

Morris Parloff
(NIMH 1953-1983)

This is an interview with Dr. Morris Parloff, former member of the Laboratory of Psychology of the NIMH Intramural Research Program, held on January 3, 2002, in Bethesda, MD.

The interviewer is Dr. Ingrid Farreras of the NIH History Office. Note: *Revised April 18, 2002, by Morris Parloff.*

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

Parloff: Oh, good. I finally have the opportunity to give my own version of that classic line, "I was born, as though through an error of destiny, into a family of workers." Actually I was born in 1918 into a lower-middle-class Russian immigrant, "causally" Jewish family. My parents' formal education, while limited, was nonetheless rich. As I recall, their mastery of English progressed well, and finally they both proudly received their citizenship papers. As I was later told, my late entry into this already fully cast family had been a source of great embarrassment to my two siblings, my brother, Harry, a 13-year-old child-prodigy violinist, and my sister, Sarah, a 9-year-old piano-playing dervish. During the first few years of my life, due to Harry's frequent hospitalizations for his juvenile diabetes, it was necessary for Sarah to act as my baby-sitter and at times as my mother-surrogate. As I was growing up in Cleveland, Ohio, for quite innocent and sufficient reasons the family moved about quite a lot. We ambled, for example, from the modest 105th Street area, to Cleveland Heights, to Euclid Heights, and finally to

prestigious Shaker Heights – and back again. When I was 16, I wrote my autobiography as an English class assignment. In the process I first discovered that I had already transferred schools no less than 13 times. As a consequence I had the unenviable experience of quite regularly being the “new boy” in both class and schoolyard. I must confess that I did present an attractive target for all aspiring schoolyard bullies. I was begoggled, overly plump, and “mild-mannered” – but unbeknownst to them -- in the manner of a short Clark Kent. But that was only true after I had successfully learned to box and wrestle. (Actually, I became a rather good boxer, and boxing continued to be one of my favorite gymnasium sports throughout my college years). Unfortunately, our family experienced tragic losses while I was still quite young. When I was three years old, Harry succumbed to his diabetes, and about two years later, my father was killed in a freak accident while at his workplace as a custom tailor. This necessitated that my mother take on the role of breadwinner. With the explanation that she had to save the insurance money for some “rainy day,” she returned to her girlhood occupation as a seamstress and dressmaker. However, in order to remain available to me, she initially worked out of our home. Later she took full-time employment as a salesperson in women’s apparel shops and became reasonably successful at it. Nonetheless, when my sister turned 16 she left school at my mother’s urging to find work and help support herself and the family. Ultimately she managed to do that very well. Finally, the combined earnings produced by my mother and sister, plus the income from my mother’s wise investments in rental properties, returned the family to a comfortable economic state. Meanwhile, I was urged to remain in

school and pursue my education. Clearly, our financial state had improved, witness the fact that when I graduated from high school in 1936 I was presented with the gift of a trip to Europe, which included touring the USSR for a full month. Finally, six years after my mother had been widowed, she remarried – sometime in 1929 – the head of a highly successful home construction firm, Oscar Goldfarb. Very soon afterward, however, we began to feel the effects of what has come to be known as the “great depression” of the 1930s. By 1933 Goldfarb’s business had gone completely and utterly bankrupt. In retrospect, perhaps the most important impact on me of that all too brief marriage was the opportunity it gave me to observe Oscar successfully deal with that unmitigated catastrophe. I developed a deep and enduring admiration and respect for him and his often demonstrated qualities of fortitude and integrity, and particularly for his seemingly inexhaustible resourcefulness. I am forever grateful to him.

By 1937 I had completed my freshman year at Western Reserve University (now known as Case Western Reserve University). I had hoped to carry a double major in psychology and sociology; however, during the course of that year I was earnestly advised that majoring in chemistry might, in view of the long continuing depression, be a far better career choice. That seemed wise, so for my sophomore year I transferred to Ohio State University, which I knew had an excellent chemistry department. In the course of that year I met a wonderful instructor who was then teaching his very first year in sociology. He made it seem so fascinating that, as his wife once put it, he had not only introduced me to the field of sociology but had seduced me into it. In the process of taking a number of

sociology courses in addition to my chemistry lab courses, I created a scheduling problem for myself. It became necessary for me to try to fit in 9 hours per week of laboratory time by intruding upon lab space formally assigned to some other students. I soon learned that this was not an ideal way to conduct the required experiments in my quantitative analysis course. All too often, my unhappy lab partners would inadvertently sneeze or cough into my open desiccator just as I was finally preparing to weigh the contents of my carefully fired crucibles. By the time I managed successfully to complete that course and the academic year, it had become quite evident to me that my talents lay in the social rather than physical sciences. But what finally precipitated my decision to leave Columbus, Ohio, and to return once again to Cleveland and to Western Reserve was the compelling fact that during the summer of 1937 my stepfather suffered a heart attack and died. At my mother's insistence I agreed to return home. This time, however, I had to "work my way." I received my A.B. degree from Reserve in 1940. Before I go any further with this monologue about the course of my education I wish to add a few pertinent facts about my mother and my sister, Sarah. In the course of my mother's life she developed a great interest in performing community service. She was very active in a number of charitable organizations and served as president of at least three major ones. Somewhere along the line she had become a very articulate, persuasive and amusing public speaker. I'm sure she would want me to add that she was also a very fine penny-ante poker player. She died in 1958, at the age of 72, from her second bout of cancer. I have heard her

described by some of her many friends as a very strong, caring, funny but “formidable lady.” I would not quarrel with that characterization.

With regard to Sarah, she continues to be a bright, alert and much treasured sister, mother, grandmother, and great grandmother.

Farreras: And she’s about to turn 92.

Parloff: Yes, I won’t say she’s hale and hardy anymore, but she’s certainly doing well.

Farreras: How did you get involved in psychology in the first place? You said you were looking at chemistry as a way of making a living but that your passion was really in psychology and sociology.

Parloff: Yes, I had long had a special interest in psychology.

Farreras: How did that come about?

Parloff: When I was 14, I came across Brill’s translation of Freud’s *Introduction to Psychoanalysis*. I thought it was very sexy. That fact alone, I believe, was sufficient to initiate an enduring interest. So naturally I began reading Freud. Later I discovered and avidly read Stekel, Fenichel, and other titillating stuff like that. But obviously, during those early years I wasn’t at all sure that psychology and psychiatry, for that matter, were fields to be taken seriously as options for life careers. Could one really hope to make a living doing that? That question was always a central consideration as I was growing up during the depression. But when I later was admitted to Western Reserve I took Calvin Hall’s freshman course in psychology. It included a particularly fascinating lecture on Morton Prince’s clinical work on split personalities. Fantastic! I expect that that experience might have been one of the more important influences that ultimately

contributed to my final career choice. But there were, of course, other significant teachers in the undergraduate psychology department, like Professor Hartley, who were instrumental in developing my interest. In any event, by the time I completed my freshman year it had become clear to me that a career in psychology might be fun but was not practical.

Farreras: And from there you then pursued a master's degree in psychiatric social work at the University of Chicago?

Parloff: Yes, that again was a decision based on the same dominant theme: I must prepare myself to make a living in the environment of a seemingly unending depression. (I did not anticipate that within a few years World War 2 would serve to end that depression.) I therefore decided that in a welfare era I'd much prefer be on the giving rather than receiving end of "relief." I decided to combine my interests in psychology and my regard for the economic realities by applying to the School of Social Service Administration at the University of Chicago. I knew it was a great university. A further enticement was the fact that they gave me a half-scholarship. So in 1940, I enrolled. Since it was necessary to support myself I supplemented my meager funds by taking some of the typical types of student part-time work – shoe salesman, theater usher, and so forth. Perhaps the best paying job of all was bus boy and dishwasher at the Ellis Avenue Eating and Housing Co-op, which I happily joined. I still remember the address – 5558½ Ellis Avenue. That association helped me also to completely defray the costs of my food and lodging. It was at the Eating Co-op, too, that I met Gloria, who, as

you know, later became my wife. On December 27 of this year we hope to celebrate our sixtieth anniversary.

When we met, she was a young sophomore at the U of C and a Co-op member, where she was a kitchen co-worker. Actually she was a food server. In time she became my favorite “serving wench.” She, quite understandably, was dazzled by my suave maturity and particularly by the deftness with which I cleared the dirty dishes from the restaurant tables. So we, quite naturally, “took up with each other.” At some point I was appointed to the nonpaying position of editor of the Co-op’s newsletter, which was appropriately titled, *The Eater’s Digest*. In recognition of Gloria’s demonstrated talents as writer and copy editor, she was soon invited to be the newsletter’s assistant editor. Years later, despite the fact that she was a geography major, her interests intersected mine when she became the assistant managing editor of the journal called *Psychiatry*, which is based here in Washington. The journal was then edited by Mabel Cohen, the very eminent psychiatrist, wife of Bob Cohen. Gloria and Mabel also used to play piano duets together. Later on Gloria became the managing editor of *Psychiatry*, working under then-editor Don Burnham.

But I digress. Between 1940 and 1942 I completed my academic work in psychiatric social work and graduated with an A.M degree. Frankly, I did not enjoy much of that experience. However, I have particularly appreciated the courses that prepared me for my later work as both clinician and researcher. Surprisingly, the training I received in statistics from, I believe, a Professor

MacMillan was terrific. The next step in my career involved spending some four years in the Army from 1942 to 1946. Unfortunately, the Army personnel office chose not to recognize the potential value of my specialized psychological training. Instead, I wound up a commissioned officer leading a small combat military intelligence unit in Germany for 18 months. Perhaps one of the most useful bits of learning that I was to apply in my subsequent civilian clinical practice and research was the ability to discriminate between a psychiatric interview and an interrogation. After returning from service I worked for some two years as a psychiatric social worker at the VA. There I had the great good fortune of working with Dr. Jerome D. Frank, a psychiatrist with whom I shared a number of cases. He later hired me to work with him and Florence Powdermaker on a VA-sponsored research project investigating the nature and usefulness of group psychotherapy in the treatment of psychoneurotics and also schizophrenics.

Farreras: When did you meet Dr. Harry Stack Sullivan?

Mrs. Parloff: That was in 1946.

