AN INTERVIEW WITH DR. MURRAY GOLDSTEIN

BY STEPHEN P. STRICKLAND, PH.D.

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Introduction and Biographical Sketch

This interview with Dr. Murray Goldstein 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. Goldstein, 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.

Murray Goldstein, D.O., first came to the National Institues of Health in 1953 as Assistant to the Chief of the Grants and Training branch of the National Heart Institute. Shortly thereafter he became Assistant Chief of that branch and for the next 3Ø years served in an increasingly important variety of positions, culminating in his being appointed Director of the National Institute of Neurological and Communicative Disorders and Stroke in 1982. Dr. Goldstein's perspective on the extramural programs of NIH, their internal workings and their impact on science and on health-related institutions is a multi-faceted one, informed by his immersion, during particular periods, in special fields such as cerebralvascular diseases, epidemiology, viral diseases, stroke and trauma, and neurological disease generally, as well as by his administrative roles. Of additional value for purposes of the current study, Dr. Goldstein has, at different periods, spent time outside NIH, including service with the California State Department of Public Health in 1958 and as Visiting Scientist at the Mayo Clinic in 1967-68. His full curriculum vitae and biography is included at the end of this interview.

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WASHINGTON, D.C.

Interview by Stephen Strickland with Dr. Murray Goldstein

Tuesday, June 3, 1986

SS: I am talking today with Dr. Murray Goldstein, director of the National Institute of Neurological Diseases and Communicative Disorders.

MG: Yes, it used to be the National Institute of Neurological Diseases and Blindness; the Eye portion became an Institute. Then we became the Natonal Institute of Neurological Diseases, then Neurological Diseases and Stroke, and now the name, which is about ten years old, includes Communicative Disorders.

SS: And it is one of the older Institutes.

MG: Yes. We were organized in 1953.

SS: That is the point at which things really started growing.

MG: Exactly. The Omnibus Bill went through at that time, which really expanded the broad base of NIH.

I have been with NIH since 1953. I started in the Heart Institute in its grant programs. I have been at the Neurology Institute since 1960. I was acting director for close to two years, and I have been director for about two years. So essentially, I've been the director for about four years.

SS: The things that are happening in Neurological Diseases are terribly important. This is a very timely interview in many respects, one of which is simply the increased attention you are going to get after the Washington Post article yesterday on the research on Parkinsonism, using live animals. I am in favor of using any means necessary to move progress forward. My mother developed Parkinsonism very late, at the age of eighty. It had a rapid and awful effect, so in the last couple of years I have been very interested in that. Could we talk about Parkinsonism? How are we doing, in your perspective?

MG: Parkinsonism has gone through several cycles of research. Not too many years ago there was a great deal of attention to the cause of Parkinsonism. There was a great deal of Parkinsonism as we know it as a result of the 1917 flu epidemic. It was believed by some very knowledgeable people that the Parkinsonism that we were seeing was a result of that virus infection, flu infection, and that the disease would disappear. They thought maybe it had been a one-time shot, and that the falling numbers of cases was a symptom of the fact that it would eventually disappear. Of course, those people admit quite openly now that they were clearly wrong; a virus infection of a specific part of the brain did cause the Parkinsonism syndrome, and therefore the tremendous increase in the number of cases, but that was not the only cause. We kind of leveled off, struggling very hard to understand what it was about. The next really big breakthrough occured in the early '60s when neurochemistry was essentially new, and the field came alive, built around neurotransmitters.

The idea, which had implications in many areas of brain research, was that the brain was the largest chemical factory in the body. We had always thought of it before then as only a switchboard of electrical signals. This was a common conceptualization, but we realized in the early '60s that the switchboard/electrical idea was a fine one to explain a number of phenomena, but that it didn't really explain many of the others.

The other explanation was a new concept -- with additional implications -- that brain cells could produce chemicals, many of which we call neurotransmitters, mainly chemicals produced by one nerve cell to send a message to the next nerve cell, or to send a message to an organ, or a muscle. In the early '60s the world of neurotransmitters was being born and explored. It was then realized that this area of the brain that was involved in Parkinsonism was producing a neurotransmitter, called dopamine, that was the key to the control of movement. It was a deficiency of that neurotransmitter that caused the symptoms. Then the issue went from basic science to the clinical arena to try to see if we could replace dopamine. After a number of very false starts, and a long history of frustrations, the chemical L-dopa was discovered which, when given orally in graduated doses, did in fact replace dopamine in the brain. Fortunately people who had been in bed for five years and had to be turned because they couldn't move the right muscles, were then able to drive automobiles and return to normal life. So this was a breakthrough which taught us a great deal, both about the basic science of the brain, and how to take care of the disease.

A number of things then began to occur, and after awhile we realized that people were having strange reaction, which we referred to as "on-off". They would be going along fine on L-dopa, and all of a sudden became resistant to it. The issue then was: what was happening? The "off" was practically instantaneous. Somebody would be sitting and talking to you, and all of a sudden would develop the symptoms again, as if somebody had turned a light off. We began to explore what was happening and realized that dopamine alone was an over-simplification; it was a main neurotransmitter, but not the only one. Secondly, like with insulin, we were treating the symptoms of the disease, but perhaps not the disease itself; the disease was progressing although we were pharmacologically replacing some of the lost function.

SS: How would you define Parkinsonism?

MG: It's a degeneration of a specific area of the brain, the substantea negra, which produces the neurotranmitter dopamine. For the neurotransmitter, which is produced by one cell to act on another cell, you've got to have the first cell to produce it; but the second cell must have a very specialized area to react to the neurotransmitter, which we call a receptor. Parkinson's Disease is a deficiency in neurotransmitters or receptors, and often both.

SS: I see.

MG: We have invested a very large portion of our funds in basic neuroscience research but depending on serendipity — luck of the dice -- is no longer the best strategy. It's a strategy we have to be aware of; one which takes advantage of a chance finding and plugs into a strategy utilizing available information. Finding a patient out there who took a street drug was pure luck, and served as the key to the MPTP story. But the hard fact of the matter is,

the kinds of problems we're addressing require a thoughtful progression. Even in the development of drugs, (we now call it "intelligent pharmacology") by knowing what the chemistry of the cell is, we can begin to perfect drugs that have the specificity to intervene with the chemistry of the cell in a specific place -- rather than just trying every drug in sight to see if anything will help.

SS: It also sounds like there is a much more coherent interdisciplinary approach to things. You were just describing how you already know how to do the implantation; what you don't know yet is what kind of implantation you need.

