5-31-1986

George Snell Oral History

George D. Snell
The Jackson Laboratory

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Oral History Collection

Interviewer's Comments

Narrator's Name Dr. George Snell

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.

George Snell's quiet voice and calm demeanor contrasted sharply with the background atmosphere of our taping session: constant interruptions from visitors, the telephone and a very solicitous spouse, and, on top of all this, a lengthy thunderstorm. George remained unperturbed, but he may have been distracted, and this may account for the anecdotal thinness of this tape. Despite his c. 40 years at Jax, Snell provides little here of the colorful vignette. He does recall his early days, living in a tent on the Lab grounds, and the locals' referring to the Lab as the "mouse house;" he also offers pictures of C.C. Little and the enjoyment they had in Lab parties, their games with the mice, and the family atmosphere that provided moral support through the lean Depression years. Never is the issue of administrative transition raised, nor does Snell get deeply into the technical areas of his histocompability work, for which he won the Nobel Prize. There is no incisive or objective look at the Lab, its merits or failings.

Snell's description of the phases of his retirement and the Lab's retirement policy is poignant. For him, as for so many Lab employees, the Jax has been a central focus of his life. It was obviously painful to be forced to lay off his assistants when his grants were cut solely on the basis of his age, and retirement status.

Snell's veracity is reliable, but the distractions may have affected his concentration. Supplement this tape with others, e.g. the Clark-Robbins-Salisbury tape, of a more anecdotal nature for a good picture of the Lab in its early years.

31 May 1986
Date

Susan Mehrtens
Interviewer's name

THE JACKSON LABORATORY / BAR HARBOR, MAINE
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Place Bar Harbor

Date 28 May 1986

George D. Snell
Narrator

Susan E. Melvitens
for the Laboratory

The Jackson Laboratory
INTERVIEW DATA SHEET

This section is to be completed by the Interviewer.

Narrator Dr George Snell Address 21 Atlantic Ave Bar Harbor Me 04609 Phone 207 288-3624
Birthdate ______ Birthplace ______ Interviewer Dr Susan Vheuntens Phone 207 244 7353
Date(s) & Place(s) of Interview(s) Bar Harbor 28 May 1986
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Complete each of these sections as the tape is processed in each step.

Received & Labeled	Collaterals Filed	Transcribing: Begun	Number of pages	Total time	Catalogued	Audited	Editing: Begun	Total time	Review: To narrator	Returned	Reread	Preface	Final Typing: Begun	Text finished	Index, Table of Contents	Proofread	Corrected	Duplicating: Transcript sent	Transcript returned	Tape sent	Tape returned	Distribution	Dissemination
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Collateral Materials Report

Narrator's Name Snell

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List and describe all acquisitions below. A typical description might be "Copy of letter from Governor Henry Horner to James L. Singleton, February 29, 1937." Provide as much identifying information for each photograph as possible. Each photograph should be labeled on its back as well as listed below.

1. copy of Nobel Prize acceptance speech

2. Letter re. own interview

3.

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Interview Contents
Dr. George Snell

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Interviewer's Notes and Word List
Dr. George Snell

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Woods Hole
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Terms:
Drosophila
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histology
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histocompatability genes
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congenic
allele
chromosome
induced translocation
antibodies
genotype
albumin
agglutinate
mammary tumor incitor
loci
lymphocytes

This is the tape of an oral history interview of Dr. George Snell, given as part of the Jackson Laboratory Oral History Porject, sponsored by the Acadia Institute. This interview was held on May 28th, 1986, in Dr. Snell's home, in Bar Harbor, Maine. The interviewers were Drs. Judith P. Swazey and Susan E. Mehrtens. RS is Rhoda, Snell's wife.

SM: Why don't we begin by my asking you when you first heard of the Jackson Laboratory? How did you happen to come?

GS: I can't actually put a precise occasion and date on it. I know the first time I met Dr. Little: There was a genetics congress at Cornell, in Ithaca, New York. This was about 1932, and I remember very well driving down to that. There was to be a partial eclipse of the sun in the area we were in. I had a friend with me, and I remember we watched for that. I had heard of Dr. Little, of course when I was in graduate school, since we both studied under Dr. Castle, but I had never met him. The congress provided the opportunity I had never had before. The Laboratory had been founded by that time, but I am not sure I had heard about it. I very likely had.

I remember one of two job opportunities I had when I finished my graduate work was at Cold Spring Harbor, with McDowell, who was one of the other very few people who worked in mammalian genetics at that time, but on the advice of Professor Parker at Harvard, who was highly respected, who urged that I go into university work, I did not accept that. I took a teaching job at Brown, which actually turned out to be a very dull job, not my interest at all. And I then read Muller's work about the x-ray induction of mutations. He had done that with Drosophila, but it seemed to me that it could very profitably be repeated with mice, and I wrote him, proposing this and outlining a plan. Well, by a very happy coincidence, he had already decided he wanted to do this with mice, and had developed a very similar plan. He actually had the mice there. He was just waiting for somebody to show up. So I went and spent two very happy years at the University of Texas, and I remember reading there--I think it was in Science--but anyway, the early papers from the Laboratory about the milk factor. You've probably heard about that, which turned out actually to be a virus which causes mammary cancer in mice, though the authors didn't hint of a virus in the original paper. That was my first real impression of the Laboratory.

Then, after that, I taught for a year at Washington University. This was when the Depression was just about at its worst. I was lucky to have a job, but I just decided that teaching was not my particular kettle of fish. Also I was having health problems which, as I learned for the first time, from tests at the hospital, were due to food allergies. Another problem was the work they loaded on the staff because of the hard financial times. I had almost no time for research, though I did manage to complete one paper on descendants of the x-rayed mice--not one of my best--with a graduate student. So I wrote Dr. Little. Essentially, The Jackson Laboratory was the only place I could go to do what I wanted to do, which was mammalian genetics. That was my training. I could have shifted to Drosophila work, and have a job at Texas. I didn't want to do that. I wanted to stick with mammalian genetics, which I felt had a real future and which was what I was familiar with. My work in Texas with the x-ray mutations worked out very well. So, as I say, the Laboratory at that time was really the only place where one could do work in mammalian genetics. I had no alternative.

