AN INTERVIEW WITH DR. FREDERICK L. STONE

BY STEPHEN P. STRICKLAND, PH.D.

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Introduction and Biographical Sketch

This interview with Dr. Frederick L. Stone is one in a series of "oral histories" focusing primarily on the origins and development of the extramural programs -- most especially the grants programs -- of the National Institutes of Health, beginning with the establishment of the Division of Research Grants in 1946. Like Dr. Stone, most of those interviewed had critical roles in the development of the extramural programs.

The grants program constituting the largest component of the NIH, the interviews also reflect judgments and perspectives about the impact of the grants programs on health and science.

Dr. Stone came to the National Institutes of Health in 1948, going directly into the Division of Research Grants where, in his view, he got splendid training in administration from Dr. Ernest Allen and Dr. Cassius J. Van Slyke. Recently out of graduate school himself, he was shortly given responsibility for the handling of research fellowships and training grants, a then small but soon to burgeon component of the NIH extramural programs. The early training in the Division of Research Grants, and in the informal practices of communication with scientists and university administrators were of enormous benefit to Dr. Stone when he was subsequently asked to help organize the newly created National Institute of Neurology. Some years later, he helped to build another important institute of NIH, being the first director of the National Institute of General Medical Sciences. Later still, as President of New York Medical College and a senior official of the Health Services and Mental Health Administration, Dr. Stone proved his administrative and leadership capacities in two very different kinds of institutions. He is, he suggests, a builder of institutions, and those who, like me, have known and watched him perform over a period of almost two decades know that institution building is indeed one of his great strengths and areas of most distinguished service.

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WASHINGTON, D.C.

Interview by Stephen P. Strickland with Dr. Frederick Stone

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SS: I am doing interviews with some of the pioneers of the grants programs of the National Institutes of Health, including you, and I am doing it on record so that, after you and I have edited and brushed them up, the transcripts will be in the permanent archives of the Library of Medicine. On the basis of these interviews, I will then do some articles and try to get some public and scholarly attention focused on the Library this year and the NIH next year. So far I have talked to some friends of yours -- Ernest Allen, Rod Heller ...

FS: Ralph Meader?

SS: I have not yet talked to Ralph Meader, but I have corresponded with him and hope to see him some time soon.

FS: Summertime is a wonderful time to contact Ralph up in New Hampshire.

SS: Ken Endicott is around, of course, and I have seen him a couple of times, and I've had a conversation with David Price. He did an interview some time ago with the Columbia University people and it is decent but not fully satisfactory for our needs.

FS: Most of the people who were sitting on Van Slyke's "back porch" are now dead, except for me. Things did not come about by meticulous planning, but because a man of some character and power had a good idea.

I. Eduation and Training

SS: Let's leap right in there then. I want to get on record your own personal professional training: how you came to be a scientist and administrator in the Public Health Service in the first place. Did you know from childhood that this was what you wanted to do?

FS: My school background starts with Middlebury College in Middlebury, Vermont, after which I taught school for two years. Then I went to the University of Rochester. My family lived there at that time. Some years later, due to the war, I got my Ph.D. in 1948 and went into the Public Health Service. I transferred my commission from the U.S. Marine Corps to the U.S. Public Health Service, with some difficulty, because no one had done it before. This came about because in the application to the Public Health Service, there was a section having to do with a desire to work in administration. As I recall, there was a way to indicate your first priority.

One day, as I was watering the plants in the greenhouse at the University of Rochester, an admiral appeared in navy blue and gold, with gold up to his elbows. His name was C.J. Van Slyke. I was interviewed then and there; he in his blue and gold and me in my shorts! Complete with water buckets and whatever else it took to do the greenhouse. He sat on one crate, I sat on another, and we had our interview.

SS: And that was about 1947.

II. NIH and the Grants Program

FS: Yes. After taking the written exams and physicals, I was told to report to the National Institutes of Health, where I arrived about the third of August, 1948. When I got there, I looked up Van Slyke, who was then director of the Heart Institute. I also saw Dr. Badger, an associate director of NIH who said, "Well, now that you're here, Stone, why don't you take a week, or perhaps even two weeks off and go around and talk to everybody and then come back and tell me what you think you'd like to do. Dr. Van Slyke speaks very highly of you and we want you, of course, to be happy." And he said, "By the way, you know you are one of the very few, one of the first we have chosen to do administration as a career. We do not think that people who have been less than highly successful in the laboratory should turn around and take on administrative duties in a senior capacity. We would rather train you from the ground up." I thought that was an unusual way to do it. The implications of what he waid were not lost on me. In three days I said I wanted to work with a very exciting man, Ernest Mason Allen. They said: "All right. Go over and talk to Ernest, and you can move whenever Ernest says he has room for you."

I interviewed Ernest Allen, and he interviewed me, and we made an agreement and shook hands on it. He said, "You have to be trained to do administration just the way a scientist has to be trained to do certain laboratory procedures. If you were in the scientific echelon in biochemistry, for instance, you would learn to clean galsswear. Well, here you are going to learn to check travel requests and learn every administrative step in your organization." Which I proceeded to do. That is what made administration so exciting to me. I knew that I had control of the tools. It made grantsmanship a great pleasure because I found under the law I could do anything that needed to be done for a scientist if his need and his ideas could withstand peer review and evaluation.

SS: So, what happened next was the creation of the grants program. I suppose that might have been already under way...

FS: It was already under way, but it hadn't been for very long. They were just moving from having National Advisory Health Council meetings once a month to having them once every three months and didn't know quite what to do. They had, I think, six study sections that were to meet once every quarter, a step ahead of the council meetings. Previously, the National Advisory Health Council had done all scientific review and approved the applications. They were it.

SS: Nobody I have talked to so far remembers having any specific outside models for what you did. Nobody said, "Well, the Rockefeller Foundation does it like this, why don't we do that way?" I take it you invented the thing?

FS: I got in on the tail end of the discussions, but when you worked with Ernest, he brought you up to date. He would sit you down, talk to you, and give you a lot of information in an hour, which he did with me. They had decided that the fellowship programs would at first be peer-reviewed by the various institutes that had research and training appropriations, and would be centrally administered through the Division of Research Grants. That was my first post because it was not administratively fatiguing; the procedures were relatively simple. Furthermore, I had just come out of that environment and I knew more about what went on than they did regarding graduate students and post-doctorals fellows.

Van Slyke, Ernest Allen, and Rod Heller decided that research grants had to be a program in which the basic decisions would be made by the scientists of the United States. The supporting funds were tax-based, and it was felt that outside peer review would be appropriate and would provide the public a view of the review and awards process. There was, however, a substantial amount of criticism from some established sources for the Division of Research Grants and Van Slyke and Ernest Allen. Those sources warned of a dire fate that would follow innovating. Some said that a program like this obviously should be run out of one of the already established groups: the National Research Council, the National Academy of Sciences, and others. Each time an academic person warned us of an impending danger, Ernest immediately asked him to serve on our review committee. For the first time many of them found that they could have an immediate, very powerful effect on the course of science, provided that they maintained -- and they always did -- the highest review quality.

SS: In creating this system -- the peer review system and grants review process of scientists reviewing their peers' and colleagues' work -- did you have any idea that it was a momentous undertaking, or did you treat it simply as a job that had been given you and the Division of Research Grants when the transfer of wartime contracts had happened, and you would see how it worked?

