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GE Why don't I begin Eli by ^{stating the different} ~~saying they settled into the~~ kinds of relationships that I've had with NIMH. I think selfishly the most significant one has been the role of recipient, and that began very early in my career. In fact my first stay at the center, in 1955-56 I wrote an application for five years of support for essentially thematic apperception test research and that application which was submitted to the center was funded and and did then carry me through that five years, and then I've forgotten the details of how many more years I continued on that line but some time during the time ~~of~~ that grant was in action, I submitted , I've forgotten whether it was for five or seven years, by that time it had moved up, but another long term application roughly comparable in size, maybe a little bit larger, for research in the general area of behavior genetics mouse research, early experience and adult temperament set of related problems. And support from NIMH for that research continued right up until the time that I became inactive in the laboratory. I think it's safe to say that approximately from age 35 until somewhere close to 50, when ~~I~~ stopped data collection I was being supported very substantially by NIMH and I had other sources of support, I was getting money from other sources too, but I would say that certainly the most continuous and probably the largest in dollar amount and the most significant in terms of its dependability and the freedom in terms of which I operated and so on, was the NIMH support. But to back up a little bit, prior to coming to the center, I had spent all of my career, first as a graduate student, and then a number of years as a faculty member at Harvard, these were days when most of us were not really accustomed to large amounts of research money. Research was done without any money or with small grants from universities, in my case I collaborated extensively with Harry Murray, who had Rockefeller money and so on, so there was some money that dribbled down from Harry and I also had money from the Laboratory of Social Relations which, I believe, primarily Carnegie support, and they would get me probably \$2500 a year, or something like that. And given the way in which I operated at that time, that seemed fully adequate. I used undergraduate honors theses, graduate students who were working on dissertations, so the whole thing seemed a perfectly natural way to operate and I felt no strong impulse to seek fundinn

GL cont. outside. Harvard probably was less professional and less sophisticated in regard to outside funding than most institutions. I think that if I had been in a Big 10 institution for example that I would have begun to seek external funding much more rapidly. I can remember that during this period that George Klein was a visitor at Harvard and George and I became very close friends and I can remember at that time George urging me to apply to NIMH. My recollection of George as one of the early grantees, and he was of course very happy with his relationship with NIMH and couldn't quite understand why I was so dumb as to not try to establish a similar relationship. In any event, there was literally no external funding that I received. I may have gotten, there was a period when the Ford Foundation gave out \$5000 chunks without application. They just gave you the money and said use it any way you want. I got one of those and I've forgotten now whether it was prior to coming to the center or at about that time. So I guess the general point that I am making here is that I had a very happy relationship with NIMH as a direct beneficiary. During that period of support I was never turned down, I was never site visited, and by and large everything just went very pleasantly. I couldn't ask for a more permissive and a more encouraging environment in which to have my own personal research supported. Now the other forms of support that I was involved in are somewhat more indirect. They were institutional, involving in all cases some kind of training grant. Now again this was a very significant part of the teaching training function that I played, commencing particularly at the time that I left the Center. When I was at Harvard, actually I had forgotten that up until this moment, I was director ^{of} _{had} the clinical program for a period of about two to four years, we/an NIMH training grant but I never really looked upon it as mine. I ^{was} some the initiator of it, there were a lot of things wrong with the way in which the program was administered, some of it I was able to adjust, others I wasn't. For example, one thing that they did was to, initially, because the NIMH statements were issued late in the game, they were unwilling to use these as a basis for support for incoming students until the statement of award came out. That meant that all the good students had been given support from other sources and what you had left, not already supported, were the worst students in the class, so that time the traineeships were

GL cont. the best sources for what we had, or almost the best, for we may have had one more or two fellowships that were as good, or better. They were better than almost anything, and of course the major alternative was some kind of teaching fellowship which involved a lot of time on the part of the recipient. So there was this crazy inverse relationship between the goodness of the award and the quality of the student who was receiving it, and they also had a rule that required that once a student received the award, then he was guaranteed to receive that from then on. Well, those things I wasn't able to change, but in any event I never felt that that was really my program because I had initiated it and there were a lot of things in the whole damn setting there that I had no control over. When I left here I was at Syracuse for one year and essentially set up a clinical program there. They had had sort of an unsatisfactory not accredited not supported clinical program, and in the one year that I was there I was able to formalize the program, secure APA accreditation of it, and get NIMH support for it. This obviously made an enormous difference in terms of whatever has happened to that program. I don't really know what has happened to the program. From there I went to Minnesota where, again, we had NIMH training support and where the clinical program was a much better clinical program than either of the previous ones and where I played an essential part in the program but never was responsible for directing it. Then when I went to Texas, the problem was somewhat like Syracuse in that I think NIMH support may have been withdrawn. The clinical program was on probation and we were again able to modify the program and make it structurally better, get a lot of new people in, cut down the number of students and again secured NIMH training for the clinical program and then I was briefly Director of the program, simply by default, because there wasn't a director, but I think that lasted no more than two years, maybe only one year, but I was also Department Chairman, and as Department Chairman I set about encouraging all the other programs, really more than encouraging but writing with the director a program of proposal. Then we ended up with something like six different NIMH training programs which formed the core of support for our graduate students. We've had virtually no sure fellowships, that is, the number of university fellowships that were available were so small a number as to be insignificant, so it was really the NIMH trainee-

