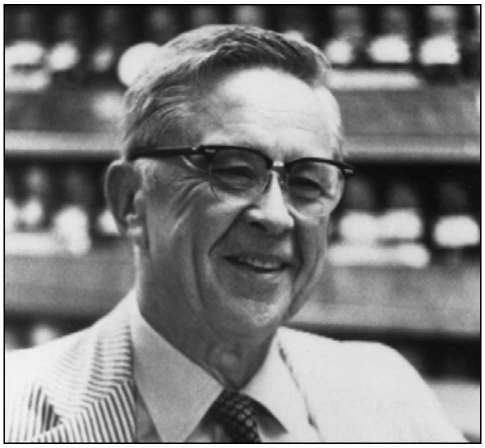

EDWARD B. LEWIS
(1918-2004)

INTERVIEWED BY
RACHEL PRUD'HOMME

July 31 and October 11, 1984

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Biology, genetics

Abstract
A 1984 interview in two sessions with Edward B. Lewis, then the Thomas Hunt Morgan Professor of Biology at Caltech. Dr. Lewis would be awarded the 1995 Nobel Prize in physiology or medicine, along with Christiane Nüsslein-Vollhard and Eric F. Wieschaus, for discoveries concerning "the genetic control of early embryonic development." In this interview, he recalls how he and a colleague, Edward Novitski (who would also receive a Caltech PhD), acquired stocks of *Drosophila melanogaster* while they were still high school students in Wilkes-Barre, Pennsylvania. In 1939, after a year at Bucknell on a music scholarship and only two years at the University of Minnesota, Lewis received his bachelor's degree (in biostatistics), whereupon he entered Caltech as a graduate student. Working under A. H. Sturtevant, he continued his *Drosophila* studies, receiving his PhD in genetics in 1942. After a wartime stint as a meteorologist in the Army Air Forces, Dr. Lewis returned to Caltech as an instructor in the Division of Biology in 1946. He became a full professor in 1956 and the Morgan Professor in 1966.

He recalls the early days of genetics at Caltech and offers his recollections of Thomas Hunt Morgan, chair of the division from 1928 to 1942, and of Sturtevant and Theodosius Dobzhansky. He comments on the state of the Biology Division after Morgan's retirement and on the arrival of George W.

Beadle as division chairman in 1946. He describes his work on the *Drosophila* bithorax complex of genes and also on the somatic effects of radiation on human beings and his part in the controversy over nuclear testing in the late 1950s and early 1960s. He recalls the visit of four geneticists from the Soviet Union in 1967. He concludes by commenting briefly on the changes in the field of genetics since the discovery of the genetic material and on his current work on the phenomenon of transvection.

Dr. Lewis became emeritus in 1988 and died on July 21, 2004.

Administrative information

Access

The interview is unrestricted.

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Contact information

Archives, California Institute of Technology
Mail Code 015A-74
Pasadena, CA 91125
Phone: (626)395-2704 Fax: (626)793-8756
Email: archives@caltech.edu

Two wings versus four. Ed Lewis's experiments with the bithorax complex (BX-C), a cluster of closely linked genes in *Drosophila*, produced a mutant with a double thoracic segment, hence an extra pair of wings. These photos were first published in 1982. Caltech Archives.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH EDWARD B. LEWIS

BY RACHEL PRUD'HOMME

PASADENA, CALIFORNIA

CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Edward B. Lewis **by Rachel Prud'homme**
Pasadena, California

Session 1	July 31, 1984
Session 2	October 11, 1984

Begin Tape 1, Side 1

PRUD'HOMME: Professor Lewis, where are you from originally?

LEWIS: I was born in Wilkes-Barre, Pennsylvania, in 1918. My father was a watchmaker; my mother was a housewife. She completed high school, but my father didn't have the opportunity to go to high school. His family was very poor, and so he was sent to a trade school to learn watch-making. I have one brother, who is about five and a half years older than I am. He went into the foreign service—he's retired now. There were just the two of us children, and we went to public schools in Wilkes-Barre.

PRUD'HOMME: What about your undergraduate education?

LEWIS: I spent my first year at Bucknell College in Lewisburg, Pennsylvania, on a music scholarship. Then I transferred to the University of Minnesota, which had the lowest fees for out-of-state residents of any state university in the country, I think, except for Berkeley—but Berkeley seemed too far away.

PRUD'HOMME: Did you know you were going to be a scientist?

LEWIS: I had an interest in animals at a very early age. I liked chemistry and biology. I also took up the flute rather early, so there was a little conflict about whether or not to major in music.

PRUD'HOMME: But you stuck with science. Did you have any special teachers that you remember at the university?

LEWIS: There was a professor of genetics at Minnesota named Clarence P. Oliver, who gave me space in his lab. Actually, I started work on *Drosophila* in high school, by getting *Drosophila* stocks that were procured indirectly through Caltech. I started that in 1934, fifty years ago.

PRUD'HOMME: So you knew about Caltech.

LEWIS: We had a good library in Wilkes-Barre. They had one scientific journal, *Science,* and in *Science* I saw an ad for *Drosophila* stocks that could be obtained from a Professor Rifenberg at Purdue. My interest in genetics and *Drosophila* was also sparked by a book by Herbert Spencer Jennings called *The Biological Basis of Human Nature*, which had some very nice diagrams of *Drosophila* mutants illustrating the laws of heredity. I had a friend in high school, Edward Novitski, who is now an emeritus professor of genetics at the University of Oregon. He and I teamed up to order the flies from Purdue. We grew them in the biology lab at the high school. It was a modern high school for the time and had a nice laboratory. Novitski began a correspondence with the professor at Purdue and was soon put in touch with Dr. Calvin Bridges, who was of course at Caltech. Although we had had to pay for the initial cultures from Purdue, Bridges sent us more strains at no charge. Actually, Novitski went to Purdue because of this connection. Purdue had a system whereby if you passed some written exams that were sent to high school students, you could get a scholarship. So he got a scholarship and went through Purdue in two years and came here to Caltech as a graduate student. I spent two years at Minnesota and got my bachelor's degree in biostatistics in 1939.

PRUD'HOMME: Two years isn't bad!

LEWIS: Well, at that time you could do it, at both institutions, by taking examinations for credit. So during the summer I'd study the courses. For example, I took calculus that way—by exam, rather than taking a course in it. I came to Caltech in August 1939. I'd had offers from Texas and from here, and this seemed a better place. Novitski had already come here.

The professor at Minnesota you asked about, Oliver, had studied with [H. J.] Muller. He was one of Muller's few students—Muller didn't have very many students. Muller was at the University of Texas, and Oliver was a Texan; in fact, he returned to the University of Texas a few years after I left Minnesota.

Minnesota had a strange chairman of zoology whose name was Dwight Minnich, and he was very eager that all majors in zoology—which was what the field was there; they had botany and zoology—he wanted them to be very cultured, so he had an awful lot of required courses outside zoology. Therefore, if you were going to major in zoology, it would take you at least four years. So I decided I liked biostatistics, and that department was in the medical school. Actually, I was the first undergraduate ever to major in biostatistics; the result was that they had no requirements to speak of. So I took some mathematical statistics and the courses they offered, and I had a major. I got out in two years because I was eager to get into graduate work and do genetics, do exactly what I wanted to do.

