AN INTERVIEW WITH DR. RALPH G. MEADER

BY STEPHEN P. STRICKLAND, PH.D.

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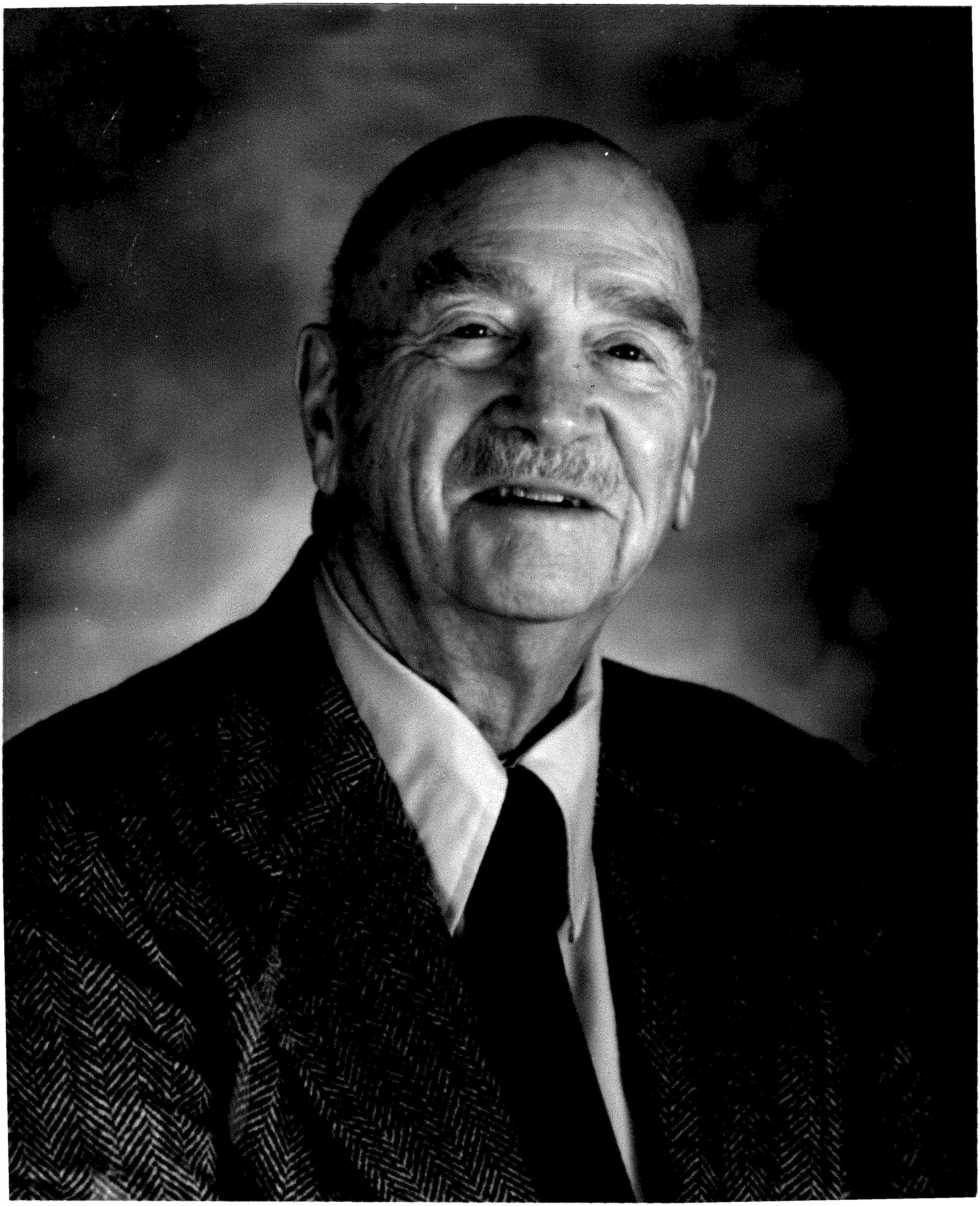

Introduction and Biographical Sketch

This interview with Ralph Gibson Meader, Ph.D. 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. Meader, 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. Meader's education and training, in biology and science, were largely at Yale University where, as a graduate student, he was a fellow in neuroanatomy at the medical school beginning in 1929 and, after receipt of his Ph.D., Instructor of Anatomy in that school. In the period 1937 - 1948, when he was Assistant and then Associate Professor at Yale, he also became involved in the Jane Coffin Childs Memorial Fund for Medical Research, being Assistant Director of the Board of Scientific Advisors of that Fund all during and for several years after World War II. In 1947 Dr. Meader became Special Consultant to the U.S. Public Health Service, assigned to the Cancer Research Grants Bureau of the National Cancer Institute. In 1948, he came full time to NCI as the Chief of the Cancer Research Grants Bureau and remained at the Institute through 1965 when he went to the Massachusetts General Hospital as Deputy Director for Research Administration and Executive Secretary for the Commission on Research. He remained a consultant to the National Cancer Institute for many years afterwards.

Dr. Meader thus brought to the Cancer Institute experience in a non-governmental research institution, and helped to shape the Cancer Institute's approach toward research grants. He was, as others have attested, especially interested in making sure that talented individuals and worthy proposals were given full opportunity for consideration regardless of whence they came, institutionally or geographically. The interview with Dr. Meader was enhanced by the recollections and contributions of his wife, Olive Root Meader, who was for many years an Executive Secretary of study sections at the NIH.

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WASHINGTON, D.C.

Interview by Stephen P. Strickland with Dr. Ralph Meader

June 24, 1986

SS: Dr. Meader, I would like to talk with you and Mrs. Meader about the history of the grants program and related matters in three parts. First, I'd like to hear a bit on your professional background and training, and how you came into the program. I think that aspect, in the aggregate, is very interesting. The individual stories are interesting, especially when you look at the backgrounds of all these great people. Second, I'd like to hear about the origins and evolutions of the grant programs and how the study sections worked, and what models were used and later how the study sections and the peer review might have been used by others. Then in the third part I'd like to talk about results: what has the grants program meant for health and science and institutions? Part one, then, is just how you came into the Public Health Service.

Olive Meader: The thing I think is so interesting, in my knowledge of the background of all these people, is that this whole system didn't exist when they were growing up and going to college and medical school. And none of them came prepared to do this particular job; they just sort of drifted into it. But because of their varied experiences and backgrounds, and different expertise, they built a core of dedicated, devoted individuals who worked together remarkably well.

I remember when we were thinking about going from Yale down to Washington, we all had the impression that civil servants worked an eight-hour day, and took lots of coffee breaks, and that was it. I thought, "That would be wonderful," because at Yale, the only way I could be with Ralph was to go the lab and sit with him. When he was a graduate student, I sat at the foot of his cadaver! But at NIH we found people like Dale Lindsay, who came from a school of agriculture, and was interested in insects and entomology. And Ernest Allen, who had been a French teacher, but who was drafted into Public Health Service, and they found he was a gifted administrator. He was a wonderful person working with people, and a very intelligent individual. All of the others, who came from different sources, were never thinking of getting into this.

SS: You were an M.D. or a Ph.D.

Ralph Meader: Ph.D.

SS: And tell me how you decided on the biomedical sciences as your field.

RM: When I was in high school in Traverse City, Michigan, we had a science teacher who was a retired physician from Chicago. He anwered my silly questions, and I was always curious about how the body worked. So I would ask him all kinds of questions, and I continued that interest as a sort of sideline. I didn't study nature much as a kid. I did sports and other kids' playthings. But when I went to college, I was working my way through at Ohio Wesleyan, and I tried to do as well as I could in my studies rather than in athletics. At first I thought I might want to be a lawyer -- apparently that runs in my family's blood because two of my brothers are lawyers. One was a congressman and another was with the OSS and CIA. Another brother was a physician, and still another was a lawyer. So there were two lawyers in the family.

