

Robert A. Cohen

NIMH 1952-1981

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This is a second 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).

Dr. Morris Parloff is also present to assist with the interview. The interview is being held on January 23rd, 2002, in Bethesda, Maryland, and the interviewer is Dr. Ingrid Farreras of the NIH History Office.

Cohen: The Scientific Director of the Heart Institute was Dr. Shannon, and when Dr. Felix took me around to introduce me to all of the Scientific Directors, Dr. Shannon was a firmly established research director, and he had been at the Goldwater Memorial Hospital. Dr. Felix at NIMH wanted me to meet him primarily because I was notified that I was going to be the Scientific Director in November, and at that time we thought we would have to move into the Clinical Center and occupy 100 beds by March [1953]. There were not a vast number of research people, and how you were going to appoint them and get them to leave their present positions in five months and move in was just ridiculous. They fully realized that and just said, "well, do the best you can". But we committed ourselves to occupying these beds. As it turned out, we didn't have to move in until July, but that didn't make a vast difference. At that particular time, and particularly among the social scientists and psychiatrists, working for the U.S.

government wasn't regarded as necessarily a good thing because of the invasion of Congress into the operations.

Farreras: How much of an influence did Congress have on the research that was done?

Cohen: Well, Congress didn't have any great influence, but the memories of Joe McCarthy were vivid among the people in the social sciences. They may have been among people in the laboratories, too, but I don't think that they were as concerned as the social scientists. I already mentioned that I had spoken to a number of people. I'd spoken to Al Stanton and others, some of whom were being called up, and finally got some agreement from several of them to come. But when Eisenhower went and stopped the Korean conflict, they all called up to say, "Well, we would have liked to have come, but obviously this is necessary in war." So everything that you read about the organization was just something that I made up on the spur of the moment. I began calling people. Morrie [Parloff] was one of the very first I called; I already knew him and the moment I accepted the job, I knew that I wanted him to come. We had met before and I was very much impressed by him. I still am. I'm trying to think of anyone else. Roger McDonald we had met. I met Morrie and Roger at the home of David and Margaret Rioch sometime previously. I had spoken to Jarl Dyrud to find out if he might want to come. Of course, he had no interest whatever in coming. But then I was simply deluged with requests from psychiatrists – all resident status, not professors – who were being called up, so I figured I would accept the ones who made the best impression on me. What I wanted was to get people who had had some experience in addition to psychiatry, like Lou Cholden, who had a

Master's degree in psychology, or who were at least third-year residents, like Norman Goldstein, and who, in addition, had a Ph.D. I looked for people who had graduate degrees, but they weren't abundant. Norman was the only one, in the first group of psychiatrists, besides Lou Cholden. We just assembled a group of psychiatrists: Charlie Savage, who had been a resident at the Naval Hospital, Bob Pittenger who had been chief resident at Yale – that would know how to run a clinical service and a few others. So I was trying to get people who had graduate degrees in other areas because I really wanted to have representation of psychology, I knew I wanted to have representation of sociology, and I was hoping to find people who had considerable experience in those areas in addition to psychiatry. I wanted to get people in the biological sciences because I had come from physiology. The strange thing about it was that here I had a clinical program, but I wanted it to have sociology and physiology and biochemistry represented. Bob Felix was the one who recruited me, which is funny on the surface because Seymour Kety was the Scientific Director at the time, and in every other institute, the Scientific Director recruited and appointed the Clinical Director. But I wasn't even introduced to Kety. All of the discussions were with Felix, and Felix took me around and introduced me to all of the other Scientific Directors, which in every other institute would have been done by the Scientific Director.

Farreras: Why did Felix appoint you to head the Clinical Investigations program?

Cohen: Because Kety had tried to find somebody and hadn't found anyone, which wasn't amazing, because although Kety was a brilliant physiologist, he had had no

clinical experience. I don't know whether he actually had an internship or not. And I had met Felix before, when I was program chairman of the Washington Psychiatric Society. But the significance of that is that somehow, although Kety and I had a good social relationship, he never invited me to meet with any of the Lab Chiefs in the Basic Research Program, and I never invited him to come to our Clinical Lab Chief meetings. And the same thing was true when Kety left that job and Bob Livingston took over as Scientific Director. We had a cordial enough social relationship but never talked about the Clinical and Basic programs together. We went to Bob Felix's two general staff conferences of NIMH Program Directors twice a week, and even more in Kety's case. For four or five years, Kety, John Eberhart, Joe Bobbitt and I would have lunch together every Friday and talked mostly about the problems of the institute and life in general. We never brought up, except tangentially maybe once in a while, issues that had come up in our own individual programs.¹

Farreras: I wanted to back up a minute to what you said earlier about wanting physiology and sociology represented within the Clinical program.

