6-14-1986

Earl Green Oral History

Earl E. Green

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Interviewer's Comments

Narrator's Name: Dr. Earl Green

This remarkable interview is just what one would expect from the Jackson Lab Director whose conscientious (compulsive?) attention to detail and careful planning are legendary. When I arrived (at 7:50 AM), Dr. Green was ready, with his 21+ pages of notes, in outline form, on every aspect of my outline that he wished to discuss. I could tell we were going to have a long interview! In fact, we talked for most of eight hours, although only c. four of those hours are represented on the tapes.

What is provided here are the recollections, drawn carefully beforehand from his personal records, of the Lab's most methodical and organized Director. Green's nineteen year "reign" left an indelible mark on Jax and its employees, from scientists to sanitation workers, as many of the other interviews in this collection attest, in their numerous references to Green.

Green is not a light-hearted soul, and his tapes are not conspicuous for funny anecdotes, but he does include interesting stories, which flesh out incidents mentioned elsewhere, e.g. Harrison's and Coleman's allusions to the IRS investigation, and Kendall Young's tax suit. Green also provides some vivid detail of the Lab staff moving the library in one festive "party-like" day.

Because he knew precisely what he wanted to say, the order in which he wanted to present things, and what topics he wanted to avoid, this interview is really a monologue. By day's end, I had managed to pose but a few of the more pointed questions my earlier interviews had provoked me to try to pose. I could see Green would not take readily to much debate, or verbal sparring, so there is little of that here.

Green's term at the Lab was a critical period, during which the Lab became institutionalized in structure and style (some would claim too much so). As we parted, discussing the current status of the Lab (off tape), Green said he felt it was vital to the Lab's spirit that scientists be the administrators, and that, in recent years, their refusal to take the time to do administration part-time would have a pernicious effect in the long-term. History will be the judge of this.

Value this tape as the most thorough of presentations, by one of the narrators with very complete and detailed records from which to draw his facts.
Narrator: Earl Green

Jackson Lab
Allegheny
Meadville, PA
Dr. Chester Arthur Darling
Green
Allegheny
Roscoe B. Jackson
Bar Harbor
C.C. Little
A.M. Cloudman
J.J. Biltner
L.C. Strong
Brown
Paul B. Sawin
Bill Russell
Elizabeth (Tibby)
Chicago
Elizabeth Fekete
George Woolley
Edinburgh
Scotland
Walter Heston
Lloyd Law
Elizabeth Keucher
Mrs. Gorer
Annie Moore
Don Harris
Norman Shaw
George Snell
Prexy Little
Margaret Creighton
Ohio State University
Alderseas
Washington
Hugh Knowlton
Harvard Club
Los Alamos
Columbia
Iowa State
Allen Griffin
Boston
Jack Schlager
E.B. Wilson
Orono
John Graydon Kidd
Dale Foley
John Fuller
Seldon Bernstein
Al Russell
Jordan Pond House

George Lefevre
Ed Murphy
Kresge
Morrill Park
Sheldon Goldthwaite
Union Mutual
Portland
Nelson Rockefeller
David Rockefeller
C.C. Little
Clarence Cook Little
M.G.L.
Frank Gerrity
Yale
Bill Murray
Don Bailey
Walter G. Arnold
Charles River
Cambridge
Douglas Coleman
William M. Evarts Jr.
Mr. Blake
Hank Neilson
California
Pasadena
Eleanor St. Denis
A.B. Mosher
Dr. Gilmore (Edward B.)
Bigelow Lab
Japan
France
England
New York
Pittsburgh
Washington
Vancouver, British Columbia
Muriel Davison
Western Reserve
Neal Miller
Peter Godfrey

Terms
lumbar vertebrae
thoracic vertebrae
genotype
Bal/B-C strain
(bag albino)
placenta
osculum
histotechnical
sipper tubes
ectromelia
salmonella typhi-murium
autoclaved
DBA-2
DBA-1
lymphocytic leukemia
leukocyte
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Place Bar Harbor, Maine

Date 9 June 56

Narrator

for the Laboratory
Interview Contents

Dr. Earl Green

First hearing of TJL, 1
Caring for the mouse colony at Allegheny College as a student, 1
Doing experiments with the mice, 2
Getting into genetics, 2-3
Going to Brown University for graduate work, with Paul Sawin, 3
Writing to C.C. Little and coming as a summer researcher, 1938, 4
Doing a Ph.D. at Brown using mice, 5
Returning to TJL for the whole summer of 1939, 6
Attending the Annual Meeting that year, 6
Tending the Russells' mice that summer, 6
GS inducing a translocation by x-rays, 7
TJL in a dense evergreen forest, 7
The physical plant in 1939, 8
The bedbug problem, 8-9
C.C Little painting with kerosene against the bugs, 9
Returning to TJL in 1946, 10
Sending breeding pairs to TJL after the fire, 10
Working for the AEC, 1953-55, 10
Seeing TJL as a dynamic place, 10
Participating in the 25th Anniversary symposium, 11
Becoming TJL Director, 11-12
His general recollections of the early Lab, 12-13
The two types of scientists at TJL, resource and bench, 13
How TJL is unique, 13
TJL's enduring commitment to its colony, 14
The research project as the functional unit of TJL, 14-15
The uselessness of departments at TJL, 15
The advantages of not having departments, 16
The three functions of TJL: research, training, production, 16-17
TJL as not democratic, 17
The staff operating independent fiefdoms, 18
The Trustees as the legislative body, 19
His difficulty in weaning the staff away from their democratic notions, 19
Role of MG at the TJL, 20
Arranging for the determination of MG's salary, 21
The Annual Meeting procedure under his tenure, 22
Staff difficulties in dealing with him, 23-24
Developing the Monthly Summaries as a way to keep Trustees informed, 25
The format of the Monthly Summaries, 26
The procedure to handle review of grant applications and manuscripts, 27-30
Overhead costs as a limitation on the projects TJL could undertake, 28
The excellent manuscript review system of TJL, 30-31
The qualities of a good reviewer, 31
The administrative organization of TJL, 33
The decision-making process, 33-34
The various committees, 34
The "placental theory of decision making," 35
The Trustees' appreciation of this arrangement, 36
The need to have scientists manage their own institution, 37
Principles he used in administering TJL, 38
Major building projects at TJL during his tenure, 39
NSF as a major donor to these, 39
Building Unit 5, 39-40
Financing Morrell Park with no public monies, 41-42
The Surgeon General's feeling that TJL was vital to the health research of the Federal government, 43
Additional capital improvements: the C.C. Little Conference Center and Library, and the Mammalian Genetics Laboratory, 44
Having the MGL named for him, 45
The change to steel cages, 46
The danger of disease to the colonies of mice, 47
Bill Murray's objections to giving up wooden boxes, 48
Dale Foley finding a source for metal cages, 49-50
Bill Murray leaving TJL, 50-51
Charity Waymouth's newsmaking discovery, 51-53
The mouse mix-up leading to improved tagging systems, 54-56
The problem of animal health, 57-58
Hiring veterinarians to monitor animal health, 59
Taking advantage of the once-in-a-lifetime chance to clean up the mouse colony, 60-61
The budget process at TJL, 62-63
Paring the budget during a lean year, 64-65
The self-study seminar in the early 60s, 66
The Natural Mutation Rate Study, 66-67
The court case on TJL's tax exempt status, 68-72
Having to pay dwelling tax to Bar Harbor, 72
Putting out a new edition of Biology of the Laboratory Mouse, 73-75
His two Manuals, on procedures and style, 76-78
Refining these subsequently, 78-79
The Manual of Format and Style, 80
The origins and operation of the Board of Scientific Overseers, 80-82
Attempts by the staff to rotate members of the BSO, 83-86
Some of the members of the BSO, 84-85
Criticisms of the operating procedure of the BSO, 86-87
His changing the working hours of TJL, 87-90
His calling for input from supervisors, 89
Moving into the new library, 90-92
His assessment of JS, 91-92
The IRS review of TJL's status, 93-103
His reply to DC's criticism, 96-99
Trustee and staff reactions to the IRS investigation, 99-101
The IRS recognition of their non-profit status, 103
Why he retired early, 103-105
The state of his health requiring his early retirement, 105-106
His post-retirement activities, 107-108
His attitude about being an administrator, 109
TJL's retirement policy, 109-111
Flexibility in retirement policies being hard to achieve, 111
His opinion regarding discrimination and economic realities, 112
Sabbatical vs. research leaves, 113
The ideal size of TJL, 114
Implications of size, in administering TJL, 115
The notion of "critical mass," 115-116
Collateral Materials Report

Narrator's Name  Paul Green

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The Jackson Laboratory
INTERVIEW DATA SHEET

This section is to be completed by the Interviewer.

Narrator: Dr. Paul Green  Address: 1013 Bay Haven Rd, Canaan, ME 04624 Phone: 207-288-4660
Birthdate:  Birthplace:  
Date(s) & Place(s) of Interview(s): 9 June 1986, Canaan, ME
Collateral Material: Yes  No  Terms: Unrestricted

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EG 1

This is the tape of an oral history interview of Dr. Earl L. Green, given as part of the Jackson Laboratory Oral History Project, sponsored by the Acadia Institute. This interview was held on June 9, 1986, at Dr. Green's home on Seely Road in Bar Harbor, Maine. The interviewer was Dr. Susan E. Mehrtens.

SM: How about I begin by asking you when you first heard about the Lab?

EG: I first heard of the Jackson Laboratory about 1933 or '34. This came about, because, at that time I was a student at Allegheny College in Meadville, Pennsylvania. I had completed my Freshman year in college in June of 1932. I was walking across the campus one day when Dr. Darling, Professor of Biology (Dr. Chester Arthur Darling), was walking toward me and when he got up to me he said, "Say, Green, you live in town don't you? How would you like to take care of the mouse colony this summer? The chap who was going to take care of the mice is going to be away." So, I said, "Yes." I'd be happy to do anything for Dr. Darling. He was a wonderful, gruff old man with a--well, he wasn't really so old but he was a lot older than I was--with a very kindly disposition towards students. He was a great practical joker and he liked to get people into situations where he and they could have a good laugh. But anyhow, he asked me if I'd like to take care of the mouse colony and I said, "Yes." So, in a little while I was involved in cleaning out the mouse cages and changing the mice from the dirty cages to the clean cages.
And I learned the routine of doing that in a matter of a few minutes. After this had been going on for several weeks, changing the cages once a week, Dr. Darling asked me if there was anything I'd like to do with the mice. Well, as a matter of fact, I had been thinking about it but I hadn't been thinking very profoundly. I told him there was a breeding experiment I'd like to perform and he asked me some questions about it. And his questions made me realize that I had not really thought out the problem very well at all. In fact I realized I had to know a whole lot more before I could even think about doing a breeding experiment with mice. But with this problem as an interest, I started to read about genetics and I started to read about probability and got into the morass of notations and symbolism that the probability theorists had developed and I realized that although I wouldn't be able to go very far on my own with it at least I might be able to get started. So, what happened was, I continued with the mice. The chap who had been taking care of the mouse colony returned in the Autumn but he apparently had lost interest in doing the job. So, for the next three years, I took care of the mouse colony at Allegheny College. I also took a course in genetics which was very enlightening because of my interest. But in addition I took a Library study project, which was to read about genetics and to read about the genetics of the mouse.
specifically. But while reading about the genetics of the mouse I kept seeing papers by people who were located at the Roscoe B. Jackson Memorial Laboratory in Bar Harbor, Maine. Well, of course at first, it meant nothing to me. It really meant just someplace outside Meadville, which was the limit of my experience. But after a while I saw this name so often, and I began to see the names of people who were connected with the Jackson Laboratory: C.C. Little, in particular, A.M. Cloudman, J.J. Bittner, L.C. Strong, as well as others. So, these names began to seem like the names of old friends. Well, in due course, I finished at Allegheny and went to graduate school at Brown University. I started there in the Autumn of 1935. At first I worked with rabbits under the supervision of Paul B. Sawin, who eventually became a staff member at the Jackson Laboratory. But in the summer of 19--probably the summer of 1938--I had decided that I wanted to resume my research with mice, in fact I had been taking care of a mouse colony at Brown University ever since I was there, even though I was not using them for research. I have to explain a little bit of why I came to the Jackson Laboratory. I first came here as a student in the latter part of the summer of 1938. Paul Sawin had found a variation in the skeleton of rabbits and this was sort of a minor discovery in that theretofore he thought that rabbits had twelve pairs of ribs and seven lumbar
vertebrae. But he found that some rabbits have thirteen pairs of ribs and six lumbar vertebrae. One vertebra was thoracic instead of a lumbar vertebra. Well, I thought if this variation occurs in rabbits, then surely it must be occurring in other organisms as well. I didn't realize that there was already a vast literature on this sort of thing. I thought there must be a similar variation in the mouse and if I could find the similar variation in the mouse, the mouse would be a somewhat better organism with which to study variations than the rabbit. They are smaller; inbred strains are already available; larger numbers of offspring are possible; controlled matings are possible; there is a more rapid sequence of generations. And so, I was highly motivated to come up to the Jackson Laboratory and find out whether there were differences in the skeletons in the inbred strains. So, I wrote to Dr. Little and he wrote back a very warm letter to welcome me—welcoming me to come to the Jackson Laboratory and he would put me under the general supervision of Bill Russell, who was on the staff at the time: Bill and Elizabeth or Tibby Russell, had recently moved to Bar Harbor from Chicago. I got samples of mice of the various inbred strains available at the Laboratory and carried out the staining process—the dissection and staining—so as to reveal the skeleton and very promptly found that there were marked differences between strains but more importantly there's a
lot of variation within strains. Some people are very perturbed by the existence of variation within an already highly inbred strain because our dogma says that the animals within an inbred strain are genetically alike, that we know how to make them genetically identical, having the similarity of identical twins of human beings. So this variation within strains is in itself a little bit of a puzzle. The obvious question is: Is this some individual genetic variability that has not been removed by inbreeding? or, alternatively, is this nongenetic variability that's independent of any genes? The general level might be influenced by the genotype and around that level is a lot of variability. Well, the conflict between the genetic and nongenetic variability was, and had been, an interesting argument among geneticists and indeed it still goes on. The nature-nurture controversy that still exists many traits in animals as well as in human beings. One of the strains that I discovered as being highly variable is the one now known as the Bagg Albino or BALB/c strain. They didn't have that notation in 1938; that's the latest notation. I decided to use that strain as the basis for my study for the Ph.D. degree at Brown University, which I did and that work has been long since published. Having come here in 1938, I met C.C. Little, Bill Russell, Tibby Russell, Elizabeth Fekete, George Snell, Arthur Cloudman,
Johnny Bittner, George Woolley. All names that were familiar to me through my previous reading in the literature. I actually was here only about a month in 1938 but I came back in 1939 for the entire summer. Bill and Tibby were going to the International Congress of Genetics meeting in Scotland, that summer and Bill asked me if I would take care of his mouse colony. Well, that went very well except, you may recall, that war broke out in the Autumn of 1939 so there was serious concern at the Laboratory that the Russells were not going to be able to get back. Actually they did and had very interesting stories to tell. During that summer I also met Walt Heston and Lloyd Law both of whom were starting their work in mouse genetics at that time and both of whom, subsequently, became very famous as mouse geneticists. One of the events of that particular summer that has stuck in my mind is that, since the Russells were away and I was in a sense substituting for Bill Russell, Dr. Little asked me if I would attend the Annual Meeting of the Laboratory, sitting in for Bill Russell, so to speak. I had no conception of what he meant by Annual Meeting so I thought that this would be something to partake of. So, the front office was cleared. This was an office that was occupied by Elizabeth Keucher (who is now Mrs. Gorer); Annie Moore; and Don Harris. It had three desks in it. So, the desks were all pushed to the side and it was made into a small meeting room. On the
appointed day in late August, C.C. Little, as Director and
Norman Shaw (former Judge of the court of Bar Harbor),
serving as Clerk, sat at the front of the room. And the
Trustees assembled and the business was transacted, which
I didn't understand at all. But I do remember two things.
One was: George Snell was asked to give a talk on his recent
work and he explained, by use of diagrams and charts, how he
had induced—by means of x-rays—a translocation in the
mouse (a chromosomal translocation). This turned out to be,
in later years one of the very most famous of the
translocation and is still in use in genetic work with the
mouse. The other thing that I remember is: one of the
Trustees said that he thought that it was very dangerous for
the Laboratory to be located in this dense evergreen forest.
In fact, you could open one of the windows of the room
the meeting was being held in—you could open the window and
reach out and touch an enormous spruce tree about 15"-18" in
diameter. Well, the vision that a fire might sweep through
the place seemed to me to be so ludicrous and indeed it must
have seemed the same way to everyone else because everyone
nodded quite indulgently—yes that's a very good point.
Then they went on to the next item on the agenda. Nobody
paid any attention to what he said. Well, of course it was
only eight years before the fire that actually burned
down the Laboratory. Imagine what would have happened if
that had been taken seriously. That would have meant that an area around the Laboratory of oh, 50 feet, let's say, would have been cleared of vegetation and what a howl that would have created to cut down all of those beautiful trees under those circumstances.

