8-22-1986

John Paul Scott Oral History

John Paul Scott

The Jackson Laboratory
Oral History Collection

Interviewer's Comments

Narrator's Name Dr. J.P. Scott

Interviewer's observations about the interview setting, physical description of the narrator, comments on narrator's veracity and accuracy, and candid assessment of the historical value of the memoir.

NOTE: Use parentheses () to enclose any words, phrases or sentences that should be regarded as confidential.

Dr. J.P. Scott was the head of animal behavioral research at Hamilton Station. He came to our meeting with a thoughtful outline, and spoke to it for most of our interview. His tape, as a result, is mostly a monologue.

This tape is extremely valuable because Jax threw away all the records of Hamilton Station, so only the accounts of Scott, Fuller and Fox (in this project) can provide a record of this aspect of the Lab's past. Scott indicates how pervasive was the network in which his work was conducted, and how many noteworthy figures or students (later to become outstanding scientists) passed through his lab between 1945 and 1965.

Whatever may have been the feelings or relationship between Scott and Earl Green, Scott is mum. His reasons for leaving Jax, he suggests, were purely personal opportunity and an eagerness to return to teaching. Others at the Lab suggest a clash between Green and Scott over the role of Hamilton Station, his rank, etc., causing Scott to leave with some measure of ill will. Certainly, once Scott left, the animal behavior work quickly wound down, which Scott notes on this tape.

Scott's loyalty to Hamilton Station is obvious on this tape. Handle this valuable tape with some caveats as to its objectivity. Its merits lie in the fact that it is articulate, thoughtful, and vital to a wider appreciation of the diverse roles Jax was playing. Supplement Scott's account here with the tapes of Fox and Fuller.

22 August 1986
Date

Susan Mehrtens
Interviewer's name

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Place Salisbury Cove, Maine

Date 19 Aug 1986

J. P. Scott
Narrator

Susan E. McIntire
for the Laboratory

The Jackson Laboratory
INTERVIEW DATA SHEET

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Date(s) & Place(s) of Interview(s) Salisbury Cove, Maine 19 Aug 1986

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Collateral Materials Report

Narrator's Name Scott

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Interviewer's Notes and Word List
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Bill & Tibby Russell
Chicago
Sewall Wright
Jackson
Rockefeller
Alan Gregg
C.C. Little
Hamilton Station
William Pierson Hamilton
J.P. Morgan
Edna DuBuis
Emilia Vickery
Mary Vesta
Wabash College
Bill Murray
Joseph Royce
Chicago
Alberta
Edmonton
Dan Freedman
Brandeis
Loring Brace
Harvard
Michigan
John Fuller
Stockard
Robert Thompson
Hebb
Montreal
York
Sheldon
Seville
E.O. Wilson
Jack King
Michigan State
Earl Green
Tinbergen
Lorenz
Dale Foley
Ford
NIH
NIMH
NSF
Stanford
Bowling Green State
George Snell
Binghamton
Rob Collins
Colorado
Texas
Minnesota
Nobel Prize
Woods Hole
Marcus Waller
Howard Moltz
North Carolina
Jerome Kagan
Frank Bronson
H.L. Mencken
Phil Gray
Cape Rosier
Benson Ginsberg
Washington
Montana
Yale
Calais
J.B.S. Haldane
Lib Keucher Gorer
Allen Salisbury
Dolores Dellabar Crary
Mary Spangler
Bangor
Eleanor Talbot
Higgins
somatotype theory
ectomorph
endomorph
mesomorph

This is the tape of an oral history interview of Dr. John Paul Scott, given as part of the Jackson Laboratory Oral History project, sponsored by the Acadia Institute. This interview was held on August 19th, 1986, in Dr. Scott's home on Bayview Drive, in Bar Harbor, Maine. The interviewer was Dr. Susan E. Mehrtens. MS is Mary-'Vesta Scott, Scott's wife.

SM: So how about we begin by my asking you how you first heard of the Lab, or came to be at Jax.

JS: Well, I think I first heard about it from my friends Bill and Tibby Russell, who were staff members at that time, in the 1930's. They had also been graduate students at the University of Chicago, with me, under Sewall Wright, and they asked me to come down and be a summer investigator, in one summer--I can't remember which summer that was, but it was in the latter part of the 1930's. And then, later on, in 1945, the Jackson Laboratory got a big grant from the Rockefeller Foundation, to set up a program on genetics and behavior. Alan Gregg, who was the person in charge of their medical research, felt that psychiatrists and psychologists were not paying enough attention to the factor of heredity in behavior, and he got together with his old friend, C.C. Little, who was the Director of the Jackson Laboratory, and they negotiated a large research grant for studying genetics and behavior, particularly with dogs. Since I was the only person in the country at that time that had a degree in genetics and was interested in the genetics of behavior, I was the logical choice for heading up this new program. So that's how I came here in 1945, which was immediately after World War II.

SM: Just before the fire.

JS: Two years before.

SM: Yes.

JS: Yes, it was two years before the fire, that's right. That did have a dramatic effect on everything.

SM: What was your initial impression of the Lab, when you arrived in '45?

JS: They, of course, had been working in a very restricted way during the War, in a very small place, there were actually seven research associates. That was the entire scientific staff of the Laboratory plus a few research assistants and several animal caretakers. Until I came, all research was done with mice, at the Main Laboratory in Bar Harbor. There were two buildings conjoined, one of which was the original, which was a brick shell with a wood interior, and the other was a more modern fireproof building. It was, as I say, a very small operation, but I thought it had a lot of possibilities, particularly since Little, at that time, was trying to run the place on a democratic model, and the staff members were playing a part in running the Laboratory. The Laboratory was actually run by an executive committee of the staff. We also had staff members on the Board of Trustees. It was a small but democratic organization, rather efficient, and had been very productive. Hamilton Station, on the other hand, I had seen first in 1938-39, when it was given to the Laboratory by the estate of William Pierson

