

INTERVIEW WITH DAVID E. PRICE, PH.D.

BY STEPHEN P. STRICKLAND, PH.D.

ON THE OCCASION OF

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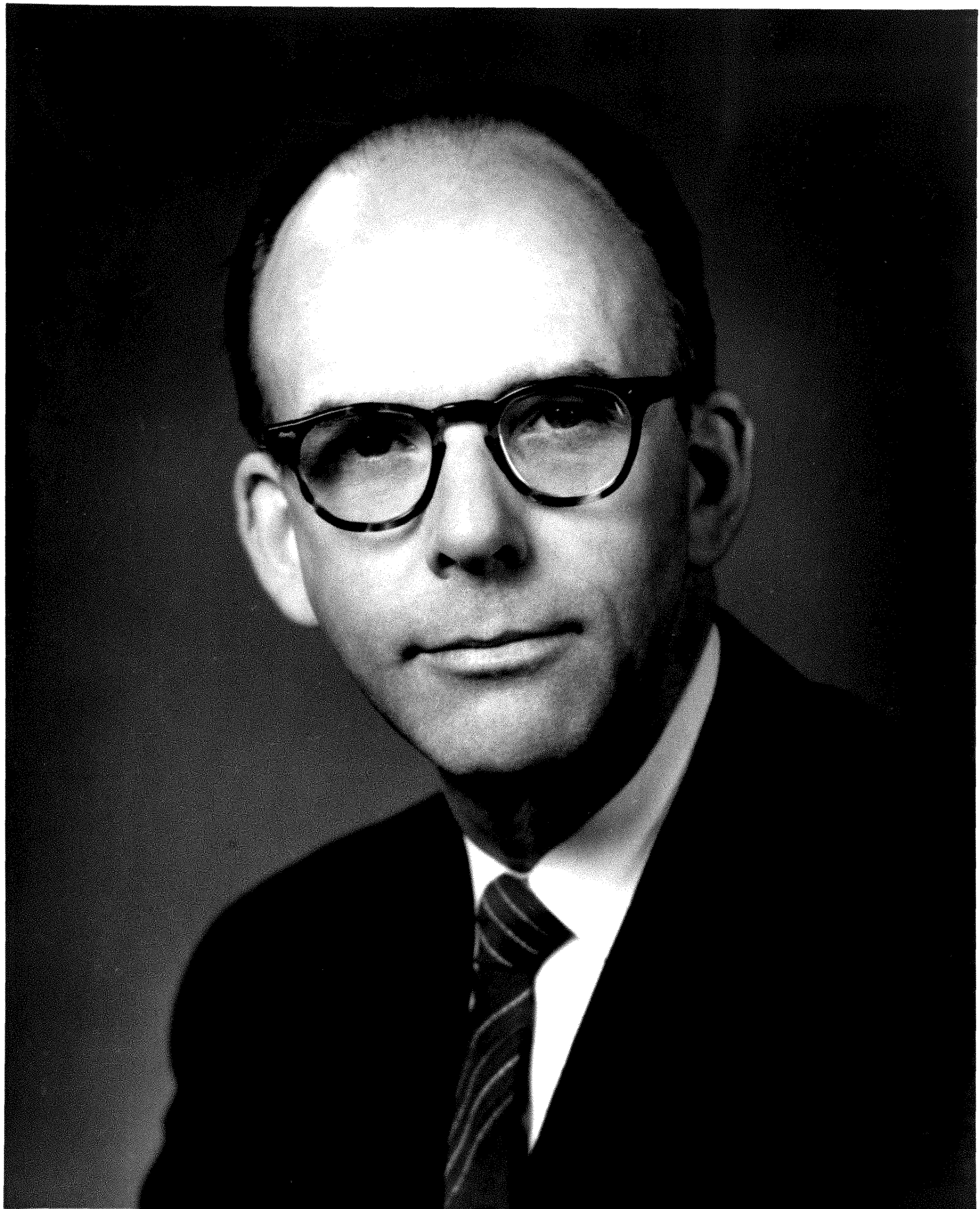
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Introduction and Biographical Sketch

This interview with David E. Price, 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. Price, 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 program on health and science.

Dr. Price received his undergraduate, graduate and medical degrees from the University of California system, the first two from Berkeley and the M.D. from U. of C. San Francisco. In 1940 he became an intern at the Public Health Service Hospital in San Francisco and was made a commissioned officer of the U.S. Public Health Service, as a communicable disease epidemiologist, in 1941. From that year until his retirement from the Public Health Service in 1965, he held a series of increasingly responsible positions, through the war in the PHS venereal disease program, and just after the war, in 1946, with the research grant programs of the National Institutes of Health, and the National Cancer Institute; subsequently he was Associate Director of NIH for Extramural Affairs, Assistant Surgeon General of the Public Health Service, Chief of the Bureau of State Services for two years beginning in 1958, Deputy Director of the NIH in 1960, and for the last three years of government service, Deputy Surgeon General. After retirement from the PHS Dr. Price served as a consultant to the Ford Foundation, as a Professor of Public Health Administration at The Johns Hopkins School of Hygiene and Public Health, and in other capacities at Johns Hopkins until his "final retirement" in 1982. The perspectives offered in this interview, therefore, represent those of an early member of the extramural programs of NIH, a general administrator of broader PHS programs, and a university administrator.

