11-7-1986

Donald Bailey Oral History

Donald Bailey
The Jackson Laboratory

The Jackson Laboratory
Oral History Collection

Interviewer's Comments

Narrator's Name Dr. Donald Bailey

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.

Regarded by many of his peers at Jax as one of the most intuitive, prescient and creative scientists at the Lab, Bailey was a very reflective narrator. While he provides one anecdote of C.C. Little's sartorial habits, this tape is largely devoid of anecdotal material, and is more a reminiscence by a scientist who was at Jax in the early '50's, who left and returned in 1967.

I tried, through a variety of questions, to probe the synergy between Bailey and the Lab, the degree to which his prescience and creativity might be due to the freedom and rich genetic resource provided by Jax, but perhaps this was too nebulous or difficult a theme: Bailey never really addressed it, and, in fact, seemed surprised to learn how his colleagues regard him.

As might be expected, given his colleagues' opinion of him as a pioneering maverick, Bailey gave unique answers to my standard questions about utopian changes to be made at Jax, and the legacy it will leave to the future. Where most others looked to things like an endowment as a way Jax could be changed for the better, Bailey would redo the physical plant to create more interaction betweeen the staff.

His memories of Jax in the '50's--the small size, laissez-faire lack of bureaucracy, and "family" atmosphere--echo the attitudes of many of the "old timers."

Value this tape as a distinctly different set of views, by a thoughtful and reflective scientist.

7 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 Ronald Bailey Address The Jackson Lab Bar Harbor Me Phone 207-288-3371

Birthdate ______ Birthplace ______ Interviewer Dr Susan McIntosh Phone 202-244-7553

Date(s) & Place(s) of Interview(s) 7 Oct. 1986 The Jackson Laboratory

Collateral Material Yes ____ No ✓ Terms ______

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

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Place Bar Harbor.

Date 7 October 1980

Donald W. Bailey
Narrator

Susan E. McIntire
for the Laboratory

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Collateral Materials Report

Narrator's Name Donald Bailey

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Interviewer's Notes and Word List
Dr. Donald Bailey

Berkeley
Everett Dempster
Michael Lerner
Prexy Little
Meredith Runner
Earl Green
Highseas
Kansas
San Francisco
Bill Murray
Dale Foley
Will Silvers
Fred Avis
David Baltimore
Tibby Russell
Charity Waymouth
Don Bailey
Pete Wetstein
Wistar
Larry Johnson
Trudeau Institute
Brown
Kenneth DeOme
Elizabeth Fekete
Edward Demaeyer
Orsay
Paris
Ham Station
Jack Stimpfling
Great Falls
Montana
Bea
Prehn
Philadelphia
Morris Browning
Earl Heiser
Ben Taylor
Herbert C. Morse III

Terms:
recombinant inbred
congenic

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Oral History Collection

Interview Contents
Dr. Donald Bailey

This is the tape of an oral history interview of Dr. Donald Bailey, given as part of the Jackson Laboratory Oral History Project, sponsored by the Acadia Institute. This interview was held on October 7th, 1986, in Dr. Bailey's office at the Jackson Laboratory, in Bar Harbor, Maine. The interviewer was Dr. Susan E. Mehrtens.

SM: Let me start by asking you when did you first come to the Lab and how, what were the circumstances?

DB: I, well I first heard of the Lab from my mentors in Berkeley, where I was going to graduate school. Just before I was to get my degree, I began thinking about where I might go for a post-doc experience. My two mentors were Everett Dempster and Michael Lerner. They had heard about the rebuilding of the Lab after the fire. They were taking on new staff, and so it was an opportunity, perhaps, to do a post-doc; that's when I first heard of the Lab. So I corresponded and applied for a grant--it actually was a fellowship, and I sent it to Prexy Little, the Director of the Lab at that time, and he handed it over to Meredith Runner. We corresponded and the fellowship was approved. So I arrived here in 1953, in the Spring of '53, and I stayed then for 4½ years, until Earl Green came on as Director.

SM: Now was there any coincidence about Green coming on as Director, and your leaving or was it the fact that your grant--

DB: No. Actually my fellowship lasted--seems like now, I'm trying to recall--about two years and then I was hired by the Lab and I don't know what position officially I had at that time. It was Research Associate, I think that was what the title was: I wasn't a full-fledged staff member, but I was hired to take care of the Foundation Stocks at Highseas. It was what we called "Middleseas," then, because Highseas was the large building, and the small caretaker's cottage was Lowseas, and the mouse Foundation Stocks were in the garage, where they are now. And I became the Supervisor of that facility and actually had a lab down there for the last couple of years of my stay.

SM: Then where did you go?

DB: From here I went to the University of Kansas for a little over a year. I left that to go to NIH for about two years, and then went to San Francisco Medical Center for six years, so I was away from the Lab for a total of ten years. I came back to the Lab when Earl Green was still Director, in '67, and I've been here ever since, the longest stay I've ever had in one place has been the last 19 years.

SM: Now did you see a change in the Lab from the first time you were here in the '50's to when you returned then in the late '60's.

DB: Oh yes, very much.

SM: What was it like?

DB: In the '50's, the Lab was more like a large family. Each staff scientist had more independence, it was less organized, had less coordinated activities, less administration at that time.

