

Theodore Paul Zahn
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This is an interview with Dr. Theodore Paul Zahn, NIMH, of the Laboratory of Brain and Cognition of the NIMH Intramural Research Program held on December 19th, 2001, in Bethesda, MD. The interviewers are Dr. Wade Pickren of the American Psychological Association and Dr. Ingrid Farreras of the NIH History Office.

- Pickren: Dr. Zahn, would you state your full name and where and when you were born?
- Zahn: Theodore Paul Zahn. I was born in Los Angeles, California on March 8th, 1927.
- Pickren: Thanks. Talk a little bit, if you would, about how you came to be a psychologist. What intrigued you about the field and how did you get into it?
- Zahn: Well, I wanted to be in pre-engineering – that was my first major – but I lasted a semester and then got drafted. When I came back out I didn't want to be an engineer anymore. I was at UCLA and I had to choose a major. I got down near the end of a list and hadn't found anything that intrigued me but I finally saw psychology and sort of impulsively ticked that off. And I liked it and never saw a reason to change it. I didn't take school very seriously for a while – I had too many cronies and was spending too much time in the pool hall – so I left L.A. and went up to Berkeley. I was able to sit in on a class with Edward Tolman. They had some very inspiring courses up there and that really got me motivated to study psychology more intensively. So I got a Master's at the University of Oregon – it was just one of the few places that would take somebody without straight A grades. I worked with Robert Leaper and he recommended me to Elliott Rodnick at Duke, whom he knew and who was chairman of the department. Oregon didn't give a Ph.D. degree at that time so that's how I ended up at Duke.
- Pickren: What kind of work was going on at Oregon when you were there for your Master's?
- Zahn: Well, it was a funny department. It was a funny school, actually, because they had a lot of stars, like Leaper and Leona Tyler. But then there were some people who nobody ever heard of again. So it was a department in transition at that point. They had a very good guy, Dick Littman.

Pickren: Who is, I think, still alive and retired there.

Zahn: Yes. He was a great teacher and I got an awful lot out of working with him. And they had people like Dixon and Massey, who were in the Stat department, so I could take statistics with them. They got seduced away. And I had a really brilliant philosopher of science named Arthur Pat who left and went to Yale. So they had a few really good people with whom I was lucky enough to come in contact.

Pickren: By this time you're in a Master's program, so you're serious about psychology. What did you find was interesting to you?

Zahn: Well, I was really in experimental and Leaper was – before it got popular – a cognitive psychologist, and that really appealed to me. The person I went to Duke to study with was Karl Zener but applied for a graduate fellowship from the NSF and didn't get it. Elliott Rodwick had a lot of money from grants. He was studying schizophrenia with Norman Garmezy and so he offered me a job as a research assistant on the grant for my last two years. Before that I had been a teaching assistant. So that's how I got into schizophrenia research.

Pickren: Until, I mean, this was really a very practical solution for you. He had the money for a fellowship, a USPHS fellowship, and . . .

Zahn: Yes, right.

Pickren: Let me back up a bit. When you went to UC-Berkeley did you get VA money since you'd been in the military?

Zahn: Well, I was on the GI Bill of Rights...

Pickren: The GI Bill, but not the VA trainee undergraduate...

Zahn: No, I was just an undergraduate, but I was supported by the GI Bill. You got \$75 a month or something and your tuition was paid, but it was a free tuition anyway, so that didn't make much difference.

Farreras: But Duke was one of those schools that the USPHS did fund.

Zahn: But by the time I got to Oregon and then Duke my GI Bill had more or less run out, so I had a tough time eating well my first year there. I got an assistantship my second year, and then I was on a pretty good fellowship at Duke, so I had no problem there.

Pickren: So Garmezy was at Duke at the time?

Zahn: Yes, he was there.

Pickren: Okay, because around the time that you went to NIMH, Garmezy was also there.

Zahn: Yes, maybe he wasn't there the whole time I was there. I know he was there part of the time I was at Duke. He may have left either toward the

end of my stay or maybe on a temporary basis and came back. But he was at NIMH for a short time.

Pickren: And then he went to Minnesota after that?

Zahn: I think he went back to Duke and then to Minnesota. I'm not exactly sure.

Pickren: What led you to go to NIMH after your dissertation? I think you spent a year there as a visiting fellow or visiting professor...

Zahn: Yes, they called me a visiting professor, but I was really still working on the research grant and taught a couple of courses. And then a good opportunity came up and I was recommended. It was an old-boy network again because Shakow had been working with Rodnick and he recommended me to Shakow, so that's how I got here.

Pickren: Yes, because Rodnick had been at Worcester State Hospital at one time with Shakow,

Zahn: Yes, they did a classic paper together on reaction time. In the first part of my career I was really following up on that research.

Pickren: So Shakow then was recruiting and Rodnick...

Zahn: Yes, at that time NIMH was just starting and there were jobs. It was expanding. Shakow needed somebody to work on his projects, aside from the rest of the Lab. He had his own section.

Pickren: The Section of the Chief.

Zahn: Yes.

Pickren: And he was continuing that long-term research he'd been doing for quite a while.

Zahn: That's right, yes.

Pickren: Schizophrenia response time and... What was that phrase he used?

Zahn: Set.

Pickren: Set, yes.

Zahn: Major sets...

Pickren: He published quite a bit on that. Ingrid, I think you had some questions about how the Lab was organized when Dr. Zahn arrived.

Farreras: Well, we know that there were six initial sections and I was curious about how the hiring came about. I understood it to be mostly an old-boys network, but I talked to Don Blough and Earl Schaefer, and there also seems to have been some active recruitment from the USPHS. I don't know whether there were published ads or certain tests that you had to take prior to being hired into the Lab...

Zahn: Yes. There were two mechanisms of hiring. There was the Civil Service, which I was under, and there was the Commissioned Corps, which was a branch of the Public Health Service, of the Army, really. You actually had

uniforms and the Surgeon General, etc. And I know at least two people who were in the Commissioned Corps – Benny [Virgil] Carlson and Mike [Donald] Boomer – who applied for the job and were hired because they scored very well on this exam. Boomer always put himself down, saying, “Well, I’m a good test-taker.” But he was also a good scientist and a really first-rate human being. I think Shakow was trying to get some eminent experimental psychologist for the Perception and Learning Section but couldn’t do it so Ben Carlson became Chief of that Section. But other people had big reputations, Jim Birren, Nancy Bailey...

Farreras: Did James Birren’s Aging Section not already exist before the Psychology Laboratory was created?

Zahn: Gosh, that I don’t know. It was a different section because most of the other sections were strictly psychologists. But Jim recruited all kinds of people: Gene Streicher – he just retired as Chief of Fundamental Neurosciences in the NINDS extramural program - he’s a neurophysiologist or neurochemist. He was working on glial cells or something. He had a big lab. And [William] Bondareff, who was there for a while.