It happens I have just been going over some of my correspondence with Prexy. I'm sort of in the process of reconstructing the diary I never kept. (laughter) And we had a very pleasant correspondence. One of his letters offers me a job here, but the Laboratory was a very struggling institution in those days, as you probably know. The salaries were hardly what you'd call munificent. Actually, Joe Murray, who was one of the original staff, had an offer of a job teaching at the University of Maine, and it was his departure that made an opening for me. Also Dr. Little was interested in x-rays at that time because of the indication that they could cause tumors. That was his primary interest in them. I had worked with x-rays, though with a different intent, so a job for me at the Lab seemed a mutually agreeable arrangment.

Actually, there was a while between my two jobs, and I spent much of this time visiting my brother in Texas. He was an oil engineer, and flying was his hobby. He had just teamed up with a pilot who'd been on the wrong side of a strike: He'd stayed with the company instead of the union. And the union had won, so he was out of a job. My brother, also temporarily out of a job, bought a used six-seater plane, and we went barnstorming (laughter) through part of West Texas, a lot of tiny towns. Quite an experience!

It was June of 1935 when I came up to the Lab. Although I had lived in New England all my life, except my few years away, I had never been in Maine. I had a great-aunt who summered on the Island for many years, but I had never visited her there. So this was my first trip to Maine. Well, I remember, having recently been in Texas, how noticeably shorter the days were. The sun always seemed to set too soon.

I arrived in Bar Harbor rather latish in the day, and I didn't know where the Laboratory was, so I pulled up at a garage, which I think was McCloud's garage, up here on Main Street, and asked them where the Jackson Laboratory was, and a man said, "Oh, you mean the 'mouse house'." (laughter) That was my introduction to the area.

Quite a bit of that first summer, well, actually most of the Spring, I lived in the tent colony in back of the Laboratory. This went back to Prexy's original contact with the Island, when he had a summer school sponsored by the University of Maine. He used to bring biology students down to the Island. You probably know about that. And the tent colony went back to that time. There were platforms for tents, that sort of thing. The tents were comfortable, but it happened to be a very wet Spring, not quite the ideal season for tenting. Shortly, however, I moved into a house, and while I changed quarters a number of times, I always had a satisfactory place to live. During my first winter on the Island, I met Rhoda, and in July 1937, we were married. Rhoda, as you may know, was the daughter of the Episcopal minister in Southwest Harbor, the Reverend Roy V. Carson.

JS: So when you came here, you arrived to stay?

GS: That was certainly my intention, yes. There were many pleasant features about those early years, as Rhoda mentioned. During the winter, there was always a monthly party. The total number of employees, including Prexy, was about 14--I'm not sure of the precise number--about equally divided between staff and the youngsters who changed and washed boxes. Instruments were very scarce. There were perhaps two or three compound microscopes, one or two dissecting microscopes, and a microtome, and also, when I came there, a Leitz camera for taking photomicrographs which was so complicated nobody could run it. (laughter) It filled a whole table, quite different from the little compact things they have now. Somebody had given the money for that. Actually, later, when I was involved in editing The Biology of the Laboratory Mouse with the help of Rhoda's cousin, who was a summer student, we did get that set up and got some very good pictures. It was not easy to use, but there was a good dark room which was a big help.

To return to those monthly parties, everybody--wives, and sometimes children, I guess--would come out and play games and I must say we had a lot of fun with that.

JS: Did you feel fairly quickly on that you had made the right choice, that you had come to the right place?

GS: I don't remember really ever having any thought of leaving the Laboratory. I had times when I was more happy than others, but I don't remember having the thought of going anywhere else. There just was no other place where I could do the work I wanted to do and I loved the Island. I will say that, although on the whole those first years were very pleasant, I think there were more personality problems then than there were during many subsequent years. Perhaps that was because you were thrown with people too much, but anyway-

JS: Do you think that was partly due to the very small size?

GS: Well, that's hard to say. I can't say it was that, or the people, but it didn't keep the Lab from being basically a satisfactory place to work. There certainly is no place I would have enjoyed more and the Laboratory met my requirements almost perfectly for what I wanted to do. I wanted to do mammalian genetics, and there was virtually no other place to consider.

SM: To what extent did C.C. Little leave his mark on the place, or to what extent was he a figure--

GS: To an enormous extent. He really dominated the Laboratory in the early days, no doubt about it. He had his outside activities. You probably have heard about that. I mean, he became Director of the American Cancer Society. He was involved in setting up the present program under which the National Institutes of Health operates, the peer review system. He had a hand in setting that up, and of course, he spent a good deal of time away raising money, but even so, he had a lot to do with the Laboratory. He was a very interesting combination of an aristocrat and a democrat. He could be both, at one time or another, but I think he, to a considerable extent, dominated the decisions in the early days, although I certainly don't think he intended to.

When I finally hit on the particular work I wanted to concentrate on, I got the inspiration from reading a chapter Dr. Little had written for The Biology of the Laboratory Mouse on the genetics of transplantation. I decided there were opportunities there that hadn't been exploited. That was a very happy choice, both from my point of view and Dr. Little's. There were a few times before that, however, when Dr. Little was not so sympathetic. For example, I brought radiation genetics here, and while Dr. Little didn't keep me from doing it, I couldn't feel any particular enthusiasm for it.

JS: How much, in those early days, because the Lab was so unique in mammalian genetics--how much contact was there with other genetics centers like yours?

