

Philip Sapir
Grant Foundation
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EAR Well, let me try to tell you where I am, and what I would hope to be able to accomplish this morning. I have written Phil Hallen again. We talked over the phone a week or so ago and he was enthusiastic about the idea without in any way giving me any encouragement. He obviously didn't have any funds for it, or ^{say} that he could do anything beyond continue to give me encouragement. However, when I told him what I had in mind, which was a little bit beyond what I had written to you, and how I hoped to go about it, he said, "Well, can you send a more extended description." I said to him, "I don't need any money for myself, but I suspect there may be some expenditures involved, and he said he would understand that. I have now sent him a five-page overview statement of how I propose to do this, a very preliminary outline of chapters which is nothing more than a very first phase description of what I would like to do, and a very rough budget of what it would cost for travel and then for some assistance and for publication and for preparation of manuscript.

But, more importantly, from the standpoint of what I'd like to discuss with you this morning is the main approach I'd like to take is to talk to personally a number of the key people involved in various stages at NIMH and to use their perspectives from the standpoint of what they have to say, and what they'd like to provide me in the way of reminiscences, or materials, or both--use that as a major source for doing other than a kind of scholarly documentary historical sort of thing. My feeling is that a number of us who have been involved in various points have interesting things that we can say about some

EAR (cont) aspects which go beyond the strictly scholarly parts of it, without in any sense going overboard in the direction of being an intimate, exposé type of thing, but rather that you, Bob, Stan, I, John Eberhardt, everyone who's had an important part in some aspect of the total development, and those individuals can provide some very interesting material for doing it from that point of view. I think I mentioned to you over the phone, (I know I mentioned in the letter) that a rough model I had in mind was the book that Steve Strickland did on the NIH. Have you seen that? You should take a look at it. You'd find it very interesting. It's called "Politics, Science, and Dread Disease." He interviewed a lot of people. It's the Commonwealth Foundation publication, and in fact I wrote to Quigg first assuming, well, that if they had done that one, they might be able to do this one. He said that was an exception and they are now re-examining their entire grant policies.

PS He's still there as vice-chairman of the board, which is a polite gesture, but Carlton Chapman is now the President. I heard about it several months ago. Do you know Carlton?

EAR Yes, I met him last April at an Airlie House meeting on human sexuality. That's an area we've gotten involved in at Stony Brook. Anyway, I do now have an appointment with Bob Felix for two days in May. I have identified on this statement to Phil Hallen, 30 or 35 people that I would like to have extended interviews with. I suspect it will more likely go to 50. I have indicated that in addition I would like to have fairly extended telephone interviews with probably as many as 100 more people, and I would like to be in correspondence with perhaps

EAR (cont) an additional 200 people. So a large part of what I have in mind, even though there's a lot of source material available (yours and Jeanne Brands' original document all the way through Dick Williams' document, a lot of stuff that Stan has in files and stuff I am sure I can get from Bert--there's a lot of written material), but I really would like to see this input as playing a major role in the actual substance of the publication.

One last thing, and then I'll stop and ask you a couple of questions. I've been looking for an approach which would serve both the substantive needs and, to some extent the literary needs for making it reasonably interesting to people who have not been there all the time and people who don't have an immediate interest in this. I don't expect this to become a best-seller, but I would like for it to be more than just a ^{dry} narrative, so that as one literary gimmick (and it really is nothing more than that, although I think it can serve a useful purpose), I am going to start the entire thing with a fairly extended description of the 25th Anniversary, how it came about, what it was all about, and use that to introduce a number of the key people, many of whom were there for the Anniversary, and use it also in a way to compare and contrast the three Directors of NIMH and a number of other people, as I say, many of whom were there, of course. And then go from there and flash back to the inception of the NIMH back in 1946, if not even earlier--some of those early days--and then without doing a complete chronological sequence, touch on some of the major highlights as those occurred from that point on, stopping at 1971. I'm not going to go beyond the 25th Anniversary for a number of reasons, some of which are obvious.

PS It's a mistake.

EAR It's a mistake not to go beyond? Well, let me tell you why, and I'd like very much to get your comments on it. I think I want to emphasize what I've already indicated--that is, the personal points of view, and I wanted to do this with a major emphasis obviously on the three Directors. Now, Stan and Bob are clearly in a different situation than Bert is. I can get commentary and input from them since they are no longer officially affiliated, in a way that I couldn't get from Bert. So that I can't really ask Bert to do the same kind of job that I can ask them. I think there's also a danger in coming too close to the present, because you don't have enough perspective. And so much in a different kind of way has happened since 1971 with forces and phenomena that I think are another order of circumstances. But I'd like to know why you say it would be a mistake.

PS Well, I think you can either take a quite personal approach in terms of biographies, and the roles of key people, or you can take a theme--a major theme--which is not the history of NIMH, but rather why NIMH came into being--what is it about mental illness, mental disorder, "mental health" that caused the creation of this thing; which is something I would think of as a major theme. I like the theme, the concept of being of risk, children of high risk for the schizophrenias, all the thing that we're supporting. And I'd play with myself being at risk as a foundation executive. I think of the successful black professional as a person at risk, because he and other people have certain expectations of: he has to act in certain ways, he has to play certain roles, he has liabilities and advantages--or the woman, or so on. If you play around with that. Now, one could take the theme that the field of psychiatry, mental health, mental illness has and

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(cont.)

will be for the foreseeable future, at risk; that but for the Mental Health Act and then NIMH, it wouldn't have gotten as far as it has, that it promised too much, that now it is becoming increasingly apparent that people are again very uncomfortable; there are very, very few foundations that will ^{give} support of any kind in the field of mental health and mental illness. I attended a meeting of forty foundations with an active interest in the mental health field. Phil Hallen held forth about this thing of his, and I was surprised at the emotion, the heat, with which people in effect denounced the field, psychiatry, and all. And they were so much happier. We really aren't a mental health foundation. We're a child development foundation. Yes, we're in the behavioral side of things (of development). Some of the things we support, and for pediatrics, or education, or law and children's rights are perhaps more significant, more productive than what the "mental health professionals" are. And I got a surprisingly positive response which I hadn't anticipated, and I also felt like taking back what I'd said, because I wasn't damning except that I had damned at that meeting about this new joint commission. I had sort of taken Bert and others to task for again trying to have psychiatry be all things to all the people. And suddenly psychiatry discovers ethics. There are people who have been talking about ethics for several thousand years, but now psychiatry comes along and it wants to march at the head of the parade. This sort of thing. So, I think that mental health again as in the forties and earlier is very much at risk, very unpopular. People get very up-tight about it for all kinds of rational and irrational reasons. This could be the major theme, of putting the NIH and the NIMH/history in the context of that.

EAR Well, it's very interesting that you should say that, because one of the things I obviously have been struggling with is a theme or, to put it in the vernacular, trying to put a document together--some kind of hook on which to hang it, and what I've come up with touches on what you are saying, I believe, and I think it needs further development.