Farreras: Bill Bondareff and Jack Botwinick?

Zahn: Well, Botwinick was a psychologist. And then later, Ed Jerome came in. He was a psychologist. But there were other non-psychologists in the Lab, which was kind of unique. I think everybody else in the Lab, except histologists or somebody over in Hal Rosvold’s section were psychologists.

Pickren: So it was a mixture of professional training dominated by psychology but Shakow also managed to pull in other people.

Zahn: Yes.

Pickren: That was a real strength there.

You were a young psychologist when you came in. What was your assessment of the Lab when you came? Did it seem to you like it was well organized? Did it run efficiently? Was it exciting?

Zahn: Well, it was exciting being here. I was a junior person in the place and there were some people who were a little difficult to deal with sometimes. The first time I had contact with Shakow I was really impressed because I had to make an appointment to have a phone call with him. And I remember, that’s really something. And he did. He had organized a meeting every week. He set a time for a meeting with his Section Chiefs and a meeting with his own section, which at the time consisted of Dave Rosenthal and me. And so every week we’d talk about what we’d been

doing and hash over ideas. And he had private one-on-one consultations every week, too.

Pickren: Was a lot of Shakow's work administrative in nature?

Zahn: I think it was. He left us alone. We hashed over ideas but he wasn't actively involved in the projects that Rosenthal and I were doing. He did have his psychotherapy project; that was his pet thing. But he hired people to do the actual work. He hired Allen Dittmann because Alan could do filming, and there was a technician assigned to the job, Oscar Farnsworth. They had to invent a special film magazine to film continuously for an hour or so.

Pickren: Because at that time films couldn't run that long.

Zahn: That's right. They had to invent a special device to be able to film for 50 minutes because they had the thing on when the patient and the therapist entered the room and it stayed on until they left the room.

Pickren: What building was that in?

Zahn: That was in Building 10, the Clinical Center. It was huge, right in the middle of the building, something like three modules.

Farreras: Were the Basic Research sections also in the Clinical Center, or only the Clinical Sections?

Zahn: Every one of them, except for Animal Behavior, was in the Clinical Center.

Farreras: Do you know what building Animal Behavior was in? They're in Building 49 now but...

Zahn: Al said something about T-9 and that was probably some temporary building where they were first. There was also a Building 9 that's still there and that they went to, eventually. The remnants of that section are not there anymore, but they were there for a long time. That's when I remember it, and that may have been sometime later.

Pickren: Was your office in the Clinical Center?

Zahn: Yes.

Pickren: I know I've talked to some folks who – when they first came out here – were in temporary buildings. Were they Quonset huts?

Zahn: No, they were just ugly, low, two-story buildings that you could light a match to and they'd go up in flames. They were wartime buildings. They weren't really Quonset huts, though. But we were all in the Clinical Center. And there was another temporary building, T-6 – I think they called it – where there was some sort of a child study project. They had residential treatment for disturbed kids, and I think it was run by somebody named Fritz Redl.

Pickren: Psychiatrist.

Zahn: Yes. Some of the people in our Lab, like Dittmann and _____, used to do research over there. Harold Raush, I think, too. That was a consequence of us not having any patient beds; if you wanted to work with clinical populations you had to beg, borrow, and steal patients from other people.

Pickren: Speaking of children, was Nancy Bailey here when you came?

Zahn: Yes, she was.

Pickren: Were any of her subjects clinical subjects, so that she had to beg and borrow subjects as well?

Zahn: I don't think so. I don't think her section worked with clinical subjects. not that I recall.

Pickren: They probably recruited subjects from the general population.

Zahn: Yes. I think they tried to make arrangements with schools and so on. Later on, when Marian Yarrow moved her own branch over to 15K, they had a real Lab where they had people coming in and playing all day.

Farreras: You mentioned earlier some of the administrative work that Shakow was doing. Did he allocate the funding for every section? Was there a fixed amount per section or was it based on how many animal subjects were needed or equipment was needed?

Zahn: The thing about NIH is that the Lab Chief is a dictator, he's a tsar. He has absolute power. And how they negotiated budgets, I have no idea.

Farreras: Do you know when the administration of CANs shifted to Section Chiefs having control over them versus the Lab Chief having all the control?

Zahn: How what was shifted?

Farreras: The individual budgets. At some point, it wasn't just determined by the Lab Chief, but each Section Chief had control over the budget for that section.

Zahn: Well, I think that's always been the case. How much he wants to go into detail about micro-managing the sections depends on the Lab Chief; it's probably a personality characteristic. Some people are control freaks and some aren't. Shakow probably gave people quite a lot of latitude, but on the other hand, I think he really knew what was going on in all of the sections. He was not somebody who'd let everybody do exactly what they wanted; he had some input. But he was very diplomatic; he handled things pretty well. Jack Botwinick used to get kind of cynical about some of these things but I think people basically got along. Shakow got along with his Section Chiefs pretty well.

Pickren: So he allocated the funds to each section, but the Section Chiefs then could determine how the funds within the section were expended?

Zahn: I'm pretty sure that's the way it worked, although they probably presented a plan to him, a general plan of how they were going to spend their money. I suspect that's how it worked, but I have no direct knowledge. I wasn't privy to that sort of thing; that was sort of top-secret stuff.

Pickren: Some of the old *Annual Reports* from the '50s, I think, list how much money – is it that each section had?

Farreras: In the mid-50s each project listed an amount and by the late 50s it was done by section. But they stopped reporting any funding allocations by 1960.

Pickren: They got nervous about somebody knowing how much each one was spending! I have another question about when you first came. There were already six sections at the time you came. Hal Rosvold, Al Mirsky, Mort Mishkin, they were all in the same section.

Zahn: Yes.

Pickren: Personality and its Deviations. Who was in that section? Do you remember?

Zahn: Well, Morris Parloff was the Chief, and Mike Boomer was already here when I arrived. And there was [Boris] Iflund, of whom I just have a vague recollection. Some other people came later, like Joseph Handlon. Harold Rausch hung around with our people, but I think he was officially somewhere else. Let's see. Herb Kelman was here for a short time.

Pickren: That's right. He had that trouble because he had been a socialist or something and he got hounded about that. And he apparently had to go through some kind of amazing administrative appeal because they wanted to terminate him over this. Yes, he's written about that.

Zahn: I'm not surprised. But he was a first-rate person, a really ethical guy, and that's his sort of field. Then they actually hired a clinical psychologist, Marv Waldman, to give tests and what not. I'm not sure exactly what he did. Everybody else was hired for research and they needed somebody who did clinical work. The people in that [Personality] Section had kept their hand in doing psychotherapy in some way. I think Parloff and Boomer did group therapy. I know Boomer, and probably Parloff, had close affiliation with the Washington School of Psychiatry, and I know Dittmann saw patients for free at NIH. And so, they kept their clinical work going part-time as well as doing research, but they didn't really do the clinical work that was required by the research.

