Parloff, Morris 2002 C

Dr. Morris Parloff Oral History 2002 C

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Morris Parloff (NIMH 1953-1983)

This is a third interview with Dr. Morris Parloff, former member of the Laboratory of Psychology of the NIMH Intramural Research Program, held on January 17th, 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.

Farreras: We plan today to talk about your work in the Extramural Program, a period roughly from 1972 to 1983. Then, in 1983, after some 31 years with the NIMH, you decided to retire from federal government service. However, before we turn to your last 12 years in the NIMH I have a few leftover questions about the Intramural Program that I wonder if you can help me with.

Parloff: I'll try.

Farreras: Al Mirsky mentioned that the name of the Lab was changed from Psychology to Psychology and Psychopathology when Shakow retired in 1966, to reflect Rosenthal's research interests, but the scientific and phone directories do not reflect the Lab's name change until 1975. Is this your recollection as well or can you remember why the Lab's name was changed in 1975?

Parloff: That's very strange. Al believes that the name of the Lab was changed in 1966, but I have no such recollection even though I remained in the Lab until about 1972. That Rosenthal should have wished to change the name of the Lab seems perfectly reasonable to me, for as I told you in an earlier interview, the names of administrative units were often changed soon after their leadership changed. It would, however, be consistent with Rosenthal's sensitivity to have delayed making such a change in the Lab's name immediately, out of respect for Shakow's continued strong psychological and physical presence. However, Shakow didn't physically "leave the building" until 1981, when he died. In any event, I suggest that you seek further to confirm Al's recollection. Of the many hypotheses that come to mind is the obvious and simplest one that none of the memories of your now aging informants can be considered completely reliable.

Farreras: Present company excepted, of course!

Parloff: Of course!

Farreras: A new section under the name of Comparative Behavior, led by Walter Stanley, was created in '68 and only lasted until '73. Do you know why such a section was established? Who hired Stanley and why? What type of research it conducted? Why it only lasted five years?

Parloff: Ah, you're now putting my hypothesis about aging memory to the test. Well, I do remember Walter Stanley. He seemed to wander about for a time looking like a poor lost soul. He was eager to get me to listen to one of his strange notions. As I recall it I was courteous but skeptical. He persisted for a time, but finally he stopped coming around. Now, with regard to your specific questions about him, I'm pleased to assure you that I have not forgotten the answers. I just never knew any of them.

Farreras: O.K. I have just one more. It seems that all the Laboratory of Psychology Sections disappeared around 1974, perhaps coinciding with NIMH's departure for ADAMHA or with a more biological emphasis on NIH's research mission. Can you shed any light on what happened to the original sections and why they didn't survive except for the Animal Behavior section, which became the Laboratory of Neuropsychology?

Parloff: Happily, by 1974 I had been long gone from the Intramural and I have no useful information about that. I just always assumed that the policy changes we spoke of earlier were gradually implemented, and people either left or were absorbed into some of the surviving units.

Farreras: You mentioned earlier that even while you were in the Intramural you had established some important relationships with psychologists in the Extramural Program. Can you tell me more about that?

Parloff: For the most part these were social relationships. For example, I was then and have remained friends with Eli Rubinstein, then head of the EM Training Division. Eli and I did some professional work, too. We organized a psychotherapy conference which led to our subsequently co-editing the book *Research in Psychotherapy* (1959). It reported on the proceedings of the first of three such psychotherapy research conferences. Each of these conferences was jointly sponsored by the NIMH and the American Psychological Association's Division of Clinical Psychology [Division 12]. Other of my close EM associates were Norman Garmezy and Joe Bobbitt. Incidentally, some other members of the Section on Personality, Allan Dittman and Mike Boomer in particular, also were warm friends with Joe Bobbitt and his wife. Those were all very, very pleasant relationships.

Farreras: But at some point the NIMH must have gotten really big.

Parloff: Of course. Because the EM and the IM had different missions, they expanded in their own directions, but the psychologists maintained some professional and social contacts. The changes weren't simply due to our increased size but to our evolving interests and responsibilities. We had a big social life in the early days that included many of the psychiatrists in the Extramural. Those, too, were maintained for a very long time. I suppose those relationships began while we were still at T6.

Farreras: What finally led to your decision to transfer to the Extramural Program and to leave the Intramural, where you had spent some 19 years as a "bench scientist"?

Parloff: Good question. Over my years at NIMH I had received a number of invitations to take one or another position in extramural programs, but routinely I had turned them down. However, in 1971, when Marty Katz, Chief of the Clinical Research Branch, first asked me to take over Sain [A. Hussain] Tuma's position as Chief of the Section on Psychotherapy Research in Marty's Branch, I found that to be a very attractive opportunity. As Chief of that Section my duties would include identifying what I believed were important research questions and directions for the field to explore, and to encourage and assist both young and established investigators to prepare and submit carefully designed research grant applications. Incidentally, Sain was simply being promoted to serve as Assistant Chief of Marty's Branch, and his wisdom and experience would, therefore, be available to me. The offer came at a particularly fortuitous time in my career. I was ready to make a change. Marty's invitation may have been prompted, to some degree, by the fact that I had earlier made quite known in my talks at professional meetings my growing concern that the field of psychotherapy was not developing in a manner that gave promise of soon developing a "cumulative body of knowledge." Researchers appeared to be content to do their own small-scale psychotherapy studies that focused on esoteric process questions. They preferred single-case intensive clinical studies, and many were content to report and interpret findings based on quite small sample sizes, such as 6 or 8. The diagnoses of treated patients were usually vague, and subjects were often described merely as "psycho-neurotics." The treatments themselves were so ill-described that they were essentially un-replicable. The outcomes of such treatment studies were often assessed by instruments that had been freshly created by the investigator and were, therefore, of uncertain validity and untested reliability. Most rigorous psychotherapy research was undertaken – with a few notable exceptions – as dissertations by psychologist Ph.D. candidates, who carefully avoided undertaking replications of published findings since doctoral dissertations were required to be "independent and original" pieces of research. As a consequence, efforts to replicate published research findings were rare. It appeared clear to me that the field of psychotherapy research required the gentle guidance and persuasion that the NIMH could best provide. As you well know, Ingrid, all major decisions, such as changing one's job, as analysts have long been fond of saying, are "overdetermined." If pressed, I can easily identify other reasons that contributed to my willingness, at that particular time, to take the leap. For example, I had spent some 10 years in the area of creativity research and had become progressively disenchanted with our "statistically significant" but essentially unexciting and clearly "uncreative" findings. Creativity could not be adequately explained by the study primarily of personality characteristics, as I had optimistically and naively expected. I increasingly wished to return to something "simpler," like psychotherapy research. I hoped to pursue research aimed at better understanding and improving the effectiveness of psychotherapy as conducted under specifiable and controlled conditions. Marty Katz gave me that opportunity. I cautiously accepted a one-year temporary appointment, and by 1972, I happily accepted a permanent appointment in the Extramural. In the interim period I had managed to bring the creativity research program to a non-screeching halt. Since Lois-ellin Datta had maintained her interest in the Westinghouse Science Talent Search project, I made all of those records available to her in the expectation that she might continue that work.

Farreras: It might be historically interesting if you could give a brief overview of some of the major psychotherapy and psychotherapy research issues you encountered during your 12-year tenure.

Parloff: Fine. But you will find, unless I am very careful, that I may slip into giving portions of some of my old lectures on that exciting but now ancient period. As I recall, soon after I joined the Extramural Program it became apparent that our Grant Review committees, who were staffed increasingly by behavioral and cognitively oriented researchers, showed a preference for well-defined and rigorously controlled research and an antipathy for naturalistic process studies. This, however, was long before the notion of applying randomized clinical trials methodology to psychotherapy research was introduced. One consequence of the increased emphasis on rigorous research was the dwindling of NIMH's previously largish inventory of psychoanalytically and psycho-dynamically oriented studies. In contrast, the number of approved and ultimately funded operant conditioning, behavioral therapy, and cognitive-behavior therapy grant applications increased sharply. This required that Barry Wolfe and I become much more acquainted with the theory and practices of these new-fashioned treatment approaches. In my case it meant that I had to keep my own long-nurtured psychodynamic and psychoanalytic biases in check. You will recall my mentioning that in response to the clamor of behavior therapists – academicians during our site visits – I expanded the title of the section from Section on Psychotherapy to the Section on Psychotherapy and Behavioral Intervention.

