Robert A. Cohen
NIMH 1952-1981

This is an 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).

Interviewee: January 18th 2002, Bethesda, Maryland

Interviewer: Dr. Ingrid Farreras, NIH History Office.

Farreras: Why don't we begin with your telling us a little bit about your family background before we discuss your education.

Cohen: Both of my parents were immigrants, but came to the U. S. under different circumstances. My mother was brought over at age two with a sister two years older. Three more sisters and two brothers were born here. During the early years my grandfather pushed a cart full of sundries through an area then rural but now part of Chicago; he would spend week day night boarding with one or another farm family, and the Sabbath with the family in the Jewish area on the west side. In a few years he bought a house in the rural area, and then built a store and more substantial dwelling. As in many Jewish families at that time, the boys went to college and the girls went to work after finishing primary school – except for the youngest who had two years of high school. My mother was working as a sales woman at Marshall Fields when she and my father married in 1908.

My father was the second of six sons and three daughters of a tailor who lived in London. All of the children attended the Jewish Free School that was supported by Lord Rothschild. Every year each girl would receive a new dress and a pair of shoes; the boys would get a new suit and shoes. At age 12 each child would be apprenticed to learn a trade. My father's older brother became a cabinet maker, my father a chair maker, the next younger brother a tailor. At age 16, without telling his parents, my father lied about his age and signed up for a six-year enlistment in the Rifle Brigade. He served first in South Africa during the Boer War, and the rest of his term back in England. He passed a series of army courses at that time which gave him the equivalent of several years of high school. Upon being demobilized in 1906 he came over to Toronto to join his older brother. In 1907 they moved down to Detroit, and somehow gathered the resources to bring over their parents and seven siblings. In 1908 he moved down to Chicago, got a job as streetcar conductor, met and married my mother. He was a conductor until I was 9, then became a salesman for a shoe factory. I was 14 when he opened a small shoe store which he operated for the rest of his working life. On my first trip to London he sent a donation to his old school which I found at the address he gave me – now operated by the Jewish Board of Guardians. I suppose my mother told me I was going to go to college when she cradled me at her breast; there was never a time when it did not come up in one way or another. She taught me the alphabet early, then had me spelling, reading and doing arithmetic. They started savings accounts for each of their children early, and, of course, lost every bit of it during the great depression because they had invested in real estate bonds which paid a higher interest than bank deposits. When I was 10 I was run over by a light truck and sustained a fracture of the femur. I was in hospital for two months. They did not have a children's ward. For two weeks I had a beautiful special nurse with whom I fell in love. The other nurses and the interns joked with me, and allowed me to spend time with them as I made my way around the hospital on crutches. By the time I left I had decided that I wanted to be like them and become a doctor. During recovery from the fracture I began to read omnivorously – at first very widely but then gradually concentrating on biography, biology and history. In high school I invested some hard-earned money in Harvey Cushing's *Life of Sir William Osler* and determined to shape my life after his. I secured a catalog from Johns Hopkins Medical School and planned my curriculum to take the courses there recommended. It soon became evident that Hopkins was beyond my means but I enjoyed the work and decided to take one-step at a time.

Farreras: When Johns Hopkins became too expensive did you decide to go to the University of Chicago?

