7-22-1986

James Ebert Oral History

James Ebert
The Jackson Laboratory

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Interviewer's Comments

Narrator's Name Dr. James Ebert

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 interview took place amid the opulence of the Four Seasons Clift Hotel in San Francisco, where Ebert was staying while working on a project for the National Academy of Science. He had just flown in from Washington that afternoon and gave this interview after putting in a fifteen-hour day. Characteristic of the man, this context of fatigue/stress doesn't come through at all: His interview is pointed, succinct and full of cogent assessments of the three Directors of the Lab he has worked with (Green, Prehn and Sanford), as both a Lab Trustee and Board of Scientific Overseers member. This interview is also rich in assessments of the Lab's current situation, its challenges and opportunities. In his vision of the Lab's ideal size and identity, Ebert echoes many others, both Trustees and scientists, in wanting to see the Lab retain its present size.

Ebert gives his own perspective on the final days of Prehn's Directorship, which should be compared to accounts given by Prehn himself, Coleman, Harrison and others.

Also noteworthy here is Ebert's estimate of the Lab's place in twentieth-century science, from a man with a national, even an international, perspective.

Given the very candid and blunt assessments Ebert makes of some people here, he may decide to edit this tape extensively. If not, value this interview for its wealth of insights and evaluations of the major players in the Lab's recent history.

22 July 1986
Date

Susan Mehrtens
Interviewer's name

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Interview Contents
Dr. James Ebert

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Interviewer's Notes and Word List
Dr. James Ebert

Washington & Jefferson College
C.D. Dieter
Phi Sigma
Woods Hole
Edwin Linton
Johns Hopkins
Beaufort Lab
Duke Univ.
Mt. Desert
Roscoe B. Jackson
Dickie
Margaret Dickie
George Snell
Nathan Kaliss
Brooklyn College
Columbia Univ.
Edgar Zwilling
New York
John Cushing
Sewall Wright
Carnegie
Washington
Baltimore
Wallace Fenn
F.O. Schmitt
B.H. Willier
Russells
Elizabeth Russell
Bill Russell
Peter Medawar
Peter Gorer
Indiana Univ.
N. Avrion Mitchison
Sonneborn
England
Nobel
Bloomington
Paul Weiss
Mac Edds
Frank Schmitt
Bar Harbor
Meredith Runner
T.W. Torrey
Lewis DeLanney
Earl Green
Gatlinberg
Tennessee
Ohio State
Margaret Green
Douglas Coleman
Andy Kandutsch
Dorothea Bennett
Philip Leder

Prehn
Sanford
Little
Woods Hole
Wright
Barbara McClintock
Basil Eleftherio
Fort Yoder
Hank Neilson
Yale
Richmond Prehn
Murphy
Lloyd Law
H.B. Andervont
Seattle, Wash.
Philadelphia
Fox Chase
MIT
John Loofburrow
Dick Bear
Hellstrom
San Jose
Fleischman
Tracy Sonneborn
Chesapeake Bay
Annapolis
Bill Hewlett
Dick Heckert
Frank Stanton
Lewis Lukens
Frank Gerrity
John Beck
Steve Petschek
Mark Boyer
Maxine Singer
Bill Bevan
McArthur
Cold Spring Harbor
Bangor
Dave Harrison
Seldon Bernstein
Barker
Bailey
Stevens
Green
Rochester
Beatrice Mintz
François Jacob
Allen Russell
Bill Dupuy
Jim Baldwin
Barbara Sanford

Terms:
teratology
ontogenesis
graft vs. host
diabetes
axolotls
teratomas
histocompatability

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Place San Francisco, CA

Date 7-8-86

James DeBerg
Narrator

Susan Shelenten
for the Laboratory

The Jackson Laboratory
INTERVIEW DATA SHEET

This section is to be completed by the Interviewer. Carnegie Inst. of Washington

Narrator Dr James Ebert Address 1530 P St N W Wash DC 20005-1910 Phone 202 387 6405

Birthdate ______ Birthplace ______ Interviewer Dr Susan McIntire Phone 207 244-7353

Date(s) & Place(s) of Interview(s) 8 July 86 San Francisco

Collateral Material Yes ____ No X Terms unrestricted

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

Received & Labeled	Collaterals Filed	Transcribing: Begun	Transcribing: Number of pages	Transcribing: Total time	Catalogued	Audited	Editing: Begun	Editing: Total time	Review: To narrator	Review: Returned	Review: Reread	Review: Preface	Final Typing: Begun	Final Typing: Text finished	Final Typing: Index, Table of Contents	Final Typing: Proofread	Final Typing: Corrected	Duplicating: Transcript sent	Duplicating: Transcript returned	Duplicating: Tape sent	Duplicating: Tape returned	Distribution	Dissemination
	None																						

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Oral History Collection

Collateral Materials Report

Narrator's Name Elbert

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

1. None

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This is the tape of an oral history interview of Dr. James Ebert, given as part of The Jackson Laboratory Oral History project, sponsored by the Acadia Institute. This interview was held on July 8th, 1986, at the Four Seasons Clift Hotel, in San Francisco, California. The interviewer was Dr. Susan E. Mehrtens.

SM: Why don't we begin by my asking you when you first heard of The Jackson Lab? How you came to be on its Board?

JE: Well. How I came to be on its Board was long after I first heard about the Laboratory. I heard about the Laboratory first some time between 1938 and 1942, probably in either 1939 or 1940, when I was an undergraduate student at a small college in western Pennsylvania, Washington and Jefferson College, where the leading professor in biology was a man named Dieter, C.D. Dieter. And Dieter had a special interest in providing opportunities for his students in summer laboratories, and ordinarily at least once each year, usually at a meeting of the biological honor society, Phi Sigma, Dieter gave a talk, largely on marine laboratories. Dieter himself went often to the Marine Biological Laboratory at Woods Hole for the summer, and Washington and Jefferson College had a scholarship named for Edwin Linton, who had been a very famous biologist, who taught at the college, and also was at Johns Hopkins. And this scholarship was used to send one or perhaps two students off to a summer station, and although Dieter tended to emphasize Marine Biological Laboratory at Woods Hole--I believed he also emphasized, if I remember correctly, the Beaufort Laboratory at Duke University.

He usually brought into the picture the laboratories in Maine, the Mount Desert Laboratory and, almost as an afterthought, the Roscoe B. Jackson Memorial Laboratory, which, of course, in those days was its name. My recollection is that he allowed as that he knew relatively little about it, but that he had heard good things of it.

SM: This would be just about ten years after it was founded.

