

Dr. Nathan A. Womack

discusses

Dr. Evarts A. Graham

and his contributions and influence on

American surgery

Table of Contents

Introduction	i
Transcript of Interview	1-48
Index	49-52

Introduction

Nathan Anthony Womack was born in Reidsville, North Carolina in 1901. He attended the Washington University School of Medicine in St. Louis, graduating in 1924. His graduate surgical training was all in the Department of Surgery at this institution with Dr. Evarts A. Graham, and he remained on the staff until 1948 when he accepted the position of Professor and Head of the Department of Surgery at the University of Iowa. In 1951 he returned to his native state as Professor and Head of the Department of Surgery of the University of North Carolina, Chapel Hill. Since 1967 he has been Professor and Chairman Emeritus at North Carolina.

Dr. Womack was selected for this interview because he was a long-standing and close associate of Dr. Graham. The interview was held on December 9, 1971 in Dr. Womack's office at the University Hospital in Chapel Hill. He was most cooperative and provided an interesting commentary on Dr. Graham's contributions and influence on surgery.

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The date is December 9, 1971. This is Dr. Peter Olch, of the National Library of Medicine. I am visiting in the office of Dr. Nathan Womack, of the University of North Carolina, Department of Surgery.

Dr. W.: Well, I might begin with some generalizations, and one of these is that this conversation is being recorded about fifty years after I first knew Evarts Graham and, therefore, will be filled with inaccuracies relating to the distortions of memory and distortions of prejudice, because he was one of my heroes.

I might try to put myself back into the spirit of the time. He went to Washington University, as I recall, about 1919, and I went there in 1922 as a junior medical student. I immediately came in contact with him, and I saw something that I had never seen before--never heard of before--and that was an application of surgery to the correction of physiologic abnormalities in the body. As the years have gone by, I think I would say that this is Dr. Graham's greatest contribution to surgery because, in so doing this, it was necessary for him to approach surgery by experimentation of a physiologic sort. He is probably going to be remembered as the man who first removed an entire lung successfully. I think this is a bit unfortunate because it was not such a great feat. Others would have done it shortly thereafter and, as a matter of fact, Rienhoff, not knowing that Graham had done this, removed a lung a month or so after Graham.

Dr. O.: Yes, William Rienhoff from Baltimore.

Dr. W.: Incidentally, your father, Yale, was the one who made the official diagnosis of cancer on this patient that had the cancer of the lung--we were both in the operating room at the time and, as I recall, Dr. Graham intended to do a lobectomy when he went in. It was only when he found that a lobectomy would not remove the tumor that he decided to do the total pneumonectomy.

Dr. O.: Yes, this is the story he relates in that film made in '53 by the American College of Surgeons.

Dr. W.: Oh, is that so? I'm glad because I never have seen this. There's one thing I hope he did put in there, but I bet he didn't because in the excitement he would have forgotten it. We were all cognizant of the fact that an embolus to a major pulmonary artery--let's say the right pulmonary artery--will sometimes kill a person. What would happen if he ligated the right pulmonary artery? Physiologically, it brings up a fascinating problem that I quite frequently ask our residents today. If you ligate the right pulmonary artery, does twice the blood flow go through the left lung? Does the cardiac output remain constant or is it decreased fifty percent? I'm surprised how few people have given this any thought--who'll go ahead and ligate indiscriminately either the major right or left pulmonary artery. Well, back to the story. He put his finger around the right pulmonary artery momentarily, and nothing happened. He then held the artery occluded for about a minute--nothing happened. Then he put a ligature around it and watched for

about fifteen or twenty minutes and nothing happened. Mind you, this patient was getting ether at the time, and gas. He then removed the lung by ligating the entire base of the lung--the bronchi, the vessels, everything--with a ligature of heavy silk, and literally amputated the lung, and this was the method that was used for a number of patients. No individual dissection at all of the various structures was carried out. This came on a little bit later.

Dr. O.: I was going to ask that.

Dr. W.: Fascinating how few of them developed empyema. Then another question came up on this first patient. It was interesting how these would be argued there in the operating room because he had a lot of good critics around him. Should he do a thoracoplasty? What would happen if he left this entire space to fill up with fluid? Would it become infected? Now, he decided to do a thoracoplasty, and he did everything except the first rib. This was fortuitous because this patient did develop empyema and I'm quite sure that this came from a blown-out or necrotic bronchial stump. With a full pleural cavity open, the patient would probably have died. Later on, obviously, he gave up the thoracoplasty, as did everybody. The first time, however, I think it was a fortunate thing to do. Well, so much for his being remembered by a pneumonectomy. This, of course, has happened to so many surgeons.

Dr. O.: I think you're so absolutely right.

Dr. W.: Billroth is an example of a great surgeon being known chiefly for an operation. Billroth, the genius that he was, to be known by an operation on the stomach is rather odd. I would say, then, if I come back to Graham's greatest contribution to surgery, it was the introduction of chemistry and physiology into the experimental laboratory of surgery.

Dr. O.: Really, in many more ways than Halsted; we could get into quite an argument with some of the Hopkins' people, but I think that this is true.

Dr. W.: Much more than Halsted. Oh yes, Halsted had an experimental laboratory, and a good one, the Hunterian Laboratory, but if you go back over the work that came out of the Hunterian up until 1920, it was, by and large, anatomical. I am thinking of Graham's approach as a physiologic and biochemical and pathologic contribution. I say this because Graham's mentor, so to speak, was a man we hear so little about at the present time, Rollin Woodyatt, of Chicago. Certainly, in this country, Woodyatt introduced the experimental laboratory into medicine--internal medicine. Graham, I think, due to his association with Woodyatt, did this in surgery. You'll get this, I think, if you go back over his education; it was such a strange education for a surgeon as we see such today--a year or so with Woodyatt in chemistry, a year with H. G. Wells in pathology-----

Dr. O.: When he did the chloroform studies on the liver of pups.

Dr. W.: Chloroform in the dog, yes. A. J. Carlson. Carlson once told me that he thought Graham was his greatest student. There was Sippy. He spent one or two years with Sippy in internal medicine. Of the surgery he had, I suppose the greatest influence would have been his father, although Bevan was important. He only had about a year of surgery at the Presbyterian Hospital in Chicago, I recall. I think he learned a lot from Bevan in terms of personality. Bevan was a two-fisted fighter for what he thought was right, and Graham was that way, too, I think.

Well, when World War I came on and the influenza-empyema problem was so terrific, Graham was about the only surgeon available who had had an experimental background, and he was placed on a commission, an empyema commission along with R. D. Bell, in particular, a ^{Pathologist} ~~chemist~~. It was their experimental work, I think, on pulmonary pressures, mediastinal shift, and so forth, that led the way to safety in thoracic surgery. This work of the empyema commission obviously had its immediate impact on the reduction of operations that produced open pneumothorax in people with pneumonia and empyema, and a lot of lives were saved by an appreciation of the suffocation that comes with mediastinal shift. It earmarked Graham, I think, in the sense that he was placed on the list of many schools to consider as head of a department of surgery when such a position became available, but he couldn't wait; he had to support a family. He moved out to a little town in Iowa-----

Dr. O.: Mason City.

Dr. W.: Mason City. This was a real experience for him. I think he came, for the first time, face to face with fee-splitting and second-rate surgery. He couldn't tolerate either and, I might say, the people there couldn't tolerate him either.