GL cnt. ships on the one hand and the teaching assistantships on the other and the NIMH support being much superior. But I came to the center, and as you mentioned, NIMH has provided support for the center for a number of years. They provided roughly \$100,000 a year in institutional support because it was particularly valuable for obvious reasons here. We could use it to fill out all those checks which otherwise are very difficult to take care of. Institutional support was wiped out. I think I was able to get one more year. I get a little confused. By then NIMH was going through similar phases, though slightly different and maybe one or two years apart in terms of which the time at which the final change or evolution occurred, but when the institutional support disappeared I was able, working primarily with Fred ElMajen but getting advice from other people, to work out an institutional training grant application that was funded for three years, we're in second year of that, and which provides, I think the first year, the total amount, direct and indirect, was roughly 150, most of it in stipends, and the first year was funded about 100 but then the second year was funded the full amount, and the third year, I guess we're in the second year of that so we have one more year to go. Again, it's been very helpful but of course cumbersome as hell. You've got to have NRSA forms. If I compare the bureaucratic rigmarole with the easy process of the 50s and the 60s, why it's very frustrating, but if I compare it with the absence of \$150,000 of course it's a very satisfactory thing. So I guess that describes fairly completely my role of beneficiary and as you can see it played a very significant part of my own professional career, both personal and the department's programs and institutions that I've been associated with.

EAR Let me ask you one question about that Gardner, before you go on to the other side of it. During all this time did you have any thoughts of, over and above the repeat of the funding, of any aspect of being an NIMH recipient per se as carrying with it something worthwhile, or was it primarily the availability of the time to do things that could be done with that kind of money?

GL Well, let me try to answer that in a guarded way. From very early in my life I looked upon individuals who were able to secure funding through external sources as representing some better level of performance than their peers who were unable to or chose not to

GL cont. demonstrate this capacity. Beyond that it occurred to me early in the game that there were different kinds of money and that some money was better than others. In my world, and I'm talking now of my world as defined by the period when I was not in administration. Not that my attitudes changed, but when I was vice-president, which I was off and on for another year, I just had a lot more contact with funding sources that I would not have had contact with as a psychologist. I guess the general point I would make is that within psychology NIMH and NSF were clearly the two funding sources that, and we're talking now about federal funding, private funding is sort of a whole different picture, but in federal funding those were the two sources where, given all the imperfections of any selection system, I think people, certainly me included, looked upon the giving of a grant as an indication that the individual's work was significant and worthy of support in the judgment of his peers, so it was like a large article being accepted for publication in a prestigious journal. It had been examined carefully by a number of different individuals who said that this was work that should be supported. So I would say, definitely a cliché....(cachet ?)

EAR In contrast, for example, to something like the VA....

GL Yes. The VA is, I think, an obvious case in point. The VA, both in terms of my direct contacts with them, I spent God knows how many painful hours as a consultant in VA hospitals. It left me with an abiding distaste for not only the VA but I think with a terrible distaste for cafeterias. I'm inclined to trace it to those VA cafeterias where I had to have lunch.

EAR This is a very interesting point. I began with the VA and I really think, at the very beginning, immediately after WW11 that the VA had an incredible opportunity to do something. And Jim Miller was there, for example, first on and Murray was the one that got Miller that job. They had an unusual opportunity, really to make a tremendous impact. They had more money than NIMH, they were there before NIMH was there, and yet over the years there's such a clear contrast between the two.

GL VA got progressively worse, as you know.

