

Eugene Streicher (NIMH 1954-1962; NINDS 1962-1997)

This is an interview with Dr. Eugene Streicher, former member of the Aging Section of the Laboratory of Psychology of the NIMH Intramural Research Program held on January 31st, 2002, in Bethesda, MD.

The interviewer is Dr. Ingrid Farreras of the NIH History Office.

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

Streicher: Sure. I was born in Brooklyn, New York, 75 years ago. My father was a teacher, school principal and examiner. My older brother taught science in high school and community college. I went to elementary and high school in Brooklyn; then went to Cornell University.

Farreras: Were there any parental or high school influences that led to your interest in science?

Streicher: I was a member of the biology club in high school.

Farreras: OK, is that what you majored in at Cornell?

Streicher: I majored in zoology and minored in psychology. After less than a year there I was drafted into the Navy and returned to Cornell after the war was over and after all the veterans were discharged. So I returned in September of 1946 and then graduated with my B.A. in June of '47.

Farreras: How did you end up choosing zoology as a major?

Streicher: Well, I was interested in comparative behavior, animal behavior, comparative

neuroscience. That was my area of interest. However, this was a time of very feverish activity, to get in as many courses as possible before I was drafted and then to finish as quickly as I could after I was returned. My interest realized only in part; I stayed with zoology since I had started with it. In fact, the courses were very interesting and I liked the naturalistic aspects of it, such as ichthyology and herpetology field trips. But Cornell had a distinguished Pavlovian laboratory of psychology; Dr. Lidell was there at the time. He was a close friend of Pavlov's and set up his laboratory there years before to study the conditioning of goats. I had thought of combining the biological background and the psychology but that was never fulfilled because of accidental happenings along the way that was very common at the time.

Farreras: But you knew from the beginning that that's what you wanted to do?

Streicher: No, those were just the two areas that sounded most interesting to me.

Farreras: What led to that interest?

Streicher: The idea of trying to compare the divergences of animal behavior with differences in brain structure.

There was also at Cornell a very famous neuroanatomist by the name of James Papez, and when the Cornell Medical School had left Ithaca years earlier to move to New York City Dr. Papez stayed there. He just liked Ithaca. So he gave a number of courses in physical anthropology and in comparative neuroanatomy. When I returned to college and finished the B.A. I stayed on for a master's degree. There was a new course given called comparative physiology given by a young but distinguished investigator, Donald Griffin. He was one of the people

who determined how bats navigate in the dark. This was due to their radar-like apparatus. At the time that I finished my B.A. degree, each of the professors in the zoology department were strictly limited to only two graduate students. There wasn't a slot open for a graduate student but one of them, who was enrolled there, took a leave of absence to do extensive field studies and therefore left a vacancy for one year. So Dr. Griffin kindly invited me to stay for that one year if I could complete a master's in that period.

Farreras: In one year?!

Streicher: One year. And so that's what I did. And then he suggested that rather than go on immediately to further graduate work, that I take a job for a while to see what most interested me. He helped me obtain a job at a place called the Army Chemical Center in Edgewood, Maryland, which is now part of the Aberdeen Proving Grounds. This was in the medical division, and the medical division was devoted to trying to protect soldiers, servicemen, against the effects of poison gas. There my own work was primarily as an applied physiologist. The problem was that these poison gases, which were originally developed in Germany and then picked up by the Russians as well as the Americans, the so-called G agents or anti-cholinesterase agents, was that they were so permeable that any entry into the body could be lethal. The group that I worked with was concerned with protective clothing – not the manufacture of protective clothing, but finding gas masks and impermeable, almost rubber-like suits. This produced great physiological stress and our job was to measure the stresses that the protective clothing caused.

At that time many professors felt it was their obligation to place their students after completion of their Ph.D.s, mostly in teaching jobs. This was an obligation many professors took seriously.

I worked at Edgewood for two years and then realized that, regardless of what I decided to do, I really had to get a Ph.D. And by that time, I was very much involved in the nervous system because the key agents were, of course, toxic to the nervous system. I'd always been interested in the nervous system, the behavioral relationship. And so I then went to the University of Chicago and took a Ph.D. there.

Farreras: Why Chicago?

Streicher: Well, it turned out that there was a distinguished investigator there, a very prominent man by the name of Ralph Gerard, who had a large group of graduate students, as did the University of Chicago. The proportion of graduate to undergraduate students was very large, and it was suggested that that was the way to go. So I went to Chicago in 1950 and got my degree in 1953.

Farreras: Was Gerard the person you had most contact with at Chicago, the person you worked with the most?

Streicher: Well, he ran a large operation so my most meaningful exposure was not only to him but to other advanced graduate students. He was very much occupied traveling, writing papers, editing papers, and so forth, so that while I saw him whenever anything came up, he was very busy most of the time. And when you saw him it was basically to present data and perhaps guidance for future work. At that time, the techniques of radioactive tracing had just come in – P32 and so

forth. Since this was after the war, there were a number of older experienced students there, they were very helpful teachers. After graduation, I worked for a number of months in one of the laboratories in the hospital that was concerned with measurement of hypothyroid metabolism. At that time I ran across Dr. Jim Birren, who offered me the job of joining him at NIH for this new Section on Aging in the Laboratory of Psychology. I came here on January 11th, I think, 1954.

Farreras: Why did he offer you the job? What type of relationship did you have that this opportunity would come up?

Streicher: When I was working at the Army Chemical Center from 1948-1950 I rented a room in a rooming house in Baltimore, and one of the other roomers was Jack Botwinick. Jack Botwinick was working at the Laboratory of Gerontology in Baltimore, which was under the National Heart Institute, and this was headed by Dr. Nathan Shock, a very prominent man in gerontology. He originated that laboratory post-World War II as part of the Heart Institute in Baltimore, not in Bethesda. Jack Botwinick was working for Jim Birren. Later, that Laboratory was transferred to the National Institute of Child Health and Human Development [NICHD] after the NICHD was established [in 1962]. But few people were particularly interested in aging in those days, and the Heart Institute was glad to give it over to NICHD, that needed some more activity; this was the human development part of the National Institute of Child Health and Human Development. And still later this laboratory became the origin of the National Aging Institute [established in 1973]. So our Aging Section in the Laboratory of

