

Virgil Carlson
NIMH 1953-1979

This is an interview with Dr. Virgil Carlson, former Chief of the Perception and Learning Section of the Laboratory of Psychology of the NIMH Intramural Research Program, held on March 21st, 2002 in Bethesda, MD. The interviewer is Dr. Ingrid Farreras of the NIH History Office.

Farreras: Before we jump into your years at NIMH, why don't we start with some family and educational background. I know you were born in Newman Grove, Nebraska; why don't you tell me something about your family, your parents, what they did.

Carlson: They started out on a farm. I was born on a farm there but my family moved to Columbus, Nebraska, which is a town of about 10,000, when I was about three years old. My father was an electrician and my mother didn't work until World War II, and by that time we'd moved to Bremerton, Washington. So I went to high school in Columbus for two years and then I finished high school in Bremerton.

Farreras: Do you have any siblings?

Carlson: One sister, living in California. My parents are dead now. I went to the Navy, and in the meantime my parents moved to Stockton, California, and that's where my sister still lives. I got into the Navy cadet program, but it was a little late, and put us in V12 first because they didn't really need any more pilots, but they kept training them anyway. So I was in V12 I think for...I went to college, essentially.

I went to Gonzaga University in Spokane, Washington, for two semesters, and it was essentially just a college course. Then they did let us get into V5, which was the aviation cadet training program, so that must have been after the two semesters...No. I went to the Montana School of Mines, which was also just plain college. It was a college engineering course. So then it was after the three semesters that I went to pre-flight school at the University of Iowa, and that was mostly physical exercise. Then I went to primary flight training, and I did learn to fly a small airplane. Then the next step would have been to go to Pensacola or whatever other naval air station there was, and in four months get wings for being a naval pilot. But the catch was, we were just given the choice. You can stay in and do that if you want to, or get out. And if we did get out, then all of that time would apply to the GI Bill. But once we got the wings, then that was considered graduation and it wouldn't count anymore. So by that time I was fed up with flying and didn't have any interest in going to advanced flight training, as it was called. But the main issue was that I'd lose the educational benefits, which were already a year and a half of college from the Navy. The Navy paid for my next two years.

Farreras: Could you choose where to go?

Carlson: I went to Berkeley on the GI Bill for a couple of years.

Carlson: Oh, yes, but I went to Berkeley because it was a good university and I wanted to go there but my folks also lived in Stockton, which wasn't too far away and I had friends in San Francisco. So that worked out pretty well. At Berkeley, I knew

Mike Boomer, so he's the first one of this group that I knew. He was also an undergraduate in psychology at Berkeley, and I knew him pretty well. He was in clinical psychology and I was in experimental psychology, but we knew each other and had some classes together. One thing about Mike people might not know about is that he got elected to Phi Beta Kappa, but he forgot to go that night. He just forgot about it. And there's a strong rule in Phi Beta Kappa that if you didn't make it you were out. You didn't get a second chance. Even if you were sick you couldn't get your key with a doctor's letter. That was the end of it. And he just plain forgot. He was doing something else and he didn't remember it until the next day, so he never got into Phi Beta Kappa, but he was elected. That's a really remarkable thing. He came to Bethesda a little after I did, maybe the next year or something, and I may have had something to do with having him come here, like suggesting his name or agreeing with it or something, because I vaguely remember talking to somebody about him and giving him a good opinion of him. So then I went to Hopkins. Hopkins wouldn't accept anybody for a Master's degree only, they just accepted students for a Ph.D. After about two years, they would decide whether they would give a Ph.D. to that student or not. If not, and he was more or less a reasonable student, they would give him a Master's degree and send him elsewhere. And the notion was that even though Hopkins was not willing to give the student a Ph.D. they were perfectly good people for Ph.D.'s anywhere else. So they would recommend the student to another university: We're not going to give him a Ph.D., but he'll be a great student for you.

Farreras: You were a psychology major at Berkeley?

Carlson: Yes. Well, the first two years of college, I was really in that engineering program in the Navy, but the last two years at Berkeley I was in the experimental psychology program.

Farreras: How did you come about choosing the psychology program at Hopkins for your graduate work?

Carlson: Well, I'll have to back up a little bit here. At Berkeley, they had a high-powered psychology course where they picked out some students they thought would be good candidates to go to graduate school. I was one of them. Mike Boomer may have been one, too, but I don't remember for sure whether he was in that class. It was taught by four of the top professors at Berkeley. And the big advantage of that was that they would then contact somebody at a graduate school, and faculty in a graduate school would contact them, "Do you have any good students?" And in those days things worked a lot more on a personal basis like that. And I don't remember whether anybody who wanted to take that course could sign up for it – because it was a regular course – but it seems to me that the students were invited into it. And so the contact was between someone on the faculty at Berkeley and Tex [Wendell] Garner at Hopkins. I think he was an associate professor at the time, because I remember he visited and talked to some of us and I talked to him. But the main reason I decided to go to Hopkins was because it was such a good deal, economically. Every graduate student got an assistantship or an instructorship, and the pay was pretty good. And we got a scholarship, didn't have to pay any tuition, and if one was on the GI Bill, we got some money back. The university was going to collect tuition from the government, so they gave it

back to us. It was a fantastic arrangement for a student. The other reason was that it was a small group. I could have gone to graduate school at Berkeley, but it was a big graduate school, and that's not so bad as a student, but then when you go looking for a job, you don't have much help from the faculty, whereas at Hopkins the faculty went in the other direction. They're going to decide what you're going to do. You're not going to go out and find something for yourself. They're going to go out and find it for you. And they did. There were as many faculty in the Hopkins psychology department as there were graduate students. Maybe there were more; I'm not sure. So altogether there were fewer than 10 graduate students and probably a dozen faculty. So that was another reason for going there. I liked Berkeley as an undergraduate, but it was a huge school.

Farreras: Was there anyone at Berkeley whom you remember influencing any career choices you made later on, or did that not happen until you got to Hopkins?

Carlson: No, the only involvement was what graduate school to go to; I don't remember having any contact with anybody there in particular about anything after that in Berkeley. So after the two years at Hopkins, they would decide if they were going to give the student a Ph.D. degree and for those who were going to stay they just gave them a Master's degree. It was a token degree. I don't even remember getting any certificate or anything. There may have been a memo from Cliff Morgan, who was the department chairman, saying, "You now have a Master's degree."

Farreras: Did you have to write a Master's thesis?

Carlson: No. There wasn't any specific requirement for it at all, I don't think, except

lasting two years there. That's all. But you didn't last if you didn't do what they were prescribing, and they had some required courses and things.

Farreras: Who did you work with there?

Carlson: Well, Charles Eriksen was probably the first one. I was his research assistant. He was at the University of Illinois the last time I knew him. But he was new at Hopkins, too. He was an assistant professor. And then I was an assistant for Dick Lazarus. Then I became an assistant to G. Wilson Shaffer, who was the dean of the graduate school, and I really liked him a lot. He was not exactly in the psychology department, but he was the dean of the graduate school, and he was a psychologist, so he had quite a lot to do with the psychology department in an informal sense. He came to a lot of the meetings and he was a real delight to hear talk. He was a real raconteur, a really enjoyable person. They named a building after him, Shaffer Hall or something. It was the first time I knew the person for whom the building was named, and that was Shaffer. Steve Muller was the next one – he was not at Hopkins when I was there. He became president of Johns Hopkins University later, and I became acquainted with him after that. Maybe there were some others. I don't know. At that time, if you saw somebody's name on a building, they'd died 50 years ago.

Farreras: So those were the two people you had the most contact with and worked with, did your dissertation with?