SS: Was your brother in Congress from Ohio?

RM: No, from Michigan. From Ann Arbor in the second district of Michigan. He still lives in Washington on Tennyson Street.

SS: So, you went to Ohio Wesleyan and then Yale Medical School?

RM: I decided that I didn't want to do public speaking, and go into law, and I became very much interested in zoology under Professor Rice, and in other sciences. But zoology was my major interest. Professor Rice created a scholarship at Woods Hole Marine Biological Laboratory and I was the first recipient of that. I spent the summer of 1924 there, just before my senior year. Then I was an assistant in zoology during my senior year. I was going to have to work to pay off my debts, and he suggested that I be a teacher, and he helped me. He got in contact with many people, and I applied in a number of places — some very interesting ones that I came in contact with later in another capacity — but eventually Professor Morrill at Hamilton College in New York wanted me. He was about 75 years old and did not want to retire yet. So I went there at the age of 21, as an instructor in biology, and stayed there for three years becoming an assistant professor. I spent my summers at the University of Michigan and got my master's at Hamilton College with those summer studies in bacteriology and biology and parasitology up at the Douglas Lake Laboratory. I taught at Hamilton, then I was invited to teach at Wesleyan University. The President of Hamilton College, Frederick Carlos Ferry was a good friend of President McConaghy of Wesleyan, and for some reason or other they were talking about one of their professors, Professor Goodrich, who was going to go on sabattical leave, and apparently President Ferry commented on me and so McConaghy asked Goodrich if he knew me in that summer of 1924 I spent at Woods Hole, in an embriology course that he taught.

There really was coincidence all along the line, which I think pervades human life anyway. So Professor Goodrich wrote to me and I accepted the invitation to go there, teaching half time, and having half time free to begin my graduate studies at Yale, which I did at the Osborn Zoological Laboratory. That was a wonderful year; it was the year Olive and I married, and went as bride and groom to Wesleyan University. I was then 24, and she was 22.

The experience there was very stimulating. Professor Goodrich was there half the year before he went away, and we became very good friends. I began my graduate study, and I took a seminar course in comparative neurology with Professor Harold Saxton Burr in the Department of Anatomy of Yale Medical School, and got very much interested in that. I started to get restless because it was going to take me a long time to complete my degree at that rate. So I decided that I would borrow the money to go to the University of Michigan Medical School. Olive was supportive of me, but just about the time I was making up my mind to do that, Harold Burr said that he had just gotten a grant from Dudley Blossom in Cleveland, who was a supporter of the Cleveland Symphony Orchestra among other things, and was a brother-in-law of a man named Bingham who had done oceanographic trips with fisheries men collecting Caribbean and other fish. Mr. Bingham gave this collection of fish to the Peabody Museum at Yale. And Dudley Blossom gave Professor Burr, who had become interested in comparative neurology when he was on sabbatical in Holland, funds for a fellowship to study fish brains.

I had been really struggling as to whether I wanted to go into medicine or not, because I feared that if I did, I would be so quickly in debt that I would have do to family practice. I couldn't imagine myself holding old ladies' hands and trying to make them feel better, and giving them pills that I really didn't know very much about -- that bothered me. So here was a chance to do research. I had always enjoyed teaching, and wanted to combine teaching and research, so I spoke to Professor Burr and said I'd be interested, and he said, "You'd better go talk to your wife about it." So I did, and she was supportive, as always, and was even excited about it.

I had thought I shouldn't marry until I completed my graduate studies because I couldn't support my wife. But she said, "Well, I'm going to have to work anyway, and I'd rather work and be married, than work and be single." So she became the secretary of the biology department at Wesleyan that year. When Professor Burr decided that he'd take me on, he spoke to Lottie Bishop, who was the executive secretary for the medical school, and they needed a record librarian in the New Haven Hospital. They were really expanding their record room. Olive had done some hospital record work in the University of Michigan Hospital in summers, and also had biological background at the biology department of Wesleyan. So they asked her to take on the job.

OM: They were moving into a new building, and changing the system of coding of the diagnoses.

SS: So you were ready for anything that was interesting and challenging?

OM: Yes, and I learned a lot of medical terminology.

SS: So you actually got into it gradually.

RM: She helped me translate German scientific papers. The first year I was at the Osborn Zoological Laboratory, they had a journal club that was run by the head of the department, Ross Granville Harrison. You may have heard of him. He studied how the sheaths of nerve fibers got established, and he was a dry, reserved sort of person, but also a very able man. He had wonderful students, and in his seminar, after they'd had one or two journal club reports from other graduate students, he said, "I think we can all read English. We should be reviewing foreign language articles in science." Another of my friends and I were due to come on in a week or two, and we went to him and said, "What would you recommend?" He gave me a reprint of two small volumes of a scientific report, and I gave those to my friend to see. He also handed me another one, which was about twice as thick as the other two! So I tried to get the other ones back from my friend but he held onto them! They were all in German. Mine was written by a German and his was by a couple of Swedes in German.

SS: So you got your Ph.D.

RM: I got my Ph.D. at Yale working on the nervous system of fish. Just before I finished my thesis, one of the instructors left to become head of a department at the University of Montreal. They asked me if I would fill in for him, so I did, and stayed on for seventeen years at Yale.

SS: Is that where you knew Dr. Stanhope Bayne-Jones?

RM: Yes. I knew him as a professor of bacteriology. I didn't take any courses. Then he became Dean, and I got to know him a little bit better. In 1937 I had a Rockefeller Foundation fellowship, something that Dr. Burr arranged with Bob Lambert, who was with the Rockefeller Foundation at that time. So, I was going to go abroad, and I had been an instructor for six or seven years. This was in the depths of the Depression. When I talked to Dean Bayne-Jones about what I should do, he said I could have a leave of absence without salary. "But before you go," he said, "I think your salary has been much too low. I think it should be increased by $300-400, so that's the rate you should be paid by the Rockefeller Foundation." This told me more about the man than I knew before.

In 1938 I worked for the Central Institute for Brain Research and the University of Amsterdam in the neurology department. In the fall of 1939 I returned to Yale to teach. I saw Dr. Bayne-Jones occasionally. He and I belonged the Nathan Smith Club, which was a medical history club of faculty and students, and the Beaumont Medical History Club.

SS: I am interested in this because he was really one of the leaders in American medicine. I want to get it in historical perspective, because he later was very important as an advisor. He sat on the National Advisory Health Council and the Heart Council.

RM: Then the war came in December of 1941. On about the 14th of January 1942, on a Saturday afternoon, I was in my laboratory looking at brain slides, when the phone rang, and a voice said, "Ralph, this is B.J. I would like to speak with you. Would you be willing to come up to my office?" I said, "Surely." I couldn't help wondering what was going on. I'd been worried about doing war service, whether I should stay and keep on teaching, or go off in some crazy way to satisfy my patriotic feelings. Olive was an invalid at that time, in '42, and we had a child. So I went to see Dr. Bayne-Jones, and he told me that he was being called back into the Army, and was looking for someone to carry on the administrative work for the Jane Coffin Childs Memorial Fund for Medical Research, of which he was the Director. He had ceased to be Dean, and became the Director of the Board of Scientific Advisors. He asked me if I would undertake it. I said, "I've been wondering how to contribute to the war effort, and if you think this would do it, all right." He said this would, and that he could leave with a good deal more satisfaction. He thought the war would prevent very much more expansion and activity, because everything would be concentrated on the war effort. He thought I could probably do it on weekends.

