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Elizabeth "Tibby" Russell Oral History

Elizabeth Russell

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The Jackson Laboratory
Oral History Collection

Interviewer's Comments

Narrator's Name Dr. Elizabeth Russell

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.

This three-way interview, between Tibby, Judith Swazey and me, took place in Tibby's home. In the days before this interview, I had interviewed many Jax personnel whose reminiscences had "primed" me for this meeting with one of the most colorful, distinctive and beloved old-time Jax scientists.

Tibby has had a remarkable impact on Jax, both in terms of substantial science and also in a human, personal way, as a friend, mentor and colleague to many of the summer students, later to be lab scientists and administrators. Where she could have spoken at length of C.C. Little and the earliest years at the Lab, she chose here to range widely over her 49 years at the Lab, recalling the fire, the Hoxie party, the various Directors, and notable changes in the Lab.

This tape is vintage Tibby: Here is seen the fascinating circularity of her thought, her absent-mindedness, her exuberance and robust sense of humor, her sensitivity and compassion.

The value of this tape lies more in its anecdote than in any deep analysis or evaluation. Tibby is not by nature critical, although she can fly her colors as a committed mouse geneticist with the best of them, and she alludes ever so obliquely to her run-ins with the non-geneticists (whom she lumps together as "biochemists") on this tape. Comparison with Andy Kandutsch's tape will reveal the mutual perceptions of the two "camps."

Everything here is supported or elaborated on other tapes, e.g. those of Fay Lawson, Kandutsch, Snell, Coleman, Salisbury. While Tibby's mind can wander, her memory is sharp and clear, and highly reliable.

7 June 1986

Date

Susan Mehrtens

Interviewer's name

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Interviewer's Notes and Word List
Dr. Elizabeth Russell

Sewall Wright	England	<u>Terms</u>
Ann Arbor	Highseas	mammary tumor
C.C. Little	Meredith Runner	teratomas
Univ. of Chicago	Tibby	Drosophila
A.A.U.W.	Ede Kent	Blackfan Diamond
Finney-Howell	Jan Southard	I-131
Michigan	Eleanor McFarland	C3H's
Maine	West Tremont	chloroform
Jackson	Hoxie	ectromelia virus
Columbia	Wilhelm Reich	recombinant inbred
L.C. Dunn	NIH	congenic lines
W.E. Castle	Ed Murphy	
Beatrice Johnson Little	SWIGAMITI	
George Snell	Seldon Bernstein	
Lionel C. Strong	Will Silvers	
Yale	Rob Shea	
Prexy	UC San Diego	
Rockefeller	California	
Roy Stevens	Paris	
Rochester	Don Bailey	
William Murray	Ben Taylor	
Hampden	Joan Staats	
Hamilton Station	Otter Creek	
Eagle Lake Road	Hook Wheeler	
Thurlow Hanna	Ann Stevens Wheeler	
Ellsworth	Maine Racquet	
Kay Hamilton	MDI	
Detroit	Bar Harbor	
New York	Peter Sellers	
Sloan Kettering	Neva Kaiser	
Katerina Hummel	Watson Robbins	
Pete Morison	John Dorey	
Samuel Eliot Morison	Hans Jorge	
Charity Waymouth	Helen Bunker	
Margaret Green		
Fay Lawson		
Harvard		
Louis K. Diamond		
Children's Hospital		
McIntyre's		
Chai		
Janet TenBroeck		
Morrell Park		
Barbara Sanford		
Brown		
Earl Green		
Andrew Kandutsch		
COA		
MPAP		
Richmond Prehn		
Ariadne		
Topsey		
Jane Barker		

This is the tape of an oral history interview with Dr. Elizabeth Russell, given as part of the Jackson Laboratory Oral History project, sponsored by the Acadia Institute. This interview was held on June 3, 1986, in Dr. Russell's home in Mt. Desert, Maine. The interviewers were Drs. Judith P. Swazey and Susan E. Mehrtens.

SM: How about we start by my asking you when you first heard of the Jackson Lab, or how?

ER: I first heard about the Jackson Lab almost certainly in the very early '30's--because I lived in Ann Arbor, and was a friend of Little's first family. My father was in the University. I didn't really know very much about it until, in '36, my husband, at that time finishing his degree at the University of Chicago, was being interviewed for a Jackson Laboratory position. Not too many people really knew anything about it at that time. There was no big science then.

SM: It was just a few years old.

ER: Yes.

SM: How did you happen to come here?

ER: Because my husband did. I fell very much in love with it. I did research, although it was a very impoverished-type place, and I wasn't paid. I enjoyed it, and was very welcome there.

SM: How long did you work there unpaid?

ER: Let's put it this way. In 1940, I had an A.A.U.W. fellowship, which supported me during that year, and in 1946,

I had a Finney-Howell fellowship, which also supported me. Then, after that, about the time I was divorced (1947) I became part of the staff.

SM: What are your early recollections of C.C. Little?

ER: He was a great character. He was a very fine scientist, also very encouraging, in getting people to work together. You may not know the Michigan part of this. He was President of the University of Michigan (1925-1929) and had a lot of troubles there. He went to Michigan because influential Michigan summer residents saw him on Mount Desert Island. While he was President of the University of Maine, he brought students down here, as summer students, before there was any Jackson Lab. People who knew him here on the island--various people in the automobile business, or connected with the University--thought that he would be good for the University of Michigan, so he went to Michigan from Maine. At that time, there really wasn't very much mammalian genetics. However, people who knew about mammalian genetics were very impressed by the work of the Jackson Laboratory, especially on maternal influence, or mammary tumor incitor, in causation of mouse mammary cancer.