Pickren: Did someone like Dittmann, who did clinical work on the side, have to get permission to do that kind of work apart from his NIMH/NIH work?

Zahn: Yes, I think so. I think you had to get permission to moonlight, particularly if you made money. And Dittmann did so on official government property, so he had to get permission for that, too.

Pickren: What kind of interactions – given that all these sections were already established and staffed – did you have or what kind of interactions were typical among the various Lab personnel? Many of you were psychologists. Did you have social times together?

Zahn: Yes, we usually had lunch together and we'd chat in the hall and meet in the men's room, that sort of thing.

Pickren: So was there a sense of camaraderie across the Lab?

Zahn: Yes, I think people went out of their way to socialize. You got closer to some people than others; for example, I never was particularly close to Dick Bell until much later but I used to have lunch with Earl Schaefer quite a bit. He was a Catholic and I wasn't so we'd talk about philosophical stuff, that sort of thing. It was kind of interesting.

Pickren: Ingrid just interviewed him.

Zahn: Oh, good. How's he doing?

Farreras: Really well.

Pickren: I went down a couple of years ago and talked with him in North Carolina.

Zahn: Did you talk to Dick Bell?

Pickren: No, we were going to – we had set up an appointment – but then he suddenly became too ill to talk to us.

Zahn: Oh, really?

Pickren: Yes.

Zahn: Oh, dear.

Pickren: We haven't heard further. This was just two or three weeks ago.
[Dick Bell died December 19th, 2001]

Pickren: You mentioned earlier, since this is an archival interview, that there were some folks who were difficult to deal with. Can you speak candidly about that? This is not for publication only to set the record straight.

Zahn: I guess the person who came to mind was Jack Gewirtz, who's now down in Florida. He was sort of a prickly man and I always felt he was a little bit arrogant, he just came on that way. That's the guy I remember as being particularly difficult and those of us in the Clinical Center didn't have a whole lot to do with the people in the Animal Behavior Section, since they weren't around. At one time later on, I guess after Al [Mirsky] left, I used to go down there for a weekly tutorial session or a little

seminar at Hal Rosvold's place that he was running. I guess he was giving a little mini-course or something in neurophysiology, which I really enjoyed. But on a day-to-day basis, we didn't really have much contact with them.

Farreras: Mort Mishkin mentioned that the basic sections - Animal Behavior, Perception and Learning, Aging – felt a little bit neglected compared to the clinical sections. Did you notice tension like that even between researchers and...

Zahn: No. Maybe they felt neglected. I didn't perceive it that way at the time. I know Perception and Learning didn't really achieve some of the status that the other sections did. Birren, of course, was a real strong chief. He ran a really good section. And Bailey had a big reputation. But I didn't really feel that Birren, for example, got short-shrifted. Maybe they did, but I wasn't aware of it.

Farreras: What about changes of names, both the Lab name change and also the sections'? Were those reflective of Section Chiefs moving in? Bailey had it changed from Developmental Psychology to Child Development and when Dave Shakow retired, the Psychology Lab became the Lab of Psychology and Psychopathology?

Zahn: Well, I don't know about the sections. I know that when Dave Rosenthal took over the Lab he felt that it was based on PR or political considerations. I think he felt that because he had somehow needed more of a practical or applied justification for the Lab. And that was his own interest, too. He didn't want a big Lab like Shakow. He didn't want to spend 90 percent of his time being an administrator. He was really involved in his own research projects, and he took the job very reluctantly.

Farreras: Was he appointed from above to take over Shakow's position?

Zahn: Yes, that's how it worked. But Shakow had important input to this, of course.

Farreras: He recommended.

Zahn: But it has to all come from...this is a very authoritarian place, as you probably know, and the Scientific Director has a lot of power. But I think Dave wanted to focus the Lab, so the Animal Behavior people split off, and I think Aging was still there although I think Birren left around that time. I forget when Birren left. So that was much reduced. And Dave and Jerome were old friends so I don't think he wanted to cut him off. So it was reduced in scope.

Farreras: You're talking about the late 60s, when Rosenthal took over in late '66?

Zahn: Yes. The child research people eventually left, too. Nancy Bailey retired. I didn't know her very well. I never really got to know her.

Pickren: But you knew Dick Bell.

Zahn: I didn't really know Bell too much until later on. I knew him in late years, but not too much at that time. The only person I was close to was Earl Schaefer, who's more my age. So Dave wanted to focus the Lab, he was trying to get rid of people, and so the name change was also an indication of the more focused mission that he envisioned for the Lab.

Pickren: So that was a deliberate move on his part, to downsize the Laboratory?

Zahn: Yes.

Pickren: They had folks assigned elsewhere, etc. Administratively, of course, that means that you're going to become less important in some ways whenever your numbers go down. That's how people higher up look at you – you just don't have the pull that you once had.

Zahn: That's right.

Pickren: Was it also that Dave Shakow had quite a bit of name recognition at the time he came to NIMH?

Zahn: Yes.

Pickren: And continued to have that name recognition, where Dave Rosenthal had a good reputation but not the status of a Shakow? And perhaps it wasn't as important to him either that he have that kind of status.

Zahn: Well, yes. I think when Shakow came, I think he envisioned this like a department of psychology, covering the whole field of psychology. He wanted everything represented, except we didn't get social because social studies were already taken [John Clausen's Socio-Environmental Studies Lab]. But Rosenthal had a different idea. By that time he was really involved in his own work and he really didn't want the job. He took it to be a good guy, to be a good citizen. He would have been very happy to have a section and just focus on the genetics of schizophrenia of interest. And I also learned – I forget exactly when it was – that he had been diagnosing himself with Alzheimer's disease, or at least he thought his mind was going. And he was aware of this very early, long before the symptoms became obvious to anybody else. I heard that from Ollie Quinn, who was a close acquaintance.

Pickren: So he knew something was going wrong.

Zahn: Yes. So I think that's another reason he wanted to go. And nobody picked up on it for a long time until it got really obvious.

Pickren: What do you think about the long-term implications for psychology within NIMH, especially in the intramural program? Was the downsizing, and

perhaps Rosenthal's leadership, overall a negative thing for the presence of psychology at NIMH? Because now it seems psychology is just scattered here and there and is not central.

Zahn: Well, that was the start of the scattering, and I'm not sure that's such a bad idea. I mean, if you want my own personal opinion, I think, in a way, psychology had become integrated into some of these other fields. In other institutes – Cancer, Heart – there are a lot of psychologists. So I think that's a positive thing. I don't think having psychology just focused in one Lab is necessarily a good idea – or a bad idea either – but if it's going to get big it can't be all in one place. Neuropsychology, for example, became a real force around here, and I don't think it would have been possible for that to happen if it had remained a section in some big psychology lab. They have their own Lab staffs; same with child development, which had its own force. It became an important part of the institute. I think it's probably easier to do it as your own branch.

