

**Robert A. Cohen**  
NIMH 1952-1981

Interview Date: January 29, 2002

This is a third interview with Dr. Robert A. Cohen, former Director of NIMH Clinical Investigations from 1952-1968, and Deputy Director of the NIMH Intramural Research Program and Director of the NIMH Division of Clinical and Behavioral Research 1968-1981. The interview date is January 29<sup>th</sup>, 2002, in Bethesda, Maryland, and the interviewer is Dr. Ingrid Farreras of the NIH History Office. Dr. Morris Parloff is also present to assist with the interview.

Farreras: I've been looking at some of the telephone directories from the '50s and '60s as a way of trying to document all the different labs of the NIMH intramural program and there were some questions that have come up that I wasn't sure about. You had mentioned that when you arrived you were really trying to hire a lot of sociologists and psychologists, that you really emphasized people with the Ph.D. degree, not just an M.D. degree. What was the reason behind that? Was it because you yourself had both degrees that you thought that was important? Why was there an emphasis on hiring other people aside from psychiatrists or M.D.s?

Cohen: It seemed to me that what was lacking in psychiatry was a powerful theory of behavior and that one had to know more about human behavior than you were going to learn in a mental hospital, and that, in effect, what I was trying to bring together. It's true that it would bring together the things that I had gone into, but my interest in psychiatry had developed not because I had any great interest but

because the University of Chicago's South Side Medical School did not yet have a department of psychiatry. It was not because it was unimportant, but because they thought it wasn't scientific, that it wasn't based on sound knowledge of behavior, like the study of medicine was based on sound knowledge of anatomy and physiology. And so I looked upon this as an opportunity to bring these together. Did you read the chapter I wrote in Stetten's book?<sup>1</sup> I give a very brief history of psychiatry and talk about the intramural program. Myer was my first teacher of psychiatry and he refers to some of the sorts of research that have been done. It seems I was in a position of being the first person to finally bring together all the things that really refer to. I wasn't trying to do anything small. But this seemed to me a unique opportunity, and in a sense an opportunity very much like my immediate professor when the South Side Medical School was established, because it was regarded as the first full-time school of medicine to use all of the disciplines were going to be involved. And the reason psychiatry was left out at that time was not because it wasn't regarded as important, but because they didn't know of anyone yet whom they felt was a good enough scientist. And what happened was that they decided that Roy Grinker, who was the assistant professor of neurology, was a good candidate to develop the department of psychiatry. He taught my class, neurological diagnosis, and then he went to Europe on a fellowship, was analyzed by Freud, and then came back – after I had graduated

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<sup>1</sup> DeWitt Stetten, Jr. and W. T. Carrigan (Eds.), *NIH: An Account of Research in its Laboratories and Clinics*. (New York: Academic Press, Inc., 1984).

from medical school – to start a department of psychiatry at the university, which was going to be a scientific department, and Grinker, as professor of neurology, actually [unintelligible] other departments, both pre-clinical and clinical. And so this was the sort of attitude that I had been exposed to from the very first. And then when I went to the Phipps Clinic, Dr. Myer in particular had a very broad interest, and he was rather critical of some of the earlier studies done at the New York Psychiatric Institute, where they had brought in physiology and physiological chemistry, because he felt that they ignored the patient. He was himself a neuroanatomist and his wife was one of the first social workers at the psychiatric hospital. And what he was emphasizing was that you should have all of that plus the study of the patient. And so the reason I was looking for people who had Ph.D.s was that I thought that psychiatrists with Ph.D.s would be interested in what was going on in other fields besides psychiatry, more so than people who were primarily psychoanalysts and wanted to learn more and more about psychoanalysis. Not that they wouldn't be experts in that, too, but I thought that Ph.D.s would be more likely to be enchanted by the fact that the NIH didn't pay very much, but gave you extraordinary support to do studies. So I would say that this attitude was really typical throughout the institution at NIH at that time.

Farreras: Would you say you were pushing for the Lab of Psychology more than Kety was?