JS: Had you been interested in mammalian genetics before-

ER: Oh, that's a very good question, Judy. After graduating from Michigan, I went to Columbia, where L.C. Dunn was in the Zoology Department, to get my Master's degree. During that year, I read a paper by Sewall Wright. You have to be a

geneticist to know how important that is! I read his 1934 paper on the physiological and evolutionary theories of dominance, and I decided I had to learn from its author, so I applied to the University of Chicago, to work with Sewall Wright. Little, Dunn and Wright, plus quite a few others, were students of W.E. Castle--they had all worked together--
JS: The Founders Club.

ER: The Founders Club, that's right.

SM: C.C. Little gives me the impression from the stuff I've read that he was very supportive of women doing science.

ER: Yes. Oh yes. His second wife, Beatrice Johnson Little, worked in the lab, at Ann Arbor. She was also a Dean of Women. He was interested in women doing things. I think he's been quite fair in his encouragement. When I said I didn't get any salary til '46, it was because there just wasn't any money. The Lab was terribly poor. I think you can make a case for having the history of the Lab as a provider of mice start from the days in '32 and '33 when they sold mice to have something to keep the Lab going.

SM: Now, although the Lab was very poor in monetary terms, apparently it was rich in terms of esprit, and family atmosphere.

ER: Very much so. Has anyone told you about the Lab parties?

SM: George Snell referred to that briefly last week.

ER: Once a month--of course, when I came here in '37, there were about 20 people on the whole payroll, including assistants,

and every month we had a Lab party. Everybody was invited, and it was a great time. During the extremely difficult financial state of '33, only one of the original people, L.C. Strong, did not go along with Prexy's idea of everybody sharing as best they could--people moved in together, and grew their own food--Strong didn't want to do that. He went to Yale. I don't think he was altogether a compatible part of the group anyway.

SM: So, some of these parties--would these be the parties where you had mouse races?

ER: Oh yes. Indeed (laughter). Maybe George told you this one. I don't know if he did or not. He built a turntable, about three feet in diameter, of wood, with a little raised edge on the outside. It could go around, at not too great a rate, and you placed bets on which mouse could stay longest on the revolving table. Have you heard this story?

SM: He described the turntable, but he didn't go into much detail.

ER: Well, if you knew about mice, you knew that the ones that were shaking their heads were not depending upon their inner ears to balance them. Their ears were no good, and they had adjusted in other ways. Thus, in spite of the fact that they looked dottie all the time, they were the ones that won.

SM: I see. Did you ever win?

ER: Oh yes.

SM: Now these were also mice you used in research? So they

were rather cavalier about their sanitary conditions.

ER: Oh, my Lord! That's the only way to say that! The difference between mouse husbandry now and then is absolutely colossal, and it wasn't just at the Jackson Laboratory.

People were not concentrating on this kind of thing. For instance, in the mouse room, the boxes were wood, with wire mesh tops, and a partition in the middle, of wood. There were quite a few stray mice around, and Little had some pet cats that went around and took care of the stray mice, but what this does to the cat-mouse tapeworm situation is rather bad, of course. It was really weird. There got to be quite a few bedbugs.

SM: Oh, yes, Allen Salisbury told me about them.

ER: Oh, gracious! Were they something! And to help get rid of the bedbugs, they put creosote on boxes, and the Lab wooden floors, and racks, and so on, were very well soaked up with creosote, at the time of the fire.

JS: Merrily we burn away!

ER: Yes.

SM: Allen said that he always thought of the fire with some measure of gratitude because there wasn't a bedbug left!

ER: That's right! (laughter) I can just hear Allen telling that story. He's a great character.

JS: How would you characterize the working atmosphere when you came to the Lab, compared to that in the academic department where you had been at Chicago?

ER: More informal. More informal and--well, Prexy and Sewall Wright were great friends, and Sewall Wright thought Little was just great, liked his informality and every thing like that. Wright himself was sort of shy, but very friendly. We had a good relationship, but not the kind of thing that Prexy encouraged all the time.

JS: Do you think the informality created a greater interchange of ideas between the staff at the Lab than in a department?

ER: I have to take a different approach to that. Up until the time when George Snell came, in '35, the organization of the Lab centered really around the problem of mouse mammary tumor. It was like what we would now regard as a program project, and so, most everybody was working on something in common. Therefore, it's not really so pertinent to say how they understood each other, because there wasn't that much diversity. You knew the strengths and weaknesses of the other people. It is important to recognize that Little not only started the Jackson Lab, he also, at the same time, was starting the National Cancer Institute, and the American Cancer Society. He was establishing all of these things which were to support science, which was beginning to grow bigger. We didn't have grants to support research at the beginning. There weren't such things. There were a few supporting funds, but not very much. At that same time, Rockefeller was almost completely supported by Rockefeller

family funds.

JS: How much did Dr. Little encourage people to develop their own projects? At one point a minute ago, you said he sort of farmed work out under the program project analogy.

ER: That's interesting. I don't think he objected, but there weren't too many examples of separate, independent projects. I think George Snell started studying more different ideas. He came from a background of studying effects of radiation. Then he got into studying mutations, and a number of other independent projects. Roy Stevens started to study changes in ovaries after transplantation to the spleen and retransplantation into the mouse. This was supposed to test the effect of hormonal stimulation on subsequent tumor development in offspring from those transplanted ovaries. Roy discovered, and became interested in, the teratomas that appeared sometimes in the ovaries. No one ever discouraged him from following the teratoma line. Roy had been an early embryologist at Rochester, so this was for him a natural interest, and led to fascinating findings. Before coming to the Laboratory, I had been working on pigmentation, which I continued at the Lab. A mutation occurred which affected pigmentation and anemia. Little asked me if I wanted to see what happened with these mutant mice. It turned out to be one of the best problems! I don't think you'd ever say that he discouraged experimentation, or exploitation of something new, and I don't think you'd say he insisted on your doing a

particular things. I don't know what he'd do if you didn't develop an idea.

