Dr. Marty Katz

March 29, 2001

NIMH interview with Dr. Marty Katz conducted on March 29, 2001. The interviewer is Dr. Wade Pickren. Dr. Stan Snyder is also a part of this interview.

Pickren: I'd like you talk about how you came to work for NIMH.

Katz: It was fortuitous because I was in Texas on my first job after my Ph.D. was, as a postdoc fellowship at this Texas women's university.

Pickren: In Denton.

Katz: In Denton, yeah. And the dean of that school was a professor of nutrition. She was one of the few experts in the country on nutrition, and she had this large grant to do research with vitamin C. And I got involved with a project that I wasn't too keen about, but it was part of my job to do things like that, where we were going to study the actions of vitamin C on mental functioning of both children and older people, so we set up a project in the schools to do that, and we could actually do something like measuring the amount of ascorbic acid in the blood of kids and relate that to their mental performance in school. Well, it was kind of a strange study, and I had no idea that it would be successful at all. I didn't think it would be. But it turned out to be effective. It picked up kids who were nutritionally under standards and showed that their performance mentally was below that of

other kids. And then when you lifted them up with this orange juice program that they had over the course of a year, they approximated what the other kids had. And the point was about vitamin C – this is in great detail – but vitamin C, the idea was that if you had the minimum amount in your system, that no additional amounts would make any difference. So the study was designed sort of "double-blind," that all the kids received orange juice to fortify their vitamin C, but only those kids who were low in ascorbic acid we could watch climb up, and that's exactly what happened. Those kids climbed up and the other kids stayed where they were. Well, the reason I go into this is because of this very interesting study. Anyway, I met Jonathan Cole at a meeting down there in Texas, and we got into this discussion. He was taking over this drug program and he got a big kick out of this double-blind vitamin C study because it fit very much what they were going to be confronting in their own work. Anyway, that was an incident. And then I went to work for the VA a few months later to do research on psychotherapy, which was, I thought, a prime, the most important thing I wanted to do at that time, you know. I came to Washington, and once here, worked at that for a year with the laboratory I mentioned to you, this neuropsychiatric laboratory, which was headed up by Morrie Parloff, who was an expert in quantitative measurement of psychopathology. And so I learned a lot that year, but I decided that was not the way I wanted to involve myself with research in

the future, and I got out of that job. And just as I was getting out of the job, it turned out Jonathan was now head of psychopharmacology, and he invited me out there to look at their program because he was recruiting people to deal with various aspects of this new psychopharmacology.

Pickren: But that branch had just been established?

Katz: Just been established.

Pickren: Fifty-six or so?

Katz: Fifty-six, and it was called the Psychopharmacology Service Center. And it was the extramural side of the psychopharmacology program. They had hired Joel Elkes to run the intramural, which was going to be housed at St. Elizabeth's Hospital at that time. So it was a major effort to promote the revolutionary drugs for mental disorders. And in this small group of people that Jonathan recruited was a senior pharmacologist – I can't remember his name – Jeff Carr [sp.], a wonderful guy, former chair at Maryland, I think; a psychologist who Stan might know, Sherm Ross, Dean Clyde, who was a quantitative type; and Sy Fisher, who was just joining them from North Carolina, who was a social psychologist. He was assembling a really mixed group of people to help run this place and to help develop these programs, because his mission was to stimulate research in psychopharmacology and to provide the funds for conducting these projects around the country.

Pickren: So this was an attempt, then, since it was intramural, to kind of reach out

in other places, not just within NIMH, but in other sites.

Katz: Right.

Pickren: What kinds of place were targeted? Were they medical centers or...

Katz: Well, it was a broad-based program. These were new, exciting drugs, and the question was, you needed places to study the basic aspects of these drugs, in other words, how they were developed, how we would develop new ones, basic research on their mechanisms of action. That was at one level. And at the other level was the applied or clinical level, which, there was a need to evaluate these drugs in the treatment of various mental disorders. They knew that they were effective for schizophrenia because the whole thing started with chlorpromazine. But, for example, they didn't know how effective it was in acute schizophrenia, so that was going to be one major area to go into. At the same time, you know, we knew lithium had just been uncovered, and in the early '50s, actually the mid-'50s, I think it was the late '50s in which the first article by Kuhn, the discoverer of imipramine, the first really big antidepressant... There were other antidepressants uncovered; they didn't turn out to have the staying power that imipramine would. That's where this, we mentioned that Nathan Kline came in. He was a great promoter of these kinds of things and was good at talking Congress into providing money for them.
But the main thrust there was – well, there are going to be a couple of main thrusts there. One was that the kind of study that had to be

developed to assess the clinical efficacy of these drugs would have to be a very large study and would have to involve a very large number of patients, it would have to involve several hospitals, different types, state hospitals, private hospitals. It required a large sample to get, to determine how representative or how general this effect was: was it just for a certain kind of patient, was it in a certain kind of setting, and so forth. So those big studies would cost a lot of money, and they would involve a lot of work on the part of staff in organizing that kind of effort. And Jonathan Cole was up to both. In other words, he hired staff to help design the study, and then he hired people to coordinate that study. So it's the study that Jerry Klerman, for example, got his start in big-time research, you might say. He was still fresh out of his residency, I think, at the time. And he took over coordinating that study for Jonathan, and I was recruited to help develop some methodology for it, which, I worked on the long-term actions of the drugs like effects on adjustment to community. I don't know if you heard about those, but the Katz [Social] Adjustment Scales came out of that. And then I remember Sol Goldberg was eventually hired to be the, to help Klerman run the study across these hospitals. But first large amounts of money had to be generated to fund them, and my first job was as executive secretary of this advisory committee, which was made up of the leading figures in the field, people like Lou Goodman in pharmacology, who's the author of the main textbook in clinical

pharmacology in the country; Seymour Kety; Klein; Joe Zubin from psychology; Sam Greenhouse from statistics; Mort Kramer from biometrics; Heinz Lehmann, the man who introduced chlorpromazine to North America.

Pickren: How do you spell his name?

Katz: L-e-h-m-a-n-n, a very famous man in psychopharmacology. So just the – this was the class of the field, and they were going to determine whether and how this money would get awarded. Ralph Girard [sp.] was the chairman of that committee and I was his executive secretary.

Pickren: So this was a group of kind of in-house and out-of-house experts who served...

Katz: The in-house was not relevant. It was maybe 10 to 12 experts from around the country, the leading people in their field.

Pickren: Typical study-section arrangement, then.

Katz: Sort of, except that they were supposed to be above study section in the sense that they could think in visionary terms. In other words, this was not just a matter of the quality of the project, which was very important. This was a matter of whether spending umpteen million dollars on a program like this was a smart thing to do. In other words, you could take that same money and give it to researchers in different places. Why put it all into this one big project? And that kind of philosophical debate went on for a year or two. I mean, I sat through those things. It was a fascinating

debate, and for a while we thought we would never get it really through the committee because we were both working with the committee, and, on the other hand, we would design the study, so the committee had to endorse the design and then the support of it. So we had quite a battle on our hands, and I'd say Jonathan's leadership carried the day on that. And the reason I say that is because I consider most of what I learned about administration and science and clinical research, what I learned working with him, because I would use a lot of these lessons that I got throughout – I think I was in my twenties when I took over that committee – that my future work, you know. So it was quite an experience for me. I really got a tremendous kick out of it. And then, you know, meeting people in psychology like Joe Zubin and Howard Hunt and Gardner Lindsey. These were quality people, and for a young psychologist, that was heady water, you know, to be involved with these people. And, of course, they treated you with tremendous respect because they could only see dollar signs on your forehead every time you walked in. So they were very respectful even though you were wondering why, but of course it was your own – you figured it must be your brilliance.

Pickren: Your own charisma.

