

ALBERTA SIEGEL Well, let's see. I went on the study section for the Research Career Development Awards which had many different names at that time and later, informally, and so forth and at the time I went on the study section they were no longer giving those life-time awards. Those had just recently been terminated, for new applications, but they were giving renewals and new awards to psychiatrists and to basic scientists in the mental health field. And it was at that time a very interesting and a very high morale committee and I gather has remained that way, but I don't know it of my own knowledge. But the meetings were like a seminar. The person who was presenting an application would not only tell you about the applicant, about what the applicant proposed to do but would also give you a history of that field and where this person's application fit into the history of that field and why it seemed promising or not. It was very interesting to go to, and the quality of the people on the study section was extremely high.

EAR And that was a fairly large study section, as study sections go. Was that more than a dozen?

AS I would guess it was about a dozen. I could try to remember...

EAR I have a vague recollection that it was a little more than that, perhaps not.

AS They did an awful lot of site visiting, by today's standards and so it may have been larger than other study sections just for that reason. Every applicant that they were seriously interested in would be routinely site visited before there was any final consideration, every new applicant and.....

EAR Who presided? Was Bert at the first meeting?

AS Bert was at the first meeting. The first meeting I went to was at Estes Park and you remember Bert used to have his annual retreat of the investigators at that park and he would bring the new study section members on for an observation meeting, and the outgoing person that they were replacing was also there, and I gather I was replacing Joe Stone. Betty Pickett was still the executive secretary at that time, and she left very shortly after that and then Mary Haworth came on. But of course those meetings were very high morale meetings because the investigators would appear and they would talk about what they were doing and present progress reports, and they were on their best behavior and then the committee members were on their best behavior because they had to look scholarly to the investigators.

EAR Was that your first contact with Bert?

AS I had met him once before. My husband and I went to Washington to investigate some possibilities for funding for something my husband was doing, I don't remember what it was, and we went to the NIMH, I think it was the day after Kennedy's inauguration and somebody steered us to Bert Booth, who helped my husband with his question, I don't recall what it was and then said, I want to tell you about this new program that we're getting under way and told us about the research career program and he obviously was quite excited about it. Now, that's my memory. Would those dates be right, 62? (EAR - right) and then I don't recall meeting him again until I went on that study section. Actually, when I was at the center, I think I was on an NIMH fellowship and he may have been the administrator of it. It wasn't a research career award. I think it may have been a one year award.

EAR Yeah. We had special arrangements for NIMH funding which Ralph had very cleverly maneuvered when he had been on the council for support at NIMH for both the center in block grant plus some of it in a number of fellowships for support of individuals at the center. I don't know whether Bert was involved in this or not.

AS I don't know either, but it seems to me, certainly his signature was familiar to me. There was some set of documents, so that when I met him I said, so this is Dr. Booth. It was not an unknown name to me. But I hadn't really known much about him until I got on that study section. He was a guy that took a while to get to know, I thought. A very proud man, a very intellectual man and of course that program was his baby and the people in it, he felt a very deep commitment to and the exigencies, the funding, the program and that kind of stuff, it bothered him because one of his people might lose their momentum and if he heard that a department chairman had resigned, the first person he would think of was his awardée, is my awardee going to suffer under that resignation, or benefit. You know that joke about the woman in New York who hears about a bus that goes off the bridge and thirty people have been killed and she says, were there any Jews, and they say, yes, as a matter of fact, it looks like one of them was a Jew, and she says, oh, the poor fellow. That's the way Bert was about his program. He'd hear about this big deal and what he would think about is what was it doing to his people and he went on a lot of

AS cont. the site visits. Now that I'm more of his age group and aware of how strenuous travel is, I appreciate more of what he was doing in going on all those site visits and I think people, some of the less egocentric ones actually appreciated what he was doing for them. I think a lot of the more egocentric ones never had a clue, really as to how much emotional support and financial support and administrative support and interference running and everything else he was doing for them. Some of them, a guy like Fred Meljus for example appreciated.

EAR I think Bert was probably one of the formost examples of dedication to the program that he was involved in. Fred Elmagen is the same type. In some respects, Fred even more than Bert, because Fred really feels that that program is his program. He was the one that initiated the program whereas when Bert came on, Phil had kind of started that program already, you know, the career investigator program, but very quickly it became and remained Bert's program.

AS I think that it made an amazing contribution to psychiatry and to medical schools. I can't speak as well for what its contribution was to other aspects of university life because I just don't have as much first hand knowledge, but from the point of view of medical schools, they've been building up a cadre of sophisticated investigators that was very important.

EAR I think what would be very helpful, Alberta, is if you would say a little bit more about not just the Estes Park thing, if you want to comment any more on that first meeting, your thoughts and reactions to that in retrospect now, but I am very concerned about being able to make an adequate presentation of the multiplicity of contributions the whole study section phenomenon provided to the growth and development of the field of mental health, way over and above the most immediate and obvious contribution of a very high level evaluation process for weeding out good from general applications.....is there a number of other very important contributions that it's made, and you just touched on one, that is for the members themselves it was a really enlightening experience it was a kind of a seminar situation. It was, and remains, I guess to some extent a communication process for dissemination of information nationwide, if only for that select group that was there, but then it rippled out, because you'd come home and talk with other people appropriately, so that I really want to make sure

EAR cont. that I appropriately characterize the role of the study section in this whole picture. I think I may have told you that my book really began, now that I think about it in retrospect, as a result of a casual remark that George Sazlow made to me many many years ago when he said to me, "Eli, you have to write a novel some time about the study section, because the dynamics of the study section meeting and everything that goes on and just the ramifications of that whole process warrant a construction that would reveal to other people who haven't had the opportunity to be involved in it what it really is all about. And while it may be a little excessive to think of it in terms of a novel, it is certainly true, and I've had it now repeated time and time again that the people who have sat in on the study sections see that as one of the high points of their professional career. The intellectual entertained, the feelings of satisfaction of doing ~~this~~ sort of thing, the rewards after the fact, the personal and professional growth that takes place, the friends that you make, you know all of that, so from your standpoint, I think it would be helpful to get your comments along that line.