I might tell a little story here. I went to the University of Iowa as head of Surgery back in '48. I'd been there only about two weeks, at which time I received a phone call from a doctor in a little town near Mason City asking me if I would talk to the County Medical Society the following week. This was an order. I knew I had to do it, and so I told him I would be happy to. I went up to this town and found that the meeting was in the Elk's Club. Nobody met me. I didn't know anybody there at all. I was having a drink at a bar they had there in the Club, and a fellow, whom I subsequently came to know as my host, came up to me and called me aside and introduced himself. I won't give you his name because, while he is dead, his son is quite active and a very good friend of mine. He said, "I think it's only fair to say to you as you start out here in Iowa City, that we're prejudiced against you on two accounts. One is, you are taking the job of one of our very good friends." I said, "Of course, I know that, and I tried my darndest to get him not to resign." "All right," he said, "the next is that you have had your surgical training with a man we consider the world's biggest son-of-a-bitch." "Well," I said, "that's the most fascinating thing I know of because when I left St. Louis, Dr. Graham said, 'Now,

I want you to watch out for so and so, and so and so, and so and so. They are the big bastards in the State of Iowa at the present time'."

I said, "Doctor, your name was on the list!" Graham, of course, had never said such. I made the story up right on the spur of the moment. He started laughing and he said, "You know, I'll bet you and I are going to get along all right together." I said, "All right, but no fee-splitting; I'll fight you on it." He said, "Okay." Well, it so happened that this fellow, a month or two later, was elected President of the State Medical Society. He came down to Iowa City to see me and he said, "I'm going to do something to show you my interest in this University. Here is the list of committee members that I'm going to appoint. I want you to go over them and see if there is a single man appointed to a committee that is an enemy of this medical school. I don't want our State Society to fight this medical school anymore; we've done it enough."

Dr. O.: That is a refreshing story to hear, I might add.

Dr. W.: This fellow became one of my closest friends, as is his son. We never saw eye to eye on fee-splitting, but I could see other qualities about him. I like this story because, although he couldn't tolerate Graham, he had a tremendous respect for Graham's integrity and I think that's what led to this thing.

Dr. O.: I find it very interesting that it's one of the few instances one hears of a true meeting of town and gown. Here's town coming and

saying, "Now, I don't want anyone on this committee that's going to fight the medical school." This is unique.

Dr. W.: Well, it's worked out that way, and I think it's still that way. They have been very, very close together.

Well, back to Graham. Philip Shaffer, of Washington University in St. Louis, had Graham on his list for a long time, and when Dr. Murphy resigned as head of the Department of Surgery at the end of World War I, Shaffer offered Graham the Chairmanship of the Department of Surgery and he accepted. Philip Shaffer, I might say in passing, was really one of the great figures in medical education. Almost single-handedly, he appointed the faculty at Washington University for many years and he picked great ones. Shaffer appointed Joe Erlanger, Herbert Gasser, Carl Cori, Gertie Cori, four Nobel laureates and two others that became Nobel laureates later--all of them appointed in their early thirties. The man had a most amazing capacity to tell what a guy was going to amount to when he was fifty years old by looking at him when he was thirty.

Dr. O.: That's an art.

Dr. W.: And he did this with Graham. I've chatted with Shaffer about this on a number of occasions, and he played this almost like a man at a racetrack; he went on performance. He never paid the slightest attention to a recommendation. He would read what a person had written and he would analyze the man's mind in that way. He had Carl Cori, for instance, over for a talk once. Carl, at that time, was at the

New York Cancer Hospital at Buffalo; few had ever heard of him. He spoke very broken English; he and Gerti had come to this country only a year or two earlier from Hungary. Carl gave us a talk on the action of adrenalin on glucose-glycogen metabolism in the liver. After the talk, I saw Shaffer and he said, "What do you think of this man succeeding Herbert Gasser?" "Well," I said, "I don't know. All I know about him is just this talk and I thought it was very good." "Well," he said, "he's written about three papers that are worthwhile. Let me give you one of them." We went to his office where he had the journal and I read it. Here was a man who not only had a brilliant idea, but also had worked out the analytical methods, and in developing this idea had come to a good conclusion. Shaffer said to me, "There's a good mind. You can't stop it. I'm going to make Cori the next Professor of Pharmacology."

And this was also the way he selected Evarts Graham. He went over the published papers. We can't do that now. Papers are published by committees. You don't know whose mind is at play, but where just one person wrote a paper, you could, and this was the thing that brought Graham to St. Louis. When Graham came to St. Louis, he had inherited something that I don't think has been really appreciated.

Dr. O.: Are you referring to the conflict with Dr. Sachs?

Dr. W.: Well, that came on later. Sachs was tremendously disappointed that he didn't get the job, but, nevertheless, Graham respected Sachs;

literally, the first Professor of Neurosurgery in this country, the first neurosurgeon west of the Mississippi River. In Sachs he inherited a man who was about to develop neurosurgery in the western part of America. He also inherited Vilray Blair who was going to develop plastic surgery, along with others like Barrett Brown and Bill Byars. In orthopedics, just a year or two later, came Roy Abbott and Albert Key. In urology, John Caulk was there who was just beginning transurethral surgery. In general surgery was Barney Brooks and there were many others. Here, then, was a group of specialists that served as a backbone for a department of general surgery. This hit me acutely when I opened up the Department here at the University of North Carolina. I suddenly realized that a department of general surgery was just the beginning. Where are you going to find that otolaryngologist, that anesthesiologist, etc.? And how are you going to determine whether they're good or not, and you've got just a few months to make this selection and not much money with which to do it? And so you might say, then, that Graham walked into a strong department, one which was just built, to introduce what he had in mind. As I recall, his first work at Washington University was the demonstration that cholecystitis was primarily a bacterial infection, bacteria from the intestinal tract that produced hepatitis and the gallbladder was infected through extension of the organisms from the liver by the lymphatics, as sometimes was the pancreas. He did this by injecting streptococci into the dog's intestinal tract and recovering the streptococci in the liver and in the gallbladder. This work I never was

quite willing to accept, and I'll go into that again in just a moment, but it was the first problem he started working on with the use of the bacteriologic laboratory and the use of the pathologic laboratory.

This interest in the gallbladder again, I think, brought out perhaps Graham's greatest single surgical contribution; that is, cholecystography. He recalled some work of Dr. Leonard Rowntree at Johns Hopkins, about 1915, when Rowntree was working with Abel in pharmacology on the action of phenolphthalein. At this time, Rowntree showed that most phenolphthalein was eliminated in the bile. When he attached sulphur to the phthalein molecule, it was eliminated in the urine, about 90 percent of it was, and he and Dr. John Geraghty came out with a functional test for the kidney, the PSP (phenolsulfonphthalein) test. It was the first chemical test of organ function that I know of that was introduced in clinical medicine. We had to do PSP's on every patient. It impressed Graham very much. He hit upon the idea of attaching some radiopaque radicals on the phthalein and seeing if he could obtain a radiogram of the liver as the bile came through. Knowing his chemistry, he hit on the halogens, and he used iodine which was the heaviest halogen. He found that there were four sites for its attachment on the phthalein radical. He then synthesized phenoltetraiodophthalein. It's a very toxic drug. At first, it killed most of the dogs. Warren Cole joined him in this project and, working with the Malinkrodt Chemical Company, they finally came out with a product that would work. The story is old and I won't repeat it.

Dr. O.: May I just ask you what role you feel Dr. Helen Graham had in this work with her background in chemistry and pharmacology?

Dr. W.: One will never know; one can only surmise. But knowing them both, and knowing Helen very well, I would say that she probably played a dominant role. Actually, she had a much keener mind than did Evarts. She had a much more original mind than did Evarts, and she had a technical ability that was evidenced in her own experimental work, such as her method of microdetermination of histamine. She was the perfect wife for a professor. She knew exactly how to stay in the background. She would never go to a meeting with Dr. Graham. She told me once she heard him present a paper only twice in her life. She was never there. This question, therefore, of who did what, you will have to surmise, but I would put her right up to the front.