Prexy ran a very loose ship and at that time we had staff meetings and the staff pretty well ran many aspects of the Lab. Many decisions were made at that time by the staff, at the staff meetings--it seemed like we had them once a month--a lot of decisions had to be made by the staff, by vote of the staff. When I came back Earl Green had really organized the Lab in such a way that the staff meeting had no meaning: It was there to inform the staff of decisions but there were no decisions made by the staff.

SM: Did you like that, or not like that? Did the early days seem to you to be so inefficient as to be maddening, or so chaotic as to be creative?

DB: Well, I hadn't experienced anything to compare it to at the time. That was my first job. One incident of interest occurred just before I left the Lab. The salaries were very low; the assistants were complaining about their salaries, but there was no mechanism to bring them up to the average in the country in this type of institution. I recall Bill Murray asking me to come up with some sort of formula for having a salary scale for the employees, merely because I had a mathematical bent in my research, doing analyses of variance, and that sort of approach. He thought I could do something in terms of employees' salaries, and of course, like a fool, I attempted to do that. (laughter) I came up with nothing! And he was wondering why I couldn't do it without it costing the Lab anything too (laughter). It was a very liberal attitude the Lab had at that time... between

Bill Murray and Prexy Little and Dale Foley, it was really a tight ship financially, but loose in regards to telling, you what you should be doing and organizing the Lab in any way.

But I benefitted the most by the attitude and philosophy of the Lab which was--the concept of independence or research, doing your own thing.

SM: Is that different now?

DB: Well, I think it is, mainly in all the red tape one has to go through, all the papers and forms you have to fill out something such as to use animals in your research. It really is a hindrance to doing things now. You can still, surely--there is encouragement to do what you want to do, independence that way, but there's always a question of: is there space for doing this? Right now I'm frustrated because I don't have help with the computer. I have purchased a computer in my grant, but I can't get anyone to do the programming. It's been over a year now that it's been sitting around. And that sort of hindrance and the need of going through committees: Before anything's done you have to process it through committees for approval, pretty much like government bureaucracy. Very much like when I was at NIH, and I suppose that comes with the Director, because that's been her experience...

SM: Right. But the early days of the Lab were quite the opposite, but you never felt that it was just too chaotic, that it wasn't organized enough or that administration was too loose?

DB: I didn't feel that was so. Of course, I was a post-doc. I'm sure I had a different outlook than staff members did, full-fledged staff members, but I liked the general attitude of the administration at that time because you wouldn't see Prexy around here, quite often he was out getting the funds for the Lab. Bill Murray was down in his office, not interfering, not really understanding, perhaps, a lot of the research, especially mathematically-oriented type research. So I enjoyed that very much.

SM: Why did you leave?

DB: Well, because I had a general philosophy of not staying in one place too long. For the first job as it was stated by scientists in general at that time, one shouldn't stay too long. I was there about five years. They still encourage post-docs to go on to a new institution before coming back, that is, if they want to come back.

SM: When you left, did you think you would ever come back?

DB: No. I never really gave much thought to that, coming back. I enjoyed the Jackson Lab but when I got away--I went to the University of Kansas, there were politics going on in the University, as might be expected, then I started realizing that the Jackson Lab was a really good place to be in comparison to that. We don't have departmentalization here. I just had no way of comparing institutions until I had left here.

SM: Do you consider the fact that they don't have departments here at the Lab a plus?

DB: Oh absolutely. As I say, politics in a university are outrageous because of competition between departments. You don't have that here. There's a lot of collaboration. You don't havemany barriers. Now we do have barriers due to increased space. I don't see people on the other side of the Main Lab like I used to-I see them only at seminars. That's because of the physical distance you have to go to visit them. We just don't go over there unless we have a specific project in collaboration with a staff member over there, or occasionally a committee meeting is held over there. It has changed quite a bit that way. As I said, it was like a family before, when the Lab was small.

SM: Because of the size, do you think?

DB: I think that's it, mainly. It's a problem.

SM: Do you remember any funny anecdotes or incidents or circumstances in the years that you've been here?

DB: Not that I recall, nothing that's really good. I recall that one instance: Summer students--Well, back in those days, Prexy had large parties, and each summer there was a get-together, a party, that involved all those visiting scientists who were here--"summer investigators" is their official title--plus the summer students, college as well as high school students, and I recall one time in particular that the students were putting on some entertainment during the party--they were putting on a skit, a musical--I think it was a Gilbert and Sullivan operetta, but it was a parody on different staff members, and Will Silvers was in that, at that time, and he was organizer of quite a bit of the entertainment. I can't recall the situation, but anyway, it was a very hilarious time. They had students performing in this and caricaturizing staff members.

SM: I can imagine what it must have been like. I just interviewed him and he talked about some songs and this just might be what he was referring to, when he was a summer student and he had these songs.

DB: Could very well be.

SM: He said he was going to record some of them. I haven't gotten the actual tape, but I've been nudging him about it. (laughter)

DB: He's most enthusiatic about the Lab.

SM: Yes, he is, indeed. Have you had summer students?

