AN INTERVIEW WITH DR. RALPH KNUTTI

BY STEPHEN P. STRICKLAND, PH.D.

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Introduction and Biographical Sketch

This interview with Dr. Ralph Knutti 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. Knutti, 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. Knutti received his education and training at institutions across the United States, graduating from West Virginia University and from Yale University School of Medicine, doing an internship at Western Reserve University School of Medicine in Cleveland, Ohio, teaching at the University of Rochester School of Medicine, working at the Trudeau Sanatorium in New York, and moving to the University of Southern California as Assistant Professor of Pathology in 1942, remaining there for nine years.

In 1951 Congress passed the Omnibus Medical Research Bill, creating several new Institutes, those charged with responsibility for Arthritis and Metabolic Diseases, for Neurology and Blindness, and for Allergy and Infectious Diseases. Very shortly thereafter, Dr. Knutti was asked to come to Bethesda to help create the new Arthritis and Metabolic Diseases Institute, as Chief of Extramural Programs of that Institute. A few years later, he was asked by Dr. James Shannon to move to the National Heart Institute as Director. In the first instance, Dr. Knutti was a pioneer in the creation of new programs, including fellowship as well as grant programs, and in the second, he led one of the two largest Institutes of NIH. In addition to working with NIH Directors Dr. Henry Sebrell and Dr. Shannon, Dr. Knutti worked directly with leaders in particular fields including Dr. Michael Debakey and medical research "lobbyists" like Mrs. Mary Lasker, and with key congressional committee chairmen. His perspective is thus that of a central figure in NIH for approximately two decades. As such, his is an important story in the history of the extramural program.

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WASHINGTON, D.C.

Interview by Stephen P. Strickland, Ph.D. with Dr. Ralph Knutti

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SS: Dr. Knutti, I'd like to start by asking you a little bit about your personal background and training, and how you got into the Public Health Service.

RK: I graduated from West Virginia University in 1923, where I majored in Zoology. From 1923 to 1924 I taught in the Grafton, West Virginia High School. In the fall of 1924 I entered the Medical School at West Virginia. At that time the school offered only the basic first two years of medicine; so in 1926 I transferred to the Yale University School of Medicine from which I received an M.D. degree in 1928.

At the time I graduated from Yale, I had planned to go into surgery and in fact received an appointment for a surgical internship at Western Reserve University School of Medicine in Cleveland. Because of staggered internships there, this appointment did not start until May 1 and the following year, and Dr. Elliot Cutler, Chairman of Surgery there, advised me to spend the interviewing time in pathology. As the result of his advice, I applied for and received an appointment in the Pathology Department at Vanderbilt University School of Medicine under the direction of Dr. Ernest W. Goodpasture. During that year I was able to participate in a research project as well as teaching.

During my subsequent internship at Western Reserve I decided that I preferred research and the academic atmosphere rather than the practice of surgery, and, chiefly because of a paper I had published while at Vanderbilt, I was accepted as an assistant in Pathology and Bacteriology at the Rockefeller Institute for Medical Research in New York City. I spent two years at the Institute, during which I worked and published on bacterial inhibiting agents and the etiology of trachoma, in addition to getting more experience and training in bacteriology and virology.

SS: What great experience. And where did you go next?

RK: In 1932 I moved to the University of Rochester School of Medicine as an instructor in the Pathology Department under the aegis of Dr. George Whipple. I remained there for nine years, from the depths of the Great Depression to the start of World War II. While there, I was mainly interested in liver pathology and published a number of papers in that field. Of course, I also participated in the teaching and service responsibilities of the department.

Then in my next to last year in Rochester I developed pulmonary tuberculosis and spent one year at the Trudeau Sanitorium in Saranac Lake, New York. There I had the time to, among other things, reflect on my future plans, and concluded that I had better be broader based than I was at the time, so it was time for me to move on. I had received an offer from the University of Southern California; so, after visiting that institution, I accepted the position of Assistant Professor of Pathology (and later Associate Professor) and Director of the Pathology Department at the Los Angeles Children's Hospital.

SS: That brings us up to what year?

RK: 1942. So I was in California for nine years, and during that time I got more and more involved in administration. I had served on a number of committees -- not only on those of the hospital, but also on local and state committees. I liked these activities as well as the administration of a large staff, and at the same time I was still conducting some research.

