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Oral History Collection

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Place Cambridge Ma

Date 14 Dec 86

Narrator

for the Laboratory

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Interviewer's Comments

Narrator's Name Dr. David Baltimore

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.

We met in Dr. Baltimore's office at the Whitehead Institute, and our meeting was short (less than 45 minutes) but direct and to the point: Baltimore credits The Jackson Laboratory for setting him on his course as a biologist, but beyond that, he sees Jax as a dated, low-key place. One speaking engagement at Jax that left him astonished to see an empty parking lot at 5PM made an indelible impression on him and he echoes the attitudes of others outside Jax--like Law and Sprott--that Jax is not a dynamic, hot-shot institution on the cutting edge of science.

Value this tape as the candid assessment of the Lab by an outstanding molecular geneticist whose views are representative of the outsider's view of the Lab.

16 November 1986
Date

Susan Mehrtens
Interviewer's name

The Jackson Laboratory
INTERVIEW DATA SHEET

This section is to be completed by the Interviewer.

Narrator Dr David Baltimore Address Whitehead Inst Cambridge Center Main St Cambridge 02139 Phone 617-258-5157
Birthdate _____ Birthplace _____ Interviewer Dr. Susan Molinaro Phone 207-244-7353
Date(s) & Place(s) of Interview(s) 12 Nov. 1986 Whitehead Institute
Collateral Material Yes ____ No X Terms _____

Complete each of these sections as the tape is processed in each step.

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Collateral Materials Report

Narrator's Name Dr. David Baltimore

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This is the tape of an oral history interview of Dr. David Baltimore, given as part of the Jackson Laboratory Oral History project, sponsored by the Acadia Institute. This interview was held on November 12, 1986, in Dr. Baltimore's office at The Whitehead Institute, in Cambridge, Massachusetts. The interviewer was Dr. Susan E. Mehrtens.

SM: How about you tell me how you first heard of the Laboratory--was it your mother who'd been aware of it?

DB: Yes, my mother somehow, and I don't remember how, had heard about the program. My mother had contacts with the academic community in New York, because she was studying, and then became professor of psychology. So probably somehow through those contacts she heard about it, or possibly from a family friend who was a biology teacher, and said would I be interested, and I said, "Why not?" And so I applied, and was accepted, and went.

SM: Had you had biology then, at that time, when you first started?

DB: I guess I must have. Yes, I did, but biology in my high school, as in most high schools, was so awful that any relationship between that and real biology is almost an accident. You know, we learned the parts of ferns, and that sort of thing.

SM: But it didn't turn you off enough to say to your mother "No, I don't want to go."?

DB: I guess not. It's very hard to think back that far, but, for one reason or another, it didn't.

SM: So you got up there, and what was your initial impression?

DB: Well, you know, it's a very odd circumstance, to come from a suburban, very sheltered life, to be thrown together with a group of people from all over the country for a couple of weeks, or months, and to try to establish relationships and to figure out what was going on, and at the same time, to be faced with "real" biology, and real research, which meant nothing, and to come to terms with that. It was very exhilarating, very exciting, a little frightening, a lot liberating, to do something like that as a high school student. It had the aspect of summer camp, and probably, in truth, not a whole lot more, because the real scientific issues that are at stake are simply not evident. I mean, you can hear about them, and talk about them, but you don't have the background to fully appreciate them, in any sort of context. And so, it is the sheer joy of it which probably is the major effect.

SM: And the sheer joy was communicated by some of the people there? the scientists there?

DB: Yes, I mean, when you deal with people like Tibby Russell, you can't help but get a sense of enthusiasm, and excitement. The lecturers--now I can't tell you what many of them said--but they certainly gave you a sense of their commitment to what they were doing, and their excitement about it and their involvement in it, and the pleasure and intellectual challenge that it involved.

SM: Did you live in Highseas?

DB: I lived at Highseas.

SM: That has the quality of a high-class dorm, I gather.

DB: I don't know about "high-class," but it certainly has the quality of a dorm. If I remember correctly, quite a decayed high-class dorm. And, you know, we had our "den mother and father," who looked over us, and in those days, people could have discipline over adolescents. I don't know if they could get away with it today. You certainly can't get away with it in college, and I doubt you can get away with it at Highseas either.