Cohen: As far as the physiology, Wade Marshall – who had been there before Kety arrived – and I had known each other since 1930, we had worked at the same lab and had gotten our Ph.D.s from Ralph Gerard. When I began as an assistant of physiology, which was what paid my way through medical school and graduate

¹ Within a few days of my arrival, I joined with Dr. Kety in attending the two weekly meetings of the entire extramural staff of the Institute in Dr. Felix's office – Monday mornings and Thursday afternoons – where we participated in all the deliberations concerning the Institute's development. Also Kety and I began regular Friday luncheon meetings with Drs. Eberhart and Bobbitt – these continued for many months until Eberhart left for the Commonwealth Fund and Kety for Johns Hopkins.

school, the other person in my first class as assistant was Louise Hanson, who married Wade Marshall. My wife, Mabel Blake, was also an assistant, and the first people we visited in Baltimore, when Mabel and I came for residency, were the Marshalls, so we had kept up that relationship. In addition to that, I had been at the Institute for Juvenile Research in Chicago as a fellow, and I had known John Clausen there. And John – and Wade – were already at NIMH doing research. John was doing research in Hagerstown. For a variety of reasons John didn't want to leave the Basic Research program so what I did was give him sections within the Clinical program. We created the first joint [Socio-environmental Studies] Lab almost immediately. When Kety would have a meeting of the Basic Lab Chiefs John would go to that and when I would have a meeting of the Clinical Lab Chiefs John would go to that as well. The Lab Chief would go to each meeting separately. As I look back on it, I don't know why it happened that way. John Eberhart was really the first real Scientific Director in the sense that he had ultimate supervision over both programs, whereas Kety and Livingston only had nominal. John and I ran the program together, partly because he was a psychologist and didn't know anything about the hard sciences, and I at least had a degree in physiology.

Farreras: How did he manage to be appointed Scientific Director, being a psychologist, with no M.D. degree?

Cohen: When Kety left they had to appoint another Scientific Director and John was a very much admired and very well-liked person. He had been in the institute from its very first beginnings as Director of the extramural program and had then gone

to the Commonwealth Foundation but I guess he wasn't as happy at Commonwealth as he thought he might be, because he came back when Kety gave up the job and Bob Livingston came.

Farreras: Who appointed him?

Cohen: Dr. Felix appointed all the people. But they were looking for someone in physiology from Yale when the idea came up of Eberhart being a very good person to have as Scientific Director given the experience he had had from the very early NIH days. He and Joe Bobbitt had both gotten their Ph.D.s together at Northwestern, and Bobbitt was Bob Felix's closest collaborator in those days.

Farreras: Now, you just said that Marshall and Clausen were already here before Kety arrived, before the intramural program was established?

Cohen: Marshall actually came before the Intramural Research program had been so designated. There's another Robert Cohen, who was both a neurologist and a physiologist, who had been instrumental in getting Wade Marshall a job. Felix knew that he was going to want to have a physiologist, so they actually set up a very complete physiological laboratory. Felix also knew that they would want to do sociological studies, and the NIMH had an operation in Hagerstown, Maryland, and John Clausen came and began a series of population studies in Hagerstown. And these were in full operation at the time Kety arrived and took over, or developed a program which included them.

Farreras: What about the Neurochemistry Lab?

Cohen: No, Kety built that up. He appointed all of the other Lab Chiefs in the Basic Research program, but there weren't so many. Sokoloff was not a Lab Chief. As