I want to call (and this is an initial title but it's helpful to get my thoughts in order)--I want to call it "Afterimage: The history of the National Institute of Mental Health, 1946-1971." And the term "afterimage" has two connotations: (1) that in fact it is going to be^a retrospective examination of highlights; (2) but, also, that just as with a visual phenomenon, afterimage often brings on the coloration exactly the opposite of what the original circumstances were. And that in many respects, as the Mental Health Program has developed, things that we tried to do at the time that we tried to do them, for reasons that were then apparent, are now seen in retrospect in a totally different way. And that we are now in many instances being criticized and condemned for things that we never intended to do, never viewed at that time, because they weren't apparent at that time, as purposes to keep in mind, in addition to the problem that you inevitably face when you go through a progression of any kind in programmatic effort. There are a sequence of steps which after you are all done, you might^{in hindsight} say, well why did you have to take seven steps to get there. You could have done it in three. Or why did you let the seventh step take you there when you should have been here. Well, it's very easy (I don't need to tell you) to say that after the fact and before the fact it's not. I think that what you're saying about the issue of being at risk falls somewhat into that category and that's what I'd like to do. I'm still not clear, though, and I want to pursue it another moment with you. I'm not clear why you feel

EAR what you've said makes it important if not necessary. . .

PS Well, maybe not. I had forgotten it was '71 and these trends I am referring to were apparent at that time, I suppose, although my own experience since then has somehow brought it home much more forcefully than I was aware of that time. And I guess the whole creation of ADAMHA with mental health now taking second place to drug abuse and alcohol-- (Maybe this is good, right, proper. Why should mental health . . .). We've always had the luxury of accepting what we said was important, whether it is ethics or whatever, and discarding what we didn't like. We don't want to remember that we once said, "Let the neurologists have mental retardation. We don't want to fool with mental retardation," and so on, and we picked and chose and so maybe it was apparent by '71.

EAR I think the seeds were there.

PS I guess the creation of ADAMHA has brought it smack up to . . .

EAR Well, let me take you back (I don't know whether it'll be a sentimental journey or not), . . . tell me, first of all, just from the standpoint of your own thoughts ^{and} /recollections at the moment how you began at NIMH, what your early recollections were of your first participation in the NIMH program.

PS Well, I guess you mean in the grant program, not my earlier career at NIMH as a "science writer and information specialist".

EAR However you want to do it. Sure, it would be useful. However you want to do it.

PS Well, all right. I started in the office of health information, public health service with Jean Henderson and Robbie Robinson's wife was there, too, and Herb Gardner, and my first job was editor of public health reports

PS (cont) which was a bi-weekly, twice a month publication. This was 1947. Then for awhile I was assigned as an information specialist to the NIMH, out of the office of health information, which was downtown, and I worked with Edith Lesser. Edith was a social worker by training, and had got into this writing field, and we did a little pamphlet, "The Mental Health Act," (I think I still have a copy somewhere), and that is the first appearance to our knowledge of the phrase "one-in-ten". Edith often refers to this "one-in-ten", and I think she got it by running around and talking to Morton Cramer, or his predecessor, Lindblatt, or somesuch--a fellow who was really a psychol . . . Charlie, Charles Lindbloom? No, he was a Swede, "scandihoovian", and he had done the annual census of patients, and so forth. But anyway, that was how she got some sort of suggestion, but it was a public relations sort of a thing, but it's still being used-- "one-in-ten" at some time in their lives will be mentally or emotionally ill, or have reason to go to a psychiatrist (however it is phrased). I don't think it had any more scientific basis than that. We produced the usual materials, press releases, brochures, and speech writing. I don't remember anything other than that. Then I left, because I refused to work under Alberta Aldman who had elbowed her way in and elbowed Edith out, and got into good graces--Alberta Jaccoby, I should say--and so I went over to the National Research Council for a year with Herb Gardner working in Divisional Medical Sciences, and Dale Cameron called me up and asked me if I would have any interest in coming back and working with Larry Kolb, who was the first Research Director on sort of a part-time basis, and I agreed to, because I didn't enjoy working under Herb Gardner, and I was doing nothing but being the minute-taker and writer for a whole series of committees over at NRC. In any case, when I came back Larry had

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accepted the position at Mayo, and it turned out that I was going to work for John Eberhardt, so poor John had no choice, he had to accept Phil Sapir, having been recruited by Dale and Larry, and I worked for John from then on until he left in '54 for Commonwealth. This was in '49. And we had a budget of 1-1/2 million, 2 million, for research, and something like \$250,000 for research fellowships. And somewhere along the line, we got an extra million or a million and a half, and we were in utter panic, how to spend all of this money. Incidentally, MHL was to W.M. Kellogg, the basic nature of the learning process, or some such title, and he is a dolphin-whale man, and it was very interesting to hear this very basic project. That was the very first one that we funded. And we were so frightened by this additional money and how to spend it meaningfully and usefully that we convened, or we did a survey of all of the members of the council and of the three committees, research training community services. What were their top priority areas for research? And we had already been going for (I guess) several years. I'm a little rusty on the sequence here. And I still have, I think, the^R/original classification, which was a very weird, pragmatic thing, of how we simply classified projects in various areas; when psychosurgery was prominent and experimental neurosis was prominent, and so on--names you never hear of any more. In any case, what I'm getting to, is that this theme of priority areas has run throughout as one of the major themes. What are the important areas that we should go into, and help open up, explore, deepen, widen, and the answers we got back were very revealing. Because there was no project that we were supporting that did not fall into somebody's Priority I, II, or III area. We could happily justify anything we supported, whether it be aging, biochemistry, schizophrenia, personal development, interpersonal relations, you use the phrase, and it'll

PS (Cont) be on somebody's list as one of the three most important areas or topics that he or she could think of. So we merrily went along our own way accepting applications from a wide range of fields, broadening it increasingly as the money became more plentiful, secure in the knowledge or belief that at least in our colleagues' minds these were all high priority projects. So you just picked the best in terms of the scientific criteria that the review groups used in selecting. But it is amusing to look back and ponder at how little research was going, how unsure we were; and of course, we still were thinking of mental health as mental hygiene. And in the course of our stay there we converted the term in our minds at least "mental hygiene" into a synonym for behavioral science in effect. But it wasn't, in those days. And as we were saying earlier, we used to argue--is mental retardation properly a part of this? And we said, "No." I also remember another brown bag luncheon meeting when John Eberhardt held forth and allowed as how he didn't think that juvenile delinquency was properly in our sphere of interest, because this was primarily social pathology, not individual psychopathology. We weren't concerned with the individual psychopathology. We probably made a political strategic mistake in abandoning mental retardation to the neurologists and the other people, excluding it from our. . .

EAR Were the first study sections NIMH, or was the NIH system already in effect.

PS No. We were not in the NIMH when the Mental Health Act was passed. We were in the Medical Services. We had the two drug addiction hospitals. We could have gone any way. Those were our committees. We had a research committee, a training committee, a community services committee, and we had a State grant and aid program so we could have gone to the Bureau of

PS (cont) State Services. We were running two hospitals; we could have gone to the medical program; or we had this research and training program which was in the NIH pattern, and we could have gone there, and that's where we did go. This was based on discussions between Bob Felix and Tom Parron. And it was agreed that we would go there, and it was also agreed that we would continue to run our own committees. Now, I must point out that as you know, the Cancer Institute preceded NIH. It was created in '38; NIH was postwar '45, and the research program was supposed to be an across-the-board program in the Division of Research Grants. And they forced Cancer (coerced), over many dead bodies, Ralph Nader's, and Vance Slike's and others, to abandon their separateness, their own councils, their own committees, and become a part of NIH's. Remember, for awhile Mabel Ross still says, "N I's of H ." But then gradually, Mental Health came, Heart came with its own money, Dental, Neurology, and pretty soon these across-the-board biomedical research support program in the Division of Research grants had been chipped away with each of these having its own program. And this peculiar dual review system, which is strictly a phoney, if you want to discuss that, (It's NIH, not NIMH history.) blew up, because DiArgie insisted that it continue to run the review groups, at least for the research which they originally had. They refused to give them up, and let them go to the several Institutes ; there was some rationale for this, to the extent they are supporting, let's say, basic biochemistry of life processes, it made sense. But when it came to more focused clinically-oriented programs, such as Lenoirs was, perhaps, one that made less sense. Anyway, the agreement finally reached was that we would hold onto our committees, and the research committee was renamed under the Mental Health Studies Act, until such time as it was necessary to create

PS additional committees and then they would go to the Division. And that's what was done. Mental health was split into Mental Health A and Mental Health B, and one was supposed to be more clinically oriented, and one was more experimentally oriented; and the problem there, immediately was that the clinically-oriented one got all the crap, and the approval rate was about 20%, and the good hard-nosed brass-instrument research went to the experimental one, and the approval rate was better, the priorities were better, and it got very difficult. We had to have some joint meetings. What are we doing here? This is supposed to be mental health, but we're supporting experimental psychology and this psychophysiology and other areas that at one time we weren't sure that psychophysiology was properly within our orbit.