Farreras: When you mentioned Rosenthal's desire to downsize the Lab then, was it because he was more interested in pursuing the research that had been done in the Section of the Chief as compared to the other areas or was it more as a result of budget cuts?

Zahn: The budget cuts probably came along with it but I don't know which came first. All I know is that when I talked with him, when he was agonizing over taking the job, he didn't want to run a big Lab. And once he got rid of Animal Behavior, for example, that part of the budget went over there. Let's see. Al Carron was another guy in the NIMH. He and Jack Gewirtz went over with Marian Yarrow at that point, so there was no more child developmental psychology in our Lab, for example.

Pickren: Is there any link between the reorganization going on within the Laboratory of Psychology and some of the folks in the relevant areas with the establishment of the National Institute on Aging and the NICHD, or are those entirely independent of anything happening in the Laboratory of Psychology program?

Zahn: I think they were pretty independent. I think at one point Aging was trying to get Jim Birren to be an associate director or something and I don't know why he chose to go to USC but I think he probably felt he could run his own show much better out there. But certainly the NICHD was, as far as I know, completely independent, because the people here didn't go there. There was no overlap, no connection. And I can see how they may have thought that an aging section in the NIMH was superfluous if there was an Institute on Aging. But it's interesting that NICHD existed

while there was still child development research going on in the NIMH. But I guess that probably happens a lot. The way that NIH has always been organized, there's physical chemistry going on in institutes that are supposed to be studying arthritis and kidneys, and there's basic research in various scientific fields going on in different institutes that have no relationship to the name of the institute, so why can't that happen in mental health?

Farreras: I was hoping you could tell us a little about the time, shortly after Rosenthal took over in '67, when the NIMH separated from the NIH and then became part of ADAMHA in '73...

Zahn: Right, yes.

Farreras: What happened to the intramural program at NIMH, and the Lab in particular?

Zahn: Very little. It was sort of transparent. NIH still kept honoring the intramural program as though it were part of the NIH.

Farreras: Extramural?

Zahn: The extramural program was probably different. As far as being in the intramural program, I personally regretted having to go with ADAMHA. But as far as my day-to-day work environment, it didn't make any difference at all. It was a very smooth transition. The same way coming back in. I hardly remember it happening. It was like reading about Afghanistan in the newspaper; it was something distant.

Pickren: It's actually unclear to me. Some folks have said that the intramural program in fact stayed within the NIH while the extramural program split off. But it sounds like you're saying that the intramural program did go with ADAMHA.

Zahn: Yes. The administrative hierarchy was with ADAMHA, but we physically stayed at NIH. I think the Scientific Director was still going to the Board of Scientific Directors meetings. So we were treated like we were still part of NIH, but I think that as far as the budget and so on, we were paid through funds allotted to ADAMHA.

Pickren: I want to get back to your career in a moment. But just to follow up on what we've been talking about regarding the Lab, Shakow retired in '66, but he stayed on. Right?

Zahn: Yes.

Pickren: He had an office?

Zahn: Yes. He had an office.

Pickren: He became like senior research scientist or something like that.

Zahn: Something like that, yes. It was like an emeritus position. He had one full-time assistant and probably some secretarial help.

Pickren: And did he keep doing his schizophrenia research?

Zahn: He did a lot of writing, historical type writing, but he didn't do any active research.

Pickren: He wrote that really good article on Worcester State Hospital, that long one that appeared in the *Journal*.

Zahn: Because when he was administering the Lab, he had a limited amount of time. Ask Carmi Schooler [?] if you want to be positive on that.

Pickren: I also wanted to ask you about the Genain quadruplets. Al Mirsky talked some about being involved and I see on your bibliography that you were also involved in that project.

Zahn: Yes. Well, you know Dave Rosenthal may have been hired to coordinate that project with the Genain quadruplets. And so they came and lived in the Clinical Center. I don't recall meeting the quads at that time. Some of these psychiatry types treated researchers in a funny way. But even Rosenthal went along with this. I think they were afraid of upsetting, they had contact with too many people. I was doing some psychophysiological recording on these ladies and he would actually put on some electrodes and tell them what to do, and then leave them and do our little psychophysiological procedure. Well, I was running equipment in another room, and we actually did this in that filming space. We had this equipment in the darkroom, and electrical connections between the place where the ladies sat and the darkroom. So I just recorded the data and then looked at their charts, but I was never allowed to meet them. Now, later on, when Al Mirsky brought them back here for another repeat testing, I got to know them, and they were just delightful, delightful people.

Pickren: What age, approximately, were they whenever you first worked, even though you didn't meet them when you were first working with them?

Zahn: I don't know. I guess they were in their twenties. Al says that they're still alive, at least the last time I asked him about it. I think he may keep in touch with them. I also remember at some point testing their mother, not their father.

Pickren: You were doing galvanic skin response?

Zahn: Yes.

Pickren: Let me segue, then, from that into how you got interested in psychophysiology, because you were the head of the unit on psychophysiology. How did your interest in that develop?

Zahn: I don't know. I just started getting interested in it a little bit down at Duke, before I came here. Rodnick got to be friends with John Lacy and bought some psychophysiological equipment, and I just started using it down there. But I didn't know what I was doing and the research I did with it down there never came to anything. And then Dave Rosenthal had had a few years at Hopkins where there was a guy named Horsley Gant who...

Pickren: Oh, yes, who studied with Pavlov.

Zahn: Right and he got interested in the orienting response from Gant, and I had some experience with this psychophysiological equipment and had some. And we may have borrowed some from another guy here who used it, Kornetsky.

Pickren: I know the name.

Zahn: Yes, he was in another Lab but I don't remember which. He was doing psychopharmacology research in a Lab run by psychiatrists and he had some of that equipment, so I finally learned how to use it. I actually made a trip out to Yellow Springs, Ohio, to visit Lacy, and that's basically how I got trained in a two-day crash course. At some other point, I went up to visit Albert Ax in Detroit, and expanded beyond the galvanic skin response. So the stimulus came first from Rodnick and then from Rosenthal and I really liked the technique. Another thing I might mention, just to give you an idea of the atmosphere of NIH, is that as a young guy the question of tenure was always in the back of your mind. And there wasn't the formal mechanism for tenure then that there is now. I looked around at various people in the institute and saw the ones who stayed and the ones who left or were asked to leave, and the difference was that the ones who left didn't have very many publications and the ones who stayed had a lot of them. So they tell you that this is a place where you don't have to worry about quick results, where you can do these long-term projects, where your budget stays constant year after year... That's great if you're an established person, but the young people have to show they can do something. So I did other stuff with them along with the psychophysiology, like using reaction-time techniques, because I could publish that right away. The psychophysiology took a longer time because I was learning how to use it, there was a lot of data analysis, I didn't quite know what I was doing, I wasn't confident about whether the stuff I was getting out of the technique was meaningful or whether it was just an artifact... I had to convince myself, as much as other people, that this was something I was doing right. So, my first publications were in reaction

time because I could do it and I could get it out. Otherwise, the psychophysiology might have taken up more of my time. I had to keep these two parts going.

