were able to express themselves, and they knew more than just the superficial symptoms that they were administering to.

That was followed by a really very early interest in Florence Nightingale. I'm glad to say that I was one of those who early appreciated the force and vigor and intelligence of this woman and despised the picture of her as the "lady with the lamp." I won't say "despised" it, but it was so sweet and soft when she really was a driver. She was a commanding officer. She, of course, was one of the early observers of the shortage of nurses because in the Crimean War, 1850, there was a great shortage of nurses with the British forces and with the French forces that were fighting the Russians at that time. Florence Nightingale introduced many, many things—new things in nursing. She had some peculiar ignorances and a hard headed objection to the germ theory of disease. She believed that one disease could turn into another one because, as she said, in the hospital she saw pneumonia turn into typhus fever. What she saw was cross infections in the hospital, but she took it as one disease changing onto another. She was very clear headed, and she introduced public health nursing, community nursing—wonderful for the time—care of the sick and wounded. She organized the British Indian Medical Service. She was a great advisor to the government of Great Britain showing what a trained nurse can do who is intelligent, able, and has the right connections. She did have the right connections. She was well born, and she was close to some of the higher officials in the British Government.

Fortunately after that, I had the good luck to know some remarkable nurses in the United States. Most of them came from what was later called the Cornell University-New York Hospital School of Nursing, but it was the New York Hospital School of Nursing originally. I knew a little about Lillian Wald who had some social ideas about the Henry Street Settlement, Miss Irene Sutliffe who was the head of the nurses when Lillian Wald was in training. The nurses who took care of the typhoid patients and the wounded from the Spanish American War in those great camps on Long Island were able people, mostly trained in New York Hospital as nurses, and they had to suffer with the usual down the nose glance of the people who were so-called employing them. They were thinkers. They were original people. Among them also in World War I was Julia Stimson, Secretary Stimson's niece. When I was at the New York Hospital-Cornell Medical Center we found a marble statue of Julia Stimson which was out on the grounds of the Philip Stimson family home—Philip Stimson being my classmate at Yale--and I persuaded him to give this to me. We put it in the hallway of the School of Nursing because it's good for those women to see heroic figures just as it's good for the medical student to see the four doctors on the painting by John Singer Sargent, or some great medical figures.

Another woman who came up through that school, or had connections with the New York Hospital School was Miss Annie W. Goodrich. Miss Annie W. Goodrich was the first Dean of the Yale University School of Nursing which was endowed by a million dollars from the Rockefeller Foundation and which became the first school of nursing in the world that required a B.A. degree for admission. Jumping ahead, I can say now that that school was ruthlessly discontinued by President Whitney Griswold about 1955, 1956--I'll check the date when I get to it. Miss Goodrich was succeeded by a very able, pleasant and intelligent woman.

<u>Taylor.</u>

Yes, Dean Effie Taylor who is still living. She's in her eighties, and she's the one with whom I had most to do in my early experience in affairs of nursing education because the budget of the School of Nursing passed through the Office of the Dean of the Medical School. I was the contact for the School of Nursing with the corporation of Yale University. Although I had no Dean's prerogatives in that school, it was a tradition in the place that their budget and many of their affairs would pass through under the scrutiny of the Dean of the School of Medicine.

My next preceptress in nursing was Miss Virginia Dunbar at the New York Hospital-Cornell Medical Center. Miss Virginia Dunbar, a rabid devotee of

Florence Nightingale and a very intelligent, able, scholarly nurse, was the gentle but strong Dean of that university school of nursing. At that time, it required a degree for entrance, and it had a very intricate and thoughtful curriculum for the training of nurses, far beyond just the practical stage of washing a patient's feet, making up the bed, cleaning the window sills, and emptying bed pans. They could do that part of it. They had ideals and capacity way beyond the requirements that so many people impose on nurses as servants and indentured students--"indentured slaves" I meant to say.

Those are background things, and I will emphasize once more that running all through this is a shortage of nurses all through the country and through most countries. There were increasing demands on them all the time. There were a great many women going in to nursing, but they didn't stay in nursing. When they had an opportunity to get married, they got married. There was considerable attrition every year because of the activities of fatal fascination of the young men with whom these young women are associated.

Curiously enough, nursing, in my opinion, has never been properly appreciated because it appears in the class of a handmaiden type of thing. It's a servant type of function in the opinion of many people who employ nurses, or benefit by care from nurses. They class them mostly as very valuable and useful servants which reminded me to some extent of my old Mammy. She was practically a member of the family, but she was a long way from being a member of the family.

The nurses in the New York Cornell School were a high grade, well selected, intelligent group of students. Their faculty members were highly educated and able women too, so much so that there came to be a division between these highly educated nurses and the ordinary registered trained nurse. That division led to inharmonious relations in some cases because competition was great, and nurses that were well educated tended to look down on the ones that were not so well educated. It was natural enough. Below the trained nurse in most of the places with which I was associated, particularly during the period in New York, there was a lower, or a group of so-called practical nurses. There were schools that trained, for a year at the most, girls to do the chores of nursing and to be comforters of patients and housekeepers, that type of thing, plus doing a great deal of the manual, hard, unpleasant work of domestic hygiene and domestic care of patients in their daily life almost without regard to whether they were sick or not. Now, the development of the practical nurse was a natural thing and should not have been opposed, I think, by the highly educated nurse. Nurses, like doctors, ought to be organized in a natural sort of hierarchy, one in which there are a group of elite people who are the top and who can make contributions, give guidance and have enormous influence on the people coming under them. This group at the top is supported by a large, or more numerous group, the less educated people, and in this case it's the ordinary, trained, registered nurse--the RN, and below the RNs are the practical nurses. You find the same thing in medicine--highly trained surgeons surrounded by less able operators, and they have many assistants who are in training and have as yet not come on to any independent operations.

The nurses in the New York Hospital, if we might talk mostly about them now, had contributed a great deal over the years to the intellectual product of the Center. The paper to which you referred that I wrote on the Nurse and Medical Progress was prompted by having seen publications, many of them, in which the nurse was acknowdedged in the footnote; whereas I knew that the nurse had really been responsible for the success of the investigation, or the success of the obswrvation, and to give my talk sort of a twist of a slogan, I was inviting these nurses to get out of the footnotes and get their names up on the author by-line which is what they deserved. That has gone on--that idea, that they could do that by doing some really oriignally designed, experimental research, to the extent that now research in nursing has producged this extraordinary volume /Faye G Abdellah & Eugene Levine, Better Patient Care Through Nursing Research (New York, 1965) 736./ which is in front of us now which is as good an account of modern experimental methods as I have seen even in medical schools, or in some departments of physics. The idea spread.

The other contact I had with building up opportunities for nursing education occurred when I came down in 1953, about, to be connected with the Surgeon General's Office in the Department of the Army. At Walter Reed Army Institute at that time, there was a very able young captain in the Army Nurse Corps named Harriet Werley who was inspired to do something to combine research in nursing conducted by nurses with arrangements for higher education of nurses at this center and elsewhere. Through the years of work by Miss Werley who was a Major by the time she retired, the result has been the development of a new school of nursing at the Walter Reed Army Institute of Research, a school of which the present Surgeon General is very proud and to which he is giving support and supplying the facilities of an unusual nature.

In New York I was fortunate to know my friend Eli Ginzberg who is a sociologist and economist of great depth and perception. He made his reputation, as far as we're concerned, in the Army at the end of World War II when he almost single-handedly demobilized the Medical Department of the Army. He brought it down from thousands of physicians to a well balanced demobilization that everybody admired very much. Thanks to that association which was always pleasant and mutually understanding, Mr. Ginzberg invited me to join an organization called the Committee on the Function of Nursing. There were some interesting people on that committee with very wide ranging interests all the way from public health and hygiene to economics and education. Studies were really centered in the Office of Mrs. Louise McManus at Teachers College, Columbia University. We had many, many meetings and many discussions, and we proposed and produced a little book called A Program for the Nursing Profession which was rather disliked by some of the nurses in New York because it was thought, I believe, to downgrade them somewhat. In my opinion, there's some good sense in this little book. It has in it this hierarchical idea that I was speaking about, and it brings out the shortage of nursing which was a national problem. It brings out also the other prolonged persistent problem, and that is the economic condition of these women.

Sometimes if nurses netted a thousand dollars a year, they were doing well. It was rare to find a nurse that got as much as three thousand dollars a year. They lived on a pittance supported often by their families, and often they supported their families--not their children, but their ailing and aging parents. How they did it is difficult to see. There wasn't enough money going to those women to give them a ride from New York to Albany once every ten years, or something like that. This condition has improved somewhat--nurses are paid better now, and, as a matter of fact, nurses began to do something that is rather untraditional in nursing. They began to organize as a labor group. You have read recently of the nurses that dropped out of the Bellevue Service because they thought they weren't getting paid enough. As a matter of fact, the doctors are trying to imitate them lately. When I was In New York there was a John L. Lewis Unit #40 which picked up employees of all kinds, and they tried to get nurses into that section of the AF of L Union. The nurses did threaten to go on strike sometimes, but I think they realized that it was abhorrent to medical ideals that they should do that sort of thing. On the other hand, the sympathies must be all with them because they were downtrodden and had very little subsistence.

In the New York Hospital the nurses were paid, and they got their meals—or at least they got two meals a day while they were there, but they had to come in at all kinds of hours and work different times. Their condition was appalling to me. For instance, they had to come in the mornings, go down to a basement that was fitted up with shabby old lockers and benches, and change their clothes from their street clothes to their nursing uniforms. They had no good places to wash, no mirrors, sort of a dungeon-like dressing room. Fortunately there was some money left over from the Vincent Astor Fund, and Mr. Astor approved a recommendation that that money be used to fix up quite a section of the basement for the nurses. We made it almost a beauty parlor, but they--even at that time they were suppressed, in the sense that they were not treated like respected human beings. We had many, many interesting talks in the faculty of the New York Hospital-Cornell University School of Nursing. I was invited to their faculty meetings, and after a while we had such a natural relationship there that I was taken into the discussion--I was going to say I was almost regarded as a nurse--which was very nice.

<u>As a practical matter at the New York Hospital because of this shortage of nurses some two hundred and twenty-two beds were not opened.</u>

Yes, but that didn't last too long, did it?

No, but the mere fact that you had that many beds that couldn't be handled....

Yes, I think the complement of employed nurses in the New York Hospital was about 750 on the payroll, and that's not counting the faculty. Most of the faculty were the supervisors.

The Minutes disclose changes in hours, division of hours, changes in salary, but the most interesting change in the picture was the suggested division between nursing as an educational function and nursing as a service function in the hospital requiring two people. Formerly both had been under the aegis of Miss Dunbar, but you got through, or persuaded....

I persuaded Miss Dunbar not to go on as the Director of the Nursing Service of the hospital. I thought that it was just as logical that she should be a Dean in her own right as it was for Dean Hinsey to be the Dean of the Medical School. Nobody ever thought of having Dean Hinsey be Director of the hospital. Miss Dunbar had this double duty. The reason it was had is that the hospital is the more overpowering, demanding, urgent, emergency type of demand; whereas the Dean ought to have time for reflection and be able to be independent of the hospital, introduce innovations without too much consideration of the actual daily operations of the hospital. Also, it was necessary to try to get the School of Nursing to provide for its own faculty and facilities.

Most schools of nursing beg, borrow and wheedle their educational repasts, if I can put it that way. For instance, I taught nurses bacteriology at Johns Hopkins. They didn't have a faculty that could teach them anatomy, physiology, bacteriology, or pathology. Those were all taught by somebody in those departments of the medical school. At Cornell-New York they had a mixed system. There were some women on the nursing faculty who could teach some of the subjects, and they got more of them, but they taught subjects as a rule in the classrooms of the medical college. They didn't have at Cornell sufficient classroom facilities in this beautiful, big building for the School of Nursing. That was largely a dormitory buidling. It did, however, contain a library which was very useful, and it contained rooms in which nurses were given practical training. There would be a long room with a lot of beds in it where they would learn how to make up a bed. The bed would have a dummy in it, and they'd learn how to turn the dummy over, the motions to go through bathing a dummy. That's the way they teach the nurse on the human figure in the bed. Now, they had any number of these, but they didn't have any laboratories—chemistry laboratories, or anatomical laboratories.

If they could get the endowment now, and if they could get financial support, I think that there would be great opportunities for nurses to develop their own educational programs and get ahead, but the trouble is that they can't get financial support. Recently, as I showed you, the New York Hospital-Cornell Medical Center has tried to raise 59 million dollars. Still, at the end of that campiagn which didn't quite reach that sum, they showed what unmet, urgent needs there were, and one of those needs was for an endowment for the School of Nursing. I think they wanted--what was it, five million. They didn't get any at all. Why they don't get it--Miss Dunbar tried to analyze it, but in her letter to me she just asked the same old questions.

<u>She does make a report to the Joint Administrative Board in the sense of commenting on the new experiment with ward aides—this new hierarchy which was beginning to take shape and form. In part, the division between nursing as an educational function and nursing as a management function in the hospital is,</u>

again, to separate into a greater heirarchy, or a recognized hierarchy--the very division of labor that is creeping into the profession of nursing generally. Everyone talks, and I guess everyone was receptive on the Joint Administrative Board. How did the other people, the hospital people as hospital people regard the problems that Miss Dunbar presented?

They were hostile people--the director and the financial side, the Governors--they were so impressed by their need to care for patients that they didn't see how they could do these things, and they couldn't. I want to put on this tape what I've said often to people, that I think the trouble with, or one of the troubles with nursing is its name. The name nursing is synonymous in the language with a maid servant, or a servant. It has always been a person subject to the beck and call of people who needed help one way or another. I used to tell Miss Dunbar that if she could get some big name like orthopedics, or orthodontia, or something, it would take nursing out of this lowly connotation. I have consulted classical scholars in Greek and Latin at Hopkins and at Yale to try to give me a name derived from Greek that would mean nursing. I got some funny words, but they won't take. I think if they could change the name nursing and call nursing something else, it would help them, but to me it's still a mystery why women who regard this as a proper profession, aren't putting more into this for nursing.

You know--I've been puzzled by that too. Implicit in this study with Dr. Ginzberg--it's not merely wages and hours that are at stake; it's a symbol. "We live by symbols." What kind of symbol projects a person into this area. It's no longer Florence Nightingale, though that is the deep tradition. It isn't. Part of the recommendation is that other professions have aided themselves by re-examining their own function with reference to a patient, or to a medical problem; in short, they underscored the research aspects, to build a deeper understanding of an area. I don't know whether that's possible. We've consulted other books here that are more recent and show that certainly some leadership in the nursing profession is pulling in the direction of greater concern for research in the field of nursing.

If they could get deep and significant problems to work on, that would be good, but--and I say "but" in this case meaning a contrast--they call observation of themselves research, chocking problems, time and motion problems. On the other hand, there are some that go very deeply into the sociological aspects of nursing and the economic aspects of it.

In part, the nature of numbers in the nursing profession put a premium on re-designing hospitals, as we noted in the books you brought in here on the Kaiser hospitals.

Indeed, they do. The shortage of everything has an effect on the design of hospitals, but with nursing it's spectacular what that has done. The new Washington Veterans Hospital, for example, has TV screens all around and intercom phones so that a nurse can sit at the control desk of the center of a group of wards and see what a patient is doing and talk with them. She doesn't have to get up and run around. The design of floor space as in those Kaiser Hospitals is also built into the Hospital for Special Surgery in New York. They use a central core and have radiating wards out like spokes in a wheel, so that the control is in the center, and the distances are cut down.

When it comes to leadership in the profession, has it really passed to the people who graduate from university schools, or is it still in the vast body of people in the profession, graduates of hospital nursing schools where they give all day effort in return for a cap at the end of a three year period? I don't know.

I'm afraid to answer that. I don't know for sure, but my impression is that the leaders are still coming from the women who have gone in for registered nurse training. There are relatively few of the high grade, educational institutions. It's supposed that this one at Yale now that took the place of the other school, which is training nurses to be worthy of getting a master's degree, is going to solve some of these leadership problems. They have very small classes, fourteen or fifteen in the whole school, and they go into long analyses of situations--a Ph.D. type of course, though it's a master's degree school.

They terminated the nursing school in favor of a graduate nurses training program. Initially with support from the Rockefeller Foundation it had been an experimental program and a degree program in nursing education. I don't know--somebody needs to shake the curtains somewhere.

Shake the pocket book.

Maybe this isn't the route because it's too well done, too refined.

You're pointing to the book on research in nursing. [Abdellah & Levine, op. cit.] That's an extraordinary book. As I say, it has great depth, and it is incomprehensible to most of them, I think.

I would think so too.

But the Simmons and Henderson book is better, I think. /Nursing Research, A Survey and Assessment (New York, 1964), 641/ It is some time since I really have looked at it, so I'm not sure. That is the same Henderson who is concerned with this listing of studies.

This volume is very good from the point of view of the bibliography at the end of each chapter as to what has been done, or for further reading and so on, but--you know, you have to have a floor before you begin to chew, which is why I raised the question as to whether the RN is still the typical development as distinct from the professional nurse trained in a university setting. I gather we're still on the road.

There may be something in the sex of the nurse that is impeding vigorous development. It's been very remarkable to me and perhaps it may be somewhat ignorant to say it, that so few women have reached positions of command and leadership in some of the professions and some of the creative arts. Women seem to me to be excellent sometimes in painting, or at least they have a degree of excellence, but not in the genius line. There's only one famous musician that I can recall among the women, and that is Chaminade. There have been lots of women who have come through Medical Schools and there are very few women, if any, that are heads of departments. There are practically no women surgeons. Dr. Helen Taussig--Blalock's associate on the "blue baby" at Hopkins was a good one, but Blalock was the leader there. Now these women in nursing are all by themselves, and maybe they reinforce the characteristics of women in these situations; whereas if they had a few men on their faculties, they might have a different management, or a different approach. There are very few women on faculties of medical schools. The only one I knew well is

Florence R. Sabin who was at the Rockefeller Institute after being at Hopkins, then went out to Denver and led the public health movement in the State of Colorado in remarkable fashion, but that's rare.

What was the attitude of doctors toward nurses? It's not one--or it doesn't seem to be one of any aid toward raising....

In my opinion, doctors respect nurses so long as they are subservient. The doctor wants help. He doesn't want to be bothered with the yearnings of the nurse for higher education. He wants good, solid help all the time, dependable, and he does that without being disrespectful. It's a sort of superiority without disrespect. Doctors are very dependent on nurses. They couldn't get along without them.

And yet some of the leaders, some of your heroes--one anyway, Dr. Welch, later Dr. Wimslow in conjunction with Josephime Goldmark, labored an ear in favor of greater educational opportunities all the time.

But I don't think that any of these people meant that every nurse should be so highly educated.

No, except that there was and should be a place for an educated nurse.

What we have done is jumped back and forth between educational and financial support, and why don't they get it--maybe that is somewhat connected with what I'm talking about; the woman aspect, the management of the whole thing.

There is mentioned in the Minutes of the Joint Administrative Board the turn-over which is a constant thing. It may fluctuate from time to time. This is in the order of the beast, but if you separate the function from the person, I still don't get it in as big a place as New York Hospital-Cornell Medical Center is with their concern for higher standards, except that perhaps most people are still RN oriented. At any rate, it's an interesting problem. You didn't tell me who the nurses were in your own orientation at the Johns Hopkins.

I can't remember their names. I remember the head nurses on several wards--two wards anyhow--very able women.

Not a little helpful?

Very helpful, very positive. Of course, Miss Elsie M. Lawlor, the head of the School of Nursing at Hopkins had much respect, but I was talking about the nurses there on the ward, and there were two who helped me a great deal--frightened me also. One nurse, I remember, wanted everything to be just right and in place all the time. That extended to the tightness of the sheet on the beds. Sometimes she was so perfect in that and so insistent on the smoothness and tightness that she almost produced footdrop in the patients. The same woman hated to see any medicine taken out of the medicine cabinet because she'd have to get the bottle filled again, or it would look a little empty, but curiously enough, those things didn't interfere with her intellectual qualities. She was good.

In the profession there is a built in mechanism for silence and subservience--you know. Take a volunteered observation that might be helpful--I don't know that very many doctors would necessarily be receptive to this. It certainly isn't in the pattern.

They are very receptive, but they put them in the footnotes. I can use as an example, ambulation. I remember when that came in. Patients were very anxious about being gotten out of bed the day after having their abdominal wall cut open, after a severe operation. The surgeon was usually too busy doing other operations to sit by their patient's bed and encourage the patient to get up and walk around when the patient probably thought he'd collapse and die. Well, all that fell to the nurses, and it's the nurses that were the observers and the supporters of the patients in those stages. Now every hospital has a special recovery room into which patients are taken after operation, and they are kept there for a day or two, or less, under the supervision and observation of nurses. Doctors are in and out, but it is the nurse who is watching the patient through all those crises, and the doctors depend on it. The nurses often make very shrewd observations about nutritional states and rashes that they see in patients. They can make diagnoses. They've seen plenty of things. The doctor then thanks them, goes ahead, and does what he thinks is right for the patient. If it becomes a publication, he may mention the nurse in a footnote, but not necessarily.