Carlson: There were three: Eriksen, Lazarus, and Shaffer. Well, no. There was also Al Chapanis and Wendell Garner. At one time or another I was an assistant to each of these people. They shuffled people around deliberately so the student doesn't

just get acquainted with only one faculty member. And then they had what was called the Institute for Cooperative Research, which was a venture that was partly sponsored by the Corning Glass Company, I think – that's where the money came from, I think. But it was a research institute, and I think Garner was the head of that, or he and Chapanis both were.

Farreras: Who did you do your dissertation with?

Carlson: Dick Lazarus was most concerned with it but I wouldn't say exclusively. There's another thing about the dissertations there. Their practice was not to have students write a lengthy dissertation. In fact, they frowned on that. In a lot of places it gets to be a monograph or a small book with all kinds of experiments. Their philosophy was that a student should do an experiment that would be monitored and approved by the faculty, and then you would go ahead, do the experiment, collect the data, write it up as a journal article and submit it to a journal. It didn't have to be accepted. That wasn't part of the requirement. The journal might reject it. But you had to write a regular journal article that didn't have to be very long. I think the journal editor would probably have a hard time with the faculty if he did reject it, because the whole department had approved this thing from start to finish, so if it wasn't suitable, they would probably call up the editor and ask him why not. So that's what it was.

Farreras: What did you do your dissertation on?

Carlson: It was a memory task, recall of emotionally charged words – that's why I think Lazarus probably had the most to do with it because he would have been the one on the faculty who would be interested in that kind of problem. It was whether

emotionally connected words were more easily recalled than neutral words. I don't remember it too well now. It did get published. It was sent to a journal and it got accepted. So that served as the dissertation. By my third year all I had to do was that one experiment and write it up, so after it was all done they figured, why are you staying around? There was another aspect to that department – and I think the medical school was the same way –, they did not want to take undergraduates from Hopkins because they didn't want a student exposed to the same faculty all the way through, so very rarely did they take an undergraduate into the department. But a good friend of mine was one of those. He did get his bachelor's degree at Hopkins and then they did accept him as a graduate student, but only on the condition that he would stay two years and then go someplace else. But that was agreed. It may be that he had difficult personal circumstances and they thought he was a good student and thought he needed the help. I don't really know why. I never asked him why. So he got his Master's degree and then went to Yale and got his Ph.D. later.

Farreras: It sounds like your dissertation was on a cognitive topic rather than a perceptual topic...

Carlson: Well, at that time there was some interest in trying to provide an experimental basis for more clinical kinds of topics. I think Eriksen's degree was in clinical psychology. I'm not sure. He left that field altogether, had nothing more to do with it. But at the time there was a big rift between experimental psychology and clinical psychology, and this was an effort toward rapprochement between the two. And then Shaffer was a clinical person. Well, he was there early enough

that I don't think they called anybody clinical psychologists, but he was. He was chief psychologist at Enoch Pratt. And Morgan was not really against it either. Morgan was quite a broad-minded person, a physiological psychologist. So there was an effort to encourage somebody in this field so they wouldn't be called anti-clinical. I'm sure that's the way it happened. Now, Morris Parloff was connected with Hopkins in some way or another, but I didn't know him when I was there.

Farreras: So you didn't meet him until the day you both arrived on the NIMH campus?

Carlson: I met him here, yes.

Farreras: How did you finish in '53 and come here?

Carlson: Dick Bell had come over to talk to the faculty at Hopkins and I talked to Dick Bell. It got decided that I'd be considered and I'd come over here.

Farreras: Was he actively recruiting, representing NIMH and looking to hire scientists, or was he there for other reasons?

Carlson: No, he was recruiting. He was looking for people for the Laboratory of Psychology. I'm sure he went to other places, too. I think it fell to him, since Dave Shakow wasn't here yet, and I would think that people like Joe Bobbitt would say, "All right, Dick Bell, you do that." And it wasn't only Dick who was involved, but he certainly was the one for me. I met him there and I came over here and visited. I had an interview with Seymour Kety. By the way, I don't think NINDB and NIMH were ever joint. They were related, but I think they were separate administratively. The only reason I thought of Kety as an Acting Director of Intramural Research at that time for mental health was because they didn't have a director for NIMH. He already was for NINDB so since he was

there, he just sort of took on the job of doing it. And then I believe the next person was Bob Livingston. But he didn't come for a year or two. I have no idea how he was selected.

Farreras: But you interviewed with Kety before coming here?

Carlson: Yes, Kety was the one I talked to.

Farreras: Did Dick Bell say, "We're going to have these sections and we need people for X," or did he say, "What are your talents?" and try to create sections around people?

Carlson: No, I don't know who reviewed these things and decided but it was never presented that way, and it was not clear what sections there were going to be exactly. Well, Nancy Bayley *had* been picked for the Child Developmentb Section. She wasn't here yet, though. She was at Berkeley. I didn't know her when I was at Berkeley, but she was there. And the only other person who was already here besides Dick Bell and Earl Schaefer was Morris Parloff. He and I came here on the same day.

Farreras: How did Bell convince you to come here?

Carlson: Oh, it was idealistic at that time. You just came and did whatever research you wanted to do. That's the way it was presented. I suppose somebody had to look at your record and say, well, this looks okay or it doesn't look okay.

Farreras: You didn't have to take any of the PHS exams? I think Don Blough and Earl Schaefer both took exams.

Carlson: I did, but first I started out in the Reserve Corps. Earl Schaefer and Mike Boomer did, too, but not Morris Parloff. He was in Civil Service. And then later the

Public Health Service wanted us to get into the regular Commissioned Corps, and we had to take the exams for that. And Sidney Newman, who was head of the department downtown, is the one who made up the exams.

Farreras: On what were you examined?

Carlson: On psychology, to see if you knew the field. And one of the interesting things I remember about this is that one of the experiments I had done at Hopkins that got published in the *Journal of Experimental Psychology* got picked as a question for this exam. I talked to Sid Newman about this later, and I don't think he was happy about it because I told him that on that particular question, I was the most expert person in the entire world. There is nobody who knew more about that experiment than I did, and I couldn't figure out how to answer the question. I really couldn't.

Farreras: Were these multiple choice questions?

Carlson: I think they were, and a couple of the alternatives were equally good. But somebody reading that paper more casually wouldn't think of why this other alternative also applied. Sid wasn't too happy about that when he found out about it. That's the only question I've ever had on an exam where I felt I knew more about it than anybody else.

Farreras: That's quite an honor to have your work selected for a national examination.

Carlson: Well, it was already published and it was just picked up. I think this exam was specific to experimental psychology, and there would be another one for clinical psychology. Anyway, we did have to take the exams, and I guess all of us who did were Commissioned Corps. Dick Bell may have already been in the Regular

Corps because I think he was in the PHS before the Laboratory of Psychology was being considered. He had gone from Stanford to the Public Health Service, but not to come to the Laboratory of Psychology which hadn't been thought of yet.

Farreras: So if you arrived in October of 1953 and Shakow didn't arrive until late spring of '54 where were you placed when you arrived? What did you do when you arrived?

Carlson: Well, we started out in T-6, right over here where the parking lot is now.

Farreras: Right. And what were you supposed to do until the Lab was created?

Carlson: Mostly we just got together. We'd have lunch every day with Bob Felix and several other people, Seymour [Kety] and Vestermark.

Farreras: I thought Vestermark was in extramural.