Farreras: So, the NIMH had initially funded a lot of psychoanalytic research?

Parloff: Oh, yes. Reflecting the treatment theories most popular at that time, both psychoanalysis and Carl Rogers' client-centered therapy were the treatments used in the most frequently submitted grant applications to the NIMH. The early research – meaning the description of process or theory, not the outcome-oriented research that Wollerstein, Menninger and few others did – of many of the most eminent American psychoanalysts had been well supported by NIMH for long periods. As time went on, however, many analyst-researchers reluctantly concluded that NIMH Grant Review committees' growing preference for rigorous "scientific" rather than naturalistic clinical studies naively interfered with the establishment and maintenance of the very conditions prerequisite for the study of typical psychoanalytic processes. As classical analysts gradually withdrew or were barred from the NIMH roster of supported research, the field of psychotherapy experienced a lush growth of new forms of psychological interventions, most of which claimed to have a vaguely humanistic orientation. By 1980 Richie Herink had identified and described the practice of over 250 so-called psychotherapies. The continued burgeoning of such therapies is indicated by the fact that by 1986 T. Byram Karasu reported having catalogued over 450 distinct psychotherapies. In response, I was fond of remarking, "No form of psychotherapy has ever been introduced without being accompanied by earnest claims that its new and unique advantages would sweep aside the therapeutic buffoonery that had preceded it. Yet, no form of psychotherapy has ever been abandoned because of its obvious failure to live up to such claims." I know I published some version of that statement a couple of times and it was widely quoted. In addition to the relentlessly increasing population of psychotherapy forms, the number of categories of "therapists" claiming therapeutic capability also expanded, e.g., therapists who did art therapy, aversion therapy, assertive behavior therapy, bibliotherapy, biocentric therapy, bioenergetic analysis, bioplasmic therapy, breathing therapy, burn-out prevention – and we have not yet left the B's. Some frankly cultish approaches arose, many of which were aimed less at treating recognized disorders than ameliorating "problems of everyday living," i.e., the dilemmas of "normalcy." Techniques for better coping with such difficulties were ostensibly also to be offered in marathon groups, sensitivity training, sensory awareness, Erhard Seminars Training, Primal Screaming, etc. Insurance companies faced with the prospect of being expected to reimburse clients for treatment by such "virtual therapies" for ambiguous but nonetheless seemingly universal psychological problems soon decided to reduce and restrict their coverage. Psychoanalysis was one of the first to be curtailed.

Farreras: Analysis, too? I thought analysts had never been covered by insurance.

Parloff: Oh, initially some health insurance companies – particularly those who provided health care coverage to federal employees, such as Aetna and Blue Cross/Blue Shield here in the Washington area – reimbursed for all such treatment services at the rate of 80% of the analyst's and the psychotherapist's fees. Well, with that sort of support, many psychiatrists soon became de facto analysts. One could see patients four or five times a week at a considerable increase in income but at a relatively small additional cost, if any, to their patients. The overall duration of treatments became notoriously long if not interminable. The availability of such insurance-supported analytic practice, combined with the presence of the Washington Psychoanalytic Institute and the Washington School of Psychiatry, made D.C. a center of analytic and psychodynamic training and therapy. The Washington School of Psychiatry was known for having earlier assembled such path-breaking educators as Harry Stack Sullivan, Frieda Fromm-Reichmann, Clara Thompson, David Rioch, etc. Incidentally, the very availability of such bountiful insurance support became the source of a short-lived conflict for some of us then receiving our psychoanalytic training at the Washington Psychoanalytic Institute. Some instructors properly cautioned that in accordance with classic psychoanalytic theory it was necessary that the fee charged be large enough to represent a meaningful personal sacrifice by the individual patient. That, presumably, would further ensure the patient's high motivation for active participation in the onerous process. However, if psychoanalytic fees were paid largely by insurers, this might inadvertently interfere with the psychoanalytic process. However, most of us "analysts" were able to demonstrate our motivation without including that factor. I do recall, too, that soon after I became Chief of the Section on Psychotherapy and Behavioral Intervention in the Extramural, some insurance representatives proposed preparing a research application to investigate the putative negative impact of reimbursement on psychoanalytic efficacy. Clearly, they hoped to save some money. Fortunately, the design was too complex and they withdrew.

Farreras: I had no idea that this was actually part of analytic theory and technique for the establishment and maintenance of patient motivation.

Parloff: Happy to be a source of such historical esoterica. The larger point, however, concerns the steadily mounting costs to insurers and third-party payers. That finally required them to set some limits. Later, health-care management agencies were created in response to the further economic threat to private insurance companies of Clinton's National Health Insurance plan. We at the NIMH assumed that soon private insurers would be demanding better evidence of the efficacy of the long-existing treatments for particular disorders. Certainly the new behavior therapists, such as Wolpe and Lazarus, had already begun publishing impressive claims for the success of their efforts.

Farreras: Yes, success rates in the ninety percentiles, especially for anxiety disorders.

Parloff: Thus, behavior therapists also claimed the "truth" of the potency of psychological interventions. Both psychotherapy and behavior therapy, contrary to what Eysenck had asserted way back in the '50s, did in fact work better than "no therapy." Don't push me too hard, however, on what psychotherapists, as distinct from behaviorists, meant by the notion that psychotherapy "worked." Apparently it worked with those who presented themselves and were deemed suitable for such ministrations. Even after the number of forms of therapy began to multiply rapidly, it remained unclear who was an appropriate patient for what particular kind of psychosocial treatment. Similarly unclear were the mechanisms and processes that effected the putative therapeutic changes. While claims and rhetoric were profuse, compelling evidence was not. Bergin and Strupp had already introduced the cautionary finding that if psychological treatment was indeed as potent as claimed, then, like any other such powerful interventions, psychotherapy must have the potential for producing "negative" or harmful effects. In addition, Luborsky's "Dodo" verdict that all tested forms of therapy appeared to work equally well provoked serious questions regarding the credibility of these efficacy claims for psychotherapy.

Farreras: When were such questions about accountability raised by health insurance companies?

Parloff: Actually the question of accountability, while an important one for them, quickly became secondary to that of sheer cost. The burden that insurers feared was primarily that of having to reimburse, indiscriminately, for all the increasingly available therapies. There was, and remains, no FDA equivalent in the field of psychosocial treatments. We at NIMH resisted being cast in that role. I was made dramatically aware of the insurers' interests during the early '70's. I can recall attending several meetings chaired by Gerry Klerman at which the representatives of a number of health insurance companies successively raised these concerns. The cost of health care was not only a worry of insurance companies but, perhaps even more important, of the keepers of the national health budget. Congressional leaders were making ominous predictions about the devastating impact of the seemingly unbridled annual increases of health-care costs. Meanwhile, back at the ranch, our own budgets were being cut.

Farreras: Concerns about excessive health care costs led to the development of the managed-care programs?

Parloff: Indeed, some psychotherapists appear to have forgotten that perhaps one of the major reasons for the current rather miserly reimbursement policies of health-management organizations was the initially generous payments for a wide range of therapies administered to a wide spectrum of patient-clients by loosely defined therapists. The traditional therapy goal of "normalizing" had been extended to include the goal of "optimizing." It was not sufficient merely to treat the disturbed, the disturbing, and the vulnerable. Clients and patients were to be helped to grow and self-actualize. Thus, the therapist's task had presumably metamorphosed from that of "shrinking heads" to "expanding minds." Health-care managers properly suspected that the field's sudden explosive expansion was due to the practitioners' ardent belief in the "Big Bank" theory of the creation of ever-expanding numbers of psychotherapies. In this context the question of therapist accountability began to be raised.