SM: There's a certain sort of basic ground rules about it.

DB: Yes, that's what I would think.

SM: In the two years I've been working on this project, I have been to some of their summer activities, and they've usually had a "graduation" day, where the students talked, like a formal presentation of what they had done. Did you have that sort of thing, with some culmination of your work there?

DB: Well, I guess so, but I don't remember it.

SM: Do you have any recollections of a project? Did you work on a project?

DB: I worked on three projects. It was the style then--I don't know if it's the style now--that you worked with three individuals, on three little research projects. And I worked with three wonderful people--Tibby Russell, Don Bailey, and Will Silvers, on--Yes, it's incredible to think back, that I had the opportunity in high school to work with people like that. It was

just unbelievable. And I did three little projects: one in mouse genetics with Tibby, on the W locus, which I still remain interested in, although I haven't done any work on it; one involved measuring bones for Don Bailey, and I don't know what he was doing, and he clearly went on to do better; and, with Will Silvers, I don't remember what I was doing--it had something to do with pigments, skin pigments, probably genetic.

SM: And you found each of them inspirational in their own--

DB: Well, in their own way. I mean, Tibby really more than the others. As a personality, I remember her much better. In fact, I really probably couldn't have resurrected Don Bailey except that his name kept reappearing later, because he's such a "very personal person." So, you know, it reflects their personality. And I remember Charity Waymouth then, again because of her personality, and involvement, and clear sense of caring and joy, more than ...

SM: So you were clearly aware that they were committed to this training program?

DB: Oh yes.

SM: What did you take away from your experience there?

DB: I took away the most important thing probably in my life from there, which was the ability to get through the first two years of Swarthmore biology. (laughter) To mince no words, and it is no secret, Swarthmore biology was just slightly out of the 19th century, and the style of teaching of some of the people was just

atrocious, and the concerns that they had--that all the faculty had--were very much in the historic tradition, the historic traditions of embryology and physiology, descriptive or physiological, and the notion of biochemistry, or of molecular biology--it didn't have a name then--as something that you would get involved with, with Swarthmore undergraduates, was just not in anybody's mind. Enders, Bob Enders, who was the Chairman of Biology at Swarthmore at that time, said outright to me that biochemistry has no place in an undergraduate curriculum. Now this is 1957 or so, four years after the discovery of the structure of DNA, when biology was probably in the greatest ferment in its history, and yet they didn't want any part of it. They didn't want any part of it for themselves. They didn't want any part of it for us. And so, that, the wonderful excitement of thinking about biology, probably from a genetic point of view--that is probably the ultimate difference--is that none of these people really thought genetically, and Bar Harbor did think genetically, and all of biology today thinks genetically. But also the level of experimentation, the kinds of considerations, gene activity,... Tibby wanted to know, in the early '50's, how genes worked, and I guess that's what got transmitted, a concern--in a sense that there were people concerned with that--is what got me through the early years of Swarthmore biology, and then, after that--Also, going to Bar Harbor kind of got me to Swarthmore, in some ways, because I had met a lot of people. I

think--it might have happened anyway, it probably would have happened anyway--because again, my mother had very close connections with Swarthmore, because she had studied at the New School, and the gestalt psychologists at the New School were all on the Swarthmore faculty. Wolfgang Kohler had been on the Swarthmore faculty, and Hans Wallach, who's still on the Swarthmore faculty, (I saw him the other day) and Solomon Asch--all of those people taught at the New School. The New School gave them their first home in the United States when they were in exile from Germany, in the late '30's and '40's, and so there was a very natural connection. When I went to Swarthmore, Hans Wallach took me around, as a friend, and so I had a very close family connection to the school, and the school so completely impressed me as a place I wanted to be, that I had no difficulty turning down Harvard, or anywhere else, but I think probably even if I hadn't been at Jackson, that would have been true, but you can't rewrite history, so I don't know. But I did meet a lot of people. I had a girl friend who went to Swarthmore, and a number of people from the group went to Swarthmore and then Howard Temin had gone to Swarthmore, so the connections to Swarthmore were deep and multiple.

SM: So at least you had proof in Temin that somebody could emerge from Swarthmore and still be interested in biology.