AS Well, one thing is that you meet in an uninterrupted block of time and you're abstracted out of your daily life and you're just living in a hotel someplace and so all of your usual conflicting concerns just fall away, and during that period you devote yourself to that one task at hand. And there is a kind of a norm, arriving promptly, staying as long as possible, nobody expects every single person to stay right to the end but there is certainly a lot of group pressure on people to meet their obligations, not to be hustling in and out and making phone calls, and all that stuff. There's a kind of a seriousness about the effort and there's an awareness that it takes a lot to get this kind of busy people together in one place and I think that makes for an intensity of the interaction. There also is a custom of listening to people without interrupting them, saving your questions until they've had a chance to say their piece, etc. all of which means that the intellectual life can flower and all the circumstances favor the fact that you're making a recommendation about a significant amount of money, tends to focus your concerns and keep it from becoming just idle conversation and small talk. The other thing is, that in a university, you spend most of your time with your students and you don't spend

AS cont. a lot of time with your peers, and when you do, its in administrative roles. You're deciding about how to run a class, or something like that. You're not really talking about your shared intellectual concerns, so that actually this kind of opportunity is not common. One would hope that it would be more common. And I think as what you say, what goes on over coffee and over dinner is an awful lot of passing on of gossip, high level gossip, maybe not so high level, but administrative news, news about people's jobs, but also a lot of people will talk about their own work over dinner and pass on what they're doing now, what their students are doing now, so that kind of communication gets accomplished as well. The particular need that the NIMH was trying to deal with was getting communication going and understanding developed between psychiatrists and other mental health practitioners and basic scientists and this, as you know, is a serious problem and it continues, but a member of a study section has an opportunity to really develop a serious understanding of what people in other fields are doing and not just an understanding of what their clinical contribution might be in some day-to-day clinical operation. So that generally if you meet a psychiatrist who served on a study section, unless he's really stupid, he has had a serious exposure to behavior scientists and he has heard behavioral sciences discussed not by some trainee in his program who had a couple of years graduate work someplace, but by a serious working behavioral scientist, so that one function that the study section system met was to train up a whole generation of psychiatric statesmen who now understood what sociologists really do and what anthropologists really do and what psychologists really do, and so forth. But I think also train up a whole generation of behavioral science statesmen who appreciated what the psychiatrists were trying to do, and who appreciated that they weren't all of the ilk of one's local friend or practitioner, that there is within psychiatry a serious group of scholars who are trying to advance the scientists of the country. But I've also served on a study section for the NICHD and I had the feeling that it was less successful in doing the same job for Pediatrics than the NIMH study sections I've been, and I've never quite understood why, but I didn't see the movement and the growth on the part of the basic scientists and the clinicians as much in that setting as I did with the Research Career Awards. I think part of the reason was

AS cont. that when I was on the NICHD study section we were often discussing whole training programs and with the other one, the Research Career Awards, we were discussing a particular investigator and his problem and we would get into that problem in considerable depth, so that it was more substantial.

EAR That's a very important point because I think that one of the accidental strengths of the study section process and noone would have anticipated it, is that in the course of looking in depth at the particular grant application, or in your case, a particular proposal, for an individual to get involved in in an area where some considerable part of his future professional life, that the intense examination of that project or that person, might then evolve into a discussion of the areas of larger context, and you might then get into a discussion of substantive issues which in the abstract would get you nowhere, but which as it derives from a particular application in a particular project, in a particular way, generates importantly into the total substantive area. And Gardner Lindzey said to me yesterday, and I think it was a very telling point, that he was on the first psychopharm committee, he was on it for six years. On psychopharm especially, in the late 1950s and early 1960s they were very much concerned with the growth and development of the total program, but doing it from a standpoint of individual applications, application by application, gave a lot more meaning to the substantive discussions than if they were to talk about them in the abstract, and when he went from the psychopharm committee then to the program project committee, which as you know was concerned with major grant applications, in the hundreds of thousands of dollars, if not millions of dollars, it became more difficult to make this transition. Then he served on Lou Wyncowski's Research Advisory Committee which was deliberately intended to be just policy and large substantive issues, they got nowhere, they got nowhere at all because it was too vague, you couldn't talk about it in the abstract without the substrate of actual operation. And I don't think we anticipated that at the study section, but I want you to talk about it, what I said is not intended to supplant what you say. Here's another serendipitous kind of way in which that study section served so well....

AS It's interesting, because I've always been involved in so-called interdisciplinary efforts. We have a program in human biology

AS cont. at Stanford, and I've worked on that. Of course, this department is an interdisciplinary department and the Society for Research in Child Development is an interdisciplinary society and I'm active in that and we have a program at Stanford called Values, Technology and Society and I was active in that, etc. and I think that that's exactly right, if you can get an interdisciplinary group to focus on a problem you can get a productive dialogue going. If you're just focusing on a topic for discussion for the purpose of exploiting that topic of discussion you get no place. And they get into the worst kinds of quibbles about scientific methods and levels of data analysis and rigor, and all that stuff. For example, people who have very strongly held views about scientific rigor, and so forth, if they're confronted by a research problem that is new and exciting and the guy isn't doing it the way he ought to do it, they will never say, stop,. I mean, they will almost never say stop. They will never articulate their viewpoint that of course, how you perceive depends on how new the problem is and how well developed. You know, they don't articulate that as an abstract viewpoint, but their judgment is sound in discussing that particular issue. People are much more sensible when they're talking about particular concrete problems. The other thing, this study section I'm on now, the executive secretary has told me about my reviews, don't concentrate so much on the people, concentrate more on the problem. The one I'm on now is called Social Problems at NIMH, no it's not called that anymore, it's called Applied Developmental Research, or something like that. It's Terry Leviton's and she's under Ed Flynn. There are two study sections within his program and one of them is adult's social problems and one of them is children's, and I'm on the latter and it's called Juvenile Research, or Applied Developmental Problems, I forget what it's called, because it's changed since I've gotten on it. She said, when she was discussing with me the critiques that I had written the last time around, I think you don't need to spend so much time on the individual personnel, more time on the problem and design, and that kind of stuff. I said, the reason I spent so much time on the personnel is that I think that's going to determine the success of the program, the quality of the people in it. And she said, well, you know, the other members of the study section don't necessarily see it that way. And I said, well

AS cont. they can be educated. By the time I'm through, they will.

But, she said, I know you used to be on Research Career Awards and of course, there, that was the specific focus, was the promise of the person, and here we're looking more at the design, and all that. So I think we certainly had permission among ourselves because of our focus to talk about things that we were actually rather good at sizing up, since we were all behavioral scientists and psychiatrists, we were rather good at sizing up people's promise and potential, as we thought we were, and I think one of the actual reasons that we enjoyed it so much was that we were permitted to put that into the equation. And also a factor in the equation was what kind of a leader the department chairman was, and would he provide a stimulating and support environment for the guy or would he manipulate him and manipulate the funds and so forth. That kind of stuff could come out and I think in this study I'm in now, it remains implicit, and everybody knows it and they're thinking it and they're using their priorities school scores to reflect that they think but cannot put it out on the table. But we could put it out in our study. I think one of the measures of the success of a committee is whether people become personal friends and in that study section we all became personal friends, we exchanged Xmas cards, if somebody was in town they'd call you for dinner and that kind of thing. It doesn't always happen with committees but it happened with that one.