DB: Yes. In fact, when I was a post-doc, I had summer students about every summer. I was encouraged to, because not too many staff members were sponsoring them at that time. That's when Fred Avis was running the program and the students were doing all their work down at Highseas, but they were sponsored by individual research staff members. They didn't work in the staff member's lab; they worked down there, but they worked on a project dreamed up by the staff member and I recall one student at that time was David Baltimore, who was actually doing projects, for three of us. There was Tibby Russell, Charity Waymouth and myself, and so he got all these three projects to conduct down at Highseas. When he visited the Lab not too many years ago at the celebration marking our 50th year--he recalled that time, and the project he did. But since then I've had students--let's see. Pete Wetstein who is now at Wistar. He was a high school student. That was one of the first years I came back after '67. And he was a hellion at that time. He was really one of the ring leaders of the bunch and he still seems to have that attitude of having a good time. But I didn't realize he was going to get along in biology, into actually doing things in the same field I started him out in. He's still working in genetics.

SM: You can never really predict, can you?

DB: I can't think of other summer students that I've kept up with and kept track of, but I did have a pre-doctoral student, Larry Johnson. He was going to the University of Maine and got his degree there, but did his thesis work here, in my lab and now he's at the Trudeau Institute.

SM: Do you like teaching? Do you miss it?

DB: I don't care to teach. I didn't teach that much, but I did at the University of Kansas. Unless I have a student that works in the lab, and we work together on a project and this I enjoy.

SM: Would you want to see more of that at the Lab?

DB: Well, I don't see where it matters whether we have more or less teaching. It's up to the staff members, whether they enjoy it or not. If they enjoy that sort of association they'll do it, but I don't see where it should be mandatory. It's nice to have young people around, and I am for encouraging that as much as possible, but not to put pressure on people to take students, which sometimes happens around here.

SM: For summer students.

DB: Yes. The argument is that we must have a critical mass.

SM: I ask the question about teaching because several people have said that they think the Lab would be a better place if there were more students--I guess more young people--more new ideas, questioning of "Why do you do it this way, as opposed to that way?"

DB: Well, they certainly have the opportunity to have them, if that's what they miss. They can fill their lab with students. They can take all they want. I don't know why they miss this.

SM: I'm not sure they mean summer though. They're talking year round. Some people have wondered if we couldn't get liaisons with schools--like Maine, but the number of students that come from UMO seem to be few, in terms of having a pre-doc--

DB: Yes, well, we certainly have tried that at different times. We have been in such a program at Maine and also at Brown, and it doesn't work out, because the faculties of those schools have first choice and they hold on to the students. Unless a student hears about the Jackson Lab and insists upon coming here, they're not going to be coming here. That's the way it works. We did have academic year students who come here during other parts of the year than the summer. So that's a possibility for those staff members that don't have the full-fledged student they want.

SM: How has the Jax helped you develop in your career? What do you think, in terms of the atmosphere of this place-has there been some synergy between it and you in terms of what you've done?

DB: Well, yes, the main thing has been the concentration of mammalian geneticists in one spot. I was at the University of California Medical Center, there in San Francisco and I was about the only one that was interested in mammalian genetics research, and it was a large institution. I did go across the Bay to Berkeley occasionally because there were some mammalian geneticists there, some of them working with rats and others with mice. Kenneth DeOme was there in cancer genetics. In fact, I heard about the Lab first from him... I used to go, as a graduate student to work in his lab. I went over there to learn some histology and pathology. I was working on my thesis project with rats at that time. In a building nearby was Ken DeOme who used inbred strains of mice and he mentioned some of the people working here, especially Elizabeth Fekete, who was a histologist working on cancer. So that's when I first heard about this place actually. To return to the subject, when I was at the University of California, San Francisco, I'd make trips over to Berkeley to attend seminars and associate with mouse geneticists. But therefore, I really felt isolated there in San Francisco even in a large medical school. I didn't have anyone to discuss my research with. So coming back to the Jackson Lab was coming back to a place where I could actually converse with people with common interests, as well as asking advice outside my own specialty, but still--the mouse being the common denominator, so that certainly was an incentive to come back here, as well as being able to do research without teaching and without faculty politics. At the University of Kansas, I had a taste of teaching and it was really not an enjoyable experience, in the sense that I had to spend a lot of time preparing for the lectures, which were in an area I wasn't terribly interested in, and it seemed like a waste of time. I enjoyed having students around, but I didn't like to prepare for lectures, and doing full-time research is what I delighted in, and if I have students working in the lab, great. I was interested in coming back here. Also because I knew a lot of the people. It was very familiar territory. However, there was a problem in going from California to Maine. I enjoyed San Francisco very much: It was very difficult to leave San Francisco. But then I enjoy the outdoors, so--skiing, ice skating were plusses for Maine.

SM: Yes, you have to be willing to take the winter here. They don't have the winter there. Your colleagues here have told me that you're probably of all the various scientists at the Lab, the one that's most out in the forefront of the future of genetics, and I was wondering to what extent you found yourself stimulated, with the support of other mouse geneticists, to go ahead like that?

DB: I don't consider myself out on the forefront at all. The forefront these days is in molecular genetics.

SM: Oh yes. I don't think they're thinking that way. I think they're thinking in terms of intuitive insights into what will be useful, or what will become fruitful avenues of research--that sort of thing.