EAR When was Mental Health A and Mental Health B first established as to concurrent committees. Do you recall?

PS Well, it was '54 or '55. It was sometime around '54, I believe. It was about the time John Eberhardt left.

EAR When did you all physically move out to Bethesda?

PS Well, we had been in Bethesda in T6 before we were (I think) before ^{were} formally a part of NIMH. We had been downtown. We went to T6, then we went back downtown, then we came back again as a part of NIH.

EAR O.K. So actually, you were in T6 for a good long time.

PS Yes. Until it was torn down, and they emptied it through my office. I'd had a seven or eight months' stint in the VD Division. Now all good people at NIH came out of VD. Ernest Allen, and Vance Slike, and Heller, and Tom Harrington, and Phil Sapir. . . and I actually occupied the same offices as when we were T6 NIMH that I had occupied when I was with the Division

PS punching a Frieden calculator on rapid treatment centers. 11-day drip, and 21-day drip, arsphenamine, and all that, and then penicillin came and wiped it all out. I was a junior public health representative. That was my official title, wartime indefinite. I kept that wartime indefinite status for ten years. For ten years I was blanketed into civil service because I did not have an advanced degree. I did not qualify for any register, and I was under Social Security, not pension. I lost ten years of my retirement there, and I should have been told, "Save your money, because when you become it, then you can pay back, but I never did.

EAR Life is full of those ironies. The first study section that you recall working with--can you tell me some of the people there, and perhaps some interesting incidents or anecdotes around that very first study section?

PS Well, I wasn't there for the very first.

EAR Well, your very first one.

PS Do you remember, they did an administrative bad thing. They had the chairman . . . the council had alternates, first of all, and the chairman of these three committees also sat in as members of the council with voting rights. Romano, I guess, was the first chairman of the Research Committee; we realized that he was wearing two hats and that he was sitting in judgment on his own recommendations as it were, and so they stopped that. But they still sat in the meeting and they stopped even having the Chairman attend the meetings of the Council. It was a large, interdisciplinary group, ranging from Margaret Mead to Houston Merit in Neurology, and this was before Neurology was created, and in order to meet the needs for review, we for awhile took on the responsibility for supporting research in the field of clinical neurology as well as mental health.

PS (Cont) And we enlarged the committee, and we had a huge committee of about 18 people and it was split right down the middle--the mental healthers, the Margaret Meads, and the Romanos, and the Benjamins, and Escolonnas, and so ons.

EAR And Dave Shakows.

PS And Shakows, I guess. And neurologists, Charlie Ehring and Houston Merit and some of the others--Woolsey, I guess. There was a gentlemen's agreement. You approve the things we mental healthers think is important, and we'll do the same for you. This didn't last very long. Neurology was created, and then they had a neurology or several committees created within DRG to meet their needs, and they happily went their way and we went ours. Again, there were some interesting tete-a-tetes between Margaret and Houston about basic value systems and all this nonsense personality, and we were very psychoanalytically-oriented; if you will remember we had most of the people we had returned to us, including Allen Gregg and Parozim, and Shakow, and Benjamin, and Romano, and Maury Levine. These were all analytically-trained, analytically-oriented, and everything was personality organization, personality development. We were going to find the clue in the fine nuances of personality development in the psychodynamic field, that was our firm belief. And we all came a proper. But that was the atmosphere then, and experimental, physiological, learning psychology was a later development. We had little interest in that sort of stuff in those days.

EAR In fact, any glimmer of that had probably been killed with the reaction to Watson Behaviorism of years before, so we were really at the crest of the whole interest in the/area of analytic, dynamic approach.

PS Developmental wasn't there, and there was a period when we used to moan,

PS (Cont) "Why isn't there more research in development, and whenever you say this to yourself or out loud, it means it's already started, and you are perceiving it but you don't perceive it consciously, and then it began to happen, and now, here we are with this enormous emphasis on the whole developmental side of things which is all to the good.

There was an amusing thing, and I guess this was somewhat later when B.F. Skinner came up for review, and I remember Harry Harlow was on the group, and I forget who else. But, there was a clear split down the middle. It was sort of the Carl Rogers approach vs. the B.F. Skinner approach. And, Harry Harlow--I always sort of respected Harry for this--said, "I am really dead set against B. F. Skinner, all he stands for, his whole approach, but since the majority (and there was a vote taken)--the majority was in favor of supporting the research, then I think it should be given a "1" priority, because if you are going to do it, you should go all the way and really test it out. And that was done. Another one, (since I mentioned Carl Rogers)--he was another split down the middle. Generally one did not have that kind of split where you really had to refer to the Council for a decision on "Policy Issues". It really was again basically, "What is your value system, or what do you think is important in life. But those two things sort of epitomize for me a real irreconcilable split.

EAR Right. Now can you pinpoint the time this was? Was this '51, '52, '53?

PS I think this was mid-fifties.

EAR After John had left?

PS No, I think John was still there.

EAR John left in '54.

PS John left in '54, so I guess it was probably before that. So much of the history is raised up in the documentation of action taken on individual

PS (cont) grants. It just isn't anywhere else really. I think that Stan's action in destroying those early verbatim transcripts was criminal and illegal, and I hold you guys to task for not preventing him from destroying government records.

EAR Are you sure they're all gone?

PS As I understand it, really.

EAR . . . because I had talked to Carolyn one time . . .

PS Lou was told that they were forced to give up their files, the verbatim transcripts of these study sections and council meetings.

EAR Well, I remember that, too, when the whole Eisenhower thing happened, but I had a vague recollection that Carolyn once said there was a set.

PS There was another set, but I think Lou said they were forced to destroy them. Someone should have squirreled them away, nonetheless. It was blatantly illegal.

EAR I may want to go back and talk to Carolyn. You're right, some of the council transcripts are . . .

PS And I always felt.. I do pride myself on having an historical sense and the need for archival materials that have values at that time that we didn't appreciate, but would for an historian later on. I'm sure that would be true in those individual project grants and the discussion would be invaluable to get the flavor of the time. I was going to say earlier, and it's not a part of this. You should go talk to the oral historians (Columbia University was an early center, and I forget the name of the guy.) just to get some pointers on what an oral historian does, even if just some pointers as to what you do with these records, and how you approach people in interviews of this sort. There's too much amateur history. Some of it can be very good. mental
Al Deutsch is an example, in his "History of/illness in America." Here's a

PS (cont) guy who never got through high school that did a magnificent job. Nevertheless, I'm sure they have practical tips and pointers that would be useful.