MG: Exactly, and how to control it to work as you want it to. So this is why, in our area, we call it neuroscience rather than cell biology because these people are a little bit of everything, working as a team rather than as individual investigators in a laboratory looking in a microscope. That isn't enough anymore. It is a whole new way of looking at the brain and the nervous system. If I had fallen asleep twenty years ago and woke up today, I wouldn't know what they were talking about. I'd be a Rip Van Winkle absolutely lost. It is a very rapidly progressing field. Just understanding how it works gives us the lead to understand why it doesn't work. These researchers are in very close contact with each other, growing at amazing speeds. They way I like to put it: we're just beginning to put the mind back into the brain. We are getting away from philosophical approaches and looking at problems in terms of the chemistry and biology of the system.

SS: It's thrilling to hear about this. I know enough about the Institution and the biomedical science network to know that this is the end of a long period.

MG: We are in biological and biomedical research where, I think renaissance man was at the time with art and mathematics, where all of a sudden whole new vistas approached in art and the use of mathematics and people began to perceive things that they hadn't thought about earlier. We're at that kind of renaissance in biology and biomedicine, where methodologies and whole approaches are now available to us that we just couldn't have imagined just a few years ago. The vistas that have opened up with genetic engineering, with neurochemistry, with understanding how these mechanisms work, instead of just philosophizing about them, have made them achievable goals.

SS: The emergence of cell viruses is an important challenge to various fields of medicine, as in connection with Parkinsonism. Are the processes for sharing information across institutional and disciplinary lines adequate?

MG: Yes. There is one kind of problem, and that is that knowledge is exploding on all fronts so rapidly that it is extremely difficult to keep up with all the new information. So we have a tendency to focus more and more, while the main problem is to make certain that you don't miss something that's going on in a related field. This is why so many scientists are spending so much time at meetings, because it gives them the opportunity to hear the other guys and to hear what's going on in a related field. Fundamentally scientists are curious about each others' fields, and in an environment like this, our intramural scientists can forget what Institute they're in, and that's just fine. They're interested in the methodology someone else is using. They ask

for help from them and then they help the next guy. So, I feel that as a research administrator, my most important job is to stay out of their way, and not try to force them into some kind of compartmentalization of knowledge. And it's working.

Could it work better? These people are knowledgeble about what is going on. They have developed what we call "the invisible colleges"; they seek each other out in meetings; they know what's going on in each others' laboratories, and they communicate. If we ever attempted to formalize it, we would probably set it back.

SS: I'm now curious about time, process, and who it is who has an opportunity to know. You can see a wider landscape than somebody working in a lab every day, even if that person goes to two or three meetings a month. Somebody like directors of Institutes may have a broader and clearer view of lots of different things going on than the scientists themselves.

MG: Yes, but it's the overview that we have. I think we're more of a turn-off force than a turn-on force. I don't think it takes too much work to begin to realize that one group of people are going nowhere. They're defining and redefining and going to another decimal point, or two decimal points, and we begin to say, "Are you really contributing, or are you only refining?" And it's in those kinds of situations that I think both our intramural and extramural systems work, in which we begin to recognize when someone isn't going anywhere. We're not as able, on the other end, to say, "Hey, that's an exciting breakthrough." When it's there, you know it. Like with pornography; when you see it you know it, but it's very hard to define it! The same thing is true about productivity in research. You know it when you see it. You realize when it's not there. So I think of an Institute director's function as more of seeing to it that the areas of excitement are receiving the support they need, and the areas with a lack of excitement are beginning to be reexamined. I don't see an Institute director sitting on a citadel saying, "I want you to do this and I don't want you to do that."

SS: I meant what about your saying: intramural scientists and the grants we are supporting are doing very interesting things in whatever the particular area, using these techniques.

MG: I am reporting on what I have learned from them, so I spend a lot of my time in meetings, also. I don't think a director of an institute, like the director of the department of medicine at a university, can tell good scientists what to do. They know when they are <u>not</u> doing well.

I think my own role, primarily, is to see what are the areas of excitement and channelling resources into them. I don't create those areas; I can't create them. If I could, I'd work in a lab. So the best thing I can do, and be successful, is be astute enough to identify where it's going, and if it seems to be productive, to funnel resources into it. And, as I said, to identify those areas that appear to be in a stalemate, and influence those people to discontinue that line of research. That is what I see as my job as

a manager, rather than as a scientist, per se. the worst thing I think I could do is to be so opinionated that I think that is the answer when it is not the answer.

SS: You have prompted me when you were describing the remarkable knowledge we now have about electrical stimulation and chemical stimulation of particular brain cells and nerve centers to ask a question. Does anybody support significant research on that nexus between the psychological and physiological fields? Like, how does a placebo really work? What does it do to the brain that causes physiological responses?

MG: You're touching on an interesting issue. Placebos have gotten a bad name, but they really shouldn't. They work. They may not work for the reasons we think they do, but they work. You give a person a substance, about which everything we know would lead us to believe it should not, in any way, intervene in the process, and yet we see results. But, as we understand the nervous system, there are the sensory and motor cells. The brain takes the sensory input, whether it's vision, hearing, smell, chemical, or virus, and arranges the information so that an output occurs. It's a fairly simple logistic problem. The big question that we have never really understood, is how the "black box" works; how does it process the information to make a determination of what is the appropriate output? But the hard fact of the matter is, if you can manipulate that black box, you can get the desired output. You can manipulate it in what we think is the real way, by giving it the correct sensory input, but you can also give it incorrect sensory input, and "trick" it into an output. Why not?

This happens all the time in real life. I can stimulate you by calling you some name. Those are just words, just sounds. I haven't really hit you or threatened you, but your blood pressure goes up, and your heart begins to beat. You begin to sweat. That's false information at work. And it happens all the time. We are taking the "box", giving it false information, which it is interpreting, to a sensory output. This is the conditioned reflex with Pavlov did with the dogs. Placebos are the same kind of thing. Why should we be surprised that, because the patient believes that a medicine is going to do something, whether or not it is chemically active, he reacts to it? We've seen it in hypnotism, in yoga, in biofeedback; it's real. The problem is that we haven't learned how to use it well.

What we're trying to understand is the processing of sensory information. We know, as an example, that in something as bizarre as a disease, with proper input into the brain, the patient can effect their own nerve system, turning the immune system on to fight the disease. Under other conditions, he or she doesn't turn it on. This issue is, what is the brain/immune system connection? We are beginning to find that out, because what's beginning to happen is that certain neurotransmitters are released by the brain. We now need to know: why does the brain release the neurotransmitter to a false signal? What's the connection with the signal? We are working on that type of thing right now.

In clinical neurology, we have spent most of our time with the output system; controlling motion, controlling speech. We have put very little time into understanding the sendory input. As an example, what is pain? What is painful to you may not be painful to me. I have had teeth extracted without

novacaine. It hurts. I know what the dentist is doing, but I don't find it unbearable. How do I trick myself into saying, "I'm willing to bear that degree of pain," while another person has got to have novacaine or they'll faint? What we're beginning to study is the sensory input. Pain is a superb model. What is pain? When does a sensation which is pleasant then become painful? When does somebody tickling the bottom of your foot give you a tickle, and when is it really painful? We're working on that, and the concept of the placebo will come into that.