FS: Well, I was older because of my six years in the military, and I was the one most directly related to university graduate work. I had taken some courses in the medical school and some in the graduate school, and I was more closely related to them than any of the others who had not had experience or whose experience had been in the past. I realized what an unusual thing it was to be totally unknown, yet to put in an application and have it reviewed by men of great national reputation. This did not and still does not happen in many foundations. They tell you quite frankly -- some of them put it in writing -- that you go in, see one of the grants officers, and he'll tell you what <u>he</u> wants.

The great strength of that new NIH grants program was that you could tell the government what <u>you</u> wanted to do. If it had reasonable relationship to the biomedical sciences, <u>you</u> got a review. To me, at that young age, that was something extra good. You didn't have to find out what was important from the grants officer, because in those days it was not usual to travel, and many of us didn't have money to go racing down to New York, where they all seemed to be, to try to make a friend of a grants officer. I mean no disrespect -- the grants officers were working for their boards of trustees and the trustees told them what they wanted to see, by and large. They guided the program, which is their legal responsibility. But how do you know that when you sit out in Livonia, New York, or anywhere else? You don't know!

SS: Dr. Allen said that early on, in addition to taking over those wartime contracts, pretty soon, within a year or so, an announcement went out through scientific channels and other special channels, saying that the National Institutes of Health is in business; we have a Division of Research Grants and we can support science if it relates to national medical problems. The response was remarkable. It was bigger and better than they had imagined it might be. It took a couple of years to consolidate.

FS: Ernest came there in 1946, if I remember, and I showed up on August 3, 1948. I had been in contact with the National Institutes of Health for six or eight months before that, and had had the time to read what they were doing. The whole nation had been impressed with what radar would do. We had something called sulpha drugs, so we could understand something about the importance of research. Then something called penicillin came along and research began being spelled with a capital "R",like His Eminence is spelled with capital letters. Therefore, those of us who had some insight into research, along with some of the younger ones such as myself who did not, recognized scientific investigation was important for all sorts of various reasons and various areas. Also, the people in agriculture were actively engaged in improving disease resistance, crop yields, etc., by selective genetic methods.

Dorothy McClintock's work was being ignored; it took 35 years for science to recognize that genes do move; that is by "translocation" and other procedures. Her recently awarded Nobel Prize clinched her a place in scientific history; however, the work was being done 35 years ago when a scientific "breakthrough" was a great event. We are blase about these quantum jumps in knowledge these days.

SS: It's good she got the recognition while she was still alive. She must have been in her eighties at the time of the award. So many times people's great works are not given credit until they're dead.

FS: Yes, she is in her eighties.

We were all very excited about scientific research. It was the time for it. The whole nation was aware that we had done so well because we had a scientific base, and we were dedicated to research, which was being done widely. Most of the basic research was clearly university-based and there were 1500 institutions who encouraged their faculties to undertake these projects. It was not being developed in the U.S. as in other countries, like at the Cavendish Laboratory in Britain, or the Kaiser Wilhelm Institute and the Pasteur. Research became important educationally and was widely dispersed in this country. We had lots of universities who recognized what you could do to develop the institution with research monies carefully spent on important investigations.

SS: Did you therefore have a sense that, in addition to building science and making improvements in medical science, you were also building institutions or you might be establishing a national, nation-wide network of competent biomedical investigators?

FS: Perhaps four people understood that we were providing a scientific base for medicine: Ernest Allen, C.J. Van Slyke, Jim Shannon, and Mary Lasker. Those four understood what it was about. Norman Topping understood too, and helped, but he had certain problems with the Service at that time and about 1951, when he was an Assistant Surgeon General, he left the Service to be the President of USC. I used to talk to him on Saturdays when he was in the NIH gardens, hoeing, and I know he knew too, because we talked about it. The scope of it all was a new thought to me, and I didn't get the sweep right off. But when I did, I had just enough scientific background to understand what it really meant. Ernest knew what it meant. Mary Lasker knew what it meant. She tied it always to cancer, or hypertension or heart disease. She knew what she was doing. She concentrated on those areas because it was easy to get emotional about them, and no one could criticize her for being emotional. It is clear too that C.J. Van Slyke and Lister Hill knew what it was about. So, it was Lister Hill, C.J. Van Slyke, Ernest Allen and Mary Lasker. Norman Topping helped as long as he was there, and after him, Jim Shannon.

SS: What about Dr. Dyer?

FS: Dr. Dyer must have been encouraging all of these men and telling them how to get things done, but he did not seem to me to take a primary part. Somebody had to run the NIH, and that was his job. But he made the crucible in which everybody else could interact with each other, and he kept the crucible whole and he protected it.

SS: When Dr. Van Slyke then went over to the Heart Institute, did he still keep his hand in the development of the Grants Program?

FS: Yes he did, because he was probably the titular leader of this band. He probably had done more thinking about it than the rest of us had. Dr. Dyer helped him. All of the Congressional contacts in 1948 were almost always done by Dr. Dyer, with Drs. Van Slyke in attendance on one side and Dr. Topping on the other. Those three men did the whole thing and the rest of us stayed out at Bethesda. So I can't specify the extent to which he was important.

SS: I ask because nothing I have read makes it clear what he did except to encourage the others. He had an interest because I do know that he was the one who sat for NIH on the medical committee for the OSRD and he is the one who spoke up and said NIH would take the grants. So obviously he was not disinterested. Once he brought Van Slyke in and got his team together, I take it he pretty much left it up to the others to run with it. What about the staffing? Dr. Van Slyke and Dr. Allen told you, "You're going to be the administrator" and then others were brought in accordingly?

FS: No, he said, "You will be in administration." Not in a laboratory but in the administration. So I went to work in the fellowship program under Dr. Byron Brunstetter, who was killed in an airplane accident very soon after I got there, and they promoted me to chief of the fellowship program of the Division of Research Grants.

SS: Fellowships over the next decade became equally important when it was realized that one of the things you had to do to strengthen the national medical research capacity was to have top people, and since you didn't have top people in every field, getting them assumed increasing importance. How did you go about that?

FS: One has to know something about the background of graduate study and the relationships between graduate students and their preceptors prior to World War II. My experience was drawn from the University of Rochester, but I talked to other people from other schools. One would be there to help his preceptor meet his academic requirements. You got a degree incidentally. To some people, this is the only way to explain honestly how some got a degree after eight years and more. I felt, when I got into the fellowship program and looked at its fundamental policies and its regulations, it was time we got a lot of things out into the open. A fellowship was not intended to give you an opportunity to shoulder half the teaching load of the department, but it was given so that within the normal period of time you would get your degree and that you would do the amount of teaching and the amount of laboratory preparation to carry on the research after your degree and to fit into other departments. You were not cheap labor, and you were not just an extra hand.

There was a certain amount of resistance to this concept out in the field, but I must say that resistance was not supported by the intellectual leaders of the country. Right away we decided that a maximum fellowship was four years pre-doctoral; and at that time, unless you had done poorly, you would come out with a degree. But we kept the emphasis on quality. It worked like this: if you could have a graduate student for four years with no interference from the federal level, you better have a good one with high competence and potential. Right away the emphasis was upon getting a person with potential, because he wasn't going stay beyond the term of the fellowship. We recognized that sometimes there are reasons why someone needs to take five years, but I remember only one or two cases when a pre-doctoral fellowship was given to anyone beyond the fourth year. And never beyond the sixth year.

