AN INTERVIEW WITH DR. JEROME GREEN

BY STEPHEN P. STRICKLAND, PH.D.

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Table of Contents

Introduction and Biographical Sketch

This interview with Dr. Jerome G. Green 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. Allen, 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.

Jerome G. Green is Director of the Division of Research Grants of the National Institutes of Health, a position he has held since 1986. His career at NIH began 30 years ago when, in 1955, he became an extramural health scientists working in the grants and training branch of the National Heart Institute with Dr. J. Franklin Yeager. After his residency in internal medicine at the Public Health Service Hospital in San Francisco, during which time he had clinical training in cardiology diseases at the University of California and Stanford, and a special research fellowship at the Cardiovascular Reserch Institute at UCSF with Dr. Julius Comroe, he became directly involved with NIH activities once again in 1960, serving as coordinator for a large multi-institutional clinical trial on hypertension for the National Heart Institute. In 1965 he returned physically to the NIH campus as Deputy Chief of Extramural Programs, in 1966 becoming Associated Director of the National Heart and Lung Institute for Extramural Research and Training and, in 1972, Director of that Institute's Division of Extramural Affairs. Dr. Green's experience at the National Institutes of Health thus encompasses a remarkable range of roles and functions, from medical researcher in NIH-sponsored projects, to internal management and administration of extramural grant and award programs and, most recently, as head of the critically important division which sits at the crossroads of the larger American biomedical science enterprise. His perspective is thus both a long one, and a recent one.

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Interview by Stephen P. Strickland with Dr. Jerome Green

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SS: Dr. Green, I would like to ask you to help me bridge the gap between "the old days" and contemporary times of the NIH. I'd like to get your insights and your perspective.

JG: In that article I sent to you, by C. J. Van Slyke himself, I found it particularly interesting that the first chairman of the malaria study section was Jim Shannon. I didn't know that until I read the article. There are other interesting things in it. For instance, as opposed to current procedures, there were one or two individuals who were members of more than one study section. In the early days that was allowed.

SS: Yes, that is a most important article, especially since it was written when the whole program was relatively new, and when people who were helping to develop the program itself were serving as Executive Secretaries. Shannon, as it turns out, was later the Director of Intramural Research at the Heart Institute.

JG: My understanding was that for a short period of time they asked a study section member to be the Executive Secretary. It took no time for those people to come back and say, "This is too much work, too much responsibility. This should be done by a Public Health Service or NIH employee with a scientific background who can do this." Several of the study sections had some people from one institution -- like Cornell or Columbia -- and there were one or two people who served on more than one section. But the whole procedure was very different in those days. There are some very old summary statements I've seen attached to the early Heart Council minutes, for example, going back to 1948, and the summary statements are very short, one paragraph or so. They were extremely concise but, of course, science was somewhat simpler then.

SS: I have met with two former Executive Secretaries, in addition to those like Ken Endicott who had that position early and then went on to other things later. The executive secretaries take a great deal of pride in the meticulousness of summarizing the discussion and evaluation, and reasons why a grant would or would not be approved. It's very interesting and I think very important for the aggregate record.

JG: When I joined NIH in 1955, the operation was considerably smaller than it is now — much smaller -- sufficiently so that every council member in the Heart Institute got a copy of each application! Not just the summary sheet. Several weeks later they would get a copy of the summary statements. But when you went to a council meeting they not only had a book of summary statements, but piled up on the table were the books of applications. That would be entirely impossible now.

SS: Which means, how many would they look at? I am assuming that they would look at them.

JG: Oh yes, because when they looked at a summary statement, very often a council member would say, "I've looked at the application, and I think this comment is entirely appropriate. He does say in his application that this is what he intends to do." So there was much more individual discussion of summary statements and applications, and much less time spent on the broad policy issues. It was an era in which discussing the individual application is how they, in fact, set policy. They went from the particular to the general. Now advisory council meetings seem to follow the reverse procedure more often -- from the general to the particular.

SS: Do you sit in on advisory council meetings?

JG: I did until very recently. I only came from the Institute to DRG in February, so I've only been here six months.

Career at NIH

SS: Would you recount your career at NIH?