Katz: Yeah. Your own charisma. So it was nice. But I worked with Joe Zubin a lot over those years because he was the most clinical of that lot, and he was... I think the field respects him for what he accomplished, but I

always sort of perceived him as the essence of a real professor. He was more the professor than a researcher, more than a hands-on guy. But he could see ahead, and he was, in a way, responsible for a lot of stuff in psychology, I think, that he doesn't get credit for.

Snyder: For a long time in New York City Psychiatric areas.

Katz: I know his history very well. We even published together.

Snyder: Who was the partner he had? What was his name?

Katz: Burdock, Gene Burdock?

Snyder: No, later.

Katz: You see, Joe Zubin created the Biometrics Research Laboratory at Columbia, New York State Psychiatric Institute back... Let's see, he was in his mid-fifties, so it has to be 40 or 50 years ago. And in that group he had Bob Spitzer, who was a young psychiatrist, and he had Jean Endicott, who was, still doing very good work 40 years later. And Gene Burdock who was more like a rating-scale developer, and Sam Sutton.

Pickren: Sutton.

Katz: Sutton is…Most of them are gone. But Salzinger, who was a behavioral psychologist, excellent guy.

Snyder: Still around.

Katz: An array of people. Like Sutton was the psychophysiologist, and Salzinger was the behavioral psychologist, and Endicott was the clinical researcher. So you had all these people. And the reason I say his impact

was great was because he was one of the promoters, for example, of the Standardized Interview for Diagnosis, which is a very, took a great deal of effort to create. He didn't do the... He led... He said this was a great thing to do, and then he left it for Spitzer and Endicott to create it. He wasn't up to dirtying his hands too much. But he had the vision that the only way we're going to get psychopathology on an objective, scientific basis is to begin to standardize our ways of collecting data, then to quantitate the aspects of psychopathology. And so he had this vision of what psychopathology should be, much like Eysenck in Britain.

Snyder: And you were learning from this as well.

Katz: I was learning from this. I maintained a lot of contact with him because I started to work at that point on the whole issue of diagnosis and classification while I was working in that executive position, and eventually, in a year and a half or two years, I worked my way out of that secretary job to work on the Claridge study, the methodology, but also to work on the diagnosis and classification issue, because in a way it was almost like the way they were building a science of psychopathology in clinical research. We didn't have any... We didn't know at that point – this is late '50s – how do we evaluate a treatment? Suppose you had a new treatment in this field. How would you actually go about testing it? It seems like a simple question now, but then there were no models for that. That's what was being developed in this Claridge study. We had a

new drug, we were going to compare it against other drugs, and we were going to compare it against placebo. What kind of instruments would we use to evaluate its effects? All these were – things were lying around because people like Morris Parloff and J.R. Wittenborn had created these instruments back in the '40s, but they were never used and nobody ever saw any real reason for them. So slowly this kind of model had to be put together.

And then, of course, as you went down the problems of creating that little science, you say, what's most basic to this field? How are we going to work? How are we going to select patients for this treatment? How are we going to select schizophrenics? We have to have a set of criteria that are objectified and that a lot of people can use to do this, which meant we had to develop a classification system that could be used in research, and that was different than what could be used every day in the clinic, because every day in the clinic, they could use any sloppy system they wanted because as long as the doctors knew the way they were going to treat these people, it didn't make much difference. But the science, we had to establish that these were really schizophrenics, these were really depression.

So this whole problem of diagnosis and classification, things that old Joe Zubin had been working with for many years and putting out papers in all of these fields, but the stuff never took hold because there was no reason

for it to take hold. There weren't really any effective treatments. There wasn't anything to test. You follow me?

Snyder: Yes.

Katz: Yeah. It was a very interesting thing. In Britain, they took that science a lot more seriously. There were people around like Eysenck and the original British group that were very keen on it. They would take patients into the clinic and study them for two weeks for all kinds of things just to get a diagnosis, even though they didn't know what to do after they got the diagnosis. But they knew that this was something, there was such a thing as a science. We never had it in this country. And Zubin might have been maybe one of the few that promoted the science. So he was – I used to think he was an old fuddy-duddy at that time, being…. this is really important, but I learned to appreciate what he was doing after a while. And certainly when we were confronted with that collaborative study and how to develop it, his ideas began to take a little...

Pickren: You began to take those ideas and apply them as you developed, if you will, the methodology, the criteria, etc., for the collaborative study?

Katz: Right.

Pickren: Now, is this collaborative study a multi-site study so that you were actually enlisting medical centers around the country?

Katz: No. The first study we're talking about. The first study was not my study. I was sort of in the middle of it in terms of it getting through this

committee and all that.

Pickren: But once you got beyond that.

Katz: Once I got beyond that, that study was going on now across eight to 10, maybe nine sites, and the sites were like, three of them were state hospitals, three of them were private hospitals, three of them were university hospitals. The idea was to get a range so that you could have a range of patients. And they set down criteria for schizophrenia. But they hadn't really solved the classification problem. They were doing as well as they could at that time with diagnosis. They made certain it was acute schizophrenia that they were after and they could make up criteria for that and get that. But the whole general problem of diagnosis was involved in that, and they weren't ready to deal with that. But as part of the foresight of that group, I was given the mission to develop that, because they knew they were going to need this in the future, that somehow we would have to develop a research system that we could depend on for classification. And I had published a little bit back in those days, even, on diagnosis across Britain and across the U.S. and perceptual differences among psychiatrists. So I was in touch with the problem and I worked a little with Joe Zubin on that and so forth, and the British group that we had at the _____ at that time.

In the mid-'60s – so this would be the middle of my career with the psychopharmacology – I was given the assignment to create this national

conference on diagnosis, and that was, turned out to be a major phenomenon. There's a book there – I'll drag it out for you. It's called *The Role and Methodology of Classification in Psychopathology*. I was working in psychopharmacology. This had to do with psychopharmacology, but it was basic to the whole area of clinical research. At the time, it was aimed at sort of clarifying the various roles, in a way – just as the title says – that diagnosis had in dealing with the mental…practical role of just being able to tell at the end of the year how many schizophrenics have passed through this facility. And then it had a certain, it carried with it certain notions about treatments even though they weren't established by hard evidence that if the patient had this, he got this kind of treatment. Remember, we're talking about the early '60s. And then you went on to the requirements of epidemiology, which was to report on how much schizophrenia we have in the country or the world, or how much depression. You needed a stable system for that that might be different than the one you have every day in the clinic.

Snyder: So DSM was not helpful here.

Katz: DSM was a mess. It was the problem. DSM was the problem because DSM was a system put together by committees and in this country had heavily biased psychodynamic terms, and in I think… had a lot to do with that. But it had nothing to do with setting up objective criteria. Anybody could read that thing the way they wanted. I don't want to, you know...

Snyder: I had copies of those original DSM.

Katz: Yeah. It's a very interesting system and it made them look like they knew what they were doing, but it really wasn't useful for anything except for everyday practice maybe. That was the thing. And it bowed in the direction of psychoanalysis, which was *the* leading notion at that time. So we needed one for research. That was our main orientation, although we went through all of this, and that was kind of like an educational tour through the field because you became aware that epidemiologists need one kind of system and practicing clinicians need another, but research needed one that would stand on its own scientific feet if it was going to be used for large studies, if it was going to be used in research. So our job was to sort of clean up that area a little bit. And then that conference, if you look at it, it's really a remarkable thing. They had – the people there were the people that created their own systems. They had the statisticians, who made certain demands on systems. They had these researchers like, you know, you may remember Roy Grinker, who was a leading figure in depression field, and he had his own system for doing this. And Morris Parloff has his system for doing this with his rating scales. And the whole history of classification in that field, and there were some beautiful papers.

Snyder: Now, was this an NIMH-sponsored conference?

Katz: Yes. Nobody else could pay for that.