But there were some nice courses in zoology I took there. And the main thing—to get back to your point—was that Oliver was very generous about giving me laboratory space and some of his time. Although I worked mainly on my own, he was very helpful, and he didn't know me from Adam! At the time, there was another geneticist there—Mel Green, who is now a professor at Davis and who was also a student of Clarence Oliver. He was a year ahead of me; he's just retiring at Davis. Oliver has had a couple of people like that as his students, who have gone out and populated the world.

PRUD'HOMME: When you came here in 1939, what was your impression of the Biology Division?

LEWIS: Well, for me, growing up in the Depression, I would say first of all that Minnesota was a wonderful place. It was very exciting. I used to play in the university symphony orchestra. And the same thing was true when I came here—it was very exciting here.

PRUD'HOMME: But very different from Minnesota, I would think.

LEWIS: Oh, yes. The graduate work was very exciting. You had people like [Alfred H.]

Sturtevant, who first mapped genes. And people like [Theodosius] Dobzhansky, who was quite the opposite kind of personality; he was a very entertaining and interesting fellow.

PRUD'HOMME: Could you describe the two personalities for me—Sturtevant and Dobzhansky?

LEWIS: Well, they were complete opposites. All of this is well described by William B. Provine, in a book [*Dobzhansky's Genetics of Natural Populations,* R. C. Lewontin, John A. Moore, William B. Provine, and Bruce Wallace, eds. (New York: Columbia University Press, 1981)]. Provine has tried to pin down why these two men had a falling out eventually. Unfortunately, he cites Dobzhansky's reasons after Dobzhansky was some eighty years old, and Dobzhansky really says some outrageous things. But anyway, I think they were fundamentally entirely different personality types. They hit it off very well at the beginning, when Dobzhansky came to Morgan's group at Columbia from the Soviet Union as quite a relatively young assistant professor. Sturtevant was older. They had a falling out, and I think the falling out between Sturtevant and Dobzhansky is largely due to the failure of other researchers to duplicate some things Dobzhansky had done. Sturtevant's name was on some of his papers, and Sturtevant was furious that he'd been involved with some results that couldn't be duplicated. Some of Sturtevant's students discovered some of these difficulties. Dobzhansky went too fast. He was a man who wrote a paper a week, and he reviewed books. He was very good at writing and reviewing things and he wrote interesting books. But as a lab scientist, he tended to go too fast and depend too much on assistance and this kind of thing and not check everything himself. He did a prodigious amount of work, far more than he probably should have done. He rushed into print, and Sturtevant was the opposite. Sturtevant was very much a perfectionist, and he didn't want any mistakes, and so on. I'm pretty sure it was almost entirely that. But it was also said that Dobzhansky was somewhat wearing, because he talked all the time. Sturtevant was very quiet and didn't say a word. The story is that Dobzhansky was in this room, next door here [William G. Kerckhoff Laboratories of the Biological Sciences]. They shared a room together, these professors. Now, that's ridiculous—two professors sharing a lab! But that was the way they worked. I think it was because in the early days Morgan had the Fly Room at Columbia, with six or eight people in it, all packed like sardines. Anyway, this was told to me by [Boris] Ephrussi, who said that Dobzhansky would spend half the day talking. Sturtevant, being a quiet person, couldn't just say to

Dobzhansky, "Well, I think we ought to move into separate rooms." One night he came and he moved everything into another room and didn't say a word. [Laughter] And that was the end of their friendship. So then Dobzhansky finally left and went back to Columbia [1940].

PRUD'HOMME: Did you know [Thomas Hunt] Morgan?

LEWIS: Yes, I knew him, but he was getting a little senile. He was on my PhD thesis committee, actually, because Sturtevant asked him to come to it. But he clearly was no longer interested in genetics, and his mind was getting a little fuzzy. He was over seventy then. He was very impressed that someone working in *Drosophila* knew something about chemistry. I had a minor in bioorganic chemistry with [Arie J.] Haagen-Smit, and that seemed to impress him more than anything else. He had his senses, but I, being quite young, couldn't judge very well what type of person he was or had been. But he clearly wasn't functioning as a scientist anymore.

PRUD'HOMME: Some people have said that he was anti-Semitic? Did you ever feel that?

LEWIS: Dan [Daniel J.] Kevles dredged up a letter in the files that had been written by Morgan to [Robert Andrews] Millikan saying that we didn't want somebody on the faculty here. But as far as I know, there was never an indication of this. I did try to find out as much [as I could], and Norm Horowitz has looked into it a lot. Albert Tyler was Jewish, for example, and I know that it was a close relationship and there was never any problem. Unfortunately, that accusation was made after Sturtevant died, and I didn't bring it up with his widow, because she would have been just absolutely aghast. Sturtevant and his wife were very liberal and would have told me the truth about this if I had asked, but I didn't want to tell Phoebe Sturtevant, because she would have been furious if she'd found out that anybody had said that about Morgan. So I asked Mrs. [Sterling] Emerson about it instead. She dismissed it. She said people were clannish then; everybody was clannish. And I think that's true, from my childhood; there was always some slur being made about some other group of people; it could be the Irish, for example.

I think it depended on how many people you were in touch with. For example, there was never any anti-Semitism in our family. On the other hand, I think we have very few friends who were…. Academic people have never had any kind of racial prejudice and it was absolutely scandalous to think that you would come out and say anything like that in academic circles. Now, it's true that Morgan had told people they might have to change their name to get a job, and that was true, apparently. I think Tyler did; that wasn't his real name. But that was accepted, because it *was* so difficult. I suspect that Millikan was inclined to be that way. The only explanation I have for Morgan's remark, not having known Morgan and not knowing anything about it, would have been that he was teasing Millikan, in a sense. Morgan teased everybody, I understand; he was a great tease, and in this letter it's conceivable he was teasing Millikan by putting a remark like that in it. He did hire, on this faculty, people like Albert Tyler—Tyler was his right-hand man—and Henry Borsook and Norman Horowitz. He was very generous to all these people. As far as I know, it never affected anything he did. But there is that letter in the file, which I quickly went and looked at. So I have my theory, and that's that.

I remember a postdoc who was here for many years, a marvelous fellow. He got a kick out of appealing to people's prejudice by pretending that he went along with it, and he would draw them out and find out how bad they really were just by drawing them out. I couldn't do that, but he was an expert at it. And he was himself not a nasty person; he liked to tease. I'm not sure that this teasing business couldn't have been involved. I'm afraid we won't know now, because there are no survivors who can tell us. Sterling Emerson is living, but his memory is shot. George Beadle probably isn't much good either; his memory's gone.

Let me say this—that Sturtevant and Dobzhansky, and that whole group of people, would never discuss with students any such matters that might suggest…. These kinds of catty, slighting remarks were not made to students. It was a very high-level relationship between students and professors here. I do know that they somewhat felt that Millikan was too conservative, but that's Sturtevant. And all I know is that Morgan would tease Millikan. Morgan hated people who were deeply religious—well, not hated them, but he tended to tease people who, like Millikan, were scientists *and* religious. What Morgan didn't like were mystical people; in science, he didn't want any mysticism. In fact, his success was that he was one of the first experimentalists. Instead of being philosophical about embryology and science, he went into the lab and did simple, clean experiments.

PRUD'HOMME: Not a believer in romantic visions of the origin of life.

LEWIS: No. There was a naturalness about that whole group, an anti-pompous attitude. They were always making fun of people they considered pompous. I remember that Sturtevant would comment that so-and-so was too pompous. And they *hated* that. They hated speculation that was philosophical. I think that attitude passed on from Morgan to Sturtevant, you see. They were reductionists—Morgan especially. They all were what you would call reductionist rather than romantic mystics.