Gs: I'm not aware of a great deal. I did keep up something of a correspondence with L.C. Dunn, another former student of Castle's who was active in mouse genetics at Columbia. I have one letter from him dated 1930. It was perhaps another ten years before there was much occasion for an expanded correspondence. In those early days, we did very little traveling. Of course, we ran into the Second World War quite early, and the Laboratory was very lucky in being able to keep open. It was partly because many mice were needed for work in tropical diseases, and the Laboratory turned on the spigot and turned out those mice, but I think all of us were able to keep our basic research going. There was a period later when I did a great deal of traveling, going to meetings, giving talks, that kind of thing, but in those early days there was very little.

JS: Was there a lot of correspondence with geneticists in other centers linking your work with Drosophila genetics, or were they much more separate tracks?

GS: Well, there was one area where I did set up contacts outside, and that was in connection with nomenclature. it was apparent in those early years at the Laboratory that gene symbolism in the mouse and the nomenclature for genes, was in some disarray, and that it would be useful to get them in better order. With this in mind, I wrote to L.C. Dunn and to Hans Gruneberg in London, suggesting the need for a Mouse

Nomenclature Committee. Both Dunn and Gruneberg agreed, and in 1939 we sent out a circular letter to everyone we knew with an interest in mouse genetics to enlist their cooperation. This was the beginning of a considerable correspondence and numerous publications extending over many years. Joan Staats became actively involved in this shortly after her arrival in 1949. (See Mouse News Letter, no. 50, 1974, p. 1; and Mary Lyon's chapter 3 on "Nomenclature in the Mouse" in Biomedical Research, vol. 1, 1981, for details). Aside from this nomenclature correspondence in the early days, I don't remember really any appreciable outside contacts. Dr. Little had them in connection both with the Laboratory and his interest in cancer.

RS: Well, I think Dr. Little was after money. You see, the Laboratory was started the same year as the Depression--'29--and for quite a few years after that, there was very little money--

GS: Oh absolutely. The Laboratory really was a struggling institution in the early days. If you give me just a minute I can bring down some records which might give you more accurate dates on a few of these things. In 1939, I have records of attending a Third International Cancer Congress, and after that there were occasional trips to meetings.

JS: About the time you arrived, during those first years, how many equivalent of today's Senior Scientist were there? You said it was a very small group of about 14--how many researchers were there, besides you and Dr. Little?

GS: My recollection is about seven, and it remained at that level for some little time. I may be off--it may be one or two more or less--but it was a very small group. Bill Russell and Tibby Russell came to the Lab before we were married. They were the first new additions to the staff that I remember, and very valuable additions, and George Woolley came about that time. I'm sure you probably have records of these somewhere, the actual dates [Russells, 1937; Woolley, 1936]

SM: And was it really a "mouse house"? That is, were you housed in one building?

GS: When I arrived, they were still in the original building, and that was really a bare minimum. It had been designed by Dr. Little's brother, who was an architect, for a minimum budget. There were rooms about twelve feet by twenty-two feet, as I remember it, on two sides of the corridor, the same both downstairs and upstairs. Downstairs, you had one of these which was your laboratory and office, and you had one upstairs, which was the mouse room. Then, there was a larger office which was Dr. Little's office [housed Dr. Little's secretary] and where we had the parties. There was a small library, with a bare minimum of necessary journals, and upstairs one large room which was the histology laboratory, because the cancer work required the preparation of sections of the cancer tissue...

SM: So you had this one building?

GS: Yes. Staff members had separate rooms, but when I had Dr. Gorer working with me, we shared the same space for both lab and office.

The mouse cages in those days were wood. The racks on which they rested were wood. The idea of sanitation in those days was, compared to what they are today, virtually--(laughter) I know before I came to the Lab, there was apparently--Dr. Little, who was interested in polydactyly in cats, and had several cats with extra toes, was interested in the genetics of it--he let them have free run of the Laboratory to pick up stray mice, and the result was, they found that the mice were getting tapeworms, of which the cat was the alternate host.

SM: Did you have trouble with bedbugs?

GS: Oh yes. That was the result of the shipment of mice, something they started very early; you've heard about that probably--the sale of mice. That started the first year, or soon thereafter--I don't know the exact date--to raise money to help through the Depression. They sent the mice out in wooden shipping boxes, which were returned as a matter of economy, and bedbugs came back on one of these return shipments, and got into the colony. And the wooden boxes with their wooden covers and the wooden racks were a perfect haven for them. I don't remember their ever bothering the people, fortunately. But it really was a problem, and I know the one way they dealt with them. They would have a little jar of kerosene and a syringe, like what you baste turkey with, a rubber ball on a tube, and squirt kerosene on the cracks, and that undoubtedly helped the Laboratory to burn very rapidly at the time of the fire. (laughter)

JS: That's right.

GS: I know I laboriously painted all my shelves at one time, to fill in the cracks, and tried to fill up all sorts of other cracks and that helped a little bit, but you could never get everybody to concentrate sufficiently on this one thing, and even if we had, it might have been impossible, and actually, I will have to say, in that one respect, the fire was a blessing, because I don't know if we would ever have gotten rid of the bedbugs without it.

SM: How did the fire affect your work?

GS: Actually, well, let me go back a little bit. Shortly after I came there, the first addition was added to the Laboratory, and that was mostly fire-proof: It had brick walls, and concrete floors. The peaked roof had wooden timbers, and in the fire, that went, but a lot was saved in that part of the building. Actually, thanks to a very up and coming assistant, all my records were moved into the fireproof part of the building, so I didn't loose them. I lost all my mice, but by that time, I had completed one piece of work on the genetics of transplantation. By good fortune, Dr. Gorer, in 1946, without really knowing about my work, but knowing about Little's early work, and the work of other members of the staff on the genetics of transplantation, a subject he was interested in, contacted Dr. Little about spending a year at the Laboratory, and he and I worked together. It happened we were both at a very propitious stage for setting up a collaborative project, and by good fortune, that project was completed just a few months before the fire, and published in a paper in which the symbol H-2 was used for the first time. That was a great piece of good luck, but the other part of my project which I had spent about a year on was lost.

