Dr. Ralph E. Knutti
Second Interview

Date: July 27, 1972
Interviewer: Dr. Wyndham D. Miles

Q: Dr. Knutti, would you like to take on from where you left off last time?

Dr. Knutti: I believe we left off in Southern California after I had been successful in getting two small research grants from NIH. I won't go into the details of those, except to say that they were extremely helpful in getting technical assistance and getting some materials and equipment that was needed to pursue the two problems.

One of the two problems had to do with renal damage caused by sulfa drugs and the effect of high-protein and low-protein diets in protecting against the damage to kidneys. In effect, rather large doses of the sulfa drugs were given to rats, and it was possible to protect a very significant percentage of the rats from renal injury by giving them high-protein diet, whereas if they were on a low-protein diet or protein-free diet, the mortality rate was 100%.

The other problem had to do with some work that had been started in Rochester relative to what we called the transfer of blood colloids. Animals that had been given gum acacia again showed marked changes in their circulating plasma proteins, and circulating acacia by changes in the diets. There was a reciprocal relationship. That is, when an animal was put on a low-protein diet, the plasma proteins in the blood would diminish, and plasma protein would increase. This protein was drawn from sores chiefly in the liver. The rats, in this instance, having been given large doses of acacia, which was stored in their livers. At that time they were receiving none of the gum. This was ended up published in the *Journal of Experimental Medicine* in 1950 and '51, I believe. I wasn't quite certain of or perhaps paid no attention to NIH policies at that time. I didn't give NIH credit for some of the work in the papers, although the bulk of the work that was published then was indeed done at Rochester. This was a wind-up.

About that time, I also became further aware of the NIH in that I had a very outstanding young man working in my department at Children's Hospital. His name was Phillip Sturgeon. He's now Research Director of the Western Region for the Red Cross. He's on our present Hematology Study Section of NIH and has had a very excellent career. I guess he was my first resident in pathology when I went to the Children's, and he had to go in the Army, so he only spent a month or six weeks during one summer. When he came back, he wanted to finish pediatric training, but he did want some training in pediatric pathology, so he alternated and ultimately wound up full-time in the pathology department in charge of hematology research and also the routine hematological laboratories of the hospital.

This was at a time when the RH factor was becoming very popular, and he adopted techniques for RH-ing parents and children. He also, I think, was the first person to describe an antibody occurring in infants who had a type of hemolytic anemia. This was published in *Science* somewhere in the late 1940s. At any rate, he showed great promise, and I had no real money to pay him. He was working, actually, on a technician's slot in our personnel set-up, and it became evident that he was going to either have to either get more money from the hospital and/or the medical school, or go into the practice of pediatrics, for which he was qualified. I realized, after having received grants from NIH myself, that it might be possible to write his salary or a portion of his salary on a research grant, so I encouraged him to write out a research grant application on a problem which he wanted to work, and submit it to the NIH.

This started an interesting chain of events and indicates how easy it is for people in the field who are not intimately connected with NIH policies can be misled by the approach of NIH staff in their extramural activities. After this application was submitted, it went to the Hematology Study Section. At that time, Leonard Karel was the executive secretary of the Hematology Study Section, and one day Sturgeon got a call from Bethesda, which was, of course, most unusual for us on the West Coast to get calls from the East about our work or our interests. He was asked some questions about his proposal. In the ensuing discussion, Dr. Sturgeon got the idea that everything was fine, that he was going to receive the grant, even though it hadn't yet been acted upon by the Study Section. This, of course, is one of the delusions that people in the medical schools used to have, and I dare say still have. Even when someone who is administering a program here talks to them about their problem, they got the idea that this fellow was back of them and he could make the decision as to whether or not they were going to get the grant, which, of course, is not the case.

A short time thereafter, they changed executive secretaries in the Hematology Study Section, and Frank Yeager, who is now our associate director for extramural programs in the Heart Institute, took over the situation. As I remember it, Sturgeon got a call from Dr. Yeager, and he gained the impression from this telephone conversation that the grant was in the bag, so he gave up the idea of accepting an offer from a Hollywood pediatrician, to go into practice with him, and decided to stay at the Children's Hospital, where he would receive a salary from the research grant.