Hamilton, who was an eccentric millionaire and a J.P. Morgan partner. When he retired, he came up to Bar Harbor and decided to set up a hundred thousand acre ranch in Maine. He couldn't get the land all in one spot, so he kept buying up little farms all over the place, and building various buildings on them which he painted with his colors: red, blue and yellow. He planned to do diversified farming and have a pheasant farm in one place, and a poultry farm in another, a dairy farm in another and so on. Hamilton Station was actually . meant to be a combined poultry and horse farm. The idea was alright, but when the Hamilton family began dividing up the holdings, they discovered they couldn't sell the place, because, while they had some beautiful horse barns there, there was no pasture attached to the place. Anyway, they ended up by giving it to the Jackson Laboratory, and my first contact, as I said, was in 1938-39, when the Lab had just received the place, and the members of the Lab were holding a barn dance in the hay loft of this beautiful barn. Of course, Hamilton had fixed it up, as a place where you could have dances, with a place for a bandstand, and also a feed bin that could be converted into a bar, and so forth (laughter)--his idea of gentlemanly farming. Anyway, that was the first time I saw it, and when I came on to work in '45, I guess it was the obvious place to put the dog research project. So the whole first year was spent we getting the place remodeled into what turned out to be, as a very good animal behavior reearch laboratory. It was not only complete with inside facilities such as nursery rooms and testing laboratories but also with outside runs. We also had three big one-acre fields in which we could study the dogs in more or less natural field conditions. We therefore had almost unlimited space and opportunities.

SM: Did you feel isolated there at all, in the sense of geography; it was the middle of nowhere.

JS: Well, yes. Of course, almost all scientists feel isolated if they don't have someone right next door who's in their own field. What I did was to try, at least once a month, to go somewhere else, to make contacts with people, either by giving talks at colloquia, or going to scientific meetings or whatever. I did get out of the place at least once a month, and that pretty well took care of the isolation. Actually, during the first years of the project, I was trying to make contact with all the major scientists in the United States who had either worked in the general field of genetics and behavior, or had some ideas on the subject.

I also tried to visit those laboratories that had been particularly successful in the past, in order to get some ideas on how I could organize this one, and that was the way I laid the foundations for what we actually accomplished. One of the first things that I noticed was that we had a very small scientific staff. At first, it was me, and two other people: one girl, Edna DuBuis, a veterinarian's assistant whom Little had brought in to help out with the dogs, and also Emilia Vicari, who had done some research on genetics and behavior in dogs but, in another area, and in another laboratory. That was it. That was the staff until 1946, when we got enough of a laboratory, and enough of a program going so that we could hire Mary-'Vesta, who at that time had just graduated from the University of Maine in psychology and became my first research assistant. What I did to compensate for the small staff, and also the isolation problem, was to set up a summer investigators' program. I had been here once before, almost all by myself, as a summer investigator, and I expanded this program. The first year, we had five summer investigators, people from universities, who were glad to come here and work for very little money, simply because of the research opportunities, and of course, the beautiful summer time climate. I kept that going for years and years. I had noticed that--well, I was very much interested in the educational aspect of the Laboratory, having worked as a student at the Marine Biological Laboratory at Woods Hole, and having been a professor at Wabash College for ten years before I came here. But, the Lab already had a summer students program, with the two Russells, Bill and Tibby, running it very competently. There was no room for anybody else, I decided that my role would be to set up the summer investigators' program, which I did, not only for the Hamilton Station, but also for the Main Laboratory. In other words, I tried to make it easy and comfortable for people to come here and work.

SM: Having taught before, did you miss students? Having been in a college environment at one time, did you come here and miss students, not having students?

JS: Yes, I enjoyed teaching, but there really wasn't much that could be done with a formal kind of education at the Lab. It was all done by a sort of apprenticeship work. A lot of the staff members who had come here were refugees from colleges and universities because they couldn't take it--eiher they didn't like teaching, or they didn't like the college or university atmosphere, and wanted to get away from it. But that wasn't my case. Actually, one of the reasons I did not stay here after 1965 was that I felt that the promise of an educational institution had not been fulfilled. You see, one of the ideas that Little had was to institute in some kind of formal academic work, particularly advanced work in genetics, more or less on the model of what the Rockefeller University eventually became, a graduate and post-graduate training program. But that never materialized, and one of the reasons I left was that I wanted to get back into education.

SM: Had he talked to you about this early on, about his dream to make some sort of graduate--

JS: Oh yes, he was a great promoter, and had all kinds of ideas, including that one.

SM: Why do you think that never happened?

JS: Well, I think there are lots of reasons. One was, Little's age. He was 55, when I came and I figured that he would have about ten more years to work. Actually, he had 12, but even so, what he could accomplish was limited. It was also limited by the amount of money he could raise.

Finally, he always had much bigger ideas than he could actually pull off. He was a good promoter, but a poor administrator. That was one of the disappointing things that happened, or rather, didn't happen. Another was that the rather free and democratic regime when I first came was replaced by a one-man show which was Little, of course, since Little wasn't very competent as an administrator, he brought in someone to help him. This was Bill Murray, who had been associated with the Lab before. One of Little's difficulties as an administrator was that he could never say no, and Murray compensated for this by being a person who could never say yes (laughter). Well, the end result was a complete stalemate, and so in the latter part of Little's regime nothing really happened. Things stayed in a very stable form. That was it.

SM: It's interesting, just as an aside--many people have remarked about how this place would be much improved if there were more students around, either as post-docs or as pre-docs, or working toward a doctorate under somebody jointly with a university and the Lab, or more of an expanded summer program, or something. It's a theme that many people have said. It's fascinating that Little had this idea.

JS: It was not only his idea. I and my colleagues entertained dozens of visiting research workers over the years. The educational aspect could have been formalized fairly easily. All that Little would have needed to do was to get a charter, a permission from the state legislature.

SM: Well, I wonder, if it is as you say, that most of the staff were refugees from academia, didn't want to have it--

JS: That's right.

SM: Didn't want the additional paper pushing that would involve.