Miss Dunbar mentions the experience of two nurses that she knew--one where there was a language barrier, where the nurse in order to care for a patient had to somehow bridge between what she understood care to be and what from a different cultural background, which wasn't disclosed, the patient thought nursing care should be--the difficulty of language and the kind of tenacious quality that the nurse showed in hanging in there in order to take care of the patient. The other one was a study, and a very interesting observation in the letter was that the nurse who made the study thought that making the study removed her from patient care--spending so much time thinking about it, though she arrived at the conclusion that the fact that she spent time squaring her observations with experience aided patient care. Well, this has taken us into two different things--not only the management, but manpower studies. I don't suppose there is a solution to it anywhere, just accumulating evidence as to where we're drifting, or moving, hopeful that at some time something will emerge, as the design of hospitals emerged.

The male nurse is an interesting figure in these things, but he is apt to be, in my opinion, less male than he should be. There are male nurses in the Army. There are male nurses on many genitourinary wards where much handling of genitalia is necessary with catheterization of males with bladder trouble, urinary and kidney troubles. Usually there is a male nurse in such a situation. There are also male nurses right in the ordinary nursing course of things, who associate with female nurses and handle patients with all sorts of conditions. I do not believe that outside of the Army and outside of the genitourinary situations that male nurses are increasing, or coming to any special importance. In the Army they have a great many of them. The Army seems to know how to use this secondary, relatively little trained, medical assistant. Army Medical Corps men are pretty good, and they've never been to medical school. The Navy does it too. The Navy has submarines, small boats with no doctors at all on them.

I should think that with the increased specialization that one has in doctors it would require more in the way of insight and specialization of the nurses--you know, to at least communicate. That may not be. I may not understand the specialization of doctors.

Most specialists--you'll find a specialized nurse who has been with them a long time, who understands the least gesture, or understands the very small symptoms to observe. At one time most of the specialty of anesthesiology was in the hands of women. Many of them were anesthetists, but the advances in anesthesiology have come from male anesthetists like Dr. Joseph F. Artusio at the New York Hospital-Cornell Medical Center, a bright young man. Artusio has developed a system by which the anesthesia can be controlled--a sort of feed back mechanism from brain waves; put electrodes on the brain, and you can tell how much oxygen you have in the blood, or when it becomes too much anesthesia, cut it off, or open it up and give more. He has contributed a great many other things of that electronic nature and origin, coming into the ordinary as well as the special surgical practices. Out here at the National Institutes of Health a year or so ago, they built a whole new surgical operating suite in which there are seven or eight channels of vital signs coming to the operator on oscillographs, or screens of all kinds, especially with open heart surgery. That takes the place of observation by nurses and assistants, but curiously enough, however, when you get all that help, you have to have more nurses.

Yes, they have standby teams up there which include nurses.

Sure.

The view then is that increased specialization in medicine with doctors hasn't had an attendant drive for a kind of specialized knowledge and astuteness for nurses.

A great many of them--they stay in some specialty. The nurses in obstetrics are practically a constant staff. The nurses in brain surgery that I've seen were pretty constant too. The pediatric nurses who get to know children rather tend to continue along the same line.

<u>But has that worked itself into a course, a body of information?</u>

No.

<u>That's what I meant--in other words, it is an associtaion with a specific doctor where he is accessible and where a nurse learns to work with this particular doctor, but as a nurse, if she had to go from hospital x to hospital y, it wouldn't be the kind of knowledge that would be necessarily useful.</u>

In a way--they have nursing manuals and nursing textbooks on pediatrics. They'll write a textbook on infectious diseases, and they'll bring out the things that the nurses should do. I know about them. Very often their chapters are written by doctors.

<u>I hope that the drive for more university schools of nursing continues.</u>

Yes.

<u>I can't think of anything else we ought to do with nurses. I can....</u>

Yes, but I think that's enough on nurses.

Thursday, July 7, 1966 A-60, N. L. M.

Is today Thursday? What happened to Wednesday? Where have I been?

I'm sure it's Thursday.

Well, you've sketched today a series of things that you call extracurricular activities. I had some sketched also, and this kind of activity has had continuity, apart from the rationale you gave before, in your life. As Professor of Bacteriology at Rochester you felt the necessity for buidling footpaths to the local community. One way was to become aware of their organizations and part of them. It was on a little different level at Yale with the State of Connecticut. How is this progression and experience altered by your being part of a big, throbbing metropolis like New York City? Circumstances to which we have already referred created a kind of electricity, a difference of view, with local organizations, particularly in the medical field, which required sudden, emergency measures to try to obtain a meeting of minds. There's more to this than just extracurricular. It was part, I think, of the whole design of the job; to deal in a kind of public way with organized voices in the community. I've listed some. You've got some others. I don't care particularly how you take them up, but this has been, I think, a feature of your own experience, certainly from Rochester days and even before at Hopkins and particularly there in the medical history field--"action's a function of interest."

As I look back on this, I do not see myself in any original role in undertaking tasks for the community in relation to the medical institution of which I was an officer. This had been characteristic of the leaders of the profession that I had been associated with as a student. It was characteristic of Dr. Welch. It was characteristic of Dr. Thayer who also did things in public affairs in relation to medicine. It's something engrained in the tradition of the forward moving school; to know that they belong to the stream of activity of their nation and their local communities. This is something that started, I suppose, in the days when life was more leisurely. There were only so many daily duties to be performed, and those men had time for consultative arrangements and meetings. As it went on, however, the tradition grew so strong that you were expected by the community and by your organization to undertake affairs of this kind, and you really couldn't resist the invitations to join in with the efforts. As a matter of fact, you often didn't want to resist them because they were extremely interesting and rewarding efforts, coupled with very pleasant and interesting friendships, so there was a recompense not measurable by any monetary emoluments. The latter were not forthcoming at all. This effort in public affairs was always done without pay by most of the men. It got so frequent in a big medical center that even in the days of the late 1940s and 1950s when I was at the New York Hospital-Cornell Medical Center, it was sometimes very difficult to have a faculty meeting because all the faculty members were out on committees. That was true somewhat and beginning to be true at Yale.

Well, what I thought I should like to do in this talk today is to point out the common elements in these undertakings and to list four or five of them that I was engaged in while I was President of the Joint Administrative Board. The common elements are what I've already expressed, the sense of rendering service to the social and professional affairs of the community outside of the smaller institution to which a man belonged. The other common element was a natural interest in the problems that were being studied and the way that very able men went at the solution, or sought the solution of these problems. I'll mention one of them in passing because we've already spoken about it, and that is the committee headed by Eli Ginzberg called the Committee on the Function of Nursing which produced this little book called A Program for the Nursing Profession. We've already talked enough about that and its relation to situations in nursing.

Really the first one that I became involved in outside the walls of the palace of the Popes at Avignon at....

York Street?

York Street! York Avenue and 68th Street. This was called the Public Health Research Institute of the City of New York, Annual Reports of which I now have before me. I see that I was made a member of its Research Council in July, 1947, which is only a few weeks after I arrived. I suppose the arrangements for this must have been made before I arrived at the New York Hospital-Cornell Medical Center. This Public Health Research Institute is a remarkable organization in its relation to the City of New York. It was established by Mr. David M. Heyman, the very wealthy, Jewish President of the New York Foundation who had a power of persuasion and a power of friendly relationships with a succession of mayors of the City of New York from La Guardia through O'Dwyer and even to the present. He persuaded the City to put up a considerable sum of money in 1947; namely, two hundred thousand dollars to be used for the support of research by the Public Health Research Institute without entangling the Institute in the politics and influences of the City Government. The City was able legally to entrust this sum of money to the Public Health Research Institute of the City of New York without requiring a detailed accounting. The money was supplemented by relatively small gifts, some from the Nutrition Foundation at Columbia University and some from other grants--grants from the New York Foundation. They were all small grants; as a matter of fact, the total income was two hundred and thirteen thousand dollars when the City appropriation was two hundred thousand dollars, so the City was really carrying the work of the Institute.

This Institute had a Board of Directors composed of public spirited men in New York of whom Mr. Heyman was the President. A very fine man named Mr. Edwin F. Chinlund was Vice-president and Treasurer. Lazarous Joseph was a member, and William O'Dwyer, the mayor at that time, was a member, and that Board of Directors ran this Institute largely with respect to scientific matters and personnel, the investigators employed in the Institute, on the recommendation of the Research Council. This Research Council was a forthright, rather strong Council containing people that we knew about from other sources--Dr. Thomas M. Rivers was chairman of the Research Council, Dr. George Beahr, and other people who were engaged in research largely in the fields of infectious diseases and nutrition. This Research Council has continued to the present. They met several times a year. I see here, for example, the Research Council of 1950-1951, where George Baehr is a member. I'm a member. The Nobel Prize winnder, Dr. Vincent du Vigneaud, a great biochemist, was a member. Michael Heidelberger in immunochemistry, John G. Kidd, the Professor of Pathology at Cornell, the very wise Walter W. Palmer, the Professor of Medicine at Columbia, Dr. Tom Rivers, and Dr. DeWitt Stettin of the Bureau of Laboratories of the Department of Health of the City of New York were members.

This was a very interesting and pleasant connection forme. It brought me into contact with people woh were doing interesting things, people who were carrying on studies in basic science which at the same time were recognized to have an import for the control of infectious disease in the population. For example, this group did some work on the influenza viruses to bring them in use as vaccines. They did extremely good work on the development of the rabies vaccine, a virus which they used for the prevention of rabies in the City of New York and Staten Island by immunizing dogs, and so forth and so on. Many interesting things were conducted very much like the way the Rockefeller Institute proceeds with its investigations, the way the departments at Cornell, or Yale, or any good scientific school would do it.

<u>What kind of laboratory did they have?</u>

They had a building at the foot of 16th Street. It was an old hospital building. It was three stories high, about a quarter of a block long, shabby, much chopped up with partitions that had been put in for rooms, decrepit elevators at the start, but habitable. Money was put in to furnishings for laboratory work, plumbing, equipment. I think the Public Health Research Institute is still there in the same building, although there was much talk of their moving to another place. [MOVED TO 1ST AVENUE AND 25th STREET]

<u>Did the Research Council initiate programs, or did they simply pass on programs presented to it?</u>

This is not a grant-in-aid thing like the Childs FUND. This is a Research Institute that has its own program. The program came from the staff members in consultation with the Research Council. The Research Council didn't receive applications for grants from outside, or programs submitted to it. It did receive information from the Bureau of Laboratories of the City on problems that were important for investigation for the City, serological problems and others. It grew very much in finances. About the time I left their budget was upwards of seven hundred thousand dollars a year with very large grants from the National Institutes of Health given to the Research Institute of the City of New York in response to an application made by the Institute to some council in the National Institutes of Health just like a university would do. Now, I believe, the budget has gone up to a million and a half with very large supplemental grants from NIH.

This seems to me an excellent example of how high minded businessmen, philanthropists, and educators, men interested in the health of their communities could devise a plan by which untrammelled, able, scientific investigators could work on problems almost of their own choice, practically of their own choice, without any interference from the political authorities of the City which was putting up most of the money.

In one of these Annual Reports there is a short history of the Institute, and one of the novel things about its history was the way they negotiated a relationship with the City, a continuing relationship over a long period of time--fifteen years, I believe it was.

It was a contract.

So that it never had to come up for review.

It was a contract over a long period. It did come up for review once when I was there, and it was a worrisome time.

This fifteen years would appear to pull the Institute out of the vicissitudes of a yearly wrestling match.

Yes. I thought it was an admirable arrangement, and I think it's done some useful work. I think that's about as much as I want to say on that subject.

<u>You haven't mentioned Tom Rivers since he delivered that speech of his at the Philadelphia meeting. He's an old friend.</u>

Tom Rivers was chairman of this Research Council for quite a number of years, and at the same time he was head of a Department of Virology at the Rockefeller Institute. He was the outstanding virologist in the country in a way, a very able person, high strung, profane, so frank that it's a wonder that he maintained friendships with anybody. Everybody knew Tom's methods, and his manners, and they let him bark.

<u>His reports on the scientific side are very good--those Annual Reports.</u>

He's clear headed and frank. I think we mentioned Tom Rivers in World War II when he took that big Navy Laboratory to Saipan.

<u>Yes, just in passing. You used to meet with him once a month on this board--well, I don't know how frequently the Research Council met.</u>

I knew him at Hopkins. He was a class ahead of me when I started at Hopkins. He developed a muscular dystrophy so that the muscles in his forearms dwindled so that you could almost put your fingers together between the bones of his forearm. His muscles between his thumb and his first finger and wrist, part of the phalanges and metatarsal, were all withered. He left Johns Hopkins in his second year rather expecting to die. He got over this remarkably enough, and he came back with great vigor and carried on. He died a few years ago. He must have been very uncomfortable. Inspite of all that muscular trouble, he was a good tennis player. He lived out at Forest Hills and played out there.

I remember him as having given you some early instruction on the nature of people where you suggest ideas to them.

I told you about that when he was with me in the department.

Had he mellowed since these days?

I never thought of Tom Rivers mellowing. I think his consonant is a B instead of an M.

It's good that human kind can throw to the surface a man like Tom Rivers.

He was very attractive. He could say what he wanted. Everybody admired him, and he did excellent work. Well, shall we pass on to another one?

Another one of these public affairs that I'd like to speak about could come next because it was an early appointment, and that's the Committee on Public Relations of the New York Academy of Medicine. A group of publications that I had been reading studying, before I went to THE New York Hospital-Cornell Medical Center, reading them carefully because of their enlightened point of view and their influential effect on developing medical improvement were the Reports of the New York Academy of Medicine Committee on Medicine and the Changing Order called Medicine in the Changing Order. Do you know those?

No.

I ought to get them for you. The chairman, Dr. Malcolm Goodridge, was one of the members of the Board of Governors of the New York Hispital, and he had around him a great group of forward thinking people--Haven Emerson, George Baehr, others of the Academy of Medicine which is an honorable body with a long history and quite above the kind of politicking that went on in the County Medical Society. This was called the Academy of Medicine of New York which is a scientific, philosophical body--and I mean "philosophical" in the sense of the natural philosophers of the 18th Century--that was above politics. It had to deal with politics in many ways because it got involved in supporting bills for the improvement of medical practice in New York State. It had many practical activities as well as some intellectual ones. It dealt, for example, with improvements in the treatment of tuberculosis. It ranged over the whole field of current medical practice, if it wished to. It went over very intricate scientific fields having to do with the control of communicable disease and having to do with the legal, professional status of groups of specialties in the professions, or large portions of the medical professions.

This Public Relations Committee met in a great, long room, the vaulted ceiling room of the handsome Academy building, 103rd Street and 5th Avenue, about once a month. It must have been forty members or more. Carefully prepared papers were presented, short papers, but very thoughtful. There was very free discussion, sometimes strongly expressed opinions, and I felt it was a great honor to be invited to join this committee. As I say, it had a very great influence on the movement forward of changes that were required for medicine to meet the needs of a changing order. I'll get those books for you.

<u>"Public Relations" has come to have a certain kind of meaning.</u>

There's nothing Madison Avenue in this situation.

I think it's important to point that out.

Well, public relations for this committee means just what the honest English word means--relations between the public and medicine, relations betweeen medicine and the social changes. The public relations were the kind of relations that you have in mind when you speak of relatives, or members of your own family, or relatives in a dynasty, or something like that. It had no tinsel on it, and it was truthful.

You indicated that legislative matters came before this committee.

This committee watched all the bills that came through Albany that had to do with medicine. There were a flock of them every year. They were changing conditions of licensure. There were bills dealing with phases of all sorts of medical practice and medical social economic arrangements. I forget the details, but they had a Legislative Committee, and when bills were coming up before the legislators in Albany, this committee would consider them and through the secretary of the Academy, we'd send a statement to the Governor, or to the sponsors of the bill--usually to the Governor.

Did you meet through this committee--not necessarily through this committee--Dr. Hilleboe [Dr. Herman Ertresvaag Hilleboe] as Commissioner of the State?

When did Hilleboe come in as Commissioner? [1947] I've known him so long I forget.

He was down here during the tuberculosis....

The new drug for tuberculosis? [Isoniazide] That was just coming in then at that time.

He may have been down here. This was during Governor Dewey's term, and perhaps the Commissioner before Hilleboe was in office--whose name escapes me--Wadsworth. No.

Wadsworth was the head of the Laboratory at Albany--Augustus Wadsworth, a long time friend of mine, very dignified, a tall, handsome man--power in his physique and power in his personality.

This Committee functioned by having visitors with it occasionally--they'd come in and tell the Committee about something that was going on. Many of its studies were made by members of the Committee, or by people on the staff of the Academy. An example of the kind of thing they would handle through, we'll say in this case, a subcommittee was a move by a group of fine women, mostly in New York City, to secure the passage of a bill for the recognition and licensing of what were called Clinical Psychologists. Clinical Psychologists belong in the heirarchy of medical assistants, so to speak, that we spoke of yesterday. It's their purpose to try to adjust people to the situations of life in which they find themselves, although they, as adjusters, are not trained in psychoanalysis. There's a whole branch of psychology that has to do with behavior that doesn't need to be tied in with psychoanalysis. These clinical psychologists are advisers to school teachers. Most schools have someone like that on the staff of the school. They are right in, closely knit with social workers. They're dealing with problems of people who are poor--I'm sure that; I don't know this for a fact, but the "war against poverty" is bound to draw into its ranks these people who are able and interested to have poor people improve their lot so that they're not so poor afterwards. Now, they were fought tooth and nail by the psychiatrists of the state, and this was a bitter, long lasting episode. I think I was on the subcommittee dealing with this and wrote some of the reports. I was in favor of the recognition of the clinical psychologists, but the medical psychiatrists said that nobody should get into this kind of work that didn't have both a medical degree and special training in psychiatry. These women didn't have either, but they were good people. The outcome was a sort of a compromise. I don't know what the present situation is. The clinical psychologists are recognized now.

Yes--they opened up a whole new field really.

That was an example of the kind of thing this Public Relations Committee would do. Another case was the time when isoniazide, the new chemotherapy of tuberculosis, was coming in and being tried out at the big tuberculosis hospital--I think it was on Staten Island.

I think there is one there.

The name has gone out of my head. I remember the episode very much because after this drug first came in, it was spectacular what happened. People in the tuberculosis wards could sleep at night for the first time since they'd been there. All the coughing stopped. The sputum cups that used to overflow with this tuberculosis sputum over night almost--great amounts of that that the sick pulmonary tuberculosis patients would bring up--were almost dry after this drug was used. It was spectacular and like all new drugs somehow or other they work wonders at the beginning, and then they begin to show their limitations. One of the limitations in the isoniazide therapy is the appearance of resistance forms of the tuberculosis bacillus. Other compounds had to be tried, but it seemed to give promise of eliminating tuberculosis because it would be a very powerful tool to reduce the spread of tubercle bacilli. If it didn't cure, but greatly reduced the spread of tubercle bacilli that were being sprayed around by persons with millions of them coming out of their lungs, it would reduce the infectious potential.

That's always been an important principle of preventive medicine. People don't easily understand the relation between therapy and prevention. An ounce of prevention is worth a pound of cure is an old trite saying, but sometimes a pound of cure will give you many pounds of prevention because it's wiped out at the source. You see that in some cases of syphilis and gonorrhea--if you can clean up the source of infection, you've done a better job than you can do with a lot of preaching. If you can cure a local native population of malaria so that they don't have any more parasites in their blood, the mosquitoes have to look for something else to do.

That drug therapy for tuberculosis was going on, and that was of interest to the Committee on Public Relations which, I say, was rather a focal point for social, economic, professional, and scientific considerations of medicine in relation to the community. It was guided by Dr. Haven Emerson who was a very eminent figure in public health and preventive medicine in New York. I think that's about all I can say about that.

<u>How does this compare with the Committee on Public Health of the Medical Society of the County?</u>

The Committee on Public Health of the Medical Society of the County dealt with regulations and a few local conditions. It also dealt with side issues that could be called things that belong in the field of public health. One that came down on me very hard was the question of discrimination in the State of New York. The County referred the question as to whether there was discrimination against Jews and Negroes to the Committee in Public Health just when I had been made the Chairman just coming in, and their intention was to fight Cornell. They had accused Cornell Medical School of discriminating. The movement to prevent discrimination got very strong in New York State, to such an extent that you couldn't ask the student what his mother's name was. Anybody knows that if he tells his mother's name, that's where you find out whether it's a Jewish name. The father's name had been changed, but if you ask, "What's your mother's maiden name?", that discloses. That was not allowed by the law after a while. No longer were you able to ask for a photograph of a candidate for admission because that would disclose whether he was a Negro, or it might have disclosed whether he was a Jew.