Parloff: Yes, I studied with him at the Washington School of Psychiatry from 1946 to 1949, when he died. By the time I went back to Western Reserve in 1949 to get my Ph.D. in clinical psychology, Calvin Hall, who had remained an ardent Freudian, discovered that I had been seriously tampered with by Sullivan's views. Calvin retained a very fundamentalist psychoanalytic position, still believing in eros and thanatos and other such early concepts. But by that time I'd been liberated and enlightened by Sullivan's notions.

Farreras: Was that after a period of, let's see, six years? The war had intervened...

Parloff: Yes. Nonetheless I had a wonderful time with Hall, to the point where, later on, I tried to hire him to be chief of the NIMH Section on Personality and Its Deviations.

Farreras: Right, the name of the early section.

Parloff: I don't know who gave it that name but I think it was an act of inadvertent hostility because it was very obvious that anybody who worked in the section would inevitably be greeted with "Oh, that must be one of its deviations." So the staff began to petition that we change its name. I have found a memo I wrote in 1957 urging that the name be dropped and that we simply become the Section on Personality. Shakow kindly did us that service.

Farreras: Do you still have that memo?

Parloff: I don't have the memo saying, "You are now converted," but I do have the memo in which I requested the change. The name was subsequently changed.

Farreras: Wonderful!

Parloff: Would you like a copy of it?

Farreras: Yes, I'd love to have a copy. We can do that later.

Parloff: I'll make a note of it. I'm bouncing around here. Can you give me some direction?

Farreras: Well, you were talking about your education, how you hated social work, and then the war broke out...

Parloff: Yes, I want to pause on that. First of all, I dictated my dissertation to Gloria. I was going to be drafted on a date certain, July 18, 1942, and I was still doing my dissertation. And Gloria said, "I can type faster than you," and better anyway.

She was an editor even then. And so I dictated it. And I never saw it in its completed form because by the time it was typed in its final form and all the rest, I was already in the Army. But I did write a dedication for the dissertation which you should know about. I wrote it in this fashion: "This dissertation is dedicated to me, without whose help Gloria could never have written it." Well, they took it out.

Farreras: They did?

Parloff: They did, indeed. They didn't think it was seemly. However, some years later, in 1977, here in Washington, I was chosen by the University of Chicago Club as the Man of the Year. I had the opportunity to deliver an acceptance speech in which I was able publicly to beseech them to restore that dedication in honor of Gloria.

Farreras: So you had begun doing dissertation work before you went off to war?

Parloff: No. This was only my master's degree.

Farreras: Okay, that was your master's thesis.

Parloff: Yes, that was my master's, in social work. Unfortunately, the military did not understand what that was about. So initially they put me into personnel work, where I was made a sergeant. Later, with my newfound powers I was able to send myself off to the ASTP.

Mrs. Parloff: The Army Specialized Training Program.

Parloff: Right, the ASTP. I had managed to be shipped off to join the psychologist training program of the ASTP, but on the day I arrived at CCNY the program had been disbanded. So I spent that night reviewing my German at the home of my former roommate at the University of Chicago, the Reverend John Porter, who

was then in New York studying to become a minister. I stayed with him overnight brushing up on my college German. The next morning I took and passed the entrance exam and was admitted into the German Language and Area Program of the ASTP at Boston University. After about six months of rigorous training I was sent to Camp Ritchie, Maryland, the Army's Military Intelligence Training School. Some of the courses were given in German. My specialization was in conducting the interrogation of German prisoners of war and analysis of documents of potential intelligence value. I mention all this because recently the media have made a great point of the fact that a CIA agent had been killed while doing similar POW interrogation of prisoners and documents analysis in Afghanistan. That was to have been my job in Germany. Luckily, when I completed my training at Ritchie, I was directly commissioned as a second lieutenant. However, by the time I was shipped to Europe, my outfit, T-Force, 12th Army Group, found other equally interesting things for me to do. I spent 18 months in Germany, during which I collected a silver combat star and the Bronze Star. My outfit was a headquarters for an array of specialized intelligence-gathering units. The peculiarities of my job may be conveyed by the fact that while much of my time was spent in Class A uniform, some of my most memorable moments were those "beyond enemy lines." I still maintain a warm association with a former member of my team who subsequently, in civilian life, rose to prominence in the State Department, where he achieved Ambassadorial rank in the UN's Human Rights Office. More recently he has served in the White House as a National Security Advisor. I just wanted you to understand that I

expect you to make me look good in this history you're compiling, since I have friends in high places. Perhaps that's enough of my military experience. It's interesting to me but probably has had little to do with my career as a psychologist.

Farreras: And you returned in February of '46?

Parloff: Right. And Gloria was living here in Washington.

Mrs. Parloff: When he came home as a social worker, he immediately had the opportunity to work as a social worker in the VA, and there were three options as to where we might live: his home in Cleveland, my home in Chicago, or Washington. And we decided to stay in Washington.

Parloff: We agreed that it might be better if we settled in a town where neither of our families resided. It was a good decision to move to Washington, D.C. The head of the Social Service Department at the VA Regional office where I worked was Cynthia Nathan, an extraordinary person with whom I still have periodic contact. She helped me a great deal, and within a relatively short time she promoted me to an administrative role. In those days, men in the field of social work were expected to be administrators. As I said, during that period I met Jerome (Jerry) Frank, who was working in the VA Mental Hygiene Clinic. As you may know, subsequently he rose to be a leading figure in the field of psychotherapy research. He was a clinician, researcher, educator and administrator. In the cases we carried together, I might treat the family and he the veteran, or vice versa. We got to know, respect, and like each other very much. While at the VA I discovered that I didn't really like social work very much, so after two years at the VA I decided

that I would return to school to get my Ph.D. in clinical psychology. I applied and was accepted at the University of Illinois and was awarded a VA fellowship. I was scheduled to work with a professor by the name of David Shakow, who was then unknown to me. You will recognize that he is the same man I was later to work with at the NIMH for some 19 very happy years. But at the last moment I decided not to accept the offer from the University of Illinois. What happened is that a few weeks before I was to return to school, Jerry Frank called and offered me an extremely attractive job working with him on a VA research grant of which he was the Principal Investigator. He planned to investigate the mechanisms and effectiveness of group psychotherapy as a treatment for hospitalized chronic schizophrenics as well as ambulatory psychoneurotic veterans who were being treated by the VA. Specifically, I was to work at Perry Point Hospital on the schizophrenia project and also consult weekly in Washington with the investigator team working on the psychoneurotic project. The salary I was to be paid seemed absurdly high. When Jerry described the position, I recall laughing out loud and saying, "Jerry, you have just mentioned a couple of fields – schizophrenia and group therapy – that I know absolutely nothing about." He replied, "Good, then you'll come to us with an open mind." How does one refuse an offer like that? So I accepted the job. I thought I can surely go back to school later. Moreover, having had some real research experience would make it even easier for me to be accepted into a Ph.D. program. So I took the job and found myself also working with a colleague whom you will recognize, namely, Dave Rosenthal. We formed a life-long friendship. In addition, I met some other wonderful colleagues who

have been great friends over the years. Interestingly, the Washington School of Psychiatry, where I had been enrolled as a student of Sullivan's since 1946, agreed to serve as our grant-administrating agency on behalf of the VA. So I began working as a researcher there and I loved it. I would have been content to continue working there with Jerry, Dave, and Florence Powdermaker but an extraordinary thing happened. We were then being supported by a Veterans Administration Grant. When what we all believed to be a routine grant review came up we were stunned to learn that the project's funding had not been renewed. Notice of that decision was given to the staff around June of 1949. I was very upset, for I thought there was too little time left for me to begin submitting applications to universities to arrange admission to a good Ph.D. program by September. Finally, in desperation I telephoned Calvin Hall and told him about my situation. He practically accepted me over the phone.

Farreras: Wow, just like that?

Parloff: Yes. Dave Rosenthal had some contacts at the University of Chicago, so he was speedily admitted there. In driving west to our respective schools we formed a sort of caravan. Dave and his wife, Marcia, in their car and Gloria and I in our vehicle then drove to Cleveland, where I would enroll at Western Reserve. Dave and Marcia stayed with us a few days and then drove off to Chicago. Fast forward. Two years later, when Dave and I had completed our academic work, Dave and Marcia drove back to Cleveland, where we reformed our convoy and drove eastward, but this time to Baltimore, where we worked together at Johns Hopkins. Jerry, bless his heart, had recruited both of us to join him at Johns

Hopkins University School of Medicine, where he was now a Professor in the Department of Psychiatry and also head of another group psychotherapy research program. Dave and I were awarded academic rank. Dave, however, was given an administrative position at the Phipps Psychiatric Clinic, and I joined the research team, also located at the Hospital. The friendship flourished. I worked closely with Jerry and his staff for yet a couple of years. I can't tell you how wonderful that was. I have been extremely fortunate with the people who have agreed to be my bosses: Cynthia Nathan, Jerry Frank, and, of course, later, David Shakow. But I'll come to Shakow soon. After a couple of years I learned about the planned research institute at the expanding NIMH and began filing the required application for government employment, but my application languished. And then one day, sheer good fortune took a hand. A very good friend of mine, Jarl Dyrud, a psychiatrist whom I had met during my VA days, invited Gloria and me to his betrothal party. We were to stand in for his parents, who could not attend since they lived too far away – Minnesota or North Dakota. Jarl was going to marry Rose Bullard, the daughter of his then boss, Dexter Bullard, the Director of Chestnut Lodge. We accepted. The engagement party was held at the home of Dave and Margaret Rioch. Happily, from earlier contacts, I already knew the eminent Dave Rioch and his wife, Margaret, herself a prominent psychologist. I had known David Rioch from the Washington School of Psychiatry, where he regularly lectured. That guy was a phenomenon. At some point after dinner I found myself seated next to a gentleman to whom I had not yet been introduced. He asked me about my work, and I was pleased to tell him about it at great length.