SM: But it is interesting how prophetic that was.

EG: Well, there was another episode that went on that particular summer, the summer of 1939. The Laboratory, at that time, was just one single, more or less oblong or rectangular building. The animal quarters were on the top floor and the offices and laboratories were on the ground floor. Well, the top floor had probably several thousands of mice, but it also had several millions of bedbugs. The boxes that the mice were kept in were made of wood and that meant that there were crevices in the boxes, but at least they were taken out once a week and immersed in hot water and cleansing fluid and washed. But, the racks were also made of wood and while they were well made racks, still with use in due time, there were gaps between the horizontal members and the vertical members of the racks. And every crevice was packed with bedbugs. And then the walls also had various pieces of wood on them and everywhere that there was a crevice they were just packed with bedbugs. The problem was to combat the bedbugs as much as possible. Some experts on bedbug eradication were brought in now and again
for consultation and after these experts disappeared or departed there would be a big flurry of activity trying to eradicate the bedbugs. The best recommendation, apparently, was to paint all these crevices and cracks with kerosene. This was to kill the bedbugs in place. Of course that meant putting an awful lot of kerosene around. But of course kerosene won't flow uphill, so, anything that was overhead was almost impossible to get kerosene up there.

There I am, a young man, a graduate student, thinking that some day I'm going to get a Ph.D. degree and I'm going to have to be more responsible than I was then, about how the world works. And so, I'm looking at the people at the Laboratory, these are all mouse geneticists and I'm watching them to see what a mouse geneticist does. So, my mind was prepared for what I then saw. One day I saw that C.C. Little's mouse room were being evacuated. All the mice were being taken out, taken somewhere else. And then the next day I came by the same room and there was Prexy Little standing on a platform with a little bit of scaffolding painting the crevices up around the edge of the room with kerosene. And that was a good lesson for a young man not just interested in the practical problem of eradicating bedbugs, but in a very much more esoteric problem--how does one behave in the position of leadership. It was a good message for me. Well, shortly after the summer of 1939, of course the country was
engaged in war and I myself was in the Air Force. I was discharged in 1946 and Margaret and I--maybe I should back up and explain who Margaret is. Margaret Creighton became Margaret Creighton Green in 1940. We were in Chicago at that time, then I went to the Ohio State University and then was sent to the Air Force and then in 1946 I was discharged from the Air Force. So, Margaret and I, wanting to get our roots reestablished in mouse genetics, came to the Jackson Laboratory as summer investigators. We were sponsored by Bill and Tibby Russell. We lived at Aldersea, a former property of the Laboratory, located on the shore in Bar Harbor, along with a large number of other summer investigators. After that summer was over, my contact with the Laboratory was very spotty, except for the fact that in 1947 after the Laboratory burned down, I was among those who sent breeding pairs of various strains of mice back to the Laboratory. In 1953 to 1955, I was on the staff of the Atomic Energy Commission in Washington, in charge of the genetics program of the AEC. And this caused me to have occasion to visit the Laboratory because the Laboratory had received some research contracts from the Atomic Energy Commission. By that time, I must say, the Laboratory seemed to me to be a more dynamic place than it had been years earlier. In earlier years, the staff was small with a relatively narrow interest, the interest was in paths of
biology and immediately related fields, but by 1953 to '55, the Laboratory was already expanding into a wider area of research fields. We got into physiology, behavior and so on. And it was taking on the aura of a more general purpose research institution growing out of its more limited domain of mouse genetics. In 1954 the Laboratory held a 25th anniversary symposium, commemorating the founding of the Laboratory in 1929. Elizabeth Russell was the primary organizer of that symposium and she invited me to give one of the talks, I think it was the first talk of the symposium. I may be wrong about that but, I think it was the first one. And I gave a talk on the skeletal variations that I, in the intervening years, had studied in the mouse. So, I became quite familiar with the Laboratory by that time.

You can imagine my surprise when in the spring of 1956--I knew the Laboratory was looking for a Director, but I had no thought that I would be considered. I was, after all, only slightly more than 40 years of age at the time. But, in the spring of 1956 one day I got a call from C.C. Little who asked me if I'd be interested. And if so, would I meet him and Hugh Knowlton at the Harvard Club in New York the next following Sunday, which I did. I talked over the whole situation with them and about ten days or two weeks later Hugh Knowlton sent me a letter. Actually, I was out at the Los Alamos Scientific Laboratory at that specific time. I was
there for two weeks giving a class on biological statistics to the staff of the Los Alamos Laboratory. Hugh Knowlton sent me the letter, which I received in Los Alamos, offering me the Directorship of the Jackson Laboratory. That was in June of 1956 and Margaret and I moved here in late September of 1956.

SM: Would you like to say a few things now about the general recollections you have of the Lab?

EG: Well, general recollections, yes. I'm overwhelmed with general recollections. I have a very, very large number of them. Possibly I should first start by talking about the research staff itself. I don't mean talk about individuals, but the kinds of people that make up the research staff and why these people are desired at the Laboratory. It's almost fatuous to say that the Laboratory is a unique institution. It's fatuous because every institution is unique in some respect. But, its uniqueness needs description. The Laboratory is unique because it is a repository for mouse germplasm. It is the primary repository in the world for mouse germplasm. This means a large number of inbred strains of mice are maintained at the Jackson Laboratory and perpetuated generation after generation. It means that mutants, animals that deviate from the type of their particular strain, are discovered and many of those are also perpetuated in a variety of quite sophisticated ways. And it takes expert knowledgeable people to do this sort of thing.
It's not just a routine operation so, this Laboratory has, and I'll say has to have, two kinds of people on its research staff. There have to be those people who are able to perpetuate the germplasm of the mouse, to keep the inbred strains and the various other kinds of strains—the congenic inbred, recombinant inbred, and segregating inbred strains—keep them in good shape so that they are available both here and elsewhere. In addition to those scientists, who I might call resource scientists, there can be another group, which I'll call the bench scientists, as a easy label. These are the people who are not directly responsible for the perpetuation of any of the mouse strains, but whose research interests have led them to want to work with mice. And so, being here means that they can exploit this resource to a maximum advantage. They could maybe just as easily be working somewhere else where they could get mice from here or elsewhere. But, nonetheless, carrying out that sort of work right here is desirable. So, the staff has to have these two kinds of people. I'll say the central core of people, made up of the geneticists, who, in addition to carrying out their own research, have a responsibility for perpetuating the various inbred and mutant strains of mice. It's this feature that makes the Jackson Laboratory uniquely different from any college or university. Even though a certain college or university may for a certain period of time have a thriving
mouse colony, let's say, or a guinea pig colony or rabbit colony or whatever. A university is not in a position to make an enduring commitment to the colony. A university is concerned with education. It has an enduring commitment to young human beings. It will make a commitment to a mouse colony, let's say, only as an outgrowth to a commitment to a particular professor. When the professor passes on the university has no purpose in keeping the mouse colony. And why should it? And so, we have seen at numerous institutions a flourishing mouse colony. I'm referring to Columbia, Chicago, Iowa State College, numerous places who've seen a flourishing mouse colony only to discover that it is disbanded upon the death or retirement of the relevant professor. Not so at the Jackson Laboratory. The Jackson Laboratory now has more than 50 years of tending to mouse colonies that C.C. Little started here in 1929. And it's an expanding and wondrous resource. So far as I know there is no other institution in this country that has that kind of commitment. There are some that may turn out to have it, but the evidence is not in yet. Now I'd like to talk a bit about the organization of the Laboratory. Ordinarily one would expect that to mean a table of organization, but I don't mean that at all. What I really mean is: What is the functional unit within the laboratory?
It took me a little while after I became Director to realize that the fundamental functional unit, a sort of cell at the Laboratory, was the research project. A research project is a unit of organization because a research project has people, it has purpose, it has space, and above all it has funds. The accounting and budgeting is based upon individual research projects. These projects can be assembled into two or more and make programs, but that's merely arbitrary grouping of the projects. A given staff member may be concerned with the one project or a given staff member may be concerned with two or more projects. Or a particular project may have two or more staff members concerned with it. And that gives an idea of the flexibility of the term project. It can be used to apply to any aggregation of research talent and equipment and mice and space to bear upon a particular issue. This means, then, that, with respect to organization, there is no purpose served by having the Laboratory organized into departments. Departments are conventional, and probably necessary, features of university organization. Departments of instruction--department for this, department for that. But there's no advantage in that arrangement within a research laboratory. It's better to think of the projects as being the units and enable the projects to come into existence and live while there is interest in them and while
there are funds to support the purpose and then to disband. So there's a constant turnover of projects. You wouldn't want to have a constant turnover of departments in a similar way. Thinking of the Laboratory as organized in this way enables one to promote an easy association between staff members. So that a couple of staff members, who happen to meet at the water fountain or at a mail box, get to talking with each other, and they discover that they have a common interest in a particular thing and thus, a project is born. The next thing is to get a few ideas on paper and then see the administration about assignment of space and privilege of applying for a research grant. And if that succeeds, then there is a new project where one did not exist before. On the other hand, of course, when ideas run out or people leave, the project can easily close down. The traditional table of organization, which the Laboratory has to have for other purposes, is nonetheless for internal operation—the traditional table or organization showing the Director, and so many Assistant Directors, and other functions—is useful really for only the administrative side of the Laboratory. Not at all useful for the research side. While I was Director, I was forced to have a table of organization to display to various outside agencies who wanted to know how we were organized. I could describe the project but I also had to have a visual display of our organization. I came to like the arrangement that showed the Laboratory as having
three functions. The research function, primarily; a training function, secondarily; and a production of animals for research as a tertiary function. But then I would say that the same staff was concerned with all three of these functions. I stopped saying that after awhile because I realized it only bewildered the people to whom I was trying to explain the organization and function of the Laboratory. Another item of the Laboratory that I might talk about is under the general heading of democracy in a laboratory. We live in a society that is said to be democratic, and it is indeed in many respects, and so we like to carry the ideas of democracy over into the workplace let's say. But to what extent can we really apply the principle of democracy in a laboratory? I raise that as a question because the question is, I felt is, one we are eternally faced with. To begin with, at the Jackson Laboratory the Director is appointed by the Trustees. So the Director is not someone who campaigns for office and if elected he is fingered for the job--he is appointed by the Trustees. Similarly, the Director appoints the staff members. He usually carries out this appointment only with the scientific overseers. The Director appoints staff members with the approval of the Board of Scientific Overseers and with the Trustees. So where is the democracy in that arrangement? It's a little hard to think of staff members as being comparable to
property owners in a village who have voting rights to determine how their village shall operate. And yet it is necessary in a contemporary organization to arrange for staff members to have as much say as possible in the operation of the Laboratory because, of course, they are the whole purpose for the Laboratory to be in existence. The way the Jackson Laboratory evolved, at least in my view, I came to think of the staff members as operating independent fiefdoms. They have their projects, they have their budgets, they have their space, and so they were operating in this independent or at least semi-independent way, that the Laboratory was really in effect a confederacy made up of these independent fiefdoms, or semi-independent fiefdoms of the staff. I think, in social organizations, there is probably some great historical precedent for this sort of confederacy, and there, indeed, is strength in it. However, we still have to recognize that within a Laboratory, in which the Director is appointed and the staff members are appointed--while the staff members have great input in determining who shall be appointed to succeed them--nonetheless, there is not an outright voting, a counting of hands to determine what the outcome should be. So the Laboratory staff, by itself, can not operate as a legislative body. It can not practice democracy in that sense. This is often a source of confusion. The Laboratory staff can't operate as a legislature because the only legislature that the Laboratory
EG 19

can survive with is the legislature that we call the Board of Governing Trustees. They are the legislative body. You can't operate with two legislative bodies; one serves the complete purpose. A bit of confusion arose out of my practice of having a policy committee. The policy committee was made up of some members of the administration and some members of the research staff, it was appointed by me. But almost every year when we had our first meeting, sometime in early Autumn, one or more members of the policy committee intuitively felt that the committee was going to be the legislative body for the staff. And I adopted the practice of explaining at each of our first meetings, that the policy committee was advisory to the administration and that the administration was advisory to the Board of Governing Trustees, which was our legislative body. And that we, as a staff, did not have any power of deciding anything unless the power to make such decisions had already been conveyed to us by the Trustees. But it took some staff members quite a bit of mental wrenching to realize that this was really the best way for the Laboratory to operate. That they were not comparable to property owners in the village. They were, however, operating as sheiks with their independent fiefdoms. Another matter that might be of interest to talk about is having one's wife as a staff member. Margaret came to the Laboratory as a staff member, that was agreed upon at the
beginning. But it was very clear to me, as well as to her, that her appointment as a staff member could not be exactly the same as any other staff member. We had had some experience at the Ohio State University before coming here, where for a certain period of time, she too was a staff member—a faculty member. And out of having to think about our relationships, with each other and our relationships with the institution, we had developed some very good concepts—good for us—concepts that were good for us about how to operate. The first thing that we established in coming to the Laboratory was that Margaret should have her completely separate laboratory. And she should have completely separate projects and functions, in that sense, completely separate from me as the Director. And I too, would have my research projects independent of hers. That we would no longer try to collaborate, as we had done before, and I think we had been successful at. In coming here we thought we should operate separately. Then when she came to apply for a research grant, I arranged that her application should be reviewed by someone other than me. And that the person who signed the application should be someone other than me; the Associate Director or Administrative Director, should sign the application. Further, I decided it would be better if she did not serve on any committees. So to the best of my knowledge, she is the only staff member, who occupied space
there for nearly two decades, who did not serve on any committees of the staff concerned with administrative problems. All other staff members got swept up in one committee or another, sometime or other. But Margaret escaped that. Specifically, she never served on the policy committee. I think she's the only staff member that never served on the policy committee. Maybe, most important of all was the arrangement we made with respect to her salary. She determined that as the wife of the Director she should receive only an 80% salary, equivalent to say, four days a week or four-fifths time. Actually it didn't matter to her, she would work full-time and would have worked full-time no matter what the rate of her salary was. But then the question was: Who is to fix her salary? Even at 80% time, the salary should change with the passage of time. So we had a very special arrangement about that. I had a salary review committee of three members of the staff, including myself, and I was chairman of the committee. We reviewed all the salaries of all staff members except Margaret. I charged the other two members of the committee to work as a committee of two, a subcommittee of two, to debate Margaret's salary and to recommend any change in her salary directly to the Chairman of the Board of Scientific Overseers. The Chairman of the Board of Scientific Overseers then presented that proposition, modified as
he thought appropriate, to the Board of Scientific Overseers for the final determination. Then the notice, to Margaret, telling her that her salary had been changed was handled by the Associate Director or Administrative Director. So that I was completely out of the normal chain of operation.

End of side one

The image I had in mind, that had gotten through my imagination, was what would happen if there were headlines in the newspaper saying somewhat as follows: Director of small laboratory on East Coast discovered arranging wife's own salary.