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WASHINGTON, D.C.

Interview with Dr. David Price

May 2, 1986

SS: I am talking this morning to Dr. David Price, who for many years was Director of the Division of Research Grants at the NIH. He was there for at least twenty years, I believe. Is that right, Dr. Price?

DP: Over period from 1946 until 1962 I was there off and on.

SS: 1946 was the year that the Division of Research Grants was set up, and you were there to receive the wartime contracts?

DP: You may have run into the fact that during World War II there was a great deal of emphasis being put on venereal disease control by the Public Health Service, and Ernest Allen had worked in connection with that. C.J. Van Slyke, who was the first director of the research grants division, had been a research worker in New York at USPHS Hospital (formerly Marine Hospital) in Stapleton, New York working with John Mahoney, who later became Commissioner of Health for New York City. Mahoney had a laboratory at the Staten Island Hospital where the efficacy of penicillin in the treatment of syphilis was demonstrated. Van Slyke was transferred to the Washington area in connection with venereal disease control activities, then was given the job of organizing the research grants program administration. Ernest Allen was recruited to be his assistant and administrative person, and I joined the two of them very soon after I had moved to the Washington area in 1946.

SS: Were you already in the Public Health Service?

DP: I was already in and had been for several years. Just before that I had been stationed in Baltimore at Johns Hopkins University working with Dr. Joseph Earl Moore who was then one of the nation's leading syphilologists, and while I was there I had the opportunity to work for a doctorate in public health administration. When I completed the two years of my assignment there, I moved to Washington to be a part of the Venereal Disease Division's headquarter's staff. That lasted only briefly, after which I was assigned to work with Van Slyke.

SS: Did this whole thing develop very swiftly — that is, the decision that the health-related contracts of the OSRD would be given to the National Institutes of Health?

DP: I think that came about quite suddenly. I don't know how long it might have been "cooking" before the event, but to the best of my knowledge it was a fairly quick decision. The person who was most influential in that, I think, was Dr. Rolla Eugene Dyer. I think Gene Dyer has very rarely been given credit for the extent of his wise leadership in the development of the early years of the grants program.

SS: He clearly is the one who secured the contracts for the National

Institutes of Health. But it sounds as though the transition was rather swift. One can imagine why. In 1945, in the beginning of that year, we were still not absolutely certain whether the war would be over that fast. So, we're talking about within the year after the formal termination of the war.

DP: I think what happened was that a decision was made to close out the OSRD when there were still active contracts in two particular fields: one of them was a big study of the treatment of syphilis with penicillin and the other was the development of anti-malarial drugs, which was a very big program at the time. Those two things needed to be seen through and closed out in an orderly fashion, so it was decided to turn over the available funds to the NIH to maintain those particular programs.

SS: At the time you got there, which was very soon after this happened, was it the idea that this program would simply last for the duration of those contracts, which I assume were transferred into grants? Or did you, that early on, see that it was a program that could grow?

DP: There were probably people who would have taken either side of this question. Many of us, myself included, had grave doubts as to whether it was going to continue for any period of time. Van Slyke, on the other hand, and I think Gene Dyer, would have taken the position that this was very likely to continue and grow. But no one at that point, I'm sure, foresaw the extent of this growth. There was, at that time, another program that had very little attention, and this was the grants program of the National Cancer Institute. It had been in existence for a number of years, I've forgotten the exact year. The Institute was created in 1937, and I don't know how soon after that they began to have some money to spend outside of supporting cancer research. It was relatively soon, but it was a very small amount of money and they had no administrative structure to oversee it.

Roscoe Spencer, who was the Director of the Cancer Institute had an administrative assistant named Ora Marshano and they ran the cancer grants program, as they used to say, "Out of Spencer's bottom desk drawer." In '46 I became associated with their activities in Van Slyke's division, and I suppose it was about a year later that it was decided to set up an office for grants in the Cancer Institute and I was asked to move over to the Cancer Institute to run it. This was when I got acquainted with Ralph Meader. During the war Ralph was a member of the faculty at Yale Medical School's Department of Anatomy. Stanhope Bayne-Jones was a professor who went away to the wars and Ralph took over the operation of the Jane Coffin Child Fund, a cancer fund which was administered through Yale and Bayne-Jones had been the administrator of it for years. This gave Ralph contacts in the cancer research field that were rather unique. So we got him to work with me part time to help introduce me around, open doors for me, and help peddle the federal money. This led to our eventually persuading Ralph to leave Yale and come to the Cancer Institute full time. He assumed the job of Director of the Research Grants Program of the Cancer Institute when I was moved back to succeed Van Slyke as Director of the Research Grants Division of NIH. My guess is that the year was 1948.