A very short time thereafter, he got the letter that many people have received, saying that unfortunately, although his grant had been recommended for approval by the National Advisory Council, that funds were not available to pay the grant. When he showed me this letter, I'm afraid I hit the ceiling, because I thought this was a real devious way of giving someone the brush-off, and if they weren't going to give him the grant, why didn't they tell him that they were not going to give him the grant instead of approaching it in a left-handed manner? This, of course, is not the case. This is a true statement, as all of us who have been here over the years know, that the councils can recommend grants for payment, but if we don't have the funds to pay them, we can't pay them, even though they may be high priority pieces of research. It depends entirely on available funds.

At any rate, I got steamed up about this, and so I wrote my old friend Floyd Daft, he being the only person I knew at NIH, a letter in longhand, in which I told him that I didn't like that method of operation; if they weren't going to give Sturgeon a grant, why didn't they just say that they weren't going to give him a grant instead of trying to lead him on? As far as I was concerned, the NIH could take their money and they knew what they could do with it. I didn't expect the letter to go any further, but I got a letter back from Dr. Daft, attempting to explain the situation. I guess I probably thought he wasn't being quite honest about it.

At any rate, I had occasion to come East very shortly thereafter, and I stopped off at the NIH. They really rolled out the red rug here. Dr. Daft had circulated this letter to various individuals. At that time, Ken Endicott had just taken on the scientific directorate of the Division of Research Grants, and so Floyd introduced me to Ken, and Ken spent about two hours going over with me in detail the way study sections act and councils act and the budgetary situation, and did such a good job at this, that I was convinced that this was a straightforward procedure, it was not devious, and it was the only way they could handle things under the circumstances. I was very greatly impressed by getting this information about NIH, and even at that time, I suggested to Dr. Daft that I might be interested some day in coming to the

NIH. I was thinking then in terms of carrying on research and not of administration. At that time, also, the clinical center was just being started in terms of the construction, and I saw plans for this, and thought this would be a nice place to have a lab.

So I went back to California. I don't remember the timing on this. You'd have to look at old papers. But not very long thereafter, several months perhaps, I got a letter from Dr. Daft, indicating that a new institute had been formed. Yes, I remember when it was. It was in the fall of 1950, because the omnibus bill was passed by Congress, I believe, in September or October 1950, and this included the Arthritis Institute and NIAID, which was then the microbiology institute, neurology institute, and perhaps one other. At any rate, Floyd indicated that this new institute was being formed, that they needed administrative staff and particularly they needed someone to head up the extramural programs. In this letter, he briefs out what an extramural program job would be like. This referred me back to my discussion with Endicott, who had done such a good job of explaining the whole operation.

Although I wasn't interested at that time, it sowed the seed, and the more I got thinking about it as time went on, the more I thought I might possibly be interested in doing something like this. I was getting mixed up more and more into administration at the Children's. I'd taken on the job of Chairman of the Research Committee. More funds were being made available for research there. There was construction of a new wing being contemplated, the laboratories were being renovated again, and I found that I was spending practically no time at the university and a good share of my time at the Children's, immersed in committee work of one kind or another.

So the time came when I wrote back to Floyd and asked him more details about this. By that time, they had appointed a director of the new institute, and that was Dr. Russell Wilder, who had retired from the Mayo Clinic as head of their Department of Internal Medicine. Dr. Wilder was 65. He was distinguished for his interest in metabolism and nutrition and also his career as a successful practitioner of internal medicine, head of that department at the Mayo.

So I came to Bethesda and discussed this with Dr. Wilder and Dr. Daft, went home and discussed it with the family, decided I would make the break. This was a little unusual for someone to leave California after nine years. The common saying is that when you go to California, after you've been there for a year or two, you're stuck and never come back East. I thought that I could probably be more productive, and there was more opportunity here in a new operation that was just starting, with no particular tradition in terms, at least, of extramural grant support than there was staying at home taking care of the store in Los Angeles.