SM: Were the pressures on a young investigator as strong as they are today?

Er: No. The need was just to keep this entity--the Jackson Lab--existing! I'm not going to try to change this into 1986 dollars, but the salary of a staff member in the latter part of the '30's was less than \$3000 per year. It's amazing.

SM: But it sounds like it was a very, very happy time.

ER: It was a happy time. It really was, fascinating.

SM: And how did they come up with that money? Who raised that money?

ER: The money that we had? The Lab was to have been endowed, supported by these people who came in the summer. However, 1929 was hardly a time to start a lab in that way, and the funds that the Lab got, they got partly from sale of mice, and small amounts of gifts, but nothing substantial until after the War.

SM: Now I've been told that one of the Murray boys had land in Hampden, and people were growing food on it.

ER: Oh, I'm sure they were. They were also growing food all around down here.

SM: Did you grow food, too?

ER: No. See, I didn't get here until '37, when the farming aspect of it dropped out.

SM: Where were you the day of the fire?

ER: At the time of the fire, I was living here, and I had two of my children with me, two were with my parents in Ann Arbor temporarily. The fire was more in the middle of the island first, and the kids were going to school in Bar Harbor. We tried to get in, to go to school until about three days before the real open fire. On the last day, we had to circle around, going past the Hamilton Station, which already existed by that time. They wouldn't let us go any farther, which was very sensible--I mean, the fire had leapt across the Eagle Lake Road, so we came back here. There was no electricity. I remember trying to cook on a camp stove for a couple of days. On the night of the fire, Thurlow Hanna, who owned the cottages next door to me on Echo Lake, thought that we should get off the island. We went to Ellsworth, and stayed the first night in the City Hall, in Ellsworth, along with loads of other people. Then a family up in North Ellsworth took us in for the next few nights. It was quite something.

JS: When did you find out that the Lab had burned?

ER: That day.

SM: What did you think of it?

ER: Well, it was awful, thinking about all the things that were gone. But I was extremely fortunate. I was writing up a lot of pigment research at that time, and my research assistant, by the name of Kay Hamilton, took two big "book files" from the Main Lab out to the Hamilton Station. I got

four papers out of that material! I never could have done it without her! I knew that day, because when we got to the hill, at Ellsworth, there was Bill Murray, who at that time was head of the Detroit Cancer Institute, standing where 1A and 3 separate, trying to get a ride down to the island. He was coming to help! On Friday night, or Saturday some time--Prexy got everybody together. As you know, it wasn't an awful lot of people, but these people didn't know if they were ever going to have a job again. It was decided right then and there that we were going to try to rebuild. Quite a few people went to New York and worked at the Sloan Kettering which was a brand new lab at that time. Others of us worked at the Hamilton Station. For quite a while, my working space was a desk in what had been the common hall of the rabbit wing. In some ways, the fire was an absolutely, tremendously positive experience for me, because I was given the responsibility of going over the offers that people were making--there were telegrams coming from all over: we've got such and such kinds of mice that we got from you; may we send them back etc. etc.... We took them, deciding which ones were the most closely related to build our colony, and so on. So I was given the responsibility of handling that, and it was extremely valuable experience for me... I kept on with it and developed ideas--they were common sense ideas--trying to make all of the mice supplied to the various staff members as much alike as possible. It's true that before the fire, each staff

member had practically his own sub-line of the particular line he was working with, which was not the best thing. We had two kinds of concern: we had the concern of building different staff members' things as much alike as possible, and of providing pertinent mice to outside people. It was clear that people began to realize that they had to have something like the Jackson Laboratory. This building we used to call the "Inbred Nucleus;" it's now the "Foundation Stock." And that kept me busy for a long time.

SM: People have spoken--the people I've interviewed so far--have spoken about your work with the Inbred Nucleus.

ER: It was extremely important.

JS: Did your own research take a back seat while you were developing the breeding lines?

ER: Yes and no. I was encouraged--now there were beginning to be grants--to apply for funds. People kept saying to me at the Lab before "Well, you've got to see, do the new stocks have exactly the same cancer incidence that they had before? I got the idea of having mice in our "Inbred Nucleus" live out their entire lives, which we would characterize. We began to collect this data, and Prexy said we should apply for a grant. I adopted this as part of my research. But, aside from that, I was also doing other things, with W-mutants and anemias. I guess I just took on two different kinds of things.

SM: Now, in this time you had research assistants, right?

ER: Yes. The size of the central colony grew. Over a five year interval, you keep everything till it dies. So the colony got bigger, and bigger, and bigger (laughter). Unit 3 was just starting to be built at the time of the fire. For quite a while, Katrina Hummel and I shared an office in Unit 2. As soon as enough of Unit 3 middle floor was built, many different staff members had their office space--I remember mine was just outside of a corner which was separated off with chicken wire to provide an office for Pete Morison--Samuel Eliot Morison's son--who was the purchasing agent. I don't know how we thought at all! Pete had a very loud voice. Eventually we moved into one of those regular labs, which are 12' by 20', with benches on either side, and as many chairs as we could fit in. We had many people keeping mouse records, which involves a fair amount of paper work. There was a time when this room was home base for 18 people. How we ever got in there I don't know! But it was very, very interesting, and it was a good group. Most of those people are still around.