JS: Well, it wasn't only that. I suspect that many of them weren't very successful teachers in the first place--, what I did to compensate for the lack of teaching, was to encourage students to come here. One of the things that we did was to have graduate students from other institutions come here and do their dissertations. Many of them came from first-rate institutions, and went on to become prominent people. One of the first ones that we had was Joe Royce from the University of Chicago, who wanted to come and do factor analysis of our genetic work.

SM: Is this Josiah Royce?

JS: Joseph Royce, and he's now, I think, head of a department, or at least a professor at the University of Alberta, in Edmonton. Another person of that kind that I can mention is Dan Freedman, who was taking his degree at Brandeis University. He did a very excellent piece of research on the effects of genetics and early experience in dogs, and then got interested in behavioral genetics, went on to do a great deal of work in human genetics, and is now a professor at the University of Chicago. Another person that I of that kind is Loring Brace, who was then doing physical anthropology at Harvard. He came up and did a factor analysis of our dog data--helped us out a great deal, as a matter of fact then went on in anthropology, and is now a professor at the University of Michigan. Those are just a few of the many now prominent scientists who were here. One that I <u>should</u> mention, of course, is John Fuller--or did I say anything about him?

SM: No.

JS: When I first set this place up, I knew that it was going to be a long-time research program. If anything happened to me, then the whole thing would fold up. This had been the history of similar programs in the past, particularly one the Rockefeller Foundation had funded for a man named Stockard. In the middle of his program, Stockard died, the whole thing folded up, and nothing really came out of it at all. It was just a tremendous waste of money, as it turned out. Anyway, I got John Fuller to be a co-investigator and be co-responsible for the behavior genetics program. As it turned out, he got fascinated by behavior genetics, although originally he was, trained as a physiological ecologist. Well, he got fascinated by behavior genetics, and then he and Robert Thompson, who was a student of Hebb at Montreal, got together--Thompson came down as a post-doctoral fellow, and wrote a book, really the first major book on behavior genetics, one that set the tone for the whole science, as it has developed since.

SM: About when did Fuller come?

JS: Well, he was here in 1946, at the same time that Mary-'Vesta.

came as a research assistant. He came as a summer investigator and, at that time, we hadn't decided who would be the second person besides myself. There were several other possibilities, but, as it turned out, Fuller was it. And so he came back as a staff member in 1947, which was just before the 1947 fire. He had come in the summer, and he had been at the University of Maine, where Mary-'Vesta took some courses with him. Incidentally, I think you ought to get an interview with him.

SM: I will. I think I may have to go to York. He's thinking he may come here in the Fall. He usually comes in September, but I would go to York--

JS: Yes, well, he could tell you an awful lot of things, and certainly supplement what I have said.

SM: So he came just before the fire.

JS: That's right, yes, and it was, of course, a very exciting time here. The fire actually started not far from Hamilton Station, over in the middle of the island, but it burned in the other direction. It never touched this part of the island, but it did eventually burn over the other Laboratory, The oldest part of the Laboratory, the one with the wooden interior, was burned out--there was nothing left but a shell. In other part, the fireproof part, the roof burned off, but the rest of it stood, but all the mice were killed. And then, for a year after that, until they got the buildings rebuilt, the Hamilton Station was the refuge of the people who couldn't work down there, so the place was full of mice, and mouse researchers.

SM: Were you still able to conduct your work?

JS: Oh yes. There was actually very little interruption, because--nothing had been destroyed of what we were doing, and so we kept things going pretty well as they had been. Actually there was a fundamental decision made at that time, because one of the things that people talked about was should they move the whole Laboratory up to Hamilton Station. I think that I made a big mistake, because I said maybe they should leave it down there, for various reasons, partly because they had one reasonably good building left, and also because there was a certain amount of sentiment involved. But really, Hamilton Station would have been a much better location, because they had forty acres on this side of the road, and they had another seventy-five on the other side of the road, with all kinds of good building land. It would not have interfered with anything else (there were no zoning laws, or anything like that at that time) instead of being in a very restricted sort of area where the Main Laboratory is now located. The other thing, is that the main Lab is far too close to the National Park. It's a kind of eyesore as far as the National Park goes, although it's better now than it has been in the past. It is, as I say, a rather unsightly place. Well, the whole history of the place might have been different if the decision had been made in the other direction. If I had talked for it the decision might have gone that way, as

Little, who really made all our decisions would have gone either way, I think. I think that the most important thing that we did at that time and later, besides providing a good working atmosphere for people, a good research atmosphere and good for the people themselves, who actually did the research--the most important things we did was to initiate important lines of research, which were really expansions of the kind of interests that I had. One of these was, of course, behavior genetics, in which I had an early interest. Eventually we ran a program for some thirteen years of actually collecting data from experiments on dogs. We had a very large operation. We had an average colony size of about 225 dogs, including adults and puppies. We did a very extensive breeding program on them, and when we got through, we had a tremendous amount of data which had to be analyzed. That data is summarized in a book that Fuller and I wrote, called The Genetics and Social Behavior of the Dog, which is still the standard work on dog behavior. Since '65, nothing better has ever been done on dog research. And we made several fundamental discoveries. One was in the inheritance of intelligence. Most people are interested in the questions: "Is the dog intelligent? Is this inherited?" Well, what we found was that, for the most part, there are relatively few differences in cognitive ability, that is, pure intelligence, among dogs. What you do have among the different breeds, and also among individuals, is big differences in emotionality and motivation.

And these, in any task that you give them, or any test, will make the animals come out very differently. A dog that is timid of apparatus or other strange things, will do poorly on the apparatus, irrespective of how much intelligence it has, and an animal not motivated by that kind of reward that you want to give it--say, food--will also do poorly, whereas the ones that are not afraid and are well motivated by food, will do very well indeed. Another important discovery was that physique has almost nothing to do with behavior. Now, there was a theory invented by a man named Sheldon, who called it the "somatotype" theory. He graded human physique according to three types, essentially thin people, fat people and muscular people

SM: Oh yes: endomorph, ectomorph and mesomorph.