a matter of fact, Sokoloff's career was the reverse of mine; I came to psychiatry from physiology, and Sokoloff went from physiology to psychiatry. He had been a student of Kety's at the University of Pennsylvania, and after the war he joined Kety because he was fascinated with the possibilities of what neurochemistry could do. My idea, at first, was that some of the people who came in simply to avoid military service would get interested in research and possibly become permanent members of the staff, and then, for the rest, that we would gradually recruit them into a long-lasting program. I was thinking of several years but it turned out to be six years before I finally got all of the people! I mentioned to you already that Dr. Shakow was frequently around the institute as it was being formulated and developed, and that he was one of those who was consulted because of his distinguished background in psychology. He had gotten his degree at Harvard, and was very active in the Chicago area, particularly in the Chicago Psychoanalytic Institute. I turned to him to get some help in finding psychologists for the program, and every person that Shakow and I agreed on would be a good person for a Lab Chief would come and visit us, make a strong impression on us, without exception would be fascinated by the ideas of doing research here, but didn't want to come. Dr. Shakow was getting just as disappointed and frustrated as I was and one day I was really inspired and thought that if we offered him a joint lab like Clausen already had that he might be willing to come. I went to Kety and Kety wondered about it and decided to give it a whirl and when we offered it to him he said yes. That's how we got started. So at that time there

were, in effect, three Lab Chiefs [Marshall, Clausen, Shakow] and two of the labs were joint [Socio-environmental Studies and Psychology].

Farreras: I thought I read in your 1975 oral history with Eli Rubinstein [at NLM] wanting Fritz Redl, as your first choice, to head the Psychology Lab.

Cohen: Yes, what I had in mind was a simpleminded idea. Here were 100 beds. How were we going to fill them? I thought that there would be three main divisions within the Clinical Laboratories. One with the sorts of patients that Redl worked with, hyper-aggressive adolescents with anti-social behaviors; one which studied disorders of mood and thoughts – schizophrenia, manic-depressive psychosis; and one that would devote itself to normals. This simpleminded idea resulted from the early days of working with Dr. Sullivan in the Washington School of Psychiatry. We had gotten the idea of proposing to the Army or the Navy a study of people who had endured and not broken down in combat. The Navy approved but Dr. Sullivan died so the five of us who were going to be in that program tried to carry that on by ourselves. During the war, all of us had been involved in getting people who were unsuitable for service excused with an honorable discharge. And we had developed almost a template that every one of these people demonstrated; involving neuropathic traits in childhood that came from broken families. So we started this study of very successful Naval officers who had gone through hell; one of them had been captured by the Japanese, others had been in the most awful combat...but they all had the same background as the patients whom we had validated out! These were people who had been Eagle Scouts, presidents of their high school class, but had all come from broken families, they

all had all of the neuropathic traits of childhood. And the thing that set them apart was that they eagerly looked forward to competence tests. They looked for things that made other people [unintelligible], and they always came through. One of them had such a broken up family that he would stay at school as late as he could, go home and have dinner, go to sleep, and then get up at night and study and then go to school. He became a mate to one of the storied admirals in the Navy. I wish we had done a better study than we did. Nevertheless, as far as I was concerned, it resulted in my saying that never again would I want to do studies without including the study of the normal population. I was looking for people who would be committed to us. I thought that I had reached the end of the beginning, but it turned out to be the beginning of the end. What happened was that Dr. Redl, Dr. Shakow and Dr. Clausen were working together and I looked for someone for what we called Psychosomatic Investigations to work in the clinical psychiatry area. I had met Joe Elkes on my trip to Europe and was tremendously impressed by him. He was professor of pharmacology. He had come to spend a year in the United States, and I was most impressed by the fact that instead of going to Harvard or Yale or any other distinguished university, he went to a Pennsylvania state hospital, which I thought made real sense.²

² With the appointment of Redl, Shakow and Clausen as Laboratory Chiefs (the latter two as “joint”) I was now looking for a senior research psychiatrist and a chief for the psychosomatic group. In 1935 I visited research centers in Europe on a trip planned by the Mental Health section of WHO. I was particularly impressed by a visit with Dr. Joel Elkes of the Pharmacology Department of the Birmingham University. He visited us in 1936, was offered and accepted the post of the Chief of Psychosomatic Medicine, but on his return to England was subjected to pressures to remain – pressures he felt he could not refuse. Seymour Kety then proposed himself as candidate for the post. He brought his own section and that of Louis Sokoloff into the Lab, changing its name to the Laboratory of Clinical Science – making it the third joint laboratory.