Farreras: Well, perhaps we should return now to the question of the outcome of the Section on Personality?

Parloff: Oh, that unfinished business. Sure. I believe, however, that I had left that Section long before its decline and fall and, therefore, I was spared watching its final collapse. I do not believe that the administration had yet devised its policy of offering "early retirement" to senior-grade investigators. As the budget became tight staff members simply made other arrangements. For example, Mike Boomer retired from the Public Health Corps at a very high grade – Director level – and accepted a position at the Washington School of Psychiatry as head of its Treatment Center. Dittmann transferred to the Department of Education, as Lois-ellin Datta had earlier done. As we talked about before, Al Caron had meandered off to some other unit in the NIMH. So the staff gradually dwindled. I don't really know who finally turned out the lights.

Farreras: Was this just people in the Psychology Lab or did it all apply to others as well?

Parloff: I'm not sure. I do know from talking with Mel Kohn that Socio-Environmental Studies had undergone pretty much the same shriveling.

Farreras: So the softer areas of the field were affected.

Parloff: Yes, the softer areas. By '71 it became very clear that the critics of the softer sciences believed they were not pulling their weight. Not me, of course, but others. It was a very sad time. Clausen, the head of the Socio-Environmental Lab, had already gone back to California.

Mrs. Parloff: Yes, we saw him at Berkeley, during "our" sabbatical year. He was teaching there.

Parloff: Yes, I remember, that was in '64-'65, so he'd already left and Mel Kohn had taken over the Lab.

Farreras: And now there's just a Section left from that original Lab, headed by Carmi Schooler.

Parloff: Is Carmi still there?

Farreras: Yes, in a Socio-Environmental Studies Section all to himself, not affiliated with any Lab or anything.

Parloff: Really?

Farreras: He's down in the Federal Building, sharing space with Ted Zahn, who's affiliated with what became of the Lab of Psychology, what is today called the Lab of Brain and Cognition.

Mrs. Parloff: Where is the Federal Building?

Farreras: On Rockville Pike, about a couple of blocks from the Bethesda metro stop.

Parloff: I've lost track of all of those machinations because my interest shifted to the Extramural and my work in the psychotherapy field.

Farreras: You said that before you were invited to join the Extramural you had made your renewed interest in psychotherapy known. To whom had you made it known?

Parloff: To colleagues during attendance at various professional meetings, but perhaps most importantly, it appears, to Irene Elkin (Waskow was her married name). That was where good fortune comes into play. That's the third force. Irene had been a post-doc in Shakow's Section of the Chief. There she served as a research assistant for Paul Bergman. I don't think she had much to do with the Shakow study of a complete course of psychoanalysis as conducted by Paul Bergman, but during that period she frequently consulted with me about issues in the general field of psychotherapy research. We had long meetings during which I told her of my views and hopes for the field. She was, of course, very bright. She continued to have contacts with researchers on Carl Rogers' studies. The point is that Irene and I became good friends. She later worked with Marty Katz, in the Extramural Program, first in the Psychopharmacology Branch and later in the Clinical Research Branch, of which Katz became Chief. So when Marty was interested in finding a replacement for Sein Tuma she strongly recommended me for the position as Chief, Section on Psychotherapy. When I was so freely grousing to her about my complaints regarding the field of psychotherapy research I had no way of anticipating that she would later be instrumental in getting me back into a position to do something about it. I recall that Jonathan Cole, who used to be at the NIMH Intramural, had earlier explored with me the possibility of my joining him when he became head of the Psychopharmacology Branch, but that didn't interest me. He was subsequently succeeded by Jerry Levine as head of that Psychopharmacology Branch. Jerry, while no great friend of psychotherapy, was nonetheless later of very great help to me. So Marty and I got together and he listened to me prattle on about what I thought needed to be done. After hearing me out he offered me the job in the Extramural. As I have already said, by then I was ready to make the move.

Farreras: I didn't realize the Extramural Program, like the Clinical Research Branch, actually conducted any research itself; I thought it only administered the grant applications submitted by universities and other outside institutions.

Parloff: For a long time that was quite correct. The Branch's various sections and its Schizophrenia Center were named simply for the research areas for which grant applications were invited, processed and monitored by NIMH staff. However, the NIMH's Psychopharmacology Branch did become generally involved in the research it supported in that it provided for the use of standardized research measures that it had developed and to some degree it directly supervised the clinical trials research used by grantees in testing selected pharmacological preparations. Still later, the Clinical Research Branch became active in developing what became known as "Collaborative Research Grants." The NIMH Treatment of Depression Collaborative Program was one of the first instances where the Extramural undertook to develop uniform research designs, implemented by carefully selected independent investigators. Another unit within the Clinical Research Branch also initiated and conducted a collaborative study on the Psychobiology of Depression. That program was coordinated by Robert Hirschfeld.

Farreras: Oh, I see.

Parloff: What I'm getting to is that ultimately we broke ground in the Extramural by, in effect, "doing" our own research. We prepared a general research statement and research protocol which was widely disseminated to the research community. We thus invited grant applications to be submitted for standard NIMH review for investigator competence and the suitability of institutional facilities for participation in our long-term, large-scale collaborative effort. When I transferred to the Extramural Program I did not have in mind initiating the collaborative study. My plan had been more modest, simply to stimulate the field to undertake studies that would permit the accumulation of a credible body of knowledge. After a number of conferences with investigators it appeared that the collaborative grant model (which had originally been rejected outright by about 36 leaders in the field) did provide a promising mechanism. While my ego was clearly involved – you have only to read some of my early papers to note that I often tended to write in the first-person singular – I was wise enough soon to designate Irene Elkin to be the Coordinator of the NIMH Treatment of Depression Collaborative Program. That left me free to administer my Branch while actively participating in the early phases of what turned into a 10-year study.

It was my style of doing things in the Extramural – together with my staff – to deal with investigators in a supportive, guiding, prodding, and, as you may recognize from most of my publications, in a light and joshing fashion.

Farreras: In a sense that feels very Intramural because it's the independent investigators who are doing the research, even though it's part of the Extramural Program.

Parloff: And that takes us back to one of the questions you raised earlier concerning the possibility of competition among the Intramural Laboratories for the finite amount of funds available from the annually determined Intramural budgets. It is a tribute to Shakow that during our long period in the Intramural we were never made acutely aware of that underlying competition. I'm sure it existed, but Shakow served as our buffer and defender. He was a fighter. My job was simply to tell him what funds I thought the Section needed, and after some discussion of the subtle but important distinction between "needs" and "wants" he would attempt to meet what he viewed as our budgetary needs. Most of our weekly meetings had nothing to do with money but focused on the research we were doing. Except for the periods of great budgetary crunches, the issues of salaries and travel expenses were not significant in the Intramural.