They thought that there was a quota against the Jews, fifteen percent or so, and a real strong discrimination against Negroes. There was some peculiar, mystical fifteen percent of Jews in practically all the medical schools, and each of them said that they made no conscious discrimination. The ratio of the fifteen percent in a medical school represents a larger number than the ratio of the Jews to the whole population. If there were few Jews in the community, fifteen percent is a large number. There were millions and millions of others--non-Jewish--and the percent of students taken from that crowd are relatively less than the ones from the Jewish, but it is curious how it happened. I suppose there may be some unconscious selection of the ones who interview for the school, but it came out around that figure, fifteen percent, though the people who are admitting don't know the number they have admitted. It's a mystical thing. As I say, you don't have a current dope sheet.

Did the Committee investigate what this ratio was?

No. They accused Cornell and other schools of discriminating and had violent open meetings on the subject. Usually the attendance at those night meetings would be a couple of hundred. When this question was up, there would be four or five hundred people there.

The Committee on New York Hospitals branched over into the relation of institutions to medical practice, and some of their efforts to keep the New York Hospital from developing a diagnostic clinic are the things that gave me trouble. I've mentioned those that came up. Partly they were heard in front of that Committee on Public Health.

Did it get into public health matters as public health matters?

Yes, it was interested in water supplies. One big problem constantly was air pollution. The Academy and the County both were concerned with air pollution because air pollution in New York was very, very bad. They had a Commission of the City of New York under Dr. L. Greenberg, a distinguished public health official and others. New York City had huge incinerators down on the East River which would blow their fumes and fly ash over the City. I lived on 72nd Street in a penthouse--not too far from the incinerators on, we'll say, 78th Street and the East River. On the little terrace up top there would be not only what you'd call natural fly ash, but sometimes there would be pieces of bloody bandages and other things that would come up through the flues and be blown over. There was a good deal of sulphur dioxide in the fumes, and that ate up the nylon stockings of ladies too fast. It was quite a problem. These fumes from New York came not only from those incinerators, but the enormous electrical generating plants.

Con-Edison.

Yes, Con-Edison--black smoke all over. That stuff would blew across over into New Jersey--Jersey City and Hoboken across the river--which had its own local contribution to make to the air pollution, and then it would blow back across the Hudson and up through Westchester and into Connecticut. This problem goes far beyond the County of New York, far beyond the County Medical Society, but it was of interest to them. It looked as if you might have to violate the Constitution of the United States by getting some kind of a treaty between Connecticut, New Jersey and New York. Not one of them could control this by itself.

So far as you can recall, were there any studies of New York City dust going on in laboratories in New York? The natural fall?

Yes, they scraped it off--particles in the dust, silicate, coal particles. We're getting away from this county medical society now.

We brought up air pollution. Did the Public Health Research Institute--were they thinking about and working on the natural fall of dust?

I don't think anybody there was, but the whole subject is interesting to any group or organization that has got public health in its title. Automobile exhaust was an interesting subject to all these public health organizations. The stuff that accumulates in the Holland Tunnel, tarry-like material from automobile exhaust contains benzpyrine which is a strong carcinogenic compound. You can produce cancer in animals by injecting the stuff you get out of the Holland Tunnell--by scraping it up. That alarms people for a while. What they breathe of it may, or may not hurt the lungs--probably does. In London you can collect benzpyrine by letting a piece of moist paper lie on the window sill. There's plenty of it.

New York's water supplies have always been watched by any of these bodies in public health. Croton Reservoir has to be protected. For example, there have been outbreaks of typhoid in New York City in the past attributable to the deposit of typhoid-bacillus-containing-feces on the banks of the reservoir in the winter. When the thaw comes, this washes in, and they have been faced directly with that kind of thing.

Are we finished?

No, this machine was just beginning to squeal. It's all right. I'll hold on to it for a while.

Well, I have no more to say about those things, I think.

This sounds very much as though the County organization was more of a wrestling, offensive-defensive kind of group than the Academy was.

Yes. The Academy is, we'll say, on Mount Olypmus. This group of people are down in the gutters.

This is the group that sent you up to Harlem to become a member.

Yes. I'm being a little bit excessive when I say that they are in the gutter. There were excellent people in the County Society but they were more interested in the mundane problems of every day life, the doctors who are making a living, than this other committee. They didn't have that trouble, or that problem before them.

What about the Hospital Council? Do you have that down there?

Yes, the Hospital Council of Greater New York was another body composed of businessmen, leaders of the profession, and some hospital administrators. Its purpose, as I recall, was to try to supervise the hospital situation in New York City and Brooklyn in order to obtain a balanced arrangement of beds and staff that would serve best the community in which the hospital was located. It made a number of studies of plans of people who wanted to add to hospitals, or to build new hospitals, to try to advise them on what would be the best thing to do for that location. We were even asked by some people on Long Island to look over their situation, and Long Island at that time was beginning to build some community hospitals, or new hospitals. Very profound and careful studies were made by the staff of the Hospital Council of Greater New York under the direction of John Pastore, who is the man I thought would make a good director of the New York Hospital.

Pastore was very intelligent. He came from the family of the Pastores, one of whom was the Governor of Rhode Island and the other is a Senator from Rhode Island--lots of talent, ability and energy in this family. Pastore had been on the staff of obstetrics at the New York Hospital-Cornell Medical School some years before this. He had made a reputation of being rather a martinet. Aside from his appearance of being a somewhat unkempt Italian, he had a reputation that was unfavorable to himself as a person in a group, but alone as the director of the Hospital Council of Greater New York, you didn't have to bother about those other things. He had a budget to manage, a staff to work with, and problems put before him coming from communities and coming from what Dr. Baehr knew, what I knew, or what the State knew.

One of their main functions was to advise the State on the distribution of the Hill-Burton money which gave them a very important position in the community. Every year we had long meetings on what recommendations we'd send to Albany on the distribution of Hill-Burton money, on new construction, or repairs. Again the Council Board was a group composed of public spirited men who worked for no salary, or pay in connection with it. The staff was paid, but the Board wasn't. I think I was a member of that organization practically all the time I was in New York, from 1947 until I left in 1953.

In a way, this Hospital Council of Greater New York was to take a greater New York point of view toward hospitals too, the service as a whole.

Yes. They extended out beyond Brooklyn and even to the middle of Long Island almost. What is that city in the middle of Long Island, back of Mr. Whitney's place?

Manhasset?

Manhasset is where Mr. Whitney lives.

Back of it?

I don't mean back of it--in toward the center there's a city.

Jamaica.

I think it's beyond Jamaica, or south [Mineola]--this oughtn't to be on the tape.

The point is that they had a point of view to sustain plus a staff to gain the wherewithal to really consider it.

They had to deal with denominational hospitals as well as municipal hospitals, and the private non-denominational hospitals. Some of the cases were of such a nature that the Council had to avoid the conflict, we'll say, of the Catholic, or the Jewish elements in a city who wanted a hospital, and the Council didn't think it should have that kind of hospital. Then the Council would be accused of being anti-Semitic, or anti-Catholic.

Was there any attempt to tie together what was available in the way of equipment in these various hospitals?

No--not that I know of. I don't think that's a practical thing anyhow.

Or, in the sense of affiliation with a central organization. It didn't have that function.

I imagine that would be handled by sort of word of mouth and personal things. If it were known, you would know it, that the New York Hospital was overstocked with respirators, and poliomyelitis existed in the community, they would either send the patients to New York Hospital, or borrow a respirator. They couldn't exchange x-ray apparatus. I'm thinking of the expensive, big pieces. This Hospital Council didn't deal particularly with the technical aspects of hospital practice. The Council had some influence on the Blue Cross plans, and some questions were brought up when the rates were under consideration. The Council furnished infomation, but didn't take a position in favor of any particular hospital to get its rates raised. They took the question of rates as a general thing.

Now, I have down here two other things--the National Manpower Commission and the Commission on The Financing of Hospital Care.

That's that one over there--the three volumes. Do you want those?

I think we can dispose of this one rather quickly.

There's a very interesting discussion there about the poor.

Yes. The Commission on the Financing of Hospital Care was established in November, 1951, as an independent, non-governmental agency, as a sequal to a previous commission which was called just a Commission on Hospital Care, an organization largely set up by the American Medical Association and the American Hospital Association. This Commission on the Financing of Hospital Care was supposed to be, in 1951, a much broader commission. It was placed under the chairmanship of Mr. Gordon Gray who was President of the University of North Carolina at that time. The membership was really very interesting--Lewis Strauss was a member, Ed Crosby, director of the Johns Hopkins Hospital was a member, Robin C. Buerki who at that time was director of the Pennsylvania Hospital. He went later to the Ford Foundation in Detroit. Howard Rusk was a member, the great one on rehabilitation--I won't read the whole list, but it was an interesting list.

The most interesting member to me was Mrs. Agnes Meyer who came on somewhat at a later time and was with me on the subcommittee I was on that had to do with the financing of care for the people woh couldn't pay anything, or the underprivileged and unpaid people. This Commission on the Financing of Hospital Care had many consultants, many widespread relationships, and many, many meetings, most difficult meetings, because the membership contained representatives of most conservative opinion, so conservative that they couldn't see the government doing anything for hospitals. Some of the members were officials in high class insurance who thought that private insurance could supply everything that was needed for making the patient able to get a policy that would pay for the cost of his illness, or the cost of his hospital care at least. People like Mrs. Agnes Meyer, who was notably a liberal person, a facile writer, intelligent, enthusiastic, sensitive to the needs of people, who had written several books, one of which I had read about this time called Journey Through Chaos which deals with the plight of the poor people in the defense industries in World War II, people living in tumbled down shack towns. I had known Mrs. Meyer before, and I had known her distinguished husband, Eugene Meyer, for a number of years, partly through Yale connections and partly through happenstance of personal dealings. Mrs. Meyer and I used to sit together at these meetings and uphold about the same point of view against such a person as Dr. Morris Fishbein, the editor of the Journal of the American Medical Association, who, in fact, was the determiner of the policy of that association and succeeded in keeping it in a most conservative and retroactive--not retroactive....

Regressive.

Yes, regressive--reach way back.

This commission had a large staff, and I think the costs of its studies ran about five hundred thousand dollars. It put out three volumes of reports, but had enormous files of statistics and the staff that worked them over. I see by the preface of the book that after deliberations, the commission came out with one hundred and eighty-one principles and recommendations, so there must have been a considerable variation in its statements. In my opinion, the effort stopped with the publication of its reports as far as a concrete event can be found; on the other hand, this brought into more consideration the knowledge, information, and ideas relative to the great problems of financing hospital care by individuals and the great problems of financing hospital care by the institution that furnished the care. Both were in trouble all the time. We talked about the deficits of the New York Hospital, the deficits of other hospitals. They were all well known.

The third volume talks about the deficits of....

Yes, the non-wage and low income group--that's an interesting lot of people who haven't any means of meeting their bills. This study of the non-wage, low income group brought out very clearly the need of assistance from the federal government; as a matter of fact, it is almost the basic discussion of what we now call medicare.

Yes.

I suppose this report went in and out of the minds of the people who were dealing with the problems of government support, state and federal support, of medical care. Also it fitted in with some things that were going on in England at the time because the reforms started by Lloyd George, altering the whole system of medical care in England, were coming more and more into operation during these years.

I found that the more significant of the volumes.

Well, it's a thinnish book too.

To get this far, it probably had to go through this commission. It may even represent a minority view.

Yes, there are statements at the end of this book opposing some of the statements that the committee took.

<u>It was the most controversial field. These are good. They help chart a "journey through chaos"--to borrow Mrs. Meyer's phrase. They put a finger on a point in time where the situation was examined and certain recommendations came out, and it's hard, confronting the problem anew, not go go back to these studies as points of departure.</u>

To me it was a valuable thing too because, although I was not in the New York Hospital-Cornell Medical Center environment while I was struggling through these meetings. I was spiritually in New York and learning a great deal that was of value to what we were trying to do in New York.

<u>I would think in all the things that you have mentioned so far as "extra-curricular" that it was like washing the same shores from different points of view.</u>

I call them "extramural." They're not extracurricular. I wrote it down extracurricular, but I meant to say extramural. I was thinking about the walls of the institution, but when you say extracurricular, it means some other course.

<u>I just meant that there was alot here that you could absorb and thus broaden your own point of view even confronting the complexities the New York Hospital-Cornell Medical Center were.</u>

Oh yes, yes. They all fit in. We'll talk of it another time--the manpower studies where we were dealing with shortages of professional people as well as nonprofessional.

<u>It's a few minutes to three. We've gone a little over an hour.</u>

Turn it off.

Friday, July 8, 1966 A-60, N. L. M.

Yesterday we sampled some additional experiences that you had apart from the Joint Administrative Board, experiences that added to insight and living and working in a community as complex as New York, working with various agencies in the city, both professional and scientific, agencies charged with the accumulation of information about certain aspects of health--hospitalization and so on, even construction, in an effort to have some basis where they could exercise judgment more properly. One of these additional experiences went beyond the metropolitan area. It was a new kind of study which was coming to the fore really, the study of what we have by way of human resources. This was related in part, I think, to General Eisenhower becoming President of Columbia University and his views with respect to the number of people who had been rejected under the Selective Service process. This was a study of what talents we have, where to find them, how to maintain them, how to use them--the National Manpower Council established at Columbia with support from a number of interested manufacturers....

And the Ford Foundation.

And the Ford Foundation. There's quite a list of manufacturers who were interested in this. What intrigues me most about it is the fellow who, in effect, terminated, cut down in a short space of time the Medical Department of the Surgeon General's Office, Eli Ginzberg, the approach he had, the process followed, the way in which this council worked. As I understand it, there was a staff that made for continuous preparation, and the Council itself met to review, or think about, talk about, discuss and come up with recommendations. This is a new area so far as human endeavor is concerned--to think of human resources. I don't know what you want to say about it, but this is a way of looking at experience that you hadn't had before.

When the study that you refer to was undertaken around 1950, or thereabouts, it had nothing to do organically with the demobilization of the Army which had taken place in the late 1940s--1946 and 1947. I'm putting this in to correct, or expand a phrase you used when you referred to Ginzberg in the same sentence as being one who worked out the reduction of the Medical Department in demobilization, and this study of the possible shortages and maldistributions of people and personnel in the United States. The two are not connected except that Professor Ginzberg had a chance in the demobilization to try his talents out on a very practical, painful basis. He's a remarkably intelligent man, and that experience gave him greater capabilities than he had before. He was interested in the study of manpower through the fact that he is an economist, and he is interested in business administration in the United States. He holds a Professorship of Business Administration at Columbia. He might have influenced the President of Columbia to undertake this study, for all I know, because Professor Ginzberg was in it from the beginning.

Various things are said as to why General Dwight D. Eisenhower, who was then President of Columbia University, got so interested in a study of human resources in the country. One of the reasons given is that he was shocked to find how many young men in this country were disqualified for service in the Army because of mental, emotional, or physical deficiencies. He also, as a soldier, was shocked by the rumors that were going around toward the end of the war that maybe the country didn't have enough men of military age to fill in for replacements and make up the losses that were occurring in the war with Germany and Japan. I think that was a real anxiety because at the time of the Battle of the Bulge, General Eisenhower was greatly worried as to whether he could get enough soldiers to meet that German offensive. I've read somewhere else that at the end of the war--I think in the report of the Chief of Army Transportation--it was stated that about the end of the war there was not one full division left in the United States, a really full and equipped, ready, trained division. From a military point of view there were anxieties about the numbers of people that would be serviceable for all sorts of things--both military and civilian--in the country.

There was a realization of this problem, of this situation among industrialists because they needed to know a great deal about what was called the work force, the labor force in the country. We don't know what the labor force is in the country. It's a variable affair. It has to do with employment, has to do with the sex of people. At one time they include all women in the labor force, and at another time they don't. One time they will include school children in the labor force, and when the schools open, they go out of the labor force. It seems to me those things also make you cautious as to what conclusion you draw from the statements of unemployment rates, or unemployment numbers. They're seasonal, and they vary a good deal. Well, there are thousands of questions connected with the characteristics of the people in the country who would be needed for one thing, or another, or who would be able to supply manual labor, or intellectual labor, or artistic labor--all sorts of capacities.

As I say, I don't know for sure how the study of the manpower of the country became so interesting to the President of Columbia that he backed it and made a project out of it sanctioned by the university and well supported by funds that were drawn into it. The committee that was appointed was very interesting in its composition. There were industrialists, economists, historians, public servants like Charles P. Taft , a very shrewd friend of yours named Robert M. MacIver. I fortunately was a member thanks to the friendship with Eli Ginzberg, I'm sure. This Council had an unusually able chairman, Mr. James D. Zellerbach who was a rich man from the timberlands and the vineyards of California--the Crown Zellerbach Corporation, manufacturers of paper and paper products was his main business, but he also cultivated the grape. The Council started meeting in New York with the aid of a very able staff headed up chiefly by Henry David, who was the staff man who did the footwork and managed the office of the Council.

The procedure followed was to hold a full meeting of the members of the Council who would carry on a discussion guided by a previously prepared agenda and by the study of some staff papers that had been prepared and sent out in advance on the topics that were to be considered on the day of that meeting. Those discussions stayed in bounds quite well, but there was no limitation on what anybody wanted to say, and occasionally, naturally, the discussions ran far beyond the subjects of the agenda. The meetings lasted all day--sometimes into the evening, sometimes several days.

One mode of conducting the meetings was to remove the Council from New York and take it up to the Harriman Estate in middle New York. This Harriman place [Arden House] built by Averell Harriman's father [Edward Henry Harriman] was like a Rhineland castle, a great, big ornate house on a granite ledge with a wonderful view over the rolling countryside, steep cliffs near the walls of the building, ornate carvings. It was old-fashioned in many ways at the time we occupied it for several days for a meeting because it was built back in the 1890s, somewhere back there--great big bathtubs in which you could swim, or settle into a marble bathroom floor with plumbing fixtures that were as large as steam valves for some kind of engines. You entered the place through a chapel. Whether that was always the way I don't know, but you came in through a chapel, through dark oak carved pews and rather Gothic-like beams leading into a vaulted ceiling. The other rooms in the building were enormous places where you could have good sized meetings. There was a very large dining room.

The meetings at the Harriman estate by the Council were populated by the members of the Council plus all of the staff, plus a number of visitors, plus quite a number of consultants that were brought in. This was a very thorough schooling for the members of the Council and provided an opportunity for a very thorough investigation of the subject, or discussion of the subject. Eli Ginzberg and his staff were very clever in what they did for these meetings, in the manner in which they managed to have their expert papers embodied in the discussions, embodied in the book that was published on A Policy for Scientific and Professional Manpower (New York, 1953) in such a way that the members of the Council had the feeling that they were doing it themselves. It was very cleverly done, but if you look at the book, you can see that it is really divided into staff papers and some of the Council's discussions. I think it took the better part of two years to produce this first book from the Council which bears the imprint of 1953. It took fully that. I know it was not published until I was down here in Washington as Technical Director of Research in the Office of the Surgeon General.

The first copy of the book was brought down for presentation to President Eisenhower who met members of the Council in the rose garden next to his low ceilinged office in the Executive Wing of the White House. He seemed pleased, but perhaps a little uneasy, because the dust cover on the volume of the copy given to him was a flaming red, and he did say,"Why did you give me this red book?"

It looked as if the communists might have had an influence on the Council and indeed some of the things that are said in this report are not only liberal, but they go rather into the future possibility of social rearrangements in the country. Of course, it's not a communist book, and the President was only joking when he pretended to be frightened by the color of the cover. The deliberations of the Council showed that there were great shortages of people in certain capacities in the country, that there was a great need for the accumulation of knowledge of the composition of the population, that a great requirement was having as exact figures as possible in order for military planning, or industrial planning to go forward.

I suppose this is not by any means the first study of this kind. I'm sure it isn't. I think John Stuart Mill long ago did something of this kind. Every nation has looked over its human resources from time immemorial. This may have been what people call a more sophisticated type of study. Well, it ought to get better as time goes on. You find out what deficiencies there were in previous studies, and you can then do something more exact in the next study. This Manpower Council, it was called, the National Manpower Council, published a number of other books. They studied--yes, Woman Power (New York, 1957) and problems of Student Deferment and National Manpower Policy (New York, 1952). The Council studied in the coming years problems of the Negro, problems of the Uneducated--I don't mean their personal problems, but the problems of national usage that came from characteristics of these groups I mentioned. Eli Ginzberg is a very prolific writer and a very clear thinker, and he is continuing to write and talk about human resources with very influential eloquence and knowledge.