SS: I think that is enormously promising, don't you?

MG: Yes. How does the brain interpret its environment, and how can we intervene so that the brain will interpret it to our own benefit? It maybe that we may want it to interpret incorrectly. We may want the brain to turn on the immune system. If we can do it with stimulants, why not?

You've read in the press about people who have delayed dying of cancer because they've been influenced. I don't deny these stories. The brain does affect the immune system. And the immune system affects the disease. So why not? This is a whole area of research which is just beginning to get it together.

But pain is a hard thing to study, because if you're going to use animals, they're going to be put in pain, and you get back to the whole issue about under what conditions you can use the animals.

SS: What can you tell me about the creation of study sections and their early mandates?

MG: When the grant system was first evolving at NIH following World War II and the transfer of contracts and funds to the Public Health Service and then the NIH, the law had established the National Advisory Cancer Council to make decisions about cancer grants, and the National Advisory Health Council, to make decisions about everything else. The original thought was that the Council itself would have the technical expertise to make the scientific judgments, and make appropriate recommendation. So the authority was vested in the Councils. As I understand it, at the first few Council meetings, the Council was handling this. Then, with the sudden growth of the grant program, and the diversity of the kinds of applications that were being presented to the Council, particularly the Cancer Council, recognized that it did not have expertise in certain specific areas. They therefore asked that expert review committees be set up to advise it, the Cancer Council. The point I'm making is that the first scientific review panels that I am aware of were the Councils themselves. This was vested in the law.

SS: I have heard this.

MG: But the Council members began to get uncomfortable. Occasionally there were applications that no member of a Council could have an informed opinion about, or only one member knew something about, in which case the Council had to rely entirely upon that one. Nobody was comfortable with this. So, the study sections were formed, and they began in the context of the Cancer Institute. What I'm not sure of is that next step when we went from panels established by the Institutes to advise their councils, to DRG, which C.J. Van Slyke

was brought in to organize, to be in charge of the study sections. The next point I do know about is the early 195Øs when the study sections existed. They were much different than they are today in that not only did the study sections advise the Councils on scientifc merit, but there were also charged with taking steps themselves to have an overview of their fields, to present reports about the status of research in their fields.

SS: This is after the study sections were with the DRG?

MG: Yes. A Biophysics Study Section was asked to analyze the field, pointing out deficiencies in research, and to take steps to stimulate needed research in biophysics by holding research workshops and conferences and other ways. The study sections in those days continued to act in some respects as if they were "subcommittees" of Councils, with initiatives in their research areas.

SS: And who asked them to do this?

MG: The Division of Research Grants at NIH; Ernest Allen, Ken Endicott. The Study Sections then were program development organizations, moving ahead vigorously in each of their fields to help build a science at the same time that they were judging individual applications. So, the working relationships for program development between the Institutes and the DRG Study Sections was quite intimate. What we found was a scientific community interacting through the study sections with the Institutes. The rather sharp lines that have evolved in the recent years between the responsibilities of the Institutes for program development, and the responsibilities of the scientific review committees were very blurred. I must tell you in my opinion, rightfully so.

SS: Rightfully for that period of time?

MG: Yes, right for that time in that it was the so called "golden age" of the NIH; it was expanding rapidly, it was trying to learn from the scientific community what the best opportunities were, and how the NIH could usefully and legally "aid and abet" in this development. In that respect, it was total national effort. The bureaucracy, the lines of authority, were also somewhat blurred. The staff at the NIH, irrespective of what department it belonged to, was fully involved in trying to forward biomedical research. Although the DRG and the NIH had their separate staffs, they functioned as a group with overlapping responsibilities.

At the turn of the decade an important and very abrupt change occurred, by edict, that the persons involved in scientific merit review were to be divorced completely from program development. Whether the scientific merit review was in the DRG or in the review committees of the Institutes, they had to be separated. I can't tell you exactly what prompted that change. Ernest Allen had already retired, and Dale Lindsey became the director at DRG. Under Dale's leadership this change occurred. The study sections were told, "Your only responsibility is scientific merit review. You are to be thorough in that sense, and not become contaminated from development; you are reviewers." This was also true of the review committees and the Institutes. The organizational change occurred mean that all of these review committees were isolated from the people in their own Institutes who were in program development. In the past, it was often true that the executive secretary of an Institute review committee was also the person responsible for developing the programs and encouraging re-

search in certain fields. In a very authoritarian and purist way, the decision was made at the NIH level that it was not appropriate for the same person who was developing the grants to be also reviewing the grants. They thought these functions should be two separate lines of authority.

So the study sections became divorced from having program development implications a la the original review committees. The reason I am making a point of this is that the study sections essentially started as sub-committees of the councils, advising the councils, at the request of the councils. They have evolved over time, because of different pressures, into independent bodies having absolutely nothing to do with either the Institute or the council, functioning as if they were in a completely different organization. So, the relationship between study sections and councils and Institute staff changed over time, but were not started with even the concept of what they are today.

SS: And they remained separate in function from the end of the '50s up to the present?

MG: Absolutely separate.

SS: One can imagine that in the early days when there was the need to develop new fields that the agency as a whole had to be more entrepreneurial. I think that in the beggining when NIH was first given the authority to make grants there were only four or five study sections. Over the next five years another ten were added. So, one can imagine that there was a special need for that sort of entrepreneurial spirit, and I assume that one of the factors that must have contributed to this was that, after a decade or so, many more fields were established; there was much more going on.

MG: There was another variable at play. When the study sections were set up, even though the priority score system was initiated right at the beggining, the primary operational function of the study sections was not to determine which grants would be funded, but to decide which ones were definitely not to be funded. The reason for this is, starting around 1954 in the bigger Institutes -- Cancer, Heart, Neurology, Metabolic Diseases -- the study section approval, irrespective of the priority score, was nearly synonymous with a grant award. There was enough money to meet all approvals, so the approval or disapproval of the study sections was the critical decision.

SS: Therefore the question was very simple -- not how excellent the investigator had proved himself to be, but whether the investigator was competent and the research was worthy?

MG: The pressure was fairly intense from the Bureau of the Budget, so Jim Shannon, as Director of NIH, said, "We will not fund the lower ten per cent of the applications." What this essentially said was that anything with a priority score of 400 or poorer would generally not get funded. It was not an absolute rule, but Jim made it clear that the councils would have to take very special action on an individual basis in order to get funding for a grant in the lower ten per cent.