TAPE 1, SIDE B

In contrast, in the Extramural, when we finally developed our large-scale collaborative study, the size of our budget did present a significant problem. Grantees and wanna-be grantees properly feared that our large study might drain off a lot of the funds that might otherwise have become available to them. By then many of the grantees had become personal acquaintances and even personal friends of ours. During the period 1977 –1980, when Irene and I were busily preparing the design and organizing the various components of the "pilot study" of the "Depression Collaborative," we didn't have a director of NIMH to turn to for advice and support. With the creation of the ADAMHA, Burt Brown, the former Director of NIMH, had been – you will forgive the rude expression – fired, and the NIMH remained without a director during this critical period. (Incidentally, a good source of information about the role of Bert Brown during that era might well be found in the personal papers of Julius [Julie] Segal, who had worked very closely with Bert as a speech writer and NIMH Public Information Officer; Julie's papers are now stored in the APA Archives.) It became necessary for me to deal, instead, directly with the new head of the ADAMHA, Gerry Klerman. Fortunately, we had known each other over many years. I had, for example, sometimes served as a discussant of some of his papers at professional meetings, and in the process we had grown to have great respect for each other. Gerry was an interesting guy to work with. When he learned about my plans for the Collaborative and my need for its independent funding, he graciously and startlingly delegated $1.3 million out of his own "Administrator's Account" to fund the first phase of our Collaborative Project. In that manner, we could honestly reassure NIMH grant applicants that we were not competing with them for their grant funds. I cannot stress enough the significance that became attached to the matter of being even-handed in the allocation of funds to grantees, quite independent of our own Collaborative Study. I have in mind, for example, that an unfortunate misunderstanding of the funding processes ultimately cost me my valued long-time friendship with my old friend and colleague Seymour Kety. Since that is an important illustration of the problem of actual or imagined conflicts between internal and external grant-funding interests, I wish to dwell on it for a moment before returning to talking more about the nature of the Collaborative Study. As I have indicated, Marty Katz's Branch included the Schizophrenia Center, then headed by Loren Mosher. Loren had raised the eyebrows of many investigators and colleagues by actively supporting the "Soteria House" grant, which embraced the brave but lonely philosophy that schizophrenia was not an illness but simply an alternate lifestyle. Kety, who had periodically left and returned to the NIMH, had once again resigned, this time to conduct his NIMH-grant-supported research at a Massachusetts hospital; he was deeply offended by the fact that his own grant support had been cut while Mosher's seemingly less worthy Soteria House study continued to be funded. Kety became outraged by the inequity – as he saw it – of Katz's continuing to fund a trivial "in-house" study at the expense of Kety's important research. Kety began a campaign of repeatedly telephoning Marty Katz at NIMH and ultimately began calling me at my home to appeal for the restoration of his budget cuts. Neither Marty Katz nor I could persuade him that the cuts had, in fact, been made consistent with the recommendations of the Grants Review Committees that had separately reviewed these projects. Seymour felt particularly betrayed by me, an old friend. How could I stand by and permit this preferential treatment to go on?

Later, when he (and many others) became skeptical of the results of "my" NIMH Collaborative Study of the Treatment of Depression, Kety chose to interpret this as yet another instance where the superior research efforts of independent investigators had been slighted in favor of a weaker in-house-sponsored investigation. Clearly, the thinking went, there must have been something seriously wrong with our study since it had failed to confirm – sufficiently strongly – the pharmacologists' earlier claims for the superiority of pharmacological treatment of depression. Similar concerns were raised by aficionados of cognitive behavior therapy – that its own unique potency with major depressive disorder had not been adequately revealed. The findings we had published must, therefore, be in error and would prove to be mischievous.

Farreras: Could you back up a bit and talk more about some of the reasons that prompted you to undertake what you have described as the first NIMH large-scale, collaborative study in the field of psychotherapy?

Parloff: Oh, thank you. That does indeed need some clarification. I'm sorry I got so carried away by my overly personalized tale. It is important that I give the context in which we decided it was necessary to resort to our Collaborative Study. As I said, the field was then in a period when its credibility as a treatment form worthy of continued support was under serious challenge. Health care costs had been steadily mounting and growing expenditures for psychotherapy reimbursements were increasingly viewed with a jaundiced eye. I recall at one point being asked by Gerry Klerman to assist him in formulating a response to a flat-footed challenge he had received from Jay Constantine, then Chief, Health Professional Staff Committee on Finance, U.S. Senate. Let me quote an excerpt from Constantine's goading letter to Gerry: "Based upon evaluation of the literature and testimony it appears clear to us that there are virtually no controlled clinical studies, conducted and evaluated in accordance with generally accepted scientific principles, which confirm the efficacy, safety and appropriateness of psychotherapy as it is conducted today" (Nov. 15, 1979). Klerman picked up the gauntlet, and on Feb. 27, 1980, he replied, in part, "On the whole, I agree with your general approach, which is to affirm the basic principle that federal funds be used to reimburse those treatments which are demonstrated safe and effective by scientific methods." By then, Klerman had already agreed personally to fund our nascent Collaborative Program.

Ingrid, I wish also to clarify my own thinking in finally undertaking the Collaborative. Although I had no ambition that the NIMH become the FDA of psychotherapy, I believed that the critical question was whether the therapeutic benefits associated with applying particular forms of psychotherapy to particular disorders (preferably those of significant public health concern) could be attributed to some credibility-enhancing "specific" therapeutic elements or simply to credibility-weakening "nonspecific" factors. I believed that this question had not yet been properly investigated and, as a consequence, the true "specificity" of effective psychotherapies may have been obscured. I hoped, therefore, to promote the conduct of research that would help to answer that question. After a time, I decided that it might facilitate matters if we, after consultation with a number of experts, designed and mounted that much needed, large-scale, multisite, appropriately controlled study.

We decided to study the comparative efficacy of two previously investigated and reputedly effective brief forms of psychotherapy for the treatment of major depressive disorder: Cognitive Behavior Therapy and Interpersonal Therapy. Important, too, in their selection was the fact that each represented a distinctly different theory and employed a clearly different set of psychological interventions. I believed that if we were to detect significant differences in the demonstrated efficacy of these carefully defined therapies, then the case for therapeutic "specificity" would be supported

Farreras: Could you briefly summarize the design of the Collaborative and its most important findings?

Parloff: I'll try. The NIMH Collaborative Study used a Randomized Clinical Trials model and a 3 x 4 factorial design. Specifically, identical research protocols were simultaneously carried out at George Washington University, The University of Pittsburgh, and the University of Oklahoma. Patients were randomly assigned at each of these sites to one of four "manualized" treatments: Interpersonal Therapy (IPT), Cognitive Behavioral Therapy (CBT), Imipramine Hydrochloride plus "clinical management" (IMI-CM), and a pill-placebo plus "clinical management" (PLA-CM). All treatments were planned to be 16 weeks long, with a range of 16 to 20 sessions. Of the 250 randomly assigned patients, 239 entered treatment. A total of 27 therapists provided the treatments. At each treatment site three clinical psychologist therapists administered Interpersonal Therapy, three other psychologists administered Cognitive Behavior Therapy, and three psychiatrists administered (blindly) the IMI-CM and the PLA-CM conditions. The IMI-CM condition was included primarily to provide a standard reference treatment. The PLA-CM served largely as a control for IMI-CM but also functioned as a stringent control for the psychotherapies. Our inclusion of the "clinical management" procedure grew out of our concern that it might be unethical to limit the treatment of individuals experiencing serious depressions only to an inert placebo for a full 16 weeks. To preserve the blind it was necessary to provide the manualized CM condition along with the active drug condition. To be eligible for inclusion in this study patients had to meet Research Diagnostic Criteria (Spitzer, Endicott, & Robins, 1978) for a current episode of major depressive disorder and to have a score of 14 or greater on a revised version of the 17-item Hamilton Rating Scale for Depression (HRSD) both at initial screening and again at rescreening one to two weeks later. There were, of course, additional niceties that may have slipped my mind.

Farreras: That's fine. What about the findings?

