CHEMICAL HERITAGE FOUNDATION

JOSHUA LEDERBERG

Transcript of an Interview
Conducted by

James J. Bohning

at

Rockefeller University

on

25 June, 7 July, and 9 December 1992

(With Subsequent Corrections and Additions)

LEDERBERG

CHEMICAL HERITAGE FOUNDATION
Oral History Program
RELEASE FORM

This document contains my understanding and agreement with Chemical Heritage Foundation with respect to my participation in a tape-recorded interview conducted by James J. Bohning on 25 June, 7 July, 9 December 1992. I have read the transcript supplied by Chemical Heritage Foundation and will return~~ed~~ it with my corrections and emendations.

1. The tapes, corrected transcript, photographs, and memorabilia (collectively called the "Work") will be maintained by Chemical Heritage Foundation and made available in accordance with general policies for research and other scholarly purposes.

2. I hereby grant, assign, and transfer to Chemical Heritage Foundation all right, title, and interest in the Work, including the literary rights and the copyright, except that I or my heirs shall retain the right to copy, use, and publish the Work in part or in full~~, until my death~~. The Work will be posted on the NLM archival web site.

3. The manuscript may be read and the tape(s) heard by scholars approved by Chemical Heritage Foundation subject to the restrictions listed below. The scholar pledges not to quote from, cite, or reproduce by any means this material except with the written permission of Chemical Heritage Foundation.

4. I wish to place the conditions that I have checked below upon the use of this interview. I understand that Chemical Heritage Foundation will enforce my wishes until the time of my death, when any restrictions will be removed.

a. ✓ No restrictions for access.
NOTE: Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation Oral History Program to obtain permission from Chemical Heritage Foundation, Philadelphia, PA.

b. ______ My permission required to quote, cite, or reproduce.

c. ______ My permission required for access to the entire document and all tapes.

This constitutes our entire and complete understanding.

(Signature) [signature]
Joshua Lederberg

(Date) APR 20 1998

Rev. 3/21/97

Joshua Lederberg, interview by James J. Bohning at Rockefeller University, New York, New York, 25 June, 7 July, and 9 December 1992 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0107).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

JOSHUA LEDERBERG

1925 Born in Montclair, New Jersey on 23 May

Education

1944 B.A., biology, Columbia University
1947 Ph.D., microbiology, Yale University

Professional Experience

Columbia University
1945-1946 Research Assistant, zoology

Yale University
1946-1947 Research Fellow, Jane Coffin Childs Fund for Medical Research

University of Wisconsin
1947-1950 Assistant Professor of Genetics
1950-1954 Associate Professor of Genetics
1954-1959 Professor of Genetics
1957-1959 Chair, Department of Medical Genetics

University of California, Berkeley
1950 Visiting Professor of Bacteriology

University of Melbourne
1957 Visiting Professor of Bacteriology

Stanford University School of Medicine
1959-1978 Professor of Genetics (also Biology, Computer Science)
1959-1978 Chairman, Department of Genetics

The Rockefeller University
1978-1990 President
1990- University Professor

Honors

1957	National Academy of Sciences
1958	Nobel Prize for Physiology or Medicine
1960	Sc.D. (honorary), Yale University
1967	Sc.D. (honorary), University of Wisconsin
1967	Sc.D. (honorary), Columbia University
1969	M.D. (honorary), University of Turin
1970	Sc.D. (honorary), Yeshiva University
1979	Litt.D (honorary) Jewish Theological Seminary
1979	Foreign Member, Royal Academy of Sciences
1979	LL.D. (honorary), University of Pennsylvania
1980	Honorary Life Member, New York Academy of Sciences
1981	Sc.D. (honorary), Rutgers University
1981	Honorary Fellow, New York Academy of Medicine
1982	Fellow, American Association for the Advancement of Science
1982	Fellow, American Philosophical Society
1982	Fellow, American Academy of Arts and Sciences
1984	Sc.D. (honorary), New York University
1985	M.D. (honorary), Tufts University
1989	National Medal of Science

ABSTRACT

Joshua Lederberg begins the three-part interview with a description of his parents, family background and early years in New York. Lederberg knew from the second grade that he wanted to be a scientist, and experimented at home with his own chemistry lab. Lederberg cites Albert Einstein as being a positive role model in his formative years. After completing grade school in 1936, he attended the Palestine Conference with his father in Washington, DC. He graduated from Stuyvesant High School at age fifteen. Due to age restrictions, he had to wait until he turned sixteen before entering Columbia University. Lederberg spent the semester between high school and college at the American Institute of Science Laboratory. He received his B.A. in biology from Columbia in 1944. While in college, Lederberg did original research with colchicine, and worked with Francis Ryan on Neurospora and E. coli. At age seventeen, he enlisted with the U.S. Navy and was placed in the V-12 program, serving as a naval hospital corpsman. While working towards his Ph.D., Lederberg continued his research on bacteria and E. coli. After receiving his Ph.D. in microbiology from Yale University in 1947, he joined the University of Wisconsin as assistant professor of genetics, and expanded the University's bacteriology research. There, Lederberg first worked in salmonella strains with his graduate students. While with the University of Wisconsin, Lederberg won the Nobel Prize for Physiology or Medicine in 1958. Lederberg concludes the interview with a discussion of the University environment during the McCarthy era, reflections on his career decisions, and thoughts on chemical information science.

INTERVIEWER

James J. Bohning is currently a professor at Lehigh University. He has served as Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has written for the American Chemical Society News Service, and He has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995.

TABLE OF CONTENTS

INTERVIEWEE: Joshua Lederberg

INTERVIEWER: James J. Bohning

LOCATION: Rockefeller University
Laboratory of Molecular Genetics and Informatics

DATE: 25 June 1992

LEDERBERG: Here is a more detailed chronological outline. It will mostly not be too meaningful to you, but it's sort of my first chronological sketch, putting in much more detail than will ever be written down in anything more comprehensive. I'm not transferring this to you; I'm letting you use it, but I'd like you not to copy it and I'd like you to return it to me. But it might be helpful to you in structuring what you want to do.

BOHNING: All right. That would be fine.

LEDERBERG: If you prepare some rough outline of major themes I'd like to see it before it goes into any repository.

BOHNING: Sure.

LEDERBERG: All right. That applies to both of these documents. You might want to go home and study them in more detail. What more do think you'd like to do today? Do you want to take a minute or two to look at these documents, or do you have some things that you already had in mind to get started with?

BOHNING: I was not really prepared to start today. I thought we would best spend our time today just discussing where we're going to go and how we're going to do it, so that you understood what I was looking for and vice versa.

LEDERBERG: Sure.

BOHNING: I think it was mainly these notes that I had indicated. As I said, I had just put together a brief chronological outline and then added some notes on to that to indicate the kinds of things that I was looking for.

LEDERBERG: Well, you'll see much more than you want to use on the chronological agenda. You can see here an answer to your question about dates. Now, I'd still not like to waste the opportunity to visit. Maybe there's some of this we could go over together right now. What's your feeling on the matter?

BOHNING: Well, if you want to spend some time, that would be fine.

LEDERBERG: I have until 2:30 p.m.; that's my only constraint, so that's an hour and a half.

BOHNING: Okay. One of the things we usually start with is parents and family background.

LEDERBERG: Okay. As much as I know is down there [referring to biographical notes]. What should I add that would come across orally but still clearly? I've already highlighted to you the centrality of my dialectic with my father [Zwi Hirsch Lederberg]. The central point is that he was an orthodox rabbi. He was an immigrant from what is now Israel, then Palestine. He was quite fluent in English. He was well educated, having more of a seminary education than a university or a collegiate one. He had been viewed as a brilliant scholar and in fact had been sent to the United States for studies here. There are conflicting accounts as to whether he was fifteen or he was eighteen. I've tried to track down documentation on that without much success, although he appears to have been enrolled in what is now the Yeshiva University. That is at least one of the places that he was connected with, although they can't find any records on him.

That was fortunate for me in a number of ways, but for one thing he got the equivalent of the Green Card at that time and the iron gates were slammed shut on immigration not long thereafter. On the strength of his prior residence, he was able to immigrate here in 1924—a point I never investigated during my parent's lifetimes. In the few surviving records after my mother died some of how that became possible became a little clearer. He had a religious vocation. I would have liked to have probed more deeply just where he stood on issues of modernity, and I suspect he was in some conflict. He had a fairly orthodox background, and there is a family background and tradition that goes back centuries in that direction. I also recall him as someone who was very much interested in America and being a good American and in keeping up with the times in a wide variety of ways.

There was a certain ambivalence when he had a child who had no interest and certainly zero in the ritualistic aspect of the Jewish faith, thoroughly involved and immersed in science, without that kind of reconciliation. That's what our debate was about. Cf. Spinoza a model. I continue to regard science as a vocation and one I think he accepted as a parallel to his, but the detail of that is something that I would have liked to work out more clearly, especially in a dialectical axis, to some degree in my own mind. I'm not sure that there's more that I am able to dig out on that issue at this point.

BOHNING: What about other relatives?

LEDERBERG: Well, the nature of that family tradition was of some consequence. I was very tardy about trying to collect genealogical information. There was no developed interest in this during my parents' lifetime, which was unfortunate, so I never got information firsthand from them. I ended up being the family historian, although the record is mostly in Israel. There's a large Lederberg clone in Israel. They're all from one family. Throughout the world I think they are, and that's a puzzle in itself. What does it mean? I have no good evidence on that point. But it comes from a town in Poland called Plock, about one hundred kilometers west of Warsaw on the Vistula. Whether there's anything left in the holocaust documentation that they've found, I just don't know. I've had one or two friends take a cursory look at those materials for a more detailed investigation.

What's more important is the sense of tradition that went along with that. There was a strongly developed tradition of Rabbinical scholarship on both sides of my family. I now realize it was more deeply ingrained on my mother's side than on my father's. Most of my father's relatives were business people, with a sprinkling of rabbis among them, including especially the progenitor, who is called the "Gaon," the Ayatollah of that part of Poland. But most of the descendants went into real estate or other businesses. They were middle class people in Jerusalem. Through the Turkish occupation, after the British advances, there is a story about my mother [Esther Goldenbaum Lederberg] at age fifteen, having been a nurse and working heroically for some of the wounded and helping to reassure the people in Jerusalem at the time of the actual conflict. What truth there is to that I just don't know. But that was supposed to have been one of the virtues that was presented to my father's family when they were negotiating their marriage. That's what I recall by way of background. This is in a way retro-Zionistic, the movement away from Israel and trying to represent the ideals of Judaism in the Diaspora. I guess I do stand for that in my own way just as strong as my father did. End of report. [laughter]

BOHNING: I believe you said he came here in 1924?

LEDERBERG: He was here in 1921. Whether he had come here as early as 1918 seems problematical, but there are pieces of paper that I don't trust that say that. But it was no later than 1921 that he was living here. (He was born in 1904.) Then he went back to Israel and claimed his bride. I'm sure it was a negotiated marriage. He brought his bride, my mother, with him back to the States in 1924. I was born in 1925.

BOHNING: What are your earliest recollections?

LEDERBERG: Well, I wrote some of them down [referring to biographical notes]. They may be screen memories, but here we go: Lindbergh; some traumatic events, (scalding my arm; falling out of baby carriage). My brother [Seymour] was born when I was three and a half. Then starting kindergarten [in 1929]. I have vague recollections from when I was four or five years old.

BOHNING: You were here in New York by that time?

LEDERBERG: Yes, I was. I was born in Montclair [New Jersey], and when I was six months old we moved to New York. I have what I'm sure is a false memory of the train ride from Montclair to New York, but I don't believe it. [laughter] This is a piece of documentation my mother saved. That's an interesting fact—and that's literal [20 June 1932 essay on wanting to be a "scientistist" like Einstein]. [laughter]

BOHNING: You said you weren't inclined to follow the way your father had followed the family tradition, as it were, a religious tradition.

LEDERBERG: I thought that was medieval, quite apart from the core of philosophical validity that there might be in Judaic teachings. Maybe I did know, and I would have allied myself with a Spinoza rather than my father. A heretic within the faith, if you like. But I chafed under the rituals. Saturdays were the best days that I would have available to go to the library, and that was forbidden, so I evaded it. I would walk a mile so that none of my father's parishioners would see me, then get on the subway to go downtown to the public library. So, I thought that was very old-fashioned, and I didn't understand why they kept doing such things. I would hark back to my father, asking if all these things were being done at the time of the Temple or are they ill-informed accretions through the experience of the shtetl when the Jews were very tightly segregated and were not part of the larger world. I was going back to fundamentalism. [laughter]

BOHNING: How did your mother respond to this?

LEDERBERG: Oh, very pragmatically. She said, as my father did, "Whatever you think about the matter, your father's job depends on your not being seen as being in violation; they would be horrified that you're doing it. We'll talk about it privately." They didn't tell me I would be damned and go to hell on these points. They had their own reservations about those deviations, but they were restrained. They discouraged me, but didn't condemn me for the deviations. And at other times they'd be very proud of what I represented. We had role models like [Albert] Einstein and Chaim Weizmann who were very prominent images in Jewish life generally at that time, but also with scientists. They were tailor-made for my view of the world. I'm sure Albert Einstein did not observe the Sabbath; I'm sure he was regarded as a wonderful and great Jew, and I would throw that up to my father. [laughter] It was not an unreasonable standard of behavior on my part.

BOHNING: I was curious about your comment about your father's job, because I've known Protestant minister's children who grew up in a small town and had that same situation. They were restricted in their behavior because of their father's position as being the religious leader, and it created problems for a lot of them.

LEDERBERG: Well, there were also other expectations. We had Hebrew school on Sunday, and I was expected to follow that faithfully. It was also expected that I would be the paragon of achievement there as well; it was something that I really didn't care much about. I had to go to services unendingly. As I've told many people, I had enough religious observation until I was thirteen to last me a lifetime, and I'll leave it at that.

BOHNING: What about your brother?

LEDERBERG: I have two brothers. I haven't probed as deeply and as directly with Seymour, who's close to me in age and has had a somewhat similar career. I think he feels much as I do about it. We have a much younger brother [Bernard] who is a religious zealot. He's in the Lubavitcher movement and thinks [Rebbe] Schneerson is the Messiah. He is proselytizing all the time. My father might even look askance at such extremism. Great surprises in family dynamics. [laughter]

BOHNING: What's the age difference?

LEDERBERG: Sixteen years. He's a grandfather. That really came home to me, that my baby brother's a grandfather! [laughter] He lives in Jerusalem now.

BOHNING: When did you start reading in earnest? You've talked about setting your goals very early in your life.

LEDERBERG: Well, I can't remember when they were otherwise. This is the only documentation I have, and it was a second grade class essay, "What do you want to be when you grow up?" That was my statement at the time. I don't know how seriously to take it. Is that something I invented at the moment, or did I just think it might be a good idea? I don't know. But within a few years of that I was very actively reading all the science I could. When I was ten, I can remember the headline when Stanley found the tobacco mosaic virus. When I went to look for it again, I could spot it instantly once I saw it on the page of *The New York Times*. [laughter] I had teachers who were already nurturing me as a precocious child. I had a contract with them—if I cooperated with them in helping the class move on with its business, they'd leave me alone and I could sit in the back of the room and study all the things I wanted. I remember confounding my algebra teacher with a phony proof that two equals one, and she couldn't work her way out of it. That's what precipitated these contracts. [laughter]

BOHNING: Which is something you did purposely? At least you had the support of these teachers.

LEDERBERG: I did when I got to that stage. There was a point where I was just so bored and didn't think they were such great scholars, which was true, but that's not the whole story, obviously, in teaching. They were very wise people and very compassionate. They would admit that to me and deal with me as an adult, saying, "Look, we both have a problem to deal with. I've got to bring the rest of this class up to what it is that they need to know, and you've got to find some way to use your time effectively, and don't do it by teasing me all the time. You probably can catch me up on these things, but is that what you want to do the rest of your life?" They would have a hard talk with me in those terms. So we worked out a very good agreement. By the time I was eight to ten years old, I was certainly solidly involved in self-study.

BOHNING: While that self-study was directed in the scientific area, did it range over other topics as well?

LEDERBERG: You might say both. It was largely concentrated in science, but I read a lot of history, philosophy, political science, and current events. I was very much involved in what was going on in Europe, what the U.S. was going to do about it, things of that kind. I tried to teach myself everything I could. I tried to teach myself music out of a book. [laughter] Imagine that! I knew what the notes meant, what the measures were and so on. I did have a very good public library and the librarians were very helpful and very nurturing. They put no limit on the number of books I could check out and helped me find things I wanted. I had nothing but help in that regard.

BOHNING: Did you have any friends your age who were similarly inclined?

LEDERBERG: No, and that was a very troublesome point. It wasn't until I got to high school that I had peers, and I felt very lonely during that interval. I did have the luck to catch up again with one of my grade school classmates, who remembers that interval. Through a strange series of circumstances, she's married to somebody I know pretty well, but I didn't know the connection between the two of them. A common friend brought that out. They lived here in New York for some time. They quite recently moved out to Cincinnati, and I had dinner with them a couple of weeks ago when I had business there. She remembered me very well even though I hadn't seen her in fifty-five years. She said that I was widely recognized as a phenomenon. I said, "You mean, a freak?" And she said, "No, it wasn't that. We just knew you were somebody pretty special and we might have to make some allowances for you." She didn't go into much detail about that. I thought I was pretty brash and rude and self-important. She minimized that and said, "We made a note of that, but we all understood." That's just amazing to me. They must have been wonderful kids! In other observations I've seen exactly the opposite, how youngsters can gang up on somebody that they're jealous of or something of that sort. I think what she said to me was genuine. I don't recall much negativity on the part of my peers; I just felt isolated from them. She gave me a different picture of that. Isn't that something? [laughter]

BOHNING: Was that isolation on an intellectual level because their interests were just so totally different than yours?

LEDERBERG: Yes, that's what she said eventually when I said, "What do you mean by allowances?" She said, "Well, you just weren't interested in the things that we were, and we couldn't keep up with you, but we knew that what you were doing was important, and that you would be something some day." I had the same general nurture from my teachers and what only occurred to me after my conversation with her is that I had viewed this as one at a time. In the relationship with my teachers, it occurred to me that they must have had some collective discussion too about what to do about poor Joshua, because there was a pretty consistent response. It had just never occurred to me before that I would have been an object of discussion. Some of the other things that Abby [Abigail Levin] mentioned made it pretty clear that I was. If you can believe this, they'd been doing some standardized tests on standardizing the IQ test, and they actually announced the results. I was supposed to have had the highest score of anybody in the eastern United States, or something of that sort. Abby was one of the runners-up; that's why she remembers it. She recalls our being presented at a grade school assembly, and Joshua was asked to comment, and "It was supposed to have hurt my votes." So I was not invisible to the faculty.

BOHNING: Did you get skip grades?

LEDERBERG: Yes, I skipped a couple of years. I finished high school when I was fifteen and a half, and I had to wait until that fall until I could enter Columbia because they had an age limit.

BOHNING: Let's discuss your selection of Stuyvesant High School. It wasn't automatic that you would go there, was it?

LEDERBERG: They had a competition for students interested in science. They offered a special curriculum and they had a special peer group. So given the circumstances it was automatic that I would apply, and I had no trouble getting admitted. So that's where I went. I think it was the peer group that made it very special. For the first time I began to have a bunch of youngsters that I could relate to and had shared interests and were as bright as I was. That did make a big difference. In some respects the teachers were not as experienced and wise as the ones I had in grade school, but maybe that's because I was a little older and knew the difference that makes. But they were fine. A couple of them were really superlative, and others were about what you'd expect.

BOHNING: This was a time when it was not uncommon to have Ph.D.s teaching in a high school.

LEDERBERG: There were a few, but not many. The best known one was Dr. [Joseph] Shipley in English who had books on etymology. I barely knew him. The principal, Dr. M. Nardroff, had his Ph.D., but there weren't very many. There were no research scholars among the high school teachers, and I was keenly aware of that. It was not until I got to college that I could meet people who really knew what science and research was all about from their experience.

BOHNING: How about the laboratory exposure? You'd been doing all this reading, even earlier on, in science. When did you get your hands onto something?

LEDERBERG: Like every other kid in those days, I had my own chemistry lab at home and nearly blew myself up a few times. I did all the recipes and made all the azo dyes and discovered new reactions and all that kind of stuff. The school labs were pretty dull. We learned analytical and worked with hydrogen sulfide. We had a few advanced placement labs. We learned how to use a balance and did quantitative analysis. There was hardly any organic chemistry, and that's what excited me the most. I had to do that on my own, and taught myself. I was able to get advanced placement when I got to college, and was in several advanced courses.

BOHNING: How early did you acquire this chemistry set?

LEDERBERG: Twelve or thirteen, something like that. I was reading [Meyer] Bodansky's textbook on physiological chemistry at that time (1). There was a little disconnection between these "great" chemical experiments and much more sophisticated reading, but they were fun.

BOHNING: How did your parents react to this?

LEDERBERG: I don't think they fully understood the risks I was taking; I'm not sure I did either. [laughter] I played with potassium cyanide with aplomb. There could have been great mishaps; in fact, with the exception of one or two fires and explosions, the opportunity for poisoning would probably have been greater, but I had a healthy respect for what they could do.

BOHNING: How did you acquire your chemicals?

LEDERBERG: There was no problem. Eimer and Amend would sell them to anybody over the counter.

BOHNING: Really?

LEDERBERG: I shudder! They sold me two hundred grams of sodium, and I was experimenting with progressive increments to see what was the largest amount you could throw into a pot of water and still only have an amusing pop. [laughter] There's a little thing in *C&EN* a week or two ago (2), when somebody commented after reading about the [Robert B.] Woodward symposium that none of these things would be possible today.

BOHNING: From what you've said, a lot of your early experience in a laboratory setting was chemically-oriented.

LEDERBERG: At school I was in the biology club and learned histology and how to make sections. I was doing a lot of that. I got interested in microchemistry and cytochemistry, and I thought that was what my career was going to be—using advanced micro-chemical technology to explore the chemical nature of the cell. That was exactly the wording that I would use when I was fifteen, and so I was systematically going through staining reactions and how they might be influenced by fixation. I got interested in the nucleolus, basophilic stained material that was Feulgen [DNA] negative. I would have been able to tell you that at fifteen. So we didn't know what it was and were trying to figure out by micro-chemical procedures by this point to determine its chemical composition. It was the appropriate scale to be asking questions like that in those days. I didn't know it, but that was just about the time that [Jean Louis] Brachet introduced the use of ribonucleases as a differential reagent. The removal of basophilia ribonuclease was his evidence of RNA. I was still fumbling with the issue.

