Dr. George Saslow

GS I might start with how I came to have anything to do whatever with NIMH, I thought that was interesting. Probably, not an idiosyncratic kind of experience. I would never have dreamed of having anything to do with a government agency at that level in the way that it turned out I was having things to do with it of my own initiative, and the reason for that was, and I have always, although strongly committed to a liberal kind of social organization, I never felt that I would undertake political activity or be involved in high governmental levels in a decision making position, that was never attractive to me. But somewhere around the early war time, about 1941-2 I became a charter member of the Society of Applied Anthropology because I had known a number of anthropologists and I knew about their work and I was working with one at the Massachusetts General Hospital, Elliot Chapel, who knew Margaret Mead, so at various meetings I would be present with a number of anthropologists who were well known, and Margaret Mead and I got to know each other, and after the war, the Mental Health Act was passed and NIMH was started. At one of these meetings Margaret Mead asked whether I had thought of becoming involved with the National Institute of Mental Health . I never had. It was a totally new idea to me, and she indicated that there were some very important potentialities for the relating of science, mental health and governmental activity in a democratic country involved in the setting up of the NIMH, and she urged me very much to see if I couldnt play a part in that. So that was a totally new idea for me, but it would never have occurred to me (a) to do it, and (b) that I'd have anything useful to contribute. So my becoming involved in NIMH was entirely due to Margaret Mead drawing to my attention at the time the social responsibility of a person who had regarded himself as a biological scientist, because I had been a physiologist, and as a physician and a teacher in psychiatry. For me it was a great expansion of what now is much more customary. It was really very different from anything I had ever imagined about the way that I would lead my life. So, I suppose, in the process that was used, was duly nominated and so on and became a member of a study section, I guess, at first. I don't remember the exact date of that really. I began to function regularly there and a number of observations were immediately very striking. In the first place, GS cont. it seems to me that at that time, although there was only one study section, the membership of that study section was from a diversity of disciplines related to human behavior and, although I probably was more widely read than many other psychiatrists at that time in sociology, the sociological literature and the anthropological literature, as well as the psychiatric literature, it was really delightful to interact with people who, themselves, had worked long in that field and had done a good deal of thinking in that field and made various contributions, and learned from these various people their various perspectives as proposals for scientific investigations came up for examination in the process of investigators submitting applications, and so forth. So I found that in a way I was learning a tremendous amount at the first time, I think, at the level that I had now attained. That's a very rare opportunity, that you can be an enthusiastic student when already supposedly having achieved maturity in your field. You can now be an enthusiastic student with people who are mature in their fields all of you together are enriching each other's kinds of experiences, it's very hard to describe the delightful nature of that kind of experience as we discussed proposals together and made site visits together, evaluated them together, monitored their outcome together over the years. So one of the most striking things I remember is how much all of us felt we were learning by participating in this joint activity, and I think nearly everybody I was associated with during the years that I was involved with NIMH felt the same way about it, that it was a most unique opportunity which we, after we had been there a number of years, and it was clear that more study sections would be needed, we felt that more and more people ought to be involved in this extraordinary opportunity to learn and expand their horizons, without having to wait to be as far along as we had been when we were the first ones to start and we realized that younger people ought to be around, not the very youngest, because we didn't think they had experience enough to make a valid contribution...

EAR May I ask you a question at this point? Did it ever come to mind, that what you've just described, incidentally a number of people have made similar comments, suggests the essence of what ought to transpire in an academic community and rarely, if ever, does?

GS Certainly. I don't know if this is , you decide what to do with it, but it is very striking to me that in a place like the NBI here, from many kinds of interactions that I have with people of different levels, from the faculty appointments and promotion committee, the fact that I'm a professor, I teach in a course, I interact with people from West down, I have known a number of them through research and other activities and I also work with as many as ten of the residents in a process group who are second year residents and hear a great deal, of course, of the way things function here. I also have supervised a number of third and fourth year residents here at their request, so I can compare what happens here from this viewpoint of the really academic community of scholars with what I have tried to create in Sepulveda with a very small number of people, we probably have less than a dozen, they have 360 or so members of the clinical faculty here and they do not have here, any more than I saw at Washington University or in most parts of Harvard or in Oregon, they do not have this kind of atmosphere across discipline lines, within a department, I have found, since I am interested in it, I haven't found that too hard to create especially since I have some choice over the selection of personnel. But where I am now, we have a group of people who have come there partly because of my interest in all of these educational processes. They have themselves been interested initially. Their interest is now encouraged and we fortunately have a group which really pays a tremendous amount of attention to this exchange of view and enlargement of experience, but in which we have become influential there even in attracting people in the Department of Medicine to become interested in a broader point of view about medicine, which itself is a very important and a serious problem in psychiatry there, as the behavioral sciences in general can make major contributions, but you're right, I think that it is far more infrequent, than is frequent, in academic communities, so that the term community of scholars hardly has any meaning in our time really. I think that's absolutely right. I was thinking of a particular example of some of the things that we learned from each other and how we worked with each other. Not too many years after I became involved through Margaret Mead's suggestion, Skinner submitted some proposals for work that I think he was doing with Ogden Mensley at the Metropolitan State Hospital. He was working

GS cont. with adult retardates who could not manage themselves outside and chronic psychotics who could not manage themselves outside and he was trying various kinds of behavioral procedures. He submitted a proposal for support for this report, he and Mensley did (is it Lindsley or Blindsly, Ogden Lindsley,)and there was a very interesting series of discussions in the study section, which still was one study section at that time, about how to respond to this. A number of the members of the study section group were educated in a rather traditional orthodox psycho-analytic way and felt that nothing that had to do with limiting itself to phenomenonological observations of behavior, to changing environmental conditions which might control that behavior was acceptable, that it was really part of a coercive robotizing of human beings. These were some of the kinds of language that were used. At the same time there was a very keen awareness on the part of the members of the study section as we had gone over many proposals together, how poor were the data on the outcome of any kind of treatment for altering human behavior. We were, of course, because we were familiar with the new research that was being proposed in the field, familiar with the rather poor documentation of any treatment intervention, especially with long followups and so on. And I can never forget that the way in which the study section finally was able to bring itself to give support to Skinner so that his work could get started with some NIMH support was through a psychoanalyst on the committee, that was John Benjamin, he's now dead, you won't have a chance to talk with him. And John Benjamin was a very interesting....he was very thoroughly familiar with all of Faure's contributions and the contributions of Faure's associates and his epigenes and disciples, but at the same time very much bothered by the poor documentation of the outcome of psychoanalytic treatment, as well as any other. It was very hard for him to see how you could deny support for somebody who had a new idea which might be useful, unless you had some very persuasive danger that you could point to like the business of the imminent danger that the Supreme Court uses in deciding about treason and things of that kind. It was John Benjamin who was most effective and persuasive in laying the fears, the apprehensions and reducing the discomfort of those persons for whom Skinner's whole way of going at things was terribly upsetting, and those of us who were neither terribly upset nor had any idea what would happen if you encouraged Skinner's