During the time that I was the Director of the Laboratory from 1956 to 1975, we had periodic meetings of the entire staff in order to debate one proposition or another. But specifically, each year we had a little more elaborate meeting called the Annual Meeting of the Staff. This was usually made on—in the beginning anyway—it was made on the first Thursday in October. It was preceded by a luncheon, at which Allen Griffin served a fish chowder. The meeting took all afternoon. It was devoted to reports of committees and staff supervisors of various functions within the Laboratory and gave us a chance to assess where we are, what we've done in the last year, and more importantly, what remains yet to be done. The point I want to get at is a problem that
arose with respect to the timing of this meeting. It appeared that the first Thursday of October was in conflict with a school vacation period arising from some meeting of school teachers during that same time. Members of the staff who had children in school had an opportunity, if they so wished, to have a little extra Autumn vacation with their children. They could go to a museum in Boston, or whatever, on this occasion. Since we had no children, and certainly we didn't have any in the school in Bar Harbor, we were totally unaware of this problem. I became aware of it at a social event when a wife of one of the members of staff spoke to me, in relatively harsh terms, about having this meeting at a time that conflicts with this school vacation period. Well, I knew nothing about that and no member of the staff had ever said anything to me about it. I said "Why don't you have your husband tell me about it?" But I never heard anything from him. Then I got a letter from a wife of another member of the staff explaining, in great detail, what I have just now explained about the great advantage of having the period off. And I wrote back to her and said that I understood completely what she was driving at but that no member of the staff had ever said anything to me about this being a conflict. And in the back of my mind, although I did not say it in the letter, I thought that if I begin to respond to what wives of staff members are advising me to do,
then it would be a very short step for me to reverse that process and start advising them about how they should serve breakfast at home. That we'd better keep a separation between home and Laboratory in that respect. So still a third wife of a staff member came to see me one day and told me about this problem. And I said "I know all about the problem, I've heard about it from other wives. But I have not yet heard it from any member of the staff. I'm all set to make the change, but I can not make it until some staff member has the courage to come in and tell me that he thinks it's desirable." So it finally came to Jack Schlager, who was a member of the staff at that time and whose wife also worked at the Laboratory, to come in one day and say "I want to talk to you about the date of the annual meeting of the staff." I said "Jack, O.K., that's all you need to say. What day do you want to have it on?" So we changed it to Tuesday of that week. It was an easy way of solving the problem, the problem was clear, no issue involved about changing it really, except when does the Director respond and when does he not? I felt it was a matter of very important principle that I at least hear a little bit of belly ache from a staff member. Shortly after I became Director of the Laboratory, indeed even within the first month, I decided that it was my duty to keep the Chairman of the Board of Trustees and the Chairman
of the Board of Scientific Overseers informed about the operation of the Laboratory. I thought I should tell them what the problems are, what progress we are making towards solving them, and progress we had already made, and what obstacles seem to stand in our way of our solving these problems. So, by the end of October in 1956, I wrote the first of what were to be called Monthly Summaries. The first Monthly Summary was directed to Hugh Knowlton, Chairman of the Board of Trustees, actually called Board of Directors at that time; and to E.B. Wilson who was Chairman of the Scientific Overseers, which was called the Board of Scientific Directors at that time. I have rather recently gone over these Monthly Summaries of the first few months that I was Director and they've turned out to be a great source for reminding one of perplexing problems, some of which were never solved, some of which were solved almost immediately, that confronted us in those days. In any case, I established the practice of writing them a note about the status of the problems of the Laboratory each month thereafter. And I'll say now for the record that I managed to keep up reporting in that fashion for every month of the 19 years that I served as Director of the Laboratory. The Monthly Summaries, now comprise something like 12 or 15 ring binders, covering all of the problems and recordings of many of the major events during that 19 year period. When I retired a set of
Monthly Summaries was deposited, by me of course, in the Library of the University of Maine in Orono under the "Earl L. Green Papers." And that's all, at this time, that exists under that heading. During the course of the years the Monthly Summary evolved somewhat. Initially I sent it just to the two chairmen I mentioned. When John Kidd became chairman in 1960, he suggested that all the Trustees should receive copies of the Monthly Summary. So I started sending out a large number of copies and very shortly someone further suggested that the entire staff should receive copies and other people as well, including the Laboratory's legal counsel in New York. So, pretty soon we had, I'll say, a large readership; more exactly I should say a large receivership. The Monthly Summary took on a more or less standard format. The first paragraph was devoted to news items and essays about current problems. The later paragraphs were devoted to summaries of what papers had been published, who the visitors had been, how many mice had been produced and distributed during the preceding months, the summary of the financial statement up to that point, all of the things reflecting the status of the Laboratory. It turned out that those later parts of each Monthly Summary, now have a very high value because of their record of the detailed events of those days. I draw upon a Monthly Summary and I will continue to draw upon the Monthly Summary to
refresh my memory about long-ago events that I have only the vaguest memory of, otherwise.

Shortly after I became Director we introduced a procedure for reviewing grant applications and also a procedure for reviewing manuscripts on route to publication. I'd like to comment on both of these, not in detail but just to give an idea of what they were and what some of the problems were. A grant application is, of course, a combination of a narrative exposition of what one investigator wishes to do and a financial statement of how much it's going to cost. So these two parts of the application happen to be developed within the Laboratory, by means of consultation between the staff member and people in the Business Office and then the director has to make certain that there is sufficient space in which this work can be carried out. And in particular, if a particular piece of research entails hiring new people, assistants or associated staff to the scientist, that there will be space to accommodate them. This can and did sometimes lead to quite extensive arguments about whether we are ready to support this particular kind of research at this particular time.

Then, there was also an issue of who can apply for a research grant. We decided, and I think appropriately, that under the auspices of the Laboratory the only person who could apply for a research grant would have to be a staff member. Well, this
seems simple and easy enough to take care of, except that there were a few postdoctoral fellows who felt that they too should have the privilege of applying for a grant. Under the laws of the United States, anyone can apply for a grant so, in that sense, there was no legal bar to stop a fellow from applying. But we felt very strongly that the Laboratory should not be committing future space and other resources to a postdoctoral fellow whose appointment at the Laboratory was of delimited duration. We couldn't authorize a person to apply for a grant without having made the commitment that that person is going to be here long enough to make it worthwhile to carry out the project. That led to a couple of painful episodes with the people who were involved. Then we ran into another problem. The way that research was, and probably still is, financed in the United States, a grant never covers the full cost of the research. It may cover 85, 90, or 95% of the cost but there's always some extra amount that the Laboratory has to be able cover. So even though the amount is so small that it can be regarded as trivial, 5 or 10% of the total cost, nonetheless, when you have a large number of grants operating then 5 or 10% comes out to be a fairly large amount of money. Well, there was a period in the early 1960's, in particular, when the Laboratory was simply not able to support additional projects because we didn't have the available general funds
to support them in a larger number. There were also all these problems of space, but I'm referring specifically to the problem of the Laboratory not having enough general funds to support more research grants. In the case of two investigators this posed a very grave problem because these two people, in quite different fields, had gotten together and cross-stimulated each other to the point where they got very, very excited about carrying out a particular piece of research. They had consulted with the Business Office, in due course, in accordance to that procedure about applying for a grant--about drawing up a budget. And their application arrived on my desk just about the same time I realized we were not in a position to support any additional research. Here was this project that--this application for a research grant--that had all of the conventional merits in ample degree and there was no reason on a scientific basis why it shouldn't be supported. But I had to defer the decision to sign the application until I could be sure that we would have ample funds. That deferral actually went on for a couple of months because money is not something that you can scoop off the shelf. I had to be sure that we were going to be able to raise the money necessary. We got past that and that particular problem did not arise again, fortunately. A manuscript review system was similar in outline, except that there's no budget connected
with a manuscript review. Before I came to the Jackson Laboratory I had had occasion to visit a large number of research institutions and universities throughout the country, particularly while I was with the Atomic Energy Commission. And I learned that one of the greatest sources of consternation and perplexity, to use polite words, on the part of the research staff members in other institutions was the long delay that their manuscripts suffered when they lay on the desk of the Director, or chairman of the department. Almost all of these institutions had some type of manuscript review and approval system that required the final authorization by someone in administrative charge before the manuscript could be submitted to a journal. Even before I knew I was coming to the Jackson Laboratory, I decided that if I were ever in a position to control such a thing I would remove myself from the final step. I would have a review system that would not allow me to be the obstacle to the final submission. So what we contrived, with discussion at the Jackson Laboratory, was a manuscript review system, which, I think, has survived the test of time and I think it can be highly recommended to any other organization. What it consists of is: the author submits his manuscript to two other staff members, whom he or she has selected as reviewers; these staff members write critiques--detailed comments upon the manuscript--and transmit them to
the author for the author's consideration. The author is not bound to make any changes, unless the author himself or herself feels these changes are appropriate. This reviewing system is not a censorship system; it is a service to authors system. Some people, particularly new staff members who come into the Jackson Laboratory from other institutions, are apprehensive about a review system. Immediately they feel it's going to be censorship, and it takes them a little while to realize that it is not censorship, it is mutual help. When the author gets his manuscript to the point where he is ready to submit it to a journal, he does so forthwith, and a copy is then circulated to the Director for the Director's knowledge and also a copy--that same copy--went to the public information office for their assessment of the possible newsworthiness of the paper when it came time for publication. A manuscript review system reveals, to me at least, a very important thing about the other staff members of the Laboratory. It turned out that some staff members were good reviewers and some staff members were very poor reviewers. And the difference was this: The good reviewers were those who would read the article very carefully and assess it with respect to its substance as being worthy or not. And who would also be alert to details of grammar and spelling and punctuation and all the other kinds of faults that manuscripts traditionally have. The
poor reviewers were, in some cases, some of the most eminent members of the staff. The poor reviewers would read a manuscript and write either o.k. or satisfactory without a single mark on the manuscript and with no comment, no assessment at all. And the author, upon getting his manuscript back from such a reviewer, would start to read it again only to discover misspelled words, bad grammar, poor punctuation, ideas that didn't flow in proper sequence, and wonder: what was that person thinking about when he was reviewing this manuscript? I got to the point where I could identify who were the good reviewers and who were the poor reviewers within the Laboratory. Fortunately, there were only two or three who were really poor. And luckily there were two or three who were really superb reviewers.

SM: You never made any attempt to try to direct people, or suggest that people see these really good reviewers?

EG: I made no selection of reviewers, the choice of reviewers is completely up to the author.

SM: Do you think that the staff at the Lab actually became a sense to workers?

EG: Came to what?

SM: Came to sense who were the good reviewers.

EG: I'm sure it didn't take them very long to discover who were the good reviewers. After submitting one or two manuscripts, each time to two different reviewers they would
soon discover who were the good ones and who were the poor ones.

SM: I would imagine the good ones would get pretty busy then?

EG: Indeed they were, and this was, if anything, the tragedy of the system, that on the shoulders—oh, let's say—fewer than six were the really good reviewers. They turned out to be reviewing the majority of the manuscripts coming from the Laboratory.

I'd like to comment on the administrative organization of the Laboratory. Not just the formal structure of titles or positions, although I should include those as well, but something of the concept of why we were organized in this particular way. The administrative staff changed with the passage of time, and of course the titles changed. But for most of the while that I was Director, in addition to the Director there was an Associate Director, there were three or four Assistant Directors, there were a number of managers of various functions, and then staff supervisors of various parts of the operation. So much for, let's say, the formal administrative organization. The part I really want to talk about is the decision-making process. The administrative staff—that means the Director, Associate Director, and all of the Assistant Directors—met more or less regularly once a week, say, Monday afternoon—late Monday afternoon—to try to decide what should be done about
any current problems. But the way the Laboratory was
organized with the Trustees as the legislative body, there of
course was a limit to the decisions that we could make.
Sometimes our decision had to be limited to: This is what we
will recommend to the Board. And for all important problems
that meant that we had to go to the Board. But after John
Kidd, John Graydon Kidd, became Chairman of the Board, through
his influence we adopted a whole new way of carrying the
messages from the Laboratory to the Board of Trustees--later
the Board of Governing Trustees. John Kidd said that what we
really need is committees of the Board organized so that the
administration can present its problems to the relevent
committees, and so we had say ten or twelve different
committees. I then, in turn, arranged it so that some member
of the administrative staff was responsible for serving each
of these committees. The financial committee, for example
would be served by the Assistant Director for Budget and Fiscal
Affairs, who was Dale Foley. The training committee would be
served by the Assistant Director for Training, who was John Fuller
at one time and then Seldon Bernstein later. The buildings and
grounds committee would be served also by Dale Foley; later,
by John Fuller. The fund committee was served by Alan Russell, who
had the title of Assistant to the Director. So you
now can think of problems and recommendations for decisions
eemanating from the administrative staff, flowing through the
committees of the Board and then sent to the Board for a
decision, and thus solving the problem. Well, we got this
committee arrangement operating so that there was a very
smooth flow from the administration through the various
channels of members of the administrative staff to the
committee and thus to the Board. And I saw that as
comparable to the arrangement of the placenta to the uterine
wall between the mother and the fetus. And so I spoke about
this and had written about it as the placental theory of
decision making. In contrast with the trickle down and
nipple theory, in which the Board, through its Chairman,
squeezes out a drop into the osculum of the waiting Director
and he takes it and digests it. Whether the metaphor
is apt or not, nonetheless, it turned out for us to be a
really magnificent and workable decision-making process.
I have learned since that government is not so much a
matter of what you give them, it's more how you do it.
Government is process and I developed confidence that there
was almost nothing we couldn't do if we did it properly.
If we followed the procedures properly, let everyone know,
talk with them, give them a chance to speak about it, talk it
over. And indeed the ideas themselves evolved in this
process. Nothing is ever so cast that it can't be changed.
And by this process of discussion, and evolution of the
ideas, and carrying it through to the appropriate committees,
carrying it through to the final channels. Nothing can stand as an obstacle, except maybe money. If, as soon as we step out of this channel, try to take some raw issue directly to the Board of Governing Trustees for example, they'd sit there in bewilderment, look back and forth from one to the other, "Well, we don't know. This catches us very cold. We don't know what to do about this." And then suddenly someone would say: "Can't this be referred to one of the committees?" And as soon as that was done we could solve the problem. Over and over again I learned a hard lesson.

SM: But yet this was not inefficient. I mean, that is to say, referral to a committee. But somehow the committee got it out.

EG: This was the committee of the Board not the committee of the staff?

SM: Yes.

EG: The committee of the Board that would meet upon appropriate notice and meet a relevant member of the staff who would present the issue and it moved. Yes. Now it couldn't move within a week because the Board itself met only every three months. But if we had to move something within a week, we had the means of doing that too, but the ordinary business took around three months. But most of the day-to-day problems of the operation were handled right at the local
level by the administration itself. We had committees of the staff, but committees of the staff were not involved in the administration of decision making. They were involved in planning, and development of concepts, and sort of overseeing operations, as the training committee had a lot of duties. But they didn't have any power about administrative problems that required immediate decisions. They had decisions about what students to admit, but that was within their authority.

It's a matter of separating the decision-making process from the functioning committees of the staff. This administrative arrangement involved members of the research staff as Associate Directors or Assistant Directors, and as supervisors of various functions within the Laboratory, supervisors of histotechnical service, supervisor of art and photo service, supervisor of radioisotopes. There must have been 15, 18 different supervisors. It leads me to a further generalization and I put it in this way: If scientists won't manage their own institution, the Trustees will have to find someone, maybe a shoe salesman, to manage it for them. This is something else I had observed in my numerous visits to other research institutions and laboratories throughout the country, that where someone else had been brought in from outside--outside the field of science--to manage the radioisotope service or manage the histotechnical service, or whatever, when someone outside
science had been brought in to manage these various functions. The scientists then were disgruntled because this fellow, however princelike he might be otherwise, simply didn't know what the problems were. It was out of this that I developed the concept that it's up to us, as scientists ourselves, to manage our own institutions. And so I stress the idea that members of the administration, not only the Director but other members of the administration, should be members of the research staff as well and take part-time duty as directors. And that all the supervisors of the various functions should be members of the staff. On the ground that if the scientist wouldn't do it we'd have to get some shoe salesmen in, in order to do it for them. Now, this procedure illustrates a case of the ambiguity that we live under in all aspects of our lives. Scientists inevitably come to feel that they should not be spending their time in these administrative and quasiadministrative functions. And so after a while they tend to rebel against this sort of thing. But I think all it takes is a period of say 6 or 7 years under the alternative and then, they are ready again to partake of administration. Among the things that a Director of a laboratory has to be concerned with is, of course, improving the existing facilities and adding new facilities. I mean capital construction, new buildings or new wings on existing buildings.
There were three major projects of this sort while I was Director. The first of these was the addition of a wing to the Main Laboratory in 1958. A wing which we call Unit 5. The purpose of Unit 5 was clearly to relieve crowding in the Main Laboratory. Several staff members were bunched up together in offices and were sharing laboratories, so a lot of space was needed. I was new as Director and really didn't know how to handle this sort of thing, maybe I never did actually learn how, but at least I think I did it a little better later. What we did in this case was to draw up the application for a grant from the National Science Foundation along with a plan for a new wing. And I don't know what amount of money we asked for, but at any rate we were awarded $200,000 by the National Science Foundation. But this was not the amount we needed to build Unit 5, it was a little more than half, but that was all. At any rate, we received the check for $200,000 from the National Science Foundation in August of, I think, 1958. And so I thought this might be a suitable event for creating a little publicity in connection with the Annual Meeting of the Laboratory, particularly at the banquet being held at the Jordan Pond House. So I invited George Lefevre, geneticist on the staff of the National Science Foundation, to come up to the Laboratory at the time of the Annual Meeting and to present the check all over again at the Jordan Pond House banquet in
a nature of a surprise. So on the appointed day George arrived and Dale Foley conveyed the check, which we had already received a couple of weeks earlier, to George and at the Jordan Pond House banquet George Lefevre interrupted the proceedings and said he had something that he'd like to say. And so he announced, that the National Science Foundation had awarded $200,000 towards Unit 5, and here was the check. And I must say that it was really quite a dramatic occasion.