EAR Yes, of course, and so it's not just, I guess again my own positive feeling about NIMH, it's not just the availability of money per se but the manner in which the money was used, the manner in which the program was developed and the outside external additional kind of

EAR cont. attitude about the organization, above and beyond. I think the two were in some ways inextricably interwoven as we say in the bureaucracy and without the money of the NIMH obviously we could not have had a very positive reputation, but it was not money alone but that's what happened with the VA. Early on it hadn't had much, certainly in terms of competency.....

GL The Office of Education is also an obvious comparison. Everything that you said about the VA can be said about the Office of Education. Looking at the Office of Education grants right up to the time that I left my last vice-presidency, you didn't consider that a faculty member was necessarily meritorious, in fact there might have been a little bit of minus if he was getting all of this support from the Office of Education. One other thing I did forget to mention was certainly a significant benefit from NIMH was the special fellowship that I received the second year that I was at the center as a fellow. Actually, I think they provided my full support, it was of very significant benefit.

EAR OK, but I do want you to say something about your other role.

GL There are really, I suppose, two other roles. One is a set of personal relationships with individuals either currently or previously at NIMH and my ties to the organization really began at the time that Phil Sapir was Director of Extramural Research. My ties had always been closer to the research branch than to the training branch and I must say if I wanted to respond to your invitation to be critical, that I would be critical of the training branch rather than the research branch. By that I don't mean that I am fully critical. Most of the objections that I have, most of the things that were done that I think were wrong, would be things that were done within the training program rather than within the research program...

EAR Beginning with this one little aspect of the tying in of the grants so that the trainees were not.....

GL Yeah. And as a matter of fact, just to be sure that I don't forget to say it, I would say that I felt that in recent years, I don't think that I felt this at all in the early years, but that in recent years, particularly with the general mood in the direction of the proposed community mental health, that training programs pushing our clinical psychology programs in a direction that I happened to personally disagree with, but whether I agreed with it or

GL cont. disagreed with it, I felt that they used too much muscle in trying to get the universities to do what they considered a good thing.

EAR I want to come back to that later because I think it's an important point to have here.

GL So my relationship with the research side of NIMH first was completely anonymous. It was by mail. In fact I've never forgotten. Somehow or other in correspondence with Lou Winkowski, and this was that first grant application that I made, and I guess Lou was Phil's assistant director, somehow my secretary made a mistake and the first letter was sent by her was sent to Miss Louisa Winkowski. Having once made the mistake, of course, the damn thing just kept getting repeated and I don't know how many other letters were sent until I finally discovered that there was no Miss Louisa Winkowski. But it seems not to have poisoned our relationship but it sure made me very memorable in Lou's eyes. But it was really Phil and Lou, and subsequently Betty Pickett that I had the most contact with. As a study section member I very reluctantly agreed to join, probably in the late 50s, I have to refresh my memory, the old psychopharm group which then became a study section, initially it had some special status, I don't remember what it was called...58 and I really didn't want to do it because it wasn't an area of any competence on my part, or special interest, but the various people who were involved (EAR - Sherm Ross) Well, I knew Sherm, but Sherm was not the one, I think it was Marty Katz who was sort of the executive secretary equivalent. Marty was the one who was really saying that he didn't want someone who was a specialist in the area, he wanted someone who was a general psychologist and I spent about six years on the group or something like that. I think I joined originally for just a year and then they extended the term and it turned out to be a very rewarding experience. In fact a large number of people who have turned out to be long term friends were people that I first met in that group, people like Danny Friedman, Lou Resania, Roy Brinker, a whole host of people outside of psychology. The psychologists I pretty well knew before, Dan Schacter and Howard Hunt, people of that sort. In fact, Peter Dood is another one. These people have tended to recur in my professional life in a large part because we developed that kind of an intellectual and personal compatibility, so that when I recently agreed with much

GL cont. greater reluctance to take on the chairmanship of an Academy committee in subsequent years because Dick Solomon had a coronary and resigned and that committee was left in a state of disarray and when I joined the committee, I got Stan Schachter, Peter Dews Lou Lasagna. I said to my wife when you look at the names of the people you think you're assembling the same old gang.

EAR The old boys club. Let me ask you a question about that. I don't want to divert you from what you were saying, but it is very important because the psychopharm program is still another dimension of NIMH and I had forgotten that you were involved almost from the very beginning. But as you well know when it began in 57 or 58 with considerable pressure from Congress to do something in this area that included perhaps euphemistically more money was available initially than might well be spent. And yet I think in retrospect that was a terribly important pump priming operation. With that obvious bias in my comment, what were your early recollections, what were your early thoughts about the quality of the work that was being done, about the long range kinds of objectives of the program, and how did you see it in those early days? Could you comment on that.