GS cont. work but recognized his ability and that he was proposing some reasonably well designed studies in human behavior, could with John Benjamin's support and alliance, so to speak, get consent for him to get some support. It wasn't support on a tremendous scale and his work really became much more widely known after that, but that was a very significant kind of decision which I witnessed in the making, and it came out of the open exchanges which people from various disciplines, various backgrounds, were able to undertake when they faced something new to all of them, and in which you had to invest government funds on a priority basis and some competition with other funds and you had to make a kind of estimate of what the possibility was of causing the government trouble, the possibility of some kind of worthwhile pay-off for the effort and so on, while all of us had a very keen awareness of how research can't be aimed directly at a target. So all these considerations were in the pot of that kind of decision and all of us learned a tremendous amount from that as we followed what happened. By contrast, a decision which it was much easier for a majority of the study section to reach, turned out to have a very comment, to demonstrate that this also happened. This had to do with the proposal by Franz Alexander and his group in Chicago to conduct a study of what they called the specificity of psychosomatic disorders. Is peptic ulcer very different with regard to certain specifiable dimensions and let's say mucus colitis, or ulcer colitis or bronchial asthma, and so forth. Or Rheumatoid arthritis was another one, there were some seven different psychosomatic disorders which Franz Alexander had become convinced had very specific kinds of constellations in the early history and in subsequent crisis responses, I guess. That was all before, Alexander's idea antedated, Alexander's notions of specificity antedated Arthur Mersky's careful studies on pepsin antigen and its genetic backgrounds, and so forth. That's one of the factors which could contribute to notions of specificity together with certain kinds of early constellations of interpersonal difficulties as you grow up in the family and then the later crisis which resurrected some of the old responses. Well, hundreds of thousands of dollars were easily voted to support that proposal of Alexander's, largely because of his reputation. He had never really done any solid scientific work. I have never been convinced, even though, towards the end of his life he wrote a paper which

GS cont. dealt with a kind of attempt at rapprochement with learning theory and psychoanalysis at a very abstract level. He never really had used any of these things and the antilogical comparisons he had made are easy enough to make, but he was determined to win scientific acceptance apparently, and this proposal was one way in which he tried to do that. Well, there was a lot of discussion about that because there were members of the study section who were not impressed sufficiently with what the outcome might be and they could point to the fact that his record didn't show really solid scientific work. But his reputation and the reputation of psychoanalysis accounted for the fact that that study was supported year after year after year, I don't know for how long, but it ultimately ended up at least five or seven years, and hundreds of thhousands, probably at least a half million dollars, and at the present time, in 1978, if you were to ask what survives of this within the field of those who practice psychosomatic medicine, like I would say Bob Pasnow here and Herbert Weiner at Montefiore or at Albert Einstein, probably extremely little. I think it's been lost within a much larger framework of Cellier's work on the adaptation syndrome and work like Thomas Holmes on the life change units which when they pile up, no matter from what source, exceed the organism's capacity to cope and so forth. There's nothing wrong with a hypothesis not paying off but I was very much struck by the unusual weight given to a person's reputation who had not demonstrated solid scientific competency in those days as compared with the way Skinner's proposal was considered so carefully from other points of view. I guess as you look back on it, Skinner was given no credibility for being other than a cynical robotizer, despite his scientific work, while Alexander was given credibility as a potential scientist despite the absence of a track of scientific achievement. Well, I am sure there were other decisions like that that were made with funny kinds of outcomes but the freedom of discussion and the willingness to examine what happened after, we'd make site visits to these projects, of course, and followed them up, there was always a self-corrective factor in there which I though was extremely admirable.

EAR Well, I'm sure you've been thinking about this from time to time and could you describe what you think are some of the variables that facilitated this kind of interaction, and let me just mention

EAR cont. a few that are almost obvious. The calibre of people on the committee usually was very high, a kind of freedom because of the circumstances from any need to demonstrate your competence to your colleagues, although that sometimes was part of the competitive nature of the interaction, but there was no self-defensiveness, a high commitment to involvement in the process because of the almost universal belief that this was a very useful scientific procedure, that it helped to extend the whole field. Perhaps partly, an identification with the Institute in the sense that everybody there saw themselves as part of this larger umbrella of mental health people, but over and above those obvious, what do you think permitted this kind of facilitation, really the self-corrective things that took place, People like John Benjamin move out from postures they might otherwise hold.....

GS I think that another thing that was very important, it seems to me, there are two things that occur to me right off. One was that we felt that the behavioral sciences, that is the higher functions as Pavlov called them, of human beings have been entirely under-attended to as there was the rapid development of biology and the hard sciences, so to speak, and we felt that that was societally disadvantageous and we had a marvelous opportunity to put things into perspective, so as to put what was human about man back to where it belonged and to give adequate consideration to this aspect of the disorders from which people suffer and which we assumed had high societal correlates of impairment in one way or another. I think most of us shared that. And another factor was that at the time that I was involved in NIMH there was a very clear divorce between your assessing proposals for work to solve some problem, placing those in some kind of prioritized order without however having actually to share out the resources, the instructions were very clear about that. Your job is to assess merit of these proposals, the actual merit, potential merit, value in a narrow sense, value in a broad sense to the whole field of medicine, even beyond the behavioral sciences and not to pay attention to how ultimately these priorities will be responded to within financial resources. I recognize perfectly well that there was an Advisory Council which had half of his members non-professional people as unlike us, they had political responsibilities, had political loyalties, had political perspectives and so on and they