Cohen: Oh, yes. Kety was an absolutely distinguished physiologist, but I don't think that he had a particularly broad general interest at that time. And that was what made me decide to take the position after a lot of agonizing, because Chestnut Lodge

was beginning to work out pretty well. I'd been a consultant over at the Naval Hospital – since I'd been in the Navy during the war and knew the people there – and Harry Stack Sullivan had reestablished himself at Washington and was very much involved in the work at Chestnut Lodge. At the time I started my residency in psychiatry there were 157 psychoanalysts, members of the Psychoanalytic Society and 1,179 members of the American Psychiatric Association. So psychiatry was really quite full in a way. There were a few departments: Boston Psychopathic Hospital; New York Psychiatric Institute, which was set up as a sort of a research center in New York; the University of Michigan had a psychiatric institute; the University of Iowa had a psychiatric institute; and Johns Hopkins had a psychiatric institute. But there was very little research going on.

Farreras: You had mentioned earlier being deluged with applications of people who were trying to avoid being drafted in the war but at the same time you mentioned that when you were talking with Shakow about all these people you wanted to bring in as lab chiefs, nobody wanted to come.

Cohen: Well, they were all interested, but the salary was a great disincentive so people had to feel that the opportunity was so great that the salary didn't matter. On the other hand, people sometimes ask, "How did you find all these people?" and I really think that they found us. But in reality, I did succeed at getting several people to come. Dick Bell was already working in Joe Bobbitt's office, and I remember Bob had told me about him, that he had passed very high on an examination of some sort, and thought he was very bright and might be someone

we should have in the intramural program, but he was in the extramural. And there were two psychiatrists here, Gove Hambidge and Lou Gottschalk, whom someone in Felix's office, had gotten interested in. They'd sent Lou Gottschalk to Chicago to get some psychoanalysis – in those days it was looked upon much more favorably than it is today – and George Raines, who was the commanding officer at the psychiatric service at the Naval Hospital, was a student in the Psychoanalytic Institute. That might have been one of the reasons that he had appointed me and David Rioch and Larry Kolb as consultants there.

Farreras: The 1953 telephone directory lists the Lab as the Lab of Clinical, Experimental and Developmental Psychology, with Dick Bell as Acting Chief, but within a year, the name changed and it became just the Laboratory of Psychology, now under David Shakow.

Cohen: That was an administrative listing. All of those are just fantasies. They're not organizations. All they do is reflect hopes and dreams. There wasn't a lab at that time in the sense of a meeting of the laboratory psychologists. That's how I recall it. Some young psychologists came. The psychiatrists who came, who were avoiding the Army or Navy, were delighted. They were very eager to come, and so it was a matter of trying to pick people out. And if someone came who had been a chief resident somewhere or an assistant resident rather than just at the beginning level, I accepted them. And if someone came who had some psychological training, that would be good, especially because he could talk to the psychologists. Some psychiatrists just look upon psychologists as servants who

give them a little information about the patient, which they're not really interested in but which makes it look more scientific. So I thought that I would do better.

Farreras: Would you say that the people you would bring in or who were following you, for example, Dr. Parloff or Earl Schaefer later on...would you say that the sections of the lab were built around the people that you were able to find to come here?

Cohen: That's right, yes.

Farreras: Or did you decide what sections you wanted and then tried to find people to place in those sections?

Cohen: No. We created the laboratory around the people. So when you see a name change, it's entirely an internal matter that grew out of the interest of our commitment to them, in a way, we were trying to make the suit fit the person.

Farreras: And once you had all the sections who hired the chiefs? Was it Shakow who had direct responsibility?

Cohen: When Shakow came, he tried to recognize the relative seniority of the various people and put them together that way. I knew that I wanted to have child development and Nancy Bailey happened to be here, and then later on, Harriet Rheingold came. It was such a small world at that time. When I had spent a year at the Institute for Juvenile Research, this was the first time that I'd really been exposed to close interaction with psychology and sociology. Phipps just had two psychologists, and Shepherd had one psychologist – who later became dean at Johns Hopkins University.