EAR On that general point, though, are there any other sources of information that you know of that would be useful for me to go to. Incidentally, you mentioned earlier a document that you said you have here, a copy of some early . . .

PS Right. A little brochure, called "The Mental Health Act."

EAR Right. What I want to do is at least borrow those and make copies of them and return them if there aren't extra copies of them; I'll just transcribe. We have a very fecund zerox machine at our place.

PS They are a calamity. We have a wonderful machine. Not I so much as our Treasurer, who is really our investment counsellor, duplicates material like mad. You know these things. It's a disease. I forget what it is, 5¢ a piece, or 8¢ a piece, if you do enough. But boy, the cost.

EAR Some of the girls don't even make carbons of letters anymore. They just take the letter, and . . .

PS That's right, and when I was at Einstein, believe it or not, some gal zeroxed or whatever, virtually a whole textbook for her students. She paid more in duplicating costs than it would to go out and buy ^{a book for} each member of the class. But she didn't have to pay for it out of her budget. It was invisible to her. It was centralized operation. It was a most incredible thing. Hundreds of pages. I forget how many.

EAR Well, let me take you back to '54. John left.

PS John left. I was his right hand man, and I sort of became the executive secretary of the Study Section, and John left. And the search for a successor started. If you're interested in my really personal history, I

PS (cont.) was just old Phil Sapir, Mr. Sapir, B.S., and they offered it to several people, and nobody took it. And I know that Ernest Allan was a staunch supporter of me, he himself being Mr. Allan, the teacher of English in high school or undergraduate college. He was very supportive of guys like himself and me, and I know he spoke to Bob in those terms. It frankly never occurred to me that I would be asked to replace John. I'm sure it never entered John's mind. I know it never occurred to John to suggest that I succeed him; and I don't think it occurred to Felix until Ernest put a bug in his ear. Anyway, Bob turned to his mentor, Alan Gregg, and I had a notable dinner with Alan Gregg, notable to me at the Cosmos Club, and I know in retrospect . . . we talked very little about Phil Sapir, who he is and what he is, but he said, "Dr. Sapir." And I said, "Dr. Gregg, I'm not Dr. Sapir, I'm Mr. Sapir." I know that impressed him. And he referred to me as Chief of the Branch, and I said, "Dr. Gregg, I'm not Chief, I'm merely Acting Chief until a successor is found." I did not realize at the time what this was all about. It is only in retrospect, I realize that this was a crucial interview, but I said the right thing, and from then on it was pleasantries. He obviously told Bob that he thought I was worth seriously considering. I was appointed as Acting . . . I think I was Acting for about a year and a half until the papers finally went their tortuous way through, and they finally got a pay raise through for me, and so on and so forth. I never expected it. That was just at the time after the Career Investigator Program had been established, and for nine months I was a Branch of one, and a secretary, Ginny Brown. I ran the Study Section. I ran the Career Investigator Committee. I ran the Fellowship Program. It wasn't that big. But it was a lot of work, and then I guess, Dick Willy was the first person

PS (Cont) I recruited to help me. Out of the Council of State Governments. Then, of course, Lou and the others came. Lou came before Bert, and Jean, and when I left we had a total staff of 125 and a budget of I think, sixty-eight million for research. We still had the research grants and fellowships branch. Not when I left. For awhile it was the research grants and fund, and then fellowships with all the subsequent changes that occurred in organization. But, I was a branch of two there for a couple of years, a year and a half or so. But the career investig^{ator} program again was very heavily analytically oriented. Alan Gregg was the Chairman, John Romano, Dave Shakow, Blosen, and Ted McGoan was the hard-nosed scientist.

EAR Was the Brain Research Institute established at that time?

PS I don't think so. No. I think he was doing his work when guys like Don Lindsay at the V.A., Veterans Administration Branch at Long Beach, California. That was where they held forth. It was very analytically oriented. In selecting people it was very much in terms of, did they have or were they in analytic training. We got into a lot of this nonsensical discussion, as we now would say about the need for having analytic training, including one's own personal analysis in order to do effective research as we then saw it in the field of mental health, and whether we would accept a person who was not receiving a bona fide analytic training in a bona fide institute (and we turned down Martin Orme because he was a little questionable; he had sort of stopped his analysis in that shady New Orleans---no, it wasn't New Orleans), anyway, there was a lot of nonsense about it. And should we pay for the analytic training, because they were supposed to pay for it, and should we have a separate thing or not, and then we would artificially raise the stipend

PS (cont) so it would include sufficient funds for the analytic training. Then we realized if we did that he would have to pay tax on that part. Whereas, if you break it out as a separate cost, then he doesn't have to pay tax on it, so we separated it out. The first career investigator awardees (none came major universities) and we were all very proud of this. Peter Knapp was Boston University; Henry Leslie was New Orleans; Notch Calloway was . . . we just had a very good record there. And the Danny Freedmans, and the Fred Wordens, and the Notch Calloways, and Peter Dass, and so on, had been . . . had played important roles as leaders. Some have stayed in research, some have become chairmen or leaders, or what have you.

EAR A lot of this is touched on in Bert's book.

PS Right, which I must read sometime.

EAR Well, you made a number of important points, and I wanted to take you back one more step. I think you were indicating very clearly how much you made use of the variety of priorities the Study Section provided for you to establish a very broad-based program. What other facilitating phenomena occurred in terms of program development in those early days.

PS Other than increased money.

EAR Well . . .

PS One can be very broad, very wise when you have money. You can say, well after all, who knows where the next flue. We went into anthropology, physiological psychology, and I think the ultimate was when we started supporting, to his surprise, the musicologist, Allen Lomax and his cantometrics. That was about as far, I suppose . . . And I think we were had a bit, and I used to struggle with the studies, and we were had by academia, because academia wants to be supported for itself. Not

PS (cont) because one might make a contribution to public health, but because anthropology is important in the firmament. And we fell for this. We turned to the academics to sit on our committees, to give us advice about priority areas for research, and we accepted their value systems. I used to argue and say, "Scientific merit is one thing, but one also has to be concerned with relevance continuously, relevance of the mission of the Public Health Service of the NIMH. But that more and more went by the board. It was impossible to handle in the sense of translating it into a priority score. One time Dave Shakow came up with a most incredibly elaborate system of rating. One would rate on scientific merit; one would rate on time limits; one would rate on feasibility; one would rate on relevance; one would rate on need for fund. No matter how you weighted these, you would come out with . . . self-serving, and I think that we were naive, and we were little bureaucrats taken in by the honored powers in academia. The strange thing, as you know, is that NIH- NIMH became the major source of support of the large fields of basic research rather than NSF. We had a far larger social science program than NSF, in other areas of experimental physiological social science, and so on, that were really more properly in an ideal, rational, logical system, but we were on the up and up. We were the wave of the future, and we were glad and we sort of saw ourselves as the Behavioral Science Institute, not merely the Mental Health Institute. But, I discovered much later, some people didn't like this, like, (and I'll mention a name) George Miller, the future president of the American Psychological Association. There's a whole group of people who resent it, having to go hat in hand and accept money from those mental healthers, those sloppy psychiatrists