PRUD'HOMME: Were you assigned to work under somebody from the beginning?

LEWIS: No, we were not assigned; we had a choice, and I chose Sturtevant. Novitski had chosen Dobzhansky when he came, and by the end of the year I think he was a little uneasy about working for Dobzhansky, because Dobzhansky was sort of a slave driver to his students—and I think that might have influenced my choice. By inclination, I tended not to want to work with somebody who was quite as bombastic as Dobzhansky was. He was a bubbling sort of personality and he might have even given me the impression of being pretty domineering. By that time, having had contact with Novitski, I knew that Dobzhansky wasn't the person I wanted. But Sturtevant was a very quiet type and it was quite appealing to talk to this guy. He was a very thoughtful sort of fellow, an ideal person to start with.

PRUD'HOMME: Did you know what you were going to do your thesis on?

LEWIS: Yes, I had already decided.

PRUD'HOMME: What was it on?

LEWIS: Well, I started working on essentially some mutations we had actually found in high school and then carried on with. They're rough-eye mutations; they appeared to violate one of the principal laws of genetics, in that they were not quite like a single series of mutations of the same gene. It looked as though the gene might be either subdivisible or…. The way we really interpreted it was as clusters of genes and chromosomes that were duplicates, that arose by actual tandem duplication and divergence and so on. And I'm still working on this problem. I've chosen another system that is more profitable to study than the one I worked on for my thesis ["A genetic and cytological analysis of a tandem duplication and its included loci in *Drosophila melanogaster*" (1942)]. But I worked on that because I'd had some lucky results when I was in college, using that.

So I came here, and I told them what I wanted to do, and they were very willing to let you do anything you wanted. I outlined the experiments and took them in one day and showed them to Sturtevant, and he seemed pleased. He never showed any sign of great enthusiasm, but he was the kind of person who, it turned out, was quite enthusiastic about somebody who did have an idea but never gave you the slightest indication that he was interested.

PRUD'HOMME: He didn't give you any strokes.

LEWIS: Right. But you could sense that he was not unhappy.

PRUD'HOMME: I should think he would have been delighted to have you as a graduate student, because you were so directed.

LEWIS: I had heard a rumor that he'd said that he wasn't sure this new crop of graduate students would amount to anything, they seemed immature, and things like that. But then later on, when I presented to him what I had planned to do, I think he might have changed his mind a little bit. I'm sure I was very immature at the time, but….

Anyway, that was a nice thing, that you were not assigned—that's the main point—you were not assigned projects unless you asked to be given something. There was another chap here, a graduate student, and he asked for projects and Sturtevant gave him a few.

PRUD'HOMME: Who were your fellow students?

LEWIS: They were George Rudkin, who is at the Fox Chase Institute for Cancer Research, near retirement; Edward Novitski; Charles Metz, who I think may be director of the Marine Biological Laboratory at Woods Hole at the moment—Charles Metz was a graduate student with Albert Tyler. There was Klaus Mampell, who had been taken on by Dobzhansky. Dobzhanksy gave a lecture at USC [University of Southern California], and Klaus had been here for a while from Germany, I don't know for how long, and Klaus was impressed by Dobzhansky, who was interested in population genetics. And Dobzhansky said, "Well, come on over and be a graduate student." That's how you got to be graduate students in those days. And then there was another chap, William Hovanitz, who got in in a very similar way and worked for Dobzhansky. Novitski and I and Rudkin and Metz all finished the same year, 1942. Hovanitz and Mampell finished the next year.

So that was another nice feature; we had a nice group of students. We interacted pretty well. I forgot to mention Dwight Miller, who was a year ahead of us. Dwight Miller is a professor, probably retired now, at the University of Nebraska. Most of those people were working on population genetics. Almost everybody was. I was working on this problem, which is more general genetics, because I'd gotten started earlier. Otherwise I might have gotten into this population work.

PRUD'HOMME: Were you a teaching assistant?

LEWIS: Yes. We taught the beginning biology course and we assisted a little bit in the genetics lab.

PRUD'HOMME: Did you like teaching?

LEWIS: No, I've never been very good at teaching. We've always accepted it as an obligation, so you prepare the lab and you prepare the lectures. I've always liked helping lab students. I've had quite a few students who would come and do minor problems and work in the lab.

PRUD'HOMME: This was part of the genetics lab?

LEWIS: Or there's an undergraduate research program for minor programs. So most of my contacts have been there. But I taught the beginning genetics course for thirty years here—something like that, from about 1948 to three years ago—once a year for a term. At first I enjoyed very much teaching genetics. I would say that for twenty years I enjoyed it, and then I began to get bored, and it got to be too big a problem to know how to teach a course like that in one term. But fortunately, with Horowitz and Bob [Robert S.] Edgar, we did have a full year course. They taught a lot of the microbial genetics in those courses, so I didn't have to do that. I worked up lectures that tried to emphasize human genetics where I could—it was not really a human genetics course. And I would try to keep up with the modern developments and get them into the course. Because you'd realize that half the class would probably become medical doctors, and medical people were not getting any genetics at that time. Medical schools didn't have courses in genetics—or very few did at the time. In the fifties there might have been a handful in the whole country, maybe three to six medical schools that would teach genetics—Johns Hopkins, Michigan, Texas—but that's about it. And often they were almost experimental courses, given by a professor in the zoology or biology department. But of course now things have changed and it's become a leading subject in the field, and there's great interest in it.

PRUD'HOMME: As a graduate student, where did you live? Where did you eat?

LEWIS: I had a room across the street, in a little rooming house. It's all torn down; it was where the graduate student houses are. George Rudkin lived at home, but most of us found a room someplace like that. There were plenty of cottages around, and people liked to rent out rooms to students. We didn't have much money. Actually, my brother had helped me get through college. He was older and had a job, and he'd sent me some money when I was at Minnesota. Here we got just enough stipend to live; I don't think I had to get extra money. So we were able to live on that. The rent was cheap and the food was very cheap. So that's how it was.

PRUD'HOMME: From what you describe, I get a sense that your fellow students and the work you were doing you found extremely satisfying.

LEWIS: Yes. The field wasn't overwhelming yet, either. Right now, genetics is just overwhelming. I feel sorry, in a way, for young people who come in. There's so much to learn, so much competition, and it's such a vast field. It's what is called "big science" now; then it was just beginning, getting off the ground. Everything seemed exciting—all the problems. They also seemed beyond solution. That sounds contradictory—to be excited about something that's beyond solution. But you could not approach genetic problems directly—that is, we didn't know what the genes were. So we tried to deduce what they were from experiments. And it's an abstract subject, which allows you to deduce the linear order of the genes without any knowledge of what the genes are, or anything. It's just amazing what you can do. We thought no one would solve that question in our lifetime—what the genes were and how the things work. So in that sense, too, it was exciting. You felt you could make a contribution without knowing the actual physical chemistry of everything. In fact, it didn't help at all to know physical chemistry during that time, or even biochemistry. It didn't really help solve things. It was a long time before the molecular approach finally allowed people to think about genes in a new way, whereby you could eventually hope to even bypass breeding experiments and analyze the DNA directly. Potentially we can now sequence, say, all the DNA in a human being, and with the proper computer analysis you could almost figure out how things work and find the solution to everything, almost—that is, all the diseases that would be molecular. All kinds of medical conditions that are hereditary could be figured out by brute force, which is what I like to call it. But at that time, you see, it seemed hopeless that you could ever study human beings and find out very much. And that's still true, but it will change very rapidly. It's changing every day, at a tremendous pace. The point is, it's almost overwhelming for a young person nowadays.