JE: This would be about ten years after it was founded. There was one other curiosity about this story which perhaps some other one of your raconteurs might elaborate upon. Dieter's very best friend at Washington and Jefferson College was Professor Dickie, whose daughter Margaret was the favorite dancing partner of my roommate in college, who later found her way to the Jackson Laboratory, where she did some serious research. She was not a major investigator, but was an interesting, serious scientist, and I have no idea whether Dieter's knowledge of the Jackson Laboratory -and his very intimate knowledge of Professor Dickie--they played pinochle often in the evenings--had anything to do with Margaret Dickie finding her way to Bar Harbor, Maine, and it was quite interesting, in that my roommate and Margaret danced almost on a weekly basis. I think their relationship extended only to dancing, because both of them, I think, remained unmarried all of their lives. Both died untimely deaths. But I would see Margaret on the average of once a week, or once every two weeks, but in all that time, we never discussed science with her on any occasion. Then I suppose I lost sight of the Jackson Laboratory for a period of six or seven or eight years. Then, when I was in graduate school, the Jackson Laboratory came to my attention several times during the period 1946-1950. And there are several reasons for this, but the primary one was that my own research began, as a graduate student, to put me in the direction of research going on at the Jackson Laboratory. I had an extraordinary graduate career, one of those careers in which just about everything succeeded, and I was working in a field which was later defined as developmental immunology. And of course, the field of transplantation immunology was being developed in part at the Jackson Laboratory by George Snell, and later by Nathan Kaliss, who is, in many ways, I think, every bit as creative, and perhaps, in some ways even more so, than George Snell. Of course, Snell was given the greater recognition, I suppose, largely because of his persistence and consistency, whereas Kaliss had his highs and lows, and George Snell proceeded always at the same relatively high pitch, in his own very special quiet way. But, at any event, I began to become aware of the work of Snell and Kaliss, who had been at the Laboratory since the mid 1930's.

SM: 1937?

JE: Right. 1937. So it's almost a decade after that. I had known Kaliss, known of him and had met him through early connections which I believe were Brooklyn College and Columbia University connections. Nat Kaliss and his wife were very good friends of a man named Edgar Zwilling. He was a very highly regarded developmental biologist. I think both of them had New York roots. I'm not certain whether they were both at Brooklyn College and Columbia, or just Columbia, but I knew they overlapped at Columbia anyway in the '40's. And so, it was through Zwilling that I met Kaliss, not through an immediate connection in our own field. But also in that period between '46 and '50, I had three other connections with the Jackson Laboratory. There was a professor at Hopkins, I shouldn't say "Professor," an Assistant Professor who was one of the pioneers of immunology, John Cushing. Cushing never, I suppose, accomplished what one might have expected of him, in the mid to late 1940's and early '50's, but he was a highly creative man, and it was Cushing who first made the statement in public that he thought that the work going on on the histocompatability antigen by George Snell would go down as landmark research. That was the first time I remember anyone making that judgment of Snell. The second factor during that period '46 to '50 which occurred roughly every spring for four years was the fact that Sewall Wright, who was one of the nation's leading geneticists--and I suppose you might want to interview him.

SM: Yes, he is 96 and still going strong.

JE: I see Sewall even to this day because I--until a couple of years ago--I had his nephew with me at Carnegie, in Washington. His nephew is not a Sewall Wright, I must add, although a very capable and interesting gentleman in his own right. But Sewall Wright came to Baltimore annually in April on the occasion of the meeting of the National Academy of Science. Sometimes Sewall Wright and his wife would come along with Dr. and Mrs. Wallace Fenn of Rochester, and Dr. and Mrs. F.O. Schmitt. And all of them would stay at the home of my major professor, B.H. Willier. What they would do would be to come to the Philosophical Society meeting, and then come on to Baltimore and stay over night on the way to Washington, or maybe stop in on the way back from the Academy meetings. On those occasions, I was fortunate in that my professor was a man who believed that your students should get to meet these important people, and so Sewall Wright came into my laboratory every April for four years, and he would always tell me what was on his mind. But he would always speak with the warmest interest about the work of the Russells, and of course, later on, the work of Elizabeth Russell. As your raconteurs will have told you, there were two Russells, and a Russell-to-be, I guess, at the Laboratory, for a number of years, but it was interesting--it was of special interest to me that Sewall tended, when talking with me, to talk more about Tibby Russell than about Bill. I think this was probably because he understood that Elizabeth--her work was of more relevance to my own than Bill's. It's quite interesting that all three Russells--Bill and both spouses--are now members of the National Academy of Sciences, with the second wife having been elected a year ago. But the third connection was, on two different occasions, in that period from '46 to '50, the very famous British cell biologist-immunologist, Peter Medawar, came to Baltimore for a lecture and for other reasons, and on both those occasions, came to visit us, in the Department of Biology at Hopkins, and during both of those visits, he and I talked at some length about the work of the Jackson Laboratory. Of course he was especially interested in the work of a man I never met, but who spent, I gather, only a limited time in the Laboratory. I suppose some historian will really want to look carefully at this, and this is the work of Peter Gorer. I suppose Gorer came into your interview with George Snell, probably would in an interview with Kaliss. There are those who think that Gorer's contribution, in his relatively short period at the Jackson Laboratory, was of enormous importance. But Medawar considered him an enormously creative person. But I never knew him. I think we did meet, on only one occasion. Of course, he died relatively young. But Medawar spoke with great interest of Snell and Gorer, and I don't recall whether he spoke of Kaliss--I'm just not certain of that--although certainly some of the work that Kaliss would have done early on at the Jackson Laboratory would have had a clear bearing on Medawar's work. Medawar was just himself edging into that field at the moment, but when he first visited Baltimore, he was in the midst of a very hot controversy on the idea of infectious spread of pigment granules, an idea--one of the few larger mistakes I think Peter Medawar ever made. It was a very sensitive subject at that time in Baltimore. So that by 1950, when I took my doctorate, I had, I think, only a partial understanding of the Jackson Laboratory, but I had a sense of the research going on in probably what were its two greatest areas, in developmental genetics, especially in the Russells, and in transplantation immunity, especially in the person of George Snell, and the people around him. I guess then for the next few years, I saw and heard very little of the Jackson Laboratory, during the period I was at Indiana University, with one exception, and this again may come out on other records, but during my time at Indiana, a man who was later to become a major figure in transplantation immunity--N. Avrion Mitchison--came to Indiana University, to work with the great geneticist Sonneborn. And Mitchison, if I remember correctly, had spent some time at the Jackson Laboratory. Later on, Mitchison was to become a major figure in transplantation immunity, in England, influenced by Medawar, along with others, and remains still, I think, a significant person to this day, and during that period, which was the great period of Medawar's discoveries, which led to the Nobel Prize, the subject of actively acquired tolerance--it was during that period that Mitchison was working in Bloomington, Indiana, and came and spoke to some of my classes, and, at that time, talked about Snell, and again, being British, talked about the importance of Gorer in his brief interaction with the Jackson Laboratory. And to me, Gorer remains one of the enigmas because I just was never close enough to understand what went on, but I think it would be interesting to have a professional historian really closely examine how much of Snell was Gorer, or vice versa. I think that would be an interesting topic. Well, I suppose it was then another several years before I really became involved, or involved more deeply, in Jackson Laboratory affairs. I first saw the Laboratory, in the physical sense, in 1956 or '57, but I didn't see it other than as a casual visitor. In the mid 1950's, the National Academy of Science's National Research Council, through its division of biology and agriculture, organized a series of special programs in developmental biology. In fact, it was at that time that the term "developmental biology" was first coined, rather than embryology. And it was coined by a very famous embryologist named Paul Weiss.