DB: Well, it's nice to hear that, but I don't see why they say that. I think that, as far as interaction with other people, I have few collaborators outside of the Lab and then its's within the field of mammalian genetics. It's only because the strains I developed other people are finding useful now, and so there are many fields of genetics that are making use of these strains and therefore I get a lot of input from them. For example, the genetics of interferon production -I just heard yesterday--I got a letter from Edward Demaeyer in Orsay, just outside of Paris--about his successful use of the RI strains I developed, and then the use of congenic strains to go with them. I think that's probably how they're most useful, RI strains and congenic strains in combination. It seems like he's been a more successful utilizer of these strains than other people. Still it's encouraging when you work out a system to locate genes very quickly, and when one actually succeeds, it's encouraging.

SM: But you had the foresight to see what would be useful.

DB: Well, yes, back in those days. That's quite a while ago.

Right now, I like to think I have foresight to see something that's useful, but apparently other people don't see it that way. I say this because my grant application was turned down just recently. My grant application went to a genetics Study Section, which I think was pretty well laden with molecular biologists who were interested in traits that are models for human disease and my interests right now are in genes that are not of that ilk. So I attribute that to why they don't see the importance of what I'd like to do.

SM: Well, it's also the case in the history of science that the greatest minds are usually so far out in front that they're not recognized for a while.

DB: Well, that's a nice way to think. Sure! But, anyway, it's very frustrating. So with the funds now being so short; I don't see where I'll get support.

SM: It sounds like you have to pander to what's going to be commercially marketable.

DB: Yes. It really narrows what you try to do.

SM: What's the attitude in the Lab to the fact you have this project and it's not being funded?

DB: Well, I'm being encouraged by the administration. I know that I'll be funded for a while but there is a limit to that. This attitude seen in Study Sections might be true of scientists in general. There's a lot of chauvinism: What I'm doing counts and what someone else is doing doesn't or at least it is put down a ways on the priority scale, and there's been this attitude here at the Lab.

You see that directly at the Lab, just by remarks that are made, sort of putting down this or that person's work, which I don't think has been helpful. I don't recall that attitude of making comparisons-back in the '50's. I don't recall that happening back then. It was more of a laissez faire philosophy--freedom of doing your own thing, and you didn't get criticism for the area you're working in. They had criticism for the scientific procedure. But now, I think it's probably due to the difficulty in getting funds, more than the general competition that you see. There are a lot of scientists now who are putting in long hours, working late nights and week-ends who feel this pressure to succeed, and the competition. When you work in an area where there's lots of competition I suppose they do it for funding as well as for fame, and it's too bad, because they aren't having fun. And I think that's what you're supposed to do in science--have fun. That's why I got into it. If it was to make money, well, I certainly would not have gone into science, that's for sure, but the general attitude of people coming in is one which I think begets ulcers. It's going to get worse.

SM: Yes, the funding is getting tight. Well, I've heard some of that chauvinism at the Lab, but it's been expressed in a curious way, of a dichotomizing between "us" and "them." There are "us," which are the mouse geneticists and then there are "them," that are the others and apparently they are things like biochemists and people in other areas of science, but they are not in mammalian genetics. Their background has not been in mammalian genetics, and I've heard it from both sides, you know--the "thems" will talk about all those mouse geneticists who run around thinking about all these strains and crosses and doing this and doing that and then there are "us," you know, the scientists, and then the mouse geneticists will say the same thing--you know, the Lab was really great until we got all these other people in that don't know anything about the Lab.

DB: Yes.

SM: I think all said with some measure of jocularity.

DB: Yes. There always has been some sort of dichotomies that occur. Even back in the '50's, I recall the behaviorists, but they stayed up at Ham Station and were working with dogs, of course. The rabbits were out there too, but there was a physical distance--what is it? seven miles, I think, out to Ham Station and they would come in for seminars or to go to the library, but they were behaviorists and I think generally people in that general area were looked down upon somewhat because of the status of their science. They were working on theories that could not be proven, or tested vigorously, so some people felt they were pseudo-scientists, used terms that tended to confuse rather than enlighten, and perhaps they still use them.

SM: Of course, and it's not pseudo science and never has been really.

DB: There has always been a pecking order. Biochemists were at the top when I first came to the Lab. They were more in demand and therefore they got higher salaries too. There was a time when they were pretty hot, but now, I guess, the geneticists are coming into their own in molecular biology and probably be in the forefront for some time to come, I reckon. But there is this competition somewhat between geneticists and non-geneticists here at the Lab because the desire by the geneticists to maintain the Jackson Lab for what it was originated namely, mammalian genetics but biochemists were brought on soon because there was a vision they should have representation here, for advice in genetic studies. And that's true of other fields as well. The degree of collaboration at Jackson depends on how interested non-geneticists are in genetic problems. For example, if you're an endocrinologist who is not interested in genetic aspects, it would be too bad because there are many mice here with defects of endocrine. And the same way with immunology: There are a lot of defects there to study.

SM: How do you feel about this current move to bring molecular genetics to the Lab?