[END OF TAPE, SIDE 1]

LEDERBERG: By the time I was sixteen, I had access to good enough libraries that I could search out this kind of thing. Cooper Union allowed me to use its stacks when I was a high school student, and so I could go through *Chemical Abstracts* and probably *Biological Abstracts*. I could search out what I needed to know in most of the literature. The literature was one percent of what it is today. Brachet's work was done in Belgium, and those papers just didn't get out from behind the German lines until after the War.

BOHNING: Did anyone point you to things like *Chemical Abstracts*?

LEDERBERG: I don't know who it was. I think I just went to the library, and it would have been the librarian that helped me if I wanted to look something up. I know I got interested in steroid chemistry while I was in high school. By a curious coincidence, I got on to Russell [E.] Marker's papers about 1940 or 1941 (3). I read them from beginning to end. What's amusing is that some years later, I met up with Carl Djerassi. Of course, Syntex was founded on Marker's work. Carl was astounded that I knew all about that work. [laughter]

BOHNING: That's fascinating. He wrote some interesting papers.

LEDERBERG: Did you ever meet him?

BOHNING: No. We have an interview with him, but I didn't do it (3).

LEDERBERG: I've met him once, at some celebration. Carl's a great fan of his, of course. He's had a weird career. [laughter]

BOHNING: Yes.

LEDERBERG: So, the libraries were my most important resource. I did some of this laboratory work. It was focused on cytochemistry. In the spring of 1941, after I finished high school and before going on to college, I had a chance to work for six months in a reasonably equipped research laboratory [American Institute Science Laboratory]. It was a predecessor of the Westinghouse science prizes. They offered a research experience, instead of the prize hullabaloo. It was a much better idea. They had a lab that IBM offered some space for and then documented years later. They did a film on it about four or five years ago. It was quite a crew of people. I keep running into them all the time. Charlie Yanofsky and Barry Blumberg were in that lab, and we all remember it very distinctly. I continued working on this cytochemistry project.

BOHNING: Was it self-directed?

LEDERBERG: Pretty much. I'd hoped to have some guidance, because that was part of what was being offered. But they didn't have anybody who knew anything about what I wanted to do.

BOHNING: Going back to high school for a moment, did the teachers leave you pretty much on your own? Were you in a structured curriculum or could you take what you wanted?

LEDERBERG: No, there was more structure, but it was more advanced so I didn't feel quite so bored. Although most of the science I did, I pretty much knew the material before the course started, and if not, it was pretty easy to catch up. But there are different grades to knowing something, so there was a certain amount of drill and knowing it inside out, which taking classes did help. I didn't feel so bored at that stage and also had some peers to talk things over with. As an educational experience, high school was much more important in terms of the social sciences and humanistic subjects. There I remember a civics course that was absolutely superb. It was a pretty advanced course in political science and economics and rational policy making. I've forgotten the name of the teacher, but I have very powerful recollections of it.

BOHNING: Are there any other teachers that played a special role or influence? You seem to have fond recollections of some of your grade school teachers. What about high school?

LEDERBERG: I got to know the biology teachers and I've known them ever since. I've kept certain contact with them. They were professional teachers, they knew teaching well, and they knew their limitations. They didn't have the personal rapport with me that there was in grade school. It wasn't quite that level of affection upon me. The fact that they were men, not women, made some difference in that regard. Nurture is an awfully strong word, but they were a positive influence with me. When I think about it, they were reasonably direct about their limitations. They just weren't themselves research scientists and at their depth and intensity they were not involved in doing research. I had positive reactions to them, and I have great respect for their classes.

BOHNING: Do you think they were intimidated at all by you?

LEDERBERG: No. I think less in a way than my grade school teachers were. It was a less singular phenomenon to have a high school student reading college level material than to have a sixth grader doing that. I don't know if I remember anything that might recall that. They just considered it a more normal part of their job to deal with bright kids and provide some channeling and discipline and go about their business. So there was some professional pride in the way they developed it. I don't recall anything like the personal touch that I had in grade school.

BOHNING: You said that the six months before you could enter Columbia was a unique way of doing things. Were there any special experiences during that time, or did you just continued on with your own project?

LEDERBERG: I was able to do it more or less full time, at least part of the semester. I think I worked during the summer. My family was, to say the least, not very affluent. Things must have been getting a little bit better though, to have enabled me to do that rather than have full-time work. Although, I did work that summer. No, it was just the fact that it was the enjoyment of being able to concentrate on one subject. Then you have the kids there. That was quite exciting to talk things over with them.

BOHNING: Why did you select Columbia, instead of, let's say, CCNY [City College of New York], which at that time was also a very strong institution?

LEDERBERG: Well, I was headed to CCNY, but I knew a little bit about scientific eminence. It was somewhat out of date, but I still associated Columbia with, if not [Thomas H.] Morgan, at least with E. B. Wilson. I had Wilson's book, *The Cell in Development and Heredity* (4). I still have my copy of it. I'd been reading that during my last year in high school. I was eager to go there. There were a couple of other possibilities. Cytology was the core of it. I probably didn't know that Wilson had died long since. [laughter] It certainly is no longer there, but it was the preeminent school in biology. I knew that unless I had special financial assistance, I had to go somewhere where I could commute. That wasn't forthcoming. I did apply to Cornell. The botanist there, Leslie Sharp, was in cytology, and I knew his textbooks (5), but I failed to get a Telluride scholarship that might have allowed it. Robley Williams was on that committee, a fact that I discovered a little later on, and I teased him a little bit about having turned me down, but it was probably a good thing for me. [laughter]

Cornell was quite discriminatory. A farm boy could get to Cornell, and in the program I had in mind, I couldn't. As a matter of fact, Norman Krechner, a classmate of mine, did get into Cornell and subsequently became head of the pediatrics department at Stanford. I'm having dinner with him tomorrow night. So a very small sprinkling of New York City students made it. That was the only thing resembling a state university. City College was crowded, very few lab facilities. A lot of brilliant people went there because there was no alternative. I did regard it as a last resort, and I was happy I could get a tuition scholarship at Columbia. I think that being able to go there was the luckiest thing that ever happened to me.

BOHNING: You mentioned earlier you had been following events in Europe, and I know that CCNY was a hotbed of communism. [laughter] I was just wondering what you were thinking politically.

LEDERBERG: I had some age contemporaries who were very keen on it and anti-fascism and so on. I was very skeptical from the very beginning. I didn't see that much difference from one form of totalitarianism versus another, and I wasn't going to buy that for one minute. My politics haven't changed much in all that time. I had thought that the war in Spain was a test of what democracy was able to do. I thought it was a disgrace that the West did nothing in those dimensions. But as far as taking sides with the Soviets in fomenting revolution or whatever, I had no truck with that. It had its faults, but America was the best place in the world that anybody could be. I saw how the liberties of people had been achieved and yes, that there were many more things to do. Most of the scientific colleagues I had were sort of on my side, and the ones in the social sciences tended to be more leftist.

I'll mention one more point. The thing that completely told us apart was the Molotov-Ribbentrop Pact [1939]. That was really the touchstone of where you stood on those matters. I could sympathize with those who had had some pro-Soviet (because anti-fascist) leanings up to that time, but when I saw drove after drove of the kids that I knew then suddenly switched off their opposition to Hitler after the Pact, I had nothing but contempt for them.

BOHNING: You were only a few years old when the Depression started. What effect did it have in your life?

LEDERBERG: I was born in 1925; that's a generational milestone. I don't have distinct recollections, but I think we were like the parson in the small town; while our cash income was very limited, quite literally the butcher and the baker would help out. I also remember guarding the telephone to make sure that if there were special calls for religious officiation, we wouldn't miss any opportunity for a wedding or unveiling or something of that sort. I've gotten hold of the minutes of the synagogue that my father was the rabbi at, and during that period that there is this information, "No money, can't pay rabbi." That's what's in the minutes. [laughter] So it was the five-dollar fee or fifteen-dollar fee for officiating at the weddings and funerals that kept us going. We were never totally destitute, and we were never very well off either, so we lived at that level throughout that period.

BOHNING: Did they provide housing for your father?

LEDERBERG: There must have been some deal. I don't know what it was. We were living in what I can see now as a pretty comfortable apartment house. It was incommensurate with the cash income, so there must have been some special deal. I had to share a room with my brother most of the time. Eventually I got a room of my own, but I didn't have to share with my parents, so we were not at the very bottom of the heap.

BOHNING: Where were you living in New York?

LEDERBERG: Washington Heights.

BOHNING: How would you classify Washington Heights at that time?

LEDERBERG: It was just on the northern border of Harlem. The public school I attended was right on the border and a lot of black kids attended the school. There was no great discrimination there. There were kids who did pretty well; they weren't at the top of the class, but they were good students. There was nothing like the stratification that we have today, and a minimum of racial strife. My problem was not the black kids but the Irish kids. There were a number of those, and the priest talking about Christ-killers, and so on. There was constant strife straight off. There were allowed zones of traffic coming home from school. If you strayed one block from that, that was invading territory. There were little pockets of Irish Catholics in a mostly Jewish community at that time, and you'd get beaten up if you crossed the line. Now, nobody ever pulled a knife or a gun, so there were differences in that regard.

BOHNING: As you were growing up, were you aware of anti-Semitism?

LEDERBERG: It was pretty abstract for me. I saw this event, but you could argue that there were ethnic groups fighting one another all the time. I would hear a lot about the difficulties that other people had in getting jobs because they were Jews and I'm sure there was some substance to that. I personally experienced very little of that. I think there may have been a considerable interval during which if you super-excelled you could make it in almost any sphere, but that, other things being equal, the non-Jew would be preferred over the Jew. So it was a superable handicap in any event. I didn't realize how much of an issue it was in the world around me. In a way I was somewhat naive and protected. I knew it was an issue in college admissions, but I got into college. I knew it was an issue in medical school admissions, and I got into medical school. I'd hear complaints from others that they had been left out because they were Jewish, and they were probably true. So there was the external evidence, but my own experience was much more protected.

When I was offered a job at Wisconsin in 1947, I had no idea until I got this from later documentation. One of the professors told me what a storm it caused because I was the first—or one of the first—Jews to be appointed to the college of agriculture and that there was a lot of resentment about that. They apparently worked it out at the time; the people in my department worked very hard and I think were quite furious at this kind of criterion. There were other elements in the school that had made a fuss, but that was all dealt with before I got there. In retrospect, I might have said that at a social level I was not as welcome in some places as I might have expected, I didn't have any standard and it was personally dealt with. I didn't have an inkling. These storms could be going around my head, and I wouldn't even know about it. So was I blinding myself to it? Professor [R. Alec] Brink was the chairman of the department at the time, and some years later just before he died, he shared this information. I have every reason to have gratitude for the part that he took in that.

BOHNING: Jerome Karle has told me that when he came out of CCNY he desperately wanted to get into medical school and he couldn't.

LEDERBERG: Yes.

BOHNING: And I think he went to Harvard and did a master's degree in biology hoping that would enhance his chances of getting in, and that didn't help.

LEDERBERG: No. [Arthur] Kornberg wrote about his experience at Rochester and [George H.] Whipple, who was deified in internal medicine, told him he was not going to get the chief residency because he was Jewish. He managed to make it, but there were certainly those issues all the time. At the university level, World War II made an enormous difference. The V-12 program, the ASTP [Army Specialized Training Program], those sorts of things that were based on examination scores only and permitted no latitude for discrimination; the faculty did struggle with that. If I had been five years younger, I think I might have been hit much harder.

BOHNING: Well, I know our time is up.

LEDERBERG: Okay.

BOHNING: Thanks.

[END OF TAPE, SIDE 2]

[END OF INTERVIEW]

INTERVIEWEE: Joshua Lederberg

INTERVIEWER: James J. Bohning

LOCATION: Rockefeller University

DATE: 7 July 1992

LEDERBERG: We could concentrate on some of the specific questions you asked, but I'd also remark that I read through the transcript, and I asked what more was I getting out of that than I already had down? The answer is, not a lot. There are a few things we went into in a little more detail. But I think it might be a more efficient use of time—unless you have specific questions, and maybe there's no other way to do the interview—to skip over this stuff that I've already written extensively about and go on to other aspects of my career.

BOHNING: I'll leave that up to you. In going through some of the notes that you had given me, there were some things that I had questions about.

LEDERBERG: Let me respond to specific questions you have, but I won't go discursively through the things I've already written at length about. If they've raised questions, you might as well ask and I'll respond to those.

BOHNING: All right. Let me go back then through some of your early childhood just to verify some things. You had commented about some early traumatic events, but you did not elaborate. I don't whether you wanted to do that.

LEDERBERG: I'm wondering what that was. I thought I said I did not have any traumatic experiences of the kind that others often refer to. I didn't lose anybody; I had parents that took good care of me. I don't recall anything traumatic.

BOHNING: You may have been referring to this one note here about the burn on the left arm; that kind of thing.

LEDERBERG: Oh, yes. That's kid's stuff; really it is.

BOHNING: You talked about remembering Lindbergh's parade. That's going back pretty early to remember that.

LEDERBERG: Oh, okay. Now I recall what it was. I recall a few accidents, and having it described as traumatic has some implications of lingering consequence, which I didn't mean to imply. So technically it's correct. [laughter] I had a few falls. My parents used to quote this as an example of curiosity killing the cat. I pulled the tablecloth that had a steam kettle on it, and they said I was constantly doing things like that. I have a burn to this day along my arm. It was a pretty extensive burn, so yes, I do remember that.

BOHNING: We talked about your grade school, but we didn't identify it. You went to P.S. 46. We talked about your teachers and the contracts you had with them. You felt they were very compassionate and understanding teachers.

LEDERBERG: Yes, I felt that was well phrased in the transcript.

BOHNING: Going back to those pretty early grades, what kinds of things were you doing while the other children were doing their regular work?

LEDERBERG: I was studying my own textbooks which would be four, five, six years ahead in grade of what they were looking at.

BOHNING: But you were drawing then basically on the textbooks that the older children were using or were your teachers helping you focus on other things?

LEDERBERG: It was mostly out of the library, and I did get some help from the librarians. There were books about chemistry, physics, mathematics, astronomy, and biology. I remember reading Huxley's *Science of Life* (5). It was a very good snapshot of general biology at that time. Do you know those books?

BOHNING: I know of them; I don't know them specifically. I was going to ask you about [Paul] de Kruif's books, *The Hunger Fighters* and *The Microbe Hunters* (6).

LEDERBERG: That's right. I've mentioned those in my writings. I don't know if that's what I had in the classroom, but it was certainly contemporaneous. I remember the picture *Arrowsmith*, somewhere around 1930, maybe 1932. I would have been about seven. I've seen that. There were the inspirational works, such as [Bernard] Jaffe's *Crucibles* (7). I've dug out what I could, and I've already written it down. I can't add much to that. But I also read adventure stories and fairy stories and things of that sort. It was pretty eclectic, and I had the ambition to know everything! I sort of knew that wasn't possible, but I was going to give it a hard try. [laughter]

BOHNING: Science fiction was starting as a genre at that time.

LEDERBERG: I don't remember that *per se*. H. G. Wells, yes, but that's about as much as I can recall of that particular kind of fiction. I sort of looked down on it. I would criticize the science that they were attempting to portray. [laughter]

BOHNING: Another thing I was curious about was the trip to Israel in 1933, which we did not discuss.

LEDERBERG: This was my mother's first return to her homeland. She left in 1924, had two children, and was bringing them home to her sisters and nieces and nephews and cousins. I don't know where her parents were; they were occasionally in the States, occasionally in Israel. They changed location, so it wasn't for her own parents. It was her sister's family. I have a picture that I can recall the taking of with them. This was under the [English] mandate. There had been some serious riots, but more was yet to come after that point. At that time it was pretty peaceful. It was the flowering of Zionism. There were new settlements coming up everywhere and the desert was being made the bloom. There was that spirit well in place at that time, but already there were problems with limits on immigration into Israel. We were not part of that; we had emigrated. But it was already a fairly inspiring place, and we did see some of the historic sights. We had as tourists, free access to all of Palestine, which took a long time and a war for that to be the case again. We did some traveling around, but mostly my brother and I were put away in a camp for the summer. We had to learn Hebrew to survive and did. I'd learned some of it in Hebrew school at home, and so that was somewhat circumscribed. I remember seeing a lot of citrus groves, the beginnings of some towns. As of that time, it was obvious that things were just being brought out of the desert.

BOHNING: You mention here something about Zionist meetings here in New York and questioning whether you were introduced to [Albert] Einstein and [Chaim] Weizmann.

LEDERBERG: My father was involved in that. Maybe that's a screen memory, but it's not too implausible. I know Einstein spoke at those meetings, and I have a very vague recollection that's exactly what happened on one occasion. He was certainly much talked about.

BOHNING: Had you developed any role models at any point?

LEDERBERG: Well, he was one of them. [laughter] I wasn't sure I was going to be a physicist, but I generalized from that. In that 1932 letter that I've mentioned, the text is "I want to be a scientistist and study mathematics like Einstein." [laughter] He was somewhere between a role model and a folk hero. It wasn't in the sense that I could have any tangible expectation of matching his accomplishments, but maybe some little bit of it might be imaginable. I just want to clarify that. On the trip back from Israel, I had a nasty scrape and ended up with in retrospect what was osteomyelitis on my shins. In retrospect I shudder that I survived it; it was treated but we didn't have antibiotics in those days. I almost drowned on the voyage, but that was in the swimming pool. [laughter] The boat was rocked by a sudden wave, and I was dislodged; that's what I remember. We stopped in Naples, between ships. For the best part of a week, I could play in the streets around what must have been a lower second-class hotel. I got some of the local color, but there was fascism all over the place. It didn't have a strong anti-Semitic tinge at that point yet, so it wasn't a matter of being personally fearful, but nevertheless Hitler had already made his start and there was some image of that. When I came back and came off the pier, there were signs in all the storefronts. It was the NRA [National Recovery Administration] blue eagle, but my immediate reaction was, had fascism come to the States, too? Purely in terms of that symbol. I didn't know then that there were people of a different political persuasion who could have said Roosevelt was a fascist, [laughter] but it was just that symbology of the NRA. I disabused myself of that pretty quickly. I'd been out of the country for three or four months and didn't know what was happening. I was perfectly capable and certainly from that age onwards, I looked at *The New York Times* every day and kept abreast of what was happening politically. It was just being caught unawares, as I said, by the symbology.

BOHNING: Since you spent three or four months in Israel, did you have any sense there of what was happening in Europe and what was the reaction of people there was to what was happening in Europe?

LEDERBERG: Oh, there was great, great fear about what Hitler was up to. I remember the headlines of the Reichstag fire and things of that sort. There was a very good radio commentator named H. V. Kaltenborn. That's where we got a lot of our news from, and if you go through his broadcasts, you'll see just what we thought. [laughter]

BOHNING: We had talked last time about the Depression, but one thing we didn't mention was your father's illness, which evidently changed the situation within your family somewhat. That would have been around 1935 or so.

LEDERBERG: Yes. I think his first symptoms were about 1932. He had a progressive ulcerative colitis, which was quite debilitating. He was barely able to continue functioning through that time. He sort of managed to get by; it was hard.

BOHNING: Did this change the role your mother played in the family?

LEDERBERG: Yes. She just had to take a more managerial role in the family's affairs. Towards the end of that decade, he was really only able to work part time, and she started working. She did various things, teaching in Hebrew school, catering, things of that sort. She worked very, very hard.

BOHNING: I have a note here about being reprimanded in school for passing prurient notes about Lucky Luciano.

LEDERBERG: [laughter] That is just an incident I happened to remember.

BOHNING: He must have certainly been in the news at that time.

LEDERBERG: Yes. He was the John Gotti of the time. I don't remember what the note was about, but it was some wisecrack. He'd been running a prostitution ring, or something of that sort, so that was the context of it. His name figured later in that he did do some service to the OSS [Office of Strategic Services] during World War II.

BOHNING: Yes. That's right.

LEDERBERG: But he was the most notorious Mafia type at the time. I don't remember any more than that. I just recall I got in hot water.

BOHNING: In 1936 you were in junior high school, and we've talked about your reading, which goes way back, and the types of reading you were doing. Were you trying to establish your own library or were these mostly books out of someone else's library?

LEDERBERG: I couldn't afford it. It was the public library. I did get Bodansky (1) as a Bar Mitzvah present.

BOHNING: Was that your request?

LEDERBERG: Yes. I got E. B. Wilson—that was the encyclopedia (4)—as a high school graduation present. I've still got those books. I don't think I owned more than two or three others.

BOHNING: Did you start book collecting in any way later on, building your own library when you were able to?

LEDERBERG: Oh, yes, as soon as I had some income. They were so precious. You see the consequences. [laughter] Please, come in here and let me show you some things.

Of all the geneticists I ever knew at the time, I'd actually read about Archibald Garrod in Bodansky. [laughter] I knew about them before [George Wells] Beadle did.

BOHNING: That's interesting. Did you take notes when you were doing this reading?

LEDERBERG: I must have, but I have next to nothing from that date. I have a couple of papers that I wrote when I was in high school. That's about it in terms of my own writing. One of them was on the theory of fixation. The other was at the American Institute of Science lab on the cytochemistry of the nucleolus. Those are the only things I have of that vintage.

BOHNING: What I was getting at was did you take notes as you were getting books out of the library or did you just commit it to memory?

LEDERBERG: Oh, no. I took notes. I'm confident of it. In fact, I used to treasure paper to be able to do that, and I'm sure I did things very systematically, like surveys. I've forgot what I did it on, but I remember once I discovered my mother had a roll of eleven-inch wide paper, used for lining drawers, and what a wonderful thing that was for writing large schema on. [laughter] That was my blackboard. I don't have any of those writings, alas. I recall writing to Louis Fieser. He had written about carcinogenic polycyclic hydrocarbons, and I had some query about whether their carcinogenic action was related to their similarity of structure to sterols. That would have been the time I was reading Russell Marker and so on. I know he responded in a not totally perfunctory way, but a fairly mechanical way. He gave me some reference or other. I was interested in mechanisms of carcinogenesis. Any chemical that could change life processes in the cell was something very exciting to me while I was in high school. I guess that's right up to the point of my research program now.

BOHNING: How did you react to organic chemistry? I've found most people have either a positive or negative reaction.

LEDERBERG: Oh, it was very positive. I thought it was just wonderful, and I wasn't daunted by the names. There's a memory barrier, learning all the names, but I had a perfect memory at that time, so that was no problem at all. I just gobbled it up, and it made total sense to me. It wasn't something that was just a list of formulas; I could deal with them very systematically. That was the autodidact mentality already operating. I actually plunged quite deeply into it, but almost all out of books. I told you I did some lab experiments at home.