I, of course, wasn't aware of the various personnel policies at the government or in the NIH. Dr. Wilder and Dr. Daft recommended that I come into the commissioned corps, the Public Health Service, and explained some differences between the setup in the commissioned corps and civil service. At that time, the commissioned corps seemed to be the more attractive route to follow. Among other things, the retirement was non-contributory, as there were the privileges—at least they looked big then—in terms of actually implementing them, the purchasing of groceries at commissaries and other things at PXs. They're not as glamorous as they seem, nor are they as helpful today as they were, I'm sure, during the war. This was very shortly after the war was over. Then there was distinct savings that could be made. Over the years here, it hasn't been particularly effective in reducing one's overall expenses. Some of this is due to the time it takes to get to one of the good commissaries and the nuisance, particularly, when one and one's wife are both working. At any rate, I decided to come into the commissioned corps. Of course, no commitment could be made by the Arthritis Institute until I was actually appointed in the Corps. I had one hurdle that loomed pretty large for a while, and that was my history of tuberculosis, because the physical standards are very high in the Public Health Service. When I went to the Los Angeles Outpatient Department of the PHS, I had quite a discussion with the lady who was in charge of X-ray there. Fortunately, I had all of the chest X-rays which I had taken at Trudeau and the ones that I had taken at the Children's as a follow-up from the time I left Trudeau. This, plus a letter from Dr. Karshner, who was in charge of radiology at the best tuberculosis sanatorium in Southern California and also in charge of radiology at Children's Hospital, convinced the Public Health Service that my tuberculosis was healed and under control, so I did pass the physical examination.

One other of the interesting features about making a move like this is that because of the delay in getting one's appointment and the fact that one owns real estate 2,400 miles away, one is hesitant about putting one's real estate up for sale before the written word finally comes through. I took that chance because I did trust Drs. Wilder and Daft.

Finally, about three or four weeks before I was supposed to start here, we were able to sell the house, and about that time, I got information that I had been commissioned, and orders to come to Bethesda and report. I was sworn in on July 2nd, and I didn't report until later in July because we drove back across the country.

In June, preceding before I had a commission, however, the Arthritis Institute brought me back to the second meeting of the Arthritis and Metabolic Diseases' National Advisory Council, and I was very greatly impressed by the way things were handled at that Council meeting. Dr. Wilder was in England at that time, and Dr. Henry Sebrell, who was the Director of NIH, chaired the meeting. Dr. Scheele, who was then Surgeon General, showed up and discussed the legislative problems of the time, very much like they were done many years later, although of more recent years, the Surgeon General appears quite infrequently at Council meetings. But it used to be the custom for the Surgeon General to attend and start to chair every Council meeting. Both Scheele and Burney did this very religiously. Very rarely did they ever miss any of the Council meetings with which I was connected. I might add for the record that Dr. Terry has not attended a meeting of the Heart Council since I've been Director of the Heart Institute. I'm sure that much of this is due to pressure of his own job, which is much greater than the pressures that the Surgeon Generals used to be under. At least it looks that way at this time.

We moved to Bethesda, and I started to work in the latter part of July 1951. At that time, the Arthritis Institute had received or was in the process of receiving its first appropriation, and in this was one million, three hundred and some thousand dollars for research grants—1,340,000, I believe.

When I arrived here, I was a little disappointed to find out that my office would not only not be in the clinical center, but also would not be near the Director of the Institute, but in a ramshackle building which was known as T6, which was recently torn down to give way to Building 31 and the parking area. My first office was on the northwest corner and the first floor of Building T6, facing the row of maple trees. It consisted of two rooms. A Mrs. Bernice Storrer had already been assigned to the extramural programs as sort of a grants assistant and "girl Friday." I had no secretary, and Mrs. Storrer performed these functions for a while until I was able to get the first secretary.

One of the first things that happened when I came here was the delineation of the programs which the Arthritis and Metabolic Diseases Institute would attempt to pursue. About this time, Ernest Allen, who was Chief of the Division of Research Grants, and Ken Endicott, who was Scientific Director, were beginning to get together their thoughts about the formation of a "bible of referral," which was to serve as a guideline as to which types of scientific disciplines and studies should be assigned to the various institutes, the institutes having increased in number quite recently. There were then obvious gray areas where certain projects might be assigned to one or another institute. I remember so distinctly that I had not been here more than a week or ten days, when Dr. Marshall Ellis, who was an officer in the Division of Research Grants, whose function it was to assign applications to study sections in institutes, came around to talk to me about the assignment of two or three research grant applications to the Arthritis Institute. Well, I knew nothing about the guidelines or the rules. I couldn't be very helpful to him, but it impressed me by the fact that we were going to have to carve out our own territory in this new institute, and to set up and describe those functions which we would propose to support.