SS: So the specific responsibility of the study sections was to make sure they labelled anything they thought wasn't worthy of funding?

MG: Yes. And they were in charge of scrutinizing the applications' budgets to

determine if the amounts of money being requested were appropriate to the research.

Back to the "golden years": the program was growing, rapidly spreading out; new disciplines were being born; funding was readily available; there were relatively few regulations, if any. There was a law, and there was policy, but their weren't many written regulations. The directors at NIH and their staffs would meet and say, "Let's do something." and everybody would say, "O.K., we'll do it." And NIH let them do it. Because of the Fountain Committee and its interrogation and questioning of the administrative basis of decision-making at NIH, all of a sudden a new document was born called "the regulations" where a whole series of "thou shalts" and "thou shalt nots" were written down for the first time as regulations which had the thrust of the law. This was in the early '60s. This also began to influence study sections, because whatever the sections wanted to recommend, they recommended; the council considered it, the Institutes considered it, and if it seemed like a good idea, they'd fund it; if not, they wouldn't. We were not tied down in a body of practice. But into the '60s the Fountain Committee's impact made NIH a government organization operating by government standards called regulations. I think an unfortunate thing occurred at this time, in that the beginning of an attitude evolved about "them" and "us", depending upon whether you were in an Institute or in a study section at the Division of Research Grants. The DRG was born as an organization to service the needs of the Institutes. Therefore the Institutes and the study sections were one at first, with different kinds of responsibilities. When the change occurred in the 1960s, the Division of Research Grants were servicing the needs of the NIH as a whole, not of the Institutes. That's what I mean by "them" and "us". The needs of the NIH were not contrary to the needs of the Institutes, but the lines of relationships changed. An example of this is that, in those days, I would regularly meet with the Neurology Study Section when they met. I would spend twenty minutes telling them about what happened in the Institute since their last meeting, and they wanted this information. After the change, I was disinvited. It was no longer acceptable because study sections were not there to meet the needs of the Institutes -- but instead the needs of the NIH specifically for technical merit.

SS: What did that do to the connection between policy and scientific priorities?

MG: They became absolutely divorced. The study sections were told "You are scientific merit reviewers. Issues of policy, of need, of directions, are to be handled only by the Institutes and the councils. Don't worry about the rest." In fact, this couldn't really be so clearly separated; the reviewing scientists, in the individual decisions, were influencing policy. In their individual scientific recommendations, they were determining what science needed to be done or not. So the study sections, acting as an invisible force, were deciding which fields needed promoting.

SS: And that effected the priority scores, didn't it?

MG: Exactly.

SS: What officially took the place of this review by members of the study sections as to need?

MG: Most Institutes had two divisions, an intramural program and an extramural

program, with very sharp lines between the two. Most extramural programs staffs were generalists; staff members were assigned to go to study sections, and they were there to make certain of the process and bring information back. But the extramural staff itself was not there because it had expertise or because it was to develop a program area. It was there to see to it that the applications were being funnelled correctly.

SS: So Institute personnel became more like watch dogs to the study sections, rather than partners or participants.

MG: They did. At that time the Arthritis and Metabolic Disease Institute was catch-all Institute. It's fields of responsibility were extremely broad, and there was intense competition at the council level of that Institute about how much money was going into which fields. The other Institutes were much more circumscribed. They didn't get the push from the arthritis people, the diabetes people, the dermatology people, or the orthopedics people -- all on the same council. There was not a natural cohesiveness. So what the Arthritis Institute set up were essentially sub-Institutes with expert staff saying, "O.K., we will have an orthopedics program, and we will have a diabetes program, etc." All of a sudden, within the staff of a single Institute, there became competition, because you brought in experts who were pushing for the needs of their fields. What evolved was something called "program staff": staff built around the program that the Institute itself identified as a subset. So, out the this program of the Arthritis and Metabolic Disease Institute evolved the present organization of every Institute at NIH in which there were "program directors". These were the days when I was at the Heart Institute, and I wandered all over the heart field. I was not identified with either blood diseases or congestive heart failure; if it had something to do with the heart circulation, I was responsible. In the '5Øs, remember, the whole Heart Institute extramural program only had three people in it: Frank Yeager, Jerry Greene, Murray Goldstein. We couldn't be specialized if we tried!

SS: What about when Lung was added?

MG: That was much later. The program orientation was already in place then. All Institutes were essentially ordered to reorganize. All of this was occurring at roughly the same period of time that the role of the DRG was being reconceived. Now, instead of my being a grant person in the Heart Institute interested in everything, I would become a grant person interested in, say, myocardial infarction, or pulmonary disease. If I were in charge of a stroke program in Neurology, I had a peer who was in charge of the Parkinson's disease program of Neurology, and in some respects we were in competition with each other in Neurology.

SS: Who was it under the new set-up that said, "I don't think the work that's being done in Parkinsonism is of the same quality."?

MG: The Institute director plus the council had the say. The Institutes were developing "sub-institutes", although they were never called that -- they were called programs or divisions. Each subdivision had program responsibility. A staff was developed with expertise, with responsibility for a particular area of research, but not for the neurological sciences. So direction was now being given to scientists by Institute staff rather than people being administrators processing paper.

SS: Except that you still had to rely on study sections, and on the councils. Somehow you also had to rely on scientific disciplines to give you information on what the best opportunities were.

MG: When the Institutes went from being very broad in their thrust to being very specific, the role of the review committees had to be reexamined. In the past, Institutes loved the review committees developing programs. But all of the sudden they were two staffs -- the Division of Research Grants and the Executive Secretaries and the Institute staffs. Therefore the issue of whose responsibility is what became important. Out of that interaction came what we presently think of as the Intitutes with their program staffs -- technical experts themselves. Remember, Frank Yeager was an insect physiologist, and he headed the whole grant program of the Heart Institute. Now what we see is technical experts who have credentials in the fields of the Institutes; Larry Shulman in the present Arthritis and Muscular Division is a hemotologist; as opposed to a Frank Yeager, or Frank's counterpart at the Cancer Institute who was a tuberculosis specialists who had been brought on as an administrator. Jim Watt, a director of the Heart Institute at the time of Frank Yeager's role, was an epidemiologist on diarrheal diseases who had been brought in to run the Heart Institute. The director of the Cancer Institute, who later became Surgeon General, was a venereal disease expert. They were brought on because they understood medicine broadly, and were good administrators -- not because they were program leaders. Now the director of the Cancer Institute has to be a cancer expert.

I am not criticizing here, I am documenting. As far as I'm concerned, the changes were a natural consequence of growth, as the extramural and leadership staff of the NIH assumed more and more responsibility for the planning and the research, and therefore setting priorities to a change in the role of the study sections.

SS: What are the advantages and disadvantages of the changes? There have got to be both.