But we were lucky. I think we lived in the golden age, in many ways, even in societal matters. We didn't have nuclear threats. We didn't have quite the overpopulation problem we have now. And also, to have come from a poor background without intellectual contacts of any kind, it was always exciting to me. You didn't get turned off in your second year of college, the way the children do now. I heard [George W.] Beadle express this. When Beadle was chairman [of the Division of Biology], during the sixties, we had all these problems with students. I remember that site visits were made here, to determine whether we should support our graduate students, or how we could get enough money to support our graduate students. The students would have to give little talks. Beadle's comment at the end was on how unhappy the graduate students were. "When I was in college, everything was exciting," he said. "You were thrilled, excited, and now these students are unhappy." That was the sixties. But I think it's still somewhat true.

PRUD'HOMME: What did you do during the war?

LEWIS: Well, I finished my PhD in June 1942 and in a few months I joined an Army Air Forces cadet program in meteorology here. I don't know how long that course lasted, but I think it was some four or five months, and then I was one of a small group who went to study oceanography at UCLA with [Harald] Sverdrup. I lived in Westwood. It was a crash course in oceanography, about six or eight weeks. Then I was assigned to the Pacific theater and I forecasted weather in Honolulu at Hickam Field, for about a year. Somehow I got to be the weather officer for the Tenth Army and I got assigned to the G-2 section. That was quite nice, because you got out of a lot of the routine. Then I was shipped out to Okinawa, where I was aboard one of the big command ships as a weather officer, using naval data to tell the ground troop air force what was happening in the local weather in Okinawa—these were mostly reconnaissance planes. So I would forecast the weather aboard ship, and it would be sent to shore, where the weather people ashore would take my weather maps and predict what was going to happen. It was a terrible job, because it's very hard to forecast. We really didn't know very much. We had terrible data. Anyway, it was a fascinating life, because I lived aboard this big ship, where you had all the services that go with a naval officer's life—silver service and marvelous food. We had air raids every night, but I didn't have to go on deck, because I was not a naval officer. The naval officers spent the night usually on deck with the anti-aircraft guns, which were fired on the kamikaze planes that were coming in every night—every single night. And we had a dense oil fog, which was so dense that the inside of the ship was like…. But it didn't bother us; it was just diesel fuel; it vaporized, so it was like smog inside. Anyway, it didn't bother me. We could watch old movies. I stayed up all night, because I was forecasting the weather, and I slept all day. So I really enjoyed that. And then finally, after about six weeks of living aboard ship, I went ashore; the G-2 had been set up by then. We stayed about three months, until the war ended. I came home somewhat earlier than I would have otherwise. My father had died, and my brother sensed that it might be smart if I got out of there. So it was very thoughtful of him; he sent a cable. Anyway, they told me to return, so I came back and stayed with my mother a little while after that.

Then I came back to Caltech in '46. Actually, Millikan had promised me before I went into the army that there'd be a job here, so I knew about that all along. When I came back, I was offered a job at Cold Spring Harbor, but I decided to take the one here, because Cold Spring Harbor had no academic status; it was a research job, and this had an academic future. So I came here as an instructor.

EDWARD B. LEWIS

SESSION 2

October 11, 1984

Begin Tape 2, Side 1

PRUD'HOMME: I want to talk about your research work on *Drosophila*. Why *Drosophila*?

LEWIS: Because of the ease with which you can find mutations and study them. The short life cycle allows you to have a new generation every ten days, and there's no other animal that can do that. There was an immense background. All the obvious things had been done by then, so you could go into greater depth and into analysis of the genes and find out whether there were subgenes and begin to try to see how a gene is constructed—though DNA, at that time, had not been determined to be the hereditary material. People thought that genes were proteins at that time—but that doesn't matter. Genetics has never depended on knowing what the genes were chemically; that doesn't help at all in terms of what you can learn by just breeding experiments. So genes were mapped linearly on the chromosomes and all of this was done. It's an abstract field, and I like abstract things.

PRUD'HOMME: And *Drosophila* was really Morgan's legacy in a sense, too.

LEWIS: Yes, that's quite right, and this is the ideal place for it. All the stocks were kept here.

PRUD'HOMME: Where were they kept?

LEWIS: Well, they're kept down the hall here. We still keep them pretty much the same as they were when I first came here.

PRUD'HOMME: What was your life in the lab like after the war?

LEWIS: Well, it was much the same as before the war, when I was a graduate student, because the same faculty was still here. I came in as an instructor, so it was easy to fit into the system. There was always a lot of freedom to do research, and a lot of communication among the faculty in biology, and we talked about problems a lot—mostly I talked with Sturtevant and Jack Schultz. They were the main people who were still growing a lot of *Drosophila* at that time.

PRUD'HOMME: Can you describe for me the 1942 discoveries at Caltech?

LEWIS: Well, that was just the year I graduated.

PRUD'HOMME: I had understood that that year it was discovered, at Caltech, that there were places in the chromosomes where what at first appeared to be a single gene turned out to be a cluster.

LEWIS: Well, I was working on a project like that for my thesis, and I took it up again when I came back after the war, using a system that has turned out to be quite favorable for this. It has turned out to be a giant cluster of genes, anywhere from ten to thirty. We don't know yet; we won't know until the entire molecular work is done. But we can identify genetically about ten units, and if you look at it more at the molecular level, you might find that there are ten or twenty or thirty or some multiple of that.

PRUD'HOMME: What is this system you use?

LEWIS: Well, we use genes concerned with making the fly develop abdominal segments. All of the segments start out alike, but certain segments have to form the head and others the thorax and others the abdomen. And this is a group of genes that very early determines the abdominal and part of the thoracic regions of the fly. In a way, the abdomen is the highest level of development in many respects. It's not a nice idea, but it's true.

PRUD'HOMME: You left briefly in '48 and taught at Stanford.

LEWIS: For just one term. Someone was on leave of absence.

PRUD'HOMME: Were the students at Stanford different?

LEWIS: Well, the classes were too big. I had very little contact. I gave a lecture there a couple of times a week. I think there were 500 people in a huge auditorium; it was quite the opposite of this place. We used to have nice small classes in genetics here—eight or ten students. Now it's up around twenty or thirty, and sometimes forty.

PRUD'HOMME: Did you find that you liked teaching better after the war? Because you said that when you were younger you hadn't—

LEWIS: When I was a graduate student, I would say it was just lab teaching. It was all right; I didn't mind. I think we all expected to do certain things like teaching, and no one was really pressing for a career other than research. And everybody who did research was expected to teach, because the general feeling was that people who work in labs without doing any teaching don't develop enough breadth. It's almost a diversity that's desirable in your life, to have a certain amount of teaching. I really liked to teach the beginning genetics course. After many years it got to be boring and more difficult to teach. The subject was much easier to teach in '46 than it is now.

PRUD'HOMME: How do you produce the mutations?

LEWIS: Well, when we started, we had nothing but X rays, which we used a lot—we're still using them a lot, actually. Later on, it was discovered that you could use a number of chemicals—feed the flies on chemicals. And that has some virtue, because the chemicals are not as destructive as X rays. But for some things we want, it's now turned out that X rays are the most desirable of all. It's just a technical point. The people who look at the DNA in chromosomes need to have some disturbance in the DNA that's considerable—a piece of DNA that has 1,000 bases turned around, or deleted. X rays do that all the time, but chemicals don't. And if you take a chemically induced mutation, generally speaking, the molecular people can't find it. You wouldn't think that's true, but their methods are exceedingly crude compared with the geneticists' method.