SS: So that you could keep on with your research.

RM: Yes, and teaching. So, Dr. Bayne-Jones left on February 1 and I went up to begin to learn what was needed in the job. I made frequent trips to Washington and consulted him on the telephone. He continued to be closely in touch with it. It was a wonderful experience for me.

SS: What was his position during the war?

RM: First he was a colonel in the Division of Preventive Medicine. He was eventually promoted to Brigadier-General, after being passed over earlier because a stenographer left his name out of a list. So he got it in the next selection. He became chief of the bureau division there, and he became president of the U.S.A. Typhus Commission, and then a number of other things.

I used to go down to Washington every once in awhile to review the report to the Board of Managers, and to review what I should do in connection with grant applications. I began to travel about to various places in connection with grant applications. B.J. sent me up to Boston to the New England Cancer Society meeting, of which he was a member, and that's where I met Shields Warren, who became a very good friend, as well as Joseph Aub and Austin Brues, and a number of other people who were subsequently very active in the war and in cancer research. He sent me down to the Rockefeller Institute to talk with Peyton Rous and others. Then I'd follow up on the grant applications and visit the applicants, trying to get some assessment of their work.

SS: When you had grant applications to the Childs Memorial Fund, did you informally have people that you would refer appications to for their judgement?

RM: No, not at that time. I would review the application and try to get additional information and I would visit the applicant and prepare a review. When the Board of Scientific Advisors met, they were individual scientists and they had copies of the applications and usually they had competence among them. Ross Harrison was one of them; Peyton Rous was another; John Morton was another; as was Rudolph Anderson, who dissected the coding of the tubercle bacillus to find out what was the active agent that caused the formation of tubercles. Dr. George Smith and Dr. Milton C. Winternitz, former Dean of Yale Medical School were also on that list.

SS: Was everybody from Yale?

RM: No, John Morton was from the University of Rochester. And Dr. Rous was from the Rockefeller Institute.

SS: Did the Board of Scientific Advisors act as your peeer review committee?

RM: Yes. And I remember very well, at one review early on, I thought it was necessary to tell them what I had found on the visits. Peyton Rous said, "Ralph, just wait a few minutes. I want to hear everybody's comments on the science, and the capacity of these individuals to do it, before I hear your report of the visit." He didn't want to be prejudiced. And didn't want me to prejudice everyone else by expressing opinions. It had a gread deal of influence on my relationship with the National Advisory Cancer Council and other organizations, to just wait and supply information if they needed it, but not unload my opinions and my findings prematurely.

SS: So that was a very important experience, running this Fund and working with the scientists?

RM: Oh, yes. Two or three members of the Board of Managers, the laymen, would be approached by somebody who had a cancer cure, and they also asked me to go and look into this type of thing, which led to some interesting and bizarre experiences. I learned to be very skeptical and critical in my judgements. Peyton Rous said once, "Ralph, you're much too vulnerable." And I said, "I suppose I am, but I haven't had the experience you have yet, but I'm getting it."

SS: At the end of the war, what was the next transition?

RM: Then Dr. Bayne-Jones came back, in 1946. I prepared to spend more time on my research, and had a lighter teaching load. My research work had gone to pot since I didn't have my Saturday afternoons and Sundays. I was going to go back to my previous pursuits, when Dr. Bayne-Jones said, "There's going to be an expansion in medical research. You have had an opportunity and responded to it, and I think there will be a need for you in medical research." And he said, "I'd like to have you stay here with me." He was also resuming is job as editor of the Journal of Cancer Research, which the Childs Fund and some other agencies had helped to create. So I thought about it. I enjoyed my work; it was stimulating. I was learning so many new things since I was forced to go into aspects of clinical knowledge that I had ignored as far as possible because I was concentrating on my own small field. I enjoyed working with him as well, so I decided to continue with him.

Bayne-Jones then received an invitation to become the President of the Joint Administrative Board of the New York Hospital Cornell Medical Center. He told me he was going to accept it, and he hoped that I would succeed him as Director, but he didn't know what the choice of the other members of the Board would be. Two other Board members I didn't mention were George M. Smith, who was a legendary character in his own right, and Milton C. Winternitz who was the Dean of Yale Medical School for a considerable time, preceding Dr. Bayne-Jones. They were almost all Johns Hopkins people.

George M. Smith was not from Johns Hopkins. He was the Medical Director for the Scoville Brass Company in Waterbury, CT, partly because he married into the Goss family that ran it. But he had been the first Director of Research at the Barnard Free Skin and Cancer Hospital in St. Louis, and he had also been overseas during the war, with a Yale unit. He was the one who got Dudley Blossom to give the money to Yale for Dr. Burr to develop the study of fish brains. He was interested in human and animal tumors and the New York Aquarium, and he was the second Executive Director of the National Advisory Cancer Council. He also directed the Anna Fuller Fund, which was a cancer research fund in New Haven. So, he was a fascinating character.

When the Childs Fund had its tenth anniversary, they invited Dr. Roscoe Roy Spencer, the Director of the National Cancer Institute, to attend, and he was accompanied to that by the man who succeeded him: Dr. Leonard Scheele. They spent some time with me in the Coffin Childs Fund office, and were very much interested in the information I had accumulated on cancer research in the United States. Whenever I went on a project site visit, I would also find out about all the other cancer research that was going on in that institution. I had the files arranged by the name of investigator, by subject, and by institution, sort of cross indexed so I could learn more about it. Drs. Spencer and Scheele were interested in this. Dr. Dyer was then the Director of the National Institutes of Health, and he and Dr. Scheele spoke to me about the possibility of coming to the National Cancer Institute. I made the decision chiefly by going to talk with various members of the Board of Scientific Advisors to see what their feelings were about whether I should go to one place or the other. I got the impression that the Board of Scientific Advisors was going to recommend to the Board of Managers that Dr. Winternitz be made an acting director for a year while they figured out what they wanted to do. Mr. Barkclay, who was the lawyer for the Childs family, and had helped to set up the Fund, said, "I don't think you're ready to be the director of this foundation. I think you need another year or more of seasoning. And I wouldn't recommend anybody to go to work for the Federal Government. I knew two people who went there: one was no good before he went, and the other was no good after he went." So the Board did decide to ask Dr. Winternitz to be the acting director, but he wanted me to carry on as his assistant. So I decided what ought to be done, then I'd consult with him and he'd say, "O.K., Ralph, go do it." He relied on me almost completely. During that year, we had continued talks with the people at the National Cancer Institute. Leonard Scheele finally persuaded me that I ought to go there.

SS: What year was this?

RM: 1946-47.

SS: And Len Scheele was Assistant Director of the Cancer Institute.

RM: He became the Director during that year. In June of 1947 I was invited to come down to the National Cancer Institute for a meeting of the National Advisory Cancer Council, which at that time consisted of only six persons. It was at the Childs Fund that I developed the technique of asking scientists who were particularly competent in the area of a particular application to review it and provide their expert opinions. I usually tried to get two or three opinions. When I went down to the National Cancer Institute and sat in on the National Advisory Council's review of applications, I was sort of horrified to find that they were getting only one expert opinion, and that was from the internal staff of the National Cancer Institute. In one instance they got opinions from two of the biochemists -- and they were diametrically opposed. One of the members of the Council said, "Well, I have more confidence in Dr. So-and-so," and I didn't like that very much.

SS: A little too subjective.

RM: Yes.

SS: This was a pivotal point, I think, with the construction of the extramural program. There were obviously some models that were used to build the study section process around. Peer review as a general proposition was obviously accepted. The Board of Scientific Advisors did that, and the National Research Council had a medical advisory group, and they also did that during the war.