SM: Yes, Dr. Schmitt has--

JE: Who was, of course, a very close friend, as you know, of F.O. Schmitt, among others, and a man for whom I had great admiration and who was very important in my own career, as we discussed on another occasion. Weiss was the Chairman of the Division of Biology and Agriculture of the National Research Council and in that year--I believe it was 1956--a Developmental Biology year, and we had a series of programs, each lasting about a week and each resulting in the production of a book, and one of these, I remember--I attended several--one of these, which I organized, was on the general area of immunology and development. The book was edited or written by Mac Edds, and myself. Edds, who had a connection with Paul Weiss, and later with Frank Schmitt, but that meeting was held at the Bar Harbor Motor Inn.

SM: Oh!

JE: So, during the course of that meeting--I suppose there were probably 30 or 40 participants--during that meeting, on one or perhaps two occasions, we went out to the Jackson Laboratory, but then, as today, one did not visit the Laboratory casually, because of the restrictions on people who had exposure to laboratory animals, and I suppose also because of the general conservatism of the Jackson Laboratory at that time. At that time, the Jackson Laboratory was a relatively closed shop, except for geneticists and people of immediate interest to the Laboratory. One didn't visit the Jackson Laboratory very casually. So that, I think we were invited formally and a few of us visited specific individuals at the Laboratory. I remember I visited especially a scientist at the Jackson Laboratory who, again, never reached a major position in science, but who was a very serious scientist, named Meredith Runner, whom you may wish to interview.

SM: Yes.

JE: You have him on your list?

SM: Yes.

JE: I visited Runner for a couple of reasons because our research overlapped slightly, but because he had been a student at Indiana University. He took his degree with T.W. Torrey, the Chairman of the Department of Zoology at Indiana. Torrey was a man who really had almost no students of any consequence and Runner was his one student of some significance and a very interesting and enjoyable man. So I visited Runner, if I remember correctly. But that was just a kind of first fleeting impression of the Laboratory, and I then did not go back to the Laboratory until 1959, when I spoke at the 30th anniversary symposium, and I don't have a strong memory of that symposium. I did go back and look at the volume. It was published in the Journal of the National Cancer Institute, monograph 2. The publication date was 1960.

The meeting was June 15th to 17th of 1959, and the general topic of the symposium was "Normal and Abnormal Differentiation in Development." And there was a lot of discussion of problems of teratomas, and I gave a paper, which was jointly authored by Louis DeLanney, and I think the title of that paper was something like "Ontogenesis of the Immune Response--Development of the Immune Response," which, of course, fit very naturally into the interests of the Laboratory. It was a paper that was very well received, as I remember, and it drew a lot from the work of Medawar and his associates, as well as our own work on the graft vs. host reaction. So that was the first time that DeLanney and I really had any signficant interaction with the Laboratory. From that point on, I suppose, I stayed, not in close touch with Snell and Kaliss, but I suppose over the next few years, our paths crossed infrequently at national meetings, but I don't believe I went back to the Laboratory. I have no clear memory of having been back to the Laboratory. When I get my next connection with the Laboratory, it was either late in 1966 or 1967, and in a very formal way, as is quite typical of him: I received a very formal letter from Earl Green, with a big package of information describing the Laboratory and almost a petition, so to speak, for Earl to come and call upon me in Baltimore, for discussions that he hoped would be of lasting significance to both me and the Laboratory. And one could hardly refuse such an invitation, or such a request to be invited, is how one should put it. And so, in due course, at some time during that year--it was probably, I would guess, early 1967--that Earl Green called upon me in my office in Baltimore, to ask me to become a trustee of the Laboratory, and a member of the Board of Scientific Overseers. And he came really for a very formal presentation. I had know Green just very, very slightly through meetings at places like Gatlinburg, Tennessee, and elsewhere, as a geneticist, but not as a geneticist in the field I commonly read. Green was a competent geneticist first at Ohio State, before going to the Jackson Laboratory, but not, I think, a scientist of any great distinction. In the science, I suppose, Margaret Green's interest interacted or overlapped my own, since she was somewhat more a developmental geneticist, perhaps, than Earl was, and again she's a very competent, serious scientist. And Green had spent some time at the National Science Foundation, and I think I had seen something of him there because, for a number of years, I played a number of different roles in the National Science Foundation. But I don't think I had ever really had a serious conversation with Green until that day in Baltimore, so that I became--I guess I must have been elected in '67, becuse the records said that I was a member of the Board of Scientific Overseers and a Trustee from 1967 to 1980, and I was a member of the Board of Governing Trustees from 1974 to '80, and a member of the Board of Scientific Overseers from '67 to '80, and Chairman from '76 to '80. Then, of course, there was a hiatus of one year, and I became a member of the Board of Governing Trustees again, beginning in 1981, and I gather I'm going to have to continue for another three years. I had hoped that this would be my last year, this meeting ending in August, because I had pushed very hard to have the nature of the Board change, to have elections for three year terms and to have a greater turnover in the Board. It was my argument that this would give the Laboratory an opportunity for change and I fully expected this to be my last year, but apparently more Trustees are taking advantage of the opportunity to step out and the Chairman called me recently to say that I had to continue for a little while. But, in any event, it's been, I guess, since 1967. Quite an interesting time.

SM: What sort of changes have you seen in the Lab?

JE: Well, I suppose, changes on almost every front--changes in the style of election, changes in the style of financial management, change in the direction of the science. I hope I have not seen significant change in the quality of science. I have to say I fear there may be. That is a little hard--we're at a point in the history of the Laboratory where that's not entirely certain. I think it's inevitable to have that thought or that fear at a time when the field of genetics is changing so rapidly, but one does not see, at the Laboratory, at the present, the quality of leadership--in the strictly scientific sense--of Snell and Russell, in the senior members of the Laboratory. There are a few scientists of originality and high quality. I especially admire Douglas Coleman and Andrew Kandutsch. Coleman, perhaps, of those two, is the one who has a degree of persistence and consistency focussing on the field of diabetes and obesity, and related subjects that mark him as a leader in that relatively small field. And of course, Kandutsch is somewhat more broad, but nonetheless, he's a person of significant stature, but I don't see, at the moment--and I hope I'm wrong--that they're going to emerge as Snells and Russells. When you get beyond Coleman and Kandutsch at the Laboratory, then you get into the next--into people in the same age group and in the next age group down--a larger population of serious people but without proven major impacts in their fields. Now, of course, the Snells and the Russells are always rare. If an organization with 30 to 40 scientists has ten percent of them at the very highest level, that's a pretty decent batting average, but it's a little unclear that beyond, say, Coleman and Kandutsch, where the next really major things are coming from. Perhaps others much better qualified than I am to judge--