DB: Oh yes, that's right. And you see, but the other side of it is that Swarthmore offers you the opportunity--it did offer the

opportunity--I am now on the Board of Managers and fighting for them to maintain that opportunity--to do better than the faculty could do for you, and that was through the Honors program. Swarthmore had/has an Honors program that's unique. It was derived by Aydelotte, who was President of Swarthmore in the '20's and '30's, and it meant, at that time, that you did eight seminars for your last four semesters at Swarthmore, the last two years, two seminars a semester. That's all. And those met once a week. And the rest of my time was spent writing papers, reading, studying, and, for one semester, doing research, and so I knew that there was a program that gave me much more freedom, and I used that freedom to study molecular biology, in spite of the fact that the faculty knew none of it. A group of us in my junior year, all got interested in molecular biology--all of whom are molecular biologists today--and we would go over to Haverford and Bryn Mawr, and use the libraries. We dug up everything in the Swarthmore library that related to molecular biology, of which there wasn't much. We wrote our papers on molecular biology, and we insisted that the discussions in the seminars follow that, and the faculty were very good about it. The other side of the Swarthmore faculty is that they're really there to help people learn, so they helped us the best they could, mostly by staying out of the way, so we got a very unsystematic view of molecular biology. We didn't know who to believe, what to believe. The whole notion of science as a human activity, rather than something on paper, was not available

to us, because there were no scientists to talk to who did this sort of thing. It was a lesson that I've never forgotten, that when people say denigratingly, "You're being ad hominem," they're wrong: ad hominem is, in fact, the way you have to be. So there were opportunities to transcend the situation coming up, and actually, I left biology after my junior year, and moved to the Chemistry Department at Swarthmore, and graduated in Chemistry from Swarthmore, just because I figured I might as well take advantage of the faculty, and learn something rather than having to teach myself for another semester or another year, and they also would let me do an experimental thesis, where Biology wouldn't let me do an experimental thesis. They didn't have the facilities for it. I mean, it was a different world.

SM: I gather it must have been updated since you were there.

DB: Oh, it's been updated since I was there, and, in fact, they're now--oh, I don't know--12 papers published last year with undergraduates involved in the research, I mean, the school has clearly taken a totally different attitude, and the faculty has also. The faculty tends to be a little behind the forefront of science, kind of inevitably in a small school, but it's not so far behind as it clearly was in those days.

SM: Speaking about "far behind," did you find the Jax people then far behind, because you certainly went beyond their mammalian, sort of classical--

DB: Oh, absolutely. I didn't find them far behind because I didn't

have anything to compare by. If I look back at it, sure. What they didn't give us was any sense of the excitement of molecular biology, because they just weren't a center that was plugged into molecular biology, but they didn't have to, at that time. I think it's been much more a problem for them.

SM: Well, they've gotten these new people. They have 5 or 6 molecular people on the staff now. Why do you say "much more a problem," because the whole trend in biology has gone in that direction, and they would just become obsolete?

DB: I believe that now there is nothing but molecular biology. I guess I take a harder line than some people, and the reason for saying that is because molecular biology has developed the power, in the last ten years, to investigate any system, whether it be mice, plants, neurobiology, microbiology, anything. So, we at MIT have a very lucky situation: We have only one biology department (actually we have two: one's Applied Biological Sciences, but it covers the same ground), and that means we can--it's all molecular biology. It has been since the late '50's, and for a long time the department was considered very narrow. The world considered it very narrow, because it didn't concern itself with evolution. It didn't concern itself with embryology. It didn't concern itself with plant biology. It didn't concern itself with a whole lot of very important elements of biology, and the reason was because somebody decided early on, and very smartly, to focus on molecular biology. But that's all changed in the last ten years.

We do have plant biology. We do have embryology. We do have all of those things, and the reason we do have, is because they all can be illuminated by molecular biology. They fit into the same framework of thinking. Today when there's a seminar, nobody says, "I'm not going to that; it's outside my field." There is nothing outside your field. You may not go to it because the approach is one you're uninterested in, or because the particular problems are not exciting--or you're too busy, or for a variety of reasons, but now no longer because it's irrelevant. I think anybody who's smart today sees that all of biology is relevant to all the other aspects of biology. That means an institution dedicated to a corner of biology, whatever that corner be, is missing out. A friend of mine asked me one day, should he go to this new institute that was being set up, and I said "No, don't go to any dedicated institution, because a dedicated institute is simply too narrow. It won't attract good people, because they want a broader area to look at, and it's a misreading of what's going on." And I think Jax has suffered from that. I think they're trying to get away from it, but it's very difficult. Difficult to undo history, probably because the strength of the Laboratory, the raison d'etre of the Laboratory, focusses on its history.