JG: I came in 1955 to the Heart Institute working in extramural programs for two years under J. Franklin Yeager, who was absolutely superb. When I joined, I shared an office with Murray Goldstein, who later went on to become the Director of the Neurology Institute. I overlapped and was replacing Bill Stewart, who went on to become Surgeon General. That was a time when we worked with all of the various grant mechanisms. Each of us took care of research and training grants, fellowships, clinical traineeships, and each of us did site visiting and some reviews. The initial review function was not so highly concentrated in DRG and was not so discreet and separate from program responsibilities. Now there is a much clearer distinction among staff at NIH so that some people have review responsibilities, and some have program responsibilities. In any case, I stayed for only two years.

I was going to leave NIH to take my residency training in medicine and cardiology when the Institute Director Jim Watt and Luther Terry, who was the Assistant Director, spoke to me and suggested that I stay in the commissioned corps of the Public Health Service and get my clinical training. So I went out to San Francisco in 1957 to the Public Health Service Hospital and started my residency in internal medicine. I was on assignment there from the Institute, so I was still a Heart Institute employee, a commissioned officer. I was there for two years. Luther Terry came out on a visit and we discussed what I should do on my third year, and I ended up at the University of California in San Francisco, just across town, in a new Cardiovascular Research Institute that was being established. The director of that institute was a very famous physiologist, Dr. Julius Comroe. I knew Julius when he was on a study section during the two years that I was here. At that time he was still at the University of Pennsylvania. I went as a "special fellow" to the Cardiovascular Research Institute and spent a tremendous year there with him. Then I was assigned as a commissioned officer to the Research Division of the Cleveland Clinic, and stayed for five years working with Dr. Irvine Page who was also a former member of the Heart Council, and a very distinguished investigator. So I spent eight years away from NIH, but during all of that time I was part of the commissioned corps in the Heart Institute.

I returned in 1965 to the Heart Institute extramural program to work as Frank Yeager's deputy. Six or seven months after I returned, he surprised everybody by retiring. I was offered and accepted his position which was in essence, Chief of Extramural Programs in the Heart Institute. Later the Heart Institute reorganized and I became the director of the Division of Extramural Affairs. So I joined in 1955 and I left in February of 1986. I was with the Heart Institute for 31 years, and I've been with DRG for six months now.

Reviewing Grants

SS: Somehow the business about separating "program" from "review" functions sounds terribly complicated to me.

JG: It's not terribly complicated. Let's take training for example -- for years training and program projects were both programs that were managed entirely within an Institute. In the case of a program project grant, somebody might come to me and say, "I would like to develop a program project in hypertension. Here is a rough draft of my application. Would you go over it? Would you come out and visit us at our institution and tell us about program project grants and the Heart Institute, and would you critique this very rough draft for us and meet with my colleagues?" And we would do that. Then the application would come in as a formal submission. We had worked with this investigator to help him improve his application. I would then have the responsibility for putting together a review team and managing the initial review for the application's scientific and technical merit. I already had some emotional commitment, as you can imagine, to this proposal, but I would do that and would end up not only as executive secretary of the review group, but as the author or editor of that summary statement ("pink sheets"), the presentor to the advisory council, and then, if the grant were awarded, managing that grant as a health science administrator. So, I was involved "from cradle to grave".

Later on, institute staffs became even more active because we began to solicit for grants and contracts by issuing "RFPs" or "RFAs" -- Requests for Proposals, Requests for Application. In that mode, it is the staff scientists who write out the work scope: what you want done. You send out the solicitation; you select the members of the review committee, and you're likely to select people who are sympathetic to your solicitation. You thought a particular area of science was lacking, that this was a good approach to overcome a certain obstacle, and you're going to pick people who think similarly.

In any case, I think there was a growing awareness that some health science administators were unduly committed to a certain approach, or had an emotional commitment, and that therefore there was something which bordered on a conflict of interest. At least it might appear that there was not a truly objective, dispassionate, rigorous review. So, NIH began to separate these functions. They said basically, "O.K., if you're going to write the RFA, the solicitation, fine. You get that through your advisory groups and your council. But now that the proposals have been submitted, someone else will handle the review and put together the review committee, and sit with them as Executive Secretary, and write up the summary statements." Then they'd say, "You can present it to the council, and if the council buys your suggestion as to which of these should be funded, then you can manage them." But the point is, they separated out from the health scientist-administrators' responsibility the crucial phase of scientific and technical merit review. I may come to you, the author of the scientific solicitation, and say, "Steve, who do you think should be on the review

committee?" And you might give me six names. But, I may not pick any of them. So the responsibility for an adequate, rigorous, critical review is mine. And if the review goes lousy, it's my fault. That's the pattern now; with a clear delineation of responsibilities.