Carlson: Yes, but at that time it was small enough that it was an informal group. We met for lunch fairly regularly and we talked about scientific matters or about whatever was of interest. And Dick Bell was often there. I don't remember how long we were there. It couldn't have been very long because we did move over to the Clinical Center. At that time the inside of the Clinical Center wasn't really done yet. They had the outside up, but on some floors they didn't have the walls that partitioned the building into separate rooms. So we moved over there after about six months or something. I'm not sure how long. There was another group consisting of these older Public Health Service psychologists that met, but they were kind of separate from the intramural research, the Clinical Center, and the Laboratory of Psychology, but they apparently had been doing this for some time.

Joe Bobbitt, Harold Skeels and Sid Newman and others had been meeting at the Navy Officers Club every once in a while. I don't remember how often, but it was mostly a social get-together. So that's how I got to know them in the beginning. I don't know that they invited any other psychologists besides the ones who were in the Public Health Service, in the Corps. I don't remember that any of the other people, like Mike Boomer or Parloff, came to those get-togethers. Bob Felix was often in on these. His main job, I think, was dealing with Congress, but he had a lot more free time then than he ever had afterwards, because once we moved to the Clinical Center, he'd always say he was going to come over and have lunch with us, but he never did. He never did. When we'd see him, he'd say, oh, yeah, I'll come over and visit; but apparently he never had the time to do it. He'd come over for a large group meeting and every once in a while he'd come over and give a talk to the whole intramural research people. He would do that occasionally. But we hardly ever saw anything of him personally after that. That was too bad because he was a really likable person. His philosophy seemed to be that anything that anybody wants to do is all to the good. We'll just get more money. If somebody said, "We ought to be studying this" and there was no one to do so he'd say, "Well, okay. Let's go find somebody to do that." He was quite a remarkable person. He essentially established the National Institute of Mental Health. He was a competent, likable and forceful person.

Farreras: So when you arrived you were in T6. How did the Lab develop?

Carlson: Well, at that time it was on hold until Shakow arrived and one of the things that

needed to be done – because there was a lot of money appropriated for equipment, and this equipment was being delivered – was decide what might be needed. This was pretty difficult, not knowing who was going to be here or what they were going to do. But the reason for having to decide was that we couldn't just hold it off; the budget doesn't carry over until the next year.

Farreras: If the money wasn't spent, it was gone?

Carlson: That's right. But it was unexpected that Shakow was delayed and I don't know when Nancy Bayley was supposed to be here, but she didn't show up until the same year that Shakow did. And Rosvold wasn't hired until that year either. Jim Birren had come fairly early, in the Section on Aging.

Farreras: Right, because he had been in the Heart Institute up in Baltimore.

Carlson: Well, there was a gerontology research center out there on the Baltimore City Hospital grounds, and there was a Public Health Service building out there, and he was in that. In fact, to this day there's a big portrait on the wall there of Nathan Shock. And I think Jim Birren and Gene Streicher both may have been connected with Nathan Shock's group. And then Jim and Gene Streicher came to the Clinical Center. But that was a little bit later, '54 or '55. I don't know which for sure. You have talked to Gene Streicher?

Farreras: Yes, and I will talk to Jim Birren over the phone tomorrow.

Carlson: Oh, okay, tell him hello.

Farreras: I will.

Carlson: He was considered for the position of director of the extramural program or something, I don't know. And do you have Ed Jerome's name?

Farreras: He was the last one left in the Aging Section.

Carlson: When Jim Birren left, Ed Jerome became Acting Chief or Chief. I don't remember which. I guess Gene Streicher was still there for a while, but then he went to the extramural program. Ed Jerome had come a little later, too. I believe he was at the Naval Medical Research Institute across the street. Jim Birren might have had some connection to that group as well.

Farreras: I'll ask him.

Carlson: Ask him about that. He was the one involved in getting Ed Jerome to come, so I'm sure he knows where Ed came from. Ed Jerome died around 1976. He had retired from the laboratory, but not for very long. The Section was called Higher Thought Processes by then, I think. I'm not exactly sure when it changed, but I don't think Ed Jerome was terribly interested in aging.

Farreras: The name changed in '66, the year Shakow retired.

Carlson: It might have been '65 because if you got it from a directory, the directory might be a year late, because I have those two dates in my head, '65 and '75, as when there were changes.

Farreras: I see. What happened in '75?

Carlson: I think that's when the Laboratory of Psychology was changed to the Laboratory of Psychology and Psychopathology.

Farreras: I meant to ask you about that because that's when the directories reflect that change. But everyone else I've talked to says that it really changed when Rosenthal took over in '66 and that the directory is mistaken.

Carlson: No, I don't think so, because we had Section Chief meetings under Shakow

almost every week, as a regular thing. These were mostly administratively meetings, not research meetings for scientific topics. We might talk about having some good professor come and give a talk, but it was mostly about administrative and personnel things. And we were the same Section Chiefs for quite a long, since '54: Jim Birren, Nancy Bayley, Morris Parloff, Hal Rosvold, and I and Dave Shakow. And that group stayed the same for quite a while although by the time Rosenthal became Lab Chief, Jim Birren had left. I think Ed Jerome was the Section Chief in those meetings. And after Nancy Bayley, it was Harriet Rheingold and then Jack Gewirtz. Harriet Reingold and Earl Schaefer didn't get along very well at all. But Earl left and went to the University of North Carolina, and Harriet left and went someplace, too.

Farreras: She also went there.

Carlson: Yes, to the same place. But I remember Nancy Bayley was still the Section Chief after both of those people had left.

Farreras: I thought Bayley left in '64?

Carlson: I don't think so.

Farreras: I thought she was here from '54 to '64 or something?

Carlson: I think she was still here after Harriet Reingold left.

Farreras: I'll check on that.

Carlson: Yeah, that's difficult. But then I don't think Rosenthal was interested in or the Administration didn't want him to keep the Sections going and there was always a problem of space. I don't know. You may not have all the people in the Section on Perception. It started out as Perception and Learning.

Farreras: Yes, and Don Blough represented the Learning portion of it.

Carlson: Don Blough, yes.

Farreras: From '54 to '58.

Carlson: That sounds right.

Farreras: And then I have a John Calhoun, who was working with animals in Poolesville?

Carlson: Well, not at the beginning. He came from the Walter Reed Medical Institute of Research. I don't know if it still but it was more of the B.F. Skinner operant conditioning kind of thing. Joe Prady and Murray Sidman and this guy who wrote the Bell curve book.

Farreras: Herrnstein?

Carlson: Dick Herrnstein. He was there. And Don Blough was actually more connected with that group than he was with anybody else, because Don Blough was a student of Skinner's at Harvard.

Farreras: He was telling me that everybody says that about him, but that he really didn't have very much contact with Skinner at all.

Carlson: Who? You talked to Don Blough?

Farreras: Don Blough. Yes.

Carlson: Oh, okay. Well, maybe that's true, but Skinner came to visit the Laboratory on more than one occasion. He always got together with Don Blough and me. I don't really know what he did. Anyway, Jack Calhoun was at the Walter Reed place under Dave Rioch, and Rioch and Shakow decided it would be good for Jack Calhoun to come to the Laboratory of Psychology at NIMH. There might have been more to it than that.

Farreras: Was the Perception and Learning Section already established?