DB: Well, we had to, for sure. One thing I objected--I recall, I was on a search committee to get new staff members, and there it was expressed that we shouldn't bring anyone here unless they are working in molecular genetics, and I thought that was a mistake. You bring people here for what they are doing, their ideas and creativity, and not because they are using a technique, or a special approach, although we want to bring people here who have had that technique, so it would be available to be applied to the materials we have available. We certainly encourage that when we look for people who had good ideas and were working with molecular genetics, but to have that as a criterion, and say it's a must before you can hire somebody--I think that was a mistake.

SM: Right. Do you think having those people here will enable some sort of synthesis between the more classical approach and the molecular approach to take place?

DB: Oh yes. I think it rubs off both ways: People who are here can make use of those new techniques and those people with the new techniques can see the multitude of biomedical problems to work on here, and I think that's the way it's working out. There's a lot of discussion between people who have a use for molecular genetics and the newcomers. And we have interest group meetings which bring those researchers together.

SM: Interest group meetings?

DB: Yes. We have different interest groups. Have you never heard of our interest groups?

SM: Well, I have but in politics and things, but not within the Lab.

DB: Oh yes, there's the genetics interest group, that meets every Monday during the winter at noon time and someone presents something from the literature or something in their own work--something they haven't completed but they are working on, and they want some discussion and input about it. There are also biochemistry and cell biology interest groups.

SM: They call these things "interest groups"? Are you serious?

DB: Well, I know they use the term.

SM: Yes--here is biochemistry and cell biology have grouped together.

DB: What has happened has been a general amalgamation of these groups. Individuals with diverse interests meet with all groups, and it turns out they're pretty much the same. They don't really separate the subjects as the titles suggest. These have evolved quite a bit over the years. At one time they had a behavior group as well.

SM: So this would be a chance for people to interact and hear what each other's doing, ... bringing up... working in.

DB: Yes.

SM: That's good. Now do you consider yourself a member of an interest group?

DB: I go to the genetics interest group meetings. I imagine you can go to all of them and spend all your time at meetings and have no time for anything else. In fact, one year--I guess it was about two years ago, we had all winter long was filled with either seminars or interest group meetings.

END OF SIDE ONE

DB:The Lab had initiated a search for new staff members and the candidates presented seminars on their work on top of our regular seminars, that plus the interest groups and that's too much. You just attend the seminars which are of intense interest to you. You're very picky. Plus, of course, you have the perennial committee work.

SM: It must, though, be very gratifying to be around so many people sharing your interests, unlike the situation in San Francisco, where you were very much alone.

DB: Yes, yes indeed. That's interesting. You have a lot of people sharing diverse interests at the Lab, but people from other institution don't realize this. We had a seminar yesterday and it was presented in such a way that very few people understood it, because the individual came from a department in which the members all have similar research interests. This quite often happens. They're used to giving talks without giving an introduction to the subject. Everyone is familiar with that subject, so they come here and go through the same procedure, and they leave a lot of people out. We just don't follow the subject well and I believe that's due to the visitor coming from departmentalized unit in a university. Here we are not departmentalized. We have people at the Lab with various interests, people with diverse expertise, we usually need an introduction to the subject and if the one who invites the speaker hasn't forwarned the speaker of that need, then it's a disaster.

SM: Right.

DB: Even though we specialize in mammalian genetics, we still have diverse fields ranging from hematology to immunology to developmental biology--all quite different.

SM: This is a hard question to answer--what might have been--but how do you think your research would have turned out differently had you not come back to the Jax?

DB: Come back, yes. What was I doing at that time, in San Francisco? I was working in radiation genetics at that time. I might very well have stayed with the radiation aspects of mutagenesis. The objective in research there was to look for mutations that occur at histocompatability loci. I looked to see if mutations could be picked up at those loci, and, of course, we didn't find any, that's the problem. They seemed to be resistant to mutations--so maybe I wouldn't have stuck to that, or at least I would have tried to find out why they were resistant, what was different about them, why these didn't show up mutations. Perhaps, they mutated but the mutant mice were selected against. They didn't survive for some reason. However, recently we've been looking for histocompatibility mutations, but we've had a different procedure. Nevertheless, we are returning to that area I left back in San Francisco. That's beside the point. It's difficult to think of what I would have done. I probably would not have stayed there because I was getting frustrated by the lack of interest of other people to discuss my work with.

SM: You've come back and done so much with the recombinant techniques and inbred strains, for which the Jax seems to be so ... such a perfect place, given its huge amount of...

DB: Yes.

SM: What are some of the rewards of working here?

DB: Rewards. Well, as I've said already the freedom to do the research you want to do, (even though you're hampered by the paper work) and living in an area without all the traffic, the fumes, the problems of the city. I think that's a very big plus as far as I'm concerned, and I think the people that stay here have that same feeling, otherwise they wouldn't be here. A lot of people come here and try it out for a while and can't stand the isolation and living outside of the cities. But I think it's very selective, so that once you stay here, you come to appreciate the environment, and of course, the freedom of the hours you work. But most research institutions are that way. They don't have time clocks to punch. Some of them are not though.

SM: Was it always like this? Some people tell me that Earl Green had fairly strict assumptions as to when people should work.