PRUD'HOMME: Who pays for the maintenance of these stocks of flies?

LEWIS: The National Science Foundation.

PRUD'HOMME: They've been going on for how long?

LEWIS: It started with the Carnegie Institution. That should be in the record—that Morgan had a Carnegie grant that supported Schultz. Schultz was sort of in charge when I first came, but I took it over, and he left in '43. Then I think the Rockefeller took it for a while, and the ONR [Office of Naval Research], and finally at some point the National Science Foundation started supporting it. It's possible the Atomic Energy Commission supported it, too; I can't remember the history of it. They support it now, heavily.

PRUD'HOMME: Can you describe your work on the bithorax complex?

LEWIS: Well, I'm hoping to write a *Scientific American* article. I was just noticing last night that Goethe, of all people, wrote on the tendency for plants to have structures that are homologous—like petals and leaves; parts of the flowers come from the leaves—and he was the first man to write that. Apparently he was anticipating Darwin. So I'm planning to put that in the article. Wouldn't that be nice, to develop that theme? I just looked it up in the encyclopedia, because I was trying to find out who proposed all of this business in plants—of having homologous organs. Usually it's the highly developed organ that reverts back to the original organ, like a leaf would be the original organ, and it's due usually to mutations or some damage. At any rate, in doing so, I found that Goethe was mentioned as having contributed to this. [Laughter]

The general idea is that we started studying a group of genes in order to investigate this so-called fine structure in the genes—namely, how many subunits there are to the gene. We don't think of them as subunits now; we think of them—and always did—as separate genes so close together that they mimic one gene. So that turned out to be verified, and it's a nice system.

But in addition, it had as a dividend a lot of information about how the developing embryo works—namely, early development of genes we were studying that control how the abdominal segments start to change. Each segment is a little different, and in order for each one to become different, additional genes turn on. I don't know exactly what the best analogy is for that. But you have a chromosome with the genes lined up in the same order in which they're turned on in development of the embryo. And it still isn't clear why they're turned on. It's probably because one of them triggers its neighbor to do something, and then it's like falling dominoes—except that instead of being destructive when they all fall down, it's the opposite. So that's what's currently attractive. It's quite a lot of fun.

PRUD'HOMME: Genes regulate genes.

LEWIS: Yes, those genes regulate the other genes. But how *they* are regulated is what really interests me, because you can ask, "Who regulates the regulators?" That's a big question and nobody has a clue yet. It may be in the same way that the regulators regulate other genes.

We're getting back to the very beginning, that's the point. You keep going back. We've backed it pretty much to the beginning. In fact, there isn't much time, because the embryo has only a few minutes in which to start this whole process going, and these are the first set of genes, almost. There are other genes that precede them in development, in the group we're working on—namely, those that establish the front and back end of the animal, which somehow has to be set. The dorsal and ventral halves are set by a few genes, perhaps—and then these genes come on. You've got to have a front and a back and a top and a bottom before you can begin. There are genes that make a certain number of segments, and [the segments] are all alike initially.

So there are some genes that precede these genes we're studying, but those other genes are not clustered; they're just scattered everywhere. Nobody knows how to analyze them; nobody can say which gene starts first or what it's doing. And then, all of a sudden, these genes take advantage of the environment they see around them and start making the segments different [from one another].

PRUD'HOMME: Do you work with a chemist on this?

LEWIS: Well, fortunately there's a group at Stanford, and another at Harvard Med that originated at Stanford. Dave [David S.] Hogness at Stanford has had a number of very good postdoctoral people and they've set up labs now at Harvard Medical School and at Cambridge, England. One fellow's just starting again in Australia; he's been allowed to start working on this. They had put him on mosquito work, or some applied work, and now he's finally got a job where he can start looking at this. And Dave Hogness still has a lot of postdocs, so there are about three or four big labs in the world working on this system.

PRUD'HOMME: Do you all communicate with each other?

LEWIS: Yes, we telephone each other frequently. About three or four times a week. The telephone has really been a great advance, you might say. I seem to communicate much better with them than with any colleagues here. Or even if they were here, I would still communicate better [on the phone]. You know how it is. Walking into somebody's lab is much more of a disturbance than telephoning them. So that's the way it works.

PRUD'HOMME: Could you tell me about *Polycomb*?

LEWIS: Well, that's one of the genes that makes the front/back difference. It was found, actually, by my wife [Pamela Harrah Lewis], when she was a student [1947]. It took us years to realize that this gene had something to do with our system. It had effects on the flies that didn't resemble the particular characters we were studying but it did have effects that are typical of another group of genes, which make head structures instead of abdominal structures. Now it looks as though there are two clusters—one for the head and one for the abdomen, and those clusters have separated in evolution, but they're still on the same chromosome arm. And *Polycomb* is working on both systems. The normal gene keeps them turned off in the front end of the animal, and we think none of the *Polycomb* substance is present in the rear end of the animal, and in between, you have a gradual turning on of the genes as a result of the amount of this substance. It got to have that name because there are little sex combs on the legs of the male and they normally occur on the first leg. But in *Polycomb* they occur on all the legs. So that's an example of this: The legs are all alike but they become different during development. You have the sex comb normally only on the front leg; you don't need them on the second leg. But this mutation loses the ability to suppress the sex comb, so they start appearing on all the legs. It does other things that are similar; it does things that we now recognize are similar types of changes that you'd expect to occur if it's affecting these bithorax genes. So, in a way, it precedes bithorax as one of those earlier genes that we were talking about. And it may be a fairly simple gene—just one, perhaps. That's a case where eventually the molecular work should pay off, in finding out what it is and how it works. Right now, nothing much is known about it. It'll be maybe five years before that can be solved. It should be less than that; it depends, really, on how hard people are pushing. For example, it's so trivial, but the one man who's doing this now has to start job-hunting. These young people, who do all of this work here, have to spend a year, at least, looking for positions. Hundreds of people are applying for all the good jobs. It's terrible! You tend maybe not to recognize the best people when you have such broad selection procedures. They've got to please everybody.

PRUD'HOMME: You met your wife here.

LEWIS: Yes. She came when Beadle came [1946]. She'd been a student at Stanford and she was helping him in his course. She had just finished and was about to go to graduate school. She thought she'd go back East to graduate school in mycology, but Beadle told her she should come here, and he brought her here when he came; that is, he encouraged her to come here. She worked as a technician for a while and took care of his stock center for a short while. But we got married after about six weeks. [Laughter]

PRUD'HOMME: So you worked together?

LEWIS: Yes. Well, she didn't work in the lab too much. I guess she worked the first year or so, and then we had a baby, and then she didn't try to work. At that time, you didn't try to keep up a career. But we went to Cambridge, England, within three months, and that was quite something. It took all of our time just to exist; England was in bad shape then. We were there for a year [1947-48, on a Rockefeller Foundation fellowship]. But she should have had a career.

PRUD'HOMME: Did she go back to graduate school?

LEWIS: No. She was very happy to have children and take care of children. But as I said, she was well equipped to become a career person, and she would have if she'd really wanted to. But what she really has is a career in painting. She paints all the time and has always kept it up. At first it was ink drawings; now it's watercolors.

PRUD'HOMME: Who are some of your colleagues here?