EAR Who was the chairman when (it was first formed?) when you first came pm?
 AS Bunny Haggard was chairman for a while. Bob Wallerstein was chairman for a while. Morris Stein was chairman when I first came on. As I remember, those were the two, and I think, would Greg Whorten have been the chairman? He may have been the chairman at that first meeting I went to, that he was going off. And David Hamburg had been the chairman a year or two before, so I heard about it from him and I'd met Betty Pickett through him, and I guess it was probably during David's time that Joe Stone was brought on and as I say, I was meant to replace Joe Stone, but I know David always admired Joe Stone and I rather think that that was what the connection was. One thing about the study section system is that you have an informal way of communicating about who are the good judges of character and about who are the good judges of ideas, and so forth, that as I watch the way the NSF works where there is a

AS cont. guy like an executive secretary and he looks at a proposal and he mails it out around the country, gets back mail responses and then makes a judgment as to how to do it. So much of that depends on his judgment as to where to send it. You write back to an NSF guy and you say, I'm not sure that I'm the right one to critique this, etc., and if you say that on a study section, you say I was assigned as the primary reviewer of this and I'm not sure I'm the right one to critique it, I actually think that Bill knows more about this thing than I do, than the dynamics are that the group will turn to Bill and they'll say, well, did you have a chance to look at this and what do you think and the executive secretary has had her knuckles rapped in a very nice way and has been told you know, you're not paying attention about how to assign these things and presumably won't make the same mistake again, whereas I don't see how the NSF system ever gets back any feedback.

EAR There's no self correcting.I don't know if we ever discussed this in the past, we may or may not, but I should tell you that having sat in on many committee meetings I, for one, as a member of staff, would often have the feeling that I knew which way the decision should go, you know, obviously, what's the purpose of being there, and sometimes you'd sit there and you'd have, because you quickly delete all that was going on, you'd have a sinking feeling that it was starting to go in the wrong way, and my gosh, what are they going to do, don't they understand this thing well enough to go the right way, and then someone else would make a comment and slowly it would shift, you know, and would move in the right direction and then it was almost like watching a runner, as if this is the person who should win the race and we're in the third quarter, and still not winning, and eventually more often than not, the committee would make the right decision. But what I am attempting to describe is the sequence of process that took place in the discussion which was not an unimportant aspect of this interaction which took place and somehow however/^{the}complicated dynamics are, much more often than not, the easy ones are obvious. There are applications that are going to get a No. 1 priority and that's where it was going to be, and there were applications that were clearly going to get disapproved, and that was the end of that. But it was the large bulk in the middle where the decision had to be made in some complicated way by this group dynamics and

EAR cont. it worked.,.....

AS We would make decisions to either approve or disapprove, and if approved, we'd put a priority score on it. Then at the next meeting we'd see the group's average priority score from the previous meeting, or maybe we'd see it on the things..... but at some point it would be fed back to us. Now that's no longer done. People still do the same voting but there's no longer feedback to the study section members as to what the joint priority score is. I think that's too bad because it was a kind of a feedback on the discussion. Sometimes you would present an applicant that you were very enthusiastic about, there wouldn't be a lot of discussion, everybody would vote and we'd go on the next one and you'd think, gee, I wonder what the hell..did they buy it or are they too polite to disagree with a lady? What happened? And then you'd get the priority score, the average, back and you'd see , gee, it was 1.50, 1.30, whatever, so you'd know that the silence represented concurrence, especially if you got hurried and there was a lot of work to be done, sometime there wouldn't be enough of the process that you're talking about for you to really know whether your point of view had been carried and then that numerical feedback was very useful, and sometimes it would be just the opposite. They would vote approval, and then you would see the numerical scores and realize that you really hadn't convinced anybody. I don't know. I've seen it go ways that I thought were bad, but on the average, what you say is right. We used to have one psychoanalyst on our study section who could never make a decision on anything. He was such an obsessive and he would tell you all the good points and all the bad points, and you would wonder, where was he going to come down, what's the bottom line? And eventually you would realize that there is no bottom line. He cant make up his mind, that's why he's in the line of work he's in, maybe. And then other members of the group would then fill in around him and after he had done his obsessing, which was all very scholarly and intelligent, but didn't lead anyplace, somebody else would step in and say, well, I agree with what you said as to this that and the other, or, and this other person would draw the conclusion for him, and he would look relieved and smile, but you could the degree of process that was specifically being used by these people to make up for his inadequacies. If ever there was a

AS cont. sense that the person was rivalrous toward the applicant, and was putting him down because it made his own work look not so good, that would always be picked up by members of the group in a constructive way, and they would say, well, it's really interesting to hear you talk about it because, of course, his work is so close to your own that it must be especially interesting.....and the whole group would be reminded, watch out, this is on tender ground with this guy and would listen to it accordingly. But it was always done in a kind way. But you do see a lot of group process and you would also see the executive secretary looking worried that it wasn't going to happen and you never really know what the exec. secretary has said in advance to the applicant and whether she has made encouraging sounds or discouraging sounds. But sometimes you could sort of see it on her face that she thought it was a better application than the group thought, and she was dismayed by the criticisms and you're almost wondering if she's thinking to herself, I never should have said xyz. And of course, the other way around. Applicants who don't look so promising to one person, somebody who knows the field better may be able to see how good they look. The feeling on that study section was a great deal of warmth towards the investigators, not rivalry with them. There was pride. It was more a father-son kind of thing, mother-daughter.

EAR It's hard to pinpoint. I think you've touched on a very important aspect of this whole thing I really think is an illuminating phenomenon, the fact that a bloc of time was set aside, you don't get involved in anything else, and the group pressure kind of pushes all the members to stay for as long as they can and not allowing outside intrusions to take place, I think all of that is very facilitating, and yet, if anything, it doesn't completely explain the very high level of dedication that everybody saw. Maybe it was self-regenerative, that is that a committee once got started in that manner, as most committees did, high esprit de corps, lasting friendships, that's a very common phenomenon, lasting friendships. Gardner Lindzey says all his drinking buddies began in some of these study section meetings, and yet, I really don't know, what else is there, personal prestige at being on the committee, I think, inevitably had that feeling, that you were selected so that you had to conform, in one sense. Important responsibilities, in your case, the careers of people, literally, much more so than in

EAR cont. a research project. If a research project gets turned down you make another research proposal. But if you get turned down for a Research Career Award, that's closing a war, in some respects. So I think you have an even greater sense of responsibility for your decisions than perhaps some of the other study sections. Is there anything else that comes to mind that helps, that very positive ambiance, there's a very very clear involvement.