I found out about the National Institutes of Health through a friend of mine, Dr. Floyd Doft, who later became the Director of the Arthritis and Metabolic Diseases Institute. At my request he told me how to apply for a research grant. This was about two years before I left California. I applied for a grant and was successful. Then one of my staff applied and was awarded a grant but the Institute didn't have funds to support it. The NIH told him that his priority wasn't quite high enough to meet the cutoff. Knowing nothing about priority rankings, I wrote to Doft asking how an application could be approved and still not get paid. Shortly after his reply, Dr. David Price, who was then Chief of the Division of Research Grants (succeeded by Ernest Allen), came to California to visit several institutions and paid us a call. He explained the policies in great detail and I was very much impressed in hearing about the system. At that time the seeds were sown which later resulted in my moving to Bethesda in the spring of 1951.

I arrived at NIH shortly after the "Omnibus Bill" had been approved. In this bill several new Institutes were established, including those of Arthritis and Metabolic Diseases; Neurology and Blindness; and Allergy and Infectious Diseases.

SS: And that really was the major expansion since the war. The Cancer Institute was already in existence, and I think Mental Health was created in 1946 and the Heart Institute in '48. Then came the Omnibus Bill.

RK: Yes, I believe so. I had been commissioned in the Public Health Service as a "Medical Director" and my specific title was "Chief of Extramural Programs, NIAMD". The Institute had just received it's first appropriation for extramural activities, so I was fortunate in getting in on the ground floor of a completely new operation.

SS: How big was that?

RK: The amount was $1,345,000.00, which at that time sounded like a whale of a sum of money.

SS: Didn't it in fact <u>quadruple</u> the amount that had been spent earlier in the field of arthritis?

RK: More than quadruple, from what I recall. The Arthritis and Rheumatism Institute, with which we worked very closely, was new, but had given some support. Some of the leaders of that organization, like Mr. Floyd Odlum, General George Kenny, and Dr. Paul Holbrook served on the original NIAMD Advisory Council.

At that time there was a feeling in some private fund raising organizations that government support for medical research might inhibit their own drives for funds. There was discussion for a number of years on that point, but in the long run there was no open controversy and each supported the other, in

such forms as joint support of national conferences and testimony to congressional appropriations committees for our budget by officials of private organizations. For example, very early on, the First National Conference on Arthritis and Rheumatic Diseases was held in Bethesda under the mutual aegis of the Institute, the Arthritis and Rheumatism Foundation, and the American Rheumatism Association.

SS: Because this was the first significant time, if not the very first time NIH had put money into arthritis research, did you have to create a special study section?

RK: Not at that time. The existing study sections handled the requests. Projects in the basic areas were assigned to the appropriate panels like biochemistry, physiology, endocrinology; those more clinically oriented might go to metabolism and nutrition or general medicine. In regard to the original orientation of the applications, before I arrived, the Division of Research Grants had circulated a letter to the heads of departments of internal medicine in all of the medical schools describing the new Institute and its potential programs. I am not certain whether Dr. Price or Dr. Sebrell, the Director of NIH, signed the letter. Responses were received from perhaps 15 or 20 departments. The first formal applications were largely in the clinical area and most of these bore essentially the same title: "The Effect of Cortisone (or ACTH) on Rheumatoid Arthritis". This title was stimulated by the work of Dr. Philip Hench of the Mayo Clinic for which he shared a Nobel Prize for his original studies of the same problem. It was only later, perhaps due to the original grants for work on this subject, that the serious side effects of this treatment were pointed out.

SS: When you created this new Institute and it extramural program how else did you get the word out? I suppose it wasn't difficult.

RK: With the information received from the responses just referred to and with the advice of consultants, I visited almost every medical school in the United States. I met with chairmen of the departments of internal medicine as well as with other faculty members interested in the field including some basic scientists.

SS: Essentially, then, you insured from the start that there was a balance in the hope that you were supporting work in the whole spectrum of fundamental research?

RK: Right. Of course, we were also supporting research in Metabolic Diseases. And "metabolic disease is a very broad term. We took full advantage of the fact" that it was broad, so that ultimately, in the ten years that I was with that Institute, we embraced diabetes, gastroenterology and kidney diseases. Like Dr. Van Slyke, first director of the Heart Institute, said: the scope of the Heart Institute encompassed the whole body, because the heart pumps blood and the blood furnishes every organ!