GS cont. had to make these hard decisions which we recognized always came in contact with the general principle that human need outruns resources, and I think we were all aware of that but we were free from that . It was very different, for example, for me to be on the NIMH study section or in the training section, it was very different to be functioning there and to be a chairman of a department, for example which some years later I became at Oregon. I've always rejected that as a part of my narrowness of view which Margaret Mead helped expand, by the way, that when invitations had come to me to consider being a chairman of a department, I just was totally disinterested. And years later, then, when I had the opportunity really to start a department which had never had a full time psychiatrist, a chairman in psychiatry, at Oregon, one of those last dozen or so schools after the war that were able to do that in various clinical fields, medicine, surgery and psychiatry, I then realized that I had to decide upon the resources. I was now dealing with a dimension which had been absent from the other proceedings. I had an idea that I had something to do with it. In a way it was like your statement that we didn't need to be defensive, we didn't really have to fight hard for the resources, we were not the ones who were doing the fighting. Now I think it was very infrequent that any mention ever was made that, well, there ought to be less support for psychiatry that was thought of as getting too much , or for psychology, and more for sociology or anthropology, I never heard any such discussion. So, we were singularly free from that, it seems to me, and maybe that was because of that division of responsibilities. That seemed to me a very desirable one. One other thing I never noticed, really, was a kind of thing which in many academic settings including this one, I hear a lot about, and that was a kind of competitive jealousy and envy on the part of members of the study section when going over proposals, that somebody else might have a reputation enhanced, might be advantaged, I hardly remember any such discussion, somebody's taking that point of view, you know,

EAR I'm sure that was very rare. Do you recall instances in which, either directly or indirectly, the projects that you were examining provided you insights about the advances in your own work?

GS Oh, absolutely. I learned a tremendous amount from reading

GS cont. applications. I would read them very carefully. It was one of the ways in which we were educating ourselves all the time. You were coming in contact with all sorts of new ideas, and again, it was interesting. I don't remember any instances in which anybody wrongfully appropriated information like that. It was supposed to be privileged and confidential. It could have been misused in some way. I've known scientists who have exploited people in the laboratories that way, but I don't remember any such instance really. It could have been done, it was tempting and inviting when one wanted to, I suppose.

EAR Well, there was somehow built up, I think, no conscious effort was ever made to do this, and I think when the study section procedure was first developed there was no anticipation that it would develop the way it did. I think there were a lot of serendipitous attributes to the whole thing. Well, you were working, before that came along, could you make any more comments about how the review procedure itself affected, aside from the obvious providing of moneys for doing the work, but how the review procedure itself affected (1) the communication network within the relevant sciences and how it may have provided the kind of side effect of informing, and you've already commented on this effect, informing the members of the committees, but isn't it interesting that before the NIMH came along there was nothing comparable, in any sense of the word, for a national network of information about what was going on scientifically. Sure there were journals being published, articles obviously came out in the scientific literature, but the interchange at the process level of development didn't exist and even, for example, writing a grant application. What did you do in your laboratory before the NIMH came along in terms of developing a proposal for what you were going to do, did you write it out in the same formal sense?

GS No, not at all. I'll give you an example. Before the National Mental Health Act, before I came to Washington University during the war and was frozen there, I was at Mass. General and Elliott Chapel, the anthropologist and I were interviewing a number of people who were at a naval base nearby, because it was at Squanton and I became interested in the fact that the interview, basic to psychiatry hardly ever had any systematic investigation. That's

GS cont. why I got interested in that part, watching some of the things that had to be done with those sailors, and I became familiar with the way in which, as a field anthropologist, he would find out how, say people in the culture unknwon to him spent their day, how they lived from moment to moment. And I found that an anthhropologist's way of looking comprehensively at the life of a person, his solitary activities, his activities with other people, was extraordinarily illuminating to me as I tried to apply it to our culture. So, at a particular point I began to interview some of the same people that Elliott Chapel was, and became interested in how you describe a person's day, a rather different framework from the psychoanalytic one in which I was also working at that time. And we then generated a project to describe a new way of taking a life history. We actually published an article some years Later, "A New Life History Form" and instructions for its use, which had to do with getting a description of a person's solitary activities, a person's biologic function and a person's interpersonal and social function, and looking for changes in those as indications of crisis situations, supposing for example a whole lot of activity drops out as when, say, some partner dies and so on, everything else changes and I began to learn how to use that device which I often used later on in clinical work. Well, I didn't ask for any support for doing that, I didn't write out any proposal for doing that. Elliott Chapel and I talked about our interest in this and we decided to experiment with various ways of constructing such a life history, then we wrote up the article and we submitted it to, as we were ready to submit it to the Society for Applied Anthropology in which it was ultimately published about three years later, but Feinsinger at the Mass. General blocked it because it was not a purely psychiatric article. And can you think that for three years he blocked it, there was no way of getting past him, until I had left Washington University and I didn't give a God damn what the hell he did. Then we got it published. It was published in 1945, I think it was shortly after the war was over. Shortly after that, once the war was over, I was freer to do various other kinds of things dealing with an accelerated medical program and so on, I wanted to pursue the study of the interview in a variety of ways and first Elliott and I developed some things to do and began to make some objective observations with a kind of a very simple interaction

GS cont. chronograph of the durations of speech and interruptions and things like that on patients and non-patients and studied people who were some of my own colleagues or residents like Sam Guzay as examples of non-patients and soon was able to establish that patients have much more restricted patterns of interaction when studied in some partially standardized way than non-patients. When they stop being patients sometimes that expands into interesting leads there and I was involved with people like Sam Guzay and Wells Goodrich and a number of others who participated with me, but none of this needed a proposal, none of this needed a grant. What we needed at one point was to be able to record some of it, we began to want to do some verbatim recordings before the days when magnetic tape recorders were cheap and easy. I remember there was a blue disc called an audiograph on an LP record on which we recorded an interview. That was the first thing that came in after Carl Rogers electro-mechanical recording was introduced, before real recorders appeared there was this audiograph disc, like a small LP record, that you could record things on through a microphone.

EAR That was even before the wire recorders?