I didn't quite anticipate the excitement that people would feel in being in the presence of a $200,000 check. Just about everybody had to have a chance to look at the check, itself. Ed Murphy, who was a member of the staff at the time, in particular, came up afterwards and said he just had to see what a $200,000 check looked like. That got us started, let's say, but it was quite a problem to raise the additional $150,000 that we needed to complete the financing. Hugh Knowlton, who was Chairman at the time, started that off by giving us a gift of $5,000. And then by canvassing the other trustees we were able to raise additional funds. The Kresge Foundation gave us one of their famous challenge grants of $20,000 contingent upon our being able to raise a certain amount of money by a certain time. And that had a great stimulating effect. Eventually, we did in fact raise the necessary funds to build Unit 5. At first we left the top floor unfinished but then later that was
finished. So that was the first project. The next one was the building of the Morrell Park Laboratory, and as I recall that started about 1959. It was in operation about 1960 or '61. The big motive for this was to get the production stocks of mice into their own separate facility, and be able to clean them up, and provide fresh new quarters, and overcome some of our animal health problems at the same time. That part of it is a separate story from the actual building. In order to get the money for the Morrell Park Laboratory we had to use such very limited funds that the Laboratory had plus loans from other organizations. We borrowed money from the local banks and from the Mount Desert Island Development Corporation, all of which had to be paid back. But that was not adequate to cover the financing of the Morrell Park Laboratory. Sheldon Goldthwait, who was at that time President of the Bar Harbor Banking and Trust Company and was also a Trustee of the Laboratory, arranged for us to borrow the money from the Union Mutual Life Insurance Company.

Well, Sheldon and I went down to Portland to see the officials of the Union Mutual Life Insurance Company only to discover that they had concluded that they simply could not give a loan to the Laboratory because this was a private non-profit research institution and we were proposing to build a building that, except for our own use, had absolutely no market value. The building might have been used for
something else if it had been located in Portland. But a building located on a former racetrack in Bar Harbor, 50 miles beyond the end of the line, so to speak, was in effect useless. So the President of the Union Mutual Life Insurance Company said there wasn't any chance at all that they could give us an ordinary mortgage loan for this building. So this led Sheldon Goldthwait to see if he could get somebody else who had the resources to underwrite a note so that then Union Mutual would give us the money, if they had adequate security. So he arranged for Nelson and David Rockefeller to underwrite the loan, and Union Mutual had no further hesitancy about lending us the money. Thus, we were able to build the Morrell Park Laboratory. We still didn't have enough money to equip it, because it required washing machines, fork lift trucks, cages, racks, bottles, sipper tubes, everything connected with the operation, except the building itself. So for the equipment money we submitted two different applications to the National Institutes of Health, National Cancer Institute, only to encounter, with respect to the second one at any rate, a very long delay. So long that we were really becoming very apprehensive. Hugh Knowlton, the Chairman, talked with me about our intervening in some way. He knew some people in Washington and he thought that we could maybe pry this thing loose. But I, as a one time
staff member of the Atomic Energy Commission, advised against any such action until we knew exactly what was the cause of the delay at the National Cancer Institute. I thought all we could do was create a further problem by trying to put political pressure on them. So we temporized and did nothing. Later, we learned that the problem was that someone had raised the question about the appropriateness of the National Cancer Institute giving funds to the Jackson Laboratory for equipment for this Morrell Park Laboratory, which was to be used for the production of animals to be used in connection with cancer research, but we were going to sell these animals to other people and we were going to provide animals through the CCNSC to the Federal government to distribute to its various contractors, and of course such animals would be property of the Federal government. Well, only years later when there was further investigation of the action of the National Cancer Institute and the Surgeon General, in this respect, did we learn that in defending this action the Surgeon General defended it by saying that the existence of the Jackson Laboratory was very important to the Health Research Program of the Federal government. And if the
Jackson Laboratory had not existed it would have been necessary for the Federal government to have created it. We might come back to that when we get around to the tax case. The third major building project while I was Director actually involved two things: the creation of the Library Conference Center and Mammalian Genetics Laboratory. We decided to build these new buildings just a bit too late to receive financing or any help in financing through Federal grants. The National Institutes of Health discontinued its funding of research facilities simultaneously with our filing of the application. So there we were left with a problem of raising the funds from private sources and shrinking the size of our proposed building. Actually what we did, think, or were forced to do by those circumstances, was actually a better solution than what we had in mind to begin with. And so we tailored the expenditure down to about $1.6 million, which was enough to build the Library Conference Center, which subsequently, became the Clarence Cook Little Library Conference Center, and solved some major space problems at the Laboratory. And the other part of the money was used for the Mammalian Genetics Laboratory. That likewise solved the problem of where we could keep research stocks, precious research stocks of mice in facilities contained where there would be minimal chance of contamination from other sources, other mice, human beings,
or other animals, or just air of the environment. Well, apparently I had talked about MGL (Mammalian Genetics Laboratory) to the Board of Governing Trustees so much that they were almost a little bit sick of hearing about it by the time it was actually built. At the time of the Annual Meeting in 1974, Frank Gerrity, who was at that time Chairman of the Board of Trustees, said to me just as the Annual Meeting was to begin: "Earl, I would like to change the order of business today. I'd like to come last. You'll see why later." Well, that surprised me a bit because ordinarily the Chairman of the Board of Governing Trustees gave his report, which was usually very brief, and this was followed by the Chairman of the Board of Scientific Overseers, who usually gave a longer report, and this was usually followed by a report from the Director, and the Director was fairly windy and so his report was still longer. And I thought if this is something new, maybe Frank wants to have the long winded statement at the end. But that didn't seem right either because he's not a speech maker. So anyhow, we got to the point in the Annual Meeting where Frank Gerrity came last and he said that the Trustees had decided that this new Mammalian Genetics Laboratory should be named the Earl L. Green Mammalian Genetics Laboratory, then he pointed to me and asked me to say something. Under those circumstances, loquacious as I ordinarily am, I simply could not say a word. And so the
meeting had to end shortly after that.
SM: It was nice that he did go last, wasn't it?
EG: I'd now like to talk about a number of relatively short, one time, episodes that illustrate some of the problems of operating a research laboratory devoted to rearing live animals, such as mice. The first of these problems is the change over from wooden boxes to plastic or stainless steel cages. The people at the Jackson Laboratory had, ever since its founding in 1929, been using wooden boxes and they were devoted to them. These were Prexy's own style of boxes, probably developed shortly after he had been a graduate student, or while he was a graduate student at Harvard sometime before 1914. A wooden box, as you could imagine, you could put your hand against it and it would almost feel warm because there's nothing to conduct the heat away. And so this way he could think of it as being a very nice comfortable place for raising animals. I'm quite sure that if the mice were left to make the choice, let's say, between a wooden place—a wooden box for a nest—and a steel box for a nest they are going to undoubtedly choose the wood because it is more comfortable on their feet. However, by the time I came as Director the situation had changed. I have already mentioned something about the bedbug problem but that problem, specifically, had been corrected by the fire. That's a very hard way of solving problems but it did
nonetheless, solve the bedbug problem. But in the early 1950s a new problem had arisen. Mousepox or ectromelia had occurred in some of the colonies of mice in the United States. Ectromelia is caused by a virus rather similar to the smallpox virus. And this particular virus had been introduced into the United States for research purposes, and had gotten loose from the biology laboratory, and had first wiped out mouse colonies at Yale University, and subsequently, had wiped out mouse colonies elsewhere. An absolutely devastating and highly dreaded disease of mice. The people who get such a disease in their colonies, at least initially, have no recourse but to discontinue the mouse colony completely, clean up, wait awhile, and then repopulate with fresh mice from an uncontaminated source. Well, that's all right. They can get mice, say, from the Jackson Laboratory or from other places. But if the Jackson Laboratory were wiped out, that would be a totally different matter, because the Laboratory is, in some respects, the ultimate source. So, that was a threat. On top of that, our own colonies here were suffering a very, very heavy infestation of mouse typhoid—*Salmonella typhimurium*—that couldn't be adequately cleaned up by using boxes that had crevices and that couldn't be adequately sterilized. Indeed, the mice could gnaw through the wooden boxes, and it was customary to find two or three mice out walking around
any day you might come in the mouse room. The Laboratory had a full-time carpenter whose job it was to patch and mend the boxes until they got to the point where they simply had to be discarded. So let's say the issue was the health of the mice. I myself had had a very unpleasant experience. Margaret and I moved our mouse colony here from Ohio State University and we took great pride in having a clean and healthy colony. Before they could be introduced to the Jackson Laboratory, they, of course, had to be certified as being healthy. To test them for their state of health, mice from the Jackson Laboratory were put into cages with our mice to see if the Jackson Laboratory mice got sick. Well, the Jackson Laboratory mice didn't get sick, but all of my mice got sick. I lost 80% of the mice that I brought here from the Ohio State University. So the situation at the Jackson Laboratory was, to put it mildly, deplorable. The issue was: What steps could we take within our existing facilities to do something about it? Most of the members of the staff, indeed, I'd say all but one of the members of the staff, were in favor of changing to either plastic or stainless steel. At that time, of course, we didn't have very much evidence about mice would thrive in these stainless steel boxes or plastic. Other laboratories, however, were using plastic cages and apparently were using them successfully. The person who objected, Bill Murray, who was the Associate
Director and longtime associate of C.C. Little, argued that we simply didn't know what the mice would do if they were put in some other kind of environment. And, of course, in the face of that kind of criticism there is no way to answer that except to get some information. It took a year. Don Bailey was here at that time—since then he left and came back—but he was here at that time. Don Bailey conducted an experiment of raising mice in environments other than wooden boxes. A year-long trial, and it turned out that they survived quite adequately. Meantime, Margaret and I couldn't wait to get rid of the wooden boxes. We had the funds available, so we purchased double plastic boxes, the sort that were already in use at the Oak Ridge National Laboratory. We decided at the end of that year that we would get the stainless steel boxes. This presented a problem. Just imagine something like a kitchen sink, but a kitchen sink is made of a big piece of steel that can be deep drawn to make a six inch depression. But to go on to something as small as a mouse cage, the problem is how to draw down a piece of steel, by means of a press, down to the adequate depth, from such a small piece of steel without having ruptures along the sides. And this presented a real problem. Well, Dale Foley just happened to meet, on an airplane on one occasion traveling between Bangor and New York, a man by the name of John Killduff who was the
President of Amesbury Metal Products Company in Amesbury, Massachusetts. And it turned out that John Killduff is an engineer, an expert on stainless steel, and operates this company, and knows all about handling stainless steel. So in due course we made an arrangement with Amesbury Metal Products to cast, make the mold, and get a new special formula of stainless steel, that John Killduff devised, in order to make these deep-drawn cages. But what I'll say is, the first box that came off cost us $12,000, of course by the time he had made two they would have been $6,000 a piece. And by the time that they had made 12,000 they'd only be $1.00 a piece, or so. And we needed thousands upon thousands of them. They were welded together to make a double stainless steel box for greater stability. I can say that this happened in the early 1960s and they've been in use for more than 25 years at the Laboratory, they are standard equipment in the Laboratory, there have been no obvious impairment of the health of the mice from living in this stainless steel environment. In fact, quite the contrary because the stainless steel can be cleaned and autoclaved and really washed up between uses, it gave the Laboratory a chance to clean up the immediate mouse environment. A little later Bill Murray came to the point of leaving the Laboratory, leaving the administration. He set up a separate mouse room at a former funeral parlor down in Bar Harbor, which was
referred to as the "Heavenly Rest," and was carrying out some mouse breeding experiments. At that time he reverted to the use of the wooden box, which he obviously was devoted to. Now this next episode really involves Charity Waymouth. This has to do with Charity Waymouth's appearance on the Dave Garroway's Today show on NBC-TV. Charity had discovered a completely defined medium for culturing cells in glassware. Prior to that time the culture media always had something such as serum added to it, the exact composition of which was, of course, unknown. But Charity had been working for many years trying to develop a medium that had absolutely known ingredients and she finally succeeded. So this paper that she published at that time, created a bit of a stir and she was in the news with an item in Time magazine or Newsweek magazine. So Alan Russell, who had just recently joined our staff as Assistant to the Director, arranged for Charity to appear on the Dave Garroway show. Well, if that wasn't enough, I had to go along with some mice to show on the Dave Garroway show, also. So the three of us, Alan, Charity, and myself, got on the plane in Bangor enroute to New York where Charity and I were to appear the next morning. Well, we got as far as Boston and it turned out that there were some reporters who had heard about the work because we had released a news article about it, and they wanted to have a
further discussion with Charity. They called the Laboratory only to learn that she was enroute to Boston, so the reporters were meeting the plane at the ramp in Boston. So this ramp was let down, I think we were on one of the old DC-6's, as the ramp was let down this little clutch of reporters wanted to know if Charity Waymouth was on board. The stewardess came back and asked if Charity Waymouth was on board, and she acknowledged that she was, so she stepped out of the airplane to talk with these three or four reporters. Well, of course the people on the aircraft had no idea of what was going on here. I went out and stood on the ramp and watched for a little while. And then I came back and sat down, and as I did so one of the little girls, about 12 years old or so, asked me "What did she do?" I realized then that the stir had created the thought that surely she was going to be arrested now. She must have done something horrible, otherwise why would these detectives be here to apprehend her. She had been taken off the plane for sure. So I said to the little girl and her mother, "Well, she has developed a completely defined medium for raising mouse cells in culture." I must say they seemed to loose interest in what she had done at that point. In any case we proceeded on the aircraft to New York. And this experience in Boston had given Charity a little extra lift. She was now looking at this whole thing in a slightly different way. So we got to New York and were checking into
a hotel where we thought we had reservations only to discover that some conference was going on and we could not have access to rooms then, and might not be able to get rooms until sometime later. The hotel was jammed up with a conference, the rooms hadn't been released. So we were milling around in the lobby with a large number of other people, who also thought they had room reservations. At which point we discovered that some reporters had come to the hotel wanting to find Charity Waymouth, and here she was in the lobby. Obviously, this was not a circumstance under which she could have an interview. But Charity's response is the significant thing. When the reporters asked if they could see her tomorrow, her reply was: "Well you'll have to see my assistant. He is taking care of all of my appointments." And she was referring to Alan Russell. I hope she wasn't referring to me, at any rate. I am using this to illustrate that Charity was transformed between the time she left Bar Harbor that morning and the time we arrived in Boston, in New York, before noon. So the next day--

SM: So you did find rooms someplace?

EG: Well, we were assigned to another hotel. Yes. The next day we appeared on the Garroway program. Dave Garroway interviewed Charity Waymouth and then he had me point to the various types of mice and say a little bit about them. And of course, the whole thing was over in a matter of minutes.
But I was astounded, in the months after that, to discover that large numbers of people from all across the country were looking at that particular program. And I'm referring, specifically, to people who know me and let me know that they had seen me and the mice on T.V. in the morning.