I got together with Dr. Wilder on this. At this time, Dr. Daft had about the same position as we now call the Director of Research in an institute. I think he was called, actually, Assistant Director for Research. So my communication with Dr. Daft had nothing to do with my job. I was working under Dr. Wilder, as was he, and it was not until later when he became Director of the Institute that we were thrown closely together in the total adventure. Dr. Wilder was interested in his own hobbies, namely, starting some intramural research in the clinical center, and he gave me carte blanche to proceed with outlining the scientific areas which the new institute would support from the extramural standpoint. In doing this, I tried to define metabolism in the very broadest sense, both from the standpoint of basic research and also from the standpoint of clinical investigation. As a result, we were able to take on a large number of clinical activities that had been neglected areas, not only the rheumatism field, but in the general area of clinical endocrinology and metabolism. Diabetes was one of the first diseases to which we addressed ourselves. Then later on, we recognized the poor condition of gastroenterology research in the United States, the metabolic aspects of hematology, and others which are well known in the current guidelines of the Arthritis and Metabolic Diseases Institute.

So the current "bible of referral," which has been updated a few times since those days, in essence, very closely resembles the first guidelines that I got together and Dr. Wilder okayed at the beginning of the Institute. There was no such thing in those days of having to go through a whole chain of command below and above the Institute director to get things started. It's not quite that way anymore.

About this time in the fall of 1951, it became apparent that I was going to have to make closer liaison with medical schools in order to be able to have a better insight into the research potentials and their interests. So I started on a series of rather protracted trips over the country to visit not only departments of internal medicine, but some grantees who already had grants from the Institute. I learned very early a lesson, that in this day and age of airplane travel, that it's better to take several shorter trips than to try to combine everything into one long trip, making one-night stands or one-day stands at various medical schools. This can be a very tiring procedure. In fact, I frequently came back from one of these trips completely exhausted from not only the time difference, but from the sheer—well, I'll get off that line, but it's not easy to take one-day stands and cover everything one wants to cover in a given medical school and see the people that you want to see. This was, of course, because of poor scheduling, because of inexperience, and we learned from this. At that time, too, it was possible to attend all study section meetings, and for a while, I suppose the first year while I was at NIH, I attended every study section meeting and every National Advisory Council meeting, including those of the other institutes except when there were conflicting dates. Then one would make a choice as to which study section and which council to attend. I finally got a secretary. This was perhaps within a month or two after I arrived. For a period of four or five years, I was the only professional person in the extramural programs of the Arthritis Institute. I'd have to look over the budget build-up to see to what extent we grew in that period of time, and it was somewhere from around a million and a half to $7 million or $8 million during that period. It was slow growth compared to the more recent advances.

We had quite a time after the decision had been made to start a traineeship program in breaking this loose. Finally, however, after a year or two—and again, I would have to refer to the budget sheets to check this—we received an appropriation of $50,000 to start a traineeship program. In the interim, I had been studying traineeship programs all around the existing and other institutes, and when we received our appropriation, I tried to sit down and write the regulations for the Arthritis and Metabolic Diseases traineeship program to include what I thought were the good features of the other institutes' programs and eliminate what I thought were the undesirable features.

It's an interesting commentary to note that one person wrote out the regulations and within 20 minutes after submitting them to Dr. Van Slyke, who was then the associate—I may be wrong on this. It may have been Dave Price to whom I submitted them. But whoever was sitting as associate director of NIH at that time okayed the regulations for the traineeship program, and that was that. There was no further clearance necessary. Dr. Wilder had seen them and discussed them with me, but he was, again, still interested in his own hobbies, and I was continuing to have carte blanche as far as he was concerned. This was one of the delightful experiences that I had during the ten years I worked in that institute, that I was fortunate in that my liaison with the director was such that we had what I would say was a beautiful, easy-going relationship, and it was never necessary for me to try to sell Floyd Daft something or for him to try to sell me something, because we understood each other so very well indeed. We never argued or fought about any particular programs or about any particular issues, and I was extremely fortunate in that he did give me, insofar as he could go under the guidelines under which he had to operate and which I appreciated, which I have appreciated more since I've come into the Heart Institute, we had complete rapport.

The time came when it became perfectly apparent that we were going to need more help in professional aspects of the extramural programs of the Arthritis Institute. I started interviewing people. I forget how many individuals I interviewed. This was somewhere in the 1955 or '56 period. I wasn't satisfied with the people that I had talked to. In the meantime, there was more and more pressure being put upon me to get somebody, not only from the increase in my own necessary activities, but from the fact that now we were running a fairly good-sized program and needed more professional help. This was recognized by the Director of NIH, who was Dr. Sebrell, and it was suggested that there would be no reluctance on the part of Building One to okay more personnel for the extramural operation.