GS Oh, yes, before the wire. Well, we needed some device like that and all told it probably would cost about $150 to $500 and the way I got that money was not by writing an application for funding. It never had occurred to me. I never requested anything. Years ago when I was working in physiology I never requested any funding. That was in a laboratory and we used what was there. When I needed this particular sum of money for this particular purpose it happened on a particular occasion that a family that was very grateful to me because of the way I had helped them with a member who had leukemia and was very apprehensive, I had worked with the hemotologist of Washington University called Carl Moore who made the diagnosis and was himself flabbergasted at how upset all the people in the family were and couldn't think he could tell the patient who was a very hypochondriacal man and asked me to help him with that situation, because he didn't know what to do. It was in the days before you talked about terminal illness. Well, the family was very grateful to me, and on one occasion the man, who was a millionaire apparently whom I had known said, I am very grateful to you, is there something that we could give to you or to the department

GS cont. that would help the work that you are doing, and at that time I said, yes, we need a recorder that would cost about $500 for which I don't have the money and the department doesn't have either, and that's how I got my first funded support. I had never had money from anybody else. I think what had happened up to that time was that whenever I thought of studying a question, I probably unconsciously took into account the limits of resources which I had and tried to structure the inquiry so that there would be a minimal of dependence on any outside resources and a maximal generalization possibility by posing the questions properly, but that was the way we used to do things before funding by government agencies got going. Now the first funding that I ever had from the NIMH came quite accidentally. The chairman of our department at Washington University Ed Gilday got a call one day from I don't know who saying that NIMH had a million dollars, I think it was at that time, it must have been the late 40s, of unspent money that could be used for research. It could be awarded if somebody would submit some kind of reasonably satisfactory proposal for investigation, it could be awarded on very short notice, and so Gilday asked a number of us. I had now made some progress in the interview research. There were a number of things that we wanted to do next on a some-what larger scale, so we could submit a proposal, but it was in a very simple form. The money was immediately available. There wasn't a year's waiting for it, now that never happened again. But that's the first time that I had ever made an application for funds. It was nothing like the process which we experienced later on. Now, to come back to the way that you first began to ask this about what were some of the other features about this interchange of people's thoughts who came from different fields, the best example I can think of that is analogous to what happened at NIMH regularly was this. Some years after I had been working at NIMH I was asked to be, I had been on the Advisory Committee of the Steering Committee for the Office of Naval Research and I had become better known on account of my being involved in these things than I had been before, I guess, I was asked to be a consultant to a big project at Michigan State, I'm not sure but what Margaret Mead had not recommended me for that too, we kept on knowing each other all these years, but what I was asked to be a consultant to was very illuminating. There was a project which they had

GS cont. attempted to set up there to study the various pressures on middle management in industry. So, sociologists were involved, mathematical statisticians, clinical psychologists, experimental psychologists, psychiatrists, I don't know if that was all of them. They found that they didn't understand each other's language. I forget now what got them interested in that particular project but apparently they had gotten some leads this was a good kind of person on whom a great many pressures may lie, like studying a head nurse who has a very complex germ in our society, but somehow they had decided to make this a multi-diciplinary study. They had, by the time I came to see them, spent two years trying to see if they could agree upon a common terminology. I came in at that particular time. Margaret Mead had already been there as a consultant to try to help them. They had apparently used consultants from different fields. Well, i t turned out that the last two or three consultants, Margaret Mead and I were among those three, we had made identical recommendations to them, namely, stop talking and begin to make some observations and find out what happens. Well that situation has some analogies to what happened regularly at the study section, where we had to come to decisions about a particular proposal. We had to learn to talk not in terms of, what shall I say, specialized jargons appropriate to our field, we had to use a mutually comprehensible language and it seems to me, I never heard any preaching about this, it probably was a necessity of the situation to which we were flexible , but that had a lot to do with making us, helping us to evaluate applicants proposals more fairly, contributing to our whole growth and our sureness about the decisions we made, I think, and in the light of our experience. It didn't happen in other settings. Imagine wasting two years trying to solve a problem which could be solved.....

EAR Right. Instead of talking abstractly they should have got themselves involved in the process, which is really what you're saying. You've just reminded me of something which frankly I hadn't thought about before. There's something curiously similar in the review procedure of applications to what constitutes the essentials of manufacturing a good game. Namely, you must begin with s omething that's mastered rather easily and quickly, but there is no limit, hopefully, to the complexity of the kinds of interchanges that take place in actually playing the game, so that bridge, for example, EAR cont. or chess. You can learn the rudiments fairly quickly so that you can begin to play fairly quickly, but you can play for a lifetime and never really exhaust the complexities of it, and in a sense, the review procedure has some of the same kind....

GS It has some kind of openness....

EAR Yes, you can learn to be a reviewer if you have the competence in the field, fairly quickly, but there is never a limit to how much effective interaction could take place in the actual review procedure...

GS And how much you could learn that adds to understanding in your own field, for instance, Strombeck became interested in the study of jury function and that was before it was recognized that things like privacy and confidentiality could have very important legal implications for a whole lot of perfectly good social and ethical reasons. Well, the discussions which one would have to have in the study section about these procedures meant that all of us who knew very little about the criminal justice system , though by that time I had had some group dynamics leadership experience with juvenile court judges and so on, which was a very feeble approximation of the thing Strombeck was going to deal with. We all had to learn, how do you study a situation like that? within some kind of principles of design that we understand from our own other kinds of experience without doing injustice to people, without doing people harm, and so on, how do you win the cooperation of judges? Well, there were so many new things for all of us that, as you say, the general rules of evaluating a project were mastered by all of us. Here was a totally new set of experiences which we had somehow to come to some conclusion about by sharing our minds.

EAR Right, and so there was a real intellectual challenge in the review of the applications.

GS Absolutely. That came up over and over again. Even though the study section had on it people who had tremendous breadth of knowledge in their fields, like, for example, Cronbach in psychological psychology and statistics would always amaze us with his erudition and so forth, we, as a totality couldn't possibly equal the sum of creativity, ingenuity and imaginativeness of the people out there whom we invited to send us ideas. We all learned that there was an awful lot of stuff out there, they lack a lot of things we had but there are things which they brought to our attention which were

GS cont. way outside of our ken up to that time. There was another beautiful thing of our experience, that is to say, that people were invited to submit ideas, instead of having to pass things through some kind of series of hierarchical gauntlets before an idea could be looked at, anybody could send anything, which is marvelous to know. We then had to make a judgment, we couldn't evade our responsibility about that.