Snyder: So, that was the first – as you say, it was the first, perhaps, of major

conference classification.

Katz: I think so, yeah, yeah.

Pickren: And did it lead to DSM-I?

Snyder: In ‘52, or so, DSM-II or -III.

Katz: Yeah. It goes on. You see, the history of that is, now, there’s this big conference, and it reviews all the extant typologies and assesses their scientific qualifications. And it goes over the history. The whole story is in there. And then there are certain recommendations made about what you can do in the future. It looks like I came at it with… It looks like, in research, you’re going to have to develop your own system because you can’t rely on any of this other junk. But there were some very good systems and, you know, for limited purposes, all of limited purposes. Well, that was that. At the same time that that was going on, the work with the U.S.-U.K. – there was the U.S.-U.K. study of diagnosis in New York and London. It all came out of this effort because we had Mort Kramer involved in it, too, who was head of biometrics at NIMH with Zubin again and Polatin along with John Wing from London and John Cooper. And there the issue was the British are diagnosing more depressions than the U.S. is, and the U.S. is diagnosing more schizophrenics than the British are. Is it true that there are more schizophrenics in the U.S. than Britain, and vice versa? And everybody said, of course, that’s ridiculous.

Pickren: Epidemiological type questions. Was Bruce Durham in this at all?

Katz: He was on another kick alongside it, but he didn't have much to do with that.

Pickren: With this particular group.

Katz: Right.

Snyder: So even though you're in the Psychopharm Branch because you were trying to understand, I guess, treatment effects of these various drugs, it led, then, to backwards, kind to the question of, okay, what in the hell diagnosis are we actually trying to treat here.

Pickren: What does diagnosis…?

Snyder: Yeah.

Katz: And how do we put together these... You know, we had a mission. You're going to have to evaluate these treatments, you're going to have to evaluate the new treatments, and you've got to develop models for that. How are you going to do these evaluations?

Snyder: So these drugs came on the scene in a period really of diagnostic confusion.

Katz: Exactly.

Snyder: Or I guess they would say nosological confusion.

Katz: Exactly. Clinical research was a very weak field. I mentioned to you that biological psychiatry was primitive, so you didn't have a science there waiting for these treatments to enter, like in other areas of medicine. You have a model; you can get a new treatment tested. Not that they worked

out their problems, but certainly we had nothing in this field. So these were sort of the Jonathan Cole collaborative. The first collaborative on acute schizophrenia was a pioneer study, and they got an award for it. Then the antidepressants came along. If you talk to Al Raskin, he was involved in collaborative studies of that treatment, for example, of those treatments. And then there would be another study on chronic schizophrenia. So these collaborative studies were going on, spurred by the branch. You see one very interesting thing about basic versus applied or clinical research. You don't have to do too much in the basic field to spur on scientists. They know what they're doing out there, you know. You're not going to tell basic scientists what they're doing now. But in the clinical field, in the applied field, you don't have the same kind of cadre. You don't have the same kind of scientists, and you have problems that are so big that even if a guy had his head on right and knew what he was going to do, it would be very difficult to do it. It's like saying, "I want to find out whether this drug is good for acute schizophrenia." It turns out that to collect the size sample he needs to do that, maybe 60 schizophrenics, 20 on each treatment, may take him seven years. So the idea of collaborative studies, well, we've got to get answers to these questions a little sooner now. So in comes this force, you know. That's what NIMH represented to them. They had the funds, the only one who had the funds, and the only ones who could create such a structure and get

it done. So that was a lesson that we learned for treatment research. We need large samples, we need lots of money, and we need a lot of time and a lot of effort on the part of a lot of people.

Pickren: So here's a role that the government could play. This is facilitating research by providing the adequate funds that could be disbursed across sites so that you'd have enough of an end, if you will, to make your results meaningful.

Katz: Right.

Pickren: That must have meant, then, that there was standardization in the various sites, that is, that you were all attempting to try the same things, the same medications, making sure your diagnosis was the same.

Katz: Right. And as it turns out, and being in a position, if it doesn't look like it's working that way, to know why it isn't. So that's what science is about.

Snyder: So was that partly your role to really address the methodological questions in it?

Katz: I had some... Yeah. Part of my mission – the assignment I was given was methodological, because I was brought in as somebody who knew something about quantitative psychopathology.

Snyder: Typically a psychologist's role, I would guess.

Katz: Exactly, yeah. That's exactly right, because they're the only people who trained ...And then this diagnosis, which of course, that brings back

psychiatry, but that brings back statisticians and that brings back all kinds of people that have something to do with this, so that was another problem to be tackled. There's the evaluation problem to be tackled, what methods we have to do this kind of stuff, and there's the diagnosis problem to be tackled. So that was a project that took a couple of years to put together. You know, the conference and the book really had a good reception, and now this was sort of a basic underpinning for what was going to happen after that. Like I say, while that was going on, we had the U.S.-U.K. study going on, which was actually going to test whether, if you cleaned up the instruments on both sides of the ocean, they both used standard – this… both used standard interview techniques, collected data in a standard way, had the evaluation instruments down, would they come up with the same conclusion: more depression in Britain, more schizophrenia in the U.S. Once they did the study, it was clear that it was the methodology that determined that they were getting that kind of result. So while I was doing that, I was associated with that effort, so we published a study showing how the British perceived symptomatology, psychiatrists, differently than American psychiatrists. So one part of the problem was getting them on the same page when it came to the methods, so that's the way we were tracking the stuff.

Snyder: And this is really, even though it has implications beyond the domain of psychopharmacology, its work that's staying focused within

psychopharmacology even though the implication is clearly for other approaches as well.

Katz: Yeah. But, you see now, my job in working from psychopharmacology, like working with Zubin was not necessarily in psychopharmacology, working with New York State, working with the British group, who also weren't doing this for psychopharmacology – they were doing it because they were interested in the classification problem. And in Geneva, you had the WHO, who was doing this large study on schizophrenia and were using the same kind of technique. They developed their own standard interview – it was called the Present State Examination – that went along with the… that came out of the Columbia group. So they were cleaning up the methodology to look at whether schizophrenia was the same in nine countries. So this was a kind of a broader thing that psychopharmacology played a role in. We were very much in touch with them and had a lot to do with the development of their programs.

Snyder: Yeah. So it's like you had the real, when you started off, you had the really practical problem. You had drugs that seemed to work, but you weren't really necessarily sure what they were working on because the diagnosis was so scattered and messy.

Katz: Exactly.

Snyder: And so, because then you had a very practical problem to work on, and you needed to know what were these drugs actually affecting and what

was the diagnosis, and that had all the implications, then, for diagnosis and classification in general, which, of course, I could see as playing an important background role to the eventual refinement of the DSM approach.

Katz: I'm going to tell you how that happened because that's part of the strength. But it's interesting when you play it back to me, the thought occurred to me that, yeah, we had to go back and create this structure because it didn't exist. Ordinarily you'd say we get the new drugs; there must be a structure out there. I remember thinking this way: There must be a structure out there to test the treatment. Somebody must have done it sometime. And you go out there and there's no structure at all. Then you find they don't even know how to diagnose in a systematic way or reliable way, so how do you really start? And then they don't have instruments to evaluate the treatment. So you learned in the process that it was nothing out there that you would have to... I mean, Jonathan was the guy who had the leadership mentality to know that all these things had to be done, and he would just assign us, you know, go out and solve the diagnosis, you know. Okay, I'll go out for few weeks I'll be back. So that got started. Now, that was my training for what I was going to do when I got to the clinical branch. And by the way, it served as a model. You see, I had started out in psychotherapy research, which was a monster. That was, you know, one of the reasons I was happy to get out of it was because I

could see these seven-year studies, and if you wanted to develop a field, you don't want to wait seven years for your first paper.