Carlson: Oh, yeah. Well, that's what we called it. It was called that from the beginning. But you have to understand this. I was just out of graduate school and I was not interested in being a Section Chief or a Lab Chief or anything. I wanted to see us hire some really outstanding person in either perception or learning, but primarily in learning, because that was a big field. It still is, really. And it's one that everybody thought should be represented in the Laboratory. But the notion was a little too grandiose because of the people who were considered for the Chief's position. The most important person considered and who did actually show some interest was Neal Miller. And that did go on for a while. He came to visit and he wrote up a proposal of what he would need, rooms, space, personnel and so forth. Well, it turned out that if he had gotten all that stuff, he would have sunk the Laboratory. I mean, everybody else would have had to move out because he was pretty grandiose. He had these facilities at Yale and Miller wanted to get more than he had at Yale and he wasn't being successful, so he was acting as if he was going to leave to provide some leverage at Yale. And in the end he got what he wanted at Yale. If he hadn't, he might have come to the Laboratory of Psychology here. But it was pretty academic because the space and positions weren't available for him. It was just really unrealistic. And he was another person who was not terribly taken with teaching, and he would have liked a position where he could do whatever research he wanted, get all the facilities he wanted, get all the people he wanted, not have to write any grant proposals. So I think there was some appeal to him there. But in the end I think he got these

things from Yale and it just became academic, but he couldn't have gotten all that here. It just wasn't available. But then there were a number of other people who were considered, and I was always hoping...

Farreras: These were all people being considered before you became Section Chief?

Carlson: Yes, this was before '65. I was the Acting Section Chief for all those years before then, from '54 to '65, and Chief from '65 to '75. One of my permanent jobs was to help find somebody to be the Section Chief. Miller was the most important one we contacted. There was general agreement that it had to be someone accomplished and with a reputation in the field of learning. That was a big field in psychology, still is. So that was the main focus. And there were a number of other people who were considered, and they would come and give a talk and we'd talk about them afterwards, or they'd say they weren't interested.

Farreras: What were some of the reasons they gave for not being interested?

Carlson: One of the biggest reasons, I think, was not having a group of graduate students to work with. These were people in graduate departments, their assistants would be graduate students, and the graduate students are sort of junior colleagues...so graduate students was one of the big things. I suppose there were some who were also quite interested in teaching. They didn't want to get out of the academic milieu. But the other thing was that it wasn't clear what they would do if they did come here. That wasn't too clear. One of the philosophies in the whole intramural program was that people would think about hiring people first and then go about getting space for them. Well, after a while, there wasn't any space to be

gotten, and there was a pileup of people and a real struggle for space. I think some of the visitors saw that or were told that and the other thing about it was that Shakow was ambivalent about that. He didn't disagree with somebody in learning, and, in fact, he went along with that principally, but he had in mind somebody in psychophysiology.

Farreras: What Ted Zahn was doing?

Carlson: Well, he had a unit. I think Walt Stanley had a unit and I think later...

Farreras: I thought Stanley had his own section? [Section on Comparative Behavior]

Carlson: Well, he was in the Section of Child Development. But that didn't seem to work out, mostly between him and Harriet Rheingold.

Farreras: You mean Stanley?

Carlson: Walt Stanley, yes.

Farreras: I didn't know that he had any contact with her at all.

Carlson: I think he was in that section to begin with. I could be wrong about some things, but that's my memory. He was in the section on child psychology and then either left or got moved out of that section. Now, there's somebody else here that maybe you don't know about, Paul MacLean. When he first came, he had a joint appointment – I don't know how legal this was or how far it went – in the Laboratory of Psychology and the Laboratory of Neurophysiology, and he came to our Section Chiefs' meetings for a while. That's where I first met him. That didn't last too long, though, and he or maybe some others decided that there was no use having him in both laboratories. So he ended up in the Laboratory of Neurophysiology but later on went out to Poolesville and headed the Laboratory

of Brain Evolution [and Behavior] or something. Jack Calhoun went with him and I think Walt Stanley did, too, but I'm not sure about Walt Stanley. I know he was out there because I would have some conversations with him over the phone, and he was out in Poolesville. He was very unhappy there. He thought he was really being given the short end of the stick out there. Jack Calhoun never complained, that I remember. But Jack Calhoun went out there and he had a unit. I don't know what that was called [Section on Behavioral Systems]. But it was a unit in Paul MacLean's laboratory. Jack, though, was quite an entrepreneur, to somehow make out for whatever he wanted to do.

But that was pretty much the basis for it. It's the lack of space. Where are these people going to go? Poolesville was one place to go.

Farreras: What was the difference between a Unit and a Section? Was a Unit part of a Section?

Carlson: No, no. Actually, it wasn't. It was just a little too small an organization to be a Section but consisted of one person, maybe one or two research assistants, but very likely didn't have a secretary. So it was a small section but what it was administratively, I'm not quite sure. But it was referred to as a Unit, and Walt Stanley, I think, had a Unit out there in Poolesville, Jack Calhoun had a Unit. He was chief of the unit under Paul MacLean.

Farreras: Ted Zahn mentioned having a Psychophysiology Unit.

Carlson: Yeah, as I had started to say, Shakow was of two minds about this Section Chief. Instead of getting somebody in learning, he wanted to get somebody in psychophysiology. He had in mind some senior people in psychophysiology, but

I don't know what ever came of that. A couple people did come and talk to us. I remember one, and Dave Shakow decided he didn't like him and didn't think he would be very good.

Farreras: Big name?

Carlson: Well, he was then. Robert Malmo from Montreal, at McGill University. He was one he thought he would like to hire but after he met him he didn't like some of his ideas or something. I don't really know. But then Shakow didn't push that too strongly and seemed to be happy with Ted Zahn and his unit, and Ted didn't seem to be...

Farreras: Why not put Ted Zahn's unit under the Perception and Learning Section rather than this Chief's Section?

Carlson: Well, that Chief's Section consisted of a number of people with different interests. It wasn't very cohesive at all. Shakow, for his own research, would hire people like Paul Bergman who would just come to work with those schizophrenic quadruplets and some other people like that who were very specialized, but would not have much to do with the rest of his Section. So it was a diverse section and not too unlikely that Ted would be in that section because the psychophysiology people were more concerned with physiological indicators of emotion, something Shakow would be interested in. I don't think Rosvold was interested in that very much at all. Al Caron was someone like that. I think he started out in Morris Parloff's Section [Personality] but his interests were in perception with infants. And don't ask me why he didn't stay in Parloff's Section. I don't know. But the problem was that he didn't want to or somebody didn't want him to. But there

again, it was the space, and we had to find space. He did it himself, mostly. He found the space. There was a place called the Institute of Behavioral Research in Silver Spring, and he set up an infant laboratory there, and it was actually rental space. I remember we had to arrange to pay rent. But that's what was done. We didn't have the space in the Clinical Center. And that's the thing. He could have gone to some other laboratory, like Marian Yarrow's Laboratory [of Developmental Psychology]. But no one had any space. And the one reason why they ended up in the Section on Perception was that the positions were still available, as well as some space. These positions hadn't gotten filled. They were being saved for somebody like Neal Miller. I think, later on, Al did maybe go into Marian Yarrow's laboratory. I'm not quite sure what happened later. Then he ended up going to Boston. I don't know where exactly. But he was a full-fledged member of the Section. He wasn't just on loan; he came into the Section and took up a position. And Josephine Semmes was in Rosvold's section in the early 60s maybe but came into my Section in 1970.

Farreras: Oh, I didn't know that.

Carlson: She became a full-fledged member of my Section but kept her own space in Building 9, in Rosvold's Section. I can't say why that happened. I think there were personal difficulties involving her and some other people. It wasn't that she was not doing a good job but for some reason... Maybe she wanted out of that Section. I don't know. She probably told me something about it, but I just don't remember.

Farreras: How long did she stay in your Section?

Carlson: Probably until '75. But she might as well have been called a Unit over there by herself, although all of the administrative things went through me. If she wanted something I had to approve it. Al Caron and I did some research together so that worked out okay but I never did any research with Josephine. You might say, technically, that I approved it, but I certainly didn't try to tell her what to do.

Farreras: And Don Blough came just a year after you did, in '54.