RM: Yes, and for the cancer field it had a Committee on Growth that Dr. George M. Smith and Bob Winnernitz helped to establish.

SS: It sounds as though it was still relatively informal. You tried to get expert advice, and would choose somebody that you happened to know was interested in the same field and knew something about it.

RM: Yes. And that carried with it a certain amount of danger in being accused of stealing someone else's ideas.

SS: While you were still at the Fund, when you proposed that there be two or three readers who would then give reports, which the Board of Scientific Advisors could then review, were they pleased with that?

RM: Yes. In fact, they often told me the person's advice they would like to have. But I couldn't have imposed anything on them.

SS: Was the number of grant applications growing? Why did they decide at some point that they themselves didn't have enough wisdom to make these judgements?

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RM: It was actually growing. It didn't drop off during the war. Both the number of applications and the number of grants increased. In some instances, we didn't get additional review, if there was more than one person on the Board who was competent in the field. So, I got these additional opinions chiefly on the basis of requests from the members of the Board, or at my suggestions and asking them if they would like it.

SS: And since you had already begun keeping a record of all people doing particular kinds of cancer research, you probably had as good an inventory of people as anybody. Maybe better.

RM: Well, I know it impressed Leonard Scheele a great deal, and they did make use of my files.

SS: In 1947, then, Dr. Scheele asked you to come down to the Cancer Institute and you decided to go. When did he want you?

RM: Dr. Price had been temporarily made chief of the Cancer Grants Branch, and Dr. Scheele wanted me to be Scientific Director for that Branch, to get the scientific review. I think it was in June of '47 that they asked me, and I had been appointed a consultant because they had not been able to get a position established. I really wasn't free to go because I still had my obligations to the Coffin Childs Fund. Finally, it was either George M. Smith or Winternitz who said "Do both." That meant I wouldn't be able to do as much teaching, but I could often combine doing project site visits on a National Cancer Institute application and a Fund application, and split the costs between them. So I did that for almost a year. My base was still in New Haven, but I made frequent visits to Washington to work with Dave Price and others. We were not using any study sections at that time. There weren't yet any study sections that were appropriate for review of the cancer applications. Either the subject material didn't fit, or the members of the study section had no knowledge of the aspects that would be important in relation to cancer. We initially started to get review from individuals within the Cancer Institute or NIH. I really wasn't very comfortable with that. I thought, "These aren't the only people who are expert in the field." But this was the pattern that they'd been following, so I expanded it to get at least three opinions.

SS: So, you had one opinion from one person in the department saying, "This is good," another saying, "This is not good," and a member of the National Advisory Cancer Council often saying something else.

RM: And we had more confidence in that man's judgement, I think.

SS: At that point, what was the approximate size of the research grant program at the Cancer Institute?

RM: I think the appropriation for cancer research in 1946 was either $100,000 or $200,000; I think in '47 it was $500,000, and in '48, I think it was up to $1 or $1.5 million. So it was growing exponentially.

SS: Did the Cancer Institute have some of the wartime contracts that had been transferred to NIH?

RM: I don't think so. Some of the people on the staff of the Cancer Institute did get sent out to do other things than the war effort -- like Leonard Scheele and a number of others.

In getting more reviews, I got into some of the internal politics and pressures, and one of the heads of the laboratories complained to Dr. Dyer that a man that he wanted to get promoted was not producing as many cancer research publications because he was doing too much reviewing for me. So the Associate Director, Dr. Norman Topping, the man who became the President of the University of Southern California, was asked for additional help to get these reviews, and he said, "Use study sections." I knew about study sections, and knew Dave Price and Ernest Allen used them, so I said, "Dr. Topping says I've got to use your study sections for review. I can't have staff people enough to get the reviews." I said, "You don't have study sections to review maybe 75% of our grant applications." They were all malaria and other things. There was a Biochemistry Study Section but none of its members were interested the biochemical aspects of cancer. So they said, "We'll create study sections to meet your need and we'll add people to the study sections. You and others should nominate people." They did create these study sections, and in 1949 they also created a Morphology and Genetics Study Section. Dr. David Price was then the Director of the Division of Research Grants, and he went to Brown University to talk with Professor J. Walter Wilson to ask him if he'd become Chairman of it. He accepted and served for a long period of time. He served on the Morphology and Genetics Study Section, and the Radiation Study Section, and they wanted him to become a member of the National Advisory Council for the research construction program. I had already let them know that we wanted him for the Cancer Council the following year. He ended up serving on both.

SS: In the days when there were only two or three people reviewing proposals or grant applications, did the person reviewing the application know the name of the applicant?

RM: Oh, yes, because, after all, the criteria were: Is the project worth doing at all? Is it worth doing it well, and can the person who is applying actually do it?

OM: The study section people were anonymous, but not the applicant. Individual opinions were never noted, but anybody could look up and see who the members of the study sections were. The Executive Secretary of the study section presented what was called a "pink sheet" and this was his summary of the points of view of the study section in general. Sometimes there was a majority opinion, and sometimes there was a minority opinion, too.

SS: The cancer program was growing, the NIH program was growing; had they already decided that the NIH Division of Research Grants would review the cancer proposals as well?

RM: Yes. We took this to the National Advisory Cancer Council to make sure that this would be acceptable to them, and they said they didn't have any experience with it. They didn't know what kind of review they would get from the study section, so they carried on both programs for a year to compare them and see if they were adequate. I did a lot of work with the Executive Secretaries and with the study sections, and I think I really antagonized some of them because I was very critical of what was going to be presented to our Council. Sometimes I had to invoke the support of Ernest Allen and Dave Price to make clear the kind of quality that we wanted. Subsequently all the Executive Secretaries became good friends, I think.

SS: How would you characterize or encapsulate what you did? It wasn't a study section, but it was obviously a peer review.

RM: It was peer review. I didn't interpret the reviewers. I gave the Council the raw data from each of the reviewers so they could make their own judgements as to the adequacy of the reports.

SS: In other words, you were to get two or three reviewers whose reports would be presented to the National Advisory Cancer Council?

RM: It was a cumbersome way of doing it, but I was trying to make sure that they had a balanced point of view. I had seen enough committees to know that one very vocal and verbal person could sometimes snow a group.

SS: Was there a point at which there was requirement that said the Council would review study sections' recommendations. The law itself didn't provide for study sections, but it did say that no grant could be made unless it had the approval of the National Advisory Cancer Council or the National Advisory Health Council. Then, in '46-'47, Ernest Allen, David Price and Dr. Van Slyke created a study section mechanism.

RM: What I know from listening to Dr. Van Slyke, and Ernest Allen and all the people involved, is that the Committee on Medical Research, of the National Research Council, had a lot of contracts -- in malaria and infectious diseases and other things -- and they had individual committees for these. The committees were made up largely of people who had contracts who would be brought together because they were the experts in the field.

SS: Today that would be considered a conflict of interest, or, as it was called back in the '60s, "convergence of interest." It was only a small community of people who knew anything, and you had to share information.

RM: They were eager to bring in other people, but there weren't that many to be brought in, because most of the capable people already had contracts. That was to be phased out after the war was over, and Dr. Dyer was a member of a group that was reviewing this management situation, and they were wondering what to do about the contracts. Dr. Dyer said "We'll take over the management of them." So the National Institute of Health took them over and called them study sections.

SS: It was later that it became the National Institute<u>s</u> of Health.

RM: Yes. At first the Executive Secretary of a study section was a staff member of the National Institutes of Health; for instance, Ken Endicott was the Executive Secretary for the Hemotology Study Section -- he was a pathologist. But eventually they got other secretaries, and these earlier NIH staff men served as a liaison, supervising, but not doing the actual administrative work of getting the review. That was the initial development of the study sections.