MG: The advantages are that the study sections review individual applications. They look at each application from its own scientific merit viewpoint, irrespective of the availability of the funding -- that's the pro. The con, or disadvantage, is that it's nonsense in the sense that the study section members are very well aware of where the priority cutoffs are, and they go chasing the money. If they have a good application, in order for it to get funded, they have to give it a priority score of 140 or it won't get funded. So now 50% of all approvals have priority scores of better than 200. Thus, the theory "Let the experts judge irrespective of money" and the practice are not together, because the reviewers do have a great influence on what kind of research gets funded.

Study sections are, in fact, in competition with each other. They do have an identity of their own, and a field of their own. They are trying to see to it that excellent research in their field is being funded.

SS: So peer review actually today means review in terms of a sub-specialty, is that right?

MG: A sub-specialty in science, not a sub-specialty in medicine. The priority score system worked very well, but it was originally used primarily to identify

which research would not be funded; the lower ten per cent. This system, and therefore the study sections, have run into severe problems when they are being used to decide which grants will be funded, when there are just limited funds to accomplish this. If we're funding 25% of our approvals, the present system suddenly doesn't meet the need.

SS: Except that the grant still has to be reviewed for relevance by council.

MG: Right, but it takes a very brave council to interfere with policies, partially because the council itself is composed of those who had been study section members at one time. What other system do you put into effect? How can you influence a priority score? Now, an Institute like Neurology had adjusted for this by saying, "We'll take 20% of our money and set it aside, out of the priority score system."

SS: That is the way they make sure that they support work in areas that they think are terribly important.

MG: Right. At the Neurology Institute they say, "We will use a per cent of available dollars to look at those applications which won't be funded by the study section system." And they reserve these funds to look at the lower percentile applications to see if there are needs in science of relevance to the Institute that need to be given special consideration for funding, regardless of what their priority scores were.

When you have shortages of funds, there is tremendous competition of high quality research, all of which isn't going to be funded. Is it not a council responsibility to see to it that the more risky areas; the developing areas; the people who because they are younger may not be able to compete; the fields that are in trouble but are trying to reorganize -- are getting attention?

What about the impact of the public? Say the Congress says, "We believe additional attention needs to be given to field 'x'." Do we set aside money for the field or do we look at the applications in the field and make one by one judgments? In the case of NINCDS what we have said is "We're going to set aside 20% of our money."

SS: What has been the response to that by scientists, study sections, by the Congress?

MG: Very positive. Because what it's saying is that the absolute priority score cutoffs will not be the decision-making endpoint. A large proportion of it will be, but there is still an opportunity for young researchers, for exploratory fields, for newly evolving fields, for the "kooky" application which we think is approvable, etc.

SS: What about the scientific judgment limitations?

MG: These have already been recommended by the study sections for approval, but they just don't fall into the upper percentiles. The issue is what is falling into the upper percentiles. Most reviewers would feel that they've been very honest in what they put at the top categories. But the question that must be asked is, "Should research that is only in the top category scientfically be the only research supported?" If the answer to that is "no", then the issue is "How much risk money should we set aside?"

SS: Have any other Institutes adopted the Neurology Institutes' 20% risk money?

MG: The approach is adopted by other Institutes, but not the 20%. Each Institute has a personality of its own. Some divide their money at the beginning of the year -- so much for field 'x', so much for field 'y', etc.

SS: Is this because, like in the Arthritis Institute, the subdivisions exist?

MG: That's one of the motivating forces. We have something like it in Neurology because we have hearing, speech, etc., and we have said outright, "We will not divide our money." We have been successful in holding off both the Congress and our advisors. We have said that when a member is appointed to our council, that member has the responsibility not for his or her own field, but for the whole Institute. Therefore, we have been able to avoid overt fractioning. We are a single council with a broad aim, so we use our money as a single pot.

SS: Do you find that the ups and downs in the patterns in the support for fields in that 20%? I suppose if it all kept going to the same fields you would have greater pressures to shift emphasis.

MG: It doesn't happen that way because our council and staff at every meeting is reviewing the applications that do not fall in the "automatic pay" line. Any time an application is proposed for what we call "special consideration", either the staff or the council member must stand up in front of the body as a whole and defend why. Application by application.

SS: Is there ever any difference of view or position between the Institute and the DRG about whether a particular grant proposal belongs with you or with another Institute?

MG: Yes. The DRG determines to which Institute the application belongs. The usual issue is not that an Institute thinks something doesn't belong to them; they want to know why is wasn't sent over to them in the first place. The DRG is vested by the mutual consent of all Institutes with the authority to assign applications. On occasion an application will be sent to an Institute who says, "I don't want this application." The DRG will do its best to try to put this in an Institute that's more sympathetic. But in the long run, the DRG may say, "It's yours."

SS: Who specifically makes that scientific judgment about where application should go?

MG: There is a group of about five people in the Division of Research Grants who are called, "Assignment Officers". They have the responsibility of reading all impending applications and making the decision of what study section it will go to and to what Institute. They make both decisions. Those are appealable by the Institutes, but only a small percent will appeal.

At Neurology, we get so many applications that we have six study sections. DRG has had to make a Neurology Study Section A and Section B to review the same type of applications. One of the arguments we get from applicants is, "You put me in the wrong study section; if you had put me in the other one, it

would have been more sympathetic." That's a tough one. The councils are usually broad enough that we don't get much argument about them.

SS: Can we talk about other kinds of grants and whether the same processes work, with the same difficulties that have to be worked out?

MG: The other big area of grants are program projects. These are usually reviewed by review committees within the Institutes themselves. What we are essentially back to with this is the late '40s and early '50s at the NIH. These are generally large grants which have many projects addressing a broad issue. In the early '50s, the authority of the NIH was to make research project grants. A group of elder statesmen in the clinical areas saw, as the NIH was growing, that in clinical research it is sometimes extremely difficult to predesign an experiment. Something happens, a patient is admitted to a hospital, and nature has designed the experiment. This is a magnificent opportunity to study that patient. If you have to apply for an NIH research grant, by the time you go through the process, the patient is already well or has died. They said, "What we need is funds, so that we can be opportunistic and take advantage of the patient when he or she comes in." These were called, "clinical reseach center" grants, in which a bonus of funds were given based on the track record of a particular team to the institution to be able to admit a research patient and pursue the research.