Psychology was disconnected from the Aging Institute; our section had closed down for lack of interest around '62, '63, when I left. The name stuck around, I think, for a while longer. Birren had already left; he went over to the NICHD's extramural program in about '64 to take care of the grants and contracts and from there he went to California to head up the Gerontology Center at the University of Southern California. So Birren went to NICHD and I went to Neurology [NINDB]. Now, there was another man in the Aging Section who stayed on, a man by the name of Ed Jerome. He was an extraordinarily nice man but a very quiet, brilliant person. And working with him was a woman whose name I can't remember. And the two of them really constituted the remnants of the Aging Section. Jack Botwinick left to go to Duke University. There was a neuroanatomist – Bill Bondareff. He left to go to medical school and then went to Northwestern and was head of the Anatomy Department. He took a residency in psychiatry, and then headed an experimental clinical ward at USC. He's been at the University of Southern California for a long time. So we had left. Ed Jerome and this other lady were the only ones left in the Section. They were psychologists. Botwinick was a psychologist; Bondareff was a neuroanatomist; I was a physiologist; and then my assistant, Joel Garbus, was a technician. He did some biochemistry and left to go to the University of Wisconsin to take a Ph.D. Except for Jerome and this lovely woman who worked with him [Marguerite Young], the Section had really disappeared by the early '60s, say '63. Jerome was a very intelligent and very clever investigator with human subjects.

Farreras: So when you met Jack Botwinick in the late '40s, he was already working at the

Gerontology Lab in the Heart Institute in Baltimore and Jim Birren was there as well? How did you come to meet Birren in Chicago then?

Streicher: Yes. Jack Botwinick left Baltimore to take a Ph.D. in psychology. Birren – and this is something I surmise because I ran across him at the University of Chicago – at some point, took sabbatical leave. He was in the Public Health Service and the Public Health Service is more tightly knit than the civil service. He took a sabbatical for either one year or two years to go to the University of Chicago to do a very complex analysis, which was in vogue at the time, called factor analysis. He worked with a group at Chicago that did advanced mathematical biology. It was called the Committee on Mathematical Biology, and it had Dr. Rashevsky and some other very famous people on the staff. He went there to do a complex factor analyses involved in aging. I think that before he left Baltimore he had prearranged with someone, perhaps Dr. Felix or people in the Public Health Service, to start this Aging Section in the Mental Health Institute when he returned from his sabbatical at Chicago. I think he wanted, as a psychologist, to proceed on the behavioral aspects of aging. In the Laboratory of Gerontology, where he was, he and Jack Botwinick did the psychological studies, but it was very limited relative to his major interest. The Lab's primary interests were things like diabetes, liver disease, kidney disease, even on an experimental basis. Shock was interested in physical illnesses. That group in Baltimore established that famous longitudinal study where they examined people from the community over the course of 20, 30 years, until they died, and measured their cholesterol, their blood pressure, their medications, etc. It's a well known longitudinal study,

and it was and still is a gold mine for papers. But behavior was not an area of real interest to Shock. On the other hand, Shock was a pleasant enough and smart person. But Birren wanted to elaborate on the behavioral side, and I say behavioral, but it included brain and behavior. And so he had arranged for the Aging Section to be created within NIMH.

Farreras: Was it specified that it would go under or within the Lab of Psychology or was it just prearranged that it would be somewhere within the NIMH intramural program?

Streicher: I don't know, but by the time I got there in January of '54, it was already a part of the Laboratory of Psychology.

Farreras: Alright, and then it went from the Laboratory of Psychology to the NICHD...?

Streicher: Oh, no. It was Shock's Lab in Baltimore that went to the NICHD.

Farreras: Oh, so it was just those people working on the behavioral aspects of aging that went to this Aging Section in the Lab of Psychology at NIMH but otherwise the Gerontology Lab remained in Baltimore and that's what eventually moved to the NICHD?

Streicher: Right, and to this day, they're still in Baltimore and now the intramural part of the Aging Institute.

Farreras: Alright, I see now. So only Birren and Botwinick left the Gerontology Lab to form the NIMH's Aging Section within the Lab of Psychology.

Streicher: Yes.

Farreras: Bondareff was not there at the time?

Streicher: No. But neither Birren nor Botwinick went to NIMH immediately. Botwinick

left to get a Ph.D. and Birren left to do his factor analysis. And then Birren recruited me before he got back to Bethesda. And then at Chicago he ran into Bondareff who was getting his Ph.D. in neuroanatomy. So we constituted the first nucleus for the Aging Section, before Dr. Shakow arrived. Dr. Shakow came later. If you talk to Dr. Carlson he will know a little bit more about the negotiations that went on before Dr. Shakow came. I think Dr. Shakow was supposed to come earlier but was recovering after a heart attack. The person who was temporarily put in charge of the Laboratory was a man by the name of Richard Bell. Dr. Shakow came in June of '54, or something like that. It's hard to tell, but I think Dr. Shakow just accepted the Aging Section. He stuck with it. It was very far from his interests. He was a non-smiling, very intelligent man, and he was into therapeutic approaches to psychology. He was very close to Dr. Cohen. Dr. Cohen came from the analytical school of psychiatry and although Dr. Shakow wasn't a psychiatrist, he came from the Department of Psychiatry at the University of Illinois and was also very analytically oriented. I believe he just accepted the section.

Farreras: I'm curious - I've heard mixed reports about this – my impression was that Felix and Shakow wanted to create a Lab of Psychology that was representative of all of the major areas within the field of psychology, so they created these various sections that were very distinct, and that then they would try to hire the best people to work in those sections.

Streicher: Right.

Farreras: But the other report I've heard is that – given that these are the early days of the

Lab at NIMH – that they were trying to hire young, bright people with great promise or vision and *then* create sections around those people. So I wasn't sure whether it was the sections or the people that came first.

Streicher: Both stories are true; it worked both ways. They had young people and very few senior people. Have you talked with Morrie Parloff?

Farreras: Yes.

Streicher: He was one of the few senior people. And there was another man just about as senior as he was.

Farreras: Hal Rosvold?

Streicher: Rosvold was senior. He came from Yale and was really established except that Yale had this policy of having dual faculties, one faculty got tenure and the other faculty didn't, but they were indistinguishable except for that. And when time ran out, he had to leave. But he came as a senior person and an accomplished investigator, and the people who came with him were sort of junior but accomplished.

Farreras: Mirsky and Mishkin?

Streicher: Both talented. So that both of these dynamics operated. Now Rosenthal was there. But that's it. They'd chosen very bright people, smart and willing and verbal. You know, their sections were really terrific. I really enjoyed them because they were so clever and they threw out interesting ideas. I remember one project which I thought was ingenious. There was a young man who worked with Parloff, Boris Iflund. The two of them were really lovely people. They had decided to do a study on a therapy. They wanted to see was how therapy

progressed in terms of subject matter. They would use a professional therapist and Parloff and Iflund would watch the therapy through a one-way window. They'd make a list of the subjects that the therapist and patient discussed during the session. After the therapy session was over they'd give the list to the patient and say, "Of these subjects that came up during the hour, can you arrange them in order of importance to you?" Then they did the same thing with the therapist, and the therapist would also arrange them in order of perceived importance. They did this after every session. What they found was that as the therapy progressed, the order of importance, let's say of 10 items arranged by the patient became closer and closer and closer to the order of importance assigned by the therapist.