SS: You had some acquaintanceship with the Rockefeller Foundation. Did the Rockefeller Foundation do what the Jane Coffin Childs Fund do?

RM: Not that I know of.

SS: I can't find any earlier formal model of a study section or system, so I guess the wartime experience was the first.

RM: More often than not with foundations, it was one scientific advisor who did the review. I know that was true for the Anna Fuller Fund, and some other foundations. My contact with the Rockefeller Foundation was chiefly through Bob Lambert, who was simply an administrative staff person. He was an able research scientist who had taken on administrative work, and as far a I know this was how it was done.

The only model I can think of is the Committee on Research of the National Research Council, who had some committees. Around 1942, when the American Society for the Control of Cancer changed its name to the American Cancer Society, under the stimulus of Lois Maddox Miller and Mary and Albert Lasker, the American Cancer Society asked the National Research Council to set up a Committee on Growth. Dr. Winternitz was Chairman of that for awhile, as was Dr. Cornelius P. Rhoads and others. That was a parallel type thing. They had similar subcommittees; the Committee on Growth had subcommitees for different subjects similar to the study sections.

SS: So once the study sections got going, they became the principal mechanism through which grants were reviewed, but there was nothing in the law that required it. Did it become just practice or custom?

RM: It was an administrative decision, I think. I remember a particular example: there was a Committee on Viruses and Ricketsial Diseases, and the NCI had about three or four grants that had been recommended by the National Advisory Cancer Council previously that were coming up for renewal. We had two or three new ones as well, and they went to the Virology and Ricketsial Diseases Study Section. I was appalled when I went to that, that they turned down all of them except two. In one they said, "This is a very good man who has done excellent work in another field and he ought to get back into that field and out of this." In another one they said, "He's a good man, but he ought to be tapered off. Let him go." About others they said, "Don't do it. They're no good." Well, there weren't any people on the Study Sections who were really interested in cancer. They thought it was a sort of an aberration. I took these to the Council, and I'd gotten my usual type of review from people who were interested in viruses and cancer and weren't involved in these particular applications. The Council said, "We'll approve all of these."

This was during the one year of having a dual track. In the meantime, this sort of shook up Dave Price and Ernest Allen, and they said, "We need to add some other people to the study sections to get a balanced point of view." Some of these applications came back the next year, and then they had the benefit of additional inputs and the loss of some people who had opposed them. I think all but one got approved. Two members of that study section came to me and said, "Here's another field where you ought to be supporting research on viruses and cancer." One of them was being supported by the Veteran's Administration and didn't need any more support, and in fact there was no arrangement then for support of somebody in another government laboratory. That did get worked out later, though.

SS: In your first year in '47-'48 what was the relative size of the private sector support in cancer research? Was it roughly comparable? When did the government leap way ahead of support by the foundations and the Cancer Society?

RM: I think that began in about '49. In '48 or '49 Rod Heller and I were asked to make a projection for the budget for the coming year. We thought we had just about saturated the market. The new crowd of investigators that we had been supporting as research fellows wasn't ready, and how much more could we spend? Well, somebody gave me a formula schedule and said, "Make projections." It was about $3 million then, and the next year it went to $4 million, then to $7 million, and to $9 million. It was then that I was asked to make this running leap into the wild blue yonder. I ended up with an estimate of, I believe, $18 million. I thought, "Anybody who sees this will say 'Ralph has lost his mind'." I was almost embarrassed to turn it in, but that was the formula arrived at. So I turned it in, and Mary Lasker and Sidney Farber and others got it raised to $21 million. I remember the Council was shocked. They said, "We're not going to change our standards just because there's more grant money available." The Council could override study sections at that time, but they were depending on them. Later the rules got tighter so that the councils couldn't really override the study sections, but they could send them back for reconsideration. We actually awarded about $18 million in grants — just what my system had projected. And I remember being called by a congressman who said an institution in Dallas had applied for grants, and was turned down. He did not understand why we would turn back money instead of spending it on them. I replied that we had five applications from that institution and three project site visits, including one by members of the Cancer Council. Three of those applications were approved and two of them were turned down after they had been reviewed by three different groups. I asked him, "Would you have wanted them supported anyway, even after three different groups of scientists did not recommend it?" He said, "Well, I guess not," and he added, "I didn't know you were doing such a thorough job of it." And it wasn't the institution that complained about it; it was one of their patients.

SS: So within two or three years after you got there, the composition of study sections was modified and continued to be adapted to new needs. The Division of Research Grants created new study sections as you needed them.

RM: Yes, but they were eager and ready to do it.

SS: There were, of course, differing philosophies of people in grants management; some wanting to go forward in a very direct way and get experts who would tell scientists what they ought to do...

RM: Let me read something about that to you from this excerpt from an article from the Journal of the National Cancer Institute, Volume 19, No. 2 in August of 1957, in an article called, "Research Grants Branch of the National Cancer Institute":

> With the expansion of research on cancer and means for aiding it, the question is sometimes asked whether it would not be more efficient and effective to combine all sources of data into one agency, with one set of advisors. This concept of efficiency and simplification fails to consider the dangers of the concentration of power in the hands of a relatively small group and the proportionate loss of flexibility as the size of an organization grows. It makes the erroneous assumption that some individual or group of individuals is omniscient in determining which aspects of research are more important and the extent in which they should receive research support. It overlooks also that very important characteristic of Western civilization which stresses the significance of the individual, whether it be how he shall do research or how he should provide aid to those who do research. It is important to the research that there shall continue to be multiple sources of aid, and that the investigator shall have recourse to

another reviewing body if the work of his proposal is not recognized by one or more organizations he approaches for his support. One has only to review the history of medicine and science to derive numerous examples of ideas now fully accepted that when they were proposed they were not accepted by a large segment of the learned men of the time. While there is room for individual or group initiative and freedom for the operation of private enterprise, it is important to have voluntary communication and cooperation in working toward the common goal. The National Cancer Institute has welcomed and encouraged and learned from its companion organizations. It has fostered close relationships and sought to integrate its efforts with those of other agencies with like purposes. This liaison is effected formally through exchange of staff representatives to advisory committees and councils, and informally through continual staff interchange of information and through cooperation and developing support of programs of mutual interest. This avoids unknowing duplication of effort, and may make joint financing of research possible if one agency would not or could not finance alone. The philosophy of philanthropy is an engaging subject which cannot be discussed in detail here, but which has many facets that are pertinent to providing aid to research to alleviate the ills of mankind. The provider and the recipient of aid need to have as clear an understanding as possible of what each expects of the other. If one thinks he is buying or selling such a product as a cure for cancer, a contract that can protect each party is almost essential. If, on the other hand, the giver is providing funds to enable the user to direct his efforts in the best way he knows toward a goal that has been agreed upon, the grant-in-aid amounts to an act of faith. In such cases, both parties recognize that the goal may not be achieved in the specified time. Few situations are so clear-cut that they fall into one or the other type. Yet the emphasis toward one or the other type will largely influence the conditions under which aid is given and accepted. The degree of freedom allowed to the recipient in the use of funds is likely to be in proportion to the degree of confidence the giver has in the integrity, wisdom, and competence of the recipient. This may be almost inversely proportional to the number of restrictions that are designed to retain some kind of control by the giver.

SS: I take it that this is exactly the point of departure when you and Ken Endicott had differences of view about how to proceed. I talked to him about the development of the contract instrument, and he is actually quite proud of developing the contracts.

RM: Particularly while he was Director of the Cancer Chemotherapy Center.

SS: Is it your judgment that the use of contracts in the '5Øs and '6Øs got too much attention or damaged the grants program in any way?