On this list of the Council from what minds did you get a real push, even if it was something you could not join?

You mean leaving out Ginzberg and the staff.

And meaning no disparagement to those that remain unmentioned, but--you know, in any discussion, there are some live wires.

Well, Professor Robert MacIver was a very live wire, carried a high charge which sparked at the least approach to some other conducting substance--I mean the mind of somebody near him. The only woman on the Council was Sara E. Southall, and I can remember her speaking to the point on a good many things, but I don't remember exactly what she may have said. Jacob S Potofsky, President of the Amalgamated Clothing Workers of America, was an intelligent and expressive man with firm opinions, but reasonable though. I think I approached him thinking he was a labor leader from the Garment District who would have very fixed ideas. That was not so. As a matter of fact, I and some other people at the New York Hospital-Cornell Medical Center got interested in the possibility of getting the Board of Governors to elect Mr. Potofsky as one of the members. That board--this is a digression, but I will forget it if I don't mention it now, the Board of Governors thought it ought to broaden its representational characteristics by having important men from religion, from sectarian religion and from labor on it. They started in by thinking that they would elect first Rabbi Stephen Wise who was a very fine man. You know of him in New York?

Yes.

That seemed too small a thing to do and possibly offensive to other prelates in the district, if you took only one branch of the church for representation, so they elected Cardinal Spellman [Francis Cardinal (Joseph) Spellman] and the Bishop of the Episcopal Church, Bishop Henry Knox Sherrill, known to me from the Institute of Nutrition days, a friend of mine at Yale a long time. Rabbi Wise didn't accept, but we did have the benefit of Cardinal Spellman and Bishop Sherrill. Of those two Cardinal Spellman was the more faithful in attendance at the meetings and had a great deal of genuine interest in hospital administration, particularly in matters of finance and insurance. He said some very shrewd things. Bishop Sherrill who had been on the Board of the Massachusetts General Hospital and who impressed the Yale Corporation very much while I was Dean there by the things he said about medical education and the management of such a hospital as the New Haven Hospital, took relatively little part in the affairs of the New York Hospital-Cornell Medical Center, and he didn't attend many meetings.

Were you able to get the labor leader representation?

No.

He's an interesting fellow--Jacob Potofsky.

The other people on the Council that I recall--of course, Mr. Zellerbach was an impressive person with a quick mind. Philip Young, who was the Chairman of the Civil Service Commission in Washington came to a number of meetings and said a good many things out of his experience because he's had to do with many personnel problems through the Civil Service Commission. A man that interested all the members of the Council was Dr. Wilbur C. Munnecke who was the vice president of the Marshall Field Enterprises in Chicago, a very substantial man. We even had good physical advice from the fountainhead of physics who was at the same time and economist and a student of human behavior, a wise man and an attractive man, Lee A. DuBridge. Do you know him?

No.

He was president of the California Institute of Technology at that time. I'm looking at the printed list of the members, and these are the ones I mostly recall. I ntoice here that one men we looked for very much wasn't appointed until March of 1953, and didn't participate in the study that we're discussing on professional manpower policy and that was Douglas Southall Freeman. I don't remember that he was ever at a meeting, but I wish I'd seen him. General Lee is one of my heroes. He wrote.../Lee and His Lieutenants7

Yes, a good four volume work. Was it the Council that determined the recommendations, or did they merely pass on the recommendatuons as presented to them by the staff? Was it give and take?

Yes, it was a give and take. The staff would draw up a recommendation--at any of these big Council meetings, you must have something on a piece of paper.

To carve up.

Yes, carve up, or hammer at. I learned that at these meetings of these important bodies and used it, as I will tell later, on this study of medical education and research that was done by the committee appointed by Mr. Marion B. Folsom later on in 1958. When we reached an impasse, so to speak, where nobody could agree on the wording of a resolution, a move was made--I don't mean a motion, a move, an action was taken to get somebody to write down what he thought the group was intending to say and then get copies made, pass that all around and chew it up. That's slow. When we were in Washington finishing up the report on medical education and research for Mr. Folsom, the Secretary of HEW allowed us to have the help of five or six stenographers, secretaries. They would come into the room when we were in a state of indecision, and they would sit down and take down quickly what one man was saying, or another was saying, or what the group thought it might want to say, go out and make ten or fifteen copies, come back--it made a little break too in the discussion which was not too harmful and which let the dust settle.

When you have something to look at with a pencil, or in talk, it's a lot easier to go about refining it instead of throwing air back and forth. Was this the way this Council worked?

Yes, they worked over those recommendations. If you'll notice on the front of the book--"a statement by the Council with facts and issues prepared by the research staff." They separate them right on the title page.

I left New York before this book was published, although the study had been completed before I left--not quite completed. I lost contact with the people working on problems of human resources, got immersed in the medical research affairs of the Surgeon General, and I really don't know what impact, as they say, all these volumes have had. There have been dozens of studies of manpower since then. For example, Dr. Leroy E. Burney, when he was Surgeon General of the Publlic Health Service, appointed a committee to study medical manpower. In our committee for Mr. Folsom we have a good deal to say about manpower. By accident just the day before yesterday I happened to wander around in one section of the shelves on B level of this National Library of Medicine, and I found books of hearings in the Department of Labor, Department of Agriculture, various ones like those small typographically manufactured tomes by the Government Printing Office, books four inches thick, a thousand pages, labeled "manpower." They came out just in the last year or so.

We mentioned the National Research Council way back.

I was on the committee on getting up a roster of the scientists of the country--that's a favorite pastime.

At the time you drew up the list for the National Research Council, this kind of thinking and approach wasn't, so far as I am aware, part of the body of material presented in universities in courses. It's become a rubric, or a way in which you can present material, or present problems in a university setting. It was not part of the scheme as of the time you were making the lists.

I was thinking about the value of those lists the other day in connection with this history of preventive medicine that I'm writing. They have in the military a classification system. It's called--every officer gets what's called an MOS number. That means the Military Occupational Specialty. There was none of that when World War II started. In 1940, when General Simmons came down into the Surgeon General's Office to be head of the newly established Subdivision of Preventive Medicine, he had only two men on his staff, and he had to build up his staff. He had no MOS numbers to look over, but he was the best personnel officer on his own. He knew everybody in the country, or he knew how to find out about what kinds of characters there were, what their capacities were. I read a very serious book recently on personnel management in the military forces, and the author says perfectly plainly that you just can't depend on MOS numbers, or lists to tell whether this man is really the man you want. MOS numbers and classifications in these lists don't take into consideration the emotional content.

<u>That's that volume.</u>

Yes, that's this volume, <u>Report of Working Group on Human Behavior under Conditions of Military Service</u>, but that's much later.

<u>That is much later. In a way without the classification system, or the listing that you came out with in the National Research Council--some fumbling effort to deal with this great sprawling thing called America that has talent....</u>

Leonard Carmichael was head of that working group at that time. He was a psychologist, always interested in behavior, and he rather guided that sort of study.

<u>I don't think the classification system was accepted until 1943, was it? It</u>

was a start as this is a start to get thinking on an overall basis as to what you have available and how to enhance it. I don't suppose we can squeeze any more out of this, except that this is the kind of committee and association with people in a process that you hadn't really necesarily had before--the cross section of humanity that is on that Council was a new thing, new insight, like Potofsky. I don't recall your dealing with a labor leader before.

It was a very broadening experience. Let me hear you say about picking teams. People have been picking teams ever since there was a Roman Legion.

You get it down on paper, and it won't wash. You have to see it run around a track for a while, and then these indeterminant things that make all the difference come to the surface, and you have to make adjustments. I think that's simply because nothing we ever do has the stamp of finality on it. It's really open ended, but at least you have better pliers, or maybe a set of eyes. I think we could leave the extramural, I believe.

Yes.

Except for one thing. It's not really extramural. Let's just deal with 1946 and 1947. In part, we've already mentioned something about it, but not with the kind of precision that we ought to, and this was getting out of the Surgeon General's Office and the basis on which you left. I found something this morning, and you also found some things, whereby you were made a consultant to the Secretary of War in July, 1946, and in that capacity came to serve as the first president of the reorganized Army Epidemiological Board, reorganized after World War II, and you were its president, something that we overlooked at the time.

Francis Blake was the first president of that board.

Of the early board during the war.

Yes.

But here in 1946-1947.

Yes, in July, 1946 to 1947, I was president of the Army Epidemiological Board. I should like to just have a chance to correct that date somehow or other. Maybe it was July, 1947 to 1948. I'm not sure. There's a contradiction. Well, the reason I hesitate to say that it was July, 1946—yes, it's all right. I was still in uniform, but I had been separated officially from the Army in May. I had a hundred and eighteen days of terminal leave, and I was staying around the place. I didn't go away during that leave, so I worked practically in a civilian status. The reason I speak of this was that we had an unwritten rule in that board that nobody in government service could be one of the board. For instance, Topping /Dr. Norman H. Topping/ was a very valuable man in rickettsial disease studies, but he was an officer of the Public Health Service, and we never put him on. You look over the lists of the directors of the Commissions and the members of the Commissions, and you don't find a soldier, or a military man, or a Public Health Service man, or a Navy man, and we thought we'd have to--I don't know that we had it written down anywhere, but I know that was a rule that guided me and General Simmons and Dr. Blake. The board could be assisted by a military man the way I assisted it by being its so-called administrator in a uniform, and how Major Aims C. McGuinness, my assistant, carried a great deal of the work of the board, but he wasn't a member of the commissions either.

Or the way we described how supplies were available, or laboratories could be used.

The fact that I was appointed a consultant to the Secretary of War when I reverted to this civilian status was simply following what this board itself had established. These consultants to the Secretary of War didn't exist before we got the twenty dollar a day people we talked about earlier in the composition of the Commissions. There was, however, in the Surgeon General's Office a military rank of consultant. I think I explained earlier--Brigadier General Hugh J. Morgan was a consultant. It was called the Consultant's Division, the Consultant's Service. The head of surgery, Brigadier General Fred W. Rankin, was a consultant. The head of psychiatry, Brigadier General William C. Menninger, was a consultant. They didn't have a civilian appointment as a consultant. All the civilians have a regular civil service appointment. It even carried the Social Security Number when that came in, but the consultants we're talking about named themselves because they had a sort of Harley Street snobbery estimate of their positions in the service. This was disadvantageous to Preventive Medicine because none of the Preventive Medicine officers called themselves consultants, and they weren't included in the Consultant's Division in the Surgeon General's Office, so much so that after the war when we formed a society that called themselves "Consultants to the Armed Forces", we had to organize a campaign to get the by-laws changed to admit Preventive Medicine people of great distinction to that society. I happened to be one of the founding members, so I had no trouble. Until the by-laws were changed, I couldn't get John H. Dingle, or Thomas

Francis Jr., or Colin MacLeod, or any of those people because they didn't have this particular title of consultant.

This record that we have before us shows that I was president of the Army Epidemiological Board, as it was called, from July, 1946 to July, 1947. I got separated from the Army officially in May, 1946. I terminated my military status in September, early September, when my one hundred and eighteen days were up. I went to try to pick up things in New Haven, and I see that one of the things that came back to me--was rejuvenated, I'll say--was Director of the Commission on Epidemiological Survey which is the same thing I had in 1941, when it started. There was a blank thereafter from 1948 to 1953. When I went to the Joint Administrative Board, apparently it was a little too much extra to try to keep up membership on the Commissions and the board, so there was a blank in there.

We didn't want to go any further than that because things get a little complicated. The note I read this morning was on a citation for a medal. It listed these dates and said, in effect, that you were in this capacity as consultant to the Secretary of War and you were president of the reorganized board after World War II. Now the "reorganized board" I don't know anything about--we have clusters of papers over here, the history from those who participated during the war. Did anything happen to the board when it was reorganized after the war from the point of view of procedure?

It was reorganized because at the end of the war the Surgeon General appointed a research board--Research and Development Board. Did you find anything about that in the papers? Where did you say it was mentioned?

It's mentioned here. "The recommendations in this report have not been acted upon. The Research and Development Board...."

That Research and Development Board is in the Department of Defense. General--well, I have his name. [Brig. Gen. William A. Borden], I didn't expect to get these boards criss-crossed like this today. The Surgeon General also had a Research and Development Board even before the war, and he revised it in line with the Research and Development Boards that were being created in the Department of the Army and the Department of Defense. General Prentiss, a very nice man, a man who did not have compelling ambitions to make all people alike, took over in the Surgeon General's Office and did have some effect on the procedures of the Army Epidemiological Board and how it was organized. For a year or two, about 1946, at the end of the war, Prentiss was doing things to draw the Army Epidemiological Board into the scope and control of his Research and Development Board, and we opposed that. He was followed by this man we have mentioned before who assured me that he didn't have any idea of changing the board too much, and that's Colonel William S. Stone whose papers we looked over one day. But there were no real changes of any consequence at that time.

The change that is coming we'll talk about maybe another time when I get you the Charter, the change to the tri-service function of the board. It got a Charter under Dr. MacLeod's guidance from the Department of Defense which made it the Armed Forces Epidemiological Board. It was to be regarded as a board serviceable to the Army, Navy, and the Air Force. Its executive secretary was appointed for a term of five years and to be successively an Army Medical Officer, a Navy Medical Officer, and Air Force Officer and around and around like that. The Charter tells about the primary interest of the Armed Forces Epidemiological Board in Preventive Medicine. The Board started in Preventive Medicine. It still is. Although it is the Armed Forces Epidemiological Board, it's interested in a great many things, and it is still concerned with aiding the Surgeon General to form policy for the control of communicable disease. It deals with immunization procedures. It deals with all the etiological factors of any disease that is communicable including now accident prevention.

There's an epidemiology of accidents which is very interesting and developed by John Gordon, an epidemiologist at Harvard, who was a Preventive Medicine Officer of the European Theater of Operations under General Hawley. It's amazing to see the epidemiology of non-contagious conditions coming to the fore more and more. There's an epidemiology of accidents. They can treat apparently any set of human experiences from an epidemiological and ecological point of view, so there is a Commission on Accidental Trauma which is a very important thing to the Army because now in peace time in the Army the deaths from automobile accidents exceed anything else. It now exceeds the death from aviation training in peace time, very important for loss of time, expense, and disability. It's a long story, and we're getting somewhere with it, but there's a crisscross of interested agencies in the Defense Department and parts of the Army are influencing congressmen to even pass a rule that the Army Medical Service must not make any contributions to the expenses of a commission dealing with accidents among troops.

The charter also makes the Armed Forces Epidemiological Board the tri-service organization, motivated and functioning as if the unification of the services was, in fact, a matter of existence and not a matter of desire, but--and I mean this but this time--the Secretary of the Army is made the managing agent of the Armed Forces Epidemiological Board, and the Army through the Surgeon General's Office pays all the expenses of the several million dollars a year of the investigations conducted by the commissions of this Board. One of the troubles the Board is having is that the Air Force has never put up more than a few nickels, the Navy hardly anything at all, though they are getting extremely good service out of the Board.

I think myself that it was a mistake, in a way, to make this Board a servant of three masters. It functioned extremely well when it had only a loyalty to the Army and to the Surgeon General. Now it's supposed to be loyal to three Surgeons General, and it is--it does a lot of work for the Navy and Air Force, and it knows that it doesn't get much, if any, support in finances from them. There are constant difficulties arranging for those things, and the Navy said that this Charter doesn't compel them to put up any money, that the Secretary of the Army is the managing agent, and management must be coupled with the burden of expense. I think psychologically however, the mere fact that it's got a three headed authority to serve has diluted its loyalties. I've told them many times that I think they would have been better off if they'd stayed an arm of the Army. Nevertheless, it's increased greatly in its monetary allotments, and it's become involved in an enormous proliferation of research agencies--I'd just say in the Army alone--tremendous.

<u>I was thinking of this critical year--or what appears to me to be a critical year, the one following the termination of hostilities of World War II. In your first chapter in Preventive Medicine, in reading it you run across a crisis to which there is a response of research--you have the yellow fever commission as an example. You always face a period of non-crises where medical research sort of goes back into an indistinguishable mass until some-</u>

thing new in the way of crisis comes along on the horizon to which it must respond. Here it would appear that in 1946-1947, with the termination of hostilities there was an effort to keep this research stable and ongoing and prevent its falling back into a sort of morass.

In the history of investigative boards in the Army, it was perfectly apparent that advances have been made in time of war. That is certainly true of the boards that Sternberg put up--the Walter Reed Board, the board to study tropical diseases in the Philippines and Panama following the Spanish American War. They, however, didn't go down to nothing right after those wars. The Philippine Board stayed a good many years, and the Board in Panama was functioning as late as 1938.

What I meant was that concern with continuity is, I think, more apparent as a consequence of World War II than it would be, let's say, in the Spanish-American War. Sternberg may have wanted continuity of research in the sense that malaria, yellow fever, tropical disease is important even in 1903, and he should have had access to continuing knowledge and research, but here you must have faced the problem whether war time research would recede and how to preserve it--Prentiss, for example.

One worry was how to preserve the interest of the directors and members of the commissions. I kept asking what we were going to substitute for patriotism when the war was over. The closer they got to the end, the more they began to think of returning to their natural academic pursuits without having obligations to get work done for urgent military situations. Holding them together was made possible by the continuation of appropriations for the Board, and appropriations increased. Fortunately we were able to ride on the waves of aroused interest in scientific research in the whole country. During World War II was the beginning of the great burst of government supported research in the United States, in the Army, Navy, and the Public Health Service, and in civilian agencies too--Atomic Energy Commission. Certainly the research effort was not at all demobilized the way the military effort was demobilized. The military in characteristic Anglo-Saxon fashion just dropped down to bare bones, or dropped from a giant to a dwarf.

You said earlier that you resisted efforts on the part of the Research and Development Boards, which were sprknging up as a kind of tent, to submerge the Army Epidemiological Board under some new heading, to preserve its continuity, or preserve its momentum, if possible, and the role and function which it supplied. After you cut off connections with the Army Epidemiological Board and were President of the Joint Administrative Board, the Korean War came on--you know, and while that war was something where a debate raged as to whether it was wise, or whether it wasn't wise, nonetheless we were there, present--and performing. We didn't talk about the effect it may, or may not have had on the workings of the New York Hospital, or the medical scientific manpower, or having this Army Epidemiological Board preserved in such fashion that it could be useful and function. Sternberg, I think, would have wanted the same thing.

The Army Epidemiological Board was not called on very much in the Korean period.

It wasn't.

No, the Walter Reed Army Medical Center developed a great research capacity of its own. There was some use of a few members of a few commissions, particularly the one on Enteric Infections because dysentery and diarrhea were so prevalent particularly among prisoners of war on an island called Koje-do. That's where General Dean was pulled into the compound by the communists, the North Koreans, the prisoners on that island. Do you remember that? No, it wasn't General Dean. General Dean was captured north of Pusan. Another general down there [Brig. Gen. Francis T. Dodd], ~~thought~~ he'd walk into the compound and have a talk with these men. They pulled him in and kept him a prisoner within the compound themselves with the other prisoners for a while. The dysentery was very bad. One member of the Commision on Enteric Infections of the Board was sent over--Dr. Albert V. Hardy. He worked with Colonel Richard P. Mason on dysentery on Koje-do. Did you ever read about Koje-do?

No, but I did bump into it in something in the papers this morning.

The other main mysterious disease in Korea was hemorrhagic fever, and that was studied largely by members of the Army Medical Center. Another great research institution that had been formed over there was the 405 General Medical Laboratory in Japan which is very good.

Let's call a halt today and get back into the basis for this a little bit better.

I'll get a copy of the Charter of the Board.

Wednesday, July 20, 1966 A-60, N. L. M.

In order to clarify your relations, both formal and informal, with the Surgeon General's Office in the Army, I think it's important for you to put in at least some indication--we were discussing this before we turned the machine on--the kind of relationship you'd had with the Surgeon General's Office during the period of the Joint Administrative Board of the New York Hospital-Cornell Medical Center. I think this will clarify the relationship and provide a bridge for us to leave New York City and think in terms of the Research and Development Board that had emerged in the Army.

