Parloff, Morris 2002 B

Dr. Morris Parloff Oral History 2002 B

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Morris Parloff (NIMH 1953-1983)

This is a second interview with Dr. Morris Parloff, former member of the Laboratory of Psychology of the NIMH Intramural Research Program, held on January 9th, 2002, in Bethesda, MD. Mrs. Gloria Parloff is also present to assist with the interview. The interviewer is Dr. Ingrid Farreras of the NIH History Office. *As revised June 17, 2002, by Morris Parloff.*

Parloff: Ingrid, with your permission, before we get into any new areas, I would like to offer some amendments and corrections to some statements I made during our previous interview. Fortunately, following our earlier meeting I was able to locate some old personnel files that included letters of recommendation I had written on behalf of some former members of my section. In some instances those letters contained facts I had quite forgotten.

Farreras: Sure. Maybe in the process you will also answer some questions that I've been wondering about.

Parloff: Good. I discovered, for example, that Jon Meyer, whom I had erroneously described as a "guest worker," had instead been a member of the Adult Psychiatry Branch. He worked there from October 1967 through June 1970. However, during that period we worked together so often and so closely that it was an easy mistake for me to make. Dr. Jerome Frank, who had initially recommended Jon to me for a research position, had described Dr. Meyer as one of his most outstanding residents. Jon was very likely appointed to the Adult Psychiatry Branch rather than the Laboratory of Psychology primarily because he was a psychiatrist. In any event, under my general supervision Jon and I conducted some research in the area of group psychotherapy. With regard to Marvin Waldman, I was able to confirm that he had been a member of the Section from September 1957 to about September 1958. I found a letter of recommendation I had written dated August 18, 1958, at his request, in support of his application for a new position. A paragraph in that letter to the Head, Department of Psychology, University of Massachusetts, helps me to better understand why I had such difficulty in describing to you any research Marvin had performed while he was with our Section. My letter states in part, "His duties here included: performing psycho-diagnostic testing, case consultation and some training of younger staff members." Marvin had, in fact, been hired simply to perform psychological service functions in order, thereby, to relieve our research psychologists from those chores. Clearly, Waldman's appointment was in response to Shakow's decision to protect his researchers' time by eliminating their earlier service obligations. Apparently Waldman had soon become discontented with the limited scope of his position. Incidentally, after his departure those who had been dependent on the Laboratory of Psychology for their clinical services managed to arrange for such services either by contract or by adding clinicians to their own staffs. In any event, Waldman's vacated position was never filled in our Section.

William Stephenson, about whom I spoke earlier, had, indeed, been a "guest worker." However, I believe he is more properly classed as a Visiting Scientist. He retained his position as Professor of Psychology at the University of Chicago and was probably on a sabbatical arrangement. He spent a considerable period with us –three months or longer. In view of his resolute efforts to promote the use by NIMH investigators of the Q-methodology he had earlier devised, I assume that the promotion of that new technology had been one of his primary purposes in arranging to spend time with us. Perhaps, too, his eagerness to be a member of the Section on Personality was encouraged by his awareness that I was favorably disposed toward that procedure. Dave Rosenthal and I -- Dave had been his student at the U of C -- had both used the technique in our respective doctoral dissertations. During Stephenson's stay he managed to supervise both Charlotte Schwartz (a member of the Socio-Environmental Laboratory) and myself in developing our "Ward Milieu Q-sort," which we employed in a variety of intensive "single-case" as well as "group" studies. William Stephenson, Seymour Perlin, and I collaborated in writing a chapter (1963) in D. Rosenthal (Ed.), *The Genain Quadruplets*. As I recall, Stephenson was an absolutely charming Englishman and a compelling speaker. However, his manner would quickly and sharply change if anyone had the bad manners and temerity to question any of his glib pronouncements. But like Boris Iflund, one of his most notable contributions was that he "brought class" to our Section. Another valued guest worker I wish now proudly to claim was Stan Greenspan.

Mrs. Parloff: Yes, he's a very famous psychiatrist now.

Parloff: He has become particularly famous for his psychoanalytically oriented work in child development.

Mrs. Parloff: At the time he joined the Section he had just come back from Israel, where he had done some work. I don't recall what he had done during his association with the Section on Personality.

Parloff: Yes, frankly, I don't recall what he worked on while he was in the Section, but certainly it was not anything experimental. My impression is that he took the opportunity to do a lot of library research in preparation for a book he was preparing. In any event when Bob Cohen asked me if I would like to renew the appointment I declined. Apparently I had come to the firm conclusion that this guy would never become a researcher --empirical or otherwise. O.K. So I was wrong-- one of my rare errors in judgment. Another important set of staff members that I didn't get around to mentioning before were the extraordinary Research Assistants I had the pleasure of working with. Once Shakow came on board, we were able to get our pick of the top graduate and postgraduate students from the very best psychology departments. Dave knew all the heads of psychology departments and they were pleased to bring to his attention their top-notch student-candidates. I shall name but a few of the many RAs who worked with me during my thirty-year career. I have, of course, earlier mentioned the extraordinary Marianne Larson (Kleman after her marriage), who agreed to accompany me and my family when we moved to Berkeley for a year. Another of my most memorable Research Assistants was Marge Klein, who later returned to work with me as a postdoctoral fellow. She became a Professor of Psychology and a most eminent researcher in the field of psychotherapy research. Finally, I wish to name just one more, Barry Wolfe, whose impressive work, I know, is familiar to you. Barry worked with me on two separate occasions. Initially he joined me in the Intramural Program and left to complete his doctorate. Later I was happy to hire him as a member of my professional staff in the Section on Psychotherapy and Behavioral Interventions in the NIMH Extramural Program. Still later I promoted him to be Chief of one of the Sections in the Psychosocial Treatment Research Branch, which I then headed. Following my retirement Barry became Head, Psychotherapy Research Program, Affective and Anxiety Disorders, Research Branch, NIMH. O.K., Ingrid, now I'll subside. Your turn.

Farreras: Well, I just had some clarifying questions about some of the things you mentioned the last time we spoke. You said Bob Cohen, who had been at Chestnut Lodge, had brought you in. That was before the Lab existed. What were you doing for that year or so before the Lab was established?

Parloff: Honestly, I don't fully remember. I think my activities – such as they were –fell under the heading of preparing research plans we hoped to implement once we moved into our offices in the Clinical Center. I know we spent a lot to time in meetings, library research, completing articles we were in the process of writing, and doing other such important things as consulting on the sorts of furniture that Gwen Will should purchase for the modern patient wards being planned for NIMH. I'd love to hear Bob Cohen's answer to your question of what we did during that early period. Once we moved to the Clinical Center I became more closely involved in working with some of the other psychologists, such as Dick Bell and Earl Schaefer, who were then on board in the Intramural Program. Ben Carlson was there, too, but we maintained only a social rather than work relationship.

Farreras: Right, and James Birren.

Parloff: Oh, Jim Birren, yes.

Farreras: He was in the Aging Section.

Parloff: Right, I never worked directly with him.

Farreras: So Dick Bell and Earl Schaefer….

Mrs. Parloff: You know Dick Bell just died.

Farreras: Oh, no. Did he?

Parloff: Oh, I meant to tell you.

Farreras: I know he had been hospitalized in early December, but I didn't know he had passed away.