Another event that happened about 1960, maybe a little earlier, I think it was a little earlier, was the famous mouse strain mix-up. At that time we were supplying mice of several different strains to contractors with the Federal government. And one of these strains was DBA/2. This is one of the famous old inbred strains of mice started by C.C. Little, possibly as early as 1910. But we got a message from, as I recall, someone from the Roswell Park Memorial Institute in Buffalo, that among the DBA/2 mice that he had received were some mice that had the reactions of DBA/1 mice. DBA/1 and DBA/2 presumably have a common origin and have extensive similarities, but there are also some well known, well established dissimilarities between them. So this is not a matter of a mutation in a DBA/2 to make a DBA/1 mouse. The only plausible explanation is: there was some confusion because the mice otherwise looked, superficially, exactly alike. So it was up to us to find out how the DBA/1 mice got mixed into the DBA/2 mice. Well, Margaret Dickie was the staff member in charge of the breeding colonies. She started to explore, and she needed Jack Stimpfling's help and
George Snell's help to identify the mice with respect to their histocompatibility genotypes. So in one of the colonies, which was supposed to be DBA/2, they found breeding pairs of DBA/1 mice sprinkled, more-or-less randomly, throughout the colonies. I'll say that the number was small, but nonetheless, there were several breeding pairs of DBA/1 mice in among the DBA/2 mice. And this was a matter of great consternation. How could this possibly be? Well, Margaret Dickie decided that it was up to her to find out, and she went about finding out by going to the source colony, which we called the Pedigree Expansion Stocks. The Pedigree Expansion Stocks were at that time located, at least with this particular strain, were located in one of the wooden buildings out back of the Laboratory, the so called Cloudman Laboratory. The Cloudman Laboratory still exists, but it has, in the meantime, been renovated, and renovated, and renovated into what suited the purposes. Thelma Stanley, one of the mouse room supervisors, was in charge of the colony and Margaret Dickie went in and looked along the shelves. And this is how Margaret Dickie described it to me: She's reading the labels of the tags on the cages of DBA/2, DBA/2, DBA/2, DBA/2, DBA/2, DBA/1, DBA/2, DBA/2, DBA/2, DBA/2, etc. She found one breeding pair, in this source colony, labeled DBA/1. And, of course, that was the answer. Breeding pairs made up from the progeny of mice in that pen had been in the
colony that had provided mice to these first contractors of the Federal government, including people in Buffalo. So there was one pen of DBA/1 mice located where it should not have been, and had been there for several months, and had not been observed. Well, the cage tags looked exactly alike, the mice looked exactly alike, all there is, is just a little numeral one to distinguish it from a little numeral two, and Thelma Stanley had not seen this. This episode led to two profound changes in the way we handled mice at the Laboratory. First was introduction of new color-coded cage tags, each strain had its own unique color. Such as, pink for one strain, and green for another strain. And sometimes there were two different colors on the tag. A great variety of different and innovative colored tags had to be invented and I think the same tags are still in use. That was one change, the other was the introduction of a wholly new numbering system. A breeding pair was assigned a number in the Foundation Stocks--the Laboratory down at Highseas, from which all the mice come--they were assigned a serial number there. And that serial number, plus other annexes to the number, was carried along to all the progeny so that if some problem occurred with a pair of mice that had, say the serial number 3660 plus other annexes following that, then one could search through the colony picking out all the 3660s and know that they all traced back,
even though it might be 6, 7, or 10 generations earlier, back to a single pair that had, at one time, been in the foundation colony. Trivial as that may seem to a person who did not breed mice as a business, the introduction of the cage tags, coded by colors, and the new numbering system, was a great help to keeping the menagerie sorted out in the future.

SM: To your knowledge, has this sort of mix up ever occurred since?

EG: There have been other mix-ups, yes. It's impossible to avoid them completely, but the others that I know about have not been nearly as pervasive or semidisastrous as that particular one was. And now of course, say a mutation occurs, a mutation is a perfectly natural event and you can't help it as evolution is still going on. But suppose a mutation occurs and it's in a pen that has the numbers 7852, let's say. You'd like to pull out all the collateral relatives to test them. Look for all the 7852s and search. The introduction of those two devices have made a search enormously easier to cut down on the inadvertent changes.

SM: As well as trace the genetic history of a particular mouse.

EG: Yes.

EG: Another problem, that I've already alluded to, but I want to expand on, is the problem I'll refer to as animal health.
I've already mentioned my own experience of having my own mice tested for disease only to discover that my mice got salmonella, and in addition to that, they were supposed to be tested by introducing vasectomized males into my various cages. Well, that was all right I guess, except that some of my females got pregnant, having been exposed to these so-called vasectomized males. That isn't the disaster, it is at least an inconvenience. Shortly after I came, and during the time that the Federal government was interested in expanding the supply of mice in the Jackson Laboratory for the CCNSC program, we began to get more and more complaints from recipients of our mice about the dismal status of the health of our animals. And there were several of us, who were ready to believe that the situation was clearly deteriorating to the point where vigorous action was necessary. At that time, Ed Les was at the Laboratory--he is still there now, he had joined the staff of the Laboratory--he had begun to collect data on the health of the animals. There were numerous diseases, not only the typhoid but infant diarrhea, and ectoparasites, and other things as well. Ed was collecting data on survival rates of various colonies. I remember the occasion at which Ed presented his data to the members of the Board of Scientific Overseers, and E.B. Wilson, the Chairman of the Board, said: "My word, this is worse than China in the middle ages." I didn't know about China in the middle
ages, but I gather that this must have been a really bad situation. At any rate, we were confronted with the problem of not having the proper facilities into which to put the mice. A building had never been constructed with the idea that mouse disease was going to be a major problem. After all, for more than twenty years after the Laboratory was founded, so to speak, mice didn't have any diseases. It wasn't really until after the fire that the disease problem began to occur at the Laboratory, clearly, because the Laboratory received mice from laboratories all over the country. And not only got mice, but mice with diseases as well. I appointed two members to the staff, Warren Hoag and Hans Meier, both of whom had degrees as Doctors of Veterinary Medicine, and they, particularly Warren Hoag, were instrumental in showing us how it was possible to clean up our colony of mice. Those of us who had the desire to do it couldn't convert desire into any practical measure. We simply didn't know what to do; it was as simple as that. We did do a few things, but we didn't know, in general, the overall measures. It took someone with a background in public health, let's say, to know what to do about the mouse cages. Then we came to the issue of how were we to move mice into the new Morrell Park Laboratory? I'm talking about the time when it was ready for occupancy and we were very desperately anxious
to get mice in there, because of the demand for mice, and the demand could be converted into some money so that we could pay for the building. There was no question about that as our motive, but on the other hand, in this cognitive dissonance in which we live, on the other hand, we wanted to feed the mice into the Morrell Park Laboratory just as slowly as we possibly could, in order to clean them up, be sure that each pair of mice introduced was free of disease or as free as we could make it. And the first ones should go in as offspring of caesarean-derived mice that have been suckled by hand, nursed by hand, so that they would be free of anything that might have come in with the mother's milk. So here is where we had a very nice split in the administration. Bill Murray was in favor of the rapid introduction of mice, in order to meet the burgeoning demand, that would get us out of debt, as quickly as possible. Everybody else in the administration, and I think the entire staff was concerned, was equally adamant that the mice should be introduced very carefully by caesarean section, and get the first mice in, in that fashion, and nurse other mice on those, so-derived, so as to be able to move in clean mice. Well, I felt caught in the middle, because I don't like to have personal debt and I certainly didn't want to have a Laboratory debt, but I recognized this as a once-in-a-lifetime opportunity to clean up the mice of the Jackson Laboratory. There would never be an opportunity of
this sort, at least not in my lifetime. We had an obligation to the scientific community to clean up the mice. This would mean a slower rate at which we could retire our debt, but in the overall we would come out ahead. So I took that view, that we must go carefully and slowly, introducing clean mice. Well, this was a hard idea to sell to the Trustees, most of them businessmen. I don't want to denigrate them but I'll say, get in, make your money, get out fast, that sort of attitude. You've got a big building, you've a big debt, get in there, get the money, get the debt paid off, and then solve the problem. As you know, from the standpoint of financial health, the fast move was the appropriate thing and made it difficult for me, along with Warren Hoag's backing, to advocate the alternative. Nonetheless, we did move the mice in slowly and we got them cleaned up, in due course, at the cost--I must say--at the cost, to the Laboratory, of Bill Murray the Associate Director. John Kidd came on the scene about that time; he was the one who arranged for Bill Murray to depart from the administration of the Laboratory and set up his independent operation at the "Heavenly Rest" in the village. Where he could use the wooden boxes. I'd like to talk a little bit about the budget, not in detail, but the mechanism by which we, each year, created the budget, and one specific episode that has left its enduring
mark on me. In general, Dale Foley, who was Assistant Director for Budget and Fiscal Affairs at the Laboratory, kept detailed records of expenditures throughout each year. And so, at budget time, in the Spring of the year, he had a notebook filled with how much the Laboratory had spent on soap, and mops, and everything else, during the preceding year. And so could forecast quite accurately what the next year's expenditures were going to be. He'd draw this up in a draft or budget document and this would be reviewed by the administrative staff, usually sometime late in March. Subsequently, it would be looked at by the Budget Committee. The Board of Scientific Overseers would see it, and the Board of Governing Trustees would finally approve it. Also, I would present a modified version of it, boiled down in comprehensible form, to the research staff, and then finally at the Annual Meeting I would present it to the Trustees. That's enough of the mechanism. Now I go back to the period of the administrative staff review of the budget. In a typical year, if there was such a thing, our desired expenditures for the next year always exceeded our expected income by somewhere between $1 million and $2 million. This was when we were operating at somewhere around $7 or $8 million budget for the entire year. So it was up to us on the administrative staff to revise our proposed expenditures downward and to scrutinize
our expected income to see if there is some way to predict more income. We were never budgeting existing money, it was always future mythical money--fictional money--that we were budgeting, anyhow. We would tamper with those figures, tamper in the polite sense. We deliberately had to change those figures, we did defer some expenditure, reduce the estimated cost of some other things, and we'd estimate the improvement in the mouse sales that we'd not allowed for before, so as to narrow the gap down to something that we could possibly live with. And the gap would then turn out to be--somewhere--$100,000 maybe a little more--which would be the amount that we expected to raise in contributions. We could then present that budget to the Trustees with a fair chance of approval. That was the task and that's what we did each year. During the year we would monitor the expenditure, and I want to talk about one year, which must have been sometime in the early 1960s, or late 1950s, when at midyear it was very clear that our expenditures were going to vastly exceed our income. If we continued at the same course we would have ended up at the end of the year with, maybe, $200,000 in the hole. And I was new at Director then, didn't have very much record to stand on, and I thought I could foresee that the only appropriate action that the Trustees could take, if that were to happen, is to get a new Director,
because we can't stand this sort of thing. So I asked the members of the administrative staff to come in with recommendations for every possible way they could see, ways of reducing expenditures. And I asked the people in the Development Office to see if there was any possible way they could increase the income. What could we do to narrow this gap? Well, they came in with a lot of good suggestions, but still not quite enough. And so there we are seated around a table in my office, and wondering where can we possibly squeeze something more out of the expected expenditures, when Dale Foley said: "Well, maybe I better tell you about the eight men we have working up in the woods." And I said: "Eight men working up in the woods? What do you mean by that?" He said: "We've got eight men working in the woods on Eden Farm, up there across from Hamilton Station." I said: "What are they doing?" He said: "Well they're making fence posts." "What do we use fence posts for?" He said: "Well, we don't really use them, we make them and then somehow or other they get used, but we don't really have a specific use for them." I said: "Why do we have these eight men working up there?" He said: "Well, we usually laid these people off during the Winter, but this year we thought that maybe we could handle them. They are poor people and they don't have very much income and, I think, practically welfare cases anyhow, and so we told them that they could make fence
posts up there during this winter." And I said: "Well, I wished that could be so, but we simply have to get rid of them." So we got rid of these eight men who were making fence posts up in the woods. We actually ended that year with a positive balance of $9,000. That was the nearest to having a negative balance that we came to all the time I was Director. Usually we had a positive balance of a $100,000 or more. Sometimes there should have been more. But those eight men have been on my mind ever since that. That's the reason--I still think of them today. I don't know their names and I wouldn't dare--

End of Side One, Tape Two.