AS Well, I think is that people were not paid for their time. To me that works to heighten the sense of commitment that obviously nobody was doing this for the money. You know, I go to those meetings of that outfit in New York that we both know about, and I wonder, because they paid so heavily, \$500 to come to a meeting and would put you up in a good hotel and I'd think, what are all these people doing here, wasting their time with this nonsense, and then I'd think it is a quick way to pull down \$500 and get a trip to New York. But Washington, that wasn't the case. They would pay very little, they would not put you up in a fancy hotel, they were expected to toss in 50 cents to get lukewarm coffee and store bought cookies and so there was a kind of a sense that nobody was here doing this for the money. There are other dedications. And another thing I think is, you know, you sit off in Gainesville, or Chapel Hill or Palo Alto, or whatever, and you read about Washington, and there's always we-they stuff, there's moral indignation about all these bureaucrats in Washington ripping us off and all this and then to actually become part of that system as a citizen, you feel like this is my chance to have an input in my government and to conduct myself in the way that I think all of Washington bureaucrats and citizens ought to conduct themselves. I think you had the feeling you were participating in really elevating the level of the federal government. Another thing was, so many people sitting around the wall listening. I talked to this gal who's running the study section I'm on now after my first meeting and she said, "What do you think I should do differently. I'm new at this, and I really don't know, and you've had so much more experience" And I said, "Why are all those people sitting around the wall listening?" Well, we were meeting at a hotel, rather than meeting out at the NIMH campus and people would drop in and out to listen. But there is something that kind of makes you feel important and makes you feel you'd better do your job

AS ca nt. if you're told that so-and-so is the director of this program and so-and-so is the executive assistant, etc. and they have all these fancy titles, and they're all sitting there listening and you think, this must be important what I'm doing, and I'd better do it well. So that was my main suggestion to her, to get some staff people in there, to show that the work the committee was doing was valuable. So I think that's another thing. Then you read these statistics in Science magazine about how many people from Stanford University serve on federal committees, and how many from Harvard, etc. All of that lend a sense of importance to it.

EAR What can you recall about a category of people on the committee. Now you just described one, a psychoanalyst who may be a gentleman for the University called North Carolina, but it may not be the same one that I'm thinking of, but there's one just like that down there, but anyway, that's one, the person who can't make up his mind. Yesterday, and I keep on mentioning Gardner Lindzey, but he touched on a number of very similar kinds of points that illuminated in another way, but he obviously was a highly organized and very efficient first or second reviewer when he made his presentations and he was criticized curiously enough by a colleague of his about this, because the colleague said that by the time you get finished summarizing the application, telling its strengths, telling its weaknesses and then coming up with a recommendation, there's nothing else anybody can say and do but vote on the thing, whereas if someone did a much less effective job it would allow for the group's interaction to take place. It may be, that the truth that comes out, or the decision that comes out as a result of that may sometimes be more valid and useful than the one that's derived at by one very efficient person. And Gardner allowed as how he hadn't thought about it that way, but it might well be true. So Gardner is another example of just the opposite of this psychoanalyst obsessive non-decider you were talking about. Can you categorize people, I'm really not sure what group you fit into, for example,.....

AS Well, organized, or try to be....

EAR Yeah, but not in the sense I think that Gardner who almost shut off discussion because he closed all the doors on the way to the stable, so to speak,

AS Well, we had one man, who was really a raconteur, and that was Ed

AS cont. Walker and I would guess a great classroom teacher. People would look forward to his reviews because he was so scholarly and he presented them with such charm. I don't remember what the effect was on the discussion. I wasn't aware, but it may have been somewhat unabling, just as you say, I wasn't aware of formulating it that clearly. There were one or two people that other people had just discounted. They clearly made up their minds that they didn't trust this one's judgment, usually because they thought he was self-serving, or in one case, it seemed to me clear, that man was from a mediocre medical school and he felt that he was meant for better things. He clearly took great pleasure in telling me what was wrong with the faculty members of Yale and Harvard and Hopkins and so forth and cutting them down. I got the idea that the whole group had started to discount him, that they had all perceived that this was happening. I'm not as aware of disciplinary rivalries as those kinds of personal things. In the NICHD you were to see frank disciplinary rivalries. People would say, you know, I don't think any of this kind of work is worth doing. I don't care whether it's done poorly or well, it's a of work that isn't worth doing, or I don't understand how this comes before NICHD. This ought to be Neurology or Cancer or whatever, and there would be some of those categorical judgments and there were sometimes discipline judgments. We didn't see that at the NIMH. If anything, people leaned over the other way. And the primatologists were unduly fascinated by clinical phenomena and awed by them, and vice versa. I can think actually of two psychiatrists who had this obsessive quality and in both cases other people just filled in around them and they would sort of sleep through their reviews and then come awake; not really sleep through, but dozed, and then come awake and show that they had been listening and that they wanted to fill in.

AS EAR Yes, it's so interesting that you describe a very complex semi-structured process within which, then, all kinds of evaluations are being made, and are able to be made, because there is inevitably enough freedom for an individual interplay, but against the backdrop of the total situation you can evaluate the actor on the stage at that moment extraordinarily well, which I think really is very interesting. Of course, you have to realize that your being able to say this puts you in the category of the better

EAR cont. actors. Some of the poorer actors I think could not have quite the degree of sensitivity that you're describing about the total situation, but most could (AS - that's my impression), I think most people on the committee could, just the few who weren't up to snuff, so to speak, may have a lot of it going over their head without their realizing it. But it's a fascinating situation in that context. Again, nothing developed in any conscious way along these lines. The fact that people sat in the room was not deliberately done for the reasons that you have just said. There were a lot of very accidental circumstances that allowed the committee to work this way and to do a satisfactory job, which is very interesting.

AS But once you've done it, you become very dissatisfied with the other methods. I consulted recently for the, I think it was for the Foundation of Child Development, it was one of the New York foundations, and I thought it was just nothing compared to the consulting for the NIMH. I got on the phone and talked to a person who was not a professional person, unlike the NIMH staff who were professional people. I talked about some of the political aspects which she seemed to be very savvy about, some of the scientific aspects which she seemed to be less savvy about, etc. but there wasn't even a written document, and it was all going into her head and she could just pull it out of her head a week later at the board meeting whatever she chose to, and could say, well I had a consultant who was a professor of a West Coast medical school who said such and such, and they had only her word that she had heard me correctly. A very different dynamics from this kind of thing and I was very dissatisfied with the consultation. Of course the other thing is that those consultations are very unrewarding, whereas the study section meeting is very rewarding. You come back feeling that you've just had this whole education that you want to share with people and that you're so much smarter than when you left. You consult for NSF or some of the others and you just put a message into a bottle.