Snyder; This is Rubenstein, right? He is doing this.

Katz: Yes.

Snyder: They had published that 1954 book, which I'm balking at the title. I can see the cover because we have a copy of it. It's basically some of the early stuff on psychotherapeutic research, 1954. There are a number of authors in the book. And it sounds like, from the way you're describing it, it was so messy that it was unclear what you're going to be able to conclude from it.

Katz: You know, let's face it. I just, for example, described how difficult it was just to evaluate a drug. Now, a drug has a certain weight, we knew how long we were going to administer it, we can control how much we give to a patient. What is psychotherapy? That's just getting…treatment. So if we had all this trouble just dealing with a little drug, which we could really describe physically what we were doing, what are we going to do with this? Every one of these guys is doing a different form of psychotherapy. What's going on? So I could see, even at that age, I knew this was not... I mean, if I want to be a researcher and develop in this field, this was not necessarily the field to work in at that time. There was a lot to go on before you get back to that. But we took the models later, as you'll see, from the drug world to the psychotherapy world, and that's...

Pickren: We will come back to that, for sure.

Katz: Yeah. That's the part that was very intriguing.

Snyder: Now, somewhere in here, I look at the NIMH listing here, in '64 you became the chief of Special Studies Section, Psychopharmacology Research Branch, which is different, apparently, from the psychopharmacology service center. So what were you doing in this Special Study Section?

Katz: Well, first of all, to clear that up, the service center eventually became a branch. That was just another way of – that was for the organization. The center was – a branch was bigger than a center. So then a branch had sections.

Snyder: And you were chief of that Special Studies.

Katz: And Jonathan appointed me chief of Special Studies because we had another instrument, we had another interest in drugs that was separate from the clinical. We also had to deal with the LSD issue. So that was a field in which there also was a special problem.

Snyder: About '64, Leary is pretty well known.

Katz: Oh, yes.

Snyder: He's almost at the point, wasn't he, of "tune in, turn on, drop out," something like that.

Pickren: It was about that time that he must have left Harvard, as a matter of fact.

Snyder: Left?

Katz: Well, whatever. I was covering that area as... He covered several areas. I was covering the LSD area, which was another world, you see. There are the treatment drugs, the good drugs, and then there were the bad drugs, you know, the wild drugs, the LSD and... Those drugs, the LSD, were, in a certain sense, much more exciting, I thought, for the future of psychological research. I mean, I feel like we really dropped the ball there altogether. That was the most exciting area for understanding psychology.

Snyder: Have you seen recently where there's been some renewed interest in it?

Katz: Oh. Well, I wouldn't doubt it.

Pickren: I mean one of the things we found in looking at Burt's stuff this morning was an old meeting on marijuana and health. And, of course, they're taking that up now in the Congress.

Snyder: At least that's... Right.

Katz: We started to study that. We started a laboratory at the prison, and we were doing basic studies of LSD versus other conventional drugs. We set up that laboratory, which was really working very nicely at the time, but we got a lot of static, and eventually acid got turned off. You couldn't do anything with it. But what Leary had, you see, what Leary had discovered was this stuff did things – it did things to you, to the mind, that told you an awful lot about psychological functioning. These were the things that were so hard to grasp in a research way. You needed a whole new world of methodology to do that because most of everything that got published

in those days was using standard psychological methods. It was the same sort of a problem, that you had new things now and they didn't fit the old methods, you know. It didn't make any sense to say that LSD slowed down thought in this area. I mean, you couldn't get a picture of what that drug did from these mundane little psychological tasks that they were giving, tapping speed or whatever, you know. You didn't get a picture of what LSD was doing. But the point was that it was doing such unusual things that there was a great deal to be studied there from a psychological standpoint. And Leary, of course, got carried away. He was a pretty conventional psychologist until he bumped into that stuff, and, of course, he took too much of it and he went off and everything. But he had applied for a grant at that time, and I was watching over the area myself and I had to go up to Harvard to see what they were doing, and got to meet McClelland at the time. He was the chairman. And it was a nice relationship for a while. Then finally Leary got bounced, and I remember talking to McClelland about it, and he said, "I wanted that guy to stay here. I wanted him to..." They loved him. But he had to stop giving those drugs to the graduate students in studies. And he just broke – I think the story was at the time that he had promised not to give them any more to grads. In other words, McClelland had taken a lot of heat for what he had been doing, what Leary had been doing. And finally McClelland says, "He did it again," he says, "and he had the balls to come in here and tell

me that he didn't do this." In other words, he lied flat-out. He's gone by that time, you know. They bounced him. He's an interesting story in himself.

Pickren: Did you ever meet him, Timothy Leary?

Katz: Yeah, sure.

Pickren: Richard Alpert is with him. Alpert was his protégé. He was... I had several conversations with him. I'm trying to remember around that time, and, Marty, you'd probably be able to refresh my memory. I think it was an issue of *Time* that had this as the cover story. What was going on in New Orleans at that? Who was doing the stuff in New Orleans? Somebody, Heath?

Katz: Yeah, yeah. Heath, that's right, you got it. It was that story... So many things were going on that, you know, the San Francisco scene with that drug, and then Leary going off to Mexico with the psilocybin thing, you know. There are so many...

Snyder: Mushrooms.

Katz: They were concerned, they put the kibosh on the whole thing and they stopped our lab...

Pickren: But it had been before... Leary, of course, sensationalized it and put a stigma on it. But it sounds like the government was quite interested in these drugs and their potential for uncovering psychological functioning.

Katz: So much as you have to... I have a couple things that I've written, but the

best guy to write about what that is all about is Huxley, and it was Aldous Huxley. I think *The Doors of Perception* is a great little book for people that are interested in what's actually going on. What it does is just open doors in the mind and...

Snyder: I think that became intriguing and frightening at the same time.

Katz: Well, yeah, because, you see, people couldn't control what went on. It was a... You see, we'd been used to drugs... You give a drug and it'll have some effects, and it might reach its peak in an hour and a half or two hours…dosage, and then you'll watch it go away. The thing about *acid* was, here you were dealing with a drug that you would take in micrograms. In other words, I'm talking about the average drug is in milligrams, which is a thousand times the size of that, and here you were dealing with a drug that had maybe 50 to 100 micrograms, not visible to the naked eye. And you would take that drug and you would be in this state once you got into it. Not everybody would go into it necessarily, but most people would if you give – everybody would if you give enough dose; 150 micrograms will do it. And they'll be in it for eight to 12 hours. So you're just embarking on a trip, when it starts to work, like an hour later or whatever it is, and you're in the middle of that trip for eight to 12 hours, and it is, like we say of a stimulant, we give you five milligrams of amphetamine or something, we're going to measure your functions and we're going to see. It's a stimulant. You're going to get a little high, you

know, like you took a little coke, and you're going to get a little high, you're going to get enthusiastic, and you're going to be in it for a couple of hours, it will last, and then you're going to quiet down and you're going to go down a little bit and then you'll be out of it. It's very easy to grasp what's going on. And you attribute it all to a little stimulation. It stimulates your... *Acid* goes in all directions and nobody knows, when you start out, how many of these you're going to go with, but you will go into an emotional dip, you'll go into an emotional high, your perception will get absolutely... I used to describe this. If you had *acid* and you would look around, you might get suddenly obsessed with the floor, and you'd be down on your hands and knees looking at that wood because you'd have never seen that wood the way you're seeing it now. You would see ever little crack and every little grain. It would look like the most beautiful thing you've ever seen in your life. And you'd carry... That would be one little thing that would happen to you in this eight hours. And the next thing, you'd be telling yourself a joke or you'd be hearing a joke and you'd be hysterical for 15 minutes, the funniest thing you've ever experienced in your life. So things like that. Now, how are you going to grasp this? See? And that's what Leary and these people had gotten hold of, this... And when you're looking at the floor, it's not like you're crazy. Your perception is different. It's different. It's much more sensitive, you see colors in much... It's as if filters have been taken off your eyes. It's

the way old Huxley described this. I'll give you an article of mine.