SS: What chair would you be sitting in if you did perform the first role -- a health science administrator who is asked to come out to, say Baylor?

JG: Let me give you an example from what I know best, NHLBI; there's a heart division, a lung division, and a blood division. I am in the heart division, and I am involved with the arteriosclerosis program. I go to Baylor, understand that they're interested in receiving research support for a program project, and I may talk to them about that and tell them about the kinds of things that we're particularly interested in supporting. But when they submit a program project grant application, it comes to the Division of Research Grants. We see that it's a program project; we send it to the Heart Institute where there is an extramural affairs division that has a review section, so the review is done in the Heart Institute, but in the division of extramural affairs by Executive Secretaries who work in that division only. Once the review has been accomplished and the summary statement is completed, they send it over to the Heart Division, to the arteriosclerosis section, and say, "Here's the review we did on the program project."

SS: It is complicated.

JG: No, it really isn't. The people in extramural affairs who are responsible for review are executive secretaries only. They do not manage any grants or contracts. The people who work in the Heart Division in arteriosclerosis are program managers; they do not serve as executive secretaries of any review group that is looking at a competing application.

SS: What has the Division of Research Grants done in the meantime with that kind of application?

JG: We log it in and put it into the computer system. We determine that it is indeed of primary interest to the Heart Institute. It might be that, even though an individual in heart and vascular disease went to Baylor and worked with them when we, as the central receipt point at NIH, look at the application and say, "This has a great deal to do with stroke. It shouldn't go to the Heart Institute. It should go to the Neurology Institute." Despite the fact that somebody in the Heart Division worked with them, it is appropriately assigned to the Neurology Institute because the focus is on completed stroke, not the pathogenesis of stroke. If it was focused on blood flow to the brain, it would go to Heart; if it has to do with completed stroke, it should go to Neurology. That is an example.

DRG serves as the central receipt point for all applications, whether we are going to review it in DRG, or whether it's going to be reviewed in the Institute. In 1985, the mail room in the Division of Research Grants received 32,000 competing applications; 24,000 were reviewed in the Division of Research Grants; 5,000 were reviewed in the Institutes, because they were training or special, multidisciplinary applications, which DRG generally does not review. Or many of them were in response to an RFA; they were solicited applications. The other 3,000 were reviewed in other parts of the Public Health Service such as the National Institute of Mental Health.

SS: That gives me a much clearer picture.

JG: So we are in charge of assigning what Institute the applications will go to, and if it's at NIH, we also make an assignment to the initial review group -- a study section in the DRG, or a review group in one of the Institutes.

SS: Do you know the figure of grants approved in 1985 out of the 32,000 received?

JG: Not off hand, but I can tell you that the recommendation rate for approval is about 90%. One of the reasons it is so high is that about 23% of the applications submitted are re-submissions that didn't make it through the first time; the applicant was either disapproved, or he got a favorable recommendation but not with sufficient enthusiasm to get funded. The applicant gets a copy of the summary sheet and sees what the criticism is. Then he revises his proposal, strengthens it, and resubmits. This, of course, means that he should do better the second time around, and that's part of the reason for the 90% approval rate.

SS: How old are the program project grants now?

JG: They started in about 1961 when I was away. Ernest Allen was Director of the DRG at that time. By the way, there is an anomaly in the nomenclature: PHS has authority to award "project grants". Somebody comes along and says, "I've got a whole cadre of people here who are working on one illness. We're all working on emphysema, but this investigator is a pharmacologist, this one is a surgeon, and this one is a physiologist, but there is a program -- a theme. I want to put all these things together in one instrument of support." The PHS decided they couldn't legitimately call this sort of thing a "program grant" although it was actually in support of a research program. As I understand it, legal counsel said, "You don't have authority to support programs. You do have authority to support projects." So it was decided to call it a "program project."

SS: Some of those who worked with program projects didn't feel quite comfortable with them. Ernest Allen had reservations about them.

JG: Yes, and that hasn't entirely ended. One of the problems is that they are complex and they are usually multi-disciplinary. The study section system was set up originally in terms of single disciplines. When grant applications came in which included more than one discipline, it was quite a challenge to the system. You can't force an application of this nature into one particular study section; it wouldn't fit. Secondly, programs like this were costly. It was a lot of money. The third point has to do with attitudes toward peer review. It didn't take long before the study section system of review became the standard. Everything else was measured against that, and people began to say, "I don't think this would have done as well if it had been taken apart and sent to individual study sections." I don't know of any evidence that that is true, because what generally happens on program project applications is that they put together a site visit team in terms of that particular application. Usually they go out and spend two days at the site and conduct a rigorous, in-depth review.