Parloff: He died on the 19th of December. We just received a note from Sherry telling us the sad news.

Mrs. Parloff: Apparently it was complications of Parkinson's.

Farreras: I see. I'm so sorry to hear that.

Parloff: During that early period he served as my guide and mentor. We had a close relationship with him and to some degree with his first wife. Sherry Prestwick, his second wife, was the one we got to know better over a much longer period. But my major point in response to your question is that there were so few psychologists in the IM during 1953-54, that none of us took seriously any hypothetical organizational chart. We sort of huddled together and arranged to meet informally or informally and worked together whenever it seemed feasible and reasonable. For example, together with Dick and Earl, I worked on some research instruments they were attempting to develop. One in particular concerned Parent-Child relationships. I wish I could remember what the instrument was called but the acronym was, I believe, something like PARI. It became a very important measure, especially in their later research. Dick's basic notion was that our field had incorrectly placed far too much emphasis on reflexively assigning responsibility to parents for an individual's characterological and pathological problems. His hypothesis was that the field had neglected to sufficiently recognize the impact of the child's own behavior and personality on his/her parents. Relationships were reciprocal in effects and shaped the continuing interactions. Some infants and children were just easier for parents to deal with than others. "Difficult" children might tend to evoke problematic responses from a wide range of parents -- even so-called nice and patient ones. It had become fashionable, consistent with early psychoanalytic theory, to tend to blame parents – especially mothers --for any and all problems evidenced by children and adults. Dick and Earl were pioneers in the study of the nature and effects of parent-child interactions and their reciprocal effects. Some of Earl's work later led to the very useful notion that early social stimulation of young children might facilitate their later learning and even their readiness for school.

Earl and Dick developed some of the early evidence that IQs of children who had been exposed to environments rich in social stimulation would be higher than the IQs of children who had received less stimulation from their early environments. Earl Schaefer finally over-optimistically concluded that socially stimulated kids -- between 15 and 36 months of age – showed IQ scores that were 17 points higher than comparable subjects in "control" groups. That sort of finding provided a rationale for the subsequently popular preschool enrichment programs. In short, we few – we happy few – tended to turn to each other for consultation, support, and sometimes collaboration. I don't recall whether I earlier mentioned it, but for some reason Earl joined my Section while Dick, who worked closely with him, became a member of Nancy Bayley's Section on Child Development.

However, aside from members of my staff the group I most regularly interacted with were psychiatrists and ward administrators in the Clinical Investigations unit. For a long time I felt as much a member of the Adult Psychiatry Lab as I did the Psychology Lab.

Farreras: Within Clinical Investigations and in Basic Research as well?

Parloff: I was primarily involved, for research purposes, with the Clinical Investigations Branch. Aside from some periodic interactions with Kety and some members of his staff I don't believe I ever worked in the Basic Research area. For purposes of responding a bit more fully to your question, I do recall that when Kety was initially planning his twin studies with Paul Wender he invited me to join him. When I learned it would involve investigation of the role of genetics in schizophrenia I recommended that he consider Dave Rosenthal, who was working at Johns Hopkins. I like to think that I played an important role in promoting that most productive twin study collaboration. Ingrid, some time earlier you mentioned the name of Don Blough as an early member of the Psychology Laboratory. I have trouble placing him. Did we overlap during those early years?

Farreras: He was from the Perception and Learning Section, with Virgil Carlson, and he was here from '54 to '58 and then left for Brown. He's been at Brown ever since.

Parloff: Wow. I'm drawing a blank. I'm sorry.

Farreras: I've written to him, but I haven't heard back from him yet.

Parloff: But that's where he was, in the Perception and Learning Section?

Farreras: Right, which later changed to Perception alone.

Parloff: Yes, unit titles change at the whim of whoever becomes the unit chief. I suppose unit titles are intended to reflect the current state of a particular area or possibly where someone thinks it is going. For example, I told you about the rise of behavior therapy and how we had to accommodate that fact in the title of one of our later Sections -- Section on Psychotherapy and Behavioral Intervention.

Farreras: So, then, how did you end up in the Personality Section of the Lab? Was that Cohen's appointment?

Parloff: I have no clear recollection. It is likely that Shakow assigned me to it. But then, where else would I go? My interests were clearly in the area of psychotherapy and personality – theory, change, development, etc.

Farreras: In Shakow's Section of the Chief?

Parloff: Yes. But since I arrived before Shakow did, it seems unlikely that I would be assigned to a then nonexistent section. I believe the primary focus of the Section of the Chief was to promote Shakow's own special interests. Other variants of the general title "Personality Section" later also appeared in still other NIMH Laboratories. For example, sometime during the 1950's Roger Shapiro was made head of the Section on Personality Development in the IM program. Since I was basically a researcher and practitioner in the field of clinical psychology, it was naturally assumed that I was preoccupied with Personality. I saw no reason to quarrel with that assumption.

Farreras: Let me ask you something Wade Pickren and I have been talking about. Were most of the psychologists in the Intramural Program at NIMH in the Laboratory of Psychology, or were there psychologists in other NIMH Labs as well?

Parloff: After the Lab's initial period there were some elseewhere. I believe a fair number of psychologists could be found outside of the Laboratory of Psychology.

Farreras: Oh, there were? All right. Right now, I can only think of Marian Yarrow in the Socio-environmental Studies Lab.

Parloff: Well, I believe there were also others even in Socio-Environmentnal Studies, but I was thinking of psychologists in other NIH Institutes. Clinical psychologists were hired to perform psychological as well as neuropsychological testing. As psychologists in the Laboratory of Psychology became fully occupied with their own work they became increasingly reluctant to provide clinical services to projects with which they were not intimately associated as initiators or active collaborators. In the early days it was always taken for granted that psychiatrists would head the clinical team – psychiatrist, psychologist and social worker. This concept was initially, at least, extended to include the role of being the head of any research team. This was so ingrained that some psychiatrists assumed that even non-psychiatrist physicians should similarly consent to performing clinical services without requiring that they be acknowledged as "major research collaborators." Coming to the field, as I had, trained first as a psychiatric social worker and later as clinical psychologist, I was quite familiar with these quaint mores. Perhaps, however, over time I had come to "identify with the aggressor" for I wasn't nearly as uncomfortable with that situation as I should have been. I worked comfortably with members of the Adult Psychiatry Branch. Thus, I routinely attended ward rounds and even provided group psychotherapy for schizophrenic patients on one of the wards. Shakow may not have been entirely pleased to have one of his staff provide such non-research functions. However, I rationalized providing such services as a constructive means of cementing relationships across laboratories and helping to identify clinically relevant areas for research collaboration. However, a negative implication of the model I had thus inadvertently provided about psychologist-psychiatrist relationships was to set up the false expectation that psychologist researchers would be content to provide services to psychiatrist researchers. In point of fact, psychologist-researchers in the Psychology Laboratory viewed themselves as "independent investigators" who preferred to conduct their own research with patients who happened to be housed on the several wards of the Adult Psychiatry Branch. In reviewing the Section's old Annual Reports I recently came across a startling statement concerning what seemed to be a wholesale exasperated withdrawal of Laboratory of Psychology staff members from continued participation on the wards. Those retreats were apparently provoked by some perceived inappropriate demands that had been made of them by ward staff and administrators. My own experience, however, was apparently more congenial for I was able both to pursue my own ward milieu studies and provide solicited consultation and collaboration with nurses and ward administrators such as Lou Cholden, Charlie Savage, Murray Bowen, and others.