Farreras: So, what criteria were you using for bringing psychologists in?

Cohen: I think that it was entirely based on my feeling about the person. They talked about their interests. For example, I was disappointed by the fact that so many of the people came from Harvard and Yale and so few from the University of Chicago.

Farreras: So there was no preference for young scholars who exhibited promise in a certain area versus people who were already established in the field?

Cohen: Well, for the most part, I felt we were going to be limited to young scholars.

Farreras: Because of the salary and because they were already established at the universities?

Cohen: Yes. There was one person, for example, Leon Saul, who was in the Psychoanalytic Institute in Chicago when Wade [Marshall] and I were in the physiology lab. I had served with him for a year in the Navy and knew him very well, and he was an established author and also knew some physiology. He actually came down and met with Shannon and talked about salary \$15,000 [unintelligible] not being able to do any private practice. George Saslow, who had a Ph.D. in physiology, was then professor and chairman at Oregon, and I thought he might be interested, but he wasn't. There was little chance that Adelaide Johnson would come – she was also a Ph.D. and had preceded me at Phipps Clinic, and her husband had been the assistant in physiology when I took my first course in physiology – but if they had come, they would have been very good. But they didn't.

Parloff: Along these lines, had you ever considered Jerry Frank? He also had a Ph.D.

Cohen: I knew Jerry Frank. Yes, I talked to Jerry, in fact. But Jerry was very happy at Hopkins.

Parloff: That was my impression, yes.

Cohen: And I spoke to Ted Litz, who had been at the clinic with his wife at the same time I was, and he said, "if you'd come to me a year earlier, I would have," but he was getting a lot of support from the extramural program at that time. I literally went from coast to coast and border to border. One of them was here, John Clausen. Seymour [Kety] had set up the appointment for scientific operation, and biomedical, and John was already working for NIMH. There was a place at Hagerstown, Maryland, he had been doing some studies, and there was a lot of information coming out of a study going on at that time about the sociology of depressed areas. So he was already here but he didn't want to move. I didn't even raise the question, I knew he wouldn't want to move because he was in a unique position, in a recognized social science laboratory. And so I'd suggested giving him some money and having him hire people and that's how the concept of the joint lab developed. When Shakow was sharing my disappointment over all these people whom he thought would surely come for this opportunity [to head the Psychology Lab] but didn't, I had the bright idea of offering it to him.

Farreras: Do you think he would have come if you'd only offered him basic sections, no clinical sections?

Cohen: Well, I don't think he would have been offered it. I was the one who was talking to him.

Farreras: That's right, Kety had been looking on his own.

Parloff: There is something that might be worth my mentioning. I believe there were two general types of appointments needed to fill two quite different levels of functions: first, the senior investigator and administrator level; and second, the young investigator-clinician level. Some of the latter group would be required also to treat research patients. In view of the fact that young psychiatrists were then subject to the military draft, it was relatively easy to recruit them to meet their service requirements in the Public Health Corps, where they would be assigned to the NIMH. However, it was far more difficult to recruit senior investigators and administrators to leave their prestigious university or hospital positions for a position in government service. Bob has been describing his attempts to recruit some senior people who were well established and were understandably reluctant to be uprooted. Shakow, however, was one who had the vision to recognize the unique opportunities that the NIMH presented. Does that square with your recollection of this, Bob?

Cohen: Oh, yes.

Farreras: Once the Section Chiefs were appointed, did they then choose who they wanted for their section?

Cohen: The Section Chief made every appointment in his section; that is, how we were going to keep anybody. That is is a Section Chief. I don't think that I ever second-guessed them.

Farreras: When you talked earlier about some people's reluctance to come, was there any

concern that Congress was going to be dictating what type of research was going to be done, like when the administration now says that 40% of the budget must go to bioterrorism or something?