JS: The Lab by that time had grown quite a lot.

ER: It was growing. The Lab grew very little until after the fire. About the time of the fire, it really began growing.

JS: Did you see that as fairly strongly correlated with the growing availability of grant funds?

ER: Oh yes, very definitely. It's kind of funny. I don't think mammalian genetics could have grown as much as it has

without some sort of central laboratories--The Jackson Lab and the National Laboratories--because most universities couldn't have large enough animal-care facilities to do it. Facilities must really be quite different than those for *Drosophila*.

SM: When did the Lab Lovelies begin? Remember the Lab Lovelies, the women's softball team?

ER: We sure had a women's softball team. I didn't play on it very well. I went and cheered (laughter). I'm not an expert softball player!

SM: ... expert softball player.

ER: Have you heard the story of Charity and Margaret Green? Now when was that? Charity grew up in England; she certainly did not have regular experience as a child in softball, but she played. Marg was running to second base, I think, and Charity was there, and some way, it ended up with Marg trying to slide into Charity arching over her; it was just terribly, terribly funny. I went to those games a lot. I remember them; it was when my kids were 7 or 8--they're all in a six year span; there were some of them that were in junior high school.

SM: They never stuck you in the infield?

ER: Oh, they did sometimes. It didn't do them any good?
(laughter)

SM: I was told that when you got up to bat, you could only bunt.

ER: Well, I'm pretty sure that's true. (laughter)

SM: Some people have a lot stronger recollections about this than you do!

ER: I think some people must be remembering things I don't remember! I'm left-handed--I remember that I always faced the other way, and batted, and then sort of tried to get to first base. (laughter)

SM: That would be good for psychological intimidation of a pitcher, to have a southpaw.

ER: Oh, I'm sure I scared them all to death! (laughter) Who told you that story?

SM: Fay Lawson remembers distinctly that you were at times in the infield, that they would never let you play any crucial base. (laughter)

JS: Memories linger on!

SM: Do you recall your being one of the first people to get a grant from the AEC to work with radioisotopes?

ER: A pediatric hematologist, by the name of Louis K. Diamond, had discovered the Blackfan-Diamond syndrome, a kind of anemia somewhat like some of my mutants. I thought I ought to get in touch with this researcher, so in the early '50's, I went down to the Children's Hospital, and got in touch with Diamond. He got seriously interested in our mice and came up here for quite a number of summers. By this time, we had discovered that these anemic mice which we were studying

were very, very radio-sensitive. You asked about support for research. I'm trying to think exactly when I started getting American Cancer Society funds for the research on the action of deleterious genes. Then Diamond convinced me I should also apply to the Atomic Energy Commission, which I did. I think I got my first support there by '53. All of these things just went on and on. But the distinction among them of what was being supported by which grant shifted a bit with time. It's been very good to have three different kinds of support. Deadlines came at different times. And you didn't get absolutely panicky, because there was always--

JS: Everything didn't end at once.

ER: Everything didn't end at once, that's right. And probably because of that connection, I've had a lot to do with various responsibilities. I've been on study sections for all kinds of NIH things, but also for AEC things, being one of their early recipients.

SM: Because you were the first one at Jax to use these radioisotopes, you were across the way or something?

END OF SIDE ONE

ER: ... At that time we were very much aware of the dangers of low levels of radioactive waste. Many people at the Lab felt that having isotopes around the Lab might lead to unwanted mutations. "We're not going to have anything like that at the Lab!" I could see that having labelled precursors would be essential for my experiments. So finally the place

where we were allowed to import and work with radioactive substances was in a greenhouse. Now why the Lab had that greenhouse, I don't know, but it was temporarily empty and available for rent. They said OK, we could start in that building. All sorts of funny things happened out there. There was the night when we were out there, and we had been working with Iron 59 label, which we injected and collected and counted what had been incorporated in blood formation. We had been looking at this bright red stuff all evening. It was in the middle of winter, January. When we finally collected our stuff up to leave, went out, and looked up at the sky and there was the reddest aurora borealis I have ever seen, and I said "Oh look!" and fell over backwards into the snow. I don't know if Fay told you this story. We all, of course, had to have radiation labels and wear them all the time working in the greenhouse. Chai was working there on rabbits, using radioactive iodine. There was some kind of thyroid abnormality in these rabbits. He had to inject them, collect counts on I-131. His radiation badge was sent in, just like the others. When it came back, they said, "Is that man still alive?" Apparently some feces got into the lab coat pocket and they radiated his label very successfully. (laughter)

SM: Can you think of other amusing anecdotes, or memorable events, or memorable mishaps?

ER: Memorable mishaps? There were a lot. This is an interesting story to me. You perhaps know Janet TenBroeck.

Well, this is about her husband, who came here after his part of the Rockefeller was closed down. He came here and worked as a virologist and pathologist for some time. While he was here, we suddenly found in our colony, where all the different strains of mice were living in the same place, that C3H males only experienced some sort of extremely serious reaction. We didn't know if it was a toxin or a virus or what, but just the males were getting extremely pale and dying. And so we had to find out what this was, and meanwhile, we set up all kinds of barricades. It turned out that someone had let chloroform out into the room, and the kidneys of C3Hs are excruciatingly sensitive to chloroform. We didn't have a fatal infection after all. But for a while it was really very weird.

JS: As the Lab grew during this time, in the grants and so forth, did the working atmosphere change substantially, with increased growth?