Actually, at this time, I was spending quite a bit of time on the new construction of the Lab. They had started the second addition to the Laboratory shortly before the fire: The first floor had been poured, and they had forms up getting ready for the second floor. There was some fire damage to what had been completed, but not enough so that they couldn't go ahead with the part of the construction, and this was quite a blessing because that was one of the first areas we had available in which to work. But another blessing of the fire was that people discovered how indispensable the Laboratory was as a source of inbred mice. And I think probably Prexy was able to raise substantially more money on that account than he would have been able to otherwise. We had quite a fair-sized construction program set up. I was quite involved in that for two or three years after the fire.

One little tale in that connection: The members of the construction committee were Meredith Runner and Dale Foley and myself, and the architect who had done the second addition to the Laboratory was a Mr. Strickland, and Dale wasn't entirely happy with the work which he had done. I think that generally it was a very good job, but there was a feeling we should change, and I remember, as a member of this committee, we traveled around to do some interviewing, and we visited one of Prexy's friends, Joe Gerrity, who was a Trustee of the Laboratory. He was a University of Maine graduate, and just by coincidence (or so it was implied) Alonzo Harriman, a Maine classmate of Joe's, who had a sizeable architectural firm in Auburn, was there. We met him and decided he was the right man and signed him up. Actually, he did an excellent job, and his firm has done some much more recent work at the Laboratory too. It was with this construction of Unit 3, as it was called, which Harriman designed, that the Laboratory began to grow and that we got a great deal of additional space. This construction really marked a turning point in the growth of the Laboratory.

SM: Do you have other anecdotes you can think of from those early years, that give a flavor of the Lab?

GS: Maybe they'll come.

SM: Some people have said to me, for example, that Dr. Little used to dress up and play Santa Claus, at a Christmas party.

GS: Yes, yes, although actually, the first person I remember playing Santa Claus--this was years later--was Allen Salisbury.

SM: Oh my goodness!

GS: Allen Salisbury changed my mouse boxes for years.

RS: A dear, dear person. He was an enthusiastic fisherman, and still is.

GS: And quite a storyteller and talker. You've met him?

SM: Oh yes, yes. He was the one that told me that Dr. Little wore the Santa Claus suit, and one time--I think he probably did occasionally--but this one time probably put him off it forever because he was climbing in a window one time, caught his pants on a nail, and ripped the bottom out (laughter).

GS:Oh yes, yes, I do recollect he played Santa Claus the very first...

JS: How would you characterize Dr. Little's vision of the Laboratory, in terms of what he sought, and what he saw it becoming?

GS: The goals of the Laboratory in those days were always stated as genetics and cancer, mammalian genetics and cancer. I think Dr. Little had a real interest in cancer because his father, so I once heard, died in very painful circumstances from cancer. He had a personal interest in it, and the high incidence of mammary tumors in some strains of mice seemed to provide very favorable material. I think he had a very real interest in the cancer work in the early days, but I think more and more interest shifted towards genetics, though people still talked about cancer research a good deal. Of course, it helped to raise money. The grant which I operated under for many years came from the National Cancer Institute, even though my project had very little to do with cancer, but I did make extensive use of transplantable tumors.

Originally, Dr. Little raised all the money. Before the NIH was awarding grants, the American Cancer Society became a source of some money... a major source in the early years, that and private donations. But Dr. Little had these connections with the NIH, and he got one of the very early grants. In those days, all the grants were in his name, but later on, the main grant which supported my work was continued in my name, and that grant ran for, oh, at least ten years, probably more than that, all told. [Actually for 23 years, though after I was officially retired in 1968, not in my name.] I think the extent to which Dr. Little was a source of all the money was one of the reflections of the major role he played in those early days.

To return to the parties, they really were a lot of fun, and they'd have two people involved in each party, one to provide refreshments, and the other to plan games. So we had a lot of fun dreaming up games. I know--

RS: Because everybody went: It wasn't just for the staff.

GS: One game, which I don't think was original there, which we played was egg soccer. We'd have blown eggs--just the empty shell.

SM: You had blown eggs, these were blown?

GS: We had a big table and two teams, and the idea was to blow the egg into the goal at the other end of the table.

SM: Oh, I see. "To blow" as in "blow with your breathe"?

GS: Yes.

SM: Oh my goodness!

GS: And of course the eggs did not go in a very straight course, and you tried not to hit the edge of the table. I remember also a game I made up. I got a big round platform, and put it on a bearing, so it would rotate. We put several mice in the middle, and turned this thing, which gave a little centrifugal force so that the mice would go to the outside. Otherwise they tended to huddle on the inside. And people took bets as to which mice would leave the wheel first.

JS: Your sense was that even though Dr. Little was clearly a shaping personality, he gave the rest of you scientists an autonomy to pursue your lines of interest.

GS: He was basically a believer in giving people freedom to do what they wanted to do, yes. He had his own quite strong ideas about research, which came across some, but certainly his intent was to give people freedom to do what they wanted to do. He was a wonderful person, no doubt about it, a remarkable person.

SM: How did the Lab change under Earl Green?

GS: Well, Earl Green was a very natural and appropriate complement to Prexy's administrative style. Prexy did not particularly enjoy details. He liked dealing with the big picture, and Earl was just the other way around. He gathered up the details that needed to be gathered up at that time.

Actually, I think from that point of view, I think the Laboratory has been quite fortunate in its Directors: Each one has been enough different from his predecessor to move the Laboratory in the direction that it needed to go.