Cole, of course, deserves a lot of credit in developing this idea and making it work. Suddenly thrown into the surgical eye was an entirely different type of approach to surgery, and I think this attracted the attention of a number of bright, young minds. Your dad was one of them. He was a pathologist. He was going into pathology as a career. He suddenly was attracted to surgery and I think it was this intellectual approach that did it. It brought men from all over the country, and from England and from Scotland. I think it did another thing also and that has to do with the character of the organization of many departments of surgery in this country. Here again, it may be my prejudice at play. Mind you, this was around 1924. That period of '24,

'25, '26 saw the establishment of several new departments of surgery and the reconstruction of several old ones. Old ones were reconstructed, let us say, like Minnesota. New ones were started, such as Rochester, Western Reserve, and the University of Chicago. We would be visited by the incoming department chairman who would stay with us a week, two weeks, three weeks--watching how the place was organized.

Dr. O.: I've heard Dr. Wangensteen speak of this very thing.

Dr. W.: That's where I first got to know Owen. I think I was an assistant resident at the time. He stayed with us perhaps a month and we became very good friends. Almost immediately, then, this concept of a surgical department was national, and I think this is the reason that nobody, except the few that are still alive--like Owen, can point back and say, "I think here's an idea that Graham put across." But it has had a tremendous influence on surgical education throughout the world, and I would say far supersedes any single experimental or clinical contribution of Graham's. I think, also, that Graham would be a little bit surprised to hear one say this, because such was so ingrained in his approach toward surgery that he never recognized it. His wife did, and in a letter to her some years ago--about a year before she died--I wrote her about this and she wrote me back--and this is so typical of her--she said, "I can think of nothing that would have pleased Evarts more than to have read that letter that you wrote, but I can also say that if he had read it he would write you back and say 'give Rollin Woodyatt some credit'." That's one of the reasons I

mentioned Woodyatt, I think, in the beginning. Well, the rest of this cholecystography story is more or less commonplace. Here was a brilliant contribution in that it was an idea synthesized out of the blue. There were very few references; the only references that one could have on this sort of thing would be Rowntree's.

[End of Side I, Reel 1]

As is so often the case, cholecystography became the focal point for a great development.

[Side II, Reel 1]

Dr. W.: This represented a unique intellectual concept to which good young men were attracted, and these young men had ideas of their own. Pretty soon, there was a lot of cross-fertilization going on, and the department branched out. It was one of the interesting things to me how much was done there during this period that has now been more or less forgotten. We had, then, a little organization called the Barnes Hospital Society. Once a month it met, and a clinical department would have the program--different departments each month. They would have to present an experimental paper and two short clinical case reports. I recall vividly--I think it was in April or May of 1924--two papers presented from the Department of Surgery, one by Graham and Cole on cholecystography and another by Barney Brooks on angiography. Brooks' work has been forgotten. Brooks was ahead of his time because he had to use sodium iodide as his contrast agent and this was so painful, so irritating that the patient had to be put to sleep for the procedure.

Then he ran into the "so-what" attitude on the part of the public. When he demonstrated an arterial block, there was nothing one could do about it. He was too far ahead of vascular surgery, and so angiography died and was reborn a decade or so later. I tell this story because it illustrates a spirit, a spirit of exploration. I wish I knew better how to develop it in young men. Only yesterday, I was making rounds with one of our assistant residents and there was a patient that was getting so-called extra-alimentary feeding, a high protein diet given intravenously, along with sugar, and I asked him if he knew who started this. He referred me to several recent papers. I said, "Have you ever heard of Dr. Robert Elman?" No, he never had. I told him of Elman's early work that made the intravenous administration of amino acids possible. It became obvious to me that he wasn't interested, after we'd been talking a little bit, because he was interested in the material that he was using now. The Amigen that Elman used had too much sodium in it, and this prohibited its use in this particular patient. The fact of the pioneering work on making the intravenous administration of amino acids safe was of no concern. Here again, I think we run into the problem of young fellows interested in agents, and not principles and the historical background in the development of the principles. But Robert Elman was one of the very good ones along the lines I am talking about. The introduction of a method for the determination of amylase in blood was Robert Elman's.

Yale Olch took over surgical pathology when Barney Brooks left, and he introduced a surgical pathology conference. This idea spread all

over the country. Brooks put it in down at Vanderbilt. Ned Lehman put it in when he went to the University of Virginia. Alan Whipple introduced it at P and S. Alex Brunswig was the one who ran it for the University of Chicago. Like Graham's introduction of the laboratory, this thing within a year or two was all over the country, and it has played a very important role in the education of the surgeon in pathology. Unfortunately, it is almost coming to an end because of the aggressiveness of the departments of pathology. Since hospitals now not only require a pathological examination of all tissues but also many, many frozen sections (there's gold in them thar hills), one might suspect a financial motive. Now surgical departments have lost control of surgical pathology. As a result, too many surgeons have lost their interest and their knowledge of morbid anatomy.

Also most departments of pathology with which I'm acquainted in this country have ceased to stress a knowledge of morbid anatomy, but there is money to be made in clinical pathology and so the surgeon now is being pushed away from it. This contribution that Yale made, I think, is falling by the wayside. It illustrates, however, the catholicism of the Department of Surgery that Graham started.

Graham was an interesting fellow in his tenacity. If he believed something, by golly, it was difficult to change him.

Dr. O.: So I gather.

Dr. W.: I mentioned a little while ago his interest in the bacteriologic

origin of cholecystitis. Well, in the early thirties or middle thirties, I started playing around with that. I came to the conclusion that cholecystitis was an obstructive disease and that the infectious role was secondary following the obstruction. Now, as was so typical of Graham, he financed this work. The Department of Surgery had no money. This was before there was any N.I.H. We had a little bit from the Rockefeller Foundation, but most of the Department of Surgery was financed by the earnings in private practice of Graham. He was willing to finance this work of mine, although it was contrary to his belief. But every time I would present a paper, he would manage to be in the audience, and the moment I would sit down, he would get up, and from then on we weren't friends. It worked out all right. We had a deep affection for each other, and we knew how to separate our hearts from our heads. Everyone had a deep respect for Graham so it didn't hurt him, and I had the data and it didn't hurt me! But this kept up for about two years and finally one day, Dr. Boyd came out with a new edition of his Surgical Pathology, and I noticed in it that he had a statement that the previous concept of cholecystitis as being primarily bacterial in origin seems to be difficult to justify now in view of more recent experimental work on the presence of obstruction in the cystic duct, usually due to stones. I laid the book on Dr. Graham's desk--he was out of his office--and opened it to this page and walked out. I didn't say a word. Never again did he refer to the subject. The moment that Boyd had put it on the printed page, he was ready to withdraw. But he was a good contender. I think I still look

back with delight on the luncheons we would have together, oh, about three or four times a week, if both of us were in town, from about 1935 until I left for Iowa. Here, I think I saw Graham at his bigness. He was a well-educated man. Graham recognized the value of the humanities in medical education, and seldom were our conversations limited to medicine. Tremendous interest, too, he had in international affairs and politics and social development. He could be described as a moderate liberal, I would say. He was a pragmatist in his liberalism. He would not get so far in front that he could not carry through with what he believed. Being from the South, of course, I was just good ammunition for him. We had an excellent relationship there, and I suppose I learned more about the basic factors in education from him during these periods than any other time. He was a stickler for proper English usage. He had a Webster's Unabridged in his office and he insisted that each of us have one. As you see there, I still have one. He wanted that rather than the Oxford, and that and Fowler were the arbiters of our arguments. We always gave him our papers to go over.

Dr. O.: This was even after you had completed the residency?