GL Yeah, well I'd be glad to. I would agree that it was an important pump priming operation and the fact that it turned out so beneficially I would consider a consequence of the confrontations, rather painful interaction of the staff on the one hand and the study section on the other. The staff worked very hard to generate proposals that were the best proposals that could be generated. The study section took the attitude which I consider proper, they usually didn't go to extremes, but obviously there were some individuals that would go beyond what I would consider a reasonable stance but the study section essentially said that they were judging the scientific merit of the proposal and if the best work being done in a given area was no good, then it wasn't to be supported. I know it must have been terribly frustrating to Jonathan Cole and Marty Katz and the others, who, no doubt, spent many hours encouraging an application, assisting the person to prepare the best statement he could of what he was doing and then the study section would look at it and say, no dice. Occasionally, they could work their way around the study section, that was pretty damn rare, by and large, the study section, unlike NSF would control

GL cont. the decision, with very few exceptions, so that I think the pump priming in the end was very effective. Money was there, it was an important scientific interception and there were already a small core of people who were working in the area and doing very good work. There were other people, who, with slight adjustments, could work within that broad framework and really not deviate from their major scientific intent. Then, of course, there were students being trained, so that the net result was that in over no more than a decade they moved from rather an obscure field into a field where there was a tremendous amount of very important work going on and the upgrading in the quality of the work was very . It wasn't just just psychologists learning about pharmacology and basic biology. It certainly was also pharmacologists learning about behavioral assay was more than a measure of toxicity. So I certainly felt that it was a rather difficult process, one that turned out well in the end.

EAR I know that this is terribly difficult to do in retrospect, but could you recall an incident or two which would hopefully illuminate this kind of interaction between staff and study section around one or another project where there was a kind of struggle, as you just pointed out on the part of the staff to get something approved on the part of the study section to maintain the standards that it felt were important? Did any one application in your recollection kind of illuminate that?

GL Well, there are some cases that I remember rather clearly but I am not sure how heavy the staff involvement would be, but we did have a recurrent problem with Nate Kline. Nate came in at practically every meeting with a couple of million dollar proposal and he would leave the room and we would vote it down. I know this was embarrassing to the staff, I don't know the extent to which they participated in encouraging..Nate didn't need any encouragement of course, I'm sure the staff would have been happy to see some kind of support given to him and of course he always managed to get some kind of support but he never got it from us. It was always through some contract or some other study section. The most amusing case that I can remember of a staff study section disjunction involved an application, I think it was Merton Gill, but I wouldn't swear to that. It wasn't psychopharm. It ended up in psychopharm because we had on the study section a number of

GL cont. psychiatrists and a couple of psychologists who knew something about psychoanalysis and it was a proposal to pull together Rappaport's unpublished work and essentially put out a volume that would make available that which would not always be available. I was the first reporter on it and gave what I thought was a balanced view of Rappaport's contributions, shortcomings and said that of course this wasn't pure research, but it seemed to me that it was significant and advised, it wasn't a very large sum of money, I think it was Gill's salary for a year, or something like that and recommended its approval with high priority. The next reporter was Roy Drinker and of course Roy tore into the proposal and psychoanalysis and Rappaport, Gill and we had a long, very heated discussion of this. The thing which I don't think the staff fully appreciated was that in that study section, although there were several psychoanalysts, I was the only one who really had any sympathy for psychoanalysis and certainly _____ was very negative towards psychoanalysis, and in the end the proposal was voted down, I think, 14 to 1 or something like that. At the council meeting the staff managed to get that proposal through so that at the next meeting of the study section I can remember being rather embarrassed at the fact that the 14 people who voted against the proposal had all been overruled and my lousy one vote was the one that ended up predicting the outcome. That really was not a typical case. It didn't belong in that study section and was not in the...the problem was that it didn't belong in any study section and they were looking for a home for it. There were a number of cases that involved, but by now I can't remember who the individuals were, but my impression of Hollister.....where the application would be for a very routine drug scheme and the study section would take the point of view that this was just not work of any scientific merit. It was not going to advance what we know about behavior and what we know about drugs and I'm sure in most cases that the staff had generated the proposal and I suspect that in many cases they would probably have been handled through a contract mechanism.....

EAR What was your feeling about Jonathan Cole's role in those days? Did the study section, and you in particular, have any feelings about the way he was doing things?