Farreras: So you were acting as consultant for the other labs within NIMH? Were there any people who were there as permanent members, the way you were a member of the Psychology Lab?

Parloff: I'm trying to remember. I'm sure there were some social workers but I do not recall any staff psychologists on the NIMH wards. As I have mentioned, Margaret Thaler Singer collaborated importantly with Lyman Wynne, but she was not an NIMH employee. There were, of course, psychologists in other Institutes, e.g., the Institute of Neurological Diseases and Blindness [today NINDS]. That Institute finally hired some, but I lost track of them. The Laboratory of Psychology itself grew to include 84 research staff members. I believe it represented, at that time, the largest such Psychology Lab in the country. But we did not do service for other institutes.

Farreras: That reminds me of a rumor I've been trying to verify. Do you know whether as part of his contractual demand, Shakow wanted to have a say in all of the hiring of psychologists in all of NIH, not just NIMH? That if a psychologist wanted to come and work here that Shakow would have to oversee that appointment, act as a gatekeeper, so to speak?

Parloff: I don't know that, but frankly it wouldn't surprise me. It's perfectly plausible, particularly at the outset.

Farreras: And that it just became too unwieldy to do in the end?

Parloff: I think so.

Farreras: Okay.

Parloff: It is equally plausible to assume that he may have run up against administrators who said, "This is my institute, thank you very much."

Farreras: Right! OK, so most psychologists were in the Psychology Lab. And also at that time – we were just talking about NINDB – Kety was overseeing the Basic Research Division of both the NIMH and the NINDB, correct?

Parloff: That's right. Let me read you something from page 10 of this book, *Research in the Service of Mental Health*: The editors wrote, "NIMH appointed a full-time Scientific Director. The appointment was Dr. Kety in 1951. Shortly after he arrived, it was decided that NIMH should join the new National Institute of Neurological Diseases and Brain" – a terrific name – "in setting up a joint basic research program, with Kety serving as Scientific Director. The programs had been combined, and Kety said "because" – and this is a quote – "progress in the diagnosis and treatment of nervous and mental diseases rests firmly upon a basic understanding of the nervous system through the biological and the behavioral sciences." That was the rationale.

Farreras: Al Mirsky mentioned a joint interest in certain topics.

Parloff: Absolutely.

Farreras: What led to the separation then? Do you know?

Parloff: I'm embarrassed to confess that I do not recall either the point at which there was such a separation or why such a split was made. I hope you get to ask some loftier administrator like Bob Cohen. I expect that the answer is something quite unremarkable like budgetary or administrative advantages.

Farreras: Yes, now it's NINDS, not NINDB, stroke rather than blindness.

Parloff: How did they ever consent to "and blindness"? Probably for sheer administrative purposes. In any event, Kety was a natural for that position.

Farreras: Was it Bob Felix who appointed Kety to this position?

Parloff: I assume so. The way such appointments are typically made is to consult with a lot of experts and develop a list of nominations. Of the nominees most decline the offer. Many are put off simply by the idea of working for the "red-tape" government, "Oh, my God, a Federal Government position." The stereotype of government constraints and low salary was not attractive to many of the most likely candidates.

Farreras: How did the idea of establishing a Psychology Lab come about? Was Kety the one who came up with the idea? In the literature we read about how the large number of World War II veterans necessitated more mental health providers at the time, that there weren't enough psychiatrists and social workers, and that World War II provided the impetus for other fields, like clinical psychology to grow….

Parloff: That's a plausible script, but I have no knowledge about that. I'm sorry. What I'm more confident about is that the entire early recruiting process required that the NIMH recruit its psychologists and psychiatrists from a limited pool. Most psychologists and psychiatrists were primarily interested in doing private practice, which was lucrative, rather than research, which was not. The senior psychiatrists who came to NIMH were an especially rare breed for they had to be willing to give up the opportunity to make a lot of money in order to work at the NIMH. The recruitment of military-age psychiatrists was, however, made relatively easy by the fact that their obligatory military service period could be satisfied simply by working for two years at NIMH – a relatively safe and certainly stimulating setting. I wish to emphasize the point that to successfully recruit the more experienced and senior psychiatrists it became necessary to provide them with an opportunity to augment their not-so-modest government salaries. The solution hit upon was to permit such candidates to supplement their income by undertaking part-time private practice. Initially this had to be an unofficial arrangement. However, the conduct of part-time private practice soon became officially recognized as a perk of the position. After a time it became necessary to extend this financial incentive to qualified clinical psychologists. Thus, I was able to begin my career as a part-time practitioner in '58. The precipitating reason for my undertaking practice was the fact that I needed the money to pay for the sizable medical expenses of my mother, who had just been diagnosed with colon cancer. I guess Bob Cohen was the one who had originally come up with the incentive of permitting part-time practice. Ultimately a less crass rationale was offered for permitting clinical researchers to conduct their own private practices. Continued exposure to clinical problems, it was argued, would aid the researchers in their sophisticated study of such basic psychotherapy issues as the nature, speed, and durability of outcomes and the unique or common elements of the therapeutic process. By the way, this is probably as good a place as any to comment on the early issue that arose about the NIMH facilitating the prevention of mental health problems. Such efforts, which later became very popular, were deferred for a long time on the assumption that we didn't know enough about the prerequisites, i.e., etiology of such problems or disorders, and effective processes for achieving and maintaining durable psychological change. Further, we were not clear about what was meant by the term mental illness. And if we don't know that, how are we going to prevent it from arising other than by undertaking some large-scale, prophylactic psychological and social engineering interventions, e.g., so-called good mental hygiene and good stuff like loving relationships, solid family values, financial security, etc. Were those to be the primary areas of study and intervention? Did they not overlap with the missions of other existing government agencies? What was NIMH's unique area of competence in the purely psychological rather than the genetic, brain, and biochemical areas? I recall writing some trenchant memos about this to the then Acting Director of the newly organized entity, ADAMHA. His name is Trachtenberg, but I've forgotten his first name [Alan]. One of his brothers, Joel, is the current President of George Washington University. The Acting Director solicited ideas for prevention research to be undertaken in the reorganized NIMH. I like to think I was instrumental in suppressing the idea of prevention research for a number of years. I argued that we could not do prevention until we better understood the mechanisms and processes of the problem. Was it a sociological problem? Was it physiological? And, obviously, most of us thought it was a physiological one. Bob Cohen, as you know, has a Ph.D. in that area.

Farreras: Yes, in neurophysiology.

Parloff: Right. You wouldn't know it the way he also encouraged the psychodynamic orientation. He fit in very well with that crowd. He was very psychoanalytically oriented, and what we got were these groups of psychiatrists who were all psychoanalysts. But, as I started to tell you before, the creativity of the psychiatrists who were assembled – especially the ward administrators – was highly valued. They were supposed to be particularly unconventional. As I mentioned in the first interview, a problem that quickly arose was that their ward nurses, in contrast, had been selected for being solid, conventional, good psychiatric nurses. They were also responsible to nursing department supervisors who expected them to conform to the accepted and standard forms of nursing care. It soon became apparent that the ward psychiatrists' novel ideas often conflicted with established ideas of what nurses should do on psychiatric wards. That inevitably produced serious conflicts, especially for the head nurses, who were responsible both to the ward psychiatrist and to the nursing departments. That arrangement was singularly effective in quite regularly "producing" serious mental breakdowns in nurses who had the designation of chief ward nurse.

