

**James E. Birren**  
**NIMH 1953-1963**  
**NICHD 1963-1964**

Telephone interview with Dr. James E. Birren, former Section on Aging and Chief of the Laboratory of Psychology of the NIMH Intramural Research Program.  
Interview conducted by Dr. Ingrid Farreras, NIH History Office. March 22, 2002.

Farreras: I was just looking over your autobiographical chapter, and since you mention a lot of your background there, I thought we might jump ahead and start with the time when you met Nathan Shock during the war and how you ended up in Baltimore after your graduate work.

Birren: I was at the Naval Medical Research Institute just across the street from NIH. It was a multidisciplinary group and the head of the group was a physiologist by the name of Harold Bloom, who was from UC-Berkeley. Nathan Shock had been at Berkeley for 10 years in a longitudinal study of child development and came through the laboratory at the Naval Medical Center to see Harold. I got to know him. He was assigned to create a Laboratory on Aging for the Public Health Service in Baltimore, at the Baltimore City Hospital. He arrived there on December 7<sup>th</sup>, 1941, the day the Japanese attacked Pearl Harbor. He was redirected from research on aging to do work related to some war projects. But then immediately after the war he started recruiting to develop the Unit on Aging, a Laboratory devoted to research on aging. He wanted me to join the group in Baltimore. I still had a year to finish of my dissertation when I went back to Northwestern to complete it but then I went to Baltimore. That was in 1947. At that point, the Laboratory or the unit was expanding, and there were several

physicians there who had two years of service to pay back to the government their time in medical school. I stayed there for three years, but there was a lot of tension there. Then I shifted, in 1950, to the National Institute of Mental Health when the new Clinical Center was being planned and built, so I was temporarily assigned to the University of Chicago. I went there for three years, until the new building was finished, Building 10. In Baltimore I had two colleagues working with me doing research on the psychology of aging, Jack Botwinick and Charlotte Fox.

Farreras: You met Gene Streicher later, at the University of Chicago, when you went those three years?

Birren: That's right.

Farreras: I see. Okay. I interviewed Gene Streicher a few weeks ago and he mentioned that the move to NIMH was really as a result of your and Botwinick's interest in the *behavioral* aspects of aging, whereas the Lab's, or Shock's, mission was more physiologically or disease-oriented. Is that accurate?

Birren: That was partly true, it was heavily medically oriented. The original laboratory in Baltimore was supposed to have been headed by a man, a geriatrician from Washington, D.C. – whose name I can't think of at the moment – and then he decided not to go to Baltimore. Nathan Shock was recruited because he had been involved in the longitudinal studies at UC-Berkeley that incorporated both biological and behavioral variables in research on child development. My background has always been multidisciplinary, and so it was easy for me to get

into the field of aging. At NIMH I was encouraged to recruit people, so I recruited Gene Streicher as a physiologist, and I also recruited Bill Bondareff, a neuroanatomist, along with Jack Botwinick.

Farreras: And then it was Ed Jerome, later on?

Birren: That's right. Ed Jerome was recruited when Jack Botwinick left to go to Duke University.

Farreras: Do you remember when you moved to NIMH?

Birren: Physically, I moved to Bethesda in the summer of 1953.

Farreras: At that time, were they already offering you a Section or a Lab in Aging? Because the Psychology Lab hadn't been created yet...

Birren: That's right. It was the Section on Aging. We were out in the wing of the building. The clinic was divided into what we called the science part and the clinical part, and we were out in the science wing. We were about the first people who occupied that floor of the wing, the Section on Aging.

I don't know when it formally became incorporated into the Laboratory of Psychology. When was the Laboratory of Psychology formally created?

Farreras: Well, I don't know exactly because Shakow was expected to arrive in '53 but then had a heart attack and ended up not arriving 'til the summer of '54; I think Dick Bell was behaving as Acting Chief until David Shakow showed up. But most of the Sections, I think, were already ongoing by the summer of '54. And I was just wondering why it is or how it is that the Aging Section ended up being placed under the Laboratory of Psychology's umbrella.

Birren: That I don't know. I simply don't know. I think they needed to put it somewhere. And since I was a psychologist, they thought it would go into the Laboratory of Psychology.

Farreras: Were most of the psychologists in that lab? I thought there were some in the Socio-Environmental Studies Lab with Clausen.