SS: So, already in a two-year period things were in fact growing and obviously extending into other areas beyond the work on syphilis and malaria

and cancer. One thing that is of particular interest to me, because it's so central to the whole grants making process, is the peer review mechanism, the study section mechanism. Nobody can recall having consciously looked at peer review models that existed elsewhere, in the private sector in other parts of government, before you created your own at the Division of Research Grants. Do you remember any such outside models that may have been used?

DP: No, I don't. I have a suspicion, which may be wrong, that the model was in the mind of Rolla Dyer, and probably was based on the committee structure of the OSRD and the National Research Council because most of the wartime research had been managed by committees. I don't recall ever having discussed or heard discussed the general subject of peer review or experience others had had with it.

SS: It was just a common feeling that you had to make sure that the research carried out was of the highest quality, and the way to do that was to get people who were expert to review proposals. So you simply did it. It was an ideal way.

DP: One of the things that we were always sensitive about, and Dyer felt very strongly about, was that the grant program ought not to be run by the intramural scientists at the NIH. That was of course the obvious easy way to go because the program at that point was small and would have been manageable to have the scientific review done by intramural scientists. But we realized that to do that would place the program in some jeopardy with people feeling that their ideas were being stolen by the government scientists. We wanted to keep any accusation of that kind from occurring, so we chose to use outside reviewers. Of course, the whole study section mechanism grew by steps; first there was a syphilis study section, then there was one for malaria, and that's all there were at first. As time went by, the number of these groups grew and the size of the groups grew and the mechanism for trying to find people to serve on them grew. One of the things that I never got around to doing but always thought would be fun would have been to try to analyze how much of the scientific time in the country was spent looking at one another's projects! It became a tremendous drain on the energies of scientists who serve on these study sections to review the voluminous dossiers that are developed on the projects.

SS: I have served on the history panel of the National Academy of Sciences' study of the organizational future of the NIH a couple of years ago, and I remember some conversations about that during that study. On the other hand, it is still an important thing to do; it is prestigious and part of the "civic ethic" in a way, isn't it, for biomedical scientists to do that? They like to be invited, they feel that they should participate. . .

DP: They feel left out if they haven't been asked to serve.

SS: It also occurs to me that that's one of the ways that scientists keep up with what's going on in the field. It must be.

DP: It gives the people who serve on the study sections an insight that would be difficult to gain otherwise.

SS: Let me ask you about a couple of individuals whom you've mentioned. Ora Marshano later became the historian of the National Cancer Institute, I think, or at least kept some notes? At one point, fifteen years ago, I looked at some of them. She is still around?

DP: I don't know whether she is. It has been probably two or three years since I heard about her. She might be interesting to talk to, particularly in connection with the general development of the Cancer Institute. To the best of my recollection she didn't have very much contact with the rest of the grants activity.

SS: The second person I want to ask you about is Dr. Dyer. I only know him through the written records, and I have two impressions of him. The first is based on a particular critically important episode: specifically the acquisition of the wartime contracts for the National Institutes of Health, which he basically single-handedly arranged. He was the NIH representative on the Committee of Medical Research of OSRD. Subsequent to that I take it, he picked Dr. Van Slyke to be the first Director of the first grants program, and Van Slyke was, as you say, positive about the possibilities of growth. In any event, I have the impression of Dr. Dyer as the conservative scientist, the science administrator, not wanting to move too fast, not wanting to have too much more money from Congress. How would you describe him?

DP: There isn't any question that he was quite a conservative scientist. I have the impression that he was not the highly visible political front-man for the NIH; as a matter of fact, I don't think NIH had made very much of a splash before the grants program developed. Dyer was conservative enough that he would not have sought funds that he didn't think could be very wisely spent. I have an idea that before he finally left the directorship that he may have felt things were moving a little bit too fast. But he was, in contrast to say, Jim Shannon, a very withdrawn kind of person who wasn't in there fighting in an obvious way for the growth and development of the institution. I should ask, have you had any conversations with Leonard Sheele?

SS: I had a long interview with Dr. Scheele in the course of doing my first book, which was published by Harvard University Press in 1972. So, in 1970-71 I spent a fair amount of time with him and had good interviews, but even then he was complaining that he couldn't recall things, that he hadn't kept records.

DP: I was going to say that I think Leonard might have some recollections about this because, in whatever year it was that I was asked to take over the office in the Cancer Institute, I guess probably he was the one who engineered it, because it happened almost simultaneously with his appointment as the Director of the Cancer Institute. He and I and Ken Endicott and Ralph Meader might be the only living people who lived through that particular episode.

SS: I will try to see him on this.