Pickren: How did it come about that there was a Unit on Psychophysiology established?

Zahn: I don't know, maybe they just wanted to recognize that this was a separate part of what was going on here.

Pickren: Until Rosenthal became Chief.

Zahn: Yes, I don't know, I didn't ask for it. It didn't make any difference; it was just an administrative thing. Maybe he felt he wanted to give me an identity apart from what he was doing, which by then was genetic projects.

Pickren: So it didn't actually involve any move of your office or move of the Laboratory or anything. It just was a title, if you will.

Zahn: It was sort of a title, yes. At one point there were other people in the unit...we recruited Monte Buchsbaum from Berkeley who worked with Notch [Enoch] Callaway. He was doing evoked-potential research and was a big deal. I know Shakow was really behind hiring him. I think he didn't get hired until Rosenthal was actually Chief, but Buchsbaum's father was a biologist who had been at Chicago and written a book called *Annals Without Backbones*. Shakow was really impressed by it and he had this hotshot kid studying with Callaway out in Berkeley. So they recruited Monty to come here and he was a ball of fire. He started doing a lot of really good stuff. He eventually left the Lab and went somewhere else but he was in that unit for a while.

Pickren: What changes were there in technology that enabled you to ask new questions?

Zahn: I added other channels. I started out with strictly electrodermal stuff, then I added heart rate, some vasomotor things and channels, and then I went to bilateral recording of the skin conductance. And I couldn't have handled the data with just analyzing by hand, so the development of the computer-assisted data analysis was really critical. There were so many mistakes analyzing data by hand...it's very tedious work and there are a lot of errors that come in. I realized I had to have a better way to do it, both for speed and accuracy. I managed to record all my stuff on magnetic tape - great big reel tape recorders - and then I could play them through the Link computer at something like eight times the recording speed. I wrote a little program to analyze the data, pick out the peaks, and you could put that out on punch-paper tape and then read that into another computer, a digital computer like IBM 1620, which is what we had for a while.

And so I developed this system to get the data analyzed in a faster and more accurate way.

Pickren: Now, when you were doing this with your subjects, were the data being fed directly into the computer onto magnetic tape, or did you have to record them in some other way and then key in...

Zahn: No, I had to record them offline on analog magnetic tape, and then the Link had an A-to-D converter to get the numbers in the computer. You tape-recorded in real time and then took the tapes and converted them to numbers on the computer at some later time. Later on in the early '70s, when I had this PDP technology, we got these PDP-11 computers that allowed you to go right through an A-to-D converter bypassing that offline step. So that made it a lot more convenient.

Farreras: I'm curious to know more about this unit you mentioned but let me skip ahead and ask you something else. On your vitae you have that you were head of this unit until '74 and it seems that most of the Lab sections also seemed to vanish around that time, except for the Neuropsychology Section that became it's own Lab in '77.

Zahn: It wasn't until '77 that they became their own Lab?

Farreras: Yes. It changed from the Animal Behavior Section to the Neuropsychology Section in '63 but it didn't become a Lab until '77. But all the other Lab Sections seem to have vanished in the mid-70s and so I'm wondering if you have any theories as to why that happened.

Zahn: I don't know if I can really help you on that. As I say, the unit title, was not something I went around talking about or bragging about to my friends. And I never really wanted any kind of administrative job. I think maybe it was an administrative decision by some Scientific Director. I don't know who, I can't keep track of who the scientific director was then. I know that the best Scientific Director we ever had was a psychologist named John Eberhart. He was a really great director.

Pickren: What era was that?

Zahn: That was during the '60s and '70s but I can't remember exactly when.

Farreras: Sixty-one to '81.

Zahn: Wow. Sixty-one to '81, that long.

Pickren: He'd been at NIMH almost right from the beginning.

Zahn: He'd been in the extramural grants program originally, I think. Yes. And a lot of that time that we were in ADAMHA, after he retired, he still became a special advisor to the director of NIH and had an office in Building 1 because they valued his expertise and confidence in administrating research. So ADAMHA did not directly impact the work I

did and I still reported directly to the Lab Chief. At some point Buchsbaum left our Lab and went to Adult Psychiatry. He was an M.D. but never took a residency. He was oriented toward psychiatry and he did this evoked-response work for a long time and then switched to PET scanning. He was out of the Lab by the time he was doing his PET scans. So that may have had something to do with it, but I'm not really sure.

Farreras: I just haven't been able to explain it: there's this big gap between the mid-70s until about '95 – when Leslie Ungerleider took over the Lab and created these three new sections – where neither the Scientific nor the Telephone Directories list any sections under the Lab, whether under Rosenthal or under Mirsky. I'm wondering if there was only a core section at the time for all those years – almost 20 years - or whether there were any other people working that were not from the core section. There's no mention of them so I don't know whether they just did not exist until Leslie Ungerleider took over and created new sections; there's no record of that anywhere.

Zahn: Well, let's see. At some point in the late '70s – and I'm a little hazy on it – Dave Rosenthal was getting sicker, and it finally got to the point where he resigned or people realized he was ill because it became very obvious. It was at one of these periodic Board of Scientific Counselor reviews that Dave got completely confused about what was going on and what he'd been doing and somebody realized that something had to be done. At that time there were other personnel in the Lab, Herb Weingartner, Ben Carlson... They were also remodeling our area of the Clinical Center and we got moved to Building 31 for several years. At the time when Dave Rosenthal stepped down Herb Weingartner became Acting Chief of the Lab for a couple of years and then they had this big recruitment for a new chief. They tried to get Mike Posner but he turned them down. And I think Sam Sutton was another candidate. I talked to most of these people. Seligman was also a candidate. I think he might have taken it, but somebody in the administration didn't like him.

Pickren: Marty Seligman?

Zahn: Yes, he rubbed somebody the wrong way. So Mirsky ended up with the job at that point, sometime in the early '80s. I can't remember the exact date.

Farreras: Eighty.

Zahn: Nineteen eighty?

Farreras: Yes.

Zahn: Okay. So then it happened a little before that. I think we were over in Building 31 by that time. I still had a Lab over in Building 10 so I had to walk back and forth a couple of times a day. But I think that was probably our only presence in Building 10. That's when they were building the ACRF and we were just waiting for space to come up in the ACRF to move there.

Pickren: What's ACRF?

Zahn: Building 10A, they call it.

Pickren: What does ACRF stand for?