When I went out of the Army in September, 1946, and moved some things to New Haven, I was uncertain as to whether or not I would return permanently to New Haven. My formal connection with the Army Epidemiological Board and its commissions ceased in the late spring of 1947. This is not to say that I didn't know what they were doing. I had many opportunities to talk with the board and commissions, but I had no more responsibilities relative to the activities of the board and the commissions. Nevertheless, I did have a formal and continuing contact with the Office of the Surgeon General through the Research and Development Division from 1946, until an indefinite time approaching the end of my term as President of the Joint Administrative Board in 1953. This contact with the Research and Development Division was in the form of two contracts for the production of two histories. One was to be a History of the Army Epidemiological Board, and the other one was to be a History of the United States of America Typhus Commission. I agreed to write these histories, and I took with me to New Haven, and also brought to New York, files of records of the Typhus Commission and some files of the

Army Epidemiological Board. These contracts provided for some secretarial assistance and travel expenses occasionally and incidental expenses connected with the work. The contracts didn't include emolument for me as a producer of the histories. They were administered in the Surgeon General's Office by Colonel William S. Stone in Research and Development. Although they were contracts for work in Army Medical History, they were not at that time connected with the Historical Division, except through the personal associations that I had with Colonel Joseph H. McNinch, or Colonel Calvin H. Goddard, or people in the Historical Division because I used to go in there and get out some of the records that I needed for the production of these histories. They were especially helpful to me, especially a lady named Mrs. Josephine P. Kyle.

Well, I worked spasmodically on those histories and produced two rather long outlines of what they were going to be. I think you've found them in these papers. Colonel Stone's remark about the Typhus Commission, as I remember, was that the outline was longer than the expected history. That was rather an exaggeration because I finally wrote for Volume VII of the History of Preventive Medicine Series in 1955, an account of typhus fever in the Army and in the world which is essentially the history of the United States Typhus Commission. [It was published in 1964] That's much longer than the pages of my abstract. How many printed pages is it? That's it in your hand. Well, altogether this one including epidemic typhus and murine typhus is about a hundred printed pages. That publication meets my obligation to produce a history of the Typhus Commission, but as yet I have not written a history of the Army Epidemiological Board. I'm planning to put in Volume I of the Preventive Medicine Series that I'm now writing a considerable section on the Army Epidemiological Board which will be a condensed history of it. The contracts helped me to get the files in order, especially the files of the United States of America Typhus Commission because all those files are well arranged and in order and have been put in the Archives Section of the Historical Unit of the Army Medical Service. Each one of those thick files has a table of contents in front of it, dated and with a short account of what's in it, a one line account.

The other relations I had during this period with the Surgeon General's Office were informal, personal, administrative and professional. For instance, in 1950, to work on the material for the history of the Army Epidemiological Board, I came to Washington and took a room at the Hay Adams House. I had my files there such as I needed, and I stayed a month. I worked a great deal in the Surgeon General's Historical Unit in the main Navy Building at that time, and I saw a considerable amount of the affairs of the Surgeon General's Office that were in process. This was about the beginning of the Korean War. The Surgeon General then was Major General Raymond W. Bliss whom I had known for a long time. The Deputy Surgeon General whom I had known for a long time, was a long time friend, Major General George E. Armstrong who succeeded General Bliss as Surgeon General and after that went in to a job at the New York University-Bellevue Medical Center which was very much like the job I had as President of the Joint Administrative Board. We had many associations of a personal nature through those activities.

I didn't become very deeply involved with anything to do with the Korean War--they talked to me a good deal about things that were going on, but that was too early for the emergence of the kinds of problems that would call for epidemiological surveys and epidemiological board type work. However, as the Korean War went on, I knew of several of their main problems and had something to do with discussions of how they would attack them. One of the main problems was the blood substitute problem. What could you use in place of jaundice producing plasma, for example? So they developed dextran. Well, dextran is a carbohydrate [polysaccharide] with which I was familiar because at the New York Hospital-Cornell Medical Center, one of the bacteriologists had been able to isolate this substance from a peculiar streptococcus, and it was a valuable addition to the knowledge of dextran. Blood substitutes were interesting for discussion and for practical purposes and, as I say, I had some talks about them, although I had no official connection with the Office of the Surgeon General in handling the problem.

One problem that arose in Korea that required Epidemiological Board type of work was dysentery among the Korean prisoners of war, particularly on the island of Koje-do. The Board, particularly its Commission on Enteric Infections, actually helped out in the investigation of that. The other problem was hemmorrhagic fever, a peculiar disease that occurred in a region north of Seoul mostly, usually fatal, often fatal, which was a mystery. I mention these things to indicate that I was not during this period entirely separated from the Surgeon General's Office, or the Board.

I knew a good many people in the various divisions of the Surgeon General's Office, particularly in Preventive Medicine, where my friend, Colonel Tom F. Whayne was head of Preventive Medicine. He'd been with us in the war at one time. Arthur Long succeeded him there in Preventive Medicine. In the Historical Division I knew the successive chiefs of that--Colonel Joseph Hamilton McNinch, Colonel Calvin H. Goddard, and a good many people on the staff. I had long time friends among the clerical people that I'd see in the Surgeon General's Office whenever I was down there--like Miss Omar Short who really goes back to service under my relative, Colonel Gorgas, in the Record Room. The Chief of the Finance Division in the Surgeon General's Office, Mr. Nephthune Fogelberg was a friend and a man I saw very often. The Chief of the Medical Statistics Division, Mr. Eugene L. Hamilton was one we consulted frequently in connection with these histories. So even though I was away and not officially connected, I really saw a fair amount of what was going on in the Surgeon General's Office.

As the time of my retirement from the Joint Administrative Board approached; namely, the end of June in 1953, in my 65th year when according to the order of appointment I was due for retirement, I naturally began to think about the future--what would a man do at sixty-five when he retired from his job? I had never been sick. I was healthy and able to go on working. I didn't know. I had no particular plans until May, 1952, when very unexpectedly Colonel, later Brigadier General, John R. Wood, wrote me a letter offering me the position of Technical Director of Research and in the Research and Development Division, as it was called at that time, in the Office of the Surgeon General of the Army. Colonel Wood knew that I was obligated for practically full time work at the New York Hospital-Cornell Medical Center until I retired, but he did suggest that if there was anything I could do on a part time basis, it would be acceptable, or at least would make an acceptable arrangement. The correspondence which we have reviewed shows that the idea of offering me this job was put in the mind of Colonel Wood by my old time teacher and friend, Dr. Milton C. Winternitz, who was then Director of the Childs Fund, succeeding me, and he was also connected with some continuing work at the National Research Council. He was in and out of Washington; in fact, he took an apartment here in Washington, and he and his wife moved down here for the NRC work. Apparently Dr. Winternitz thought I could do the job in the Surgeon General's Office as Technical Director of Research, and that it would be possible to make a part time arrangement covering the period from May, 1952, until I could come to Washington, say after July 1, 1953.

After thinking that over, and of course talking it over very carefully with Mrs. Bayne-Jones, I decided that it was just the thing I wanted to do, a very interesting and unexpected pleasure. She was pleased with it too because she was from Baltimore and she had friends in Washington and looked forward to coming to Washington with much interest. The proposal for part time work for the remainder of the time--the last half of 1952, and the first half of 1953, was not frightening because, as I say, I knew all these people, and they knew that I wouldn't be pushed around to do things that were not possible. The vacancy had occurred by the tragic death of Dr. Francis Gilman Blake whom we have mentioned often as one of the founders and first President of the Army Epidemiological Board. Dr. Blake had moved in to the Research and Development Division in the Office of the Surgeon General early in 1952, to be the Technical Director of Research. Unhappily he died suddenly in the office, or shortly after he had a heart attack which occurred in the office, and I think he died within a few hours. It began to frighten me somewhat because Dr. Blake and I had leap frogged, or followed each other. He followed me as Dean of the Yale Medical School and I followed him as President of the Army Epidemiological Board. He then became Technical Director of Research in the Office of the Surgeon General, and he died, and I was asked to follow him. I thought that perhaps this was the moment to put a termination to this following. Don't you think so?

It does give you pause. Was this a recent office that had been established?

Dr. Blake was the first Technical Director of Research, and as a matter of fact the Research and Development Division as constituted at that time had a name that had been hallowed by time and protected by neglect for many, many years. At this time, however, they really wanted to make a Research and Development Division because there was a great upsurge of research of all kinds in the country, in the Army, in the government, the Department of Defense, and there was a great desire for the Surgeon General to continue the kind of research that had been done particularly by Preventive Medicine on communicable diseases during the war. The plans were to continue the Army Epidemiological Board which came under the Surgeon General's Research and Development Division and escaped being included in the Research and Development Board that Colonel Roger G. Prentiss had gone into which was largely controlled from on high and outside.

The future looked very good and interesting as outlined in this letter of May 29, from Colonel Wood to me in which he tells me about what the functions, or what he calls the "position description" for the Technical Director of Research. What he actually told me was that I could name my own ticket which is probably what he meant by this long page of words. I came down in 1952 and in the first half of 1953, about once a month, would spend a day or two, mostly talking. I carried on some correspondence about the work from New York, but not very much. It was, as I say, sort of a godsend, an unexpected thing.

How much influence in its development did the Korean War have?

In my opinion the Korean War had no influence on this development. It was already underway, so to speak, in the office from the time World War II ended. This was a carry-forward of what Preventive Medicine had been doing all through the war. I don't believe that there was any real break. I'm not sure when this Research and Development Board and Division was appointed.

I don't know either.

It wouldn't be in that list I gave you because--I could go and get that.

Why don't you.

Hold it then. Maybe it's in there.

From a list that had been compiled for me by Major Albert C. Riggs Jr. several years ago, I find that on 17 August 1945, the Army Medical Research and Development Board in the Office of the Surgeon General was established, and at that time it was under the command of Colonel Roger G. Prentiss who is listed as chairman. On July 8, 1946, this board had the same title, but it passed under the chairmanship of Colonel William S. Stone. Colonel Stone continued as chairman for about a year when Colonel John Don Longfellow took over as chairman. Then in June of 1948, Colonel Stone returned as chairman, and then on 21 August 1950, Colonel John R. Wood was made the chairman of this board, and that's where the connection comes in, but, as you'll see, this was continuous right from the end of World War II on through without any relation to the Korean operation at all.

To go through with the other people who were there during the time I had connection with this Army Medical Research and Development Division, I can recall that Colonel Wood was succeeded by Colonel Richard P. Mason on

1 August 1954, and that Colonel Robert L. Hullinghorst succeeded Colonel Mason on July 1, 1956, and on 23 August 1958, Brigadier General Josph H. McNinch succeeded Colonel Hullinghorst and became Commanding General of the newly established Medical Research and Development Command which was established as a Class II activity under the Surgeon General. The main point of that, aside from the record of the people and the times, was that it was a continuing activity.

<u>That's the folder you let me see before.</u>

Yes, this goes over into the Army. This is outside the Surgeon General. I must put that in another file. That's the Army Scientific Advisory Panel.

<u>This was as a civilian in the Surgeon General's Office.</u>

Yes. I was a civilian. I retired in the grade of Brigadier General.

<u>1950.</u>

Then not long after that--no, I retired as a Brigadier General in December of 1949; as a matter of fact, you have to retire. Something happens when you become sixty. I became sixty in 1948, and I can remember that I got a letter from the Adjutant General on my birthday saying, "Sir: You've reached the age of statutory limit for general officers, and we hereby transfer you to the honorary reserve. Happy Birthday."

They must have a tickler, one of these circular tickler files so that when your birthday is coming up, you get a letter. What I was getting to was not only was I retired in the grade of Brigadier General, but it wasn't so long after that that the Secretary of Defense wanted to have a ready reserve of all able bodied general officers, so he called the chiefs of services and asked to have the retired general officers recommissioned in the reserve. I was slow about doing that. I thought that maybe if I got into that, maybe I'd get into conflict of interest, or I might be called out when I was more interested in working as a civilian in the medical research part of the Surgeon General's Office. I couldn't carry through the plan of remaining aloof because General Armstrong told me, "Go on over to the Adjutant General."

I was the only retired general officer in good physical condition who hadn't gone over and gotten recommissioned in the reserve, so I went over, and I still have a commission in the Reserve, so that's why I have U.S.A.R. after my name--United States Army Reserve. That's gone aside from what we started to talk about.

What did you find when you went down there in 1953--the scope, dimension that the task presented?

I will attempt to answer that question, although it's hard for me to separate activities specifically by years when they've been continuous from one year to another. I don't know of any break, but what I found--I can recall two main things. The program of the Research and Development Division was enlarging all the time, getting more money for research contracts and projects. The staff was greatly enlarged. The Division included a surgical research branch, a medical research branch, and a neuropsychiatric--I mean a psychological research branch under Colonel Charles S. Gersoni. This was very interesting, tackling modern problems of hearing, visual processes, acuity, and sensory phenomena in manners that were new to me and extremely interesting. The visual studies, for example, had to do with depth perception, dimensional perceptions, and those observations, of course, were applied to aiming devices. This psychology branch in the Surgeon

General's Office, while studying the physiology of these aspects of vision, was also helping to make gun sights, reticules for firing tank guns. So much of the Surgeon General's deep physiological work undertaken for scientific and medical purposes actually contributes to the offensive power of the forces.

The other tactile studies were carried on by Professor Frank A. Geldard at the University of Virginia Psychology Laboratory where they were putting a little vibrating pack about the size of a nickel on different parts of the body, and these could be set in vibration by electronic, radio signals so that these little things planted on the shoulder, across the chest, could be given alphabetical designations so that they could send a message that way. The first one that we sent, I remember, was "What hath God wrought!" which was what Alexander Graham Bell, or somebody said. That was done in the basement of this building at the University of Virginia. A soldier was lying on a table in a room somewhat away from the sending device, and he spelled that out—he got that message, "What hath God wrought!", although he was rooms away from where the message was sent, and he heard nothing. It was just little tickling sensations on different parts of the body, labeled alphabetically. The point of getting that kind of work done ~~with~~ some urgency perhaps at that time, did have a relation to the Korean War, although the take off point had occurred in World War II, and that was how to make it safe for mine detectors, men using mine detectors to signal what they found. They do mine detecting in the dark, and they couldn't use flash lights to flash messages. They'd be intercepted, but this was something to help the mine detecting soldier communicate, and it was very interesting. I think it had some practical significance. It's obvious—the importance of studying hearing and the apparatus that's connected with equilibrium and balance and all sorts of things. These are useful to pilots, or people in machines that are going fast, and it's enormously important now in this space business--witness Colonel John Glenn. The method--I'm getting into beyond just this casual part time work now. Do you want to go ahead with that?

Yes.

You asked me what I found there. I haven't got there yet in the real position. Suppose I get there.

All right.

Retirement from the Joint Administrative Board at New York was a very heart rending affair and very pleasant too. It's hard to part with friends. I have very fine statements from my friends about what had been going on in the six years I'd been there. The ending, however, was an enormous feast at the Links Club attended by Mr. Whitney, most of the Board of Governors, some of hte Trustees of Cornell, Cardinal Spellman, members of the faculty, and one of the things I remember--I thought at the time of Dr. Winternitz who had retired from several positions. He told me that it was always good to retire often because they gave you a good dinner when you retired. That's been my experience too.

It was easy to come on down to Washington in early July, 1953. We did it by car and moved temporarily into a most delightful house on Wisconsin Avenue, the Dolly Madison House in the Friendship School region. It was owned by our friend, MRS. GEORGE L. HARRISON FORMERLY Mrs. Cary T. Grayson, the widow of Admiral Grayson. This is a large Georgian House built about 1800, I think, and Dolly Madison used to stay in it, a very handsome dignified place set in grounds of about ten acres right there in the middle of Washington, lawns and terraces in the back sloping down to 37th Street, N.W. Then there was not too much building there. There were box trees that were as big around as a kiosk at a fair, sycamore trees three feet in diameter shedding leaves everywhere, a very pleasant place, a very handsome place. We stayed there nearly a month, and then we rented a house down on 27th Street just below Dumbarton Avenue, just above Rock Creek Park close to the bottom of Rock Creek Park, just above M. Street.

Well, to return to the Surgeon General's Office--it didn't require any holiday to refresh me for the new work because it was familiar work, and I have throughout my life taken very few holidays. I didn't go off on holidays very much. The work in the office--to go back to where I was saying what I found--the division of the office into branches of special designation, of special interest which I have named. In addition, the Army Epidemiological Board as it was called then until October, 1953, was administered from one of the branches of the Research and Development Division. They may have called it the Epidemiology Branch, but the Board had its separate office and management with an executive secretary and administrator and a president who was not in residence. A very able woman is still the mainstay of the daily processes and much of the policies of the Board; namely, Miss Betty Gilbert who is now administrative assistant. The Board finances were held pretty closely under review by the Research and Development Division, and that has been a source of constant trouble. The Board never knew what money it would have to spend in a year. We'd tell them to go ahead and decide on what projects they wanted to support, and the Research and Development Division would try to find the money.

That led to various troubles which we might as well speak about now because they're perennial; namely, the variability in the budget of even such a great organization as the Defense Department and a lesser organization in the Surgeon General's Office. Apparently the higher ups thought nothing of suddenly cutting the budget in half in the middle of a year. It went up and down, over and over again. With a planned thing all of a sudden you'd have to take a million dollars, or more out of the processes. I never knew the art of managing a budget under those situations, and I didn't try to get into that side of it very much, except for my side as Technical Director of Research, to look into the scientific value of the proposals for work, the competence of the people who were going to do it, the evaluation of the possible results from the work, and including, in my functions, the explanation of this to the authorities that had something to do about supporting it; namely, the Chief of the Division and often the Surgeon General himself.

This budgeting up and down the scale of funds was very hard. It could be rather dangerous at times because it was all on an annual basis in the first place, and being on an annual basis the closing and beginning of a fiscal year of appropriations didn't correspond with the calendar year. The Congress often did not make its appropriations that you ought to have ready for the oncoming fiscal year, July 1st of any year--sometimes they lapsed until September, as you know, from the way the Congress functions. Some of the appropriation bills they don't pass until September. Fortunately for the Board's side of the work, the universities carried the expenses for that time and were reimbursed later.

The other kind of work in the Research and Development Division was also contractual, but it didn't go through the Board. It went through those branches that I mentioned with advisory and supervisory committees. Those contract supported things also felt the variations in the budget, but somehow or other they got along. The Division also was supporting the research at the Walter Reed Army Medical Center. That I had little to do with even as Technical Director of Research. It was intimated that that was not my business. The Army Medical Center had a highly competent management and was practically independent of the Research and Development Division in the Surgeon General's Office, although they made catalogue like reports to the Surgeon General. Nevertheless, the Army Medical Center formed an Advisory Scientific Board of which I was a member, and we knew actually what they were doing.

The other important relationship in which there were a number of problems was the relationship with the National Research Council. For some years the Army, Navy, and the Air Force had been paying the National Research Council sums in varying amounts for scientific advice, advisory services. The Navy and the Air Force paid relatively little. The Army was paying about two hundred and fifty thousand dollars a year. When I got to the office, I found that the usual procedure had been for the project officer on the staff of the Research and Development Division to look over the proposals that were coming in and reports and send them routinely over to somebody in the National Research Council for evaluation, comments and advice. That was an easy way to do, but it did not develop a competence in the Office of the Surgeon General's Research section, so one of the things I set out to do was to try to get the branch chiefs to develop a competence to pass judgment on these things and not depend entirely on the National Research Council.

The other difficulty that arose was that the National Research Council, perhaps seeing the rather supine attitude of the branch chiefs and the chief of the division and not being loath to have an importance in an operating field, began to take charge of the supervision and conduct of contract research of the Surgeon General's Research and Development Division, so much so that, for example, in the blood research program which was chiefly concerned with blood substitutes, perhaps some twenty investigators under contract were reporting to the person in the National Research Council who had these contracts under her supervision. This person when I got there was not only receiving the reports directly, in a manner that prevented the Surgeon General's Research Office from seeing them until that person was ready to release them, or to send an abstract, but this person was also promising future contracts to other applicants for financial support.

Gradually Colonel Tyrone E. Huber and I worked hard to develop a competence in the office. We did so. We formed some committees on nutrition, for example, on liver diseases, and on resistance of insects to DDT, a big program--pesticides. We had no large program in malaria at that time, although resistance to antimalarial drugs was beginning to be noticed, and the chemotherapy of malaria was in a stage of revival, after it had lapsed at the end of the war when they thought they had solved all the problems. That field of work was conducted largely by the Walter Reed Army INSTITUTE OF RESEARCH--not much downtown. This kind of work that the Technical Director was doing there was to study all these projects, to write reviews of them, and comment to the project and branch chiefs, but the director himself didn't really direct and didn't go into any experimental research on his own.

<u>Let's stop today--all right?</u>

I think so.

<u>I'm right at the end here.</u>

Thursday, July 21, 1966 A-60, N. L. M.