I told him that I had just finished my dissertation study, which I had conducted while working at Hopkins. He pressed me for the details and I gladly gave them. I had just completed my final data analyses of the findings, and I was happy to rehearse them with him. They appeared convincingly to support my hypotheses. He appeared curious and questioned me gently but intently. As we continued to speak together I noticed that Dave Rioch had come over and was standing behind the couch on which I was seated. He, too, seemed to be listening attentively to me as I prattled on about my central thesis, namely, the primary importance in psychotherapy of establishing and maintaining the therapeutic relationship. And then Dave volunteered his considered assessment, “What a crock of shit.” We all laughed, of course, but I was crushed. After I returned to Baltimore I tried to put the ugly incident out of my thoughts. A couple of days later, I received a telephone call from a man who identified himself as Bob Cohen, my attentive listener at the engagement dinner where we had met. We chatted amiably for a few moments about what fun the dinner party had been, and then he revealed the purpose of his call. “How,” he asked, “would you like to come work with me?” After an awkward pause I finally responded with, “Dr. Cohen, forgive me, but I must ask just what do you do?” I had no idea who Bob Cohen was or what he did. I knew only that he was Mabel Cohen’s husband.

Farreras: This must have been after he had just arrived at NIMH then? 1953?

Parloff: Yes, I expect he arrived a bit earlier than that. His reply to my question was, “Oh, forgive me, I’ll come out to Phipps and introduce myself more properly.” And he did. He visited with me at Hopkins and told me in some detail what he was

hoping to set up at the NIMH. He was and remains a true gentleman. At some point we completed our negotiations and I accepted the position.

When I arrived in Bethesda, I soon discovered that the salary and grade level he had offered me would not be honored by the Civil Service moguls. Despite the fact that I had completed all my Ph.D. requirements, the degree would not officially be awarded to me for several months after my NIMH appointment had begun. I had completed my dissertation “in absentia.” I now want to interrupt this line of reporting and give you a bit more background on this matter. While Dwight Miles, my major professor back at Western Reserve, was my official thesis advisor, at Hopkins, where I had actually perpetrated my research, some of my valuable counsel was provided by my colleague Herbert Kelman. He was then a freshly minted Ph.D. social psychologist who had been awarded a postdoctoral fellowship to work with Jerry Frank and his research staff for a couple of years. His purpose was to learn more about the dynamics of psychotherapy groups. Herbie and I soon became great friends. Since he tended to work late at the office he often found me there, working in the evenings on my dissertation. Periodically he would volunteer to help me with some of the trickier analyses that I was attempting to perform on my data. I don’t believe he ever viewed himself as my “advisor,” but his help was invaluable. We later published several articles together based on my dissertation research. Clearly, I learned a great deal from him. Now, to return to the NIMH situation. Shortly after I received my degree I was, indeed, promoted to the grade level I had initially been offered.

I would like now to try to give you a glimpse of the circumstances that greeted me when I arrived at the NIMH in 1953. The promised offices were to be located in the then new Clinical Center (Building 10) but that building had not yet been completed. We were, therefore, initially housed in a one-story structure affectionately identified as Building T6. All of the NIMH staff members managed, somehow, to be housed there. Frankly, I do not recall the existence of any formal administrative subdivisions at that time. I believe I was directly responsible to Bob Cohen without having any intervening administrators. With the exception of the Adult Psychiatry Branch, and several biochemical units, under Seymour Kety, those psychologists then aboard remained under the relaxed and benign supervision of Bob Cohen. That united psychological-psychiatric administrative arrangement also continued for some period even after we moved into the Clinical Center. The NIMH eventually occupied three floors. The research staff's office space was restricted to the North wing of the building while patient wards, and their associated clinical staffs, were to be located on the South wing. When finally the signal was given and we eager homesteaders streamed across the border into the Clinical Center we found the North wing pristinely vacant. The wards were in the process of being furnished. Behind the easily removable wall panels in each research office, builders had thoughtfully installed pipes that could provide ready access to oxygen. The planners of Building 10 envisioned that such space could quickly be converted into hospital rooms to serve residents of Northwest Washington in the event of an atomic attack.

When the initial few staff members entered the Clinical Center, we found spread before us vast open areas of as yet un-partitioned office space. Happily and greedily, we each staked out our territorial claims. My early office space was huge. Only as new and office-less staff trickled in, did we each reluctantly relinquish portions of our domain. Ultimately, each of us was destined to occupy a relatively small cubicle uniformly painted in what came to be recognized as government standard, *mal de mer* green. The laboratory of psychology, as a functioning administrative entity, did not formally come into being until, in 1954, Shakow made his long-awaited appearance.

Farreras: He hadn't arrived yet.

Parloff: No. I believe his originally planned arrival date had to be postponed due, in large part, to his having suffered a heart attack while he was still working in Chicago. While I had never formally met Dr. Shakow before he finally arrived at the NIMH, Bob Cohen had assured me that Shakow was fully acquainted with my work and had given his enthusiastic approval of my appointment. I would like to believe that but I remain unconvinced. The fact is that when I arrived I discovered there were other psychologists who, similarly, had never been interviewed by our prospective Lab Chief. For example, among the initial Intramural staff members there were Dick Bell and Earl Schaefer. Their appointments had been made via the Public Health Corps mechanism without the pre-approval of Shakow. Both Ben (Virgil) Carlson and I happened to report on the same day. As far as I ever knew, Ben Carlson had also gone un-interrogated by Shakow. Ben had been a graduate student of Dick Lazarus, a colleague of mine while I was at Johns

Hopkins, and Dick had highly recommended Carlson to me. Lazarus also arranged our meeting while we were all still at Hopkins. I didn't really get to know Ben until we later again met at T6. I know that Dick thought he had helped get Benny appointed. Despite the differences in our ages and experience Carlson and his family soon established a cordial relationship with mine. I believe that shortly after Shakow arrived he confirmed my appointment as Acting Chief of the then-labeled Section on Personality and Its Deviations. That section became the refuge for some psychologists who identified themselves as interested in conducting research in some area that could, however imaginatively, be viewed as falling under the umbrella-term personality. As I shall report, the section, at one or another time, housed people showing catholic interests in such areas as group processes, infant development, creativity, psychoanalysis, and so on. This might be a good time to tell you the names and activities of the people who worked in my section over the years of my incumbency –1953 through 1972. Frankly, I cannot distinguish the names of those whom I personally recruited and those who found admission by transferring in (with my consent, of course) from other units within the Laboratory. If you like I can describe some of the activities of some section members beginning with Mike Boomer.

Farreras: O.K. Fine.

Parloff : In 1954, Mike Boomer [Donald S.] and I arranged his transfer from Fritz Redl's program on hyper-aggressive children into our section. It appeared that Mike had become disenchanted with Redl's notions of what research conditions were sufficient to permit one to make credible interpretation of one's research findings.

Similarly, Wells Goodrich, a psychiatrist with whom Mike had been working, also chose to leave Fritz's unit. Wells was then assigned to yet another unit within the NIMH. Mike and Wells continued usefully to collaborate together. Later, as Mike's interests in the processes of psychotherapy grew, he turned to psycholinguistics and kinesics as offering potentially helpful mechanisms for such study. In the process he conducted investigations of the phonetic clause, the basic unit of speech formulation and comprehension. On this basis he studied periodic changes in an individual's speech patterns as indices of the speaker's emotional shifts in response to events occurring in the moment-to-moment flow within conversations or psychotherapy interchanges. Tongue slips was one of the areas of study. I was particularly happy to support Mike's work. At one point we worked together in the use of kinesics as a means of enhancing therapists' understanding of their patients. Allen Dittman joined the section about a year after Mike had. Dittman was a man of many talents. He was particularly helpful to a large-scale process-of-psychoanalysis project by developing the instrumentation to record by audiotape and color movie film each session of an entire course of psychoanalysis. It was necessary to use movie technology since videotaping had not then been adequately developed for Shakow's research purposes. Ultimately, the entire course of an individual psychoanalysis was thus recorded. That particular treatment spanned well over 600 analytic hours. Dittman was brilliant in also developing some novel equipment that helped our research team to study in detail each of those recorded sessions. Since those films were to be made available to qualified researchers, including some students outside the NIMH,

each tape was transcribed and the content was multiply coded in the hope that we could anticipate the psychoanalytic interests of potential investigators. The coding system was organized and supervised primarily by Dittman.

Farreras: When did Dittman arrive?

Parloff: About 1955, about a year after Boomer.

Farreras: And Goodrich was also around the same time?

Parloff: Well, Goodrich never joined my section. He transferred from Redl's unit to the Adult Psychiatry Branch.

Mrs. Parloff: The person who might know the answer to your question about the date of Dittman's arrival is his widow, Laura. We can put you in touch with her.

Farreras: Laura Dittman?

Parloff: Yes. Incidentally, I have written out the names of all the people who ultimately served as members of the section.

Farreras: Great.

Parloff: Earl Schaefer was one of the original section members. As I mentioned, he was one of the first psychologists to join what was to become the Psychology Laboratory. He worked closely with Dick Bell and they developed an inventory – a scale – designed to measure the quality of parent-child relationships (PARI). Dick, incidentally, early on, joined Nancy Bayley's section dealing with child development. Earl, however, conducted research on the factors affecting educational and intellectual development of children. His early work demonstrated the importance of verbal stimulation in the development and maintenance of intellectual performance. This was found to be of dramatic

importance with lower socioeconomic infants (25-36 months). I believe this work provided some of the bases for developing the national Head Start program.