It's interesting to me that everybody says that the NIH or any part

thereof shouldn't move faster than the scientific basis for movement warrants, but obviously different people see that differently, as to how rapidly or how slowly you should grow. Dr. Shannon was considered by some people to be conservative, certainly by people like Mary Lasker, and even occasionally by people within the Institution.

DP: Most anybody would be a foot-dragger in Mrs. Lasker's view!

SS: Tell me how this movement between the Division of Research Grants of NIH and the Cancer Institute worked? I was under the impression that once the DRG was set up that it took over review of cancer grants as well as NIH grants.

DP: These things developed a little at a time. Initially, there was the Office of Research Grants that Van Slyke presided over, and there was the office in the Cancer Institute, and each of these operated sort of independently, each by its own application forms and procedures. There wasn't a great deal of interchange. As time went by, it became apparent to some of us -- and I was one who went to bat on this -- that the applicants were put in a very difficult position, not always knowing to which institute to apply, and sometimes having to send in duplicating applications in order to be sure that somebody might find an interest in what they were doing.

I pressed very hard for the development of a centralized application and paper processing operation. During the time that I was director of the program, I tried to get agreement among all of the various institutes, and they were of course developing, in larger numbers all the time, as the political process worked, and NIH was being forced by outside pressures to create new programs with new appropriations. I said we should at least have something set for research grants, so we could say to the applicants: "This is the NIH front door. Come in this way and don't worry about which Institute you apply to." We then began accepting grant applications and deciding in the Division of Research Grants which Institute or Institutes ought to be interested in reviewing them. For a long time each of the Institutes then had its own review mechanism, its own study sections. We finally got away from that and got a single group of study sections that all the Institutes felt would serve their needs. In this way we tried to simplify the process for the applicants.

SS: Basically that approach continues today on that basis?

DP: Yes, I think that has worked out fairly well. The Division of Research Grants didn't really have a grant program of its own. It didn't have a particular field that it was trying to promote; it was a service organization for all of the institutes and a go-between for the applicant and the Institutes and the study sections.

SS: Except that at some period, didn't the DRG have a little money with which it could cover some bets otherwise not covered?

DP: My recollection is that it did have some money that wasn't earmarked for any particular purpose. Eventually the Division of General Medical Sciences was set up in order to keep the Division of Research Grants purely an administrative, not a programmatic entity.

SS: It's wonderful to think that Dr. Dyer was a more conservative scientist, but he's the one who initiated the grants program, fundamentally by getting wartime contracts and creating the mechanism to deal with grants thereafter, and that he's the one who had in mind the wartime experience of review of research possibilities by committees of the OSRD. He conceptualized the mechanism which would preserve excellence in both outside pressures, which in fact, made it possible for the grants program to grow, and made it, if not invulnerable, in a very strong position. He positioned the NIH for growth, through the mechanisms he created, even though he seems to have been more conservative in pace.

DP: That's about the way I would look at it.

SS: You were there as Director in that office in a quite dynamic period in the '50s. What basically changed, if anything, in the fundamental way you did business?

DP: I don't know quite how to respond to that question, because my recollection is that the basic nature of the research grants division underwent relatively little change. It grew much larger and the volume of paper work was tremendously increased. The numbers of study sections that it was servicing grew very greatly, both in numbers of members and numbers of subject matter fields. But I can't say that I recall very much change in the general nature of the operation. The thing that changed most was the increased number of program-promotional offices that each of the Institutes had: contact people, people going out visiting the universities, trying to interest scientists in directing their research in ways that would seem to have some impact on a particular problem, like arthritis or digestive diseases, etc. So, a lot of the organizational development, and I suspect a good deal of the growth in numbers of professional staff, was related to the programmatic office development of the individual Institutes.

SS: Yet, that function, from what you said before, was not absolutely new; that is, you said that in the early days Ralph Meader took you around and introduced you to new people so that you could talk to them about cancer research.

DP: That was as a program representative of the Cancer Institute, rather than as a grants administrator.

SS: On this point, some people have the impression that the NIH is more reflective of activities going on in particular disciplines and particular institutions, and science generally; that is, what the NIH does is a reflection of what is initiated elsewhere. The development of program representatives going out and talking to members of departments of medical schools of universities suggests the opposite. Would you say that the NIH was more activist or more passivist during the period you were there? Or did that change?

DP: I don't know that I would recognize a very great change in that regard. One of the things that did change, and this came about particularly as these institutes that were more narrowly devoted to some particular field of science and health broadened, the NIH became more directive. This is, I think, more apparent in the cancer field than in any other, but I suspect it is true in some of the other fields as well, that the NIH, probably operating as much through its advisory committee as any other way, has identified some problems that they wanted to get some work done on in a very highly targeted manner. Or they wanted to set up a large study of some kind that involved a tremendous amount of centralized coordination. This has been done by taking a degree of initiative at the NIH level that was lacking in the early days of the program, something which I think was inevitable, but has been deplored by a good many people.