Birren: Yes, in fact, we had an interdisciplinary study on aging that was published later that involved Marian Yarrow, who was in the Laboratory of Socio-Environmental Studies under the direction of John Clausen.

Farreras: Yes, Bob Cohen showed it to me. When you tried to move the Section down to NIMH from Baltimore, were there any particular people you spoke with here at NIH? Or did you choose to have it moved to NIMH?

Birren: I didn't move the Section when I moved. The section didn't exist in Baltimore. The terminology is confusing. I think that the Baltimore laboratory was a Unit, formally, and then it moved up the scale. But I was an individual investigator in Baltimore; part of the Unit on Gerontology, part of the National Heart Institute. The Section on Aging was created after I joined the NIMH. My move to the NIMH was discussed with Dr. James Shannon, Director of Research for the National Heart Institute who approved of the idea. Later he became Director of NIH. One thing that made the move easy administratively was that I was a commissioned officer in the U.S. Public Health Service.

Farreras: Okay. So it was just you and Botwinick who moved down to NIMH?

Birren: After his time in Baltimore Jack Botwinick wanted to complete his Ph.D. He had done military service in India. When he got out of military service he received a Master's degree from NYU. Then I had the opportunity of rehiring him later when I went to the NIMH and he received his Ph.D.

Farreras: I see, so you moved down to NIMH from Chicago on your own and started the Section on Aging once you arrived here.

Birren: Right.

Farreras: Had you asked to be placed within NIMH, or was that the NIH Director's idea?

Birren: It was discussed with both the Heart Institute staff and the NIMH. I met with Dr. C. J. Van Slyke, the Director, and Dr. Shannon, who had been the Director of Research at the NHI. I met the people in the NIMH, Dr. Robert Felix, of course, also Dr. Vestermark and Dr. Bobbitt.

Farreras: Oh. I thought it was Seymour Kety; I thought Vestermark was extramural?

Birren: Well, Kety came in as intramural later. Right. I met him later. I don't know when Kety joined it. I believe my transfer to the NIMH occurred in 1950. When I was at the University of Chicago I was already a part of the NIMH personnel, before the intramural program was even set up.

Farreras: So before you arrived it was already agreed that it would be within NIMH.

Birren: Right. Before I came, because I was administratively under the NIMH when I was in Chicago, I was still an employee, a commissioned officer.

Farreras: Why Chicago?

Birren: Well, there was a group of people there that was interested in proposing some new

uses of mathematics and statistics. In fact, a man by the name Korenchevsky, had a committee on mathematical biology. I spent some time with his committee and I published an article there on mental functioning and age using a mathematical biology model. L. Thurstone encouraged me to do a factor analysis of a mayor intelligence test in relation to age, the WAIS. I got the standardization data from the copyright holders. I rotated around and also spent some time as a visiting professor in the Department of Anatomy. That was with Patrick Walld, studying aging in neural conduction velocity in aging rats.

Farreras: And it was during those years at Chicago that you met Gene Streicher?

Birren: That's right.

Farreras: How did you come about picking him or asking him to join you at NIMH?

Birren: Because aging was beginning to be recognized as a multidisciplinary area, it wasn't one discipline, and he represented physiology. Someone told me that Streicher had the highest preliminary examination score of any graduate student in physiology and he was interested in aging. Bondareff was also there. Bondareff got his Ph.D. in anatomy, so there was a cell of people that I met at Chicago.

Farreras: And so when you arrived, you all went directly to the Clinical Center, not to T6, where most of the other psychologists had been?

Birren: That's right.

Farreras: Were the other Sections already ongoing?

Birren: I'm not sure. I doubt it. I think the organizational process was still going on when I arrived. I arrived a few months early and the space was not quite

complete. I got a room down at the Library of Congress and did some review work in journals for a few months in the summer of 1953. Then I moved physically into the building in the fall of '53. There was a little confusion because finishing the building and hiring the people to move into the space didn't quite match.

Farreras: Were the other Section people for the Laboratory hired by Shakow? Shakow clearly had little to do with your coming to NIMH.

Birren: Right. He inherited me. But Rosvold, Mishkin, and Mirsky may have been hired by Shakow. I don't recall meeting them before Shakow's arrival, although I may have.

Farreras: Was there much contact at all between people in the Sections? They *were* different areas of study.

Birren: We had meetings occasionally with different speakers from different Sections.