Cohen: Yes. I graduated from high school in February 1927, and then attended Crane Junior College (at that time the Chicago School System's only community college). I completed two years of work in 18 months, and entered the University of Chicago as a junior in September 1928. The first course for which I registered was an introduction to physiology taught by Ralph Gerard. He had just returned from a two-year National Research Council Fellowship during which he had worked in the laboratories of two Nobel prizewinners: Otto Warburg, with whom he had studied nerve cell metabolism, and A. V. [Archibald Vivian] Hill, with whom he had measured the heat generated by the nerve impulse. He strolled into the room garbed in an acid-eaten lab coat, sat on the table, lit a cigar and asked, “What is life?” To each answer he asked how could one prove it. At the end of the hour he told us to go to the library and come back with reports of studies which demonstrated our theses. This was the pattern followed through the entire course; there were no lectures. The students moved from system to system, read the literature and attempted a critical discussion of the reported material. In the laboratory, after demonstrations of the equipment, we were told to develop experiments with the proviso that approval would be required to start and to stop the study. My lab partner and I learned how to pith a frog, to make hemostats out of matchsticks and rubber bands; to record blood pressure, found that adrenalin sometimes caused a rise, at other times a fall of blood pressure and attempted a study of the conditions which might predict the outcome. In the next seven years I majored in Physiology and secured my S.B., Ph.D. and M.D. There were two other graduate students in Ralph Gerard’s laboratory at that time: Mabel Blake and Wade Marshall as well as a National Research Council Fellow, George Wald, who was later to receive the Nobel Prize. We were all part of a close-knit group of a dozen graduate students, almost all of who earned Ph.D.’s and seven M.D.’s as well. All three of the women married fellow students; Mabel and I were married in 1933, Louise Hanson and Wade Marshall a year later. Wade was appointed Instructor in Physiology at Johns Hopkins shortly after graduation and Louise had their first child. They were the first friends we visited when we came to Baltimore for residency training in 1937. Gerard's wife, Margaret Wilson Gerard, who was a Ph.D. in neuroanatomy, had become a psychoanalyst while Gerard was overseas. I asked her where she thought I might get my first experience, and she recommended either Johns Hopkins or Boston Psychopathic. I applied to both of them but Hopkins came through first and I accepted it. Hans Stetten's roommate at Harvard, Jerry Frank, was a year behind me at the Phipps Clinic at Hopkins. We went to a lecture by Frieda Fromm-Reichman, who talked about her work, which we found fascinating. Another experience, when I was a resident at Hopkins, in order to have vacations they would close one of the male wards and one of the female wards. We would send the most disturbed patients to another hospital. We had a very sick patient we sent to Sheppard Pratt Hospital. Three weeks later she walked into my office. I thought, "What's going on at Sheppard Pratt Hospital that could bring about this miracle?" So I suggested to Mabel that she apply. Later, to complete my training, I applied for a fellowship at the Institute for Juvenile Research and for a Rockefeller fellowship to support analytic training. The analyst who was assigned to analyze me was so busy that in the course of three months' waiting, we decided that we liked the Washington group better and returned to Sheppard Pratt. Back in the Hopkins days, a Navy recruiter had persuaded many of us to join the Naval Reserve. Groups of three of us were called up before Pearl Harbor. One was Larry Kolb, whom I knew from

Dr. Meyers' brain-modeling course at Hopkins, who was in neurology. The other was Donald Dodge, who was the chief resident at New York Neurologic. The three of us turned out so much work that they kept us there for over a year. Then our chief became Chief of Psychiatry in the Navy and he sent us to duties where he thought our interests and strengths were most useful. I was Board Certified in late 1941 and had the good fortune of being assigned duties which were professionally challenging with responsibilities which would have come only much later in civilian life. In the last 18 months of the war I was ordered to the Office of Naval Command in the State Department and detailed to OSS. I was assigned to a group headed by Henry Murray engaged in evaluating volunteers for service behind enemy lines. Although I had come to value and respect the psychologists I had worked with during residency training this was to be my first experience of working with a very sophisticated group as colleagues with full sharing of responsibility. Here is a list of the psychologists who were in that group [from the *Assessment of Men: Selection of Personnel for the Office of Strategic Services* book]. Gardner later became secretary of Health, Education and Welfare.

Farreras: They're mostly M.D.s.

Cohen: Yes, M.D.s or Ph.D.s That's when I became very well acquainted with psychologists for the first time.

Farreras: Was there a difference in the duties of psychologists and psychiatrists?

Cohen: No, we were all working together as a group. During the war Mabel had worked full-time at Chestnut Lodge Hospital in Rockville as had her friend Josephine Hilgard. They had finished psychoanalytic training at the Psychoanalytic Institute and were three years ahead of me. Mabel had also served as a consultant to OSS Jack Hilgard, Josephine's husband, who was later to become Chairman of Psychology at Stanford. They were examining people who were volunteers to go overseas. After release from active duty in April 1946 I divided my time between private practice and Chestnut Lodge, where I shared an office with Larry Kolb, who was working at NIMH at the time. In 1948 I went full-time to the Lodge; I was appointed Clinical Director, continuing classes at the Psychoanalytic Institute from which I graduated in 1950 and finishing analysis with Frieda Fromm-Reichman. In addition, I was Consultant in Psychiatry to the National Naval Medical Center and served also on the Panel on Human Relations and Morale of the Research and Development Board of the Defense Department. Larry [Kolb] and I were both examiners for the board exams and we were in Frank Braceland's group of examiners. We were among the group of people who were active in the relative beginning of the psychoanalytic movement. When I started as a resident in psychiatry, there were 1889 members in the American Psychiatric Association, and probably no more than 100 were psychoanalysts.[1] The new president of the