Finally, a young man who at that time was doing some research in the microbiology institute got the word, and I think he got the word through Ed Rall, who is now Deputy Associate Director for Extramural in the Arthritis Institute. Ed Rall at that time was the Executive Secretary of the Microbiology Study Section. Ed alerted me there was this young man who was working with Bob Coatney in the malaria project, who was a pharmacologist who graduated from Yale. The time came when John Sherman came down to see me about the possibilities of coming to work in the Arthritis Institute's extramural programs. I was duly impressed by the way Dr. Sherman handled himself. I was further impressed by the depth with which he went into the whole situation and its possibilities, and I was even more impressed by his past record, not only his military record, but his record in New Haven and at the NIH. So the happy day came when he signed on as an assistant in the extramural program, and his career since that time is well known. You will, I'm sure, if you haven't already done so, have this on the record from Dr. Sherman himself. That was the start of an escalation of the staffing of the extramural programs which continued, and I assume is now continuing up to the time I left the Arthritis Institute.

To go back for a moment in terms of programs that were developed in sequence, the research grants program was the first, as I've indicated. The traineeship program was the second, and then the training grants program came along a year or two after the traineeship programs were instituted. I was called to Dr. Sebrell's office one day to discuss the extramural programs with him. Dr. Sebrell took an active interest in everything that was going on at the NIH. At that time it was small enough, 1 guess, for him to do this. On the other hand, he did not intrude in the operation, but he would pass on suggestions from time to time. On this particular occasion, he suggested that it might be interesting to look into the rehabilitation field. I remember so well he said, "There's gold in them thar hills." Just about that time, Howard Rusk had given a talk at the Surgeon General's staff meeting which I attended, and I was greatly impressed by Dr. Rusk's arithmetical approach to the rehabilitation problem. Also in those days, I think almost everybody who wanted to attend could attend the Surgeon General's staff meetings which were held, I believe, every two weeks. At the present time, I don't know the guidelines, but I think that Dr. Sessoms is the only person that attends the Surgeon General staff meetings at NIH. There was a time later when it was limited to institute directors, but because of the great increase in staff of the Public Health Service as a whole, I believe that it's limited to just the one representative from the NIH. If it's not limited, only one person goes, usually.

After hearing Howard Rusk and talking with Dr. Sebrell, I took a trip up to New York and spent a morning with Dr. Rusk. He was very enthusiastic about the possibility of the Arthritis Institute getting interested in the field of rehabilitation. It was possible for us to do this at that time because the internal NIH guidelines had not yet been set down that our training programs were to be strictly oriented toward training for research. In the Omnibus Act and also in the Hart Act, it very specifically spelled out that traineeships are "for the training of young physicians in the diagnosis, care, and prevention of patients with arthritis and other metabolic diseases or of heart disease and so forth," which gives one a broad approach to the total categorical disease problem. Later on, a local ruling precluded activities in these areas, with the exception of the Mental Health Institute and certain activities of the Neurology Institute. So sooner or later, the other institutes got out of the clinical training business. It's interesting to note that as of this moment, it looks as though our supplementary appropriation for fiscal year 1966 will come through with $3 million for the National Heart Institute for clinical training. So this is picking up something that the Heart Institute has supported years ago, along with other institutes.

To go back to Howard Rusk, it seemed to be reasonable to support certain trainees in the field of rehabilitation if they were engaged in or spent at least a fraction of their time in rehabilitating arthritics. So I believe all of the first group of trainees that were awarded with the $50,000 that we had then in the first year of our traineeship program were in the field of rehabilitation. We also explored the possibility of training ancillary personnel for rehabilitation centers.

I will tell this story as I saw it, and I am not sure of the facts, so it's pieced together with hearsay, discussions I had with various contemporaries and other people, and the facts will have to be brought out by a more accurate evaluation of the story. But it is how the Arthritis Institute got out of the rehabilitation business. At that time, Fred Stone was my counterpart in the Neurology Institute. Obviously and for good reason, neurology was interested in rehabilitation. Also at that time, Dr. Van Slyke was Director of the Heart Institute. The legend goes that the Heart Institute had something like $300,000 that they were going to have left over that year and were exploring ways and means of "spending it wisely." At any rate, there had been some ground work laid for this, and one afternoon I was invited to a meeting at Dr. Van Slyke's home, which is in the building here on the reservation that Dr. Mider now occupies. At this meeting were Dr. Rusk, Dr. Van Slyke, Dr. Pierce Bailey, who was Director of the Neurology Institute, I think Dr. Fred Stone was there, and Dr. Sebrell. The plans were laid at that meeting, and this I know about, but I don't know the details that took place before the meeting, to turn over the $300,000 that the Heart Institute had to the Neurology Institute to start a comprehensive traineeship program in the field of rehabilitation, with special training stipends which were above the training stipend level which was then available to the other institutes.