As I mentioned before, among the more "creative" ward administrators who produced the most interesting and most contentious wards were Lou Cholden and Murray Bowen. Lou, unfortunately, died in an automobile accident, and Murray decided, with some encouragement, that the government constraints were too limiting on his expansive notion of "research." After these two psychiatrists left, the Adult Psychiatry Branch settled down and the mental health, at least, of the nurses improved markedly. The fringy character of the research wards was trimmed. But Charlie Savage, still another ward administrator, adventurously began to combine the use of psychotherapy with some of the new antipsychotic medications in the treatment of his ward's schizophrenic patients. Still later he began experimenting with the use of LSD. Back in those days it was distressing to some of his staff psychiatrists and nurses to discover that medications appeared to be even more consistently useful than the long-term application of the revered Chestnut Lodge style of psychological treatment of schizophrenic patients. That was part of the learning process going on in those early days. And as I am sure you will hear from Bob Cohen or read in the 1971-1972 Annual Reports, he decided to shift emphasis away from the sociological-psychological to the biochemical. That was associated with important budgetary changes.

Farreras: All right, yes. I'd love to know more about those budgetary changes; I haven't been able to locate any budget reports. So we have the Psychology Lab as the fourth intramural lab created at NIMH. It had that long name for the first year, Clinical, Experimental, and Developmental Psychology, and then it was changed to plain Lab of Psychology. Was that Shakow's idea?

Parloff: I don't know. I can't cite a reference to this, but I assume that he would prefer a more general and less constrained generic term that would permit him the greatest leeway.

Farreras: Okay, was it Shakow who wanted that broad perspective?

Parloff: Yeah. He subsequently added a very broad range of units to the lab. Not only the ones you might find listed in a general psychology text. For example, the section headed by John Calhoun on animal ecology. He was particularly interested in the effects of overpopulation of rats.

Farreras: He was out at Poolesville?

Parloff: Yeah. It was quite wonderful stuff. The expectation was that the findings would somehow apply to human ecology.

Farreras: Is that facility still there?

Parloff: I don't know. But my point is simply that Shakow's conception of the field of psychology was that it should encompass most of the concerns of the psychological world.

Farreras: Okay, so that was a name that, before Shakow came, people gave to the Lab as they were predicting what types of areas were going to be covered by it.

Parloff: Right. It is as if you looked through textbooks of the time for the major areas.

Farreras: And as far as funding, did the Lab get a certain allotment that Shakow then divided in a certain way among the scientists or among the Sections? How did that work?

Parloff: My impression was that Shakow worked within a defined budget that was allocated to him; however, its precise size was not made known to us. In turn, he allocated portions of that sum for support of each of the Sections. In point of fact, however, it was not until I became chief of a section in the Extramural Program that I was made aware of the specific budget I had to work within. While working with Shakow I do not recall ever having to prepare or submit a budget to him for approval. In our annual report we would simply report on the research activities of all members of the section and than indicate what we proposed to continue, terminate and undertake in the future. None of this was, of course, news to Dave as we would earlier -- and quite regularly -- have discussed, in detail, what each of the section members was doing and hoped to do. The formal annual reports, however, merely summarized what we had done and what we proposed for the future.

Farreras: That's where you have to ask for the money to do the research ahead of time.

Parloff: In effect. However, precise amounts of money were not discussed. We chatted with Shakow about what the plans were of each member of the section and any equipment, additional space or special travel that might be required. Most of the budget, of course, consisted of investigator salaries. Space limitations made up one of the major problems.

Farreras: So you just said what you had done and what you were going to do. That's a big difference from the Extramural Program, of course, where you have to ask for the money to do the research ahead of time.

Parloff: That's right. With regard to space, however, you will recall that when we first moved into Building 10 each of us simply seized sizable amounts of open space and planted our own flag saying, in effect, "This is mine." Then reality intruded on us, and our offices on the north wing, where we were housed, began to be too small, for as we added new people we quickly ran out of available space. In order to make room we cleverly began dividing up the rooms we had acquired. They ultimately became small cubicles. If you go there, look at those little green cubicles -- unbelievably small. I recall that the husband of Sally Kendig (Child Development Section), who happened to be a high official in the federal penal system, remarked upon first seeing Sally's office that it was so cramped that he couldn't legally house a federal prisoner in that inadequate amount of space. As Section Chief, my initially large office had to be partitioned to make space for my secretary's adjacent office. I know that shocked visitors, on first entering my office, would exclaim, "This is where you work? My God!" Of course, Rosvold, Mishkin, and Mirsky had an entire building, which they generously shared with their subject-monkeys. Those of us in the Clinical Center were limited by the constraints of the original structure. For a long time we didn't even contemplate getting additional space in other buildings. Ultimately we did, of course. So space became a very important issue, perhaps more than money per se, except for foreign travel funds, of course. The management of the Section's budgets, however, became a major responsibility for me when I moved to the Extramural Program.

Farreras: Right. I'm thinking of the Annual Reports I've seen from the '50s that list the specific projects that people were proposing. They'd mention who the main PIs were and who the collaborators were, and each project also had a specific budget. I think they said "total obligations," "direct obligations," and some other type of obligation which combined with the direct ones amounted to the total obligations.

Parloff: In the Intramural?

Farreras: In the Intramural. But it's only printed that way in the mid-'50s; by the late '50s the obligations are listed by overall section, not by individual study within each section. Then they stopped reporting obligations altogether. So we only have that information for those few years in the '50s. And "man-years" was the other thing reported, "1/3 man-years", whatever that referred to.

Parloff: Right. That's right. Except for the man-years issue I don't know that I ever was seriously concerned about that. But hopefully Bob Cohen will recall how that was done. It was certainly the administrators' responsibility to eventually make the hard choices of where the money went. There was a finite amount. Last week I was talking about a period in which our sense was that there was an infinite amount.

Farreras: But by now it's finite? When did it become finite?

Parloff: As soon as the Republicans came in. I'm serious.

Farreras: Oh, I believe you; I've heard this elsewhere as well. Yes.

Parloff: And that meant losing permanent positions and resorting to part-time workers, but there are only so many part-time workers you can get if you don't have the space for them. And I can recall some real skirmishing about that. So that's one of the reasons why I emphasize space as being a major determinant of what could be done. I think we finally had somebody working in a room on the north corridor that had the pneumatic tubes that served as one of the message centers of the Clinical Center. Nonetheless we had somebody actually sitting there. People would be streaming in, putting messages in those tubes. We were reduced to that.

Farreras: What was the criterion that was used to allocate money? Was it number of publications or how was that decided?

Parloff: I'm not sure. Certainly that was an obvious requirement; the staff members who had published a lot and were recognized by their peers for their contributions would be able to command more money. I recall that in attempting to recruit big-name investigators that was always a bargaining issue. National reputation was important.

Parloff: When it did come time to replace Shakow, Rosenthal's wide public recognition based on his twin studies and quadruplet investigations was a critical determinant of his ultimate choice.

Farreras: I hear Rosenthal didn't want it and had to be pressured to take it.