Farreras: Was there much collaboration? It seems like you might have collaborated more with people outside of the Lab than with people within the Lab.

Birren: That's right, our interests were quite different. That book shows that we were collaborating across laboratories with Marian Yarrow. And then with Louis Sokoloff in Kety's lab.

Farreras: I don't have the exact dates but did you move to the extramural program in the NICHD in '63, or '64?

Birren: It was in '63. There was an administrative problem... I reached the end of the line, as far as stature or status as a commissioned officer. There wasn't any

further promotion policy for Ph.D.'s in the Commissioned Officer Corps, and I couldn't transfer to civil service. There was outside interest in developing an institute of child and human development, and the Kennedy family was interested in retarded children. The thought was to broaden it to have developmental studies over the life span. They approached me about leaving NIMH and heading aging within the NICHD, and what was interesting about it is that they moved the Baltimore branch under the NICHD. I became Nathan Shock's boss, which was very odd. I was responsible for both that intramural program in Baltimore and for extramural grants. But then again, pressures developed for aging over time, and I had the personal issue of reaching the end of the line for my promotions there. I left the NICHD in '65. Now, some other history may be more subjective. As a result of the study published in 1963 which you saw, Robert Butler, who was a psychiatrist on that project, and I, proposed in 1963 to the NIMH that we establish a Laboratory of Aging within the NIMH. It didn't go very far, although all the people discussed it openly. There was a dominant feeling at the time that the early life events were the important ones, and psychoanalysis, which was very important for some of the senior people, including Bob Cohen, was a dominant orientation. They felt that individuals' lives follow a pattern that's laid down in the very early years of life. So the idea of a Laboratory on Aging was turned down, and Bob Butler went into private practice. The pressure on NICHD to support research on aging kept growing and growing. In 1975 the National Institute on Aging was created, but they were planning it for several years. But,

ironically, they offered me the job as Director first and then Bob Butler, the better man is Bob. He was better able to relate to members of Congress than I would have been.

Farreras: They offered you the position first? But you didn't take them up on the offer.

Birren: Yes. I was at the University of Southern California then. I had been there for 10 years and my family was settled in California. Also, I don't think I had the skills that I think Bob Butler had. He and Bob Felix were so good at representing Institute programs to Congress.

Farreras: So then Bob Butler became the director of NICHD.

Birren: Yes.

Farreras: I see. When did Ed Jerome arrive? Ed Streicher seems to recall that by the early '60s, the Aging Section within the Lab pretty much consisted of Ed Jerome and Marguerite Young alone – thus the change of Section name to Higher Thought Processes, to reflect his interest – and that the rest of you had left...Streicher to NINDS...

Birren: Jerome stayed in the Laboratory of Psychology. He was a very good researcher but I had left to go to the NICHD. Have you spoken to Botwinick, by the way?

Farreras: I've corresponded with him and he's responded to many of these questions on paper.

Birren: I remember the Section on Aging as a very active unit. Streicher was doing research using cells from old and young rats. This led to the study of the role of the mitochondria in aging. One of the lab assistants from Hopkins published a

paper on it.

Farreras: Okay. Streicher seemed to think that once everybody, except for Jerome, had left in the early '60s, that there wasn't very much of a section left, that it was just Jerome and his research.

Birren: By 1964 that was true; we were not replaced.

Farreras: Any reason for that?

Birren: I think we were not fitting the pattern of the NIMH there. Again, perhaps the dominant psychoanalytical point of view did not give much priority to aging.

Farreras: I just interviewed Ben Carlson yesterday, and he mentioned that when Shakow had retired in '66, the Section Chiefs were asked to get together and suggest someone who would be a good, new Lab Chief, and they recommended Hal Rosvold. But I guess Bob Cohen, and I don't know if anybody else, turned that down, and Carlson had thought that it might be because they were more interested – being very psychoanalytic – in having a clinical person lead the lab versus a basic person.

Birren: That was '66. I left there in '64 so I'm not familiar with those details. It's another issue of basic paradigms of the senior staff. I don't mean to be overly critical because I think the administrators were very reasonable people but they had a particular slant. That was illustrated when the treatment of mental disorder by medication came up, and the NIMH opposed it, and it really broke through by public pressure on Congress. That was another example in which the bias influenced the direction of events.