SM: Well, it had this sort of mission from the time Little set it up, in genetics.

DB: Right. And that was something extremely important. But now,

we have mammalian genetics here, along with yeast biology, and Drosophila biology. We have ... molecular biologists, and whatever else, and it fits in perfectly comfortably. We have guys up on the fourth floor here who do lots of mouse crosses, who do--there's almost nothing done at Jackson that couldn't be done here today, although probably not on the scale that Jackson can do it.

SM: Why, because it has all these mice?

DB: Yes.

SM: In that respect, do you think it's playing an important function for the scientific community?

DB: It is, in providing mouse strains, and in maintaining surveillence, large-scale surveillence of mouse colonies, and picking up oddities; both of those things are extremely important, and one of the great difficulties in working with the mouse is the very small number of mutants that there are, relative to the overall genome. And getting more mutants is really critical. But that may change, and may have changed already.

SM: Some of the scientists there even wonder if in ten years-

DB: Because the ability to make mutants is coming.

SM: Bioengineering, right?

DB: Well, partly bioengineering, partly--Vernon Bode, in Kansas, I guess, has realized that, with the right transplacental treatment with EMU, you can get about one mutant mouse at any given locus, out of 2,000 progeny, and that means that's it's

conceivable to screen for specific mutations now, or for groups of people to get together, and do a large-scale mutagenesis, and then look for ten or twelve different kinds of mutants. That's much more productive than waiting for them to come before your eyes, and secondly, looking for mutants visually, which is usually what Jax has been doing--coat color mutants, or deformities, the things that waltz or whatever--you're looking at a small spectrum of the overall possibilities. Particularly what you want to get is developmental lethals, and those have to be isolated in animals by breeding... but then there are the directed mutagenic approaches, and there are mutagenic approaches using viruses, which have a tremendous strength, if you want to get the gene out again.

SM: People have said to me that they find Jackson Lab to be geographically isolated.

DB: Oh, it is geographically isolated. You're a Maine resident, so maybe it doesn't bother you so much, but I would think there are only certain kinds--I had a very interesting thought. The last time I went to Jax, I guess one spring or something, anyway, it was not summer, and I gave a lecture. I had driven a car in from wherever I'd arrived, at Noon--whatever time I arrived--and the parking lot was full, and I found a space, and I gave a seminar, and right after, I came out, we talked for a while, I came out from the seminar which was--it wasn't late--roughly 5:30, or whatever it was, the parking lot was empty. That clued me in to start asking people what is the rhythm of life at your laboratory?

Well, the outside world's view probably would be that the rhythm of life is one of intense dedication, and 24-hour a day commitment, because what the hell else is there to do in the winter? Not true at all. Just the opposite. We're more dedicated here, in the urban environment, with all of its opportunities and distractions, than the people are up there.

SM: Well, in the sense of--

DB: ...time, I'm not talking about quality or I'm not really denigrating anybody, but--

SM: Yes, it's much more a 9 to 5 mentality.

DB: That's incredible! As I say, I would have thought just the opposite. So the values of isolation are not obvious, is what I'm saying.

SM: Oh, you find that there are values to isolation?

DB: No, I would have thought that one of the values I see is the ability to avoid all the other distractions and become totally involved, but that is--

SM: No, the distractions are just different. The distractions are the cross-country ski trails, or horseback riding, hikes in the mountains, or--

DB: Wonderful, I agree, and I try to do those too, but I would think that the level of distraction in Boston has got to be higher than the level, the potential level of distraction. So it's a matter of selecting for different kinds of people, and--So I don't think that's particularly good for science, and I think it's a real

problem. I would never--to put it right out on the table--I would never suggest that any of the people here go to Jackson Lab, and it's simply a matter of style. It's that experience that I had that made me realize that they are looking for a different kind of person than I want to create.

SM: It's a very special person that they recruit, and they have said that in multiple times on these tapes, that when they recruit, they have to be very pointed in asking a person how they live, and what they want to do. They have had people who come and didn't know that, and were there about a year, and were climbing the walls.