SS: I remember that: "The whole body is bathed in blood," was his phrase.

RK: Right. So when I went to these medical schools, I talked not only about arthritis, and not only to clinicians, but also tried to set up little meetings. And, as a non-experienced promoter, you might say, it could have been done much better, but we did get some inklings, and I transmitted these to the appropriate individuals.

SS: And who were they?

RK: The Division of Research Grants, particularly the Study Section Executive Secretaries; also the extramural programs, the Director of my Institute, and at times, certain members of the National Advisory Council. There were certain policies in the Division of Research Grants that some of us didn't always agree with; certain details which were matters of continuing discussion.

SS: Can you give me an example of those issues?

RK: One of the problems that I encountered was with their fellowships program at the time. Some of my counterparts and I felt some dissatisfaction with the fellowship programs and how they were run. Incidentally, I learned a lot about other extramural programs from my peers because we were the only Institute at that time that had money for nothing but research grants. Heart, Cancer, and Mental Health had broad programs: traineeships and fellowships and the like. Before the end of that fiscal year, though, we were able to split off some of the $1,345,ØØØ to start some traineeships, and later we were able to embrace some fellowships from the then existing program in the Division of Research Grants.

That has been a continuing process at the NIH. As new Institutes developed, and as new programs were started, they surveyed what was then being supported. One had to make arrangements with the Institute directors and the extramural managers to transfer projects from one Institute to another. For example, when the Child Health Institute was formed, which was before '61, we were told that each Institute would be expected to transfer a certain number of grants to that Institute if they seemed to be more pertinent to its programs. For example, Dr. Robert Aldrich was the first Director of the Child Health Institute. I had known him previously because he was involved in some of the NIAMD programs in metabolism as Professor of Pediatrics at the University of Washington in Seattle. He visited each of our extramural chiefs personally, and went over their whole list of programs. Bob was a little hungry, and sometimes we had to defend ourselves a little bit, but his Institute did build up a basic program that way, from the transfers. And I think that was good. But we all had our own "little empires" and we shielded them as well as we could.

SS: Who was the director of the Institute of Arthritis and Metabolic Diseases?

RK: When I first came, Dr. Russell Wilder. He had been the chief of medicine at the Mayo Clinic in Rochester. Dr. Wilder gave a great deal of responsibility to his senior staff, in which I was included, which meant that my shop had a considerable amount of responsibility. He retired after about 2-3 years and was succeeded by Dr. Floyd Doft.

SS: Your budgets were growing in this period, weren't they, in the late '5Øs and '6Øs?

RK: Oh, yes.

SS: So I assume that was the factor that encouraged you and others to make your own decisions.

RK: Yes. At least, we never kept any secrets. One would go to Dr. Wilder or Dr. Doft, or whoever it may have been, and say, "I think this is a pretty good thing. Do you want to look into it?" And I remember when we started the train-

ing grants program, which followed the traineeship program, I went to Dr. Van Slyke, who had moved from the Heart Institute to be the Associate Director for extramural programs to discuss the guidelines with him directly.

SS: And you could go directly to him because it was still relatively small and everybody knew each other, so you could do it a little more informally?

RK: Yes. I would tell Dr. Doft that I had studied the programs of the other Institutes and their training grants programs as examples, and I had come up with a plan for our Institute. I would ask him to look it over and if he said, "O.K., go talk to Van," I would do the same thing with Van Slyke. Then he'd look it over and say, "Send me a note." So I would send him a note through Doff. I understand they don't do it that simply anymore.

SS: I guess that's because it's more complicated now.

All of this suggests that in fact you came in, you oversaw the development of the establishment of a grants program in Arthritis and Metabolic Diseases. You went out and told basic researchers and clinicians about this, and I take it you also recommended people for study sections and advisory councils.

RK: Yes. We didn't have any authority to select study section members, but we had the privilege of nominating them. In my position I could also recommend council members to my Institute Director. With his approval, from there they would go through channels to the Public Health Service, then to the Department.

SS: Did the secretary appoint council members?

RK: I believe the Surgeon General signed the letters.

SS: Was Dr. Dyer still there when you came?

RK: No, he'd gone. Dr. Sebrell was Director of NIH when I came in 1951.