And then I read a paper by David Hamburg in *Psychiatry*, a journal of which my wife was managing editor, and tried to get him to come early on, but he was very much involved in the Institute for Advanced Studies in California. David and Elkes finally agreed to come but Elkes – after we set up a clinic to his specifications – then went back to England. Seymour Kety, who had already gone to Johns Hopkins to be professor, was profoundly depressed and wondered if he could come back and become the Lab Chief. I thought that would be wonderful. And so he came back and the only person he brought with him was Lou Sokoloff and a couple of junior people. He took over the tentative laboratory that we had already assembled, which included Seymour Perlin, Bob Butler, Roger McDonald, Phil Cardon, Julie Axelrod, Ed Evarts, and Irv Kopin. Elkes, a year later, wrote and said that he was ready to come. I had a very good friend who was assistant superintendent at St. Elizabeth's, and I had talked to him about getting a ward there, and he was as interested as we were. I thought it would be good to have a ward in a mental hospital setting, and do some of our research there. Felix, Kety and I went over to have lunch with Dr. Overholser, and Dr. Overholser said, "Sure you can have a ward, but why not take over a building?" and Dr. Felix said, "That would be great" and my heart sank. But then Joe said he would come back, and so we asked Joe to go and look at it. He came, stayed for two weeks, looked at the building, took a set of architectural prints back, and sent back a complete laboratory development for the new building. And then Dave Hamburg came, and I thought we were then going to settle down and work.³

³ In 1958 I succeeded in recruiting David Hamburg to be the first Chief of Adult Psychiatry. The same year Felix

About eight months after he came, Elkes and I went to a meeting of the American Psychiatric Association where he was going to be a discussant. He gave one of his spellbinding discussions and before we got back to Washington, Hank Bronson had tried to get him to go to Pittsburgh. And then he was simply bathed in invitations. He told us that he had accepted a professorship at Stanford and everyone felt awful. We didn't say anything, but it didn't take even a pharmacologist to tell how we felt about it. But he reconsidered and thought he shouldn't leave and decided to stay. He stayed for about two years after Kety had left again and then Hopkins, which had been trying to find another professor, came and got him. By this time, John and I began to work together. Up to that point, the joint laboratories would meet with each of the program Directors separately, the Clinical Laboratory Chiefs with me and the Basic Laboratory Chiefs with John. But for the first time, I began to participate with him in issues that came up in the whole intramural program. At first I thought it was going to go so well because we began to have one dinner meeting each month with the six Lab Chiefs – Kety, Elkes, Clausen, Redl, Shakow, and Hamburg – and each of them took turns going into not only what was going on in their laboratory, but

and I had gone over to St. Elizabeth's Hospital to ask for access to one of its wards so that we might have a more conventional setting for some of our clinical studies. The superintendent offered us a whole building – which Dr. Felix eagerly accepted. Elkes had just sent us a message stating he was now available – and thus the Clinical Neuropharmacology Research Center was born. I had assembled the dream group which I hoped would work together productively for many years. It was a very congenial group; each new member had been appointed with the full agreement of all the others. In addition to meetings at the Clinical Center we had dinner meetings at the homes of the members. They were interested in all the research programs – but after two years Kety was offered a Professorship at Harvard – “the only one he could not possibly turn down.” Then Clausen was appointed director of the Human Development Institute at the University of California at Berkeley; Hamburg the Professorship at Stanford; Redl the Distinguished Professorship at Wayne State; and some months later Elkes the Professorship at Johns Hopkins. Shakow alone stayed on – also after his official retirement. All their replacements were filled by promotions from within, of men and women who in making their reputations made ours.

also into where the whole intramural program might go and what things we might do. I thought we would work together like a smaller group might have done, and it seemed to be going that way but then they dropped out [Hamburg, Elkes] and Clausen was also invited out to take the job of Director of the Institute for Human Development at the University of California at Berkeley. But the younger people who had come and stayed had developed into fairly substantial people in the takeover as Lab Chiefs themselves; Lyman Wynne, for example, had finished his Ph.D. while he was with us and took over the Adult Psychiatry Branch.

Farreras: Was it common for junior people to be leading the way versus hiring senior people, like Fritz Redl, who already had an established record?