The first of these were general grants, because nobody knew when it would be a heart disease, a cancer, epilepsy, etc. So general clinical research centers were established in which funds were pre-awarded without knowing the details of the research that they would go to. The Institutes finally started saying they'd like to have similar opportunities for their fields. So the next step was the development of "specialized clinical research centers". A line item was developed in the budget for this. The NIH went to the Congress and said, "We want to do this, because all we've got is research project authority, and we want to develop these clinical research centers, all specialized." Congress gave every Institute over time its own line item called "specialized research centers". So, the clinical investigators were getting a bolus of funds to explore a broad field. Then we began to hear from the basic scientists who said, "Can we get a bolus of funds?" The response was, "Why would you need it?" Well, it was just at this time that technology was really exploding. So they would say, "If I'm going to get an electron microscope, I can't justify it on any individual project. We need a central resource for basic research just as much as you need one for clinical research." So we scratched our heads and said, "But all we've got is project authority and they are looking for program support. Hey — why don't we give them program projects?" And that's how the name "program project" was born.

SS: What year was this?

MG: It was the late 1950s. The reason I say that is because I was part of the discussion. I was in the Heart Institute then, and I left the Heart Institute at the end of the '50s, so it had to be in the late '50s. Program projects were born, but never became a line item in the budget because they were research projects. They were research projects with support programs. So clinical research centers were born first, during the time when all of NIH was growing at a remarkable rate, and clinical investigation was being emphasized, with the need for these clinical centers.

SS: What about the review of the program projects?

MG: The argument arose about who was going to review both the centers and the program projects. There was quite a debate about it. Finally it was agreed that they would be reviewed within the Institutes by Institute-based study sections called "program project review committees". That still exists today and every Institute has one.

SS: So those don't go to the DRG at all?

MG: No. The only thing that goes to the DRG is the applications received by the DRG who recognizes program project applications in a certain field and assigns is to the appropriate Institute special review committees. These committees are charged just like DRG study sections, and they are operated with absolutely the same division -- they have to be in a separate part of the Institute and not influence by "program staff". When these were started, the program staff usually were executive secretaries of the study sections who were doing both; they were helping the applicant write applications, then being charged with the review. These functions were finally divided.

SS: What about training grant? There are not very many these day, I take it.

MG: No, there are lots of them. A lot of money goes into training grants still. The original authority for the NIH was to give research fellowships and traineeships, which were for the training of clinicians. It was decided that there was a need for a specialist called a "hemotologist", for instance. So one could get a clinical traineeship or a research fellowship.

SS: Did the fellowship grants go to individuals?

MG: Yes, and the traineeship went to the individual; there were no training grants. The distinction was that the fellow was doing research training, and the trainee was doing clinical training of the highest caliber. The only training grants available were in three Institutes: Cancer, Heart, and Mental Health, for undergraduate medical education. These were training grants to the institution to train at the pre-doctoral level. Those were the only training grants available, and they were strictly for undergraduate use. Then every Institute had authority for clinical traineeships and research fellowships; one to train specialists, and the other to train investigators. There were no graduate training grants. Then a number of institutions said, "Look, every year we get four research fellowships from you for training in, say, enzimology. We've had this for the past five years. Why don't you just give us four fellowships every year and let us pick our own people. Why don't you just give us a block of research fellowships and we'll pick them. If we pick the wrong people and do a bad job, you can drop us." This seemed like an immanently good idea. There was nothing that said we couldn't do it. Just when it got formalized I can't tell you, but when it did, it was called the "graduate training grant program."

SS: Were study sections set up to review it within DRG?

MG: No, in the Institutes.

SS: Were there a construction grant review committee within Institutes?

MG: There were a few Institutes that had construction grant authority, and those were organized, but that was centralized, so DRG was asked to do it. What happened in those days was that when a new program was evolving -- construction and graduate training grants, etc. -- Ernest Allen and the Institute person generally charged with extramural programs would sit down and talk about it, discussing together what would be the best way to review them. Ernest, being the kind of person he was, was never threatening to the Institutes; they liked the way he worked. So the precedents were set in that way, and some of the program project committees were in the DRG because people asked Ernest to do them. Other Institutes sometimes preferred to do it themselves and Ernest would say, "Fine."

SS: So what is the pattern today?

MG: Today most Institutes have their own review committees for program projects and centers and for training grants. The clinical traineeship program, with the exception of the Cancer Institute, died. The Congress raised questions about why it was spending federal money to train all of these doctors. So both the undergraduate training grants for the training of medical students, and the clinical traineeships, died. NIH dropped them when Congress kept questioning them. The graduate training grants are generally reviewed in the Institutes; the individual fellowships are generally reviewed in the study sections. But it all evolved on the basis of precedent and on the basis of personalities; Ernest Allen is the kind of person that every Institute trusted. All the leaders of DRG became somewhat provincial -- "this is my authority, that's your authority" -- and Institutes began to draw back. But that's how things evolved, and the graduate training grants, which now became training grants, all evolved out of the concept of gentlemen's agreements to give an institution four or five fellowships with which they could choose who would receive them. They sent us the names and we awarded the fellowships. We essentially agreed to accept the nominations they made.

SS: What problems with respect to the grants program are most urgent today?

MG: The critical general issues are, first, the provincialism of the Institutes -- "what's my grant, what's your grant" -- because there area more Institutes now than there were in the past. This provincialism has grown and is not to the benefit of science as far as I can see, and not to the benefit of the character of NIH. That is a problem inherent in any organization.

The second major issue is that the NIH peer review system is being taxed badly now by the number of applications. A system and a process that were born to handle a relatively small universe of grants, both in terms of the scope of the research and of the number of applications, is just being taxed beyond its limits. One of the problems NIH faces is whether applications are receiving adequate review, or are they being over-reviewed? Can the system accept more applications? That question has been a really tough one.

Third, the instability of the funding process -- one year funding 40% of the applications; the next year only 25% -- doesn't provide for stable support of scientists. The other side of the questions is, should there be stable support for scientists? Are the variations themselves healthy for science? Is extreme competition healthy? I think most people would agree that the answer is yes to both. So where lies the balance? Jim Weingarten has proposed this in the sense that if 40-50% of approved grants can be funded, that provides for

the competition that drives scientists, and the stability that is needed for the advancing their careers.

SS: Does that take into account renewed funding?

MG: It takes into account anything that is competitive that year. In other words, I have said that when you fall below having one out of three funded, your science is in danger. If you can approach 50%, your science is stable. If you go over 50%, then it's exciting. You can invest in risk, but once you fall below one in three, the system at NIH, the study section council, is not precise enough to make that kind of measurement. Once you begin to approach the 40-50% mark, then you're in areas where, if you make a mistake, it probably won't be as bad. It's important to the individual applicant, obviously, but I'm talking about the whole thrust of science.

SS: Isn't funding relatively stable right now? Hasn't it been less problematic than some of us imagined at the beginning of this Administration?

MG: No. Granted, Chicken Little's "the sky is falling down" did not occur, but what saved it? Across the board, it was arbitrary cuts in grants.

SS: In new grants?