In 1955 I successfully recruited Dr. Herbert Kelman, my old friend from our Johns Hopkins days. Herb had just completed a year at Palo Alto, as a Fellow at the Center for Advanced Studies in the Behavioral Sciences. While he was clearly a social psychologist – and an eminent one at that – I apparently was able to convince John Clausen, the then Laboratory Chief of Socio-Environmental Studies, to let me bring Kelman into my section. Herb then conducted several social-psychological studies on our clinical wards (Depression and Schizophrenia) and also continued and extended his earlier work on developing procedures of social interchange designed to promote conflict resolution. While Herb was in his first year with the section, still completing the Civil Service prerequisite “temporary appointment” period, he simultaneously achieved justified fame for his “conflict resolution” work and unjustified notoriety for being the first investigator at the NIMH, during that paranoid McCarthy era, to be dismissed for nebulous “security reasons.” Soon, however, he became even more widely heralded for his unprecedented victory in successfully challenging and overturning his unjustified firing. Not only was he fully exonerated of the ludicrous charges and reinstated by the NIMH but he also received a letter of apology from the then Secretary of Health, Education, and Welfare, Marion B. Folsom. Herb’s lawyer who had successfully conducted this fight was, strangely enough, none other than the young man I earlier spoke of who had served as a member of my combat intelligence team during WWII, who had subsequently

become a very fine lawyer. This was Richard Schifter, who yet later in his career served in the State Department and represented the nation at the UN. So, chalk up another bit of evidence in support of the “small world” thesis. Herb subsequently published a full and detailed account of this great and convoluted case. Despite my eagerness to tell you all about it, I’m afraid it would merely distract us from our main purpose today, which is, of course, to further aggrandize me. Kelman then graciously returned to our section, but after a short time he accepted an appointment at Harvard. Some years later at Harvard, he was appointed the Richard Clarke Cabot Professor of Social Ethics. Now I wish to talk about another staff member, Albert Caron, who came aboard in 1959. Using his knowledge of personality and cognitive-behavioral theories he conducted research on motivational bases of human cognition. More particularly, his studies concerned mechanisms of insight and concept formation in creative problem solving. He also investigated the conceptual development of the preverbal child. During his stay in the section, Al and I did some work together and published a few papers on the results of our efforts to test specific theories on creativity. However, as his interests in basic conceptual theory evolved we finally decided that it might be more appropriate for him to transfer into the Section on Perception and Learning, where he could work with colleagues who were doing research more closely related to his own interests. Such transfers were not unusual in the Lab since, clearly, researchers’ interests may tend to shift. I estimate that he left the section around 1966-67.

Farreras: So Al Caron left the section but continued to work in the Laboratory.

Parloff: Yes, and, of course, Al and I remained warm friends. Next, there was Lois-ellin Datta. We worked closely on the section's largest creativity project, a particularly interesting investigation. One of its major purposes was to determine the association between personality characteristics and creativity. It involved a study of the best applicants among male high school seniors who competed in the 1963 and 1965 Westinghouse Science Talent Searches. Scientists serving as judges independently rated the science projects of almost a thousand students, enabling us to distinguish between the "more" creative ones and the "less" creative. These data provided a base for various analyses resulting in the publication of a modest number of collaboratively written publications.

Farreras: Yes, I remember seeing Datta's name. When did she arrive?

Parloff: She joined us in September 1963, and left in 1968 to become the National Director of Head Start Research with the Office of Economic Opportunity. Ingrid, with regard to entry and final departure dates, the fact is that some time ago I finally disposed of most section personnel records. I thought that after an interval of 30 some years it was unlikely that I would ever again be asked to write letters of recommendation for any of these former staff. I hope you will recognize that although I do have various documents for many of those dates, we are dependent on only an aging memory for some of them. We come now to Merton Gill. He was then a most honored scholar, theorist, training analyst and author in the field of psychoanalysis. In orthodox analytic circles, he was also considered to be one of the more provocative thinkers and controversial writers. Dave Shakow brought him to my office one day in January 1967, explaining simply that he

thought we ought to meet since we would undoubtedly get on well together. He was so right. What Dave had neglected to mention, however, was that Merton, a dear friend of Shakow's, had just been awarded one of those new and rare NIMH grants, the Lifetime Career Award. Dr. Gill was shopping around for a stimulating place, close by to Dave Shakow, where he might do his research. After Shakow left the two of us alone, Merton turned abruptly from our earlier amicable and relaxed discussion, and like a TV quiz master when the red light comes back on, began sharply and pointedly quizzing me on my theoretical beliefs. He did so in a manner I took to be gratuitously confrontative, badgering, and at times scornful. I responded initially with what I prefer to call good-humored joshing but then moved to a level of challenge and enthusiastic debate usually used only with colleagues with whom one is quite close. In the interchange I believe I gave as good as I got. When he had satisfied himself, he concluded the session by standing up and smiling at me. He then unexpectedly hugged me warmly while loudly proclaiming his great pleasure with our discussion. Clearly, I had passed his test. In retrospect I found that he had similarly passed mine. Dave hadn't warned me, though, that Merton suffered from manic-depression. I later learned that Merton's engaging "manicky" manner was a fair representation of his normal state. He was also prone to long periods of profound depression. A couple of weeks later, I was pleased to receive Merton's letter in which he formally requested to join the section in the role of a "Guest Worker." I delightedly agreed to that arrangement. And thus began a life-long friendship. Merton remained with the section until June 1968. Joseph Handlon

arrived, I believe, in September 1957. He was still another member of our burgeoning UC-Berkeley contingent. Like Boomer and Dittman, he had received his Ph.D. there. Joe initially collaborated with investigators at the Walter Reed Hospital, where he worked on studies designed to increase our understanding of endocrinological influences on psychological functioning. He collaborated with other military investigators there in a research department headed by the distinguished David Rioch, of whom I spoke earlier. Joe later worked closely with me in the area of family therapy and participated in our efforts experimentally to enhance creative problem solving. You will recognize the operation of a basic principle, namely, that everyone in the section, being an independent investigator, did his or her own thing. They could choose to work with anyone whose research studies interested them and would have them.

Mrs. Parloff: Excuse me. This [reprint] indicates a little bit more about Caron's and Dittman's interests; it also confirms that Allen came to the lab in '54.

Farreras: Wonderful. Thank you.

Parloff: Oh, '54, I thought it was a little bit later. Before I conclude this listing of section members, which I plan to end with a brief but stirring summary of my own research efforts, I wish quickly to mention the names of others who, at some point during my tenure with the section, were briefly associated with it. The roster of "guest workers," in addition to including Merton Gill, embraced two additional psychiatrists: Jon Meyer, and Stanley Greenspan. Five psychologists who worked for relatively brief periods of time with us included: Boris Iflund, the creator of the Iflund Personality Test, Dan Berlyne, Sandy Unger, who conducted some

early research on LSD, William Stephenson, the developer of the Q-Sort Technique, which I adapted for my ward-milieu studies, and Marvin Waldman, whose research efforts, I believe, never quite got underway. Now, if we can have a flourish of trumpets, I'll give you a summary of six primary research areas in which I conducted most of my work. They range over the following categories: 1. Creativity: specific studies included (a) identifying personality characteristics of creative young scientists; and (b) attempting to enhance creative performance of individuals by providing personal psychoanalysis, and more commonly the use of specific behavioral and cognitive techniques, e.g., relaxation, problem-solving training, "suspending critical judgment," etc. 2. Psychotherapy research: studying the impact of quality of patient-therapist relationships on outcome; distinguishing the roles of specific and common factors in effective psychotherapy; the role of therapist characteristics in treatment processes and outcome; therapists' ability to recognize and respond to incongruent facial and bodily cues; the impact of psychotherapy research on health policy; and the comparative efficacy of specific psychological treatments of major depressions (the NIMH Treatment of Depression Collaborative Program). 3. Study of a complete course of psychoanalysis: I have earlier touched on this work in discussion of the work of Dittman. Actually, we were only two of the five-member team headed by Dave Shakow, who had initiated and directed what came to be known, affectionately, as Shakow's Folly – recording on film a complete course of psychoanalysis for concurrent and retrospective study. The other investigators were Bob Cohen and Mabel Cohen. The analyst was the eminent psychologist and lay analyst Paul

Bergman. Ingrid, since I have already prepared a detailed description of that project in a document I earlier wrote for Sam Greenhouse, I will save time and breath by simply giving you a copy of that summary. 4. Group Psychotherapy: a range of studies of its therapeutic dynamics and mechanisms. 5. Ward Milieu Studies: therapeutic and nontherapeutic impact of ward milieu on patients and ward staffs at the NIMH and Chestnut Lodge. 6. Family Therapy: issues regarding the uses and abuses of small-group dynamics. Ingrid, while I hope that this summary will give you an overview of the range of research interests and efforts made by members of the Section on Personality, I expect that in the course of our discussion I shall wish to add and detail some of my recollections. I now want to say something about the research philosophy that I attempted to follow and promote. The philosophy is one that was so beautifully stated by James A. Shannon, the Director of NIH, in his welcoming chat with Lou Sokoloff and myself. Shannon indicated that he hoped that we, as well as all other NIH researchers and administrators, would choose to follow it. He told us of his firm belief that research flourished best in an environment where freedom of thought was cherished and promoted. It was his goal to provide such an environment. Perhaps I can best convey his views by quoting a brief excerpt from the later published version of an address he delivered at Harvard University, July 21, 1959: "There must be room in American science for more environments which foster creativity by extending a protective cloak over the few with real imagination, some of whom, hopefully, will become creative." That is a fine creed and it was certainly honored by John Eberhart and Hazel Rea. After I had settled into my

spacious office at the Clinical Center I received a visit from John Eberhart, who, as I recall, then held the lofty title at NIMH of Director of the Mental Health Intramural Research Program. John and I had known each other from our earlier contact when I was working at Hopkins. He was then the Public Health Service Project Officer who oversaw Jerry Frank's group psychotherapy research grant. I do not believe that our previous association explains why he chose to come to my office rather than inviting me up to his. I believe it simply reflected his gracious manner of dealing with any staff member. He then delivered what I took to be his typical welcoming speech. It went something like this: "I am a research administrator here. My job is to make sure that as a researcher at NIMH you are provided with whatever services, equipment and assistance you may need in conducting your studies. So, in addition to welcoming you here, my purpose is simply to ask, 'How can I help you?'" I had never heard that kind of talk from any administrator before. I thought to myself, "This guy can't be for real. Unbelievable." But John sincerely meant it. And so did Hazel Rea, who was then a young administrator. As you know, she is about 92 now but has only recently agreed to accept full retirement. Over the years she has risen to high-level administrative positions. In my work with her I found that she always tried to implement the expediting administrative role that John had expressed. Her particular genius appeared to be knowing how to get around what we viewed as obstructionist government regulations. Initially the federal procedures available to the NIMH were those devised for purposes other than research and did not suit the needs of many of us. As time went on many administrative rules were developed

that were more congenial to our needs. However, pending their implementation, Hazel and I would spend a great deal of our time together scheming how to get around existing regulations. During those early years when NIMH budgets swelled regularly, the life of an administrator was far easier than during the lean times that followed, when budgets were routinely slashed. In any event, investigators, particularly in the lush days of rapid hiring, were administratively encouraged to be guided by their emerging research evidence and their own changing interests. Moreover, when research interests changed investigators were quite free to seek other sponsorship within the NIMH more consistent with their new interests. Thus, Caron was able freely to move from our Section to another. Interdisciplinary research was particularly strongly encouraged. However, some early problems arose between psychiatrists who designed and conducted their research on wards of the Adult Psychiatry Branch, and clinical psychologist-researchers who were members of the Laboratory of Psychology. Consistent with psychiatrists' previous experience in clinical settings, they had come to assume that clinical psychologists should be available to them for purposes of performing such services as patient testing, statistical analyses, etc., without expecting to be viewed as study collaborators. Psychologists in the Laboratory of Psychology, however, properly viewed themselves as independent investigators. They were, of course, prepared to collaborate on projects whose purposes they endorsed and whose designs, ideally, they had helped to develop. In addition, clinical psychologists, such as myself, had been quite willing to respond to requests of any ward administrator in the NIH for patient psychological testing services

aimed at enhancing clinical services. However, soon after Shakow arrived, members of the Laboratory were no longer available to provide pure and simple psychological testing services. Shakow wished to protect all members of his staff from any such distractions from their primary research objectives.