PS (Cont) and clinical psychologists, rather than getting it in their own terms, their own right. They never did have a psychology or behavioral science institute, but Fred Stone in the Division of General Medical Science tried to play that role, and some of the conflicts that arose between Shannon and Felix, and Shannon and me, and I felt and still feel pretty strongly about Shannon and his imperceptiveness about NIMH, what was it about? They were given a story evidently by people who were more oriented toward biological psychology or psychiatry, or what have you. The George Millers, and the neurology people who had an interest in behavior but in an entirely different context and framework. Maybe some pediatrician, for all I know, too. It was mainly neurological, psychophysiological, and so on, learning,--persuaded Shannon that we were merely supporting the applied, the clinical, those things that were relevant to mental health, and they were evidently saying, " We want to be supported on our own right and not merely those things that are germane to the interests of psychiatrists and others, and the mentally ill. There's a lot more to behavioral science and psychology and social science than this, which was nonsense. If one only knew enough to look at the list of grants, and of course, we used to be belabored on the one hand because we didn't support enough clinical psychiatry, clinical psychology; and on the other hand belabored because we supported too much. And it was so lousy that we knocked ourselves out trying to find enough good stuff that was core mental illness, mental disorder, and we couldn't find it. And anyone that even had the glimmer of an idea, would get, despite his priority rating on scientific merit (it could be 485), he would get it for a period anything that was approved, regardless of priority, would get paid. And we could be

PS (cont) at fault, I think, more for supporting all this crap, but a major theme throughout its history was this fact that we were a festering sore within NIH. Our audience was different. Only 20% (I can only talk about research and well, obviously) of our money went to the medical schools. The great bulk went to the graduate schools, or independent, or to graduate schools. We had an entirely different orientation. As a result, our clientele, our audience, our consultants, our advisors were different. And I feel that guys like George Miller did us dirt. And so, they went off and sold this bill of goods to Shannon and Fred that there needed to be a basic behavioral science training and research program that was separate. It fell apart, and then they chopped out. Then the Kennedy Institute for Retardation came along, and they chopped out a third of our program and took the physical from Heart, and created Child out of that. And they glommed onto behavioral science, but it was last on the bandwagon and the first off when the going gets rough. I think that these people have made a big mistake. I still think as a mental healer, an NIMHer, and I think they made a mistake by going off in these directions; because even now NICHD, the behavioral approach^{es}/to child development take second place to reproductive biology, population, mental retardation, and so on.

EAR Well, you know, of course, that when Fred Stone came along and got the go ahead signal from Shannon, and George was brought in to write this special report, which was intended then to revive it, he did it. I remember those days as you well, do, do too. And I remember very well the comments that I would have with the intermediary, namely, J.H.U. Brown and Co., about if you're going to do a good job, we obviously don't want to keep it all for ourselves, but if you're going to do a good job, you have got to hire good people like we have here.

PS They just wanted to spend the money, that's all.

EAR I wanted to carry you back, though. We skipped over a couple of things I wanted to touch on. Let me give you an illustration of what it is I am talking about. There are a number of events that took place, some of them fortuitous; perhaps some of them thought through, which really made major differences in the subsequent growth and development of the program. (1) It's been talked about many, many times, but I think it's an interesting case in itself is how a small grant program began where you really had, because as I recall and understand it, the recommendation of one member of the committee, namely Harry Harlow, that to do something like this, that you really had the development of the major new influence on how support developed, and how young investigators were supported, etc.

PS I will tell you, and that isn't quite the way it happened. I hold it against Harry to this day that he took the major credit for that.

EAR O.K. Now, I want to straighten that story out.

PS Well, you judge. First of all, there had been some noises within the Council Committee Study Section about the need for some such program and reference was made to other such programs. Sigma Psi had such a program, and the National Research Council had such a program. Very small sums. My father had a grant of something like \$500 or \$800 to go do field research with Indians way back in those days, and it was regarded as ^a very successful program. But we did nothing about it. Harry came as Chairman of the Division of

PS (Cont) Anthropology and Psychology, whatever it was called in those days. It is now the Behavioral Sciences Assembly, but it wasn't so named then. He was the Chairman, and Glenn (or Guy, his name escapes me) came and said, "Would it be possible to get a grant to support their small grant program within the National Research Institute. They used to have it up with Rockefeller money. It was very successful, and they cited the names of all the grades--the Edward Sapirs, and the Wernernitzs, and the Clark Halls, and the Frank Beeches, and others. It was a very distinguished list of an earlier generation of people who got support. Couldn't it be done?" I said, "Well, I doubt it. I'll talk to Ernest Allen about it, but I didn't think so, because there was a very strong policy against sub-contracting and giving money that would ordinarily be given to the primary recipient of the funds, a policy that we subsequently began to believe more and more was mistaken, but that was our policy as you know." Then the answer, of course, from Ernest was, "No, we can't do it." But the idea was still banging around in my mind, so then I said to Harry, "Well, we can't do that, but maybe we could set up such a program of our own, and do it, and it wouldn't be quite the same. We wouldn't cover the same fields. Our interests are different from yours, but we could cover some of the fields, and maybe we could do it. Well, Harry was delighted to hear that maybe something could come out of it. Although this was more in his role as the Chairman (I guess he was Chairman of

PS (cont)the Study Section. Was he ever Chairman of the Study Section?

No, he was the first Chairman of the small Grant Committee because of this.). In any case, he thought this was a fine idea. I talked to Ernest again. Ernest was interested. He was willing to go along. Ernest was a very agreeable personage. But he felt he had to check it out. He checked it out, and Shannon or whoever, Vance Leiden (I think by this time he had moved up into Shannon's office as Social Director, and Ernest had replaced him as Chief of DRG. This was almost as bad as Phil replacing John Eberhardt.), and Shannon said, "I'm tired of each of these Institutes setting up their own career investigator program." Neurology had its own senior research scientists. Heart had its lifetime heart fellowships, and so on and so forth. No. If it's going to be done, it's going to be done across the board. Then took place 18 agonizing months of in-the-house arguing on my part through the ECEA (Executive Committee for Extramural Affairs). We argued and argued. There was a famous meeting where all the pros and cons were laid forth, and the guy who really helped put it over the top was Ralph Maeder of the Cancer Institute, who took great umbrage at the idea and snorted and snarled and sneered, and ^{said} "I suppose next, Phil, you're going to have a medium-sized grant program. Then you're going to have a large grant program. Then a super." Which, of course, is what subsequently happened with these so-called project grants. But we didn't know it at the time. Well, he so exaggerated in one of his long three-page single

PS (Cont.) spaced memorandums that went to everybody, that I could very easily go down point by point and refute what he was saying as to what we really intended, what the purpose was. It was a little romantic and idealistic--the hot lead--we never found those hot leads (the interim, the emergency, the neglected younger investigator, the small liberal arts colleges); we went down the list of all these things, some of which came to pass.

EAR Don't forget, you gave to Jolly West . . .