JS: Yes, His theory was that a person's somatotype had a lot to do with personality and intelligence. Well, when we measured, among other things, the physique of these dogs and its-the correlation between it and behavior, there was no connection. In other words, the breeding experiments, showed that physique--aside from the obvious things, such as a dachshund not running as fast as a greyhound, on account that it has short legs--aside from obvious things like that, physique has very little to do with behavior. Our research thus contributed to the death of the somatotype theory, which was well deserved. Another thing that we did was to study the effects of genetics on behavior developmentally, starting with birth and continuing up to a year of age. This is one of the ways of finding out how genetics affects behavior, and in the course of describing the development of the dog, and in doing experiments with behavior at different times, we discovered that there was a critical period in development, particularly for the formation of social attachments. This runs from about four weeks until twelve weeks, which is pretty close to the end of it, there's a period in which the puppy will form attachments to other individuals very rapidly--they can be either dogs or other animals, or places, or people--and if you don't allow them to form attachments at this time, then you can never get a very satisfactory dog.

SM: You get a wild dog.

JS: Yes, exactly. We tried that, raising animals without any contact with people, and they became, by the time of fourteen or fifteen weeks of age, just like little wild animals. We could tame them, but we never got this close association that you think of as being a normal part of dog behavior. And there is not only a critical period for attachment and primary socialization, which of course lays the foundation for the other kinds of things that you can do with dogs--there's not only a critical period for that, but there's also a critical period for learning new things, from about eight to twelve weeks. Up until twelve weeks, puppies will pick up almost anything you try to teach them. They can't do it very well--they're so young and immature--but they're interested and easy to work with. Later on, when they become adults, that ability is gone.

You can still teach them things, but with a great deal of difficulty. Anyway, this critical period idea not only applies to dogs but has a very general and broad application to all kinds of organizational problems, including the human ones. I also followed up one of my earlier interests in research. One of the reasons I got into animal behavior research was that I was interested in problems of aggression, the fact that fighting can be such a destructive influence, particularly among human beings and especially the problem of warfare. So, before I came here I had been working with fighting mice•was the first one to demonstrate that there were strain differences in fighting behavior among mouse males. Subsequently, other researchers showed the female mice will also fight, what I started there has developed into a very large field of research in its own. There are now dozens if not hundreds of people working with problems of aggressive behavior in mice. We also did similar work with the dogs. Of course, we couldn't and wouldn't want to have them actually fighting, but as we observed the little puppies growing up, they did fight among themselves, without provocating by us. We would watch that, and got various ideas about it, and one of the things that we found was that, while there are genetic differences in the tendency to become a fighter or not to become one, and whether to become a successful fighter or not, these are strong situational influences on the occurrence of fighting. For example, you can take the same mouse and either make a fierce fighter or make a peaceful animal out of it, by appropriate training methods, and the same is true of dogs. Some of this work is now coming to fruition. In last May, a group of scientists got together in Seville, I among them, and we produced a "statement on violence", which essentially says just what I've said here about violence, namely that genetics, of course, has an. effect on it, but it does not determine that an animal or a human being must fight. Therefore, the argument that war is an innate human quality, that people must go to war, is false. On the contrary, depending on the culture, people can live in peace and harmony for centuries, or they can live for centuries in a state of warfare. Anyway, we hope that that statement will do something toward promoting world peace. My whole idea, all the way along, has been that what we're trying to do is use the research on animals to understand human behavior and so to modify it and improve it where possible. Another very important thing that came out of this research was research on abnormal behavior. This is a very fundamental human problem. I would say that, in some ways, maladaptative behavior is a more important human problem than is the problem of cancer, because it affects more people. Some statistics show that about one person in five will have some kind of mental problems during his/her life and about one person in ten will get to the point where they have to be institutionalized. These figures were gathered several years ago, indicating a tremendous human problem, and one in which there's still an awful lot of progress to be made. We didn't deliberately try to make the dogs abnormal, but one of the things that we did was to try rearing animals in social isolation. Now, they were perfectly comfortable, well fed, and in good physical surroundings. If they started off as young puppies without any companions, before the critical period for attachment they didn't seem to mind it. In other words, you could watch them through a one-way glass, and they seemed to be perfectly well adjusted, and getting on fine. They ate well and didn't have any trouble with anything else. But, if you kept them in there beyond the critical period and then brought them out, they would have tremendous emotional reactions, would be very upset, and it was very difficult to get them to behave like normal animals. Now, that's the most extreme form of what we called the "kennel dog syndrome," or "separation syndrome." There's another form of it, that occurs commonly in ordinary pet dogs. If you take a puppy and leave it in a kennel until it's six months of age or older, and then take it out and try to make a pet out of it, it will have a terrible time adjusting to the outside world and never become a really satisfactory pet. It may be afraid of everything strange, or and in other cases, it may become antagonistic and bite people, becoming what dog owners call a "fear biter." In short, while you can do something to try to mollify this condition and restore their normal behavior, such a dog essentially remains a pretty abnormal kind of pet, and very unsuccessful one. The application to humans of this research is not that humans exhibit this exact behavioral syndrome, but they do show similar emotional reactions to separation from beloved individuals through death or other reasons, or to separation from places--it's a very involved emotional reaction, and it leads to all kinds of problems, one of which is depression. People who have been moved out of their accustomed environment often become depressed. That's one of the more important things that came out of our research at Hamilton Station. I also did some of the earlier work on sociobiology. I use the term "sociobiology" in a different way from Wilson, in that I think of it not as the foundation of all human social sciences, but rather an area in which the social and biological sciences can cooperate. In other words, the social sciences have a great deal to contribute as well as the biological sciences and the two interact with each other. What we were doing, of course, was to try to set up some research models on animals that might carry over to human beings. E.O. Wilson has gone off in quite a different direction. His interest has been primarily the evolution of social behavior, in trying to explain social behavior solely on the basis of evolutionary genetics, which you can't do. That's why so many controversies have arisen out of his thinking. It leads to an over-emphasis on genetics. As a trained geneticist, it sometimes seems to me I've spent most of my life saying "No, no, genetics can't do that." (laughter) I just gave you a very quick summary of some of the research that I personally was involved in, always with my colleagues. Why don't I tell you a few success stories? One of the early people who came to work with us was Jack King, who is now a professor at Michigan State University, just retiring. He was with us for about eight years. Then he was encouraged by Earl Green to leave, which was a foolish thing to do, since he was one of the best staff members we had. Recently there's been a book "Leaders in Animal Behavior". It includes autobiographical chapters by the Nobel Prize winners Tinbergen and Lorenz and also of all living scientists of comparable stature. Those included were chosen by a committee of their peers. Among them there are three people who were in our Hamilton Station group: myself, and Fuller, and Jack King. As well as the satisfaction of having done good work it's nice to have other recognize it.