SS: And they put this special group together in terms of the components of the project so that there is an expert in each of areas of the project?

JG: Yes. Their site visit report then comes back to a "parent committee" which can be quite different than the site visitors. So, it's almost as if there is a

tri-level review, not a dual review it goes from a sit visit, to a parent committee, and then to the council.

SS: Who might the parent committee be?

JG: It's a standing committee, a program project committee in the Neurology Institute, or in the Heart Institute, etc. Generally, what you would do is make sure that one or two people from that standing committee are on the site visit. But the other twelve or fourteen people on that site visit are not. They are there as the experts.

SS: Does every Institute have a program project committee?

JG: As far as I know the answer is yes. There was a period when one or two of the Institutes were saying, "We can't do that. DRG, will you do it?" And DRG would set a special study section and go out and site visit. Then it would come back and go right into the council. But that's not being done any more.

SS: What about training grants?

JG: Each Institute has a training grants committee, a standing review committee.

SS: And training grant reviews all take place in generally the same way?

JG: They're not site visited. They used to be site visited almost routinely, but that stopped when Caspar Weinberger was at the Office of Management and Budget, which called the Bureau of the Budget in those days. For a time training money was frozen. The Nixon Administration "impounded" it, which led to the Congress passing a budget act stipulating that the President could no longer "impound" funds in this way. There was a whole re-casting of NIH training programs. So, when the Weinberger program was launched it said, "We will no longer do site visits unless it's a very unusual case."

SS: That was also about the same time that new medical schools stopped being created, I assume, roughly in the early '70s.

JG: Yes. But this was reseach training for Ph.D.s as well as M.D.s. It was all post-graduate research training for the physicians. In the early years NIH was supporting clinical training as well as research training, but I would say since the mid-'60s the emphasis has been heavily tilted toward research; now it's virtually all research.

SS: When you say there were 32,000 competing grant proposals received in 1985, are you talking about individual research grants and training grants, as well as program project grants?

JG: Yes. And including fellowships. There are also small business innovation research grants, the SBIR program.

Let me say something historically about training grants. I cannot vouch for this, but I have been told several times and years ago, that the training grant as a mechanism was the brainchild of Cassius Van Slyke, and that it was a very creative and innovative development. Until that time, there were individual training "awards" at NIH; research fellowship awards which were made to

individuals. Some Institutes had "clinical traineeships". This was at a time when, for instance, there weren't many pediatric cardiologists, or pediatric neurologists. So we were supporting residency training. Clinical training. Van looked at it and said, "These are individual awards. This is doing business retail. How about if we did business wholesale? If we decided that the University of Chicago is a superb place to train pediatric cardiologists, let's give them a grant and build into it stipends for individuals. Then they'll select the individuals. Not us." He even saw that we could build into that training grant, aside from stipends, some money for the faculty. We could build in money that would allow them to purchase, say, a motion picture machine. So we could build in salaries for the trainers, money for equipment, money for animal costs and laboratory costs, if they were giving that kind of training. Of course, most of the money built into the grant would be for the stipends of the individuals. My understanding is that it was really Van who developed that, and it was a tremendous leap forward, conceptually.

SS: The great bulk of the grants, though, are still to individual investigators?

JG: Oh, yes. And unsolicited. The bedrock, the foundation, at NIH is the unsolicited research project grant.

Attitudes Toward Research

SS: Does this add up to major biomedical research initiatives still coming largely from individual investigators?

JG: Yes. I think that the general attitude at NIH is that the best ideas and the most innovative things come from the large biomedical scientific community that's out there. They have the ideas. There are some ideas that come from NIH staff, from NIH-supported workshops and conferences, and from some of the study sections or advisory groups or councils that we have. But the major reliance is still on individual scientists out there who submit a proposal and say, "I think this is an important problem and I think I've got a way to resolve it." That's a deliberate strategy at NIH, and I don't see any real movement to alter that. In the past several years, as monies have become short, there's been a deliberate tilt at NIH to protect this program, the "R01" program, and it's been done at the expense of the contract program, at the expense of training. Jim Wyngaarden's testimony this year to the appropriation committee said: "This has gone too far. We must support centers; we must provide better support for training. We support clinical trials with contract money." Why? Because there are multiple institutions recruiting patients and evaluating new therapies or diagnostic techniques. We want to do it by contract so that the protocol is exactly the same in all of these institutions. At the end of the study period, we can then legitimately and comfortably pool data from these various institutions. In order to maintain that degree of control, we have to use contracts. He told them that there are too many clinical trials now on the back burner because we've tilted so heavily toward research grants. Now the strategy is going to be to try to reverse that a bit.