Farreras: Can you say more about how the medical approach to mental illness came up and was opposed by NIMH? What time period are you talking about?

Birren: The dominant view of mental health of the period was psychoanalytic and several of its tenets had an impact on the way aging and some other issues were viewed. One of the tenets was that the basic character or personality of the individual was laid down in the first years of life. A second was that it would take too long for persons over age 50 to retrace their lives in psychotherapy to be feasible. Since the view was that the basic structure of an individual was laid down through early experience, basic treatment was thought to be retracing the pathways of experience and that the use of medication was merely a palliative and not fundamental therapy which was psychoanalysis. Another feature of the physiological orientation of the period was to minimize the role of the central nervous system in the organism. It was not widely accepted that the central nervous system was the primary regulatory organ of the body, e.g., metabolism, blood pressure, respiration, and body temperature. The nervous system was regarded as being responsive to other body organs, the endocrine glands. The adage “you are as old as your arteries” typified the thinking of the period. The cumulative effect was to depreciate the study of aging. Also, it created a negative climate within the NIMH regarding attempts to treat or study the effects of drugs in relation to mental health problems. Public pressure began to build up with regard to both medication of mental illness and also the study of aging. The resistance of the NIMH to drug research gave way in response to Congressional

interest. Later demand for research on aging external to the NIH paved the way to the creation of the National Institute on Aging in 1975.

Farreras: I would have thought that once all of these psychopharmacological agents became popular and with Axelrod getting his Nobel Prize in 1970, that the push for a medical approach to mental illness would have gained acceptance.

Birren: But Axelrod's early research was on the liver. When I asked him about 1960 why he didn't do research on the nervous system he said, "it's too messy."

Farreras: I know Morris Parloff mentioned that in the Annual Reports of '71 or '72 Bob Cohen's introduction pretty much relinquished the mission to a more biological approach to mental illness.

Birren: I think the roots of it were in the '60s.

Farreras: What led to the decline of the psychoanalytic dominance?

Birren: There began to be more studies about the efficacy of psychoanalysis, and it was very difficult to prove the efficacy of psychotherapy. I also remember David Shakow had his own activity there involving the taking color-and-sound movies of psychoanalytic sessions.

Farreras: Yes, they called it "Shakow's folly."

Birren: Oh, is that right?

Farreras: Yes.

Birren: Individual psychoanalytic sessions were recorded in great detail, but they had no way of extracting from that information that was cumulative and have it yield some useful generalizations. Not that there isn't some validity and insights from

all these areas of study, but it's hardly the coverall for all the science on mental illness.

Farreras: What would you say were some of the strengths and weaknesses of a scientific career working for the government?

Birren: I think it was a period of expansion, and it was very exhilarating. We probably all learned from it. I carried forward to my later career a sense of living in an expanding intellectual environment. That's what I would call that time, an expanding intellectual environment, a period of breaking down old assumptions about the human organism.

Later, money got to be a bigger issue. Costs went up and budget issues got to be more and more important. I recall walking across the grounds at NIH from Building 10 to Building 1 with Louis Sokoloff, and we were talking about the brain and aging, and that's what led to that project and the publication of the 1963 book. That was a walk across the campus in which we could develop the opportunities of the day. You didn't have to file too many reports.

Farreras: I was looking through some of the forms that are bound with the *Annual Reports*, and I was wondering whether the forms are proposals for research you wanted to do or whether they were summaries of the research that you had already done. Some of them ask for an amount of money and people or personnel that you needed. Do you remember any of those?

Birren: No, there may be some information. My impression was that we didn't have to file much in the way of formal research proposals.

Farreras: Okay. Bob Cohen also mentioned, when talking about the early mid '50s years, that when he was trying to recruit people to come to NIMH he had a very hard time because either the people they were trying to recruit were already important people who were established in academic settings, because the government wasn't paying what they were making in the academic settings, or also because they were concerned about working *for* the government, doing research for the government.

Birren: That I think is an important point. Early NIH had not much status as a research organization. I remember talking to a physician who asked me, "Where are you going?" I said, "I'm going to go work for the National Institutes of Health," and he said, "Why are you going there?" It was like his asking, "What's that?" NIH was not the dominant health institution here in the world then. It was beginning to climb up the prestige ladder. Universities were the sacred places. The people in the early public health movement really had a vision. The Institute of Experimental Biology and Medicine later went out of existence but the pioneers had a vision. I remember Van Slyke, who was the Director of the Heart Institute, saying, well, when the Korean War was over, they had the opportunity of directing Congress to use the money that had been spent in the war, to add to the NIH budget.