Pickren: Did you investigate things like this.

Katz: Yes. It was my thought at the time that we would do something important with it, at the same time watching over what other people were doing with it. But we developed this laboratory, and the idea was that nobody was doing sort of basic work on elaborating psychological states that were created, and we developed new methodology for it. We did some interesting things. But we were only on the doorstep. It was...

Snyder: Did you get kind of shut down then?

Katz: Everything got sort of shut down toward the late '60s.

Snyder: Yeah. The social stigma attached.

Katz: Social stigma and the fear of, well, breaking chromosomes, all kinds of threats and so forth. And I'm not going to say it was... It was potent stuff. This is not stuff that people would be walking around on the street with, you know. And we did know, and there were a lot of people, who had taken it too many times, and there were lots of problems, I mean, lots of difficulties. You couldn't...

Pickren: When that session began to then shut down or perhaps.... How did it happen that you moved over to the clinical research area?

Katz: Well, I – at the time, I think it was about '67, I decided that I was thinking of doing something else, and since I'd been involved in cross-cultural work for quite a while in the years with the World Health Organization,

with our own projects in Hawaii, I got offered a fellowship at the East-West Center for a year, and I thought it might be a good time to take it because I wanted to rethink what I'm doing, and the psychopharm group had started to break up a little bit. Jonathan had left and somebody else was taking over the branch, and I didn't necessarily want to work in that setting that much, so I was thinking about other things. So I went there for a year, focusing on cultural issues and schizophrenia. We had a study going there. Toward the end of that year, Lou Wienckowski, I think it was '67, became head of extramural programs, and this Clinical Research Branch had now evolved out of a restructuring of the institute, and a man by the name of Don Cohen who had run it for about a year, but they needed a chief. They needed somebody to take it over, and it really was right down... What they were going to do was right down, linked up very closely with all of my training over the years. And what it would do, it essentially would be to have a broader canvas for clinical research problems that went beyond psychopharmacology. In other words, psychopharmacology was going to continue, but just like an issue like diagnosis and classification, that would become part of this world, as would all...

Snyder: As would psychotherapy.

Katz: As would... Well, see, we created the psychotherapy.

Pickren: That's something that I really do want to know quite a bit about, is the

psychotherapy research, because this must have been how Morris got involved with you, Morris Parloff . So you came back about '68, and you've done the chair or chief of that clinical research. It's in Lou's area now.

Katz: Yeah. Lou was in charge of all extramural programs.

Pickren: So when you said... This particular clinical research, is it for extramural projects?

Katz: It's an extramural program, right. But it had the same authority that the old psychopharm group had. That was one of the reasons I think he recruited me, because it was going to get involved in collaborative research, and they needed that kind of experience because the issue when I came back in, the only going, really hot program when I came back in was the biology of depression, and as soon as I came back in, he started to plan this conference on the psychobiology of depression, and out of that was going to come a mandate for collaborative research to solve some of the problems there. So that fit pretty well with my experience. I knew how to do that, knew how to go from the drawing board on a mandate like that and into a real study. So we both did – we had to run the extramural program on depression and schizophrenia, psychotherapy, biological research. That's the grants program. And we also had to take responsibility for stimulating the whole clinical research field in depression with the new drugs and the new biological hypotheses. It was

a time – I don't know if you remember – that the catecholamine theory of depression came on the scene. It was a major scientific development. Axelrod – I don't know if he had already won the Nobel Prize for...

Pickren: No, not quite.

Katz: Not quite yet. But the idea was, he was studying the mechanisms of action of these drugs and learning that the drug actually did impact the noradrenergic system, you know, the chemistry of depression, and that this could help explain, had to do with uptake of norepinephrine in the system, and could explain why those drugs were working. So we had now a scientific underpinning for a lot of the stuff that was happening. And the interesting thing about the collaborative of depression study was the collaborative study, that model had been used, developed to evaluate treatments. That was the whole point. And it had really stuck. A number of those studies had been done, as we mentioned, with depression, schizophrenia. The VA had done a whole series of them, the people like Hollister [Leo E.] and overall. So that thing was out there, and they were very effective and they got a lot of important work done in that field. We saw the collaborative mechanism in depression. This was after this big conference at Williamsburg, where we did the same sort of thing as we did for the classification. We brought together everybody and said, "Now what should we do to work through this field?" because, again, its applied research in a way, and it has to be pushed, you know, it has to be guided.

And the kinds of studies that were going to be developed were so large that no one investigator was going to do them, so we had the same thing. So these people came together. We reviewed all of what was going on in biology at the time and came up with this set of recommendations about collaborative research, and that's how we began that program. The difference would be that that model had never been used before for anything other than evaluating treatment, and were going to, for the first time, use it here to actually test hypotheses about the mechanisms of depression, using a collaborative mechanism to do that. And the reason was that you had these studies, these small studies, identifying catecholamine, say, as a source, another set of studies identifying endocrine factors, you know. I remember the ST, the dexamethasone test for depression. That was endocrine, this was neuroamines. Then you had electrolytes. You had all these... No place in the country do they even look at a couple of these systems simultaneously. Each of them had to be done separately, much less look at behavior while they were doing it, because they just weren't equipped to do it. The laboratories weren't big enough. And then you needed large enough samples to be able to test something. You wouldn't accumulate enough in one. So, one major rationale for a collaborative program here was to assemble a large enough sample to link together several kinds of laboratories. Peter Stokes had one of the best neuroendocrine laboratories in the country up at Cornell. Jim

Moss had the neuroamines at Yale. Eli Robins had the best pharmacologic laboratories, drug laboratories. These people sort of had to be put together, and then you had to put the six hospitals together so you'd get enough patients. So that was the ingenious aspect of that whole thing. So then we're going to use the collaborative mechanism to test a hypothesis, like test the catecholamine hypothesis, use it to do that. That was the interest in it. And it took us two or three years, just like the first collaborative, clinical psychopharm, to convince the committee, and this time it was like Dave Hamburg and Dan Friedman and people like that that were on this committee, Ed Sacher. They had to be convinced that this would work, and they weren't. It was two years, same kind of situation. But we persuaded them eventually and got the grants, got the program started, and that was a big development.

Pickren: Yeah. That's a big project.

Katz: That was a big project.

Pickren: To think it entirely through as to what are the criteria that you're going to use in selecting your patients or subjects, I guess.

Katz: There you go. One, what came out of the conference was, the first step is, where is this objective diagnostic system that we're going to use for this new study? Before, we got away with it with the clinical studies because you couldn't do a little less care. But this was supposedly a basic study, and how do you handle that? So one of the first...We let a contract. The

first contract we let was to a committee made up of Bob Spitzer, Jean Endicott, Eli Robins, and Joe Mendelson, I think it was. Eli Robins' group had come up with the research diagnostic criteria. I don't know if you're familiar with that field, but that was a set of criteria for each of these diagnoses that were based on operational notions. In other words, whatever we knew at that point we would put down an operational criteria, and theory was taken out of it. That's the end of psychoanalytic theory anyway in this country, so all of that was drummed out of that system. And then these criteria that were set up, it was John Feighner, a young psychiatrist working for Robins, who did this. So we were going to start by taking that set and giving it to this committee, Spitzer et al., and they were going to refine these criteria, get to them to the point where everybody agreed this is what they are, and they were going to develop a standard data-collection instrument, which would be the standard interview, and that would be the form, which you may have heard of. Jean Endicott and Bob Spitzer would develop that big, thick interview form, you know, with the standard questions, and the use of that data instrument and these research diagnostic criteria, we would elaborate these diagnoses and then test the whole system for reliability, make sure it all worked, do it across our six centers in this case, make sure it all worked. And we gave him a grant, we gave him a contract, I guess it was, for a couple of years to do this, and they did it. They came up with this, tested it. We put it to

use. And because of the, because everybody knew about it, it was very good business going on, the APA appointed Spitzer as chairman of the diagnosis.