DB: Absolutely. That's right. But, the other thing, and this has got to be paramount, is that science is a competitive world today, and the people that spend more time at it, do better. The people are better organized, committed and more involved. And so I think that they probably were in the earlier parts of this century, selecting for a rare group of people who had a love of a system, and could do more with it, on a 9 to 5 basis than the world was going to do on any basis.

SM: They are aware of this too. They talk about the grant world, and federal financing, and cuts in that, so they are aware of that. And I think it was partly that that led them to bring these molecular people on. Whether they'll keep them is another thing. I myself have interviewed several of them, and they seem very ambitious hot-shots, and whether they'll stay there is questionable.

So what would you say, as you see it, is the future of the Jackson Laboratory?

DB: I don't know. I don't really have the answer.

SM: Do you think mouse stocks will be important?

DB: Mouse stocks are being important. There's no question about that, but, as they become more important, more and more people will have them, or will be able to derive them, and they will become less critical, and the institution will become less critical as it becomes more central to what's going on. Their recombinant inbred strains, which are an enormous resource, and need a lot of people to work with Ben Taylor--one of these days, some company is going to put out blots of recombinant inbred strains, and you won't need Ben Taylor, you just order a blot. You'd have all the DNA set out for you; and you'll be able to map right in your own backyard. But when you have to use animal-level traits, that's not true.

SM: You say when you have to use animal-level traits, you mean the actual animal.

DB: Yes. I mean, if you are measuring coat color, rather than a DNA polymorphism, then you really need the animal there, but once we've cloned a lot of the historic mouse genes, we'll be able to identify the genes and their alleles using molecular techniques, and we'll never have to look at a mouse. I'm not saying that the mouse is going to become unimportant. When I built this institute, I devoted most of the second floor of the institute to a

mouse colony, and that's gotten very large, and it's going to get a lot larger.

SM: Why do you think the mouse is important?

DB: The mouse is important because it is the experimental animal closest to human beings that everybody has focussed their attention on.

SM: And you don't see a day when you never have to have the body of the mouse.

DB: No, I don't see that day. At that day, biology is dead, because biology is the study of animals and types of organisms, not the study of molecules.

SM: I've heard people at the Jackson Laboratory say that.

DB: No, but it's true. It's true, and I find--I mean, I've talked to a lot of people about new biology facilities because we built this building, and I've become a great guru of facilities, and I find most people who have not been exposed directly to the tradition of mouse genetics underestimate where the future's going to lie, in terms of the needs of animals. That's my belief. The funny thing about biology is that it's gone back to animals. For a long time, we didn't need--for a long time, Jackson Lab was in a world unto itself, because the serious issues of biology were being fought out in bacteria, in viruses, in very simple systems, and that's no longer true, and it's never going to go back to it. So, I fully believe that what they're doing is important, what they've done is important, but whether they have created an atmosphere

that will be appropriate for exploiting the opportunities five, ten, twenty years from now, is where I have a significant doubt.

SM: What would an atmosphere--how would you have to change it?

DB: They would have to be much more high-powered in their general outlook, and have a really strong group of people who can bridge those disparate disciplines.

SM: The NIH people said the same thing.

DB: Oh, alright.

SM: It's interesting. When I have left the island, and asked people who either worked at Jax, have done reviews of Jax grants, and study sections, or other people that stand on the sidelines of genetics and watch, they all say the same thing, and the people at Jax say things like "Well, we have an opportunity to integrate the classical approach with the new approaches," and that sort of is where their thinking lies. Whether they will do that, I don't know.

DB: They have the opportunity, but they've got to look at what it takes to do it.

SM: Right.

DB: And simply wanting it to be is not sufficient.

SM: It sounds as if Jax played some early initial inspirational role in your life, in terms of science--

DB: Yes, it did, in those terms. I feel an enormous gratitude to the institution for that reason, and credit it for that. I've been very happy that they try to exploit my association with the Jackson Lab

because I think that's so.

SM: Have you ever sent people to the Lab for some of their programs, or--

DB: No, I haven't, partly because I don't have an opportunity to send people to summer programs in general, but I would be more likely to suggest they go to Cold Spring Harbor, as I did, for instance, with a student this year, than to go to Jackson Lab.