EAR And incidentally, if you didn't know, and it's important to point out that in the total financial situation the study section operation was not expensive. Now if it was partly a function of the volume of applications that we got at a level of the total budget there aren't many other organizations that can run a full-fledged

EAR cont. study section operations as the NIMH could. If you're not

running in a hundred million dollar plus operation you can't handle that kind of operation. But it really was not expensive, not only because we didn't pay very much but because the volume of what was accomplished was incredible.

AS And another thing about this system that enters, there's a certain accountability on the part of the primary reviewer. There's a certain sense in which that person is putting his judgment on the line and saying, "You ought to give this person the money because I say so, and I know what I'm talking about. It reminded me of the admissions committee in a medical school. There's such a pressure for admission to medical school, and I've served on an admissions committee for a number of years, I can't remember how those years juxtapose with the study section years, but my point is that each medical school applicant who is considered seriously has one interviewer, and that interviewer has the task of defending the applicant to the group or can say, refuse him, don't admit him. Well, very rarely are judgments made against the recommendation of the interviewer. Two years later if the kid is lousing up in medical school the members of the admissions committee come walking in and say, "Hey, Alberta, do you remember a year ago a Jones who you were so high on, well let me tell you about Jones, he's in my clerkship and he is a bad actor. Now tell me again what's so wonderful about him.....and the admissions committee had that relation to him. In fact, you say to a student, who's your interviewer, and they hardly ever remember who the interviewer was, but the other members of the committee remember. I remember once, there was a boy who I was very enthusiastic about, who I did some strong-arming for, and he was admitted to Stanford and he did very well there, the way they mostly do, and when he graduated he received a prize at commencement for being the outstanding student in some department. As he walked over to get that prize, three different members of the admissions committee who were wearing their caps and gowns and sitting among the faculty, turned and looked at me, and they were all remembering that he was my kid and that five years earlier I had argued for him. There was a little bit of that on the study section and when Walzer got into somuch grief up at Harvard about the XXy and XYY I had been his site visitor, and I had been reviewer on his project, and I felt very personally offended that my judgment

AS cont' was being challenged. And in Science Magazine it said, he had received this money in the halcyon days of the 60s when there was no concern about ethics, etc. Well, the fact is that we had spent a long time, in the study section meeting we had spent perhaps thirty minutes to an hour discussing the ethics of his research, but the site visitors, Herb Wiener and I, Mary Haworth, had talked about it at length with each other. It had been raised at the preliminary meeting, before we site visited, when the committee first discussed his application. It had been raised subsequent to the visit, and we had discussed it at length with him and with Park Jerrold, who was one of the sponsors, etc. so I read in Science Magazine that back in those days nobody cared about ethics and I felt personally affronted. I really felt like writing a letter to Science Magazine saying "How do you think we site visitors worked in those days?" I talked to Julie Richmond about it, who was Stan Walzer's superior officer at Harvard. I don't remember exactly what the relationship was. Julie was head of the Judge Baker and Stan worked at the Judge Baker, and I said, look, I read all that stuff in the papers about Stan Walzer and all this heat he's getting, and I feel bad about it, because I was his site visitor, and all that stuff was gone over and he has very sensible answers for all that, it is not the way it's being represented in the press. Julie said he had made that point to the dean of the Medical School, when the dean of the Medical School had proposed that the faculty of Harvard Medical School should review Walzer's project and should see what the processes had been, whereby certain decisions were made. Julie Richmond then said, "I think you had better get the site visitors here. After all, they did review this and they did recommend that it be funded and they ought to have a chance to at least weigh in at this point." So Julie, at least, saw how the system worked, and appreciated it. But there is a sense, I'll run into members of the study section and they'll say, Well, your guy Walzer sure is making a lot of heavy headlines, isn't he? You know, there's an accountability back to the person who made the recommendation and of course, NIMH has accountability, an individual accountability,

EAR Let me ask you about another aspect of the study section wondering, well, I guess in your study section it wouldn't have come up very often, and that is, did you see, did you ever consider it, was it ever part of your concern that you may have had in your review, as

EAR cont. to what was going to happen when those recommendations went up to Council. Now yours didn't go up to Council, ⁱⁿ that sense, you never really had that direct relation with Council. That never really came up in any situation.....

AS There would occasionally be remarks about something of unusual social interest, or too bad, this isn't a better application because it is just the kind of thing that one would like to see done well and Lord knows this is the kind of thing that in Council people wish could be done well. I don't remember anything much more specific than that.

EAR It wouldn't have been in that case. I realized, when I was asking the question, it's a different situation. Okey, anything else that you want to say about study sections, yours in particular, and study sections in general.

AS I think the fact that the executive secretaries are professional people is very important. It gives the members of the study section a basis for communicating with them, in a sense that they're not just blowing in the wind, that they're being heard by people who understand what they are saying and I don't know how you maintain the professionalism of the executive secretaries over a long period of time, I know that in the NICHD they experimented with half time executive secretaries and half-time researcher, and I gather that it didn't work. But one of the strengths of the NSF system is that they do bring in university based people who aren't vulnerable to going stale and losing touch with their fields. they rotate them through. I guess they're vulnerable to lack of accountability. By the time the project blows up, why the guy who approved it has disappeared. I guess the danger with the NIMH thing is that an executive secretary over the years could become kind of jaded and cynical and start categorizing the arguments like the man on the moon argument. We used to have that. The man on the moon would be this guy is studying a very interesting phenomenon. He's the only man in the country who has access to this phenomenon. Therefore we have to support him, even though his methods are lousy and he's not very smart, because he's the only man we have on the moon, was the idea. Well, I could see that over the years the executive secretary would say, well, we have one of those man on the moon cases coming up today, and they could get kind of cynical and numbskull, and in fact haven't seen that.

EAR No, I think that, by and large, from my experience, good executive

EAR cont. secretaries who were good at the beginning remain good, and those who weren't very good at the beginning, just weren't very good, period. I don't think very many of them really got jaded. They vary obviously in their style. People like Jack Lasky, whom you never had an contact with, was a totally different kind of executive secretary from Bert Booth, and Bert's a totally different executive secretary from Ozzie Simos, etc. But among the large proportion~~na~~ of NIMH exec. secs, both in research and in training, I think that changes that have occurred over time have been much more a function of the changes in the Institute rather than changes in individuals. I think that in the halcyon~~o~~days that we were involved in, I would think that there were very few really poor executive secretaries. Those who were poor left pretty soon, or were pushed out pretty soon. There are p~~ro~~ple who have been exec secs. for a long long time and I think still do a very good job. Now of course, some of them worked their way up. You know, Phil Sapir started as an exec. sec. and Betty Pickett started as an exec. sec. So you have that kind of professional growth and development in terms of responsibility. A lot of people who were exec. secs. look back at that part of their career as the best of all. That's when you really were involved in a program area. You had as much satisfaction out of the committee meetings as the committee members. And they felt that they were making a major contribution to program development by being an exec. sec. Bert Booth, that was his life, I mean, in a real sense, and when he moved up to become chief of a branch, he was much less satisfied with what he was doing at that time. Okey, I have mixed feelings about talking about the TV thing with you.....