Pickren: The APA here is the American Psychiatric.

Katz: American Psychiatric, yeah. Appointed Spitzer as their chairman for classification. So he took this whole system over to American Psychiatric, and then came three or four years of battling and whatever, and he, you know, battling with the psychoanalysts, battling with everybody, but he won. And that set, the criteria became the basis for the DSM-IV. So that's where the...

Snyder: DSM-III, 1973.

Katz: Right. DSM-IV is later.

Pickren: That's fascinating. It actually came – it really came out of this program that got started in your branch.

Katz: Right. It was the first step to – in our study, not for that purpose. We didn't give a damn about the American Psychiatric Association, their problems. We needed, in order to sell our committee on going forth with our collaborative study, that we had a solid diagnostic system, and that's what we got. And once we had that, we could go ahead with our study. And that was resolved in the early '70s. I think the study was funded... The contract on diagnosis was funded, I guess, '71, '72, and the study proper, the two collaborative studies that we started, one with Jim Moss as

the chair and one with Klerman as the chair, those were, I think, funded in '73 and '74, once we got that step. They made us... I say we did it, but they made us account for that area before they would give us the money for the next.

Pickren: And then these two collaborative studies were obligated to basically use Spitzer's approach.

Katz: Absolutely, yeah.

Pickren: You developed a standardized approach so that you can be sure that across centers you're basically doing the same thing.

Katz: Absolutely.

Pickren: So the lessons you learned in the psychopharm work is really what you used.

Katz: Absolutely.

Pickren: You've pulled forward on that. Tell me about the work, then, okay. That's in the biology of depression.

Katz: Right.

Pickren: Tell me about the work that got started somewhere in this time on psychotherapy research because Morris comes over about this time.

Katz: Yeah. When I took the job, they gave me – one of the conditions of the job was I could restructure that branch because it had to, not that I could, but that our group could restructure the branch so that it would really be in tune with what clinical research was going to be like in the future. And, of

course, depression was the big thing at the time, so we established a depression section. That had already been there. The depression section was there, and the biological psychiatric section was there. And we bought that. We needed that center for depression, and we did need the biological research because that was where the excitement was in those days of biology of schizophrenia, biology of depression, and so forth. So those two – that's what existed when I came. We identified... And then, also, we had a center for studies of schizophrenia, so that was also there when I got there. So those were the three parts. I remember we did two parts. One is basic psychopathology. We needed to have; we needed to make methodology for that area, that area itself was important because that's the science basic to all clinical research. And even though we didn't know exactly what to do...

Pickren: The days with Zubin…

Katz: Yeah, exactly. This is what Zubin would have done. And so we established it. That became psychopathology, and that's a goal for all other clinical research on schizophrenia, depression, whatever, you know, that wasn't biological really and then there was psychotherapy. We had to do something about psychotherapy. There were quite a few grants out there for research in psychotherapy. There was no focus in the institute for psychotherapy researchers. And although it seemed like a pretty high mountain to climb, we thought eventually you're going to have to develop

a science in that world. So we organized psychotherapy as a section and created a section. So that would be like '69, right after I took the job. We restructured and Lou was very instrumental in that, very helpful in that. He went along with all of that, and he and I were able to argue with Bert Brown and get it through. So we had the five sections, in a sense: depression, biological research, psychopathology, psychotherapy, and schizophrenia, the Center for Study of Schizophrenia. The idea for psychotherapy was to do the same kind of stimulatory work that we had done in other areas, so now the only difference from what was going on in the field at large was that we believed, at least I did, that you were going to have to create a model for evaluation of psychotherapy off the model for drug research, and that sounds like pretty primitive, pretty low-level thinking. Drug and psychotherapy are pretty – conceptually, there's a lot of distance between these two. But psychotherapy is going to get lost or it's never going to get accepted in the society unless you can start to show the same kind of practical effectiveness that you've shown in the drug [therapy]. We saw what happened with the drug stuff. The drug stuff took off, worked very well. And for good reason, those treatments worked. I mean, when I got into the depression field, I was still, 10 years later, as skeptical about drugs as I was almost when I came in, because I never really watched them work. But when I got into this clinical research – not when I got into it; I was in psychopharmacology – but I saw them at

work. This is not a placebo. This is real stuff. And the most striking example is in the depression field, where you take people who have been depressed for two or three years and heavy psychotherapy, and all the good intentions in the world. Nothing changes. Then you put them on the right drug and three or four weeks later they're walking out of their cocoon. It's like seeing another person. It's like a person's been hiding for two or three years. So when you see a few cases like that – and I saw enough of them at work – you don't think about this is not a, this is no placebo. This is... And mostly you see it with the really severe depression. You don't see it with the mild stuff. The mild stuff is a lot of placebo. But these were really severe cases in hospitals, about to commit suicide, all of that kind of stuff. So that…is about that. Then psychotherapy is going to have to, if it's going to work in the field of depression, where it does work, it's going to have to show it to be able to compete with drugs over the years. So we did a few things there that we've done in other areas, like, first of all, how you are going to evaluate psychotherapy, much like we raised the question with drugs originally. You're not going to be doing it with a Hamilton Depression Scale. Is there a little less anxiety, is there a little more this. You know what I mean? The aims of psychotherapy are much more involved, and you have to get better methodology for that. So Irene Elkin, who has worked... First of all, Tuma took over that section when we first set it up. He had done

quite a bit of work in psychotherapy research.

Pickren: So in '69, he became...

Katz: Yeah. He became sort of the – he was assistant chief of the branch, and he took on this other responsibility of running that section. And he also had with him, Irene Elkin Waskow. She was an on-the-ground person because Tuma had a lot of things to do. She set up a conference on psychotherapy change measures. That was the first thing. It was a pretty large operation. But in the course of it, we had a dozen reviews of methodology written for the purpose of sorting it all out, and a lot of very important people participated, like Don Fisk from psychology and a lot of other people. And there's a book on that, which I have in here, called *Psychotherapy Change Measures.* It was a first task. The second task, at least the way I saw it, but I was a little on the outside of this now because I wasn't going to be in the day-to-day workings just like the other stuff, but to me, eventually you're going to have to do something like this collaborative project to evaluate psychotherapy in the same format. You're going to have to learn how to do that somehow. You're going to have to develop some model for that if you ever expect that to have staying power in this field. See, we could see the drugs were going to like... Just like…depression, drugs would sweep through it, you know, unless they developed some sort of a... What is it they can do that's either as good as or different than or whatever, and you had at that time, you had cognitive

therapy coming up and you had behavior therapy. You had Tim Beck's approach, you had the behavior therapy approaches, and these looked like workable treatments that were going to be effective. And you're going to have to put them in some way against the drugs, see if you're dealing with the same population and so on. We had another conference which, at that time, which was called *The Psychology of Depression*, in which a lot of these people participated, like Tim Beck and Martin Seligman, Paul Ekman. You know, these are all psychology people. And that was a – that conference was a big hit because of all these wonderful people. We had some pretty good people, like Charlie Ferster was there. They each represented these different settings.

Katz: They laid out what was going on in the psychology of depression. This was going to be the basis for psychotherapy of depression. Have we learned anything about the psychology while we're doing all this work on the biology? And, yeah, there was stuff, the learned-helplessness stuff and so all of that was laid out. And that book was got very good reviews. I don't know if you've come across it, but it was in *Contemporary Psychology* at that time. They thought it was… That's the one, Friedman and I.

Snyder: Okay.