Zahn: Ambulatory Care Research Facility, something like that. In other words, it's designed for outpatients. The Clinical Center was strictly inpatient and it had clinical units that were basically inpatients. ACRF was reflecting a change in clinical care toward a more outpatient basis.

Pickren: The Lab remained the Lab of Psychology and Psychopathology under Mirsky's direction?

Zahn: That's right. Mirsky kept the name.

Pickren: But it's now called the Brain and Cognition Lab...

Zahn: Yes, Brain and Cognition. But it's completely changed. When Al took over, some of the people in the Lab, like Rich Coppola, for example, was doing EEG work with Monty Buchsbaum. But he was not a psychologist, he was actually an engineer. But he became an ex-officio psychologist but he left. Then Herb Weingartner also left – I think Al didn't want to give him everything he wanted, which was considerable – to drug; he's in alcohol now.

Pickren: Is this early '80s?

Zahn: Yes.

Pickren: Not long after Al came in?

Zahn: Yes, and we had some new personnel, like Connie Duncan and Bruno Anthony. So the Lab changed. You expect, with a new Chief, that the Lab would change a little of course. But a lot of the old people left. I somehow hung on. And then Connie didn't get tenure, Bruno Anthony left, and I think they were looking for a way to get more brain imaging going. Al had actually hired Jim Haxby away from the Aging Institute with Stanley Rapoport. And then there was some decision made at some point that they would change the focus of the Lab and hire a new Lab chief. Al got a Section. They got Leslie Ungerleider to...

Pickren: How that happened there, why Al was no longer the Lab Chief?

Zahn: I think they wanted to expand it but not create a completely new Lab and I think they saw Al and his particular research was in decline. And I'm getting old myself so we were sort of expendable.

Pickren: So this is why they brought in Leslie.

Zahn: Yes. She'd been working in the Neuropsychology Lab with Mishkin and I think they wanted to promote her because she's a really first-rate person.

Pickren: What year did they bring Leslie in to be the chief?

Farreras: Well, she started as Acting Chief in '95 and became the Chief in '97. She seems to have just taken her Section from the Neuropsych Lab and moved it to the Brain and Cognition, and then added James Haxby's Section on the Functional Brain Imaging Section.

Zahn: And she got Alex Martin.

Farreras: Yes, Alex Martin in '99.

Pickren: When did the name change?

Zahn: I'm not sure. I don't really remember.

Farreras: Ninety-seven. So, it didn't change until she became the Chief.

Zahn: Yes. I actually retired on my own terms. I was ready to retire. Forty years is a while. And I made a decision before that to stop collecting data in about '95 or so, because I knew that as long as I continued to collect data I would not be able to get out from under it. So that was the first step, and then my retirement wasn't going up anymore because after you get 40 years in you don't get any increase in your retirement, so there was no benefit for me. I was tired of working. I couldn't really give it a good, honest work week or workday like I used.

Pickren: That's certainly understandable. It sounds like, at some level of decision-making at NIH and NIMH, there was some decision made that if you update this whole approach, and change the Lab name to Brain and Cognition, incorporate newer technologies, like imaging, based on physiological or brain processes, with a very current, contemporary spin. Of course, imaging has become very popular since about the early '90s.

Zahn: Yes, well, we had to keep up with the field. This is a powerful technique and its sort of displacing psychophysiology to some extent.

Pickren: When I interviewed Steve Hyman recently, he said that when he became NIMH director, he really wanted to make all the various efforts of NIMH congruent with one another, so this emphasis on imaging certainly fits in with what other scientists are doing, no matter what their disciplinary label. Maybe this was part of the change, part of the work that Hyman facilitated.

Zahn: Yes, I think that's a good assessment of it. It's always been difficult around here because people get tenure for some work, but then the field moves on, and either they move on. So, it's difficult... I know there was a period of time when they probably had too many tenured people and they felt hamstrung by the and when the budget stopped increasing very much or they filled up all the offices – because there's always been a space problem – it's always a problem for the administrators to respond to or create new initiatives, when new technology arrives, for example. There's always been this genetic revolution and microbiology. So, I think that's been a continuing problem, particularly recently, when there's been this explosion in new technologies. But they're doing okay.

Pickren: When I look at NIMH over the past 50 years – very much as an outsider but trying to look at it as a historian as well – it looks like starting somewhere in either the late '60s or early '70s, the emphasis of the Institute overall has become increasingly biological rather than psychological.

Zahn: That's right.

Pickren: That's your sense as well from?

Zahn: Yes, when I first came here, psychiatry was almost all psychoanalysis, and even the people who worked with schizophrenia and like that were basically analysts. It was actually very difficult to work for these... I have always had to scrounge patients from hither, thither, and yon. For example, for a long time, I was getting patients over at St. Elizabeth's. I set up a Lab over there, and I'd trek over there almost every day or several times a week to test patients. That was a consequence of the Laboratory of Psychology not having any beds. But even so, the beds on the Clinical Center – I worked on units run by Seymour Kety, for example – would hold the same patients for maybe 13 weeks or so. If you run two or three studies on the patients in the first few weeks you don't have a lot of turnover unless you've got some sort of a long-term involvement where you go into their projects and look at drug effects on the same patients. I forget how we started this.

Pickren: The switch from psychology to biology.

Zahn: Oh, yes, right. So that was an aside.

But the analysts were reticent to let you use their patients but around the '60s, and I don't know how this started, but, biological psychiatry started coming on like gangbusters, and pretty soon the NIH just had to keep up with the changing emphasis in the field. So they gradually got rid of the analysts and there were some people that could manage... I remember a

psychiatrist named Jim Moss who would go to a meeting or a seminar on some dynamic psychiatry topic and he'd ask these really intelligent questions from that point of view and then go back to do neurochemistry with dogs in his Lab. He had a foot in both camps, and he could do both very well. There are few people like that, who started out with a psychoanalytic bent. Fred Snyder was another good example. He started out with an analytic point of view and then ended up studying sleep in weird animals and what not. I mean the basic neurobiology of sleep. So people would change individually as well as changing personnel to get this new emphasis on biology.

Farreras: Of the original section Shakow had, representing all the fields of psychology, the ones that have survived were psychology.

Zahn: Yes.

Farreras: Aging, more hard core areas of psychology. the softer areas.

Zahn: Yes, that's right.

Farreras: Well, the extramural, however, seems to fund a lot of the softer areas of psychology, in contrast to the intramural program. I'm thinking, for example, of studies like Parloff's treatment outcome research.

Zahn: Yes, I guess he left because he couldn't do that kind of research himself here. I mean, he had some control over the projects, I guess, because he knew how to do research and to direct some of these projects. Have you talked to Morrie recently?

Pickren: Yes, a delightful man.

Zahn: Yes, he is, isn't he?