SS: I don't have a clear sense of Dr. Sebrell's personality.

RK: He is a very old and close friend of mine. He is retired now and living in Florida. His wife was a classmate of my wife in college, and Henry and I are the same age. He was a career Public Health Service officer, then he went to New York when he retired and worked for the American Cancer Society for a short time. Then he set up and became the head of the College of Nutrition at Columbia University. He was of the old Goldberg school of nutritionists that started out in the hygienic laboratory of the Public Health Service.

SS: Nutritionists are coming back to the fore now, so that must please Dr. Sebrell.

RK: Yes. After retiring from Columbia, he became the Scientific Director of Weight Watchers. He has retired from that position, but he is still going to the nutrition meetings all over the world and is very active.

SS: I do want to try to see him.

RK: You'll have a nice trip to Florida if you want to see him, if you can catch him. He's in Pompano Beach. I'll give you his address.

SS: I know that he worked very closely with Dr. Scheele, who, some years ago, told me that. But, was Dr. Sebrell a real entrepreneur? Was he sort of leading the way ?

RK: Oh, yes. He is a pretty remarkable man.

SS: Regarding your move from the National Institute of Arthritis and Metabolic Diseases over to the National Heart Institute, you were saying that your understanding is that you were not Dr. Shannon's first choice.

RK: That's true. His first choice was Dr. Coles Andrus, who had experience with the Heart Council, and was in charge of cardiology at Johns Hopkins Medical School, and was about to retire from his position there to go into private practice. Apparently Andrus was not interested in an administrative job of this type.

SS: I know his name because I remember that he was one of the outside witnesses sometimes called to testify before congressional committees. He seemed to be effective. So, you were pleased to have that opportunity to head one of the two most important Institutes at NIH?

RK: I was flattered, yes.

SS: Was this a pattern of Dr. Shannon's?

RK: He made the recommendation. The decision had to go up to the Surgeon General, who at that time it Luther Terry. I don't know if it went beyond Terry or not. I doubt it.

SS: Did the Secretary make the appointment, or the Surgeon General?

RK: I would assume that it would be the Surgeon General. At any rate, the recommendation was approved and one day Jim Shannon called me on the telephone. He caught me at the airport as I was on my was to Israel! They paged me at the airport and Jim said, "Ralph, I'm prepared to offer you the job." I said, "Let me think about it and I'll let you know when I get back." He said, "I've got to announce it and I'm pressured to get an answer right away. Are you ready to accept it?" So I said, "Sure."

SS: But I take it you had some conversation before this?

RK: Of course we had talked about it on several occasions. And he had told me that Andrus had been offered the job, and that I was basically waiting in the wings.

SS: So you moved over to the Heart Institute and there you had a somewhat different situation. You had the second oldest Institute; you had large outside organizations like the American Heart Association; you had medical research lobbyists like Mrs. Lasker who was interested in the heart and was always pushing; you had an established scientific field that had been funded for a good while. Did those elements make the way you approached your job any different?

RK: Well, it didn't give my any inferiority complex because I wasaccustomed to an association with people of that type. I already knew Mrs. Lasker from my time at the Arthritis Institute. I knew some of the people who were prominent

in the heart field, like Irvine Page, who was at the Rockefeller Institute the same time I was there.I knew a pretty good share of people in medical research in a number of fields at that time, again because of my communication with departments of medical schools. After all, both the heart and metabolic diseases are divisions of departments of medicine. I had also been dealing with foundations. I met a lot of people after I became associated with the American Heart Association whom I had not known before. But the responsibilities were not new. I think that at first, I spent more time meeting the intramural staff of the Institute and educating myself about its programs than I did with public relations.

SS: What about relationships among the directors of the Institutes; that is, one has the sense, particularly in the early days, in the '5Øs, it was very collegial, an esprit de corps, and things were easy and informal?

RK: I guess in comparison with the present time, yes. But when you get any group of humans together in the same areas, there is bound to be competition and varying ideas. Some guys are promoters and some guys are dreamers and some guys are practical while others are not. In the day-to-day association with these people, you get to the point where you understand each other and can get things done. It depends on how seriously one takes it, but sometimes you bled, and sometimes you won, and other times you just "went along". There was no petty competition. I don't know of any Institute Director in my experience that I didn't assume liked me; I liked all the Institute Directors, and I think they all liked each other; they were broad people. Although their opinions might differ -- they might fight like hell about a point -- they still respected each other. That's the way I saw it.