Occasionally, administrative efforts were made to stimulate collaborative research. I recall one such scheme designated several tables in the Clinical Center Cafeteria as “stammtisch,” where researchers from organizations within the NIMH and the NIH could more readily meet for lunch and meet investigators outside of their own bailiwick. There they could learn about ongoing or proposed research that hopefully might pique their interest or possibly have direct implications for their own work. This plan may have had some positive effects but it was finally abandoned.

Farreras: Yes, if you're small enough, you can do that.

Parloff: Well, unfortunately most of our people who ate in the cafeteria seemed to have the attitude, “Go away, I've got enough friends.” Speaking of small enough groups, the first NIMH staff meeting I ever attended – in 1953 – was able to include all members of both the Intramural and the Extramural divisions. We met together in the conference room at Stone House. You see, back then we could all fit into a single room. During that meeting I was briefly introduced to the entire organization and then the business meeting proceeded. It was then that I was able to observe John Eberhart in his directorial role. It was very reassuring. I learned something more about his values as well as his administrative philosophy. A young administrator in the Extramural division arose and hesitantly asked if John

was REALLY going to require that all NIMH grantees sign loyalty oaths. In response, John grimaced dismissively, and then directing himself to his entire audience he said, “Come on, doesn’t anyone have any serious questions?” Much relieved laughter followed.

Farreras: Can you tell me more about Boris Iflund?

Parloff: Boris Iflund, yes. You know about him? Boris was a very elegant man. He may not have completed much publishable research during his stay but he certainly brought class to the Section.

Farreras: I know of him. He’s deceased now. But I don’t know when he arrived or left nor what he did.

Parloff: I’m afraid I don’t recall the date he arrived, or for that matter, even who hired him.

Mrs. Parloff: I have something on that.

Parloff: Boris must have come rather early on because he was also from...guess where?

Farreras: Berkeley?

Parloff: Exactly. That whole thing about Berkeley really intrigued me. Maybe that’s why I later took my sabbatical¹ at Berkeley, which happened during the time of the great campus upheaval led by Mario Savio. Boris managed to administer his test to a wide range of subject populations and planned to do a cross-cultural comparison of responses to his projective personality test. My best recollection of Boris is that he was a superior cook and host. He had mastered the fine art of boning a

turkey in such a fashion that he could then serve it in its restored whole state. His great talents as host were matched only by his graciousness as a guest.

Mrs. Parloff: This [reprint] says 1958, and he was [listed as] already working in France.

Farreras: Oh, so he had left by '58 then...

Parloff: Yes. He later died while teaching at Pace College. Incidentally, since I may not have the opportunity to get back to it, let me digress a moment to tell you that while I was on sabbatical at Berkeley (1964-65) I was able to study Mario Savio closely, ostensibly because he was a subject in my ongoing creativity study. Mario had been a winner of an earlier Science Talent Search contest but unexpectedly emerged during my Berkeley visit as the leader of what the press labeled "The Free Speech Movement." I was officially a visiting Professor at the Psychology Department. I had been invited there by my old friend and former NIMH colleague, Shelly Korchin, at that time the department chairman. In addition, I concurrently worked at the Institute of Personality Assessment Research (IPAR). That Institute has long conducted studies on adult creativity. I was especially delighted when Donald MacKinnon also invited me to join his IPAR staff to work with such researchers as Frank Barron, Harrison Gough, Regina Helson, Wallace Hall, and others. I should explain that the notion of awarding the Laboratory of Psychology staff members sabbaticals – full 12-month sabbaticals – was one that Shakow had introduced into the Laboratory in keeping with his effort to establish and maintain an academic tradition. I was even able to

¹ I know sabbaticals were adopted by other Laboratories within the NIMH but I am not sure about other institutes. However, it does seem unlikely that once Shakow initiated them at the NIMH that the staffs of other institutes would not also have demanded them (e-mail communication, 5/17/2002).

bring along one of my regular Research Assistants, Marianne Larson Kleman. She was a tremendous help to me. Unfortunately, she met a young man whom she married while we were out there and decided, for reasons that continue to escape me, to remain in San Francisco with her husband rather than to return with me to NIMH. My sabbatical period was a truly extraordinary time. I was able to view the Berkeley uprising from my vantage point as a faculty member and also from the eyes of Mario Savio, with whom I developed a warm relationship. Gloria and I still remain great friends with Mario's ex-wife, Suzanne Goldberg, who lives here in town. She subsequently married a guy who in some ways is very much like Mario. He's a physician, Sidney Wolfe, a longtime colleague of Ralph Nader's and Director of Public Citizen's Health Research Group. He is also a recipient of a MacArthur Genius Award and is frequently invited to appear as a medical consultant on television programs, where he represents the liberal point of view. Now, to return to Iflund - and you thought I had forgotten - as I was saying, perhaps his greatest achievement during his stay at the NIMH was to perfect his modestly titled test, The Iflund Personality Test, subtitled, the "picture recall test."

Mrs. Parloff: He died of an accident in his apartment. A heater malfunctioned, and he died of carbon monoxide asphyxiation. This is by way of a reminder about Norman Goldstein. Morrie, did he play any role that you want to mention?

Parloff: Oh, yes, Norm Goldstein. He was a psychiatrist in the Adult Psychiatry Branch, and he and I worked together on a number of studies. We were interested in testing whether psychotherapists may regularly, albeit unintentionally,

communicate some of their own values to their patients. This issue was of some interest since the debate then continued in psychoanalytic circles whether therapists, who thought they were simply presenting themselves as “blank screens,” might, nonetheless, unwittingly convey their “unexpressed” attitudes to their patients. Our research provided evidence that patients rather regularly recognized the therapists’ presumably unexpressed values regarding such treatment matters as what they considered to be theoretically pertinent content. Prior to my undertaking this study I had, with the encouragement of David Shakow, applied and been admitted to the Washington Psychoanalytic Institute for training. This you, as a taxpayer, will be happy to know was all paid for by the Government. So I completed the whole four-year training shtick (classes were held each Saturday morning from 8:00 to 12:00), and in the process I graduated as the first nonmedical candidate to be admitted and graduated as a “research psychoanalyst.” Informed by my analytic background Goldstein and I engaged in several psychotherapy process studies. In one instance, unbeknownst to Norm, I tested my hypothesis that the two institutionalized paranoid schizophrenic patients whom Norm was treating via psychotherapy – we were then in the pre-routine-medication era – would soon recognize and respond to Norm’s theoretically based preferences for particular patient topics. Norm, of course, remained oblivious of the fact that he routinely rewarded his patients selectively – by a special show of interest – for return to topics which Norm believed had particular “dynamic” interest. While the patients learned to speak more often about those issues that they had correctly inferred were of greater interest to the therapist, this did not

help them avoid such topics entirely. However, I noted that often, when a patient ventured to return to a topic that he had correctly inferred was unwelcome to the therapist, then the patient would speak with an intensity and degree of rage which puzzled the therapist. However, when the research evidence permitted the identification of a disparity between the patient's and therapist's interests, I was able better to predict the points in therapy where our patients would suddenly become enraged. This research finding was then fed back to the therapist, who became better able to respond more usefully to the patients. Goldstein later left the NIMH to work at the Mayo Clinic. Oh, I just remembered a cute study that Marge Klein, who had initially been my Research Assistant and later returned to work with me as a postdoctoral fellow, collaborated on with Dittman, Merton Gill and myself. It deals with our attempt, around 1968-69, to learn about behavior therapy as performed by Wolpe and Lazarus. They were then still working together and talking together. I arranged with both Wolpe and Lazarus for them to invite Dittman, Klein, and myself to spend a week chatting with them and observing their work with patients. We hoped to learn firsthand how they actually translated and applied their theories in daily practice. Their form of therapy appeared to be a startling departure from the psycho-dynamically oriented therapies we had been familiar with. I had by then read Wolpe's book and was surprised by its flat statement that Wolpe's seemingly simple-minded theory and non-dynamic techniques were routinely effective in producing rapid and enduring "behavioral" change. He claimed to have achieved a phenomenal success rate – I believe around 83 percent. No other form of psychological treatment –

psychotherapy or psychoanalysis – had ever had the temerity to make such a claim. The memories of Eysenck’s assault on our field were still fresh. I thought we owed it to ourselves to take a look. Wolpe, to his great credit, promptly welcomed our visit. He was a most gracious host.

Farreras: Where was he at the time?

Parloff: He was in Philadelphia, at the Eastern Pennsylvania Psychiatric Institute.