current members of the Board of Scientific Overseers, Dorothea Bennett, Philip Leder, I think would be someone who would have a real sense of the quality of the Laboratory. The Board of Scientific Overseers has the function--the style of its function is such that it's too often put in a position where it has to make a quick judgment in time to make a statement at the Annual Meeting. I often worried while I was a member and Chairman of that group, and I worry today that meeting and then immediately with the Chairman of the BSO having to give a report to the Trustees immediately, sometimes results in a kind of pablum, a kind of general endorsement, without as hardnosed a view of the Laboratory as one might have, or want to have. And also, the format doesn't really permit the Board of Scientific Overseers to look intensely at any one individual. It doesn't allow you to say the kinds of things the Director of the Laboratory needs, to effect a change in it, although the tenure at the Laboratory is supposed to be relatively limited, such tenure as the Laboratory may provide. Unfortunately--and this is true of many organizations--I have a problem in my own, although our regulations are more strict, making it easier for me to be mean if I have to, a kind of de facto long-term tenure arises, makes it very difficult to make a change, so that we can talk about these changes under any number of different formats. I suppose the clearest change was really given the nature of the three individuals that I've seen as Director--Green, Prehn and Sanford. I met Little only at the very end of his career, probably toward the end of his life, and I did try to dig out and failed--I assume that my notes of the period are squirreled away in Woods Hole somewhere, but I met C.C. Little through Sewall Wright, and I tried on that occasion, as I recall, to get from Wright after Little left us, some feeling from Wright as to Little's involvement with the tobacco industry, and I couldn't get Wright to comment on it. But obviously for a number of people that I know, that was a very, very sore point, that Little had sold out to the tobacco industry, but I'm not really qualified to talk. All I can say essentially is gossip, but it's hard to understand how a man of Little's stature could have taken the directions he took in his--but Earl Green was not a Little in any way at all. Little was a relatively free-wheeling person who operated the Laboratory out of his hip pocket. At least that's how the story goes. You've probably heard many of these from more reliable sources than me, and there's no point in my repeating secondhand anecdotes. Green was a highly organized man, so organized probably, that he could never have become a great scientist. I think he was meticulous to a fault, is meticulous to a fault, I should say: he is still very much with us. But, thinking of him now, one thinks of his science in the past, so it is fair to say "was." But I have never in my scientific career met a person who put such great weight on trivia, and who was so intensely loyal to his staff members, loyal to a fault. He found it very difficult to accept criticism of his staff and one of his greatest weaknesses was in allowing people to remain on the staff after they had been shown to be either incompetent or bordering on charlatanism. He simply liked to make up his own mind, and he didn't like to be unsettled by the feelings of others. There's no doubt, however, that he had this very quality, you see, was among the qualities--probably the greatest quality--that allowed him to protect a person like George Snell. If you realize that Snell was operating in a highly innovative way, being ignored by most of the scientific community. Snell was not ignored for as long and to the degree that, say, Barbara McClintock was, about whom we all know--she didn't receive her Nobel Prize until she was in her 80's--but George's position up there in the woods in Bar Harbor, coupled with his very, very special personality--being shy--"diffident" is a better word than "shy" for George: He's not really shy. About his personal style, his geographic location, and the fact that his research was off the beaten track from most geneticists led to Snell being recognized much later than many of his peers. And there's no doubt that Directors of a laboratory, who were more interested in fadism, more interested in the most recent fashion, more interested in a hot name, might have wanted to dispose of George Snell, but that unique devotion of Green's served as a great buffer for Snell, who needed to be buffered, and it served as a buffer for Basil Eleftheriou, who was a person about whom many members of the Board of Scientific Overseers had grave reservations, and who ultimately had to be removed from the Laboratory. During, I believe--if memory serves me correctly--it was during the brief time that Douglas Coleman was the interim Director. But those warning signals had been on the horizon for several years. But the doggedness with which Green defended Snell, he also defended Eleftheriou. So it was a trait that served him extraordinarily well and extraordinarily badly. Of course, other traits that Green had were his very tight-fistedness. He was one of these kinds of people who took great pride in being able to say, at the end of the fiscal year, that he was turning money back. I had a Director like that until recently--our geophysical laboratory, for that reason, for a time, was referred to only half in jest, as Fort Yoder, and I think that the Jackson Laboratory for a time might have been referred to as Fort Green, because Earl really did--there were times when people like Coleman and others jokingly referred to Earl Green as the ... (laughter). But, with all this, he was a man of great charm. He had the capacity of being able to say to the Trustees, in relatively few words, what he felt they ought to hear. I've just never quite seen a Director in any organization who could screen the information going to the Trustees as effectively as Earl did. Again this is a source of great strength and great weakness, because there are times when Trustees really have to know what is going on, but Earl revealed just exactly what he wanted to reveal and he used to be very troubled when members of the Board of Scientific Overseers would tell one or another of the Trustees something more than Earl wanted to have told. I suppose that the nature of Earl Green--his personality and his style of direction--in a way accounted, in part for the choice of his successor, because the Trustees were adamant that the next Director of the Laboratory should be prepared to lead the Laboratory in new scientific directions, to cut a wider swath in science. Hank Neilson at one time said that they wanted--and I'm reluctant to say this for the tape but perhaps I should--that Neilson said that they didn't want another prissy Director (laughter). And so, they got someone who was just the opposite, the swashbuckling type, who came for his first interview at the Yale Club wearing a bright red sweater.

SM: Yes, this has been remarked.

JE: But Richmond Prehn, however, despite the bright red sweater and despite the fact he did not remain long at the

Laboratory, had the potential, I think, if he had allowed himself to do it, and if the Laboratory had been prepared for it--had the potential of really moving the Laboratory ahead, even more significantly than he did, and he was--and the Laboratory didn't stand still when he was there. I think Prehn could certainly point to major achievements. I think with Earl Green, the Laboratory was a kind of skewed Laboratory. There were a very small number of extraordinary individuals--Snell, Russell and, in his own special way, Kaliss, who was very creative, although not recognized as much, but then there were a very large number of other people who were mediocre plus, but who performed important services. There was a large emphasis at that time, and even today, on mutants arising in the mouse colonies.

END OF SIDE ONE, TAPE ONE

JE:... the natural history of mouse mutants, who were good at recognizing--Margaret Dickie would be an example. Other people who had some interest on the cancer side of things, like Murphy--a substantial number of people whose ultimate achievement, apart from service, was relatively small, but whose service achievement, I think, loomed very large, in the history of the Laboratory. I think something future historians will want to examine will be the extent to which the Jackson Laboratory's contributions have been more important for the great innovative discoveries--the Snells, for example--or whether the larger contribution has been in the service function and in the science related to the service function. I think you might get very different assessments from different populations of individuals. I think it might be very important to hear from Lloyd Law about the early cancer research at the Laboratory. I think that would be an extremely interesting set of observations that I can't really contribute to--the early thoughts about the milk factor, or the mammary tumor work and so on. Law and H.B. Andervont is another--I think Andervont is dead now, but Law would be able to comment on Andervont. Both Law and Andervont were on the Board of Scientific Overseers when I first was initiated into the Laboratory. So that Prehn's coming on as Director was interesting in another way, and that is that it is very rare for the Directorship of a major organization to go to someone who has overtly applied for the job. Usually, in today's world, for purposes of--for legal reasons, if for no other reasons--position are advertised, but ordinarily, major jobs are filled by very careful letters to leading individuals who--and you set out with half a dozen names in mind who you'd like to have, people well equipped to a point, and the Search Committee for the Directorship started out with that in view. I think there will be records which will show who the leading candidates were, and at what point they were invited and said no. I don't think I should elaborate on that, but it's fair to say that Prehn's was clearly not the first choice of the Search Committee but when the Search Committee goes beyond its first obvious choices, it then begins to look at a second list, and lo and behold, here was a person who applied for the job. Now this is very rare. I know of only two or three jobs which have existed, in which I've been involved in which someone was selected who actually applied saying he wanted the job. Prehn had appeared on no list. And I can't remember, but my guess would be that we must have written to a minimum of 60 or perhaps 90 major individuals in the country--alumni of the Jackson Lab oratory--asking for their thoughts, but I suppose we had, at one time or another, a list of prospects numbering 100 or in that range--typical of this kind of search. That number would not be unusual. You have to cast a wide net because, after all, Bar Harbor is not everyone's cup of tea.