SS: So the DRG's fellowship policy itself was necessarily conducive to getting the best people.

FS: Yes. It wasn't unusual in those days to find someone, still as a graduate student, in a large university who had been at his post six years, still working on his doctorate.

SS: Were the fellowships given to both M.D.s and Ph.D. candidates?

FS: Yes. The M.D.s were post-doctoral. Many times the Ph.D.s were post-doctoral depending upon the inflection point in their career development. It came out roughly 5Ø-5Ø% pre-doctoral/post-doctoral, pretty soon because I felt that was a reasonable balance.

SS: And how long did it take you to organize the research training grants program?

FS: In the research fellowship program it took me about three years. I think at the end of 1952 I transferred to the Neurology Institute, and in that length of time, I think the program went from about a few tens of thousands of dollars to perhaps $6ØØ- or $7ØØ,ØØØ. It went up very rapidly.

SS: If you are in charge of a program like that, you see what's happening in institutions of higher learning around the country. You know where the good departments of biochemistry and physics are. At what point was there any conscious focus on the matter of geography? Surely the more aggressive schools in the first instance were the ones that were not only the most prestigious, saying, "We've got all kinds of good people who should have training grants." If there was a sense of developing this national base, of supporting good people who already existed as trained professionals, but developing others, you had to think in broad terms.

FS: The research grants program gets the great credit for taking on geographic representation. The man most responsible for this is Ralph Meader. He had experience with cancer research development and support through the Jane Coffin Childs Fund of Yale. He recognized that many people who were largely unknown to the National Institutes of Health were doing good cancer research. So he would travel. He and Franklin Yeager at the Heart Institute at that time seemed to have the same idea. Whether or not Van Slyke as director of the Heart Institute imparted it to him, I don't know. But he was very busy visiting people from whom applications had not yet come, and I recognized that this was probably something I ought to do. They had a fairly strong research porgram so I talked to Yeager and to Ralph Meader, and Ernest Allen as well. The three of us, Stone, Allen, and Meader, worked most weekends. We would go see each other, and call each other. I learned about the kind of incidental information without which a program never really develops; it doesn't develop by issuances from any President's office. Then I would go around to these places and right away I could see how a fellowship would fit in with their general academic goals and their research goals. We had four people doing the developmental work. Each had an interest, and Yeager and Meader had supported their budgets. They transferred the money to our account at DRG and I admini- stered them. But they worked for a very strong program. Ernest Allen travelled a great deal. We all did. It was nothing to travel 200,000 miles a year.

SS: So, within four or five years you really had the program established?

FS: As far as mechanism goes, a good sized program. A good operating program was in place at the end of the third year. I left it at the end of the third year or beginning of the fourth, and went to the Neurology Institute.

SS: That was all set up and then you moved over the Neurological Institute, which was called what?

FS: The National Institute of Neurological Diseases and Blindness.

SS: And that was the fourth Institute, after Cancer, Heart, and Mental Health?

FS: Mental Health was brought up out of the Division of Mental Health and Hygiene from "downtown" by Robert Felix. Then there also was the Cancer Institute and the Heart Institute. There was the Institute of Experimental Biology and Medicine -- not a legally mandated separate institute. Likewise there was the Institute of Allergy and Infectious Diseases. The latter two were called institutes but at that time, under the law, they were not. So there ere six institutes, and Neurology was the seventh.

SS: And you went to help set up the Neurology Institute?

FS: I believe I was the third employee in the Neurology Institute at that time. If there was any setting up to do extramurally, I had to do it.

SS: What had led to its creation? There was the effort since 1948 to get the Heart Institute created; the Cancer Institute had been in existence since 1937. Was there a scientific groundswell or did Mary Lasker and some of her friends generate interest?

FS: There was a groundswell, of which I was unaware for a long time, provided by the American Association of Neurology. There are two such professional organizations: the American Neurological Association and the American Association of Neurology; the latter of which Pearce Bailey was leader. He was, I think, chief of neurology in the Veteran's Administration at that time. He and Abe Baker and a few other gentlemen in neurology agreed that that was the time to push neurological diseases, because in the VA they had demonstrated that when neurology was subordinated to psychiatry it did not prosper as well as when it was a separate discipline. Neurology as a part of medicine did better, but still did not prosper as well as it had in the few examples when men of great personal power and stubbornness had said: "I'll come to your school, but I must have a separate department, and I shall be the chief. I shall not be a 'part' of psychiatry nor of medicine." In those cases, things progressed very fast and very well.

Pearce Bailey got together with Mary Lasker and marshalled national support of the neurologists. Robert Felix was very helpful also. Felix never gets the credit at NIH for being as far-seeing a man as he was. He was the one who pushed initially for a clinical center. He had to beat down a lot of people who said, "If we have a clinical center it will get all the money and research will suffer," meaning the kind of research they were doing would suffer. It proved to be entirely different, because of the strength of the various directors and what they felt was the right way to run a scientific center; that was, you took care of research in your Institute equally with clinical center projects, which were not permitted to dominate the budget and recruitment process. The Institute directors had a direct stake in the basic research that supported a clinical outlet, but strongly supported a basic research outlet as well.

SS: That's been true throughout, hasn't it?

FS: That is one of the unique points of view of the National Institutes of Health. It has been its great strength scientifically.

SS: At some points there have been misperceptions about that, and those who are anxious to conquer cancer tomorrow say put all the money into cancer.

FS: These are usually people who have been successful in engineering ventures, who feel that all you have to do is to talk about men, money and materials being at the proper place at the right time. But try and get that to happen on public appropriated money! What I see happening in NASA today could have been avoided if some of the wisdom at NIH had been employed.

SS: You mean in peer review of various elements of their program?

FS: That is exactly right.

SS: Let me ask you about public impressions and support and interest in neurological diseases. Cancer, from the first to the present, was the "dread disease". Meanwhile it became clear in the early fifties that heart disease, cardiovascular disease, was killing more people than cancer, so it was natural that the Congress was supportive of putting research into that. What particular problems in the neurological area attract concern?

FS: Neurological diseases disabled more people and often had a greater social impact. We had not yet conquered polio, and everyone was concerned. Today it is difficult to remember the peoples' fear when they had little children in the family and polio was a neuromuscular disease without definite prevention measures. It was controlled by infectious disease methods, but nevertheless it was one of the neurological diseases. Multiple sclerosis was coming to the fore, as were arteriosclerosis and muscular dystrophy; epilepsy too, with H. Houston Merritt's work showing biochemical means for controlling epilepsy; and stroke was yet pretty much "terra incognita" at that time. But the men-days lost by stroke's disablement were very great. People suffered thirty or forty years disablement usually because of neurological problems. The Congress was made aware of this when Pearce Bailey was a master politician at the NIH during the early 1950s.

SS: I remember that he was one of Senator Hill's favority witnesses.

FS: That was because, although usually he was a very quiet man, he was enough of a ham actor to know when to speak up, and he was a fine clinician. Robert Felix admired Pearce Bailey and talked to me about what a tremendous clinician Bailey was.

SS: So there was immediate support evidenced and demonstrated potential in the neurological field; therefore the Institute was created and you were there to help set it up. You used, of course, the same peer review system, that is, it was a centralized system?