Farreras: Right. What about the concern that academics who might be giving up their job to come here might not have as much academic freedom to pursue their research as they would otherwise?

Birren: Yes, I think personal freedom was more of an issue than academic freedom.

When Gene Streicher worked with me, the head of personnel at NIMH one day called me on the phone and said, “We have an inquiry about Gene Streicher, when he was a student at Cornell University, he joined the National Association for the Protection of Colored People.” I’m not certain now of the title. It was the National Association for the Protection of Colored People [*National Association for the Advancement and Protection of Colored People*]. And I said, “Well, I don’t know if he did.” And he said, “Well, please find out.” Then I went back to Gene to discuss it.

Farreras: So they were asking you, not asking him directly?

Birren: They were asking me to snoop. So then I went to Gene and said, “Gene, did you belong to that NAACP at Cornell?” He said, “Yes, when I was at Cornell, there was a faculty member there who had a big influence. He gathered students together and he encouraged them to join.” Gene said, “I did.” And I said, “Well, what came of it?” He said, “Well, I’ve drifted away. I’ve forgotten about it now, about my student days at Cornell. So then I began to puzzle why I was asked this question.

Farreras: And how they even found out!

Birren: Right! So then I spoke to my neighbor – I was living in Bethesda then. He had been associated with the Supreme Court, and he said, “Well, what you should do is ask the man in personnel to put in writing what he is requesting, and you tell him you will put in writing your reply.” I thought, “Okay.” I went back to do this because I felt I was involved in a snoop. So I called the head of personnel and

said, “You know, if you put in writing your request, then I will put in writing my reply,” and he said, “That’s a good idea.” And I never heard about it again. He went up the ladder, presumably told the inquiring agency that it would be required to put their questions in writing, and they were hesitant to do it.

Farreras: I see.

Birren: And that was the McCarthy period. So there’s an example of how an undergraduate student could belong to the NAACP and then later get queried as being suspicious in some way. But I had another incident – I don’t know if you’d be interested in it.

Farreras: Oh, yes!

Birren: Well, when I came back to Washington in ‘53, I had a colleague who acquired my job at the Naval Medical Research Institute, and he had been questioned by McCarthy. I called him on the phone when I arrived back there. He said, “Don’t talk to me on the phone. My phone is bugged.” And I said to myself, “Well, that could be paranoia but I was wrong.” It turned out that he, at one point, had been a Catholic priest, left the priesthood, got a Ph.D. in psychology at Columbia, and married a woman whose first husband had been an officer in the American Communist Party during World War II.

Farreras: Wow, convoluted!

Birren: So McCarthy called this man to a hearing, and the man refused to testify on moral grounds that this was his wife’s first husband, wasn’t a relative of his and he didn’t want to testify. Again, it’s the issue of snooping. They put pressure on the

Navy and they fired him. So he lost his job. And then he went to Yale and became professor of psychology at Yale. So those were very tense days. It was incredible. Later I talked to the man who had released the story that was published in the *Washington Post* that led to McCarthy's rise. He accompanied McCarthy on a political trip to West Virginia, where McCarthy gave a speech and this man recorded it, and sent it back to the *Washington Post*. They didn't have much to put in the *Post* that Sunday, so they ran the article on the first page and they put a startling headline on it that said, "Communists at State Department." So, bango, McCarthy was off and running. And this man felt so apologetic for it, he left reporting as a career. So those were difficult days. Because of my living through that period, I also have some other stories about it that are somewhat relevant.

Farreras: I seem to recall that Herb Kelman, from the Personality Section, also received a lot of pressure from NIMH. I think several people really defended him and they ended up letting him stay but he left shortly afterwards for Harvard. There's a copy of a statement he published in the February 1947 *SPSSI* newsletter at the National Library of Medicine. Are there any other recollections of people or events that I haven't covered? I've been adding material to this as I learn more about the Lab but if there is anything that I am missing please fill me in.

Birren: Well, there's the factual history and then there's the subjective history.

Farreras: Yes, and it's the subjective history that I am trying to collect from people who were here at the time.