SM: I was going to ask you about that.

JE: But Prehn simply said that the Laboratory was of interest to him for a number of reasons. The first, of course, was its reputation for research with the mouse, and he was, after all, primarily a mouse person. Although a bit later he got interested in certain amphibians as well. Apart from that, however, he liked Maine and places like it: He liked Seattle, Washington, among other places. He was a sailor, a great sailor, specializing in Chinese junks, as you may have already learned from others, and, as a former Philadelphian, he had made various connections with the Jackson Laboratory, which has strong roots in Philadelphia, among the Trustees and other individuals. I think that Prehn had been known to Hank Neilson who was a member of the Board--if memory servies me correctly--of the Institute for Cancer Research in Philadelphia, Fox Chase. I think that's important if you want to follow up with Neilson. I can't remember whether Neilson knew Prehn personally, or just knew people who knew him in Philadelphia, but Neilson's input on Prehn through the Institute for Cancer Research was an important input at the time. So there were quite specific reasons for Prehn's interest and we did discuss with him at length his interest in and willingness to truly direct the Laboratory, and, in fact, we discussed--he and I discussed especially--the possibility that he would want to restructure the Laboratory to have a major Executive Officer, but my own thought was a situation comparable to Frank Schmitt's style of running the Department of Biology at MIT in the old days, where first John Loofbourow, and then Dick Bear were his Executive Officers. Ultimately, Prehn decided not to move in that direction, and he continued pretty much the style of several different Assistant Directors for different activities, as Earl Green had had, and I think that possibly his unwillingness to depart soon enough from Earl Green's style may have been a serious factor in his later undoing, because I think Prehn was a man who might have been better served by having everything funneled into one person immediately below him in the chart, and someone whom he could trust intimately. And he didn't do that very quickly. But there's no question that Prehn, at the time he was appointed, was widely believed to be a scientist of major stature. In fact, there is a letter in the files from a very distinguished man named ... Hellstrom, which states categorically that he believed that within the next five to six years, Prehn certainly would be a major candidate for a Nobel Prize. We didn't take that thing, I should tell you, at face value, although the committee had a very high regard for Prehn. Prehn is one of those people--of all the people at the Laboratory, I think, in terms of scientific contributions and style--Prehn was more like a Kaliss than a Snell. As I remarked earlier, I have a very high regard for Kaliss's work. Kaliss was quite an experimenter. He was quite bold. He got burned more often than Snell did, and this is a bit the nature of Prehn. I suppose Hellstrom saw Prehn in those periods when he was up high, and he sort of ignored the spots when Prehn missed the boat. But, in any event, the decision came down to Prehn and one other man who some members of the committee saw as a bit more a plodder than Prehn. It turns out that the other man is not quite a Snell, but is more on the Snell side, and that is Kaliss in a straight line, and he actually accomplished more than Prehn has. Whether he would have been a better Director is just anybody's guess. I think it is fair to say, however, that we didn't get our first several choices for the Directorship. I think Prehn's appointment has to be looked at in that light. I can't today say that he was the fourth choice, or the third choice, or the fifth choice. Fourth is probably the best guess. As usual, you are very careful not necessarily to virtually offer the job to everyone, but you feel them out and you're sure that they won't accept it, so that your final choice doesn't feel he's too far down in the pecking order. But we were comfortable with Prehn and Prehn got off to a very good start. But he found--he was ineffcent with chains of command. He bridled at having to report to committees of the Trustees. He was troubled at being essentially an ex officio member of each committee, and yet, at the same time, he did not want to give his Assistant Directors sufficient authority to really proceed on their own. He didn't want to be bothered managing and yet he was reluctant to let others manage. That's not unusual with some kinds of persons, people who are in administration for the first time. He was probably a better Director than a manager, if I can make that distinction. If somehow or other there could have been two Directors, a scientific Director,

and a managing Director, he might have been more successful.
SM: He himself has said this.
JE: That's a hard one, really, to assess. Probably what was needed was for Prehn to have one or two personal successes in his own research, and that just didn't happen. Prehn was the kind of man who, failing to really make a contribution of his own, kept stumbling, and each time that he did that, he would reach out and bring in someone else that he somehow felt might lead him to a discovery. He decided, for example, that he wanted to move into research in tumors in amphibians, especially in axolotls, which are relatives of salamanders, salamander-like animals, and there was a great colony of these in the laboratory of Louis DeLanney, whom I have mentioned earlier, in Seattle. And so, Prehn brought DeLanney and his axolotl colony to Bar Harbor, and, in addition, Prehn brought with him from Philadelphia at least one other investigator who worked with these amphibians, who turned out really to be not very good, and who subsequently left the Laboratory, and is now in San Jose with Prehn. Prehn did not use good judgment at selecting people removed from his own field. It's rather curious. Usually people behave just the other way, but he was too inclined with the person who was going to work immediately with him to bring people who were somewhat more weak. Perhaps he wanted to run his own personal show, himself, and therefore he didn't want