EAR You mentioned a few moments ago the self corrective process, and I wonder if you have some illustrations, and what I am referring to is that, and let me just bring the staff into the picture for the moment. For those of us on staff, we sat around the room listening. I am not talking about the executive secretary who was immersed in the process himself, of course, but if I'd go in to sit in on a grant review session and if it was something that I knew something about the proposals, I would have an informal point of view of my own, this is a good applications and these guys ought to approve it, or this is a horrible one and what are they wasting all their time for, without meaning to say that my consideration was always accurate, what more often than not occurred was you'd listen to the interchange and on some occasions it was quick and cut and dried and you knew they were going in the right direction and that it was going to end up exactly as you had hoped it would and that was that. But there were other instances in which it seemed somehow that the discussion was oing off in left field, and oh my God, they're not going to do that with that application and then someone would make another comment and it would come back again and there was something in the interchange that was in a real sense self corrective in that, whether it was a John Benjamin willing to give money to B.F.Skinner or whoever, do you recall instances in which that same kind of development occurred?

GS Offhand not with the clarity that John Benjamin has, but what you mention reminds me of the important role that the chairman usually played, let's see who was the chairman before....it was John Marquis, I believe, I don't think Cronbach was ever chairman, was he? I think it was Marquis, but I'd had some kind of things to do with him. I think we were both, by the time I was asked to be chairman, I think we had both been together on the Advisory Committee of ONR and I developed a very high respect for his group skills, which was some= thing new for me. It was after I got on the study section and got

GS cont. to know people in other fields, like Ron Lipset, that he invited me to see what they were doing at Bethel, I read about them, I knew some of those things , I had read reprints and teaching medical students at Washington University. He said, do you know anything about what we're doing, and I said, no, sir, and he said, I think you'd be interested and he invited me to come out there. And I began to know something about the procedures which they used. Don Marquis had been there and he knew something about the procedures and so forth, and what I noticed was, and I tried to imitate was behavior such as the chairman's being very careful to pay attention to whether there was repetition of themes in the discussion and I learned after a while, I forget whether I got this directly from Marquis or in some casual conversation with somebody else, well, I began to say myself, I remember hearing myself say over and over again when I was in the chairman's spot, now look, we have said these things three times, that means nobody is really thinking anything new, we are now ready to vote. That was one way that the chairman could tie things together. Another way in which there would be a situation like one that you mention where something would be decided very quickly, it would be very apparent that there was agreement right away and I learned that it was possible to say, any disagreement, assuming that we knew what the agreement was and that would be true, but then, what that presupposes is a freedom of expression of view so that no significant view was left unattended to or unexpressed, that these expressions had been heard and a consensus had been accurately defined. We rarely heard a protest about that, because people would be perfectly able to say, look, I'm not ready to go on, I want more to say...Now, the business of wandering from point to point on apparently trivial issues, again I wish I remembered some concrete examples because there were a number of them, and they often had to do with the fact that a proposal, which, in and of itself, wasn't sufficiently attractive to win approval easily, nevertheless raised important issues. Maybe it was work in a field where hardly anything had been done and somebody was trying to make a beginning and we would be wrestling with, how can we possibly support, at least making a beginning here to set a pattern, perhaps for somebody else who would do better and maybe this person would be able to do better, and it was at times like that that a suggestion would come up over and over again

GS cont. for discussion which first had come from John Whitehorn which was, why doesn't the study section recognise that there's a ; lot of expertise within it and become a consultative body to people out in the field who are sending in applications and whose ideas are new. Often they could, with encouragement, do very much better. We would repeatedly consider that and repeatedly reject it on the grounds of a kind of conflict of interest, like the one which would have occurred had we had to dispose of the resources. I don't know if you remember some of those, but that would come up over and over again, Well, one of the main reasons for, what to somebody sitting on the side lines as you described staff members as doing, for what looked like a wandering discussion about an issue that didn't seem worth giving that amount of time to, often had involved such considerations as, isn't part of our obligation to encourage even a rudimentary beginning attack on a problem which everybody has neglected......What happened really, I suppose, was that in a group of that size where people, as diverse in background as all of us were, there would be some people who were more sensitive to the need for such encouragement and would make a strong case for it, which had not occurred to the others who might be tempted to throw out the procedure entirely on the grounds of inadequacy of a high level of design, or something like that,leaving out other values. And I thought that it was appropriate that these other values be heard, so to speak, so I had an idea that some of the circumlocutions and circumstantiality which we were talking about had to do with issues that probably we could, as a study section, have defended on devoting time because they bothered us. There were very rarely any situations like that which so disturbed anybody that there was a tremendous amount of, say, post-meeting talk on long distance phones, and so on on what bothered them. That was very rarely the case, apparently there was enough time to air these things, it seemed to me.

EAR Well, you remind me of something else, though. To what extent do you think the conviviality that developed, the sociability, the lasting friendships which developed from the study section par= ticipation obviously were a function of a partially mutual admiration society, and high respect. But I'm wandering from the point I want to make. You weren't always sitting in the formal review process, you had dinners together, had been on site visits together and after the meetings, evenings when you weren't in a study section, EAR cont.. small groups would get toether, there was a lot of sociability. Wasn't that also often addressed to the general topic, that is, people continued the discussions.....

GS Very often. That's right. There's no question about that at all. It was very rare that that study section group, say at a meeting at Bethesda, simply broke up into a series of isolated members who would go off separately to different places. Once in a while a couple of us would go to a meeting and the day ended especially exhausting, and say, well, we just have to stop this, but almost always there was some kind of discussion during sociability time afterward about some of the issues which we wanted to explore further, maybe to test our own judgment, and so on. That was pretty common. Let me come back to another point about the monitoring thing. One example that I remember involved the Franz Alexander project. Hans Lukas Toyber and I were asked to site visit a number of projects in Southern California, it probably was the first time I ever was out of here, and by the way a very interesting thing happened in that year, I think it was about 1958. There was a terrible plane accident of a place which was flying over the Rockies and crashed. That was the plane we had been supposed to go on and we couldn't get a place on it, we were very lucky, both of us would have been killed. Anyway, Hans Lukas and I, I don't think anybody else was on that trip, were asked to visit a number of places, one of them was all of UCLA and the Department of Psychiatry as it was at that time, Tom Lindsley was still in the Department, Norman Brill was the chairman and you may have haard some of the things that came back from that, that he had an intercommunication system on his desk, he wanted to hear everybody's conversation, it was an extraordinary situation. Lindsly was suffering terribly under this. Anyway, among the projects that were a part of our assignment was the Franz Alexander one. Hans and I, with enough experience toknow what to do, as soon as we got here we insisted on arranging with Franz Alexander or whoever was acting for him, I think it was Renecker, or something like that, we insisted on talking to the people who were in charge of the subdivisions of the project. That was one way of monitoring that occurred. If site visitors had enough experience to recognize that you ought not to have your meetings limited to the whole group present at one time with the big chief there or just listen to