BOHNING: Dyestuffs, things like that?

LEDERBERG: Yes. I remember I made a lot of different azo dyes, experimenting with a variety of different coupling reagents. I played [William Henry] Perkin all over again. What I had no idea of then, and it's taken a long time, is the recency of that. You know, anything that happens before you were born is all lumped together as prehistoric. I would have found it very hard to comprehend that there were many men still living at that time who had been born before aniline dyes had been discovered. I could see a date in the 1860s, 1870s, but that might as well have been B.C. [laughter]

BOHNING: You mentioned Perkin and Kipping; I think they were both alive yet in the 1930s. Kipping was later one of the forerunners of silicone chemistry.

LEDERBERG: That's reductionism taking hold. I really felt that if I could understand physical organic chemistry, the underlying atomic theory of chemical reactions that it would be indispensable to try to understand biology as well. It's partly true, partly not, and let's just skip over that detail for now. Doing an x-ray diffraction of DNA is somewhere in between. I worked hard to get the mental apparatus to be able to do that. It's not a bad paradox; I still relate it to students today to get as deep a grounding as they can at that level.

BOHNING: The idea that biological systems had a very important chemical nature is really what I hear you're saying. At that time, was that a generally accepted view, or were there still enough of the traditionalists around?

LEDERBERG: In the books I read there was a lot of optimism that it might be an infinite quest, but that was the way to go. I never questioned it; I thought that it was sort of old-fashioned and silly to invoke anything outside of chemistry to explain biological phenomena. I would have followed Huxley-Wells pretty closely on that. I think that's pretty much their perspective on it.

I was never taught anything to the contrary either in school. The question was either skirted or a fairly mechanistic approach was adopted. There were the different levels of vitalism; there was *de jure* and *de facto,* and there would have been people who would have scoffed at the idea that you in practice could dissect the gene chemically. That was so awesome that it could be another five hundred years. I may have been tinged with a little bit of that; just a great respect for complexity as you've heard me articulate elsewhere. So there was that ambivalence of an ultimate optimism but a fair amount of humility on the way.

[END OF TAPE, SIDE 3]

LEDERBERG: Then, as now, I was willing to put some questions as being operationally inaccessible and therefore let's not argue about them. The nature of mind or of consciousness, things of that sort, I would have said that they will ultimately have a chemical explanation, but our detailed knowledge is just too dim. If you couldn't think of an experiment—I was a Popperian before Popper as others were—then there was no point in pressing the question. The question would be meaningless unless you could frame an experimental test for it. I don't know where I got that, but it may or may not have been what people like Ernst Nagel would have taught, but that's what I extracted from my readings like that.

BOHNING: What about the taxonomic aspects of biology like botany and zoology?

LEDERBERG: I thought they were pretty dull and detailed, but they needed to be known if you wanted a picture of all of life. This was the way that one's image of it could be organized. I thought morphology was a pretty shallow basis for that kind of description. I wasn't thinking of DNA in those days as much as different enzyme systems, the proteins that might be expressed. I looked forward to more of a chemical taxonomy coming along that might be somewhat more meaningful. But I respect it, and people had to do that. I would never have scoffed at it. I might not have felt it was my own cup of tea.

BOHNING: The reaction of going to a natural history collection in a museum is one of going on mental overload pretty quickly—a room full of birds, or a room full of insects, or something of that kind.

LEDERBERG: I tend to suppress detail. I can skim a book. I can skim an exhibit and still not get turned off by it, but extract what there could be of interest. I would visit the American Museum of Natural History quite often and enjoyed those displays without feeling drowned by them.

BOHNING: What about the other cultural aspects of New York? Were your circumstances such to allow you to do more than just visit the museums that were free?

LEDERBERG: I might have gone to theater once in my young lifetime. I'd go to the movies. I don't think I ever visited the opera during my first residence in New York. They were financially inaccessible, if nothing else. And I wasn't that interested in going. I did play around in the Metropolitan Museum; in those days kids were allowed to walk into the Egyptian tombs and things of that sort. [laughter] It was great fun! And I enjoyed that a lot.

BOHNING: I can imagine.

LEDERBERG: To tell a little story, and I wish I could document it more clearly, but this was pretty early in high school. A friend of mine and I got interested in hypnosis, and we wanted to experiment with it. We managed to nab a subject, and boy, were we treading on thin ice. He felt very guilty about masturbation, so we said, "We'll see if we can help you with that if you'll be our subject." We didn't intend to do anything to hurt him, but, my God, what an IRB would think of that kind of involvement. I was maybe fourteen at the time. This kid was probably fifteen or sixteen. He was a Puerto Rican. He was in the same junior high school that we'd been in. We'd read about hypnosis, post-hypnotic suggestion and all the rest of the books, and we sort of went through the drill. He was a very willing, very suggestible subject, and we did manage to do this. I remember that we got him to the point that with the code was, "Oom, oom, sleep!" He would just go right under. We had him conditioned to that. I don't know what books we were looking into, but we had read about regression under hypnosis; we just thought we'd explore this a little bit.

We had the shock of our lives! We asked him to think back to when he was an infant and he gave appropriate responses. We asked what was he before that, and what was he before that. Then we said, "Well, okay. Were you ever reincarnated?" He said, "Of course!" We said, "Well, let's go back. What are you now?" Before long he was a scarab in Egypt, and we were asking him to describe his environment. Here was a kid who was barely literate, and he started writing out hieroglyphics. I've never been so astonished in my life. [laughter] I can't give credit to this idea of how in the world am I going to account for this phenomenon. We got a couple of pages of this kind of stuff, and we were trying to figure out if we could translate it, if we could figure it out. What could be the provenance of all this? When he was awake he confessed no knowledge of anything about it. And believe me, he would have been startled to think that he'd ever heard of a hieroglyphic. We finally managed to see one of the Egyptologists at the museum. I don't know if we told him what we were up to or not, but we just asked him, "Can you date this material? Can you identify it?" He looked at it for a while, and he said, "This looks like some of the popularization of [Jean François] Champollion's work of the mid-nineteenth century." There were mistakes in it, and they were not completely accurately rendered, and that's how he was able to tag them. He wasn't able to point to a book that this was copied out of, but he said it was of that genre. To this day, I can't imagine where this kid had ever picked that up. [laughter] It's a totally unresolved mystery. We were pretty scared when he first started producing this. We just didn't know what genie we'd let out of the bottle.

BOHNING: It's almost like the traditional speaking in tongues kind of thing, in written form.

LEDERBERG: Yes, but it's also told me to just never underestimate anybody's intellectual potential; it can be overlain with all kinds of things, and if you only get to root of it, you can get all kinds of fantastic productions. I have no idea what's happened since, and I have no idea whether we "cured him of his habit." I'm not even sure what our view on the matter was, but anyhow there you are.

BOHNING: Did you try any more hypnotic experiments after that?

LEDERBERG: No.

BOHNING: I'm amazed how easy it was for you to be able to do that.

LEDERBERG: Well, he was pretty suggestible and we were pretty confident. [laughter]

BOHNING: That's a good combination.

LEDERBERG: I have no doubt about the authenticity of it. There was no way he could have faked it. We went through a lot of the routines, including suppressing pain reflexes where we would stick pins into him. There were a few post-hypnotic things. We did nothing cruel; we were not malicious. We could have been careless.

BOHNING: So your life was pretty much concentrated on your own self-study.

LEDERBERG: That was the core of it.

BOHNING: Was there any interest in athletics?

LEDERBERG: My mother would chase me out of the house every now and then and say, “Joie, you’ve really got to go out and play. You can’t stay indoors all the time.” I’d occasionally do it. I might get into some gang or other that would allow me to join in, but I guess our main sports would be stickball or stoopball. We lived in a very good location for that. We lived right off a cul-de-sac, so there was no traffic coming in or out. I enjoyed going through the woods and looking at the natural history of what was there. I remember bringing home a praying mantis and putting it in a bottle and keeping it as a little pet for a while. My parents were somewhat horrified. It was a very formidable looking creature. There was a swimming pool up at Highbridge Park in the summer time. It was a great thing to go to, a public pool. But I was more likely to be at the library than any other place. And it was pretty well stocked. I’ve been there since; it’s nothing like it used to be in terms of just the range of texts, the range of specialized material. They had Bodansky there; that’s where I heard about it.

BOHNING: I wanted to ask you abut that, because in Bodansky’s introduction, he mentions other books that you have said were very influential early on. Was Bodansky the one that got you started in that sequence?

LEDERBERG: I don’t remember that. I’d have to look at the introduction. It wouldn’t be unreasonable. He had written on physiological chemistry. If there was such a thing as pathological chemistry I thought that would be really exciting. [laughter] There’s just not very much available on that. There is a text by Wells called that (8), but it’s quite disappointing. I’ll have to see that to refresh my memory. By the time I was in high school is the time we’re talking about here. Since the time I got the book, I certainly would have looked up some of the articles he had in footnotes if they were things I was especially interested in. I don’t recall which ones they would have been, although the alcaptonuria story would be a good candidate. I wasn’t reading German, and so many of these are in German. Here is H. G. Wells—that’s a different one than the science fiction writer—*Chemical Pathology* (8). I do remember looking that up, and this is under theories of metabolism, so I was imbued with that young. [laughter]

BOHNING: How old would you have been when you got your copy?

LEDERBERG: I got my own copy when I was thirteen. I was already very familiar with it. It was a Bar Mitzvah present, dated May 31st, 1938. In this introduction, the reference to [Joseph] Needham (9) would have excited me. I know I’d read that, but that was in college, very likely. E. B. Wilson, *The Physical Basis of Life, in Colloid Chemistry* (10). Boy, those are all very familiar. I probably did look into those. [Robert] Chambers’ “The Nature of the Living Cell as Revealed by Micromanipulation” (11). I attended a lecture Chambers gave; it had to have been about 1936. A friend of mine, who’s a little older than me, five or six years older than me, tells me that he’d been there too, and there was this young kid who got up and asked what he felt was a very penetrating question. That was me. [laughter] I was eleven. I asked about the reality of spindle fibers. I must have already been reading about that, and that is cross-referenced here, so it could have been a lead. Just as likely, I'd just go down the library shelves, and in that section I'd just look at every book on the shelf and pick out things I thought I could understand.

BOHNING: You got an unabridged dictionary at the New York Post office?

LEDERBERG: It was an advertisement that you bring in the coupon, and you get it for a dollar, or something like that. I remember taking a trip down; it's just off the East Side Highway, right around here. I got it and brought it home and it was one of my books. I don't have that one any more. It had etymology in it, and I tried to teach myself Greek and Latin roots by just compiling the roots of the words that I looked up there. I wrote my own concordance out of that. I remember now—that's what I used some of those big rolls of paper for. [laughter]

BOHNING: At the same time you missed a word at the spelling bee at Radio City. Did you consider yourself a good speller?

LEDERBERG: Well, I was the champion in my school. They had this competition, and I won a chance to be on the radio. I was struck out. The announcer—and I could clearly hear it—said "emullient" and I was a little torn. I knew the word "emollient," but he was pronouncing another word. I spelled it with a "u" and I was struck out. I looked it up in the dictionary; there was no word with a "u" and I had no case. [laughter]

BOHNING: That same year your father took you to Washington to the Palestine Conference. [February 1936]

LEDERBERG: Yes. I guess that must have been when I graduated from grade school. That was my first visit to Washington. I quite recently ran into my autograph book from public school, which sort of doubled for that, and that had a little record. It had some signatures of some of some of his colleagues down there, so that's what pinned that date down for me. Mrs. Louis Barst, Maurice Samuel, Samuel Goldstein, Charles Cowen.

BOHNING: Do you recall anything specific? The situation in Europe was certainly deteriorating by this time. Did you attend any of the conference or were you just there?

LEDERBERG: I don't think so. I think I just toured the sights in the city. I was deeply impressed—the Lincoln Memorial, the Washington Memorial, all that wonderful clean marble and the sense of power that there was in the White House, things of that sort. [I would never have dreamed I would be commuting to Washington weekly on the air shuttle.]

BOHNING: I didn't realize you'd been a member of the Boy Scouts either.

LEDERBERG: Yes, locally.

BOHNING: It was the thing to do in those days, wasn't it?

LEDERBERG: Yes. There was one organized at the local Y, and it was a social activity. There were skills to learn, and there was some natural history. We did a few hikes. I learned about Morse code and knots and some things of that sort. I didn't stay in too long, but I was there. I guess I made second class scout. Some people criticized it for being militaristic; I didn't see it that way at all.

BOHNING: I was struck by your pile of *The New York Times* here, [in office] because there's a note here that says you saved the daily *New York Times.*

LEDERBERG: [laughter] You're absolutely right. These I clean out every couple of months, but I didn't do that at home. I just felt that here was history going by, and how could you sort of let it go? I thoroughly ingested and wanted to read things that might have been a week or a month old. Just maybe I'd want to see it again, and sometimes I did. I was thrilled to learn that there were archives in the libraries where you could get them, and subsequently was very disappointed that hard copy of old newspapers doesn't exist any more. That's a bitter blow. [laughter] They used to have a rag paper edition that I would consult in the Cooper Union Library. I don't know if I mentioned this before, but I felt that I ought to know something about World War I, which I'd just read a very little about. So I just scanned *The New York Times* for the entire war just to get some sense of what it was like to have lived through it. That had to have been when I was in high school. Cooper Union was a couple of blocks away.

BOHNING: You've already talked about the tobacco mosaic virus story that was in *The New York Times*, but I've forgotten what year that was.

LEDERBERG: That was Wendell Stanley in 1935.

BOHNING: That's much earlier. In addition to the political scene, were you also trying to watch the scientific scene? Was this one way of getting up to date on what's happening?

LEDERBERG: Well, *The Times* certainly included stories like that, but I didn't leave it at that. I didn't expect that to be my primary source of information. There was something called *Scientific Monthly*. I suspect that I got it in the library whatever. That's probably the thing I read regularly. I didn't read *Science* yet as a routine, but that's probably the one.

BOHNING: *Nature*?

LEDERBERG: No. When I got to college, that would have been the journal that I would have consulted regularly for current developments. There was one other one. *Science Digest*. I remember Watson was the editor. It would have been in the library at Stuyvesant; I would have gone for that. I doubt if *Science* or *Nature* would have been there. *Scientific American*. I'm trying to recall the format; it didn't look quite like what it does today, but it covered a somewhat similar kind of ground. *The Sunday Times* used to have a regular science feature; I remember that. There was more there then than there was for a long time thereafter. It would have been the weekly equivalent of the Tuesday issues that they've had more recently.

BOHNING: Your high school yearbook said "CCNY Biochemist."

LEDERBERG: Yes.

BOHNING: By this time you were already doing work in cytochemistry and histology, and I just want to talk a little bit more about that.

LEDERBERG: I still saw that as a branch of biochemistry, but I didn't know that biochemists mostly did other things than that. [laughter] But it was not illegitimate. It's just that cytochemistry would not have been mainstream for most biochemists. If I'd known better, I would have said cytochemist.

BOHNING: How were you envisioning the work you were doing and what was happening in the larger world, so to speak. You say you were keeping up with it to a certain extent. Did you feel that you were ready to make some original contributions at that point?

LEDERBERG: The larger world you've just referred to is the political scene, and I felt utterly powerless personally to do anything in that sphere. I thought if one could marshal enough intelligence, one might be able to figure out what to do better, but I didn't feel very comfortable about my own world scheme. It wasn't until the 1960s that I felt well enough educated politically to be able to put in my own two cents in any reasoned way, other than parrot what other people might have said. Scientifically, I thought it would be quite a while before I would be making original contributions. I thought the quest was important, learning how to do investigations. One would come across interesting problems, and then something would emerge. I didn't expect it in high school. I didn't expect that in college I would be making a significant contribution. I did not accurately predict the future in that regard.

[END OF TAPE, SIDE 4]

LEDERBERG: That was very formidable. There were people like Einstein out there, with a hierarchy of contribution and accomplishment.

BOHNING: Had you developed any new role models by the time you were getting through high school?

LEDERBERG: Not in the sense of an Einstein. I'd seen these marvelous books that I just quoted to you, and I thought they had a lot to teach. I didn't identify with them; I was still the student looking at what teachers had to say. But the images of people like [Louis] Pasteur and [Robert] Kokch and the others that de Kruif talked about were there. I guess I hoped I might someday be a person like some of those without being too closely identified, but I'd have to work very hard and be very diligent. There's a real paradox. On the one hand I had pretensions about being the smartest person I knew and I was going to learn everything, and I did know more about most things than most of the people that I met in terms of my book learning, certainly. At the same time, I underestimated myself and if I look again fairly objectively about that I can't quite piece that out. I did have a unique mentality, but I didn't explore the full meaning of that term. I guess I felt there must be somewhere hundreds of other kids like that, if I could only get to meet them and find some day at the university some group of that sort. I did not have a clearly formed picture of where I would stand in that hierarchy. To have ended up having won a Nobel Prize by the time I was thirty-three for work I'd done at twenty-one—I had no dream of anything like that. That might have been the end of a lifetime of very hard work.

So it's in that sense not totally accurate. But there's that paradox. I'm still trying to resolve this in my own head about where I would have placed myself. I may have seen myself as being the biggest fish in a very small pond, but there must have been oceans around that I didn't know anything about that I would have to think about. That's probably the closest metaphor I can think of. I would have thought it would have been blasphemous for me to have compared myself to Einstein, let's say. Maybe I still do, but that isn't what I meant when I said, "to be like him." A more accurate reflection of it would be to be some pale image of that kind of personality.

BOHNING: You've had this interest in science virtually as far back as you can remember. Did you have any broad picture of science in terms of its usefulness rather than its just being an intellectual exercise?

LEDERBERG: Oh, sure. They were all merged. The scientific method would be the salvation of our political and social problems, if we could only think that way. If we could be dispassionate, we could end up being more effective and more compassionate in the long run. One had to distance oneself from a problem in order to really effectively deal with it, so there again there was that kind of ambivalence. I had no thought about science for weapons, and a great deal of indoctrination about all the advances in medical science, so *Microbe Hunters* (6b) would have been the paradigm. Here many wonderful things, extraordinary things, had come about, and yet they were all based on very basic research, whose outcomes could be predicted. It was all a seamless web, so the picture I have now I'm sure was pretty close to what I had then. I had a little more faith that scientific accomplishment would more or less automatically work out to human good, because I thought that its method, its focus on long term goals would be part and parcel of how it would be used. That was obviously somewhat nave. To that degree the Bomb was certainly a turning point in one's thinking about that.

BOHNING: You were already at Columbia when Pearl Harbor occurred, is that right?

LEDERBERG: Yes. But while we're still at that epoch, I want to just recall about the World's Fair and the Museum of Science and Industry. That was a great treat. I've just taped a film of a reminiscence about that. It was on Channel 13 (WNET-Ed TV) the other night (12). Those are very vivid images—the trylon and the perisphere and its symbolism. This was the new theology and that's the church steeple, but in the name of science. [laughter] There was an optimism about the new technology, and then the paradox of all this happening just as the world was going to war. That message wasn't lost. The image was, here's this wonderful opportunity: "if people would only think scientifically"—that's a phrase I would have used in those days. But they don't, and human folly is going to result in the misuse of all that technology. If we would only somehow inspire a more—I would have then used the label—a more rational use of those kinds of resources. That was the basic paradox, and I haven't totally resolved that yet. There was just all kinds of stuff.

I remember they had the transparent woman. They had all the organs laid out in a wonderful way, and this is a piece of natural history nowhere better. There was Polaroid, and I'd keep going in line again and again to get the little free samples of these things and play with this stuff. There was Bakelite, and I would again grab samples of that and cook it up at home. [laughter] Now I know it's a formaldehyde resin. There's some chemistry there that I'm still involved with in my current research. But there again, these were images of technological utopia. I'd already also read Aldous Huxley and *Brave New World* (13). I understood quite early what the downside might be, about the potentialities for self-destruction. What was going on in Germany was perfectly evident. Here was a people who sort of allowed themselves to be taken over and then become a menace to the rest of the world. I didn't feel I could understand those phenomena, and I still don't on a social and political level. That's our biggest challenge: to know how to keep ourselves from doing all those things. The Museum of Science and Industry was somewhat earlier. It was in Radio City.

BOHNING: This was in Chicago?

LEDERBERG: No. There used to be one here. It was not a bad match to the one that still exists in Chicago.

BOHNING: Really? I didn't realize that.

LEDERBERG: It was the same sorts of things I've mentioned at the World's Fair. Half of it was sort of silly but straight out of Detroit—all the ways you can make gears turn. And they had very funny looking gears—square ones and elliptical ones. Machines that were balls bouncing off of a steel plate but with perfect precision, and it did give one a sense of determinism. That even something that you think of, flipping a coin as being a random event, you realize that's because you don't completely control all the impulses that were put on the penny. They had some very early demonstrations of television. I remember telautography: distance writing was there. I was imbued with what communications were going to generate for us as the epitome of that time. Those were the main messages. But there again there was a euphoria about how wonderful technology was. I did a poster in junior high school, which was my own version of "Better Things for Better Living through Chemistry." I've just retrieved that from the Du Pont archives, and I'm going to send that in as one of the things for the *Oxford Dictionary of Scientific Quotations*. Did I tell you about that?

BOHNING: Yes. Did you get my letter about Kekule?

LEDERBERG: Oh, yes. I just got it; it was in this morning's mail. So I dug that out—at least it was one of my icons. [laughter] It's just so emblematic of that sense of optimism. They don't even dare use the word chemistry these days. What a difference.

BOHNING: Dow Chemical is one of the few companies that purposely kept the name "Chemical" in its name, although they seriously considered removing it back about twenty years ago.

LEDERBERG: Is this when they were making napalm? [laughter]

BOHNING: Among other things. They had Agent Orange, they had mercury in Lake Huron. They had a number of problems they had to deal with.

LEDERBERG: Anyhow, I just wanted to recreate that sense of optimism in that era.

BOHNING: Where was this museum located?

LEDERBERG: In Radio City. Quite recently, in the last six or eight years, I was talking to Bess Meyerson, who was then the Deputy Mayor for Cultural Affairs. There was some rumor about getting a thing like that started again, and I begged her to get it set up again in its old place or the AT&T building. But she got into some political deal with Borough President Mannes over in Queens and it got stuck out in Flushing. It's all right, but I think it would have been better in a more central location.

BOHNING: How long did that exist? Did it have a long lifespan?

LEDERBERG: No. It was folded up probably in 1942. The World War did knock out a lot of things like that.