AS You and I have talked about it a few times before.

EAR I know, and yet maybe, just on~~e~~the record, it is important to say something about the difference of that committee for one, and the study section, in a sense, perhaps, some of the things that you said didn't exist in the study section atmosphere unfortunately did exist, for other complicated reasons, in the TV committee. I mean, it was a totally different animal, whichI really don't care Alberta, If you want to talk about it, fine.

AS Well, one thing is, those were terrible times for universities, 69 to 71. I don't need to tell you about it. You were studying it. I would leave Stanford to go to Washington for those TV meetings

AS cont. The management were so convinced that they were doing the right thing that they said to us, Of course, the Board doesn't do decision-making in this area but we want you to know...and the management....

EAR He came to a meeting?

AS No, he came in for a party, and I didn't go to that party..... But it was a very similar kind of thing. The chairman of the Board and the president announced, this is the way it's going to be done, not in that tone of voice, in a different tone of voice but, you could like or lump it, but I think that was really a very smart decision to get those scientific reviews out of that committee. One thing about the study section, everybody on it has sort of his own area of expertise and is the acknowledged person on that study section for his area, so that one person is the Child Development person, and another is the child psychiatrist, and another is the old-time psychoanalyst and another is the neuro-biologist, etc. That wasn't as clearly true in the Surgeon General's committee. On the Surgeon General's committee I think everybody sort of felt like they knew the field as well as the next person, the total field, and I very rarely saw the people deferring to each other's expertise. Another thing was that there was a great range of universities represented on that committee from the most reputable and esteemed like Harvard and Yale through universities that are not well regarded, like the University of Denver, and I think we saw on the part of the people from the low regarded universities the same thing that we used to see on the study sections, a feeling like, you guys better realize that I'm just as good as you are even though you're at Yale and I'm at the University of Denver. There was a sort of defensiveness, but I think it was heightened by the fact that everybody felt competent in the area, and so you couldn't come on like, I may be from the University of Denver but I know more about my thing than anybody else in this room, which you could do on a study section but you couldn't very well do on that committee. An awful lot of what people had to read was completed work and they didn't feel like they had any input to make on it, whereas in a study section, you have this fantasy that you, maybe by participating in this review, you might even shape the research in some minor way, in a constructive way. You can see study section members all the time trying to do that.

AS ca nt. "I'd love to approve this, but I can't stand that...you know.

Can we move, yess I know, no, I mean...but on the Surgeon General's committee most of what we read was the completed work and it was a question of what to make of it, for the policy questions ahead, rather than, how could it have been designed better. I guess that line of question is more divisive. I don't know, do you think there were lasting friendships from that committee. You and I have a lasting friendship.

EAR I don't, unless they were there before. I don't know of anything that happened there that resulted from those interactions, except for some staff relations. I'm still very close of course with George. Not so close with his predecessor, but no, I don't think so. In fact, I think that some people in a curious kind of way have disavowed their relationship with that group, not written down, "I never was on that committee" but I think, and I don't want to mention any names, but I think that some people there felt that it was a bad scene, they lived through it and don't want to think about it anymore. It's really interesting.

AS I think that some friendships were made. I think that I became a friend of Evelyn Omwake's. We don't exchange Xmas cards and that sort of thing, but greetings will come to me at meetings. Someone will say, you know, Oh, Evelyn asked me to say hello.

EAR Oh, on that level, I think Charles and I have had that continuing relationship. And then I have mentioned many times, ^{when someone sayd} /you know you ought to have a psychiatrist, I was the one who brought him into the Reston conference. He really didn't know what he was doing there and I said, well, for what it is worth, Charles, I thought it would be very worthwhile your being here and I've remained on reasonably cordial relationships with all. Irv Janis was nice enough to suggest that I do a review article for the American Scientist on the whole field, since he's on their Board of Editors, and even Joe, and all the rest of them who were on that committee. I think, in that sense, yes, but in the very unusual way that you described of the study section phenomenon of people making lasting friendships, and reaffirmed by what Gardner was saying yesterday, I already know that collegial relationships, both in a personal and professional way were made many many times in study sections which have become an important part of this national inter-connection, cross-discipline, cross-subject,

EAR cont. area, it's really been most unusual and very very productive for the total field. There were so many beneficial side effects that accrued from the study section system, for one, from the feelings of allegiance to NIMH on the part of trainees, on the part of fellows, career awardees. I just think that in a curious way we've built up a national fraternity or sorority or congregation of NIMH family which still exists in a real sense.

AS I would guess that more meaningful emotional links were made on the part of the investigators. You know, if anybody sits around and has reunions, it's probably more the people who were funded and had deadlines to meet and outlines to follow, you know, similar positions that they worked under and I feel like I made some links to investigators like Aletha Stein, really I wouldn't have known her except through that program, maybe Andy Collins, some of those. In the committee itself, the questions they were trying to deal with were so divisive and the times were so bad....

EAR And we had internal problems, no getting around that, staff problems of one kind or another, and committee problems of one kind or another....

AS Well, I remember when I first talked to you about it, you and I were talking from time to time when those hearings were going on, we would talk on the phone, and I remember you called me and told me about getting the committee set up, etc. I came back and mentioned it to David Hamburg and I may have told you this, and asked him whether he thought it would be a useful thing for me to do. He said, you know, in the past the television industry has tried to destroy the reputations of people who criticized them and that would be the risk you would be taking. You could talk to Al Bandura about it and find out what his experience has been, but other than that he thought it would be a useful thing to do. But, you know, those folks play rough and there's a lot of money on the line, with some of these things, I guess.

EAR But interestingly enough, and I think it's a curious contrast. George Comstock has accomplished everything that Doug Foukes could have accomplished, if Doug had done what George Comstock did. Comstock's career has literally been made by that situation. He is now a highly distinguished, not researcher obviously, but in his position now at Syracuse, he is called on all the time .

EAR cont. He's just been commissioned to write another book by a publisher.

AS That's interesting. I go to a discussion group on Thursday noons now. The discussion group has to do with television and education. There are three or four graduate students who are paid to participate in this and do the library work and stuff like that. At the moment what we are discussing is the allegations that reading has declined as a result of television and what would it take to investigate whether that's true or not, what is being asserted and how to go about investigating it. This is some kind of an exploration under NIE funds, maybe leading toward a grant application on the part of some of the people, but Comstock is cited as the last word. I don't remember the questions, but the graduate students would say, well we reviewed the literature and what Comstock says is, you know,

EAR Well, he know the program very well, He's been a gifted writer and he hasn't done anything original of his own, but the interpretations he makes are very good. Well, before we close, I want to turn to one other area, unless you want to say more about that committee. I'm not sure there's much more.