But one of the fascinating things has been to see the Laboratory grow. It was shortly after the War, about 1945 or 46, when a little bit more money began to come in. Up till that time, the Laboratory had had almost no research assistants. Betty Failor, now Betty Woodworth, was one of the very few in the early days, and she and Cloudy and I were involved in some projects, but beginning about 1944-45, I was given the job of Scientific Administrator, which meant you took some of the administrative chores on your hands, and there was enough money to hire several assistants. That was a very interesting project. The War had generated a shortage of jobs in other laboratories, so we were able to line up a number of very excellent assistants. Helen Bunker, orignally Helen Parker, came at that time. She's retired, by the way, and this Friday is her retirement party. Sally Lyman also came about that time. The people we hired had really excellent records. I know Middlebury wrote that Helen had the highest score on the medical aptitude test they had ever seen. The addition of these assistants was a really big help. It was then also, or actually after the fire, that we began to have enough space to really start expanding. It has been interesting to follow that growth, and I must say the

Laboratory, by and large, was very fortunate in the people who came. I must say I enjoyed those years very much. It was then that I got on to the project that I eventually concentrated on. That was a very satisfying thing to work on although some of it didn't go very quickly. It was definitely a long term project that required time.

SM: And the Lab was quite patient: You didn't have to come out with quick results; they were willing to give you time to--

GS: Well, I have to go into detail there. I decided there were two potential ways of identifying the histocompatability genes. One involved the use of what are referred to as marker genes. If you want to work with a gene whose effect is not easily demonstrable, you can often work with it by finding a gene with a visible effect that is closely linked to it. Actually, this was a technique that was frequently used in Drosophila, and my familiarity with it was one of the spin-offs that was a great help from my year in Texas where all the genetics was done with Drosophila. I decided that histocompatability genes, which were very difficult to demonstrate otherwise, could be spotted if you could find a visible marker with which they were linked. The second method of demonstrating histocompatability genes that I settled on was to

END OF SIDE ONE

put them on the background of another inbred strain which had a different allele at that histocompatability locus. By doing the appropriate crosses, we could develop two **congenic** lines that were essentially identical except for this one difference. Well, that took four or five years, but I was lucky in picking up, by the first method, one linkage almost immediately, and this became the basis of the project on which I worked with Dr. Gorer when he was here. And then I picked up three other linkages with marker genes in the process of the congenic line crosses. In a relatively short time these linkages made it possible to identify three other loci, H-1, H-3 and H-4. So actually, I didn't have to wait four or five years without getting results, and I was able to publish several papers.

I always enjoyed writing papers. Some scientists seem to hate to write papers. I really enjoy writing them.

RS: But then when the fire came, you had to start all over again.

GS: Well, I lost about one year's work on the congenic lines. Actually, Helen Bunker had worked with me on these, and they were lost, and we had to set them up again.

GS: George, were you the first to take the marker technique from Drosophila to mammalian genetics?

GS: As far as I know. I'm not aware of anyone else.

SM: But you had to do a lot of the hands-on work yourself in the early days, without research assistants.

GS: Oh, I made all the original histocompatability crosses.

Also the work I did on the chromosome changes (translocations) which I brought with me from Texas, I had to do entirely myself. Yes--record-keeping, mating, everything. Actually that was a very interesting project. Tony Searle, an English geneticist, has been working on translocations in mice for quite a number of years. He was very kind in citing a paper describing some very odd results which I got and which I couldn't explain, but which he has now explained. It's really a very interesting point regarding mammalian development which these odd results established.

JS: George, in terms of the observational powers of a scientists, and the ability to make new connections and essentially come up with new insights and new discoveries, do you think that anything has been lost to modern science by the fact that there isn't nearly as much hands-on work by the scientists themselves, that so much of it is now done by assistants or machines? Have you thought about that change over the years?

GS: That would not be my impression, no. To go back a little bit--this is the same general issue--I'm not a techniques person at all. That's why mouse genetics is a good thing for me because it's mathematical, rather than being based on techniques. I did later do work which involved techniques, but I was fortunate in having people like Marianna Cherry working with me to handle these techniques. One of the tests which we ran used the action of antibodies to demonstrate the presence of cell-surface antigens. The antibodies were radiolabelled and there was a machine which would automatically run your vials through and count the level of bound activity. You could put a load in of, oh, a hundred vials. In the morning, they're all counted and you have the results recorded. I don't know what we'd have done without that apparatus. But if there's anything I wonder about now, it's a need for sizeable teams for a great deal of work, and that, I think, that must change the situation somewhat. I worked with a group but it was a small group of people who spent a great deal of time together.

JS: When you had that early very small group of, say, seven scientists--

GS: It was rare in those days, in my experience, for more than three staff members and perhaps their assistants to collaborate on a project.

JS: In your first year, was there a great deal of interchange among you about the different projects you were working on, like cross-fertilization?

GS: Definitely, yes. One thing that's always been true of the Laboratory--I think it's very fortunate--people have been free to move from one subject to another as necessary. The day you have formal departments, that may be difficult. Because the Laboratory has never had departments, if peoples' interests change, they move to another group, they shift from one group to another. Certainly we would discuss our results and things of that sort with other people, but I think it was largely a matter of people whose work happened to fit together, just working and exchanging information, and helping somebody else with a particular technique...

JS: Do you think that is still a characteristic of the Lab, that makes it different from departments in universities?

GS: So far as I know, this freedom to move from one area to another is still true there. I think it's a very fortunate, very necessary feature of the Laboratory. But these modern genetic engineering techniques which are being increasingly used at the Laboratory are amazing and fascinating. They're fascinating, but actually, they wouldn't be my particular kettle of fish--

JS: You're not a techniques man...

GS: I think now I might go into something like computers, or something like that. (laughter). We actually have a son who works with computers...