SS: This has always been the philosophy of NIH, particularly in the grants program, and yet there are special programs with special emphases, and these change periodically. Yet, NIH leadership has been very activist in certain ways. From the early days, when Ernest and Van Slyke and Endicott and the others went out

to the medical schools and universities and "talked up" particular new possibilities and programs, or program needs. I assume that still goes on, although in other ways, like your conferences or workshops, which are means of letting the research community know what concerns or interests you have.

JG: Yes. This is also done through publications. An Institute might issue a "program announcement" which is not very specific; it's not really a solicitation. It's a reminder. They don't say that we're setting aside a particular amount of money for a particular area of research, or announcing an intent to award a certain number of grants. We just let the biomedical community know that we're interested in particular areas. Or we might be telling them about a new area that we're interested in -- that's a program announcement. When you get to an RFA, a Request for Application, that's a bit more specific, saying what disease, what age group, and how much money will be set aside for that effort and how many grants we hope to make. The third, and most specific solicitation is the RFP, Request for Proposals. RFAs are for grant applications, and RFPs are for contract proposals.

Leadership

SS: Let me ask you about some of these individuals you mentioned. Some of them I know. One thing that I am very interested in is the nature of scientific leadership, particularly in an institution or an enterprise where such value was put on the notions, ideas or imaginations of individual investigators. The NIH itself has been the leader in biomedical science development in the last thirty to forty years. In your experience, who have been leaders, pioneers, inspirers, persons who got things done or fought battles to protect the independence of scientists?

JG: Let me talk about the extramural program first, because that's the part I know best. It's remarkable when you look at the NIH Directors, how many of them came either directly or remotely from the intramural program. Rolla Dyer was an important one, but he was before my time. Jim Shannon came from intramural programs, as did Don Fredrickson. The directors of NIH who have given the most to the national research effort have really been distinguished research investigators in their own right. Without naming names, there have been a couple in the last twenty-five years who were not as distinguished in terms of their own research productivity. But definitely Shannon and Frederickson and now Wyngaarden, who also came from the intramural program, have a great deal of respect and have made some very seminal contributions. Their styles have been very different; Shannon's in particular.

Shannon, in a quiet way, was a very "directive" person. When I first joined NIH, I remember proposals going up, but no formal written answers coming back, or else it was only something very brief that just said, "No" without a real reason. On a couple of occasions, having worked very hard on the development of some kind of a proposal -- perhaps a new grant program, or an increased emphasis in a grant program, to be directed at some particular problem -- a negative answer would come back, and I'd ask why. The first couple of times I didn't understand. A piece of paper would come back saying, "SSS". I finally found out that meant, in-house, "Shannon says so."! That would stop all discussion! If Shannon said so, it was not appealable.

As the organization has grown and become much more complex with all kinds

of structures and sub-structures, and echelons, things are not nearly that directive. Also, the degrees of freedom have changed remarkably. In the early years people like Frank Yeager made decisions that now an Institute director could not make unilaterally or independently. They were able to be much more responsive. Let me give you an example. I don't remember the particulars of this very well. In 1956, I believe it was in Puerto Rico, there was an outbreak of streptococcal disease in the form of Scarlet Fever or some other manifestation. Somebody called Jim Watt, the Director of the Heart Institute, and said, "There is an outbreak of strep and it would be great if we could follow those who have it to see how many, and in what pattern, will develop rheumatic fever, if at all." Jim was able to say, on the telephone, "What do you think you need to get started on that?" When he got that information he telephoned several of the council members, and in not much longer than twenty-four hours, he called this invetigator back and said, "Go. You are going to get a grant to do that." It was that kind of situation in which you had to act immediately to take advantage of the opportunity or forget it. It was a time-limited opportunity. Unfortunately I can hardly imagine that happening today. There was that kind of flexibility.

Today, I'm afraid, NIH might have to refer an individual investigator to a private donor or to a voluntary health agency like the Heart Association which might have funds for such emergency purposes.

SS: Tell me about Dr. Watt as director of the Heart Institute.