LEWIS: Well, there was of course Sturtevant. Sterling Emerson. Beadle. The person I've been in closest touch with is Norm Horowitz. He and I have always discussed everything together, and I still see Norm several times a year.

PRUD'HOMME: Can you describe Beadle to me?

LEWIS: He was a brilliant man who still had his feet on the ground. He was very practical. He was born on a farm; he knew how to fix everything; he would wash his own dishes when he was doing bacteriological work. He enjoyed washing the dishes himself and didn't want any fancy electric dishwasher. He wanted to be in touch with the material, I think. I think that came out of the early days. He was here as a postdoctoral fellow, so he grew up in an atmosphere in which there was no money for research at all. He recognized the need for modern equipment and saw to it that everybody got it, but I think fundamentally he was always prepared to make do with the minimum and use simple methods.

He just had the touch of gold when it came to experimentation. He would figure out what would work, and then he'd go to the lab and do it, and usually it worked. That was his style. He didn't plug away. He'd finish something, and his mind was such that he wanted to do something different. He got bored extremely quickly, so he would work on a project, settle it, publish it, and go on to something else.

PRUD'HOMME: I think the thing that impresses me most about your work is that you have hung in there and you've stayed with it. And it's gone on and on and on.

LEWIS: Yes. I diversified, in the sense that I did look at leukemia and things like that during the nuclear testing period, and some of us got pretty alarmed at this business—shooting bombs off out here and people saying there wasn't the slightest danger. Geneticists knew that was ridiculous. There's plenty of danger from exposing people. I don't really like to jump into new areas, because of the tremendous effort it takes to get a background in it. I don't read rapidly, and I just find that once you've gone into depth on something and have it tucked away in your head, you might as well exploit it, because the problems that arise are quite exciting, they really are. But if you jump into a new field, you tend to do just what's been done somewhere else with some new organisms. That wasn't true with what Beadle did, but he did follow up some things in *Drosophila*. Essentially everything that he did with *Neurospora* had been foreshadowed by what he and Sturtevant and Ephrussi did with *Drosophila*. They just wanted to nail it down a little better and expand it a little more. They were really looking for organisms that would grow even faster, and the only thing that could be faster would be things like yeast and bacteria. At that time, it was the beginning of the era of working with microorganisms, when they first didn't even know they had heredity. But that was a big factor.

PRUD'HOMME: In 1966 you were made the Thomas Hunt Morgan Professor of Biology. Is that an honor for you?

LEWIS: Yes, it was a big honor, sure. I think it was partly because Sturtevant must have retired about then. It had something to do with maintaining the *Drosophila* tradition, because I was the only person who was full-time faculty doing *Drosophila*.

PRUD'HOMME: Can you tell me about the visit of the Russian scientists [in 1967] and Lysenkoism and that?

LEWIS: Well, I guess there must have been a thaw about then, to allow this group of four people who came. There was [N. P.] Dubinin and [D. K.] Belyaev, [S. I.] Alikhanyan, who'd done a lot of fly work. [B. L.] Astaurov was the senior person of the lot. Astaurov was a very fine man who was mainly a developmental biologist, and Dubinin had headed a big group of geneticists.

But apparently he has turned out to be somewhat of a—not a Lysenkoist, but he was so concerned about saving his own skin, I guess, that he was never a champion geneticist. But we didn't know that at the time. And it was very sad. He is said to have denounced Astaurov in the Academy just because I guess he was trying to increase his own power in the system. He's still an active person. Belyaev is very nice; he's in Novosibirsk. He invited us to spend a week in Novosibirsk in his lab. And Belyaev was responsible for getting some of the geneticists who had been thrown out of Moscow to his institute in Novosibirsk, where they were allowed to work. But genetics is still greatly repressed there.

PRUD'HOMME: Lysenkoism was an entirely different genetic theory.

LEWIS: It was essentially what we'd call Lamarckism. A very naive kind of business. [Lysenko] was a kind of practical breeder who knew nothing about the laws of heredity and all that. He was a person who had come up through political shenanigans. The problem was that genetics ran somewhat counter to Marxism. Marxism says you can mold the individual; genetics says there are limitations on how much you can change. So they obviously could exploit that, the ones who wanted to push Lysenkoism. It was a sad period.

PRUD'HOMME: But they came here, which was terribly exciting.

LEWIS: Well, these people are all geneticists, and they were allowed out partly because of a thaw. It was the Khrushchev era. But they were probably watching each other; that is, it's not clear who was trusted and who wasn't trusted. I think two were trusted maybe and two not trusted. Belyaev was a Party member, so he would have had no trouble. And they were academicians. Belyaev, I think, is a good scientist and there's no reason to believe that he was a Lysenkoist. He was very outspoken about problems; his brother was executed by Stalin, this kind of stuff. So, although he has a position of priority, I guess you have to get that by being willing to—well, if they tell you to fire somebody, you fire them. You see, that's the kind of life of tyranny that you live under.

PRUD'HOMME: What did they do here at Caltech?

LEWIS: What happened was that I had some job in the Genetics Society of America—I might have been president. So they came to this country and landed at Harvard. I made a lot of effort to be sure they got around and saw things, and we entertained them here. We took them to Disneyland, and we did a lot of things while they were here and put them up at the Huntington Sheraton. We got funds for the trip in this country, because they didn't have enough summer clothes and various things. And I think it worked out. Dobzhansky told me afterwards—Dobzhansky was back in New York then—that they had really enjoyed their stay. He wrote me a nice letter about it.

Then they went on to Berkeley, where there was a genetics congress. We had them at our house when they were here, so Belyaev invited us when we went to the Soviet Union. Dubinin would never have any contact with us again. We went to the Soviet Union in 1976. When we were in Copenhagen in 1975, Belyaev invited us. He gave us a marvelous trip. But the people in Moscow were very wary. Belyaev had a young scientist who looked after us in Moscow, but it was pretty clear that they were scared to death of having contacts with the West.

PRUD'HOMME: In '68, you were elected to the National Academy of Sciences.

LEWIS: Yes.

PRUD'HOMME: You studied the relationship between radiation dosage and the incidence of cancer. Can you tell me something about that work?

LEWIS: As I said, it was probably partly [because of the nuclear] testing and probably partly because Sturtevant had taken quite an interest in that, because of the statement that had come out of the White House that the amount of radiation was far below that which could cause any damage at all. Sturtevant got really annoyed at this, and he gave a little paper when he was president of the Pacific division of the AAAS [American Association for the Advancement of Science] and he had to give a talk [1954]. He included this in the talk, about how likely it was that there would be a small, but not negligible amount of damage. That was a few years before I was doing anything. But what had not been done was to look at the question of what are called somatic effects—cancers and so on. The geneticists had never entered this field—except Muller had said that probably skin cancers and so on from radiation were due to mutations, somatic mutations. His paper appeared in *Science* and proved that X rays make mutations. [H. J. Muller, “Artificial transmutation of the gene.” *Science* 66:84-87 (1927).] He mentions in a little sentence that somatic damage, too, would be due to mutation. But nothing much was ever done in the way of thinking about it genetically. And the big question was: Did cancer increase with a so-called linear relation to dose, the way X rays had been shown to produce mutations. There was thought to be no threshold, as it was said, for genetic damage. When I started getting interested—this was around ’56 or ’57—there were enough data to indicate that leukemia was behaving this way. So I wrote a paper in *Science* that summarized three or four months’ work, maybe five months. [E. B. Lewis, “Leukemia and Ionizing Radiation,” *Science* 125:3255 (1957)] It happened to be well timed, because there was a big testing controversy. In Washington, the joint committee that existed then on atomic energy had a whole lot of witnesses to testify about the dangers of somatic mutation, and I went to that. But I had to talk about the leukemia, the genetic part of it. Muller and a couple of other people talked; the geneticists talked for the most part only about the genetic damage. And the somatic damage was considered very controversial at the time, because that area was dominated by medical people, who really had never learned any genetics.