Cohen: It was common with us. Well, Lyman had already done substantial work, and it promised to be more so. Dave [Shakow] had stepped down as Lab Chief and then Dave Rosenthal took over. As I look back I don't know why he took over as Lab Chief rather than Morrie, both had been equal candidates. But Rosenthal led the way and did an absolutely superb job in the study of the Genain quadruplets, the name that he himself gave to them. Ed Evarts had been the Acting Lab Chief of the Laboratory of Clinical Science but he didn't like to be Lab Chief. I made him promise me that if he didn't want to stay Lab Chief that he would let me know instead of resigning in order to get out of the responsibility. And sure enough, he promised, and then he was offered a professorship at North Carolina. But that was the time when Kety had returned and when he did Evarts said that if Kety took over the Lab that he'd stay. And when Kety left again, Julie Axelrod was already a distinguished scientist. He hadn't gotten the Nobel Prize yet but John

Eberhart and I had proposed for three years running to promote him to a GS-18, which is the highest level available, and he turned us down each time. And it was just before he got his Nobel Prize that it finally went through. But we knew that Julie wouldn't take the job so we asked Irv Kopin to take it. But I simply gave up on the idea of having structured dinner meetings, even if all seemed receptive. If people were going to come together, they'd find their way. I got over feeling terribly disappointed. And from then on, I think the program went along with a modest degree of success. When Dave Hamburg had his going-away party I felt that it was a compliment to us that the people whom we found and worked so hard to bring together were very much wanted by distinguished universities. But it was the sort of compliment we could do without. Actually, the first person to leave was Fritz Redl, and that was because of Bob Felix and one of the two things I have against Felix. In a way, he was not only the first but in many ways the best director we had, but he was very upset because Fritz Redl would talk to social-work groups, etc. – there's a Redl House in the Washington area – instead of giving more talks. We were dealing with that, and we'd gotten Joe Noshwitz – who wrote a very superior book on child psychiatry – to come. And Jack Masur had said to me that he couldn't think of anything more important than working on these hyperaggressive, socially disturbed children. At the same time, we took the adolescents out of the Clinical Center and built a special building. Jack Masur said that he was sure they'd make a mess of it and I bet him a nickel that they wouldn't. And he gave me the nickel back and said, "You win." But from then on, we had such problems. The whole governance of the program gradually

changed. We got a note that Herbert Kelman, who became a leading figure at Harvard later, had to be let go because he had participated in protests and Bob Felix said, "Well, what can you do." Fortunately, Kelman got a lawyer. I'm not sure that this would have happened without a lawyer, but Shannon and the Surgeon General met on a Sunday and decided that he was not guilty of anything, and the Secretary of Health, Education and Welfare sent a letter of apology to Kelman and a letter of reprimand to the people who suggested he be let go. That was the best thing that ever happened in the Eisenhower administration. And Shannon and the Surgeon General were much more concerned than Felix. Felix, having decided, in his own wisdom, that Redl wasn't talking to medical scientists enough, refused to allow Redl to stay in Europe for a month because there were two meetings that he had agreed to talk at that were a month apart. So Redl resigned and I wrote a letter to Felix saying that I would resign if he ever did that again. I'm sure he didn't tremble over it and he himself left very shortly after. Other than that, he was generally a good Institute Director. Felix used to come over once a year to give a talk to the intramural scientists and he would always tell them, "I need to have a gimmick when I go before Congress, so if any of you ever have an idea or particularly some little discovery being made that I can tell them, it'll be very helpful to me." Here's an example of our heroism in those days. Senator Lister Hill asked Felix, when the budget of the institute was about \$15 million, "How much do you think you'll eventually come ask me for?" and Felix said, "Senator, I can foresee the day when I'll ask you for \$25 million." And in a sense, we were thinking about how nice it would be if some day the

government would have two or three institutes like the NIH in different parts of the country, because this was such a fantastic opportunity to do full-time research. Of course, that changed completely now that the ex-director pours out more money than he gets from private contributions.

Farreras: Why was the NIMH pulled out of the NIH?

Cohen: My feeling is that Shannon tried to think of the NIH as a whole, and each Institute Director would tell him what he would like to propose to Congress, and Shannon would decide whether to go along with it. I think Felix felt he would have a freer hand. And so most of the people in the intramural program were worried about our being pulled out.

Farreras: Why?

Cohen: I think it was general. I know that it was true of John Eberhart and me. But we admired Shannon so much, we could discuss problems with him, and we both felt that this was a valuable operation. We had also run into a problem in our program as far as giving a little too much responsibility to one of the young doctors, Jordan Scher, who, without authority, had admitted to his unit six patients who were schizophrenic but who had the good fortune to come from families with a tremendous amount of political influence. And he told them, "I will not only treat your beloved children and cure them, but that they can stay here for life." When I found out about that I didn't say they would have to leave right now, but that they could not stay here for life even if we couldn't cure them. But they applied so much pressure...