...In the early 1960s, there was a self-study seminar. A group of us that included John Fuller, Tom Roderick, Jack Schlager, two or three others, and myself, decided that we would like to study a new book that had just come out. This was Douglas Falconer's book on *Introduction to Quantitative Genetics*. The way we organized this self-study seminar, I think, was very important for its eventual success. We met one day a week for lunch and each time we met, one member of the group was supposed to conduct the discussion. It might be Chapter 3 or the first half of Chapter 7, or whatever, as we worked our way through the
book. But who conducted the seminar on a particular day was not determined in advance. Instead, it was determined by a random sampling method after we had assembled. So everyone who came, supposedly, had to be prepared to conduct the seminar. This meant that none of us could goof off, at least without imperiling ourselves by failing to read the chapter. We had not only to read it, but to read it in the same way that a teacher has to prepare herself, or himself, facing the blank stares of the students, and it is the most frightening prospect that any person ever goes through. So we each had to be prepared to present and discuss with some sense of comprehension, what was in Douglas Falconer's book. By this means we worked our way through the book and all the members of that particular group--well, I think all the members, nearly all of them anyhow--subsequently, designed experiments that made use of the principles of analysis of quantitative traits as outlined in Falconer's book. Another project that got started relatively early in the 1960s is one I'll call the Natural Mutation Rate Study. The background for this goes earlier. In 1954, when Margaret and I came up here for the 25th anniversary symposium, I had occasion to talk with C.C. Little about the natural mutation rate in the mouse. At that time, I was with the Atomic Energy Commission and so mutation rates--radiation-induced mutation rates--were a matter of some concern and we
really needed to know what was the spontaneous or natural rate. When I asked Prexy about it, he said: "Oh, we're getting data on that now." And I thought that was great. Later, when I came here in 1956, that question arose again. The geneticist in the Secretariat of the United Nations asked me if we had data on the natural mutation rate in the mouse. And I said: "I don't, but I think we can get it. I think that somebody else here knows." I asked other members of the staff about the natural mutation rate and it turned out that no one had done such a study. I think Prexy must have misunderstood me or maybe he thought someone was actually doing it. At any rate, in 1956 or shortly after that when I came here, I then realized that we did not have the proper information to get a natural mutation rate in the mouse. By 1960, after we had moved into the Morrell Park Laboratory, it occurred to me that the circumstances were right now for collecting the appropriate data. Now, this is no small undertaking. This means carefully observing every mouse with respect to a standard search procedure. It is a little more costly than just picking the mouse up and changing it from one cage to another. It means carefully examining every mouse and recording any abnormality, but also recording if it does not have any abnormalities; that is, detectable. If it does have any detectable abnormality of whatever sort: eyes, ears, coat, feet, tail, white spotting, behavior, whatever, turn that
mouse and its collateral relatives upward, downward, and sidewise, over to someone such as Margaret Dickie for further study. To see whether it is in fact an inheritable variation or whether it is a nongenetic variation. I think it was about 1963, maybe a little earlier than that, we actually got organized with a group of people trained to examine the mice--Margaret Dickie saw to that--and began to get the requisite data. Gunther Schlager, better known as Jack Schlager, joined the Laboratory at about that time or a little before then; he became a collaborator on the project. I wrote the draft of the first paper when the first data came through, very preliminary results. And I think we published that in *Mutation Research*. Subsequently, Margaret Dickie and Jack Schlager published a series of papers up through the end of the 1970s about the results of the study giving the best estimates that existed up until then at any rate, and maybe even up until now, the best estimates of the natural rate of occurrence of recessive mutations, and the natural occurrence of various dominant mutations. These estimates serve as a base line for other studies on animals or populations that are exposed to mutagenic agents: radiation, or chemicals, or whatever. In 1963, we had what I call, the famous court case on tax exemption of the Jackson Laboratory. It also involved the Mount Desert Island Biological Laboratory. I first heard of this when we were at
a town meeting in the Spring of 1963. Someone approached me and said: "Did you hear on the radio that the Laboratory is going to be involved in a tax case?" I had not heard anything about it. In the next two days and the next two months, I heard more about it than I wanted to know. It turned out that a man by the name of Kendall Young had recently moved to Bar Harbor from Baltimore. He was a retired income tax accountant. He discovered that he would have to pay taxes on his property in Bar Harbor and he felt that the taxes were too high. He therefore endeavored to get the State Tax Assessor to compel the town to tax the Jackson Laboratory and the Mount Desert Island Biological Laboratory, and thus relieve the property owners of some of the tax burden. Well, the State Tax Assessor, Ernest Johnson, decided that he wouldn't do that. So Kendall Young brought a suit, which was legally called a Writ of Mandamus, against the State Tax Assessor to compel him to do it. Otherwise, the State Tax Assessor would not do it. In due course, this case of Kendall Young versus the State Tax Assessor came to trial before Judge Randolph Wetherbee, in Bangor. And we, along with the MDI Laboratory, were interveners in the suit. Our lawyers were William Fenton from Bar Harbor, and James Mitchell and John Ballou from Bangor. Young's lawyer was Orman Twitchell from Bangor. Another person involved was the court recorder who took down
everything that was said in the courtroom. The issue as it unfolded was whether or not the Jackson Laboratory was raising mice as a profit-making activity. The background for this goes back some time, I think in the preceding year. Someone had performed a study of property use in Bar Harbor and had written an extensive article that was printed in the Bar Harbor Times about how the various pieces of land around here were being used. So much commercial, so much residential, and so on. But in doing so, this person had not mentioned a single thing about the Jackson Laboratory. There was no mention at all of the Laboratory being a research institution that was tax exempt and yet it was bringing in $6 or $7 million to the town each year. Amory Thorndike, one of our Trustees, wrote a letter to the Bar Harbor Times, in which he said that this analyst had completely missed the Jackson Laboratory and among other things the Jackson Laboratory was selling mice on the open market. Now, I remember seeing Amory Thorndike's letter and I remember that phrase "open market" and I thought, the next time I see Amory Thorndike, I must tell him that we are not selling mice on the open market. And I did, indeed, tell that to Amory Thorndike and Amory put his hand up to his head and said: "Oh, my gosh, what have I done?" I said: "I don't think it makes any difference, Amory; no one is going to worry about that. But just for your own information the Jackson
Laboratory does not sell mice to just anyone. For years the policy has been that we distribute mice only to bona fide research workers, we don't distribute mice to snake farms and pet shops, and we don't make them available to people who want fancy mice. And we have to be sure that anyone who asks for mice is at a bona fide research institution; that's the nature of our operation." It doesn't satisfy the ordinary definition of selling on the open market. However, Kendall Young saw that and he regarded it as the basis for his suit against the Laboratory. So that was the issue: were we indeed engaged in selling--breeding and selling--mice for the purpose of supplying the open market? Well, the hearing went on for several days. C.C. Little was called to testify, I was called to testify, Dale Foley was called to testify, many words, but always there was this issue of what exactly were we doing. So I had to explain to him, explain to the judge, that we were indeed involved in breeding all these mice, there's no doubt about that. And that we were in fact selling them but that we were using the mice as a means of learning how to raise large colonies of mice, for one thing. We were also examining all the mice to see if there were any mutations among them, and if there were, we were harvesting the mutations to serve as a basis for future research, that these mutations were valuable even though they only occur at about one or two per million mice examined. Nonetheless, they are the foundation
work for tomorrow's research. Just contrast this with the so-called commercial breeders of mice who have yet to discover their first mutation. Our operation is different from those. Well, the outcome of that court case was that our related business activity of that sort was regarded as tax exempt. However, it turned out that we had three pieces of property on which there were dwellings that were occupied by staff members or other people associated with the Laboratory. I had deliberately arranged, after I became Director, that we should pay taxes on that property and we were in fact paying taxes on the property, but we were not paying taxes on the dwellings. I didn't know there was a difference between property tax and dwelling tax, but it turns out, at least in the State of Maine, that the dwelling tax includes the physical structure above ground plus a little bit of ground around it and the property tax—real estate tax—is out beyond that. So, in this sense, we lost the case: the judge ruled that the Jackson Laboratory should henceforth pay taxes on these dwellings, of which there were three. To the town of Bar Harbor, this increased our tax liability by about $700 per year, which I must say we should have been paying in the first place. It was only due to ignorance that no one, Dale Foley likewise, knew about this distinction between dwelling tax and ground tax. And so we agreed to that judgment. I do want to insert this statement, however, that during the course
of the hearing, Orman Twitchell, Mr. Young's Lawyer, endeavored to establish that the Jackson Laboratory was indeed operated like a commercial venture. And among the things that he wished to establish: When we came to build the Morrell Park Laboratory, we had to borrow money from the Union Mutual Life Insurance Company. And I said: "Yes, we did borrow money from the Union Mutual Life Insurance Company." And he said; "That money was borrowed, was it not, as an ordinary mortgage loan?" I said: "Not so." He said: "What do you mean by that?" I said: "Union Mutual wouldn't give us a mortgage loan. The president of Union Mutual said that we did not qualify, he did not want to have this building as the asset to--" He said: "How did you get the money otherwise?" I said: "We arranged that the note was countersigned by Nelson and David Rockefeller." Judge Smith, Edwin Smith, of Bar Harbor who was the attorney for the Mount Desert Island Biological Laboratory, told me afterwards that I should have seen the look on Orman Twitchell's face. The first thing a lawyer is supposed to learn is never ask a question that he doesn't know the answer to. In the middle of the 1960s the Laboratory, got engaged in producing a new edition of the Biology of the Laboratory Mouse. This was a book that had become the "Bible" of mouse biologists. The first edition had been published in
1941 by Blakiston Company and was edited by George Snell. In fact, that first edition had been in preparation while I was a student at the Laboratory in 1938 and '39. So I saw it, in that sense, from the beginning. By 1963, it was clear to me that we needed a new edition. So much work had been done, and the old volume was obsolete in some respects, but just as fresh as today, in other respects. Anyhow, in 1963, I asked George Snell if he would care to serve as editor of a new edition. He seemed to think that he could do that, but as it turned out he really did not have time to do it. I talked with him further and it looked as though he and I might be joint editors. But still George didn't have time to do anything about it. Well, I didn't feel as though I had time to do anything about it, but somebody had to do it. So I decided that along with everything else, I would simply find time to be the Editor of the next edition of the Biology of the Laboratory Mouse. So I plunged in, and I must say that I wish I had done it earlier, but I eventually did it anyway. I learned about commas and hyphens. I learned about the dozens of arbitrary conventions of our scientific writing. It's amazing to me that one can go along writing, what he thinks is good English, only to discover years later that there is still plenty of room for improvement. The various members of the staff were in due course organized and assigned which chapters to write. And then I had to cope with
the various styles of authors, had to figure if they were all going to be published in the same volume, that there had to be some uniformity. I recall now, I think the exact number was famous memo Number 20--memo 20 about the Biology of the Laboratory Mouse--through which I had finally set forth a definitive style about when we were going to symbol %, or to write percent solid, or separate it with a space as per cent. There were three choices; which one do we take. All of these things had to be decided. So we got that all done, we got all the chapters assembled. Just two authors, Pat Dagg and Henry Winn, seemed to have trouble getting their chapters in. But eventually even they came along, and the whole business was shipped off to the McGraw-Hill Book Company. Then I discovered that it was up to me as the Editor to make an index. This was a refreshing and informative experience--I shouldn't have said refreshing--it was an overwhelming but informative experience. Also, for the second time in history we got colored plates of various types of mice published. The first time was in C.C. Little's doctoral dissertation at Harvard University in 1914, this was the second time. The book came out in 1966 and went through a couple of printings by McGraw-Hill Book Company, and then was reprinted by Dover Publications in 1975. Since that time it has been superceded by a new four-volume work called The Mouse in Biomedical Research.
I now would like to talk about two documents under the title of *The Manual of Policies and Procedures* and *The Manual of Format and Style*. *The Manual of Policies and Procedures* had a lengthy and somewhat disconcerting birth. It came about because of my own inconsistency in making administrative decisions—small administrative decisions. For example: We arranged that staff members, who were entertaining visitors, should be given a certain entertainment allowance. And this allowance was declared in advance. Well, on one occasion I allowed, say, $25 for entertainment expenses and on another occasion, which was essentially similar, I allowed $40 for entertainment expenses. Well, that's all it takes to cause an uproar, and this came up at one of the staff meetings. And Charity Waymouth is the one who said: "What we need is a manual of policies and procedures." Well, I didn't need to hear that twice to know that that was exactly what we needed. But needing something and getting it are two different things, and it really took the next 15-20 years to create it. And even by the time I retired, I would not say that the manual was completed in every detail, but it had most of the routine procedures: travel requests, what you do when you want to do this or that, routine for appointing new staff members, grant application procedure, manuscript review procedure—all of this is written down. But in the act of trying to create this
manual, we ran into some difficult problems. For example: A travel request, which the Laboratory was using, and with which people were traveling. I tried to find out how that the travel request was actually handled. A travel request was handled by something like seven different people. It went in an orderly process, but all that each one of them knew was what the given secretary or clerk in one office or another did, and then passed it on to somebody else. No one knew the whole thing, and I had to interview all of the people involved to find out who was doing what. The poor applicant, who wanted to travel, if he wanted to interrupt it at any point had no idea what to do, because no place had any explanation what happened to this piece of paper, which you turned in and eventually were allowed to take the trip because of it. Our problem was that plus another feature that instead of things being written down in the active voice of who shall do something it was in the passive voice of something was done and by whom was not stated. So I was complaining about this to Dale Foley one day, when he said: "I've got just the thing for you." There had just been an article published in one of the accounting journals about the use of playscript, which means actor and the words spoken, the way playwrights use for writing plays. Here's the voice and here's what is spoken, actor and action, and this is called playscript. "That's what you need,
a playscript." Put down the actor and put down what the actor does, and you can't be in the passive, because the actor is right there. You know exactly who's acting. That's another thing I didn't need to hear twice. That's exactly what we needed. So we created this Manual of Policies and Procedures in active voice form. Nothing is done except by designation of who is doing it and all the steps are setforth. We had also decided to revise our various forms: travel request forms, manuscript review forms, grant application forms, whatever—we had to revise the forms to display who was doing what at each stage of the way. But when it was all done it made it very easy for a stranger, for example or a new staff member coming to the Laboratory to get a sudden acquaintance of how things go. If he learned how to use the Manual—that in itself was a problem of course—but if he learned how to use the Manual he could proceed relatively quickly. We did, however, have one disconcerting upheaval. After the Manual had been in use for about a year or two, and we had a category of numbers assigned and one of them was, say, number 13, for the mailboxes, the secretary who was in charge of assigning numbers issued some instruction under the magic number 13 for mailboxes. Then I raised the question about the use of mailboxes, why should we issue a number in the memorandum about mailboxes, that ought to be listed under something else. Her answer was:
"Well, there's nothing else under mailboxes, that number was available so I put this in mailboxes." We can't do it that way or no one will be able to use this Manual. So I took that as the occasion to really revise the Manual with a whole new numbering system. I then asked George Vose to be specifically in charge of assigning numbers to everything that goes into the Manual.

SM: Now, since several people have spoken about this, was the Manual dynamic? That is to say, could parts be added as situations developed?

EG: It was in a ring binder. The ring binder and some parts were revised every year, had to be because of change in circumstances. Other parts would endure longer than that, indeed there might be some parts that didn't get changed at all. But it was deliberately arranged so that any part could be replaced as needed. But, of course, getting something new required a fresh act and sometimes hard work to keep up with it.

SM: It was quite interactive then in terms of the Laboratory needs. In other words it didn't lock the Lab into a straight jacket of how things are done; it was changed as situations developed.

EG: It certainly wasn't supposed to be that way. Of course you know and I know that as soon as the administrative office issues an edict, it either becomes sacred and we can't change
it, or it becomes so offensive we'll do the best we can to do otherwise. But it was not intended to be either way, it was supposed to be: here is our idea of how something should be done today, and we'll do it this way until we find reason to do it otherwise, which has got to be regarded as mutable if it did change. Several years after the Manual of Policies and Procedures had been in use, we realized that our secretaries needed some guidance with respect to format and style of letters, and memoranda, and grant applications, and all other kinds of documents that the Laboratory is concerned with putting out. So with my, now burgeoning experience as Editor of the Biology of the Laboratory Mouse and Manual of Policies and Procedures, I decided that I would create a Manual of Format and Style, which I did. The first issue came out in 1971 and it, at least, was used for the next four years. I don't know whether it was used after that, but it covered all the things that I've mentioned about letters, memos, and other documents, but also about word usage and some guidance about punctuation and the various things that I, myself, had learned the hard way in the past.

SM: Now, you've hardly mentioned the Board of Scientific Overseers, and could you elaborate a bit about how it operated?

EG: Yes, I'll be glad to try. The Board of Scientific
Overseers was a creation of many years ago, long before I became Director of the Laboratory. The general concept is that the Board of Trustees, or as it has come to call the Board of Governing Trustees, would be largely made up of businessmen or people who have a business background, who would be concerned with the financial status of the Laboratory and would be concerned about expenditures and about raising in contributions. But they might not have much comprehension about the scientific work. So there should be a board of people who might have marginal or even negligible interest in the business affairs, but who could in effect perform a scientific audit, rather than just a monetary audit of the Laboratory. And a group of people who, upon discerning some problem that's arising either in the administration or the staff, might be in a position to advise the administration, or better yet to advise the Trustees, as to what would be the proper course. And so I fell in with the idea of a Board of Scientific Overseers, almost as if it were my own idea, it seemed to be so good. I was very clear from the beginning that the ultimate authority lay with the legislature, the Board of Trustees or the Board of Governing Trustees, and that the group of people who became to be called the Board of Scientific Overseers were, in fact, a special committee of the Trustees. They were
not an independent board, they were sort of an elaborate, special committee of the Trustees. Indeed they have some rules of operation and they elected their own members, and, most of the time that I was Director, the members served three year terms and then could be reelected for additional three year terms. There were nine of them altogether and these, in general, were eminent scientists whose names could be readily recognized among biologist the world over. Some of them were Nobel Prize winners and some of them were members of the National Academy of Sciences. I made it a regular practice to consult the Board about appointments to the staff. They also reviewed the budget each year, not in as much detail as the Budget committee and the Trustees did, but they reviewed it. And they also spent at least one full day at the Laboratory having person to person, I'm trying to say single person, conversations with members of the staff. Following which, in the evening, there was a meeting of the Board with the Administration, with the Director in particular, in which they could ask me questions about clarifying one thing or another, that they had heard about during the day. Or, alternatively, advise me of problems that they may have heard about and thought I ought to know about, so that I could do something about them. But then just before I retired, a year or two before I retired, a problem arose and different people had different
solutions for it. The problem was that one member of the Board in particular, and maybe there was more than one member, had through the passage of time become an old man. He was no longer acquainted with what was going on in any field including his own, and he was not really competent to carry out the conversations with the staff members and even to perform his other duties. Now, I was aware of this and everybody else was aware of it, and it seemed to me that the easy solution, the appropriate solution, would have been for the Nominating Committee of the Board simply to face the question and when his term came to an end ask him to resign. Or just fail to nominate him would be the straight forward thing to do. But they didn't do that, instead they nominated him and, of course, he was elected again. And this was, frankly, a deplorable thing to do. Well, members of the staff became aware of this also and so, two of them in particular, Seldon Bernstein and Douglas Coleman, took it upon themselves to try to get the Board to change its way of doing business. They had an idea, the idea itself wasn't too bad, but it was the mechanism by which they handled it that caused trouble. What they did was propose this change directly to the members of the Board without informing the Director of the Laboratory of what they were doing. What they suggested was that members of the Board of Scientific Overseers should be elected for a term of three years, then could be
elected for another term of three years, but then they would have to go off the Board, be off the Board for at least one year, before they could be elected again to serve on the Board. The first I heard of this was not through the proper channels with members of the staff communicating to the Director, but instead I heard about it at the so-called Great Friday Evening Meeting of the Board of the Scientific Overseers. And the Board of Scientific Overseers was all set to adopt the change and go from there, but I pled for more time. This was the first that I'd heard of this idea and I didn't think we had studied the possible consequences of it. I would like to have a little chance to see if there was any merit in this idea. I wasn't really opposed to the idea as such, but it was new. I'd like to know what its effect would be. So they agreed with that and they did not act on the proposition, and that gave me then the responsibility to see what would have happened. What I did was take the names of the then-current members of the Board, and past members of the Board for that matter, and take for example: James F. Crow, who had been a member of the Board of Scientific Overseers since something like 1962, and he's still a member of the Board. And what would have happened if we'd had a regulation of that sort, as proposed, in force in 1962. Jim Crow would have served from 1962 to 1965 and then he would then be reelected to '65 to '68, let's say.
EG 85

Following which he would be off the Board for a year and then he could be reelected. Well, half a dozen other institutions around this country would just love to have Jim Crow as a member of their overseers board, or whatever. My idea was if we lost Jim Crow for one year, we've lost him forever. And the same thing would happen with Neal Miller, a member of National Academy of Sciences. He was serving as Chairman of the Board of Scientific Overseers, a winner of the National Medal of Science: Take him off the Board and he'll be swept up by somebody else. Leonard Carmichael, who was Secretary of the Smithsonian Institution and who was, at the time I'm speaking of, Vice President of the National Geographic Society, and Director of the Research Department, he likewise would have been lost. If this rule had been in effect during the recent past fifteen years or so, we surely would have had a lot of turnover of the Board and it would have been force upon us by this rule. But only to our disadvantage, we would have lost some really very good people. So I took this analysis back to the Board the next time we met, and said this is what I think would have happened. I said what I think is the easiest solution to this problem, and that is you people choose from your own membership a Nominating Committee each year. Why don't you agree that you will recognize among your own members those who are no longer really capable of serving.
This comes at about 70 years of age or even earlier sometimes. Think about whether they are capable of serving, and let the Nominating Committee perform its right and proper function of not nominating. I think that's where the responsibility lies, require the members of the Nominating Committee to reflect seriously on the competence of the other members of the Board, and not renominate those who are not competent enough to serve. Then they all looked around at each other and decided that was the right and proper thing to do. The usual phrase is how to deal with the dead wood. An organization such as the board or whatever, if it were a tree would have a lot of branches and some of the branches die. If that's a good analogy, would you in every year, in order to get rid of the dead wood, just automatically cut off a third of the branches. Should you instead look at what you are cutting off, and make sure you are getting rid of the right one, instead of ones that may be the best branches of the whole tree.