Farreras: Shows you who's running the show!

Streicher: That's it! But it was just so clever, so simple. That was Iflund and Parloff; just an example of how bright they were. These were the early days. Then there was a person interested in child psychology. That's a different area.

Farreras: When you came were those other areas or sections already there?

Streicher: Yes, well, there was a Section on Perception and Learning.

Farreras: Headed by Ben Carlson.

Streicher: They were in the same shape we were. Ben had been hired and they eventually changed the name from Perception and Learning to Perception because he was doing the Perception but they never got around to hiring someone in Learning, so he just dropped the name.

Farreras: And Hal Rosvold's Animal Behavior Section.

Streicher: Yes.

Farreras: Nancy Bailey's Early Development Section?

Streicher: She came later. And here again, she was a giant in the field. She was involved in the original study of talented children many years ago. It has a name, but I've forgotten it. And Bailey re-measured these kids as they grew into adults. I forgot the name of the man she worked with, a very famous psychologist as well, out in California. Then she came here and she could have whatever she pleased.

Farreras: Dick Bell and Earl Schaefer were already here when she came, weren't they?

Streicher: Yes.

Farreras: But there was no Early Development Section yet was there?

Streicher: Well, everyone knew that they were interested in child behavior, so whether or not there was a Section initially on child behavior, this interest goes back right to the start of the Laboratory. These two people who were interested in child development. That was or became a section.

Who else? Parloff...

Farreras: In Personality?

Streicher: Yes, right. Parloff was exactly the guy Shakow wanted. Rosenthal, too.

Farreras: But Rosenthal was in the Chief's Section, with Ted Zahn and Al Dittmann...

Streicher: Yes. The Chief's Section was a hodgepodge. Again, they were all very talented people and very smart.

Farreras: Did Shakow do most of the hiring at the level of Section Chief and then the Section Chiefs would hire their own people or did he oversee all of the hiring of psychologists?

Streicher: Well, he had nothing to do with hiring Carlson. I don't think he had much to do with arrangements for Birren either. Ben Carlson was here. Schaefer was here. Bell was here. Then Dittmann and Rosenthal were basically interested in what Shakow was interested in, which was aspects of dynamic psychology. And Parloff was interested in personality. Parloff got involved in a very important study after he left the Laboratory. It was very influential on the effectiveness of various treatments; they basically didn't make any difference. But that was after he left the Lab. Dittmann left to go to the Department of Education downtown, HEW. Rosenthal and Dittmann both died.

Farreras: Were there any other psychologists outside of the Laboratory of Psychology?

Streicher: Yes, there was a large Laboratory of Socio-environmental Studies.

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Clausen was a very effective guy. His group had social psychologists.

Farreras: Oh, not sociologists, but social psychologists.

Streicher: Well, they had both, but from the studies that were done, there was a heavy emphasis on psychology. The point is, it may be artificial, for the studies they did, to pull them apart. But they were psychologists or social psychologists. And they did a fair amount of research. Of course, they were interested in social class, for example, schizophrenia as a function of social class; other psychological aspects as a function of social class and other sociological variables. That

laboratory met the same fate as the Laboratory of Psychology later on, but one of the survivors is still around.

Farreras: Carmi Schooler?

Streicher: Carmi, yes, right! Now, I'm trying to think of Ted Zahn. Was there a Section on Schizophrenia?

Farreras: The Section of the Chief was where most of the research on schizophrenia was done. Shakow, Rosenthal, and Zahn were all there.

Streicher: That's right. That's why I think Rosenthal sometimes used to call it the Section on Psychopathology.

Farreras: Well, I meant to ask you about that – this is where I also get conflicting stories – in the Telephone and Scientific Directories, the name for the Lab of Psychology changes to the Lab of Psychology and Psychopathology. Al Mirsky thinks that that name changed when Shakow retired and Dave Rosenthal took over the lab.

Streicher: That's a good guess.

Farreras: But that was '66, and the Directories reflect that name change only in '75. So I'm not sure whether they're just slow in catching up on the change or whether it was really in '75 that the name was changed.

Streicher: I don't know, because, for example, the Section on Aging was mostly over with, for the most part, in '62, '63, but I don't think the name was changed. Or I think Aging was dropped for some other name.

Farreras: Higher Thought Processes, in '66.

Streicher: Right, that really was Ed Jerome. He was still interested in aging and he and Dr. Young devised problems for older people and found that aging was very

susceptible to complexity in problems they had to solve. As we were struggling to find minimal changes, he really found very significant ones; he was a very clever investigator. I don't know if you ever heard of Woodworth, a famous psychologist of years ago.

Farreras: From Columbia?

Streicher: Yes, Columbia. Jerome was his student, and he had a very thorough background in experimental psychology. And he would devise mechanical equipment to present problems. He'd have a box with a light in the middle and nine lights around it, and the problem consisted of getting that middle light to light up. It was a red light and the other lights were white. He'd give the box to someone and say, light up the middle, red light. And all these other lights had buttons. Well, the person started out very simply, pushed this button, the light went on. In the next problem you had to push this button first, then that button, and then the light went on. He proceeded stepwise in difficulty and when he compared college students with older people the differences were qualitative as well as quantitative. Now, these differences are taken for granted. But he did a very fine job of showing the importance of the relationship of aging to the problem of complexity in thought processing, and problem-solving. At that time, it produced hard data. From '62 to '66 it may have still been called the Section on Aging, but that was the only thing that was going on.

Farreras: Did anybody, whether in the Aging Section or in any of the other sections, collaborate with people from other labs? You said Socio-Environmental Studies had a lot of social psychologists...perhaps collaborations with them?

Streicher: No, there really wasn't a lot of collaboration. There were a lot of BS sessions, which is inevitable because you have psychologists who do their research in areas such as personality and psychotherapy. They were extraordinarily intelligent, and the experiment that I described to you was one of those they cooked up, they devised others that were clever and insightful. Shakow had always dreamed of photographing a psychoanalysis...

Farreras: And he did.

Streicher: Yes, he did. And you can get a much better evaluation of that from people like Parloff. But it seemed to me that there really wasn't much collaboration. Everyone did their own thing.