GS cont. the chief alone, but if you talked to people at various levels you'd then find out a great many things which otherwise might surprise you. We found out immediately, for example, that one of the important parts of the project was under the responsibility of Heather Bogard who was a psychologist with some considerable reputation. It didn't take more than five minutes for us to find out that Franz had not consulted with Heather Bogard before he drew up the application. There was no give and take between them in an application that was very complicated, and a tremendous sum was involved, a lot of experimental studies were carried on and we were absolutely flabbergasted. It hadn't occured to us that that was possible. Well, we found that with a number of other parts of the project and it's this kind of monitoring. I guess this must have been done before there was final approval for the project or maybe at a time of request for extension or renewal, probably that was it,

EAR Yes, it was.

GS Well, again, the wisdom of people who did the site visiting, their experience and their ability with tactfulness to have access to the person they needed to have access to, to know how to obtain information and not upset everybody, that had a lot to do with the possibility of monitoring how wise your decision had been, and of course, when you had a couple of experiences like that you soon became much more sensitized to cues when you were considering the application. That was that kind of feedback from especially investigating applications up for renewal and applications about which there had been some doubt in the first place. This must have increased everybody's sensitiveness to what you read. It was that that I had implied. I don't remember more specific examples than that.

EAR Okay. Let me ask you to turn for a moment, unless there is something else, and please interrupt if you have some other thoughts on this, but you did spend time on the Psychiatry Training Committee and that was a somewhat different set of circumstances. Would you want to comment on that?

GS I don't know where to begin with that. At the time I was in that, or was chairman of it, it had multi-discipline membership, I don't know if it still does, so you had to be responsible for, let's say, GS cont. the anthropology component of a psychiatry program, if that was in their proposal, or a sociology component. At that time, in those early days of NIMH, so to speak, there was a lot of encouragement to expand the multi-disciplinary aspect of work on mental health which began to seem, to more and more of us, as we became familiar with the research that pointed to a multidimensional way of looking at all human problems. It was very natural to us. Well, by and large, the Training Committee, it seemed to me, adopted a number of very sensible policies, for example, if there was a new medical school or a new department of psychiatry, especially in a place where psychiatry hadn't had any reasonably adequate recognition, there was always a disposition to give a high priority to helping them get started. For example, was this done for me? no it wasn't done for me by NIMH when I came out to Oregon as the first full time chairman, but the Foundation's Fund for Research in Psychiatry, which had adopted a similar policy and as in some other things had had that policy subsequently followed by NIMH was in the habit of allowing $10,000 either in one lump sum or a couple of years to a new department to get started, and things like that. Well, the Training Committee, I thought, was very generous in helping new departments of psychiatry get started. The Training Committee became aware that in some instances there were empire builders who were creating programs adding whatever seemed at the moment to be possibly funded as a new development. Now there might suddenly be emphasis on a psychosomatic service in a place where there really weren't competent people to carry out that activity, at least nobody that we knew, or there would be a proposal for a sudden expansion of an anthropological component for no good reason that we could see. By and large, though, I guess we began to pay attention to the persons who came out of those programs, what was their subsequent history, where did they go, and at that time I don't think we had very good data, compared to what we've had since, about say, the amount of time the psychiatrists give to public services and things, we didn't know about that. But we paid such attention as we could through site visit information and through raising questions which were suggested to us on the applications about what the general goals were of these programs, how effective they were in reaching their goals, and what happened to the people they trained, most of us, I think, were rather disappointed that at least at the

GS cont. beginning of the work of the Training Committee a very high percentage of the people who benefitted from HEW stipends went into private practice and really did not do any community work. We wrestled with that problem from time to time, but I don't think that at any time was there any suggestion to do anything coercive about that. We thought an educational approach would be better for them, so far as one can think that.

EAR Well, I guess I didn't make the point clear that I was referring to, and that is that there was a different tone in the Training Committee interaction than there was in the Study Section because you weren't dealing with scientific merit....

GS And we weren't learning as much from these.....I guess now that you've mentioned it, a way of looking at that is that in that committee, we were more in the position of allocating resources and that's a different kind of decision, it seemed to me. Although merit considerations entered into it, they weren't of quite the same level. It's hard to describe why that's so, but it isn't quite the same.

EAR Well, it was not an intellectual process in the way the review committee for the study section was.....

GS It evolved rather different things, like deciding does the country, does this part of the country stand more in need of supporting a new psychiatry department, it was a different kind of decision.

EAR I haven't done my homework well enough, because I really should know the exact years you were on the Training Committee....

GS I don't remember that.

EAR It was in the mid 60s, I'm sure, I think it was in the late 60s. And I don't even recall , and I should know this too, who was on the committee with you. I should have tried to check on that.

GS But mustn't those records be available?

EAR Yes, they are.

GS I wouldn't trust my memory. Have you ever heard the famous Chinese aphorism what goes back to the 5th century BC, it runs something like "The palest ink is better than the best memory". So get some pale ink for it. I often haven't put this down in Curriculum vitae, I forget all about it. I never paid attention.

EAR Which reminds me, I would like to have a copy of a CV, if you have one available in your office. I'll drop you a note. But I do have what I did get from NIMH when I first began this was a complete

EAR cont. list of all of the National Advisory Health Council minutes from year 1 up through 1971, and of course there are a lot of other records that I still have to look at. Let me turn for the moment then, do you recall the Woolrich Committee and some of the background development and do you know why you were asked to serve on that?

GS No, that I don't know.

EAR I can't answer that. I just wondered whether anything had come out. I don't think it was Margaret Mead again.....