FS: Of course. Right away we had a neurology study section. Just that simple. They reviewed basic science and clinical applications for a while. And when the work load got too large, we split it down the middle. At that time, you didn't have to write a bunch of memoranda to get something done, and you didn't have to clear it with the Secretary; you didn't have to clear it with people who knew vastly less about it than you did, and you got the appropriation money to run it.

SS: Would you talk a little bit more about that kind of "esprit de corps", that camaradie of those early days? It sounds remarkable.

FS: It was. It was like being in the Marine Corps all over again. I think it came from the recognition of what the goal was on the part of all of the staff. We didn't have Congressmen saying, "Well, you have been so successfful in clinical research and basic research, you ought to take over development." This is very disturbing to people who know the significant difference between research and development. Congressmen don't know that, but they figure that if you've been very successful over the years and all of your traditions are in the research area, whether clinical or basic, you could take on the applied and do the job just as well. This has never happened; I hope it never does. It really should be somebody else's job to take on the technology. I read the New York Times and every once in awhile I see some Congressman deciding what the NIH should do, even though it is not part of what the NIH people feel should be done.

SS: This is on outreach and demonstration?

FS: Outreach and demonstrations are allowable, but to take on the job of technology is primarily the job of industry, and of schools of science and technology like Cal Tech and the engineering schools. That's really where it ought to be done, by those institutions in conjunction with medicine, in whatever way is proper.

SS: I see.

FS: Now, the camaraderie came about because we were all working together to achieve a goal that we could easily understand. Science was capital "S" and research was capital "R", and we all thought we had the dread diseases on the run. I guess in terms of time, we did, because we really pushed back what Dusty Rhodes called the "areas of ignorance" or the "frontier of knowledge". We did push that back tremendously in the thirty years that I was connected with that program. Tremendously. We prepared everything, so that when we had a real emergency (like AIDS today) we were ready to do significant things.

The scientific base of hematology and blood-banking has been developed largely over the last thirty years. Thus, we had the knowledge and technologies to segregate blood from high-risk sources. We recognized that in those days, although we could never specify which direction things would go, we were a group who had more or less learned how to work together in various wartime occupations. We had a very fine place to do our work because the staff at the NIH were very encouraging. We were in a perfect ferment of intellectual and scientific activity, which to most of us was very exciting. We had a clear goal, which was to support health-related science. We had the money to do it, and the freedom to do it.

SS: But it is implicit in everything you say that you had your eye on health and health status. And science and research were the means.

FS: Every time you went past Building Number One you saw the National Institutes of Health. You didn't see the "National Institutes of Science."

SS: Let me ask you about the creation of the National Institute of General Medical Sciences. For a long time, I take it, the Division of Research Grants had some funds that it could control and make sure that no important bets were being missed.

FS: Right. It gave us the right to say that no good application goes unsupported. That's important. People would submit applications to us as a "port of first call", not a port of last call. We had the pick of the intellect, the pick of the ideas. Ernest saw to it that those of us who might have been over zealous would be reigned in, and he would use the funds he had to maintain the proper balance in support of all the various sciences.

SS: At some point, however, it was felt that it was needed to put general medical sciences, not specifically related to a particular disease, on an equal basis.

FS: Yes. This came about when we were starting an Institute of Child Health and Human Development in the early '60s. It was originally thought that through this institute, with the name "Human Development", we could support all of the categorical projects.

In basic, non-categorical research with the aid of DRG study sections, and with the newly established Division of General Medical Sciences -- around 1962 -- we could support all the research projects not logically falling within the assigned areas of responsibility of the named categorical Institutes. This plan, strongly supported by Drs. Ernest Allen, Ralph Meader, and other program directors, led to the tremendous research program expansion of the 1960s. All health science research grant appliactions approved by peer review had a reasonable opportunity to be supported. The simplicity of this idea generated reliance in these programs and in the credibility of staff operating these programs.

SS: So nothing had to fall through the cracks .

FS: Ernest Allen had the particular genius to recognize that certain groups of people can do much and do it well; that they should be left free to do it, not burdened with other jobs that are quite dissimilar, because they've been successful in one certain job. He protected the review function, when a lesser man would have reached out to support the grants themselves, and would have had "fights in his own family", instead of having institutes out there doing the job of making the final awards and the DRG making the scientific reviews. Allen firmly sponsored the support of non-categorical grants.

SS: And you were asked to come to this Division of General Medical Sciences?

FS: After I came back from a year and a half leave, I went into the Division of General Medical Sciences because it was felt all around that, after my record at Neurology, I was a builder. I learned that you don't build things by putting little increments of money in many little pockets. You have to "go for broke" occasionally. I felt very secure with my decisions on where our resources should go. My kind of a person tends to be frustrated by the continual emphasis upon analysis, because in the end, instinctive reactions have a reality of their own. Research is carried through by people not all of whom can express themselves equally well.

SS: So it took a while sometimes to see evidence of the value of supporting the non-categorical applications.

FS: We supported many projects that were not the highest priority, like those of young, unknown investigators. But they all had approval from the study sections. Our National Advisory Council was serious in its concern that good investigators find some level of support. We stretched the money to cover as many approved applications as we could.

SS: And I'm sure some areas of research received a great deal more support from Congress and private institutions.

FS: Yes. There were areas that had strong Congressional and university pressure and which were being supported in a purposeful, organized fashion by the other institutes with mandated program areas. It was thought that a neglected area of research and training existed in pediatrics and human development; of course, as with all areas of responsibility, we were interested in building solid programs of basic investigation supporting clinical research leading to direct clinical application of new knowledge at the bedside. With the great interest in children which resides in this country, plus President Kennedy's direct interest, the discussion and study soon developed into Congressional action and a bill was drafted in both chambers to establish a National Institute of Child Health and Human Development. This was, to the best of my memory, around 1962.

For the first time there was considerable discussion and speculation regarding establishment of an institute at the NIH dedicated to the support of research and training in the general (read "basic") medical sciences. This semantic jiggery-pokery was thought necessary in order that as little confusion as possible would arise between the proposed basic science-dedicated institute at NIH, and the National Science Foundation (NSF) which had been previously established and was striving to develop its own program goals.

SS: Understandably you wanted to avoid the appearance of having a rivalry in programs.

FS: Certainly. At this juncture it was clear to me that probably the fate of the NIGMS bill would be to die in committee unless some leadership was exerted on its behalf. It was just the time when having one new institute, the NICHD, would have answered many needs; and the President was on record as favoring it. I sought out Mary Lasker in New York City and asked for her help in pushing through the NIGMS bill. She said she needed time to think over what I said supporting the NIGMS, since she was not convinced of the need for it.

I got the best advice I could from Drs. Dempsey, Farber, Dr. Papper. These gentlemen were wise in things political and in the ebb and flow of science. Their advice, separately given, seemed almost rehearsed in concert: they each advised that the NIH's greatest challenges lay ahead in terms of research; the successes that its various institutes may have in clinical areas depended upon basic findings, not yet known, in all the sciences basic to medicine and health; and basic medical research must have its own institute with concern for the development of sciences basic to research.

Mrs. Lasker consulted Farber, Dempsey and Papper, and their views must have prevailed, for she threw all her resources back of both bills and marshalled extensive lobbying efforts, and two new institutes were added at that time (1962-63), NICHD and NIGMS.

SS: Marvelous. And what was Jim Shannon's attitude about the creation of these institutes at that time?