Pickren: People like Morrie and Marty Katz, even though they were technically in intramural programs, also collaborated with external investigators. I guess they had unusual arrangements to do so. Did you ever participate on the intramural side in a larger study in which there were also extramural investigators?

Zahn: No, I can't recall ever doing that. I used to serve as an ad hoc reviewer on research grants sometimes, but at some point they started discouraging that, and I haven't done that recently. But I've been on site visits and had reviews. I think there was originally a little more of an attempt to get intramural and extramural scientists to have more contact, but I guess when they moved out to the Parklawn Building, it became increasingly difficult.

Pickren: Was that because they did that study in Israel with the kibbutz children?

Zahn: Yes, but that was on a sabbatical.

Pickren: Oh, okay.

Zahn: He went on a sabbatical and set up those two projects. He spent six months in Israel and set up the kibbutz project, and then he went to Denmark and started the study there in collaboration with Shulsinder [?]. So yes, but that was a special case and these were special populations. Dave also had a collaboration for a long time with people at the Patuxent Institution, the place where they – I hate to call them criminals – had people called defective delinquents, who had committed crimes they couldn't get off on by reason of insanity but were still mentally ill people who committed crimes. They had a special institute for that in Maryland, in Patuxent, and he had a long-term collaboration with those people.

Pickren: Okay, so that was an unusual thing.

Zahn: Yes. Now I've always had to collaborate with people in other branches just to get patients. And we set up projects at Walter Reed and at D.C. General early on, but I never really collaborated with the people there. We just went over there and they were kind enough to provide some patients for us.

Farreras: I don't really have any more questions at this point. We haven't really talked about NIMH's return from ADAMHA to NIH in '92.

Zahn: Is that when it happened?

Farreras: I think that's when the various directories reflect it, yes.

Zahn: Again, I can't remember.

Pickren: Because, for you, it didn't make a difference.

Zahn: It didn't make any difference.

Farreras: Do you know why they decided to return to the NIH? I think Earl Schaefer mentioned that they left because they realized it was becoming too applied and too clinical.

Zahn: I think it had to do with the Reagan Administration budget hawks as much as anything, but I don't know that for sure. But I know that Reagan wanted to get rid of a lot of social-based research, and so they tried to get the hardcore scientific corps of NIMH back into NIH and then they set up this other SAMHSA.

Pickren: For service-related stuff?

Zahn: Yes. That's more service-related stuff, but with a much-reduced budget. Now, why we left, it also probably had to do with former NIMH Director Stanley Yolles, who wanted more service-oriented stuff than NIH could tolerate.

Pickren: Apparently, for a while to try to do something like that.

Zahn: Yes. For a while there was another push when maybe all of our sections disappeared, and that was when the people in the administration were

concerned about duplication of effort. They didn't want a Child Research Branch and also have a Child Research Section or Developmental Section in another Laboratory. So that's when some of our child-oriented people like Gerwitz left and went and tried to work with Marian Yarrow. And maybe the Aging Section was another example of that. There was even some effort at that point to avoid the appearance of duplication of effort. Even these people were doing quite different things, but it didn't sound right for the name "developmental psychology" to be appearing everywhere.

Pickren: What about your own research? I looked at your vitae and saw that early on you did schizophrenia research and then you did what they used to call attention deficit disorder, the minimal brain dysfunction.

Zahn: Yes, right.

Pickren: And you've done some work on obsessive-compulsive disorder. So maybe just briefly talk about how your own interests changed over the years?

Zahn: Well, I've always sort of felt that I'm basically interested in schizophrenia, that's my first love, but I went into the other fields partly because I couldn't keep myself busy enough just with schizophrenia. There were not enough patients available. And I got interested in these other disorders partly as a comparison basis. I just wanted to see how specific my findings in schizophrenia are to schizophrenia. With minimal brain dysfunction – which is now attention deficit hyperactive disorder, there's a name change every decade or so – I was really interested in studying the manifestations of attention. I was able to find collaborators in these institutes who were interested in having me test their patients and be a collaborator with them. It was a combination.

Pickren: Back to the question about comparisons across these populations.

Zahn: In social encounters I always found her delightful but then that's a different kettle of fish.

Pickren: Yes.

In the comparisons that you were able to make then across schizophrenic populations, minimal brain dysfunction, etc., what did you find in terms of the tension?

Zahn: Well, I don't know if I can really say this. Since I've been retired for six months I'd have to pull all of this stuff together and maybe think about it. One of the things that I've been paying particular attention to, however, has been the increase in physiological, autonomic activity between a passive, resting situation and a task situation, and it's amazing, across the

board of the various illnesses I've studied. People have shown an impaired increase in arousal during a task compared even to particular anxiety disorders. And even when you control for baseline differences they're somehow not really differentiating their autonomic states according to the environmental demands. I would think this is very prominent in schizophrenia, but it looks to me at this point like it's probably not too specific to schizophrenia. A lot of the things that people in the field have come up with in schizophrenia have been due to the fact that they're always testing patients on some sort of neuroleptic treatment. Here, being in a research institute like this, it's been very useful really to look at these things because I've always been able to get patients off medication. I've just published a big study on taking patients, over a number of years, off the effects of medication – fluphenazine – and trying to show how much of the findings in the field have been due to medication, not schizophrenia. Medication makes a difference in some things but not others. But it depends. The last study I did in the hyperactive field was with kids – a lot of whom had conduct disorder as well as ADHD – but I couldn't repeat what I'd found before, what I thought the literature had shown. So I don't know. It's not easy to come up with any firm conclusions about attention. Now I think ADHD may not be so much an attention disorder but an impulsive disorder. And I think the attention deficits are much different than what you find in schizophrenia, and certainly from the ADHD kids.

Pickren: Is that true in the ADHD adults?

Zahn: I don't know, I haven't looked at that. I don't know anybody that's really done good laboratory studies on that.

Pickren: A few years ago I read – and I can't remember the investigator's name – a report that some investigator was suggesting that perhaps there are some similarities with ADHD adult males and some schizophrenics in terms of attentional qualities. I thought that was quite suggestive. If you think of the old idea of the schizophrenic spectrum, perhaps those adult males who persist with attention problems, ADHD or ADD problems might fit somewhere in that schizophrenia spectrum.

Zahn: It's possible, it's possible. But I'd like to see some data collected under the same conditions in both groups to see a direct comparison.

Pickren: What about your work with obsessive-compulsive disorder? You've published several things on that.

Zahn: Yes, that's kind of interesting. I collaborated on two papers dealing with both groups, adults and kids. The title of the paper was "Is Obsessive-

Compulsive Disorder an Anxiety Disorder?" I think my work supported the idea that it probably isn't, it's something else, and so I think that indirectly supports the idea that some sort of a neurological disorder associated with tics, manifestations like that.

Pickren:

Disorders.