SM: Did you ever participate in the summer students' program?

GS: Yes, I had quite a number of students over the years. Let me give a little background here.

I had decided quite early that, because the rejection of tumors was apparently an immune process, immunology would be of major relevance. Hence, I read fairly extensively in immunology, and tried to train myself some in the techniques. I remember just as a matter of my own education, developing an antibody against egg albumin, and showing how you demonstrate that it agglutinates the albumin. For quite a while I was just looking around for what would be a promising area to get into, and I thought possibly that you could demonstrate genetic differences within the sperm of one individual. Of course, each sperm would have its own particular assortment of genes, but the normal thinking is that these are not expressed until the fertilized egg. That probably is true, but I decided to take the chance that some of these might be expressed. There had been reports that animals could form antibodies against their own sperm, so I tested this in mice and found it a very striking phenomenon. One of the first summer students I had, Helen Poucher, worked with me on testing different immunization schedules, to get a good response. Helen--she was an excellent student--actully took this back to college and finished it up, and we published a paper on it. She subsequently married a New York M.D., and they come here summers.

RS: They still keep in touch... wonderful.

SM: So how much did these summer students help you in your own work?

GS: Well, I did generally choose the problems for them. They had to be something you can finish in about eight weeks. Actually, there was one chap, Ralph Barth, who had worked up a skin grafting technique before he came here, and he and Roy Stevens and I did publish a paper. I think he perhaps brought more in the way of a particular contribution of his own than anyone else, but of course, inevitably some projects just don't pan out. You can get useful experience but the results aren't of real significance. It was always stimulating to have the students but I think it's a pretty fifty-fifty proposition; we hope they get at least as much out of it as you do. Of course that's the way it should be.

SM: I remember reading the paper, the address you gave on winning the Nobel Prize, and you mention a lot of work with Dr. Stevens, and, I think, Dr. Cloudman--

GS: Well, in the early days, yes, Cloudy and I teamed up. Dr. Little had used transplantable tumors in his studies of transplantation, and Dr. Cloudman maintained quite a number of these tumors, so that I turned to him for that part of my work.

Actually, one of the things I did get into, which was a quite interesting project that I carried along for some years, was based on transplantable tumors. There were reports in the literature with regard to immunity to transplantable tumors. That is, if a mouse had been innoculated and grew a sizeable tumor and then recovered, the next time you put in a tumor, it wouldn't grow at all. One technique that was used was to place a graft in the tail, let the tumor grow, then cut off the tail. Then the mouse would be immune. Now this was usually a tumor from a foreign strain. In some special cases, it would apply to a tumor from the native strain, but that's a very special case. What struck me were a few reports about inducing immunity with non-living tumor tissue, and I thought that would be interesting to follow up. There were reports in the literature at the time of killing tissue with a minimum of modification, by freeze-drying. Freeze-drying was just coming into use at that time. This was before they had concentrated orange juice, and that kind of thing. I remember developing an apparatus to do this with the help of Gerald Mosley, a local mechanical genius who came to my rescue on several occasions. He made me a cylinder in two parts with a beveled joint that gave a tight seal when the vacuum was on. We connected that up to tubes of drierite, to absorb moisture, and then this to a vacuum pump. The frozen tissues were put into the cylinder and the vacuum turned on. The vacuum dried them out very quickly and the rapid evaporation in turn kept them frozen. Cloudy and I worked together on this, and got very interesting results injecting this tissue prior to tumor innoculation. We used two principal tumors, each in several different strains. One was a leukemia, the other a mammary carcinoma. The leukemia in mice which had had prior injections didn't grow at all. The injections entirely inhibited growth. The other tumor, instead of being inhibited, grew and killed all the mice--mice which normally would have grown a fair-sized tumor and then shown regression. We followed that up quite a bit. We actually gave some thought over the years to applying that principle to kidney grafts. Dr. Kaliss, when he first came to the Lab, became involved in the study. Actually, I was still doing some work on it at the time of the fire, although I was getting more and more into histocompatability studies at that time. It was really quite an interesting project, but although there have been some attempts to apply it to organ transplants, it does seem to be approaching a dead end.

I think you asked me a question that got me on to that, but I'm not sure just what's your next...

SM: I was just interested to see that you were working with people like your colleagues at the Lab, that they were, in fact, spinning off your research, and you were probably fertilizing theirs, and so forth.

GS: Yes, well, that is quite true. The Laboratory was doing cooperative research right from the beginning. The first project the staff took up, actually, was what Dr. Little had worked on in graduate school, not as a graduate problem, but something he had become interested in as a result of a paper by Prof. Tyzzer at the Harvard Medical School. This was the genetics of transplantation. This was one of the first problems the staff took up, but they had completed that by the time I arrived in 1935, and all the talk then was about the mammary tumor incitor. Actually, it was not until Prexy wrote his chapter on the genetics of transplantation for _The Biology of the Laboratory Mouse_ that I became acquainted with that earlier work.

JS: And that was a major trigger that shifted your interest into transplantation genetics?

GS: Yes. I was very consciously looking at that time for a really promising major project, and this finally seemed to be it.

JS: You sure found one!

RS: How did your approach to that problem differ from Prexy's?

GS: Well, what the early work had shown was the existence of the histocompatability genes, and there were quite a number of loci concerned, about ten or a dozen, but they had not been able in any way to pinpoint individual loci. They were like a group of people all wearing the same mask. The problem was to rip the mask off, and get the individuality, and that's what I thought should be possible by these methods.

SM: Did you ever experience frustrations working at the Jax? Did it seem, for example, too far away from others of your colleagues that you'd want to meet with?