GS No, I don't know, but my memory of some of the sequence of events was that Representative Fountain had become very critical of money being wastedat NIMH, and then the general question was raised, should we have anything like this going on at all? And then the attempt was made to find a group of persons who had not all been associated with NIMH actually, to look at what they had done and initially they had hoped to get many people who had nothing whatever to do with NIMH, who had never received any grants or support, they found that was impossible. We had to go to Mars, I said at one of those meetings, to find that, and then the assumption that persons who had benefitted from some activity would have zero impartiality I thought was really a very ridiculous assumption, so we ended up really with a committee that had people like General Doolittle on it and a fellow who was the Chancellor at the University of Utah, he was a computer expert and a statistics expert who helped create the kinds of samples that we had, I can't remember his name, he was a very interesting fellow with a lot of mathematical and statistical training, And the Woolrich Commission had to decide on do you use just a random sample by using a table of random numbers and on which project you examine, obviously that wouldn't have done justice to some tremendously large cooperative projects like the perinatal study project involving 16 collaborating institutions as well as small ones and so forth, there was a national collaborated diabetes project, so they tried to pick an adequate sample, that was what this fellow did, of large projects, small projects, collaborative projects, more to investigate the kinds of projects which had different kinds of objectives with regard to their breadth, whether they were dealing with long range studies or immediate targets of some kind or other, and scattered over the country.

Gs cont. It was a very complicated kind of procedure and then teams were created which included people who were both administrators, had financial experience, I forget whether there was somebody with legal experience and people from various fields. I was simply asked to be the chairman of the Behavioral Sciences part of it and I could have various people working with me, like Mickey Stunkert, and so on, and the site visits were absolutely fascinating. You'd go to a place like the University of Maryland, you were close to that, and you'd find that over the period through World War 11 their student body had expanded by God knows how many thousands percent, but their administrative staff was numerically absolutely exactly the same number, their typewriters were the same God Damn typewriters, and you had to go through the same years of struggle to get one new typewriter. They were totally disarticulated, these various components of the university from each other. But, as you know, in general the conclusions of the commission were that Fountain's idea, let's abolish the whole thing, it's totally corrupt, you know, a boondoggle, was simply not justified. A lot of people on the commission who were upset by Fountain's allocations, could they possibly be true, came up with the conclusion that there was a very small percentage of unwise decisions that had been made and unwise investments of government money. That was a fascinating experience.

EAR Yes, and you know there were a number of others. That was precisely to explore the whole of NIH, it was beyond NIMH, it was the whole of NIH. But, I should tell you, if you didn't know it then, or you've forgotten, that this whole involvement with Fountain began with a very curious approach from our standpoint to the grant process per se. Jim Shannon, originally, as the head of NIH, and he voiced this, had the philosophy that essentially a grant was a gift to an investigator, who was found by a group of his peers to have the qualities that would make likely the success of his project, nothing guaranteed of course, but the major emphasis was, that by having been reviewed by a group of his peers he was now given this money to conduct this research. If it came out well, fine, and if it didn't, it was nobody's fault. But with that philosophy, as a gift, it then meant that once he had the money, there was no longer a responsibility on his part or on the government's part to insure that he did exactly what he said he was going to do, exactly the

<u>EAR cont.</u> way he had described it. Fountain came along and said to Shannon, now, wait a minute, these are government funds, the man signs a grant application, it is not a gift, it is a contract between the government and an investigator, and as a contract, he now has taken certain legal obligations to himself, which means, okey, that he doesn't have to buy three dozen test tubes if he says he's going to buy three dozen test tubes, he can buy two dozen, but he may not make a dramatic or drastic change, he must recognize that he has signed this contract and therefore is legally responsible to pursue the work essentially as he has described it, and it is no longer the kind of relationship that you, Dr. Shannon, have described it to be. Well, that difference really made a very very dramatic contrast, so from that point, once he won that argument, and unfortunately he did, ...

GS Did he win it in Congress?

EAR Yes, he won it in Congress, there was nothing passed, but I mean he forced NIH to accept the point of view that they had now established a series of contracts with people, not gifts, and from then on, which was in the late 1950s when he first began this with Delphis Goldberg it was in a sense downhill all the way from our standpoint, in regard to the way the whole grant process worked.

GS Yes, I think there was a natural extension, which you may not have thought about in this connection. It happened when LBJ was president, the notion that what the hell have we gotten out of all this money for research, where are your cures for Cancer, Heart Disease, the notion that that's the way science works is simply an extension of this idiotic kind of notion.

EAR Exactly. You had to document that you had used the money well, by the results.

GS As an example of how silly is that kind of thing to a scientific worker, I can tell you that I spent a year and a half trying to, when I was a physiologist, trying to duplicate the findings of a fellow named Jacobs in Hemotology at Pennsylvania, which I found hard to believe. I duplicated them then and then abandoned that line of research. Well, if I had received a government grant years later, that was before those things, and had come to that conclusion, how could I help but change what I was doing? If you are not allowed to do that, that's when corruption gets in, that's when applications begin to be submitted for experiments which are

GS cont. proposed to be done, which already had been done and the outcome is now as falsification. That's a beautiful example of the response to governmental regulations. What was the date of that?

EAR In the very late 1950s is when that occurred. He called Shannon before him. And that's an interesting story in and of itself, which I think is again one of the things that I'm trying so hard to do. It seems to me that the story of NIMH is in some measure the cumulative effect of the happy circumstances like the Peer Review Committee, the calibre of people that came, the reputation that NIMH developed over time, the need that was fulfilled, because in fact there was a tremendous need to catch up, to do things in the field, Shannon's viewpoint and of course, not in any way insignificant the tremendous support from Lister Hill and from John Fogarty, the chairmen respectively of the two House Sub-Committees, and then, I think, the fortunate fact that we had people like Bob Felix and other people at points of high responsibility to do the kind of extraordinary job that they did. But with all of that, at each point a critical decision or a critical turn of events, good, bad or indifferent, was the result of some series of circumstances that you almost couldn't predict would take place, accidental or what, I mean, why did Fountain come along when he did, why did he have a man named Delphis Goldberg, who was his assistant, a man of extraordinary ability to really get in there and dig and try to do the kind of job. I dont want to say that a history is accidental, but I think it's important to illuminate those kinds of events to point out....