Parloff: Right. As a matter of fact, he took it only on the condition that someone else would assist him with some of the routine administration. That proved to be me. I had been doing some administration for Shakow for quite some time. Whenever Shakow would go off on holiday, I would serve as the Acting Lab Chief. I certainly never had the scope and range of reputation that Rosenthal had. Dave Rosenthal had been doing tremendously good work in the area of the interaction of genetics and environment. He had already written the book, *The Genain Quadruplets*. He was nationally and internationally known. So he was impressed into that job. We sort of pleaded with him.[1] Reluctant leaders are the best kind. He was a good guy to do this. And I remember his statement to me was, "Only if you'll help me, because you know how this is done." "Fine, I'll do that." I didn't mind because not only were we good friends but more important, I knew Dave to be a very bright man, overly modest perhaps, but certainly no stranger to the role of administrator. He had run the Phipps Outpatient Psychiatric Clinic for a number of years for Jerry Frank. He had done a terrific job. Nonetheless, he really didn't enjoy that sort of work. He was basically a very shy man. He liked to do his own work, particularly his writing! He'd write beautifully and disturbingly quickly. While we're talking about Dave, I want to finish the story of Dave's arrival at NIMH, which I began to tell earlier. By the time Seymour Kety had decided to do the study on twins to get some more information on genetics and environments, he and I had become good friends, and he respected my work, and so he invited me to work with him on that.

Farreras: When was this?

Parloff: Mid 50s, is when that project started. And I said, "Listen, I've done group therapy with schizophrenics, but this is not my thing nor my major interest. I'm in psychotherapy. But I do have a friend at Hopkins who know a great deal about schizophrenia," and that was Dave Rosenthal. Dave was not that well known at that time.

Farreras: About what year was this?

Mrs. Parloff: We moved into this house in '57, and they were already here.

Farreras: So they must have moved here around '56.

Mrs. Parloff: Either '56 or '57.

Parloff: Well, that was the appointment I'm getting at. After I told Kety about Rosenthal he then began talking with Shakow about him. Now, it turns out – this is really a strange coincidence -- that Shakow had been one of Rosenthal's professors at the University of Chicago and, sad to say, Shakow had not thought very highly of Rosenthal's dissertation. I assured Shakow about Rosenthal's subsequent fine work at Johns Hopkins and strongly recommended they meet for lunch in order to get reacquainted. They did and Shakow was impressed. So I like to think I may have been instrumental in helping Rosenthal get appointed to the Laboratory. And, as the saying goes, the rest is history. But Rosenthal was a brilliant researcher and soon brought great credit to the Lab. I had been chairing the Quadruplets Research Committee, which included all members of the research team as well as the four psychiatrists, each of whom was treating one of the Morlock quads. Morlock was their real name before Dave dreamed up their publication name, the Genain quadruplets.

Farreras: Dave Rosenthal did?

Parloff: Rosenthal, right. Frankly, I'd already found it difficult to work with such members of the committee as Jordan Scher and Sy Perlin of the Adult Psychiatry Branch. Jordon Scher, by that time, was also serving as the administrator of the ward on which the Quads were housed. For these and related reasons I thought, "Hey, this committee would be a great thing for Dave Rosenthal to chair instead of me." I was delighted to turn the committee over to him when he joined the NIMH. After Dave accepted that job he organized and edited that great book on the Genain quadruplets. He was an effective chairman of that research committee. Unlike me, he had great patience and could deal well with all of the people involved. As I recall, his management technique was to listen very attentively to what committee members had to say and then do exactly what he had originally planned on doing. As a result, he spent less time in meetings and more time in getting things done.

Farreras: How did the interest in genetics start? If you had a particular interest in X, like the genetic aspect of schizophrenia, would you propose this topic to Shakow and have to have him approve it? Or were there any influences or pressures to work on certain topics?

Parloff: Everyone's own research interests and ideas were routinely discussed with Shakow during regularly scheduled weekly meetings with him. At that time Shakow might simply agree or suggest modifications or strategies or even bring up new directions for possible interest. These were, however, treated as collegial suggestions rather than commands. That's where you go back to the Lab's philosophy. You come here because you've demonstrated not only your interest but your competence in the field, and in a particular area, and you are given your head to do your work. As I mentioned to you last time, Shakow had suggested to me that it would be just jim-dandy if I worked on creativity, like his friend Moe Stein had done. That area hadn't occurred to me. I didn't come in with a great passion for working on creativity. But I thought that was not an unreasonable suggestion, particularly since he had also proposed sending me to the Washington Psychoanalytic Institute for training as a psychoanalyst so that I might be better equipped to help people express their own creativity more effectively. That plan was consistent with my notions about the NIMH being designed not only to help people with their problems but also to assist individuals more broadly to better actualize their potentials. That suited me fine. And so Shakow arranged to fund my psychoanalytic training out of his own budget. Under those circumstances the study of creativity began to sound like an interesting idea. I'm sure I could have refused but….

Farreras: Was this Shakow's idea, or was Shakow getting pressure from above to work on this? From Kety or Felix?

Parloff: No, no, I don't know that anybody ever successfully pressured Shakow. Not Kety nor Felix nor anyone. Seriously, there was never any indication that Kety, in particular, was ever interested in pressing for anything outside of basic research. He was happy to be doing his own thing. Kety may have exerted some pressure with members of his own group, because he had a very fertile mind and had a lot of good ideas. And I'm sure people would be eager to work with him on his ideas. Periodically, Felix did make rather blunt suggestions in which he would indicate some particular research areas that members of Congress had expressed their interest in. Bob would often say that it would be helpful to him if someone in the Intramural research program would be willing to pick up on such ideas. I know about this simply from having attended a number of his periodic conferences with scientists at the Intramural. These were held in the Masur Auditorium. There may, of course, also have been private meetings with researchers that I don't know about. Based on his resigned manner of public cajolery I gather these pleas were not frequently very successful.

Farreras: In summarizing what you've said about funding then, would it be accurate to say that if the person were known, nationally or internationally, had a good reputation, published a lot…that those were the main criteria for funding allocation?

Parloff: Of course. The main criteria.

Farreras: What about the criteria for productivity or successful research careers? Were there any minimal standards, explicit or implicit, that people had to meet?

Parloff: Well, let me answer that this way. While there were no firm publish-or-perish rules, it was expected that researchers would show an appropriate record of publications consistent with the nature and complexity of the problems they were investigating. Sheer productivity was always a matter that was carefully noted and weighed by promotion boards. If one were working in areas that took a long time to develop – as was often the case with investigators in the field of psychotherapy -- the investigator was given proper leeway. Since the members of any NIMH promotion board were drawn from a number of different NIH institutes it often became necessary for Shakow to interpret why Lab of Psychology promotion candidates often appeared to have lower publication numbers than did similar grade candidates in other institutes. We frequently heard representatives of other institutes sarcastically blurt out concerning one of our promotion candidates, "My God. I've got research assistants who have more publications than that." Shakow frequently made the point to us that one of the critical distinctions between his Lab and a university psychology laboratory is that we at the NIMH were expected to work on high-risk and long-term research problems. As a result, rapid and frequent publication was not one of his expectations. Nonetheless, I have vivid recollections of having to defend one or two members of my section who after a period of some two or perhaps three years had failed adequately to publish much. But I think this relates to another point that I wanted to introduce about the budget. The story I want to tell you about Felix was – and I don't know how true it is. At one point he asked for a billion-dollar budget -- apparently the first billion-dollar budget submitted for a single institute -- and "they" allegedly replied, "That's it. That's more money than such-and-such institute and this institute gets put together. So if you expect to get a billion you had better make arrangements to be given your own agency, not a mere institute. You'd have to be an independent entity. We're not going to handle that." And so, in effect, he finally said, "O.K., fine." It is my impression that just the fact that he had the temerity to ask for a billion was sufficient to get us kicked out of the NIH. My impression was that we were finally kicked out. Presumably it was feared that giving one institute such a large sum might set a bad precedent in encouraging large budgets for other institutes. And so his story was that he was told, "Go make your own agency. We're not going to administer that." Now, whatever the actual facts were about why we left, at some point we were renamed and became a new agency. Later the ADAMHA was created, and the NIMH became but one part of it.