As The other thing that I did with NIMH, you know, was the research consultant.

EAR That's what I wanted to get to, unless there's something else you wanted to say about the Surgeon'General's committee. That's so anomalous a situation. It's going to be in the book, but I don't think it illuminates anything but itself, so to speak, because it was so idiosyncratic in many many ways. But I think that I did want to ask you some questions about that committee you were on. Do you want to talk about that for a little bit.

AS Bert Brown put that thing together. He made telephone calls to individuals and asked them to serve. I guess, by the time we got on Board, there had been a series of groups who were doing self studies. There were ten of them, and they came up with ten little volumes, really, most of which were really quite good. There was a party at Julie Siegel's house, who was running it, and Bert Brown was there and we got to talking about the Watergate, which was just starting to open up then, I guess it was somewhere in 73, and finally Bert Brown turned to me and said, Dr. Siegel, you don't sound like a Republican. And I said, I'm not, I'm a life-long

AS cont. Democrat. He said, that's funny, your dossier says you're a Republican. And we'd all been drinking, so I put down my drink and thought, what in the world is that all about? And then I realized, I am a Republican, I'm a registered Republican. I changed my registration so I could vote for Pete McCloskey in the primary, so I said, and he had gone on to talk to somebody else, so I said, Dr. Brown, I've been thinking over what you just said to me, you're right, I am a Republican. I'm a registered Republican. Then I said, ^{if} the people who put together your dossiers don't know about Pete McCloskey's district in California and know that the Stanford campus is full of lifelong Democrats like me who have registered Republican to get him past the primaries, I don't think those dossiers are worth the paper they're written on. So he said, I'll have to talk to Pat Gray about that. Well, anyway, my point is that I think I was the token Republican on that committee I think that NIMH had become quite politicized by that time and I looked around at that group and I thought, my God, I'm not in the league with these people. These are really very distinguished investigators and distinguished statesmen in their field, I'm not in their class and how did I get on this committee?

EAR How many other women were there?

AS You could even have done much better if you were restricted to women. Let me think now, who were some of them on the committee? Clearly I was the token woman. I'm trying to think if there were other women on that committee. But you know, Herb Simon, Josh Lederburg, Neil Miller, Dan Friedman, Norm Garmazy, David Hamburg, those people are in a totally different league than I am and I don't think there was another woman, but I will be embarrassed when it comes into my mind who the other woman was. Mildred Mitchell Bailey, and I don't remember her at all. I don't remember if she came to the meetings.

EAR She may not have come to the meetings. She's the psychiatrist who was head of the program in West Virginia.

AS It's funny, I can't even visualize her.

EAR She may well not have come.

AS So, it was perfectly clear that I was the token woman in that. It seemed like, and also, there were so many people from Stanford. David was from Stanford, Josh Lederburg and Jack Hilgard and so I wasn't the token Stanford, as I always thought I was on the Surgeon-General's committee, if they had to have somebody from

AS cont. Stanford, I was less offensive, but that was a very high class bunch of people and it was a very good experience, and I gather some good things came out of it. I got a call from Lasky not too long ago. I don't know Lasky, but he wanted me to do a site visit on a center application, to a center, and I said to him that I wasn't able to, for whatever reason, and he said, well, actually, I'm really hoping that you would be able to serve on our study section. I said, Gee, I don't think so, I've just gone on a study section and I'm an old NIMH hand and did a lot of stuff, and then I mentioned that I had been on this, and he said, oh, then you really ought to be on our study section because our whole program is a result of that. So I thought, does he mean we accomplished something? But apparently that center effort, in some sense, emanated from this report. It may also be that we gave some support to the intramural program, some useful support or needed support. What I never understood is to what extent we're dealing with the question of where the NIMH belongs in the structure of HEW, does NIMH belong under NIH, etc. while we were meeting ADAMHA was created and we had this rather dramatic moment.....

EAR Well, the intent initially was that it would serve as a means of preventing that from taking place, but this took so long that it took place in the interim and that was that. Initially, it was a political effort that Bert and Jim Isbister created and that's not the first time that things like that were ever done, so it was not unique in that sense, but I think it took longer than they anticipated and it just got terribly involved and a lot of people spent a lot of time, staff people spent a lot of time, not just Julie himself who done similar kinds of things before in putting together papers, but you know there was committee after committee after committee of staff people involved in each of these ten different areas and a great deal of time and effort was spent on it. Well, one of the points I was wondering whether you wanted to comment on, I don't remember how many meetings you had, no more than half a dozen, really....

AS Probably not that many

EARbut whether you had the same kind of feeling that Gardner mentioned, in the sense of its being too vaguely really to allow for effective interaction, but you were given documents from time to time to look at, at various stages of development, was that a

EAR cont. major function?

AS I didn't feel that was a problem. I did feel there was a problem that the quality of the people who were staffing this, they tended to be science writers, and some of them were I think brought on, on a more or less ad hoc basis, to do this job....

EAR Yes, that was Julie's stable of writers.

AS In talking to them, one did not have the same sense, as in talking to executive secretaries, like that, Well, I had the feeling that bringing Neil Miller to talk to a science writer, that it was hard for Neil Miller to master that, that he was good in talking to the other people, and of course, they respected him enormously, and all that, but I would repeatedly hear from the staff calls for the kinds of statements and the kinds of inputs, but these people weren't in a position to give, so for example, people were making the usual arguments about basic research versus missionary oriented research and the staff people would say, but we need an example, and they would say, well, okey, then the example is such and such, and the professional people would understand all the implications of that and the science writers wanted somebody to spell it out for them, you know, dictate me a story kind of thing, but that was the kind of thing that came up. I felt that the staff at that group were not equal to the task and meeting with those individuals, and I'm not talking about Siegel, who seemed to be very well qualified to do it, but the people working around him didn't. Apparently for Brown and Isbister and Siegel the meetings were useful and and we met out one week end at some institution place near Friendship airport, I don't remember the name, and over the years people have told me, like Betty Pickett and Lou Wyncowski and others have said that to have been a high point in the career of some of these people, the feeling that the kind of feedback they were getting and the usefulness of it, well, good, I'm surprised because what I was looking at was the faces of some of these science writers, which were alternately expressing bewilderment and overwhelmingness, that they just couldn't master what was being said. Apparently Brown and some of the others did understand what was being said and profiting from it, I remember making a little speech, which was subsequently referred to as my "Statue of Liberty" speech, in which I held up all the documents that were given on research questions, like development, cross-cultural, psychopharm, biological substrated mental illness, whatever the organization

AS cont. was, you know, we were given this big heavy thing, and the relatively slim documents on organization and delivery of services in mental health centers, and whatnot, a little bit here and an awful lot here, but in the final document this got much more disproportionate, and this got cut back and so I held up these various documents and just compared them, and this got known as Alberta's Statue of Liberty Speech. That to me was the most striking thing, that what existed in those original documents was certainly pared down and they never got published, and it was almost as if the research had been hidden.