Parloff: Yes. Yes. Could we please have a drum roll? The fact is there were many findings. One of the problems with studies such as ours that go on for long periods is that additional data continue to be gathered; in our case, some modified those we initially published. Since our initially reported findings provoked criticism, Irene undertook to conduct additional analyses and reanalysis, and to use "new and better" data reduction techniques. As a result, the project continued over time to publish updated and somewhat revised findings. I estimate that Irene and her colleagues published about 40 articles, some of which reported additional research evidence. I'm afraid it has, therefore, become increasingly difficult for either friends or critics to be fully abreast of all findings. However, the fact that we have presented such a "moving target" has made it difficult for our critics ever to hit our research findings square on. Let me illustrate. The initial data analyses (Elkin, Shea, Watkins, et al., 1989) included some that were based on the entire sample, and some that were derived from the sample dichotomized according to symptom severity. In the interests of brevity, I shall risk oversimplifying some of the study's findings. To the surprise of some, data analyses based on the entire undifferentiated sample showed that patients in all four treatment conditions – placebo included – showed a significant reduction in symptoms and a significant improvement in general functioning over the course of the treatment period. There were virtually no significant differences among the three active treatments, although there was a consistent ordering of treatments at termination, with the drug doing best and the placebo doing worst, and the two psychotherapies in between but generally closer to the IMI-CM. The analyses carried out on the total sample without regard to initial severity of illness found no evidence of greater effectiveness of one of the psychotherapies over the other and no evidence that either of the psychotherapies was significantly less effective than the standard reference treatment, IMI-CM.

When analyses were based on the dichotomized sample, divided into the "more" and "less" severely depressed and functionally impaired groups, more illuminating findings were obtained. Significant differences among treatments were present only with the more severely depressed and functionally impaired group. With these patients there was strong evidence of the effectiveness of IMI-CM and some evidence of the effectiveness of IPT. In sharp contrast, for the less severely depressed and functionally impaired patients there were no significant differences found among their response levels to any of the treatments, including the placebo condition (PLA-CM).

A later reanalysis of the overall sample (not dichotomized on severity) was made using the random regression models (Gibbons et al., 1993), and these findings generally supported the earlier ones, showing clearer differences between the drug and placebo conditions and evidence of the more rapid effects of the drug. "[T]here is at best a very small difference between the CBT and IPT conditions in their rate of reducing depressive symptoms" (p. 749). At the 16-week treatment point, the drug condition was not found to be significantly superior to either of the psychotherapies. Finally, there was a considerable improvement across the 16-week course of treatment in all groups, including the placebo condition**.** However, at 16 weeks IMI-CM was no longer found to be significantly superior to the psychotherapies. In addition, there were no Treatment x Site interactions found in the RRM analyses. Watkins et al. (1993) confirmed that IMI-CM had a more rapid effect. That the RRM analyses had detected a larger number of differences among the active treatments for the more severely ill patients may have been due, in large part, to the greater power of the random regression models (Elkin, Gibbons, Shea, et al. 1995). While we were, of course, pleased to have teased out some "Treatment x Depression-Severity" effects, our satisfaction was shared neither by psychopharmacologists nor by our former Cognitive Behavior Therapist friends. I won't go so far as to call them "poor sports," but clearly they were not content that the long-awaited findings from this zillion-dollar NIMH study had brought forth so little they could view as encouraging or even credible.

Kety, along with many other skeptics, chose to believe that these offensive results must be the unfortunate consequence of a series of grotesque technical errors. There then followed several years of public controversy – data reanalyses, critiques, further heated public debates, and relentless and interminable rebuttals.

Such tortured research evidence alone rarely provides a compelling basis for changing one's firmly held convictions. In any event, I never regained Kety's friendship.

Farreras: That must have been a sad loss.

I wonder if you can help me with an organizational question? I'm trying to picture how this all fits organizationally: at that time NIMH was within ADAMHA, so you had Gerry Klerman as the head of ADAMHA... Where does the Psychotherapy and Behavioral Intervention Section fit in?

Parloff: That Section remained where it had been, within Louis Wienckowski's Branch.

Farreras: Was the Schizophrenia Center then under the Clinical Research Branch?

Parloff: Yes.

Farreras: So you're at parallel levels within this one branch?

Parloff: Well, I think the Center was at a somewhat loftier administrative level. It even published its own Journal, *The Schizophrenia Bulletin*. It was in the Branch, but it was clearly distinguished from a section. My section, however, was elevated, in 1980, to branch level and became known as the Psychosocial Treatments Research Branch. It might be helpful to you, with regard to organizational structure, to talk further with Lou Wienckowski. Have you been able to do that?

Farreras: No.

Parloff: I believe you will find him very much available. His administrative knowledge may be particularly valuable to you.

Mrs. Parloff: He lives out in Rockville.

Parloff: Yes, I can give you his telephone and all the rest. We meet periodically, and he calls me on occasion, particularly to inform me about who in our former organization has recently died. He also continues to call together former members of his organization to go out to lunch. What was originally a largish group has now dwindled because of infirmity and death. But he was a very important figure there. He'll know many of the administrative and personal details. In view of your current interests you might wish to explore with Lou whether he might be willing to convene one of our luncheon meetings at which you would be invited to ask your questions relevant to your history mission.

Farreras: Yes, that would be wonderful. Thank you.

So Klerman set aside money that wasn't interfering with money that was going to outside research.

Parloff: That was the idea.

Farreras: And it also wasn't interfering with money going to the Schizophrenia Center?

Parloff: My understanding was that the Klerman funds were quite independent. I frankly do not know for how many years he continued to provide us with this separate funding. Our Collaborative Study went on for about 10 years. He and I were long gone before the study ended. With regard to the Schizophrenia Center, I'm not sure at what point Loren was persuaded by Katz to finally take his leave. But after much travail, leave he did. He was replaced by a far less contentious member of the Center, Sam Keith, then a young psychiatrist.

Mrs. Parloff: Tuma knows a great deal about the Schizophrenia Center.

Parloff: Sein Tuma, yes, because he was Marty's assistant, his administrator, really. He'd remember.

Mrs. Parloff: He lives in Annapolis?

Parloff: Yes, but he comes in often. Sein is really a marvel about administration, a most interesting guy, very low key, very quiet. He sometimes tends to underplay his importance to the NIMH, but don't be fooled by his modesty.

Mrs. Parloff: There was another notable administrator. I'm not sure where he fits. Jack Lasky.

Parloff: Jack was the chief grants administrator. The grants administration business is another issue. As an executive grants officer he had the vast specialized expertise required to review particular grant applications and was able to match individuals possessing special qualifications. He arranged the various site visits.

While our collaborative research program had, in fact, been initiated before the Collaborative Grant was created by the NIMH, it was in place by the time the submitted grant applications were to be reviewed. A special Collaborative Review Committee had been created by then and appointed by Jack Lasky, and the review was handled much like any other grant review. In addition, since we were to be an ongoing project, Irene and I developed a standing Advisory Group to assist us. We couldn't use the term Advisory Board, as we had originally planned, because the term Board apparently is reserved for high-level administrative groups. Our group had only a limited advisory function. You know how jealous government is of its terms and designations. Our use of the Advisory Group was intended to provide us not only with valuable counsel but also some necessary "cover." In government circles one learns to "cover" oneself at all times. We must not only do what is right but perhaps more important, give every appearance of doing what is right. You don't just devise a project and plunge into it. You must first consult, hold meetings, surround – or cover – yourself with all kinds of well-recognized and highly qualified consultants. The composition of our Advisory Group was in part a political operation. It included Paul Chodoff, M.D., Sol Garfield, Ph.D., Donald Klein, M.D., Perry London, Ph.D., myself, Jeanne Phillips, Ph.D., Hans Strupp, Ph.D., and Eberhard Uhlenhuth, M.D. I had particularly insisted upon appointing Don Klein to that august group not simply because of his great stature and vast knowledge but because I anticipated that unless he was fully acquainted with the design and our implementation of it he might tend later to be a powerful critic of any unhappy results of our efforts. While he was indeed very helpful, nonetheless he did become one of the study's severest critics, particularly after its unwelcome findings were made public. Some of my most difficult activities on the project ultimately involved trying to mediate between Irene and Don. Gloria was particularly helpful there because she had edited a book he had written with another friend of ours, Paul Wender, and still enjoyed a good relationship with Don. Don, however, was not so easily mollified. He was a most formidable adversary.

Farreras: Were you in charge of designing the project, deciding what questions you were going to look at, etc.?