SM: Can I ask you one question, this has come up several times in the interviews I've done, about the Board of Scientific Overseers. Some people have asked me, the scientists have asked me: If the Board of Scientific Overseers is to give input to the Board of Trustees and the Director about the scientific opinions shouldn't they be here for a longer period of time than they are? What do you think of that?
EG: Well, to be short, the Board had two principle meetings during the year. It met once in the Spring to review staff status and promotions, and that sort of thing. And it met once in August, a full-day meeting interviewing the staff and meeting with the Director. Yes, it would be ever so nice if they spent more time, but remember now who these people are; Vice President of the National Geographic Society; Professor of Genetics, University of Wisconsin; Professor of Psychology at Rockefeller University, and so on and so on. If we were to require any more time of them, in the nature of volunteer work, I think some of them would bear a little bit more, but others would say: "I'm sorry, I just can't spend that much time. Two days is all I can take, I've got too many other things to do." So I will admit the desire being a valid desire, but the feasibility might be very difficult.

End of side two, tape two.

A year or two before I retired, I'll call this: The Change of the Working Hours of the Laboratory. Up to this time the, or at least in recent years before this time, the schedule of working for the regular employees—I'm not talking about staff members who don't have regular working hours—the posted working hours started at 7:30 in the morning and went
to 4:00 in the afternoon with a half hour off for lunch. And I think that makes eight hours, something like that. 7:30 to 11:30 to 12:00 to 4:00, that would be eight hours. And that was for five days a week, so it was a 40 hour week. Many of the people who work at the Laboratory are motivated and maybe compelled by their circumstances, particularly during the Summer, to have a second job--moonlighting. So some of them had other jobs that started at, say, 4:00 in the afternoon running to midnight. Working at motels, on gardens, or whatever. And I'm the first one to acknowledge the need on the part of some of the people to do this in order to be able to make ends meet. It's hard to find an adequate source of income here.

What happened was one day Tom Hyde—who was at that time the Manager of General Services at the Laboratory, and had told me in advance that this would happen--brought in a piece of paper in the form of a petition. Petitions aren't really appropriate for this sort of organization: this was sort of a town government business, but all right you have to excuse the names because people don't know this. So they brought in this petition, signed by quite a large number of the employees, the animal caretakers from the Morrell Park Laboratory. And they wanted a change in the hours. I don't remember specifically what they asked for, but they wanted a change and I think they quoted the reason I've just given, they each wanted to be able to carry a second job. And in
particular, Tom explained to me the first two signatures on this petition were by two young women who were really in quite a desperate situation. They had taken the jobs at the Laboratory and now summer was coming and they were going to have other jobs, they were going to be carrying two full jobs, two full eight hour jobs. And with the Laboratory's present schedule they simply wouldn't be able to work it out. And they were really desperate. I must say, in my own defense, I worked on this problem of possibly changing the working hours just as fast as I could. I consulted with other members of the administration. Remember, as the Director I am traveling, I spent usually about three days a week at the Laboratory. I had numerous other outside commitments. I consulted with the administrative staff, I talked with the members of the staff, I then had a meeting with all the supervisors, because this was an aspect of the Laboratory I was really not totally familiar with, and asked them: How does it work out if we changed the working hours, and then I specifically suggested that we changed the hours to 7:30 to 3:30, which is just an eight hour interval. But if we take a half hour out for lunch, that will be a paid half hour. So it would be eight hours with a paid half hour rather than an unpaid half hour. And I asked them if there were 15, 17 of them--I asked them: "Do you think this will work? And will you back it up if I put it into effect?"
Everyone of them, absolutely no questions asked. They were enthusiastic about it, they thought they could make it work. And of course, they probably had some personal investment in the idea too. Ok, they were going to make it work, so I authorized the change in the Laboratory hours from 7:30 to 3:30 and that went into effect, and remained in effect while I was Director and maybe after that. Oh, in a matter of ten days or so I saw Tom Hyde and I said: "Tom, how do those two young women like this new schedule?" Tom said: "They don't work here anymore, they resigned right after they filed the petition." Not too long before I retired, the new Library Conference Center was finished and was ready for occupancy. Here on the top floor was this wondrous new trapezoidal space that was to house the Library, but the Library was on the third floor of Unit 4 of the Main Laboratory. How to get the precious Research Library and its many valuable reprints from the existing location to the new building? Well, maybe it wasn't my business, but I started to think about it. At first I could see a chute from the Library, running across and depositing into the new space. I could see precious manuscripts flowing down this chute and maybe a little bit of wind blowing the paper off, hoping maybe no more than a two percent loss might be possible. Well, then I thought there must be another way. So I thought, get a line of people and
have a bucket brigade sort of thing, a book brigade, we'll pass the books one at a time. In a couple of days we'll have the whole thing moved. Well, that didn't sit quite right, so then I thought, why don't we sort of make a gala occasion out of it, we'll have a party. We'll make it an all-day party. We've got enough carts and rolling stock here to--and enough muscles that we could get all this stuff on carts, wheel them to the elevator, down the floor, around, and then up into the new space. O.K., so I outlined this idea to Joan Staats, the Librarian, and Joan prides herself to never having agreed to any new idea. I think that's the appropriate characterization of her. And so I outlined it to her anyway, and she scowled and mumbled a little bit and didn't say anything more. And so I let it pass. A couple of weeks later, I said: "Joan, have you had a chance to think about the plan I outlined for moving the Library?" And she said: "Yes, only to the point of condemning it." So I let that pass. The next thing I did was ask Peter Hoppe, Hans Heiniger, and Harry Chen to mastermind a plan for moving the Library by the general method of putting the books on carts and wheeling them around to the new place. I said: "Now, whip it up, get enthusiasm worked up so that everybody's involved and everybody's doing something, that includes people preparing a picnic lunch and if it takes all day we'll have cocktails at the end of the day. And we'll make a day
of it, it will be a family affair. For people who have children, arrange for babysitters. We'll do everything so that this can all be done in one day." Then I went to Joan Staats, and I told her this is the way the Library was going to be moved and I told her, since it's necessary to order her rather than to ask her, I told her what I wanted her to do was to get the new shelving all properly labeled so that if someone came in with copies of the *Journal of Morphology*, they would know exactly where the *Journal of Morphology* was to go and they would just set them in the right place. Joan did that and was doing a good job at that, she did it exquisitely. She had all the books identified at the beginning by some code number and all the places they were to go to identified by the same code numbers. It was easy for people who had any brains at all to find where the books were to go. So this was one Saturday in October and the October must have been in 1974. In October of 1974, we made this *The Day We Moved the Library*. Well, we had Alan Russell and Bill Dupuy taking moving pictures of the whole thing, so as we could remind ourselves of it and Alan Russell arranged for one of the biggest, huskiest fellows by the name of Mark Kristal to be photographed sort of sitting in a lounging position in the new Library and drinking Coca-cola or something and as a cart came by he put his empty coke bottle on the cart. According to the movie he sat there all day, actually he didn't. And
then at the very last thing in the afternoon when all the work was done, he stood up and turned out the lights. Just before the very last thing the movie shows the final cart coming, and as it comes to the door it ejects the Librarian, Joan Staats, into the new Library. We had the picnic lunch, the work went on into the afternoon. I got tired out, I had to leave at about 3 or 4 o'clock. I came back in time for the cocktails anyhow, and as far as I know the party lasted all day and on into the evening. But the children were there, the spouses were there, the whole families were there, people coming and going out with this line of carts, all of this rolling stock that was usually loaded with cages. Everybody working all day long, the whole thing was done and everyone had a wonderful time. From my point-of-view it was too bad that we didn't have a Library to move every month or so. The final thing I'd like to talk about, on this occasion at least, is the very last thing that happened during my period as Director of the Laboratory, namely, the review by the Internal Revenue Service of the Laboratory's tax exempt status. I first thought there might be a possibility of such a review when, in the spring of 1975, I saw a small note in the newspaper saying that the Internal Revenue Service was going to make a special project out of reviewing the tax exempt status of the so-called nonprofit institutions. And in fact they were going to start in
New England. That was just a 3,4,5, line article in the newspaper, and I interpreted that to mean that, if they were going to start anywhere in New England, they would pick out one institution in Boston, and the next one would be the Jackson Laboratory in Bar Harbor, Maine. So I was not really too surprised when we received a letter from Walter G. Arnold of the Internal Revenue Service office in Boston, saying that he would like to come up and have a talk with us. Which he did; he came up in March or April in 1975. He spent two and a half days with us. He asked a lot of questions. I had quite a long interview with him, but I was fearful that he did not really gain a good grasp of the Laboratory in that length of time, and that turned out to be the case. For example, I told him that we published papers in the scientific literature. The way that came back in his review was we advertised our mice in the scientific literature. That seemed astonishing at the time, but I must say that in the years since then the Federal courts have ruled that if authors are required to pay for having their papers published in scientific journals, as many journals do require authors to pay for publications, then those articles must bare a notice saying that this is an advertisement paid for by the author and the word "advertisement" has to be used to characterize it. So Arnold was using the language of the future when he referred to our published papers as
advertising. Well, at any rate, when we received his letter, making a number of assertions and asking for a lot more information, I circulated a copy of his letter to members of our Board of Governing Trustees and to members of staff. Because there was no question about it, this was going to be a Laboratory wide issue, a Laboratory-wide concern, and it would occupy our attention for many months to come. After I had circulated that letter to the Trustees I got replies back from quite a number of them. And most of them were calm and thoughtful letters, and I want to be sure that's in the record, but some of them expressed great apprehension about the Internal Revenue Service examining the affairs of the Laboratory. One person thought it was a vicious attack on the Laboratory, another thought it had been instigated by people at the Charles River Breeding Laboratory in Cambridge and advised that we should find out who Arnold's superiors are in order to force him to withdraw, and others felt it must surely be a hostile approach, given the way Arnold had phrased his questions, to the Laboratory. Well, the reaction on the part of the staff was not quite in the same vein. They had concerns of the relationship of the production operation to the research operation. By and large, they were all enthusiastic about preserving the relationship as they, at that time, had it. However, I do have a memo from Douglas Coleman in which he says:
"I feel I must preface any comments I make concerning your letter to Mr. Arnold by saying that I am not in sympathy with our being tax exempt under the present circumstances of our operation. I consider the production endeavor to far out-weigh the research effort both in emphasis and in total support." Well, I had to assess these various comments and judge what should be my appropriate response--my appropriate attitude about how I should go about trying to deal with Mr. Arnold, and I really concluded the same thing that I had concluded before, that this was a routine investigation by the Internal Revenue Service. They had decided that they were going to look at the non-profit institutions, where else in New England can they look? They'd given Mr. Arnold the job of looking at us, and he didn't have any ulterior motive at all: except to earn a living, this was his job. And that whatever questions he asked, we should try to answer them as honestly and forthrightly as we could. To try to give him a picture of how we operated and so I wrote this memo on August 7. And I would like to read a memo at some length. This memo was actually to Douglas Coleman, in response to his letter; but it also refers to things the Trustees had also said. "With respect to your ideas of relationship between production and research, I think there may not be very much difference in our views on general philosophical grounds. However, I have not had the luxury of taking a dogmatic position on this
question. There are four forces at work: our customers who want more mice, our staff who want more money, our Trustees who would like to be identified with a thriving institution, and the Internal Revenue Service. The challenge has been to keep or to find a point of balance between these forces. Each time that we have been on the brink of a major expansion in the production effort (and of the research and training efforts, too), I have taken care to discuss the issues with the staff. Only upon being satisfied that the staff favors the move have I then carried it to the Trustees. I don't recall any strong stand against any of the expansions we have taken in the past. I do recall that I, myself, have been somewhat puzzled by the eagerness for the expansion on the part of some members of staff. As the record adequately shows, however, the building up of production has provided the means for building up of research and training, both with respect to facilities and annual operating costs. You refer to dissociating production from the rest of the Lab. I thought through that proposition to the best of my ability a dozen years ago. Frankly, I do not like what I can foresee. [And I had written a long memorandum on this in December of 1963, which was ten years earlier.] "You asked if we are so greedy for the almighty dollar that we must permit production to become half of our dollar income. I think you have put your finger on the real problem. Scarcely a day passes, but that
some member of the staff comes to my office to plead for more money for something: more for seminar and lectures, more pay for assistants, more books and journals for the Library, more salary for themselves, more space for fellows and associates, more paid research leaves, more funds for bringing in new staff members, more equipment, more mice, more space for equipment and mice, more electrical power. On and on it goes. I regard most of this as expression of normal desires to move forward or, at least, to keep pace. I feel that it's too bad that we can't do more, but some of the pleading borders on greed. In the fiscal year that just ended the production effort yielded nearly $300,000 that was applied to the benefit of the Research and Training programs. If we hadn't built up production a few years ago, we would not have had these funds available last year. I don't think we can close our eyes to the benefits that the Lab as a whole has enjoyed because of the production income, to say nothing of the advantage of having the large population instantly available. On numerous occasions over the last 15 years, I have written at length in the Monthly Summary and elsewhere about the problems of a research institution operating a related business activity. Minutes of Trustees meetings show that I have spoken of keeping a balance between these parts of the Lab. I have resisted those who want to impose business behavior upon us, by such things as advertising, market research,
salesman, franchise satellite breeders, and so forth. I have also resisted those who want to separate production from the rest of the Lab because I see that as an unmitigated disaster. I think that in unity and balance we can have strength. Frankly, at this point, I don't yet believe the IRS will rule that the production income is taxable. But, of course, that remains to be seen."

What we did then was draft a reply to Mr. Arnold, or to the Internal Revenue Service of which he was an agent. And we consulted with our counsel, Bill Evarts in New York, who got an associate of his, Mr. Blake, to work with him and work with us. They took these items that I had written down, that more-or-less spilled out of me, you get excited and it all comes pouring out. I had something like 21 different points. They, in a little more dispassionate fashion, could see that many of these points were interrelated, and so they boiled them down, and, I'd say, magnificently. They took this raw material and worked it around and put it into three major points. To group the argument in a more cohesive fashion. Well, then we came into a kind of turbulent time. This was now already August of 1975 and we were to have our reply in by the end of August. The first thing I did was to plead for an extension to the end of September of 1975. That would be, coincidentally, my retirement, which was for the end of September. And we had a lot of controversy about exactly what should be said in the letter.
I was striving to have statements, such as 'that, if we ever had to go to court, I would be comfortable saying that's the truth, that is the truth as I understood it, and I don't know anything else. But some people wanted to make out a full blown case that every mouse raised everywhere in the Jackson Laboratory and no matter where it's raised everything is absolutely for research. Well, that's a little too strong, while we use the mice at the Morrell Park Laboratory, scrutinizing them to see if they are mutants, indeed we have another motive, we are going to sell them. The ones that aren't mutants we are going to sell. Why? Because we need the income. It's because of what we have in the pair of shoes dangling on the end of these shoe strings. We need the income from the sale of animals. And under the rules of the Internal Revenue Service we can operate a related business activity. We fall in the category of a 501(c)3 institution, with a related business activity. All we have to do is to establish that this is a business activity but that it is related to our primary purpose. So the judgment will rest upon the circumstances upon which we are carrying out the activity. I became, in a short while, an expert on this section of the Internal Revenue Code and I could see that all we had to do was to tell them the truth and we'd be all right. We don't have to hire the sharpest IRS tax lawyer in the whole country in order to defend us. If these are reasonable people at the IRS
they will see that we have a defensible case.
SM: When you sold the mice, would you sell them for a lot of money? or just about what it cost you to raise them?
EG: We sell them for a slight profit, if I may use the word profit, we sell for a slight bit above our cost so that we could generate some income. We had to sell them for a fairly high price in order to avoid, let's say, competition with the commercial mouse breeders whose operations were taxed. We dared not sell them too much below that price, but I think for the most part ours would be more expensive than theirs anyhow, because we had so much expert expense. We had the source colony, the Pedigreed Expansion Stocks, the Foundation Stocks, the Mutant Expansion Stocks, all maintained in the very best conditions we could provide, so that our costs were always very high. I doubt that we could have competed with a commercial operation. So here we are in August and we're still worrying about the text of our reply to Mr. Arnold and the Trustees are getting very excited and are about to have an Annual Meeting. The members of the staff are seeing nothing but doom lying ahead, and I'm having a hard time keeping any sense of equilibrium about the whole thing. The Annual Meeting, I thought, was going to go off tranquilly, but it turned out at the last minute that they wanted to have another recasting of the letter to Mr. Arnold. And I raised a question with our Chairman, who was Hank Neilson at that
time. I said; "Hank, who is going to sign this letter?" I thought it was going to go out over my signature, but I was trying to say to Hanky that if these people--some of our Trustees were meeting with our lawyers in New York--if these people manage to change this letter too much I'm not going to be able to sign it." He said; "Well, let's wait and see."