EAR Well, that was the political part of it that had to be pushed and, unfortunately, in one sense I think Bert and Jim, being somewhat more political animals, they were more than willing to do it that way for reasons that may have been well insufficient. Anything else, either about that committee or other interactions at NIMH that may come to mind at this point?

AS Well, one particular thing that interests me is my own field of developmental psychology. Does that belong under NIMH or does it belong under NICHD? We grew up and flourished under NIMH. We were transferred in good part of NICHD at some point in the 60s and now people in developmental psychology are so appalled by that is going on at NICHD that they are wondering whether we belong there at all. There's been a lot of talk about was that a wise decision and its only people my age and older who even know about it, because younger people have grown up in a system where it was always that way, and a sense has been that the psychiatric profession has been much more receptive to inputs from the basic sciences, than pediatrics and gyn, ob-gyn, to the extent that NICHD is really dominated by those two branches of the medical profession. I think the likelihood that we'll ever get a fair shake at NICHD is diminished. A lot of intellectuals go into psychiatry and the people who go into pediatrics and ob-gyn are nice guys, but they're not intellectuals as a group. They're really known for liking children, liking working with families, interested in primary care and that kind of thing. And out of that you select your research people who undoubtedly are more ^{intellectual,} ~~intelligent,~~ but as a group they're nowhere near as intellectual as psychiatrists, and that may be the thing that explains it, but I think we were in a much more hospitable environment and what I'm glad to see is that a lot of developmental

AS cont. psychology has been maintained at NIMH and the day may come when we'll want to see it all move back to NIMH.

EAR Well, I think that what you're saying is a kind of commentary on the initial strength of the NIMH program. I think that in so many of the areas in which the Institute was functioning there were good people on staff and among the consultants and good program development, and in a sense each part feeding on and feeding from the whole of the program in ways which I think were good for the various parts, and certainly good for the whole. I think it was an honest effort on the part of NICHD when that became a new institute to give appropriate emphasis to the fields that were being highlighted by that new term, but I don't think that institute ever really got off the ground the right way, but I just don't think it had the professional staff, it didn't have the program leadership, and perhaps came along at a time when that was difficult to start de novo, but it just really never did the kind of job that had been done, and to some extent continued to be done at NIMH, and that's a shame. Our larger question in this whole thing is what happens over time to organizations and here I'm really talking about the totality of NIH, as well as the totality of NIMH, because NIH at one time was X numbers of institutes. Then 2 or 3 or 4 four were added, each of which had a very meaningful and important contribution that was supposed to be made but hardly making it. I don't know what Betty Pickette feels about the National Institute of Aging now, it's still too early to say. I would kind of doubt that it's going to be able to make the impact that it could have made years ago, when NIH in toto was formed, more than Aging for example, while it did make to some minor extent at NIMH. I talked to Bud Bussey a couple of weeks ago. He was covertly bitter about NIMH never really putting the field of aging in proper perspective, with its never really being a full-fledged member of the NIMH family. It may be partly a function of him, but I think it's a valid criticism. So, I don't know, this is now 1978 and we're talking about days when things were really much more vigorous and much more active.

AS Well, it probably could have gone a different way. I remember when John Eberhardt was looking for the first director of NICHD and he came out and was talking to me about it and went over some names with me. He wasn't personally committed to the medical notion

AS cont. that it had to be a medical director. He wasn't committed to the idea that it had to be a cell biology. Now that's really the thing that ~~development~~ psychologists find so hard to take, that the only basic science that the pediatrician seems to know about is cell biology. On the other hand, I just read this book on the children of Kawai, because I was over there at Kawai, in which Amy Proborner studied all the children who were born alive in Kawai in 1955 and has followed them right along, the cohort of over 700 children and she rated their prenatal and paranatal experiences as severe distress, moderate distress, mild distress, no distress and then followed up on them and the children who had severe perinatal distress turned out to be either mentally retarded or had learning disabilities, but they don't show up disproportionately among the mild mental health problems. They do show up among the severe mental health problems and they don't show up among the adolescent problems. She has a group of children that at age 10 were judged to need short term mental health services by a panel of a pediatrician, social worker and a clinical psychologist and she has another group that were judged to need long term mental health services, and then she goes back and looks at their birth records and so forth. For the ones who need long term mental health services, she does find some prenatal and perinatal distress. The short term, she doesn't. Certainly not severe, maybe mild. But what she finds is a lot of early environmental things, lackadaisical mother, matter of fact mother, neglecting mother, overprotecting mother. But these appear to be biologically intact kids who have postnatal life experience problem. Then in adolescents, she has a group which she calls new problems in adolescence, and these were kids who were not serious problems at age ten, while at age 18 they are. They're mostly unwanted pregnancies and juvenile delinquencies and those kids do not show up as being biologically impaired, you know, birth defect problems or prenatal or perinatal distress, but poverty and disorganized families, alcohol, you know, just what you'd think. So I'm thinking to myself, here we've got this NICHD dominated by pediatricians and gynecologists. What they worry about are the infants who have had bad birth experience and who look bad at birth, low birth weight, small *for age* and that kind of stuff, and what do they do, they

AS cont. worry about mental retardation and they worry about communication development, which is exactly right, which is what she's showing those kids have problems with. So we actually don't understand what those pediatricians are doing. Now the other children are more numerous and they may not be more costly to the institutions of society. I mean these brain damaged kids, they cost a pack before their life is over to a lot of people, but you can put a dollar cost on that faster than you could put a dollar cost on juvenile delinquency, early pregnancies and unwanted pregnancies, and all that. So I can really see what the pediatricians....they can look at it anywhere in the state of Hawaii and say, you see, it does make sense, there is a package here. Prenatal, perinatal, postnatal, ob-gyn, remedial care, early intervention, and then, follow that with mental retardation and running out of kids. They could see there's a real natural package here and if you developmental psychologists aren't interested, then go work someplace else. If I were Noah Kretschmer I would be touting this book because it gives the arguments for his approach. Not being Noah Kretschmer I am kind of worried about where we belong, where developmental psychology is.

EAR Maybe on that point we should stop.