Birren: One of the things that I'm interested in, from a historical perspective, is the evolving paradigms of how we explain things, and that is related to the issues of the psychoanalytic dominance of the 1950s. Earlier, I got from the National Library of Medicine some reports of a survey on health. It was done in 1935-37 under the Works Progress Administration during the Roosevelt administration. They had the results of a national survey, and found that the blood pressure of blacks living in the cities was higher than that of blacks living in rural areas. I don't know how the reports came into my possession. Across the top is written, "Not for Publication;" the implication being that these are inappropriate to release to the public. I think it was partly due to the fact that there was a concept that health can't be influenced by social conditions, since they had the earlier model of the infectious diseases in which the organism is influenced by a disease which is an outside agent. When public health moved into the period of chronic disease where the host is a major contributing factor, they had to shift paradigms to explain the etiology of heart disease, cancer, arthritis, all of the chronic diseases. The early paradigm was, you have an otherwise almost perfect organism that is influenced by an outside agent. That took a while to change. And then later when I was there, there was an article that was written by a man in the Cancer Institute who worked down the hall. His article was not cleared for publication because it suggested that food additives were carcinogens. I thought, oh, that's a change; today it would be accepted. I had an example very close to home with that book that you were shown. When I was in England on a sabbatical from '60-

'61 they were trying to wrap up the publication of the book. I thought it was all done before I left, but it got hung up because Marian Yarrow, who was the social psychologist, found data in it that suggested that psychosocial loss was a pre-determinant of physiological decline. The biologists wouldn't buy that, including my friend Lou Sokoloff. You couldn't have social, if you will, social determinism. The causal chain had to be exclusively biological. And so they disputed that and they waited for me to come home so they could load it on me. We eventually just backed away from the issue and went into publication. But that's another example, you see, of a change in paradigm. At the moment, I think we're getting a little bit too much of the genetic determinism that excludes environmental.

Farreras: Yes, I noticed much of the current version of the psychology laboratory focuses on neural imaging and genetic epidemiology, and very little is left of the earlier psychosocial theories.

Birren: And it isn't that it's false. There is certainly biological determinism, a lot of it, but it's not exclusively that. My son is a co-director of the Human Genome Project at MIT, so I'm exposed to it. But you get a basic paradigm of the time and there's pressure to give it exclusive priority, and the economics of supporting science fit in. Anyway, I wanted to emphasize the shifting paradigms from the infectious disease to the chronic diseases and to environmental contributions during the evolution of NIH.

Farreras: And here it seemed to swing from heavily psychoanalytic to very biological.

Birren: Right, that's the way the pendulum...

Farreras: It's interesting because within the field of psychology, there was also a strong behavioral approach and later a cognitive revolution and those don't seem to have been reflected within the intramural program as much as in academia.

Birren: Well, like psychoanalysis, the behaviorism of Skinner had a dominant influence on American psychology. The individual was a product of differential environmental reinforcement, the extreme in the other direction. No bio-determinism was allowed in that. I remember arguing with colleagues about that. Aging challenges a single variable interpretation; you have to adopt an organismic approach. An organism adapts to its environment and it is influenced by genetic, behavioral, and social causations. This puts the history of institutions into a more philosophical context.

Farreras: Well, that's really the historical context I'm most interested in. The Clinical sections were obviously the most psychoanalytically oriented. What would you say about the Basic sections?

Birren: Well, Kety was interested in brain metabolism and brain circulation, and that was passed on to Sokoloff, and that's what we were looking at in that NIMH study. Instead of going into nursing homes and getting subjects we recruited subjects from Philadelphia and they came down by train or car and they stayed at the NIMH for a week, and they were subjected to these measurements of brain metabolism and brain circulation, Marian Yarrow's social studies and our more cognitive studies. One of the big things that turned out of that was that, within the

limits of the measurements of the day, these men didn't show much change in brain circulation and brain metabolism, whereas the previous samples – largely recruited from dependent people in what I would say would be nursing-home contexts – showed declines. It's easy to recruit people in mental institutions. But here was an example where these older men – their average age was 72 – had the brain circulation and brain metabolism equal to young men. So there was another example of a shift, but you had to go out and get the new data. And that led to, I think, a bit more emphasis on getting representative samples.

Farreras: I want to thank you for your time, this has been most helpful.

Birren: I have a lot of papers at home.