BOHNING: You graduated from high school in January of 1941. You spent the spring semester at AISL [American Institute of Science Laboratory]. Maybe you could talk some more about that, because we sort of skipped over that lightly. For example, you mentioned here about the first time you saw IBM punch cards, or something like that. I'm just wondering if you could tell me a little more about that whole laboratory situation.

LEDERBERG: It's been documented in this IMB Think story (14), and in fact somebody's done a film on it (15), so you can get those objective materials. They had some sort of examination, qualification process, and about twenty-five or thirty kids were given this opportunity. A very fine, then very young, person, Henry Plaut, who was a Ph.D. psychologist, bumped into Tom Watson at the World's Fair, struck up a conversation, and the idea for this was hatched right there. By late 1940, IBM sponsored a laboratory for high school kids. He was the administrator for it, and it was just that. By design, they would have mentors to guide people's research, but they never found anybody who knew anything about what I was doing. So I ended up doing histochemistry and cytochemistry. I was going to study the two things that I mentioned to you before—how the change in staining properties of cellular materials under different fixation regimes might be clues to their chemical composition, and the specific case, what was the chemistry of the nucleolus. I was in really deep water on that. I could do experiments in which I fixed preparations, things like oxidants, different pHs and different solvents and so forth.

In retrospect, none of the reagents that I knew about would have told me enough to reveal much about what was there except lipid solubility. But Brachet did do the right experiment with enzymatic extractions. I didn't know enough to extend the reagents. There are still a lot of aspects of staining that we don't understand and I was just trying to get some sort of rational framework for why one dye works better than another one. Is it the pK and other binding properties? I was in quite over my head, but I had the literature and I had done a little work on staining of model substances under those conditions. I remember one paper that stuck with me for a long time, by [H. C.] Eyster (16), and this had to do with specific uptake of methylene blue by charcoal. What it boils down to is whether there are sites on charcoal that are specific for things like sulfonamides. He thought he had evidence that he could use methylene blue as a blanket reagent for adsorption, and he could display some of those sites with more specific ligands.

And I tried to repeat the experiments, and I didn't succeed in corroborating what he had described. It's been something I've been puzzling about to this day, whether this isn't something worth looking into. Think of charcoal as just a random ensemble of sites, and you could use competitive displacement on it or much more specific source of separations than we're doing today. That would have been ideologically connected with the issues of specificity and staining. That's what I was up to. I think during that time I learned something about colchicine, and I'm pretty sure I started that project there, and then continued it when I entered college. I was very interested in what the physiology of mitosis would be, and here was a very specific reagent, which seemed to do nothing else but disrupt mitosis. I just wanted to see if I could understand more about its physiology. We didn't know zilch about what the receptor for colchicine was; it wasn't until some time later that we knew about tubulin, which it specifically adsorbs on. It was a good idea, and how to make it applicable with the available technology is another story, but I started looking at other metabolic poisons and what they could do to mitosis. I tried to see if by using cyanide and urethane and fluoride and the range of metabolic inhibitors as was known up to that time, would you get some clue as to how this particular inhibitor was working or what was the dependence of mitosis on energy sources, things of that sort. There were a few very reputable scientists, as I soon found out, doing not too distant kinds of things. It never ended up being all that productive, but that's because you're dealing with very loosely coupled issues. If you interrupt energy sources, obviously the tractile mechanisms in mitosis are just one of thousands of things that's going to be hit. But it was a way to learn more about an interaction of known metabolic inhibitors with some unknown biological process that I was trying to get into.

BOHNING: You've mentioned Brachet's paper. So he was doing the work essentially the same time you were, but it was unknown because of the war.

LEDERBERG: Yes. That's right. I had some correspondence with him about that just four or five years ago. He died just about a year or so ago. I'm a little puzzled how he was able to continue to flourish and publish even though the work didn't get out. He was obviously not a member of the Resistance. I don't know what else was going on in Belgium. But there was a lot of, to me, startling sort of "life is normal" aspect about that during that occupation. If his name had been Lederberg, he wouldn't have been able to do that.

BOHNING: Yes. I think we've already talked about how you wanted to go to Cornell, or, at least, you also applied to Cornell. CCNY was also there for you.

LEDERBERG: It was a last resort.

BOHNING: Then you got the scholarship at Columbia.

LEDERBERG: It covered tuition, or most of tuition.

BOHNING: You were still living at home and commuting from Washington Heights. Started at Columbia in September of 1941. Had you spent any time on the Columbia campus before you arrived there as a student?

LEDERBERG: I might have seen it and more or less been outside. I knew it by reputation, but, as I mentioned in the other transcript, I was not aware that E. B. Wilson was no longer there. [laughter] But I'd known about Thomas Hunt Morgan and Wilson and believed, not totally inaccurately that it was a great center of biological research. But nobody advised me, nobody really knew zilch about the scientific capabilities there. Schools had reputations in those days that had much more to do with their football teams than anything else.

BOHNING: Sometimes they still do.

LEDERBERG: Well, at least there are other avenues of inquiry. But anyhow I don't know—yes, what I did not know was the feasibility of going to a good state university. It would have meant traveling out of state, I would have had to work in order to get money for board and room. And it was an option nobody mentioned to me. You know, maybe even out-of-state tuition even in those days would have been enough to have hindered it, but the main point is the vacuum of advice on those points. Not every kid was going to college in those days although I suppose most Stuyvesant graduates were expected to. But I don't recall ever hearing anything sensible from any advisor. When I compare that to what kids go through today, it's astonishing.

BOHNING: So Stuyvesant sort of left you on your own.

LEDERBERG: It seems so. I have a complete blank on that. They must have been some help to me in contacting Columbia to apply for the scholarship and so on. But I think they may have just decided that financial circumstances were sort of hopeless. I was going to go to the City College and if something else came along, well, okay. But what I resent is that nobody pointed out the land grant state universities. I went to teach at a great state university not too many years after that, and nobody ever mentioned that possibility to me. I mean, I may not have been able to make it, either. Well, Columbia worked out just fine. I'm very lucky to have gone there.

BOHNING: It's surprising in a way because a school like Stuyvesant, with the reputation it had, as you said, that most of the students were expected to be going on to college—that they weren't more vigorous in student advising.

LEDERBERG: Well, I do find that difficult to understand. I may be blocking out some history, but it's not the kind of thing I would expect to have forgotten. Kids are nurtured more carefully today, generally. When we talk about these age issues, it was less startling then than it would be now. Kids were expected to be on their own a lot more than today.

BOHNING: Did you go to the Admissions Office and take care of your application?

LEDERBERG: Well, it would have been by mail. I don't remember doing it personally. I just don't recall. I could have had someone do it.

BOHNING: So when you started in September of 1941, were you getting any advice then from Columbia faculty, or were you just thrown in as a standard freshman?

LEDERBERG: No. As soon as I was there, I was in a very different milieu. And I don't remember exactly whom I met first there. But within a week or two I'd met Barbara McClintock and talked to her about my paper on the nucleolus. She helped me understand it more deeply. I was very aggressive about quickly locating and ingratiating myself with all the talent that was there.

BOHNING: This was the paper you wrote in high school [on the nucleolus]?

LEDERBERG: Yes.

BOHNING: Was that published, or was just a paper you wrote based on what you were doing published?

LEDERBERG: Well, I'm trying to recall a little more. Oh, I know one of the first people I met was my zoo 1 instructor, H. Burr Steinbach, was one of the first courses I took. I'd started working on the nucleolus, but I wrote this paper during my first semester there, or at least another version of it. And it was during, for the preparation of that paper that I went to consult with McClintock . So I would meet the professors in my courses and I did have an advisor who was, I think, was originally a physicist called Robert Von Nardroff, whose brother was the previous principal at Stuyvesant. Then I had Fred Keller, the psychologist, who I've kept in some contact with. So I started getting very good advice as well as easy access to teachers and graduate assistants and things of that sort. So I more or less lived in that department from that time on, I had a wonderful time.

BOHNING: You had already been so advanced in what you had been doing up to this point. Did they start you out in regular introductory courses, or could you start much further down the line?

LEDERBERG: Well, we discussed that. I was able to place in quite a few of them. I did get some list. I think I have my curriculum summarized there. Yes, these are my first courses. Well, I started right off. I had—well, it was just as well; I didn't know the comparative anatomy and morphology that was there. And then went straight on to the next level course in embryology, morphogenesis and so on. This is the paper I just mentioned to you. And then by the next semester or during that year I was already into the graduate courses. The three digit courses were all graduate courses.

BOHNING: In my file I have copies of that correspondence with Stadola.

LEDERBERG: Yes. He was just a wonderfully nurturing person. I placed to an advance level in his course, and then completed it over that summer. And, well, you saw how encouraging he was. He died just about six months ago. I recently got a notice of it.

BOHNING: What about original research?

LEDERBERG: I was playing around with colchicine during that first year, and the only new finding I made was that there was a susceptibility gradient down the axis of the onion root tip. The most actively dividing cells right at the tip of the meristem were less susceptible than the ones behind it. And trying to make some sense out of that. But the phenomenon is real. You could find critical concentrations where you get the interrupted mitoses up to a certain level, and then they'd be normal below it. I never was able to straighten out whether that was differential absorption, which you could call the pharmacokinetics of it, or the intrinsic difference in the cells. I still don't know.

[END OF TAPE, SIDE 5]

LEDERBERG: I was intrigued by the fact that the crocus (Colchicuin) itself is not susceptible to colchicine, and there are species differences in it. I never got into it though. I was going to use that as a clue about how to understand this a little bit more deeply. The next year I met Francis Ryan; he was away that first year. I'd heard about him and people had spoken very admiringly of him and that he'd be someone I'd want to know when he got back, as was indeed the case. He came back in the fall of 1942 with Neurospora. He learned that the previous year with Beadle and Tatum. I camped on his doorstep. He had no choice but to let me come and work in his laboratory, and I was his disciple ever since. I just put away my other work in favor of learning what he had to offer and then started research on Neurospora.

BOHNING: Pearl Harbor occurred at the end of your first semester. How did you react to that? What was the reaction on the campus in general?

LEDERBERG: There was a sense of inevitability and a mixture of gloom and optimism. The gloom was that there was a pretty formidable opponent who had hit pretty hard at Pearl Harbor. It was not too soon to pitch in and rid the world of these pests. It ended up being not unrealistic. I think the level of sacrifice that Americans paid was about what was anticipated. I don't think we realize that it could have been a lot worse. In fact, eventually it was pretty harsh, I think. I can count the names of half a dozen people I knew who were killed in action. Excepting one school chum, none of them was very close to me. When you consider we had probably in our own armed forces nearly as many casualties in Vietnam as we did in World War II—I think that's right—we've become inured. But the main thing was, my God, can we clear the world of that menace some way or another? I was very young, and I didn't see what personal role I could play. In a couple of years I would be old enough to be drafted, and then I would do what my country told me to do. I didn't think it would be an efficient use of me to make a combat infantryman out of me. I doubt if they would have—just because of my own combination of physical and mental capabilities. But when the opportunity came along, I did enlist in the Navy and let the Navy decide what to do, but had an opportunity to continue my skills in education. If the war was going to go on long enough, I would be able to use those at a much higher level for what value I could contribute. What I saw was the national interest and my own converged, absolutely, and I didn't hesitate for a second as soon as I heard about that program, a little bit to my parents' consternation. They thought I was rushing myself, getting signed up literally a year and maybe almost two years before I was vulnerable to being drafted.

BOHNING: You were still, sixteen, seventeen?

LEDERBERG: You had to be seventeen to actually sign up, so I think it was on my seventeenth birthday. It worked out just as well by every account. I still pay my dues. I'm spending this Friday at the CNO's Executive Panel, briefing them again two weeks from now. [laughter]

BOHNING: Let's just explore that a little more. What were you thinking at that time?

LEDERBERG: I was a premed and thought I was going to go into medical research. Until I got deeply involved with Francis, I would have thought neurology was the medical discipline that had the flavor and the tastes that both in practice and research would be at the frontier of basic biology and would count the most. I probably didn't understand that many specialists viewed it as the most futile or dismal of specialties; it was probably the area where you can do the least for your patients. But that's still a challenge. So I was signed up as a premed. I was accepted into P & S [Columbia College of Physicians and Surgeons] fairly early. (I'll have to get the date on that.) They had sort of an advanced acceptance list and there was an accelerated program where most of the work got started. I was in the V-12 program as a prospective medical officer, and I'm sure they even had the name of the ship I would eventually be assigned to as part of their manpower alignments. I got into uniform on July 1, 1943, which was just past my eighteenth birthday, but I had signed up when I was seventeen, before that.

Life more or less continued, except I was in uniform and now living on campus. I didn't have to commute any more. I just lived in the dormitories and was actually getting paid to go to school; it was quite a bonanza. I understood the necessity for drill and a little bit of military discipline and I didn't mind it. Some of my classmates would bitch about it, but it didn't seem in any way unreasonable to me. They had very objective standards. You got into V-12 if you passed your exams and maintained your grade, and if you got down to a "C" you would flunk out and would just join the ranks of other naval services. So there were pretty high incentives for sustaining academic performance, but it didn't bother me at all. It probably meant a better academic morale among my classmates. If this had been peacetime there might have been the usual conflict between the nerds and jocks which we were somewhat spared. The other consequence, though, was that I did not have an uninterrupted college life, and every semester there was an issue—I had a fixed date for entering medical school, but how was I to spend my time before then? Optimizing my general education was not the Navy's objective. Their objective was the minimum amount of time to meet the formal requirements, and any other time was to be spent on other active service. So I did end up with everyone else spending the best part of a year, but in blocks of four months at a time, working as a hospital corpsman in the naval hospital. It was just interdigitated with my assignments to complete my premed.

BOHNING: Was that here in New York?

LEDERBERG: It ended up being at St. Albans (Long Island). It could have ended up being anywhere. It was just the luck of the draw, but that's where they decided to assign me. I was assigned, and it could have been anything. A lot of the V-12s were put into the clinical labs because they had some background for it. I ended up in the clinical pathology lab. Captain Sheldon Jacobson had been a reserve medical officer. I guess a would-be sailor, and for him it was something of a life's dream that he actually had a command and call to active duty. I had a very good relationship with him; all of us did. He used us, but at the same time he thought he would help us continue our education in the lab. I got the parasitology assignment, so my job was to do the blood smears and fecal floats and so on, looking for parasites. We had most of the Third Marine Division. There were some other units back from Guadalcanal, and two-thirds of them had malaria. I had to do the slides that would monitor the course of their treatment and whether they had P. vivax or P. falciparum. I got to learn a lot about malaria. I've probably seen about as much of it as anybody in that setting. [laughter] Actually peering at it through the microscope all day long, I became very familiar with its life cycle. I thought about its cytology, its cytochemistry. I was probably the first person to try doing Feulgen stains to see that they're active chromosomes that had DNA in them, Plasmodium and so on. So I really did get some intellectual benefit, and I was imbued with the idea of a microbe having a sexual cycle, which certainly spilled over to when I thought about bacteria later on.

BOHNING: Was that the first time that you had reached that point, that thinking?

LEDERBERG: Not quite, because the same was true of Neurospora, but this is something that you think of being a little closer to bacteria than this fungus that's got these macroscopic threads. Malaria is a microscopic microorganism. Nevertheless, as tiny as it is, you can tell very directly it's got a sexual cycle. But mostly by the accident of having followed it, both in the mosquito and the human host. I had other life experiences. I was on the morgue watch. I knew that I'd have to deal with cadavers *in extenso* when I got to medical school, and I had my fill of them at the hospital. That meant if a patient died during the night and I was on call, I had to get up and help with the movement of the remains and help a little bit in setting it up for autopsy. I was already eighteen by then. Also I was very impressed by the attitudes of the other sailors. The other sailors who were going to ship out were very resentful of us. The marines who came back were not at all. They said, "You guys are lucky you didn't have to face it, but bless you." We took the best care we could of them, but it was a very sharp contrast. We were really persecuted by the seamen second class [laughter] that we had to deal with. Well, you can understand it. But the latter was unexpected, that the returning marines would take that line, that view of it. It was very generally true. I did get a very close sense of the war. We heard lots of war stories about what they'd been up to.

BOHNING: That must have made quite an impression on you, hearing their experiences at the front.

LEDERBERG: Well, it can't be anything like being there yourself, but I certainly had a sense that they'd made a lot of sacrifices for my benefit. Not resenting it made it all the more poignant. It may be a very sentimental attitude, but I've never forgotten that. I've had assignments where I could be of some particular help to the Marines, and I've never forgotten it. I've gotten to know P. X. Kelly pretty well; he was the commandant for a while.

BOHNING: Let's go back to Ryan. You said that he'd been on leave and when he came back you camped on his doorstep. Could you tell me a little bit more about your experience with him? What kind of a person was he?

LEDERBERG: I first have to say some things about age. I looked up to him, very literally, as a father figure. In retrospect, when I see pictures of us together, he looks more like an older brother. He may have been eight or nine years older than me, something like that. Not much more. He was also a very bright, precocious fellow. He did his undergraduate work at Fordham and got his Ph.D. in pretty quick time. He was a year out of his Ph.D. when I met him. He went to Stanford for his immediate postdoctoral experience. For someone as young as he was at that time—I'm speaking now in retrospect—he had a very paternal attitude, philosophical, nurturing. He was remarkably uncompetitive and just one of the most marvelous teachers that I've ever encountered. Everybody who knew him subscribed to that. He would not instruct you, he would draw things out of you. He had a wonderful Socratic method in how he dealt with that. I think I was an intellectual challenge to him. I may have been pretty trying to him at times, but there was certainly a bond of affection as well. I adored him. I enjoyed very much any occasion for some kind of intellectual sparring, and those were numerous. I think I gave him something, too, as young as I was. He understood one of the first things I needed was some more discipline in how I organized my work, handled myself in my lab, kept my notebooks, a little bit about being more systematic in my thinking, more focused. He helped in defining a strategic approach to deciding what you're going to work on. I owe all those things to him. He was able to get a very small grant—I think from the Rockefeller Foundation—for his Neurospora work, and he hired me as a helper to do that. I did everything. I would recover used agar for him. There were great shortages in those days. After he finished an experiment and it came out of the autoclave, I would filter it and coagulate it and purify it, and prepare fresh batches of agar for him. [laughter] Pouring plates, inoculating the colonies, all that kind of stuff. I assisted him in his work, gradually getting more and more into the genetics of it. He was more of a physiologist than a geneticist; and he was sort of veering over a little further. He did teach me what Beadle and Tatum had to offer. I don't know why I would say I came with a strong genetic impetus, but that's the way our experiments went. His own work was on factors that regulate the growth of Neurospora and its nutrition and using it for setting up assays for different vitamins and amino acids.

I wanted to know more about mutation and things of that sort. So that's where it went. I don't remember exactly which came first, and I'd have to check my notes on this, but I first met him in September of 1942. I spent 1943 sort of half at Columbia, half at St. Albans. Ditto for the first half of 1944. I entered medical school in either July or October of 1944; I'll check my notes on that. But I continued to live downtown. I no longer had access to the barracks, but as a medical student you could get a housing allowance. I did get an apartment with a graduate student, Kim Atwood, in the neighborhood, so I could spend a large part of my time, even when I was in medical school, in his lab. In February of 1944, [Oswald T.] Avery's paper came out (18) and I got very excited about that. I suggested to Francis to try and do the same experiment on Neurospora. I'd been helping him working a mutant, which required leucine, so I said, "Let's try to transform the leucine negative gene into a positive." If we could get transformation with DNA, or whatever it was in Neurospora, there would be no doubt that we're talking about gene transfer. We'd have big arguments about whether the pneumococcus transformation was really definable in biological terms. But in the course of those experiments, the controls showed reversions and so we were really never able to use it very cleanly. We did have some experiments where we had some crude extracts, which we hoped included DNA. But even before that got very far along, it was plain that the controls reverted, and you couldn't really have a reliable way to test for the transformation.

Amazingly, that was the new finding and in retrospect it's hard to believe that. The idea of gene reversion was not the expected phenomenon. So Francis said, "Okay, why don't you study this phenomenon for now as your own special project." And so I did, looking a little bit at the dynamics of where these reversions occurred and then verifying that they really were reverse mutations that you could localize where the gene was. Was it the same gene that had mutated? That meant a lot of genetic crosses on the one hand. A puzzle that still hasn't been solved, is that if you apply a modest amount of leucine to the medium, you seem to suppress the wild type and that obscured the dynamics of when the mutations would be observable. We were able to show it wasn't the initial mutation so much as what happened in mixtures of leu+ and leu-. These are in heterokaryons, these are continuous filaments that have mixtures of nuclei of the two kinds. They can move freely throughout common cytoplasm. It looks as if leu+ is at a disadvantage compared to leu- as long as there's some leucine in the medium to allow the leu- to proliferate. I still don't understand why. It's a real paradox because if you do a growth tube to measure the greater progression as an estimate of growth down a long tube, you inoculate one end of it and it grows through the agar. If you have full concentration of leucine and you start out with a mixed inoculum, it grows from beginning to end. If you have a minimal media without leucine, it starts somewhat fitfully and then it grows from beginning to end, only what's at the end is only the leucine+. If you have intermediate concentrations, it'll grow up to a certain point and then stop and when you sample what's at the stop; it's all pure leucine-less. So it's almost suicidal from the point of view of the complex; by killing off the leu+—or diluting them out with nuclei—you end up with a final product that's unable to grow further. That's bizarre. No further progress has been made on that since 1946.

That was the culmination of that experiment, but I still felt very frustrated that we had not been able to do more about Avery's finding. So I said, "If we can't transform Neurospora, maybe we can do genetics with bacteria after all, and in that way bring the Avery phenomenon and bacteria into the mainstream." By this time, having had a year of experience in using selective methodologies to pull out whatever genotype you want, that's when the germ of the idea arose about using a mixture of two auxotrophs, selecting for a prototroph and using that as an index of whatever recombination could take place, and deciding to apply that to bacteria. I think I have some notes someplace. The most tangible note I can find is some scribbles in my class notes in bacteriology class which are essentially the design of that experiment. That would have been the summer of 1945. So at Morningside Heights I started doing that kind of experiment with another strain of E. coli, and the rest is history. I've pretty well written all that down (19). I wish I could recall my discussions with Francis about doing this experiment and going on further, but I'm afraid I just have no reliable recollection of it. We certainly had intense dialogues about it.

BOHNING: What kind of a group did he have working for him? Was it a big group?