Parloff: We wrote the initial research protocol. We then advertised the NIMH's intent via standard announcement channels, and invited all interested researchers to attend a meeting with us at the Parklawn Building to discuss the plans and to ask questions. The announcement advised that the National Institute of Mental Health Treatment of Depression Collaborative Research Program (TDCRP) was planning to undertake its first multisite coordinated study in the field of psychotherapy research. It gave the rationale for the choice of patient population and treatment forms, and summarized the research plan for both the pilot study and the outcome study. The issues of purpose, terms of collaboration, qualifications needed, facilities required, etc., were fully described. Those investigators who were interested then completed and submitted the required forms for grant application review.

Farreras: I'm wondering whether this might not be something that the Intramural Program could have done? Could you have done that type of a project here on campus?

Parloff: Well, at one point I thought it should be done in the Intramural, and I presented the idea to Fred Goodwin, who was then in the Intramural Adult Psychiatry Branch. Incidentally, he later became the NIMH Director. So, yes, it did occur to me that that might be an easier way to go. However, I soon recognized that it would never work. Technically, it might be hard for the Extramural directly to fund an Intramural study. Also, Fred seemed far too eager to take over the project completely. He did not seem inclined to follow the protocol we had prepared. That didn't sit well with me so I decided to go back to my original plan.

Farreras: Under what Section or Lab would you have placed this project, given what was available in Intramural at the time?

Parloff: In whatever unit Goodwin then headed, but I don't think I really had quite thought through the funding problems involved in funding an Intramural project with Extramural funds. I don't believe that was technically feasible.

Mrs. Parloff: I think the collaborative feature wouldn't have worked from the Intramural or Extramural points of view.

Parloff: No. I never pushed that issue since I decided Fred was not likely to collaborate with us. He would have done the whole thing. And so I blew that off and decided we could direct this monster thing ourselves. We got a lot of administrative assistance later in creating a more appropriate collaborative research instrument. Irene's unit, within our Branch, served as sort of a headquarters for implementing the study. The sheer administration of the large project I have described became a serious strain on our limited staff. I was unable to provide Irene with adequate staffing because of periodic hiring freezes. Irene was placed in a bind because ultimately only she and a couple of other staff associates ran the whole thing. Now, I've got to explain more when I say "the whole thing." We did parcel much of it out, but the sheer work of administration and coordination of three research units, three training centers, and a data management and analysis unit that was located at the VA (C. James Klett, Ph.D., Joseph Collins, Sc.D., and Roderic Gillis) was more than a full-time assignment for Irene and her small staff. Let me give you a small example of the problems she had to deal with. In monitoring the quality of the training program it became necessary to review samples of the therapist-trainees videotapes and compare them with the ideal treatments as described in the manuals. We had to negotiate a contract with the VA Data Management and Data Analysis Center because of its long experience with managing data collected in VA clinical-trials studies. One of the elements that made our study unique was that it was the first NIMH effort to apply the clinical-trials model to psychotherapy rather than to pharmacotherapy. This involved the development of manuals for administering not only the two experimental psychotherapies but also the drug conditions – Imipramine and the pill-placebo. Further, it required the development of a manual for the standardized administration of the clinical-management procedure.

The very notion of using treatment manuals was initially greeted by psychotherapists as quite outrageous. Ingrid, I don't know if you've ever read *The Naked and the Dead*, by Norman Mailer, but there is a scene in that book that describes a bumbling general as he finds himself, almost unwittingly, mounting a massive attack. That description, in retrospect, seems to parallel our own gradual implementation of what ultimately became an enormously complicated research program. Irene and I bumbled and stumbled our way into mounting this huge assault. I started developing the project in 1977 but didn't get it funded until 1980. Just the basic preparation of each of the manuals was an enormous and time-consuming undertaking. That was the first step; we had to get them to write the manuals. There were many other steps that have escaped me now. So we finally wrote the project, and we conducted a pilot study for almost two years. The initial grant for the actual treatment study was initially for another three years. Our staff held many of the planning sessions around this very dining-room table where we are now seated. Many of our best bull sessions were held downstairs in our recreation room. Finally, Irene and I, with one assistant, Suzanne Hadley, put the initial grant application together and submitted it to the Grants Review Committee. That was about '79. And you won't believe this but our Review Committee met and turned down that initial grant submission. That experience of actually submitting our own grant application rather than simply critiquing applications of others led me to formulate yet another in the list of mythical "Parlovian Laws," namely, in the process of writing a research application one immediately loses about 10 to 15 IQ points. We had been so preoccupied with getting the details of the "final" treatment study right, we neglected to include in our application a description of our plans for the prior and prerequisite pilot study. While we kept alluding to the pilot study we hadn't actually included it in the application package. Actually, the committee merely deferred action on our submission until the next review cycle rather than outright disapproving it as some had threatened. Nonetheless, it delayed matters considerably. I remember the humiliation. And we were the grant experts! We were the people that other applicants consulted with about their grant submissions because we knew how those things were done. It was a humbling experience. Just terrible. So the Collaborative didn't get funded until 1980. And I retired in 1983. In retrospect that seems exceptionally wise. But the weight of the project fell crushingly on Irene. One of the great people who worked with her was Tracie Shea.

Farreras: Tracie Shea, who's at Brown now?

Parloff: Yes, and she completed and published a tremendous follow-up study on the Collaborative while at Brown (Shea, Elkin, Imber, et al., 1992). Well, some of the implications of that paper were among the most sobering that came out of the entire study. The durability of the changes the study had so carefully analyzed at 6, 12, and 18 months after treatment had ended turned out to be extraordinarily modest. Of those patients who entered treatment and provided follow-up data, the percent who recovered and remained well over the follow-up period did not differ significantly among the four treatments: 30% for cognitive behavior therapy, 26% for interpersonal therapy, 19% for Imipramine plus CM, and 19% for placebo plus CM. Tracie and her colleagues concluded that the major finding of this study is that a period of 16 weeks of these specific forms of treatment is not sufficient for most patients to achieve full recovery and lasting remission.

These findings did little to reassure health-care managers. They continued to cut back on their payments for psychotherapy and imposed arbitrary time limits on what they considered reasonable periods of treatment. Perhaps one of the benefits to the field, however, was that the unfettered expansion of the psychotherapies slowed precipitously. In the context of this and other outcome studies about the treatment of depression the field has had the opportunity to learn a great deal. Most notably, it has simply declared victory and turned its attention away from the specificity-nonspecificity issue to a search for "empirically supported therapies." The NIMH, in turn, appears to have shifted its attention from supporting rigorous clinical-trials studies to urging the use of "hybrid" designs that combine efficacy and effectiveness studies. The preferred locus of research has also shifted from the laboratory to the community and the community-based "Practice Research Network Model." Research is now to be conducted in clinic settings on self-selected, loosely defined disorders suffered by ill-defined patients/clients treated by the fortuitously available but not necessarily best-qualified psychotherapists. Nonetheless, I prefer to believe that our venture into clinical trials may yet have contributed some useful experience and knowledge that is available to practitioners.

Farreras: If practitioners read the research at all.

Parloff: Well, of course. But, seriously, with the plethora of research evidence, practitioners cannot be expected to read, integrate, weigh, and "prudently" interpret our burgeoning research evidence, much of which may appear to be contradictory. So the field continues to seek mechanisms aimed at assisting practitioners to make their pragmatic decisions.

Farreras: Matarazzo has complained about that, too.

Parloff: Oh, yes. The fact remains, I think, that the field's researchers have profited greatly from the opportunity to learn more about the treatment of serious depressions and thoughtfully to critique the premises and findings of this huge effort. Perhaps one of the less fortunate earlier effects, however, was that for a while NIMH Grant Review Committees became overly impressed with the potential value of the random-clinical-trial model applied to psychotherapy research. As a result, and for a number of years immediately after the initial Collaborative results appeared, that model was unfortunately made a prerequisite by NIMH grant reviewers for conducting psychotherapy outcome studies.