ER: Yes and no. One thing, it became clearer and clearer that, if we were going to both get good results in our research, and provide mice to other people, particularly the latter, we had to be a lot cleaner. I must have served on a million different committees about improving husbandry, and cleanliness, and the way the cages were washed. We also felt that an absolutely essential thing was to maintain genetic integrity. We had to have exact records, with ear-punching. People from Animal Health felt you had to have it very, very

clean, and tended to use clean non-inbred mice for all of their studies. They had not developed ways of working with mice which had to be ear-handled. There has been a great increase in combining together, improving both top quality genetics and top quality husbandry. A lot of work has gone into developing good relations between the research personnel and colonies and the animal resources activity at Morrell Park. In the Main Lab, the same individual usually weaned mice, and made up new matings, and kept records, and, at the same time, was doing more about counting the mice, and so on; they weren't changing the mice in the boxes. Somebody else was just changing the boxes, so at the Main Lab there was more of a distinction between the box changer and the research assistant. In Morrell Park, there was not as large an amount of record-keeping, but there was record-keeping and the same person was changing. People who aren't thoroughly familiar with the Lab still are saying, "Well, you must know so-and-so and so-and-so; he's at the Jackson Laboratory." If he's at Morrell Park, I may never have seen him! But fair ways of dealing with the differences have been developed. One newish development at the Main Lab, which I think is just absolutely wonderful, that keeps the various aspects or research in the Main Lab in contact with each other is different types of discussion groups. I think those are just great. I think we're very fortunate to have Barbara, because she had research experience. She was in immunological genetics,

getting her degree at Brown. She sees the importance of doing these genetics things. Of course, Earl saw the importance of doing the genetics things, but not always the physiological ones.

I really do want to show you that by the time of the 50th Anniversary, when I'd be asked to speak about the development of the Lab, I could say with pride that the diversity of research carried out at the Jackson Laboratory had increased very much during the time when everything was also growing so fast.

SM: Did that ever create any tension between the mammalian geneticists from the other scientists?

ER: To a certain extent. The geneticists could see always the importance of maintaining a connection between research geneticists and the supply of animals to outside people...genetic responsibility. Maybe I sensed this more than other people, but I think that it certainly is true. We didn't want to have the animal supply get too far separated from the geneticists on the staff, because, you know, we're the ones that will see to it that the genetic control is done right. A biochemist might consider animal supply as something that was there in order to provide funds. Geneticists don't feel that way. I'm sure this doesn't pertain so much anymore, because the biochemists have become more genetic. A biochemist might say, "Oh, why don't you just set Animal Resources up as a non-profit corporation that

supports the Lab?" Well, that would only work if there were somebody to be sure that the motives of this corporation were not just to make money. Another tension is "I'll never catch up with the molecular biology thinking. I can't understand all that, so I'll keep on doing something different." I'll tell you something I worry about. I've worried about it for quite a while. It's not just right now. The contact between COA and the Jackson Lab is complicated by the fact that there's a different outlook for ecological-type and evolutionary research, as against "inside the animal" work. But that's not just a Jackson Lab situation...

SM: When you usually worked at the Lab, when did you think it changed most, or what event changed it most? Was it the fire? Was it Earl--

ER: The fire was awfully important. I don't think there's any doubt of that.

SM: Was it Prexy's retirement? Was that significant?

ER: Well, Prexy's retirements was important. Prexy was informal and he'd change the organization of the Lab over and over again. Earl was almost too much the other way. He was exact. Now you might think I didn't like Earl's way. I liked it very, very much. I think it was important that there was somebody like him, and it's possible that some of what I was describing about the different attitudes of geneticists and--"biochemist" is a code word almost--was exaggerated by Earl, who particularly didn't think in biochemical terms at all. And he tended to not realize

how much lab space, and how much lab equipment, was required for this different kind of research.

SM: But there was never any question of--I mean, he never tried to close it down?

ER: Oh, heavens no!...

JS: But did the atmosphere of the Lab, as a working environment, vary significantly with the four administrators that you worked with?

ER: That's a terrific question, and I doubt if I personally can give a whole answer to it. In a way, there were 5 people. There was a time when Prexy was directly leading everything; this was the time of inspiration, which lasted up til the time of the fire. Things changed soon after the fire, when Little was doing less of the direct contact, and Bill Murray was doing more. Bill was a worshipper of Prexy's; maybe this was sort of a standstill time. And then when Earl came in, there was much more fixed, written down. We had manuals, operating guides--you've heard of MPAP?

SM: Yes, MPAP.

ER: Manual of Policies and Procedures. It got so, it was 3 thick volumes. I don't think there's any answer for administrators, I really don't, but there is the problem that, if you change a rule to fit one particular new circumstance, it's going to wreck three other things along the way. And if you have it immovable, when and how do you change administratively. Prexy worked by changing the whole

system, but the Lab was smaller in his time. Everybody knew about everything.

JS: You didn't have to account to a federal grants office, and all those things.

ER: No, no. Maybe we adopted personalities at particular stages so that they were somewhat exaggerated. They really were very different. After the long time when it had seemed that no enough attention was given to providing research facilities, Prehn came. Somebody told him that what they wanted was a scientist, somebody to lead scientifically, and that there was too much routine. Well, apparently that was all that he heard, because he was ready to change all types of things. He was not particularly concerned about the responsibility aspects; he was not genetically minded. He certainly did not object to people doing research, and working that way, but he didn't want to devote an awful lot of his own time to just seeing that it worked smoothly. The enlargement of the Lab that happened during his time suffered from not getting sufficient advice from enough staff early enough. There are some defects in the additions to the Lab, that didn't need to be there. Of course, some complication is inevitable. Anybody who's been in a hospital knows what that's like. (laughter) It just goes on and on getting more and more complicated. You're lucky if you can find your way through it.