LEDERBERG: No, there were two, three, four other people in the lab. Lillian Schneider was his mainstay, a research technician who was with him for many, many years. On and off his wife Elizabeth worked in the lab. I just talked to her the other day. She still lives in the area. There were one or two other students who came in and out; I'd have to scratch to remember who they were. There were some very distinguished people who were there at some time after I left. I don't recall who they were just at the time I was there.

[END OF TAPE, SIDE 6]

LEDERBERG: He had a contract of OSRD [Office of Scientific Research and Development] to study the nutrition of Clostridium Perfringens and some of the other wound-infecting anaerobic bacteria. And he had somebody working for him on that project. That was aimed at developing therapeutic management of those infections.

BOHNING: You were still on the Hayden fellowship?

LEDERBERG: Oh, no. That was until I got to V-12.

BOHNING: Oh, I'm sorry. So V-12 was paying the way from there on. You said you had intense discussions with him, or you're sure you did, but you don't recall the nature of them. Was he there a lot?

LEDERBERG: Oh, yes.

BOHNING: Was he in the lab all the time or was he a person who wondered through once in a while?

LEDERBERG: Not at all.

BOHNING: So he was intensely involved.

LEDERBERG: He had a great zest for doing the experiments himself. I suspect that about that time that, I was doing most of the Neurospora experiments, either ones on my own or at his behest. And he, with his own hands, was mostly working on Clostridia. I didn't do any of that. He had a full-time teaching load, so he wasn't there all those hours.

BOHNING: I guess in those days, financial support wasn't all that great, was it?

LEDERBERG: I should say. Well, there were the particular constraints that if it wasn't war work then what was your excuse for doing it, and so on. So I was sort of smuggled in. Strangely enough, the Navy was paying for work on Neurospora [laughter] and recombination, and OSRD was supporting this other stuff.

BOHNING: Did your group interact—I'm just trying to get a feel for the middle of the war. I'm sure a lot of people were being drafted. What was happening with the other research groups within biology and what kind of interactions were there?

LEDERBERG: A lot of women came in as graduate students during that time to fill in. The university was being turned to a variety of other projects. I had a faint inkling that the Manhattan Project was nuclear energy, and to my mind, it was confirmed when I bumped into Harold Urey going down the stairs one day. [laughter] It couldn't be anything else from my point of view. But that was just a surmise. They called it the metallurgical project, as you may recall. They were training large numbers. Columbia was taken over by the Navy pretty much, V-5, V-7, midshipmen and so on. And then there was still a trickle of undergraduates. It was still a boys' school, so as far as undergraduates were concerned, either it was the V-12s in uniform or a very small number of kids who could get deferments for one reason or another. A few were able to get deferments as premeds. If they're going to go straight to medical schools, then they could get deferred to do that. They weren't drafting anybody out of medical school at that stage and apparently that was certainly wise. They were then committed to a term of service afterwards, which the services decided unilaterally to turn off. They didn't even want the reserves. Demobilization was pretty much complete in 1945. Anyhow, these experiments began during that summer [1945] and it was pretty momentous as far as the way the war was going. Then in November, I thought service was still going to continue, and that there would be a post-war Navy, and I would still have to continue with my service obligations. Then rather suddenly, actually with painfully short notice, they decided to demobilize us in November. They didn't need us anymore for anything, and they weren't going to pay us anymore. We really had to scramble to try and figure our where tuition was going to come from and so on. For some students, there was some GI Bill availability, but it was quite limited. If your only service was in training, I think you had zero or very low eligibility for that. So it was quite a sudden turnaround.

BOHNING: What kind of interactions did you have with the chemistry department? Did you have any?

LEDERBERG: I took a number of courses there. I remember Professor [Charles O.] Beckmann. I had a minor in chemistry; I think that's the way it was listed. I took a course with Louis Hammett. I did some physics. I had Willis Lamb as my instructor, studying radio, as a matter of fact, electronics. [laughter]

BOHNING: Well, I notice something here from the Institute of Radio Engineers, I don't know what the date on it is.

LEDERBERG: Oh, that's a different strand. That's my NASA connection, Lloyd Berkner was the president of what was then the IRE [later IEEE] and asked me to sign up.

LEDERBERG: I took a sort of beginning graduate courses in physics. I had one course in theoretical physics.

BOHNING: Were you aware of the Manhattan Project, the work that was going on by Urey and his group?

LEDERBERG: I only guessed that it was nuclear energy. There was this mysterious classified project called metallurgy. I couldn't imagine what else he'd be doing if it weren't just that. That was my only inkling of it. I now realize Leo Szilard was on campus at the same time; I got to know him very well later, but I can't remember ever talking to him then. There were large blocks of several buildings that were sealed off.

BOHNING: They were working in Havemeyer.

LEDERBERG: Also in Schermerhorn. There were several areas that were closely guarded.

BOHNING: Where would you have been?

LEDERBERG: Schermerhorn is where zoology was.

BOHNING: Okay. Where's that, just for my own information, where's that in relationship to Havemeyer.

LEDERBERG: Well, it's just across campus. They have Havemeyer on the west side, and Schermerhorn on the east side.

BOHNING: All right.

LEDERBERG: I think it was during that summer that I suppose we were anticipating some kind of leave or vacation period. The school had been going non-stop throughout the war. And Francis suggested that maybe I'd want to pursue this experiment with Ed Tatum. Ed was coming to Yale from Stanford. I was told later by Johnny Moore that one of Francis' motives was to find me a patron who would be more potent in the establishment than he himself was. He thought being an Irish Catholic from New York was sort of next worse to being a Jew from New York, and that we needed something more powerful to be listened to. I was not aware that it was a consideration at the time, but Johnny was quite firm in saying that that was one of the things that Francis that Francis had in mind about sending me to Ed. I didn't realize then how low on the totem pole Francis was. I mean, he was my god, but he was an instructor and then an assistant professor. And I was at the point of announcing some icon-breaking matters; there's probably some merit that that might not come so easily without a recognized sponsor, but I didn't know that. Anyhow, I, at Francis' suggestion, I wrote to Tatum, outlined the experiment that I had in mind to do—and that's all on the record—asked if there would be an occasion for me to visit his lab. Francis had smoothed the way for that with him, and Ed arranged a fellowship from the Child's Fund to do that. I'd gotten up to a certain point, but I needed a wider variety of mutant strains and I was starting to make more, but Ed already had a library of mutants that he'd already derived. And I was happy to have a chance to be in a different setting, and so it looked like it would all be a good idea. So we did arrange it. History permitted it, and

I was able to get there. It was in March of 1946 that I actually arrived in New Haven and got started.

BOHNING: Originally you were still planning to go to medical school.

LEDERBERG: Yes. I was actually still in medical school. I was registered for external research, and my standing at Yale was as a Columbia medical student, as a guest at Tatum's laboratory. And the thought was I would do that for the spring quarter or semester, presumably spend the summer there with my vacation time as well, and then reenter the regular annual cycle. I wouldn't have slipped a class, I mean, just my regular class, the following fall. So that was the game plan. Now, I don't know how I thought I was going to finance it, but I imagine I had some scholarships coming, that were available at the school and had some savings. I think I earned fifty dollars a month in the Navy, and somehow or another was going to make it. This job also had the advantage that I could hope to save a little bit from that and help out the following fall again, too.

BOHNING: When did you realize you wouldn't be going back?

LEDERBERG: Not for some time. The experiments worked out very quickly that spring, and I recounted that in detail (19): I won't repeat it again. And so during the summer, wrote and asked for an extension. After the summer I was on leave from Columbia and Ed arranged for an extension of my fellowship from the Child's Fund to enable that. I thought after a year of that I would go back to medical school and was planning to pretty much to the end until Ed said, "Maybe you want to consider an alternative." That was the job at Wisconsin that had opened up, and he had come recently from Wisconsin, so he felt pretty close to it. And did look into it, and with a lot of self-examination decided that that probably was the better thing to doing research was what I really wanted, I could pursue that better by not interrupting the work that I was doing. I still felt quite a wrench about being disconnected from medicine. Madison didn't offer that kind of an opportunity; it was in the Ag school. That was an important negative consideration. I ended up repairing it. I had no anticipation that I would be able to, but in 1955, seven or eight years later, I did start a department in the medical school at Wisconsin. So that's how it worked out. I had to register retroactively as a Yale student for that year when I was really on leave and had to fork up tuition for that—I remember that very vividly. The professors all signed up and said, "Josh was at all the seminars and lectures." I had already done the work for the dissertation, so we patched up a Yale Ph.D. out of the experience.

BOHNING: What kind of a group did Tatum have, and did you interact with them very much?

LEDERBERG: Oh, yes. There were about half a dozen people there: Ed Adleberg; a fellow called Sheldon Reaume; Mrs. Fruton—Topsie; (Sophia Simmons) was in the lab there with him; Polly Bunting. Her husband had died the year before, she was widowed quite young. She went back to work; she'd been a microbiologist, done some work in bacterial variation in Senatia. So she was in the lab then. She later became the president at Radcliffe, started the Bunting Center; I'm sure you've heard of her name in other connections. That was much later on. There were one or two other students. They overlapped different parts of my time there. Charlie Yanofsky came in I think the very end of my time or shortly thereafter. Ralph Lewen, likewise. Those are the main names I remember. Mrs. Tatum, June, worked very actively in the lab then. He was sort of just getting underway. There was a fellow named Ray Barrett. It was almost entirely a Neurospora lab; I was the only one working on E. coli. Carl Beam. Ed never really pushed very hard himself with that. He'd been a bacteriologist. The way I reconstruct it, he had two medical students at Stanford, C. H. Gray and Sara Anderson respectively that he sort of gave the job of looking for mutants, and that was the beginning of that collection. He didn't talk much about it, didn't seem personally to be that deeply involved. He liked the idea that bacteria might do some of the same things. But he was much more of a biochemist than a geneticist, if you look at the detail of the work he was doing. He could do all the things he wanted to do very well with Neurospora. He really enjoyed it—he loved that organism. So that was the division of labor.

BOHNING: When did you know you had the final result?

LEDERBERG: I've documented that here. I don't remember the dates; I've looked them up. It was pretty early: I spent a lot of time on the controls. I didn't dare do an experiment until I was sure the controls were clean. The last thing in the world I've ever wanted was to have an exciting, provocative result where I would then still be uncertain. So I much prefer to clean up first; I'm willing to wait. Now, there's probative kind of work. If it's not that important and you're trying to figure out what's the best way to do something, I'll do exactly the opposite—I'll do quick and dirty. But when I have what I believe is a critical experiment, I'm sort of scared to do it until I know it's right. I don't want to be caught either with an unwonted disappointment. I don't like the disappointments, but the unwonted is that I hadn't thought of some variable that I should have had right at the very beginning and have to scramble later in order to rescue the experiment. Even worse is when it's contaminated by a misleading result and have to be nagged by the idea maybe it's true, maybe it isn't. I find that intolerable, and I work very hard before hand to keep it from happening.

Well, I did that here. I spent two months on the controls and then, whammo, the very first experiment with mixed cultures, it was such a clear result we didn't really need the controls. But it's just as well. That was in late May or early June. It was a month or some weeks before the Cold Spring Harbor symposium. It seemed like a very short time, but I must have done a dozen repetitions with different strains and different markers in that month. I had no doubt that there was a phenomenon, and we already knew a lot about it by the time July rolled around. Ed was already on the program. A number of graduate students, including myself, were welcome to attend. Even after we were there it was problematical whether we were going to say any more about it. But when a number of other people were saying either categorically how awful it was that there was no sex in bacteria, or, that there are some hints that maybe there's this or maybe there's that, we thought we shouldn't hold back anymore. And Ed asked [Milislav] Demerec if he could just assign a special interval and we did find some time and presented it. There was a long debate afterwards. I've been trying to get some reconstruction of it without very much success. I've circularized everybody now extant who was there, and had just one or two replies. Nobody kept any notes. Nobody can even tell me exactly what day it was in that meeting. [laughter] And oddly enough I don't have that record. So I can span it to within an interval of four days. The American Philosophical Society has Demerec's records. They do not include the 1946 symposium. It's a mystery what happened to that particular file. I've wanted it for another reason, namely on the reception of Avery. And it's been a tall point in my argument on that, that far from being neglected, he was invited to present at this symposium. I would like to get documentation about who suggested it, the wording for the letter of invitation and so on. I know that in the actual event, Mac McCarty came instead of Avery, but there was a paper on the pneumococcal transformation. You can't sensibly argue in my view that nobody was paying any attention to it. But I would have liked some deeper documentation on that point. That's another reason I lament the absence of that particular file.

BOHNING: What's your sense—since you have very little feedback—what's your sense of the reaction and the discussion?

LEDERBERG: Oh, it was a wonderful opportunity. I mean, I recall the debate pretty well. Andre Lwoff kept asking, "How do you know it isn't just a mixed culture?" He had worked on syntrophic interactions. I said, "I've thought about that, and if it was a mixed culture, then it was a mixture that just didn't know how to separate." I had indicators like lactose fermentations, so I could spot white and black colonies on EBM media; I didn't have to pick them one at a time, and they were completely homogeneous. I also had selective markers, I had a phage resistant and a phage sensitive pair, and they would segregate out. Some of the prototrophs were all sensitive, no resistant residuals from the supposed presence of one of the components; others would be pure resistant. I couldn't be as confident about that homogeneity. I could certify those to ninety-nine and nine tenths percent. The ones that are totally sensitive I could certify to five decimal places because I could pick up tiny residuals of resistant organisms. I'd verified that by making mixed cultures. I felt a little put out that one, they'd think that I hadn't thought of it, and two, that even after I presented what I thought were very meticulous experiments to answer the point, that they didn't seem to listen. [laughter] But most people did accept it very promptly.

My view has been informed by thinking about this with Harriet Zuckerman, about the social dynamics of that. It was a rare opportunity to be able to make that presentation to the group of movers and shakers and within the discipline of an organized conference. They were really on their honor to complain or keep your peace, and you don't always have that opportunity. I could have had all kind of sniping and resistance unfocused, if it hadn't been for the occasion to bring it all out. So it was a wonderful confrontation. It was like Pasteur's meetings that were organized by the French academy. I didn't appreciate the importance of that at the time. I mean, I was glad to have the opportunity, but it never occurred to me, "What if that hadn't been there?" Maybe if I had just dropped this in the hopper and it had been published in one of the routine journals, and a lot of people would have thought of all kinds of reasons not to believe in it, not to have the kind of confrontation that this reflected. Max Delbruck didn't want to believe it and sort of held out for a long time. I repeatedly begged him to give some arguments. "What's wrong with it? How could these experiments go wrong?" He actually said something very wise, but it was done in a sufficiently abrasive way that I couldn't see through his resistance to where there was some good advice in it. He said, "Don't bother me with it. Until you've worked out the kinetics it doesn't mean anything." A biophysicist would do it those terms. Well, by kinetics I thought he meant the yield as a function of the concentration of the inputs. I had done those experiments and, yes, it's a bimolecular reaction. Kinetics could have meant the time course of a mixture with interruption, and that's the experiment that [François] Jacob and [Elie] Wollman did. Of course, that was a very important contribution to understanding it. It never got resolved on any intellectual ground. That's the only significant resistance of which I'm aware. Most people who were there and were able to experience the debate and the argument adopted it. [Salvador] Luria was very, very positive about it. He was probably the main person who would have had reason to have an opinion on the matter at the time.

BOHNING: Did you realize the importance and the magnitude of the importance of what you'd done and the effect it was going to have?

LEDERBERG: Yes, I think so. I mean, look, the purpose of the experiment was to bring bacteria into the mainstream. Behind that was to bring DNA into genetics. Yes, this was the master molecule that was going to be available for further experimentation. I can say that without qualm.

[END OF TAPE, SIDE 7]

LEDERBERG: I was also quite confident that it would have practical applications in medicine. I didn't dream that there was going to be a biotechnology industry with all these startups and the Wall Street involvement in it. I thought it would be incorporated into what the existing drug companies could do and become part of the mainstream of their research and, yes, it would be profitable and enhance what pharmaceuticals could do, and so on. I had no idea it would become the entrepreneurial game that it's become. It didn't need to. If the big firms had been awake on their watch, they would have assimilated it twenty years earlier and not necessitated the neoplasia that we've seen.

BOHNING: Why do you suppose they were asleep?

LEDERBERG: It's the problem of organized large-scale research and getting so caught up in the dynamics of what you've been doing that it's hard to make room for anything that's more novel. To some extent it's that your managers are the scientists of twenty years earlier, and it's a little hard for them to wake up to real innovation. Those are the two main factors. There's a little bit of the dynamics of doing research that requires more than a nine-to-five mentality, and there the incentive systems, for what the entrepreneurs can make out of it, does start to play some role. Why should the people in a large organization exert themselves? The usual problems of bureaucracy can be folded into it, and the entrepreneurs are the counter-bureaucracy. It wouldn't have to be that way, but it takes, more enlightenment than exists in those ranks to do a better job. They have their successes, as well as failures. The development of antibiotics was what I had as a paradigm. This was a major advance in medicine that was taken up by the big firms. It didn't have a lot of entrepreneurial colonization as the way this has happened. It has worked out differently.

BOHNING: I don't know what your time constraints are. It is three o'clock.

LEDERBERG: If there's a natural ending point sometime soon, I've just got a pile of paper on my desk that I've got to take care of.

BOHNING: Well, this might be a place—we've got you to Cold Spring Harbor and that meeting.

LEDERBERG: Why don't I finish up till I get to Wisconsin? We could do that in the next few minutes.

BOHNING: All right.

LEDERBERG: It's actually a fairly short story because the original discovery that there was such a thing as recombination formed a set of next generation questions: what would they be? What are the interacting units? Well, they're cells. You don't get activity from filtrates. I got nowhere trying to get transformation from extracts. It took a long time before E. coli would work that way. [Andre F.] Boivin caused a whole flurry when he talked about DNA transformation in E. coli, but that didn't pan out. I guess Bernie Davis did what he called a bundling board experiment, where he had a filter separating the two cultures and they communicate nothing through a filter, and in contrast to what you get with vial transductions which came out a couple years later on. Adding deoxyribonuclease to the medium does not interfere with the process of genetic recombination. So the presumption is DNA is being exchanged, but in a way that's protected from the external medium and requires intact cells. Two, are there mating types? And our first answer was no. Later on we discovered there were mutants that showed that there could be, but a least our own bank of cultures were promiscuous. Three, how many markers could be involved? It was an indefinite number. We kept throwing markers into the strains, getting multiple mutants and showing you could get all the combinations imaginable, but at different frequencies. Okay, can you make a genetic map? The answer is yes; there are constraints because of the need to impose selection on the progeny. You don't have an unbiased recovery of all the progeny, you can only get the ones that already recombine on the markers that you're selecting on. It seemed to work out pretty well; up to a certain point it did. It's consistent with what we got later on.

So those are the main findings for that year. The first linkage maps and the range of markers and then getting some excitement about what might now be done with genetic analysis of those gray markers. I glommed onto lactose very early. I thought it would be a very good paradigm for a gene-controlled enzyme; that way we could do detailed genetics with it. It's worked out that way. Sort of use E. coli the way one had done before with Neurospora. I wrote up those papers, used them for my doctoral dissertation, spent a summer at Woods Hole doing it and reading all of the antiquarian literature. I had seen some of it before then, mostly from Dubos' book (*The Bacterial Cell*), and I got a good sense of what the history of the subject was at the same time. I've written a little bit about that; I don't know if you've seen that (20). The last issue of had a little paper on that. I'll dig that out for you.

There's very little biographical information. This paper has an interesting history because I gave it at the Pasteur centennial in 1988. It was the keynote paper, and they were going to publish it in a book. But they ran out of money and they only told me about six months ago they weren't going to publish it after all. I sort of wanted it to go someplace and tried to figure out where it ought to go. I think it ended up in just the right place. It's the news bulletin of the ASM, but they truncated the bibliography.

BOHNING: Oh, that's what the separate is?

LEDERBERG: Yes. If they'd looked at the page layout, they would have seen there really was room to add quite a bit more, but they—anyhow I've had to restore my original full bibliography to it. It reads a little bit awkwardly, but that's because of the history. I few places are off. That gives all the sort of prehistory of that subject. It was really quite late that summer that the question of Madison came up. It was really late in August that I actually made my first trip to the university. It was my first airplane ride. [laughter]

BOHNING: You hadn't really traveled that much up to that point, had you?

LEDERBERG: No, New York to New Haven. Well, Israel when I was a little kid. And that worked out, so we really quite hastily changed our plans. Oh, I'd gotten married in the meantime—that's that "we". Okay?

BOHNING: Yes. I think that's a good point to break.

[END OF TAPE, SIDE 8]

[END OF INTERVIEW]

INTERVIEWEE: Joshua Lederberg

INTERVIEWER: James J. Bohning

LOCATION: Rockefeller University

DATE: 9 December 1992

LEDERBERG: If there is a definite historical structure you're trying to fit, that's fine, but I sent you a little note about thematic issues.

BOHNING: Yes, I have it right here.

LEDERBERG: I had a couple of others that I've just been thinking about and was talking to some of the people about other matters, but they sort of reflected back on this. This is not very carefully structured. [laughter] I think I've seen this before, but in any biographical inquiry you have the tension between looking at your subject in terms of his uniqueness and the other in terms of how he's an example of the genre. I guess those always fight, one versus the other. But I think it's an interesting question to ask about almost any stage. Then I think we all live out some kind of a script. We change it from time to time, we look back and discover it didn't always work out the way we thought, or we were working through a different script than we thought, but, what was the script? That's a life model kind of issue. I think we've already mentioned we're trying to focus on what were major decision points, which is sort of another way of looking at the last previous question.

Then quite apart from my personal history, there's the evolution of the science in which I was embedded, and how that was moving and what was the perceptual framework. Not necessarily focussing too narrowly on what my own contributions were, there was a lot else very interesting going on that I was both an observer and a participant. The take home message is an issue of philosophy. What is it that in describing a life you're trying to communicate to others besides some matter of ego presentation or portraiture. I guess as much as anybody I've lived a life of, in, and about science and then tried to apply that mentality in a wide variety of other contexts as well. That's part of a life model in a sense, but how do the particular things that you were doing or describing at any moment bear on that issue. Then there were a bunch of other sets of circumstances. These sort of go together, and they have to do with how to relate to others in the scientific environment, which is primarily your work life. What was the laboratory environment? Who you were dealing with? I've talked a lot about my mentors. I might want to say a little bit more about those for whom I've played that role in turn. Then there are the various gates and the gatekeepers that you encountered at various places—how they structure your interests and your opportunities, issues of publication, granting, getting positions and so forth. There's another kind of bottom line here, but the take home message there is to the world, and this is a take home message about people in your own immediate arena, what can you draw from your own life that could be of some use to them.