Farreras: Is there any sort of veracity to the claim that David Shakow wanted – in terms of what you were saying about psychologists doing research versus doing testing – to have control over the hiring of psychologists at NIH, as a way of protecting the field of psychology, acting as a gatekeeper or sorts?

Streicher: No. He would have *liked* to have done that. If it had been possible or could have been arranged, yes, he would have liked to. I'm sure he would have wanted to control that. But whether he did or could have, I don't know, because in Clausen's lab, where you had the social psychologists – he didn't hire them. But yes, Shakow would very much have liked to do that. In the Neurology Institute they had a succession of very fine psychologists, one at a time, who did very, very important work on deficits that occurred in epileptic patients when certain areas of the brain were operated on. They had nothing to do with NIMH but these are the people who established which areas are non-operable, because the deficit is too

drastic. They were classically trained psychologists, and I'm sure Shakow would have loved to have been consulted but I don't know if he ever was.

Farreras: Weren't NIMH and NINDB together in those early days?

Streicher: On the basic side, but not on the clinical side.

Farreras: Oh, I see.

Streicher: On the clinical side they were very separate. The Laboratory of Psychology also had a basic and a clinical side. For the most part we think of Dr. Cohen as Clinical Director but Psychology was clinical and basic as well. The interests in basic research of NINDB and NIMH were indistinguishable in the early years and so they just divided the costs up between the two institutes. For example, a very important laboratory, the Laboratory of Neurophysiology, under Wade Marshall had a Section on Cerebral Cortex in NIMH and a Section on the Spinal Cord in NINDB, and the single laboratory was divided up between the two institutes. On the clinical side, they were completely separate.

Farreras: So the funding only went to the joint basic labs.

Streicher: Right. Their shared interests in electrical activity of nervous system, neurotransmitters, neurochemistry, etc., and so they shared the costs. But later they completely separated.

Farreras: Do you know when that was? Before NIMH went to ADAMHA?

Streicher: Yes, I think so. I think it was an opportunity to double the space; that is, each institute had its own basic side, so you could have the Laboratory of Neurophysiology in NINDB and a Laboratory of Neurophysiology in NIMH.

Farreras: Okay, so that was the reason why they ended up splitting up?

Streicher: Yes. The other reason, I think, was that the Clinical Director of NINDB, Milton Shy, an ambitious person, believed that because of this combination NINDB didn't get its fair share of recognition.

Farreras: From Congress or the scientific community?

Streicher: From the scientific community, because the basic side was more often associated or identified with NIMH than with NINDB.

Farreras: But he was in clinical, why would he care...?

Streicher: He was, but he was interested in a larger and more influential institute. It also may have just been part of the idea of having the money and the space to become independent and to increase the number of people in the basic side. I'm not certain. But they did separate.

Now, after Mental Health became part of ADAMHA...

Farreras: Do you know what led to that? Because before ADAMHA it even became a part of Health, Education and Welfare...

Streicher: Well, this was after I left but as I understood it it accomplished several purposes. They had two new initiatives, which were feeble at first. One was alcoholism and the other one was drug abuse. They were obligated to support research in that area.

Farreras: Because of pressure from Congress?

Streicher: Yes, and they also had no intramural side to start with. It takes time to create a laboratory. You need space. And so, in order to bolster this, they added NIMH, which was relatively a juggernaut, to give this substance. Alcoholism, drug abuse, and mental health...together there was substance to it. All of this effect

was extramural. Intramural stayed exactly where it was, not a smidgeon of a change. Everyone did his own thing intramurally and was not affected at all. This was all to provide recognition, PR. One of the rationalizations of NIMH separating from NIH was that at creation NIMH had a different charter than the other institutes. It was a long time ago and I may be in error but within the documents that created the institute there was a provision for community service, which was different and unlike any of the other institutes. The other institutes were strictly forbidden to do any kind of medical or community service (as opposed to research). The patients in the clinical center were all research patients and they had no direct interaction with the clinical communities, except through extramural funding grants in centers. But it was part of the NIMH guidelines that they would provide community centers and they would have other extensions into community services, and I think that was one of their rationalizations. They were different from the other institutes in this way.

Farreras: So this seems to be something that happened outside of NIMH decision-making. NIMH didn't really have a say in any of this?

Streicher: Right.

Farreras: So who is making this type of decision?

Streicher: Well, it was primarily a congressional push, that alcoholism, drug abuse, and mental health should go together. On paper, they looked like they should go together, and they were all related to community problems or to community behavior. It looked like a natural union, but they needed some substance. But the intramural people were left completely alone. The only thing that happened, I

think, is that the administration moved over to one of the buildings on Rockville Pike, the Parklawn Building. So you had the NIMH headquarters move from the campus over to the Parklawn Building, where drug abuse and alcoholism were also located. And what happened was that NIMH had a lot of experience with extramural grants and contracts and was able to help alcoholism and drug abuse get started, put together review committees and so forth. But I can't remember anyone here who was enthusiastic about it.

Farreras: I've heard that when NIMH left NIH and became part of ADAMHA – but was still geographically here – that it caused tension as far as space was concerned. That because there's always such a need for space here on the campus that the other institutes would have liked for NIMH physically to leave the campus in order to open up that space for the remaining institutes...

Streicher: Well, that probably did happen. People aren't very kind to each other, space is golden. Space means potential positions and so forth. So I'm sure there were some people who said, "Okay, you're in a different agency. Find space someplace else." I think what they did in the end was the correct, mature thing to do. The new agency really had no congressional relations with respect to the intramural program. Many people wanted NIMH to expand because of the great need for mental health research and services. There was always the feeling on the part of some that not enough attention was being paid to practical problems. For example, until just a few years ago, people bemoaned the fact that very little was going on in research on schizophrenia, compared to the problems of children with

learning problems, clinical depression...and this was an opportunity for them to get more money, to get an agency, and a name.

Farreras: Would you say that congressional pressure or contingent funding influenced the type of research that was done at the intramural level?

Streicher: Oh, no, not in the least. It had no influence at all. When I worked at NINDB extramurally I'd get calls requesting information on the funding of disease-related research, "How much money are you spending on Parkinson's disease?" I was in basic science in the extramural program. On occasion someone would comment, "You're spending so much money on basic science. How much are you spending on Parkinson's?" "How much are you spending on spinal-cord injury?" "How much are you spending on epilepsy?" Money spent on basic research takes a long time to become clinically relevant, and yet there are so many thousands of people who are in pain. Many feel that money equals progress. If you can get enough money thrown at a problem, it'll move faster. It may be less efficient, but it will attract more investigators, and even if they're stumbling around, they'll do more, which is better than not. So this gave them a target.