SS: But how did this impact on the grants program and the peer review process and the separation, in a way, of consideration of grant proposals through the DRG mechanism, from the categorical institutes?

DP: I don't know what impact it may have had on the DRG mechanism, but it certainly has led to some of the Institutes acquiring technical staff, presumably capable of planning and directing some of these large studies.

SS: Does that affect more the internal, intramural program, than the extramural program?

DP: I don't know what effect it has had on the intramural program. My impression is, and a lot of this has happened since I had anything to do with it, so I am going on the basis of hearsay and speculation, that this has probably led to a much larger amount of centralized planning and promotion of very specific kinds of research — even to the point of specifying, in a rather contractual way, what will be done with the funds.

SS: Of course, the Cancer Institute has used the grant as a contract mechanism much more than any other institute. That's my impression. During your tenure, did you see a difference because of this phenomenon, in the character of grant proposals themselves?

DP: No, I can't say that I saw a change in that regard. There used to be a good deal of argument; as a matter of fact, we used to have arguments with some of the people in the general counsel's office at HEW, and the Federal Security Agency before that, over the nature of the grants document. Their position was that it was just a different form of contract. But I think it's pretty clear that most of those early grants were based on rather general proposals of what the investigator wanted to do, with no real promise that he was going to follow a particular line of investigation. One of the early tenets of the program promoted by Dyer and Van Slyke was that the investigator should be given the freedom to change the direction of his research at will. So there has been a schizophrenia in the way the program has developed.

SS: But only because the lawyers for the government, whether from the Department or Public Health Service or anywhere else, typically want to impose tighter controls on the spending of money.

DP: They like to see things neat and clean, and enforceable.

SS: I would be surprised if grantees today were told they could change the direction of their research; the grant mechanism, the grant form, is pretty complicated, but you still do have freedom. With this particular project I'm doing, and the two books I've written dealing with NIH, and lots of other work dealing with NIH, this is the first time I've had a grant from NIH to do it. So, that part is new to me, and there are certain things that one has to do, but they're not terribly restrictive. Any scientist, whether like me, a social scientist, or a biomedical scientist, has a pretty clear about what he wants to do and how he wants to do it, but can't predict every pathway that might open up for him to follow.

Let me ask you generally about key people in the period that you were there, which turns out to be closer to thirty years than twenty, doesn't it? Leaders, for example. Who, in addition to the ones that are terribly obvious like Dr. Shannon and Dr. Van Slyke, perhaps Dr. Scheele, were great leaders who seemed to have a special talent for inspiring people, who seemed to have a special vision of the possibilities in biomedical research in the Institutes? Who were those persons who more nearly saw the potential for medical research and in the NIH grants program?

DP: You are thinking now about governmental people who were involved or people on the outside?

SS: Both. I'd love to hear about both sectors.

DP: On the inside I would certainly have to say that Van Slyke was an outstanding advocate, not only in his leadership in the early days of the research grants program, but also as director of the Heart Institute, when the Heart Institute was established. As long as he lived he exerted considerable influence on the people in the Service as well as people outside.

A person who I think was, perhaps, as influential as anyone else was Sidney Farber, from Boston Children's Hospital. And, of course, Mike DeBakey, Mary Lasker; these people stand out in my mind because of their advocacy role and the influence that they had politically and professionally within the Service.

SS: About the political advocacy of those people like Mrs. Lasker and Dr. DeBakey and Dr. Farber, interested in particular diseases, and expanding appropriations -- did these outside activities ever, in your judgement, adversely affect the internal operation, the quality of science done?

DP: I don't have any idea that it adversely affected the quality of science. It seems to me that there has almost always been a larger reservoir of ideas and good research workers to utilize funds that became available. There have been many occasions when a new institute was created and a new appropriation made and there was some lag in the ability of the institutes to "tool up" and get things moving, because with the appropriations process what it is, you can't very well anticipate some of these things, or at least you can't spend money in anticipation. So there frequently was some lag in that. But I think these outside pressures, not only from individuals like Lasker and

Farber and DeBakey, but also organizations, like the Arthritis Foundation, the Cancer Society, the Heart Association -- have been very effective lobbyists and, I think, frequently doing this out of their organizational self-interest as much as any other. A lot of the things that have come about have happened because of the interest groups of that kind.

SS: You've been out of the Public Health Service for a few years, but I take it you keep up a little bit. You just mentioned going to your meeting yesterday of retired Public Health Service officers. What is your overall impression about the investment we've made over the last quarter century in biomedical research, mainly through NIH, and the value and progress and problems?

DP: One of the things that we haven't talked about, which I find inextricably associated with the research grants program, are the research training programs. I think that these may have had as much if not more importance in shaping the destiny of science as the support of research itself. I retired in 1965 from the Service, so it's been a good long time, and during a good bit of that period, I have been associated with Johns Hopkins University, so I have had an additional opportunity to see the effect of these programs on an educational institution and a training institution. A place like Hopkins and almost any of the big universities are bound to be dependent on these sources of support. They've done a great deal to shape what's happened.