The next is the philosophy of science, philosophy of discovery—a much more abstract question than some of the others. Then there are issues of styles of scientific work and the question of risk taking. While that's not the only one, it's one that's dominated the kinds of polymorphous perverse enterprises that I've been in. This is one aspect of a longitudinal enquiry, but there is an evolution of role, in that starting as an undergraduate and as a graduate student, totally immersed, personally doing experiments that more and more you're at the first and second and third remove. And I guess I've gone the whole way in going full time into administration and then back again, but it's not a sharp demarcation. I think a lot of people don't understand the extent to which working scientists don't spend a very large part of their time actually at the bench. People ask me if am I now back in the laboratory, and my honest remark has to be, "Yes, no less than any of my other colleagues as professors at the university." But I have a little bit of a twinge that it's not a completely honest statement to say I'm in the lab. I'm in here talking to you. [laughter] I do visit my lab from time to time, but I spend most of my time interacting with what's going on actually right here and of course relating to the literature, as well equally important. But there is an evolutionary developmental aspect of that detail of that work involved as a scientist. I didn't know what other things like that you've encountered, in the long experience that you have.

BOHNING: You've actually outlined it very well, and I think this version is an extension of what you sent to me early. Those are exactly the kind of things that I keep looking at when I'm talking to people. We've touched on a little bit of that; we're now reaching a point where I think we want to explore some of those issues in more depth. I think it might be easier to do it chronologically.

LEDERBERG: Episodically, at any rate. It just occurred to me as we were both speaking at once that perhaps the point of leaving New Haven is an episode of which to just go through these themes and see if there anything more you'd want to bring out from that interval before we leave it that might reflect on each of those issues. I suspect that one way or another they've been covered, but I think it's not a bad heuristic to do that at different stages. So I agree with the chronological framing of it. What did you have in mind to go on with now?

BOHNING: I had some pre-Wisconsin questions.

LEDERBERG: Let's make sure we cover all that.

BOHNING: As I said, unfortunately, for some reason my notes did make the trip with me. At least I can't find them where they should be, so I'm going to have to rely on my less than successful memory.

LEDERBERG: After I had done all the work, and it was in the context of an alternative to my going back to medical school, which would have mooted the issue of a Ph.D., that he thought I would have more flexibility, which was of course true. So we arranged for de facto retroactive registration and the big stumbling point was paying the fees, which I had to cough up. It wasn't easy, but we managed to do that and the paper that I was drafting for publication was accepted in fact as the dissertation, plus another twenty or thirty pages of general commentary. That has become much more routine; in those days a dissertation used to be thought of as being a completely independent manuscript. I think Yale was just then transitioning to the idea of accepting other published work as legitimate dissertation material.

BOHNING: Tatum obviously was doing all of this for you. Do you think there was any objection on anybody's part?

LEDERBERG: I don't think so, but the work stood for itself. I think it was recognized there quite promptly that it really was pretty important. And it had Tatum's imprimatur.

BOHNING: I noticed in your publication list that you indicate your Ph.D. thesis as being some forty-five pages long.

LEDERBERG: There is some additional commentary, mostly on some of the mathematical aspects of calculating linkage maps. But there's really nothing substantial that hadn't been published in the other papers.

BOHNING: We didn't say anything about that summer at Woods Hole. Basically you were finishing writing up when you were down there. That was the summer of 1947.

LEDERBERG: Yes. Entirely using the library. That's where I dug a little deeper into the background history of variation in bacteria. I think I read everything that anybody's ever written on the subject. They had an excellent library, twenty-four hour direct access to the stacks. I may have given you a reprint of that. I've revived some of the material I dug up at that time for this *ASM News* (20).

BOHNING: I wanted to pursue that because you had a very eloquent statement in there, talking about today's students who are allergic to the dust in the library stacks, as you put it, and that the recent journals are all they need. I was struck by that comment. I've had that same experience. But I'm wondering whether in your career that attitude has changed or is it more current now than it was when you started?

LEDERBERG: The distaste of students for antiquarian inquiry is a more recent phenomenon. I don't know how deep-seated it was at the time. There was an inclination to not bother much about historical stuff, as far back as I can remember, on the part of other scientists. But I had broader interdisciplinary interests anyhow than most people did, so this fed into that strain. I can't really scold our youngsters today. They've got more than they can handle in trying to keep up with the current literature; it's next to impossible to do that. So it becomes an ever more obviously losing battle. But there is stuff there that can be quite informative and quite stimulating, and I mentioned a couple of specific examples that were very clear. I don't know how much of that would still be pertinent. This is at a time of opening up not an undiscovered continent, but one in which the existing inhabitants of microbiology barely had a few new biochemical tools—that was rising discipline—but the role of genetic analysis was even more closely coupled with issues of natural history. There is an affinity between history and natural history, in part because people could make useful naturalistic observations with quite primitive tools, so that literature goes back three hundred years. I'm sure there are still things that have been recorded a long, long time ago that are going to be revived from time to time. But whether it's the most cost-effective thing to do, it rather depends. If you're looking for major problem areas, it may be just as useful as the current literature. If you try to solve an existing problematic challenge then it's probably true you can't sample it all, and if you do nothing but last year's annual reviews of the current literature, it's not perfect, but it's probably as good a use of your time as anything. You may have reason to feel badly if you find you've already been anticipated, but it's hard to know what better heuristic to offer. I wasn't suggesting in my *ASM News* article that students abandoned everything they're doing now, but just to have a little bit of sensitivity and respect for that type of inquiry. Since not many other people are doing it, I thought I would exhume a number of old issues that are still sitting there. I'm hoping that somebody will pick up some of the themes that I've mentioned.

BOHNING: In that regard, it used to be very common to have a history of chemistry course in a chemistry department that was required for majors. Did biologists do the same thing?

LEDERBERG: No, there's very little history of biology offered. There may be four or five chairs in the country that are doing this, and hardly at all for biologists. So there's a little bit of a specialty interest in it, but the general answer is no. Now, a little bit of history is incorporated. If you take a textbook of any of the biological disciplines, you'll find some historical information. The better books have more of it, in my opinion. And we're beginning to see more reflection. We've been through a stage of such extraordinary dynamism in experimental biology in this century, and a number of people are starting to take stock. I have the fun of being able to do this within my own lifetime. The stuff I used to hear about and used to think about is now history. There's a series on developmental biology, for example, that [Scott F.] Gilbert has been editing (21). He's got some wonderful essays and reflections. There's beginning to be a school of history of recent biology and of course some of the stuff down in Philadelphia is an important element of that as well.

BOHNING: If I'm correct, there is no discipline center for history of biology as there is for physics and chemistry, *per se*. Is that true?

LEDERBERG: No, nothing. We're riding ragtail on your chemistry project, or a good piece of it. Biology's such a diversified set of activities, it would be more difficult to do it or know how to centralize it. And we don't have the societal organization, which is again a reflection of the same matter to be that kind of a focus.

BOHNING: Do you think those people so inclined have more of an interest in the history of medicine, in that aspect of biology?

LEDERBERG: The history of medicine is a more settled discipline than biology, and a lot of physicians have turned to history, certainly in larger numbers and proportion than biology.

BOHNING: You've commented than the [Maclyn] McCarty paper of 1944, which was a landmark paper, changed your life.

LEDERBERG: It changed all of our lives, [laughter] but it did it in a very personal and gripping way.

BOHNING: I want to explore that a little bit. That's sort of in keeping with what we were talking about here. First of all, did you see that paper as a matter of course in your reading, or were you aware of the results before the paper appeared?

LEDERBERG: We heard a little bit of it before hand. I can't give the precise dates, but [Alfred E.] Mirsky was a frequent traveler between Rockefeller and Columbia. He was collaborating with Arthur Pollister, and so I'm sure we heard in various seminars what was going on with nucleic acids. Mirsky had his own interest in nucleoprotein. There was undoubtedly a certain amount of envy relationship, between him and Avery. That's been overplayed in later commentary; while he and Avery were in dispute at a later stage, Mirsky in fact was the principle communicator of what was happening. I'm sorry I can't give more precise dates on that point, but I was going in and out. I was intermittently on campus. I was being shuttled back and forth between the naval hospital where I was working as a corpsman when I was in the V-12 program, and then back to school. I know there had been some talk about it, but we didn't get the journal in Morningside Heights. I knew that Harriett Taylor, later Ephrussi, had a copy. My guess is she probably gave a journal club on it; she had some close connections. She later went to work with Avery, a year or two after that. At that time she was a graduate student. So I asked to borrow her copy of it, and that's when I first actually read the detail of it. I've actually written in my diary about it. I think you've seen the text quoted in one of my articles, that kind of very gripping recollection of it.

BOHNING: So the response to that was, I won't say instantaneous, but very close to it.

LEDERBERG: Oh, it was. There was some prelude to it, but it was very fairly general talk. Then I read the paper and was able to see in some detail the nature of the experiments. That told me this was solid gold. While I didn't have to believe everything about it, the paradox is that because I thought it was so important, I tried to keep an open mind about the details probably longer than most. I would still be sympathetic to Mirsky's arguments that it had not yet really been proven that is was DNA alone and not DNA plus protein. I was content enough to believe either one or the other, but I thought something as important as that really ought to be nailed down very solidly before you take it as a matter of faith and go on from there. Other people either ignored it or bought it and in a certain sense didn't think any more deeply about it. I just wanted to explain that paradox.

BOHNING: In that somewhat lengthy argument about protein and DNA, did the people who supported the protein argument finally give in, or was it a long drawn out affair?

LEDERBERG: It was long and drawn out, and I guess giving in was more by exhaustion than by any single event. I'm writing a piece right now about the Watson-Crick paper in 1953 (22), which coincided with the interval of "let's not make any more fuss about it." It is DNA and in some measure that was because the Watson-Crick model very beautifully correlated with the pure DNA model. It wasn't that there was any more experimental evidence on that point, but it did fit things together. There are people who will argue that the issue had to do with whether DNA was capable of having the informational diversity needed for genetic activity. That was certainly in there, but as soon as one kept an open mind about the [Phoebus Aaron] Levene tetranucleotide model of DNA structure and could imagine any irregularity in base sequence in the DNA, and you could do that without going all the way to the double helix, then the possibility about the informational content was there. That was already tacit in Avery's first discussion of the matter. So I would say it was rather a question of cleaning it up, both figuratively and literally, just to be sure that protein wasn't sneaking in and playing some role in the source of specificity.

Curiously enough, [Alfred] Hershey is often credited in the Hershey-[Martha] Chase experiment in 1952 with having given the final experimental evidence that it was DNA alone that was sufficient in this case to maintain the genetic propagation of bacteria phage. You may remember he did a double-labeling experiment where he labeled the DNA with P-32 and the protein with sulphur, and found that after infection he could find the phosphorous in the infected bacteria, but not the sulphur. I've had arguments with people and I've finally found the source that corroborated my recollection of the matter. In the 1953 Cold Spring Harbor symposium, in the same meeting where there was an early announcement by Watson and Crick of the DNA structure and in the discussion of the paper, Hershey was still expressing some reservations and was saying that if you would ask him right now, he still thinks it is nucleoprotein. His experiment leans in the other direction, but he knew better than anybody that it was not that conclusive. You could still have had several percent contamination of the nucleic acid and not have seen it. What it did show was that most of the protein is shed and most of the DNA gets into the cell. So he was even at that stage still reluctant to buy the pure DNA story. But that was the last peep that I ever heard in a serious scientific vein. There were a couple of other people. There was a paper by Barry Commoner and there was one by Carl Lindegren, still protesting—this was during 1953—that it hadn't been proven that life is DNA. There was no serious protest after that time. The Watson-Crick model crystallized—figuratively, again, as well as literally—a way to picture DNA structure that would be compatible with the rest of the construct.

BOHNING: Was Commoner attacking on scientific grounds?

LEDERBERG: It's hard to say. Barry's quite an ideologue. He's the same guy who's stopped us from incinerating anything. I don't know if you've heard his name around the country. [laughter] I don't know why he dipped into that. He had done a little bit of work with plant viruses, but he was not really a geneticist or a DNA biochemist. He just thought it was too simplistic. There were still some rumblings about this or some lack of realization that this really has to be taken seriously as the chemical basis of genetics, and that it was no longer just an interesting hypothesis but had to be regarded as the foundation of any further work in the field. As I may have mentioned to you before, I felt strongly enough about that when the time came for me to compose my Nobel Prize lecture, I did something quite unorthodox. I only referred incidentally to my own work and instead wrote a commentary on the field. It's called "A View of Genetics" and it was a manifesto that says DNA is the view of genetics (23). I refer to my work within that framework. I thought it would have been folly otherwise. I didn't think it would make too much sense just to talk about my own experiments, which had been done in a way that was inspired by DNA models but did not make direct use of them, much to my frustration (I would have very much liked to do that) and then ignore this important revolution that was still going on.

[END OF TAPE, SIDE 9]

LEDERBERG: That paper was in a sense a protest in the Lutheran sense of nailing the theses on the wall, that there was still something to protest about. It was not so much opposition as sluggishness, that the main line of genetics really could no longer be diffident about it. We weren't opposed to it, but it was a huge cultural change, to have to think this much biochemistry. In effect I was saying, "Look, a lot what we've been doing is now obsolete and we have to make a completely fresh start and look at things in a new way." There were some folks who were not about to do that overnight.

BOHNING: Was it a generational thing?

LEDERBERG: In part.

BOHNING: There's many examples in the history of science where it takes a generation to disappear before something new is accepted. You were young, in your early twenties.

LEDERBERG: Yes. But Avery himself was not. Don't forget that. [laughter] But Avery was not a geneticist. So it's marginality as much as generational. But certainly it is. You have to have students who would then have been trained from the outset to specialize in that direction and not be too encumbered with too much else. There were things lost as well as gained in that process. I guess the image I had then as much as anybody was [Theodosius] Dobzhansky, a towering figure in the field who had no feel whatever for any kind of chemistry. He did not do a lot to encourage it. I know Francis Ryan had lots of problems trying to sustain his place in that department while Dobzhansky was in charge of it.

BOHNING: In that year between the 1946 Cold Spring Harbor and the 1947 conference, what kind of things were happening scientifically? In other words, you had given your results in 1946. What was the status by 1947?

LEDERBERG: By then Luria had tried the same experiments in E. coli B and failed. He made less of that failure than others. I heard later that Delbruck was touting this and saying he's not sure there's anything in it. [Aaron] Novick and Szilard wouldn't have repeated the experiments until after that. There was no one else who asked for the strains, and I don't think they would have wanted to. I think there was a certain amount of a sense of latitude—don't jump on somebody's back immediately because it is the first announcement. Give them a little bit of breathing room. You don't see that today. I would have had some ambivalence about sending the stuff out. I think if anybody had asked me point blank I would have done it, but I was still very, very busy following up on the first immediate observations. I must have had something started by 1946 with Max Zelle because he interceded in my debate with Lwoff as to whether the recombinant clones really were clones, really were derived from single cells. He was pushing the idea that they were simply continued mixtures of cultures and complementation. That's a perfectly plausible hypothesis for the prototrophs and he had his own experience with cross feeding of the nutritional requirements. I resented a little bit the implication that I was not totally aware, both of his work and of that hypothetical possibility, and I'd gone to great pains to exclude it. This had to do with producing more than one kind of recombinant. It was very difficult for me to see how you could invoke mixed cultures to account for both a virus-resistant prototrophe on the one hand or a virus-sensitive prototrophe on the other. Virus-resistant segregating is a mark of that kind of a cross. This would have been a very sensitive indicator. If you have virus-sensitivity, you know there are zero virus-resistant cells in the culture. That's easily demonstrated with mixed cultures. So there were a variety of ways that I was quite certain that had been excluded. Almost everybody else wasn't bothered to look at it, but Lwoff persisted and said, "Until you've isolated single cells, you shouldn't call them clones. Don't rely on ordinary bacteriological plating methods. You can't be sure, et cetera, et cetera." I continued to give arguments why that was a needless enterprise, but Zelle said, "Look, it's not that hard. I'll show you how to isolate single cells under the microscope and it isn't that hard." So he got involved fairly early in some corroboration. I think in the early experiments that I simply sent him some of my prototrophe cultures and he re-isolated single cells, made clones and they were still prototrophes. That was his contribution.

The first repetition was done by Novick and Szilard. That must have been after I got to Wisconsin, so it would have been in early 1948 before that was done. They did and they circulated that news. Luria was quite content with it. It was Luca Cavalli-Sforza who later on in 1948 wrote to me and said that R. A. Fisher had been very much impressed with this work. I'd exchanged reprints with him. I'd met him at Cold Spring Harbor in 1947 at the Biometrics Society. Luca at that time was a young postdoc at Cambridge. Fisher was interested in crossing over and mapping, and correctly thought that, with some complications, this might be very good experimental material. He suggested to Cavalli-Sforza that he look into it. So Luca wrote to me and asked if I would send him the strains. And of course, I did. Outside of my own laboratory, he was the first person who actually jumped wholeheartedly into working on the system. Within a very short period of time he made a very important discovery and that was the so-called Hfr, high frequency of recombination. It was just a piece of luck that a particular strain popped up that had a thousand-fold higher rate of recombination. It just made the technology of crossing very much simpler. We began a collaboration at that time and it's gone on ever since. He wrote a little piece about that in *Genetics* (24). Did you see that?

BOHNING: No, I don't think so.

LEDERBERG: I'll have to get that out for you. I think it's quite important. I was very busy in my own lab. Other people were doing other things. There was more going on regarding phage recombination during that period. Luria wrote a review in which he referred to the E. coli work and positively affirmed its significance (24). I guess you'd have to look at the 1947 symposium to see what else was happening scientifically. The most interesting thing relevant to our own work was that Boivin popped up. It was in 1947 that he published the paper (25). He and Tatum had some correspondence and he claimed to have some sort of DNA transforming system in E. coli. Since that was exactly what I'd gone into bacteria to do in the first place, I was very excited about it. But we were unable to corroborate it and after a while he was as well. There's no way of knowing what was happening there. It was another ten or fifteen years before we knew how to get DNA into E. coli. We've had the legacy of K-12 as a by-product of that technical glitch.

BOHNING: You've used the term "messy" in terms of the laboratory work. I'm wondering whether that was literal or figurative.

LEDERBERG: You mean my own style?

BOHNING: No, in general at this particular period.

LEDERBERG: Work in bacterial variation generally was very messy. It was a little bit of experimental technique, but mostly it was conceptual muddiness. If you didn't even have to discuss the concept of the gene, it's a little hard to see how you can do very crisp experiments if you didn't understand the difference between a cell, a clone, and a population. Likewise, I think most of the microbiologists at that time were just not accustomed to thinking in those terms. They saw culture and they thought it was an entity. A tube of bacteria was like one organism. It seems unimaginable that one could have that view, but it was quite prevalent at the time. So it became very difficult to understand exactly what it was that others had done, or how a lot things that were called transformations of cultures were very clearly overgrowths of selection, sometimes contamination. It was a little hard to pick the wheat out of the chaff. The other thing that was often confused with genetic change was enzymatic adaptation. If you take E. coli in the right conditions and provoke it with lactose, ten minutes later you have large quantities of the lactose-splitting enzyme present in the cells. That was very muddled with changes of genetic competence to make the enzyme. It was a totally different phenomenon. That got mixed up in many of the older people's writings. It was a confusion at that time.

BOHNING: One other thing that you've commented on in the same time period was the importance of networking with other scientists and that you were actually developing that network at this point, from 1946 on.

LEDERBERG: Know it or not, I was, yes.

BOHNING: From the Cold Spring Harbor meeting on, you certainly were thrust into the forefront of being in contact with a huge number of people.

LEDERBERG: I was very fortunate in the opportunity to do that, the happenstance of being in the right place at the right time. That Cold Spring Harbor meeting was an extraordinary opportunity. Since it is simply what happened, I rather took it for granted; it's only in retrospect that I can begin to imagine what would have been the consequences if it simply hadn't existed. Or even worse, if it had, but I for one reason or another had not been admitted to it. [laughter] Did I tell you that Johnny Moore told me that one reason that Francis wanted me to go to work with Tatum was that he thought Tatum would be much more useful for me in introducing me to those networks and that was, as Johnny said, the special disability of being a New Yorker, which I shared with Francis. Even more, I was a Jewish New Yorker and Francis thought that those were prior impediments that would need very special attention. I was oblivious to that at the time, but just quote that as something that a very dear and wise friend had mentioned.

BOHNING: What was Tatum's status in that community at that time? Was he well-established by 1946?

LEDERBERG: This is a very complicated story. There's kind of a Beadle school and a Tatum school. They never had any public dispute on the matter, but a lot of this was going on in the background on the part of their friends. They had been very close collaborators. Tatum was much younger; he was at least junior. He had the microbiological and the chemical training; he didn't know very much about genetics, although he had the right instincts fairly early. The issue revolves around who should have gotten the credit for the Nobel Prize on biochemical genetics of Neurospora. Ed was not a deep thinker. He was a very pragmatic kind of person, loved to do experiments, and had a very good intuition. You might say if you judge by the outcomes, Ed was a lot shrewder and insightful than he made out to be. Some of his articulation of biological issues was fairly shallow, but he had wonderful instincts, and what he really knew, whether he knew it or not, went deeper than that. He had a lot of lore that again didn't seem to be so obvious at first sight. I'm sure he was thoroughly familiar with Garrod's work and that Beadle was not; that was a major anticipation of biochemical genetics. He knew about fungi and how to grow them; I don't think Beadle ever knew anything about that. He had a sense of biochemical pathways. He knew how to actually isolate and crystallize and characterize something. Beadle wouldn't have known how to do a melting point, but Beadle did have the theoretical grasp, the oversight, the strategic insights. I can't help but feel that he rather resented having to share the glory with Ed, and he did very little to further Ed's career. I don't know if he actually obstructed it, but it's obvious at Stanford that he was either totally incapable—which seems unlikely—or unwilling to go to bat for Ed in terms of getting him a permanent faculty position. This was between 1941 and 1946.