FS: I consider myself lucky to have had Dr. James Shannon as Director of NIH during those stirring times. He never asked either Dr. Aldridge or me for an accounting. I suppose that he trusted our good sense not to be clumsy. Somehow we always knew what he wanted to happen.

I might add, Eunice Kennedy Shriver served on the Child Health National Advisory Council, by Presidential appointment, naturally. Although the General Medical Sciences Council came on the scene together with the National Child Health Council, they sponsored quite different programs. Dr. Aldridge and I traded resources back and forth so each program prospered because of the combined strength of both institutes. Those were halcyon days indeed.

But back to Jim Shannon -- In the fall of '64 I was out fishing on the bay and Dr. Shannon put out a State Police call for me. They found me and said, "Come back to NIH." So back I went. He had a proposal which was, would I accept a directorship if I was appointed? Of course I said yes. That is how I was appointed Director of the National Institute of General Medical Sciences. The previous NIGMS director retired to unversity life after a distinguished PHS career.

SS: Would you talk about your friendship with Senator Hill, how you worked with him in various ways?

FS: In the early hearings when Dr. Shannon and others would make general statements, it happened that I was the one who would have to answer the questions. In the process I did not hesitate to talk to Senator Hill's Committee Clerk -- a powerful position. Such a career official is always a senior member of the professional staff of the U.S. Senate. Such a position permits much transfer of informal information back and forth. At that time, Mr. Harley Dirks was in the position. For a number of years, Mr. Dirks and I had a close relationship on all program matters. To this day, Mr. Dirks has a rare insight as to what Congressional reaction will be to program innovation; when to push and when not to push. Program justifications that Mr. Dirks recommended usually would be viable and would later become formal appropriations.

SS: How did Dr. Shannon feel about that? He himself had a lot of broad vision and a pretty big agenda.

FS: Sure, he did, but he didn't mind. How can you be against advances in basic science? You may as well be against motherhood! And Harley Dirks was the one person who would see to it that I had five minutes with "The Chairman". Senator Hill would not say much, but he would listen, nod his head, and say, "I'll think about it." He and Harley and others must have thought about it in agreement because it would come out that way.

SS: Would you describe how you managed to get the large sums for training grants?

FS: We discussed the role of the National Institute of General Medical Sciences with Dr. Shannon and with other staff as required; later on I would arrange to see Mr. Dirks. All of us felt that the way to build a training program is not through a series of individual fellowships. Seldom if ever do individual fellowship awards pay all the direct costs of the awards, and almost never pay adequate indirect costs. The training grant mechanism, however, more nearly reimburses the institution for its costs in training increasing numbers of scientists. Also, in my view, team training, many times clinical -- but not always -- is better done with the grant mechanism.

So, working with the leadership of the NIH and the PHS over the interval 1964 through 1967, we developed a very large training grant and fellowship program which supported several thousand trainees per year. The emphasis was upon the training needs of the basic health sciences; however we soon recognized that clinical training in special cases could greatly profit from one to two years of high level training in a science fundamental to that particular specialty.

NIGMS started a program of basic science training coincident with clinical training in anesthesiology. Great freedom was permitted to the individual trainees' programs. This program flourished, and the "bottom line" was that several generations of needed clinical specialists enjoyed a strengthening and upgrading of the scientific area upon which their specialty was based. This successful training program in anesthesiology was the model for others, including general surgery, radiology, and clinical pharmacology and toxicology. At one time NIGMS had 18 operating training grants committees as well as three or four fellowships committees.

It was a very large training program overall, and required labor-intensive dedication. The basic techniques of program management were adopted from the research grant program. The "sine qua non" of training award success is to identify those laboratories or departments which have first-rate records of training successful younger scientists. And we had to look at them carefully, since intensity and accomplishment in research do not always provide a training environment of the same quality or reputation.

III. Professional Accomplishments and Medical Science Impact

SS: Of this thirty years that you spent at NIH, what the the half dozen most important accomplishments that you were involved in?

FS: It was twenty-seven years.

I think the policy change in the operation of fellowships was one of the most significant things I did. Changing policy in fellowships placed new purpose on a United States Public Health Service research fellow at the predoctoral level. The new purpose was to pay the fellow to pursue his Ph.D. degree by doing the amount of work in the department necessary to be a good instructor. He was paid a stipend for that purpose only. This concept changed some of the cultural patterns in our country. Doctoral students no longer had to work in their departments as a source of instructors, nor did they have to work in order to earn their living at the same time they were earning their degree. This change of emphasis was markedly for the better.

The second thing I am proud of was the clinical research program in determining the primary causative agent underlying retrolental fibroplasia, an eye disease associated with premature babies. At that time, if such a child lived a normal lifespan, it would cost society between $120,000 and $150,000 minimum to care for such a child until maturity, even under depressed living conditions. It could cost easily up to $200,000. Such a premature baby was not expected to live a normal lifespan, however; nor was such a child expected to mature into an adult who could be productive in any real sense. Further, it was noted by experienced pediatricians that a significant degree of mental retardation accompanied the blindness. There developed a clinical opinion that a positive correlation existed between the appearance of the two disabilities.

In those days, 1952-53, it was estimated that about 1,000 to 1,200 premature babies would develop the disability. Some lower weight infants inexplicably did not do so, however. Accordingly, the experts judged that about 950 premature babies per year would develop that disorder and would require intensive care throughout their lives.

This became one of the first extramural programs in which we employed controlled experiments using humans. It was touchy, but I was largely unaware of the extent of this touchiness because I was brand new in my work at the time. But we were truly fortunate to have a statistician, Dr. Fay Hemphill, at Ann Arbor, University of Michigan, who set up statistical controls, so the research proceeded under the traditions and regulations of the time. Within eighteen months, we had some answers -- a very fast response to a vexing clinical problem.

About ten physician-scientists involved in the project decided to test the presence of high concentrations of oxygen in the prematures' bassinettes. Experimental results after a few months appeared to indicate that higher than atmospheric concentrations of oxygen continuously, over a 48-hour period, was the responsible agent. Before this was known, high concentrations were regularly used because the infants seemed to improve; their skin would be rosier and they seemed to eat and sleep better. Later data and study of results by the experimentalists confirmed these results and they were published. Dr. Hemphill's statistical experimental plan was credited with much of the success.

SS: And your experiment permitted you to identify this problem in just eighteen months?

FS: Yes, and after these results were published, the clinical management of prematures improved and the incidence of the disability decreased markedly. As I recall, the high oxygen concentration acted against the normal development of the retina in these cases.

SS: How fascinating.

FS: We found that oxygen poisons the enzyme systems in the developing retina, and therefore caused the fibroplasmic phenomenon. Everyone knew that the retina is really an outpocketing of nervous tissue from the brain. But the chemical significance of it in a premature child was not known then. Pediatricians, optimologists, and my good friend Fay Hemphill saved us, by seeing to it that we handled this information correctly. We hitched it all together with something we thought was fancy at the time: the teletype. When a newborn arrived who met the criteria, the teletype got busy and that baby went in the study or did not, based on a chance draw. Very soon we had something in the neighborhood of ten cooperating clinical units, and it didn't take long until we had the number needed. Then other tests we performed indicated right away that we had hit pay dirt.