Carlton: I didn't know him or about him until he came to the Laboratory. I bet he was recommended by someone from the Walter Reed Army Medical Research Institute because Shakow and others was acquainted with them and Dave Rioch was well known by NIMH people. It was probably through that he came because their Laboratory had a pretty strong representation of the operant conditioning kind of research that Don Blough was interested in and was doing. I don't think he ever really took to the NIMH too well. I think he was more interested in being in an academic setting, working with students. It seems to me he really never had a whole lot to do with the rest of people in our Laboratory. He had a lot more to do with the people who were at Walter Reed. I suppose it was because there wasn't anybody else in our Laboratory that was much interested in operant conditioning, and that was a big thing over at Walter Reed. I think he was a lot happier in an academic setting. And he became chairman of the department at Brown.

Farreras: He's still at Brown.

Carlson: I guess he is, yeah. He must be about ready to retire.

There were a couple of visitors who came to the Section as well, like Judy

Rapoport. I think she was in my Section for a couple of years. She was on a visiting appointment of some kind. I knew her and her husband very well when I went to Uppsala, Sweden. They were there at the same time, so it was after that. It must have been '64 because I came back in '63. But she and her husband stayed there for a second year.

Farreras: It's a nice city.

Carlson: Have you been there?

Farreras: Yes.

Carlson: That was really a great experience. And you know what happened there? Gunnar Johansson was the professor of the department there when I went there, and at those old-time European universities, the professor is the big cheese. Nobody else has tenure. He does and he decides. He's the one everybody has to please. I arrived but after a couple of months or so, he was going to Cornell on a visiting arrangement so before he left he had a meeting with the whole laboratory – all the staff and all of the technical people – and says, "I'm going to be gone for the rest of this year, and when I'm gone, you treat Herr Carlson as if he were me." He really set it up. And I can remember one of the people who was working on a dissertation and was doing an experiment in perception. He and I were talking about the apparatus, how it wasn't working very well, and I said, "Well, you know what you ought to do? You ought to get the people in the Technical Development Section" to do something or another. He said, "Yeah, if you went to them and said, 'Do this for me,' they would do it. If I go there it'll take 10 months to get it done." So I was treated very well. I liked the people there, too,

not just the people in the department. They were very congenial..

Farreras: How did your going to Sweden come about?

Carlson: Well, at the time there was a fad of people going off on sabbaticals.

Farreras: I hear David Shakow tried to emulate the academic sabbaticals within his Lab...

Carlson: That's right. And this was one of the perks that he and I suppose other Lab Chiefs, too, thought, was a very good thing for NIMH to emulate. So that's the way it was set up. Jim Birren went to Cambridge in England. And so then it came up, where do I want to go? I think I just decided that I knew of Johansson and a couple of other people who were in the Psychology Department at Uppsala, so I just picked them. That's where I'd like to go. And it worked out fine. I guess everybody went someplace. Jack Calhoun went to the Institute for Advanced Study in Stanford, I think the same year. But for that, you didn't just pick, you had to be invited and recommended. And he was and he went there.

Farreras: And when you returned that's when you became the formal Chief of the Section?

Carlson: I think that was in '65. I'm not sure that the other sections got renamed at that point. But I think what really determined that was that it finally became pretty clear that we were not going to hire anybody like Neal Miller in learning.

Farreras: Who else had you considered besides Miller?

Carlson: Frank Logan was one. I remember him as being at the University of Arizona, but I don't know if that's where he was at the time. Judson Brown was another one. I don't even know if some of these people even knew they were being considered. Maybe we were considering them but decided not to invite them out.

Farreras: But Neal Miller is the only one who actually visited and considered coming?

Carlson: I don't know that he was the only one. I just remember him because he was the most important one and the one that it would have taken the most to get. There was some talk of Kenneth Spence, too, but people like John Eberhart just didn't think anything of his research ideas. So I don't think that it was ever even mentioned to Spence, and I don't know that Spence would have had any interest. I was told he was pretty much like a child: really bright when it came to his research and academics but if he didn't have a wife telling him when to eat dinner then he wouldn't eat dinner. He was the absent-minded professor that was childlike. I never met him but I read a lot of his work and there's no doubt that he was a bright guy. But John Eberhart didn't think much of him. He was not a person liked by a lot of people. But by the time it was decided to be the Section on Perception, Shakow or Cohen and Eberhart just thought that we were fooling ourselves if we thought we were going to find anyone and by that time positions had gotten very scarce, space had gotten very scarce, so there wasn't anything to offer anybody.

Farreras: And by then no one was doing learning either, since Blough had left in '58.

Carlson: Well, the people who fit under the rubric were Don Blough, Jack Calhoun, and to some extent, Walt Stanley. Walt Stanley was a real expert in learning, very knowledgeable about learning theory, but that was not what he was doing. But he would have fit there okay. Judy Rapoport came as a visiting fellow or something. It wasn't visiting scientist because it had to fit into the civil service rubric. Fellow seems to me to be the term that went with it. She came for a couple of years. And one who technically fits into that category is Leo Szilard. I don't know if

anybody remembers him coming to the laboratory; this was earlier, when Bob Livingston was Scientific Director. He belonged right along with Einstein, Fermi, Niels Bohr, and Oppenheimer. He's one of the fathers of the atomic bomb. Somehow Livingston got acquainted with Leo Szilard or knew somebody who knew that what Szilard wanted to do was to go someplace and develop and write up his theory of aging. From his viewpoint he had a theory that explained the basic process of aging in a neural way. So Livingston got him to come to NIMH. This was never advertised very much and Szilard never showed himself much but he was put in my section because I had some space and because the position was there. So technically I was his supervisor. The thing about it is that he would have been more appropriate in Birren's Aging's Section, but I don't think Birren had any space or positions. But this was not the kind of theory that Jim Birren was interested in anyway. Szilard had no interest in behavior at all. The basic notion was that the aging process consisted of ordinary, everyday, natural radiation gradually killing cells. If you could put somebody as a baby in a lead room with 10-foot walls so that no outside radiation would reach the person they'd live forever. That was the basic notion as I remember it. And the other thing was that Gordon Klovdahl was the administrator at that time. In my opinion he was the best administrator we ever had, a really top-notch, competent person, under Livingston. So this proposal was made, he was going to take up some space I had and Gordon Klovdahl took me aside and said, "Now, look. We don't want this to be known, and I'm just telling you because we need the space. Szilard has cancer and is going to die before long, and here is this great theory

he's got and he needs a facility someplace to work on it, write it up, and get it published before he dies. So we've got to do this right away." I don't think it would have done me any good if I had argued against it and I liked Livingston a lot and didn't really disagree with this notion but Jim Birren and I talked about this and thought that this theory of aging didn't really sound like it was going to get anywhere. So he was in one of my rooms but I never got to know him, really. I'd say good morning to him if I met him, and he'd look at me and nod and walk right on. If he wanted anything he went to Livingston or even the NIH director and some thing he wanted were just foolish. For example, he wanted them to build a canopy out on his outside window. Why I don't know, but that's one of the things he'd said he wanted. And he probably wanted some plush things for his office. But Livingston wanted to do it. He thought it was a feather in NIMH's cap to have him there. But hardly anybody knew it. He didn't come to any of our meetings, he didn't give a talk at any of our meetings, he hardly even talked to Jim Birren, who was the head of the Section on Aging. I don't know how long he was there, maybe a year, but probably less than a year. And he did write this theory up and it got published in the National Academy's journal, the *Annals of the National Academy* or something. It was a reasonably long article and I remember reading it; I don't know if I understood it all, it had a lot of mathematics and a lot of physical discussion of what happens to particles and neural cells. But it got panned. It wasn't accepted in the scientific community because it was just too simple a notion to attribute all of aging to this radiation effect. I also thought that it didn't have anything to do with development and that

it didn't go very far toward explaining aging. If you start with a child or with babies, how in the world can you attribute that kind of aging to radiation killing cells? I don't think he dealt with that at all. Anyway, that episode came and went. He did publish it. And then Szilard did die, but he died of a heart attack, not cancer. He was probably around 70. He didn't seem decrepit to me at all or in any pain. He might have been, though, because he just didn't talk to us. He was like a ghost. Mostly we wouldn't see him. If we did see him, we didn't talk to him. As far as I know, he didn't talk to anybody else either. So anyway, I don't think there was anybody else in the section.