JG: Jim Watt existed in different era. He did a marvelous job, but a person with his background wouldn't be made Director now. He is a physician who was trained as an epidemiologist, expert in diarrheal diseases of the newborn and infants. He was not trained in cardiac diseases. His strength, in terms of the Heart Institute, is that he was innovative, flexible, and that he was oriented in the way that he would find out from the experts what was really worthwhile, then he would get moving and get it done without being unduly constrained.

Once, we needed biostatisticians and there weren't many available. If you're going to do good research, you need biostatistical advice not just at the end for interpreting data, but at the beginning, to tell you how to plan the design so that you can do it most efficiently. Jim Watt said, "If we don't have enough biostatisticians, then let's train them." So training grants were awarded in the schools of public health to train biostatisticians. People came to Jim Watt and said, "I cannot train people in cardiovascular biostatistics. I've got to use examples from cancer, or infectious disease, or arthritis." Jim said, "Fine." They'd say, "But you understand that when I finish training these people some of them will not go into heart research. They'll go into cancer research." Jim had no problem with that. Another area was primate research. The original NIH-funded primate research centers were non-categorical and funded by the Heart Institute under the leadership of Jim Watt and Frank Yeager and their effectively advocating that concept to the council. It was built into the Heart Institute budget. Those were non- or multi-categorical programs, and the attitude then was, "Somebody has got to do this, and we have some real interest in it, so let us do it."

SS: This reminds me of the famous Van Slyke criterion, which was: "We can support anything -- the whole body is bathed in blood," talking about NIH as a whole. Has this spirit of seeing things in the larger perspective -- not being constrained, as you say, by narrow definitions. Has that been handed down?

JG: It is part of the history of institutions that they become fractionated. In the early years the Division of Research Grants awarded grants because they were multi-categorical or non-categorical, but in such basic phenomena that it was of interest to all Institutes. Later that was taken out of DRG and put into a Divison of General Medical Sciences. Later yet, the Institute of General Medical Sciences developed. Now that degree of free-roaming exploration is more likely to run into problems, with one Institute feeling that something that is assigned to another Institute should be with them. One has to work harder nowadays to develop cooperation, co-funding for example, across Institute lines. That kind of thing takes a great deal of effort. From time to time there are pressures to give the NIH Director a pot of money that he might then allocate; if he sees a particular opportunity arising in one area, to take part of that money and give it to an Institute that is in charge of the specific area. I'm sure NIH Directors would prefer to have that degree of flexibility and responsiveness. On the other hand, the Institute directors want to testify to Congress, make their own case, and get their own appropriations.

SS: Have the Jim Watts of NIH history also had special political sensitivities and skills?

JG: Yes, but then again, it was a different era; one of "gung-ho" growth with people like John Fogerty and Lister Hill who, in the respective houses, were responsible not only for appropriations but were also chaired authorizing committees -- they held both positions. When you were in one room with Lister Hill, John Fogarty, Jim Shannon, and Mary Lasker, you had it all. They had more opportunities that they capitalized on, but they definitely had more access to the Congress and there was far less of a role from the Bureau of the Budget. When I joined NIH, if a your grant application was favorably recommended, you were funded. That was it. The priority score was meaningless -- all you cared about was whether you got approved or not.

Back to Jim Watt for a moment: Jim Watt became an Assistant Surgeon General, or Deputy. He was involved downtown at the Surgeon General's office with international health and he travelled a great deal. Jim was an unusual person. In some ways, he was not specifically direct. He would talk about ideas at length, but it was in sort of a "velvet fog"; you could come out of his office certain that you had a "Go ahead" but you weren't precisely sure what you could do.

SS: I think of Jim Shannon that way as well; more focused in his ultimate judgment or point, but still very indirect.

JG: If you were talking about an area, yes. But if you came to Jim Shannon with a proposal, you were more than likely to get a decision. You might have gone in to see him with the idea of "Jim, what do you think of this?" and you'd come out with a "No." before you even had your idea fully developed. When you went to see Jim Watt, on the other hand, if you had a poorly developed idea, he would love to have you sit down and talk about it. In some ways you felt like you were involved in a socratic dialogue. He would take off and make suggestions, but many times you'd walk back to your own building, and if someone asked you how the meeting went, you'd shake your head and say, "I don't really know." I remember times when Frank Yeager and I would sit down and try to write down what Watt had said after we'd spent an hour in his office. A couple of years ago he came back and talked to the advisory council of the Division of Research Resources. They now administer the primate research centers. They recognize

that Jim had the key role in the establishment of the primate centers, so they had him come back and talk to the council about those early days. I went to the talk just to see Jim, and I was amazed! I understood every word he said! There was no "fog".