Farreras: Why do you believe that was unfortunate?

Parloff: Oh, for a number of reasons. The RCT model itself was an excessively burdensome and inordinately expensive one for university investigators routinely to undertake. Practitioners, in turn, dismissively observed that any findings derived from mere "laboratory" settings could not possibly be usefully generalized to naturalistic settings where patients tended to have multiple problems and would be treated by each therapist's own idiosyncratic and infinitely more flexible and sensitive clinical interventions. This often repeated and possibly even correct belief remains, as I have already suggested, quite independent of the original theoretical purpose of our study. However, once the equivocal findings of our rigorous efficacy research had been published, embattled psychotherapy service providers and their representatives made it clear that they wished researchers to provide far clearer evidence of what they themselves believed to be psychotherapy's clinically well-established "effectiveness." The specificity-nonspecificity hypothesis with which we had been concerned was not an issue that particularly grabbed them. While scientific questions must, of course, ultimately be pursued, the field of psychotherapy must first be preserved. It is not sufficient to show that a treatment can work in the laboratory (i.e., efficacy research). We must also show that it works in actual practice (i.e., effectiveness research). Unfortunately, the currently preferred effectiveness research occurs in undefined practice with heterogeneous patients, and primary data are often based on retrospective reports of surveyed clients. In my view such research can give only pseudo assurance to clinicians and patients. But, what the hell, in psychotherapy practice that's always seemed to suffice.

You will note that I have been freely giving my own personal and probably biased opinions. It is, therefore, necessary that I exclude Irene from them. The fact is that she and I have always held somewhat different views of the major purposes of the study. That's probably due to the fact that my role was primarily to initiate it, and hers was to conduct and "coordinate" it (Parloff & Elkin, 1992).

Farreras: Why was interpersonal and cognitive-behavioral therapy the only psychotherapies used? Was it because of cost, because of…?

Parloff: The primary reason for selecting these forms of therapy was that the developers of each had agreed to develop research treatment manuals for purposes of training therapists to pre-specified levels of mastery and for ease of the treatment's replication by others. While we initially thought that the interpersonal therapy as described by Weissman and Klerman was psychodynamic and modeled after that of Harry Stack Sullivan's notions of interpersonal psychotherapy, it soon became apparent that it was not. What mattered more to us, however, was that Aaron T. Beck's cognitive therapy and the Weissman-Klerman interpersonal therapy could be clearly differentiated from each other not only in terms of theory but in terms of actual techniques employed. As you may know, Steve Hollon developed and used an instrument which demonstrated that the interventions employed by each were clearly different. We were not primarily concerned with comparing an echt psychodynamic therapy with a cognitive one. You will recall that I was concerned primarily with testing the specificity-nonspecificity hypothesis. Any form of therapy that claimed specificity and permitted its rigorous testing would do. I thought if we got some therapies that were remarkably different in theory and in practice, and if they both treated the same kinds of patients, and if you got differences in outcomes, that might give us some leverage in answering our question. Then we would be in a good position to try to pursue the question of what the actual processes and mechanisms were that effected these observed changes. Incidentally, Ingrid, I was not about to risk – even if they would let me – testing the efficacy of psychoanalysis, which then represented "the third rail" in our field. Nor was I going to test any other widely practiced form of psychotherapy. At the time we began planning our study (1977) neither Beck's form of cognitive therapy nor the specialized form of interpersonal therapy available to us was widely practiced. I didn't think that if we got negative findings that we would be threatening the livelihoods of a significant number of practitioners. Mainstream psychotherapy would go on unscathed.

Farreras: There are many behavioral components to Beck's cognitive therapy as well.

Parloff: Perhaps you are referring to the recent work of Enrico Jones, who claimed that from his vantage point both of our forms of psychotherapy appear to be more behavioral than dynamic. I will grant that interpersonal therapy, the way that Weissman and Klerman devised it, was not dynamic in the Sullivanian sense.

Farreras: So it wasn't meant to be a study about which psychotherapy worked best…?

Parloff: If pressed, I would say I was not particularly interested in conducting a "horse race." But the little known truth of the matter is that I did, fortuitously, have a horse in the race. A member of my family was then suffering a severe depression and at my recommendation he had chosen to enter cognitive behavior therapy with a member of Tim Beck's staff. (The family member was not a subject in the study.) For purposes of my inquiry it would, of course, have been useful if either of the therapies had proven to be clearly superior to the other.

TAPE 2, SIDE A

Farreras: You didn't have any psychotherapy controls in addition to the drug and drug-placebo conditions, did you?

Parloff: We had no psychotherapy placebo groups because, as I mentioned earlier, I didn't think it was feasible to produce a meaningful placebo for psychotherapy. Let me explain why. I believe that a placebo control must satisfy at least two major requirements: (a) all of the hypothesized specific active elements claimed to be essential to the examined active psychotherapy must be absent from the psychotherapy placebo, and (b) to control for the powerful nonspecific "suggestion" elements associated with all active therapies, the therapist must be willing to communicate to the patient his/her own faith and high expectations regarding the efficacy of the "placebo" psychotherapy. These conditions are difficult, if not impossible, to establish in a psychotherapy placebo. We were unable to come up with placebos suitable for cognitive therapy and interpersonal therapy that met these two conditions. None of the so-called placebo conditions we had either reviewed or devised satisfactorily omitted the putative active elements of the experimental psychotherapies. Our placebo conditions were all judged to be either too fanciful, implausible, unethical, or, in fact, simply versions of existing forms of treatment that had earlier been classed as "active" psychotherapies – relaxation therapy, bibliotherapy, etc. In addition, we found it impossible to create a "placebo" psychotherapy that our participating therapists would be willing to present to their patients with the necessary degree of actual or simulated confidence and enthusiasm for the full 16-week treatment period. In psychotherapy research, unlike drug efficacy research, the therapist cannot possibly be kept "blind" to the form of therapy he or she is actively attempting to deliver. I once attempted to detail, at some length, my problems with the psychotherapy placebo issue (Parloff, 1986). I'm afraid that my effort was not particularly well received.

Farreras: And yet a lot of the studies now seem to focus on that placebo or these nonspecific factors that Frank was talking about as probably being the core active ingredients in all of these theoretical psychotherapies.

Parloff: Absolutely. Jerry's [Frank] conception of "demoralization" and its treatment as the central issue in psychotherapy is well known and respected. However, many therapist-researchers believe that the conditions of expectation, hope, confidence in therapist, etc., which he has eloquently described are also prerequisite to establishing and maintaining the so-called "necessary and sufficient" condition of the effective therapeutic alliance. The specificity-nonspecificity problem has clearly not been set to rest,

Farreras: What about those studies that compared a particular type of psychotherapy with a wait-listed control, or where you'd have people come in an talk in a group setting but would not actually be implementing any theoretically-driven type of treatment?

Parloff: Strictly speaking, the wait-list control is intended to control for the effects of the "passage of time." Its value, however, may be attenuated, particularly in randomized treatment studies, where patients are routinely told that their admission to the "actual" treatment had unfortunately been delayed for a time, but that they would later be admitted to one of the "real" treatments. This may provide a mind-set that serves to inhibit the patients' "improving" during the "no-treatment" period. Change, they may believe, must await their receiving the "real" treatment. If, however, the patients had been oriented to expect that the group-talk setting that you mentioned would be helpful, then of course the condition might qualify as a placebo, particularly for an experimental leaderless group therapy. In the early days, when I worked with Jerry Frank, we used wait-list controls all the time. But when we got to understand some of the peculiar ways people tended to interpret the enforced "wait" period, ultimately we abandoned it.