to tolerate highly independent people with him. But he would give them, instead of bringing them in--well, of course, DeLanney is a retired person; he could not bring in as a staff member--but others he brought in as staff members, when he should have brought them in on short-term appointments or as research associates, or something of that sort. And so, gradually, while there were some clear successes--the development of the new research building, for example, in which the major gift was a gift from the Fleischman Foundation, which I arranged. He made headway at that time, but gradually the Board began to lose confidence in him. He began really shooting from the hip, making appointments without going through the Board of Governing Trustees, reporting after the fact, ignoring the advice of the Board of Scientific Overseers, and ultimately appointed a major Assistant Director whom the Board of Scientific Overseers really found was just unacceptable. And it was at that time that the Trustees really didn't know quite what to do about the situation, and so I simply crystallized it, by resigning as the Chairman of the Board of Scientific Overseers, and Prehn took that as a vote of "No Confidence" and resigned. And so, resigning from the Board of Scientific Overseers, of course, then I also resigned from the Board of Governing Trustees, and that's why there's a hiatus in my record from '80 to '81. I guess my philosophy was since I'd brought him in, that he and I should step out together, so to speak. I wasn't certain that he would leave, but I was reasonably sure that that kind of signal would make him examine himself and his behavior. I think the tragedy of the situation is that he has not really effectively recovered from that reversal. To the best of my knowledge, he has not made a serious contribution in science since he left the Jackson Laboratory. He is a very complex man, I think a very intelligent and probably who history will show was better as a--the history of ideas will show him to be important, but not the history of discovery, and I think that's an important distinction. In genetics, for example, he would be more like a Tracy Sonneborn than like a George Snell, more like a Kaliss than a Snell. These are probably curious kinds of comparison that have no meaning, but that's the way I think of him. I think of Prehn as being important in the field of transplantation immunity and cancer and of having greater potential than he acted upon. I don't think that he will go down certainly, as the greatest Director in the history of the Jackson Laboratory, but I don't really think that, in fifty years, that it will appear that he was a poor Director. I think there were major things done at the Jackson Laboratory as a consequence of his being there. And certainly he was, in his style, a strong antidote to Earl Green, which I think was probably important. I think, again, then, the choice of Sanford, which is much more recent history, again was--

and this is one where I can't really comment a lot because it's such recent history, and I don't think one should say too much about the most recent history, just because it's too hard to view, too recent. But I think there's no question that the choice of Dr. Sanford was motivated by the desire to appoint someone with great skill, which was perceived as having great skills in management, a conservative, solid citizen. She certainly did not have a major record of scientific achievement. It's very hard. She did some science, serious science, but she's not a person known for science. And it was very clear that in her arrival at the Laboratory, the Laboratory was choosing someone unlike Prehn. There was the hope when Prehn came, that he would make major discoveries as well as directing the Laboratory. When she came, there was absolutely no pretension of major discoveries. She was selected to come in and reorganize the Laboratory, and that is essentially what she has set herself to do, and her selection was made during my year away from the Laboratory, and I think Dorothea Bennett was one of the prime movers in her selection. I think Dorothea Bennett may want to comment on that when you interview her. So that the Laboratory clearly has had several different styles of Directors. All of them individuals who found the Jackson Laboratory ... important to their own personal lives, and that is always a factor for the Jackson Laboratory, that many of the figures, either themselves or their families are just going to be reluctant to be in ... all the time. One encounters that problem to some degree, at Woods Hole, at both the Marine Biological Laboratory, and the Woods Hole Oceanographic Institution, and just yesterday, I went down to the Chesapeake Bay Institute, which is a part of Johns Hopkins University down on the Bay, where I'm helping to recruit a new Director, and one encounters the same kind of problem there--it's an hour and twenty minutes from either Washington or Baltimore, and in a very small town about the size of Bar Harbor, but even less attractive, I would say. And no one really wants to have to commute from Baltimore or Washington or Annapolis to work, and there are relatively few people who want to live in that kind of community. So that the Jackson Laboratory has its limitations.

Now, what are the great strengths of the Jackson Laboratory? Well, I suppose the first is its Governing Trustees. Now let's look at the Board. You may say, "Why is it a strong Board?" Well, there are some nationally recognized--and I suppose it depends on the field you're in, how recognized they are. But, if you compare the Jackson Laboratory Board with my Carnegie Board, my Carnegie Board of twenty-four has, I suppose, fifteen names that everyone would recognize: Bill Hewlett, and number one man at Dupont,..., and we formerly had Frank Stanton at CBS--people of this kind.

There are relatively few people of national recognition on the Jackson Laboratory Board, but there are a very large number of exceptionally devoted, hard-working people. I don't think I've ever, apart from my own Board, which is such a very special one--I think the Jackson Laboratory is the other really very good Board that I've worked with, far better than the Board at the Marine Biological Laboratory. Far better in several ways: "Devotion," I think, is an overworked word, but at the Jackson Laboratory, a very high fraction of the Jackson Laboratory Trustees do come to work, and they come to the meetings. I think if one looked at the attendance record over, say, a decade, you'd find it surprisingly good, compared to most Boards that I find. And I think, although they constantly say that it's not as good as it should be, the record of percentage of giving of the Jackson Laboratory Board is higher than most. It is true that there are no--probably no, it's hard to say with assurance--but there probably are no members of the Board capable of giving, say, ten million dollars. I'm sure there are some people who have given possibly five or one, or a half-million; there are lots of people capable of giving in the thousands, but it's important that they give to the extent that they can. That's the crucial criteria, but they do come. They come to work seriously. The meetings of the Board of Governing Trustees are handled very efficiently ordinarily, and there have been a succession in my time of quite serious, capable Chairmen. Lewis Lukens I remember early on, from the Philadelphia contingent, and Frank Gerrity, John Beck, Hank Neilson, Steve Petschek were all gentlemen with whom I worked at one time or another. And each had very positive strengths. A great weakness of the structure until recently has been the fact that there were two Boards of Trustees, a Board of Trustees, and the Board of Governing Trustees, and--I can't remember if they were called just "trustees"--I think they were just the "Board of Trustees," the so-called "big board" and the "little board." And the so-called "big board," as far as I could see, were largely summer residents and others who in other settings, might have been called Associates of the Laboratory, but up there were considered "trustees without portfolio," with no function: they didn't serve on committees. Presumably it was a group that could serve as a spawning ground for members of the Board of Governing Trustees, but in truth when one wanted to be a Governing Trustee, one looked at the larger board, didn't see much that he liked, and reached out to get a more serious person. So several years back, we began--I began--to push for reorganization. Also I should say that the Board of Governing Trustees was self-perpetuating to a fault. There people were on and on and on.

SM: Wasn't it passed down through families, even?