Presumably, Dr. Sebrell had not heard of this before, and I think it's only fair to state that he seemed like a pretty unhappy man at this meeting, and he left before the meeting was over. The scuttlebutt has it that this was actually so said and sealed beforehand that Dr. Sebrell really had no recourse to object to this. At any rate, this is how the Arthritis Institute got out of the rehabilitation business, because the stipends were such that according to our regulations, we could not match them. Special regulations were written for this very special program, and we no longer dabbled in training in rehabilitation, although for a number of years we did support some research in rehabilitation of arthritics, which was done in Howard Rusk's department by Dr. Lowman.

One other note about Dr. Sebrell, again, the NIH was so small that the scientists on the campus could get together, and there used to be a weekly NIH staff meeting which was held in Top Cottage, which was just about under where I'm now sitting. Intramural scientists, both clinical and basic, and extramural staff and administrative staff would attend these staff meetings, and it was the custom to introduce new staff members at the NIH at those meetings, and also to hear a scientific paper or a progress report on the development of extramural activities. It was a very good means of communication. It obviously outgrew itself and became too ponderous to handle in a few years, but through this method we kept pretty well informed about what was going on at the NIH during that period. I would suspect that this ended about 1955. I'm fairly certain that it ended before Dr. Sebrell left the NIH.

T-6 was an interesting place, too, because it was possible to not only house the extramural staff of all of the institutes, but there were certain institute directors who had their offices there. Among these were Dr. Van Slyke, Director of the Heart Institute, and later Dr. Watt, Dr. Bob Felix, who was Director of the Mental Health Institute, Dr. Pearce Bailey, who was Director of the Neurology Institute. The extramural staffs of the institutes, as well as the staff of the Division of Research Grants, including all of the executive secretaries, were so small at that point that we could all get together for staff meetings in the one big meeting room that was there in T-6. I don't know how many hundred people now occupy such positions that would have made them eligible to attend the staff meetings we had in those days, but I would say that there were probably not more than 30 or 40 individuals *in toto* who had to do with the total extramural programs at that time. This, again, was good and was healthy, because the executive secretaries of the study sections and the grants branch chiefs of the institutes could get together and discuss mutual problems and mutual interests in policy and be very helpful in formulating policy.

This type of communication was superseded by the ECEA, Executive Committee for Extramural Affairs. I sat on this for many years. Admittance to the ECEA was only for grants branch chiefs. This was one step which took the grants branches of the institutes further away from the Division of Research Grants, and I'm not in the position now to know how much communication there is along these lines. The extramural activities are all in the

Westwood Building—not all, but the primary extramural activities of the categorical institutes are in the Westwood Building, along with, I think, most of the Division of Research Grants. But I'm fairly certain that all executive secretaries and all grants people can't get together anymore in a meeting that could be effective, and if they had such a meeting, it would have to be in the auditorium of the clinical center, I should think. So that communication has necessarily become a problem because of sheer increase in the size of the activity.

However, we had very close liaison with the executive secretaries and there were very few study sections, actually. There were probably not more than six or seven. Again, we'd have to go back to the record to look at this, but among some of the outstanding executive secretaries were Barney Brunstetter, who was the Executive Secretary of the Pathology Study Section. Dr. Endicott and myself both being pathologists, used to needle Barney Brunstetter, who was not a pathologist, in his dedication to doing something about stimulating research in pathology. I suppose if any one person deserves the credit for the upsurge in pathological research, it was due to Dr. Brunstetter' s activities and his dedication and hard work at this. He, unfortunately, was killed in an airplane accident in Albany, New York, in the mid-1950s. It was one of a rash of people from NIH who were killed that year in airplane accidents.

Irv Fuhr, who is still an executive secretary, was among those present. Don Larson was an executive secretary even before I came here and still has one of the biochemistry study sections. Marshall Ellis, who also assigned research grants, was Executive Secretary of the Surgery Study Section.