SS: And in the last decade support for training grants has declined significantly, so does this impair our future scientific capacity?

DP: I suspect that it does, in some degree. One thing that's happening now, and I'm not close enough to it to evaluate it, but there is obviously a great deal more emphasis on educational institutions obtaining support from industry than ever in the past. Hopkins, for example, through a variety of affiliations with industry, with joint operation of laboratories, is achieving perhaps what it would otherwise have achieved through federal support.

SS: Is there any particular danger in that? Industries, and I take it you mean those industries like pharmaceutical companies or health-related or science-related companies, obviously have a product in the back of their minds; on the other hand, the Cancer Institute or the Heart Institute have a discoveries in particular fields in the back of their minds. Everybody hopes to move toward a breakthrough of some kind.

DP: That's very true. One of the things that's been a terribly difficult issue to deal with is the government patents policy. The feeling for a long time on the part of industrial organizations was that they didn't want to get mixed up with the federal dollar because of what it might do to impair their ability to patent and license and create a product.

SS: This wouldn't have happened, or would it, under a grant? I mean, if for any reason a patentable discovery was made in the course of a research under a grant, would that have been a problem?

DP: Yes, it would have been a problem. I spent a number of years working

with the Government Patents Board that was set up, trying to develop a coherent patent policy that would be subscribed to by all the federal agencies, and the degree of developmental initiative that the different government agencies had was quite varied; the Department of Defense maintaining quite a different sort of posture than, say, the Public Health Service did. We were forced in line with the government's overall policy of requiring certain reporting procedures and patent ownership policies on grantees as well as our own employees. I don't know to what extent this has impaired the development of anything, but it has on many occasions led to an investor organization declining to participate in something because of a fear that it was going to steal their profit.

SS: What else would you "put on the table" as an issue worthy of attention? Looking into the past, what have we left out in review of how this enterprise was constructed?

DP: One of the things which has been a big issue is the indirect cost participation question.

SS: There's a big new effort now to squeeze down the indirect costs permitted at universities and learning institutions.

DP: When the NIH took over the old OSRD program, there was a tremendous reaction of opposition to paying any indirect costs. Apparently during the war years (that was before I had any contact with this so I can't speak from personal experience) there was apparently a great deal of resentment on the part of scientists at the large indirect cost payments that were being made to their institutions. They saw no benefit in this so far as they were concerned and it was just the guys up in administration who were skimming off the gravy that should get down to supporting research. So, when the program was started, this was largely on the basis of our outside advisors, they insisted on paying no indirect costs.

After a period of time, say a year or two, it became apparent that just in managing the program and getting the necessary cost accounting reports to the government, that it was costing the institution something to permit their scientists to accept these grants. Gradually they began paying a larger percentage of allowances and this finally became generalized across the board in government agencies. An Audit Agency was established in the Department of Defense that audited grants on our behalf and applied whatever the criteria were, determining how much indirect costs would be permitted. I don't know what the situation is these days, but that's what it was then.

SS: Basically, Dr. Price, your impression is that the mechanism set up 25 to 35 years ago has operated pretty well, remaining fairly strong?

DP: That is the impression I have.

SS: That is rather remarkable. The grants program is, as it turns out, the major component of the National Institutes of Health. The grants program is a way American citizens, through their tax dollars, support most of the biomedical research in this country. It appears that the way that program was set up and put in place in the early days, as modified and strengthened in that first decade, continues to serve us all well.

DP: I think it's been flexible enough to roll with the punches and strengthen its weak points and discard some of its poorer practices, as time has gone by.

SS: And what began as a program to take care of research grants was then expanded to research training grants and other kinds of grants, but the basic practices -- central administration, peer review through study sections comprised of experts in the fields -- still is the way they operate today.

DP: One thing I've often wondered is whether -- as time had gone by, and the importance of the program and the political attention which the program has had, and political forces within science and society generally, have modified the process of selecting peer reviewers -- this has been all for the good. There has been so much attention paid at times in the past, I don't know what the situation is right now, to geographic distribution of the funds, of the advisors, and at times in the past political pressures have caused us to select someone who obviously was less qualified as a scientist to be a judge of his peers, simply because he was the only person available in some neglected area of the country.

SS: In a way I would think that might have been a greater problem 20 years ago than today; that is, today there are institutions and individuals of excellence in many fields across the country. Expertise is no longer attached to a few institutions in only a few parts of the country. That obviously is something that would bear attention. I should ask about that when I talk to people who are still working in the grants program.

Would you suggest to me other questions I ought to ask others when I interview them, and other people I should talk to? I told you some of those I have seen already.

DP: One thing that occurs to me that undoubtedly you've already considered; do you know the history of the Public Health Service that was written by Ralph Williams?

SS: No, I don't think I know of that. Is it relatively recent?