Yesterday we got you to Washington. In looking through these appointment books, I find you attended all manner of advisory committee meetings--all manner of them. You went to Denver, to Chicago--you were pretty busy. You were also scheduled to go to Fort Bliss, but that was cancelled. This was by way of orientation, I guess, to get some relationship to new things that were being developed. I wanted you to indicate, if you would by way of illustration, some of the things that came before you as idea. As I understand, in part, the job was to advise on new programs, or program. You indicated this morning that there were some distinctions in the total program that you wanted to explain and clarify which we didn't put in yesterday--as between "in-house" and "ex-house" work, to fix more precisely those areas with which you had to deal. I showed you a study yesterday--the Camp Kilmer Study. I found that you went up there a couple of times. I showed you an idea on the ineffective soldier which, I think, demonstrates the broadened thinking within the Army as to what to do with its resources. It's hard to fix this in time, I know, from 1953 to the present. I read a report this morning that's most recent, and it's alive with new ideas, but suppose you begin with this clarification of the "in-house" and the "ex-house" work, and then perhaps if you can come up with some illustrations as to how ideas passed through, it will be helpful.

Well, what I wanted to say was that as the Technical Director of Research I found that the administration of research activities by the Research and Development Division in the Office of the Surgeon General was rather sharply divided into concern with what were called "in-house" operations; namely, the research conducted in the laboratories of the Medical Department, and I can't avoid attaching to the other activities the odoriferous epithet "out-house." We always tried to avoid calling, or contrasting between "in-house" and "out-house." It's difficult to avoid it. I think we had "in-house" and "extramural operations." The "in-house" operations were research projects, for example, conducted at the Walter Reed Army Institute of Research, or at the 406 Medical General Laboratory in Tokyo, or under the encouragement of the Research and Development Division research conducted in the General Hospitals of the Medical Department. That was a relatively new development which we tried to foster because it's good for a hospital to be engaged in some research, even though it isn't too serious, or important. The amounts of money involved were not great; in fact, they were small, but the establishment of these experimental laboratories and the conduct of experimental investigations by members of the staff of the hospital put a new kind of life into their intellectual associations, and it brings forward occasionally some able young medical officers who otherwise would have gone on in clinical routines. That was done at Fort Bliss, at Fitzsimons.

We had a large undertaking at Fitzsimons General Hospital. The Army Medical Nutrition Laboratory was located there and that was strictly an "in-house" operation on a very large scale. In addition, there was a tradition of investigation which was enlivened and enlarged in the 1950s for research on tuberculosis and related problems at that hospital because Fitzsimons General Hospital was the main tuberculosis hospital in the Army. I will digress here a minute to mention more about this Army Medical Nutrition Laboratory. It was originally housed in Chicago in connection with the food processing operations of the Quartermaster General in a building right near the slaughter houses, a rather stinking location but it was fairly productive.

Then it was moved--shortly before I went in; no, after I went in as Technical Director of Research it was moved from Chicago to converted barracks buildings on the grounds of the Fitzsimons General Hospital. It was a great center of nutrition research in the Army and did extraordinarily good work on many phases of the chemistry of foods, chemistry of food additives, accessories like vitamins, and on the function of organs in the body under various nutritional conditions.

One large undertaking there was to study the effect of diets of different kinds on human beings who came into that hospital under the auspices of the Army Medical Nutrition Laboratory as volunteer human subjects for prolonged dietary experimental research. These were religious objectors /Mennonites/ who were under control of the bishop of their district and were handled by him. They were brought into this work through a contract arranged by the Army Medical Research and Development Division and the University of Colorado Medical School in Boulder and Denver, Colorado, through the good offices of the President of the University, Dr. Ward P. Darley. It was not possible for the Army to pay these experimental volunteers directly, so we arranged for a contract with the University of Colorado Medical School by which these volunteers would be regarded as employees of the medical school. They got a regular stipend for their work. I think that is going on at the present time and many, many men have been used for the study of various diets and food effects in various conditions.

A very interesting and important outgrowth of the work of the Army Medical Nutrition Laboratory began in about the second year of the Korean War. The Surgeon General heard that there were nutritional defects in the Korean troops. How great they were was not known, so the Surgeon General through the Research and Development Division and other connections sent over to Korea special investigators one of whom was drawn into this work from the Public Health Service, Dr. William Henry Sebrell, who is now the head of the Nutrition Institute at Columbia--Nutrition Institute having a sort of Yalish background, if I might say, because it happened after the Yale Nutrition Institute was not approved. Other investigators including Dr. Harold R. Sandstead and Major Carl J. Koehn were sent over. They examined these Korean R. O. K. troops, found that they were so malnourished, undernourished, and lacking in proper food that they thought that the Korean Army couldn't sustain a week's fighting if they were pushed. They came back and made their report to the Surgeon General, and I can remember General Hays saying,"This is not only top secret, but it has to be kept under lock for only people who need to know to see it."

That was an alarming situation. Shortly after that the news got around that the Surgeon General had conducted an important investigation in nutrition in Korean troops. Ambassador William Christian Bullitt heard about it on the Island of Taiwan, and he was interested because he had a place on the shore and nearly got shot one evening by a Chinese Nationalist soldier who was patrolling the beach who didn't see very well. It turned out that the man didn't see very well because he had what's known as night blindness, and night blindness is due to a deficiency in vitamin A. It turned out that there was a considerable deficiency in vitamin A in the Chinese Nationalist troops on Taiwan. Immediately a request was made for an investigation, and a non-official investigation was made by Dr. Herbert Pollack, a noted physician and a student of nutrition. As this knowledge spread around that the Surgeon General was interested and had a capacity in this field, requests for nutrition studies became so numerous that the Surgeon General couldn't possibly meet the requirements, or the demands, or the requests.

Dr. Howard T. Karsner, who was the Technical Adviser of Research for the Bureau of Medicine and Surgery in the Navy, a position like the one I had in the Army, and I went to the Assistant Secretary of Defense for Health and Medical Affairs, Dr. Frank B. Berry. We told him about this difficulty, and wondered if the Department of Defense couldn't do something to enlarge the capacities to conduct nutrition investigations with a view to strengthening measures for national security. Dr. Berry did this, and he succeeded in obtaining from the Department of Defense fairly large sums of money for the conduct of nutrition studies primarily among soldiers, primarily among soldiers of armies of countries who were allies of the United States. There was then formed, growing out of this original operation of the Army Medical Nutrition Laboratory, the Interdepartmental Committee on Nutrition for National Defense. It was made an Interdepartmental Committee in the 1950s, I think, certainly by 1954, made an Interdepartmental Committee because it was necessary to bring to bear on these administrative as well as scientific problems, thoughts from the State Department, Department of Agriculture, the Army, Navy and the Air Forces, the Public Health Service, and other governmental agencies. The National Research Council too, non-governmental, but it was very much concerned because they had a big section on nutrition.

The Interdepartmental Committee on Nutrition for National Defense, organized first under the chairmanship of Dr. Sebrell, enlisted the interest of practically all the nutritionists in all the universities in the United States. Some two hundred of them have served. The plan that was devised--and it is still being carried on--was to organize a group of nutrition investigators, clinicians, agronomists, agriculturalists, chemists, economists, into teams, equip them thoroughly, and send them over in a plane to a country to make an investigation primarily of the nutritional state of the troops in an allied country--outside the Iron Curtain, of course. It was realized even before the work was undertaken that you can't know anything about the nutrition in troops unless you know something about the nutrition of the populations from which the troops are drawn. So the study soon expanded to a study of the nutrition, we'll say, of the population of Iran, or Pakistan, or parts of India, or Spain. Over the years since this began, twenty-two countries have been surveyed in this manner. The studies were conducted by these teams in the countries during a period of about ninety days. The United States team was matched by native nutritionists, chemists, officers and other people interested in the subject who would work side by side with the American investigators in the fields, in the clinics, in the laboratories, and learn the methods. At the end of this period, the American group pulled out and left all the equipment as a gift to the country and left a trained group of people, trained in modern methods of nutrition investigation.

This American group would then come back to the United States, make a very thorough statistical analysis of all their findings, and prepare a report of some fifty to a hundred pages. This draft of the report was sent back to the country for review by their officials, State Department, rulers, whatnot, and reviewed in this country. This report contained pages of recommendations as to what should be done to improve the food supply of the country, the quality of the food, to correct conditions of anemia, vitamin deficiencies, that had been found to exist among the people as civilian people as well as among the soldiers. Those recommendations were studied in draft form and finally a final report was put out some of which was published, mostly mimeographed and actually utilized in the countries by their officials to improve their conditions. It was a very intelligent and useful scheme, you see, of doing a piece of work for the benefit of the country while at the same time giving that country a capacity to continue that work with their own people and their own resources.

I won't go into all the difficulties of the funding of this operation. Suffice it to say that the Department of Defense had to drop out of giving much money to it when the Director of the Bureau of the Budget noticed how much the study of civilian conditions was going on. It made them think that it wasn't a proper activity of the Department of Defense to be so much engaged in civilian studies. At a certain period, it was well refinanced through AID partly, and partly by the National Institutes of Health which always had been contributing to the funds and the laboratories, the equipment and information gathering of this Interdepartmental Committee. It is still continuing. I describe it this way, as an instance of how a great thing can grow from a very small little acorn. I think persons from the Army Nutrition Laboratory at Fitzsimons Hospital have worked on nearly every team that has been sent abroad, and have made a big contribution.

It's still referred to as an Interdepartmental Committee.

That was the name of it. It's not called the Interdepartmental Committee for National Defense any more. It is now called the INTERDEPARTMENTAL Committee on Nutrition for National Development because "National Defense" has a war-like connotation.

Is the Department of Defense, the Army still represented?

The Committee is under an executive director who is in one of the sections of the National Institutes of Health. I am a consultant still to the Surgeon General of the Public Health Service for nutrition, particularly in connection with this project. The Army never did have any official executive membership, but people from the Army always attend the meetings, different people from sections of the laboratories and from the Medical and Research Development Command--and Navy people, Air Force people come to the meetings and get the reports. Well, it's another example of how important nutrition became not only for the strength of a fighting force, but for the strengthening of the source from which the force comes.

All sorts of interesting problems of taste and acceptability of foods have come up. For instance, the population and soldiers on Taiwan lack vitamin B and A, B particularly, because they ate mostly polished rice, and the problem came up how could you put these vitamins in polished rice? They did get vitamin cakes, but they were yellow. The additives make their rice look yellow, and they didn't like the yellow look of the rice, but they would eat it.

The other thing that we found on Taiwan was that the sweet potatoes that are abundant on the Island of Taiwan have large amounts of the very vitamins that the people ought to be taking--B group and others, but sweat potatoes are called "pig food", and they wouldn't eat the sweet potatoes. Maybe they've educated them to eat it since then. Food preferences are customary. I told you, I think, earlier how I just can't bear to eat honey.

<u>Yes</u>.

People get conditioned to preferences and dislikes in various ways. Some of them are religious, but some of them are just accidental. Well, I've dealt with the "in-house" and "out-house" problem. I didn't expect to go so far afield as to talk about the Interdepartmental Committee on Nutrition for National Defense, but it belongs in the same picture, the same frame.

The studies at Fitzsimons General Hospital extended beyond nutrition and tuberculosis, although the tuberculosis work was very necessary because tuberculosis material from these large wards goes down into the sewer. You can get tubercle bacilli out of the sewage disposal plants up to a certain point. More than that, however, the virus of poliomyelitis can be gotten out of that sewage even to the last effluent sometimes. It wasn't killed by passage through the ordinary filtration and decomposition processes that go on. I might put this in the record--I suppose it's known--the effluent of the sewage of the Fitzsimons General Hospital was used to water the golf course, and when they found that the virus of poliomyelitis could be obtained occasionally from that sewage effluent that was used to water the golf course, there was a good deal of anxiety because not only were the adults playing golf, but children of the officers were romping around on the fairways. It was also a favorite golfing ground for Dwight D. Eisenhower. You can see how a humble piece of investigation that deals with viruses and sewage can have political import as well as matters of health. We were able to improve the sewage out there sufficiently so that neither tubercle bacilli, nor viruses are coming through any more, but for a period of a couple of years there was much worry. That was carried on by a laboratory, a bacteriological laboratory really, that was part of Fitzsimons Hospital, right next to the Medical Nutrition Laboratory, and it was aided by liaison with the Department of Bacteriology at the University of Colorado Medical School as well as, of course, much consultation across the country. It is perfectly obvious that none of these things that I'm talking about are an isolated affair, or an event. It's part of the whole fabric of life in the country.

Did I have anything else down here?

Yes. There's one laboratory I wish you'd talk about.

The Armored Force Medical Laboratory about which I think we have a considerable amount of material in some of these papers, but it was of great importance and still is. It began in the 1940s really, or 1941, through some suggestions from General Simmons, through very hearty cooperation and understanding by the intelligent surgeon of Fort Knox where the Armored Force Headquarters were at that time, Brigadier General Albert W. Kenner, who almost got to be Surgeon General. General Kenner was the surgeon, however, of the Expeditionary Force of General Patton, landing in Morocco, Casablanca, and he then had a very high position, after the Surgeon General's matter passed by, in the headquarters of public health affairs and medical affairs in SHAEF in the European Theater of Operations. General Kenner's successor, Colonel Thomas J. Hartford was also a backer of preventive medicine ideas and of experimental studies.

The thought that prompted the establishment of this laboratory at Fort Knox was the practical military one of finding out the best conditions for the construction of vehicles, such as tanks, that must be devised according to anthropometric measurements and the physiology of drivers and gunners who were in the tanks. The Armored Force seems to have started by taking a platform, putting a cannon on it, and covering it with steel sheets without thought of how the poor fellow inside was going to move around and actually survive the motions of the tank. I have ridden in a tank and have fired a ninety millimeter gun, a fairly modern tank, toward the end of World War II, and I noticed then some of the smaller things that hadn't yet been attended to. When the ninety millimeter gun goes off the tank sort of rears up on its hind legs and sort of flops a bit, and you get quite shaken up. Well, when you're firing that gun, your chin rests in a cup, and there's a little band in front of your forehead to hold your head steady so that you can sight the gun. When the gun fires that hits you like Cassius Clay. Also on my right hand side was an ammunition rack, just two or three strips of metal bent at right angles to make a sort of open work box. Well, that had a very sharp edge, and the gun recoil shaking the tank, made that ammunition rack quite intimate with my right arm. The temperature in tanks, the ventilation of tanks, the noise, the traumatic experiences of a tank going across rough ground, or going up over a slant and then suddenly pitching forward as they do--all had not been studied at this time.

The Armored Medical Research Laboratory was built in record time--less than a year--a fine building with laboratories in it. Fortunately they were able to get an extremely capable director, a research director, and a capable staff, and they did superior work, far beyond just what I've talked about so far. The work extended to physiological studies, personality studies, and even now if you go and see the laboratory at Fort Knox, the Armored Medical Laboratory, you'll find a sprawling group of barracks about a mile long in one direction and maybe a half mile across on that length, full of laboratories of neurophysiology, a visual laboratory, visual studies, radiation studies, studies of x-ray effects, plus the studies of x-ray mechanism. For instance, out of that laboratory was developed a very small portable x-ray machine that was activated by a small capsule of COBALT, radioactive--I think it's COBALT. I'll look it up to be sure. That machine could be carried around and do x-rays of wounded men on the field.

This laboratory contributed to general medicine, to physiology and to physics--a very good thing.

<u>During this period there were meetings of what was called an Advisory Committee on Psychephysiology out there.</u>

We've spoken of that--psycho-physiology is concerned with reaction times, visual affairs. That psycho-physiology laboratory so improved the reticules used in sighting tank guns that some General down there said that it had paid for itself over and over again by increasing the accuracy of fire. The name of the director should be in this record, and that's Colonel Willard F. Machle. He's still an active investigator. I think he lives in Florida now, a very able person.

<u>There wasn't, I think, a new laboratory established at Natick, Massachusetts.</u>

That's a Quartermaster Laboratory.

<u>Wasn't one of them an Army Medical</u>....

To go back to the Armored Laboratory---I want to mention that it had a jungle room. It had a temperature control room, a big room where you could reduce the temperature to freezing, or you could make it hot and humid like a New Guinea jungle. It was big enough to take a tank and let the tank waddle around in it full of soldiers and see what would happen to them. It also gave you an opportunity to march the soldiers around in various environmental temperatures with all their packs and all their equipment, and to study fatigue. These studies were monitored, the effects were monitored by electrical devices attached to the soldier that would give his heart beat, his respiration, and, much to the disgust and discomfort of the soldier, an electric thermocouple thermometer in the rectum so that you'd know what his temperature was. The soldiers didn't fancy that too much. It was in this jungle room that the atabrine studies that I mentioned once before were carried on, so that it was used not only for simple fatigue studies, but it was also used to study the action of a drug under different environmental temperatures applied to human beings.

At Natick the Quartermaster built a great big series of laboratories among which were physiological laboratories of various kinds, including even larger environmental laboratories such as the Fort Knox one for producing heat and cold in the environment. There's always been a war between the Surgeon General and the Quartermaster General to determine, if possible, under whose jurisdiction these experiments should come. It's the point of view of the Surgeon General that anything concerning a human being is medical. Of course that can't be applied fully. The Quartermaster also has problems that concern human beings very extensively, not only for diet and food, but for clothing. You can't make the proper clothing unless you know something about the physiological effects of the clothing. The Quartermaster also has a large solar laboratory where you get temperatures of seven, or eight thousand degrees from reflections from the sun. Those are necessary--they don't apply that much heat to a man, but they do need to know what radiation will do to a human being. All the Quartermaster problems, most of them, have a medical implication, if you take the point of view that anything that has to do with the physiology of a human being is a medical subject. There are medical officers assigned at Natick, and at the time I was there, they were assigned under the Quartermaster. Since that time, I understand that the

Surgeon General actually has more to say about what goes on in the medical experiments, and there is an arrangement rather like, or even better than the Medical Research Laboratory under the Chemical Warfare Service at Edgewood. Colonel Wood who became General Wood who brought me down here was the head of the Medical Research Laboratory of the Chemical Corps, but that was under the Chief Chemical Officer not under the Surgeon General, although the Surgeon General supplies the medical personnel.

Ground studies became increasingly important.

Climate studies are very important. The ones that seem to me the most difficult and arduous were the ones in arid lands. The Surgeon General and the Army Epidemiological Board usually had somebody studying the effects of radiation and heat out in the Desert Training Center. The cold studies were done mostly in cold places. In maneuvers in Alaska there were many studies of the effect of arctic temperatures on clothing and the ability of men to survive in the cold, or how should you heat the barracks, at what temperature, whether there are layers of temperature in the barracks in the cold--all sorts of problems. Of course, the interest in those studies is not confined to the Army. The Air Force is equally interested in everything I've said, and they have some wonderful laboratories in which these environmental effects are studied. Physiological effects are studied extensively by the Aeronautical Research Laboratories of the Air Forces at various places.

The Navy is intensely interested specially, for example, in conditions produced by the blackout of a ship in the tropics. If you black it out by closing most of the ports, the ventilators are not equal to carrying the load of carbon dioxide and other exhalations of the many men that some of these ships carry, and the temperature goes up astonishingly, and it's humid. I've talked with officers who have been in a blacked-out ship in Singapore, or down in those regions--very, very trying.

I should think so. You mentioned before we turned the machine on today--dust studies in Texas.

Coccidioidomycosis. That was largely in the San Joaquin Valley in California--the dusty airfields. What I think I said about Texas is that there is a sharp line toward the eastern border of Texas where coccidioidomycosis stops and another fungus infection, histoplasmosis takes over, but that's sort of a line of vegetation. It changes from a sort of arid land to a more verdant land. The main studies on coccidioidomycosis were made at the Air Force flying fields in California.

I think that you mentioned this, that since 1953, there were efforts to study coccidioidomycosis in another area because I think that earlier study had drawn lines pretty close to El Paso.

Yes, lots of people are working on it besides Charlie Smith--Chester W. Emmons in the Public Health Service has been at it for years. There are different kinds of fungi. In Arizona they had the necessity for studying it because that particular study involved the possible infringement of the Geneva Convention. The Japanese who were taken out, removed out of California in 1941, were moved into camps in Nevada and in Arizona, and in Arizona they built some hospitals for these men who were really prisoners of war in a way. A good number of them developed coccidioidomycosis in the hospital because they were exposed to the dust laden with the spores of this fungus, or the infective form of the fungus. That, I think, was overcome by moving them out of the hospitals and putting them some place else.