Farreras: I thought Felix left in '64 and Yolles took over for eight years. Wasn't the move to ADAMHA under Yolles?

Parloff: Oops. I'm sure you're right about that, but my point was that we all felt we had been kicked out of the NIH for being too demanding. Bert Brown followed Yolles. Yeah. It was Yolles who had upset us also with his plan to move NIMH to Columbia, MD. Do you remember any of this, Gloria?

Mrs. Parloff: I think Yolles was here as early as '61 or '62. Is it possible he was later elevated to the position of Director?

Parloff: Maybe so. I think I first met Yolles at Joe Margolin's home, where Joe's wife was giving him a surprise birthday party. Yolles was crouched behind a couch waiting for the birthday boy to enter the room.

Mrs. Parloff: And that was Joe's 40th birthday party, and he was just 80, so I think that was probably in '61.

Parloff: Good, thank you. Okay.

Farreras: But why were you separating? What was the rationale behind that? Are you saying that the other institutes didn't want NIMH to be a part of NIH if they were going to keep asking for so much money?

Parloff: That was my recollection of the rationale.

Farreras: So it was the other institutes that didn't want NIMH?

Parloff: And, of course, I think the Director of NIMH would have been very happy to be elevated to heading his own agency.

Mrs. Parloff: Did it have something to do with Kennedy's election, because I think the sympathy for mental health went way up with Kennedy's election...

Parloff: Oh, that's a very important point.

Mrs. Parloff: Especially Kennedy's interest in mental retardation.

Parloff: Well, yeah. Kennedy didn't make any special distinction between mental retardation and metal illness. He may have been sensitized to the general issue because of his sister. He really did intrude himself. He actually would make phone calls to the heads of various NIMH units to ask questions. People were delighted by his interest. I think that with the expanding budget that expectation may have fed into the fantasy of developing the NIMH into its own agency.

Mrs. Parloff: I think the worry about drug use played a role because they elevated the importance of drug use in ADAMHA.

Parloff: Yeah, drug abuse research and alcoholism research were then integral parts of the NIMH. As a matter of fact, Bill Pollin, an old NIMH colleague, became the first director of the Drug Abuse Institute when research in drug abuse and alcoholism was separated from the reorganized NIMH.

Farreras: Because the shift to ADAMHA is what I know the least about…so there was some political motivation for elevating mental health?

Parloff: Ingrid, I would take all this with a large grain of salt. I just have that recollection of Felix's story, but as you have pointed out there was a great time lag between his tale and the subsequent two reorganizations. As it turned out there was less "elevation" in the end than separation. I do think there was some resentment of the NIMH from within the NIH but I don't have any clear evidence I can give you.

Farreras: From the other institutes?

Parloff: There was a lot of unhappiness internally and externally about this.

Farreras: Internal as in within NIMH?

Parloff: I'm referring to the fuss within NIMH about leaving the NIH. Yeah. Many of us didn't want to leave NIH. There's a lot more status that goes with being part of the NIH – a basic research organization -- than there was with being either a quasi-independent agency or part of an agency that stressed applied services.

Farreras: Mort Mishkin mentioned that that was Felix's idea, the sort of service-oriented, community approach to mental health. Was it Felix's idea or Yolles's idea?

Parloff: I think it was probably more consistent with Yolles's orientation. The NIMH began emphasizing the notion that its research-supported findings should be translated and quickly communicated directly to community agencies and hospitals in the hope of enhancing such community and hospital services. Howie Davis was head of the NIMH services branch, which was dedicated to disseminating promising research findings to community service organizations.

Farreras: He was in Extramural?

Parloff: Yes. This was under the directorship of Stan Yolles. I do remember Howie sort of pleading with us to help get our research information out quickly to the community. And that emphasis sort of frightened me since many of our research findings required replication and further analysis before they deserved to be widely applied.

Mrs. Parloff: Under Kennedy, the idea of community psychiatric clinics flourished, and….

Parloff: Was it Kennedy or Johnson?

Mrs. Parloff: Yeah, or was it Johnson?

Parloff: With the demise of state hospitals emphasis was placed on an expanded role of community clinics. States were happy to be relieved of the burden of funding mental hospitals and to have those great expenses shifted to the federal government in the form of supporting community outpatient clinics. That led to the era of the great emptying of mental patient wards on the shaky premise that most patients who were in such hospitals were merely being "warehoused" and could be far better served by outpatient clinics. Soon community psychiatry became a very important part of the NIMH. The assumption that very disturbed patients would take their essential medications without supervision and voluntarily seek assistance from inconveniently located community clinics proved ill-founded.

Farreras: Wasn't that the deinstitutionalization under Reagan?

Parloff: Continued under Reagan. That's easy to check in terms of the dates when all this transpired.

Mrs. Parloff: That also brings up the fact that you all worked very closely with the Laboratory of Socio-environmental Studies, especially with Mel Kohn on schizophrenia.

Parloff: Well, he kept us informed of his work. As a matter of fact our Laboratory kept in close touch with much of the work being done in Socio-environmental Studies. We had very good relationships with Morris Rosenberg, who did some important work on self-esteem.

Mrs. Parloff : And Stan Coopersmith.

Parloff: Right. I was thinking particularly of Morrie Rosenberg's Self-Esteem Scale, which was used by many in our Lab.

Mrs. Parloff: While we're talking about names that hadn't come up, a name that I don't recall you mentioning was Jack Gewirtz.

Farreras: Oh, yes, Jack Gewirtz from the Child Development Section.

Parloff: Yeah. I was going to get into the Child Development Section in connection with your earlier point about changes in the names of Sections.

Mrs. Parloff: And I guess you could talk about the Redl project.

Parloff: Well, that brings us back to the important point I neglected to mention earlier about psychologists who weren't in the Laboratory of Psychology. There was Fritz, of course, but also Harold Raush, and initially Mike Boomer before Mike transferred into our Section. Redl's group, incidentally, was one that did move out of the Clinical Center and finally even got its very own building.

Mrs. Parloff: Yes. That's where Mike started from. But he came over after he and Goodrich became disenchanted and left Redl.

Farreras: Why don't we back up and finish the part about the ADAMHA. You mentioned that there were institutes that wanted NIMH out, that there was pressure toward a more community-oriented approach – translating basic research to hospitals and more community-oriented programs – and that there were people within NIMH who liked the status associated with NIH and didn't want to leave. Was that across most labs or any one in particular?