PS This was Sleep, this was the TV March of Dimes, whatever it was--the all night telephone, and we wrote the application, typed it up for him, he signed, and from the time of application until time of award (at least notification of award) was two weeks. That was a record. And we had got a delegation of authority which was subsequently ruled illegal, but we had a delegation from the council to take action after the committee met, and we were getting awards out within a month--six weeks, and it was great. Somebody made the mistake of asking a lawyer, and of course, got all No's. Of course, that was after I left, I guess. No, maybe it was before. But the idea was to be flexible, to be quick, to have a separate committee with its own criteria, its own standards. And those guys could spend as much time over a \$3,000 grant, as more than a program project giving 3-1/2 million dollars away (Cecil Whitson). Anyway, I wrote up the specs for this thing, and I gave it (I was very modest in those days) to Harry. He said,

PS (cont) "Well, why don't you present it, Phil. You wrote it." I said, "No, this was your idea. You present it." So he presented it to the Study Section which bought it and recommended it to the Council, and I forget if Harry presented it to the Council or I presented it to the Council, that this is what I think. And the Council bought it. Then there was this long battle, after the Council had strongly endorsed it. Eighteen months later, it finally was agreed to. All the other Institutes were going to have a similar program, and they immediately killed the program on Day 1 by refusing to set up separate Committees, and instead referring it to the Study Section. The Study Section said, "What's this? It's a separate program, but it has to be reviewed at the same time and concurrently with regular ones. There never was a minimum. You could always ask the \$3000 if you wanted to. But our argument was that they would get short shrift if they didn't have any way to get / going; the arguments were /clearly there, and the program got going, and then all the other Institutes fell by the board, one by one, although the allergy and infectious diseases Institute (I forget his name now; he was the program director for research grants at that time. He bounced around from Institute to Institute.). Anyway, there were two or three other Institutes that sort of liked it, but some of them didn't and it just didn't make sense, because they refused to set up the appropriate mechanisms for it. We were left alone. I guess at that time, all the arguments about NIMH being dismembered or

PS (Cont.)being kicked out or walking out had gone, and they really couldn't care less, and it's still a good program; it's still one of the best things that happened, and I think, myself, that the idea of a small grant program and a medium-sized grant program is a great idea, and people could then be measured comparably. We have a small grant program here, only it is up to \$25,000. I think that would be a good limit. And then, one could have up to \$100,000, and then you're looking at things that are comparable in size, scope and the like. The other thing was our fellowship program. And this isn't known to many; and our anthropologists--let me start again--the fellowship was predoctoral and postdoctoral support. Very few, and there was no support for the cost of the research except for maybe a \$500 supply grant, and the thought there and with NIH , of course, was chemicals on the shelf, and the like. The anthropologist has to go off to Bongo Bongo Land on a field expedition. This can cost several thousand dollars and what can one do. We had a meeting. Dave Aberly was on at that time, and I had a meeting with them, and I again decided to see what could be done. I came up with the idea, presented it to the subcommittee which bought it and sold it to the study section, and reported to the Council. It came into being. The idea was this: That there would be a corollary research grant for the cost of the research, but this would be reviewed by the Fellowship Committee, not by the Grant Committee or Study Section. And so this was bought. A man applied for his stipend and he

PS (Cont) applied for his grant. We received them simultaneously by the Fellowship Committee, and we had a pot of money set aside for these things, and everything was go. The interesting thing about that program was that all of a sudden the anthropologists started pouring in. Here was the thing for them, and we had a far bigger program for support of predoctoral and postdoctoral training in anthropology than NSF had, and 18% of our fellowship program (it was so popular) went to anthropology, and some of these grants got to be quite sizable. I remember one something around \$14,000. Anthropology, by that time, had learned to live high on the hog like all other academics. They had to have their jeep; they had to have their paid informants; they had to have their tape recorders; they had to have their Zeiss cameras. I know I felt the need to cut back on the thing. It was really getting too excessive. I will have to say that I got increasingly uncomfortable as to what the religious practice or the agricultural practices or the primitive law practices (whatever) of the Bongo Bongos in New Guinea had to do with mental hygiene or mental health, no matter how broadly defined. We had really let ourselves go. These guys would throw up their hands and say, "Phil, you're asking the impossible. We can't say what is more or less relevant." And then we got into linguistics, and social linguistics, and psycholinguistics, and they just found it impossible. They said, "Well, if it has to be done, you have to do it as a program person." Or the Council, and this theory of the dual-review which was never really the split between scientific review and policy that

PS (Cont) subsequently was given as a rationale. The original rationale was that the Council didn't have the time to do this, and the committees were supposed to be subcommittees of the council. They did the details, but they were supposed to be concerned with policy. That was one of the charges to the Study Section-- to be concerned with policy, and what the needs are, and how best to develop the field. All for the purpose of furthering the public health. And it was very difficult to do. There I am off on another tangent.

PSR Well, those are two things that happened. The third deviation from the core research grant where the emphasis was put on the man and his experience--his competence and the research proposal he submitted was the clinical research center and the program project. . . Something I was opposed to, that I fought. I was in on the original NIH Committee that argued over whether as a subcommittee of the ECA or some such thing advisory to Shannon, and I was "agin" it. It was something that came out of Cancer, it came out of Sidney Farber, and the Farberettes, and his original idea was not a little clinical research center, but a great big Federally supported Cancer Institute in the major Universities for himself and others. It's funded at the level of several million dollars a year; when it came into being it was a metabolic bed unit of six or 10 beds where the Federal government paid the research bed costs and all it cost for a metabolic unit and all the rest of it. And then the idea was O.K.'d; now it was going to support the bed unit; now it

PS (cont) was going to support the cost of insurance that goes on in these clinical research units, and then came this horrible term that Ernest Allen dreamed up, "Program-Project," and the only reason Ernest Allen did it was because the law refused the term "projects" and not "program" and he refused to use the term "program grant", and so it was a "program-project" grant.

EAR Now, by that time was the Fountain Committee already underway?

PS I think so. But, to talk about Ernest again, the reason for the dual review system was that Ernest saw the DRG melting away, Ernest and his confreres, with nothing to do, because each Institute was getting its own money and its own Review Committees, which they were allowed to keep in training, because Ernest didn't care about training. He was a research grant man, and so he devised the idea, "O.K., we will run the study sections and the Institutes will be responsible for policy, but we will be responsible for scientific review, and all of that." Now, DRG for a long time had its own money, and that was a great idea, because it could step in and fill in the gaps if Heart ran short, or arthritis, or the like. Then Ernest could pick in and pick up areas that weren't like popular, /anesthesiology, or surgery that didn't seem to fit into one of the "categorical" Institutes. And I think it was too bad when they did away with DRG and created DGMS. Then it became another categorical Institute fighting, and it was very nice to have this sort of a safety valve.

EAR Really, a number of points in what you said are emphasizing very nicely for me. What I really want to be able to make as vivid and correctly ^{given} /as possible is the interplay of individuals, and

PS (cont) what they brought with them as individual points of view, and biases, if you will. Like Ernest's. Ernest didn't care about training, so that was that. And NIGMS, formerly DGMS, largely took its character from people like Fred Stone, who had a major interest. Anesthesiology is an area where Ernest really tried to push, you recall. I'm sorry, not Ernest, but Fred. Fred came along and thought, "Oh boy, here's something to pick up." And so he did. Ernest Allen, English professor, got into the whole act through the VD rapid treatment centers. And he was a program representative who went around selling the program down South, and that's how he came in touch with Vance Slight. Vance Slight then became chief DRG, and Ernest had done a good job as a public relations man. Then he brought him up there, into the Division of Research, to help Romanos, the Program Administrator. And that's how it all started.

EAR And now, if I can get to talk to Shannon, I will.