Cohen: No, I don't think so. I think that people may have had some misgivings to the extent that I, too, had wondered about that. And what I was actually afraid of was the durability. I shouldn't say "afraid of" but at least I wondered about how durable the entrants would be. Let me clarify what I mean by the concerns about working for the government. One is what you're picking up, of the government or Congress giving orders, the stability and funding of that on an annual basis, so there's the question of that kind of stability. But I also had in mind that the government – which we quickly ran into – is organized for the administration of – excuse the expression – clerks, and so everything, including promotions, went along with the established way of dealing with government work. And some people, wisely, objected to that. So initially, there was a problem but the government later modified its structure to accommodate scientists.

Farreras: "Later" meaning when, more or less?

Cohen: Well, when they started the supergrades for scientists. When did the supergrades come in, do you recall? First it was just going beyond 15. I was an 18 from the first but the time that I started and I took the job, Felix thought maybe we could get one or two more. As it just developed, we'd gotten enough to make the lab chiefs an 18. But originally, Kety and I were the only ones in the program.

Parloff: Then there were special mechanisms for keeping physicians. There were special

bonuses or something.

Cohen: At first it was difficult to find one's way through the maze of governmental regulations, and in a sense it was the administrative officer in Felix's office and the administrative officer in Shannon's office who made the impossible possible. And very early in our program, Bill Jenkins, who was a regular Public Health Service officer, took over as chief of clinical care to be the person who was most responsible for seeing to it that the operation satisfied the Public Health Service on the one hand and the medical needs on the other. We decided that we should get our own assistant administrative officer, and he knew Hazel Rea because he was working as the assistant of one of the extramural programs, so we brought Hazel over. And she turned out to be somewhat of a jewel in a way because unlike most administrative officers, she tried to understand as best she could the substance of the research. I think she got to feeling that she understood more than she really did, but nevertheless, she would find ways of doing things, whereas before that the administrative officer would say, "No, you can't do this." Sometimes the discovery would not always be based on the most noble reasons. For example, Fritz Redl naturally wanted to hire a wide representation of people to work with the children and he brought in a number of blacks. He would send the boys out for recreation, to play in the park or whatnot, and then what would happen is that we would get a communication from on high that would reach Felix, and it would finally come down to me, that there were white boys playing with black men, and there was an automobile that had come in with HEW on it,

and what were we going to do. This was before Masur came back to the institute, and the director asked what are we going to do about this. And so Hazel came up with the idea of simply taking the HEW name off the car. That's a practical solution but not an official solution. When Masur came in, he was much more... By that time Shannon had taken over, and Shannon would not have stood for anything like that. But, nevertheless, those underhand and appropriate actions would be found by the administrator, and then Hazel really ran up the ladder.

Farreras: I've contacted her, but I haven't heard back from her. She'd be someone else I'd like to talk with. I've heard that the peak number of Psychology full-time equivalents was 84. Do you know when this peak actually happened?

Cohen: I wish I could but I can't give you a date.

Farreras: A decade maybe?

Cohen: Well, it was in the '60s, that was clear.

Farreras: That's helpful.

Parloff: I went on sabbatical in '64-'65, and my impression is it began to go down after that. At least I became more aware of it when I returned.

Farreras: Okay, it had to be prior to the mid-60s then. But Walter Stanley and his Section arrived in the late 60s. Who was involved in bringing Stanley in?

Cohen: Stanley came because we got access to the NIH farm and Paul McLean was going to take charge of that. I've forgotten exactly how Walter was appointed, but he came to work at the farm and the reason his Section was dis-established is that he had a manic-depressive psychosis. We arranged for him to get therapy, which

went very well for a while. Samuel D. Thompson at that time was over at Chestnut Lodge. He was a psychoanalyst who had a Ph.D. in physiology and then Walter never came back from that.

Farreras: I see; I wondered why it lasted so little.

Cohen: He had written a book before he came, and I think that there was a basis for appointing him, but he was one of the people that was appointed.

Farreras: At around that time, '67 or '68, there was a new division created, Clinical, Behavioral and Biological Research, and then it was separated into Biological and Biochemical, and then you were in charge of the Clinical and Behavioral in '69. What led to all those divisions?