As the Arthritis Institute developed and got more interested in clinical investigation, I had the feeling that clinical research grant applications were not being handled properly. At that time, they were sent to the study section which had the greatest degree of sophistication in the basic problems which were spelled out in the application. There were at least two study sections which were clinically oriented. One of these was the cardiovascular study section, with which the Arthritis Institute had no contact. Another was the Surgery Study Section, with which the Institute had a little contact in some of the orthopedic surgery applications that wer submitted to the NIH, and there were very few of these indeed. But the applications along the lines of clinical investigation from departments of internal medicine that were within the realm of interest of the Arthritis Institute were sent to such study sections as metabolism and nutrition, endocrinology, sometimes even to more highly scientifically oriented study sections like physiology and pharmacology.

The percentage of approvals of these applications was, we felt, extremely low compared with the percentage of approvals of applications in the real fields of interest of the members of the study sections themselves, the more basic study sections. Dr. Daft and I had many discussions about this, and we finally approached Ernest Allen and Ken Endicott with a proposition to form a new study section which would address itself to a clinical investigation in those areas which were not then included in clinical investigation. This was agreed upon, and we set to pick members of this study section. I submitted a list to the Division of Research Grants. This was circulated to some of the other institutes for their additions or deletions, but as I remember it, a great majority of the members in the original General Medicine Study Section were names that had been submitted from the Arthritis Institute. These were individuals who were distinguished themselves in clinical investigation and in various areas.

The guidelines for assignment of research grants were amended to admit this new study section, and I believe that Dr. [Clifton] Himmelsbach was the first executive secretary. If he was not the first, the first lasted for but a short time, and Dr. Himmelsbach took over that position until he subsequently moved to the clinical center into his present position.

It's of interest to note that we thought that we might get more sympathetic understanding from people who themselves were engaged in clinical investigation, but after a few meetings of this study section, on an analysis and comparing the actions of the new study section with the old method of operation, which was to distribute the grant applications to a series of other study sections, we found no significant difference in the approval and disapproval rates. So that from the standpoint of trying to push clinical investigation and perhaps get more applications approved than had been approved previously, this study section was a failure.

Although from the standpoint of taking the load off some of the other study sections and concentrating clinical investigation in one study section, it was a success, and I think it's worked very well ever since. I think this goes to show that the axiom that we've talked about here for a long time is probably true, and that is that the more an individual knows about a subject, the more critical he's inclined to be about his own subject. Therefore, the less likely an application of somewhat less than high quality has a chance of being approved.

We did an interesting rundown along these lines one time in the General Medicine Study Section, and it was composed of roughly three individuals in the field of rheumatism, three individuals in the field of metabolism, three individuals in the field of endocrinology and diabetes, chiefly diabetes, and some others. In studying individual priorities that were given individual applications, we found that the people who knew least about the field of arthritis, for example, were the people who would give the more liberal priority scores to the arthritis applications, and on the other hand, the arthritis experts on the panel would give the most critical priority ratings which were usually lower than those people who were not experts in the field.

I don't know if I said that properly the first time or not, but I'll try to clarify it. Those people who were not experts in the field of arthritis would give a higher priority score—that is, a better rating—to an application. The people who were experts in the field of arthritis would give a poorer rating to the same application.

I've been talking about some of the events in the early days of the extramural programs. Some small items that indicate how times have changed come to mind. I remember, for example, how difficult in the early days it was to get through a requisition for an electric typewriter. I also remember the paucity of duplicating equipment in terms of our modern means of duplication. There was a time when there were but two or three duplicating machines at the National Institutes of Health. I think these were in Building One, the clinical center, and they were Thermafax machines. I became aware of the need for duplication other than mimeographing fairly early when it was necessary to have copies of applications and correspondence to distribute more widely than one could do by the ordinary stencil, and looked into the types of equipment that were available at that time, finally settled on the Eastman-Kodak Verifax. This was a real chore to process this equipment. I forget now all the red tape concerned, but we finally got it.

It was interesting to note that this was the first duplicating device of any type other than mimeograph or ditto in any of the extramural institute setups, and it was requisitioned by Building One during the polio problem, when it was necessary to copy letters that had signatures in blue ink, because the Thermafax would not take off blue ink. If you compare that to now, a somewhat primitive device and the only one of its kind then, to the numbers of duplicating machines that are here now, it simply points up how really primitive we were at that time.