SM: I'm lucky if I can find my way through Jax. Every time I

go in, I feel I should be Ariadne with my string, to find my way out! I think it really will be thought of as, like Topsey, it just grewed.

ER: That's right. And to see some of the stirrings that were going on, where people were trying not to have so and so happen! You don't hear that kind of problem any more.

There's getting to be more actual common sense to it.

(laughter)

JS: Has there been a sense as the Lab grew, as there is in most academic and research institutions, that the whole administrative structure is the tail that wags the dog?

ER: Yes. And none were big enough--I'll say something about core grants because I think they have something to do with it--but now, I'll try to answer Judy's question. There are certain rules--I guess I'll do it about the importation of animals, that has had an extremely interesting history. Back when there was the Mouse Club, before there was any Jackson Lab, people all exchanged mice, and they went back and forth, and all around. We could see when we imported mice after the fire, we were taking them from a whole lot of different places, and we had a whole lot of diseases in the Lab. No bedbugs after the fire, but, as it became important to have clean animals, you couldn't afford to get, for example, ectromelia--ectromelia virus is one of the worst. You know Jane Barker? Her first job at the Jackson Laboratory, just as she graduated from the University of Maine, was to import

some mice from England--there were about five different genes. I know I was to get two or three out of these five. And England, the country, had endemic ectromelia. It's like chicken pox and almost anything endemic, if you have it, the majority of mice have been exposed to it, and don't have much sensitivity to it. Could we import mice from England? They might be carrying ectromelia virus. What Jane was asked to do was to receive these mice down at her parents' cottage--they had an extra place she could fix as a lab--put laboratory mice with them, to see if anybody got sick, raise mice down there, test them in many ways. Only after tests that lasted about three months could they be brought up here. Well, we never got any ectromelia from that importation. Now, we don't take anything in. We might get, not just ectromelia, but other things. It's true. It worked, but the question of choosing between genes that seemed absolutely essential for certain types of research, and getting the possible infections is really very serious business.

JS: ... Has the administrative structure gotten too large and complex?

ER: OK. Alright. Protection of background stocks is an example. It started by putting Foundation Stocks over by Highseas, and then sending them to the Pedigreed Expansion. They did that for years and years. It got so that the way mice were taken care of at the Main Lab was a good deal better than the way they were taken care of at the Foundation

Stocks, and the question was "How can we improve this?" It used to be that the space of a mile or so between Foundation Stocks and Pedigreed Expansion was considered crucial. Now they're probably going to move the Foundation Stocks into a much better maintained building right on the main grounds, because they know how to do it. Among the things that are very important about the mouse supply, and the role of the Jackson Laboratory is, that when there is something new in genetic needs for research, then it's kind of up to us to find a way of meeting that need, and examples are mutant genes. So you have to establish a supply, but the mice usually have to come from the Main laboratory. To get mice from the Main lab to Animal Resources required sterile caesarian derviations. It was something. It was hard. Now, they've got it so everything is top-notch. Larry Mobraaten maintains frozen embryos of most strains. That's the only way you can do it, to get everything lifted up to top quality.

SM: Are scientists sometimes irritated though, with the paperwork and red tape?

ER: Of course. I will say, I think the younger guys somehow or other get the appreciation of it built into them now. Maybe it's just history, but it doesn't seem to me to be the kind of a problem it used to be. From this business of everybody trading mice, and having cats that spread tapeworms, we've gotten to the point where, for a long while,

if I was on a site visit, I could not go into anybody's mouse room, to avoid staying out of Jax mouse rooms for a long while. And anyone coming into the Lab, had to check in--you still have to check in--and you either became a red visitor, in which you were very limited in where you could go, or a green visitor. Well, now, the restrictions for red visiting are not so great, they are fair, but not so great, and there's a new class of yellow, where you can go almost every place, and there's green, which is OK. This is much easier to live with, partly because other people know, and partly because we don't have quite such ridiculously restrictive rules. I don't know exactly why or how all of a sudden, but mutual understanding, and mutual improvement, I think, has to do with it.

SM: What are some of the rewards you've had, or frustrations you've had working at Jax?

ER: Well, it's been a lot of fun. And I get a great kick out of the number of young people that I've had something to do with training. The summer before last, or was it last summer?--anyway, they had a time when people were back.

SM: There was a summer reunion last summer.

ER: That was absolutely wonderful. I had ten returnees, including quite a few staff members, you know. Well, to me, that's always been a great satisfaction. It's obvious to me--people who don't know too much about the Jackson Lab, or mammalian research, or scientific research in general, think

of research as being a solitary activity. I'm pretty sure that a fair consideration in my mind for keeping on coming to the Lab is the many people. And opportunities for travel. I certainly think it's true that women have been particularly well received in mammalian genetics. We have a good satisfactory situation.

SM: Do you recall your first meeting with Fay Lawson? She recalls it very vividly.

ER: I don't think I do.

SM: She thought she was coming here to work with Meredith Runner. And then somehow between the time she'd come up and spoken with him, and was taken around, that you--

ER: I had a place for somebody, and Meredith didn't, I think. That's right. This was in the Bill Murray time.

SM: So, she's sitting there on the bench in the lobby, waiting for Meredith Runner, and she sees this woman walk by, walk up, and with a white coat, but all full of holes, and she thought it was the cleaning lady. (laughter)

ER: Oh, that's possible!