Parloff: I am more confident about the Psychology Lab and Adult Psychiatry, but I think it was a fairly general view. As a matter of fact, there was a lot of support for the need to remain within the NIH from the general psychiatric community. Danny Freedman, then editor of *Archives of General Psychiatry*, was very outspoken about this. When this change was made I think I was in the process of transferring to the Extramural Program. Yes, because I remember Bert Brown making the announcement. We were in the Parklawn Building, and it was a great shock. It was also a very poignant moment for many of us since one of the implications of that announcement was that Bert would soon be losing his job as Director. Merging into the ADAMHA was something I had not anticipated. So I must have heard the announcement made sometime in '72 or something like that.

Mrs. Parloff: The actual transition was on September 25, '73.

Farreras: What happened between '68 and '73, when NIMH first separated from NIH, before it became part of ADAMHA? All I have is that NIMH separated in '67, it was assigned to the Health Services and Mental Health Administration.

Parloff: Oh. You're right. The HSMHA. Even the acronym was offensive and non memorable. I had almost forgotten that transition since that entire period seemed so unremarkable.

Farreras: And then in '73, five years later, it went to ADAMHA.

Parloff: Yeah. There was no physical movement involved for us during that time, I believe. We stayed where we were. It was just another administrative change. I remember I then began to deal with Seymour Perry's office in that agency. It had the term Technology in its title. Actually, that shift was personally not very jarring. Dr. Perry was then a neighbor whom we knew socially. His son Grant was then, and remains today, a very close friend of our sons, Mike and Roger.

Farreras: And was this only the Intramural part, or also the Extramural? Was it all of NIMH, do you know?

Parloff: As I recall it was both.

Farreras: But physically, you were all still here, you weren't moved?

Parloff: Yes. I was still in Building 10, the Clinical Center. That's right. That didn't change.

Farreras: So despite NIMH people complaining, it was still separated from NIH?

Parloff: Yes, the association with the NIH was important but the loss wasn't as bad as it was later when we became part of the ADAMHA. There the NIMH lost its original identity when they split off alcoholism and drug abuse research from mental health. Then NIMH felt quite changed.

Farreras: Why not just become a separate entity? Why get sucked into ADAMHA?

Parloff: Those were decisions made at an administrative level I was not privy to.

Farreras: I'll ask Bob Cohen.

Parloff: I think it was even beyond his level. But he might have some better notions.

Then, after a time when we seemed rudderless, Gerry Klerman was appointed as the Administrator of the ADAMHA. Since I was by then in the Extramural Progam and housed in the Parklawn Building, where Jerry was housed, I had the opportunity of easy contact with him. But that's a different story.

Farreras: All right.

Parloff: But otherwise the lab remained intact. I mean, it was a different administration, but everybody just went about their own research. There was no intrusion except for the administration, which had changed. I do remember when Reagan came in with his "hatchet" man, who was assigned to cut the size of the Intramural Program. I've forgotten what his name was. That was a very difficult time in terms of our losing positions. It was difficult to make staff appointments – fulltime or part-time. The aim of reducing the size of the government was one of the big goals of that administration. It was a red flag to all of us.

Farreras: You had a different administration but you're still here on campus. Was there any resentment from the other institutes for using precious space?

Parloff: Their space. I'm sure there was. There was a nagging discomfort that began to be felt, but we were buffered from any such direct confrontations. Shakow had been very good at buffering us from such outside pressures as long as he was around. He was known as a big fighter, and Bob Cohen helped with those protective efforts. But evidences of our being viewed as interlopers did trickle down. There were joshing reference like, "So what are you guys still doing here? You're not even part of NIH anymore!" We joked about it, but it wasn't always a joke.

Farreras: Okay. I'm sorry I interrupted you, you were going to talk about Jack Gewirtz and the different sections changing names.

Parloff: I just thought the array of titles given over time to the area of Child Development in the Lab of Psychology might illustrate the ease with which such names were changed. Changes were associated with section leadership changes. For example, when Nancy Bayley first came in I'm sure she gave her section the name that best suited her. And then when other Section Chiefs followed her the names of essentially the same unit were altered. We had an array of wonderful child development people, including Sally Kendig and Harriet Rheingold.

Farreras: She just died, last year I think.

Parloff: Yeah. I just heard. And Jack Gewirtz, Dick Bell and others. They may all have been there for administrative purposes and housing, but they all did their own thing. Clearly Schaefer could have fitted in with them but for reasons I either never knew or have forgotten he was with me.

Farreras: So it was the Section Chiefs who, depending on what their interest or their research was, changed the Section names?

Parloff: Yes, and Marian Yarrow, who could easily have fitted into one or another of the Child Development Sections, ultimately developed her own research group.

Farreras: She was in the Socio-environmental Studies Lab with Clausen first?

Parloff: Yes, right.

Farreras: And then later on, I think it was the Developmental Psychology Lab in the mid-'70s or something?

Parloff: Uh-huh.

Farreras: Okay. Was she a psychologist?

Parloff: I always thought so. But perhaps not.

Mrs. Parloff: Weren't she and her husband sociologists?

Parloff: Well, her husband, Leon Yarrow, later became head of an Institute or something. I just assumed he was a psychiatrist. My main point was that each unit head selected the name they believed best described their own particular emphasis and also reflected the current thinking in their field. I think that makes sense.

Farreras: Mm-hmm, because I'm noticing that most of them, Aging, Animal Behavior, Perception and Learning, and Developmental…changed in '66, which was the year Shakow retired. So I'm wondering if there was any correlation between those two facts or not.

Parloff: There probably was, but that wouldn't necessarily be inconsistent with the hypothesis I've been proposing. Perhaps Shakow's retirement provided an opportune moment to make such changes.

Farreras: And then you mentioned that you were influential in getting David Rosenthal to take over Shakow's position, and then Shakow really, really retired.

Parloff: Yeah. When Shakow retired he was given emeritus status and had a couple of offices on the fourth floor. Although he remained in the building, he never appeared in any of our offices except by invitation. He was always accessible to us, however, in his suite. I would never have predicted that he could keep out of the daily administration of the Lab once he had resigned. But, unbelievably, he really did. He set a great model for me in my subsequent retirement.

Farreras: Did things change in the Lab at all? I think Ted Zahn had mentioned that Rosenthal really wanted to narrow down the scope of the Lab because he didn't really want to have so much to administer.

Parloff: I'm sure that's true, but by the time Rosenthal took over there wasn't all that much left to administer. The size of the Lab had already been cut back considerably from its high point of about 84 employees.

Farreras: Okay, so it wasn't really Rosenthal wanting to narrow down the Lab but that the Lab itself had already been dwindling?

Parloff: Right. I had had the bad luck to be serving as Acting Chief during a couple of Shakow's brief absences, when the Lab suffered sharp cuts. That's why I speak so confidently about the reduced size of the Lab when Dave R. took over. I think we were down to about 60 or less. But nonetheless, what you say is probably quite accurate. Rosenthal was a guy who would feel more comfortable administering those sections that he felt most knowledgeable about. Ingrid, there is something I have to mention. Some time after he followed Shakow, Rosenthal began to show signs of suffering Alzheimer's.

Farreras: But he worked for quite a period of time – several years – before people even knew about the Alzheimer's?