RM: That's hard to tell. There doesn't really have to be much difference between a contract and a grant. The Office of Naval Research gave contracts that were just as free and as liberal as the grants of the NIH. Some of those were models, in a way, which affected the grants program at the NIH. The Office of Naval Research, as I understood it at that time, did not have a formal advisory body. They had a director for that segment of their scientific program who did consult with an advisory group which did not have control. The manager for that program could make a grant, in the form of a contract, without specific advice, and that was one advantage of the contract system. It also was one of the dangers of the contract system; it allowed for favoritism or mistakes of judgement of one individual. But they did use advice from their colleagues from scientific Institutions. So, the instrument itself is not the important thing, because it can either be a very liberal contract or a very restrictive grant. It's the administration and the interpretation of it, and the extent to which the administrator feels designated and responsible to make the decisions. And to recruit people to do the things he wants to see done.

SS: At certain junctures it seems to me, from listening to people talk and in reading a lot of the documents and knowing some of the history of NIH, that there was a very activist attitude. You were speaking earlier of you and Dr. Price going out and talking to chairmen of medical departments about particular things; finding out who was doing what and telling them about your new programs. That's very activist, and I assume that one of the things that you were trying to do was to stir up interest.

RM: Yes -- to let them know about opportunities, and, in some instances, to try to get them to do something. I never felt like saying, "Come on, boys, you'll get this if you do it the way I tell you to." I would consult with groups of them about opportunities. At some point around 1960, Jim Shannon, I think, wrote into the appropriation language something about institutes of biological research to be promoted at universities. It got put into the Cancer Appropriation Act. So it was part of my obligation to try to carry that out. I remember going to a number of universities to talk about this. In some places they were very receptive, but in other places they didn't want to have anything to do with a "sub-organization".

The same thing happened later when the cancer program developed cancer "centers". There were some advisors who were associated with these centers who tried to develop criteria by which these could be inserted at university settings. Most of the advisors were from individualized centers, like the M. D. Anderson, and the Sloan-Kettering Institutes. There was an association of cancer institute directors who wanted to promote this type of thing, and it bothered me to see non-academic people trying to run academic institutions. Often they had a vague relationship to an academic institution, but they were promoting their own kind of organization. I had always envisioned a university or college as being a collection of scholars, who had minds of their own, and would not be led astray, even by money, into doing things that they didn't feel desirable or capable of doing. But money is a very attractive force, and gets in the way sometimes.

SS: Dr. Knutti was talking about the first grants made in arthritis, for example, and he said that in the first couple of years, the Institute wanted to encourage more research on arthritis, but almost every proposal that came in had to do with the effects of cortisone on arthritic joints because that had been a recent finding, and had gotten a lot of attention, so people responded and tried to do variations on it.

RM: I recall that when the work on cortisone was done at the Mayo Clinic. I believe through Mary Lasker's influence, and others, the NIH gave a contract for the manufacture of cortisone. And that produced a sudden drop in the cost of it. So there was money left over and it was a part of what went into the expansion of the NIH program at that particular time.

SS: The question I'm getting at is: when is it appropriate for the National Institutes of Health to try to encourage the expansion of particular fields, and how is it appropriate for them to do so, and when is it not appropriate?

RM: I considered this to be self-regulating. The best scientists could not be bought. You couldn't induce them with money to do something that they didn't think was worthwhile spending their time on. So, often you would attract the less capable scientists who were glad to have somebody telling them what to do. They might have had the technology to do things, but they wouldn't have as many ideas, and wouldn't take their results further than that they were originally contracted to do, like the brightest scientists would. I can't really criticize them; I'm an example of someone who was led elsewhere from what I originally thought I wanted to be, which was a professor of anatomy. But I became a medical administrator, so I'm an opportunist, although I didn't think of myself as that. It was more serendipity that I was asked to do something that I enjoyed doing. Nevertheless, if you do persuade people to do something who don't think it's a good way to go, you'll get the less qualified people.

SS: Is that true with respect to younger investigators, do you think?

RM: Yes, to a considerable extent. Many times they haven't really established their firm interests completely, so they're flexible enough move around in their studies. They usually don't have as much of a commitment in their careers. But I felt that it was a self-regulatory situation, like the body's control mechanisms which prevent excesses in one direction or another. You could find examples throughout history of fads, where everybody rushes into an area until it peters out. But the more competent people would have seen this before and concentrated on another field.

SS: My reservation is the case of cancer virology, because that is a pursuit that has gone up and down in popularity. Fifteen years ago it was a very big enterprise and people thought they were onto the answer. After a decade when nothing really was discovered and they couldn't identify the human leukemia virus, although they had found it in monkeys, it just dropped off. Now it's going back up again. So I wouldn't think that you could describe these areas of research as fads.

RM: Perhaps that's not a good word. I should have said "opportunities". But usually there comes some kind of a breakthrough, at least in the technological sense -- like the creation of the electron microscope or analysis of immunochemistry. New techniques open up new opportunities.

Tissue culture is a good example; there was a technique that was used to solve a particular problem, and then was expanded by Alexis Carel, and eventually it became a technique looking for a problem. Tissue culture provided the opportunity, and it was tissue culture that made possible the polio vaccine. That came about because George Guy was interested in cultivating tumors and eventually cultured some cells from tumors; I believe it was cervical cancer of Helen Lane, and he called it "HE" and "LA", and they became the hela cells. They were the cells on which Drs. Enders and Weller and another doctor did their work for which they got the Nobel Prize, and then Salk took advantage of that and developed his vaccine. √

Another important aspect is the introduction of concepts. Work on viruses goes back to 1907 or earlier, initially done in Denmark, I believe. Then in 1912 Peyton Rous found a chicken tumor that he could pass in cell-free filtrates, and became it known as the Rous sarcoma. This was a sort of isolated thing. Early on, Peyton Rous was a member of the Board of Scientific Advisors at the Jane Coffin Childs Fund, and encouraged the Fund to support Francisco Duran Reynals, who had come over from Barcelona, Spain, I believe, and was at the Rockefeller Institute but needed a more permanent place. So they set him up at Yale in the Department of Bacteriology semi-independently. He was watched over by the Jane Coffin Childs Memorial Fund's scientific advisory group. Francisco kept developing new information about chicken tumors in particular.

Harry Green, a professor of pathology at Yale, was interested in rabbit tumors and he adopted the methods some other researchers had for growing tissues in the anterior chamber of the eye. There was much less immune reaction there, so these things would grow well and you could observe them as they grew.

SS: You think, then, that if there is an effort to put the spotlight on one particular problem, or a particular method of investigation, that's all right and that it's actually a self-correcting process? And if others come up with ideas?

RM: Yes, and others will modify it.

SS: I don't fully understand yet what the point of tension was between you and Dr. Endicott. What did he want to do that you didn't want to do?

RM: I didn't think there was any individual who had the wisdom to guide this. I am always leery of a man who's going to ride his own horse and get everybody to get on the horse, or get on the horse's tail and ride along. It's a personal reaction on my part of not having that kind of confidence in any individual, myself included.

SS: That could be a product of your own experience in other settings.

RM: Yes, but I know you can go further along alone than you can with a group of critics, if there are people associated with you who are freely exchanging ideas. I felt that there was a sort of "scientific intelligence," so that if there was a promising development, either in the concepts, philosophy, or in the technology, which would allow exploration of other concepts that had not been explored further because the technology wasn't conducive, then the new concepts could lead to the development of technologies for its "exploitation", and vice versa.