Zahn:

Yes, I think that's the tack that Judy Rapoport's and Sue Swedo's groups are taking right now, that it's quite different from a standard anxiety disorder.

Pickren:

Could it be that it got placed in the anxiety disorders with the DSM classification system because of older theoretical considerations about what an obsession might reflect and what a compulsion might reflect?

Zahn:

Yes, possibly, but the treatment is similar. Both anxiety and obsessive disorders respond pretty well to some sort of cognitive-behavioral therapy, and Prozac or serotonin re-uptake inhibitors are good treatments for a lot of things, including obsessive-compulsive disorders. So there are those grounds for similarity and for looking at it as an anxiety spectrum disorder, too. We might not expect a neurological disorder. I don't know if they tried an anti-convulsant in those people or not. It would be interesting.

Pickren:

So your sense of OCD is that it's probably not an anxiety disorder as other anxiety disorders.

Zahn:

Right, I don't think so. I think people get anxious when they can't do their rituals, but I think it's superficial. But the psychophysiological data would suggest that it's probably not an anxiety disorder.

Pickren:

Do you include or consider imaging data when you mention psychophysiological data?

Zahn:

No, I wouldn't, not for these purposes.

Pickren:

Do the imaging data and the psychophysiological data match on OCD? Are there correlations between the two so that you see activity or lack of activity in certain brain regions?

Zahn:

I can't really say that. I might be able to get some data on some of these disorders on the same patients that have imaging data and try to make some correlation, but I haven't been able to. I haven't done that yet. That's one of the things that I'm hoping to be able to do before I really pack it in.

Pickren:

I have one more question about schizophrenia, one that actually comes from my undergraduate days, and that is the notion that using the label schizophrenia is perhaps a bit misleading, that perhaps we should say

schizophrenias as they may reflect, in fact, fairly distinct disorders. Can you reflect on that some and comment on it?

Zahn: Well, I think this suggestion comes up all the time, and everybody seems to think, yes, there are different kinds of schizophrenias, but nobody has really come up with a classification system that hangs together or that is reliable. Years ago, general paresis used to be thought of as a dementia, and it wasn't until they discovered the physical basis that changed to whatever it is.

Pickren: Spirochetes.

Zahn: Yes, but people had made a clinical distinction before that, but it never really took hold until they found a physical basis, and then they split that off from the schizophrenias. So I think that's going to have to be what happens with this because I've seen these classifications of subtypes of schizophrenia my whole career, but they keep changing. When I was in graduate school, it was the Gudenpoor pre-morbid [sp?], and that's what we were studying at Duke, and I think there's still a lot to that, but that's not really how people look at it nowadays. And there are classic subtypes, paranoid and hebephrenic symptoms and so on, and there's a lot of that going on, too. But that only has a partial overlap with the pre-morbid and the positive-negative symptom thing. You don't know what's going to end up being the real separation now. I think that until they find some really clear physical or genetic difference we won't know if these things transmit true. They probably don't. I think the person with paranoid schizophrenia in their family can have some other kind or have a kid with some other kind of schizophrenia. So you don't know. I remember one of the groups which we studied a long time ago were discordant twins, and it's amazing how screwed up a lot of these discordant twins were. I mean, the ones who were not schizophrenic had their own problems. They just weren't called schizophrenia. I remember one that's almost a classic obsessive-compulsive disorder, and there were very few that you'd call completely healthy. Now, of course, a lot of that has to do with living with a schizophrenic twin, which is not going to be the best environment. But I think this classification into diagnoses is inexact and somewhat arbitrary and until there's really a good physical basis for separation, you're going to get a lot of ambiguous cases.

Pickren: And perhaps the answers will come from genetic research?

Zahn: Well, that's just one tool. Nobody quite knows what makes people schizophrenic. Genetics is only a small part of it, I think. But nobody knows what the basic physiological mechanism is that's driving it. The

brain disorder – I mean the developmental-disorder hypothesis – is probably as good as any, but nobody quite knows what it is exactly. I think this hypo frontality finding in the brain imaging is probably a pretty good start in figuring out what's going on. But you don't know that's a cause. It could be a cause and effect. You don't know if that's the effect or the basic cause. It's really difficult.

Pickren: After 40 some off years of your own work we're still talking about not knowing. How is that for you? Is that frustrating for you, or is it just part of the mysteries of human nature?

Zahn: Well, sometimes I wonder what it would have been like to have gone into something where there's real progress. Yes, it is a little frustrating, I'd like to see how it all comes out. But there's progress. We know a lot more about schizophrenia now. In graduate school I learned that there were schizophrenogenic mothers.

Pickren: We're beyond that.

Zahn: We're beyond that, yes.

Farreras: Related to this theme, what about your 40-year long research career at NIMH, a governmental research institution, versus having tried to create a career in an academic or an industrial setting? Any thoughts about that?

Zahn: Well, what I miss here is having students. I never thought that I'd be here 40 years. I thought I'd be here a few years and then get an academic job. I did, at one point or more than one point, make some inquiries into the academic field, but somehow, every time I did, it seemed to be that everything was drying up and there weren't any really good opportunities, and you have a family and it's very difficult. You have to be sure of what you're doing before you move to southern Illinois or somewhere out in the back. And I've always been a coastal person. So personal reasons are another factor. But I think it would be really neat to have students, and I can't say I miss teaching, but I would have liked to have been able to try to become a decent teacher in an academic setting. But then I would have missed...it's very difficult to do that and also to have a first-rate research project going because the grant business is so difficult. And I don't think I would have been able to do that; I've never been able to think very far ahead. I think trying to come up with a grant proposal every few years would be very difficult for me. I think that there should be different ways of funding. Maybe this is one observation I've had. I think the grant business is fine but now that even the intramural program is increasingly trying to get people, in effect, to write grant proposals for their research, I think that's a big mistake, because I think there are other ways. I think

there should be alternative methods of funding research based on just giving people a budget and just saying, "Look, use this effectively," and let them follow their nose instead of trying to come up with a two- to five-year plan every two to five years. I think this is not possible. I remember reading once about Leo Szilard, the physicist who was in the Lab for a while. He was in on the atomic bomb, I think. But he had an office in Birren's Section for a few months or longer and he had some idea about aging being a matter of being hit by cosmic rays. He was studying this by growing wheat in a cave or something like that. I don't know with wheat without sunshine. Anyway, that's just an aside. I never knew him, just saw him once in a while. But somehow Birren got him in there.

Pickren: An offbeat kind of idea.

Zahn: Yes.

Pickren: Produced some results.

Zahn: Anyway, I read one time that he had never applied for a research grant unless he'd already done the research and he knew how it would come out. So he was always two years ahead of the game. Unless you're a guy like that... But he was a guy who followed his nose. He had this crazy idea that this may come up with something.

Pickren: Well, thank you for today. It's been really nice and really interesting.

Zahn: All right.

End of Transcript