Dr. W.: Oh, yes. Even when I was a professor there, I turned my papers over to him. This was for two reasons. One is, he wanted to see our thinking on the thing so he could talk with us about it; and number two, he just wanted to correct the English. One day, he handed a manuscript back to me and said, "I don't like your usage of this word." I said, "I know you don't but I think it's good usage." He said, "If you don't

mind, let's change it." He said, "Why is it that nobody has to correct my papers?" I said, "You never turn them over to us." He said, "Well, God dammit, here's a copy of a manuscript. I'm leaving right now to give it at Princeton the day after tomorrow. You read it over and see if you find an error in it." With delight, I took that manuscript home that night and I never worked so hard in all my life, but I did run across one little error, and that was a dangling participle. I immediately went to the Western Union Office and sent him a wire--I knew the hotel where he was staying. My wire went something like this--"For God's sake, don't dangle the participle in the first paragraph on top of Page 8 before the faculty of Princeton University," and I signed it "Galsworthy." He never said a word. Never heard from him at all. But I knew he was burned up. (Laughter) It was a very important relationship as you can see, and I think you can see why I started out in the beginning by saying that I would be prejudiced in some of the things that I said.

Beginning, I suppose, with the time that Graham was elected President of the American Surgical Association, he became much more involved in national organizations and in national surgical education. This evidenced itself first with the American Board of Surgeons. Perhaps he was the dominant character in putting this Board across. I went on the Board to succeed him; that was four years afterward, which would be 1941. That was close enough to appreciate what he had contributed, and he contributed in a big way.

Dr. O.: Of course, even before this, he had taken on the College of Surgeons in '33.

Dr. W.: Yes, his later interest in the College of Surgeons was a switch around of about 180 degrees. He was opposed to the College of Surgeons, not perhaps to the College as much as he was to two or three people in the College. One of his bête noires was J. B. Murphy. There, again, was a Chicago influence. He, mind you, was with the Bevan group, and he had very little tolerance for the Murphy group. This carried on to some of the people that succeeded Murphy. Graham had really a strange fondness for many of these people he fought so. One day, one of them sent him a letter--Graham showed it to me. It said, "Dear Evarts, I have just had an annual physical examination done by Joe Capps (an internist in Chicago). Joe says I am in perfect shape and should live many, many more years. I thought you would be the first who would love to know this."

Dr. O.: That's Franklin Martin, I imagine.

Dr. W.: That was Franklin Martin. And I think, therefore, that that exchange of letters would not have taken place between two people who somehow or other in their makeup didn't have a little bit of affection for each other. Neither one would ever admit it. It was after Franklin Martin's death that I think Evarts saw that something constructive could be done for the College of Surgeons, and then he got interested in it. I think something has been done in the College of Surgeons, now a very responsible organization. It was not just Graham, but so many,

many others, and particularly the group in charge right now, who have done so well. Again, I think one must be very careful not to give a single man too much credit because there were several contemporaries that molded a new era in surgery. These were all close friends.

One was Dallas Phemister at Chicago; another was Alan Whipple at P and S; Harvey Stone over at Baltimore; Fred Coller at Michigan; and Sam Harvey played a role in New Haven, although Sam was never much of an organizational man. He did a lot of excellent work with the American Board of Surgery, but Sam was never particularly interested in the College of Surgeons. He was President of the American Surgical Association. These men all had the same concepts of surgery, the basic background that I mentioned earlier that concerned Evarts. They had something else so badly needed at that time; they had integrity. They took an aggressive specialty that was not too proud of its excellence in terms of accomplishments, overcompensated in fees, that ran local organizations with sometimes a bit of despotism, and converted it into a specialty that we are now so proud of. All of these men and others played a part. I doubt if you can separate the contribution of one from the other because they were such good friends. To have sat in on a dinner, and I had the privilege many times with this group that I just mentioned, along with people like Stewart Rodman, Fred Rankin, and Owen Wangenstein; and to hear Harvey Stone, for instance, or Sam Harvey tie into the American College of Surgeons and listen to Evarts and Fred Coller defend the institution, was really a great experience.

I think these arguments perhaps forced some changes in the American College of Surgeons.

Dr. O.: This would have been in the forties perhaps, when Dr. Graham had been President and then was on the Board of Regents.

Dr. W.: The late forties. This was after World War II--'46 and '47 particularly. These men knew their abilities. They knew their accomplishments. They were not going to let the achievements in American surgery be harmed. The American College of Surgeons couldn't have helped itself from becoming big. It had to be dedicated to the best in American surgery. These men would not have tolerated it otherwise. Just let the American College of Surgeons make one error, and such a group would be right down the throats of the members of the Board of Regents. So much of this was done in small, informal group meetings, such as the one I mentioned a moment ago. It had kept all organizations in line, and this is where the deep integrity--I'm not talking about the integrity of stealing or anything of the sort, but an integrity that was much more subtle--came into play. "That you cannot do to education; you cannot do this to a university; we will not permit it!" This informal guidance helped make American surgery great.

Dr. O.: While you're on this theme, would you say that there was a period of time--that period you were referring to, possibly from the late thirties into the mid-forties or thereabouts--that you could almost pick half a dozen to a dozen people in this country who, because

of their close relationship and friendship, and because of being able to sit down like this, were very instrumental in molding and changing surgery? In other words, were things small enough at that time that these powerful figures who had this integrity--like Whipple, Graham, Harvey, and so on--could control the direction of American surgery?

Dr. W.: I think there's no doubt about it, Peter, I think there's no doubt about it.

Dr. O.: I think this is a very interesting thought. I've certainly never seen it expressed before.

Dr. W.: You see, up until Graham's period at Washington University, there were no full-time clinical professors. I once wrote an editorial on it and looked up the data--I couldn't find this editorial the other day; it was in the Annals of Surgery some twenty-five years ago.

Dr. O.: Halsted became straight full-time in 1914.

Dr. W.: Halsted would do private surgery, though not much. Halsted would not give up his freedom of accepting a fee from a private patient. While I don't have this editorial with the references, I think I'm correct on this. At any rate, who's first doesn't make any difference. The Rockefeller Foundation sponsored it. The idea was that up until this period, the head of a department of surgery was usually the man in the community that had the biggest practice and the biggest influence, and therefore his influences would be local. Here, a group of people was developed within just a decade or two that had different loyalties.

These loyalties made it possible for them to spend their energies on ideals. While they were most practical, pragmatic people, they were really idealists in the long run. And I think we owe them a great debt. They exerted a great influence on professional appointments.

Let me tell this anecdote to illustrate. An organization that you and I belong to called the Halsted Society, at one time was called the Halsted Club. It had a very small membership--30 or so men. We were meeting in Cincinnati--I guess this would be 1939, I won't be far off--and it was just at this time Dr. Dean Lewis had retired as head of Surgery at Hopkins and they were looking for a successor. One night--it must have been around two o'clock--a group of us was sitting around at some club in Cincinnati; Mont Reid was the host, and as you will recall, Mont was Halsted's favorite resident, and Halsted to Mont was a religion.

Dr. O.: Oh, that's beautifully expressed--I love that.