Scientists as a whole are alert and communicating with each other all the time. In fact, sometimes I'm afraid they spend so much time communicating they don't have time to find out more to communicate! They are eager to explore all of the new avenues, so I have a greater confidence in that kind of ferment and you can find examples to support it. You can also find examples where one person has harbored one idea, and has driven on it all by himself for a long time before he convinced other people. The virus theory had that problem. It went up and down.

Joseph Beard is an example of one who had made the "mistake" of going into cancer research at Duke. He had done some very fine work on influenza and other diseases. He was an M.D. but he used physical chemical principles and isolation of these organisms, then studying their effect on tissue. But he got interested in the viral aspects of cancer. It was his project that that the early study sections said, "Let's go ahead and support it so he can get it finished and get back into something more worthwhile." They did come to see the significance of it after awhile and became good supporters of it.

SS: In this kind of scientific enterprise, centralized in the sense that the NIH became, in the '50s and '60s, the primary vehicle for the support of research, and therefore, despite the fact that there were study sections comprising peers from all of the major institutions from across the country, how does one lead, and what are the characterisitics of leadership in science in that situation? I'd like you to talk about particular individuals.

RM: I think, from the administrative point of view, if you want to find out what direction to go, you consult knowledgeable people from a variety of fields and hope that you find some people who will say, "Here is an opportunity that needs exploration." If you get enough of that kind of opinion, then you find the leadership -- by finding people who have some degree of compatibility in their ideas, and then expose it to others. If others agree, they may want to participate in it.

With leadership, I think it's crucial to be sensitive -- hearing with your ear to the ground, and talking with competent individuals or anybody who has ideas. If you find a strong consensus, good leadership is saying "Let's explore it " -- even if you have to divert a portion of your activity to that. Then you get the salesmanship of a director of the institute, or the Surgeon General who says, "This is something we want to push, so we're going to talk about it to the politicians." And you find people like John Fogarty and Lister Hill who are sympathetic with it, and persuade them.

SS: You were at the NIH for 17 or 18 years?

RM: My first employment was supposed to have been in August of 1947, because I went to the International Cancer Congress in St. Louis, and I think I had already begun to work on their research grants. I left around September 13 of 1965.

SS: In that period of time, who at the National Institutes of Health were the great dynamic forces?

RM: I think Dr. Dyer and Dr. Van Slyke were the major forces. Dr. Dyer got Dr. Van Slyke to come to the NIH. I met Dr. Van Slyke when he was at Staten Island with the Public Health Service. When I was trying to keep the Coffin Childs Fund informed I went to see him and his colleagues there. I met him again when I went down in June of '47 to the NIH. Dr. Dyer was greatly interested. He had been a venereal disease researcher, and Van Slyke had been associated with him at the venereal disease office. Rod Heller was with them, too. A lot of people came out of that specialized program.

SS: It was a big program, as well.

RM: Yes. Dr. Dyer got Dr. Van Slyke to come to help with this program that they'd undertaken with the contracts from the Committee on Research.

SS: That program was parallel to the National Research Council. There was something called the Office of Scientific Research and Development, and the Committee on Medical Research was a part of that.

SS: Dyer, Van Slyke, and Heller were certainly pioneers, but I can't quite get a picture in my mind, whether, when they took over the contracts, they had in mind consciously and purposefully building this great enterprise.

RM: I don't think so. If things seem to grow, and if they're good, they'll expand. They may expand too much, and get too musclebound. If you get large enough, people notice you and begin to control you. Politicians and people concerned about it want to make sure you have rules that you can live by.

I think when you start out you usually have a group of "eager beavers" who believe in what they're doing. They don't fully visualize what the effect is going to be, but they see a definite opportunity to do some good. So they go ahead and run with it.

SS: So that was the spirit that prevailed in 1947-48.

RM: Yes, I think so.

I might add perhaps another remark on this. When I went down to the National Cancer Institute as a consultant in June of '47, I was taken with and charmed by David Price and Ernest Allen. They treated me wonderfully. They were courting me for the position there since I hadn't made up my mind to come. But that was when I learned that there were government employees who had the same kind of eager beaver attitude that I had. I didn't convince myself that we were going to solve the problem of cancer in two years. At the dedication of the Sloan-Kettering Institute, Dr. Cornelius Rhoads predicted that within ten years they would have the cancer problem solved. I remember being shocked by the temerity of anyone saying this. And, you know, we are still waiting for it to be solved.

SS: The enterprise that you joined certainly grew rapidly over the next 17 years or so, and it must have been increasingly hard to manage -- just the administration of it. But one of the things that your approach toward grantmaking insured through the study sections was that you only supported good science. Were you always convinced in each case that it was only good science that you were supporting?

RM: I think so. I can't think of an instance in which the study section scientists and the council made recommendations to the Surgeon General that I would have opposed. I was temperamentally or experientially trained and inclined to seek advice from people in whom I had confidence, respected, and whose comments and rationale made sense to me. If there were applications in which I felt there were defects, I tried to make sure that they were brought out for consideration. If the grant were recommended in spite of those, then this was a good bet, but one that we would have to be concerned about.

SS: In any case, you had protection against unwarranted pushing.

RM: Yes. I was in what might be called a very favorable position. If an individual applicant received a grant, he gave me a lot of credit for it. If he didn't receive the grant, it was because of "those s.o.b.'s who didn't understand and weren't sympathetic."

SS: The main thing that everybody focuses on is the research grants. But then there were the training grants, traineeships, and construction grants, and research fellowships. The thing I know least about is the construction grants program.

RM: I had a good description of that which I'll read to you: "After World War II funds were made available in increased amounts in the form of grants and fellowships by a number of agencies to assist the experienced investigator to get back into the laboratory and to enable young scientists to continue their training interrupted by the war." This is also from the report of the cancer research grants branch of the Cancer Institute that was in the journal of the National Cancer Institute, Volume 19, No. 2, August 1957, on page 243. Going on:

> With this emphasis on research and personnel, it became apparent in a few years that space for research was becoming a limiting factor. As a result, the National Cancer Institute became the first of the National Institutes of Health to award grants for the construction of research facilities, when the appropriation acts of 1948-49 and 1950 provided funds for that purpose. The Acts described the program's mission as follows: "To make grants-in-aid for research and training projects related to cancer, including grants for drawing plans, erection of buildings, and acquisition of land therefore." From December 1947 through 1950 the Surgeon General awarded, upon recommendation of the National Advisory Cancer Council, 64 grants-in-aid, totalling $16.3 million. Support was awarded to build clinical and laboratory facilities for cancer research at 49 non-federal institutions in 27 states and the District of Columbia. In nearly all instances, the grants supplemented local funds. Although the largest portion of the funds was used to improve laboratory facilities, encouragement was given to the development of a better balance between laboratory and clinical facilities. Grants were made chiefly to medical schools and hospitals closely affiliated with medical schools. In these cases, personnel and facilities were available for both clinical and laboratory research, making possible a broad, well rounded approach to research problems and facilitating the rapid transfer of laboratory discoveries into clinical evaluation and use. Some grants were not associated with medical schools or hospitals. The only restriction placed on the use of these construction grants was that the facilities constructed must be devoted to cancer research.

When we got this construction program, I needed somebody who knew something about construction. So I was able to get William W. Payne to come to work with me as my associate. Bill Payne was a civil engineer in the Public Health Service. He helped me write the report I told you about. He was a wonderful person, both in his skill and his personality. As a matter of fact, when he wrote letters that I signed or when he wrote reports, I felt as if I'd written them, and I rarely made any changes in them. He subsequently handled the construction program for the Heart Institute and then helped to develop the rules for the NIH institute-wide health research facilities grants.

SS: What kind of review system did you have for construction grants?

RM: The construction grant review system combined the technical scientific side of construction with sort of general research policy and relationship to cancer. All of these were criteria.