JE: Yes, and there's still, I'm afraid, some of that tendency today, but so that we have reorganized the Laboratory. I was the first talking...for this idea. There was a committee chaired by--I've forgotten who the Chairman of the committee now was--but there was a committee on reorganization of the Laboratory of which I was a member, but I simply wrote a paper describing some other organization, and we essentially used the model the Woods Hole Oceanographic Institute has, in which there is a corporation, rather than a so-called "big board," and one may be elected to the corporation and one may be re-elected to it for several terms. I think it's going to be limited to 100, if I remember correctly. Then the Board of Governing Trustees will be composed of twenty-four individuals, plus the Director and the Chairman of the Board of Scientific Overseers ex officio. Three classes were elected for a three-year term, and renewable for one three-year term but after six years, you must step off for a year. So that does give an opportunity for change, and we quite clearly want to view the corporation as a spawning ground for Trustees, and hope to involve these people in committees, and we're beginning now to try to recruit significant people. I just told the Chairman, Mark Boyer, that I just recruited two new members of the Corporation, Maxine Singer, who is a very famous molecular biologist, who is also a Trustee at Yale... and Bill Bevan, the Senior Vice-President of the MacArthur Foundation, who is coming on the Corporation. They'll both be elected this coming August. I assume they'll be elected this coming August. But that's the kind of person that we want to move toward, and we hope that we can--and in both these cases, in recruiting these people, I told them quite frankly that in a year or two or three, they might be themselves--if they like the Laboratory, and the Laboratory likes them--as possible Trustees, because I do think we need to reenforce the Board now, because I'm a little afraid that there may be a tendency by certain of the Board members to want to put their sons and daughters on the Board as they step out, like that, and that, I think, can be a little unhealthy if it's carried to too great an extreme. I don't think a Trustee should name his own successor, family or otherwise. Not the best way for enriching the mental resources of the Laboratory, but by and large, the Trustee organization has been positive and the Board of Scientific Overseers has been positive, with the one reservation that the manner of review of individuals by the Board of Scientific Overseers leaves something to be desired. I sometimes wonder whether the Laboratory should have, in addition to the Board of Scientific Overseers, whether they might have some kind of an ad hoc structure to bring people in just for a very specific review of one or a small number of individuals, when you get into sensitive areas. The question of tenure, as we mentioned earlier, is a longstanding problem, because obviously the Laboratory doesn't have the resources for providing true tenure. I wouldn't want to--of course, I'm in an organization which has none, which is an unusual one, and I have argued that the Laboratory ought to move in the direction which Cold Spring Harbor Laboratory does, of having essentially no tenure in the strict sense, but having a kind of revolving five-year terms, and that is that once you reach a position which normally, in other institutions, would be tenured, that you essentially have a five-year span, and at any one time, as long as you work satisfactorily, you have the potential of another five years. In other words, you have an automatic renewal. But the Laboratory hasn't chosen to regularize that kind of arrangment. They still hold fast to the idea of having the different staff titles, and one of them looking towards such tenure as the Laboratory may provide, but this has very little meaning, in the sense that there is no endowment.

Of course, another great strength of the Laboratory which, surprisingly, shows no sign of abating, is the mouse resource. There have been predictions of doom and gloom over the last decade that the number of mice used for research in the country will drop off, that people would turn increasingly to other kinds of experimental systems. That fortunately for the Laboratory, hasn't happened, and, of course, the fact that the Laboratory has maintained a very high quality laboratory product, I think, has been instrumental in keeping that going.

SM: Do you think at any one time there was a possibility that that mouse production facility would come to be felt to be more significant than the science?

JE: That's a constant worry on the part of some of the scientists, and I think a constant worry on the part of the members of the Board of Scientific Overseers, especially because the Board of Governing Trustees from time to time--many of them being businessmen--were taken by the fact that the mouse resource was an extraordinarily important one, and you could get a more interesting and lively discussion in the Board of Governing Trustees from time to time about whether the mouse production units were being handled successfully than about the science. That was the point that Rich Prehn came to the fore. Earl Green tended to emphasize mouse production, and Prehn, being more scientifically inclined, would try to talk about science. The problem was that Rich Prehn did not have the facility of talking about exciting science in an understandable way, and one of the things that I couldn't ever persuade him to do was to practice enough so that, when he gave his scientific report, to make it clear to the layperson. If he could have done that, he would have done a much greater service to the Laboratory. But you can't discuss science with Trustees at the same level you talk about it at a scientific meeting, and Rich had a very hard time learning that.

SM: Because several people have said to me--and these were all scientists--were concerned that the place be known as a "mouse factory," or a "mouse house."

JE: Well, it is well known in the nation and the world for that, but I think that its scientific reputation is also very solid. It's a very interesting test now as to how rapidly they can move into molecular genetics, how effectively they can move in and how they bring off the wedding of conventional genetics and molecular genetics. They have had more turnover, I think, among young molecular geneticists than one would like.

SM: I have interviewed two of them.

JE: Two who are still there?

SM: Well, they've just been there ten months. Both of them quite impressed with the degree of support they are receiving, both financial and moral support, too--encouragement, an active concern that they be content and stay and the recognition when they were being hired that they know Bar Harbor and that they know that environment, lest they have surprises, because apparently some come and--

JE: That's very important. It's the kind of place you either grow to like very much or you grow to hate very much, I don't know. It's not a place I could survive at: It's just too far from all the--symphony halls and that.

SM: If you talk about Woods Hole being out in the boonies, my goodness! Woods Hole is what? two hours and you're in Boston, and two hours from Bar Harbor and you're in--Bangor!

JE: No, it's not the sort of environment I could possibly endure, and my wife even less, but, on the other hand, in recruiting, you simply have to go for the Kandutschs and the Colemans and the Snells and so on. It means that your recruiting has to be geared to that. Another weakness of the Laboratory, which is gradually being improved, I think, is the relatively small number of post-doctoral fellows, compared to the number of staff scientists, and in today's period, I'm a firm believer in roughly three post-doctoral fellows for each staff scientist and that ratio is far, far below that.

SM: Many of them have said that. How would you like to see the Lab develop or grow?

JE: I don't want to see it grow.

SM: In size?

JE: Absolutely not. I think it may well be that the current Director and I, we differ on that, but I think one of the problems here is that, if you elect not to grow, then you have to be tougher in whom you keep and whom you let go. I have the advantage--I've had the advantage for the last 31 years--of working in a system where I could push people out the door if I want to bring someone in. The Jackson Laboratory could do it, but I think they are very reluctant to, I think, again, the difficulty in recruiting people into that environment, I think, makes them pause before they send someone away who's happy in Bar Harbor, because they're not sure that the next one is going to like Bar Harbor an awful lot, so that I think the recruiting difficulties make it more difficult to let people go, or very simple to go.

SM: I think another factor in that environment too, is the almost conscious legacy they have of it's being a "family," of a rather warm group with a great deal of camaraderie, that apparently C.C. Little fostered.

JE: Yes, and it's hard to send a family member away. Well, I believe that a place like that, you either have to maintain it at about the present size, or you have to make it very large. I think they're at about a critical mass now, it seems to me, for the kinds of things they do effectively. If you got it really far larger, then you would have to do much more mouse production, just to keep the flow of funding and I think that the mouse production probably is about as big as it should get, probably about as big as it can get, so that my own feeling would be that the Laboratory should not increase in size. I think some very hard decisions have to be made, as to the directions of research. The costs are different obviously, to do molecular genetics in mammals and mice, but I think they have some fields now where they should either strength them or get out, aging for example. They went through a period of really trying to develop programs in aging and Tibby Russell was involved for a time, and essentially now they have one person, and that's--

SM: Dave Harrison?

JE: Harrison, and Harrison has not become a major figure. He showed some promise...way to go, but I think now he is an average person, in a field that is sort of average. There are really very few leading investigators in aging in the nation. Certainly, Bar Harbor, the Jackson Laboratory, has not emerged as a center. I think a decade ago, some thought was given to trying to make it a major center, but you can't make a major center without somebody with some truly creative juices, and that didn't happen. I think Tibby Russell came into the field too late to really have an effect. I think the work on the genetics of blood cells, which was in its heyday with Tibby Russell has now developed into a somewhat average group, with Seldon Bernstein and Barker, who, I think, is a good, solid journeyman person, but not a leading scientist. And I think the immunology is not what it once was. There's no Snell and no Kaliss and the leading person probably is Bailey and Bailey has not emerged as a major figure. The Laboratory missed a great dent in the field of teratomas. In that, one of the real pioneers in that business was Stevens, but Stevens--this may have been an Earl Green problem, in part, I don't know--but Green was very reluctant to see modern cell biology and so on come into the Laboratory and I think maybe Stevens was brainwashed at some point or other, or maybe he just did it to himself, but that field has exploded and there have been enormous contributions from others, which have left him back at the gate. He was one of the true pioneers. At one time, Stevens was just about...