Farreras: Their families?

Cohen: These people had so much political influence that for many months, Shannon got letters every morning from senators, representatives, the White House... For three months, every morning I would start my day in Shannon's office and would speak to him and/or Elsie, who was a wonderful administrative officer, about how we were going to take control with this? When it came to his attention Shannon said, "Tell me what happened, and, what you've done and why you've done it." And after we did that, he said, "I think you were right." And the families asked for an interview with Shannon, and Shannon had the interview with them with me there, and it was right, but we're not just going to turn the patients out. We'll see how it goes. There was an article in the *New York Times* where I got the most publicity I'll ever have, in which they indicated that here was another government business just like Enron, where the sure cure for schizophrenia was being tossed out because we were turning the program over to Kety's laboratory. here were many other things like that about Shannon. I just felt like he was the one who created NIH the way it was and why it was that way.

Farreras: So you wanted to remain a part of NIH.

Cohen: We felt that we had no need for another institution to develop. But we went. But we were never made to feel that we weren't part of NIH.

Parloff: I have a fable about this that I recall Felix talking informally with us about. He explained that he had finally gotten enough courage to submit a budget for a billion dollars, at which point he was told, "Go make your own institute," and that's the way he presented to us why he was leaving, because we were very unhappy. Incidentally, we all shared that view of Shannon. I first met Shannon

when Sokoloff and I were invited to see Shannon where he presented the institute as a place where you were anointed and you would go do creative work and the administrators would assist you, and Eberhart and Hazel Rea supported that point of view to an unbelievable extent. We were very loath to leave the NIH, and Felix's story was that he had been pushed, that it wasn't altogether his wish to go, but that he had been encouraged to go make his own institute.

Farreras: Who had done this? Who had encouraged that?

Parloff: Congress ultimately. But as I understood it the other Institute Directors considered it presumptuous and ridiculous for the NIMH alone to have a budget of a billion dollars. That's not the budget of an NIH Institute; that's one for an independent agency.

Farreras: So the Heart Institute, for example, wasn't getting that much money?

Cohen: No. I'm not even sure Cancer did. And I remember one day when Shannon called John [Eberhart] and me in and said that while he was satisfied with the way the programs were run, he thought we were going to need to have some other independent evaluation of the programs, scientific counselors we would select to meet regularly and come. At first John and I met with the Lab Chiefs each year and went over every person in the program; how the work was going, how productive they were, whether it was time to consider promotion. And then we decided that that was probably not the best way to go about it, so we advocated for the appointment of a scientific promotion committee from the intramural scientists, who would make recommendations to us.

Farreras: When you say promoting scientists, could you tell me what type of ranking system there was? Now it seems that there is a Lab Chief, Section Chiefs, and then each Section Chief has several post-docs and technicians. But it seems that back in the early days you had a Lab Chief but also people like Mishkin and Mirsky who were not post-docs.

Cohen: Actually, the Lab Chiefs while I was still there. Lyman Wynne, I thought, could be a senior person, although I think that he had strengths as well as weaknesses in that job. Actually, when Lyman left to go to Rochester, he took most of the Adult Psychiatry Branch with him. Before he left, we had felt that the group under him had been very productive, and he was already was a Lab Chief, but Goodwin and Murphy, who were working with him, really had become quite independent of him, so we appointed them as Lab Chiefs.

Farreras: Wasn't Butler one of the members of the Adult Psychiatry Branch? How did he get to be Director of the Aging Institute?

Cohen: Well, when I first arrived I received a letter from Bob Butler, who had just finished his first year of residency and said he would like to come. But I was getting a lot of letters at that time and we were only able to give people a year's residency and on that basis I said, "Well, we can accept one person." So it just goes to show, if you wanted to get credit, you have to do things differently. Bob Butler always speaks about how Seymour Kety had wanted him, but the fact is that he was always a member of my program and I didn't want to lose him, so I sent him to Chestnut Lodge for a year of residency credit, and I paid his salary, which was something that made him very pleased. As it turned out, he would

have been pleased. And then he even offered to keep him another year but I said, “No, you can’t do that.” And Bob Butler came back and wanted to start a program people. And I had decided that Fred Snyder and I were going to appoint him as Lab Chief, and we did. And I think that Fred would have been wonderful if he would have stayed, but he was a rather tortured soul. He actually did exceptional work. In any event, to go back again to the early part of the program, our first interdisciplinary study, and Bob Butler and Seymour Perlin [from the Laboratory of Clinical Science] were the psychiatrists assigned to us. I remember Jim came to me and said, “But, you know, Butler is much more interested in these old people than Sey Perlin.” He really made a very solid presentation to some big government people [Arthur Fleming] and finally, when the Aging Institute was being developed, he was a natural candidate for it.