Farreras: Okay, so you became Section Chief in '65 and then Shakow retired in '66. Were there any major changes to the lab after he retired?

Carlson: Well, as a matter of fact, I remember several of us were asked to write up proposals for suggestions for Lab Chief. I don't remember that for sure. But Shakow didn't want to continue because he had collected a lot of data over the years and he wanted to retire from administration and work on his data. But before Rosenthal was appointed we were asked to suggest a Lab Chief. I don't remember who of us did that. It would presumably be the Section Chiefs but I remember was specifically not involved because we recommended Rosvold. It was Dave Rosenthal and me and there must have been another person or two, but I'm not sure who. Several of us wrote this proposal up and said we recommended Hal Rosvold for Lab Chief and Rosenthal agreed with it. I don't think Rosenthal really wanted to be Lab Chief.

Farreras: So the other Section Chiefs had to have been Ed Jerome in the Aging Section and

Harriet Rheingold for Developmental, and Morrie Parloff for Personality...?

Carlson: It might have been Jack Gewirtz in Developmental. And not Parloff; I don't remember that he was involved. I don't know why not if he was Section Chief. Maybe he was. And then we also had some conversations with Mel Cohen, who was the Chief of the Laboratory of Socio-environmental Studies and talking about who should become the Lab Chief. But then either John Eberhart or Bob Cohen or both rejected this suggestion. We were told that Rosenthal was going to be the Lab Chief, and he was made the Lab Chief. We didn't disagree with that too much, we liked him, but it was pretty clear – his research was in behavioral genetics – that he was not going to be much interested in the other parts of the lab, and I don't think we had Section Chief meetings anymore. The Psychopathology was added to the Lab's title to take attention away from the rest of the Lab Sections. And then Dave Rosenthal developed Alzheimer's later on and Bob Cohen became Acting Chief of the Laboratory for a while until they got a hold of Herb Weingartner to be Acting Chief for several years. We moved over to the northeast corner of Building 31, either the fifth or sixth floor. And Rosenthal had an office over there to begin with, but he began to get lost in the neighborhood. I asked his wife Marsha one time, would it be good to come over and visit him? And she said, "Oh no. If anybody he knows from the lab comes to visit him he goes into a tantrum after they leave. He just can't stand that. It makes him really upset. It's far better if you don't come and see him." So I never did. That was a sad thing.

Farreras: Going back to what you said earlier, why was Rosvold not supported for the

position of Lab Chief after you'd all agreed that he would be the best person?

Carlson: Well, not all of us agreed. Only several of us made this proposal. But I think the reason was that Cohen and Eberhart were more interested in building up Rosenthal and behavioral genetics. But I don't remember that we were given any reason for it. And the other thing might have been that they had other things in mind for Rosvold. I don't really remember talking to Rosvold about it, although surely we did. But that's my memory, that Rosenthal wasn't too interested in being a Lab Chief. He wanted his own section and he wanted the facilities to do his own research, but he didn't really want to be constrained by being Lab Chief.

Farreras: Did that contribute to the sections dwindling by the mid to late 70s?

Carlson: I think so, although I think the biggest issue was positions and space. It wasn't that Rosenthal was taking it away it was that it had already disappeared. Almost from the beginning, space became more and more scarce because more and more people were being hired, but space wasn't being provided before they were hired. But I do think Rosenthal was not really interested in administering a whole laboratory, including people who were not part of his own research. And Rosenthal was also connected with Seymour Kety in his research at the time. Later on, Kety went to Boston. Of all the people who should have gotten a Nobel Prize, some of us thought Kety really deserved it. He was a well-known scientist, he was very good, and a terribly likable person.

Farreras: Do you think some of the Sections in the Lab might have dwindled after Axelrod got the Nobel Prize, because then there was such a focus or shift from all of the multiple perspectives in psychology to a very biological, physiological...

Carlson: Biological, physiological, and medical. Well, that was true, but I remember when Julie Axelrod's office was just around the corner from mine in the Clinical Center, and I knew him pretty well. When he won the Nobel Prize, he and I were talking about how this had to be a terribly good thing for the Intramural Research Program because we were getting such short shrift. In general the notion was that going to ADAMHA was a big mistake and what this meant was that the Intramural Research Program, in order to look important, was going to have to be much more medical, much more biological, and oriented toward curing diseases and things like that. So I remember this conversation with Julie Axelrod in which winning the Nobel Prize, not only for him but for the whole organization, was going to be a big boost. I don't know if it worked out that way, but we never liked that business of shifting to ADAMHA. What we thought – but it might not have been true – was that the administrative people went up a notch because NIMH became equivalent to or was at the same level as NIH.¹ So the director of NIMH was now on an equal level with the director of NIH. That was the only reason for it; the administrative people wanted this promotion, and they engineered it. And I don't think any of the research people I ever knew thought it was a good idea. We always thought that being part of NIH was all to our advantage, especially since mental health didn't have great press with the public. For example, the public would think, well, why don't they cure schizophrenia? That's the institute that should do it. Well, we didn't have any big gains like

¹ NIMH separated from NIH on January 1st, 1967 and was assigned to the newly established Health Services and Mental Health Administration on April 1st, 1968. It then became a part of ADAMHA on September 25th, 1973.

that, and being on an equal level with the other institutes was all to the good. It was advantageous to us to be part of the NIH research community and not a separate institute. So I don't remember any of us thinking that it was a good idea.

Farreras: But if NIMH had equal standing with NIH why then be put under ADAMHA?

Carlson: Well, it was within ADAMHA. Fred Goodwin was a Section Chief in the Intramural Research Program for a long while, and then ended up the director of ADAMHA, I think. I don't know really know how that came about, but he went from being a Section Chief to be the Surgeon General or somebody.

Farreras: So this decision to become part of ADAMHA was made at the administrative level.

Carlson: Well, we thought that. I don't think any good reason was ever given to us.

Farreras: And you, as intramural researchers, didn't have any say in the matter?

Carlson: None. I don't even know how far up that decision went. It might have been decided by Congress.

Farreras: Did both the extramural and intramural go?

Carlson: I think it was the entire institute. What happened was that the Intramural Program stayed here in the Clinical Center and functioned pretty much as it always had but with a different organization. But we didn't get any extra space or positions or anything.

Farreras: If anything, I hear that the other institutes were pretty upset because, if you were not a part of NIH anymore, what are you doing here taking all of this space?

Carlson: Well, that could be, although I think they were sympathetic. I mean, I remember at one point the director of NINDB at the time – Donald Tower – gave a talk to us

at some pretty large meeting. Things were so bad at NIMH that he said that NINDB would go to bat for us and help out, provide some positions if necessary. It sounded like a magnanimous gesture and I don't know if it was ever done, but he was certainly sympathetic. The general atmosphere was that we were in very bad shape. Now, Herb Weingartner – I'm not quite too clear – must have become Acting Lab Chief after Rosenthal and Bob Cohen. I'm not so sure about this because there was still discussion about getting a new Lab Chief, and that's when Al Mirsky was recruited. I'm thinking about the sequence of things here because those of us who were left were saying, "Well, Herb Weingartner would make a good Lab Chief," and we talked to him about it, and he said, "Oh, there's just no chance of that. They're not going to make me Lab Chief. They want somebody else." They didn't want him.