Katz: You know that one?

Snyder: Yes.

Pickren: So this is early ‘70s, then.

Katz: Yeah. This was a conference that followed up the big one in Williamsburg. So that is the psychological root. We had gone through this big biology thing and it got so it was my job to promote all of that, but not to leave psychology behind, and so that’s why we created this conference, which was not something that they were demanding. It was something that we thought had to be done, you know.

Snyder: This was a case, then, where NIMH, and really, and your staff, decided that this is something that we need to--bring psychology along. It wasn’t some request from psychologists saying, “Why doesn’t NIMH help us?”

Katz: No.

Snyder: So this is really a role where NIMH has played as an initiator.

Katz: Oh, absolutely.

Snyder: Now, how much of this was you?

Katz: See, the drug was something else. Congress wanted that. Nobody cared about psychotherapy.

Snyder: How much of this did you do with this shaping of psychotherapy research along the lines of drug research? Was that pretty much coming from you saying we need to do this?

Katz: I think... Well, it didn’t actually happen that way, but it was my idea to make it a section. That much I take credit for. I thought if you’re going to get any visibility, if it’s going to do anything, it has to have visibility.

People outside have to know that there's a place they can go to and get some help with funding and consultation. And we had some pretty good people like Tuma, who had done a lot of research, and Irene at that time. She was just very bright and she worked with Paulo Machado. She was a protégé of Paulo Machado. So they carried the weight there. And then the idea that the collaborative and the drug model, yeah, I think, because I felt that you could create a model like that. I wasn't going to do it; I didn't know how to do it.

Snyder: You were involved in the mechanics, but no idea if it was, okay, here's a way we need to standardize, and we need to make sure that we've defined what our treatments really are.

Katz: Right.

Snyder: Now, when we interviewed Morris, Morris talked about this as the origin of manualized treatment. Is that so?

Katz: Well, that's right. This stuff wouldn't... You know, when we brought Morris over, we brought him in eventually to become chief of that section, and then eventually became chief of that branch. But we brought him in because he was the wise man and he'd done a lot of research in that field. He was good. So he would lead the effort into the future in a sense. But he didn't think this way, no. When he says manualize, you see, the manuals come out of the researchers, like the drug research. The manuals – I don't think it was my idea but probably more Irene; I don't know who,

but the field knew it, because we didn't do them, the field did them. So the point was, again, that example. If you have a drug, I can tell you this drug has to be administered at three pills every morning, each 5 mg, 15 mg. You give these other people 30 mg. But at any point you say, "How are you treating them?" "Well, I'm giving them 30 mg. a day." I can tell you physically what we're doing. And we can isolate that physical treatment and ask, "Did it do anything?" You know, we have a placebo, we have a... What do you do with psychotherapy? You kind of know the people who are administering it. They're not three different pills. I mean, they're... You know, what are they doing? And if you know anything about psychotherapy, you know that every one of them operates in a different way. But they were claiming that there was such a thing as behavior therapy, for good reason. There is a systematic way. That's what behavior therapy gave to psychotherapy. It gave it a systematic approach that could be measured, and so you know what goes into it. Then you had to ask the question, if each of these people are following the same manual, does who they are make any difference? And it seemed like they didn't. Once you got the thing standardized, you could actually look at a standardized behavioral therapy. But you absolutely had to have the manual, you absolutely had to have a systematic training procedure, you absolutely had to have a way of determining whether these people were actually taking the manual in, whether they were using it, to be able to say,

"I'm studying," because, in the end, everything has to do with generality. We showed behavior therapy is better than a drug, and somebody comes along and says, "What do you mean by behavior therapy?" "Well, here are the principles of behavior therapy." "How do you know it was administered in the same way?" "Here is the evidence it was administered the same." "In other words, if I take these eight steps and I do what you did, I will get positive effect." "Yes." You've got to be able to answer that. And if you say, "Well, no, I'm not sure if you do them this way or that way," you don't move any further than where you are.

Snyder: So the gold standard had become, although I'm not sure that I understand how it happened exactly, but the gold standard had become drug treatment.

Katz: Well, no.

Snyder: So psychotherapy had to match the gold standard.

Katz: Now, I'm presenting it that way because I came from that field. I don't think psychotherapists would think of it that way. But the point to me was that – and you'll see what happened as this evolved – was that you want to know what relationship do these psychotherapies have. In that sense you're right. What are the most effective treatments for severe depression? Yeah, at that time it was imipramine. And you want to know, is psychotherapy any better, any different?

Snyder: You want to be able to get replicated sites. This is the definition of

collaborative research from their point of view. It's very interesting today for me to hear this, because when they talk about interdisciplinary collaboration now, which has become the big watchword, it means something different than the setting up of research projects in this way.

Katz: Oh, yeah.

Snyder: Very, very different. And I'll say it, I ask the question now. At the time that all of this was going on; the basic training for psychiatrists was still highly psychoanalytic throughout the country, with some notable exceptions. And the institute was still, you know... First of all, during Burt's tenure, the main emphasis was on community mental health and community mental health centers, really, and it came from, I mean, that's when all of this started. And, really, when Burt was removed and Jerry Klerman came in and ADAMHA and Herb Pardes came in, there was a big push then for biological psychiatry. That's when it really sort of...

Katz: Oh, no, big push.

Snyder: Even before. But then it was the most dominant.

Katz: Yeah. I think what... I agree with you. Biological was very big starting with the drugs and starting with those discoveries. It came into its own and was being pushed by people like Seymour Kety and for good reasons. It had been completely neglected from the standpoint of research, and they weren't getting the grants, and they weren't producing any good science at that point. But they helped a lot in getting that started. But what you're

right about is that the psychoanalysts were still in charge up to a certain point, and probably by '78, they were on the way out, because people like Dan Freedman and all these people were psychoanalytically trained. But they were the new breed of academic chairmen, of which Dan Freedman was one, Ed Sacker [sp.] was another. They brought science to psychiatry, or they were intent on bringing it to psychiatry. But, still, psychoanalysis was hanging around. I agree with that.

Pickren: Well, Ted Beck had been trained as an analyst, I think.

Katz: Beck had been trained as an analyst.

Pickren: When I interviewed David Hamburg he talked about his role, and I think he's a fairly self-effacing person in some ways, but then when he went to Stanford, he left NIMH and went to Stanford, he really embarked on this very conscious effort to train psychiatrists to be scientists.

Snyder: But Dave is one of the ...

Pickren: Yeah. Let me ask you, following through on the psychotherapy thing, to what degree during your tenure there did you encourage or were you encouraged to encourage psychotherapy researchers to pursue specific, to focus on specific DSM categories, that is, develop psychotherapies for depression, psychotherapies for anxiety disorders?

Katz: No, not really, no. I think we hit on depression as the area to do the psychotherapy collaborative study because it was such a likely target, a practical target. It was something that the cognitive people had claimed

that they could be effective in and had some evidence for it, it was something that the interpersonal psychotherapy people claimed they could handle, and it was something that the drug people had demonstrated efficacy. It was the cleanest area to work in for psychotherapy. The anxiety drugs were not, you know, they were so-so, and certainly with schizophrenia was...

Pickren: See if you could get kind of a more or less clean measure of psychotherapy's effectiveness compared to the drug action.