Farreras: So the interdisciplinary study was between Birren’s Aging Section, the Adult Psychiatry Branch, and the Socio-environmental Lab [Yarrow]?

Cohen: Let me go get the book that came out of that very early study... Do you recall this acknowledgment of Perlin? [After the five authors are listed – Birren, Butler, Greenhouse, Sokoloff, and Yarrow* there is “(*With the early editorial collaboration of Seymour Perlin)”. I thought that was insulting. It was classic. Perlin wanted to be on everything.

Parloff: He did. I wanted to come back to a point you made before about Goodwin and his role. This was one of the first studies that made me feel that the original idea was going to be a success, where people from different labs got together. I always thought it was interesting that, while Birren had the big name in the aging

field, that Butler wound up as the Aging Institute Director. Now, Butler is an extraordinary person, no quarrel with that. But I was always puzzled about what happened in that process. Goodwin came from the intramural, and I always thought that he had a lot more appreciation for what the intramural was doing and could do than, say, Pardes [Herbert Pardes] . When I was working with Pardes, Pardes seemed to be, politically, an instrument for the American Psychiatric Association. That was the scope of his concerns in most of our discussions – in terms of the extramural. To represent psychiatry properly was very clearly his major goal in life. So I was interested in what you said about Goodwin because he would have a different view, one that was a little bit more expanded. Just for clarification, John Eberhart held the title of Director of the Extramural and the Intramural but he never used the extramural part, he just held that title. Am I correct in that? [NOTE: *He's listed in the phone books as Chief of the Research Grants and Fellowships Branch (in the early 50s), Associate Director of Intramural Research from 1961-1967, and Mental Health Intramural Research Program Director from 1967-1981*].

Cohen: When I first knew him, he was Director of Extramural, not Intramural. I met him because he made a site visit to Chestnut Lodge while I was still there. Then he went to the Commonwealth and later became Director of Intramural when he returned. The Director of the Extramural eventually was Lou Wienckowski at the time that Eberhart came back.

Parloff: Well, I recognize that that's correct, but there was one puzzling aspect very early on. I, too, knew John [Eberhart] from his grant work because he provided the

grant overview for Jerry Frank's work, with which I was associated at Johns Hopkins. But when I came on and Eberhart came to greet me and tell me about his role of helping me implement my research, I discovered that he was listed, at least early on, as Director, Intramural and Extramural. And when that was raised with him, he said, "Well, I never do that extramural anymore." But it was listed, I thought, for many years. He was fantastic.

Cohen: He was a very remarkable man, a pleasure to work with.

Parloff: Absolutely. It was a very exciting place.

Cohen: Well, it was a different place. There's no doubt about the fact that it was a golden age for those who were there.

Parloff: I was reminded of when Oveta Culp Hobby [1st Secretary of the Department of Health, Education, and Welfare, 1953-1955] had the temerity to suggest cutting the budget and Congress censured her. Those were extraordinary days.

Cohen: I certainly must say that I felt very fortunate to have been able to be there in those first years.

Parloff: Jordan Scher's activities were probably not among the highlights of your years at NIMH. That was a period of turmoil that I well remember. I had to work with Jordan on the committee, until Dave renamed the quads as the Morlocks. But he was an extraordinarily difficult man to deal with. So I was doing Dave Rosenthal no favor when I urged him to take over that committee; I thought he could deal with it, and he did. Dave would just consult respectfully with each member of the committee and then do what he wanted to do. This enabled him to get a lot of work done.

Cohen: Well, Dave's story is so tragic. He was such a remarkable man. And somehow or other, I felt so sad that he contributed so much to the program, to the development of the field of genetics, and his name just isn't mentioned that way anymore. I think that's sort of sad.

Farreras: Returning to your earlier comment about Butler, why is it that Birren wasn't asked to be Director of the Aging Institute, given what a big name he had in the field?