Farreras: We're most interested in unpublished material, things like old correspondence, minutes of meetings, old photographs, that sort of thing. We would love to make a copy of anything you have that may be pertinent.

Birren: Old laboratory photographs?

Farreras: Sure. Jack Botwinick sent two photographs, but those are all we have.

Birren: Okay. I have all my correspondence. There was another group we had meetings with when I was at the NIMH, sort of like a lunch group on aging, and we discussed issues of aging across the institutes.

Farreras: Yes, that may be helpful as well.

Birren: That was an informal thing. It would never have appeared in the records.

Farreras: I'm sure it wouldn't, that's why it's so valuable.

Birren: I noticed I have a folder with that. I probably have the names of people who

spoke. Well, I will look. I'm very interested. I guess I've come to an age when I'm interested in history, too.

Farreras: That's all the better for us! Let me know what you find and we'll be more than happy to make photocopies of letters or photographs and any other unpublished documents which you might have kept. I just got back from the University of Akron, where David Shakow donated all of his personal papers. He sent all of his papers to the Archives for the History of American Psychology there, and he literally kept everything. There are even notes that he took during his graduate seminars kept there!

Birren: Oh, my goodness.

Farreras: I know, very, very thorough. But there wasn't that much material from the NIMH years, and I'm not sure if that's because once he arrived, most of the business was conducted orally or over the phone, and therefore there wasn't that much of a paper trail... That's why I was hoping to obtain additional personal papers from some of the other members of the lab.

Birren: Well, I have a drawer in my files at home and I'll go through it.

Farreras: Wonderful and thank you, because it is very hard to learn about the dynamics and relationships between people and the decision-making that was going on by reading published papers.

Birren: I remember talking with Van Slyke and Shannon and Felix, and then there were the three senior people in the NIMH before the intramural program got started: Vestermark and Joseph Bobbitt. They used to kid in those days, "You know, the

reason why we're among the most successful institutes here, is we have Bob Felix, who is the id, and then we have Bobbitt, who is the ego, and then we have Vestermark, who is the superego. If a grant request ever comes in for something, Vestermark will say, 'But what are the implications of this for the people?' Bobbitt will go at the design of it, and then Felix will say, 'But how do we sell this to Congress?'

Farreras: A psychoanalytic metaphor from day one!

Birren: That's right. But the exchanges, you know, reflect something of the ease of the day. We would kid this way and it reflected some of that atmosphere that I called intellectually expansive. Ideas were born. Well, it was a delight to talk to you.

Farreras: Yes, you, too. Thank you so much.

Birren: How did you get interested in history, by the way?

Farreras: Well, I had been a philosophy major as an undergraduate.

Birren: Where was that?

Farreras: At Clark University. I guess the J. Stanley Hall early history rubbed off, and although I started off in clinical psychology I switched from clinical to history. The University of New Hampshire has a history of psychology doctoral program, and I'd done my dissertation on David Shakow's earlier work in the scientist-practitioner model of training for clinicians. That's when a colleague asked me whether I knew that he later had come to NIMH to lead the Psychology laboratory and I didn't, so I proposed to come and study that lab for my post-doctoral fellowship.

Birren: Oh, for heaven's sake.

Farreras: Yes. It's interesting that you talked about NIMH's success because Al Mirsky always thought that NIMH had an inferiority complex compared to the other institutes at NIH. Not during your years here, though?

Birren: Not in the early years. It grew very rapidly. And these men had some prior experience before they came. They were in the Public Health Service during World War II, on a submarine base or some military base on the East Coast. The military and the Public Health Service mixed. Just another word about J. Stanley Hall. I had a copy of his 1922-23 book on *Senescence: The Second Half of Life*. Did you know he published that book?

Farreras: No, I didn't.

Birren: How about that. He published the book late in his career. When he retired from Clark he got interesting in aging, and he did a very, very good summary of the literature of the day. It was both biological, medical, also social, and I think he was stimulated by his own retirement to look at it. In one part, he said he sat in front of the fireplace and threw a lot of papers in the fireplace, he set some aside for going to the library, and some he was going to keep, but was sorting through. It would probably be in the National Library of Medicine there. You might look at it.

Farreras: Yes, I'll look for it there. Thank you for telling me about it.

Birren: Right-o.

Farreras: Again, it was very helpful to talk to you. Thank you so much.

Birren:        You're welcome. Bye.