DP: No, it's not recent.

SS: What I have read recently is Wyndham Miles' history of the National Library of Medicine and I have read Hans Stetten's compilation of the intramural program history and its current status.

DP: This is Ralph C. Williams. I don't know whether it has another title other than "History of the United States Public Health Service". My guess is that it was probably written in the late '50s. He has been dead for a number of years, but the book has some information about people who were involved, and I think it covers, rather superficially, the development of the grants program, but there are some things in there that might guide you to some people. There was a notorious Williams family in the Public Health Service,

I think for about 3 generations, and Ralph Williams was one of a pair of brothers -- the other was Charlie Williams, Sr. -- and then there's a Charlie Williams, Jr. who is a contemporary of mine. R.C. was fascinated by the history of the Service and did a lot of research in putting this together. It might be of some help to you.

One man that I have'nt heard about for a long time is Dr. Franklin Yeager.

SS: Yes, Ernest mentioned him and I'm going to try to find him.

DP: The people who were in that generation, just earlier than Scheele, in the Surgeon General's chair, I think have all disappeared. Tom Parran was Surgeon General at the time it all got started, and I think he exercised a good deal of influence over some of the decisions about people who were involved in the early days, but he's been dead for a long time now. His deputy is also dead. After Tom Parran retired he worked in a foundation for quite a period and became the Dean of the School of Public Health at the University of Pittsburgh.

SS: If you think of anybody that you think I should see, be sure and let me know.

This has been enormously helpful and you've filled in some details that others have not given me, and I am very appreciative. If you have additional thoughts, drop me a line or call me. May I ask, would you also send me a resume?

DP: Sure.

Born: San Diego, California 5 July 1914

Education: Public Schools, San Diego, California

University of California, Berkeley and San Francisco

A.B. 1936

M.A. 1937 - Thesis on a problem in experimental endocrinology. Department of Anatomy and Institute of Experimental Biology.

M.D. 1940

Johns Hopkins School of Hygiene and Public Health

M.P.H. 1945

Dr. P.H. 1946 - Thesis on development of relationship between Federal Government and States in public health. Rockefeller Foundation Fellow, 1944-46

WORK EXPERIENCE:

Teaching Assistant (Anatomy) U. of Calif. 1936-37

Intern, Public Health Service Hospital
San Francisco, California 1940-41

Assignments as Commissioned Officer, U.S. Public Health Service:

Communicable Disease Epidemiologist
San Diego Health Department 1941-42

Clinician - Venereal Disease Rapid Treatment Hospitals,
Hot Springs, Arkansas and Norfolk, Virginia 1942-44

Clinician - Venereal Disease, Johns Hopkins
Hospital, Baltimore, Maryland 1944-45

Laboratory Teaching Assistant - Epidemiology
and Biostatistics, Johns Hopkins School of
Hygiene and Public Health 1945-46

Administration of Research Grants Programs of
National Institutes of Health in various
positions 1946-50

Associate Director, National Institutes of
Health, for Extramural Affairs 1950-52

Assistant Surgeon General, principal aide in
Immediate Office of the Surgeon General 1952-57

Deputy Chief, Bureau of Medical Services -
Operation of PHS Hospital system and foreign
quarantine 6 months - 1957

(continued)

Chief, Bureau of State Services - Operation of Regional Offices, consultation to State Agencies' community health and environmental programs, grants to States, operation of Communicable Disease Center 1958-60

Deputy Director, National Institutes of Health - General policy and administrative direction with special attention to relationships with educational and research institutions in area of grants and training 1960-62

Deputy Surgeon General - share with Surgeon General the policy direction, management and representation of the PHS; responsible for operating the Office of the Surgeon General which comprised Divisions of Finance, Personnel, Administrative Services, Public Health Methods, and the National Center for Health Statistics, and the National Library of Medicine. Until retirement, 9-8-65 1962-65

Consultant to Ford Foundation - Program Specialist and team leader of consultants in public health and family planning, New Delhi, India 1965-67

Professor of Public Health Administration - Johns Hopkins School of Hygiene and Public Health 1967-emeritus 79

Director of Planning, Johns Hopkins Medical Institutions 1967-73

Executive Director, Medical Planning and Development Committee, Johns Hopkins Medical Institutions 1973-75

Executive Assistant to Vice President for Health Divisions, Johns Hopkins University 1975-78

Special Assistant to Provost, Johns Hopkins University 1978-82

SPECIAL ACTIVITIES:

Member, Medical Advisory Committees, National Foundation for Infantile Paralysis 1952-62

Chairman, Committee on Constitution and By-Laws, American Public Health Association 1960-65

Alternate Delegate for PHS, House of Delegates, American Medical Association 1962-64

Alternate Delegate for United States, Directing Council, Pan American Health Organization 1959

PHS Representative, Executive Committee, U.S. - Mexico Border Public Health Association 1958-59

(continued)