SS: What about, for example, those who wanted to go faster and those who did not want to go so fast?

RK: Our rate of speed depended upon the Congress. It also depended upon the number of applications from the extramural standpoint, as well as successful intramural research progress.

SS: But inevitably a larger appropriation brought more applications and you always had more applications approved on merit than you had money to fund, didn't you?

RK: Yes. One of the problems was that sometimes we got more and more money. Occasionally the basic tenets of the NIH were to blame for this, plus the fact that, at that time, and all the time that I was at the NIH, things changed very rapidly. Early on, Congress, the Administration, and everybody else were for "home, mother, and medical research". It wasn't a hard job to <u>get</u> money then.

SS: That was one of my questions. Did you feel like you were in control with respect to appropriation?

RK: No. One felt like the Congress was trying to help, yet the amount of money one asked for was completely controlled by the President's budget. So you didn't go to the Congress to ask for money for what <u>you</u> thought was necessary or for what your council thought was necessary, <u>you</u> went to the House and Senate to defend the President's budget. In those days, the President's budget was raised a bit every year. In addition, we were helped greatly by private witnesses at the annual hearings. We could always justify asking for more

money for extramural, because we always had a backlog of unpayable requests. Once, for a short time, we got more money than we were allowed to spend, so we had to turn back to the U.S. Treasury $19 million! The reason for that was (I think it was during the Johnson Administration) the Bureau of the Budget froze expenditures shortly before the end of the fiscal year. We had already approved commitments, which we had to abrogate.

SS: This is valuable to me particularly in connection with the motto that you repeated a while ago, that the program was for, of, and by the scientists. Yet, somebody else was saying, "Here's the pie you can carve up." I take it that wasn't terribly out of kilter with what you and your colleagues and the scientific community thought was appropriate -- the size of the appropriation.

RK: Well, the thing that always disturbed me a little bit, and I think I have talked about this before, was the pressure to try to spend more money when there was some question as to the quality of the new program that was being supported. Also there was always some question as to whether we were encroaching upon support of medical education instead of research.

Sometimes there was a feeling that it would be just as good if we kept the levels of existing programs even higher, and didn't have to dream about starting new programs, like the famous "program projects" thing, which you may have heard about. I was not enthusiastic about that. I thought that was a little too diffuse.

SS: Could we talk about your perspective of how much scientific and medical progress was made in the period you were there? You could see programs and fields developed, and that had to give you considerable gratification.

RK: I had some gratification in being the first person to sign off some contracts for the development of the artificial heart. Now, as time passes, I wonder if it wasn't a mistake. This was started de nouveau in the Institute -- the Heart Council decided that work should start on thinking about an artificial heart. The Council really dreamed this one up. It was my job to defend it in Congress.

SS: Who on the Council was particularly interested in it?

RK: Practically all of the members. Mike DeBakey was not in favor of the implantable type of artificial heart. He was in favor of a booster type heart. At this time I think he was right.

SS: I had a conversation with Dr. DeBakey last month in Houston and we were basically talking about the Library of Medicine. As you know, he played a role in having the Library separated from the Armed Forces and become the National Library. He very much wanted it to be right adjacent to NIH. He thought that was terribly important. I talked to him as well about progress in medical science and medical care, and, as I relistened to that and listened to the transcript, I was struck with the fact that, these days, he talks much more about prevention and things that we can do to keep from having to have aorta operations and other kinds of bypasses, and on the basic side as well. I know we understand much more of the elements involved.

RK: It's phenomenal what has developed. In so many fields, particularly gene splitting, microbiology, and immunology; there is where the greatest progress has been made, I think.

SS: Would you talk a little bit about Mike DeBakey and his relationship to the Heart Institute and NIH? He was in such a preeminent position and has been a pioneer in various clinical advances, especially surgical advances. He also is a great promoter. Was he a good guy to have on your side?