Lazarus, of course, was there, too, but clearly he then held a second-banana role to Wolpe. At some point during that week I persuaded Merton Gill to join us and give us the benefit of his psychoanalytic insights, and he sat in on our meetings with our hosts. Gill was quite brutal in his inquisition. He seemed particularly unhappy with Wolpe when he learned that Wolpe had been through some psychoanalytic training but rejected the preoccupation with covert unconscious motivations in favor of his own emphasis on the role of cognitive awareness. Gill was, however, particularly helpful in getting Wolpe and Lazarus to clarify their thinking and theory for us. During the week I spent there I came to especially appreciate Lazarus’s approach, which seemed much less dogmatic than that of Wolpe. I decided I would like more time to immerse myself in his approach. To skip ahead, I was later able to arrange to commute weekly to work with Lazarus at Eastern Pennsylvania Psychiatric Institute during the period he was drafting his book *The Basic Id*. After our small group of explorers into the realm of behaviorists had returned, we prepared our reports for presentation at one of our regular weekly Laboratory meetings. As I recall, Klein, Dittman and I gave our measured but clear endorsement of the notion that behavior therapy should be

further studied by the NIMH. Merton, on the other hand, gave his negative minority report. During the lively discussion by members of the Psychology Lab and its many other visiting researchers, Shakow arose and delivered his solemn pronouncement. He was quite prepared to accept the notion that the procedures of behavior therapy, like other such interventions that depended on the mechanism of suggestion, could produce temporary beneficial effects. Behavior therapy, as we had described it, however, involved a much more ominous component. It was manipulative and, therefore, humiliated and dehumanized the patient. It was, in short, fundamentally immoral. The mere fact that it seemed to work was completely overshadowed by the simple truth that it demeaned the patient. That was, of course, the popular reaction of psychoanalysts at the time. It is important that I also point out, however, that subsequently Shakow fully endorsed my weekly visits to continue studying with Lazarus for almost a year, and when Lazarus and Wolpe had a falling out and Lazarus needed a job, Dave approved of my offering him a position in my Section. Dave also agreed with my decision that prior to publishing our report we would directly incorporate into our article all clarification and rebuttal arguments that Wolpe and Lazarus had prepared for the article. The final paper thus became very important and credible. To ensure my own objectivity, however, I insisted that Marge Klein be the lead author, with the authority to determine what the article would finally report.

Farreras: You earlier mentioned a Sandy Unger.

Parloff: Yes. As I recall, his major interest became the study of the LSD experience. I'm afraid he got swept up not only in the work but by his own excessive use of the

drug. While at NIMH Sandy began to work closely with Charlie Savage, who was investigating LSD as a possible means for better understanding the bases and nature of thought processes of schizophrenics.

Farreras: LSD was very popular in the '50s. Where was Dr. Savage?

Parloff: Charlie was one of the ward administrators in the Adult Psychiatry Branch.

Mrs. Parloff: Wasn't there some thought that learning something about the reaction to LSD would provide some insight into schizophrenia?

Parloff: Absolutely, absolutely. You see, by that time, Axelrod had done his early work on serotonin, and it appeared that it might further our understanding of schizophrenic thinking. Many of the staff began experimenting with taking LSD themselves. Frankly, the Branch had accumulated some very extraordinary people and they – or shall I say we – didn't require the help of LSD. Among the strangest were our early ward administrators. Charlie was one of them.

That reminds me that one of my most interesting avenues of work – the study of the effects of different forms of ward milieu. Originally I intended to study the wards' impact on the patients housed on them, but quickly it became apparent that the milieu had an even more obvious and powerful impact on the ward nurses.

This was due to the fact that the nurses, who had been hired by the independent Central Nursing Department for their demonstrated mastery of basic psychiatric nursing skills, now found themselves having to work for psychiatrist ward administrators who required that they accept entirely new and sometimes alien nursing philosophies. In short, these nurses were required to meet dual loyalties. They were to be loyal to the nursing standards set by the Central Nursing

Department and simultaneously be loyal to the idiosyncratic nursing requirements demanded by their ward administrators. While the nurses continued to be supervised by nurses representing the central nursing office, they were also “supervised” by the psychiatrist ward administrator. The psychiatrist was responsible for establishing and maintaining a defined experimental ward milieu, in the hope of testing its hypothesized therapeutic efficacy. Each ward administrator had been hired by Bob Cohen on the basis of his putative creativity, as most clearly evidenced by his unorthodox ideas. The ward nurses were thus exposed to an inherently conflict-provoking situation. They found themselves having to adapt to the unique and sometimes bizarre requirements imposed by such psychiatrist ward administrators as Lou Cholden, Charlie Savage, Jordan Scher, and Murray Bowen. Each of these ward researcher administrators was quite capable of driving even the most stolid and sober nurse – how shall I say it – nuts. When I first recognized this, I devised a set of observational studies focusing particularly on Cholden’s and Bowen’s wards.

Farreras: Was this around Gordon Paul’s time, his behavior modification work with schizophrenics?

Parloff: Yes, it was. Gordon’s work, however, aimed at helping institutionalized schizophrenic patients to learn more adaptive behavior. My research was modeled after ward milieu research that had been conducted by Morrie Schwartz and Al Stanton much earlier at Chestnut Lodge. They had found that when significant conflict arose between the patient’s therapist and his ward administrator, the patient would frequently become quite agitated and disturbed without, apparently,

being able to explain to anyone what had upset him or her. There certainly appeared to be similar interstaff conflicts on the NIMH wards. As a matter of fact, I also did some parallel studies regarding nurses' attitudes and behaviors concurrently at Chestnut Lodge. The nurses on the NIMH wards came to know and trust me. As a consequence, when the stress levels between them and the ward administrators became acute, I would often be invited to serve as moderator at their ward meetings.

Let me give you a couple of illustrations of the sorts of problems that periodically arose on Lou Cholden's ward. He wanted to establish on his ward a unique but extreme version of what sounds like a simple, straightforward philosophy. He required that his nurses give primacy to the needs of their patients rather than to their own needs. The problem, however, lay particularly in his eccentric interpretation of the "needs" of his patients. For example, Letty was one of the most regressed, chronic schizophrenic patients of the entire group of such patients who had only recently been transferred from a St. Elizabeths back ward to Lou's new research ward. One of Letty's first responses to her new environment was to set fire to the expensive new drapes in the common room. Some of the nurses promptly put out the fire. When this was reported to Lou, he called a meeting of the ward nurses and demanded to know whose needs they thought had been served by putting out the fire. Since no one gave what Lou believed was the correct answer, he supplied it, saying that clearly Letty's wish had been to destroy the drapes and thus her need had been frustrated. It was the needs of the nurses that had been better served instead. A few days later he discovered one of his

nurses interfering with the attempts of a patient to etch her initials onto a prominent surface of the new ward piano. Cholden once again expressed keen disappointment that his nurses continued to appear to be more concerned with the well being of inanimate objects than with meeting the needs of their patients. I'll give only one more example of the conflict between Lou and his nurses. A nurse who had been unable, for about ten days, to persuade an increasingly reeking patient to resume bathing voluntarily, suddenly gave in to her caring impulses – and to the pleas of the increasingly discomfited ward members – and bathed the patient herself. She had clearly flouted the Cholden dictum. However, this time, at the inevitable chiding meeting that Cholden convened, the ward nurses announced they would no longer take any nursing action on his ward other than those they could clearly recognize as necessary for the protection of a patient's physical health. And then they ceased performing their so-called research services. This work-action was quickly resolved by high-level negotiations which succeeded in restoring most of the prerogatives of these expert nurses. Lou concluded that the institution had not evolved in its thinking to the point where it could properly support his innovative research. Tragically, Lou soon afterward died of injuries suffered in an automobile accident.

I hope I have given you some picture of why I became interested in conducting ward milieu studies. Perhaps we have time for just one anecdote about Murray and his nurses.

Farreras: Of course.

Parloff: What Murray became best known for was his family therapy work. At the time he was at NIMH this interest was just beginning, but already he had initiated an extraordinary study which involved studying families of institutionalized patients by actually admitting to his ward not only the primary patient but all major family members and studying the family unit's ongoing interactions for extended periods. What I want to tell you about, however, is the incident that I believe was critical to his decision to leave the NIMH. One of his young female patients, loosely diagnosed as a borderline personality, had the presenting symptom of periodically lightly slashing her upper arms. She continued this on the ward with good effect. These slashing episodes greatly upset all of the nurses on duty at the time. As a result the young woman received much special attention. Over a period of time, however, Bowen was successful in simultaneously lowering the nurses' anxiety and reducing the incidence of the patient's slashing behavior. Then the patient hit upon a different form of self-mutilation – she would insert a bobby pin into the large vein in one of her arms. That never failed to trigger a deeply concerned medical response from the nurses, who came running, hovered about her bed, and urgently dispatched someone to call Dr. Bowen at his office or home to come and excavate the foreign object. In my frequent conversations with Murray he frankly admitted that this new routine had become annoying because he hated being so obviously and effectively manipulated. Ordinarily his manner was composed, unperturbed, and controlled. He was cool. He was proud of this persona and was offended by those rare circumstances which pushed him out of that role. He proudly confided, "I can outwait anyone." As he told me the story,

one Friday evening he was having dinner at a friend's house. Near the end of the meal his host was summoned to the phone and returned to tell Murray that he had an urgent call from his ward. Murray took the call and learned that the patient had once again inserted a bobby pin in her arm, but this time it had completely disappeared under the skin. The nurse on duty asked him to please come immediately as she was unable to remove it. The extraction might even require a small amount of surgery. Murray assured the nurse he would be along shortly, but first he wished to finish dinner. He returned to the party and found, to his pleasant surprise, that the initial anxiety had completely left him. He decided he could wait a while. As the hours passed, he became even more pleased with the calmness with which he was handling this situation. He was quite able to resist her manipulation. Finally, he went home and with but few qualms went to bed. He reported all this to me with obvious satisfaction. He had demonstrated to himself and to others that he really could do it. He could outwait the nagging discomfort and the several increasingly agitated calls he had continued to receive. In the morning he had a relaxed breakfast, read the paper, and then finally presented himself at his hospital ward where his patient was housed. He found that the nurses had finally spoken fully with the institution's "on call" physician. That physician did not grasp nor share Murray's satisfaction with his personal victory over his anxiety. Indeed, when the patient ultimately developed a slight case of cellulitis, the hospital medical board brought charges against Murray for patient neglect. But Murray calmly concluded his benign recital of this untoward incident with an observation very like the one Lou Cholden had given me earlier,

“So, Morris, I’m very much afraid that this place is not now able to provide me with the environment I require either to treat my patients or to conduct the research I had hoped to do.”

Farreras: Wow.