Cohen: I don't know why. Jim Birren was certainly very highly regarded. He was in California by then. But Butler's very skillful, he's a very good speaker. So is Jim, as far as I'm concerned. But I guess that Butler – in addition to whatever else he may be as a scientist – is also a very engaging person. He made a very good impression on Fleming and I think that was the way. Butler was one of the younger Institute Directors when he was appointed. This was after he got the prize for his book on aging. I was trying to think of the last Lab Chiefs when we reported after Goodwin, Murphy and Ed Evarts. When John and I decided to retire, we gave Pardes four names that we considered fully eligible, and they were Evarts, Kopin, Bunney, and Goodwin. Who was the woman who worked Intramurally with Hazel Rea appointing people? I know they were very heavy supporters of Goodwin. So what happened was that Goodwin was appointed and Evarts stayed, but Kopin went over to the Neurology Institute, and Bunney went to the University of California and is now a distinguished professor there. Bunney really was the senior in terms of experience. He was the one who hired Goodwin. We no longer felt that we were in a position to decide on a new Lab

Chief for the Psychology Lab so we got a committee of people who had been on the Board of Scientific Counselors to advise us as to whom to appoint. Actually, Alan [Mirsky] wasn't our first recommendation, but he was the one who accepted.

Parloff: I know Shakow had very high hopes for Mirsky. He and we felt very, very positive about him. I was thinking about the question you raised before about the impact of our having left NIH, yet we were physically still located here and occupying space that other people could have been chomping at the bit about. I would like to get your impression of that. Shannon certainly wanted everybody to stay but that doesn't mean that the people who were around were always happy about that, particularly when space was such a big concern.

Cohen: Well, I do remember when Bob Berliner was the Deputy Director. He came around and asked us whether we should keep Socio-Environmental Studies. And he was suggesting that it could comfortably move out. But John and I were reluctant to do that, and he agreed with us that it should stay. And then the very first thing that Fred Goodwin did was suggest that it move the whole thing out. He certainly cut back on the support.

Parloff: That was my impression, that the support was gradually withdrawn. And I had already left the intramural, so I don't recall the demise of the Laboratory of Psychology but I do know people had disappeared, either by natural attrition or by being encouraged to leave.

Cohen: Very definitely. Two things are rather interesting. Fred started to write his textbook a couple of years before I retired, and then he told me about having invited Jamison to help. And I was rather interested that he did that, and I think

that Jamison was as well, the social science, such as there, is in that textbook.

Actually, in the 50th anniversary celebration that they had, Jamison was one of the speakers, and she was the only one who spoke about the importance of love in the treatment of manic-depressive illness. And, amazingly, there was clapping.

Farreras: And the second? You said there were two things.

Cohen: Fred, I think, had controlled interest in the social sciences. I suppose that I, being brought up the way I was, still feel that mad rush for science. It tends to propose that life experience plays a relatively less significant importance in personality development, as I look upon it, even this really quite good book. She says something about psychotherapy that, obviously, is entirely secondary to everything else. And I sometimes think that back a hundred years ago, which make other developments is a very important statement. And somehow or other, that's something that they deal with. I think we have overlooked one of the NIMH directors, the one associated with the Decade of the Brain. Do you recall who? There was Pardes and then Frazier, but somewhere in there, there was another Director who became known for having instituted at the NIMH the study of the Decade of the Brain.

Parloff: Yeah, I don't know him, he came after I left. But he's professor at the University of California at San Diego.

Cohen: The reason I mention him is that it coincides with the re-medicalization of psychiatry and the de-emphasis of the sociological, the developmental...

Parloff: Sociology as we knew it, psychology as we knew it...I think that's true. They put their faith in genetics.

Cohen: I was a little cheered to have read that there's getting to be much more acceptance of the notion that, while the brain functions and neurological functions are critical, they can be influenced by the psychological experience, and so they're getting away from this 'it's all genetics' and 'it's all how you were raised'. It's rather that how the mechanisms work is getting to be better understood and respected.

Parloff: Well, even Kandel, who got the Nobel Prize studying the sea slug, described how experience does play a role in the behavior of the slug, and once the slug has had experience, it changes the behavior in significant ways.

Farreras: Well, shall we stop for today? Have you had enough?

Cohen: Yes, although Morrie and I can keep talking forever.

Farreras: Oh, and I can keep listening.

Cohen: The question is, have you had enough?

Farreras: Well, we can set up a time to meet again soon. But I want to thank you both for being so generous with your time.

End of Interview