We had a lot of freedom in those days to dream, to discuss, and to do important things. Needless to say, this research project had great significance. The leadership of NIH was more sensitive than I was to the portents of this study, whether successful or not. After a short period, perhaps a week from the time all the advisors had met and made their recommen- dations, Ernest Allen and I met on a Saturday morning to have a final review. After talking about it at length, Ernest said, "Fred, you either have to go or not go, based on the informed opinions and data you have. The ball is in your court." Following this no-nonsense statement, I returned to my office for a final, lonesome review of our status. Suddenly the decision to go ahead with the project was clear, so I called Dr. Hemphill in Ann Arbor and told him to start.

Our success with this project was used for many years thereafter for program support and budget justification. I'm sure that the Congressional committees and the NIH were thoroughly bored with our many recitations. However, no one ever faulted success in public.

SS: That is a fine example of a great success. What is another area you consider vitally important from your time there?

FS: The development of training programs when I was Director of NIGMS was a third important accomplishment. They were the least expensive and most productive tool in the development of scientists and in the conduct of research. A relatively small investment in training grants could result in a massive amount of research in later years. With the encouragement of Frank Yeager, Ernest Allen, and Ralph Meader, at one time we had as many as 10,000 trainees; all of whom were given access without hindrance, to the best that the training institution could offer. Because of their fellowships, their expenses, tuitions, and everything else were part of the package, all they had to do was to "be as good as you can be", the current Army slogan.

SS: How was this organized?

FS: The training grants were reviewed by national figures, scientists who had a superb training record. It did not take us long to learn that some laboratories were respected and very productive in terms of papers, but turned out very few trained scientists. Other laboratories turned out fine graduate students who became fine scientists, but who were not known as achievers in scientific research.

Dr. Vincent du Vigneaud, the Nobel Prize winner, discussed that idea with me, politely indicating that he was not as interested in predoctorals as I was.

He wanted postdoctoral fellows, and he hoped others would do training of the predoctorals while he concentrated on the training and use of postdoctorals. That is an example of a renouned scientist who did not emphasize the predoctoral training of scientists in his laboratory. For most university types, that was the hardest money to get, because they could get postdoctorals supported by research grants.

SS: What about fields of scientific inquiry? I assume that one thing you were trying to do was to make sure that certain fields that you thought were important were not ignored, and that good people went into them.

FS: It was obvious that there were about thirty to forty, maybe fifty, laboratories in basic biomedical science that did the bulk of the training for predoctorals. It was obvious, too, that there were at that time, about eighty medical schools, about sixty of which had active postdoctoral training through their residency programs. Thus we could determine in the clinical area where we were going and how we were going to get there. At one time, we were supporting work in as many as twenty fields. Usually the number was smaller than that, but I would say in twleve to fifteen areas I was closely tied in with the committees, and predictable shortage areas. Training is a long-term vernure, and should be approached on that basis.

One of our problems was what kind of training should be supported. Does one train students in the image of the preceptor, or try to train students who will be scientifically better trained for issues of the future? Over time it was clear the committees were concerned with the next thirty years of those young people's live -- not the last thirty years of the preceptor. But believe me, the choice was not an easy one.

SS: I'm sure it wasn't.

FS: The fact that we could look ahead opened the way for unusual achievement. It made it possible for us, working with F.O. Schmidt of MIT, to develop the field now called biophysics. F.O. Schmidt, one of the movers in this country, was brought in by Dr. Shannon, but they seldom agreed. I had problems running back and forth, to see to it that they both approved of particular plans. But after awhile, everything worked out well. In essence, we pushed the field, and I think through F.O. Schmidt and the research grants he obtained, which in turn developed into seminars and scientific meetings, the field sold itself. But it sold itself far faster than if we'd left it to the unimpeded or unhurried dynamics of scientific effort of the time. What F.O. Schmidt did in three years might otherwise have taken fifteen.

SS: There seem to be one or two such vital, dynamic personalities behind most of the great advances I've been hearing about.

FS: So, the three splendid advances I was directly involved with are: the fellowship program, retrolental fibroplasia, and the training grant program. Those are the three very important things to me. It's interesting that two of the three involve training.

Our Institute and its efforts were not central to the NIH's main interests as were the Heart and Cancer Institutes. In order for our people to go to scientific meetings and seminars, they would have to leave where they were and go to the main campus. I was invited to attend several and went as time permitted.

The big thing was the study sections. Study section meetings were of more use to the staff than were than were technical seminars on cancer chemotherapy. I hadn't the background for that, so I did the best I could to see to it that our people in training and research went to study section meetings. Those meetings were still the main business of the DRG and constituted the scientific center of our programs.

SS: I know the study sections were composed of some of the brightest and best scientists in the country.

VI. Other Important Advances and Discoveries

FS: I should share an important and interesting story. An engineer at MIT, who worked with a basic scientist at MIT, had developed an improvement on the computers of the time; large vaccuum-tube models for which you had to have tremendous air conditioning potential. I believe their names were William Shockley and John Bardeen, with the help of another scientist named Walter Brattain. They had devised a way to cut down the size of computers, taking up half the space on a desk. You could wheel one into a laboratoryl That was amazing at that time. Their dedication to their goals in the development area was such that they became uncomfortable at MIT and MIT became uncomfortable with them. The difference centered on policy on the proper balance between teaching and research.

Years ago I had a faint glimmering of the importance of the PDP-type computer, now a trade name. If it could be rolled into a laborabory, even on a truck, it was different. I arranged through Dr. Edward Dempsey, Dean of the School of Medicine at Washington University in St. Louis, for the scientists to be welcome there, and for the grants to support them. I have never asked the people at MIT how they felt about losing these people, but it seemed to me that initially they were relieved that another suitable home had been found for them.

The MIT Vice President for Academic Affairs, Charles Townes, was recipient of the Nobel Prize for laser development and he was a theoretician of considerable power. I met him at the Cosmos Club and discussed with him the manner in which one prepares oneself to take the next leap into the unknown. He told me how it happened with him. One day, sitting in the garden of the Cosmos Club, an idea on the laser "just came to him," and he went immediately back to his laboratory and did the work. He couldn't say exactly how, but if you work hard enough at it, and think hard enough about it, some new construction appears. I believe he was quite frank in his statements to me, and I would be quite disappointed to learn that he was pulling my leg.

SS: I'm sure a lot of great discoveries originated as a "flash of thought" at an unsuspecting moments.

FS: I'm sure you're right.

Dr. Egon Lorenz of the NCI became a friend and a source of scientific advice. He was interested in the working of the extramural programs of the NIH and the NCI largely because they seemed to be so fast-moving and had enlisted so many non-governmental scientists as scientific counselors and application reviewers (consultants). These programs were unknown to him in Germany where he was trained in his youth. In an effort to be helpful to him and his interests, I put him in touch with Dr. Delta Uphoff, who had been a graduate student with me under Dr. Curt Stern. In due time, Dr. Uphoff came to work at the NIH for Dr. Lorenz. She has had a fine independent scientific career in her own right.

Well, it seems that Dr. Lorenz, who also developed the theory that before tissues are implanted in or upon one animal or another; e.g. injecting bone marrow cells from one laboratory animal into another, the two genotypes should be compared. The desired effect — tissue function and growth in the host animal — only succeeded when a close match of the respective genotypes could be arranged. If the close match could not be found, the graft would react against the host. It has since been termed the "graft versus host" reaction, or the "gvh" reaction. The significance of this reaction and the need for as close a match as possible, genetically speaking, was obvious. Some bone marrow injections seemed successful in some humans and in others did not.