PRUD’HOMME: They didn’t take genetics in medical school.

LEWIS: No, not in those days. So they didn’t understand how it could be a mutation at all. It took a long time, and only very recently has there begun to be an acceptance of this; because for a long time—not then, but afterwards—there were viral theories that had great prominence, and all the research money went into that. Which is all right; the only way you can approach some of these things is to have viruses that can be manipulated. You can’t manipulate the genes and somatic cells very well, because you can’t propagate the cell and mate the cell to another cell and get the progeny—though there are indirect methods. At any rate, all I looked at were all the groups of people who had been exposed to radiation, within which there was evidence for leukemia arising.

PRUD'HOMME: And there was a greater incidence of leukemia—

LEWIS: It seems to be related to the dose they get. Even people like radiologists, who really weren't putting themselves under the X ray machine, were [still] getting scattered radiation and they might have accumulated quite a bit. They never got high-intensity radiation, yet they died of leukemia at the rate you'd predict if they had been working around the so-called permissible dose, which was supposed to be far below damage. It was known that they died of the leukemia, but it was never known whether it could have been a diagnostic problem, where they were more likely to be recognized as having leukemia because they were doctors.

So I looked into these problems. I collected 450 death certificates, for example, of radiologists, and I worked up a paper. That was in 1963; I'd kept going on this. But for the leukemia paper, I simply used some rather crude methods of analyzing the death rates and so on.

Begin Tape 2, Side 2

LEWIS: Anyway, I tried to do as much research on that subject myself as I could, to fill in gaps. And we got a lot of information. Beadle helped get some of the basic information from the Atomic Energy Commission.

PRUD'HOMME: What about research on the Hiroshima bombing?

LEWIS: Well, it had been going on, and [the AEC] had the data, and they were sitting on a lot of it. He helped me to get a look at some of it, and I acknowledged all of this. I think it made them really mad that I had used some of it. They wanted it kept from the American people. Just wait and wait and wait—and they never would have published it. So, I used the literature; I looked at everything that was known.

PRUD'HOMME: You also had concerns with radiation exposure limits in groundwater.

LEWIS: Well, that was only an episode that happened here in this area, when a professor I think at Stanford in environmental health—it was called sanitary engineering in those days—thought it would be a good idea to put tritium in the groundwater to see where the water was going. And he calculated that the amount in our drinking water would be trivial.

PRUD'HOMME: This was in this local area here?

LEWIS: Yes, right here. So I really got annoyed at that. But that was mostly done without much publicity; we put a stop to that foolishness. It was a very traumatic time in some respects, although I made a lot of contacts with public health people. I went on a committee in '59—a Public Health Service Committee—that had been formed to look into radiation matters, so I saw a lot of the people who were involved at that level, and they were almost completely at odds with the Atomic Energy Commission, which kept them under its thumb. The AEC classified much of the material. It was a very bad period. When I published the article in *Science*, the editor wrote a very strong letter and said that this proves that radiation is dangerous and we'd better not go ahead with atomic energy without being well aware of it. And the AEC staff rushed over to *Science* and gave him a very bad time—he told me all his problems with them.

PRUD'HOMME: Are you still involved with this?

LEWIS: No, I haven't done anything now for several years. I stayed involved a long time because I was on a number of National Academy committees. The National Academy would set up a committee in which the geneticists would meet and discuss genetic damage and would just reiterate all the things they had said before. And then the somatic effects were in the hands of people who knew absolutely no genetics at all and were still trying to present the material the way they did in 1950. Another fellow at Stanford and I were about the only people who were pretty effective, I think, in getting out a report that made actual calculations of the risks involved. The data are better for human cancer induced by radiation than for any other environmental agent. When they say that this chemical or that is going to produce cancer, that may be true, but it's based on the most sloppy, terrible evidence. It's usually based on some rat colony that they've overexposed to some chemical, and you don't have any idea what would happen to human beings. But to be safe, they say, "Oh, it's very dangerous!" Well, maybe it is, but there's a point where you are not doing science anymore.

The radiation evidence is not only good enough to make some practical, sensible risk estimates, it also has a scientific value, in that, as far as I could see, it proved that the somatic mutations are a cause of some kinds of cancers. This is now becoming generally acceptable. If a virus alters a gene, well, that's what we call a mutation. X rays do a much better job than any virus, and the viruses that do alter genes do it mostly where the organism is an inbred animal that's very sensitive to certain viruses, and the virus goes in and wrecks the whole system. So some mice all die of lymphoma; every single one of them, if they live long enough, automatically dies of a lymphoma. It's not quite clear why, actually. We're not quite like that, of course—fortunately. There's very little evidence that any of our cancers are directly due to viruses. They're due to spontaneous mutations, and if you induce more mutations, of course that's not very good.

PRUD'HOMME: I'd like to discuss Thomas Hunt Morgan a little more. What was he like?

LEWIS: Well, I'm afraid when I knew him he was completely out of it. He was very nice, but he was deaf. It was hard to communicate with him. He was doing embryology again. He'd returned to his old loves; he was just working on marine animals that were rather refractory to any kind of analysis. But they had self-sterility, and he was always interested in things like that—things that were, you might say, difficult to analyze—and he would just play around with these. There's an animal called the sea squirt—*Ciona*. He went back to working on that, and no one yet understands that kind of system, because you can't breed the animals and figure out what's going on. He was down on the first floor; Albert Tyler looked after the lab for him and was very closely associated with him. In all the years that Tyler was a student and then on the faculty here, Tyler sort of looked after Morgan, and Morgan appreciated that. But by that time, Morgan had retired from the division chairmanship and there was a group of people for a while—Sturtevant, Haagen-Smit, and Borsook—who ran the division. It was Sturtevant, by the way, who figured out that we ought to get Beadle back here.

PRUD'HOMME: How was it, to have a triumvirate ruling a division?

LEWIS: Oh, I think it was a disaster! I guess Haagen-Smit might have been the main person who ran it; he had more sense about finances and things; Sturtevant was hopeless when it came to that. And I don't know if Borsook did anything or not. There was no sign of outward difficulties, but it took Beadle to get the money and get the support and liven the place. He was a real live wire, that guy, and he stepped in just as [Caltech president Lee A.] DuBridge came [1946]. Those two were very similar, and together they got money for the institute. Everything was popping in those days; science was gradually beginning to get support. Beadle went out and got all kinds of money, and he could give lectures and persuade people that they should support us. He did marvelous things. But Morgan I can't tell you much more about.

PRUD'HOMME: Morgan died [1945]; Dobzhansky left. Can you describe Dobzhansky?

LEWIS: Well, as I say, he was much the opposite of Sturtevant, but they got along very well when they first came here. He tended to use graduate students as slaves; he certainly had that attribute. He expected people to get certain results, and he'd be mad if they didn't. So he was somewhat in the European tradition of the big professor. He was young enough, though, that he wasn't quite at that stage when I knew him.

PRUD'HOMME: How was Sturtevant as head of the department?