RK: Yes. He was very dedicated to the development of things cardiovascular as well as in large NIH support to things like cancer and stroke. He had gained a tremendous reputation and ability as a surgeon, and he had the gift and the intelligence to be able to get things done, just like Henry Sebrell had in his field. One would not always agree with him, but generally one found that if they didn't agree with him, in the long run he was either probably right or won out anyway. Like with the artificial heart. We don't have enough human hearts to give to everybody who needs one, and maybe someday the artificial heart will work out, but our preliminary trials have been sort of ghastly, it seems to me. Nevertheless, you've got to start somewhere. Maybe this will be proven wrong in the future.

SS: Was Mrs. Lasker considered more of a help or a nuisance?

RK: Mrs. Lasker knew more about my programs, at one point, than I did. She was really on the ball. She invited me out to see her once; she did this routinely for all the Institutes. This was when I was quite new as an Institute Director, She had a little notebook with her and asked me about a couple of programs that she had in that notebook that we were supporting which I had not yet heard of! If there's one lay person who I think is responsible for the growth of the NIH, I think it's Mary Lasker.

SS: But, I take it in some points of time, at least Dr. Shannon thought she was pushing too much or going too far.

RK: I never heard him say that, but I had that feeling.

SS: Would you talk a little bit about Dr. Shannon and his overall leadership and managership. He, of course, gets great credit, and I'm sure it's appropriate for his guiding NIH through its period of greatest growth. But did he simply preside and judge?

RK: No. He got things done. Sometimes he got things done in ways some of us didn't agree with. There was one time there was a strong feeling between the individual Institutes and the front office about the fact that Jim was trying to take over the whole show, which didn't turn out to be the case, as I look back on it. He was trying to get a better means of communication between the Institutes and the front office. In retrospect I think it was all for the good. Jim had a vision of good organization, and he was a courageous guy.

I told some friends recently about his behavior in the House of Representatives committee hearings one time, when Congressman Flood of Pennsylvania, who was Chairman of the committee, accused the NIH and the study sections of "playing footsie and scratching each others' backs" with investigators and medical schools all over the country in promotion of research grants. I never saw the Congressional Record of this statement and I don't know how much of it went on the record, but I was there. After a long tirade, Flood finally finished, and Jim slammed his fist on the table. Jim spoke mildly, even before he had his problem with his larynzx; he had a very soft voice. Sometimes it was difficult to hear him at the end of a table. But he said, in effect, "Mr.

Flood, you are utterly and absolutely wrong in everything you have said about the relationships of the NIH with the study sections and investigators." And he spent 10 or 15 minutes expounding on this, addressing each point by point, one, two, three, four, five. And he negated everything Flood had said, and Flood sat down. He was courageous, and not many people would figuratively smack the chairman of the committee that was responsible for your appropriations. But Jim smacked that guy.

SS: He was an absolutely remarkable man.

RK: He was a tremendous help to me. Particularly during the first hearings in which I had to testify to defend the program and the budget.

SS: Now, how about a few words about the grants program itself? It started as a relatively simple process. The idea remains relatively simple. The mechanisms may have become a little more elaborate, but not much more. In your fifteen or so years at NIH, did you see changes in the grants program, i.e. the review system?

RK: Inevitably, one would not be able to attend all the study section meetings. When I first went there, I attended every study section meeting, for the first two years that I was there. I wanted to get the feeling and hear the discussions, and also to see the method behind this because I wanted to see what programs might very well fit into the interests of my Institute. We were then searching and groping for things in the field that might be transferred. In other words, we were all trying to grow and develop because we realized the potential for this in the United States was not only of high quality, but it was tremendously valuable. There was also some low quality research which needed improvement. And I think the study section peer review system is the fairest thing that was ever developed to screen all these things -- a priority system. In the 21 years since I retired, I am certain many changes have taken place, but the study sections still exist.

SS: From where you sat -- going to meetings all over the country, people coming to you -- you probably had a better sense of developments and new possibilities than just somebody who was chairman of a given department, for example. Did you ever have the feeling, either in the Arthritis and Metabolic Diseases Institute or in the Heart Institute, that the study sections in some ways were too conservative? Maybe didn't see the potential of certain innovative approaches?

RK: I think the study sections <u>were</u> conservative, particularly in regard to dollar amounts of grants. There were some study sections which were more conservative than others. I don't know of any study sections that would cross the border into the real left wing attitudes. Somehow or other, we were able to pick really top people in the country for section members. I believe that the great majority of them were objective and fair, but their mission was in the area of specific scientific disciplines, not in the field of broad program decisions.