1 When I began residency training in psychiatry in September 1937 it was not a widely accepted specialty. My 1935 class at the University of Chicago did not have a single lecture in the subject; my wife's 1937 class had six lectures. Harvard, Yale, Columbia, Michigan and Iowa had residency programs as did some of the large private mental hospitals and a number of State Hospitals. In September 1937 there were 1889 members of the American Psychiatric Association. The American Board of Psychiatry and Neurology had just been established in 1936 – many years after such Boards had been established in medicine, surgery, cardiology, obstetrics and gynecology, ophthalmology and other specialties. It would require three years of residency training, two years of practice for eligibility to take the examination. In 1937 there was only one staff member at Johns Hopkins who had taken and

American Psychiatric Association was always invited to join the group of examiners, so that's how I met Bob Felix. George Raines, Chief of Psychiatry in the Navy, had decided that the medical officers in the armed services should be analytically trained. I won't go into the difficulties and the problems that caused, but at least Bob Morse and I were appointed by the Psychoanalytic Society to work out a program where it would be possible for five military officers a year, Navy and/or Army, to get analytic training for four years, which was somewhat of an achievement with the rigidities that already existed there. So I guess my having a Ph.D. degree, an M.D. degree, and psychoanalytic training led Bob Felix, late in the summer of 1952, to offer me the job of developing the Clinical Research Program at the Clinical Center – with 100 beds on six wards, two on each of the three floors – which was to open in March 1953. Bob Felix had decided to go into analysis himself so he'd learn something about all of this and had taken courses at the Washington School of Psychiatry. Seymour Kety had also tried, unsuccessfully, to recruit a psychiatrist. Then Bob Felix asked me. I agonized about it and thought the plans were utterly and completely crazy. I asked Felix what sort of program he had in mind and he said, "Anything, anything that you want to do. You can have $1 million to hire staff. Don't worry about nurses or social workers; they'll be hired out of the hospital budget. You can go anywhere in the country or out of the country to observe the work underway or bring in consultants. You can invite anybody to come." My $15,000 salary would

---

passed the examination. By World War II there could not have been more than 3000 psychiatrists; by 1967 there be the top of the Civil Service scale – all others would be lower, but within that constraint it was expected that I would have at least $1,000,000 to start out. There was no flexibility with respect to the opening date; Congress had been promised that research would begin in March. He agreed with me that ideally it would be preferable to grow more slowly, to have time to find several senior staff and to develop with them the program that would be instituted. But he was certain that we would have complete freedom and full understanding from experienced administrators. He introduced me to leading administrators in each Institute, partly, I imagine, to expose me to them as much as them to me. I knew several members of the NIMH staff. John Eberhart, Director of Extramural Research, had visited Chestnut Lodge when Stanton and Schwartz applied for a grant for the sociological study of a mental hospital ward. It was the 51st grant approved, one of the earliest in that program. Donald Bloch from the Lodge staff had enlisted in the PHS Commissioned Corps and was working in the office of Dr. Joseph Bobbitt, head of the Community Service Division. Wade Marshall had set up a very sophisticated physiological laboratory even before the creation of the Intramural Program. I knew of John Clausen's earlier studies at the Institute for Juvenile Research although he came there long after I had left. More recently he had carried out important studies at the Public Health Center in Hagerstown. Marshall and Clausen already were established as branch chiefs in Kety's Basic Program. I was deeply conflicted by the offer. On the one hand I felt that the

---

were 15,813 – largely the result of the financial support of NIMH.