Another thing that became apparent very early in the development of the extramural programs was a need for some type of substantive program analysis to be able to not only justify budgets, but to have an account of progress in research in the given area. As a matter of fact, when John Sherman came into the extramural programs of the Arthritis Institute, his biggest responsibility, as I put it to him, was to analyze in a substantive manner all of the research programs that were being supported by the Institute, to start an analysis and some technique of data storage and retrieval, so that we would be able to spell out precisely the development in any given scientific area that we supported. Unfortunately, the press of other matters during that period precluded Dr. Sherman from ever getting into this, and up to this point, I doubt very much if any of the institutes have this type of an analytical situation under control, although we've made many efforts to set up systems whereby we could keep a running analysis and a retrospective analysis of research advances that have been made through our support.

Along this line of program analysis and substantive matter, as the programs grew, it also became necessary to have more sophisticated and accurate fiscal analyses available at the drop of a hat. At that time, my wife was Clinical Director of the Miners Memorial Hospital Association, and I used to hear her tell about the Univac setup they had for the coordination of the data in the ten hospitals which they ran. I became more and more convinced that the only answer to both the substantive and fiscal analyses of programs was in a modern computer setup.

I had developed an analytical scheme for keeping tab of the programs in the Arthritis Institute, which was composed of McBee cards, with the punch needle system. This worked pretty well because you could get all the data about one research grant that seemed to be necessary at that time, including fiscal and substantive information about the grantee and the institution on one of these cards. But the time came when the program grew to the point where the cards toppled over on their own weight. You just couldn't do a complete analysis because the program was so large, that it took time and also took a lot of time to punch these things, and they became quite ineffective.

So I proposed for the extramural programs of the Arthritis Institute a more modern setup, namely a data storage and retrieval system on something using some equipment as the Univac or IBM equipment. From the time that this was first proposed until we finally got it, about three or four years elapsed. This developed into a running controversy between the various individuals who were concerned with overall NIH program analysis and budget office, and the recognition that something like this was actually needed and would be important for an institute to have within itself. It made perfectly good sense to me that any device like this in an institute should be compatible with a centralized device, but the bedrock of data at that time was in the intimate operations of the institute extramural area, its own budget office and its own programming activities. This was the raw data on which any centralized device would have to feed.

So there were a whole series of meetings and frustrations associated with the procurement of equipment for this purpose. It was vetoed up and down the line. In fact, for a while, Dr. Daft himself was not quite sold on the idea. This in no way negates what I indicated earlier about our close working relationship, but simply a hard-headed look by him at our problem and also evaluating the chances that we might have from a realistic standpoint in being able to get this.

The way in which it was finally worked out was that we managed to write a contract with a firm, the name has slipped me now, whose business it was to give advice for business organization. So they came in and did a study not only of our problem, but of our problem as it related to the Division of Research Grants, to other institutes, and to the NIH as a whole.

They came up with a report not only on the reorganization of the extramural program of the Institute, but also a suggestion and recommendation as to our data storage and retrieval mechanisms.

This culminated in a meeting in the conference room of Building One one day, in which all of the experts in this field who were then at the NIH met with us and representatives of this company. It was decided that we could go ahead and purchase the equipment that was needed. This was the first institute that went into its own modernized data storage and retrieval system.

When I came into the Heart Institute four years ago, even then the Heart Institute was depending on McBee cards, and it didn't take us very long in the Heart Institute to get set up on the IBM scheme. This has worked very well indeed in the Heart Institute, I assume it has in the Arthritis Institute, but we've been very fortunate here to have in charge of this Mrs. Janet Welsh, who was trained with Documentation, Inc. With her background and training in this very field, the Heart Institute slid into this very rapidly and readily, and it's, of course, been extremely helpful.

I'm sure that this will integrate with the new computer setup that we heard about a few weeks ago that's being set up here at the NIH, and indeed, I think that the NIH has come around to realizing that at the operations level is the only source for raw data. This is a day-to-day operation, it's never the same fiscally from day to day because of negotiation of sums of money involved in research grants, where the Council sets the ceiling level for the grant, but the actual amount of money that's awarded depends upon negotiation between the staff and the institution. This is extremely important in dealing with very large grants. The Heart Institute, for example, has at least one grant that's over $1 million a year, but it's very difficult to estimate from one quarter to the next as to how much money is actually going to be needed and used by the grantee. So this brings into play very large sums of money that are in continual fluctuation. So that one really doesn't know where they stand until about a month or two before the end of the fiscal year, and the only way it can be done is through computer operation. At least that's the way it seemed to me, and I think that this is generally recognized now in a program that's as large as the total NIH programs.

End of interview