Were there any other areas that you were interested in?

Farreras: Well, let me see. I was just going over Dr. Mishkin's first transcript, and he mentioned the division between the basic and the clinical sections, that the basic sections were muttering to each other because he didn't feel that they got as much recognition or attention as the clinical sections. Did you also have that sense?

Streicher: He's right in that. Mishkin is a very productive man and he and Rosvold did tremendous pioneering work in identifying brain areas functionally in primates –

in monkeys and chimps – and in a way they were never given the recognition that they deserved because they were really world-famous within their field so I can understand his feeling. But they were left alone to do their thing. They were never deprived of funds or told, “you can’t do that”.

Farreras: So funding wasn’t taken away from them and allocated to the clinical sections.

Streicher: No, they would have appreciated more help and they felt that – given their accomplishments – they deserved to expand. They basically only had a few postdocs at a time, but they were very productive. And later they became a laboratory.

Farreras: Yes, the only original Lab Section to have survived.

Streicher: Right, that was, eventually, their recognition, but that was late in the game. But, on the other hand, I think Mishkin may have overstated it in a way. He and Rosvold and their group were very good. He knew he was good, and I don’t mean that in a boastful way, but I think that may be the origin of his feelings.

Farreras: So the Aging and the Perception Sections didn’t really feel that much of an antagonism with the clinical sections?

Streicher: No, to be honest, both of our sections were pretty much ignored.

Farreras: By...?

Streicher: Well, I think Shakow was nice enough. I think that we were basically ignored and allowed to do our thing. I want to point out that during this period, Birren and Jack Botwinick made some pioneering monumental contributions to the modern basis of the study of aging. Birren organized this large study on normal older people living independently in the community.

Farreras: The interdisciplinary study that ended up coming out as a book?

Streicher: Yes, right. Most previous studies were on the sick elderly but these were normal people. And one of the investigators became the first director of the Aging Institute, although a young man at the time, [Robert] Butler. He did the psychiatric evaluation of these people. They had their cerebral blood flow measured, they were tested psychologically, and that volume was extraordinarily influential in showing that these older people, living independently, were really in good shape. So that was one major contribution. Another one was the concept of timing in the nervous system. Early in the game Birren felt that the time that it took to perceive something or the time it took to react to something was important. He did these extensive reaction-time studies but reaction time is a very crude thing. When the light goes on, press the button. What happened was that the older people were slower. Now, we're talking about the difference between 150 milliseconds and 200 milliseconds or 225, a very short period of time, and for the most part, what difference does reaction time make? It probably also wouldn't make much difference in time of a reflex such as pulling your finger away from a hot object, because the pathway is very short. On the other hand, he mulled it over extensively and realized that, in most behaviors, the pathways are very complex, many neurons and many synapses are involved, so that while there may be no measurable difference in a synapse, when you add them together in a complex chain, there very well could be important timing differences. For example, how many numbers can you remember. Botwinick, early in the game, came up with a very significant finding, and that was on the Wechsler adult

intelligence tests. The older people did poorly on a number of these tests. What he did was have older people take the test without any time limit, just take it 'til they were finished. Now, of course, they took much more time...

Farreras: But the performance wasn't impaired...

Streicher: Right, they did just as well. A clue that the time was important. Nowadays, you talk about cognition time, registration time, and so forth, but that's all based on these early contributions to some of the underlying features of aging.

Farreras: Why wasn't Birren made Director of the Aging Institute? Why did they end up assigning Butler?

Streicher: Well, Birren wasn't a physician.

Farreras: So it was necessary to have a medical degree?

Streicher: Oh, yes. There were only a handful of directors who were not physicians. There have been a few, but not very many. After he left NIH, Butler settled in the area and practiced psychiatry and then specialized in geriatric psychiatry. He made a name for himself but this came much later. He turned out to be a very good choice. It turned out he was very intelligent and politically astute in terms of understanding the ongoing give and take between institutes. When you're dealing with normal people, i.e., in development of children, that research is assigned to Child Health. When you study epilepsy in children that research goes to Neurology. In other words, pathologies, what divides normal and abnormal determines the institute assignment. When people became interested in the aging of the nervous system because of Alzheimer's, Neurology claimed Alzheimer's as its own. But Butler had already given testimony to the Congress on Alzheimer's

and the Congress believed it appropriate to leave it with the Aging Institute.

Without Alzheimer's the Aging Institute had very little on the clinical side. He was very astute.

Farreras: I thought that institute was primarily involved in basic research?

Streicher: Now, that's different. In Baltimore, it's still primarily basic, and they do their longitudinal study and so forth. But when Butler came in he started a clinical program on Alzheimer's. A friend of mine was running it, Dr. Rapoport.

Farreras: Related to Judy Rapoport?

Streicher: Her husband, yes. You know Judy?

Farreras: I know of her Lab. I don't know about him.

Streicher: They're both very well-recognized people in their own fields.

He ran the clinical side of things, which did cerebral blood flows, all sorts of imaging and set the stage for the investigation of the Alzheimer's pathology, a tremendous amount of work. At the time that was the only laboratory in the Aging Institute that was on the campus; it was at the Clinical Center where they utilized the radiological facilities. They also did basic research, he and Butler were very close. Then Butler left and the new Director was not interested in the clinical area, so the Laboratory of Neuroscience that Rapoport headed was drastically cut back. They gave up their clinical work, and NIMH and NINDS picked it up. They continued the work that Rapoport had begun and Rapoport is now doing basic research in other areas. They gave up their Alzheimer's connection intramurally. The rationale for that is that many laboratories and hospitals throughout the country are interested in Alzheimer's and can be

supported through the extramural program while giving up this very expensive intramural program. NIH is a place where you are encouraged to do innovative research and where you're not worried about the next grant. Rapoport came up with many important contributions on the normal aging brain, and the Alzheimer's brain. But that's past history; his former post-docs have now picked up that work. He's satisfied to have his laboratory and to be working away in a very interesting, important area involving fat metabolism in the brain. Many think of lipids in the brain as being inert structural compounds. It turns out that certain aspects of it are very dynamic, closely related to impulse transmission.

TAPE 2, SIDE A

Farreras: I realize that this was after you left but do you have any thoughts as to why the Sections of the intramural Psychology Lab disappeared? Well, the Neuropsychology Section became its own Lab of Neuropsychology but what about the other Sections?

Streicher: I don't have a clue but I was amazed at the change. Now, you know that Mirsky left here...

Farreras: In '60, I think.

Streicher: Right, and then came back in '80 to pick up what was left of the Laboratory. He's an extraordinarily nice person. I wish we met each other more often.