DB: Well, that's true, but he did have some flexibility, in that he would expect you to be at work at least half of the day, any working day you should show up at least half of the day. But he did encourage you to be at the Lab a full day and come in the same time that your assistants come in, to show them the style:

show them you're working the full eight hours and are industrious and that attitude, the work ethic. Even though he had this philosophy that a scientist never stops working--24 hours a day, 7 days a week, because his mind is continually on his work, thinking up experiments and all that. It's true, one does spend a lot of time thinking about work outside of hours, not 24 hours (laughter) but I do wake up in the morning, say, about 4 o'clock, quite often, thinking about my work and what kind of experiment to start working on, or correct the problems that exist. So Earl Green had this attitude about the working hours and he would try to encourage you to-well, he encouraged you to come in at certain hours that were respectable (laughter) but he realized you might not come in for half the day, if you could get away with it.

SM: Well, why I asked that is that several people have told me, who work, or tend to work either late at night or tend to have erratic schedules as the spirit moves them, but tend apparently to be extremely productive, came hard up against Earl's sense of propriety of schedule.

DB: Yes. I can see that, sure. So there are a lot of stories about that sort of thing about Earl. There are a lot of stories about how they purposefully frustrated Earl (laughter).

SM: Yes. I've gotten some on tape.

DB: I'm sure. Have you interviewed Henry Winn?

SM: Oh yes (laughter).

DB: I'd love to hear that tape (laughter).

SM: Well, they'll all be upstairs--the transcripts--I mean, to listen to the tape, you'd have to spend 90 minutes, but it would take you 10 minutes to read the transcript, but oh yes--he was apparently Earl's nemesis, and he didn't--apparently Earl didn't actually do it, but Henry knew that when he went in and said to Earl that he was leaving, you could almost see the applause in Earl's head (laughter). So it must have been funny.

DB: How about Jack Stimpfling? Have you interviewed him?

SM: No--he was not on my list. Where is he?

DB: Well, he's too far away--in Montana, Great Falls. He's retiring this year. I understand he's coming here.

SM: Oh. He has been mentioned to me but I think that the fact of where he is and the fact that I really had to limit my travels--

DB: But he might be able to do something now.... he might be willing to write down some recollections. I didn't realize he was a post-doc in 1957. I thought he came on the staff at that time.

SM: Who has been your favorite Director, since you've known now all of them?

DB: Well, my favorite Director. I suppose Prexy Little because of his personality--the type of person he was--very gregarious, very friendly person, and his general attitude toward what research was about--it was for having fun, you did it, not get too serious about it. Now, it has become serious, even cut throat.

SM: To what extent do you think he set the tone for the Lab?

DB: Well, he certainly--at the time he established the general theme of what the Lab was about, namely mammalian genetics.The theme has continued on and is still expressed by our present Director. Our present Director is very strong in maintaining this mission of the Lab. I don't know how many other Directors after this will perpetuate it, but I think up til now Prexy set that. And also the structure of the Lab, because, I suppose, if you have all of these independent labs that never got departmentalized, I think that other Directors in turn realized that this was a strength of the Lab, so the organization stayed the same, pretty much, although because of the size and growth of the Lab, you have to have more administrators, more professionals, professionally trained administrators, a personnel department, public relations etc. It certainly has changed in that respect. But in general, in its structure and individual scientists, I think, it's pretty sound.

SM: He seemed to have a certain informality of style.

DB: Yes.

SM: That seemed to carry on at the Lab. Several people have said to me that they really worry that the Lab might change that way, because they now see, especially among the administration, these paper pushers, that people are now wearing ties (laughter). So apparently that informality is felt to be a feature of the Lab that they value and I wonder if that goes back to Little?

DB: Well, of course it goes back to Little. Yes. He was extremely informal, in fact, he'd come to work, -he never wore a suit. He might have his coat and his pants not match. His wife was always concerned about this. Finally--you probably heard this--about the birthday party where Bea gave Prexy his birthday suit, which he joked about. He finally got his birthday suit and he was going to wear it for something, because she wanted to be sure he had a suit where the pants and the coat matched (laughter).

SM: I see. I had not heard that. That's funny. If you had a magic wand and you could wave it and change the Jackson Laboratory in any way you please, what would you do?

DB: Well, first of all, I would rebuild the whole thing so that you didn't have this maze of corridors that you spoke about, but also to make attractive buildings of all this different hodge-podge of architecture and take that away, so I could still see Schoodic Mountain with the snow on it. There's the new wing. That's what Prehn put up. He came from Philadelphia and he put that up, not realizing that he should keep the view available as much as possible. And to get to the cafeteria, you have to go through an area which is out of bounds for visitors who have recently handled mice, you know, it's quarantine, restrictions, so that if you have visitors, redbadged--you know about red badges?

SM: Yes.