Farreras: What was he doing prior to this time? What was his affiliation?

Carlson: He had some connection with Hopkins, the medical school or the department of Psychology after I had left. We said, "We want to support you if you want to do it." Well, he didn't say he didn't want to do it, but he said, "There's no chance that they're going to do it." And they ended up with Al Mirsky.

I think I had left by that time. I'm not sure.

Farreras: He came in '80, so you must have left by then. Had your section disappeared by then or did you leave before it happened?

Carlson: I think it had already pretty much disappeared.

Farreras: Well, not only yours, but most of the other ones, too. Any thoughts as to why those sections disappeared?

Carlson: I think they just didn't have the visibility in a medically oriented institution. Not only medically oriented but biologically oriented and disease oriented and what we had been doing in our sections was not connected with that. Shakow's philosophy, too, was that we don't understand normal behavior well enough to say anything about abnormal behavior. And that's pretty much the way we all felt about it, but that didn't go over in the political sense. And by the time NIMH became part of ADAMHA, I don't think the people running things had any interest in the things that the Laboratory of Psychology had been doing. What I still thought we needed was somebody who would make a big splash in drug abuse or something, but the Laboratory of Psychology didn't do that. That was being done by Dick Wyatt at St. Elizabeth's [Hospital] but he ended up, I think, with the Section or Laboratory in psychopharmacology. There was Ed Evarts, and the psychologist in that section was Conan Kornetsky. He was the person doing research with drugs and he thought he should be made a Section Chief, but I don't think he was, so he left. He went to Boston University, I think. Sensibly, it would have been smart to put him in the Laboratory of Psychology and give him a Section. Now, I don't really know much about what happened after Al Mirsky came here. He was the Lab Chief and I guess the Lab at that time was located in the outpatient part of the Clinical Center because I visited there a few times.

Farreras: Could you summarize the type of research that you did all those years in the Perception Section?

Carlson: Well, it had to do mostly with visual perception, perceptual theory, things like

perceptual constancy, size constancy, shape constancy. It was the kind of thing one would find in an academic university department. And then Al Caron and I did research with infants and at the same time I did some research connected with Conan and Kornetsky with LSD and some sleep deprivation studies with Bob Feinberg, a psychiatrist in NIMH but at St. Elizabeth's Hospital. He was the person running the ward. I'd been associated with him for a long time; we had done some research at various places. Once we went to Ann Arbor, Michigan, to test some schizophrenic patients there. So my research was directed primarily toward basic processes, basic research, and I never was very much interested in studying any diseases, except it was interesting to see what would happen to schizophrenics.

Farreras: Morrie Parloff mentioned that a lot of the clinical people were experimenting with LSD as a way of emulating an experimental psychosis they could then study.

Carlson: That was the way it was first. I had forgotten that. But as far as I know, it didn't lead to anything. There would be normal subjects who came and stayed in the Clinical Center and were given LSD. They hated it. The effects were terrible. And later on, people were taking this as a psychedelic drug. It never made any sense to me. I never did take any myself, but I saw why those people just absolutely hated it. I didn't know anybody who liked it. They had agreed to be subjects, so they went ahead and did it, but none of them liked the experience. But there wasn't the cult associated with it where a lot of people tell you that it's great.

Farreras: Didn't Maitland Baldwin and John Lilly also do sensory deprivation work?

Carlson: I don't really remember Baldwin himself. I remember the name. I did know John Lilly; he came from the Eli Lilly family. His research involved putting electrodes in monkeys' brains and hooking them up to a big display board. Lights coming on and off would show what the brain was doing. And then he got into this business of dolphins being able to communicate. That was after he left NIMH. And I don't know that that ever made any sense. This business with the lights going on I'm not so sure made any sense either, but it was a spectacular display, because I don't think anybody knew what the pattern of lights *meant*. He wasn't around too long.

Farreras: Aside from the members of the Laboratory of Psychology, Evarts and Kornetsky in Clinical Science, and Savage in Adult Psychiatry, were there other scientists involved in LSD research, for various purposes? Maybe some of the psychiatry sections, other people doing LSD research? Because I'd read that the CIA was funding LSD experiments for mind-control research right around the time of the Korean War and after that, and I wasn't sure if it was related to the LSD research that the NIH people were doing.

Carlson: The people primarily involved in that LSD research, it seems to me, were Ed Evarts, who was prominent, and Conan Kornetsky. Oh, when I first came to NIH, one of the first lab talks I heard, somebody presenting his research, was Savage. That's the first I heard of LSD research. He was talking about the effects of LSD. I don't remember what he said, but I do remember that that was probably the very first talk I heard at NIMH of a scientific meeting. And I did get a little acquainted

with him, but I think he left not too long after that. There were other psychiatrists here, but they pretty clearly were on temporary appointments of some sort or another. They were psychiatrists who were going to go back to their private practice and were discharging a military obligation and had no intention of staying. They weren't too serious about research either, although they weren't against it either, but I didn't get the impression that they thought it was very important. So we did have quite a bit of contact with the psychiatrists and other laboratories. We went to each others' meetings. I often went to the neurophysiology meetings, and they would come to our meetings, too.

Farreras: When did you leave NIMH?

Carlson: I think in '79. I had some kind of special arrangement. It seems to me that it was more advantageous to retire from the Public Health Service then and not wait. It had to do with how the pay would get calculated or something. So I retired from the Public Health Service. But then I got Bob Cohen or John Eberhart or somebody to give me a guest-worker appointment or something and so I did that. And then I got an appointment as an instructor at the University of Maryland. This was not a tenured appointment. This was so they could pay me more money than an assistant professor but not involve tenure. That must have been 1980. So I taught over there for a year, I guess, and didn't like it too much. When you're in that kind of position, the senior faculty have their choice of what they're going to do and what they're going to teach, so for a person like me who was essentially just a visitor, I got to teach what they didn't want to teach, and I didn't want to either. And the other thing was access to students. The really serious graduate

students were taken up by the tenured faculty. I had some good students working with me, but they were undergraduate students. So it clearly was a secondary job as far as the department was concerned. And I don't know that I was so taken with teaching anyway. But I thought my time at NIMH was very enjoyable. I liked the people. In those days, I think even more so, you got to know everybody, and that's the way things worked out. You got a job someplace because you knew the right person, not so much favoritism or anything but just because you knew him. It was small enough group of people. If an appointment were coming up, you'd know about it, where other people wouldn't know about it until after it was decided.

Farreras: What were some of the pros and cons of a career at NIMH? Bob Cohen mentioned that he had a hard time recruiting people to come in the early 50s because everyone was afraid of doing research under the government and McCarthyism was still...

Carlson: Oh, yes, it certainly was thought, on the part of the academic people, as a step down from a university to go work for the government. Although the Public Health Service didn't have a bad reputation, I don't think. What didn't sound so good were these military research labs that were set up. There were quite a few of those. Human engineering was one big thing. But next to the universities, a place like, Bell Labs had prestige. IBM was another one that had a good research organization in Yorktown Heights, New York. And the big advantage of those places, and to some extent also of NIMH, was you could get your research funded and you didn't have to go begging for money, write grants...although at that time,

in the 1950s, it hadn't gotten to be such a big grant operation in the universities. It hadn't gotten to the point where that was the total business, where you didn't have any money if you didn't have a grant. And you also wouldn't get tenure if you didn't come up with some research money. So there was that going for it. But at the time people certainly still, by and large, preferred to go to a university. In fact, Al Chapanis and Wendell Garner, the principal professors at the Hopkins Department of Psychology – with Morgan – each took a year's leave and went to Bell Labs to work there and to see how would they like it, and if they liked it, they were going to stay. They both came back and absolutely didn't like it.