Dr. W.: Mont said, "Look, you guys express yourselves here." There were perhaps six of us at the table. He said, "They are looking for a successor to the Professor this Club's named after. Whom should Hopkins get as Chairman of the Department?" One member of the group, "Chubby" Grant (Francis C. Grant), who was head of neurosurgery at Pennsylvania, and who had an incisive tongue at times, although a very fine character and an extremely able surgeon, said, "Mont, I've heard your name mentioned, and if it's offered you, I hope you don't take it. The reason is that you're too old; you are burned out. You cannot go

to Hopkins and spend ten years building up a department. You've got to think not only of the kudos that come to you with the offering, but also your obligations to Hopkins." Mont was such a sweet person that I think everyone of us at the table resented that remark. I know I did, and I fussed with "Chubby" about it for a long, long time. He didn't mean it to be malicious. It hurt Mont. Another guy who was there and who I thought was asleep--he had his head down on the table--was Claude Beck, who recently died. Claude had thought of himself being head of the Department of Surgery at Western Reserve, and in this period right after the depression in which Western Reserve was broke, the job was given to someone else. Beck roused from his pseudo-slumber and said, 'Mont, I would agree with "Chubby" but for different reasons.'" He said, "I think you should turn it down, too, but turn it down because the only thing that any university can offer you are facilities. This is a weird thing to say but I've had my Garden of Gethsemane, as you well know, and I have had to rationalize things. I found that I had been given all the facilities that I needed at Western Reserve, and what I did with those facilities is all I could do with similar facilities anywhere in the world." He said, "You have been given all the facilities you need to build a Department of Surgery at Cincinnati. You've built a great one. I think you've built one of the great ones in the world. If you go to Johns Hopkins, you will have similar opportunities; you will build a similar department. It will take you ten years. Don't do it. You already have it. Let them copy after you." That was on a Friday night; I caught an early

plane into St. Louis and it was a rainy Saturday afternoon. I remember it well. I got a phone call from Dr. Graham. I knew at the time that he had been offered the job as head of surgery at Johns Hopkins, but it had never been made public. I knew he hadn't made up his mind. He called me up and said he wanted to talk to me about something. I went over to his office and he said, "As you perhaps know, I've been offered the Chair of Surgery at Hopkins." I said, "Yes, I've heard it." He said, "What should I do about it?" I said, "Hell, why ask me? You know the situation here as well as I do, and the future of this place. You know the situation at Hopkins better than I do; you've got the facts; you make up your own mind." He said, "Yes, I know, but what should I do?" "Oh," I said, "do anything you want to, but I can tell you this. When you are ready to step down at the Johns Hopkins, you'll look around you, and you'll see several institutions that you could not quite equal, and one is the one you've left behind." I said, "The reason for that is the group in the specialty services (a thing I mentioned to you a little bit earlier). You cannot put a Wilray Blair in charge of plastic surgery at Hopkins, and so forth, and therefore you are not quite equal with the place you left for it." I said, "What do you want here anyway?" Well, at that time, he was not on speaking terms with the Chairman of the Board of Directors of Barnes Hospital.

Dr. O.: Johnson?

Dr. W.: No, Rand. They had, at one time, been very close friends and it was a silly thing that separated them and it should never have

happened. Then, he told me what he wanted and I knew that he practically had that right then, and I said, "Would you mind very much if I call up Mr. Rand (whom I knew quite well) and ask him if he would entertain supplying you with these things?"

[End of Side II, Reel 1]

[Side I, Reel 2] December 9, 1972 (contd.)

Dr. W.: I then said to him, "Now, before you express an opinion here, I want to tell you a story." And I told him this little incident that had happened the night before in Cincinnati. He didn't say anything. I then called up Mr. Rand, and Mr. Rand said, "Why, these are very simple things you are asking for. Of course, they can be granted. I'll put it in a letter and send it out this afternoon to Dr. Graham by messenger." Which he did. Graham said, "All right, let's call this one settled then. Now, do you think Mont will take the job at Hopkins?" I said, "It will be my guess that Mont won't after last night." He said, "I think he should be offered it." I said, "I think this means too much to Mont not to have the opportunity to refuse the offer. It won't have to be made public and it won't hurt Johns Hopkins." He said, "All right, who do you think is the man?" I said, "You know as well as I do. To begin with, Hopkins must have as a Chairman of the Department of Surgery a Johns Hopkins' man. You see that the previous man just couldn't turn the job. This Johns Hopkins' impregnation that gets into a guy is so deep that no outsider can come in and run that institution, and so the one man available for this job, and who is

young enough, is Alfred Blalock. I think Alfred would do it well."

Dr. Graham called up Mont--I was there at the time--and he said, "Mont, I've been offered the job as Chairman of the Department of Surgery at Johns Hopkins and I'm turning it down and I'd like to recommend you; would you take it?" And Mont said, "No." And Evarts said, "Well, I think we ought to recommend someone if you're not going to take it. What do you think of Alfred Blalock?" Mont said, "I think he's perfect." Evarts then called up Dr. Isaiah Bowman, the President of Johns Hopkins, refused the job, and a meeting was set with Evarts and Mont and Dr. Bowman. I think it was to be in Chicago a day or so later. After the conference, the President of Hopkins flew on down to Nashville and went directly to Alfred, and Alfred accepted it. Dr. Bowman went back to Hopkins, called a meeting of his faculty and announced who the next Professor of Surgery was. There was no committee appointed. But he also did, unconsciously I'm sure, what turned out to be a very bad thing. In Nashville, he did not consult Barney Brooks; did not even speak to Barney Brooks.

Dr. O.: This is the President of Hopkins?

Dr. W.: Yes, Dr. Isaiah Bowman. Barney was not only a Hopkins' graduate, but he was one of Halsted's interns. Barney, of course, was too old to take the job. I don't think he would have taken the job. I don't think he should have even been offered the job, but I think he should have been told, "we're looking over one of your fellows; what do you think of him?" But he wasn't consulted. Barney heard about it in

the newspapers and Barney never forgave Alfred Blalock. It hurt Alfred tremendously.

Dr. O.: Ravitch alludes to this in his little biographical sketch of Dr. Blalock.

Dr. W.: Alfred went out of his way many, many times to smooth this over. He could not. Barney was getting a little bit old. He, I think, rather relished it, but it broke up one of the real fine relationships. Such a failure of consultation is now so common that nobody pays any attention to it. If someone looking at a member from this Department of Surgery were to write me a note, I would feel flabbergasted. These courtesies have all gone, but at that time those courtesies existed, and this was overlooked. I tell you this anecdote to illustrate a thing we were talking about a number of minutes ago, and that is the smallness of the group that ran American surgery. It wasn't a club, but it was a small group. They knew each other, knew each other so well that they determined, literally, the departmental chairmen throughout the country. They could talk to each other about places that were to be vacant, for there weren't many becoming vacant, really. One every year or two years, or something of the sort.

Dr. O.: I've seen people with some anger, like Loyal Davis in his history of the College of Surgeons, making reference to groups like the Society of Clinical Surgery and their academic political power. I think it's not so much the Society, but the fact that the members of the

Society of Clinical Surgery were also members of the ASA, many of them the Halsted Society, and so forth, and they spent many evenings sitting around a table together, as you've pointed out so clearly. Individual relationships, rather than organizations, were responsible for the "political power."

Dr. W.: They couldn't help themselves; they were obligated to control the type of surgical education they had instituted. Now, where I think they did control, certainly a little bit too much, was the content of some surgical journals. I'm glad you mentioned Loyal's name in this because Loyal, I think, exemplifies so well in his editorship of the Surgery, Gynecology & Obstetrics, that a surgical publication belongs to the surgical public--that because a paper was read at this organizational meeting or that organizational meeting, does not necessarily mean that it's going to get in the Surgery, Gynecology & Obstetrics. Let's take another one, the Annals of Surgery, a truly great surgical journal. For many, many years, the Annals of Surgery, as it does now, publishes the papers of the American Surgical Association and most of the papers of the Southern Surgical Association, but for many years also, the Philadelphia Academy of Surgery and the New York Academy of Surgery. There really wasn't much of a place for a person west of the Mississippi River to get a paper in the Annals of Surgery, and many of these case reports that came out in the Annals were second rate. One of Graham's fights, when he was put on the Editorial Board of the Annals, was to have the Annals dissociate itself from the Atlantic

Seaboard. We chatted about this. In fact, when I went on the Board of the Annals myself, he cautioned me on this--that the Atlantic Seaboard will take over the Annals again if you don't watch out. I'm afraid he's right. I see it coming back in right now, but I'm too far gone to fight it anymore. A small group, therefore, can do damage, as well as good, particularly on a publication. A publication has to be universal. And I think this has been done so well by Loyal and his SG&O. I find myself, as a result, not infrequently sending my manuscripts to the SG&O rather than the Annals. Well, I should anyway because I'm on the Editorial Board of the Annals and a member of an editorial board shouldn't publish in the journal.