PS I want to get back to the Clinical Centers. This idea got going with the Sidney Farbers and the like, but it was Mike Gorman who glommed onto this, and he, of course, had been beating us over the head regularly because of this basic research approach of ours. Why didn't we give money to hospitals, and then came along HIPs, and so forth, and he was very strong on clinical research centers in the field of mental health, and this didn't mean just the basic core component, a metabolic unit, a research bed unit. He meant the whole thing. We were all frightened, and we were frightened that something would happen, which in fact

PS (cont) did happen, that if you couldn't get your 50 or 100 thousand out of a research committee, then you asked for \$3,000,000 and you got it through the Program-Project Clinical Research Center Committee, which is exactly what happened. And what happened was that word got around that NIMH had this new program and this bundle of money, and promises again were made to Fogarty and Hill, and so on, in the person of Mike Gorman, and Nate Klein and others, and everybody submitted his application which applications were a crime to humanity. They were all these clinical directors of State Hospitals in the New Jersey Neuropsychiatric Institute and places like this, with a lot of these third rate biologically oriented guys looking for the schizococcus or schizo-amine, or whatever it was, or the schizospike, and the amigdeloff, poured in with / ^{their} huge million dollar proposals with nothing to go on. We turned them down left and right, including Nate Klein, for a big clinical center out at Rockland. Which didn't make Mike Gorman happy, didn't make Nate Klein happy, didn't make Bob Felix too happy, and that's when I first . . . and I was referred to in Mike Gorman's testimony as that "untutored, untrained, uneducated person" riding herd on this research grant which was absurd, because here was Mike Gorman, this "untutored, untrained, uneducated" person wheeling and dealing, but he didn't like it when it was Phil Sapir. Anyway, there did come a meeting, and I resent it and hold it against Bob Felix to this day (I have a long memory, a vengeful and resentful person. I don't really now.). But

PS (cont) Bob Felix was feeling under the gun politically to come and do something. Here was this program, all this money, and he had Phil Sapir, this low-level bureaucrat trying to run it logically, rationally, by the book, and there were a couple of things that had been either approved with low priority, or disapproved with a split vote, and one was Cecil Whitson, who was a "macher" and one was, I think, Jack Ewalt. And Felix went around whispering to the council members that it was very important to him politically to have this done. Gorman was on his back. Fogarty or Hill was on his back, and he had to show something, so the council reversed these things or referred them back to the committee. We went through this nonsense. Anyway, the long and the short of it was that I was not brought in on it, and I just wish that Bob had come to me and said, "Phil, this is a political thing. Don't worry your bureaucratic little soul about it. It has to be done." He didn't do it, and he left me in an impossible position sitting up at the head of the table, arguing vociferously, rationally, logically, defending the Review Committee, and he had already briefed these council members that he had to do it for political reasons. And I was completely in the dark and didn't realize it until much later what was really going on. And that program caused nothing but static, from then on, and we gave money away by the hundreds of thousands and millions of dollars for utter trash that would never have gotten through any responsible research review committee. And it's still going on, I gather, for better or for worse, and mostly for worse.

PS (cont) There are other chapters. There's the International Program, in which at one time, we were so heavy that the Research Program for NIH as a whole got up to something like \$17,000,000. The next was going to be \$28,000,000 or something. And all of a sudden, something happened. The dollar balance came along, and rather than withdraw troops, or other things, the finger was pointed at DHEW and similar programs, and we had to cut back and in a relatively short time it was successfully cut back, and we were left with nothing but old PL480 funds and restrictions abounded; but we were going strong. I recruited Jeanne Brand to head up an International Research Program, and we thought we were being incredibly wise, foresightful, broad, and all the other things with all this money. And I think we did do a good job as long as the money lasted. After all, we said the cancer cure coming out of Japan, China, or Russia, is just as good as coming out of Massachusetts Mental Health Center, or MGH. Too bad, that it went by the board. Now we have the Fogarty Center. That was a big thing.

Historical research was something I will start . . . and I won't talk about Rosen, if you don't want me to. But this was something that I had to fight through at ECEA. I don't remember if you were in there in those days or not, but a lawyer argued that it was illegal. That this was not research. Research to him meant test tubes, chemicals, retorts, rubber tubing. This was research. Historical research was not research. It was illegal. The law didn't permit it. But we had learned not to

PS (cont) directly ask the lawyer a direct question. You asked for an informal opinion, which is not binding or holding. You just asked him for his sentiment. And I snorted at this guy, whose name I forget, luckily, Goldblatt, or something like that. He had an incredibly narrow vision of what research relevant to public health was all about; but anyway, we got it going. It started, and this again was something that we worked up through Study Section; it was council endorsed; again we had to take it to the NIH to get parental blessing and it had to be across the board. But at first it was within NIMH. And we had our own little Ad Hoc Committee. And Dave Shakow was on that and some historians that Jean had known, like--well, I forget the names--Rosen was one of them. And then, when they saw it was successful they created a life-science history study section. And our things went to it, and there we had our program. And Jeanne and I presented a joint paper at a meeting of the American Historical Association, (I have a copy of that) simply reporting on this fact. And I guess it's still going on. I think it is a very good thing, and should be very much continued. You don't really want me to talk about Rosen, do you?

EAR No. I know enough about that, but I'll get more from Jeanne. Which reminds me. Just one quick interjection. Have you talked to Jeanne recently?

PS Never. I have not seen Jeanne since. She keeps saying on her postcards that the next time in New York she'll look us up, but she never looks us up, and I know she was at an AAAS Meeting,

PS (cont) but for better or worse she has never done it. She has a standing invitation from us to look us up.

EAR Well, I talked to her several months ago. We turned in an application to the National Library of Medicine. She sounds fine. Seems to be enjoying what she is doing.

PS I gathered so from her notes. It sounds cheery and she's carved out a real niche for herself there, and she has all this counterpart money. She has become a Polish expert. She goes to Poland a lot, and Israel, too. I thought 480 money ran out, but now they use contract money, or something.

EAR Well, O.K. Now. incidentally before we finish, I do want to come back, obviously, (you haven't hardly touched the surface) at least a couple more times as this thing develops further. Psychopharmacology is one. What I am obviously trying to do, and I think you are reinforcing for me that this can be a good approach, the way you are commenting on some of these things. I do want to touch on a lot things that I don't go into some depth on, but there are a number of areas in which they provide beautiful case illustrations of exactly the major theme that we talked. Now, how did psychopharmacology first come along? What were the expectations and the intentions of that time. What were some of the political problems? What were the staffing difficulties? Why did you hire Sherman Ross? Whom I saw last week.

PS I didn't hire him. Jonathan Cole did, and I told him to be very careful, but Jonathan was semi-autonomous as you know.

EAR I have to make this one additional point because I know you'll find it amusing. I saw Sherm in Washington. He's at Howard University, but he also has a foot in NRC. Henry David, you know, is gone; and Betty almost got that job. I guess they offered it to her.

PS Well, no, she turned it down. Dave Goselyn, he's/^{was}there at Russell Sage; he was vying with Bert Brim for the Foundation for the Child Development job.

EAR Well, anyway, I was there last weekend. I had an APA meeting, and Sherm Ross had an APA meeting. He was downstairs in the Board room, and someone told me he was down there, so I went down to say "hello". Now, I haven't seen Sherm in more than five years. Now, we go way back. We graduated from CCNY together back in '39, so I know him. And at the time that Jonathan hired him--in fact, Jonathan talked to me about the possibility of that job, too, because I was then in the *downstairs*; but anyway, I go down to say "hello" to Sherm. He cannot even say hello without making it sound as if he is giving a lecture . . .

PS Or he's trying to get something out of you, one or the other . . .

EAR It's so ingrained. Of course, I shouldn't be surprised. It's obvious that people don't change at that age. But anyway, we spoke for about five minutes . . . I guess we stopped in the middle, but that's all right. I do want to come back and talk with you some more. Are there any other (while we're kind of rambling around here)--are there any other circumstances--I want to hold the psychopharm subject. But are there any

EAR(cont) other circumstances besides psychopharm or other things that I really should be alert to that I may not have been, that I should keep in mind as . . .