LEWIS: Well, that was rather brief. He was not a good administrator. The department at that time, though, didn't have many severe problems. The Rockefeller gave us money, and when that was beginning to run out, Beadle came on the scene and got the money going again. But Sturtevant wouldn't have been able to go out and get money. He couldn't give lectures that would excite people at all. Beadle would go out and lecture at all the local luncheon clubs—Kiwanis, high schools, and everywhere, and he gave exciting talks and everybody liked him, especially some of the people with money.

PRUD'HOMME: A good public relations man.

LEWIS: Right. Which is surprising in a way; because at the same time he was able to do all the exciting lab work that he did, which was quite phenomenal. And he commuted to Washington;

sometimes he flew back here at night and went off the next day. His second wife, I remember, was furious one time when he hadn't been home twenty-four hours before he took off again.

PRUD'HOMME: Do you think the Biology Division at Caltech is still as preeminent in genetics as it once was?

LEWIS: Well, I think it went down quite a bit, because we lost several people. Bob Edgar left the field, and he was one of our shining new lights. We've had a lot of retirements without replacing them—that was a bad thing. On the other hand, some of genetics took a different form, in that there was more and more work being done at the microbiological level. But for a while, genetics itself was going downhill in the eyes of the world. I think what happened was that the genetics of what are called prokaryotic organisms was so thoroughly worked out that people thought, "Well, there isn't much more to learn, so I'll do something else." Max Delbrück, I know, felt that genetics was dead. About ten years ago, the tendency reversed; there was a sudden rebirth when it turned out that you could start cloning genes. And now you can do the genetics of any organism, even those that don't breed very fast, because you can do the DNA analysis directly. And for that, you still need an understanding of genetics.

PRUD'HOMME: Do you think there should have been a medical school at Caltech?

LEWIS: No. I was very opposed to that.

PRUD'HOMME: Why?

LEWIS: Mainly I think we were worried about financial considerations. It tends to drain the institute of funds. Also, that was a period when people thought that scientists can't learn very much more basic things. "Let's apply it, because society's in such bad shape; let's apply everything." It was something generated as you know, in the sixties and seventies—almost an anti-science wave that was coming.

PRUD'HOMME: It would have taken away from the research aspect.

LEWIS: Yes, it would. And we've always been successful, I think, because we kept small and didn't expand in all directions. It's difficult to interact and reach decisions with two groups of people who train so differently. One is research-oriented, the other is not. And I thought it would be horrible if we got into that.

PRUD'HOMME: Can you compare Millikan, DuBridge, [Harold] Brown and [Marvin L. "Murph"] Goldberger as presidents?

LEWIS: No. I wasn't very close to any of the power struggles, or the seat of power, at any time. I think Brown was too concerned with economizing and not enough concerned with raising money. He was popular with the trustees for that reason, but I think that was a bad thing about him. He was very willing to listen to all sides. Millikan, of course, was in a different era and ran things more or less autocratically. Goldberger is somewhat of an enigma right now. I like him—I like the openness that he seems to have. He says just what he thinks and has a wonderful position on peace—how to achieve it, and so on. He's not playing the line of the Pentagon or anybody else. He's not a hawk, and we have had a history of people who were rather hawkish—DuBridge was, and so was Brown. So in that sense, Goldberger's run into some awkward situations, not altogether his fault. He may not be the very clever administrator that the other fellows were, and I think that's unfortunate for the institute, because it apparently takes some peculiar personality to run the place. Do you know what I mean? Someone different from the research-oriented or academic-oriented person. Beadle was very exceptional in that way.

PRUD'HOMME: DuBridge was exceptional, too.

LEWIS: He was exceptional, too.

PRUD'HOMME: Because he retained the respect of the academic community.

LEWIS: Right. And he also didn't try to keep up a research and teaching interest at the time. Beadle was able to teach and keep close contact with the research—but of course he wasn't president, so it was a little less responsibility that he had.

I got along with DuBridge; at least, there was no conflict. And everything was going at a fast clip, too, and there was more and more money. Now Murph [Goldberger] faces a period in which nobody's going to give us any money, because there is no tax advantage to giving your money to Caltech, or very little. So to raise money is very difficult now, and he may not be the best person in the world to raise it. That's a serious problem. That's the only serious problem we have—how to get the old support we used to get from local people.

PRUD'HOMME: What do you think is the current state of the institute? And what do you think its future should be?

LEWIS: Well, I don't, of course, see any reason not to go on supporting basic research and trying to get good people. I think we're doing quite well in biology, and I don't pay much attention to the other divisions. I see the other members of the faculty here and there, at lunch. But everything is so complex now. Astronomers talk about the billions, almost, that are needed for what they're doing. Everything is really high-priced, and they're all a little disturbed about whether they'll get support and how much they'll be cut. Biology has fared pretty well.

PRUD'HOMME: Everything is so inflated and so expensive, all of the new equipment and machinery.

LEWIS: Yes. By having a good division that has a big reputation, we are, I think, able to get funded better than most places by NIH [National Institutes of Health] and NSF [National Science Foundation]. And the administration has not cut back in science as much as we first expected; I guess they've got some pretty good advisors who have tried to keep a heavy scientific program on.

PRUD'HOMME: What is your present work?

LEWIS: Just working on the bithorax.

PRUD'HOMME: You mentioned a film you were working on.

LEWIS: Yes. I've made a movie that shows the live flies that exhibit the characters we're studying. And then I have a film that uses animation techniques to show how we think the genes work. That's an animation stand over there, which we use to do this. I gave a Beckman lecture about ten years ago and I started it for that, and I put it away, and recently I found the drawings and I got an interest again, so I set it up again. Actually, I did this because there is a little fund now for professors, and I'd always wanted to do a better job—so I used the fund to support this, because you couldn't ask a research agency to support this. I have a motion picture camera and the animation stand. But that's a hobby—well, it's not really a hobby, it's a way for me to illustrate my stuff when I give lectures. It's very hard stuff to present, and with the film it's very much easier, and everybody's interested in that. It's novel and it's a lot of fun. I'm more inclined to go out and give talks with that as a prop. I've been accepting a lecture a month or so.

PRUD'HOMME: What are you most proud of in your work that you've done so far? Or is it the whole body of work?

LEWIS: Well, I think it's partly…. There is one phenomenon, we call it transvection, which is about the only thing that has come out of it that's absolutely unique and not understood, and it must be important. That is pretty exciting.

PRUD'HOMME: What is the phenomenon?

LEWIS: Well, it's a phenomenon in which two chromosomes have material flowing back and forth between them. No one knew that that could happen. Something is migrating off one and onto the other, over short distances. We don't know what the explanation is; we're beginning to get an idea. But it lends itself to a direct genetic analysis. Biochemists can't touch it, yet. It's very subtle, and they tend to have to grind up whole animals or grind up lots of things. What we're studying is something that would be a minute part of the whole mess that's in the cell.

PRUD'HOMME: Do microbiologists get involved in this?

LEWIS: No, they can't find the phenomenon, and the reason they can't is that it depends upon having two chromosomes paired. Well, you might do it in yeast, but they haven't really proved it yet. They haven't even looked for it. I think it has some importance; it might be quite important. It has something to do, I think, with regulating the gene, the method of how to turn on genes, especially the ones that are lined up in a row and influence each other. There's something going on there, and that's what this phenomenon will tell us—how it works. So I'm hoping that works out. It leads to direct experiments; it's easy to design experiments to test things in genetics. Genetics is very exciting; it's always been exciting, no matter what the trends were. Things go in cycles.