In this loose conversation, perhaps I have been describing a sort of tapestry, a tapestry in which you can look at the woven image of one person and follow it through and see tremendous accomplishment. You could walk over just a little bit and see the woven image of another person, also making accomplishments, and when you come up just a little farther on the tapestry, you find that they are interwoven with their accomplishments. I think it probably represents an epoch in American surgery of about thirty years, thirty-five years perhaps--an epoch which is now giving way to a depression of the individual and an elevation of groups, committees, and so forth. I don't particularly welcome it because I relish salty characters, and no salty character ever lasted very long on a committee, nor does he get very many grants at the present. I think, however, it's the salty characters that make for advancement. You, as a good historian of medicine, know how

few real advancements in medicine have come from committees and groups. The history of medicine is the history of individuals. We are cutting that out now, and I think, interestingly enough, that this may be the result of too much standardization by many of these very great individuals to whom I have alluded. By the mechanism of specialty boards and surgical society requirements, they have made our graduate curriculum in surgery identical all over the country and, therefore, the men who finish our residencies are identical all over the country.

Even research has become standardized and a group project. Now, if one starts a research problem or a research program, he does it in a different way than Graham did and others of his type. At that period, what was done was first to have an idea. You started working on the idea; if you needed another dollar or two, you'd look around for it. You'd find a little space to carry this on; it wouldn't be much. You would run into some apparatus you needed--well, you'd make your apparatus, you'd have to build it. You had no money to buy it and, even if you did, nobody to make it. So apparatus was simple. As the department expanded, there were more ideas, there were more men, and you had more money and more space as you needed it. Now, what we do is look for space first. If you interview a man to come into your department, the first thing he wants to know is "how much space am I going to have?" Then he wants to know about his equipment. The last thing that he considers, and usually the thing that he never has, is an original idea. There results a reduplication of expensive equipment.

You can see it here in our own institution. As one walks through our corridors, he will easily see fifteen or twenty refrigerators out in the hall. We don't even have room for them in the experimental laboratories because everybody has to have a refrigerator of his own. Two people can't use the same refrigerator; it's degrading! Each man has to have his own centrifuge; it's degrading to use somebody else's centrifuge! The same holds true to the electron microscopes. This thing has become so common that it is about to destroy, I think, some of the things that many of us in the experimental laboratory cherished for a long time--the interchange and cross-fertilization of ideas that come from sharing space and equipment. We had a talk here yesterday by Congressman L. H. Fountain, who, contrary to the ideas of some others, has, I think, been a great friend of ours in experimental medicine. I think L. H. Fountain is trying to prevent some of this silly waste of money and----

Dr. O.: You mean the Congressman and Fountain Committee? The name strikes fear in the hearts of many a medical researcher!

Dr. W.: It certainly does for I think he's going to make the National Institutes of Health bear down on these institutions that have been wasting money. Just because a fellow's a nice guy doesn't mean that he should have the right to throw away money. And, besides, it's bad on his thinking.

Dr. O.: I don't want to interrupt you, but this brings up a line of

thought that I'd like very much to hear your views on. In the area of NIH Study Sections, so-called peer review.

Dr. W.: I can't speak from experience. I've never served on an NIH Study Section. They came on after I was too old. I like to tell myself that's the reason I was never on one. I was on the National Research Council for awhile.

Dr. O.: The feeling I've had; I've often wondered; I've sat in on a very few, but I must admit I can't help but think Congressman Fountain has a point. Even though I'm saying this about many people I respect, how in heaven's name can this group of close friends of many, many years' duration sit around a table and dole out this money when it has got to involve close personal relationships, to the degree sometimes that a man just gets up and walks out of the room when the group considers a large grant to his own institution or his own department?

Dr. W.: I don't know. I think it's awfully bothersome. Once when I was on the National Research Council (and this was a long, long time ago), one member put in a request for what, at that time, was an outrageous sum of \$400,000 for several research projects, and he walked out of the room while it was being considered. As we went down those research projects, they were the sort of things you would put a senior medical student on for his medical thesis or turn a first-year resident loose on. It was a cookbook-type of research, but you saw that the men who were in charge of this were really top flight guys--and you could see that what the chairman of the department was doing here was

paying the salaries of the men in his department with this grant. I said, "I think this is not right. I think that if this comes into play in a more universal way, it will destroy something very precious in American medicine." A very strong supporter of my objection was Dr. Blalock, sitting next to me. The thing was voted down. The member came back in and was so informed. The next year he got the \$400,000 and neither Blalock nor I was on the Committee. Now, back to Congressman Fountain and the N.I.H. You'll be interested in this. I was dumbfounded at the vote of the Senate not long ago on the Kennedy cancer bill. I thought it was completely wrong, and I wrote Fountain this and spelled out a little bit of the reason why. I had not read the so-called Rogers Bill that was being presented to the House at that time, although I subsequently did, and I found I wasn't too far off. I got a very long letter from Fountain, oh, it was about three pages--not the usual thing you get from a Congressman--"Thank you very much for your letter. I appreciated your opinion and I will do my very best"--not at all. This was a long discussion, and in it--I'm not quoting him completely--the inference was: "I agree with you thoroughly. Nothing can be allowed to happen that will undermine the National Institutes of Health." He is firmly sold on the great things that the NIH is doing. One of the real friends NIH has in Congress right now is L. H. Fountain. I think it's almost as a father spanking his son.

Dr. O.: But they need it.

Dr. W.: Need it badly, I think.

Dr. O.: The heyday, post-World War II, when we were looking for places to dump money into medical research--that day is long gone but the philosophy still hangs on.

Dr. W.: Well, it will change. One sees then in our present situation a change of attitudes, almost a change in goals. Here on my desk is a manuscript sent to me by one of the very good young men in surgery on the organization of a surgical department. It's a long thing and I won't go into it, but I am interested in that at least the last half of it has to do with the delivery of services. He draws an assumption that a function of a medical school is to deliver services throughout large areas, selling prepaid health insurance, and so forth. He loses completely an attitude of the previous group that I mentioned a moment ago, that the function of a medical school--particularly the Department of Surgery--is to develop great minds, to teach, to stimulate. And those minds that are good will go out into their communities and establish proper medical care.

The function of a university, therefore, is education--primarily excellent education. I'm bothered about a loss of this goal because maybe as a corollary I see coming out now in many parts of the country a situation by which institutions are becoming much more interested in quantity rather than quality. It is seen in the concept of educating doctors in three years. We are limiting the breadth of surgical knowledge in order to decrease the five years previously required for surgical residencies to an experience of three years. We are told

that we need fifty thousand more doctors. Nobody raises the question: How do you know? We are trying to solve our medical problems by admitting many more medical students and educating them quickly and, oh, golly, let's have people we call physicians' assistants. We seem to be trying to load the country with second-rate education as quickly as we can, and this I see our great universities getting into, and it bothers me no end. I can hear Evarts Graham on this one now!

Dr. O.: I've seen some delightful letters to some of his colleagues at the time the Cancer Institute, which was the first of the National Institutes of Health, wanted to dole out money to develop what he referred to as cancer specialists, and he just raised the roof! He thought this was something that should be in the hands of the university, that these people needed well-rounded training, and so forth.