SS: Once Luther Terry became Surgeon General, was he very visible?

JG: Yes. Luther was always the physician. One always had the feeling that he was patient-oriented in the sense of the individual patient. Bill Stewart and Jim Watt, from word one, thought about populations -- big numbers, public health. Luther was always the physician. You could so easily imagine him in a white coat with a stethoscope! You don't associate Jim Watt or Bill Stewart in that way. Luther was much more one-on-one.

SS: Did you have direct contact with Mary Lasker?

JG: Frank Yeager dealt with her directly to some extent. Jim Watt was the focal point. She was on the council when I was there, but I did not have very much direct contact with her. When I joined the Heart Institute in 1955, council meetings were closed: public people could not come in; staff could not come in. If you were in the Heart Institute, you were not allowed to attend that meeting unless you were invited. So staff representation around the room was very sparse. At the first council meeting that I attended, Mary was there. My impression of her was that she was deeply committed. She latched onto some issues she had been advised on -- and sometimes, in my view, poorly advised. But all in all, her impact was very positive. She had access to very important people and she did a great deal of lasting value.

Yesterday and Today

SS: Looking at the extramural program structurally, is DRG pretty much what it was twenty years ago (except in size, of course)? Were the foundations so solid that all you've done is extend from the early days?

JG: The peer review process, which is actually review by experts, has remained. DRG remains the "palace" of peer review, in some respects setting the standards for the rest of NIH and the Public Health Service. There are other dimensions that have been added. The management and structure of committees is now much more concerned about geographic distribution, minorities and women as members; the overall profile. There is increased concerned about making sure we don't have an "old buddy" system, continually recycling the same kinds of reviewers. There is also increased concern about how you recognize innovation and creativity, unorthodox ideas that may be risky, but worth the risk, in such a large endeavor. As dollars get short, how do we deal with the tendency for these review groups to protect their own disciplines by giving better grades?

There's another whole issue at DRG now with computer technology; there are certain kinds of statistical and analytical resources and services that we make available to NIH and to the Institutes. We're looking at the pattern of reviews and recommendations -- the funding patterns that now exist. DRG is still viewed primarily in terms of its review function. But there are all kinds of other things that have been added. In my mind, we must have concern for the long range. How far can we go with this system? We now have around ninety review groups plus the special study sections. We just keep adding more and more as

the number of applications continues to increase. I don't think that's feasible. We've achieved a certain economy of size. The cost of reviewing an average application in DRG has in fact come down because of better efficiency. In 1983 it cost $950 to review an application. That's the average figure, whether it's a fellowship or a research grant or a small business innovation research grant. In 1972 it cost $1,800! Those two dollar figures are in terms of 1983 constant dollars, so they really are comparable. Each study section is now reviewing a greater number of applications, and we now have computer technology; in addition we are conducting fewer site visits.

SS: Is there still a spirit of civic responsibility to serve on study sections?

JG: It is an honor to be chosen for a study section. It's a sign of great peer recognition. Most people put it in their resumes. I've seen it in "Who's Who", in grant applications, and when people apply for positions. On the other hand, it is a considerable burden -- a lot of work. Lately I am seeing more and more of those invited to be on a study section declining, for a variety of reasons. Some of them have individual financial problems. In advance of each meeting they spend an average of two or three weeks doing homework, reviewing these applications at home. They are not remunerated for that in any way. They write up a critique of the proposals assigned to them; their secretary types it up. They come to NIH for a three-day meeting during which they get a consultant's fee of $100 a day, which they consider inadequate. They get $75 per diem to pay for their hotel and meals. In a four-year term, they spend about three months of each year working for NIH, and they get paid for only ten or twelve days. Some of them have also commented to me that if the funding rate is only about 30%, then maybe their time would be better spent in the laboratory doing research.

SS: How many study section members are serving now?

JG: About 1,500 on standing committees.

SS: What should I be especially mindful of and looking for as I continue my interviews?

JG: We receive a fair amount of comment about "the creative and unorthodox". If a young investigator with an unusual proposal is right, then the reviewer's research and reputation could go down the drain. Is there some kind of vested interest; don't the study section members represent the current view and the current orthodoxy? How do you recognize the creative maverick? This is an unresolved problem.