<u>Another thing you indicated before we turned this on was the development and ramifications that come from a man whom you first met at Yale dragging an electric eel by the tail.</u>

I mentioned that because you had said--you used the phrase "lack of continuity" in some of these programs, and I mentioned one that had a long continuity, to my knowledge of over seven years, and that was a contract with Dr. David Nachmansohn, a great biochemist at Columbia University. I met him first when I was a Dean at Yale, and I facetiously said that I saw him coming down Cedar Street leading an electric eel on a string. He wasn't leading the eel. He was carrying the eel. He came to see Dr. John F. Fulton because the shock, the electric discharge from such an eel, is conditioned by an activity of the central nervous system, particularly the passage of an impulse, and a charge can accumulate through the chemical reaction that accompanies the nerve impulse. Nachmansohn did discover that this mediator of the nerve impulse was that it passed what is called a synapse at the end of the nerve. This was a reaction between acetyl choline and choline-esterase, an enzyme that breaks acetyl choline down. He worked on that for years. His marvelous Harvey Lectures are on the subject. He worked in Dr. Fulton's laboratory for a while and then down in Columbia.

In World War II--the end of it--our investigative teams in Germany had discovered, or found that the Germans had a very lethal gas called nerve gas. It has various names. I can't give them all--tabun is one. It was a phosphorylated compound. It produces unconsciousness and convulsions--death.

The 28th of January I thought I ought to seek the aid of an able friend of mine in Washington, Dr. R. Bretney Miller. He kindly came to see me, took my temperature, and found that it was high, and he looked over the records in my book where I had accounts of practically normal temperature every morning and a hundred and two and a hundred and three above in the evening. He couldn't find anything the matter with me except that the fever existed. He called a comsultant, a Dr. W. Dabney Jarman who is interested in the genito-urinary tract, and he couldn't find anything wrong. He called in Dr. Worth Daniels who saw me, however, only after I was admitted to the Washington Medical Center. In the Medical Center, I had all sorts of examinations without being able to provide my friendly physicians with any clue as to what the trouble was. Fortunately, however, Dr. Miller had taken a blood culture when he first saw me, and im two or three days that was reported as positive for this gamma type of streptococcus. He took another one which was also positive in number two sample, and then sent me over to the Johns Hopkins Hospital.

I went to the Johns Hopkins Hospitla in a taxi cab with Mrs. Bayne-Jones carrying the culture of the streptococcus. At Johns Hopkins I was admitted to the Marburg Pavilion, or ward which is the ward for private patients, practically in the same location where I hads erved occasionally as an intern, a substitute intern, in 1913, and 1914. Marburg hadn't changed at all. The only change that had been going on was that they were erecting the Children's Center just outside the wall, and across my window there were rising these enormous steel beams with hammering that was even louder than the machine guns that I had heard during the war. The culture was positive at Johns Hopkins twice so I had four positive blood cultures to my credit. Then a very intelligent Professor of Medicine, a friend of mine named A. McGehee Harvey, took charge of my case and ordered that I lie on my back and have a needle in my vein attached to some rubber tubing which was attached to a glass jar which was attached to a pole about six feet high on the side of the bed filled with a solution of penicillin. I got about ten million units of penicillin a day intravenusly for three weeks or more. That was all right because the blood culture was no longer positive after a few days of that, and I'm regarded as a cured case of trouble that was thought to be quite dangerous. The physicians and the others who were taking care of me were really alarmed, but I thought it was all very amusing. Perhaps I wasn't in the right condition of mind to appreciate it. I told stories and I quizzed the interns on medical histryo, and to my amazement I found that mist of them had never heard of William Henry Welch.

That's a little surprising--even in the condition you were in.

It's astonishing. How in the Johns Hopkins the great founder of the place could have passed out of memory in such a short time as far as the incomig younger people were concerned! I told them a good deal about Dr. Welch and other people. They listened patiently, but apparently they were not too much interested. This was a long and tiresome business, but not uncomfortable, except when the penicillin leaked out of the vein in my arm and caused some irritation. It was uncomfortable when they started to search all through my body for a focus of infection. They couldn't find anything in the kidney, or the lungs, or the liver, or the teeth, or the skin, or anything to be the source. We concluded that the spurce of this infection might have been a dental phrophylaxis where it is known this type of streptococcus can get into the tissues from the scraping of the gums.

I came out of that, came home to Washington, and took about several million units of penicillin per day for the next month. Since then there has been no recurrence at all.

This was only my second serious sickness in my lifetime that I remember. The other one was the infectious hepatitis that I got in 1916, when I was with the 5th Maryland Infantry in camp at Laurel, Maryland. The fever spells that you mentioned in World War I were, I think, what we called trench fever.

During this illness of mine much hardship was produced for Mrs. Payne-Jones. She had the house in Washington to look after. She came over on the bus to see me often, and finally she got a room in the motel across from the Johns Hopkins Hopsital and could stay in Baltimore. It was a blustery season. There were four or five heavy snow storms, ice storms, and it was very difficult travel for her in the bus. Of course, she couldn't see well enough to drive a car. She herself was not feeling well at this time. Actually she had an attack of pain which was hard for people to diagnose, but which she and some others were fairly well convinced indicated gall stones, although the x-rays taken at that time didn't show the stones. Later on at the George Washington University Hospital she very wisely insisted that these x-rays be repeated with what they call soft x-rays, and I think she's responsible for making her own diagnosis of finding gall stones. Then thereafter, she had an operation which relieved her from the attacks, a very skillful operation done by Dr. Vincent M. Iovine at the George Washington University Hospital.

How much this spell of fever going back into August may have influenced my judgment and behavior especially in relation to the troublesome times of the Board on Cancer and Viruses, I don't know. I don't think it did, but I lost about fifty three pounds in weight, and I might have lost some of that weight from what little brain I have--I don't know.

<u>It is a unique occurrence in your life--that's for sure.</u>

Yes, I had never been really sick before.

<u>Let me turn this over, and we'll talk some more about smoking. All right?</u>

Yes.

Thursday, July 28, 1966 A-60, N. L. M.

On September 27, 1962, you received a telephone call from the Surgeon General, Luther Terry, and there was a followup communication, a written communication, to you with respect to this Surgeon General's Advisory Committee on Smoking and Health. So far as I am aware, that is the first beginning for you, though I know that it had a deeper context and perhaps you may want to comment about that, and then go into the work of the committee, and, as you know, my interest, the way--well, as I put it before we turned this machine on, you're primary source material for the manner in which committees function, the way in which a group--in this case, ten able men--have set before them a problem, I gather the scope and dimension of which was not really seen at the time the problem was set, and how this group handled its material, handled its work and discharged its function, the interplay between these people. I don't want to smoke any more of the substance than that. I'll turn you loose on this problem.

As I had long been connected with investigations of cancer, at least since 1937, I was aware during that period of the number of studies, called retrospective studies, which since 1939, had numbered twenty-nine that pointed out the toxic effects of smoking tobacco, particularly cigarettes, and the indication that smoking tobacco was definitely linked with the occurrence of cancer of the lungs, so there was a body of information before scientific and other people a good many years before this committee of the Surgeon General of the Public Health Service was called into existence. There were notable studies in the British Research Council, in Denmark, Sweden, American Cancer Society, American Heart Association, and others pointing out the dangers of smoking tobacco. Before our committee began to work, the British had actually published a bock on this subject called <u>Smoking and Health</u>. We took their title and put it on the name of this committee which was to advise the Surgeon General on smoking and health.

Early in 1954, the tobacco industry research committee was established by representatives of the tobacco manufacturers to sponsor a program of research into questions of tobacco and health. It had a scientific director, a scientific advisory board, and an office for this research committee in New York which was making grants. This was under the guidance of Dr. C. C. Little. They collected an immense amount of information and had a number of studies, and while I cannot say, don't want to say, that they were biased because the scientists observed freely and said what they thought, there was an impression that Dr. Little and his associates were using the Tobacco Research Committee of the tobacco industry as a mechanism for propaganda directed against the idea that tobacco smoke could cause disease and in favor of the fact that smoking was innocuous, or in favor of the sort of advertising used by the tobacco industry.

The Public Health Service became engaged in an appraisal of the data on smoking and health on June, 1956, when the Surgeon General organized a study, an investigation, which reviewed a number of independent studies and had a joint association with the National Cancer Institute, the National Heart Institute, the American Cancer Society, and the American Heart Association. Those groups were impressed with this possibility, or the evidence of the possibility that there was a causal relation between excessive cigarette smoking and cancer of the lung. The report of that study committee impressed the Surgeon General Leroy E. Burney so much that he issued a statement on July 12, 1957, reviewing the matter and declaring, and I'll quote this,"That the Public Health Service feels that the weight of evidence is increasingly pointing in one direction that excessive smoking is one of the causative factors of lung cancer."

That's a sensible statement, but it didn't have enough strength to be influential. It's correct, but less important than it might have been, coming from such a high authority as the Surgeon General of the Public Health Service. It expressed, however, what may be regarded as the official position of the Public Health Service; namely, that cigarette smoking is associated with increased chance of having cancer of the lung. There was no change in that statement of the Public Health Service position from 1957 on, but little was done until about the middle of 1961, when the Presidents of the American Cancer Society, the American Public Health Association, the American Heart Association, and the National Tuberculosis Association sent President John F. Kennedy a pretty strong statement advising him that he ought to form a presidential committee to study the widespread implication of the tobacco smoking problem. The President didn't want to undertake this kind of a study, and I might say here frankly that the high officials of the government dread tampering with an industry that was known to be a four billion a year industry, something like that, employing eight hundred thousand people, pouring tax money into the government and into the states, and having very strong political connections, so I think probably the truth is that the President put this recommendation from these societies and organizations aside.

In April, 1962, after a good deal of other discussion, Surgeon General Luther Terry made a more detailed proposal to the Secretary of Health, Education and Welfare to set up a committee which ultimately was called the Surgeon General's Advisory Committee on Smoking and Health. Well, you see, our committee was not a presidential committee. It was created by the Secretary of HEW on recommendation of the Surgeon General. Dr. Terry outlined what he'd like to have done, new studies of various kinds--an examination of the whole subject. On June 7, 1962, Surgeon General Terry announced that he was establishing an expert committee to undertake a comprehensive review of all data on smoking and health. It is to be noted that he limited this to the comprehensive review of data. The advisory committee was not supposed to conduct research, or to support research in experimental laboratories. I think that was a wise thing because the whole subject is so complex and the time required to obtain knowledge from studies, experimental studies, was so great that it never would be finished in the time available. However, contractual services and study services, for the assistance of the committee, for supplying new data for the committee's consideration was possible, and as I'll point out, or I might as well mention it here, the committee entered into a number of contracts with experts in various parts of the country to produce reports, to make actual pathological studies and in addition without a contract secured the extension and early completion of the extraordinary epidemiological studies that Dr.--at the American Cancer Society.

<u>Are they referred to here?</u>

No, that's not it. I hope you can turn this tape off until I find the name.

<u>All right.</u>

For example, the American Cancer Society altered its own investigative program and did work for the Advisory Committee without any compensation, and this was the work largely done epidemiologically by Dr. Cuyler Edward Hammond who for years has been working on what you call retrospective and prospective studies of very large groups. The retrospective studies are made by taking a large number of histories. Prospective studies take a group and follow the members year after year for a number of years. In Dr. Hammond's group what was underway at this time was a study of two million individuals of different ages, races, sex. He put aside his planned work and put these data from this huge group on his computers in 1962, and supplied the committee with information it could not have received in any other way.

Some people are interested in the early lesions of the lung in smokers. Special studies on that were made for the committee by a group in Buffalo at the cencer establishment there [Roswell Park Memorial Institute], so the committee was not entirely limited to the reviews of published data, or data already in reports, but could collect with the help of other people. As I say, we did not go in to investigative work. Fortunately we did get some direct help through confidential communications from Liggett and Myers. Liggett and Myers at that time were very much interested in the effect of tobacco smoke on the trachea and were developing the filter that went in to the Lark cigarette. Dr. Louis F. Fieser was an adviser to Liggett and Myers in Boston and was able to arrange getting at their material. The committee was a little worried that somehow or other its interest might lead to an investment in Liggett and Myers, but I don't know of any conflict of interest arising, but the stock went up during that period. Those are antecedents and jumping a little ahead to the way the committee dealt with this problem of examining data.

The Surgeon General of the Public Health Service announced about July, 1942, I think, Well, at least he had a meeting around July 27th at which a list of more than a hundred and fifty scientists and physicians working in the field of biology and medicine was compiled. Out of that they were able to select a committee of ten. Contributing to the process of selection was a man who became medical coordinator for the committee; namely, Peter V. V. Hamill, and he also did much in travel, in visits to those whose membership was desired on the committee to explain to them what it was all about and to enlist their interest. He made a great contribution in that part of the process of forming the committee.

It is to be mentioned also that the Surgeon General had announced that this study would be in two phases. Phase I was to be the assessment of the nature and magnitude of the health hazard by this expert advisory committee and to produce a report to be submitted to the Surgeon General, containing evaluations and conclusions. Phase II was to be recommendations for actions on these conclusions, but there were to be no decisions on how phase II would be conducted until phase I was finished. Phase II led, of course, into economic and legal considerations of great magnitude. What would be done would affect the industries, affect part of the national economy, affect international relationship, possibly disturb labor relationships as well as the laboring individuals. It was so important from a governmental standpoint that I doubt whether any clear notion of ever undertaking phase II through this mechanism was envisioned by the Public Health Service.

To undertake phase II you'd have to have an interdepartmental type of committee representing the Departments of Commerce, Labor, Agriculture, certainly the Treasury, and certainly the Office of the President of the United

States itself. It was made perfectly plain to the scientific advisory body now being appointed that it need not concern itself with phase II; in fact, the Surgeon General was very plain and frank and emphatic in telling the members of this committee, after they assembled, that they were not to give any more than incidental attention to the economic aspects of the tobacco problem as contrasted with the scientific aspects of tobacco smoking. Nevertheless, when the committee began to meet it received some reports from the representatives of governmental agencies that had interest in this subject and it also had sitting around the wall behind the table at which the committee members ultimately met representatives of those agencies who were privileged to listen to the discussions of this committee, and who of course would report back to their headquarters. Among those men who were there were representatives of the Office of Science and Technology of the Executive Office of the President. Well, that was a high scientific body. Dr. Kenneth Clark, Dean of the School of Arts and Sciences of the University of Colorado, was quite helpful in some discussions for a while. The other representatives were from the Federal Trade Commission, the Food and Drug Administration, the Department of Agriculture and the Department of Commerce. I think those representatives attended only about three meetings of the committee because we asked that the practice be discontinued. It didn't seem to allow us to have the freedom of discussion that we needed, if we had a group of observers sitting behind us capable of and required to report to outside agencies as to what was afoot, so those gentlemen gracefully accepted the discontinuance of the occasion for their attendance.

Now, shall I talk about the committee itself?

Yes.

We'll deal with the members of this committee alphabetically starting with myself. I was a late comer to this committee, in a sense. Most of the others had received their appointments before mine. Surgeon General Terry, however, wanted me, as he told me, to be a consultant to him and to represent him in a way without any official representation, but to let him have some close connection through a member of the committee. The title given me in addition to the title of member of the committee was special consultant to the committee staff. That was a little embarrassing at first because that statement was interpreted by Dr. Hamill as meaning that I was a staff member under his direction which was not what Surgeon General Terry told me. I was in Washington, of course, and could go in and out of the office, so I received in these first meetings some assignments as though I were a member of the staff. I was able to get Dr. Hamill to excuse me from those assignments because that was not part of my function. Toward the end of the work of this committee, I put in practically full time with the staff in the office getting the report in order.

The next man to mention is Dr. Walter J. Burdette who was head of the Department of Surgery at the Utah University School of Medicine in Salt Lake City. Dr. Burdette is about six feet two inches tall, very lanky, slow of speech, intense, a very scholarly man who was not only a good surgeon, but he was also and is one of the leading animal geneticists in the country. Genetics is his main field. His primary interest in cancer was on a genetic basis--aside from his surgical work and his experimental work. That led to some difficulties later on between Dr. Burdette's point of view and the point of view of Dr. Leonard M. Schuman and others who saw this problem from the epidemiological aspect. Dr. Burdette and I had been friends on an almost affectionate basis since about 1935. He had been the most brilliant student at the University of Texas that they'd had in the history of the place. He'd had nothing but straight A RANKING in everything he took down there. Once when I was in Austin and saw the Vice Chancellor who was particularly interested in medical education, I told him that I had a Dean's choice in the Admissions Committee at the Yale University School of Medicine every now and then, as I have mentioned before, and that I would take anybody that he could send in without any question. After a while when I got back, he wrote me about Walter Burdette and sent me his record, and we admitted him immediately to the Yale Medical School, so that he could go through the Yale Medical School with equally high medical grades, if we gave them. Burdette worked at Yale, and he worked at various places, New Orleans too, and got to be an excellent surgeon and keeping up all the time his profound work on genetics. He published a big book on genetics.

He contributed a great deal to the work of this committee as he was assigned the chapter on Cancer of the Lung particularly. He wrote a hundred pages or more on that chapter, and when it was circulated, it was found not to be satisfactory, so we had a very painful hard time. In other words, he neglected the epidemiological aspects which he didn't know about, and he and Schuman locked horns. Most of the committee were on the side of Schuman. Neither one of those men withdraw easily from a position that he has taken. Sometimes Schuman would disagree with half of a sentence that Burdette had written, and the two would sit down with me in a room, and we'd talk for two or three hours and come out with the same adherence to the wording. It was very difficult at times. Also it was hard on Burdette to have his chapter turned down, and we nearly lost him. He began to think that he should resign because he wasn't contributing anything. Fortunately he was persuaded to stay on, and the chapter in the report on Cancer is a combination of Burdette Schuman and others, and it is excellent. That decision to alter his chapter was made at a part of a very long meeting at the Washington Motel that I mentioned to you at another time.

A most charming and witty and valuable member of the committee to be mentioned next was Professor William G. Cochran who was a Professor of Statistics at Harvard University. His field was mathematical statistics with application to biological problems. He took a rather original and active view of the statistics of the effects of smoking on the human economy and body and wrote the chapter that deals with the statistical treatment in the final report. It's a profound mathematical analysis. It has original mathematical considerations of his own that he introduced into this. He was very faithful in all of his attendance at the committee, and as I say produced this extraordinary chapter on mortality, largely on mortality and populations. The contribution that he made, I think, is probably more impressive than what was brought out about cancer of the lungs following cigarette smoking. He showed that the mortality of smokers, the death rate of smokers--just a second. I'm looking for the figures because I can't remember them. Without getting the figures at the moment--I might lay them in later--the mortality of smokers was many times greater than the mortality of people who didn't smoke, male and female, but particularly noticeable in the males. This is so striking a general effect that the committee thought that perhaps it ought to be emphasized more than it has been.

For example, in the discussion in this final report on excess mortality, the mortality ratio among smokers is very much higher than among the non-smokers, and very large populations were used in this study, figures from large populations. Cochran was a wit, a very nice person to talk with, lively and picturesque. I will say what he said about a part of the report that I'd written that had to do with the difficulties that the committee faced in proceeding on a wide front where its regimental segments had to have an area cleared by another unit on the same line before it could move forward. It was very difficult to arrange any echelons. That held us up for some time and in the preface that I wrote, I brought those facts out, and Cochran said to me,"B-J, reduce the lamentations" which is an illustration of his cheerful point of view. He didn't want to be any cry baby.

The picture does have a sardonic quality about it, doesn't it--the face?

He's not really sardonic, or sarcastic--ironic.

A most interesting, intense member of the committee was Dr. Emmanuel Farber, Chairman of the Department of Pathology of the University of Pittsburgh. He knew a great deal about the pathology of emphysema, irritations of the trachea, the bronchial tree, pulmonary changes and was always insisting on the liberty of the scientist, particularly himself, to reach an independent conclusion. He stood up for the independence of our committees against the threatened inroads of the staff of the Office of the Surgeon General and held a position of genuine independence throughout. He worried us because he was so strict at one time in the interpretation of words that he would hardly admit, even though he saw the results that were attributed to smoking, that you could use the word "cause."

I wrote Chapter 3 of this report called "Criteria for Judgment" which showed that we really meant that smoking was perhaps a primary cause in cancer of the lung, an important cause in emphysema and an important cause perhaps in some others, but that we could well realize the multiplicity of causes that were operative. It was an easy conception for me. If you will remember what I told you about the textbook of bacteriology when I revised it with Dr. Zinsser and not only was able to bring in ideas about bacterial variation and show that the monomorphic conceptions of Koch and others were too strict, but that there were pleomorphisms, in the same way there was a multiple etiology of many diseases. Tubercle bacillus alone doesn't cause tuberculosis. Smoking alone doesn't cause cancer of the lung. You have the genetic constitution and all sorts of things, so that this helped in the "Criteria for Judgment." While not giving away anything at all, and we'll speak of it later, it satisfied Dr. Farber, and he didn't make any objections to signing the report which was a unanimous document. Farber was always cheerful and tense, and forthright.