GL Well, I suppose all of us felt that he was more strongly motivated to spend the money than we felt was appropriate. In retrospect, I

GL cont. would say that that was probably his role and he was fulfilling his role and he was always good natured about being turned down. I suspect it's also true that the study section felt in general that while Jonathan was an amiable well-informed person, that he probably wasn't the same calibre as the members of the study section, which given that particular study section, I guess would be true. It was a very high calibre group. I don't think it was any personal animosity towards Jonathan.

EAR No, I wasn't thinking of that. I think it would be helpful, because again, it's a good illustration of one of the programs which began literally de novo at NIMH. In a number of others, perhaps not as visible, suicidology, for example, is a case in point, and I am going down to see Ed Shneidman next week, but I think that the fact that it really had its birth at NIMH and that now it plays the role it does in the total spectrum of behavioral sciences contribution to the field I think is something that deserves some considerable place in the total picture and Jonathan being the one who was there at the beginning, I think, has to be placed in that context. You point out something, I want to label it so to speak and then perhaps you can comment on it. There was a kind of push and pull relationship between the study section and staff, and if you want to say that in an organizational setup, that kind of constructive tension between the consultants and staff is good, that certainly is a very appropriate point of view, that would be one way of looking at it. And another would be, if perhaps there were, and there may well have been, some kind of informal interaction between some members of the study section and some members of the staff which played its role in covert ways during the actual operation of the study section and you just described a case in which inadvertently I would say you were coopted with the Rappaport thing. It just so happened that being the first reporter, having some positive feeling about it, wanting to present it in its best light, that in a sense you were exactly the right person to be asked to do that. Now do you have any knowledge of whether you were specifically...

GL Oh, I feel confident that that was the case.

EAR Okey now. So there was some kind of behind the scenes maneuvering on the part of staff which took place. It was not unusual in more cases than not individuals were selected with some expectation that they might perform in this way. Now, it is equally true that in the

EAR cont. interaction in the study section itself there were 14 people who in a sense voted against it. What I am trying to say, and I would want you to comment for or against this interpretation, that that was a very good way of trying to come to a balanced point of view on these kinds of things. Do you feel that way?

GL Oh yes. I think that the choices that are made by the staff in terms of particularly the first reporter, but then also the other people who are given some responsibility, obviously play a very important role in terms of outcome, but they don't determine the outcome. One of the things that I learned out of those years in the study section, which I think may have come rather late in the game, came from Peter Dews, who I considered a close friend and somebody I have a boundless admiration for. Peter was never wrong, was always sound, he has strong scientific biases but they're all certainly out in the open, so I look upon him as really an admirable colleague. At one point, after one of these meetings I had said, and I think it was late in the time that I was in the understudy section, and by that time Peter and I had become good friends. We were sitting in one of those dreary motels drinking, and I was complaining about somebody in the study section, there were a number of them who would be asked to report, and they would have their papers turned around, and they would say, well let's see now, and they would read something from the report that we had all read and sort of free associate a little bit and they'd waffle...well, you're losing your mind while they'd get to the point, and sometimes they never did get to the point. They didn't say anything. After bitching about this particular performance Peter said, well, that is a bad performance and in many ways is a model of how not to go about presenting. But you know there's a danger in the way you present, he said. You come in, he said, and I had always come in with my thing typed out. I didn't read it but I had it all typed out and began with a factual summary of the major points of the study, and then there was a discussion of what I saw as the major strengths of the study, the major weaknesses of the study, then a brief attempt to pull it together in a synthetic statement and then a recommendation for a specific action. Peter said, well the way you present those cases, you frequently eliminate discussion. You point to all the strengths that people can identify, you point to all the weaknesses that people could identify. You then added it

GL cont. up and come out with a decision, and unless the person has spent an awful lot of time on the proposal, he's not going to be able to differ with your recommendation. So you shut the case down before it was ever even opened up. It's very efficient, but not necessarily the best way to arrive at truth. And I had to agree with him. Upon reflection I had to agree that although I would much rather listen to somebody make that kind of presentation, that the likelihood of the recommendation being affected by the subsequent discussion becomes quite small, whereas this guy who fumbles all over the place really leaves the decision mostly in the hands of ^{position} the discussion group, because he doesn't have a well articulated/result. It certainly was a significant bit of education.

EAR On one aspect of this very point, how do you think that the study section.....well, let me put it another way. What, in effect, were the dynamics as you viewed them in the study section? Granted that there are people of high intellectual ability, of clear professional competence, whether it was directly in the area or not, as you point out was your case early on, and that they come for a variety of complicated reasons, motivated to participate, and I think the intellectual interaction is inevitably stimulating. As I've heard from a number of people, and if it's wrong, please contradict me, that's its a stimulating situation to be in. It's intellectually stimulating to listen to Peter Dews on the one hand, or for other people who listen to you, for example, were there any other aspects that come to mind of the dynamics of the situation as you saw it in this kind of interaction.