GS I would put that in a slightly different way. I think that these accidents occur but the way I thought about these things, because this is not a unique example in the history of institutions that have been successful and then disappeared, probably even of civilizations, for all I know, I think that the fact that an institution in our society anyway is successful, and the fact that it works well and so on, has very little to do with its survival. That's the heart of the problem. There is no guarantee that the merit of an institution would have anything to do with its survival.

EAR Well, there's one more point, George, and I'm sure you've thought about this too. It seems to me, it's a forced analogy to some extent, but organizations go through a life cycle, to some extent, the way people do, and I think that the NIMH early years on, the

EAR cont. first twenty five years were extraardinarily successful, and then you get to a point where its inevitably downhill, and I think that's what we're seeing now, unfortunately.

GS That's the way it's been.

EAR Well, are there any other aspects of your involvement with NIMH that come to mind in view of what we've said up till now that you want to put on the record?

GS Well, the only thing I can think of is a negative thing, that I would no more think now of submitting an application to NIMH than flying like Daedalus to the moon on a spaceship. I just wouldn't go through the trauma. For example, I'm at the VA now, after I retired from my job in Oregon, and I don't have to really get any tremendous resources that the VA can't supply. Not that I need money from them, I don't need that either, to carry on studies which I have been long been interested in, the criteria of competence of residents or medical students. I'm fascinated by how you define those things without becoming over-bureaucratic about it. I believe in a kind of a minimal perturbation appraoch to life, both clinically and in education. What are the minimal indicators you need to tell you that if those are present things will be alright, without your sticking your nose in everything, you know. Well, I'm inivted from time to time to participate in something at the VA, to submit a proposal, get some money for the hospital, absolutely zero. I will have nothing whatever to do with the procedures as they have now become over-regulated. There's a daily experience with that almost in most medical schools and hospitals through the activities of the Joint Commission on Accreditation in Hospitals. There was a recent visit, not to our hospital, at ours we have another one coming up I guess in the fall sometime, there was a visit to Brentwood and when they have a visit we send one of the people from our department over to participate in the final summing up, to get an idea, what are they making the point that Brentwood was deficient in that we could correct if we knew about it, but to find that a competent person whom we considered for Greenblatt's successor as Chief of Psychiatry at Sepulveda, but rejected, hoping to find even somebody more competent, is paying attention to whether there is an adequate number of soap bars in the toilets, this is an indication of how I look at applying for support in a new that now is necessary. I wouldn't even dream of it. It's too burdensome. I prefer to do

<u>GS cont.</u> things totally different. I guess there'e another thing that I've concluded, and that is, that by and large the very large scale projects which at times have seemed to be necessary, such as the cooperative study in diabetes, or cooperative which I have heard about through the Woolrich Commission, the cooperative perinatal study, I have learned, I guess there was another one, the chemotherapy study of the Cancer Institute, those are extremely difficult to carry out when you use the example of how difficult it is to have a small staff work together, as that group at Michigan State I am increasingly skeptical of projects the larger they are. I have an idea we have got to think in rather a different way and it can be a very productive way from the viewpoint of generalization of what you learn and extrapolate step by step. I'm interested in that because I'm interested in how our democratic process doesn't do that. For example, if you wanted to change the welfare system or the social security system, or the pension system or the retirement system, to me it seems the way to do it, since the numbers of people involved and the numbers of issues are so large, you would start with a pilot, here and there you would set up two pilots, or three pilots, some very small number, in which different dimensions are being tested, and then you'd compare them before you decide on a national policy in a country with a 200 million population. Well, I think it's possible for organizations like those within HEW to make the mistake of trying to work on a scale that is bigger than we have the wisdom to do. Right now in the accountability area, that's very clearly exemplified. There's a tremendous pressure for doctors, say, to be accountable, there may very well be accountable lawyers too. According to Supreme Court Justice Berger, you know, the lawyers are not so hot either. And there's a lot of discussion, I think there is something over thirty states which are getting ready to say that you have to be recertified once you've got a licence in some professional field, whether it's law or psychology or medicine, they're talking about that, but all of those expectations involve available measurements measures of something called competence but the development of implements to do that is so far behind, the whole field of looking at criteria of competence dates only from about the 1960s in a serious way. We learned that by my setting up an education evaluation seminar to help us in what I want to do, but the field is really that recent. So here is

GS cont. another example of a public policy that has been decided upon way ahead of the ability to implement it. Well I think some institutions within government fall into the same pattern. Although at one time, like many young people growing up when I did, around World War I there was a notion that if you reorganize the government, lots of problems would be solved. One becomes less and less convinced that that's really possible. The USSR is such a horrible example of the convert.

EAR No, and I think you mentioned some time ago interdisciplinary and multidisciplinary and that effort to produce large interdisciplinary or multidisciplinary programs as almost always failing. It was so difficult to bring those people together.

GS It doesn't grow that way, does it?

EAR And maybe in some way that one curious successful example from another field has given us undue expectations, and that is the Manhattan Project in World War II, where a crash effort to do something had such a spectacularly successful result that people think you can do this anywhere. It's just obviously not so.

GS That's a very interesting question in itself that a single example of success may be extrapolated way beyond what makes any success. A good example is in biology and medicine. The acute infectious disease model, somebody gets sick, has a symptom of measles, you know there's a causative agent and if you could put those two things together then there's a regular treatment, then you'll be successful or you can prevent it by means of vaccination, then you assume that those are the simple dimensions of the problem. But then when you're faced with a couple of additional facts you suddenly realize that that can't be so. For instance, how do you fit that simple model to these facts? 1) In the United States among infants the mortality from disease is one in 100,000, in tropical Africa in infancy the mortality from disease is 12,000 in 100,000. So how does a simple biological model take into account, there must be totally different factors, like widespread malnutrition, the absense of health services, so even in what looks like the simplest model that's uni-dimensional there's a kind of under-dimensionalizing of man's problems which is extremely dangerous and destructive and certainly non-predictive. I think we suffer from that all across the board. I don't know what else to say about....

EAR And that gets you back to Franz Alexander. That's where Franz Alexander's project probably went off the beam.

GS Absolutely. He was trying something way beyond what anybody had the capacity....

EAR Well, George, I really appreciate your taking the time to do this and I really wanted to get you into this because of the fact that I really had the feeling that you had gotten me started on this, so when it eventually occurs you can remember that you started somehow............

NLM NOTE: Interview tape ends abruptly here