Farreras: I remember reading some of the criticisms regarding the reliance on the Hamilton. That if you're giving meds the Hamilton would naturally show improvement much sooner than if you were using the BDI, for example, but that if you were using cognitive-behavioral interventions, the Beck, which looks at cognitive improvement rather than vegetative or physiological improvement, would show more improvement. It seemed that if you were a psychologist, usually using the BDI, your theoretical allegiance would show that therapy is better than or equal to medications. But if you were a psychiatrist, usually using the Hamilton, then the meds would always seem to do either better than or equally as well as psychotherapy. How would you tease that apart?

Parloff: What you said correctly captures some of the criticisms. But the study did not rely on the Hamilton instrument alone. Our initial outcome analyses were based on four scales. To represent the clinical evaluator's perspective we used: (a) the 17-item Hamilton Rating Scale of Depression (HRSD), and (b) the Global Assessment Scale (GAS) to assess the individual's general level of functioning. In addition, to represent the patient's perspective, we included (c) the Beck Depression Inventory, and (d) the Hopkins Symptom Checklist-90 Total Score. The HSRD and the BDI were, at the time, the accepted instruments for measuring depression symptoms and had been successfully used in other studies comparing either interpersonal therapy or cognitive behavior therapy with tricyclic drugs. Over time many criticisms have been made of our selection of instruments and we had been forewarned of Beck's reservations.

Farreras: So you knew about it before you did all this?

Parloff: Well, as I've said, Irene bashed on "irregardless." The first paper regarding the study appeared in 1985. This delay reflects some of the problems that inhere in doing research by a committee. It wasn't until '89 that she published the first data paper reporting our results. Unfortunately, one of her 1986 presentations at a professional meeting was prematurely picked up by the *New York Times,* and that created a great uproar among the pharmacologists and, of course, the cognitive behavior therapists. However, to respond more directly to the criticism you cited, I wish to remind you of the findings revealed in the more powerful random regression analysis undertaken by Elkin et al. (1995). A post hoc analysis between pairs of treatments revealed that the drug condition remained significantly superior to all of the other treatments in reducing depressive symptoms on both the HRSD <u>and on the BDI</u>. Thus, the findings that were disappointing to the cognitive therapists were found even on Beck's own preferred instrument.

Farreras: What about prophylactic effects?

Parloff: Tracie's later paper (Shea et al., 1992) dealt fully with that, and it was anything but reassuring. It did, however, usefully suggest to other researchers a need either to lengthen the period of treatment or to have patients plan to reenter treatment briefly on a periodic basis.

Mrs. Parloff: Was that based on the severe part of the spectrum?

Parloff: Well, the analyses included both the less and more severe groups. While our findings continued to be challenged by cognitive-behavioral therapists, we never got much feedback from the interpersonal people.

Farreras: Well, how many people actually do that type of therapy..?

Parloff: You're quite right. There are many fewer interpersonal therapists. Well, as I told you earlier, I hadn't anticipated that cognitive behavior therapy would become so popular. Beck has extended his treatment to include not only depression but also anxiety and other psychological problems. Tim made it clear from the time he first "testified" before our first Grant Review Committee that he didn't believe we were prepared to perform his cognitive therapy "appropriately" in our study. Oy, and I had thought he was going to be one of our star witnesses. I remember my amazement, when he began with this fast opener: "Before you begin the questioning, I want to make one thing perfectly clear. I think the NIMH Treatment of Depression study's use of cognitive behavior therapy is not likely to represent my treatment approach adequately." Thank you, Dr. Beck. Fortunately, when he tried to respond to the reviewers' questions it became abundantly clear that he was being overly defensive and was not sufficiently familiar with either our carefully drawn training plans or even the basic research design. His unwarranted fearfulness became so apparent that the committee appeared to dismiss his expressed concerns. When the study was finally approved and funded he essentially dissociated himself from it, except to conduct the initial training phase, namely, the seminars used in training the CBT therapists. Some of his best-qualified colleagues did provide the supervisory training phase and the assessment of trainee competence.

Farreras: But the [University of Pennsylvania] was not one of the sites involved in the project was it?

Parloff: No. Beck's program at Pennsylvania was not one of the treatment or training sites. But, as I have mentioned, Beck did provide the initial phase training seminars in cognitive behavior therapy (CBT) at his university to our sample of candidates to be trained in CBT. These trainees then went on to the Practicum and Supervision training phase, which took place in each therapist's "back home setting." The CBT Practicum and Supervision training program per se was conducted by Brian Shaw, Ph.D., and T. Michael Vallis, Ph.D., who were CBT trainers fully acceptable to Beck.

Farreras: Wasn't one of the criticisms about the study that you didn't have the experts of each form of therapy actually doing the therapy? Because otherwise therapies might not turn out to be as effective because they didn't have their best therapists representing them?

Parloff: Yes, indeed. That was an often repeated complaint. From the very beginning of the study we fully recognized that it was crucial that our therapists fully master the particular form of treatment that they were to represent in the study. We therefore instituted rigorous and lengthy training programs for each of the psychotherapies and, incidentally, also for the drug and clinical-management conditions. We hoped to train our therapists to achieve a high level of mastery and "expertise." Since this is such a critical issue I'll review it in some detail. All trainee candidates first had to have completed at least two years of full-time clinical work following completion of their professional training. For psychologists this involved receipt of the Ph.D. and completion of their clinical internship. For psychiatrists, it required the M.D. degree and completion of psychiatric residency. All candidates must have treated at least 10 depressed patients. IPT candidates had to have previous training in psychodynamically oriented settings. CB therapists were to have had some cognitive and/or behavioral background, and the pharmacotherapists' prior training had to include "a considerable emphasis on psychotropic drug treatment." As I have already indicated, each training program included two phases: first an intensive didactic phase, varying in length from two days for pharmacotherapy to two weeks for CB therapy, and second a practicum and supervision phase. It is important to note that the latter lasted between 12 and 19 months. The length varied with each candidate's need to reach a predetermined criterion level of mastery and competence in performing the particular treatment form. Over the total training period seven therapists had to be replaced, four of them for reason of having failed to achieve certifiability. These initial training sessions were conducted by the previously named training staffs for IPT, CBT and pharmacotherapy. The IPT supervision was also conducted by Myrna Weissman, Ph.D., Eve Chevron, M.S., and Bruce Rounsaville, M.D., and the drug, placebo and clinical-management training was performed by Jan A. Fawcett, M.D., and Phillip Epstein, M.D. As I said, the CBT supervision was provided by Brian Shaw, Ph. D., and T. Michael Vallis, Ph.D. No one has seriously challenged the expertise of these trainers and supervisors.

The integrity of each of the active treatments, including the drug and clinical management, was fully tested and reported in the publication by Hill, O'Grady, and Elkin (1992). We were very rigorous about that. By the time our therapists had completed this lengthy and demanding training I thought it was fair to call them "experts."

I believe that fair-minded reviewers will accept the evidence of our success in bringing our carefully selected therapists to levels that fully represented the particular form of therapy they provided in the study. Nonetheless, I acknowledge that the unequivocal assessment of the vague construct therapist "expertise" has yet to be made definitively by anyone.

Farreras: Did Beck complain after the results came out?

Parloff: Absolutely. Our findings did not confirm those earlier reported by his cognitive- behavior-therapy-friendly researchers.

Farreras: What does "clinical management" consist of exactly?

Parloff: The clinical-management manual sought to provide guidelines to clinicians for providing support and encouragement to patients and also for sparingly giving direct advice, when deemed clinically necessary. In effect it approximated a "minimal supportive therapy" condition. Note, too, that the drug-placebo condition served as a control both for expectations due to administration of a drug and for contact with a caring supportive therapist.

Farreras: Well, we can stop here unless there's something else you'd like to add….

Mrs. Parloff: I have the telephone numbers of Sein Tuma and Lou Wienckowski.

Farreras: Oh, that'll be very helpful, thank you.

Parloff: I would again urge that you speak with Sein. He is one of the nicest guys I've ever met. I'm sure you will find him very helpful.

Farreras: I will. Thank you both again for all of your help.

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