GL Well, obviously there are various kinds of coalition formation that go on in all of these groups. There are personal styles. Some people's responses can be predicted in the study section setting with almost 100% accuracy. You just know where they are going to stand on every single application. In some cases because they are going to say no to everything. I suppose from the point of view of the intellectual gain of the study section members, putting aside what you learn from reading the proposals, I think it only enhances the interaction within the meeting, that falls into two categories. One category being the formal meeting in which the discussion takes place around a particular proposal, and sometimes, as you know, the study sections would, in fact, leave a proposal and get into a substantive issue and talk about the issue independent of any

GL cont. proposal. And then the other form of exchange which was largely limited, I suppose, to those members who drank, which included most everybody on the study section and which would take place in bars, or usually your own hotel room where you did your drinking and it was in effect settings where you learned more about other disciplines, etc. Well, I was just saying that the quality of what went on obviously varied with the study section. I should go on to mention that the other two groups that I had a formal affiliation with at NIMH were the program project committee and the advisory committee on research which met periodically. The advisory committee on research was I think altogether not successful even though it had some intelligent pleasant people on it, I'm sure I enjoyed one or two meetings. It didn't meet very often as I recall.

EAR If we could stop, because that is an important point. There were a number of such committees which had on it people who had achieved some prominence and prestige and appropriate so, and had something important to say, and yet somehow there was one example. Do you think it was the agenda, the objective itself, the fact that it wasn't a group, could you say in retrospect.....

GL I just think it lacks specificity. It was not a clear mandate that the group could respond to. ~~It seems to me that if the group had~~ been given a set of specific questions to deal with, if it had been asked to establish priorities within some set of alternatives, if it had had anything that had a tangible quality to it, it might have served a useful function, but to bring together a group that I'd say largely relatively visible well established people, most of whom took themselves rather seriously and to expect them to, somehow or other, cohere on some set of unknown in advance recommendations dealing with any topic that had to do with research within NIMH is unduly optimistic.

EAR Okey. So this reaffirms then I don't think this was done in any purposeful sense but serendipitously it reaffirms the constructive attributes of the study section format, in that you have a clear responsibility and yet around that responsibility you could often discuss, as you said a moment ago, larger substantive issues which illuminated attributes of the program operation itself and at the same time served the immediate need of providing guidance on individual projects. It wasn't set up that way, and yet I think

EAR cont. it was an important aspect.

GL I certainly would agree that having a moving agenda, which is what a study section has, which in fact samples a very wide variety of research issues, and being asked to make concrete decisions with regard to these specific proposals often leads to a very highly intellectual exchange. Given a carte blanche to talk about priorities, we have this in this Sam Glaser clinic at the present time, establishing priorities for mental health research, that's nonsense. There's no way in which any small group, or any large group, is going to be able to achieve something that will be useful. To continue the comparative notion, I would say that the program project committee in comparison with the psychopharm committee was substantially less rewarding, Although it included again a number of...I've forgotten exactly who was on that committee, but I'm sure was on it. There was some overlap with the psychopharm group and there were some people I knew from other settings, by and large it was a congenial group and I liked them and thought well of them, but the difference between that group and the psychopharmacology study section, which I would take as illustrative of the study sections dealing with project proposals, we were dealing with these damn things of a million dollars plus, the net result was that first of all, it was very difficult to have a discussion among these, because it had so many arms and legs and appendages going out and if you started to talk about any part of it in much detail, you didn't have enough time to talk about the other parts. Another, I think, kind of satisfactory aspect of those discussions and those proposals was that there was always some strength and always some weakness. Maybe there were some that didn't have any strength. But certainly there was never one that didn't have any weakness. And the enormous amount of the outcomes determined by what one individual group member might do to one part of that proposal, so you could zero in on that and tear it to shreds and make it very hard for anyone to make a case for supporting the whole thing because if there's one part of it that's really rotten, why then it's sort of questionable as to whether you should support the whole thing. I think it was generally shared between all of us on the group that the experience was less rewarding than dealing with project proposals and I think we all had the feeling that the sense of return for dollar was less than it was for the project