SM: Why did Earl Green get rid of him?

JS: He just didn't like him very well. Or perhaps Green was unable to recognize good work outside his own special field of mouse genetics.

SM: This leads to another question: How close an involvement did you have with the main Lab, in Hamilton Station? Was Earl Green ever a real fixture at that time around--

JS: Well, I think that this again points up the weaknesses of an autocratic system, having things organized so that everything is dependent upon one person. I've already pointed out how it worked out rather badly with respect to Little, especially as he began to retire. Everybody could see his obvious weaknesses in running things. So when Green came in, his idea was to consolidate everything. He wanted everything moved down to the main Lab, including the dogs. There wasn't any good place to put them, so I had to resist that, and it led to--well, not any real friction, but continual pressure on me to do something about the matter. I couldn't see it, because, if we had done it, we would have spent at least a couple of years moving, rebuilding, the labs and kennels, and that sort of thing, and we would have lost that much research time. Also the area around the Main Lab was not anywhere near as good a place as that we already had. Evaluating Green as a Director, one had to respect him in many ways. He had a lot of integrity--I mean, he was a good geneticist, scientifically a good person, and also he respected science in other people. In other words, he never interfered with anybody's research, and never tried to. One of his chief accomplishment was to make the services of the Laboratory run better. By the time he got through, we had very good services with respect to photography, statistics, animal caretakers, and mouse production. It wasn't all him, because a good deal of that was accomplished by Dale Foley, who was another person you ought to get to--

SM: I have.

JS: You already have him on the list.

SM: I have interviewed him.

JS: Because he ran the business end of the thing.

SM: Right.

JS: I think that he and Green worked very well together on that, but the interesting thing was that, in the next ten years following 1957, when the Sputnik went up and the United States began to spend a lot of money on basic research, there was sort of golden age of research in this country. There was a great expansion of research during that time, in both quantity and quality. It was also the time when

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Green at the Lab was trying to get the institution on a sounder financial position. To a large extent, he succeeded in doing, just that, but there was no expansion: everything was held on a dead level--you could keep going with what you were doing, but you couldn't do any empire-building, or even expand your own research very much. I think it was not a terribly good atmosphere for creative research scientists.

SM: Now, through all this time, you continued to be funded?

JS: Oh yes.

SM: Was it still Rockefeller money?

JS: Well, the Rockefeller had originally promised us ten years of support. They gave us fifty thousand a year, plus another fifty thousand for setting up the laboratory. Of course, at the time that they started, in 1945, the dollar was worth at least five times what it is now.

It was really pretty generous funding. They actually kept up support for thirteen years. In the meantime, we had gotten some funding from the National Institutes of Health. For instance, one day fairly early on, a couple of fellows came up from the National Institute of Mental Health, and said, "Look, Congress has just given us a lot of money to fund research, don't you want to apply?

SM: Oh my goodness! How times have changed!

JS: That's right. So, they gave us about a four page blank, in which there was a space about this big, for telling what research you wanted to do. We sent that back, and so I got a grant, MH 123 supporting my research on mouse aggression, and that kept going for many years. It supported first Emil Fredericson and then Jack King, and then, when King left, he took it with him. At that time NIH acted on the theory that if you seemed to have a good idea, and were somebody that had a good reputation, you could have the money to carry it out. Very reasonable, and labor saving. Of course, it is all different at the present time. What first happened was that they took academics in to be on advisory panels. These professors were accustomed to evaluating graduate proposals and supervising theses, and so, what eventually happened was that a person now applying for a research grant really has to write out a thing like a Ph.D. thesis proposal, that reviews the literature, tells everything he's going to do except--

SM: Sort of like you can write it after you've done it.

JS: That's right. As a matter of fact, the easiest way to do that sort of thing is to apply for something you've already done (laughter), or at least have gotten well started, and then, while you're finishing up that with that grant money, you start another one, which you can apply for in the same fashion. One can adjust, at the cost of considerable unnecessary effort which also costs money. But the real problem is that there's been no increase in funding, in terms of real dollars, since about 1967, and so everything has been more or less on hold, as far as the federal government goes. And since the Reagan administration, it's been held down even more. After the Rockefeller Foundation, we had a grant for a couple of years from the Ford Foundation, on abnormal behavior, and then after that, we shifted over to the National Institutes of Health, and the National Science Foundation. By the time I left, I had three grants going, and oh, I don't know, a total of $125,000. That was in 1965, and it supported my own research and that of at least one other investigator. Fuller had similarly expanded his research support independently. To give you an idea of the importance of the behavioral research at the Jackson: At one time, there were about 25 people who had doctoral preparation at work in the whole Laboratory--fellows, and staff members and so on. Of these, eight were working at Hamilton Station on behavior, so we made up about a third of the whole Laboratory. But we never expanded beyond that.

SM: Why was it that you left?