Katz: You wanted psychotherapy to use a strong suit. If there's anything they can be effective in... After all, Beck was a depression expert. If cognitive therapy is going to work in anything, it's going to work in there. That was, his whole system was built on that. He wanted them to have the best possible situation for this to come about. We weren't out to expose it as nothing. We were out to make it, give it as good a test as possible because you needed it. I mean, this business of drugs is great, I mean, in terms of the severe cases and getting them out of episodes, but you have to face the fact that the run-of-the-mill depression is a milder case who walks around the community all the time, and unless you want to keep everybody on drugs, you've got to work with him in his problem and get him out of it. And these two systems looked very well suited to this problem, and they'd already shown some preliminary evidence of success. This should be a very nice way of doing it. What happened was a lot of fire, but it was a

good idea. So these were steps. So you might say this psychology of depression conference, which brought these stars together, who made their cases and made good cases. Then I think Irene's big effort in evaluation measures, which is a little book there, that was very useful. It's the way we worked before, you know and then the collaborative effort. In the meantime, you see, before the collaborative, we had examined, we had a little conference on behavior therapy. That's in there. We had a conference at Esalen on all of the new treatments, the encounters. We did the whole thing. So this looked like the best bet. It turned out to be a very, very difficult...

Pickren: Did you get a lot of criticism? By this time, by the '70s, psychologists are dominating psychotherapy, and I would suspect, given, I mean, looking from our present state, looking back, that it must have made some psychologists relatively nervous that you're going to put psychotherapy to this kind of test. Was that so?

Katz: I didn't get that much, but Irene would be a good one to talk to there because she was probably confronting them all the time on that. If you're asking about it from the other standpoint, with psychiatric, well, you can see what happened there. Did you follow the literature on that once the results were published?

Pickren: No..

Katz: Well, that's where your friend, Don Klein, comes in. They were livid

because this was one of those cases where Irene and her statisticians – she had some very good advice and good people; you know, we always had the best because we could always pay for it. We could go out and get the best, so she had a good one. I was gone, by that time, from the scene. But she had her own entourage and excellent people like Jim Klett, and she ran into some trouble with Sam Greenhouse.

Snyder: That would always happen.

Katz: Yeah. But she had excellent statistical guys and published a study, and it was one of those things where you show the drugs and the psychotherapies to be relatively equal.

Snyder: I remember that.

Katz: Yeah. Now, what you show as a secondary finding is that if you take away, if you just segregate the most severe of the depressions, imipramine came out looking a little better, so that was a qualifier. Now, into Don Klein, who thinks he's the greatest statistician in the world and greatest psychologist in the world. He's a little much. He gets on after the study because they didn't want imipramine looking the equivalent to psychotherapy. They want it to look better. And I'm interpreting. But if you go back to, the question was, suppose you analyze this study the way they wanted to analyze it and interpret it the way they want to interpret it, then the drugs came out looking better. And it was a matter, some of very technical statistical issues involved about what you compare with what.

So they got into pretty heavy water. But he came out very strongly against her, and she didn't get the protection she needed. When the results did come out from the institute, they were already off somewhere else. And he hit on her very hard, and she's – Irene is a tough dame, but not tough enough for this kind of stuff. She had done her work and she had done a nice job and published nice papers, and they just came out viciously, I mean, Klein and this group. So the whole thing was to undermine what she published as the results, she and Morris, actually. I think Morris [were on] the same papers. And there, I'd have to go back myself to go over the whole thing again, but they made her life miserable, absolutely miserable. It's as if he… it was a very difficult project and there were many years of effort, and what you get from it is like, it isn't unusual science, I don't think. You come out with something you think is terrific. Whatever you want to do, an old friend of mine used to say, don't tell them anything new. They'll kill you. I think she was a classic example of, boy, they came after her.

Pickren: Well, let me ask you to reflect a bit on your 20 years, really, with NIMH. It seems to me that you came there at a time when Felix's program was in full swing, which is, well, let's look at everything, you know, that might contribute to both mental illness and mental health, so the big kind of psychosocial approach, anthropological approach, almost anything would go, and got funded, apparently. So that's the late '50s. By the time you

leave in the late '70s, it seems to me – and you're the best person to actually answer this question – it seems to me that NIMH had already made a major step toward giving the primacy to biological factors, biological being very broadly construed here.

Katz: I think that's very accurate. What happened, you see, I think I mentioned to you the last time we met, that when I came up, he would look at their portfolio of grants, and you would see behavioral science, I think maybe as Stan put it, behavioral science was the science for psychiatry, so everybody was happy with that. They didn't have any science for psychiatry, so this was the best. So they went along with the psychological science, you know, whatever it was producing, and they got most of the money. These biologists, the people who were doing biological research at that time, were doing pretty primitive stuff, and even if you want to be nice to them, I mean, like I didn't have anything against them because I had a lot of biology in my background and I expected someday to see the biological roots of all disorders, but when I looked at their research, it was really bad. Every week another chemical that's the cause of schizophrenia, some chemist who had never seen a schizophrenic in his life, I can remember the name of some of these people, too, who would publish something, and that would be the chemistry of schizophrenia. So there was such junk that the real… whether biological or psychological, would know it, and wouldn't take them very seriously.

Now, with the introduction of the drugs and with that, I think that earthmoving discovery of the neuroamines, the neurotransmitters, you suddenly had a new picture. It's conceivable that these so-called psychological disorders were really basically biologically rooted, that you were going to start finding biological roots, and schizophrenia was a great example because we never did understand schizophrenia. I mean, there were all kinds of elaborate theories, but to this day nobody really understands it, unless you want to call it a brain disease, and then, of course, you can get any kind of behavior with that. But that's the way... And suddenly, the first chemistry that worked was for schizophrenia, which reinforced the idea, there are some people that this is a biological disorder, and they were eventually going to find something which will cure it. So you had that. Then you had the discovery of antidepressants, which were even more striking in the fact that tranquilizers quieted the schizophrenic psychosis, but the schizophrenic was there. You know what I mean? Nobody would say it was wiped out. But depressive episodes, very acute, serious depressive episodes being wiped out in two or three weeks, now, that's striking. To the people who were close to it and saw it, now we had a different scene. And a very influential figure in all of this is Seymour Kety.

Pickren: Who died.

Katz: I went to the symposium that they had. But he was head of; he was

Felix's choice to head up intramurals. You see, Felix, even though he did all this, he really understood that there's something around the corner. And he brought Kety, and in doing that, he brought one of the most brilliant psychologists in the field. And Kety was a very strong-minded son of a gun, and he started in as soon as he got there. Well, no, I guess I didn't have that much contact with him, but he was on our committee, that's right, in psychopharm. They were coming on strong about more biological research is necessary – not that it's psychosocial. And then when I came back in the clinical research position, there was the biological section there, and when you looked over that portfolio at that time, it looked a lot stronger than what you'd seen the decade before.

Pickren: So in 10 years' time, basically, you'd seen a real increase in the depth of understanding, driven, it sounds like, if I'm understanding you correctly, driven by the discovery, which almost was accidental, of the effectiveness of psychotropic drugs.

Katz: I think so. You know, you had to open your eyes. See, one of my problems with psychology over the years, because I drifted out of the field in some way by linking up in psychopharmacology, there aren't that many psychologists in it, and I'm a member of the ACNP for 34 years, and that's the American College of Neuropsychopharmacology. There is a handful of psychologists in that, and most of them are basic people, behavioral psychologists that work in the drug industry or something.

You don't find many Jean Endicotts, Jean Endicott was part of it; Joe Zubin was part of it.

Pickren: From Vanderbilt?

Katz: Oakley Ray. He's the executive secretary of that. So psychology was there, but not really. And my problem with psychopharmacology, with psychology has always been that I think it's a practical thing, and maybe that's what the prescription business is tied up with, and they can't deal with drugs, so they can't build their research careers on them. They've got to stay away from them. But in the practical – that's the practical side, and I think it's a very important side of it. I mean, there's good reason for them to avoid the field. But to make believe that this stuff is not very powerful and to try to pass it off as placebos all the time to me is setting them back some because they close their eyes to developments that could be very important for psychology. If they'll just accept some of the basic aspects of this, they can go on and create and do things. But because they shut it out, there's very little interaction, and it's very difficult to work in.

End of Interview (abrupt cutout of tape)