GS: No, that was never a problem with me. I remember after the fire, Dr. Rhodes, Director of Sloan-Kettering and a friend of Prexy's, urged Prexy to move down to Long Island where some buildings might be available. Prexy sounded out people at the Laboratory, and I don't think one person at the Lab approved of this move. Prexy certainly didn't--he couldn't have had the nice fishing, among other things. (laughter)

GS: It was a unanimous decision to...

SM: Were there other frustrations that you can think of?

GS: Well, ... worries...into some blind alleys, and making mistakes. On a couple of occasions I had people working with me who wanted a particular piece of apparatus. I didn't check sufficiently as to the real need for these pieces, and we ended up with white elephants on our hands. That was one of the frustrations. And I made my own mistakes. There were always some problems, but they were minor. I loved my work, and I think--I haven't known a single person at the Laboratory who can't say that. They all loved their work.

JS: Do you think the Lab's relative isolation geographically attracted--was a strength for the scientists there?

GS: Well, one argument they used to use for it in the early days was that living on Mount Desert Island, you didn't need to run off somewhere for a holiday (laughter). It was right here. Particularly in those early days, you had the same problem as the farmer: You couldn't leave your livestock. You had to stay around. You could go off for short periods, but not very long.

SM: What do you think are some of the strengths or weaknesses of the Lab, then and now?

GS: Well, in the early days, it was the only place to do mouse genetics, but also it was certainly a very struggling institution, and I'll have to say that the salaries paid, and what they set aside for the future--the Laboratory had no annuity policy of any kind--for quite a number of years was pretty meager. I came across recently some correspondence with Prexy I had entirely forgotten about. Prexy and I had many very pleasant exchanges, and I certainly tried to not make this complaining, but I said that we were not getting enough money to educate our sons. This was when I began to think seriously about writing some other book, and when in 1953, I had a sabbatical leave, I used it to gather material. All I have gotten out of this project so far is a lot of intellectual stimulation, but I have no regrets concerning my involvement. [P.S. I recently have found a likely publisher.]

SM: When did this start to change? When did you begin to get better paid?

GS: Well, I think this was about the same time that the Laboratory began to grow, although it was quite a little while before they instituted any kind of annuity policy.

One of my pleasant early recollections is summers in Vermont. My folks had a place there in South Woodstock which they had acquired as a run-down old farmhouse in 1900, three years before I was born. We always spent summers there, and the family kept that place for a good many years, but ultimately had to sell it as a result of the Depression. However, I kept thirty acres of land, set out to pine trees, and for years, every September, we would go over there. There was a cabin which was just across the road from it which we could rent, or we stayed with a farm family my folks had known. Rhoda and the boys and I always looked forward to those trips.

SM: But you attribute a lot of the financial prosperity of the lab to things like the development of the federal--

GS: Oh yes, they've been almost--I was going to say "almost entirely" but that's a little bit strong, but by all odds, the National Institutes of Health has certainly been the source of financing, and, as I think you probably know, the Laboratory has a very excellent record in success for its grant applications. My work was almost entirely financed by this one NIH grant; it went on year after year. I did have at one time or another a few other small grants for special projects, but definitely federal grants were my main support and the main support of most other work at the Lab. I was cut back some after my retirement, for several reasons, and that was one of my minor gripes. I felt I still had the work going, and I wasn't able to carry it on at the scale I would have liked to have done, or could have done. Compared to the gripes I might have had (laughter), very minor indeed.

SM: Did they cut you back because you were retired?

GS: Well, it was due to several things--

RS: Excuse me if I interrupt. Don't you remember the Laboratory had a policy when people retired, they had to cut their time back, and their funds were automatically cut back.

GS: Well, you're officially retired at 65, and after that you can be reappointed on a yearly basis, but at progressively reduced salary, and presumably time; but you can't do research part-time. I put in essentially full time. But also I had, at Earl's suggestion, applied for a Research Career Award at NIH, and that ran for a number of years, and they cut my grant a corresponding amount, because my salary came out of the grant. That was fine, but after I was technically retired at age 65, although still working, I couldn't get that Research Career Award, but my grant was not correspondingly increased, so that was one reason the funds were cut. Also the Study Section did not give me quite all I requested. I could no longer pay Marianna, although her interest was still there, very much the same, and I had to beg help on some portions, from somebody.

JS: When did you officially retire?

GS: Well, I was born in 1903, so I was 65 in '68, and I was fully retired in '73, which was 13 years ago.

JS: Do you think that the tremendous growth in the Lab, in terms of its full administrative structure, was an inevitable part of the grant and the contract funding mechanisms?

GS: Well, there's always pressure for growth in any institution of that sort. People want more space, they want people to come and join them. There's always pressure for more space. Richmond Prehn had quite ambitious ideas about growth. He handled the details very poorly, but I think actually, it turned out to be a good thing. And frankly, every time the Laboratory grows, I worry a little bit that its character will change too much, but aside from that, I think the people there don't know everybody particularly well, and actually it's lost that quite considerably. I think the general atmosphere, as far as I can tell, remains the same.

SM: People often describe to me, the people I've interviewed, as a "family"--the Jax is actually like a family. Do you think--

GS:Yes.

SM: Do you think it's too big now to be a family?

GS: Well, that's a very interesting question. They tell us that at the hunter-gathering stage of human evolution, which went on for a long time, a particular tribe--40 was a number often used, though there was of course much invididual variation--and I think there is a limit of something like that number of people with whom you can actually interact enough to establish a close relationship. So far as the staff goes, we haven't exceeded that size yet, but--of course, I'm pretty much out of touch, but my impression is that the atmosphere of the Laboratory is still unusually happy.

JS: If Dr. Little had appeared as an anonymous site visitor for a week before you retired, before you were fully retired, would he have known the place? Would he still have said, "Is this the Lab I created"?