When Beadle went to Caltech there was no hint that he was going to ask Ed to go down with him. So there are those tensions in that relationship. My own relationship with him was pretty clear-sighted. Ed gave me one wonderful thing and that was E. coli K-12, that specific strain that he had worked on, without knowing that there was anything very special about it. We were all very lucky, myself in particular. He gave encouragement. He gave me a laboratory. I can't think that he made a single conceptual contribution to the work. I could be wrong about that. I've said of Francis Ryan that he was very careful, in his mode of encouraging me, to hold back on his own part. He didn't immediately give me all the detailed directions that he would have been very capable of doing so. I don't think that was the case with Ed, but he was there, did all the right things at the right time. Every now and then something would come to the surface that would tell you that there were things going on that were fairly deep, but you didn't see them very often. I have no doubt he brought a very special perspective to that work, and he's certainly as deserving as Beadle. In Jan Sapp's book (26) he's gone through the [T. M.] Sonneborn papers and the correspondence between Sonneborn and [Boris] Ephrussi around 1958, when the Nobel Prize was awarded. Their consternation was about what was it for and what new thing had Beadle done? (Beadle had collaborated with Ephrussi.) Have you seen the [Richard M.] Burian and [Doris T.] Zallen stuff about that (27). They've gone into that in some detail. They were quite challenging about that. Beadle was a very aggressive go-getter. Nobody was surprised when he took the presidency of the University of Chicago. It was perfectly obvious those were the kinds of ambitions that he had and he had a salutary effect on the development of biology at that level. Lily Kay has written about his role at Caltech as well, pulling biology together and making a big business out of it, since 1946 (28). Ed was none of that. Ed loved working in the lab himself and puttering with Neurospora. He loved the organism and did a lot of good things. He certainly made much more of a contribution in helping to develop many people. After his work for Beadle there was nothing really very startling that came out of his own experimental work.

BOHNING: You have mentioned, and I quote, "Harriett Taylor's paper on pn. transformation an irritant." I was wondering what you were referring to. This was in March of 1947.

LEDERBERG: Oh, that's the paper she gave at Cold Spring Harbor (29). Yes. She didn't want to buy a straight Mendelian view. It was pretty mystical. Trying to read that paper, I still can't figure out what she was after. I saw what she was getting at was a very nice story. There were three or four subunits within the polysaccharide gene in the pneumococcus and she was trying to make something else out of it; I couldn't make head or tail out of it. She was criticizing me for trying to force E. coli into a Mendelian mold. There are problems with that, but they're not the problems that she was addressing; even at the micro level of genetic structure that's held up very, very well. She was six or seven years older than me, a very attractive and intelligent woman. I was a pretty young teenager when I first came to Columbia; I won't startle you if I tell you I had a kind of a crush on her. I had not met many women like that and I did not succeed in making very much of a personal impression on her, given the age issues. I was a sixteen or seventeen year old, she was in her mid twenties. I knew that, and I had no unrealistic expectations, but that sort of colors my relationship with her. After a time she got to recognize I was a peer sparring partner. I just have to record that as part of why I lamented that we couldn't get to see eye to eye at a later time. At the same time, she was encouraging, but I really had to be very careful not to get her nervous in any way about my personal attentions, because I thought that she would be unable to relate to me in the ways that she would be willing to do—talk about papers and so forth. I watched the progress of her romantic life with some interest and it was pretty complicated. She ended up marrying Boris Ephrussi, as you know.

BOHNING: You were married at the end of 1946. I don't know how you want to talk about that or not talk about that; I'll leave that up to you. But obviously that has to play a point in some of the things that were happening in your life at that time.

LEDERBERG: Let me just give you a few facts and then I won't go into much more about it. Esther Zimmer was a graduate of Hunter High School and Hunter College. She went to work for Alexander Hollaender at NIH [National Institutes of Health] in 1942 or 1943, more or less. She then applied for a position as graduate student at Stanford and went to work for Beadle and Tatum as a master's student. She was there in 1945-1946. She did some work on a mutant Neurospora that requires para-aminobenzoic acid. I first heard her name in connection with wanting to get hold of the mutant. In those days Beadle was farming out each biochemical mutant as somebody's personal province. Today we make them by the hundreds of thousands, but that's the way it was then. He was a little bit slow and wanted to give her a chance to work it up. In 1946, the Beadle-Tatum group at Stanford was breaking up, at Stanford, and she went looking for a job. When Tatum came east he agreed to hire her as a technical assistant. After coming to New Haven, she went to work for Norman Giles; she was Giles' research assistant in their group. I don't know whether Tatum had recruited Giles or Giles was there beforehand, but there was a group of microbiologists in the botany department and that's where I met her. We spent some time together during that time, and in a rather short period of time we were married in December; we had met in August. The common thread of interest was in laboratory work and in genetics. We went to Wisconsin together, and she continued. I encouraged her to try to get a doctoral degree, which she did in the department. She worked in my own laboratory thereafter, through the Wisconsin time, came with me to Stanford, and worked with me up to the time of our divorce. I've encouraged others to take the benefit of this experience. The romantic notion of working day and night in the laboratory next to your significant other is fine and productive for a while, but it's not the best way to pursue a long-term relationship. Let me just leave it at that.

[END OF TAPE, SIDE 10]

BOHNING: You had been planning to go back to P & S to get your M.D. degree. Until Tatum told you this, had you given any other thought to doing something other than going back to P & S?

LEDERBERG: No. I was hoping to find a way in which I could continue to do research part time, as I had been doing before then. Ryan would have accepted me into the lab. There was some serious questions about the support, and I was not at all clear about how I was going to manage it. It might have ended up that Esther would have been working to support us both while I was in medical school, a not unfamiliar scenario in those days. But that was unclear. I had applied for a Merck fellowship. For worse, Beadle was on the selection board and I didn't get it. I never had any more information on the matter, but I have my continued suspicion that he was not particularly warm on the matter. I don't know who did; that would be a rather interesting, curious point to try to find out. If you have any way to do that, I'd be very curious about that. [laughter]

BOHNING: I think I do, as a matter of fact.

LEDERBERG: A way to do it, or you know who it was?

BOHNING: I don't know who it was, but I know the archivist at Merck who may have access to that information.

LEDERBERG: It should be public information; it should have been announced. That would have been for the class of September, 1947. So that was one issue, trying to figure out how to do it. I don't know that I would have qualified for the GI Bill or not. I did spend a year not just in uniform, but actually in active duty. I worked as a hospital corpsman; I might have had some help there. Before I went to Yale, of course, I was in the V-12 program. In November of 1946, after the war was over, that was just very abruptly canceled, and they weren't even accepting applications for reserve commissions. I looked into it, and they weren't interested. Two years later they were desperately reversing their policy. [laughter] Times had changed a little bit by then, or I might have ended up in Korea. So there was a financial issue, but I was still making that plan. I had no other alternative plan. I was going to have some sort of part-time job that would help eke it out, and would help keep me involved in the laboratory during that time. The alternative was a full-time job, not the M.D.; that would allow me to devote myself unremittingly to continuing this research activity. I think I made the right decision and never really regretted it. But I very much regretted loosening my connection to medical affairs. When I was at Wisconsin I worked hard to restore them and eventually did. So that was the trade off that was involved.

BOHNING: How much did you debate about this in your own mind before you made that decision?

LEDERBERG: Quite a lot.

BOHNING: It's a major turning point in your life.

LEDERBERG: That's right. That's right. I certainly discussed it with Esther, with Francis, with Ed, and in my own mind. So it was not an impetuous decision. It wasn't clear at all whether I would like Madison. I'd never lived in the Midwest, but I had a very warm reception from the people that I met there. I welcomed getting away from urban hurry, which New Haven didn't do. That was no different from New York in that regard. It met my expectations.

BOHNING: When did you first go out to Madison?

LEDERBERG: That would have been at the very end of August, or maybe even September.

BOHNING: But you didn't accept it until you went out there first.

LEDERBERG: That's right. Oh, well, they didn't offer it to me either. [laughter]

BOHNING: What I was after was your initial reaction to being there. It was your first plane trip.

LEDERBERG: It was not very intense in a few moments. I saw the town, saw a little bit of the university. The people especially impressed me. There was an excellent bacteriology department, folks like Perry Wilson were very warm and welcoming. I was very inexperienced on the issue, but I had to think through what I was doing in a college of agriculture. I also knew the important scientific output that had come from especially biochemistry, but also bacteriology. Ed Tatum had been there himself and it looked like it was a good liaison between the genetics and bacteriology. The one thing that was disappointing was the medical school. There was hardly anyone there I could relate to. There was not much going on; it was not much of a research establishment. I don't remember exactly whom I visited during my first trip out there, but I doubt if I saw anybody from the medical school. So I sort of wrote that off as being on the loss side.

BOHNING: It sounds like it [University of Wisconsin] was tailor-made for you. Why were they looking for a person of that nature?

LEDERBERG: That's a very interesting question. I think R. A. Brink has some stuff in the archives on that, so I don't really need to repeat it for documentary purposes here. But before him the just retired founder of the department was a man called L. J. [Leon] Cole. He was an extraordinary man who had built the department on the basis of important applications to agriculture and animal husbandry, and at the same time felt that it needed to have a strong basic scientific foundation. He had actually written a paper in 1918, in which he talked about bacterial variation as an example of mutations (30). (I found this out a little later on and I'm curious to know when I did.) It was on the "pure line" concept in bacteria, and it was very clear minded. So he did have a lot to do with inspiring the department to go in that direction. They decided they just wanted a basic scientist and that new things were coming along; their instincts were exactly right. That was the right place to go for exciting new developments. It was a perfect match. It was a very good thing for me, and me for them. The fact that I was not that narrow, that I could take an interest in what they were doing and assimilate that into my own concerns, appealed to them. It worked out very well on all sides.

BOHNING: You commented at the end of our first session about what you found out later regarding the problems that came up in the department about your own background. You said you had a warm welcome on that trip, and that you were unaware of any of that going on behind the scenes, which is, I think, something in itself. If it was of any magnitude, it's amazing that somewhere somebody didn't give you that impression.

LEDERBERG: Well, I wasn't looking for trouble, so it may have been that element too. It was something of the nature that this Hebrew (that would be the phrase they would have used) may be pretty smart, but I don't want him in my club. But they were also, in the best sense of the term, waspish enough that they would not be discourteous or impolite, at the same time. That's the way I can size it up. Now, I know there were people there who had opposed my appointment on those grounds (and I only know this retrospectively) with whom I never had a warm relationship, but from whom I never heard a whisper. They were not eager to become personally friendly, but I think they respected my work. It would have been beneath them, and they wouldn't have wanted to think of themselves as anti-Semites or capable of bigotry. It was sort of at an instinctive level. It didn't stop them from raising the question. But I've seen very little of the complaints. Brink did not share with me what he had to face. He did share with me what he wrote in response to it, and the whole explicit statement was, "In spite of his race, Lederberg has such extraordinary intellectual accomplishments that I think we ought to go for him anyhow." That was the way it was finally sold. He told me that had a tough time getting that across. [laughter]

BOHNING: You were only twenty-two at the time that happened? You weren't any older than the graduate students.

LEDERBERG: I had one or two students who were older than me.

BOHNING: Do you think age had anything to do with it?

LEDERBERG: It's hard to say. It might have gone both ways. It certainly meant objectively, while I was mature for my years, I nevertheless didn't have very many of them. I'm sure I was brasher than they were accustomed to on that account, plus whatever New York manners might have developed along the way. But it also made it singular. It said this is not the ordinary cut and dried kind of a thing; its a very idiosyncratic kind of a situation. In a way it might have made it easier.

BOHNING: When they made you the offer, what was involved in that offer?

LEDERBERG: It was as assistant professor. I would teach the courses in microbial genetics. Not an onerous schedule, but more than one sees these days. Basically, it meant one course each semester. I would collaborate with the bacteriology department and set up a research program. The teaching loads in the Ag schools were less than in H & S, because we were part of the agricultural experiment station, part of the funding for agricultural research. I'd write reports every year for the director of the agricultural experiment station, describing what I worked on. I did not have much of a lab. I had a room about this size [ten by twenty feet], up right under the eaves. It was very hot in the summer. It was pretty tough working there in the summer time. I essentially gave up trying to do serious work. The plates would never congeal. No air-conditioning. But we got a lot of very exciting work done under those conditions. I think I eventually got as much space as these two rooms together.

BOHNING: This was the genetics department in the Ag school?

LEDERBERG: That's right.

BOHNING: Why it was in the Ag school and not in the medical school?

LEDERBERG: The medical school didn't have anything research-wise. Genetics had been an outgrowth of plant breeding and it had practical agricultural applications. It was one of the first departments to be called genetics, not the other. That was L. J. Cole, who was trying to enhance the scientific foundations of the school and of the field. We had people like E. B. Fred, who was first the dean of agriculture, then the president of the university. Connie [Conrad A.] Elvehjem was his successor, a well-known nutritional biochemist. And Fred's predecessors were much of the same. Much as was happening with medicine some years later on, they saw that for agriculture to prosper they needed some sort of important fundamental scientific base and this was just one example of it. You saw the same in bacteriology. It covered the gamut from better strains for making cheese to nitrogen fixation to very fundamental studies.

BOHNING: How many people were in that genetics department?

LEDERBERG: It must have been between eight and ten faculty at that time. We had Bob [M. Robert] Irwin. Irwin was a very distinguished immunogeneticist. He did very practical work on getting genetic markers in cattle, which helped to raise pedigrees. He and Ray Owen laid the groundwork for what we know today as the histocompatibility system in mice and in other animals. It's a very good example again of covering the gamut.

BOHNING: You mentioned Ray Owen. Wasn't he at Caltech when you arrived?

LEDERBERG: I think the first year I was there he was on leave at Caltech and then he decided to stay there.

BOHNING: Wasn't there some question about references at Caltech?

LEDERBERG: Yes. When my credentials were under examination—again I only found this out much later through Brink's correspondence—they checked me out with Ray, asking what about this fellow Lederberg? Ray quoted a fairly negative report from Delbruck, a diffident one from Beadle, but said "Everybody else thinks his work was just great," and he didn't think there was any substance to the other two's criticisms. They were not that deep-seated and there was no content to them, so he was willing to give his own vote and that was the decisive one in terms of getting me into the department.

BOHNING: You've made the comment that you looked forward to interacting with these people in the Ag school, eventually in what one might call biotechnology. What kinds of interactions did you have with them on a practical sense?

LEDERBERG: It ended up being less than I anticipated. Basically there would have been a good opportunity to go into genetic analysis on any and all of these kinds of issues. There were a few starts. Perry Wilson was interested in the biochemistry of nitrogen fixation, and I was eager to collaborate with him in working out the genetic basis of it. That ended up being later on a very, very fruitful area of inquiry. There was also Rhizobial plant interactions. But to do it would have meant trying to locate a graduate student who was willing to work in a joint program, and I don't think that ever eventuated. We would have joint seminars. I would teach some of their students. I picked up a little bit of the technique and the lingo, but my efforts at transplantation were only partly successful, so I can't really say that anything firmer actually came out of it. I learned about antibiotic fermentation, and was eager to have them use mutational approaches for strain improvement. (This was in the late 1940s that we're talking about.) There was a little bit of it. Ken [Kenneth B.] Raper was working on that; he'd done penicillin improvement during World War II. They were already doing some of the cut and dried things, but it never really caught on anything beyond that.

BOHNING: What kind of an agenda did you set for yourself in taking that position?

LEDERBERG: It was a comprehensive research program, and I think I've outlined to you the different themes that I thought I would get into. I don't know whether that was all worked out in advance. Some things couldn't have been because there were fortuitous discoveries after we got there, like the F system and the special lambda. But I did write a proposal, which was the basis of what we were working on, for the Rockefeller Foundation. I asked for support from them and from the WARF [Wisconsin Alumni Research Foundation], which is the patent licensing intermediary for the [Harry] Steenbock patents and later the Warfarin patents. They were very lucrative sources of income to help support research there. That work was the genetic basis of control of enzyme formation. The lac system is what we worked on there, and I think we did make some significant contributions to that. A lot of other things just fell in—transduction in Salmonella, specialized transduction with lambda, more on the mapping, the Hfr, the heterozygotes. That's on the list I've given you, and we can go into those in some detail one at a time. But the core theme, the over-arching one of genetic systems in bacteria, was what were the mechanisms of genetic exchange and then the application area for genetic analysis which was genetic control of enzyme formation. Those were good decisions and with all we know today, wouldn't have altered it a bit. They were right on the button of what E. coli was good for and did have important consequences. Starting around 1949 and 1950, we began to see very intense activity at the Pasteur Institute. They had the wonderful advantage of being in Paris and attracting a lot of Americans postwar to spend sabbaticals there. They had a large stream of American scientists going there and they fertilized the place wonderfully. It's just a lot of hands. It was very, very difficult to compete with my tiny laboratory with what they were churning out. They had some very good minds there, too, with people like François Jacob.

BOHNING: Norton Zinder started with you very early on.

LEDERBERG: He was my first graduate student. The year after I arrived he came the following fall. He's told the story of his youth (31). I'd been intrigued with the idea of recombination in salmonella, even before I did the E. coli experiments, just to get the natural history of serotypes. It seemed to me to make it a foregone conclusion that there was going to be some kind of recombinational mechanism. Using Occam's razor, I thought, "Why invent new ones. Let's look for what we already know is in E. coli. E. coli is a pretty close neighbor to salmonella." So when Nort came to the lab, I said, "Go to town. Here's some salmonella strains and go to it." I sort of left him alone at that point. It kept not working. Then he got some flickers of things, and some single mutant crosses seemed to give some prototrophes. It looked like it was more than you got from the parent cultures alone, but I was very worried those could all be artifacts. We didn't know how much continued growth there was on the plates, so I refused to acknowledge it till he had a double mutant that would work. He then found with a particular double mutant strain that he could get prototrophs. Finally there was a phenomenon that could be investigated further. He did ninety percent of the actual lab work on the matter. He was in constant consultation with me on the matter, very, very directly. He thought to repeat some of the experiments with a bundling board that Bernie Davis had done to show it took cell contact to get recombination in E. coli. The idea here was that you put your two cultures that putatively could cross with one another in a U-tube that was separated with a sintered glass filter that did not allow cells to go through, but would allow soluble products of the cells to be exchanged freely.

That experiment showed that whatever was responsible for crossing in salmonella did get across this filter, whereas if you did the same experiment in E. coli you could not get it. So we knew there was a different phenomenon now. This now became the hunt for the filterable activity, and this eventually turned out to be a bacteriophage. The story gets a little bit complicated because you have two salmonella strains. They're not of identical clonal origin. We always did experiments with that design, among others, because we were never sure whether they might be mating types. We would maximize the chance if you had one of male, one of female if they were of different origin. If you have an identical origin they might be the same mating type.

[END OF TAPE, SIDE 11]

LEDERBERG: The story finally got itself worked out. Strain A, in which we'd made some double nutritional mutants, was carrying a bacteriophage, which we called PLT-22, phage of Lilleengen type twenty-two. [K.] Lilleengen was the guy who gave us the strains; he had a collection of salmonella strains. The other strain might be LT-7 and was susceptible to the phage PLT-22. So the PLT-22 phage starts from the PLT-22 strain grows on the LT-7 strain. Now you have a transforming phage. That phage can be reabsorbed back onto cells. It might be a fresh batch or a different genotype of PLT-22 strain origin. When it does so it also carries genetic markers from the LT-7. Those are the recombinants that we'd seen. Viral transduction is quite different. It's not cell to cell contact, it's phage particles carrying tiny bits of genetic information from the host in which they've just been grown, to the new host that they're going to enter into. So it was a new phenomenon and I wanted to give it a distinctive name, and I called it viral transduction. This has had very important repercussions and is now the basis of gene therapy; when people are using retroviruses to import genes into human cells, it's essentially the same phenomenon. It also opened up doing genetics in salmonella. There are rather far-reaching implications from the view that a virus could have genetic functions; you couldn't think of it only as a pathological entity.

As soon as it was possible to do this, I differentiated my own interest in the salmonella story, the pursuit of the serological types. I went to visit P. R. [Philip] Edwards, who was the honcho of salmonella serology in this country at that time down in Chamblee, Georgia. That was the unit that's now the Centers for Disease Control in Atlanta; it was in a suburb at that time. I spent three weeks there picking up his lore, getting him interested in it. We had a very effective and extensive collaboration and very promptly corroborated that indeed you could generate a wide variety of new serotypes by transductional recombination. No one doubts that that's the natural historical source of this kind of diversification. There is still a lot of unanswered questions about Salmonella virulence and serology, but those fundamentals have stood up very well. In retrospect, Norton thought I was very dense about not seeing that phage was the mediating factor. I don't recall the detailed incident, but in his write-up he says it was Harriett Ephrussi who said, "That's what it must be," when he described this general pattern of events. At one stage we knew it was associated with phage; we could get the filterable activity defined as that which transforms cells of the nutritionally negative phenotype to the nutritionally positive one. That was by bioassay. They were present in phage lysates, but my not yet being prepared to insist on the view that the phage itself was a genetic vehicle, I thought that this was a way of liberating, solubilizing genetic constituents in the cell, which is certainly true, without necessarily having to be packaged within the phage particles. When the issue was put that way we then continued to fractionate the activity and showed that it sedimented with the phage. That was the evidence that it really is in the phage itself and not some ancillary material connected with it. With specialized transduction, which came up a little bit later on, another one of my graduate students, M. L. Morse, was working on the genetics of the galactose genes in E. coli; this is an extension of the work on the lactase loci. These are enzymes in galactose metabolism. He ran into somewhat similar kinds of observations, and quickly on the basis of what had gone on before was able to come to the conclusion that the phage lambda, which is present in E. coli K-12, could pick up the gal gene. This was with salmonella, and not a lot of the others. It was quite specialized, so it would be just that one factor, and that would remain associated with the lambda during further propagation. There the association of the genetic activity with the phage was clearer right from the start.

It turns out it's a defective phage in that case; you can generate lambda particles containing the gal gene, but those particular particles then do not convey a full-blown lambda infection. You have to replace a vital gene of the lambda with a gene from the host, and that makes the phage defective as far as further infection's concerned. So it got to be a little bit more complicated. But here were now two systems of viral transduction going on side by side, and enough difference between them to suggest that we're into a whole new family of phenomena. We ourselves didn't push for many more but others have and it's now known that there's a wide variety of interactions of which these are special examples. That in turn supported the more general philosophical concept of the plasmid. It's just a way of looking at genetic particles that says, let's not waste a lot of time deciding whether something is a virus or a gene. That has to do with just what impact they have on the further development of the cell. In either case, this is genetic information; it can be used either way. That's been an important unifying concept. It's removed all the quarrels about whether something was a genetic phenomenon or an infectious phenomenon; it says it really can be both, little bit of a wave-particle dualism that one can resolve in that way.