Now, at that time, the tissue transplantation research field was active with lots of genetic and immunologic experimentation being carried out within many animal groups by a series of research laboratory teams. This was, to the best of my memory, in the early 1960s. The Tissue Transplantation Society was holding its annual scientific meeting in New York City. Dr. Uphoff brought me a message from Dr. Lorenz urging me to discuss the possibility of getting scientific support for my "embryo project" of human gene typing. The ideas and concepts were theirs, the idea of a specific project might have been mine.

I met with Dr. Uphoff and Dr. Bernard Amos of Duke University, an immunologist. The three of us were in the habit of meeting at national or foreign gatherings. We were then 25 years younger, and our careers were exciting, and running in parallel. I had decided to try to find auxiliary funding and have a really nice, private, sit-down dinner and seminar about human gene typing. Could it be done, what laboratories, what costs, and how to do it? What would be the arrangements for a collaborative project?

Previous to this series of events, I had by chance become acquainted with Dr. Jules Stein, President of Music Corporation of America (MCA) and with Mr. Louis Friedland, a Vice President at MCA. I looked up Mr. Friedland and we had lunch together. Upon his request to know how things were with me at NIH, I told him of my problem of finding a suitable place for the private seminar and how to finance it. His response was immediate: "Have it here, and I will pick up the tab."! Until then, I did not know that he had an interest in the very fashionable restaurant in which we were lunching at the time. He asked only one favor: that he be allowed to sit in on the seminar as an observer. This deal was sealed by a handshake in ten seconds!

SS: So fate was on your side.

FS: Yes, fortunately. The next night, eleven scientists, led by Dr. Bernard Amos, Dr. Billingham, and Dr. Uphoff, arrived at the restaurant. Mr. Friedland and myself were joint hosts. After cocktails and a fine dinner, we reviewed the field of "human gene typing," the state of the art, the scientists who were the leaders in the field — five of them were in the room — and the costs of the management structure. To sum it up, the vote to start a collaborative program under the general leadership of Dr. Amos with DGMS/NIH to provide the money, under standard extramural procedured, but with what I termed a "Secretariat" to monitor progress and be generally useful, was unanimous.

In a year, we made great progress, and had located and identified crucial genes and their alleles; it was a very active group.

Sometimes success is too widely heralded. Dr. Shannon soon heard about this collaborative genetic study, and because it was beginning to involve human immunogenetics at the clinical level, he felt it would be better to transfer it to the Allergy Institute (NIAID) because their assigned program interests were closer to the area of human gene typing than those that were assigned in DGMS/NIH. In other words, I had strayed into another institute's area in my program development. So once again I had the personal satisfaction of starting an important scientific program with immense practical clinical significance. We regretted having it transferred to another institute, but we were soon busily engaged in computer development, the zone centrifuge, and many other program areas.

SS: And do you think the Allergy Institute accomplished as much as you would have?

FS: The NIAID did a fine job of carrying through the genetic program, and their final success can be noted every day in the newspaper when another clinical/laboratory program for organ transplantation is announced.

The Nobel awards came from the basic science research done on this problem: to Sir Peter Medawar (Mill Hill, London), and to Sir MacFarlane Burnet (the Hall Institute, Melbourne, Australia). Ultimately, the staff in DGMS and NIGMS who supported and furthered the early programs had great reason to be proud and pleased with the success of their efforts.

SS: Let's address the question of the particular NIH leaders: you were an activist director of institutes; others seemed to sit back and wait for proposals to come in and then send them to study sections. What is your assessment of that? Is that correct? You certainly were, but was the institution "activist" in this sense?

FS: I personally was an activist; others were not. I always relied on the advice of a small, informal group of advisors. After discussing the various characteristics of a proposed program development thoroughly, I felt secure in making the necessary initial moves. One always had the acumen and integrity of the study section and the council upon which to rely. The risks were minimal; the advances great.

Dr. Edward Dempsey, Dr. Sidney Farber, and Dr. Bernard Brodie, from time to time discussed program development in the general medical sciences area with me. These three gentlemen spent a good deal of time with me on my institute's affairs. Dr. Lewis Thomas and Dr. Emanuel Papper also gave much advice and guidance in their respective fields. Over the years there were others, and these specific scientists come to mind as examples. The NIH provided me with the most valuable and pleasant relationships I can imagine in a working environment. My experiences with these men and women were stimulating, exciting, and left me with a profound respect for them as individuals.

Dr. Dempsey, who mildly warned us about the mounting upward pressure upon indirect costs applicable for research grant awards. He pointed out, prophetically, it now seems, that universities can shift emphasis internally to provide a greater or lesser in-take of funds over time, in whatever way proved expedient or useful. Together with Dempsey, I met from time to time with Dr. Farber, Dr.

Philip Handler, Dr. Bernard Amos, Dr. Papper, Dr. Robert Aldridge, Dr. Manuel Morales, and many others who at one time or another were members of our institute-sponsored training committees or special review groups.

SS: It must have taken some doing to make this process work.

FS: Yes. As you have already noted, the questions or problems discussued, usually over several sessions, had to be chosen to fit the background and interests of the physician or scientist concerned. For instance, Dr. Dempsey felt more at home in discussions of basic endocrinology, electron microscopy, and the historical development of programs at Washington University School of Medicine, St. Louis. He was the Dean there and in parallel developed an extremely active laboratory group which turned out some outstanding teachers and investigators. Dr. Bernard Amos, an Englishman, as a physician didn't practice medicine himself, but consulted with attending physicians quite often. He was very helpful and patient with my deficient background in the sciences undergirding tissue transplantation. Of course, he did a great deal of down-field blocking in providing the credibility needed at that time for my program development. You may recall my previous comments concerning the human gene typing program and how that area of collaborative research was given a real push by our extramural research grant program.

One area of organization in a medical school that puzzled me for a time was the function of an active basic science program in an equally active clinical department which had a heavy clinical load for both full-time academic personnel as well as for clinical attendants. Dr. Amos provided a classical demonstration one week-end. At that time, his lab and activities were in the surgery department but not in surgery. My sessions at his laboratory were valuable and useful immediately as I was soon requested to undertake a program review of 42 medical schools with Dr. Edward Dempsey.

SS: So, your relationships with university scientists were not only pleasant, but of great use to you as learning opportunities.

FS: Absolutely. It was likewise with Dr. Sidney Farber. For about a year I would appear at the Children's Cancer Center on Saturday morning and return to Bethesda late that evening, having spent most of the time as a guest of the Center. He even assigned me an office attached to his suite of offices for my convenience in reading or working. Dr. Farber discussed work progress and plans with his staff on Saturdays; I attended these and found them most valuable. When Dr. Farber visited his patients in the "Good Sam" which was attached to his laboratory, I was sure to be in tow. The interchange and patient care discussions between the doctor and the attendings, residents, and fellows were invaluable to me when I was later responsible for the general clinical research center program at the Division of Research Facilities and Resources. Once Dr. Farber took me to a meeting addressed by Mme. Indira Gandhi, and at another time the visit of Dr. Hami Bhabbha, a Laureate in Physics. Visitors of this caliber always elicited lively and exciting discussions and differing viewpoints regarding research, social matters, and politics.