DB: So they can't go over there. They put that in a situation where it's away from the lobby and general entertainment area. So that was poorly planned in that respect. When you ask an architect to build a building, the first thing he or she thinks of is putting it in a position where it's the highest position and brightest color and that's what happened here. But anyway--so I would change the physical plant, I guess, but I think it would be nice to have it smaller, but I realize you can't have it smaller and have a new technology brought in--You can't bring in new people without expanding, unless you make it uncomfortable for the established researchers to stay around and that is not a fair policy. We need more space, so buildings should be constructed here for the new technologies. The formality could very well be something that is growing upon us, but the main thing right now to improve the Jackson Lab would be to have administrators that understand the science. You have people making decisions now who don't know what science is about. They come from areas of industry or some place of business where they don't know about research, or about scientists and their frustrations. I recall Earl Green-I think there were a lot of concepts which Earl had which were right and one of them was that scientists are like children, and you had to treat them like children. Well, I figure in a lot of ways that's true, because they are egocentric--they think of their own work all the time and they throw tantrums and scheme to do this or that, so Earl was right.

I think they could be described as being child-like. I think to have the type of mind to do this type of work, you're really playing games and you have to have an open mind, a child-like mind, to keep at it, so sometimes you behave like a child in many ways. So an administrator has to deal with this type of person. But the administrators here don't understand--a lot of them have been brought in from the business world. We have some very good ones and they are very helpful and their whole attitude is to assist you in getting grants and they go out of their way--Morris Browning and Earl Heiser, for example, but others I think they try to get in your way (laughter).

SM: I've interviewed Earl Green and as he recollects, he tried to make the scientists participate in the administration.

DB: Right. Well, I guess that's what I'm getting at. One thing I liked about Earl Green--he was overly organized, but he did have scientists help at first, if not making decisions, at least were advisory. So all the supervisors of different areas were scientists on the staff and the Assistant Director for Research was a scientist, a rotating position and I held that for a couple of years. I was very unhappy at the time I was asked to do it but looking back at it, I think it was quite helpful for the staff to have someone in that position, to advise as a representative scientist.

The Assistant Director for Research was one of the senior scientists of the Lab, so he had been around for a while; he was working for the Lab so he could give the attitude of other scientists whom he represented. Now there's no scientist who's advising the administration, but I guess the Deputy Director, or whatever--I think that's the title--might be encouraged to sit in on some meetings, but I don't think there's mandatory attendance. I don't know really what decisions are made at meetings where he is present.

SM: Is that perhaps due to the changing nature of the grants world, where so many grants now are renewed just for like a three-year interval and a scientist has barely enough time to write the grant, start doing the research, write the interim report and write the final report, start looking toward the next grant--I mean, several people have commented on this very fact, that the scientists aren't part of the administration now, with gratitude for the fact that they don't have to bother with it now, because they don't have any time.

DB: Well, that's why I was angry at the time I was asked, or coerced into becoming the Assistant Director for Research--that it was taking time from my research when I felt I was my most productive. I think it would be better to take somebody about ready to retire. Then they wouldn't have the same attitude. So I can see the pros and cons of that, but I felt like my time was too valuable for holding that position. As far as being busy all the time, 24 hours a day, 7 days a week, that's just about true--you are-but some people have more of a drive and are working at the Lab longer hours--let's say, 12 hours a day, 7 days a week, and I don't think it requires that to be successful in science, but then-see, I have a different attitude about doing science. I don't like to go into an area where there are lots of other people. I don't like to compete that much, doing the same thing, trying to beat somebody else in getting the data out. I like to try doing something that's quite different, that no one else has thought of and in that way, keep my independence and not be frustrated by what other people are doing, and eventually come out with something that will influence other people and therefore I gain their respect by something coming into its own at a later time, so I don't feel quite that pressure that I think I see other people feeling when they compete in large laboratories, with a lot of post-docs and team work and they are cranking out the papers, and see people being beaten by others in the publication world coming out just before they come out with their great discoveries. It's frustrating for them, I'm sure, but I try to avoid that.

SM: Apparently, your colleagues feel you have the intuitive insight to do so.

DB: Well.

SM: Not everybody does.

DB: Well, one takes a chance--a risk on being unfunded.

SM: A risk on being not understood and that's not negliglble. I mean, that happens a lot in science. What do you think the Jax is going to be remembered for? What's its great science?

DB: I suppose for what it's doing right now--it's being a repository for genetic strains of mice, but you know, how long that's going to last really is questionable, as I can perceive, ten to twenty years from now--the inbred strains and all the mutants will probably be of no use.

SM: Why?

DB: Because of the advances made in molecular biology: You can do a lot of things by manipulating the DNA, so that you can produce directed mutations. With transgenic mice, you can create your own mice with a mutation, a change you want, in the DNA. Eventually, you have to come back to organisms. I've always felt you have to come back to the cell and the organism in some way, because it's at that level that has the phenomena you must eventually explain. A lot of people have discussions about this and there's agreement among most biologists that that's true, but the molecular biologists now don't seem to think this is necessarily true. Their attitude seems to be that everything can be answered--even developmental biology--what happens in embryogenesis, they feel, can be answered by looking at things at the molecular level

SM: That's the hubris that comes from its being a fad, don't you think?

DB: Yes. I think there might be that aspect. This attitude might be carried over in the schools in such a way that you don't have people coming up through our biological science training to understand about levels that are higher than molecular, so I don't even know if the new staff coming here are learning about genetics--classical genetics. It seems that it's an education for them, and they're anxious to learn what we do have here and that's formal genetics. Also a lot of them haven't had courses in embryology or comparative anatomy, which makes it difficult to discuss things with them because they don't associate what they're doing with what happens in the embryo, a different level of organization. So there might be a period in there where there will be a scarcity of biologists who are interested in problems--or questions in that area that need to be answered--and there won't be anybody there to understand what the problems are, or how to ask the questions properly.