GS: Who knows? Well, of course, the original building, although they had to tear down all the walls, it was rebuilt in a somewhat similar form. I'm sure that Prexy would recognize the old stones--I'm thinking of the physical aspects. I think he'd be very happy at what's happened. I think he would be very happy. The Lab's still centered on mammalian genetics, and that's exactly what he wanted.

SM: Do you think in its basic mission as a center for mammalian genetics, the Lab has become somewhat out of date, considering the rise of molecular genetics?

GS: The continuing and in fact growing demand for inbred and mutant mice would certainly suggest that the Lab's role as a center for mammalian genetics is still essential. But the Laboratory has also taken on a number of people who are using molecular genetics. Now, as I say, I'm out of touch, but my impression is there's very good work along that line being done there. There are always opportunities for interaction. The Laboratory being a center for conventional mouse genetics, it's the ideal place for applying molecular genetics to conventional problems.

One very interesting thing I have seen occur is the steady development of new methods for identifying new loci. All loci were originally identified by their visible effects. One exception was the histocompatability genes, which were known to exist, but did not produce a direct visible effect. You could demonstrate them with a tumor transplant, but you couldn't see the actual end result of a particular gene. Practically all the original mutants were spotted by chance, many of them by fanciers who kept them as curiosities, and more visible mutants were gradually added, as new mutations turned up. Genes determined by enzymes were one of the early groups that was added, variation in the enzymes being identified by chemical methods. You couldn't do it by the ordinary visible means. There are now a great many genes known that determine enzymes. Then individual histocompatability genes were identified, and that work in turn led to studies of genes identifiable with antibodies. Gorer was the first one to do that. He originally demonstrated H-2 in that way. This is the fortieth anniversary, actually of this locus first demonstrated by use of a blood group antiserum. Then this was extended. I was much involved in identifying cell surface antigens of white blood cells, and that has become a very sizeable group of genes. And more and more of these highly technical indirect methods of identifying loci have been added, one of them being molecular genetics. This new discipline is indeed a gold mine. I am vaguely conscious of the principles, but I don't know any of the details. That's been one of the interesting things to watch--these new methods which are developed for identifying loci, and I'm sure more methods will come up. The number of known loci is now up in the thousands. What we really want to know is how many loci there are. That's one of the big unknowns. Estimates vary very widely, but 40 or 50,000 is a very common estimate. My guess is that that estimate is too low.

JS: Still a lot of mapping to do.

SM: Yes, there's clearly a lot of work there. How would you compare the Jax to other research institutions you've worked in?

GS: Well, the only other place I worked after graduate school is the University of Texas. Professor Muller, who won the Nobel Prize for demonstrating that x-rays will produce genetic changes in Drosophila, was there at the time, and that's the reason I went there. And that was a very active department for Drosophila genetics, and I very much enjoyed my two years there. It was a very interesting group. Of course, that was a university, a university department. I think within that department, the atmosphere was that of a very friendly group, which you also have at the Laboratory. I know that at the Rockefeller Institute they set up formal departments at a fairly early stage, and from gossip I gather there was some friction between departments.

JS: Was there any discussion at the Lab of that type of move towards the more formalistic university-like structure, of the Lab setting up really separate departments, or programs?

GS: Not that I'm aware of, no. I think they're very conscious of the desirability of the flexibility of the present structure. They do have interest groups, seminar groups that meet on a particular subject, but people are entirely free to move from one to another.

JS: They have consciously tried to keep it as unified as possible?

GS: I think so, yes. Of course, the whole Laboratory has this basic theme of mammalian genetics. They do some cancer work, but that's really a minor part of the problem now, and tied to the genetics. Hence there is a degree of uniformity and common interest there though people approach mammalian genetics from many angles.

JS: I have to say that one of my early heroes, Sir Charles Sherrington, would have been very pleased with the integrative action.

GS: Yes.

SM: Now, was this a big point with Prexy, that everyone should sort of stay--that it shouldn't fall into departments?

GS: Well, I don't think that while he was there, the Laboratory really got big enough to think about this. I never heard any discussion of it at the time. Actually, aside from very informal talk about it, I never heard him discuss the Laboratory. There were signficant changes in upper level organization and administrative structure, but the staff was not brought into this very much. To the best of my knowledge, nobody ever thought seriously of setting up departments. I must say, one of the very happy features of working at the Laboratory has been, as I said, it's one big family... I think also that, when you're in a small town, you have social ties that also are close to the Laboratory. Actually

I have been very happily surprised that on the whole there have been very good relations between the Laboratory and the town. I have heard of some exceptions, but my general impression is relations are very good.

RS: Is that your feeling too?

SM: Yes, well, I've interviewed some people locally, and they say that initially the Lab was--well, people thought it was a "mouse house." And they didn't know quite what to make of that, and so there was some initial skepticism, but I think the locals are always that way about anything, but after about ten years or so, especially after the fire, there was a definite impression that the Lab was valuable. It was bringing in money. It was hiring a lot of people, and they were generally treating their employees well, and I think they were often treating their employees better...so I do think you're right that the town-Lab relationship is--

RS: You mentioned about when you first came, you asked the way to--

GS: The "mouse house," yes.

SM: And then if you look at the role that many people in the Lab have played, like Dick Sprott, who was a consummate politician, and very active in the town, in terms of his role on the town council and all.

GS: Well there have been several people who have been very involved in local affairs, and I think it's very healthy.

SM: The Lab has made positive additions.

GS: Yes.

SM: So do you have a summation on your years at the Jackson Laboratory that--the Jax was obviously your institution, in the sense of your major work being done there. Do you have anything you'd like to conclude? We have about two minutes left there.

GS: Well, the Jackson Laboratory was certainly a great place for me to work. I couldn't have done what I did anywhere else, and I think I'm a specialist in my talents, but a generalist in my interests. That's the way I characterize myself, and I got into exactly the right line of work. For one thing, this was the place to do it.

END OF INTERVIEW