I mustn't forget to metion two other very important personalities from whom I gained wisdom, Dr. Lewis Thomas and Dr. Michael DeBakey. Not only could I call on them for advice, but each consented several times to tetify before the Congress on behalf of NIGMS. Their strong support of the singular role of NIGMS was deeply appreciated by the staff. Notably, favorable comment concerning the collaborative roles of the basic researcher and the clinician were generated when members of the committees receiving testimony noted the clinical interests of these individuals in behalf of fundamental investigation and the training of younger scientist educators.

SS: This is excellent background as to what basis you felt secure in being activist and developing new programs and new fields.

FS: I cleared everything with my council. We had a night-before-the-council meeting where, if there were any problems, they were dealt with privately. We would fight them out in private, and the next day we would vote on them officially. People wondered how we could come to a consensus in five minutes' discussion! The formal minutes had an addendum with all the discussion, of course. We discussed policy issues on that previous night, and I would have those scientists the council wanted to hear give their opinions "in camera". That was exciting.

V. Leaders and Leadership

SS: You have mentioned the early leaders, Dr. Allen being principal among them in a way, and Dr. Van Slyke, and you mentioned Dr. Shannon, who had the longest tenure, of course, as Director of NIH.

FS: Dr. Shannon and I had an unusual relationship. I had great respect for him; in particular I respected his ability to see over the horizon -- farther than I could. He could anticipate trends better than I.

SS: Was Shannon a great leader? I find him a visionary on the one hand, but vague in his vision on the other. Compelling in a sense, in public interactions, but amorphous about particulars. I am not a bit surprised to have heard you say that sometimes when Dr. Shannon testified before Cogressional committees you, or someone else, had to come in and give concrete examples of what it was he was trying to convey.

FS: Dr. Shannon had to lead off Congressional testimony that extended across the length and breadth of the NIH. In so doing he would seldom have time to touch upon the details of certain programs in the various Institutes. He seldom could be expected to know the details of program interlocking between two or more Institutes. Quite often, it was the Institute directors who answered in the required detail. Dr. Kenneth Endicott and Dr. Donald Whedon were excellent at testimony and could easily explain in lay terms the rather abstruse technical matters. In the early days, 1948-5Ø, Dr. Norman Topping did most of the testifying, when the NIH was smaller and less complicated. But Shannon was excellent with the general goals -- the overview. The NIH was simply much too complex for him to know all the necessary particulars for testimony.

Now I wish to get on the record the tremendous contributions made early on by Senator Lister Hill and Congressman John Fogarty. These two men made the development of the health science base in the U.S. and in other parts of the world possible through the NIH. Their reputations are well known and their individual effectiveness has been attested by others many times. Without their insight and skill in using the power of their respective positions, the growth of the NIH would have been slowed or perhaps even truncated by other agencies; i.e. the National Science Foundation, and the scientific leadership apparent now would very likely never have come about. The U.S. and the world owes much to these two men with whom it was my great good fortune to work.

FS: This is a great enterprise. My task is once again to build a scholarly record that others can use in their historical studies. And second, to really get in the public record in a dramatic way, if I can do it, this inspiring story. So it is a pleasure to talk with people like you who know so much about it.

FS: Marty Cummings was one of the people who didn't get the credit he should have gotten, in my view. He had great insight into the problems that faced us as a nation. He developed the National Library at a time when other institutes were being held in check, and he developed acceptance that the Library, the building, and its function should go forward even though other institutes were not encouraged to the same extent at that time. It was a great accomplishment.

SS: Indeed, it was.

FS: I left the NIH to be president of New York Medical College; after which I came back to government at the behest of Dr. Vernon Wilson, then head of Health Services and Mental Health Administration (HSMHA). Dr. Wilson had worked with me previously in the construction grant program in DRFR. I gave up educational administration in New York to return to Bethesda to work on the large programs at that agency. Having bridged the gap left by the hasty departure of the previous president, and set up the necessary systems of financial control as recommended by Price, Waterhouse and Company, I set about the construction needs of New York Medical College. In due time, about three years, I reported for duty at the Parklawn Building in Rockville.

SS: I would say your career has been quite significant in terms of advancement in health research and discoveries.

FS: Whatever I have done is because the leadership at the NIH and under Wilson permitted me to do it. Whatever you write, make sure you emphasize that. I did things that would otherwise have been difficult to do, in organizational terms, and I was able to because Vernon Wilson said, "You're probably right, so go ahead and do it." And my reaction would be, "Don't we have to clear this?" But I had then spent some time with businessmen -- bankers and lawyers and industrialists in New York, and when they gave you a job to do, you were supposed to do it. Not come back for pats on the back or to solve problems. Just do it right. So I got a little bit more aggressive in this second time back. The signal difference, you might say, between the federal sector and the private sector is that there is significant accountability linking responsibility and authority in the private sector. If you take the advice of a committee, you don't come in and tell Mr. Frank Glock, the executive vice president of the Bank of New York, that you took some bad advice. He was the one who told me, "We've hired you and we're paying you one of the highest salaries for a Ph.D. in New York, and we expect you to do the job."

SS: Let me leap on to impact questions. The research grants program that you helped to develop in general in the early days, the late '40s and early '50s, you next expanded with training grants, research fellowships, and with program grants. These programs have evloved further, and now in the aggregate are the way we, as a nation, spend most of our money on biomedical research; they are the way that we think we are going to conquer disease and improve the peoples' health. Would you talk a little bit about the cumulative impact on health and disease problems and on institutions?

I might add that, as an Alabaman, I have watched the development of the University of Alabama. When I was growing up, here in Birmingham, the medical school was in one building, and when I was in college it was in one square block. Now it is this vast and successful, productive enterprise we are sitting in right now. I think it is one of the better ones in the country. I know from direct experience that the key to that has been the research support from the National Institutes of Health. Senator Hill helped see to that. The good people here were the real reason this center became first rate, and the state of Alabama as a whole can certainly say the NIH has made a difference in their community and their lives.

FS: I don't think any one man can depict effectually what the impact has been. In those days, coming out of World War II, universities were not used to supporting research in the depth and breadth that rapidly came to be possible. For the first time, scientists had the freedom to seriously undertake long term research -- money for which was the hardest money to obtain. From the '5Øs through the '6Øs, a scientist could pretty well count on a moral commitment to have his grant paid, because it would be paid if it were possible. The NIH staff made good on that because we knew that the scientists had to be able to count on a commitment. In 197Ø under Richard Nixon, there were obviously some technical changes.

With the grants, scientists were freed to dream and to plan their careers. The universities tried manfully to provide the supporting money necessary to pay the full costs of research that were not paid by the indirect cost plus the direct cost. The number of scientists that became active is, I think, exponential over the years. Universities bought land and developed animal colonies, built laboratories, built hospitals, modified everything in sight. The $5Ø million a year that went for construction during the three years Frank Schmehl and I were associated with it, was the catalyst which brought $5ØØ million of general university construction over the years. With one brick, we provided nine bricks -- through private donations, state funds, and the $5Ø million!

So, words do not easily explain the freedom for scientific investigation that came about, because essentially, if the scientists were any good, had good ideas and were technically competent, money would be found. And they relied on it, sort of as an article of faith. We, in turn, responded because when we had a commitment, it was a sacred thing -- as sacred as things can be in this secular world.

I think the effect on universities was definitely one of freedom to investigate. The enthusiasm for the U.S. Public Health Service as a part of universities came from being able to look to Washington and not see scandals or sharp dealings, and to feel that we approached fully what they considered ethical performance of our duties.

SS: You've given me a wealth of information, Fred. Thank you for your time.