Parloff: I have another story about Bowen that might be of interest. One Friday afternoon while Murray and I were amiably chatting in his office we were interrupted by one of his long-term patients, who apologized and explained that she needed Dr. Bowen to deal with a matter of some urgency for her. She was about to go off on a week-end pass and needed him to sign a form authorizing the ward nurses to supply her with a sufficient number of her prescribed pills to last her the weekend. Murray promptly signed the document but then the patient volunteered the information that she was so depressed she would like to commit suicide during the weekend. Murray expressed the routine curiosity about how she planned to suicide. She replied that she had been saving up some of her prescribed anti-depression pills but was afraid she might not have quite enough to do the job properly. Murray, consistent with his unflappable manner, did not appear visibly dismayed by the unexpected turn the conversation had taken. Instead he simply asked, “How many more pills do you think you would need properly to kill yourself?” The patient pondered for a moment and came up with a figure. To the apparent surprise of the patient and certainly to my own amazement, he took a bottle of pills from his desk drawer and slowly counted out the required number of pills, one by one, into the palm of her hand. As she dumped the fist full of tablets into her purse, Murray placed his hands on her shoulders and gently but

gravely spoke to her, "I'm sorry that at this moment you feel so bad that you are thinking about ending your life. I want you to understand that the reason I have given you the pills you asked for is *not* because I think your plan to do away with yourself is a good one. I certainly don't. I think it's a very bad idea. The reason I have given you the pills is simply to show you how much confidence I have in your strength and ability to decide to live. Only by living can we both have the opportunity to help you work through your problems and depression. Have a good weekend and I'll see you Monday." With that, he returned to his chair and resumed his conversation with me. The patient let herself out of the office. Murray later told me that on the following Monday the patient presented herself unharmed, and smilingly returned the excess pills to him.

Farreras: Yikes, that was awfully risky.

Parloff: I must add, subsequently a young psychiatrist on another of the three NIMH wards, inspired perhaps by Bowen's intrepid behavior, similarly chose to allow a very depressed patient to go off on week-end pass. Unfortunately, unlike Murray's patient, she successfully suicided. The therapist, was of course, much censured by his colleagues and superiors for his patently poor judgment. I believe he and his superiors were also sued by some members of the patient's distraught family.

Farreras: And what ever became of Unger? I have a listing for him at Springfield State Hospital in Sykesville, Maryland, but I don't know whether that was where he later worked.

Parloff: No, he worked there while he was still with our Section.

Farreras: So that was a collaboration?

Mrs. Parloff: Yes. He did become a national expert on LSD. We published one of his articles in the journal Psychiatry.

Parloff: I'm afraid I thought his preoccupation with LSD had become a bit extreme but even more important, his research behavior had also become erratic and extreme. Finally, we encouraged him to find a position elsewhere.

Farreras: I think Don Blough mentioned that he did the same type of work. He was doing it with animal subjects and Ben Carlson was doing it with human subjects. And there were a few others – Maitland Baldwin and John Lilly – who were also doing sensory deprivation work.

Parloff: Yes, indeed. Lilly had done some of the early work on sensory deprivation. Lilly was one of the more interesting characters who ever graced the NIMH.

Farreras: What became of these scientists and all of the LSD research that was done in the 1950s and 1960s?

Parloff: For the most part LSD research became unfashionable if not downright disreputable after the hippies began abusing it. More importantly, however, the research effort dwindled in response to the finding that the research pay-off was meager.

My impression is that Charlie Savage continued for a number of years longer at NIMH and had many other interests. Sandy Unger, however, had become seriously impaired by its frequent use, and was ultimately encouraged to leave the NIMH. I believe he did a stint at Spring Grove, ostensibly as a researcher, but finally disappeared from my radar screen. I recall Lilly as a leader in the early

work on “sensory deprivation.” The NIMH built him a water tank for his submersion experiments. Later he achieved great fame for his efforts to communicate with dolphins. It is my impression that he had left the NIMH long before the LSD craze took hold.

Farreras: And the only other person I have for your section is Margaret Thaler Singer, who’s now at Berkeley.

Parloff: Margaret was a wonderful clinician, and we did some consultation together, but I do not believe she was ever a member of our section. She worked most closely with Lyman Wynne on some of his studies in the Adult Psychiatry Branch. She also worked at Walter Reed.

Farreras: Was she the one who could read Rorschach tests really well?

Parloff: Oh, she was a witch. She had a fantastic ability to interpret projective tests.

Farreras: That’s exactly what Mirsky called her!

Parloff: She was known as the psychologist witch. What she would do with the Rorschach was unbelievable. I would have loved to have her in my section but I’m afraid she wasn’t.

Mrs. Parloff: She gave Lyman tremendous support for the studies that he was doing.

Parloff: Yes. So those were some of the main things that we were doing. Let me see if I’ve missed something here. Just to summarize the research areas of the section, the primary areas were: creativity, psychotherapy, communication processes, psycholinguistics, kinesics; intellectual development; personality development; and personality change via psychoanalysis. I think those were the main things we did during my stay in the Intramural research program.

Farreras: Excellent; that's a very helpful summary. Let me back up a little bit. These people you've just mentioned and the type of research they were doing, did you oversee their coming to NIMH?

Parloff: Good point.

Farreras: In the sense of your saying, to Shakow or to Cohen, "There's a really good person who does research on X. We should bring him in."

Parloff: Yes, most of the time. There were many whom I recruited and hired, but as I described, there were some, like Boomer, Schaefer, and Iflund, who were simply transferred into the section for – as we used to say in the army – administration, rations and quarters. But as I have stressed, in some cases, like those of Caron and Unger, it didn't make much difference what I had originally hired them to do because ultimately they did the sort of research they wanted to do.

Farreras: Once they got here.

Parloff: And, of course, each of us would freely collaborate with others whose work we found of mutual interest – for example, the collaborative studies on kinesics, social psychology, cognition, etc. And that was consistent with the basic philosophy of the organization. Have you had a chance to read the Report of the Research Task Force of the National Institute of Mental Health: Research in the Service of Mental Health 1975? It summarizes the past work of the NIMH and offers a framework for future research. It may answer some of your questions.

Farreras: Yes. This is probably one of the few printed sources which I found helpful for my proposal on this Lab's history.

Parloff: Let me cite what Kety wrote in it about guidelines: “Our guidelines were excellence, which can be judged, rather than relevance, which can only be guessed at” (p.10). And that was both our mandate and the philosophy that guided us. We picked people who we thought met the standards of excellence, and then they would be encouraged to follow the research evidence as it emerged.

Farreras: So there was never any type of pressure or coercion to work on certain projects?

Parloff: In our Laboratory organization there were well-differentiated Sections, each designating particular specialization areas: Personality, Animal Behavior, Perception and Learning, Aging, Child Development, and a miscellaneous section called Section of the Chief. Within those areas an individual might be encouraged to contribute to some ongoing major studies and also to develop studies of his or her own. In the case of my research on creativity I frequently recruited investigators who were already working in that broad area. Caron and Datta are examples of such recruiting. However, as I have indicated, Caron’s interests shifted considerably over time. As time went on and budgets became smaller, the opportunity to recruit investigators who were doing novel and important research of their own also became smaller.

Farreras: What about Shakow’s personal interests? Were there higher-up pressures on Shakow for his people to work on certain things?

Parloff: If there were special pressures from above on Shakow we were never told about them. However, periodically Bob Felix would come to the Clinical Center and address the entire NIMH staff, at least those who assembled in the Masur Auditorium. One of the standard parts of those speeches was a statement,

something along these lines, “As director of the NIMH, I quickly learned that the director cannot direct. However, it seems very odd to me that no one here is working on a problem that we in the Extramural are all very interested in.” Then he would identify some issue that he thought any right-thinking researcher should be eager to address. As far as I know, no one in the Intramural ever took up such an invitation and no explicit pressure on any individual was ever exerted.

With regard to Shakow’s pressuring any of us, that’s more difficult to answer. He did, of course, supervise each of the Section Chiefs and in the process felt free to make a range of suggestions for our consideration. Those, however, were suggestions, not directives. It is my impression that once he had created the various Sections that constituted the Laboratory and had selected the Section Chiefs, he trusted us to follow our interests and to be guided by our own detailed understanding of the directions and new evidence in our fields. Since my Section may have been particularly close to his own area of broad interests, I expect that he felt a bit freer to make suggestions to me. I doubt that he felt as free with Rosvold or Calhoun, for example. In any event, he did not ever crudely seek to impose his own ideas on me. I do recall, however, that without his active “encouragement” I might not have gone quite so blithely into the field of creativity.

It’s a funny business about exerting pressure. Later, when I moved over to the Extramural and I was charged more directly with shaping the direction of research in the field of psychotherapy, I found that direct pressure was not the way to go. Cajolery and holding out the prospect of the availability of large research grants

worked much better. In effect, I learned to operate in accordance with the government's version of the Golden Rule, which, as you know, reads, "He who has the gold makes the rules." But humor and joshing, used appropriately, worked well. You will note that my papers, particularly those I wrote while in the Extramural Program, were generally light and amusing but contained some serious points for the field's attention.

However, Felix's vain efforts to persuade members of the Intramural staff may be due in large part to the orientation we had received from Shannon, which had repeatedly been reinforced by Eberhart. We were, clearly, a specially anointed and autonomous group of researchers. There would be neither special reward nor penalty for refusing to drop what we had been doing in order to begin work on something that was clearly of greater interest to Felix than to any of us.

However, by 1971-72 things changed for the NIMH Intramural Program. You will find an account of that in Bob Cohen's Annual Report of the Laboratory. There's a sea change – the decision to accept the need to make a transition that would begin to deemphasize the prevailing psychodynamic research emphasis and to increase the budget for biological research. That was a horrendous wrench in Bob's thinking. NIMH's decision to give up its early primary emphasis in psychosocial research on the psychodynamic orientation in favor of the more promising biochemical-genetic orientation and emphasis followed the widespread recognition in mental health research that the psychodynamic and sociological "soft sciences" had not significantly advanced knowledge – certainly not at the pace demonstrated by the "hard" sciences. Given the fact of inevitably

finite research budgets it was necessary to prioritize the areas in which research could be supported. As a consequence, research programs in the intramural, extramural and academic research programs shifted their emphases to the more promising “harder” sciences. This shift was more keenly felt, perhaps, in the intramural NIMH Adult Psychiatry Branch than in the extramural, where the mission (with a few notable exceptions) remained primarily that of promoting, evaluating and funding non-NIMH, investigator-initiated research grants, fellowships, etc.

Farreras: Who was Director at the time of this shift in orientation?