GL cont. proposal. I say this with a lot of reservations, because

I think that block grants or large grants are essential to institutions. The old NSF traineeships, where they would give a block to the university and then the university would disperse them within the specified disciplines were just of enormous value in terms of operating a graduate school and for NSF to have gone in and done in molecular fashion the task of selecting, case by case, the individuals to receive that would have been not only inefficient but would have interfered with the operation of the university. So I am in general in favor of the large grant but I have to say that the esthetics of the people who are involved in making the decision is much less positive than in the case of the project proposals. And that's of course one of the reasons in my judgment why large grants have a very hard time in peer review, whether its NSF or NIMH or whatever. By and large the peer review system is most comfortable when they are dealing with a tangible product which is not too much larger than the scale in which they're used to operate with. There are some exceptions, when you get into the big machines in the hard sciences, and so on. So I would say, if I were to rank order the three groups in terms of what I got from them and how I felt about their outcome, that I would certainly say that psychopharm was the best, program projects was not nearly as good but better than the advisory committee which really had a very brief existence. It was tenuous.

EAR Did you attend more than three or four meetings?

GL No. My memory is about that.

EAR I remember one you were at. I was there at the time as I recall. One of the early ones that Lou called. This was a meeting that Lou called, not that I held.

GL Yeah. That was Lou's. And I know Lou was very unhappy about it and I think, and I consider Lou to be a good friend, but I think Lou on occasion felt that I had behaved badly because, in effect, I was saying, well what are we supposed to do? Give us an indication of what the nature of the advice is and what our responsibilities are.

EAR Did you never think what the secret agenda, or the hidden agenda, might have been that day?

- GL Oh, I expected I'd hear about it. I don't recall that I had any clear conclusion as to what it was. However, at that time various tensions within the NIMH that I only knew about very imperfectly.
- EAR Well, I'll tell you that one of the hopes was that he would serve as an important buffer between the program operation and the National Advisory Mental Health Council.
- GL That was one of the things that I had suspected which....
- EAR By this maneuver Lou had a very prestigious group which hopefully then could provide him with a series of recommendations about program operations that he could then use to counter perhaps something that might come up at the Council, perhaps he would provide the structure that might not have occurred at the council meeting because there weren't that many people in a concentrated fashion who were directly interested in the research program. Although I should tell you that when it was whether like someone like Josh Lederburg or someone else was on the council, obviously the research program was seen as the most important one. Some people felt the training program was more important because they had responsibilities and interest in training and education, rather than research. But clearly the scientific clout and the scientific prestige was inevitably in the research program. So that was one of the reasons the research didn't work.
- GL Well, it didn't work, as we both agree'
- EAR And I think you touch on a very important aspect. It's very difficult to talk globally, even though everyone there would like to, in a sense. I don't think the objective of the group was at issue, it was the fact that you couldn't grab at this damn thing very effectively. Okey, any other aspect of your involvement with NIMH that perhaps we haven't touched on that you want to comment on.
- GL I don't think so. I haven't emphasized the personal relationships but they certainly have been significant. Phil, in his bristling way, he is a difficult man, and that when I've known him for a long time. If I hadn't known him for along time, why I'd find him impossible to interact with. It gets complicated when I get to the study section because there are a lot of people that I know very well that at some time were study section directors. But of the people above the exec. secretary level, my primary contacts were with Phil, Lou and Betty and I would stay in touch with all of them, no longer in a very functional way but I do have a little

GL cont. attentional, kind of, and they developed with Betty and NIA although in a very minor , mostly, I'd just see them and talk about what was going on....

EAR You had had some interaction with Bert Booth, of course?

GL Oh, yes. I had forgotten Bert. I knew Bert quite well. I was never, of course, part of the program, but I knew Bert, in fact we were close here at the center the year that he died. Of course I knew Bert Brown quite well, but we were not close. In fact, if I had to describe our relationship, it was ambivalent, not hostile. In a sense, I almost feel sorry for Bert although I didn't agree with most of the things that he wanted to do.

EAR And you hadn't known Stan very well.

GL No, I'd say that my contact with Stan was quite minimal and the same thing was true with Bob Felix. I knew him but never had any extensive dealings with him/

EAR Well, it's an interesting aspect of the whole program that these three people are in many ways so very different, and in some ways so very similar. The similarity may be partly a function of the responsibility, the differences are certainly a function of their essentially different personalities. But one of my own biases in this, having worked very closely with all three of them, is that in a curious and perhaps fortuitous way I think that each of them was there at the right time for their respective strengths in terms of administration. Felix, a very happy, a very jovial, a very friendly, a very cordial, a very corny guy who really had the ability to stimulate people to do their best, and who was not at all hesitant about putting people into positions, even though on the face of it, they might not be the appropriate individual, witness Phil Sapir who had no degree, and yet was given a position and did it admirably....