Chairman, Exchange Mission, U.S. - U.S.S.R. Live poliomyelitis virus vaccine. Moscow	1960
Lecturer in Public Health, Georgetown University	1958-60
Alternate, Citizens Health Council, Regional Planning Council, Maryland	1971-75
Vice Chairman, Planning Committee, Maryland Hospital Association	1973-75

TRAVEL:

Dominican Republic, Guatemala, Mexico	1949	Inspection NIH research
Europe and Israel (6 weeks)	1949-50	Aide to Federal Security Administrator
Mexico	1952	Vacation
Europe (6 weeks)	1957	Inspection Quarantine Offices
U.S.S.R. (Moscow only)	1960	Exchange Mission
Brasil	1961	Research Contract negotiation
France	1963	SHAPE conference
Europe (5 weeks)	1963	Vacation
India (4 weeks)	1964	On leave as consultant to Ford Fdtn.
Switzerland	1965	Int'l Family Planning Conference
India (moved to India, Sept. 1965)	1965-67	Consultant to Ford Foundation
Nepal	1965	" " "
Thailand	1966	ECAFE Family Planning Conference
Thailand, Hong Kong, Japan, Taiwan, Turkey	1966	Vacation and study of family planning in Taiwan
Singapore, Japan	1967	Vacation
Italy	1981	"
Italy	1982	"
England	1982	"
Greece, Turkey	1985	"

MEMBER

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PUBLICATIONS: list attached

RELIGIOUS AFFILIATION: Episcopalian

FAMILY: Jean Shearer Price (wife)
William David Price (son) born 12-16-40. Ophthalmologist, Summerville, S.C.
Janet Ruth Stauss (daughter) Housewife, Silver Spring, Md.

PUBLICATIONS

- Price, David E., M.D.; McNairy, D. J., and White, E.L., "Severe Asthma: Delayed Sensitization to Penicillin", Jour. Amer. Med. Assn., 128:183, May 19, 1945
- Price, David E., M.D., D.P.H., "Public Health Research for Veterinary Institutions", Jour. Amer. Veter. Med. Assn., Vol. CXIV, No. 867, June 1949, pp. 397-400
- Reynolds, Frank W., and Price, David E., "Federal Support of Medical Research through the Public Health Service", American Scientist, Autumn 1949, Vol. 37, No. 4
- Price, David E., "Relation of Pesticides to Health", Agricultural Chemicals, October 1954
- Price, David E., "Interprofessional Cooperation and the Special Disease Interest Groups" (delivered at Marquette University's 75th Anniversary Conference, January 1954), Interprofessional Cooperation for the Improvement of our Health and Welfare, Marquette University Press, 1956
- Price, David E., "Radiation as a Public Health Problem", Public Health Reports, Vol. 73, No. 3, March 1958, pp. 197-202
- Price, David E., "New Custom-Tailored Private and Public Health Services", Urban Sprawl and Health Report, National Health Forum, Philadelphia, Penn., March 17-19, 1958, Chapter 3
- Price, David E., "Nursing Home Goals", Public Health Reports, Vol. 73, No. 8, August 1958, pp. 599-713
- Price, David E., "Conference Goals", Report on National Conference on Nursing Homes and Homes for the Aged, 1958, pp. 17-21
- Price, David E., M.D., "Goals and Problems in Saving Sight", The Sight-Saving Review, Vol. XXIX, No. 2, Summer, 1959, pp. 76-79
- Price, David E., M.D., "Accent on Health", Public Health News (New Jersey State Department of Health), Vol. 40, No. 8, August 1959, pp. 261-266
- Price, David E., M.D., "Is Man Becoming Obsolete?", Public Health Reports, Volume 74, No. 8, August 1959, pp. 693-9.
- Price, David E., M.D., "Radiation as a Public Health Problem", Low-Level Irradiation, No. 59 of the American Association for the Advancement of Science, Washington, D. C., 1959, pp. 91-99
- Price, David E., M.D., "Is Man Becoming Obsolete?", Trustee, Vol. 13, No. 1, January 1960, pp. 28-32

Price, David E., M.D., "Challenge to the Primary Journals", Fed. Proc.,
22:1008-1010, July-August 1963

Price, David E., M.D., "Programs of the Public Health Service, Jour. Nat'l
Med Assn, 55:104-108, March 1965

Unpublished reports cited in Bibliography of Substantive Writings,
Ford Foundation India and Nepal, Second edition 1967, New Delhi

FP-65-64 "Observations and Suggestions about Family Planning
Program Possibilities in Nepal", December 1965,
(photocopy), 6 pp.

FP-66-74 "Excerpts From Report Presented at the Second Meeting
of the Central Family Planning Council, Bangalore,
27 June 1966", abstracted and paraphrased by David
E. Price, (mimeo), 4 pp.

FP-67-109 "In Search of Influence", End of Tour Report of the
Foundations Chief Consultant in Family Planning,
August 1967, (typed), 44 pp.