government taking responsibility for a widespread human problem was socially very desirable. While the salary was low – I was getting $17,500 at Sheppard Pratt – my wife and I were fortunate in that together we would continue to have an adequate income. But the prospect of rushing to create a functioning world class 100-bed research institute with one senior person meeting a newly formed group of men and women who had never worked together within the larger setting of a 500-bed hospital similarly constituted seemed like attempting to unscrew the inscrutable in the words of my old professor Adolf Meyer. I called Felix and declined his offer. But my conflict was obvious. A week later he called to say he could offer me two or three additional senior super grade positions. I thought it would be worth a whirl. I spoke to people who were being called up because of the Korean War and several good people said, "Well, we'll come" so I accepted Felix's offer and came toward the end of 1952. Before the Clinical Center opened the professional specialty boards had agreed to recognize two years of service in the various specialties as counting toward certification. The Korean conflict was in full sway in 1952. Men who had been too young to be drafted during World War II were being called to active duty; many preferred the Public Health Service to the military. I was literally deluged with applications, and had my choice of men who were being trained in excellent medical school departments. I tried to find young women, too, but succeeded in recruiting only one – Julianna Day from Johns Hopkins. I approached men and women at the professorial level whom I knew personally or by their publications. I covered the country from coast to coast and border to border but totally without success. Three longtime colleagues who were superb clinicians and highly regarded teachers who had not served during World War II said they would come if they were called up for service. Then the war ended and they sent me best wishes. So here I finally had accepted this position and the men who had told me they'd come called me to say they were not drafted. There I was, with no staff. Then many residents in psychiatry applied and that's how we started. The structure of the Clinical Center at the time was actually a little different than it seemed. Seymour Kety was the Scientific Director of both NIMH and NINDB. In ordinary circumstances, the Scientific Director would be the one in charge and my superior, but it wasn't that way. He was the Scientific Director but he had no clinical experience so he tried to find a psychiatrist and made offers but had had no luck.

Farreras: Why did it have to be a psychiatrist?

Cohen: They would have had to have a psychiatrist, to take medical responsibility for psychiatric patients. But I'm sure that if Seymour had asked me to come and I would have been responsible to him, I would not have come, because I did not want to be responsible to someone who was not a clinician. Bob Felix said I would be responsible to him. When I arrived on December 30th [1952], the first person I saw was Edward Evarts and his wife, Josephine Semmes. I was wondering at that time whether I had made a terrible mistake. But they were a pleasure to talk to and interested in what we might do. They had both worked with Lashley [Karl Spencer] at the Yerkes Lab. Josephine Semmes had a fellowship at New York University in psychology and Ed was still a second-year resident. I told him to get in touch with me when he finished. He called up after we'd been going for six months and told me Public Health Service turned him down because of a heart murmur. I called Felix, who arranged to get a waiver so he could come on active duty. We were interested in LSD, thinking it could create an experimental psychosis, and at that time we believed it was safe to take. Evarts and Charlie Savage organized a series of studies on LSD, not only clinically, with volunteers and patients, but with animals as well. He wanted to learn where and how LSD acts. He finally went to Bernard Brody's Laboratory at the Heart Institute to determine its site and mode of action. I was in Paris visiting the hospitals in Europe when I got a letter from Ed saying, "I met a young pharmacologist, Julius Axelrod, and we should have him in our lab." Ed enclosed letters from Dave Shakow and Bill Jenkins, who was the chief of clinical care, and they agreed that Axelrod was a good person. Axelrod agreed to come if he could be promised a professional appointment. The appointment Axelrod had in the Heart Institute was essentially that of a technician in pharmacology while he was getting his Ph.D. at GW [George Washington University]. The reason Axelrod could come was that NIH, in those days, had a crazy requirement: that if someone below the professional level earned a graduate degree, that qualified him to be employed professionally. He could return to NIH but not in the institute he was working in as a sub-professional. Axelrod's appointment was Ed Evarts' doing, and it turned out to be a marvelous appointment.

Farreras: When did the intramural program start?

Cohen: The day I arrived. Nobody had been appointed at that time. Redl had agreed to come. I was in an empty office. And Hector Regas had given me a book with a lot of PHS regulations. Ed didn't arrive until six months later.

Farreras: But Wade Marshall was already here when you arrived…?

Cohen: Wade Marshall was already here.

Farreras: OK, so the Basic Research Division was already functioning…?

Cohen: Yes, Wade had been doing research for some years by the time Kety arrived.