SS: Was there any suggestion in this program, in the laws or in the appropriation committee's language or elsewhere, that there should be any consideration given to spreading them out across the country?

RM: There was that thought in mind. I don't think it was explicitly spelled out anywhere. There was concern for that, but there was more concern for excellence of some aspect of their work — in their clinical research, or in their laboratories. The initial construction program came about because Congressmen Keefe from Wisconsin, who was, I believe, on the Appropriations Committee and was very familiar with the McArdle laboratory at the University of Wisconsin, felt that this laboratory would be greatly strengthened if the clinical research facilities were expanded so that the laboratory findings could be carried over into the clinic. There were some strong clinical people there to help develop that, and he virtually pushed through the first appropriation -- for about $3 million -- with the McArdle Laboratory in mind. I was sort of horrified by this. I think this kind of legislation for particular institutions is very bad. Well, Len Scheele, the Director, took a more philosophical view and said, "Sometimes you have to take advantage of opportunities. They're good people there aren't they?" and I said yes, in the basic science area, but I didn't think they were as good in the clinical area. "Well", he said, "This may be a way to perhaps help them become better. And this is a way to get a construction program going." The next year it was up to $8 million. (Not all of the $3 million had gone to the McArdle Laboratory.)

As I recall, the first grant went to the Jackson Laboratory in Bar Harbor, Maine, where Dr. Clarence Cook Little had developed a fine laboratory that was destroyed in a fire that devastated the island in 1947. The first construction grant went to rehabilitate that.

SS: Your mentioning Dr. Scheele again reminds me to ask you about him and other Surgeons General. He must also have been very dynamic and far-sighted.

RM: Yes. He had a very persuasive manner of talking, and he knew his facts. He was very intelligent and very affable. I remember sitting up the better part of one night talking with him. He was not arrogant or over impressed with himself; he was a very fine person.

SS: You've now identified Drs. Allen, Dyer, Parran, Van Slyke, Scheele, and Dr. Price. Who else was particularly important?

RM: Rod Heller came on duty in April of '48 as director of the Cancer Institute. I developed a great respect and affection for him and felt that it was reciprocated. We had a very good relationship, but it was hard for us to find time to talk together. If we happened to be in the mens' room at the same time we'd hold little conferences there! We agreed with the man who said, "That's a place where you know what you're doing."

Rod was a Clemson graduate. He was at Johns Hopkins when he was told that he had Hodgkins Disease and probably wouldn't live very long. Somebody had done a biopsy on his neck node and found some Hodgkins cells, so he thought he had a death sentence. But it never happened. And we had from 1948 - 60 when he went up to Sloan-Kettering as president of the joint boards. But we kept in touch even after that.

Dave Price was a very good friend all the time that he was in the Public Health Service. I enjoyed working with him. There was just one time that I got annoyed with him. He had been made Associate Director of NIH in charge of extramural affairs and I felt the need for some help in the cancer research program, which had expanded. For a time I was able to keep up -- I knew every grantee -- but when there got to be close to 900 grants, my mental computer couldn't take it anymore. I asked David for some kind of help, and he said, "Ralph, you're doing such an excellent job, I don't know if we can afford someone else like you. It costs money to administer programs, and it costs more to do it the way you do it, so our overhead is really high." I said, "Dave, is that your opinion, or is that what you've been told to say?" And he said, "Well, it's both." I told him I was really disappointed, because I had been concerned to do the very best job I possibly could. A little while after that some new programs were dumped on me by congressional action. Then they decided that I did in fact need some help, so I got it. But this is a unique incident with David Price, because he was a wonderful colleague and superior officer.

SS: I think that covers most of what I wanted to talk about. What else do you think we should get on record?

RM: Training grants and research fellowships were very important developments. Then there were teaching grants, and clinical traineeships, which were not part of my responsibility. They were part of the cancer control division. They were also responsible for the cancer teaching grants. Evenutally they disappeared and that division was wiped out, so that fell into my program.

Then the research training grants came along around 1956 or '57. I think there had been some training grants at the Heart Institute that Dr. Van Slyke had promoted. They were allocating extra money for research fellowships, and he thought that was a good time to start some cancer research training grants. This meant sending money out to the institutions and let them select the cancer research people they were going to train, to provide the facilities for research, some stipends for the trainees, and some salaries for the teachers. I had help in this from Ken Endicott and others. We realy invited people to submit applications, not with promise of results, but to help us define and regularize the program.

We had a special meeting of the National Advisory Cancer Council in Philadelphia to review these applications. Dr. Isadore Ravdin was a member of the Council at that time, as was Dr. Sidney Farber. They had been pushing for some of this cancer training money. The Council found some applications to be good, and some to need further development. I think they ended up approving two or three of those first training grant applications, which didn't exhaust all of the money. Then we went on exploring the opportunity and doing the promotion, and we formed a committee to review training grants. This was like a study section. This was not a disciplinary approach, but multi-disciplinary with a focus on problems of cancer.

The research professorship was to be a lifetime program that Dr. Shannon, as Director of NIH, imposed a five-year limit on. The next lower Research Investigator Award was the interim transition from fellowship to full faculty status somewhere. Those programs came in at about 1960. We had the cancer centers and research professorships and research investigatorships, as I guess they were called. I remember in 1960 trying to mount a 4-program series of project site visits by Council members and other scientists that I'd recruited to help the council members. I nearly went crazy that fall, because all this money suddenly became available in August and we were going to have a Council meeting in November, so we had to get review of these. Sometimes I felt like a train dispatcher then.

SS: I can imagine.

RM: One of the very important developments in the grant evaluation process was the devising of the "priority score" technique and its modifications, by which recommended grant applications were rated and arranged in order for award as far as the available funds would permit. I believe this system was initially introduced by Dr. Frederick L. Stone in the review of fellowship applications. It was modified over time to minimize the inequities that might occur because of the human variability that seemed to be characteristic of certain study sections. This device for determining an order of priority for award and payment of recommended grants has been described, analyzed, praised, and cursed. I will comment only that it seemed to be the least unfair way of determining how insufficient funds should be allocated. It was not meant to measure quality of research.

SS: I think you've satisfied just about everything I wanted to know and more. We've covered a great deal. Thank you for your time, Dr. Meader.

RM: My pleasure.

CURRICULUM VITAE

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EDUCATION:

1925 - A.B. Ohio Wesleyan University
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1927 - A.M. Hamilton College
1932 - Ph.D. Yale University
1956 - LL.D. Philadelphia College Osteopathy
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EMPLOYMENT HISTORY:

1925-27 Instructor of Biology at Hamilton College
1928-29 Part-time Instructor of Biology at Wesleyan University
1929-31 Fellow in Neuroanatomy, Blossom Fund, Yale University School of Medicine
1931-37 Instructor of Anatomy, Yale University School of Medicine
1937-45 Assistant Professor, Yale
1945-48 Associate Professor of Anatomny, Yale
1942-48 Assistant Director, Board of Scientific Advisors for the Jane Coffin Childs Memorial Fund for Medical Research
1947-48 Special Consultant to the U.S. Public Health Service as Science Director of the Cancer Research Grants Bureau of the National Cancer Institute
1948-60 Chief of the bureau of Cancer Research Grants
1960-65 Associate Director of Grants and Training
1947-65 Executive Secretary, National Advisory Cancer Council
1965-76 Deputy Director for Research Administration and Executive Secretary for the Commission on Research, Massachusetts General Hospital, Boston
1976-77 Consultant, as assistant to Associate General Director, Massachusetts General Hospital
1977- Consultant, grants and contracts; and member of the Commission on Research and special consultant to the National Cancer Institute.