SS: That's a very good question, not just because of the historic conservatism of scientific establishments, but because these days there is a tremendous pattern of specialization and sub-specialization; people get on their own tracks and don't see beyond them.

JG: With funds short, there is a natural disposition to "invest wisely", to go for the sure thing. That happens in the preparation and submission of applications also.

SS: This is a great deal of new information to me. Thank you, Dr. Green.

October 1986

CURRICULUM VITAE

Name: Jerome George Green

Date of Birth: June 20, 1929

Place of Birth: Brooklyn, New York

Marital Status: Married Marie Charlotte Roder, August 1952
Two Children: Karen Ann, born January 1958
Paul Jonathan, born May 1959

Education:

1947- Brooklyn Technical High School, Brooklyn, New York

1950- B.S. Magna cum laude in Biology
Brooklyn College, Brooklyn, New York

1954- M.D. Albany Medical College of Union University, Albany, New York

Post-Doctoral Training and Employment:

1954-1955 Rotating Internship, Albany Hospital Medical Center, Albany, New York

1955-1957 Extramural Health Scientist, Grants and Training Branch, National Heart Institute, NIH, Bethesda, Maryland.
Program and review responsibilities in research and training (under Dr. J. Franklin Yeager).

1957-1959 Resident in Internal Medicine, PHS Hospital, San Francisco, California.
Clinical training in University-affiliated residency program (University of California and Stanford); emphasis on cardiology (Chief, Dr. Richard H. Linn; Deputy Chiefs, Dr. Louis Gaul and Dr. David Horwitz).

1959-1960 Special Research Fellow, Cardiovascular Research Institute, University of California School of Medicine, San Francisco, California.
Research fellowship; Sponsor, Dr. Julius Comroe, Jr. Pulmonary physiology with Dr. Comroe. Circulatory dynamics (peripheral and central) with Dr. Elliot Rapaport.

1960-1965 Senior Research Fellow and Clinical Investigator, Research Division, Cleveland Clinic, Cleveland, OH.
Clinical research in Atherosclerosis/Nutrition/ Hypertension with Dr. Irvine H. Page. Concurrently, served as NHI liaison and coordinator for a large multi-institutional clinical trial (feasibility), The National Diet-Heart Study.

1965-1966 Deputy Chief, Extramural Programs, NHI, NIH, Bethesda, Maryland.
Management and administration of extramural grant and award programs.

1966-1972 Associate Director for Extramural Research and Training, NHLI, NIH, Bethesda, Maryland.
Responsible for all extramural programs (except contracts) in terms if development, initiation, review, administration, budget development, etc. Development and implementation of reorganization plan for separation of program and review.

1972-1986 Director, Division of Extramural Affairs, NHLBI, NIH, Bethesda, Maryland.
Administration and management of grant, contract, and cooperative agreement research/training support mechanisms. Coordination among, heart, lung, and blood programs. Staff: approximately 110 individuals; scientists, administrative, clerical, and support.

1986-present Director, Division of Research Grants, NIH, Bethesda, Maryland.
Management of central receipt and review of NIH research grants, National Research Service Awards, research and academic career development awards, etc. Development and implementation of grant and award policies and procedures. Collection and analyses of data on character and direction of NIH research and research training support.

Service:

Commissioned Officer, USPHS, since 1955
Grade: Medical Director (0-6), 1965-present

Medical Certification:

Diplomate, National Board of Medical Examiners
Board eligible, Internal Medicine
Current medical practice licensure:
New York, California, Ohio, and Maryland

Professional Societies:

Fellow, American College of Cardiology
Fellow, Council on Arteriosclerosis, American Heart Association
Fellow, Council on Epidemiology, American Heart Association
Fellow, Council on Cerebrovascular Diseases, American Heart Association
Member, Council on Thrombosis, American Heart Association
Fellow, Council on Epidemiology and Prevention, International Society of Cardiology
New York Academy of Sciences

Research Interests:

Cardiopulmonary physiology
Atherosclerosis
Nutrition
Epidemiology of cardiopulmonary and thrombotic diseases

Honors:

B.S., Magna cum laude
Phi Beta Kappa
Alpha Omega Alpha
Member of Executive Committee, National Diet-Heart Study
Letters of Commendation
PHS Meritorious Service Medal 1973
PHS Distinguished Service Medal 1982
PHS Meritorious Service Medal 1985
Assistant Secretary for Health's Award for Exceptional Achievement 1985