SS: So you had such a diversity. What about the age range? Not everybody was "of hoary head".

RK: No. There were bright young people as well as members in their middle years. I don't think anybody was really "hoary", but I don't know of anybody

in the study sections that I can think of offhand, when I was around who was not in active academic or scientific work. The older guys were frequently on the councils, but not the study sections.

SS: So this system of "for the scientists, by the scientists, and of the scientists" didn't, in your experience, ever retard new opportunity?

RK: I was told once upon a time that this motto was naive. It may have been naive after the NIH got so big. After it reached a certain point one couldn't fight the bureaucracy any more. The bureaucracy ultimately took over.

SS: What were the bureaucracy: the Surgeon General or the Secretary?

RK: I'm talking about right up to the President of the United States.

SS: Making certain initiatives that you had to go along with?

RK: During the Nixon Administration, they tried to slaughter the NIH. In the Reagan Administration, I am at a loss to understand their attitude. Maybe NIH is too big. It certainly is very big now. There is a great feeling of frustration among people I still know at the NIH about the events that have taken place in the past sixr years. We've had some top-notch, world people at the NIH, and still have a few, and how you can attract good people if you bind them up, I don't know. Our motto about the intramural programs was "Give the scientist his head", full freedom. And we defended that one, both in the Heart Institute and the Arthritis Institute. We were able to recruit and train excellent staff.

SS: How much did you rely on the contract mechanism at the Heart Institute?

RK: Before and during my tenure contracts had been used -- these were usually small, perhaps less than $25,000, and were mostly aimed at a particular service, not research per se. For example, we contracted with Dr. Helen Taussig of Johns Hopkins University to travel to Germany to investigate the thalidomide tragedy which had been reported there. Her visit resulted in stopping its use in this country.

The largest contracts were made at the beginning of the artificial heart program. These varied in scope, from the formation of a primary advisory committee to that of the payment for specific areas of investigation relative to the production of an implantable heart. They bincluded such items as power supply, lining surfaces and other items of that nature. This initial advisory committee was of particular assistance in counseling on the development of contractural relationships; the "systems" approach to program development -- more of the aspects haveing to do with planning rather than the nitty gritty details of the product itself. On this committee were individuals like Hugh Dryden, then head of NASA, William Baker, Director of the Bell Telephone Laboratories, as well as people of distinction from academic, government and industrial sources. The results that followed occurred after my retirement, but I had the satisfaction of signing off the first contracts.

SS: In a way that was always the case with NIH, wasn't it? As soon as a new program was established, people would respond to it. I think that's one of the great accomplishments of NIH over the last quarter century; it's putting the spotlight on something which people will respond to and come up with good ideas.

RK: Yes. If one can identify a really important need, which takes bit money to support, and can justify that need, mechanisms are available for its establishment.

SS: That's very good. What have we left out of importance that you can think of that we might add?

RK: Well, I think you asked the right questions.

SS: Dr. Knutti, this has been a very informative session and I thank you for your time.

RK: My pleasure.

Ralph Eddy Knutti

Curriculum Vitae
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Assistant in Pathology, Vanderbilt University 1928-29
Intern, Lakeside Hospital, Cleveland, Ohio 1929-30
Assistant in Pathology & Bacteriology, Rockefeller Institute for Medical Research ,NYC, 1930-1932
Instructor in Pathology, University of Rochester 1932-35
Assistant Professor of Pathology, University of Rochester 1935-1942
Director of Laboratories, Park Avenue Hospital, Rochester, NY 1939-40
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Associate Professor of Pathology, University of Southern California 1948-51
Director of Pathology and Laboratories, Los Angeles Childrens Hospital 1942-51
Medical Director, United States Public Health Service 1951-1965
Chief, Extramural Programs, National Institute of Arthritis & Metabolic Diseases, NIH, Bethesda, Maryland 1951-60
Associate Director, NIAMD, NIH 1960-61
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Executive Officer, Universities Associated for Research & Education in Pathology 1965-72
Executive Officer, American Society for Experimental Pathology 1965-72
Member, Board of Directors, Los Angeles Child Guidance Clinic 1949-51
Member, Directing Council, Pakistan-SEATO Cholera Laboratory 1961-65
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