JS: Why was it that I left? Well, I wanted to get back into the educational field. In fact, when I originally came here I thought I would work in the dog project for about ten years. I actually stayed here twenty. Another reason for leaving was that some of Green's policies made it very difficult to do anything with graduate students here. He said that students should not be paid for doing their own theses, which meant, that they didn't have enough time and money to do much more than do the staff members' research. So they just couldn't come here anymore. That kind of cooperative graduate training and research was very much curtailed. So I wanted to get out and also I thought it was time to do something different. So I went out to the Center for Advanced Study in the Behavioral Sciences at Stanford for a year, as a fellow, while I was there, I started looking around for other places, and I ended up by picking Bowling Green State University, for two reasons, one of which was that it was undergoing a critical period. I'm a great believer in critical periods. Actually, the Jackson Laboratory was in a critical period when I first came here in 1945: It was getting reorganized and expanding, and just about everything was possible. With a very little effort, you could accomplish a great deal. Similarly, at Bowling Green State University--it was shifting over from an undergraduate teaching institution to a real University with advanced degrees. There was a chance to organize graduate work from the beginning particularly in the Department of Psychology with its new Ph.D program, where I was going. That was interesting in itself, because my original training was in zoology and genetics, and but over

over the years, I had been studying so much animal behavior that I eventually knew a great deal about psychology, so they were happy to take me on as a full Professor in the Department of Psychology.

SM: What did Sewall Wright say to all this? I mean, he's still alive, Sewall Wright, and he was your teacher. Did he feel as if you've "betrayed" the field?

JS: No, I don't think so. At least he never said so, and actually, I was working in genetics, which was his field, and carrying out a good many of his ideas, but in a very different way, and, of course, developing my own ideas. I wasn't going to be bound down by what Sewall Wright laid down. Of course, he's a very great man, one of the few real geniuses I've ever known, but, yes, I don't think he ever understood what I was doing--(laughter)...and actually there are a lot of his ideas which have tremendous import for behavior, especially the evolution of behavior, but he didn't realize how the two things interacted because he knew nothing about behavior. Another reason for my leaving Hamilton Station was that while I could keep the place going, I didn't feel that I was getting the kind of institutional support that I could have used. Also, after a while, you run out of new ideas. If I had stayed, an awful lot of my time would have been spent in getting more grants, and not really getting too much research done, although I did have some very good ideas for them which would have been nice to carry out, and some of which I actually did carry out at Bowling Green. So I did not leave too much behind. Earlier, Fuller had seen the handwriting on the wall respecting Hamilton Station and he had moved down to the main Laboratory, and worked largely on mice, and also became the Associate Director for Scientific Affairs, so that he had quite an influential position at the Lab, but was no longer doing very much with the dog, although he did quite a few things which were really major pieces of research, particularly his work on the effects of social isolation on the puppies. He had not entirely switched his research to mice, but it nevertheless became more and more of a push to try to keep things going here, and as I said, I felt that I would get a lot of stimulation by going to a new place that would provoke new ideas. A place like the Jackson Laboratory, as it is now and was then constituted as not very much of an educational institution, is a great place to go if you have some good ideas that you want to carry out, because you could probably carry them on more effectively, quickly and more efficiently in a place like this, than you could at an academic institution, but once you've done that, I think it tends to be a rather poor place intellectually because you don't get the stimulation of students, and a wide variety of colleagues from different disciplines.

SM: Well, it would attract a certain personality for whom that is attractive.

JS: Right.

SM: I interviewed George Snell, and he said that he came here hoping that, within the first few years, he would find his life's work, and he would just be able to do it, and do it his whole life, and he did.

JS: Right.

SM: So, for him, it was really perfect.

JS: That's right, exactly. I think a lot depends on the individual person. Then, another thing I have observed about the Lab is that from the beginning, they have provided almost ideal opportunities for research. It's limited in some ways, but at the time I first came here, every person on the staff had an office, a laboratory, a research assistant, and the animals and equipment to work with. If you provide that kind of atmosphere--you don't have to compete for it, as you do in many academic institutions--if you have that kind of atmosphere, even a person of mediocre or average ability can do great things. Then, if you get somebody like George Snell, who has much more than average ability, you can do really phenomenal things. While that sort of person may work out pretty well, the thing that doesn't work out well is for the individual who doesn't generate new ideas. That sort of person can get stuck in a routine, and can't get out.

SM: Then I gather you've kept in touch with the Lab since you left--

JS: Yes, more or less.

SM: Or with people that you knew there, and what's your reaction as to how it's evolved?

JS: At the present time? In the last few years, I haven't kept in very close contact. It's got beautiful buildings, and I think some very good research is being done, but I can't evaluate it because I haven't had the contacts with the staff members. One thing that someone did tell me, a year or so ago, was that they now had more support staff than staff members. In other words, the administration tends to become bigger-

SM: Yes, it has proliferated.

JS: And that the scientific staff hasn't grown at all. In other words, everything has become institutionalized. And, of course, the behavioral research has almost gone. When I left the Hamilton Station, it was in good running condition, with strong grant support for dog research. I had tried to get people in here who would carry it on, but actually, after I left, they were not able to keep it going more than a couple of years, so the dog research got all phased out, a couple of years after I left. Then Fuller stayed on, for several years after I left--I think about five--but he was working at the main Lab with behavioral genetics research on mice. He was doing some very good work, and had several associates. Then, he decided to do the same thing that I had done, namely to leave and go into an academic institution. It was very similar, as a matter of fact: He went over to the State University of New York at Binghamton, and helped them set up a graduate program, and did a very nice job of getting some graduate students trained for several years there, but after he left, his associates were not as strong as he was, and they just have not been able to keep things going. There's actually only one person left. That is Rob Collins, who's still working on behavioral research. Part of the reason for the decline of Fuller's line of mouse research, I think, what you can do with mice on behavior is limited: there are certain kinds of things that mice don't do. There are certain things for which they do make good research subjects, and Collins is actually doing some very good research, but that whole field has weakened at the Jackson Laboratory (which I'm sorry to see) whereas, in other places, it has flourished. There are very strong behavior genetics institutes, at the University of Colorado, for example. The kind of thing that we started has been taken over by such places as the University of Texas and the University of Minnesota. The kind of reserch that we started has been taken over by other institutions, so I'm not feeling bad about it. I don't know how much more you want me to say on this.

SM: Well, it's hard to do, but if you could stand back from your time, and assess the Jax as a scientific research institution, what do you think it will be remembered for?

JS: Remembered for.

SM: What mark has it left in the history of science?