Cohen: Under ordinary circumstances, the Scientific Director would have been involved in the appointment of the Clinical Director, and Kety certainly did try to find a Clinical Director, but he didn't succeed. I had met Kety by that time, actually, because I was the program chair of the Washington Psychiatric Society, and we had invited my old professor, Ralph Gerard, to give a talk, and we thought Kety would be a good person to discuss his paper. And I knew of Kety because my Ph.D. was on hyperthyroidism and brain oxidation. And I remember asking him after the meeting what this NIMH was all about and he didn't know very much about what it would be. The net result was that Felix called me up one day and asked me, "How would you like to be director of Clinical Research? We can give you approximately a million dollars and six wards at the Clinical Center, and we'll pay the nurses and the social workers." The chief nurse was Gwen Will,

who had been at Chestnut Lodge, and whom I knew very well so...

Farreras: You're talking about the early days, when you first arrived?

Cohen: Yes, and so, ultimately, I agonized. But it was never proposed to me because I didn't talk to Kety again. He was Director of Basic Research and so the job that was offered me is that I be Director of Clinical Research.

Farreras: So how did those end up merging in '67?

Cohen: And on the other hand, he was also given the title of Scientific Director, and in every other Institute, the Scientific Director would appoint the Clinical Director but because they didn't wait long enough for him to find somebody, it was never that way in either of the two institutes NIMH and NINDB. NB: *today NINDS*, because Milton Shy came in the Neurology Institute with the neurosurgeon he was working with at Colorado, and brought a ready-made operation to begin with which they simply expanded here, whereas I really started with nothing and built it up. But Milton Shy and I both got to be quite good friends.

Farreras: Now, by the late '60s, John Eberhart was the Scientific Director, not Kety.

Cohen: Kety had built up the program of basic research, and then one day he came and told me that he'd been offered a noted professorship at Hopkins, and was weighing if he was attracted to it. And, after all, he didn't know anything at all about psychiatry and wasn't very interested in it. I remember I told him that I thought that he could do very well because I couldn't imagine that it could be anything but impressive to medical students to have contact with someone like him. And I also felt that for him to leave NIH, that things were looking up now.

It looked like things were going to work out very well and that I thought he ought to stay. But he decided to take it. And then, in the interim, they got Bob Livingston to come as Director of Basic Research, although Bob Livingston had even less contact than Kety had with the clinical program. And in addition to that, in the interim of their coming, I met with the Scientific Directors regularly. As a matter of fact, there was a little committee formed with Hans Stetten, Bob Berliner, and me, which met on a Sunday to discuss some of our complaints about the way the NIH was administered from downtown. So I really established a good relationship with them. Then Bob Livingston came, but as had been the case with Kety, the Basic Lab Chiefs would have meetings with Kety and later with Bob Livingston, and the Clinical Lab Chiefs would meet with separately, with me. So those who were joint Lab Chiefs would meet with each of us separately. And then later on, Bob Livingston decided to leave, too, and in the meantime, John Eberhart had gotten in touch with us. In the early days of the program, John Eberhart had been the director of extramural research, and I had met him first when he had come to do a site visit at Chestnut Lodge. And then for the first year that I was there Felix would have staff meetings for the program directors of the institute, in which Kety and I and Joe Bobbitt and John Eberhart would meet twice a week, and in addition to that, Joe Bobbitt, John Eberhart, Kety, and I always had lunch together on Fridays, so we knew each other very well. And then Felix, after Bob Livingston's retirement, thought that he'd like to have John Eberhart come back to be the Scientific Director. Felix wanted to drop

being an intermediary and have me working for Eberhart and under other circumstances I might have said, “Well, not for me,” and might have left but I knew John very well and liked him very much so that wasn’t a problem.

Farreras: Before this happened your program was called Clinical Investigations and Kety’s and Livingston’s was called Basic Research. Under Eberhart yours was changed to Clinical and Behavioral and his to Biological...

Cohen: We simply arranged the program.