Farreras: OK, the earliest record I have for the Clinical Investigations division is March '54, an Adult Psychiatric Services under Robert Pittinger...?

Cohen: I was absolutely deluged with requests from psychiatric residents who were striving not to be drafted. I had a hundred beds to fill with patients. And I had to have doctors to take care of them. Pittinger, for example, had been Chief Resident at Yale, and so I appointed him. I knew he'd be able to run a unit.

Farreras: Then there was the Child Research Branch under Fritz Redl.

Cohen: Before I reported for duty the first senior investigator I recruited was Fritz Redl.[2] He was the only person who came who already had an established reputation. He was Distinguished Professor of Behavioral Science at Wayne State. He had been a student of the established psychoanalyst August Aichorn in Vienna (the author of

2 The opening of the Clinical Center was scheduled for March 1953 but was steadily moved back to the final date of July 7, 1953. On that day, Dr. Redl admitted a group of children of staff members as a first step in getting acquainted with a new treatment setting. At that time, too, all those who had reported for duty were assigned offices in the Clinical Center and were able to move out of temporary quarters scattered over the reservation. Wade Marshall's [Neurophysiology] had been operating in one of the temporary buildings which housed also all the extramural programs; John Clausen's sociological studies had been carried out at the U.S. Public Health facility in Hagerstown.

Wayward Youth). He was widely known for his studies of the disorganization and breakdown of behavior controls, and had a degree of success in developing treatment programs for these difficult children. Among his books were *Children Who Hate* and *Controls From Within*, about hyper-aggressive, antisocial children. He was a graduate of the Vienna Psychoanalytic Institute; a fellow student with Erik Erikson with whom he had kept in close touch as they formulated their ideas. So he was the first person who became part of what I looked upon as a permanent staff. Most of the others were qualified to fill temporary positions. I wanted to have the sort of organization that I had dreamed of as a possibility. In the early days I talked to many colleagues about the areas of research in which we might be engaged and the disciplines that might be represented. We might study behavior disorders in children, mood and thought disorders (manic-depressive and schizophrenic patients), and psychosomatic disorders (disorders of body function) – and in every instance take advantage of our freedom to study and compare patient behaviors and processes with those of normal controls. The disciplines represented would include psychiatry, clinical and developmental psychology, sociology, anthropology, physiology, biochemistry, and pharmacology. These, of course, were day dreams that at best would be implemented one segment at a time; but one essential difference between the program I envisioned and that of any psychiatric organization of which I had been a part was the hope that the study of the clinical condition would always be interdisciplinary and that whatever was studied in the pathological would be studied as well in the normal. I had in mind a variation of the model of the Physiology Department in which each member of the faculty was engaged in research in one system: e.g. the digestive, the endocrine, the circulatory or the nervous, but where everyone kept abreast and was concerned with all advances in each area. I called several younger men whom I had met in recent years for whom I had high regard: Morris Parloff in psychology, Roger MacDonald in psychosomatic medicine, Charles Savage and Irving Ryckoff in psychiatry. I had interviews with a multitude of residents and tried to choose from among them those whose interests and/or experience fell within the several areas of study in which I hoped we would engage.

Farreras: Do you remember the oral history Eli Rubinstein did with you in 1975 that I was telling you about?

Cohen: Yes.

Farreras: In it you had mentioned that Kety's first choice for Lab Chief of Psychology was Roger Sperry, and that David Shakow was his second choice.

Cohen: Shakow wasn't his choice either; it was mine.

Farreras: Oh, the transcript mentions that your first choice had been Fritz Redl?

Cohen: When I was looking for possible Lab Chiefs I had gone to Shakow for advice. Shakow had been to important places and was a distinguished consultant. When the Division of Mental Hygiene was elevated to the status of an Institute in 1949 Felix had assembled a group of leaders in the relevant fields to advise him on the structure and activities of the new organization. Among them was David Shakow, then a Professor at the University of Illinois and the University of Chicago whom