Farreras: Yes, he is. And I think all of the Sections had pretty much disappeared by the time he returned in '80.

Streicher: Yes.

Farreras: Morrie Parloff seems to remember the decline started in the late '60s, and he volunteered that it might have been this increased emphasis on a biochemical approach to mental illness in the early '70s.

Streicher: Yes, that was in my mind. The Adult Psychiatry Branch underwent a 180° change.

Farreras: Lyman Wynne's Branch?

Streicher: Yes, Lyman Wynne. Until that time, with Dr. Cohen as Clinical Director, it was heavily analytically oriented, not only in individual psychology but also family psychiatry, and the people involved were traditional therapists, with an analytical perspective. Did Parloff tell you that he himself underwent psychoanalysis?

Farreras: Yes, Shakow sent him for training.

Streicher: Yes, exactly.

And it was my impression that things changed when the pharmacologists and biochemists came along with drugs. What were the first drugs?

Farreras: Thorazine?

Streicher: Yes, right, and then the emptying out of institutions. This represented, perhaps by coincidence, a decrease in interest in the more clinical aspects of the Laboratory. The Socio-Environmental Studies Lab was also hit hard. Clausen, the Lab Chief, fared very well. He did the same thing that Birren did – he went out to California and became head of some institute. But they were decimated. I think Parloff's guess is the same guess I would have. It coincided with this more mechanistic, materialistic perspective where behavior is controlled by brain chemistry and to

get people well you did it with drugs. As a matter of fact, I remember that Dr. Kety was very interested in the biochemistry or biology of mental illness, and he helped found the Journal of Biological Psychiatry. And I once either heard him or heard him quoted as saying something like, “Well, lysergic acid makes you crazy and thorazine cures you.” I’m obviously exaggerating his remarks, but he did say the equivalent. If these various drugs produce abnormal psychotic behavior and other drugs reverse it, you have it. You just had to find out what happened between the two. Needless to say, no one has yet found that out.

Do you know what Parloff did after he headed up this very large study on therapy? That was the last I heard of him. It was a huge and important project. Harking back to the beginning of the lab, he certainly was one of the smart, bright people together with Iflund, Carlson and others. They had interesting ideas and they liked to discuss them. I enjoyed sitting in on their coffee discussions because it was fun, because they were so bright.

Farreras: Why did you leave the Aging Section?

Streicher: Well, the Section came to an end, really. It was ‘62 when I left, and Bondareff had left, Jack Botwinick had left, and Jim Birren had left to take over the extramural part of NICHD that was dealing with aging. The Section stayed on under [Ed] Jerome and it was just he and Marguerite Young. They worked together but that was just a remnant. They changed the name of the Section to Higher Thought Processes, meaning problem-solving. So few people at NIMH were, at that point, interested in aging, believe it or not. It was only later, with the advent of funding for Alzheimer’s, that they quickly got back into the business.

But there was no interest at that point and I think they probably wanted the floor space anyway.

Farreras: These were all in the Clinical Center?

Streicher: Yes.

In the early days it was all small and very cordial. Felix always pretended to be the country boy from the midwest, but he was extremely intelligent. The place was small enough that when he would testify downtown, he'd invite the staff to join him afterwards, mostly to tell about the faux pas he committed. He was just a very humorous, very nice man, and it was small and cozy.

But I often wondered what happened to the Laboratory. You should talk to Ben Carlson, he's retired from the Public Health Service and was one of the earliest people on site. He had taken his degree at Hopkins, and then, after he retired, moved back to Baltimore because his wife was so taken with the area. They're now living in the original port of Baltimore, at Fells Point, and very much involved in civic problems. I think he probably retired around the same...

Farreras: I think it was the late '70s.

Streicher: Yes. Again, a lovely guy, a very nice person. And I've been retired for a long time, but I think he left at that time. He was eligible to retire.

I think Parloff was right, I think that change in emphasis to the more mechanistic view of mental health problems was probably influential. People did disperse, and I went to the Neurology Institute to the Section on Experimental Neuropathology.

Farreras: That was in '62?

Streicher: Yes, in '62. In '64, believe it or not, the director of the Neurology Institute decided that the Section on Neuropathology had to change from research and do clinical neuropathology. My boss, Dr. Klatzo, declined and we thought that was the end of the Section. So we went to look over certain jobs. This was in '64. Our prospective employers had the feeling that since we came from NIH, we would just bring a couple million dollars with us, build a building or a new extension, and after it was completed, we could occupy the building. Someone in the extramural program called me and said, "Well, I understand your Laboratory is closing. How would you like to be the Executive Secretary of a Training Committee?" The Neurology Institute supported the training of neurological residents at various institutions. There were so few people in neurology at that time, practicing neurology, that the institute itself gave training grants to institutions so that they could recruit people who would think about becoming academic neurologists. They paid for their residency – and they got paid much more than normal residents. After that, if they stuck with it, they could get an additional three years of support to work in the laboratory. Neurology had four training committees. One in vision eventually transferred to the Eye Institute, one in ear, nose, and throat, otolaryngology eventually was transferred to the Deafness Institute, one in basic neurology, and one in clinical neurology. What happened was that as an executive secretary of the committee, they used to use people who were residents who had to do their service time. This is a long time ago. That is, physicians were obligated, after they were deferred. To complete their residency or complete their medical school, to spend two years in the Army, Navy, Public

Health Service. One of these fellows, who was supposed to be the executive secretary of the neurology training committee, never showed up. It happened that I had been looking for a job. So I decided to try it on a short-term basis but it turned out to be a job I really liked. I was with the Training Committee for a couple of years until they could replace me with a physician, because the training was all done at hospitals, and people on the committee felt more comfortable with a physician who understands the problems of hospitals and patients. And then, I went over into basic neuroscience administering research grants in areas with which I was quite familiar. And I stayed there from June '64-May '97. And most of the work dealt with basic investigators, many of whom were having a difficult time getting funding. You try to guide them, help them, find out what the problems are, and work with them. And most of them were eventually successful. And along with that, you kept track of how many grants you have in each area and how much money and a variety of reports. Each institute has a national council that meets three times a year you provide them with information at the council meetings. You also present them with issues or new developments that have come up which they may be interested in. The Director took care of political issues, and people like me took care of some scientific issues. Many people didn't enjoy it; I was one of those people who worked in an area in which I had done research, so I knew the area, the difficulties and the problems. At that time they felt that "generalist" health science administrators could administer anything, some of these people weren't very happy, didn't really know the subject matter except that they were taking care of budgets and funding, but they had no...

Farreras: ...background in that area.

Streicher: No background or empathy of feeling. So I really enjoyed the work. I was very, very lucky.