SM: And then you sat down and started to talk to her, and she immediately perceived that it was not the cleaning lady! But she had no idea who you were, and you introduced yourself as Tibby, and that didn't give her any clue either.

ER: I can imagine she was really--that was something, this brings up something that I have found. Have you talked to either Ede Kent or Jan Southard yet?

SM: No.

ER: They're not sure that they have things to say. They have a great deal, and you will enjoy them. They were long, long time research assistants, in the Inbred Nucleus days.

Eleanor McFarland is someone from this time, and Ede Kent and Jan Southard also. You have to go over to West Tremont to meet with them. This group of people, most of them women, but not all, were just very good close friends, and we had lots of picnics and parties, and Christmas get-togethers, and that sort of thing, that were really very good, and if you want to get these two women--see they're both retired now--ask them about the Hoxie party. This was in the days of the dominance of Bill Murray. There was a mouse worker who thought that cancers were caused by exposure to rays that were emanating all around. Hoxie sort of had ideas like Wilhelm Reich, orgone energy, and he thought that people needed to be sheltered from this, in order to keep from getting tumors. NIH knew that this was ridiculous, but they wanted to have an experimental demonstration thereof. We were characterizing mice, all their tumors, and life spans etc. If we put in another group, who were kept shielded from orgone energy, maybe they'd be different. Bill Murray accepted the challenge. Boxes were lined with materials that kept the orgone out. There was absolutely no difference attributable to orgone. When we finally knew that this was going to come out alright, we had a party in which everybody acted out

different parts. Bill Murray was the sort of head of the site visiting team--

END OF TAPE ONE

JS: I guess that raises the question "Was there any point in the Lab at which people seriously considered moving into a more normalized mode of operation as far as it became more specialized?"

ER: Well, not really. I'm not sure I'm answering the question because I'm not sure how it really works. Each Director seems to have either an advisory council, or this, that or the other, and there are some responsibilities that people are paid for as part of their job. It's very interesting--we still have a Director of Training, and we have the Assistant to the Director who sort of works with a lot of things, and a number of people are on committees. I'm still on the committee on genetic resources, and that sort of thing, but either it's something that you really have as a job responsibility, or something that's sufficiently advisory so that you need practical help. I started to say something about the Director of Training. There's one thing that is quite generally true, and certainly true of Dr. Prehn. You know Rob Shea? Well, Rob is thoroughly nice, a thoroughly responsible guy, who is in charge of all of the doing of things about the training program, but then there is a staff member who is Director of Training. Rob is very responsible, but he doesn't decide the ideas. He doesn't want to. That's

just his way of being.

SM: I've interviewed other staff, the science staff. For them, one of the rewards of being at Jax is not having people isolated in certain areas--

ER: Oh, I think that's very important myself. I don't think we can get much bigger. If we were, we'd have to have other ways of organizing.

SM: For you, what would be an optimal size?

ER: Certainly no bigger than it is. I started to say a little bit smaller, but I'm not sure that that's true.

SM: It has about 40 people now.

ER: I certainly do not think it could go above 45. Of course, there is the problem--and this is why I say a different form of organization--there is a--I think it's going to work itself out, but there has been a problem of whether to have a place related to animal supply somewhere else.

SM: "Somewhere else" meaning geographically--

ER: Geographically. That's the chief objection to that is that you should be having research-minded geneticists associated with any such place, so if this is another institute, how do you control it, etc.?

SM: Why should it be in some other geographic place?

ER: Well, one, is protection. If one place got blasted out, there would be another place. I don't know how UC, San Diego works. It has separate colleges within colleges within one

university. What is the relation of all of them to each other? Are they more alive than they would be if they were spread out like the campuses of the University of California? How do you manage something when it grows big enough so it can't interact appropriately? Does it make a difference whether they're right next to each other? I think in terms of the relation of genetic research with the question of responsibility of supply to other places. That probably just comes naturally to me, but I believe it's important. I remember going to a U.N. International Commission on Laboratory Animals, in Paris, in '58, I think. This was sort of at the height of "we must have all the labs clean" and "we've got to have them all genetically correct and have animals all over the world as much related to each other as possible." At that time they weren't sufficiently concerned about the genetics. And way back there, when we met in Paris, improvement was certainly just beginning. By the present, new genetic developments like congenic lines and recombinant inbred lines must be supplied. You have to have a geneticist, or a genetic appreciator, in every one of these activities.

SM: Do you like the direction the Lab is going now, as far as you can perceive them? Are there things you would change?

ER: I like it, but I'm not sure that I'm completely unbiased in my liking. Barbara manages the genetics so well that I tend to think it's right. So I'm happy. You haven't talked with Joan Staats yet, have you?

SM: No, I was to do her yesterday, but now I'll do her next week.

ER: Yes. Well, I think she has some doubts. She is a little bit more someone who sees a way that worked, and is not sure another way will work. A very good friend.

SM: I was told that they used to have at the Lab something called the Otter Creek--

ER: Chowder and Marching Society! You have to go back! To get the background of the Otter Creek Chowder and Marching Society, you have to know that there was a time when A.A.U.W. was the center of everything! And the men got a little tired to this, so they started something called MAAUW--Men Against A.A.U.W. (laughter). And then that gradually developed into the Otter Creek Chowder and Marching Society, which had a meeting once a month, and a different guy would cook marvelous things. They also had scientific speakers every once in a while.

SM: Now is it possible for one sex to meet still separately from the other?