Dr. Louis Fieser, a member of the committee, was the Professor of Organic Chemistry at Harvard. His field for years had been the chemistry of carcinogenic hydrocarbons, methylcholanthrene, dibenzanthracene, benzpyrine--all that group. I had known him from early days particularly from the beginning of the Childs Fund in 1937, because the Childs Fund was interested in carcinogenisis, and I think that one of our early grants was to Fieser's department. He's a chemist with a very wonderful sense of what they call stereoisomerisms. He saw molecules in shape and forms moving around. He's made a great many models that are so helpful telling where hydrogen and oxygen are in the molecule. He's a great synthetic chemist, and he's probably the greatest organic chemist in the universities of his time. His contribution to the report was the chapter on carcinogenisis, particularly the carcinogenic compounds. In tobacco there may be seven hundred compounds in tobacco smoke. He knew a great deal about the chemistry of that, and some of those compounds in tobacco smoke, particularly the tars, produce cancers in animals regularly when you inject it. Well, what about all the other compounds--I think there are seven hundred of them that he had to list and we talked about--not all in detail.

Fieser was constant in his attendance, but he was also constant in smoking cigarettes. He would smoke about four packs a day, so much so that our committee got a little worried about him and said,"Louis, you'd better cut down a little bit."

He did cut down for a while and stopped the incessant cough he had for a bit, but he went back to it. Just last year, however, he really got reformed. He developed a cancer of the lung. He had an operation. A small cancer of the bronchus was removed, and thereby he'd come down the sawdust trail and was converted. He wrote an account of his case which he distributed widely because he thought that he was fated to be a horrible example that would aid propaganda against smoking. I didn't answer his article or write to him, as some people thought I should, because I don't think, scientist as he is, that he was being entirely objective about it. He had been working with carcinogenic hydrocarbons for thirty or more years. I have been in his laboratory--we'll say that he's working on benzpyrine, and it's still on his desk. He'll put his cigarette down on the desk and pick it up and smoke it and how much benzpyrine he picked up on his cigarette and put in his mouth is not known, but whether it was his smoking or not, he lived in the atmosphere of carcinogenic hydrocarbons and whether that caused his cancer is not entirely clear. However, it was the kind of cancer that is usually seen in the bronchus of heavy smokers.

The next one to speak about is another old friend of mine named Jacob Furth who is a Professor of Pathology at Columbia University and Director of the Pathological Laboratory of the Francis Delafield Hospital--that's the cancer hospital at P & S, corresponding to the Memorial Hospital Center down on 68th Street. The Delafield Hospital had city support mostly, with some outside support. Jacob Furth is a profound student of cellular changes, is an international figure, Austrian by birth, I think, and still has foreign construction of speech and was a little difficult to understand. He was a very valuable member of the committee in bringing critique to bear on pathological grading of tumors that was promoted by Dr. L. Kreyberg of Denmark. He was a very argumentative man, who was difficult to convince and difficult to understand always because of his foreign type of speech. However, he was as agreeable as anybody else on the committee and worked out all right. He made his contribution.

Dr. John B. Hickam who was chairman of the Department of Internal Medicine at the University of Indianapolis, was assigned a very difficult part of our study; namely, what is the effect of tobacco smoking on the heart and the blood vessels. His specialty is cardiovascular physiology. He worked very hard on that without being able to come to a very definite conclusion. If you'll give me a moment, I want to get some words that satisfied him. If you'll turn that off a minute.

It seemed from his studies that there was some connection between cardiovascular disease, but it was not possible to make a flat statement so that the conclusion of his chapter is that male cigarette smokers have a higher death rate from coronary artery disease than non-smokers, males, but it is not clear that this association has causal significance. I think he really thought it did, and he's gotten some more information since this was published. Hickam is a humerous man too, not as sharp as Cochran, but a sparkle, quick to appreciate nuances in speech, followed everything alertly, would not talk very much, but what he said was worth listening to. I think you can see that in his face.

Yes.

The next one to mention is Dr. Charles LeMaistre which you know how to spell.

It's right in front of me.

He was the Professor of Internal Medicine at the Texas Southwest Medical School in Dallas. He had the best pulmonary disease study laboratory and department in the South. His field of interest for the Committee on Smoking and Health was the very important subject of emphysema, a condition in the lungs where the walls between air spaces break down, and a patient becomes very short of breath and finally dies of a kind of suffocation. Now, as Dr. LeMaistre went forward with his studies, it became more and more apparent that chronic bronchitis and cough and so forth was related to cigarette smoking and the emphysema that followed long cigarette smoking was probably causally related. Cigarette smoking seemed to be the main thing. LeMaistre was a very cautious man. He was an enormous help in the expressiveness of the committee, would say nothing for a long time and then he would hand me across the table a written sentence, a piece of paper which said in a few words what people had been struggling to say for the past hour or more. There are men like that at committee meetings, that sit and digest it all and finally often pass it on to somebody--they did to me because I was the sort of unofficial committee chairman, whereas the official chairman was an Assistant Surgeon General of the Public Health Service. LeMaistre was devoted to this work, to the medical work in Texas and, as I say, had a very fine clinic and laboratory for the study of pulmonary disease of some of this type in human beings.

A tragedy came to all of us in the work of this committee, the death, the assassination of President Kennedy on November 22, 1963. LeMaistre came from Dallas, and he was shocked that this happened in Dallas. He was ashamed of Dallas and so was his son. Lots of people were. They really took it very personally. He was quite disturbed at the assassination of President Kennedy. Aside from the general political importance, it involved his relationship to his home. He's gone back to live in Dallas and has now just become a Vice Chancellor for Medical Affairs of the University of Texas. He will pass into administration, and studies of emphysema will lose thereby. See how serious he looks in his picture.

Very sensitive too.

Yes.

A remarkably productive member of the committee that we'll mention next is Dr. Leonard M. Schuman, Professor of Epidemiology at the University of Minnesota School of Public Health, in Minneapolis. He's interested in health in relation to environment, but he's interested in the whole field of epidemiology so that epidemiological studies were assigned to him before the committee itself realized what secepe was involved in that--epidemiology of cancer of the lung, epidemiology of cancer of the lip, mouth, stomach--we went into all of those things. They required a great deal of study of records and data. We were assisted in getting the record by the National Library of Medicine. The director assigned Mr. Charles A. Roos to prepare bibliographies for us. He prepared bibliographies from the recorded literature on cancer and smoking and other environmental things, perhaps six thousand, seven thousand titles. Those were distributed to members of the committee. Members could then ask for a copy of the article, and Mr. Roos would see through the Library's staff that the article was xeroxed. We had a Xerox machine, and although the law said that you should not make more than one copy of a copyrighted article, we made enough copies to distribute to all members of the committee. Schuman probably got the bulk of it because he had to study all these conditions. He was a little slow in getting ahead with this work. He's a man who does about seventeen things at once, but when the pressure was on, he worked hard, at home, at the meetings, and harder still on the telephone. Some days he would call up and dictate the result of his past several days study, and the dictation might take four hours on the phone, so it piled in, and that's one way to get it down and done.

Schuman, a very intelligent man, differed from an equally intelligent man, Burdette on aspects of cancer, particularly with relation to the lung, and I think his point of view won out because he upset Burdette a good deal by bringing forward opinions that belittled the genetic basis of the development of cancer. To Schuman the committee owes much of the material that is in Chapter 3 that I wrote called "Criteria for Judgment." It's not a bad idea, I think, to put in this tape recording what the criteria include and show you that we didn't deal simply with percentage statistics in reaching a conclusion. There are five criteria that are used for judgment. One is called "the consistency of the association." If, we'll say, cancer of the lung follows heavy cigarette smoking over and over and over again in relation to the number of cigarettes and so forth, that's a consistent association. It isn't a proof, but it is one of the things that helps you to interpret. "The strength of the association" is a factorial sort of a matter as to whether the association occurs, we'll say, in 10%, or 100%--a very important thing. "The specificity of the association" was also examined as a criterion for judgment. For instance, the factor, inhalation of cigarette smoke, would bear on specificity of the relationship, or the temperature of the burning of the cigarette, the length of the cigarette--all those are specific factors that had to be studied. "The temporal relationship of the association" means whether it is something that happens quickly, or whether there's a span of time over which the thing occurs. Mostly in cancer the indication period in human beings is about twenty years. How long would you have to smoke before it would start the process in that twenty years? We don't know exactly, but there is a temporal association, and in cigarette smoking it can be shown by pathological studies that if the heavy smoker does stop smoking, the precancerous lesions will disappear provided that they haven't gone too far. Then the other one is "the coherence of the association" which is another way of saying that it should make sense. Well, you see, that it wasn't a study based on rates, or percentages only. Association is used there as a sort of broader term than rates.

Cochran handled the mathematical side of those things but stayed away from Chi Square computations on which people base so much of their opinion as to the validity of a conclusion. Schuman's material had to be worked over a good deal, but it was very valuable and, as I say, it certainly broadened the point of view of the geneticists.

One more man to be mentioned is Dr. Maurice H. Seevers of the Department of Pharmacology at the University of Michigan at Ann Arbor, a square built, positive man who for a long time had been studying pharmacology of various types of addiction. His point of view about smoking was that it was not an addiction. It might be habit forming. It might have some slight withdrawal symptoms, but the smoker is not as dependent on cigarette smoking as a Marijuana addict is, or a man who is addicted to morphine, or alcohol. Seevers guided the committee through this difficult field of addiction, habit of smoking and some studies of the relation of the psychology of individuals as well as psychical structure to their habits of smoking. Seevers was a distinguished man, during the course of the committee work he took a little trip to Japan and got the Order of the Rising Sun. He's later become the chairman of the American Medical Association's Committee on Tobacco and Health. Hickam and Lemaistre are also on that American Medical Association Committee.

After our committee finished its work, the American Medical Association which had been rather conservative in its attitude toward the possibility that smoking would cause disease, and the American Medical Association was sometimes suspected of being subservient to the tobacco industry because pages of the AMA Journal and other journals are lucratively adorned with advertisements provided by the smoking people, particularly the sexy ads that Madison Avenue helped them to put out. After our committee finished, the American Medical Association set up a committee to make grants for studies of tobacco and health and appropriated five million dollars for it. They drew

Hickam, Seevers, and LeMaistre on as members and now lo and behold, I've just accepted an invitation from Dr. Seevers, the chairman of that committee, to be a general chairman of a four day session of that committee and all of the holders of grants under it at Colorado Springs in October. It's interesting that the relationships formed in this committee here in Washington continue in various ways. I don't suppose that they will ever separate us considering what we went through together.

That does what I want to with the members of the committee, and there is a fair amount of information about the work of the committee.

I think there is one area that you might go into, and that's the amplification given to this on the part of the committee--freewheeling discussion.

Yes. I wanted to turn back to that in a little broader way than you said. The Surgeon General Luther Terry was the chairman of this committee. He then delegated his chairmanship, so to speak, to Dr. James M. Hundley who was an Assistant Surgeon General. Dr. Terry appeared, I think, at only two meetings in which he talked generally about the point of view that the committee might have and said repeatedly that no power on earth would make him put pressure on this committee to get its work done in a certain time, or he hoped that it would be done--once they said six months, and then they said a year, and then he tried to drop off any deadline. He repeatedly assured the committee that he didn't expect it to sacrifice scientific thoroughness, completeness and accuracy for the sake of meeting any particular deadline. As I'll point out later, the next sentence gives a little escape from that bold burst for freedom. He said nevertheless the committee appreciates the importance of completing its work in a reasonable time and is following a work schedule which would permit them to meet the target date mentioned earlier. According to Mr. Celebreze /Hon. Anthony J. Celebreze, Secretary, Department of Health, Education, and Welfare/ that target date was by the end of the year 1961--I think, wasn't it? No. 1963. I might as well go on as to what happened over the deadline--along where the President comes in. In September of 1963, President Kennedy was asked at--that ties in with his assassination. I must know the date. Well, President Kennedy was asked....

May 23, 1963.

Was that the date of his assassination?

No, the date when he was asked at his press conference....

May 23, 1968, he was asked at a press conference when the committee would make a report?

Here it is.

Well, May 23, 1963, is a general statement about the general sensitivity of the subject, but later on he was asked in 1963--I'm not sure of the date--when the committee would make the report. He hedged the first time that question was put to him and said, "I'll let you know in about two weeks."

At the end of that time--in two weeks, he said that the committee would report in November which was a great shock. Following that Surgeon General Terry wrote the President a letter saying that the committee couldn't possibly do it in November of 1963, but promising a report by the end of the year. The committee was at that time not consulted by the Surgeon General and was floundering in a morass of detail and difficult things that required decision and didn't see how it could finish its report by the end of the year, 1963. Nevertheless, it did under the vigorous management of a new staff director who had been appointed to take the place of Dr. Hamill; namely, Eugene H. Guthrie. We really worked after he got on there. Sometimes we would have meetings four days long--day long and night long. Dr. Terry also assured us several times that he would not let any pressure bear on us to formulate one kind of opinion rather than another, which he lived up to. He couldn't help himself over this other part.

The influence that came to bear on the committee's actions came through Dr. Hundley who sat at the head of our table as the chairman of our meetings from the start in a rather school masterly manner. He took rather positive stands on administrative matters, but was not well enough informed about the scientific, medical sides of it to participate in the discussion, or to have much influence. He was, of course, from the start very much concerned about the production of the report. So was the committee. So was Dr. Hamill.

What are you looking for?

Hamill's final leave. I would like to say a word or two more about the way the committee progressed through the fields of study that it had outlined should be done. I have indicated already that most studies could not go more than a certain way until a part of the front had been cleared by another section of the committee. The committee dealing with the assessment of carcinogenic actions in material in smoke had to clear its mind over a great deal of conflicting information before it could say anything positively that would be useful to the man, we'll say, who was studying carcinoma of the lung in relation to smoking. Actually there was a great deal of difficult--I'll say, floundering to get by certain points. This was so much apparent that somebody advised Dr. Hamill to accept the assistance of an expert on management from another section of the NIH, a very mild mannered lady. She knew how to draw diagrams and schemes, and she came over and presented to the committee a flow chart that was on paper which stretched across two black boards--beautiful diagrams of subjects moving up until they coalesced in one main conclusive statement. We called it the "laundry list" and didn't pay much attention to it because it didn't fit the situation.

About that time, however, two things had happened. About April 30, 1963, when Dr. Hundley had become considerably worried about the slowness of progress of the committee's deliberations, the committee decided that it would rather have a private talk about its affairs in the absence of Dr. Hundley and asked him if he would kindly not come in to a meeting that the committee wanted to have in privacy. He did this, and the committee did meet, had a closed meeting for an hour or so, decided pretty clearly how it wanted to proceed, and then invited Dr. Hundley to return. I was asked to give Dr. Hundley a summary of what the deliberations had been which was in a way a declaration of independence along the lines of Dr. Farber's reiterated statement that the committee report had to be a report by the committee. Hundley said that if we couldn't do it, he'd have the staff do it. That would have been fatal. Part of the staff of the National Institutes of Health wanted to write parts of this committee report, or might have had a chance to write all of it if Hundley had been followed. That cleared the way though, and it can be said that the report as finally issued is the commitee's report.

Shortly after that--where's the Hamill sickness? Oh yes. Shortly after that, or at least several months after that Dr. Hamill began to be worn out by this curious and difficult working of the committee. He developed symptoms of fatigue and odd pains, so much so that he was rather incapacitated and emotionally disturbed so that on July 31, 1963, Dr. Hundley issued a statement to the committee that it had been necessary to place ~~on~~ Hamill on indefinite convalescent sick leave and that he had been replaced by Dr. Eugene Guthrie. Dr. Eugene Guthrie was a career service officer who was in charge of the Division of Chronic Diseases of the Bureau of State Services, an important thing. Guthrie knew a great deal, was a man of vigor and tact and worked well with the committee.

He looks like a vigorous fellow.

Yes--he is, isn't he.

It's right in the photograph.

Yes, if it hadn't been for Dr. Guthrie, I don't believe that we ever would have gotten the report. He knew what to do, and he could cut through red tape. He even got the Government Printing Office to print sections of this report that would be scattered through the volume later on. The Government Printing Office usually wouldn't touch a manuscript unless it was complete, but we were printing while we were writing. What time is it? I'm getting out of voice, but I'll try to finish. Don't you think this is enough on the committee?

Yes, that's good.

I think I'll wind it up if that's enough on the procedures of the committee--I think I'll wind this up by telling what happened toward the end of 1963, and January of 1964. Is that the press conference?

<u>This I don't know.</u>

We had it. It's right at the top of a page in my handwriting.

<u>Let me turn this off and find it.</u>

The committee's report was printed by the Government Printing Office. It's a document of 387 pages and composed of fifteen chapters with a very impressive list of acknowledgements of people who were helpful to the committee. There must be two hundred names or more on that list--eight printed pages. The report contains an introduction and all the summaries and conclusions are brought together in a chapter, and then the other chapters deal with cancer of the lung, cancer of the--well, I'll just lay in these chapter headings. It will be of some use--maybe. The main chapters were "Consumption of Tobacco Products in the United States"; "The Chemical and Physical Characteristics of Tobacco and Tobacco Smoke"; "Pharmacology and Toxicology of Nicotine"; "Mortality"; "Cancer" and that's chiefly cancer of the lung, but it also took in cancer of the lips and the mouth, stomach and other organs; "Non-Neoplastic Respiratory Diseases, Particularly Chronic Bronchitis and Pulmonary Emphysema"; "Cardiovascular Diseases"; a miscellaneous chapter on "Other Conditions"; "Characterization of the Tobacco Habit and the Beneficial Effects of Tobacco" of which not many could be mentioned; "Psycho-Social Aspects of Smoking" and the "Morphological Constitution of Smokers". All of this is objectively presented in this volume without any propagandizing. No effect is made in here to advise anybody to do anything. It was simply a presentation of the facts thoroughly documented with long bibliographies and tables and chapters supporting the statements that are made.

The report--I may be exaggerating about this--but I think four hundred thousand copies of this report were printed and distributed, some sold and some free. It went all over the world and was thoroughly reviewed in all sorts of journals and articles, lay journals, trade journals, governmental journals, the deliberations of governmental bodies. I think I can say with assurance that nothing in this report has been controverted by the tobacco industry, or any other workers on the subject since it was first put out in 1964. A few questions have been asked, but the conclusions and the presentation have hardly been questioned. It's an honest and objective report. It's main effect was apparently to reduce the consumption of cigarettes for a few months and then to stimulate the consumption of cigarettes afterwards because billions of cigarettes made and smoked have gone up some billions since then.

It was not the concern of the committee to suggest legislation. That's part of phase II, but some legislation was suggested and some has gone into effect. It is required now to put on a cigarette package that it might be a hazard to health--just a general hazard to health. It doesn't mention cancer of the lungs. There have been suggestions--Senator Neuberger and others would bring in rather drastic and restrictive bills. Nothing like that has happened yet. The British have gone further than we have in this country. They have actually put up some penalties and have done some things to reduce cigarette smoking.

This report was presented in a big press conference. It was presented to the Surgeon General of the Public Health Service in a big press conference in the large auditorium of the United States State Department. All members of the committee were presented The room was full of reporters, observers, and visitors, and the committee sat down in the row of chairs on the big stage.

Surgeon General Terry presided at the lectern. He made statements and then questions were asked by people in the audience, and he would call on some member of the committee who seemed competent to answer. It lasted about an hour. It was not unfriendly--mostly an informational affair. After that the committee was taken by the Surgeon General to the very pleasant dining room of the Officer's Club of Fort McNair where it had a delicious luncheon and a few little speeches.

Was the committee discharged with the presentation of its report?

Yes, the committee was discharged with the presentation of its report, and another group came on whose name I'll have to get from downstairs on C Level. /National Interagency Council on Smoking and Health/ They are situated there on C Level now. They have been collecting information and having correspondence about cigarette smoking, keeping up with the literature and issuing a little propaganda and perhaps trying to influence some legislation. They are still working, but they have no organic association with the previous advisory committee, although they do call in for consultation some of its members from time to time. Surgeon General William H. Stewart, the new Surgeon General of the Public Health Service, seems to be a little more emphatic as to what the Public Health Service might do in an educational manner in support of studies at various places than Dr. Terry was. Dr. Terry's decision for his activities was that the best thing to be done was to put on an educational program--rather hopeless for the Surgeon General to move the government to do anything that interferes with a large segment of the gross national product.

It's very difficult for anyone going into the field to erase this report as though it didn't exist.

No. They can't do that. I just noticed that this report doesn't have any date. Did you notice that?

Yes.

No printing date. Yes--1964. You can always find out on the last page. Well, that's it.

You're exhausted.

Yes.

I have only one more thing for you to do--that's written. The next thing following the transcript I'd like for you to comment on the process having been a subject in an experiment, and part of the story the transcript will tell won't be available in the transcript, but available in terms of the experience we've had as to how we've gone about what it is we've done.

You mentioned that the other day.