Farreras: So, looking back, what would you say are some of the pros and cons of working for a place like NIH, a government agency, compared to working in academia or industry or some other government agency?

Streicher: Well, in the early days I thought NIH offered opportunities that were not readily available elsewhere. First of all, you spent your day doing your research most of the time. Things weren't terribly expensive. You got whatever money you needed. Each person, at the time, worked with one or two assistants in small sections, and that was nice. The distinction, I think, between NIH and the large universities has since disappeared. A job either in the intramural or extramural program could be wonderful or it could be terrible. It depended on your boss. But that distinction has disappeared. Now, intramurally and extramurally, there's a lot of competition, which needn't be... You know, one of the things that was almost always absent from the intramural, which is really a shame – and this was true of all the institutes and true of academia – is that I can never remember a Director or a Section Chief or a Laboratory Chief coming up to someone and saying, "I read your manuscript and I'll pass it on for review [for clearance]. It was just terrific. That was a wonderful piece of work." Never, never, never. It is just too bad that people just can't be generous enough, can't say to you, "That was a great piece of work, you really did a good job." You almost never hear that. So the competition and the psychological view of no one gaining without someone

losing... Now, of course, when you look at research papers and journals, many of them have six, eight, 10 author names. I was just reading an article Rapoport published in the Journal of Neurochemistry, and he had his name last. There were probably about eight names on there of fellows and collaborators. He's been out of the laboratory for a long time but he still helps out some people who are doing cerebral blood flow; he's an old hand at protocols. But he has only a few post-docs who are working like hell and hope to get their own laboratory someplace. They now have a tenure system here. The system has changed here to become as close to the academic systems as possible.

Farreras: How recently?

Streicher: Not so long ago, it depends on each institute. In Neurology [NINDS], it was changed probably about five years ago.

Farreras: Oh, recently then.

Streicher: Right, Mental Health [NIMH] was probably the next one. The next Director, Hyman, came in and followed Hall in Neurology and did the same thing, trying to kiss the old boys good-bye and bring in new people. Meanwhile, the old system – if you weren't encouraged to leave after five years it was assumed that you could get tenure – was over. The rug was pulled out so that many people suddenly...

Farreras: There was no sort of grandfather clause...?

Streicher: No, they'd say, "Well, we don't have positions with which to hire new people." People also bring in people they knew – the old boys network. Now you have these laboratories with many post-docs, not factory-like, but the area of research is so narrow, so specialized, and the person is hoping for a number of papers.

And one of the ideas behind having eight authors is that you have more papers. But, on the other hand, for the person doing the hiring, you don't know how responsible an author was for the pieces of the work. The competition is horrendous.

Farreras: And it seems that if you want to do cutting-edge research today you almost invariably have to be working on a multi-site project, based at a large institution, and preferably with international collaborations.

Streicher: Yes, that's right. And you have to keep your connections right. You want to work at Harvard or Stanford with distinguished people. The other colleagues are well-known, distinguished people who then arrange for you to spend a year at Mill Hill in London with so-and-so in pharmacology, and perhaps somebody in Italy, since you have a contact there. That's the sort of capitalism that operates in the scientific realm. You have people who've done their graduate work, received their Ph.D.s, did post-doctoral work, and finding positions still becomes very tough. So you do another post-doc. And people give out post-docs. While it used to be, years ago, that you could get any number of them, now you have to get a job. I've known a number of people who've been neuroscience students and are now doing other things. There was even a course on campus, which I didn't attend, which basically consisted of what you could do if you couldn't work in your Ph.D. field. For example, you can work for a stockbroker.

Farreras: For pharmaceutical companies...

Streicher: Yes, right. And people who've done their post-doctorate work here have too much invested. They've gone too far. Those who are very intelligent, very

energetic, very focused, and who keep up their relation with their Harvard mentor are going to make out, but there aren't many of them who are very smart, focused, and work like hell because then the family is neglected also. But part of the business is to accumulate post-docs, get the work done, and those people will make out.

Farreras: There aren't that many Research I institutions out there who can take such a large group of post-docs when they're done, though...

Streicher: That's right, that's exactly right. I really feel this business of trying to indoctrinate high school and college kids into science doesn't include the problems. I think science is great from an educational point of view, the scientific method, the history of science, but from a capitalistic point of view it's a survival thing. And by the time a person has gotten established, he may no longer be in the Laboratory, is no longer doing things but rather is directing things, writing grants, and traveling to different places, because it takes a year or more for something to get into the literature. By traveling, visiting, you find out ahead of time what will be in the literature. For example, when I run into Rapoport, we can either be walking down the hall or having lunch together, and he'll run across someone and it's often, "Oh, I've been doing this," and Rapoport says, "That's wonderful, let's do a collaboration," and set it up right there. "You do this and I'll do that." Very intense. So that I think there are survivors, but there are also a lot of unhappy people, burned out people, people who lost interest. As a matter of fact, in giving out grants over the course of many years, I saw that among the old-timers, there are only a few who keep up. They learn the new techniques, they go

back to school, they do their research and learning, they stay with it, use the library, and continue to get their grants through the years because they are now retreaded continuously and stay in front. There are very few of them who have been able to do that, and it's a full-time proposition. And I don't think you can approach a kid with that sort of view of things because interest is the most important thing. How would one like to spend one's life? And one can only say, well, if you work hard enough, if you're good enough, do what you really want to do. There's no other advice. It should be possible for people to see the other side of things early in the game, not necessarily dissuade them from doing research but show them what's ahead. I've visited places where the graduate students have said, "Gee, I've watched my professor scramble, and if he's having such a hard time, how am I going to manage?" And if they have a family it's a heavy responsibility. So there are any number of people who say, "Well, I've trained in such-and-such," but they're not doing anything related to that. That's what happened to many attorneys; when there were too many attorneys they started to do other things. That's the impression I get of science as well. One of the minor saving graces is that, as the NIH appropriation and NSF appropriations increase, that increase is mostly into research grants. The intramural program is usually increased just minimally, and that determines how many investigators can be supported. And as that amount goes up, more people can find funding. And the bigwigs in the field can have three, four, five grants... People invest their money where they feel money has been well invested before. So it's not easy. I, personally, feel extremely fortunate. I could have trailed Klatzo around. But

when I took this job, the Director of Neurology basically ordered the laboratory to convert to clinical work, and Klatzo refused. Then the Director lost his job, and Klatzo stayed in his laboratory for another 20 years.

Farreras: What an irony...

Streicher: Were there any other aspects you wanted to discuss?