Farreras: Was it also just a cosmetic change, or did it involve more?

Cohen: It was sort of... Let me get another piece of paper.

Farreras: Alright.

Parloff: An important part of the history, of course, is the comings and goings of Kety and other administrators. They account for many of the changes in the names of the organizations. One of the functions of the administrator is to put his stamp on an organization by changing its title. But as Bob was explaining in this case, some title changes were permitted because of the idiosyncrasies of relationships.

Cohen: This is how things ultimately got arranged, and by this time, toward the end, I wasn’t going to the Clinical Directors’ meetings, so Mike Heber [sp?] was appointed as Acting.

Farreras: Was Sokoloff’s Cerebral Metabolism Lab Kety’s former Laboratory of Clinical Science?

Cohen: The Laboratory of Clinical Science was always in my program except, when Kety came in, it was a joint lab. The only contribution Kety made – which I shouldn’t

call 'only' because it was a considerable contribution – was that he and Sokoloff came and simply took over the laboratory. And then he tried to re-create it at McLean, but it was never as good as the one he left behind.

Farreras: I also noticed in the telephone directories of the time that once this division happened in the late 60s *all* of the Psychology Lab sections and all of the Socio-Environmental Studies Lab began to be listed under Clinical. What led to that move?

Cohen: I don't know that anything happened.

Farreras: Really? Because until this time the basic sections were listed under Kety and Livingston and the clinical ones under you.

Cohen: Oh, well, it follows with when John [Eberhart] came. For a while, when Bob Livingston was here, he would have his Lab Chief meeting and I would continue having my Clinical Chief meeting. John and I had had a long social relationship and John was a gifted administrator and a wonderful person, but I don't think he felt as comfortable meeting with the basic Lab Chiefs alone and he wanted to come and meet with my Lab Chiefs from the beginning. Of course, I was delighted to have him meet with them. And since he always wanted to have my opinion about these things, even the things that were going on in the basic program, since I knew more about them than he did, he suggested, "Well, why in the world don't we just meet as one program and meet with all the Lab Chiefs?" So it was some time after Bob Livingston left, but I won't go into that. Bob Livingston had hoped to stay but some things came up and his welcome by his

own Lab Chiefs was somewhat less warm than he desired so he left.

Farreras: What actually happened in the mid- to late '70s when those Psychology sections seemed to vanish?

Cohen: I think that wasn't at our level of administration. I think that it was simply what was going on in the Laboratory during David Rosenthal's tenure.

Farreras: Okay, because you [Parloff] said it started to decline after you got back in the late '60s...

Parloff: No causal relationship!

Farreras: No, of course not. But what was happening in the Lab that might have contributed to the decline in the sections?

Cohen: Well, Morrie could probably say more about that than I could. I think one of the things was Dave [Rosenthal] got sick in the end, and this came about very slowly. It was a heartbreaking experience for us and more, of course, for them, because he was such a remarkable man. I always thought that only Dave Rosenthal would make it and would dedicate that book to four people who died too soon. But the appointment there was the last one that John and I made, and we no longer felt we were acquainted enough with what was going on in the field, so we got together a committee of all of the psychology consultants who had served on a Board of Scientific Counselors, and with their help generated a list of 10 people whom they thought might seriously be considered as Lab Chiefs. And finally Alan [Mirsky] was the one who accepted the appointment, and tried to reconstitute the Laboratory.

Farreras: Because the Neuropsychology Section became a Lab of its own.

Cohen: Oh, yes, that definitely was a very good, very powerful section. Alan had more of a problem re-creating the clinical things because Allan [Dittmann] had retired by that time and Morrie had left us to go to the extramural program.

Parloff: And Boomer had left the section.

Cohen: Right, so in a sense Alan had a free hand in trying to develop the laboratory.

Farreras: Why not abolish the Lab altogether? What I mean is, if the sections ended up disappearing – except for the Neuropsychology one, which became its own Lab – why not just do away with the Lab altogether?