SM: To what extent, then, is the Jackson Laboratory a repository for ancient wisdom?

DB: Well, I don't think there is here any sense that we represent a repository.

SM: Or the mouse itself--if you think about the cells, or the organism, as a form of wisdom, The Jackson Laboratory is sitting on all these strains.

DB: Well, that's what I foresee possibly happening--is that we are freezing away our mice, our strains, mutants can be put in the icebox, but if there's no one around to know what those are good for, ... to start with--

SM: You think it will be that bad?

DB: Well,I'm not an optimist, as far as the history of the world goes.

SM: You seem like me. I was trained as a medievalist and I look at all these machines that have every kind of floppy disk and when the day comes that Rome falls, nobody knows anymore what a floppy disk--even what it means, right--who's going to be able to read? At least in those days they had pieces of papyrus, parchment and people could write. What will come of everything? No one will be able to read the manuscript that is in essence all these strains of mice that are now going to be so technologically preserved.

DB: But then, as I say, maybe there won't be a need for these--I think they're important now and a lot of other people think these strains of mice are important, but I don't want to be close-minded. I can see the possibility of them being useless--just a collection--because I can see how fast things are moving. But I look on it philosophically: What difference does it make, really? A hundred years from now, who'll know the difference? or possibly twenty years from now--things are moving very fast.

SM: Do you sometimes feel stale or old-fashioned, or becoming out of date?

DB: Yes--I have this conflict. But I keep dodging it in myself. There are a lot of things, a lot of questions in this world without converting my lab to a molecular genetics lab although there are some questions I have now that I could very easily answer by doing that, but there are still a lot of others I could answer without doing that and--but it is a question of doubt. There's always that.

SM: I don't know that it's so much "doubt". The fellow that I just finished this project on--Schmitt at MIT and how he was always concerned to be current--not even there, on the forefront of things--and so he didn't so much have doubt as he had anxiety that he was going to be left behind.

DB: I think I see that in my colleagues. I think a lot of them go into molecular biology because they think this is the thing to do without really having a good reason for doing it.

In other words, it's a tool, a technique that you can use for answering certain questions, but if you don't have questions worth answering, then why use it? Because I see a lot of it is very similar to--for example, my attitude toward--a lot of the studies in genetics have been locating genes on maps, on the chromosomes. To me, that was a very boring thing to do. I've done some of it myself. It was not very interesting--it was just sort of a routine thing to do, but some people spend their lifetime finding the location of genes on maps. I think that in a lot of respects, is what molecular biologists are doing: They are finding the sequence of nucleotides and relationships of different sequences on a chromosome and that's about it--very much a parallel situation to finding the location of genes on chromosomes. They're not looking for hypotheses and testing hypotheses... it was just describing what they find at the molecular level in relation to the chromosome. So some people get bored after a while by taking this approach, although in some labs, you encounter concepts about control, regulatory mechanisms--but those are rather special labs. They have a lot of money coming in and big teams of people going at the work. I'm not sure that sort of thing would come out of the Jackson Lab because they can't compete, unless they have some really brilliant ideas. I don't know what the new staff members are bringing, I haven't seen anything yet, but they haven't been here so long as to reveal their strengths yet.

SM: Are they open to collaborating with other people at the Lab?

DB: The new staff? I haven't seen much evidence of that. Maybe there are some that I don't know about, so I can't say for sure. Yet, I haven't seen any evidence where they collaborate-they go to each other for advice, on techniques and approaches to take, but actually doing experiments together, I've seen a few, but nothing extensive. It's an interesting question because prior to this last group that came in, the new staff members would complain they were sort of turned loose--they were on their own, especially after coming out of school. They came from a situation, I guess, where they were in a large lab and people working in teams and all of a sudden they are on their own, turned loose and they felt isolated,and they didn't have the prior advice. That's why I felt they'd like to work under somebody's wing for a while before they went on their own. But for this present group of new staff, I think each one is more independent.

SM: That's right. I get that distinct impression in my interviews with them.

DB: I don't know why. Of course, I think they came from labs where they were working with many other people. I think there are very strong egos involved in that group. It seems that way... which is a nice attitude in that you have a lot of confidence in yourself... not think about self doubts. It's sort of a self-selecting process when you come here, and when we recruited candidates for new staff positions, a lot of them expressed their doubts about coming here, that they didn't want to have the insecurity the Lab offered--the insecurity in the sense that they were expected to get everything--their own grants, self-supporting and they would rather go to some school or institution where there was a salary and then seek their grant to support their research, and so a number of them turned down positions here because of that. But I guess the ones who do come here tend to be the ones who have more self-confidence.

SM: I think there's a great deal to come out of the collaboration of the molecular people and the more classical geneticists, but I wonder how much will actually take place, with those personalities like they are.

DB: Yes, I see there is that problem, I guess, but it's not because of the Lab structure, but it's because of the individuals who come here and maybe the changing times--the type of scientist we bring here.

END OF INTERVIEW