Farreras: I think not because much of it happened after you left to go to the Neurology Institute. Most of the changes in the Lab were later on, where you see that strong biological emphasis. Now it seems it's all neuroimaging and genetic epidemiology. Is there anything that I haven't covered that you'd like to talk about, because I'm limited by what I know about the Lab and the intramural program.

Streicher: There was one aspect that might be interesting. When Birren formed the Aging Section, it was a multidisciplinary section. I was the physiologist. I hired a technician, but he was a biochemist. Then Jack Botwinick was the psychologist. Bondareff was an anatomist. No one gave us a hard time but we were definitely not in the mode that Kety or Shakow envisioned. That's why I say that I think that Jim Birren's arrangements for that section were...

Farreras: ...decided over Shakow's head?

Streicher: Right, Kety was brilliant person, a nice person, but very formal in terms of our section. He felt that this was not the model; that if you had a collaborative project going, then some person from the Laboratory of Neurophysiology would work with some person from the Laboratory of Psychology, who might work with some person with the Laboratory of Neurochemistry. In other words, people worked in

the laboratories of their specialty, with other people in the same specialty, and that any collaboration was done across labs.

Farreras: Not within a section...

Streicher: Right, across labs but not within sections. He rationalized this, he'd say, "You don't have people to talk to." But there were a lot of people around. But he'd rationalize that one wouldn't have colleagues immediately available to talk with. Of course, that wasn't true of Botwinick and Birren, and the fellows that came in were psychologists. The Riegels [Klaus and Ruth] who came in were both psychologists, and there were other fellows who came in for one or more years, and they were all psychologists, so they did have colleagues. And they were all interested in aging. But the idea of having a multidisciplinary representation within a section was not what they felt was a good model. And, as a matter of fact, my hunch was that Shakow was glad to see us go. But he wasn't unkind. I think he was sort of different. Now, on the other hand...I'm thinking back to the remark that you made about Shakow, that he would like to have passed on any psychologist that was hired at the institute – that falls right in with that, that the Laboratory of Psychology was for psychologists. _____

TAPE 2, SIDE B

_____ and if possible, any peripheral people would identify with him, or at least touch base.

In any case, if any questions come up, please feel free to ask me.

Farreras: Okay. This was most helpful. I look forward to speaking with Dr. Carlson as well and hearing what his perspective is. I'm supposed to do a phone interview with Dr. Bondareff shortly. He's at USC. And I've been corresponding with Dr. Botwinick, and he sent me a few photographs from the old days. I think I might have them handy to show you. I haven't been able to reach Dr. Birren yet.

This [photograph] is of you and Botwinick...

Streicher: Yes, right, this was the occasion of the dedication of the Dental Institute building, Building 30, I think. This is Representative Fogarty, who was drinking pals with the director of NIH, James Shannon. They were thicker than mud. He took care of NIH in the House of Representatives just as Lister Hill did in the Senate in those early days. This is the Surgeon General, Terry, who was formerly with the Heart Institute at NIH. And this is Jack and me. What happened was that we were on the second floor, that's my laboratory. Fogarty had just come from the dedication of the Dental Institute Building and he was being trailed, by a photographer, a very nice man named Jack Silverman, who was the NIH photographer at the time. And Fogarty was looking around for a laboratory that had lots of sophisticated equipment to use as background. He was disappointed with my Lab because he just saw test tubes and bottles and so forth. But the door opened and in he walked, suddenly. Of course, I recognized him and knew him slightly. And he just said, "Tell me what you're doing." So I did. He wasn't listening. Silverman was taking pictures, trying to get as much apparatus into the picture as possible, but this was a chemistry lab. No smiles or anything. I was

explaining what I was doing, I was analyzing calcium at the time. As they walked out, Silverman sort of hung back, smiled and said, "That's the way it goes."

Farreras: That's the wording Botwinick used to describe what he said, "He looked at me, smiled, and said, 'So it goes, Doctor'."

Streicher: That's right. Now, in this other one is Joe Brinley. He was taking his degree at Catholic U. and was a very, straight-arrow, lovely man. He took a job, after he got his degree, at St. Louis University, and I think he stayed there for years. This was Joel Garbus, who was my technician, he did biochemistry, and he left to go to the University of Wisconsin. And this fellow was Jim Birren. This guy here, Sam Umburger, originally started as our animal man. He had a grade school education. He came from Germantown. Germantown, at that time, was a transplant from Germantown in Virginia, it was all farms. And he had just gone to grade school, but he was a very, very smart man, and we made him into an instrument maker. He made instruments. And this was the secretary, Mrs. Oest, she was just lovely. She came from Cumberland, Maryland, and she was very discreet, very intelligent, and she used to break in young black clerks, teach them to be secretaries, how to type carefully, take care to produce beautiful letters and so forth. One of them later replaced her when she retired. Absolutely dedicated, lovely person. Birren was a wonderful Section Chief in the sense that he was a very thoughtful guy, not a lot of spontaneity, always thought things through carefully, but a very lovely man. He wouldn't just answer a question, he'd be thinking about what the significance of the question was, how to answer it in the most appropriate way so as not to give a wrong impression about this or about

that or to indicate something is wrong here or there...very calculated, but in a nice way. And that's how he ran the place, very well organized, experienced, and a nice, nice man.

Jack Botwinick would be fun. He's a very straightforward guy, doesn't take many things too seriously, tells it like it is, and he found a home in St. Louis, at Washington University.

Bondareff – and he's probably changed – presented himself as an eccentric, really. He threw out a phrase here and there to startle you or surprise you. I guess he's the one who sort of lived the most in terms of his conflicts with people at Northwestern University. In essence, a guy who was determined to do what he damn pleased, even if he didn't know exactly what that was, but he did it anyway no matter what. He wasn't at NIH for too long. The problem with the anatomy of fixed tissue is that you try to get it fixed hopefully as close to lifelike conditions as you can. That's very hard to do and you try to avoid as many artifacts as you can. But when you superimpose on that the changes in the brain that occur as a function of aging, it gets super complex. One day he decided to go to medical school, and this was a few days before the semester began at Georgetown or George Washington. So he put in his application and they had a cancellation; he just hopped in and became a medical student. But I think he had this great difficulty handling tissue, difficult techniques, arduous, and at the same time perhaps had that feeling he may have had a long time ago or perhaps his parents had a long time ago that perhaps he should go to medical school. And from there he went to an internship in Baltimore, and then Northwestern was a whole new set

of problems. Anyway, he ended up at USC and I think he's become very devoted to the geriatric field, still doing research but I'm not sure. But a nice person.

Farreras: Well, I want to thank you again for being so generous with your time and coming to see me. I really enjoyed it.

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