END OF TAPE ONE

JE:...would have emerged as a major scientist. I would have bet five years ago that he would have been a member of the National Academy of Sciences, but in truth, he got to a certain point, and then kept on doing the same thing over and over again. There are other major laboratories--Beatrice Mintz and the Institute for Cancer Research, and François Jacob at the Pasteur Institute in Paris, and many others used the teratoma carcinoma system to great advantage, exploring it in all different kinds of ways. So Stevens is sort of an important father figure in that field, but he has not really--and no one else at the Laboratory has really--taken on the system and run with it. So as we remarked earlier, when you look at that group of people, up there toward the top, there really are Coleman and Kandutsch, who sort of have a shot at being considered major national figures, in relatively limited bailiwicks, and then a very large number of people, as the Laboratory goes, who are quite good journeymen scientists--there's nothing wrong with them--they continue to get their funding but they are not likely to be people who make major discoveries, and I think the lifeblood of the Laboratory is having some fraction of people who you think have the capacity in some foreseeable time of making a major discovery. There are models of such places, the Carnegie Department of Genetics at Cold Spring Harbor, I'd say in the 1950's and 1960's was such a place, in which five or six major staff members all became members of the National Academy, and two won Nobel Prizes, but that's unusual, when you have 100% of winners, but that's extraordinarily rare, but you need a higher percentage than the Jackson Laboratory has now. The question is are Prehn and then Sanford--are they making selections of people in the newer groups, who have that potential? And will those people develop that potential, if they have it, in the absence of enough role models? So that's why the Laboratory of today--although it's financially very healthy at the moment--and it is producing. There's a flow of published work, but whether there's the potential, or the nucleus of major discovery, I don't think even the Board of Scientific Overseers can say right now. The Laboratory--institutions get a life of their own, and you have to ask the question--later on, not today: Some future historian will ask whether Barbara Sanford's achievements have largely been in reorganizing management. There's a lot of concern constantly expressed about the Laboratory that there are too many middle managers now. You may have heard that from others more qualified than me to speak, and there are. Of course, there is an awful lot of governmental regulation and there are more things to be done that require more people to do the jobs, but Barbara has surrounded herself with a great many middle level people, but beyond that, what has been her success in recruiting? and 2) It's much too early to tell--Prehn was not a great recruiter. That now is pretty evident.

SM: If the federal government cuts back in terms of funding, since so much of the Lab runs on NIH and NSF, and other kinds of grants, do you see any implication for the Lab, perhaps in the redirection of science, or the role of the Trustees?

JE: Well, I'm not too concerned about the federal government cutting back. See, the Laboratory is in a curious position, that is, that especially early on, the Jackson Laboratory is somewhat like the Woods Hole Oceanographic Institution, or this Chesapeake Bay Institute I visited yesterday. They're in a kind of--I won't say it in exactly those terms, but let us say that proposals come in in roughly similar quality from universities X, Y and Z and from one of these laboratories.

Now the university scientists have their salaries underwritten by the university, which are not underwritten by these other institutions. So the success rate, you'll find, if you look closely, and I wish someone would do a study sometime--the success rate of places like the Jackson Laboratory are a shade higher than at universities because there is a reluctance to cut off the salaires of these individuals. There's no question about it, but they're not obviously--if university X's proposal is substantially better than anybody from the Jackson Laboratory, the university grant will get funding; there's no question about that, but assuming they're roughly equal, the Jackson Laboratory person will have a slight edge, or the Woods Hole Oceanographic person, because this is the only source of money these people have and the foundations are, I think, reluctant to see too many people fall off the merry-go-round. So that, I think in the first place, if the Jackson Laboratory maintains its current level of quality, where its success rate is better than 50%, I think they'll be able to continue. I don't see that they'll be pushed in any direction. They tried to take themselves toward aging, because they thought there was money there, and they didn't succeed. I don't really see a substantial change in the direction of research directed by the financial constraints. The Trustees are not a major force to be reckoned with at the Jackson Laboratory. I suspect the level of giving of the Trustees is just as small, and there is a new Development Officer, who, at least, in just one year's experience with him, looks to be the best the Laboratory's had in the history of the Lab. There really have been just three major people--Allen Russell (perhaps he's someone that you might interview)--he was at the Laboratory for a very long time and was assisted by Bill Dupuy for a good period of time. Then there was--oh, there may have been some others in and out--and then Jim Baldwin, who unfortunately died last year, so he's not available for interview. So, at the Laboratory, one of the problems is that the Directors do not realize that the Development Officer does not himself raise money: They make it possible for the Director to raise money. Major people don't want to deal with Development Officers: They want to see the principals. That was very hard for Prehn to understand: He just expected the Development Officer to do it and they never will do it. And I think Barbara Sanford is finally understanding that, and is now willing to invest more of her time in seeking money. Of course, you see, there is a point where the mouse resource becomes a negative factor, because many foundations will see the mouse resource making a profit, which fortunately the IRS hasn't seen quite yet as a profit. It is related to the major purpose of the Laboratory, and as long as they can continue to make that argument, they're safe.

But nonetheless, there are foundation officials who feel the Laboratory is well enough off. So it takes a very innovative kind of approach to foundations.

SM: Do you think you can possibly stand back from history and your own time, and evaluate the Lab's impact on 20th century American science?

JE: Well, not in any depth: It's too early, but I think there's no question but that the Laboratory has had an impact through specific individuals. Very clearly the thrust of the Laboratory in the study of histocompatability--I think that alone would have justified the Laboratory's existence. But I think there have been a number of other things, like the work of Russell and others that have been work of major consequence, but nothing that stands out like the leadership of Snell and related programs. I think secondly, the continuing role of the Laboratory in emphasizing the genetics of the mouse and the mouse as an experimental system. I would put that a step below the histocompatability contribution, but I would not demean that. I think that the combination of the Laboratory focussing continuing emphasis on the genetics of the mouse, coupled with the Laboratory as a mouse production facility--I view those really as part of the same overall picture--I think the combination of those two achievements really make the Laboratory a major center. I think if, apart from the histocompability work, that the

Laboratory will not stand in history as a great center for ideas, compared to say, the Marine Biological Laboratory, or Columbia University, say, in the history of genetics, or the Marine Biological Laboratory in the history of development--things of that sort, but I think the Laboratory has had a very distinguished fifty years or so. There's no question about that. I've enjoyed being associated with it on the periphery, and I'm anxious for it to do better.

SM: Everyone is that I speak to. No one is complaisant. Do you have anything else to contribute?

JE: I think that's enough.

END OF INTERVIEW