Cohen: We never considered that. I think John and I both felt that psychology should still be represented in the clinical program. But we didn't feel that we were sufficiently close to new developments in the field so the group made a number of recommendations and we actually invited one or two of them.

Farreras: Do you remember who those other people were, in addition to Al Mirsky? Ted Zahn mentioned Marty Seligman was one but that he rubbed some people the wrong way and wasn't offered the position...

Cohen: One of them I know that we did make an effort to reach and someone recommended Michael Posner, a very, very highly regarded psychologist we went to see and we invited him but he didn't come. I do remember that he was at Cornell at that time. This is the Scientific Council that Pardes got together. The people I checked here had been in the intramural program in my day there. That's 22 of his people.

Farreras: So Al Mirsky finally accepted.

Cohen: Mirsky accepted.

Farreras: Were there any changes in the Lab when he came? Was there a different mission?

Cohen: No. If a mission appears anywhere, it was buried somewhere deep. The mission was always in the heart and soul.

Parloff: I'm delighted to have that confirmed, because when we were doing the task force and Mike Boomer had written the mission statement, Eberhart delivered himself of a great thought about the value of a mission statement. A mission statement is something you place in the safe and keep there and you bring it out only on ceremonial occasions. Oh, yes, we have a mission statement. Whatever it is, we don't operate on it because we're much more creative. Could I come back to ...I'm puzzled about Zahn, Ted Zahn. He was there for a very long time, and remains there.

Farreras: Still there, yes, in the current Brain and Cognition Lab.

Parloff: He was associated with Monte Buchsbaum. Does that name ring a bell?

Farreras: Oh, yes.

Parloff: They had gone off in a different direction.

Farreras: Zahn was in the Section of the Chief.

Parloff: Did he shift to that Section? My recollection is that Zahn had been working in Perception and Learning?

Farreras: Ben [Virgil Carlson] Carlson's?

Parloff: Bennie Carlson's, yes.

Farreras: Zahn mentioned that Dave Rosenthal appointed a Unit of Psychophysiology for him, but that was the only time that I've seen him as somewhat separate from the Chief's Section. He's now sharing space with Carmi Schooler, down at the Federal Building.

Cohen: Really? Carmi I lost somewhere.

Farreras: Well, is there anything you want to say that I haven't covered? My questions have been very limited to what I know so if there is anything I don't know about please feel free to bring it up.

Cohen: I was wondering the other day, what one would say about NIH today, and one of the things I considered when I took the job was that it's a good government thing to have people to supply them with support that one could only imagine, even if the salary was low. It was an important enterprise, and we would make a contribution to the health of the country. And I felt that for a long time. But then it's become so crowded, and the salaries have gone way up. And where I thought that the discoveries would lead to cheaper medicine this led to good medicine, which are almost impossible to reach unless you have very good insurance. And I was thinking of calling Morrie up to ask him what he would say, whether the NIH should be continued? Before Pardes left, he appointed a committee to outline how the Scientific Director should be appointed, and the Scientific Counselors should meet to consider each project which is undertaken at least twice on that project, but the person should not be appointed unless the Scientific Counselors agree that he should be appointed. And what is NIH doing with these fabulous

resources that couldn't be done before? I was recently somewhat dismayed to read in papers of the American Psychiatric Association that one of the current Lab Chiefs was brought in to lend a little polish to the program. What is it doing that isn't being done at the university? NIH is horribly crowded. Everybody wants to be doing something about everything. And I don't think there's the sort of integration that was there in Shannon's days. In the beginning, when people would come, I would take them around the Clinical Center, walk them into the auditorium, which then was the biggest auditorium in Washington. And Nobel Prizes we got only five, and the most recent ones come from extramural. So I don't know if it's any more unique than the way it was, and I certainly don't think it's as comfortable a place to work as it seemed to be. It was a smaller world; the modern world is different. Now the Congress gets into it much more than we did. And the thing that's distressing to me is that maybe we stayed too long, although I think other people stayed too short.