MG: In all grants. Including committed grants. There were reductions in the size of the grants. If someone was to get $100,000, he got $90,000 instead. One can say, "That's all right." But what would happen if your salary was automatically cut by 10%? They don't like it, but that's what we're doing. What we used to be able to do was fund more grants so that this wasn't a problem. My own philosophy has always been that a commitment is a commitment; if you made an honest judgment about the amount of money a person needs for the research, you don't like cut it 10%, but you adjust. But I would much prefer saying to that lab, "You have stability for three years from this grant." For those coming up for competition, we have no commitment to them, and they might not get funded because we don't have the money. I don't think cutting the amounts gives stability; it does if you want to measure the number of grants awarded; it doesn't give stability in the laboratory.

There's been another very important change that I don't think a lot of people realize. When the grant program was born in the '40s and '50s, the philosophy was geared toward the universities; it was for what the university and its staff wanted to do. What they were asking the federal government for was merely assistance -- some funds to help you do what you want to do better than you could have without it. But it was for <u>their</u> grants, their initiative, their research and investigators, and their responsibility. We were contributing to the research which they wanted to do. In 1986 it is now called government sponsored research, and the university says, "Do you want us to do this research?" So there's been a tremendous change in the whole relationship. It has driven a lot of decisions such as indirect costs; now they say, "We want you to pay our costs, because it's your research." Our response is, "It's not our research at all. It's your research." There are people who have endowed professorships. The university now has to distribute their salary; because they're speding 40% of their time on the research grant, they want 40% of their salary in the grant. We say "You've already got the money for their salary. Our grants are for sponsoring research." This drive has changed our relation-

ship. I understand why the universities need the funds, but are the research grant funds suppose to underpin the operation of universities? If that's true, why shouldn't they just become federal universities, as exist in other countries of the world? Then we would run them. But they don't want it like that and neither do we. But the concept of "the grant" is changing nevertheless. It used to be that if the government wanted certain research done, it gave contracts. But the original grant system was not based on that. It was supposed to be an assistance program.

I must say taht everything we've talked about in terms of study sections, councils, and all the programs, have been strongly influenced by that change in philosophy from grants as aid to, in fact, government sponsored research. The day we agreed to government sponsored research, indirectcosts went up, the average cost of grants went up, and the entire biomedical community became more dependent on the NIH because the issue became "If I want to do reserch I've got to get a grant, so I'll have to do it in something the NIH wants to see done."

SS: What about places like the Howard Hughes Institute?

MG: If you do research supported by the Howard Hughes Institute, you are actually employed by the Institute, not an employee of the university. The university is merely accomodating you. They list you as a professor, but you work for the Institute.

SS: I see. I know both sides of the argument, and I can see merit on both sides. In any event, what has happened is that the grants program over forty years has in fact been the principal vehicle for the construction of a nationwide, dynamic enterprise.

MG: It is a great program.

SS: My question is a practical one in terms of dollars, but also an attitudinal one with respect to, are they investigating scientific and medical questions because the government wants them to, or because they think it's important and the government agrees with them and will therefore give them some support.

MG: All we've discussed is some of the problems because you asked about those specifically. The system is the envy of the world; every nation and every committee that has been selected has said that it's a magnificent system in that it permits people to enter the system. It gives them an opportunity to compete. It is a benevolent system that is responsive to science, and yet also responsive to the needs of the public. If one were to sit down in the abstract and design this system to meet those kinds of objectives, it would probably look like the NIH.

NIH was not designed to be the way it is today — it evolved. It evolved in what I consider to be the best inter-relationship between the government, the public, and science. By interaction, everybody wanted to listen to the other two. It has it's problems, but it has been highly successful at serving all three of these groups.

SS: You've covered everything I needed to know with good detail, and I am certainly glad I was able to get your perspective. Thank you very much for your time, Dr. Goldstein.

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CURRICULUM VITAE AND BIBLIOGRAPHY

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Assistant Surgeon General, U.S. Public Health Service

Born October 13, 1925, New York City; Married, 2 children
U.S. Army (1943-1945), U.S. Public Health Service (1953 to present)

I. EDUCATION

B.A. (Biology)	New York University	1947
D.O.	Des Moines Still College of Osteopathic Medicine	1950
Rotating Internship	Still College Osteopathic Hospital	1950-1951
Resident (Internal Medicine)	Still College Osteopathic Hospital	1951-1953
Public Health Trainee (Epidemiology)	California State Department of Public Health	1958
M.P.H. (Epidemiology)	University of California School of Public Health	1959
Clinical Neurology	Mayo Clinic and Mayo Graduate School	1967-1968

II. EXPERIENCE

Medical Officer, Commissioned Corps, U.S. Public Health Service (Present rank: Assistant Surgeon General)	1953-present
Director, National Institute of Neurological and Communicative Disorders and Stroke, National Institutes of Health	1982-present
Acting Director, National Institute of Neurological and Communicative Disorders and Stroke, National Institutes of Health	1981-1982
Deputy Director, National Institute of Neurological and Communicative Disorders and Stroke, National Institutes of Health	1978-1981
Director, Stroke and Trauma Program, National Institute of Neurological and Communicative Disorders and Stroke, National Institutes of Health	1976-1978
Director, Extramural Programs, National Institute of Neurological and Communicative Disorders and Stroke, National Institutes of Health	1961-1976

Visiting Scientist, Section of Neurology, Mayo Clinic and Graduate School, Rochester, Minnesota	1967-1968
Executive Secretary, Joint Council Subcommittee on Cerebrovascular Disease, National Institute of Neurological Diseases and Stroke and the National Heart and Lung Institute, National Institutes of Health	1961-1967 1969-1975
Chief, Special Projects Branch, National Institute of Neurological Diseases and Blindness, National Institutes of Health	1960-1961
Assistant Chief, Research Grants Review Branch, Division of Research Grants, National Institutes of Health	1959-1960
Acting Chief, Section on Virus Diseases of the Central Nervous System, Bureau of Acute Communicable Disease, California State Department of Public Health, Berkeley, California	1958
Director, Epidemiology and Biometry Training Grant Program, Division of Research Grants, National Institutes of Health	1956-1958
Assistant to the Chief and then Assistant Chief, Grants and Training Branch, National Heart Institute, National Institutes of Health	1953-1958

III. PROFESSIONAL AND SCIENTIFIC ORGANIZATION MEMBERSHIPS

Fellow, American Academy of Neurology
Fellow, Council on Stroke, American Heart Association
Fellow, Epidemiology Section, American Public Health Association
Fellow, American Osteopathic College of Preventive Medicine
Fellow, Pan American Medical Association
Honorary Fellow, American College of Osteopathic Internists
Honorary Fellow, American College of Neuropsychiatry