ER: Well, how can you prevent it? Do you mean an organized, repeated meeting? I have no idea. As it happens, I haven't heard anything about it, in complaints or anything. We have parties. We have difficulty sometimes having parties. We try to have parties in certain frameworks, and the source of division is usually Main Lab vs. Animal Resources.

SM: The Lab certainly is too big now to have parties in people's houses.

ER: What do you do when you get 500 people?

SM: Is the Lab still thought of as a family?

ER: Probably not. I don't know. Certainly not in the way it was. I don't see how it could be. There are parties every once in a while trying to get together researchers, administrators and so on. There's nothing like the month to month, but there are--and often, the parties take the form of pot-luck suppers, which is a very good way of having parties anyhow. What did I go to recently, I really loved? For some reason, I can't think what the basis of it was, but it was something that was clearly appealing to children, and there were a whole lot of people with their kids. You know the conversation pit--it turns out to be a kids' pit--it's wonderful. And everybody came, at least an awful lot of them. Oh, I know what it was! It was the most amazing thing, you can hardly believe it. One mistake was made. The mistake was that people hadn't focussed on the fact that Hook Wheeler and Ann Stevens Wheeler were having a baby, about the same time as the other two, but there were young staff members who were having babies almost at exactly the same time, and they had a huge shower for everybody, and the fathers did much unwrapping of the gifts. Oh, it was lovely! It was just wonderful, and a lot of people brought their kids to that. That was really fun, and I think there were a lot of assistants, and mouse room people there because they knew these people. In some ways, it seemed a better cross-section than many times. I guess each summer there's been an all-

Lab outdoors party, in the middle of the summer...games and that sort of thing, but it seems to me pretty good. I think people do a little more of--where there's a group that goes to Maine Racquet and Fitness, playing racquet ball, and there's somebody doing--now they're starting again baseball, and different things like that. To me, I think they're pretty good. I think there may be some people who think that there isn't enough. I just don't know.

SM: You said before that there was a lot of A.A.U.W. activity.

ER: Oh Lord, yes!

SM: How did that manifest? Did it actually have a separate chapter?

ER: There still is a branch on the island, MDI. Now we have a difficulty: There are just too many things--League of Women Voters, A.A.U.W., and this and that and the other. But when it started, I think there was a real need for it. An awfully good thing was the literature study group. I had a lot of fun with that. It was not exclusively Lab. But Lab people belonged to it.

SM: What do you think in general have been the town-Lab relations? You mentioned the fact that this A.A.U.W. chapter had other people. Were there other ways the Lab had contact, or Lab people had contact with the town?

ER: Well, at first, they were those "queer people."

SM: "The mouse house."

ER: And then the Lab people gradually had input into the social organization, the town, churches. MDI churches have a Unity Sunday. At the last one, the number of different Lab people who were reading services was amazing. I think churches are a more eminent part of Bar Harbor's social organization than in many other places.

JS: Very, very focal.

ER: And now, I think myself, there are more organized things than the community can make full use of.

JS: In terms of population on this island, if something went Poof! and the Jackson Lab disappeared, there would be a sizeable decimation of the population.

ER: Oh yes. It's the second largest organization in Hancock County, isn't it?

JS: Tibby, if Prexy walked into the place today, what would he think of the Jackson Lab?

ER: Oh my gracious! That's a marvelous question!

JS: Would he know the place? ...

ER: He'd be absolutely amazed. He died in '71. I think he'd be amazed at the size of it. I think he'd be excited by it.

JS: Would he feel "mission accomplished"?

ER: I think he would find that the mission was bigger than even he had imagined. I think he'd like it. I hope so.

JS: Is there any place else in the world that you know of that does basic scientific research that is at all comparable in organization and how it tries to go about doing research?

ER: There was a time when I think Rockefeller Institute was.

JS: Before it became a university?

ER: Before it became a university. You don't know Peter Sellars, do you? You do. Sometimes when he talks about the Rockefeller University when everybody had lunch in the dining room, with lots of contacts among the people. They never in their early days, were as dependent on grants as we were in our early days, and they don't have an activity like providing mice. I suppose they have a fairly large endowment.

JS: They have a very active annual development... that they have to go out and raise. As Neva has said, it's hell raising money for Rockefeller University: everyone says, why do you need it?

ER: (laughter) Yes. Neva's really getting along with writing her book. She's going to get her degree very soon now, and she's writing on social economy, social and economic ethics. Very interesting.

SM: Anything else you can think of?

ER: No. I can't think so. It's been fun.

(machine turned off, then restarted)

ER: ... He [Watson Robbins] came to the Lab just about the same time I did, and this was in the days of wooden boxes. They had to be scrubbed in these big vats, and he tried to get all the shavings and feces and so on out of them, and I remember seeing him, quite a young guy, you know, scrubbing

them, banging them down upside down, to try to get everything out of them, just as mad at them as he could possibly be. In a way, you could see how that would lead to the guy being in charge of box sterilization. I think Watson's pretty good. I don't always agree with him, but I think he was a good person. Have you done John Dorey yet?

SM: No.

ER: I think that will be interesting.

SM: I think, well, he might have just gotten back from Florida.

ER: He is now.

SM: OK, because I've been trying for weeks.

ER: Hans Jorge and Helen Bunker are both leaving, and there was--we do more of this, having get-togethers when people leave, or something like that.

SM: You saw him at this retirement party? Well, I tried repeatedly.

ER: And he said something about just having returned. Let me say I think the colors in your sweater are absolutely beautiful.

END OF INTERVIEW

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Place At Desert Maine

Date 3 June 1986

Elizabeth S. Russell
Narrator

Susan McInters
for the Laboratory

The Jackson Laboratory
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