So the newly revised letter came back from Bill Evarts in New York and, frankly, as it came back it could not possibly be signed by me. I have commented that the statements that were made in it were not ones that I could defend. So I called Bill Evarts, more exactly I called Blake at his office, and talked with Blake and I said; "Now look, the way this is written, you may as well know that I can not sign it. And I don't know who the Chairman is going to get to sign this letter. If you will change the things that I tell you now to change, I will sign it, and there will only be a few changes." He said; "Well, what are they?" So we went through it paragraph by paragraph and got that straightened out, because I was not going to live with that. And he took out all the, what I called, offending statements from this document. September now is drawing to a close. I was President of AIRI--that's Association of Independent Research Institutes--we were meeting in California; at one of the institutions in California. I remember being there, and I
remember the setting, the Pasadena Research Foundation, we were meeting at the Pasadena Research Foundation. I would be back in Bar Harbor just about a day or two before the end of September. While I was en route to California, Bill Evarts arranged that their final draft would be delivered to me at the airlines ticket counter at the Boston airport. Chancy as that might seem, that's exactly what happened. I got the copy there and then I arranged to stay in California over Sunday and worked at revising that copy, the final copy. I got back on Monday and gave the letter to Ellie (Eleanor) St. Denis to be typed. Ellie St. Denis, here it is. Here's a copy of it signed ESD-Eleanor St. Denis, 16 pages single spaced, there's the reply--the final reply of the Jackson Laboratory to Mr. Arnold along with 5 or 6 appendicies, the whole document with 3/8 of an inch of paper. I signed it at 4 o'clock of the afternoon of which I retired from the Jackson Laboratory. We made it that close. We waited and we waited for something to happen. On the 8th of December in 1975, we received from H.D. Mosher, District Director of the Internal Revenue Service in Boston, a letter saying that we are pleased to tell you that as a result of our examination for the period ending June 30, 1974, we will continue to recognize your organization as tax exempt. There will be no change.

SM: So now we're down to the point you have retired. Several people have wondered why, because you weren't 65 yet
EG 104

were you?
EG: I was 62.
SM: So why did you take early retirement? Did this sort of thing with the IRS make you feel that you were getting out in the nick-of-time?
EG: No. The decision to retire had been reached in the Spring of 1974. I'll try to talk about it as calmly as I can, but it has some emotions involved. I have really never talked to anyone else about it. This is divulging state secrets, you might say. But I guess it's time to do so. Well, just before I retired the Library Conference Center and the Mammalian Genetics Laboratory were being built. When ground was broken for those buildings in April of 1971, I think it was about then, I remember there was still snow on the ground, when I looked out my window you could see the backhoe and so-on breaking ground. And I said, "This is it. I have now reached an age, and there are many things left to be done, but I have now reached an age where I'm not going to have the time to do them. If I can see this project through, get those buildings operating, then that's the time for me to retire." So I'd say as early as 1971 I was already thinking that I should remove myself so that the Laboratory could go on. Again, I'd had the opportunity to see other institutions in which Directors had stayed on into their 80s and the institutions just gradually dwindled down of died while standing still. Then in the Spring of 1974
almost simultaneous with my sending a letter to Frank Gerrity who was Chairman at the time, saying that I wish to retire something like 18 months hence, I was suffering from, I would say, deep fatigue. I was just tired, I could go to bed to sleep and be tired in the morning and be tired all day. Take a vacation and be tired all along. I had not had a physical examination in more than 20 years, I didn't have any reason to have a physical examination, but I felt that the time had come. So I went to see Dr. Gilmore in Bar Harbor, Dr. Edward B. Gilmore, and he wanted to know when I had my last physical examination. I said well, probably something like 30 years ago. He examined me. He called me on the phone later after he had the results from the laboratory. He said; "You might come down, I have some problems that I would like to talk with you about." He informed me that I have chronic lymphocytic leukemia. It's a little problem saying that sort of thing to a biologist, because the first thing he does is go to the Library to see what it's all about. The expectation is that the people who have this kind of condition have a certain chance of living three years and have a much higher chance of being dead within seven years. So I thought in three years I'll be 65 and I think I'd better leave the Lab now. So I did a lot of things Dr. Gilmore recommended I should do for my general health, such as taking a nap every afternoon, that alleviates chronic venous insufficiency,
keeping my feet up on stools all day long. Also lie
down for about 20 minutes in the afternoon. So I started
doing that and then after I retired, actually I started doing
a lot more physical labor rather than sitting at a desk and
worrying about the IRS, and mouse production, and training
programs, and money for research, and everything else. So
even though I took on part-time teaching at the College
of the Atlantic, I nonetheless had 10 years of physical
exercise, splitting wood, mowing grass--

SM: And digging compost.

EG: And digging compost, and so on. So I was right out
straight and now here it is 12 years later. I still have the
condition. For a while it got considerably worse: the
laboratory tests showed that the leukocyte count was very much
higher, but then it dropped and it's back down about where it
was at the initial discovery. This condition, chronic
lymphocytic leukemia, is one that is customarily diagnosed in
aged males in our society when, by chance, they go for a
physical examination. There is no other good sign except for
the general tiredness and the need for more rest. This
information had not been generally known. Maybe a couple of
people know about it, but I think to an extent it's still a
mystery to people, but this is the reason. Furthermore, I
guess, I didn't see that I, as Director, could really move the
Laboratory to what had to be done next. And I didn't have
the time, any period of time, to mount a campaign to see it through. So with those two considerations I thought that maybe it was time for me to retire. And having done it, on a purely personal basis I don't regret having done it at all. It was just exactly the right thing to do. For the first year or two I spent most of my time writing a manuscript for a book, which has long since been published on Genetics and Probability in Animal Breeding Experiments and some other writing; a couple of book chapters, teaching at the College of the Atlantic, and I was on the Board of Trustees at the Bigelow Laboratory and served as Chairman of the Board for a period there, and now in the last year I have been swept up in the new concert hall and museum at the University of Maine. I'm on the advisory committee of the Maine Center for the Arts. That involves a meeting once a month or so, and then each meeting is followed by a day in which I have to write a memo about something which I had to use a word-processor for. If I may continue in this vein, a couple of years ago, we bought this IBM personal computer. Margaret used it as a word processor in connection with her work on the manuscript on the genetic variants and strains of the laboratory mouse. I have learned word processing, but just adequate to write letters and memos, and a little over a year ago I decided I would teach myself
the BASIC language for the computer. I'm not very good at going to courses to learn that sort thing, but I do like to read manuals. It is something that I can do on my own schedule, on my own time. So I'm very far from being a computer programmer. I have nonetheless learned to program the machine, somewhat. A year ago this winter, I produced an interactive computer program called: Computing the Recombination Probability by Means of Observed Numbers Times Scores. I put a note about this in the Mouse News Letter back in August and I've had requests for copies of it from Japan, France, England, Boston, Yale, New York, Pittsburgh, Washington, Vancouver B.C., and so on. And of course there is a copy of it available at the Laboratory. Muriel Davison has a copy of it.

SM: So for you it's been just as Dr. Schmitt's retirement--this man for whom I was doing an oral history too. For him, retirement means that you get a party and then you get another set of tires to put on the end of your feet. So you can go another 20 years.

EG: Very good. I like that.

SM: He's been retired now four times. Well, that has many times come up, people have wondered to me why you seemed to retire before your time.

EG: I think I've articulated the two principle reasons, I
could say that I needed a respite from the concerns of administrative work, and that means two things; one is sitting in a chair all day in an office, and the other is struggling with problems that deal with a lot of emotions as well as rigid facts, and after awhile you lose your enthusiasm for dealing with them. Particularly, if they are problems that you have dealt with once and here they've come back again. I thought I had finished with that and here we go again. Administration! I was at it 19 years, and I would say that is probably too long, and certainly long enough. So I was glad to be released from it. I had no regrets, I have no regrets about it, I'm glad I was Director for the Jackson Laboratory. I'm also glad that I'm not Director now, I was Director long enough.

SM: It is a problem that the Lab has, that so many people—not bad it's good—that they have a very stable staff, in the sense that people are there—well, if I wanted to do all the people that were there for 25 years or more, I would have an enormous number.

End of Side One Tape Three

I won't say it's a problem necessarily, but it is a side effect of the fact that they're so stable. The people who come here like to be here.

EG: Well, I understand that if a person is practicing medicine
or almost any other profession, and these individuals have to be licensed, that there are now criteria, and various hurdles of examination that they have to perform periodically during their lives. Research workers, fortunately, can escape that kind of formal review and stand for license renewal, and yet it's inescapable that the particular field of biology, and the relevant part we're talking about, genetics, has been moving so fast in the last 25 years, and in particular the last 10 years, that obsolescence is just part of the business. It is extremely hard for a person who gets his degree in one given year to still be on the forefront of research even 10 years later. Members of the staff of an institution such as the Jackson Laboratory have an enormous problem, as I say, in keeping up, or keeping an interest in aspects of research that are still sufficiently on the forefront so that they can be approved for funding. In a university, there may be an easy escape for this because there's so much committee work to be done, and older faculty members can spend their declining years on committees, but in a research institution, that's not a very easy solution. So staff renewal and rejuvenation is a difficult problem. I would say that while I was Director, 2 or 3 of the older staff members got offers for jobs elsewhere, and when a person leaves, there's always a sense of loss and tragedy, and "Oh my, it's too bad." and so on, and yet—on looking at it more dispassionately, I thought, "Maybe this is a good thing." And without being specific
about any individual, I just say generally, with certain obvious rare exceptions, but generally, by the time a person has reached, say, 50 or 55 years of age, his field of research has very possibly now long been superseded by other things that are jumping over it, so if such a person gets an offer for a good academic professorship at Columbia or Yale or Western Reserve, or wherever, I'd say, "Take it. Help train the next generation." He still has value in this respect, but he may not be able to carry out for many more years, at any rate, research that is really on the forefront of knowledge, and I don't think we should regret such reassignments, or indeed, if the person has been doing active research, and takes an administrative position at 50 or 55 years of age. Some people might regard that as a great social loss, but I would say that's a kind of readjustment necessary in our society. It's not a big disaster if that happens. Now I'm not talking about everybody: there are exceptions of people who are still alive of 75 or 80 and can "cut it," so to speak, can still make a go of it, and that's all to the good.

SM: Did the system have a flexibility, though, so that those people can remain active in it?

EG: Well, a research institution--if I understand your question with all its implications--that kind of flexibility may be very difficult to achieve, because certainly a research institution such as the Jackson Laboratory, which
starts out with no funds of its own—we are paid almost 100%
out of research grants—the salaries come from research grants.
Therefore, when a person, at whatever age—26 or 76, it doesn't make
any difference—at whatever age, if the person does not have the
ability to win a research grant, the Jackson Laboratory
doesn't have the means to support him any other way.
SM: Let me ask you this question then. From what you have
seen, is there some feeling—see, I heard this in the Schmitt
project, that there's some feeling among granting agencies
that people who are in their 70s—it's harder to justify
giving a grant to someone in his 70s because of age. I
guess they fear the guy could croak before the time period of
the grant was up, or something, so do you think that there's
some either overt or covert or subtle age discrimination that
works in the granting process?
EG: Well, I think there has to be a judgment. That's the
trouble with the word "discrimination": it's a loaded word
now. So you make a choice. Let's say you have so much money.
You have to distribute it around. You put it on your best
bets, and this may mean that, if a person 76 is competing
with someone 56 or 36, and otherwise the projects look to
have equal merit, you might be likely to put your money on
the 36 or 56 year old, and say, "Well, this 76-year-old
fellow, he's had a full life. We don't have anything against
him. He's a good guy, but maybe for the advantage of
society, whose money we are spending at this time, it may be
better for us to put it on the 36 or 56." If the resources are not limited, then of course, give the money to the 76-year-old too. We never have that situation: it's always a limitation. The word "discrimination"--don't forget: that's your word--the word "discrimination" makes it sound as though something ugly has been done, where it's really just the same--the judgment has to be made any time there's a choice between A and B. You're making the best choice you can under the circumstances.

SM: Is there any policy to recycle people through a system of sabbatical leaves?

EG: Well, the Laboratory and universities, in general, have plans for sabbatical leaves. There is a plan for doing that, that makes it possible for people to achieve a certain rejuvenation by doing that, and it is generally a good thing. It's not without problems, however: How to pay for it, for example. How are we going to administer it? How are we going to pay for it? So, if there's plenty of money, OK, everybody can have a sabbatical leave right on schedule. If there's not plenty of money, that's not what happens. It's as simple as that. It's not that they don't believe in it. It can't be done. There is such a thing as research leaves. I make a distinction: A sabbatical leave is one in which a person is free of all responsibilities. He can go off and do anything he wishes. Research leaves can be paid for, as there are grants for research leaves, in which you go to some specific
place to learn some very specific technique or procedure, or acquaint yourself with some new concept that would be useful in your own research. So research leave, which is really nothing more than a paid stay at some other institution, makes it a little more feasible to achieve this rejuvenation, where "sabbatical leave" means "take off and paint pictures by the seashore"—the protracted vacation. It's too often interpreted as being just that, and maybe it is not always that.

SM: Have you ever thought about the ideal size for the Jackson Laboratory?

EG: Indeed, I have.

SM: What do you think it is?

EG: Well, the ideal size of the Jackson Laboratory, or any other research institution, is that size which the then-Director feels comfortable in administering. Now what does that mean? I felt comfortable with a staff of about 35, and when I think of "size," I think of size of research staff. There can be 400 or 500 employees, but that doesn't make any difference, so long as there are other people to take care of them, so long as the research staff is in the neighborhood of 35. I think at one time while I was at the Laboratory, the staff went to 39; other times it would be as low as 29. It would range around 35. Now that didn't exclude the possibility of 42. Two or three more could be accommodated, but that was a size that I was comfortable with. If someone else
thought, "Well, 60 is the number I like," all right, if there is space, and there's money to support 60, fine, have 60. But my idea was that I didn't want the Laboratory to get so big that, upon my retirement, it would be necessary to hire a professional administrator, rather than a scientist, as Director. I thought there was a special value in keeping the Laboratory of such size that there was a prospect that it could be administered by a scientist. Now that means a scientist who had teaching or bench experience, research bench experience, and may not know very much about the rites and rituals of administration. I certainly didn't. It took me four years to gain some concept of how to operate the Jackson Laboratory, and that was maybe under the most pleasant circumstances that you could imagine, and it might not be that easy at other institutions. So, 35--I felt "If I can handle it, well, almost anybody else can handle it," and I would hope that the Laboratory might be able to continue at a size that would be attractive to scientists as administrators. If it gets so big that scientists feel restrained, then the Trustees would have to hire that shoe salesman I was talking about, and put him in as Director. There's another aspect to size that I'd like to get on the record, so to speak. Staff members often speak of having a critical mass. They--whether they are embryologists, or biochemists, or anatomists, or biologists, or psychologists--whoever they may be, are very much concerned about critical mass.
And you know the term "critical mass" comes to us from physics, where you have to have a certain mass in order to get a reaction. So staff members use the term "critical mass."

Having heard this term used repeatedly under various circumstances at the Laboratory, and having heard members of the Board of Scientific Overseers use the term, I never heard it mean anything except that we should have more in a particular field. One day when Neal Miller reported to the Board of Governing Trustees, he said that there was some question about the critical mass in a field at the Laboratory. One of the Trustees, Peter Godfrey specifically, said "But what exactly is meant by 'critical mass'?" And Neal Miller pointed to me and asked me to answer the question, and I said, "Well, I've been hearing the term "critical mass" for several years, and I now know that what it means is "one more person than the number we now have." (laughter)

END OF INTERVIEW