Parloff: Exactly. I was in the Extramural and did not hear that his functioning was becoming impaired until Bob Cohen called me and asked, "What's going on with Dave?" He then told me a strange story about Dave's difficulty in performing a simple spatial relations task. When Dave told Bob that he and Marcia had decided to sell their summer home, Bob responded that he and Alice were in the market for a summer home and asked Dave to describe the place. Dave had such difficulty doing that, that Bob finally asked Dave just to give him a rough sketch of the floor plan. At first Dave tried to do that in Bob's presence but couldn't come up with a drawing that seemed remotely plausible. He went back to his office to work on it but still couldn't do it. Bob reported it was then that he first became concerned about Dave's conceptual abilities. Some time earlier I had witnessed Dave behaving badly in our home toward Marcia but I dismissed that as a consequence of his still being in a recovery state from a retinal detachment hospitalization. I had assumed that was simply Dave's unfortunate reaction to the understandable profound stress. So, my initial reaction to Bob's strange news was to become irritated with him, "That's ridiculous," I said. "You know, Dave lives right down the street from us and I see him often. There's nothing wrong with him." I had a great deal of difficulty accepting that Dave was showing signs of Alzheimer's disease. I didn't directly witness the impact of Dave's disability on his functioning at the office since I was long gone from the Lab. Finally, I heard some sad stories from Marcia, however, about his forgetting the route he had daily walked from his home to his office for many years.

Farreras: When you say that you saw how the Lab dwindled from the 84 people it once had in the mid-'50s or so to about 50 people in the mid 60s…what were some of the reasons behind that decrease?

Parloff: Economic. I think largely budget cuts. And there's another point that has to be reckoned with. The NIMH salary levels were not keeping up with what scientists could get in academia and elsewhere. It was always the case. The NIMH in this respect adopted a rationale that since it was such an honor to work in the Laboratory, we didn't have to pay high salaries. It was the same kind of thinking I had been exposed to at the University of California at Berkeley, which actually prided itself on being able to pay less because, after all, there was the great climate and compensating prestige associated with merely being at Berkeley. I learned this for a fact when they offered me a professorship in '64. We at NIMH had great difficulty retaining people in the Lab primarily because our salaries were not competitive. Our salaries in those early days, in the '50s and '60s, were typically $2,000-$4,000 off the market rates, and that was considered a lot of money then. It remained a problem until the government salary policy for researchers was finally appropriately changed. We lost some staff, whom we were never able to replace, primarily because it had become necessary to work within a sharply reduced budget.

Farreras: And this was across all of the Labs?

Parloff: Oh, sure. And we never even aspired, of course, to meeting the salaries offered in industry.

Mrs. Parloff: Do you think any of it was the discovery that certain lines of investigation weren't going to be fruitful?

Parloff: Yes. As administrators, we always liked to say that. Well, with the big priority shift, it became necessary ultimately to deemphasize psychosocial research in favor of the greater promise of recent biological and chemical advances. As I said, the drift became policy around about '72, when the major policy decision was announced. It was a decision only reluctantly reached by Eberhart and Cohen that it was time to recognize that the anticipated contributions of sociology and psychology, especially regarding knowledge about more effective treatment of mental illness, had been disappointing and limited. It now appeared that the important advances were going to be made much more rapidly in the hard rather than the soft sciences. It became a matter of resetting priorities. So that was recognized. And I'm sure these views were encouraged by the NIMH National Advisory Committee and other such advisory boards. In recent years I have learned there have been some harsh and brutal assessments made in some of their reports about the early intramural program's organization, staffing and clinical emphasis. It is far easier to recognize programmatic limitations in retrospect than when you're actively developing and participating in them. I know Bob Cohen was very hurt by some of the retrospective critical assessments of our work.

Ingrid, before we wind up today's session I'd like to express some personal views that go counter to some of the negative recollections I have been dwelling on. I think we ought to get on the record some of my very positive and lasting impressions about the Intramural Program. In the course of my 19 years in the Intramural I thought seriously about leaving the NIMH for another position only twice. I was tempted first in '64, while on my sabbatical, when I was offered a professorship at the University of California at Berkeley. I had always wanted to work at that university. Berkeley was such a great place, and I had enjoyed my year there enormously. As I told you earlier I was a visiting professor in the Department of Psychology and also fully associated with IPAR. So it was a very serious question of whether I would accept that offer. It was very attractive. But I finally decided against it. I must admit I was initially surprised by my decision. At the end of my sabbatical year, when I was about to return to the NIMH, I asked myself a critical question: do you want to be a researcher more than an academician? The straightforward answer was researcher. The next question was, well, since you want to be a researcher, where better than at the NIMH? And that settled it. But one of my main reservations about giving up Berkeley and the academic life was that I had rediscovered the enormous broadening value of being able to interact with students on a regular basis. I then realized that at NIMH my high-caliber research assistants could and actually did fulfill that function admirably. Thus, at NIMH I had, in effect, the desired stimulation that is usually provided by students. Bright and challenging students help not only to provide intellectual stimulation but also keep us intellectually honest. My appreciation of the role played by our RAs, who were often equivalent to graduate and even postgraduate students, became even more clear to me. I then happily returned to the NIMH. I have earlier mentioned a couple of RAs who were among the most important to me, Marge Klein and Barry Wolfe. The second time I considered leaving was quite near the end of my career when I received what seemed like an extraordinarily good job offer. I was then in the Extramural Program. By then the personnel office at NIMH presented me with the prosaic but compelling argument that by leaving when I was so close to meeting the 30 years of government service hurdle, I would be giving up a sizable chunk of my potential retirement benefits. So I completed 31 years of government service, hired and groomed my successor, John Docherty, M.D., and leisurely wound up my work. Incidentally, Ingrid, I have prepared a complete list of names of those who served with me in the Section on Personality. The list includes some more of the RAs.

Farreras: Oh, the staff members. Great. This will be very helpful because you have dates on it and everything.

Parloff: Unfortunately, there came a period, long after my retirement in 1983, when I decided finally to throw away almost of my section files. In part, it seemed unlikely that former staff would ever again ask for letters of recommendation from me. Time had passed. So I threw them out. Now, unfortunately, I don't have some of that relevant material. I'm sorry.

Farreras: I'm thrilled that you have all this information as it is!

Parloff: I have only a sampling of our old annual reports. I don't know what happened to the rest. I even included some report of Dittman's early drafts so you get a more detailed picture of what he was doing. Dittman was an important figure.

Farreras: Good, thank you.

Well, I don't want to intrude on your lunch hour again. I appreciate the time you've spent talking with me today and hopefully we can set up a different time to discuss your years in the extramural program.

End of Interview

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[1] While I do not recall ever formally being invited to nominate a Lab Chief to succeed Shakow, I certainly understand the plausibility of Ben Carlson's recommendation of Hal Rosvold. Hal was clearly one of the most prestigious and internationally known investigators in the laboratory at that time. I also recall, however, that by the same token, he had attracted great controversy. Rosvold's research approach of brain section ablations had, by then, fallen into question in some powerful sectors of the scientific community. In addition, the animal rights people had taken to periodically staging public protests on the NIH grounds. In any event, Rosenthal was also a major figure in that his research was effectively attempting to bridge some important genetic and psychological areas. While I never pictured Rosenthal as being particularly sympathetic with the psychodynamic position, I can well believe that Cohen might have been more content with having him represent the lab (e-mail communication, 3/27/2002).