Dr. W.: He was opposed to specialized institutes, I suppose, up to the time of his death. I recall once, right after the building of the present Memorial Hospital for Cancer and Allied Diseases in New York, a visit from Dr. James Ewing. Dr. Ewing had made the Memorial Hospital almost single-handedly. The old Memorial Hospital was moved from way uptown to downtown. Before they opened the hospital, as a matter of fact, he came by St. Louis and he, Graham, Fred Taussig and I had lunch together. Ewing posed the question: What should be the goal of the Memorial Hospital? What should it do? And he said, "I posed this question yesterday to Dr. Arthur Bevan up in Chicago and I'm a little

bit bothered about asking the question again because after he'd listened to my story, he said to me, 'Ewing, if you want to contribute greatly to American medicine, the best thing you can do with the Memorial Hospital is to burn the goddam thing down'." Ewing said, "You know Dr. Bevan, don't you, Dr. Graham?" Everts said, "Yes, I've known Arthur since I was a boy." Ewing said, "What do you think of him?" "Well," Graham said, "he's quite outspoken. Every now and then, I agree with the things he says, and every now and then, I disagree with the things he says. This time, I think, I agree with everything he said about the Memorial Hospital." That was the end of our lunch.

(Laughter) Dr. Graham felt that most of the great developments that come in the treatment of a disease come from people who are working in areas outside of that disease. For instance, he said if you had a hospital for the treatment of pneumonia, I daresay you would have to rely on departments of bacteriology and microbiology to supply you with the ideas for the treatment of pneumonia. He felt this to be true of a hospital for the treatment of cancer, that the ideas that are going to be developed in the treatment of cancer will come from biologists; will come from people far removed from so-called cancer on the hoof. Therefore, in establishing a specialized institute, you demoralize the institution--the bigger institution. We have that problem with us right now--in a spot like Chapel Hill which has not too much money. Suppose it's offered fifteen million dollars for a cancer institute. What would we do? That's more money than all of us have put together. You can see the mouths drooling all around when that sort of thing becomes available. Those people who would stand back

and say, "For God's sake, turn it down" are pushed over in the corner.

Dr. O.: People don't realize it can become a tail wagging the dog.

Dr. W.: The tail wagging the dog. I think this, unfortunately, has happened to many universities as a result of the medical schools. I think this happened for many, many years at Washington University, when the medical school ran the University. This happened at Johns Hopkins for a long time. Johns Hopkins Medical School and Hospital really controlled the University, more or less. University presidents are frightened at medical schools--the budget is so great. They're frightened at hospitals. They're frightened at salaries. They, therefore, tend to appoint a vice-chancellor or vice-president in charge of health affairs and let him handle it. The university president tries to hide this skeleton as quickly as he can. We all probably are going in that direction. I don't know. I hope not. But I was very happy in our beginning here in Chapel Hill to see that the University was willing to bear its cross, so to speak. I found, for instance, that I myself was on such committees of the general university as the Committee for Established Lectures for the General University, the University Press, the Committees on Honorary Degrees, associations that brought you in constant contact with the humanities and the School of Law, the School of Religion, people of that sort. The town is small enough for this to continue, and we all become very good friends, so much so that at a dinner, very seldom do you see a bunch of doctors. There will be one or two there, but you'll also be chatting with the

head of the Philosophy Department or similar people. If we can keep the medical school from capturing the university, I think we can get a breadth and a vision that will keep medicine from going astray. But if we start educating guys and giving them an M.D. degree five years after they leave high school, pretty soon they are the ones who are going to be teaching the students.

Dr. O.: That's a thing I find very frightening.

Dr. W.: Just a day or so ago, I wrote my good friend, John Hubbard.

Dr. O.: Oh yes, I know Dr. Hubbard.

Dr. W.: Do you know John?

Dr. O.: He was Chairman of the Board of Regents of the Library a few years ago.

Dr. W.: Oh, I didn't know that. Well, great. I think the National Board can be of great help in this respect. I told John that I thought that his group at the National Board had the information that would make it possible for them to look over the performance of a student graduating this year, let us say, and a student graduating from the same school five years ago and ten years ago, to see whether that school was deteriorating or not as it changed its curriculum, and that the National Board of Medical Examiners had the duty to so inform that school. I said not a single school is going to ask you to do this-- you've got to do it on your own, and you owe this to the Foundations

that have been so generous in their support of medical education. We must rely on the National Board, therefore, for some way of measuring excellence and informing us. I don't mind bringing this into our discussion because I think I'm mouthing a subject about which Graham would express almost identical views, if he were alive. Of course, I saw him involved in this all the way along.

Dr. O.: This is the other area in which Graham stands out as such a powerful figure other than his bringing the physiological laboratory into surgery. This is his continual battle against mediocrity and I don't think it's enough to say that he was just a fighter and took on Martin and the American College of Surgeons. He took on the International College of Surgeons; he was one of several involved in the founding of the American Board of Surgery. These examples can be tied together as an honest concern for trying to right what he felt was a mediocre situation. He tried to reform the American Surgical Association.

Dr. W.: A committee can't do this; a man has got to do this!

Dr. O.: Somebody has to be the stimulus to get up and say this is wrong and it should be remedied.

Dr. W.: Well, it's all so massive and it's all so big that perhaps a single person or even a group of messiahs, so to speak, cannot alter the course of events. Maybe the course of events is going to turn out to be great. Perhaps I'm too much of a pessimist on what I see at the present time. I'm no pessimist on the scientific achievements. I think

they're fine. And I'm no pessimist on the social achievements. I think they're fine. But I am a pessimist on the development of the individual. Now, if this sounds anachronistic, it's because the individuals that are going to bring these achievements about. I see developing a standardized form of surgical service and surgical education, and I don't mean this in terms of governmental socialization, but equalization of all people, and we're not ready for that yet. It was expressed so beautifully by Jefferson when he said that "all men are born free and equal." Jefferson, of course, realized that if one has freedom, one does not have equality, and if he has equality, he does not have freedom. So our democracy all through the years has been a contest between freedom and equality. Just now, I think we see in medicine the tendency toward that equality, and when one gets equality, the equality is at the lowest common denominator. Oh, there'll be a breakthrough if it gets too bad; somebody will rise up and say, "we can't go with this any longer," and the struggle will start all over again.

This philosophizing here toward the end, I think is perhaps in a way, a summation of the contributions of Graham and the group around him over a long period. It seems to reach a certain pinnacle and then come to rest. Graduate education in surgery suddenly caught fire in those early days. Where, in 1935 or 1936, there were less than twenty places in America that gave as much as two years education surgery, by 1940-41, there were over four hundred. Two or three years later, we

began to organize, so we established a committee that inspected these places. The power of these committees makes us forget that these places were great before the committee was formed! Now the committee prevents any more experimentation. I'm talking, for instance, about the Joint Committee on Accreditation. One must conform! You cannot experiment! I cannot say to a resident of mine, "Look, lay off the wards here for about six months and finish this swell idea you're working on. I don't think it would bother you at all when you come to take the Board examination because I think you're good enough right now to take them." He can't do that because he is not allowed to spend that much time in the experimental laboratory.

Dr. O.: There's no flexibility.

Dr. W.: So we end up with what could have potentially been a good educator becoming a good surgeon in a community hospital. Both are good things, but I sometimes wish we had an easier opportunity to develop good educators. Now, have I missed out on anything you wanted me to talk about?

Dr. O.: Oh, this is great, really. This has been a most lucid commentary on Graham and his contributions.

Dr. W.: I hope I haven't been too general.

Dr. O.: No.

Dr. W.: There were, of course, personal relationships to which I have

not referred. We used to play golf every Wednesday afternoon when I was a resident, and from then on I hated golf. I hated it for several reasons. One is, I never had enough money to buy any clubs and I never owned more than three clubs--what were called in those days, a driver, a mid-iron, and a putter. These were old bent wooden clubs that friends of mine had thrown away. I never had any lessons in golf and so you can imagine my score. Graham was a little better; he'd break a hundred. I never could break a hundred. One afternoon, I was going down the fairway, had not gotten into trouble for some strange reason, and I heard the most violent profanity I had heard in a long time. I didn't know what in the world was going on. I looked over behind a clump of trees in a ditch, and there was Graham trying to get his ball out from behind a rock--so honest he wouldn't lift it at all. He was fighting it out. I said to myself "a game that will make a fine man such as Dr. Graham become so profane and lose his temper is not a good pursuit for either one of us." And so the next Wednesday, I said to him, "Let's go to the baseball game instead of playing golf." Well, this suited him all right and, from then on, on Wednesday afternoons we went to the ball game. Pretty soon, we were joined by Barry Wood, when Barry came there in medicine; and Sherwood Moore, who was head of radiology; and this was one of the most pleasant foursomes that I can imagine. We knew many of the players on the Cardinals' team; this was back in Dizzy Dean's time. In fact, Mr. Braden, who owned the Cardinals, was Dr. Graham's next door neighbor.