Parloff: Obviously, many things have changed. When we were joking a moment ago about the mission statement, I think in large part, it's the success of the extramural research program of NIMH that universities have been doing great work, and that is supported by the NIH. So what is the function of the intramural program at this point? When we came in it had a great function in the sense that we were first to do things that needed doing, but that meant we were operating on a totally different system. You take people who look at their field and decide what needs to be done without being hampered by the notion that you have to publish or

perish – because that’s the university setting and this is different – and you get supported to do the preliminary work, and then you can do the work that you need to do, and it may go elsewhere. It may go in a surprising direction, and that’s fine. But you, again, take the view of creativity, of what is needed, and you are supported to do that. Now that’s a very precarious thing to undertake because you never know quite where you’re going, and you also have hospital beds to fill. That’s another burden because that puts on a constraint because Congress will come in and say, “I have a daughter who...and why aren’t you doing research on...” But the basic research is at a different level because you are given support. You write your Annual Reports only on what you have done, not what you are going to do, and extramural is “What are you going to do if I give you this money, and how will you do it?” It’s a big rigmarole. Not that that’s bad. I’ve been in part of that. I can see the value of spending money that way, but it’s a different program. And, actually, the biological research will always come in with research that they had already done so that they could justify it, and then they would do something different, which was because NIH would give them funds to go on. So that was a value. I can’t picture a world in which that kind of support is not useful. However, I think there are other things that have changed in terms of budgeting, in terms of what’s already been discovered, and the fact that some fields have petered out or they’ve combined their view with somebody else’s, and so you shift. But I can never envision a time when there will not be a good use. I can vote for an intramural program dedicated to the principles that Shannon had

originally enunciated. But, of course, the extramural has its own function and it's been brilliant.

Cohen: Well, at the moment, the President has increased the budget.

Parloff: Exactly.

Cohen: But that's not the NIH budget.

Parloff: So many people found their way into the institute in the early days, and they did wonderful research. I hope that will continue.

Cohen: Every time I look at Pat Goldman's name [Patricia Goldman-Rakic]... I remember when I told her that she could come for three years, but I warned her that she might not be able to stay, that at the end of the three years, she'd have to find a job somewhere else. But it was a thrilling experience to see these people come in and hear about their ideas and be able to give them an appointment, and then see the contributions that they made. As I look back on my life, I feel so happy that I took the job. The relationship that I had with Eberhart was a very satisfying one. And I remember when I was in Paris in 1954 I got a letter from Ed Evarts. We thought that LSD might be a model psychosis and that if we could find out what was going on in the brain, we would know what was going on in schizophrenia. And so he'd gone to Bernie Brody's Laboratory in the Heart Institute to get some help to find out where in the body the LSD was working. He wrote to me about offering this chemist by the name of Julius Axelrod, who was only a GS-12 – he was working on his Ph.D. at Georgetown, a job – and there was a letter enclosed from Dave Shakow who had talked to him and about work on

schizophrenia. So I wrote back and said yes. And when I introduced Julie I said that I had the creation of several notable enterprises, that this was one of them. It really defined my role.

Parloff: I don't think that role has gone out of style. I really would like to see that preserved.

Cohen: Julie said there was no chance for a graduate of City College [NY] to get into medical school. He didn't have any money. And he had a job in one of the laboratories run by the City of New York testing various substances – for their purity – for various departments. He'd gone over to the Goldwater Memorial Hospital to find out how to do a particular test, and he was so impressed with Shannon's program and what was going on that he quit his job and applied for a job with them. And he came down and worked with that group from the Heart Institute. He published a paper describing the anti-pain reaction of acetaminophen. That was how I thought it was going to work. That's Tylenol, which the drug company then simply can patent. He's just a marvelous teacher. At the time he had had 40 people who came to work with him. By the time the 40<sup>th</sup> came the first 30 had all become chiefs of their own laboratories all over the world.

Farreras: Well, I want to thank you both for meeting with me again today. This has been most helpful. I appreciate all of the time you have dedicated to these oral histories.

*End of Transcript*