PS Well, yes. Something pops into my mind about the NIMH-NIH feud.

EAR I've got a whole chapter on that.

PS NIMH, of course, insisted on this tripartite research professional training and service programs. And NIH disliked this and wanted us, the NIMH, continuously to give it up and fly right and be like all the others. The interesting thing is that if you read the basic charge in the original Cancer Act, and the original Heart Act, Congress had exactly that in mind for them, and they were supposed to do those other things, too, but instead they sent them "downtown", and you remember for awhile Cancer had this sort of Applied Research Demonstration Program under Ray Kayser, I think was his name. But they were always unhappy with it. They had been captured by Academia, and they thought that way, and they wanted no part of it. And I think they would have had much better programs, and they would be much more secure (hindsight) today if they had mixed up this stuff; and think that if Fogarty and Hill had really been on the ball they would have perceived that the original Acts had not been properly implemented, and that instead, their forces had been dispersed throughout public health and the like. And it was not Academia for Academia's sake. Which they tried to make even more official with the Huntley Report, and that's an interesting chapter in and of itself, we'll want to get into.

EAR (cont) Well, the whole issue of how NIMH did not sit in NIH things, serves as a beautiful counterpoint for what we did do. I want to ask you a question about one strategy about which I'm really a little bit uncomfortable because it seems to me that the whole Intramural Program could be a book in itself. There's just so much that has occurred, and that has been achieved, and has been accomplished there. Yet, I cannot do two books; I'm really not competent to do a book on the Intramural Programs. Maybe I'm not even competent to do this one, but I think I can. And yet, I don't want to completely ignore the Intramural Program. So I have in this outline that the Intramural Program will be included, and of course I'll talk to John and Bob and Dave and everyone else there. What would your feeling be? Or give me your reactions.

PS I'll give you a few reactions, and I think you should at least address yourself to these kinds of points, which is the interface between the two. The rationale for the Intramural Program was that only the Federal Government could fund the kind of a Research Center that it is, that it was thought of, that it became, that was built. This was before the Grant Programs grew to their subsequent size. If it had been known that that kind of money would become available. In other words, the growth of the Grant Program cut the ground from under the rationale for Intramural. But I would argue that once it existed, it existed. It was one of the outstanding World Centers, and therefore, it should continue. True, it would not have been

PS (Cont) and should not have been created if it had not turned out as it did, that all of these monies would become available to support these kinds of expensive research activities outside of the government. And so that was the rationale for it. Now, there were a lot of people who were in Academia who were very afraid of the Intramural Program, because they thought it meant that the Intramural Scientists would ride herd on the Extramural, and they would approve and disapprove things in the light of their own personal preferences, and their own personal research and philosophy, which the Child Institute had sort of tried to carry out, and some others subsequently. And some people, I believe, in Congress, used to believe that yes, you should use your Intramural Scientist to make these decisions. Why call outside people. And this really scared a lot of people on the outside who knew these guys. They said, "Why should I be judged by a Sherm Ross, (by a whoever, a Seymour Ketting)." And if Seymour Ketting had been running Extramural he would have run it with his very strong biological orientation. John Eberhardt, who I had dinner with the other day, was complaining about Seymour. I never heard John say a critical thing about Seymour in my life. They are very close friends. But you can see Seymour is still beating him over the head as he used to beat me. Why don't you guys do more. You're still all that personality, clinical, psychoanalytical, interpersonal crap. What you need is good hard-nosed basic brain science. He's still on that thing. He used to argue that we should fight

PS (cont) and I did fight the noble fight with Neurology, at least when it came to brain and behavior, but that wasn't enough for Seymour. Seymour wanted the whole brain, with or without behavior. And I argued, why bother? They have lots of money. Let them support that. Then we are free to support the stuff that nobody else but mental health will support. You get into some fields, and there are four Institutes fighting over it. There's NIMH, there's NINDV, there's a Child Institute, and there's GMS, like Communicative Science. Fred wanted communication, Child wants it, we want it at NIMH, if I can put that hat back on, and the Dental Institute wants it. It turns out that anything from the neck up, that is adjacent to, or supportive of the oral cavity within which the teeth are located, and the Dental Institute has a very strong program in Communicative Science. From away back, for a number of years.

EAR There was Aaron Gans. You knew Aaron Gans, didn't you? I won't interrupt you. Go ahead.

PS Anyway, Intramural, I think, certainly can be mentioned in the book. To clarify what things were like then, and the rationale of why it was created, and how, perhaps, a mess it turned out to be in the light of subsequent events.

EAR Right, and also there are in part of the effort to describe (delineate) some of the dramatic differences between Bob on the one hand and Stan on the other, it seems to me that what happened in the relationship between the Intramural Program and Bob, and vice versa; and Stan and the Intramural Program, that

EAR (cont) there are many things that can be said without making invidious comparisons. There are many things to be said as the relationship with the Intramural Program occurred, that would further dramatize the difference between those two people, and the way they handled their individual Directorships, and I think--in one sense, I feel that I'm in a kind of an unusual position because of my own personality in relationship to all three. I don't think there are too many people that have had a reasonably positive relationship with all three at quite the level that I have--literally with all three. And so, I think I know their strengths, and I think I know their weaknesses--all three, and I think I can do a reasonably honest job of presenting them that way, because obviously there are strengths and limitations in all of us.

PS I'm not sure what you are referring to in the differences, but an important theme that we touched on is this whole question of, "Can you program research? Is it meaningful to do targeted research?" And this came out in not only Extramural, but Intramural. Some people felt strongly that Intramural should be targeted research. Vs. the support science, the best of science, who knows what fundamental research will turn out to be more relevant than some applied thing. We just don't know that much and so that is the basic argument, the basic dilemma. And maybe this is what you referred to--the difference between Felix and . . .

EAR That's part of it, although I think that is more easily described from within the Intramural Program. I think I still have the

EAR(cont)piece that Bob Livingstone did before he left, you know, and all that stuff. And I certainly am talking to John and to Bob Cohen and to Dave. We'll get that point very dramatically presented, I think. No, I was talking about a very specific issue which I think relates to the way people responded and reacted to Stan when he took over, and I think it was perhaps most dramatically indicative in the Intramural Program. Where they felt that they had lost their friend in a sense when Bob left, and that Stan, because of his proclivities and intentions was really going to downgrade the Intramural Program dramatically. Now, my bias will begin to show, because I talked to John on many occasions in the past about this, and he was always concerned about what was Stan up to? And I do know and John was there on many occasions in small staff meetings when Stan was very supportive of holding monies for the Intramural Program and giving Intramural support. What he didn't do that Bob did, and this is an essential difference--Bob was in some respects more liberal with delegating responsibility than Stan. Stan cannot allow anybody else to have the final decision.

PS In retrospect, I am amazed at how much Bob did delegate authority, and how nice it was, until this Clinical Research Center came up, and then until Bob began to get stars in his eyes, and was pushing for the Surgeon Generalship. Bob's strength, and he didn't know it was his ability to turn to good people and follow their advice (like Allen Gregg and others). When he decided he could go it alone, he got into trouble. But it is certainly true that we had a delightful degree of autonomy and the sense of responsibility

PS (cont.) that went with it, which you didn't have with Stan.

EAR And I think that is the important difference. The other thing I want to bring out. This I think may fall flat on its face, but I have in mind that to some extent the time of development of NIMH was somewhat consonant with the strengths and weaknesses of the three Directors. I don't think Bob . . .

NLM NOTE: Interview tape ends abruptly here