JS: In other words, you're asking what is the historical significance of it? Well, I think that several important research findings made here, some of them fundamental. The first--in the cancer field, for example--was the demonstration of a breast cancer being caused by a virus, which Little didn't want to believe (laughter). Actually, the evidence was overwhelming. That was very definitely a breakthrough in a better understanding of what produces cancer, among other things. Of course, there's George Snell's work which eventually got recognized by his Nobel Prize. In his case it was fundamental because he discovered the the genetic basis of immunity reactions in the mouse. As it turned out, they are quite similar in other mammals, including humans, so that was a very important thing. I remember that at the time when George did this, I was trying to promote publicity for the Lab, among other things, and I thought, gee, this is really a very important kind of discovery, but it was published, and nobody paid much attention, at the time. But a good many years later, other people working in this area began to produce things which were obviously of great medical importance, and they couldn't give a Nobel Prize to these people without giving one to George. That's the way it worked out. But anyway, he's an awfully nice person, I've known him ever since around 1929 or '30, when I met him down at Woods Hole, when I was an undergraduate student. I've had contacts with him off and on ever since. Then there are these fundamental discoveries in behavioral research that I mentioned that should be associated with the Jackson Lab. They are discoveries of equal importance to the science of behavior and behavior genetics I also feel that one of the most important things that I and my associates were able to accomplish was, by making this an attractive place to work, an interesting place, and a stimulating place, we attracted during those twenty years of my residence, almost all of the people who were then working in the fields of comparative psychology and animal behavior in this country. They came down here and spent some time, not only as casual visitors, but mostly as summer investigators, or post-doctoral fellows, or graduate students or research assistants, so that we had a tremendous impact on this whole field. Most of them went elsewhere and got into other jobs. Some I think of offhand are Marcus Waller, at the University of North Carolina, Howard Moltz, at University of Chicago--Mary-'Vesta, what's the name of the man at Harvard, who's working in child development now--Jerry something?

SM: Jerome Kagan?

JS: Kagan, yes. He was up here for summers.

SM: WOW!

JS: Oh yes--anybody that you can mention that is prominent in this field, of a certain age, anyway, has been to Hamilton Station.

SM: That's wonderful.

JS: Now how they got affected by it, of course, I can't say. There was some effect, I think.

SM: It sounds like you were real pioneers.

JS: Yes, as I say, we made it an attractive and interesting place, and put emphasis on what I at least consider to be the important problems rather than the minor ones, which many scientists get into when they get into detail, and forget about the main problems. In the biological area, there was Jack King, at Michigan State University, and Frank Bronson, who went down to the University of Texas, and so on. Someday, I ought to try to get together a list of the people who were here-there are really dozens of them. Part of the problem is that, when I left this place, I left most of my files and papers behind, in other words, the correspondence. I thinking, that after all, the Jackson Laboratory had paid for them as they ought to belong to the Lab, but about a couple of years later, someone threw them all out.

SM: Yes, they have no interest in history. The Lab is--

JS: Yes, that's right. There was just very little documented history. There are a few things, records, down there, committee reports, Annual Reports, that sort of thing, the library, but that's about all there is. I had a few things that I took with me, but without that, it's awfully difficult to check back on records and get a complete list-I ought to tell you a few other success stories. For example, one of our animal caretakers, we noticed, wasn't doing very wonderful work. He would rush through his work with feeding and cleaning up for the animals, then sit down and read a book. Well, we noticed the book he was reading was H.L. Mencken (laughter). We discovered--his name was Phil Gray--that he had been brought up, I think, around Cape Rosier, a fisherman's village. He had never gone through anything but the eighth grade, went off to the Army, he never got the G.I. Bill. He was a tremendous reader and scholar, but he somehow got the impression that he was the only person in the world that had this kind of interest. When he came here to the Jackson Laboratory, it was probably the first time that he had ever had contact with people who had similar intellectual interests, so Benson Ginsburg, who was then at the University of Chicago, arranged for him to take the high school equivalency exam, which he passed with no trouble at all, and then he went to the University of Chicago, which at that time was doing a short course--I think a two or three year under-graduate course--he got that all done, then he went on to graduate work, first at the University of Chicago and then later, at the University of Washington, a Ph.D. in psychology-

SM: Oh my goodness!

JS: He's now a professor at the University of Montana.

SM: My goodness! Started in your lab.

JS: That's right. Another success story is that of Mary-

'Vesta, who illustrates what happened to a lot of research assistants. What I tried to do was not to exploit the research assistants, which is easy to do, but to encourage them, after a year or two, or maybe three years, to go off and get more education, and do further things. Mary-'Vesta, after a couple of years, do you mind my telling this? Mary-'Vesta went off to nursing school at Yale. Nursing had been one of her interests, and so she took an M.N. in Nursing, and then later on, she got into Public Health nursing--and I can't really tell her all your career--

MS: A Masters Degree in Public Health at Harvard and as Paul mentioned later a Ph.D in social psychology from Boston University. I grew up in the east, in Calais, and caught a glimpse of what might be done from my work here and also to fit the research that Paul was doing into what I did later.

JS: So, anyway, she's now one of the leaders in the whole field of nursing research. And there were many other people with similar careers of that kind.

SM: You should definitely write this up, definitely write about the people that came through your lab, and what's become of them, really, truly.

JS: I keep in touch with quite a few of them, but I have lost track of others.

SM: But it's a part of the Lab that got shunted off to the side, and it sounds like a really pioneering place.

JS: Well, it was.

MS: The sky was the limit. You got all the encouragement you needed.

SM: That's wonderful.

JS: And that's really the way people felt. This was after World War II, you know. Everybody had been held down for five years or so, and they really felt, after the War was over, that you could do anything. It was very stimulating, and to a lot of people, I think inspiring too.

SM: Right.

JS: Of course, there were a lot of funny things that happened, as well as serious ones. We had a lot of people, famous visitors, come by, from time to time. One of them was Haldane, from Great Britain.

SM: Oh yes. Oh my goodness.