Farreras: Do you know why?

Carlson: The only thing I ever heard – because Bell Labs had enormous resources – was that Chapanis was disgusted because when he and his wife went out there and bought a house his superior had to go out and buy a new house because he was his boss. And from Chapanis' viewpoint there was a miserable sociological situation like that. In fact, he said he was told by his superiors that when you go to a scientific meeting like a convention that you don't take public transportation. You take a taxicab wherever you want to go and don't act as if you're poor in any sense. You're representing Bell Telephone and you want to look the part and act the part. These kinds of things just rubbed Chapanis the wrong way, and Garner too, I'm sure, although I didn't hear Garner talk about it so much. But they both came back to Hopkins. And then Garner went to Yale, and I don't know what happened to Chapanis. He may have stayed at Hopkins until he retired. I'm not sure.

Farreras: So one of the good things about a career at NIMH was that you had all these resources. You didn't always have to be asking for money and writing grants.

Carlson: That's right. And there also wasn't anybody telling us what to do. I suppose that's another thing that rubbed governmental and administrative people the wrong way. There should be a hierarchy, a bureaucracy. The one up here should be telling the people down below what to do. I can remember that at one time our notion was that the senior research person was the Section Chief, not the Lab Chief. The Lab Chief had administrative problems to deal with and the Section Chief was only supposed to do his research. He was the senior research person and he should be paid as much as anybody. I don't think that ever happened really, but we all did get promoted. And in the civil service and in other administrative parts of the government, the promotion would depend very likely on how many people you were supervising. And we had a lot of difficulty writing job descriptions because, from our viewpoint, this person is a scientist, he's doing his research, and he deserves a high grade level even though he's not supervising many people. M.D.s did get a special incentive pay; that wasn't too unreasonable because, at that time especially, medical people could command a lot more money. If they didn't get them some more money they weren't going to keep them. They weren't going to get them in the first place. So one way around that was to institute this special incentive pay, but psychologists didn't qualify for that.

Farreras: Would you say that might be a negative feature of spending a career in a government agency like NIMH, if you were a psychologist?

Carlson: Well, I always thought that NIMH was a lot better than a lot of other government agencies; we weren't subjected to a lot of the nonsense that one would have to put up with in an organization like the VA. And the people above us in the hierarchy could have caused us trouble, but they didn't seem to want to. So all in all it was quite pleasant except for the fact that things went downhill toward the end with support not going in our direction. Even early on, Shakow had a little bit of a demotion, because when he first started out as Lab Chief he had a big office on the fourth floor of the Clinical Center about twice the size of an ordinary module. And then he got moved out of that into a much smaller office on the second floor.

Farreras: When was this?

Carlson: Oh, pretty early on, '54, '55, '56...

Farreras: Oh, way at the beginning, then.

Carlson: Very early on, yes. But I remember he had that big, spacious office to begin with because we had Section Chief meetings there. When we got down to this small place and then we had to have a Section Chief meeting in half a module. That might be one reason why there weren't more Section Chiefs, because there wasn't room for any more. But those Section Chief meetings lasted quite a long time and they were oriented toward Shakow. He would bring up what he wanted to bring up. He'd ask for opinions about what he wanted our opinions about, and that's what we would talk about. And he was quite willing to listen to criticism. We'd bring up things we didn't like and he'd say something like, "See if you can do something about it." He didn't criticize us for doing that. Those meetings were

reasonably enjoyable for being administrative meetings. One person that maybe has been brought up is Sally Kendig. Have you heard about her?

Farreras: I've heard her name but I don't know very much about her.

Carlson: I guess she was in Shakow's section because sometimes, if Shakow weren't there and there should be a Section Chiefs' meeting, she might be there. She was a terribly likable person and a very, very experienced clinical person, a very sensible one, too. We had respect for some of the clinical people, and she was one of them.

Farreras: That reminds me of something. Mort mentioned that the basic people – Aging, Animal Behavior, and the Perception and Learning Section – were usually muttering to each other because they weren't getting enough attention or acknowledgement – compared to the clinical sections...

Carlson: I think they were, to some extent. But it didn't seem to be delivered on Shakow's part. I don't think Dave Rosenthal was much interested in the basic side of things, but he didn't do anything against it either. But he didn't go out of his way to be very supportive. Bob Livingston felt very strongly about psychological basic research. He was an M.D. but he seemed to think that the Psychology Lab was every bit as important as Neurophysiology or any of the other laboratories.

Farreras: So if it wasn't from Shakow, Rosenthal, or Livingston, where did that sense come from?

Carlson: I don't think from Kety either. Kety was quite supportive of us. Bob Cohen was mostly a clinical person. He was psychoanalyst, for one thing, and I don't think he really had his heart in supporting basic research, but he wasn't obstructive either.

John Eberhart would be the least supportive of basic research, but with him, it's hard to say because he had to be really concerned about the reputation of the place and how it was coming across with people downtown. So I think it was just easier for him to support physiological, medical, maybe even psychiatric research, because that was what a lot of people would think was the mission of the NIH: If you're going to do basic research, especially in behavior, let the universities do it. And the extramural program, of course, supported a lot of psychological research. I think what's a little surprising is that psychopharmacology didn't get supported better than it did. I don't quite know why, because that certainly had that going for it. But then it hadn't gotten to this problem with drugs yet, either, at that time. And I think there wasn't enough interest yet in the problems of aging either. It has gotten to be now, but not at that time. I don't think most psychologists were much interested in it. Well, there probably are some other significant people we haven't mentioned today.

Farreras: That's ok. Once I send you the transcript of the tape, you can jot down any that come to mind once you leave.

Carlson: Okay. There were directories of the Commissioned Corps. Every member got one, especially reserve or regular. But then that doesn't cover the people who were in the Civil Service, half of the people must have been in Civil Service. In a way I'm sorry to see such great changes going on. There used to be a lot of open space out here with trees, it was a very pleasant campus. But it's all getting built up. It's surprising to see NIH like this. The reason I liked it here so much was because I could talk to somebody on the phone that was going to pick me up

and say, “Just drive me into the circle and stop, and then I’ll come out.” I’d just look out the window and see if they were there. It was really nice; I wouldn’t have traded it for anybody’s office. Then we got moved down to the Center. I guess they then built this surgery facility or something. And then they built something else that just obliterated that space. We moved down to the middle of the building on the second floor, and I could look right out over the front and it was a beautiful area, trees, shrubs.... The entrance to the Clinical Center, to begin with, was quite nice. I remember one morning looking out the window, and here comes the bulldozer knocking down the trees. I just stood there and watched him, and it didn’t take very long. They had uprooted everything and hauled it away. And then they came in with the construction material to build a new building. It was really sad. And then we moved over to this building. I guess that’s the last place I had an office, the other corner up here, up on the top. Dave Rosenthal had a corner office that was pretty nice. And then when he wasn’t Lab Chief anymore he had to move to a smaller one and Herb Weingartner got the bigger one. I guess that always went with being the chief, except for Shakow, who started out that way but then got a smaller office. I never did quite understand why that was true. He didn’t seem to mind, or if he did he didn’t tell us.

Farreras: Well, if there isn’t anything else you’d like to add perhaps this is a good place to stop?

Carlson: I don’t think there’s anything else.

Farreras: Well, thanks so much for coming down. This is very helpful. I was planning on going up there, but I’m glad it worked out that you were coming down here for a

visit. Thanks again.