And speaking of baseball, it was your father that introduced me to a very lovable character named Ruth.* How it happened, I don't know, but during the prohibition days, the brewmaster down at Busch Brewery gave your father some Budweiser yeast. Mr. Busch figured that some of these days, prohibition was going to leave us and so he kept his brewmaster and one or two other oldtimers there on his payroll. Your father took this yeast and learned how to make some of the nicest beer I think I ever tasted. Quite frequently, on Saturday night--many of us weren't married at that time--we'd meet out at your place, and we would drink beer and sing. It was a nice go-round. Your father would get that bull fiddle and he'd plunk base chords while we sang. One of the guys who used to love these soirées was Babe Ruth. Whenever the Yanks would come to town, you could bet your life that Babe would be right there. I usually would take him out to your father's place, out toward Webster Grove, and I'd bring Babe back into town. He was a delightful child. He never did grow up.

Dr. O.: I never had heard this story. My father never mentioned this.

Dr. W.: It was a period in which house staff and younger instructors were never married. I suppose much of our--I won't say most--but much of our experimental work was done at night, and this would go on until one or two o'clock in the morning. We'd show up at ten o'clock the next morning--nobody punched a clock--actually, we never had a job; I don't think Graham ever told me what to do. Sometimes he'd go out of town and would say, "Would you mind covering the service for me?" When

*I. Y. Olch well remembers the beer parties and the ball players but does not remember introducing Babe Ruth to Dr. Womack.

he'd come back, he'd never take it back over again; you'd cover the service for several months instead of for a weekend. Nevertheless, if you had a patient come in there and ask for him, he'd better see that patient, or you'd better let him know before you operated on a patient; that is, when you were a resident. A resident never had the privilege of operating on a patient without the attending man knowing about it. The department was small enough--the hospital was small enough--for the resident to be responsible for all surgical patients--every specialty.

There was an intern with me named Gnagi, who was a two-fisted, extroverted lad from Wisconsin--a man with real character. He and I were going over charts one Saturday night along about midnight on the colored ward--we had a segregated ward at the time--and he said, "Look, here's a man from your home town." And sure enough, he was from Reidsville, North Carolina. I never had heard of him, but I said, "Bill, I'll bet you a dollar I can go in and wake that fellow up and ask him if he knows me and he'll say 'yes'." Well, Bill wasn't informed about how in a small southern town, everybody knew everybody, and he took me up. He said, "It's dark. I can't see." I said, "Do you know Miss Sue Womack from Reidsville?" He said, "Sure I know Miss Sue Womack. You're one of her boys, I betcha." And I asked him what was wrong with him. He said he had a tumor in his abdomen. Well, we let him go back to sleep, and I came back and looked at his chart and saw he had an abdominal aneurysm. The next day, Graham made rounds with

me and, as we went down the ward, he had to know what every patient had. If there was yellow card on the bed, the patient was on urology; if it was a red card, the patient was on neurosurgery; and if it was a yellow and it was a man and he was sixty years old, you would say he had prostatic hypertrophy. Graham knew what we were doing all the time, but he couldn't prove it. Finally, this particular morning, we came to the colored fellow, and Graham said, "What's he got?" I said, "He's on medicine." He said, "I know he's on medicine! What does he have?" "Oh," I said, "he has an aortic aneurysm." Dr. Graham was crestfallen. He thought he had me. I didn't tell him the story or anything. He went over and put his hand on this fellow's abdomen and said, "Look, don't let this man get out of here without Barney Brooks seeing him." And so, after rounds, I got in touch with Dr. Brooks and told him we had a patient on medicine with an abdominal aneurysm, and Graham thought he'd be interested in him. Brooks came down right away to see him, and later on, operated on him. This was the first successful ligation of the abdominal aorta. That's how close it came to being missed; if it hadn't been for that dollar bet on a Saturday night----

Again, I think it brings up the value of house officers being on the ward at night. They would shoot me if they heard me say this today, for at the present time, they find it not very good, but I know so few fellows who have learned much surgery at home. It's hard on wives and maybe they shouldn't have wives until very late--not until they finish

their residency. It's too much for most wives to stay at home night after night alone taking care of the children. And yet, this is one of the few ways a man has, I think, of climbing above that level of mediocrity we have pulled out for him. You are hearing a past generation speak right now.

Dr. O.: It sounds good, too.

Dr. W.: Well, look, I've wasted too much of your tape now.

[End of Side I, Reel 2]

Index

- Abbott, Roy, 10
- American Board of Surgery, 19, 21
- American College of Surgeons, 20-22, 29, 41
- American Surgical Association, 19, 21, 30, 41
- Annals of Surgery, 30, 31
- Beck, Claude, 25
- Bell, R. D., 5
- Bevan, Arthur Dean, 5, 20, 37, 38
- Blair, Vilray P., 10
- Blalock, Alfred, 28, 29, 35
- Bowman, Isaiah, 28
- Boyd, William, 17
- Brooks, Barney, 10, 14, 15, 28, 29, 47
- Brown, James Barrett, 10
- Byars, William, 10
- Carlson, Anton J., 5
- Caulk, John R., 10
- Cholecystitis, 10, 17
- Cholecystography, 11
- Cole, Warren H., 11, 12
- Coller, Fred, 21
- Cori, Carl, 8, 9
- Cori, Gerty, 8

Davis, Loyal, 29, 30, 31

Elman, Robert, 15

Empyema Commission, 5

Erlanger, Joseph, 8

Ewing, James, 37, 38

Fee-splitting, 6, 7

Fountain, Congressman L. H., 33, 35

Gasser, Herbert, 8, 9

Geraghty, John, 11

Graham, Helen Tredway, 12

Grant, Francis C., 24

Halsted, William S., 4, 23, 24

Halsted Society, 24, 30

Harvey, Samuel C., 21, 23

Hubbard, John, 40

International College of Surgeons, 41

Johns Hopkins, 25-28, 39

Joint Committee on Accreditation [Conference Committee on Graduate Education (Training) in Surgery], 43

Key, J. Albert, 10

Lewis, Dean, 24

Martin, Franklin H., 20

Memorial Hospital for Cancer and Allied Diseases, N.Y.C., 37, 38

Moore, Sherwood, 44

Murphy, J. B., 20

National Board of Medical Examiners, 40, 41
National Research Council, 34
Olch, Isaac Y., 2, 12, 15, 16, 45
Phemister, Dallas, 21
Phenolsulfonphthalein test, 11
Pneumonectomy for cancer, 1-3
Rand, Frank, 26, 27
Rankin, Fred, 21
Reid, Mont, 24, 25, 27
Rienhoff, William, 1
Rodman, Stewart, 21
Rowntree, Leonard, 11, 14
Sachs, Ernest, 9, 10
Shaffer, Philip, 8
Society of Clinical Surgery, 29
Stone, Harvey, 21
Surgery, Gynecology and Obstetrics, 30, 31
Taussig, Fred, 37
U.S. National Institutes of Health, 33, 34, 35
 Cancer Institute, 37
Wangensteen, Owen H., 13, 21
Washington University School of Medicine, 8-10
Wells, H. Gideon, 4
Whipple, Alan O., 21, 23

Wood, Barry, 44

Woodyatt, Rollin T., 4, 13