JS: After the War, with his wife, who was also a geneticist. I was invited to show them the Hamilton Station, which was just being built at that time. It so happened that it was in January, and there had been an ice storm. The whole ground was covered with ice, so I took them out to show them the dog runs and things like that, and the first thing that happened was that Haldane's feet flew out from under him and his wife grabbed him, to keep him from falling down. He apparently had a weak back, and he hurt his back, and he just gave her what for (laughter) doing this to him. Dismayed, she stood away from him, and then <u>she</u> fell down...so I said, "Let's just go back in the building. Let's get away from this ice," and then <u>I</u> fell down!

SM: Oh no! That will teach him to come to Maine in the wintertime!

JS: Well, he never came back. He went to India (laughter).

SM: Where he wasn't going to have snow!

JS: Stayed there for the rest of his life.

SM: That's funny.

JS: Another funny thing was--this was in the very early days --it must have been very early, around 1945 perhaps. The Lab was always hard up for money, and this was before the fire, and Lib Kuecher, who was the secretary there for Dr. Little--I think she's around, by the way--she came back from England. She was down there in this wooden room, which was never finished: there were cracks, rough wood, you know, cracks in the floor, bedbugs from the mice crawling around (laughter). It was a terrible place, but anyway, she was opening up the mail,and a big yellow envelope came in. It was kind of heavy, so she shook it up. What came out but $20,000 worth of gold certificates, yellow gold certificates! This of course fascinated everybody--all this money! (laughter) ... Down there, people were dancing around the room, tossing money around!

SM: Oh my goodness! Was this legal--gold certificates?

JS: They were illegal, you see. They had been illegal for ten years or so, and somebody apparently had hoarded them, and decided that the way to get rid of them--was to anonymously send them to the Jackson Laboratory. Nobody ever found out who did it, of course. No way of tracing it, and the federal government did allow the Lab to cash them--

SM: Oh my, that's nice.

JS: That took care of the deficit of the Lab for that year.

SM: She must have been somewhat mystified (laughter).

JS: Yes, but, it wasn't really very mystifying because, it was perfectly obvious what had happened. It was, as I say, for a few years there a gay place. Most of the staff members were young. They liked to give parties. And also there was no status and very few status problems. Everybody was more or less accepted as equal.

MS: There was equality. After the summer investigators left, then the regular staff held seminars at various staff members' homes to which we were all invited and were expected to contribute. We were never treated like second class citizens and were told that we had a very important contribution to make and we did our best to make it.

SM: I've interviewed several of these people, and that's what I've heard, the impression they gave me.

MS: And there were some very funny stories about Allen Salisbury.

SM: Oh yes. I interviewed him.

MS: Did he tell you any of his stories?

SM: Yes. He told me some, but he told me, I mean, he told me enough to whet my appetite. I wish I could hear him for about three hours more.

JS: Yes. He's a fascinating character.

SM: He is, especially with his Maine accent.

JS: And his loud voice too, in that old Laboratory, which didn't have any walls, really, to speak of. We would hear Allen coming from anywhere in the building.Whatever he said, you could hear him.

SM: They tell funny stories about him too. Darrold Dorr tells the story that Allen Salisbury had some helper--apparently he ran the store, the Jackson Lab store, or supply place--and he had a helper, and one weekend, Darrold heard that the man in the Jackson Lab store had died. "Oh, my friend Allen's died?" So he was incredulous, and he called the Salisbury house, and he gets Allen on the phone, and he says, "Is it true?" without thinking, or recognizing the voice, "Is it true that Allen Salisbury's died?" So Allen gets on, "Well, I don't know. I'll have to investigate. If it's true, don't send flowers, send food or money." (laughter) And then, of course, by that time, Darrold realized that Allen was very much alive, and it had been the other man that had died. That was so typically Allen.

JS: That's wonderful.

MS: There were often very funny things that happened after the fire, like Dolores Dellabar Crary--remember her?

JS: Yes.

MS: There were a few nice that survived in part of the Laboratory that burned, and someone decided that they should be fed with a medicine dropper (laughter)

and Dolores said she was going to leave, if she had to do that.

SM: Yes, I would think.

MS: It was very funny.

SM: Now, in fact, did she have to leave, or did somebody else do it, or did they just not--

MS: I don't know if she did or not.

JS: No, she didn't.

MS: ... She packed her bag and said she was going (laughter).

JS: She actually stayed on for years. So they probably got somebody else to do it.

MS: At the time of the fire there was a research assistant named Mary Spangler whose father ran a grocery store in Bangor. He sent groceries to us, and we used the carrots that the Lab had been feeding the rabbits to get meals for those whose housing had been burned.

SM: They were very resourceful. Right. Everyone I've interviewed suggests that it was a very jolly place. They had a fairly good spirit, even when they had tragedies like the fire. They seemed to be resourceful.

JS: Yes, I think that was true, in those early days, and, of course, some of that carries on. Once you've got an institution started along particular lines, it tends to continue, but there are also tragic stories as well. I remember one case--there were a group of people, research assistants, actually, were driving a car down the dock, off Bar Harbor, and somehow or other, drove it off the end of the dock, and a couple of them got drowned.

MS: Oh yes, Eleanore Talbot, and who else was part of the group? Higgins, and there was a third who was a school teacher, and two of them survived, and Eleanore Talbot was drowned.

SM: Can you think of other things you want to add?

JS: Anything else I want to say? Well, I think I've pretty well covered most of what could be said in this short time. But I might wind the thing up with the story of Hamilton, the Station that started out as a rich man's toy, then became a very serious laboratory for many, many years, then was abandoned and sold by the Jackson Laboratory. For a year or two, it was owned by someone who wanted to develop it into an amusement park. Fortunately, the town Planning Board was able to stop this. Apparently, it has now been bought by another wealthy person, so the thing has come a full circle. He has bought it, so we are told, for his children.

SM: It was interesting...

JS: As I say, it has gone through the full cycle. I don't feel badly about the place, although I think it would have been better if they could have developed it into something useful for the Laboratory, which was what it was designed for, and for which had very good facilities. It didn't happen. In any case, I think that the important thing is not the building, but the work that got done there. That's all I want to say.

END OF INTERVIEW