SM: How do the two institutions differ?

DB: Well, Cold Spring Harbor is an institution focussed on molecular biology. It's almost a founding home of molecular biology. But Jim Watson, who doesn't miss a trick, saw a long time before most people, that animal biology was coming up, and built himself a good animal quarter, and has a serious mouse program, and, in fact, they run a better course in mouse biology there than Jax does, because it's imbedded within molecular biological considerations.

SM: What do you think Jax will be remembered for? It's hard for us to say now.

DB: For developing the fundaments of mouse genetics. I think, with no question, the development of inbred strains, the maintenance of them, the insistence on rigor in their derivation and maintenance, and the identification, characterization of mutants, and the very early realization of the power of mutants to understand animal physiology. You know what's coming back, in a funny way I see more references to it every year: When I was

there, there was this funny thing going on in Ellsworth or somewhere, of animal behavior studies by Fuller, right?

SM: Oh yes, Fuller and Scott.

DB: Fuller and Scott. OK, and they had barkless dogs, and they had rabbits, and they were trying to do the same thing. They were trying to do the genetics of behavior, and people now credit them with having been very far-sighted, and still refer to that work as the most up-to-date work that exists, although I guess they've abandoned it for many years.

SM: Oh yes, it was abandoned in the '70's.

DB: But nobody ever did better.

SM: I knew that it was pioneering, in the work that I had done preparing for my interviews with Fuller and Scott, and it's been astonishing to me that the Jackson Laboratory has so little regard for its history that it threw out every scrap of paper that relates to that.

DB: Is that right?

SM: Everything! I mean, even the plans of the sewer and water lines, so that when they sold the property, the people had to have everything resurveyed.

DB: Well, I think, it's a matter of the times. People just did not appreciate--genetics is the most abstract part of biology, and the part of biology that's hardest to get through to students. It's hardest to get people to understand. I mean, it's on everybody's tongues today--genetics, but I don't think they

know what they're talking about when they talk about it. And they certainly have no idea of its complications. So, to do the genetics of behavior--an impossibly difficult science wedded with an impossibly soft science--who would take it seriously? And it wasn't. And I think they were whistling in the wind. They were just doing their own thing, although people clearly acknowledge that it existed, because lots of people recognize now that--

SM: And a lot of people cruised through it. If you look at the names of people who went through the program, especially summer people--they had a summer course and all that--in terms of behavioral genetics, everybody cruised through at one time or another. It's amazing. If this project were open-ended, and money were no issue, I would have another dozen people to interview who were connected with that side of it. I've only done--

DB: Where are Fuller and Scott today?

SM: Well, both of them are retired. Fuller is now in York, Maine, and Scott is in Lexington, Massachusetts, with a connection to Case Western Reserve in Ohio. I think that's where he went next, after he left Jax, and the other sort of "alum" of the program is Dick Sprott, and he's down at NIH, in the aging division of NIH. I've interviewed all three of them, and they have given me some material from their files, because I told them that everything had been thrown out. But, yes, that was an ambitious program, and apparently the work that they did, their monographs and books, are

still classics in their field.

DB: That's what I understand. I haven't gone back and read them. I may want to.

SM: C.C. Little had a shotgun approach, apparently, in the sense of, the mouse was the core, but he had all kinds of other animals, and he wasn't adverse to having these dogs, and cats, and rabbits, and all kinds of mammals in the Laboratory, and they have definitely tightened up the focus of the Lab, in terms of the animals, and also, in terms of how they want to focus right on that.

DB: But perhaps they have more to offer in behavioral genetics than in anything else there.

SM: That's right.

DB: I mean, it's a very nice open-ended area in which--you'd still have to bring some very odd people into it, and which requires space, physical space. I'm not sure there's that much space around Bar Harbor anymore.

SM: Well, it's still much more open than here. (laughter) The place is changing, but not that fast! They predict that within ten years, the road between the Jackson Lab and Ellsworth will be one solid mattress, because they are building up all these motels, but it hasn't reached that point yet, but that is the prediction. Can you think of other things you might want to say about the Laboratory, its role in American science, its legacy to the future?

DB: No, I don't think so.

SM: Great.

DB: Is there anything I should be saying that I'm not?

END OF INTERVIEW