I knew only by reputation – but among other experiences he had been associated for some years with the research program at the Worcester State Hospital in Massachusetts - one of the few settings where substantial psychiatric research had been systematically carried out. I sought his advice in identifying psychologists in both clinical and developmental areas. First we went through the list of senior and junior men and women I had worked with at OSS. Shakow would go over the list with me, pick out people, and then I would get in touch with them and have them come. A number came to look us over, told us how intrigued they were at our setting and our plans and wished us success – but not with their participation. Nobody ever came for less than he was making; I was the only one. Kety had similar experiences in his efforts to recruit experimental psychologists. Earlier on he had tentative expressions of interest from several who indicated that they might be interested when the Clinical Center opened – among them Roger Sperry – but when the time came they had made other commitments. And so I was consulting with David Shakow and it was apparent that Shakow was as distressed as we were by our failures. He felt that psychology should be strongly represented in the new national program. It occurred to me that he might come himself if we offered him the opportunity to develop the program for both the Basic and Clinical Divisions. There was already a pattern for that – since Clausen had established the Socio-Environmental Studies Lab in Kety's program before I was appointed – and I definitely wanted a group in that area in my program. I had given him funds and support to appoint staff who would work in the clinical program. After some consideration Kety agreed; Shakow accepted and Psychology was established as the largest Lab in the intramural program.

Farreras: Was Shakow considering himself as part of the picture at that time? Part of the pool of possible Lab Chiefs?

Cohen: No, he was a consultant. Every appointment I made in psychology, I discussed with him, and Dave was very much interested in the fact that I was interested in trying to record a psychoanalysis, because he was interested in psychoanalysis. He responded favorably to the idea that I wanted normal child development to be part of the program. And I had first met Harriet Rheingold [later in the Laboratory of Psychology's Developmental Section] when we were both at the Institute for Juvenile Research years ago.

Farreras: Why was it important that you make an offer to Shakow in both programs?

Cohen: I thought that if he were going to be offered the job of developing a Lab in both basic and clinical that he would be more interested and more likely to accept the position. Seymour himself, by that time, had been turned down by a number of people.

Farreras: So Kety was searching on his end, on his own, and you were doing your own search?

Cohen: He was looking for somebody and Sperry had not agreed to come.

Farreras: I see. Do you know who else he considered for the position?

Cohen: I don't remember who they were.

Farreras: Were the people you were consulting with Shakow from this list [*the Assessment* *of Men: Selection of Personnel for the Office of Strategic* Services book]?

Cohen: Yes, I'd gone through that list with him. But the idea of coming just wasn't attractive to them.

Farreras: Was it because of salary, because NIH was a government agency or…

Cohen: Both. Many people in the field were really negative about the government – very early on, one or two people were turned down because they might have been involved in liberal causes, etc. One day I got a telephone call from a doctor in Lansing, Michigan, describing a set of schizophrenic quadruplets. We sent a social worker out who confirmed they were monozygotic and that the whole family would be willing to come. They all came and stayed together in the hospital for three years. Dave Rosenthal was the one who organized the integration of the study. Each of the patients was seen by a psychoanalyst and each week, the group of psychotherapists met with [unintelligible]. Five laboratories took part in that study. Dave Rosenthal kept in touch with those patients. One thing I've always felt a little bit sorry about is that Dave's name got lost with his illness. So many people talk about it as Seymour Kety's study. Dave was an equal principal, the three of them worked very closely together. But in the first book that came out, Dave was sole author [*The Genain Quadruplets*, 1963]. His illness was just tragic. Looking back, the plan I developed for the Clinical Intramural Program could be considered grandiose, but there was a sense of urgency, a belief that this was to be a one time opportunity not subject to growth and gradual development. The NIMH budget in 1952 was close to 12 million dollars. When Felix testified before Congress, Lister Hill asked how much he thought he might one day request; he told us that he took a deep breath and responded, “Senator, I believe the day will come when I will ask you for 25 million dollars.” There was pressure to fill the beds or to lose them. Doctors from the Cancer and Heart Institute came to visit our wards desperately seeking space; we surrendered one unit which we could well have used later on. Early on there was a sense of urgency in our search for staff. As I look back some of that was gratifyingly successful, but I believe, in balance, we did not find them as much as they found us.

Farreras: We’ve covered much more than I was expecting to cover on our first day. Would this be a good time to quit for today?

Cohen: Yes.

Farreras: Alright, I want to thank you for spending so much time with me talking about the early days of the establishment of the Clinical Investigations Division. I look forward to learning more about the later years at our next meeting.

# # #