Parloff: Bert Brown. In terms of the budget, reality had already impinged and there were great budget cuts. The early days of ever-increasing budgets had ended. We wistfully recalled the days when the [first] secretary of Health, Education and Welfare [4/1953-7/1955], Oveta Culp Hobby, had had the temerity to submit a budget to Congress in which she had proposed cutting the NIMH, and Congress rose up and censured her for even contemplating such an unseemly act.

Farreras: Cutting all of the NIMH Intramural Program?

Parloff: All of the NIMH. And they censured her. You don't do that. Well, one did when the government's priorities changed. The political philosophy had changed. Maybe not the mission but certainly the availability of money for research changed during that period. Overall budget cuts – apart from research area priorities – in the NIMH were almost invariably in response to the policies of the political party in power. Republicans tend to cut “big government” and taxes while Democrats seek either to maintain or expand such budget expenditures and

incomes. Intervening wars, catastrophes and other such emergencies periodically drain money from all federal budgets. This pattern simply repeats itself within these parameters. In recent years the intrinsic value of research has increasingly been demonstrated and increasingly been accepted. The priorities that exist among research areas have, as I have suggested, periodically shifted largely in accordance with demonstrated and anticipated payoff.

Incidentally, when the Research Task Force of the NIMH was preparing its report, Mike Boomer, who served as an Associate Editor, took it upon himself to draft a new mission statement for the NIMH. Julie Segal, the report's Editor-in-Chief, called a meeting to which he had invited the entire Task Force, together with Louis Wienckowski, Director of the Extramural Research Program, and John Eberhart, Director of the Intramural Research Program. There, Mike slowly and sonorously intoned the proposed new NIMH mission statement in its entirety. It was a solemn moment. When Mike had concluded, Eberhart arose and broke the reverential silence with a eulogy that went something like this: "That was most elegant. It is thoughtful and beautiful." Then he continued, "That mission statement belongs right up there with the best of our former mission statements. I congratulate you. Like all such grand mission statements, however, it should not be distributed but kept in a safe. It is to be taken out of that box and read only on specified ceremonial occasions and then immediately returned to its vault for safekeeping. For the rest of the time the business of the institution is to proceed unencumbered but greatly inspired by ever-dimming memories of any of its specifics." Lou echoed these sentiments. I believe that ended the matter.

The only instance I recall where a serious question was raised about whether or not a particular piece of research conformed to the NIMH mission was the time a member of the Intramural board of review questioned the appropriateness of my own creativity study.

Farreras: You mean The Board of Scientific Counselors?

Parloff: Right. As I began to present that work on creativity to the Scientific Counselors, the chairman of the Board, himself a Nobel Laureate – I’ve forgotten his name – interrupted me and said, “Dr. Parloff, I may have misunderstood you. What did you say you were doing your research on? It sounded like you said creativity. Is that within the scope of the NIMH?”

Farreras: That’s encouraging!

Parloff: Right! But I must say, however, by the end of my presentation, he had concluded that the topic and the design of my study were just jim-dandy. He thought that the aim of identifying the factors that characterize creative scientists for purposes of facilitating such creative performance was clearly an appropriate mental health concern.

Farreras: When Boomer was writing this mission statement, what time period are we talking about?

Parloff: I believe that was around 1972. By that time the NIMH had had some 25 years of research experience and was looking forward to its next 25 years. The question posed was what should the NIMH plan to do next. I’ll mention just one of the lesser issues as an example. When we first arrived at NIMH there was some concern about the name of the Institute. Every other Institute at the NIH, be it

Cancer, Heart, Arthritis, Neurological Diseases and Blindness, or whatever, was named for the disease or diseases it was trying to ameliorate and cure. The name of our Institute, however, suggested that we were setting out to stamp out Mental Health. Shouldn't the Institute's name be "Mental Illness"? Then we went through that period where many thought that we should attempt to "optimize" the mental health of the nation. Obviously my rationale for conducting our creativity research appeared consistent with that popular goal. A serious question arose about what priority was to be assigned to the conventional goal of seeking the better treatment of psychiatric disorders and to the alternate goal of attempting to optimize the nation's mental health.

Farreras: My recollection is that during the early congressional proceedings Overholser complained about the name. I think it was supposed to be Neuropsychiatric Institute or something like that, and he disapproved of the NIMH designation.

Parloff: You know that history much better than I, but I think there was always the recognition that the NIMH should concern itself primarily with psychological disorders. There was still a lot of schizophrenia and other such psychoses "going around." Similarly, there were serious and chronic depressions, anxieties and the like that needed much more effective treatments. The NIMH would always have to give high priority to those health problems of concern to the public and, of course, to Congress. Nonetheless, during the '60s our thinking, which had previously been confined to an illness scale where the goal was to reduce dysfunction to a zero point, shifted to a consideration of helping the individual move beyond the zero to a broader scale that also included degrees of positive

mental health, growth, self-actualization, and other such popular ESTian (Erhard Seminars Training) notions. I confess I wrote some very fine papers advancing that point of view. Shakow may have latched onto the theme – and this may relate to your question about pressure – that I might consider exploring the area of creativity much as our colleague Morris Stein was doing at NYU. I did feel that Dave was really trying to seduce me into that somewhat alien field.

Mrs. Parloff: A big factor in the creativity study was that the Russians had beaten us into space.

Parloff: Oh, right, right! That was also a very important political factor. How could the Russians have gotten so far ahead of us? Clearly something was wrong. We needed to be more creative. Initially I was very hesitant about making this leap, but I did finally spend a number of years on it –with some profit – although not as much as I had hoped. I was never satisfied with my limited focus on personality of “the creative individual.” We were dealing with such a large sample that my research assistants managed regularly to find statistical support (low level) for almost any of our hypotheses. I’m afraid I ultimately lost my enthusiasm and wished to return to my primary interest, which was psychotherapy research. Ingrid, if you wish, I could now go off into a discussion of my Extramural period where I did that, but that would take us away from the Laboratory of Psychology.

Farreras: Well, this looks like a good place to stop. Maybe I should come back another time and discuss your Extramural years. Why don’t we just tighten up and summarize the early years you just covered?

Parloff: OK. Fine.

Farreras: You arrived in ’53. Do you remember what month?

Mrs. Parloff: I think you arrived October 1, 1953.

Farreras: All right, October '53. And you were here because Bob Cohen recruited you. Was he already Director of Clinical Investigations at the time? Were you supposed to work for him?

Parloff: I believe he was already the Director and yes, I was supposed to work with him.

Farreras: OK, because the Lab of Psychology didn't exist at that time.

Parloff: It didn't, so we psychologists – like Dick Bell, Earl Schaefer, Ben Carlson – were pretty much on our own. To give ourselves some necessary sense of structure we had to reinvent some of the usual academic administrative mechanisms that under ordinary circumstances we would have objected to. But since they were self-imposed they were O.K. We soon found ourselves organizing meetings, arranging for literature clubs, research discussion groups and all that sort of thing. We lacked any formal structure and had few if any delegated responsibilities from Bob Cohen's office.

Farreras: How did you get appointed Section Chief?

Parloff: For a very long time I was the Acting Section Chief before I was finally appointed the Chief, Section on Personality. I no longer recall how I was initially appointed Acting Chief. I can only assume that Bob Cohen asked me to serve in that role on behalf of that nascent section since Shakow had not even arrived. The section members who had gradually accumulated appeared content to have me continue to deal with the necessary administrative stuff. Over time the NIMH administration began to act as if I were in fact responsible for administering the various research activities of the Section members. Finally, it became necessary

for the administrators to formally delegate to me the title of Acting Chief and the authority consistent with the responsibilities I had. However, I was not appointed to the exalted position of Section Chief until 1958. After a four-year search, during which time we invited and carefully considered such eminences as Silvan Tomkins, Calvin Hall, and many other prominent personality theorists of the day, Shakow finally decided to place the scepter in my trembling hands. I like to believe that in the interim I had amply and persuasively demonstrated my suitability. Shakow apparently had reluctantly concluded that the messiah he had been awaiting was going to be a no-show. Many years later, at Dave's funeral, where Norman Garnezy and I each delivered a loving tribute, I took the opportunity to repeat Dave's typical summation speech that served to conclude many of the numerous candidate evaluation meetings. These were held following each applicant's presentation to the Laboratory. At the conclusion of our evaluation discussion Dave would say something along these lines: "This candidate is a great candidate. He is clearly a great scholar, theorist, writer, etc. His research is great. As a Chief of the Section on Personality, he would also be great." Then he would pause for emphasis and add the clincher, "Unfortunately, he is merely great. For this Laboratory we should look for someone who is more than just great. We need someone who is grand." As a matter of fact, Ingrid, David Shakow, as I came to know him, was, indeed, truly "Grand."

Farreras: How did Shakow get appointed? I heard that Kety wanted Roger Sperry.

Parloff: That might be. I never heard that story. I can only assume that Bob Cohen prevailed on this.

Farreras: So Bob Cohen wanted Shakow.

Parloff: I think so. Nothing in any of my conversations with Bob over the years suggested otherwise. I'm sure you can ask Bob when you interview him. I always assumed that one of the reasons he might be eager to get Shakow was that Dave shared with him a strong interest in promoting psychoanalysis.

Farreras: So it was in '58 that you became Section Chief?

Parloff: Yes, '58. Shakow wrote a memo to the Laboratory simply announcing my appointment. The fact is that while he and I may have gotten off to a rather shaky start, during our 19 years of working closely together we became very close friends and colleagues. Consider our beginning. Shortly after Shakow arrived I came and showed him a letter I had just received that invited me to spend a year at what was then called the "think tank" at Stanford, also known as The Center for Advanced Studies in the Behavioral Sciences. It was a great honor. I had hoped he would be properly impressed with me. Instead, he said, "I was never asked to write a letter of recommendation for you." Then he added that he did not wish me to leave so soon after his arrival. I was very disappointed.

Farreras: You needed his permission to accept the invitation?

Parloff: Oh, yes. He made it clear that I did.

Farreras: Oh, you did. I didn't realize that.

Parloff: Similarly, when he read the first paper I had ever submitted to him he responded with much less enthusiasm than I was accustomed to receiving. Again I was disappointed. I was convinced we were off and running in the wrong direction. But as I said, my 19 years with him were tremendous.

Farreras: That's great. Why don't we stop here; this seems like a positive note with which to end.

End of transcript