American Neurological Association
American Osteopathic Association
American Association for the Advancement of Science
Association for Research in Nervous and Mental Disease
Society for Neuroscience
Research Committee on Neuroepidemiology, World Federation of Neurology
Research Committee on Cerebrovascular Disorders, World Federation of Neurology
International Brain Research Organization
Distinguished Practitioner in Osteopathic Medicine, National Academies of Practice
Honorary Member, The American Association of Neurological Surgeons

IV. ORGANIZATIONAL RESPONSIBILITIES

A. International Organizations

Consultant (Neurology), Pan American Health Organization (1974-1976; 1983-present)
Consultant, World Health Organization Program on Neurosciences (1975-present)

Director, W.H.O. Neuroscience Collaborating Center at Bethesda (1981-present)
Chairman, Symposium on Cerebrovascular Diseases, World Health Organization (April 1978)
Corresponding Member, Italian Society of Neurology
Corresponding Member, Peruvian Association of Neurology, Psychiatry and Neurosurgery
Council of the Neurology Section, Pan American Medical Association

B. Professional and Scientific Organizations

American Academy of Neurology
- Long Range Planning Committee (1972-1975)
- Committee on Manpower (1979-1985)
- Committee on Neurology in Governmental Services and Institutions (1979-1985; Chairman, 1981-1983)
- International Affairs Committee (1981-1987; Chairman, 1981-1983)
- Committee on Government Relations (1983-1985)
- ANA-AAN Delegate to the World Federation of Neurology (1981-1987)
- AAN Committee on Public Communication and Legislation (1983-1985)

American Neurological Association
- 2nd Vice-President (1982-1983)
- ANA-AAN Delegate to the World Federation of Neurology (1981-1987)
- Advisory Committee on Honorary Membership (Chairman, 1980-1982)
- Committee on Constitution and ByLaws (1979-1980)
- Executive Committee, Joint Committee for Stroke Resources (1970-1978)

Joint Commission on Neurology, ANA and AAN (1972-1974)
Membership Committee, Society for Neuroscience (1977-1980)
Trustee, American Osteopathic College of Preventive Medicine (1977-present)
Trustee, Uniformed Services Organization of Neurologists (1983-present)
Consultant, Bureau of Research, American Osteopathic Association (1985-present)

C. Scientific Groups

NINCDS Liaison Member, Board of Trustees, Princeton Conference on Cerebrovascular Disease (1968-1980)
Vice-Chairman, NINDS Commission on Stroke (1972-1973)
NINCDS Liaison Member, Board of Directors, Chicago Conference on Neural Trauma (1974-1980)
Award Jury, William Thomson Wakeman Award for Research on Spinal Regeneration (1973-present)
Award Jury, Albert Lasker Medical Research Awards (1982-1985)
Award Jury, Alan Taylor Prize for Medical Research-Canada (1985-1987)

Member, Directorate, Department of Defense Vietnam Head Injury Study (1980-1985)
Member, DHHS Orphan Products Board (1982-1986)
Member, DHHS Task Force on Alzheimer's Disease (1983-present)

D. Educational Organizations

Member, Board of Trustees, Des Moines College of Osteopathic Medicine and Surgery (1967-1972)

Member, Board of Trustees, Kirksville College of Osteopathic Medicine (1973-1976)
Consultant, American Association of Colleges of Osteopathic Medicine (1974-1975)
Visiting Professor of Medical Research, Semmelweis Medical University, Budapest, Hungary, September 1975
Co-Chairman, Board of Governors, New York College of Osteopathic Medicine (1976-1979)
Clinical Professor of Medicine (Neurology), New York College of Osteopathic Medicine (1977-present)
Visiting Scholar, Henry Ford Hospital (1979; 1980)
Visiting Lecturer in Neurology, Mayo Clinic (1980; 1986)
Member, Advisory Board to New York College of Osteopathic Medicine, Rockefeller Brothers Foundation (1980-present)
Member, Board of Directors, John P. Robarts Research Institute, London, Ontario (1983-1987)
Member, Mayo Alumni Association, (1983-present)
Visiting Lecturer in Neurosciences, India Council on Medical Research (1984)
Guest Lecturer, Hungarian Academy of Sciences (1985)
Senior Lecturer; Uniformed Services University of the Health Sciences (1986-present)

E. Public Organizations

NINCDS Liaison Member, Executive Committee, Council on Stroke, American Heart Association (1970-1980)
NINCDS Liaison Member, Professional Advisory Board and Board of Directors, Epilepsy Foundation of America (1972-1976)
Board of Directors, United Cerebral Palsy Research and Educational Foundation (1972-present)
Vice-President, Eisenhower Institute for Stroke Research (1975-present)
Medical Advisory Board, The American Parkinson Disease Association (1976-1985)
Commission on Alternative Health Care, U.S. Olympic Committee on Sports Medicine (1980-1983)

V. PROFESSIONAL AND SCIENTIFIC JOURNALS

Associate Editor, Stroke, A Journal of Cerebral Circulation, AHA (1976-present)
Editorial Board, Osteopathic Annals (1973-1985)
Editorial Board, International Journal of Neurology (1980-present)
Editorial Board, Neuroepidemiology (1981-present)
Editorial Board, Hospital & Community Psychiatry (1980-present)
Editorial Board, Alzheimer Disease: An International Journal (1985-present)

VI. LICENSURE AND SPECIALTY BOARDS

Osteopathic: Florida; Maryland, National Osteopathic Board
Medical : Maryland
Joint : New York; Minnesota
Boards : Board Qualified, American Osteopathic Board of Internal Medicine
Board Certified, American Osteopathic Board of Preventive Medicine

VII. HONORS

Doctor of Science (Hon), Kirksville College of Osteopathic Medicine
Doctor of Laws (Hon), New York Institute of Technology
Doctor of Science (Hon), University of New England
Doctoris Honoris Causa, Medical University of Pecs, Hungary
Doctor of Science (Hon), Ohio University

Beta Alpha Epsilon, Biology National Honor Society, N.Y.U.
Psi Chi, Psychology National Honor Society, N.Y.U.
Sigmi Alpha, Osteopathic Scholarship National Honor Society, College of Osteopathic Medicine
Delta Omega, Public Health National Honor Society, University of California

Founders Day Medal (S.S. Still Medal), University of Osteopathic Medicine and Health Sciences, Des Moines, Iowa
Honored Guest, Neuroepidemiology Training Program, Division of Science and Technology, Bombay, India

Certificate of Merit, American Neurological Association
Silver Star, Purple Heart, U.S. Army
Meritorious Service Medal, U.S. Public Health Service
Distinguished Service Medal, U.S. Public Health Service

VIII. AREAS OF SPECIAL INTEREST

Medical Science Administration, Graduate Medical Education, Epidemiology, Cerebrovascular Disease, Nervous System Trauma and Regeneration

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