GL He not only had no degree but his style was one that would lead the casual observer to think he was the last person you'd want for the job.

EAR But he did an absolutely superb job. And other people. I was the first non-psychiatrist to be given responsibility of the training program. With Bob - okey. Stan was the one who made me assistant director but this is another aspect of NIMH. With all the traditional, conventional kinds of constraints in terms of a medical model, there were many instances in which the organization was able

EAR cont. to transcend that kind of tradition and put people into

responsibilities where supposedly they should do more the kind of work that they were suited to do, and I think in most instances it worked very well. Stan came along, a totally different individual from Bob, certainly not as friendly and certainly not as corny, unable to interact in that kind of way and yet absolutely a superb administrator, who took over the Institute at a time when it became too large for someone like Bob to be comfortable in..And Bert had his own style... One last point, if I could just ask you perhaps you may have said it already are there any other aspects of NIMH that are worth your mentioning in contrast to some of the other organizations? You alluded to the fact that for example the study section operation at NSF is a totally different entity than it was...

GL Well, it's no longer that different. I'll admit it surely is changing, and I had some role in that. There was an Academy committee that I was a member of that looked at the social sciences within NSF and made some very specific recommendations in regard to the relationships between program officers and peer review analysts that were very much along the lines of removing from the program officer that very high degree of autonomy.

EAR This is of relatively recent vintage.

GL Relatively recent, yes. It's really just been going on for the last year, and is still sort of in process, but they are now under much heavier constraint to deviate from the recommendation. First of all, the old NSF pattern was to try to avoid even getting a vote, they just wanted discussion and then the program officer would decide, well he probably had decided already, and I objected to that in principle, not saying that in many cases the program officer wasn't an excellent person, although certainly in the heyday of NIMH I would say that that the behavioral scientists on the staff were substantially superior to the equivalent staff at NSF. This is something that is undergoing some adjustment. Dick Loutitt, of course, is one of the key figures at NSF in the behavioral sciences. But in any event the general tendency is to make the program officer accountable to his panel. If he deviates from their recommendation he tells them about it and tries to justify it. I think it's a very healthy thing. I don't think the program officer should be bound, it's about like the department chairman, I don't think the depart-

GL cont. ment chairman should be bound in his recommendations by what the vote in the department is, but I think he should be accountable to the department and should inform them of what his recommendation to the dean is and why he made the recommendation.

EAR Well, that's interesting. I didn't realize that. I still am trying to get a last word in this. Anything else about NIMH, especially of a negative aspect that ought to be put into the record?

GL I mentioned the fact that I thought that the training grants, particularly in recent years, by recent I guess I'm going back to the point when they became individuals who were strongly committed to moving away from a particular bolder model of clinical psychology, which is fine, as long as you're talking about a multiplicity of models, but individuals who are convinced that that particular model shouldn't be abandoned and that community psychology or community mental health is really the only model, and it seemed clear to me that in least some instances that they were representatives from NIMH and in some cases staff. It's hard to say where the responsibility is because you have both staff and consultants involved in the site visit and so on. But where they were pushing very heavily on one particular set of convictions on how this kind of graduate training should take place I considered that unhealthy. I don't think that particularly an institution that had as much as the training money in this particular area in their pocket should be in a position to say that this is the form of training that we are going to support, and we are not going to support anything else. I think that's really my major objection to a lot of general negative things that happened at NIMH, and you know more about it than I do, and all of which is related to changes that are going on in other agencies too. The increased amount of time in filling out forms, the increased amount of concern that you have to devote to issues which are essentially unrelated to anything having to do with the quality of research and the quality of training. As for that, I don't look upon that as a criticism of NIMH....

EAR As you know, in an interesting way it really begins when Congressman Fountain forced Jim Shannon to say that a grant was not a gift, but a contract, and it was as simple as that because I think that in the early days the atmosphere was that if you

EAR cont. find a good person, you give him some money and give him his head. When it was shifted from that to a contract, rather than a gift then all of us started to.....Well, really I appreciate your taking the time Gardner. You've been very helpful and let me get this thing closed as I do by trying to relieve any discomfort you may have that this is just one very small part of a total jigsaw puzzle and I'm trying to put the whole thing together, so if it sounded somewhat disjointed to you and if you wonder how it fits in, it will. I feel it's terribly important to me to get all of this input.....