That's been quite important in straightening out a major source of confusion in biological development. Sonneborn got to be impaled on that. He was very excited about cytoplasmic heredity in Paramecium, as he should have been. Then he came under attack when people said, "Oh, these kappa particles are merely symbiotic bacteria, they are not genes." I tried to say it is not a contradiction between those two statements. Sonneborn himself never completely bought it, but I think that's the way most people would view it today. Jan Sapp was here last year; he's written his third book. This one is on the history of symbiosis, and he's done a very good job of integrating that part of the story into his writing. He knows much more about the Sonneborn archives and correspondence than I do. It's really a pleasure to hear from him about that. That book will be coming out pretty soon (32). The plasmid concept was fired up by what I just told you—the ambiguities of what is lambda. Here we have a bacterium, which every now and then gives rise to a bacteriophage. We were able to even localize the map location of the lambda-generating factor on the chromosome; it happens to be right next to the gal factor, which is not a coincidence. So you could again have this dualism of something that was in the chromosome coming out and being a virus.

Then there was the other phenomenon which again just fell into our laps, both the case of the original discovery of lambda as a phage in resident in E. coli and the discovery of the F factor, which is the factor necessary for fertility in E. coli crosses, which turns out to be another cytoplasmic particle. In fact, it doesn't make a phage; it's a little like lambda. It can live either in the cytoplasm as a plasmid, or it can become integrated into the chromosome and then it becomes a "supermale" Hfr, when it's so integrated. But in this case it cannot package an externally viable particle, which is what a virus can do. So it has an incomplete cycle as a virus and it can only get from cell to cell by cell conjugation. In normal life history the F plasmid doesn't have a preliving state of a packaged virus particle: it wouldn't last very long as free DNA, if it ever got out of the cell. Those observations led to the formulation of the plasmid concept, so they were a re-integration of this variety of experimental observations. They were not part of my original research program. In those days you could walk down a path and stumble on a stone and pick it up and there would be a golden newt under it almost every time you went anywhere. I was about to say that the initial observations, both of lambda and of F, came from Esther's very astute abilities to observe what was happening in the course of other very routine work. There were two very similar occasions when she would show me these plates and there was this curious anomaly; she had just noticed that things weren't going the way they were supposed to in certain combinations of crosses. She does certainly deserve very great credit for her observational skills. It took more than that to go through the experimental analysis and I won't say any more than that.

BOHNING: I'm looking at the time frame from starting at Wisconsin to when you went to Berkeley in the summer of 1950. How easy was it for you to get graduate students?

LEDERBERG: I got them, but it wasn't easy.

BOHNING: How did you attract Norton Zinder?

LEDERBERG: I can't remember. I suspect mostly by writing letters in the informal network. We didn't do much advertising in those days, but I don't recall if we did. I'll have to look up my [M. L.] Morse correspondence. In Norton's case he was recommended to me by Francis Ryan. So it was also word of mouth. E. R. Lively and Miriam Fried were word of mouth associations.

BOHNING: You said Norton didn't start your second year, so were you doing much of your own work that first year, or were you just getting things set up?

LEDERBERG: Setting up. We did get along, and here's where Esther worked like a Trojan in the laboratory. I had a research assistant. It would have been probably three people and maybe a part-time dishwasher. That was the lab that first year, and it didn't get to be much larger than that. There might have been two or three others.

BOHNING: You received offers of other positions at least twice in this time frame, once from Chicago in 1949 and once at Oak Ridge in 1950.

LEDERBERG: Well, there were more than that. But up to 1950, yes, that's right. I didn't take Oak Ridge too seriously. I'd always admired Chicago for its intellectual richness, and its preoccupation with academic matters. But it came a little too soon; we'd just moved and the idea of living in Chicago on the South Side was not very appealing. I think that was what it hinged on. We did have contacts there [the Novicks] that we visited fairly frequently, so we were not totally bereft.

BOHNING: Your notes talk about the first press notice in May of 1949 in a Madison newspaper about bacteria having sex life.

LEDERBERG: I'd run into this somewhere, and I just thought I'd catalogue it that way.

BOHNING: This is something you've done retrospectively, rather than at the time. I was after how you reacted to seeing something like that in print at that time.

LEDERBERG: I wasn't too interested. In fact, a few years later, there was a well-known Wisconsin painter, Aaron Bohrad who told me *Time* magazine had commissioned him to do a portrait; I was going to be a cover personality on *Time*, and I said I didn't want to do it.

BOHNING: Why?

LEDERBERG: I didn't think that public hoopla would be of any particular use to me or to the public. I didn't think the press would handle things in a particularly accurate or graceful way, and I didn't like being a subject at somebody else's hands. I guess I still have some feeling about that. I've refused to be sculpted and refused to be subject to some great photographic artists. For them to pursue their profession is fine, but I have something else to do than to be their clay.

BOHNING: Let's jump ahead a little bit then since you're talking about that. You couldn't avoid the media when you won the Nobel. How did you handle that?

LEDERBERG: I tried the best I could. I don't think I went out of my way to attract attention, but it was not really in my hands. So I began to think a little bit more carefully about what use to make of it. It was both unavoidable and easily obtainable, and there's a fine line between the two. Maybe I developed a little further confidence that I could control what would be a very difficult interaction, and that I'd better get into it. So I began to start thinking of being a voice for something. It was a question of what? It eventually materialized that not that many years later I decided I wasn't going to entrust my interactions with the press to the press; I was going to write on my own. Starting in 1966 I had my own column in *The Washington Post*. That's flipping from one extreme to the other, but I was avoiding what was my real fear and bane, that of being misrepresented, over-sensationalized, and dealt with in a shallow or inaccurate manner by the press. I guess I still feel that way. [laughter] I much prefer to speak for myself than have somebody else do it for me.

BOHNING: That headline from the Madison paper is typical of what the press will do with something like that.

LEDERBERG: Yes. I remember being offended by it, but it was what I knew would be the fate of any press mangling. I didn't know any Walter Sullivans or Larry Altmans in those days. If I had, I think I might have been more than happy to confide in how they would deal with it. I was really quite inexperienced in this matter. I started out with certain prejudices and then had the feeling that I wasn't sure that this couldn't get out of control, and I had better shy away from it. I was not interested in public publicity. I didn't see what it had to do with my scientific work. I was never adverse to trying to teach others, but it didn't seem to me that this press coverage was going to do that, either.

BOHNING: Maybe I could pursue that a little more, because what is the responsibility of the scientist towards educating the public?

LEDERBERG: I don't lay a trip on individuals. I think it's very important that the public be educated. I think that scientists who have the knack don't need to be told. I will say that it's a responsibility in the sense that we shouldn't chide or downgrade people who exercise it. I think Carl Sagan has got some inappropriate knocks for that reason. I wince a little a bit when I see some of what he's had to do, or wanted to do, but after that instant reaction I go back and congratulate him. I'm glad he's a headline hound because it helps motivate him to do something that somebody really ought to do. [laughter] But it doesn't mean I want to operate in that mode. I speak to a little different level of audience than he does, and I think they're both important roles. I don't think I wrote anything for public interest or consumption before the early 1960s. The first place can recall that is the Ciba symposium, The Future of Man, which was held in London in 1962 (33).

BOHNING: How did this column in *The Post* get started?

LEDERBERG: I was sitting on the airplane from Nice to London with Nigel Calder, and then I was going to on go back to New York. We were both attending the meeting of COSPAR [Committee on Space Research]. He was reporting on it for either the *New Statesman* or the *New Scientist*, if the latter was in being by that time. He asked me if I would be willing to write a regular column for it. I said I didn't think so, but I would give it some thought, and I found that I was giving it some thought. As is my wont in those cases, I said, "Well, don't just jump. Try to understand the context of this interest, and if you're going to do it, ask yourself is this the optimum of that genre that you want to do?" I thought, for the effort given, there would be a rather modest audience in the *New Scientist*, especially at that time. If it were *The New York Times*, I might think that the effort would be worthwhile. I tried that and didn't get very far. I did have some contacts with the Graham family, and also with Howard Simons, who was the science editor of *The Washington Post*, and that materialized. So that's how it happened.

BOHNING: How long did that run?

LEDERBERG: Five years and then some. It kind of petered out during the sixth year. I stopped doing it primarily because I could not turn myself into a reporter in the sense of writing a column or doing a piece and then putting it out of mind and doing something else. I ended up continuing to research the columns that I did, often by a factor of ten or a hundred times more. [laughter] Once a subject had opened up I couldn't let it go. I decided, that's okay, you may pull all that material together in various ways, but you can't keep multiplying it. I was too much of a scholar to jump around from one topic to another and then give it the depth that I felt it needed.

[END OF TAPE, SIDE 12]

LEDERBERG: It was also a drain on time. A year or two into that, I'd been through a divorce and married again. I know it was quite a strain on my new family relationships to have this on top of everything else; a very sharp deadline makes a difference. [laughter] Week after week after week.

BOHNING: So it was weekly then?

LEDERBERG: Oh, yes. Without remit. So that was part of it. But mostly it's what I just said. I've still got forty or fifty linear feet of files that pertain to the subjects I was researching in that vein. So that was my education in public affairs; basically, the research that I started and then continued to do around this newspaper column.

BOHNING: What kind of topics did you select?

LEDERBERG: They were not organized *a priori* with a particular reflection. About half of them had to do with what we would now call environmental issues of one sort or another, or of related matters concerning to health and the world we live in. There wasn't much environmental news in the press in those days, which is hard to remember as well, so today there would be much less need for it, or else it would be countercommentary, rather than commentary. If that's half, maybe a quarter was devoted to national security, military, arms control related kinds of issues. SALT, nuclear weapons bans, things of that kind. The stuff that would appear in the *Bulletin of the Atomic Scientists*, which is one of the sources of impetus to be involved in this way; Leo Szilard is somebody who kept urging me to do more. And the other was just miscellaneous scientific topics that might usually be of some interest about human nature. It was called "Science and Man," and today it would have to be "Science and Humankind."

BOHNING: You've mentioned Leo Szilard a number of times. I picture Leo Szilard in a totally different vein. How did he become involved in this kind of work?

LEDERBERG: Richard Rhodes has a beautiful account of his role in the making of the bomb (34). After the war Szilard wanted to have nothing more directly to do with it and decided to go into biophysics. He got an appointment at Chicago and recruited a then-young fellow called Aaron Novick to be his lieutenant to be his laboratory implementer. Novick had also worked at Los Alamos. They were a very interesting team. Aaron was interested in doing experiments and bringing things to an interesting conclusion, and Leo was wandering all over the place and theorizing about the nature of life in all kinds of dimensions, buttonholing people and using that very special physics mentality that he had for pursuing biological questions. He also very shrewdly organized what he called a phage seminar. Phage is a buzz-word for the general area. There was a lot going on in the Midwest in those days, and we'd meet once a month, usually in Chicago, but sometimes in Madison or Urbana or Bloomington. Those are the main centers that we'd get together in and just talk about what was happening in this new area. This was ostensibly for his education but it got us together and also had the benefit of his kind of probing. So he was actively involved in related areas. His lab did the first repeats on crossing E. coli; that was not published. Their research got onto the uses of the chemostat. This was a method of regulating the growth of bacteria by a continuous flow of a nutrient. You'd set up a bacterial culture, inoculate it and then you'd have a drip that one drop every minute there would be fresh culture medium and one drop every minute the culture as a whole would be withdrawn, so it was a constant volume system and it would come to a steady state. It could be set up in ways that was nutrient limited, so you had bacteria in a physiological steady state, but of various degrees of nutrient limitation. How fast would a bug grow if it had concentration of six micromolar tryptophan compared to ten micromolar and so on. They were able to do a lot of very quantitative studies on bacterial growth using that apparatus, and it suited them just fine as physicists to operate in that mode. They had some very interesting things come out of it. At the same time, Leo was very active in arms control matters and certainly inspired me to try to pick up those same themes.

BOHNING: That was in the height of the cold war days, wasn't it?

LEDERBERG: Yes.

BOHNING: If I read your notes correctly, you were consulting with someone at Fort Dietrick. I was wondering whether there was a biological warfare aspect to any of that?

LEDERBERG: I didn't know how much of an offensive program they had going on, and I had no relationship to it. Werner [von] Braun was my main contact and he had a research program on the genetics of brucella. From what I knew of it, it was for the development of vaccines. I was vaguely aware that we were developing offensive weapons. I was very much interested in arms control arrangements that would abolish it. I understood the need to do that bilaterally. At least at that time, I felt that we couldn't just unilaterally drop our interest in the matter and have the Russians continue with unhindered and unlimited programs and that was something ought to be bargained about. But I didn't get involved in the politics of that particular issue in any significant way until much later. I informed myself about it and saw BW [biological warfare] as a horrible threat that had to be stopped. But the focus of that would have been arms control arrangements. It was hard to see how that was ever going to be negotiated and how it would be verified. Until we had overhead inspection, we had nothing that would have been of any benefit, and as we know from our experience with Iraq, even with it and with a treaty, it's of limited utility. So that was my connection with that.

My main consulting then was commercial, starting with the Bristol Laboratories in 1950. They were in Syracuse, New York. It was a little bit of a nuisance getting there from Madison to, two towns that don't have direct air service. [laughter] Joe Lein was the person in charge of microbiological antibiotic development. He wanted some input on how to use genetic technologies. They listened with great interest and they paid we what seemed like a great stipend. I don't know, it might have been a hundred dollars a month, or something of that sort; maybe it was fifty. But they never did anything with it. [laughter] But it gave me some early introduction to how R & D works in that industry and left me with less than an upmost opinion of how far sighted it was. They were themselves the world leaders. They were betting on a different horse. They had [John C.] Sheehan doing the chemistry and they worked out all kinds of semi-synthetic penicillin. They had great success; it made them less desperate to try to look to what would today be called biotechnology as a route to improvement. He would attend some of our consultants meetings.

BOHNING: Sheehan?

LEDERBERG: Yes. He's written a wonderful book about that experience (35). So that's how I got my first familiarity with how that kind of thing went on in the world. That was a long time ago.

BOHNING: You said flying was difficult. It was difficult under any circumstances in 1950.

LEDERBERG: If you had a direct connection, it was great. Those DC3s were wonderful airplanes. [laughter] It just took a little while. It was more the schedules than the aircraft themselves. If you were going across the country it took a little longer.

BOHNING: During that three-year period before you went to Berkeley.

LEDERBERG: Berkeley was just a summer, you know. It's an interesting turning point.

BOHNING: Yes.

LEDERBERG: All right.

BOHNING: Were you feeling more and more comfortable with a university position? You started out at Wisconsin and had wanted to do lots of research. Now you've got three years and you've got graduate students and you're getting the work done. Are you feeling more comfortable being in a university setting?

LEDERBERG: I was never uncomfortable with it, never have been, never was, never will be. [laughter] But I was becoming ingrained with it, and I think feeling that I was well-integrated into Wisconsin. I think whatever might have been in the background before I got there on anti-Semitism, it was never an issue. I don't think it was even an issue underground after the first introductions. First of all the world was changing, and then as you get to know people it becomes less important. It was also notable that a number of other new appointments were Jewish people. There had always been some in the H & S, but I may have been the very first one, or if not, very close to it, in the school of agriculture. But then the school as an academic unit was upgrading, taking itself much more seriously, setting higher and higher admissions standards until they were just as rigorous a program. It used to be a little bit demeaning that it was kind of the easy degree for dumb farm kids. That's crazy on all sides. They're not dumb, they don't get an easy degree, and I think it was doing them a disservice to set less than the highest standards for them. They've done just fine in a more integrated basis.

BOHNING: So at this point you were pretty well convinced that you would stay in the academic track?

LEDERBERG: Oh, I never doubted it. I'd been on the academic track since I entered Columbia.

BOHNING: But the M.D. thoughts disappeared very rapidly?

LEDERBERG: Oh, M.D. versus the rest?

BOHNING: In your writings you seem to hang on to that little bit of "what if I had stayed in medicine?"

LEDERBERG: I saw the medical school as part of the university and I would not have been interested in the medical school if it wasn't. That was roughly the case for the one at Madison until about 1955. I met John Bowers probably in 1950 (it might have been in 1953; it might have been both) at Curt Stern's house in Berkeley. Stern was a professor of genetics, a real wonderful elder statesman in the field. His life work had been in Drosophila genetics, but he wrote a textbook in human genetics and he taught that at Berkeley. It's a wonderful text (36). He'd gotten to know Bowers, who was the director of one of the major programs in the AEC [Atomic Energy Commission] on biological effects of radiation, which included a fair bit of genetics, genetic damage and that kind of stuff. He was an experimentalist, what I would call contract big science these days, but I didn't make those distinctions at that time. I didn't know when I met him that he was being considered to be the new dean at Madison. I'd gone on at some time about what I thought was right and wrong with medical education, that it didn't have an adequate scientific base and so on. He turned up as dean and asked me to come and see him. Was I serious about my interest in the matter? I said I sure was. What would I want to do about it? I said, "Well, how about starting a department of genetics in the medical school." And he said, "Sure." [laughter] So, that happened. [laughter]

BOHNING: Just that easy.

LEDERBERG: He wanted to make a splash and wanted to do something distinctive. It was something he could have some feel for himself, and it was very rational thing for him to do, given all those premises. There were some issues about how to pay for it. When push came to shove, it wasn't always as easy to get the appointments through that I thought he had promised in that direction. But his heart was in the right place.

BOHNING: Where was biochemistry?

LEDERBERG: In the Ag school.

BOHNING: In the Ag school, too?

LEDERBERG: Yes. He set up something called the Enzyme Institute, which had some autonomy but at least grew out of the Ag school. It may have been biochemistry's answer to this question about having an acknowledgement of its roots in basic science. It was the same cast of characters. A couple new people were recruited. David Green came in. I had high expectations of it, but I couldn't get any of them interested either in DNA chemistry or in protein synthesis. They thought it was too hard. It really wasn't until I left that they recruited [Har Gobind] Khorana to the enzyme institute. I might not have gone if he'd been there. [laughter]

BOHNING: What happened to genetics in the Ag school? Are they still there?

LEDERBERG: What eventually happened, and it's sensible, is that the two departments have merged, and they have a rather unique situation. It's a department which is both in the Ag school and in the medical school. It has a unified administration and gets some of its budget slots on each side. I think the medical side of it is the predominant one today in terms of where the action is.

BOHNING: The Berkeley summer. Your notes are filled with all kinds of things about Berkeley.

LEDERBERG: That was an exciting experience. I had met Mike Doudoroff and Roger Stanier at Cold Spring Harbor a couple of times. I think Esther had known one or the other of them, too. She'd spent a summer in [C. B.] Van Neil's course at Pacific Grove, the Hopkins marine station at Stanford. Stanier was very much Van Neil's protege and collaborator over the course of that. I think that was the immediate instigation, but, yes, they were not segregated departments. There was a big department of life sciences, and they just invited me to be a visiting professor for the summer and sort of bring bacterial genetics to that campus in that way. It was exciting. It was my first trip to California. The Berkeley campus was a rather richer environment, especially the depth that was going on there. With the virus laboratory and the microbiologists that I just mentioned, it seemed like it would be a somewhat more stimulating place. And you could work there in the summertime, which you couldn't do in Madison. [laughter] So I really first developed a yen that I might want to move West sometime. I visited again more briefly in 1953; this was to receive the Lilly award lectureship, and that reinforced it. At that time Stanford was still pretty sleepy. Ed Tatum had gone back there, but not much else was going on. The medical school was still at San Francisco, so I didn't give much thought to Stanford as a place. I think we began talking about possibly some day I might come to Berkeley. So that started in the summer of 1950.

BOHNING: The Korean War started then, too.

LEDERBERG: Yes. While we were en route. We drove across the country, and I remember getting the news on the way.

BOHNING: That was also the [Joseph R.] McCarthy era.

LEDERBERG: That's right.

BOHNING: Were you caught up in that at all? Did you have discussion with the people there about that?

LEDERBERG: The discussions were more in California than Wisconsin. Until he became indiscreet, and it was his downfall, McCarthy was very careful not to bring this stuff home where his own constituents could look more closely at it and know what bullshit he was peddling. There was a very limited degree of that kind of red baiting in Wisconsin. There was a little bit of it, but nothing compared to what you saw in Washington and elsewhere. They did make a big fuss about the loyalty oath. My view was that I didn't see any problem with the substance of it, but I thought it was egregious that the regents felt it necessary to impose it. It was humiliating and insulting and it has done exactly the opposite. The presumption of innocence until proven guilty and there's no reason in the world to think there's any problem. I don't think there was; I don't think any security risk has ever come out of Berkeley in that regard. But to this day I still have mixed feelings about that whole episode. I think there was a core of pro-Stalinism in this country. Anybody who could survive the Ribbentrop-Molotov Pact in 1939 and not see Russia as the autarchy that it was, was being pretty much of a dupe. I took quite seriously the issue of the defections. Oh, who were the spies who were tried and then convicted?

BOHNING: The Rosenbergs [Ethel and Julius Rosenberg]?

LEDERBERG: The Rosenbergs, yes. I thought and still do think that there's a pretty good case that they were traitors and that there was some level of serious revolutionary activity that was a conspiracy to overthrow the government. It was supported from Moscow. But McCarthy just amplified it way out of any kind of reasonable proportions. I didn't think then and still don't now, that any of the people he himself fingered were guilty of what he was claiming. He was drawing on a current of fear and concern that had some tiny core of merit involving anybody. I also felt that we were being victimized by the communists as well as by the right. They kind of needed one another, and this notion of covering up on whether you'd ever been a member of the party and the rest of it was something that the hard core communists were eager to inculcate, not primarily to protect themselves, because the most evident of them were quite visible, but in order to divide and weaken the country. I guess I would have favored a view of the matter that would have said, "Yes, I was a communist. I didn't do anything wrong. I was complying with the law. I'm going to stand by that." If more people had done that, and eighty percent of the ones who took the fifth amendment would have been in that category, it would have disarmed McCarthy in the first place. McCarthy was a tool of the hard core communists as much as vice versus. I really felt we were being done in by extremists on both sides. I had a lot of sympathy for what was then authentically the liberal element view of the matter. I thought there were civil liberties questions involved in those kinds of inquiries. So I supported the opposition to the oath. But I thought it was a bad strategy to oppose it and it would have been a better one to say, "Yes, I was a communist and I had good reasons for it at the time, and if I'm not today I have repudiated. Maybe some others don't share that and maybe they have undertaken some activities that ought to have further inquiry, but there's no reason for me to be involved in that." It's a very unpopular view, and I don't espouse it too loudly anywhere. People might misunderstand it and it would come under attack from both the right and the left today.

BOHNING: It's a very plausible one. Certainly for people who joined the party in the 1930s in the midst of the Depression, there was a rationale.

LEDERBERG: Thousands of people did, and they had no reason to hide it. They began to, it because if it became something that you were ashamed of, people would think that where there's smoke there's fire. I really think it could have been disarmed. That would have taken some courage. I don't know if I would have had the courage myself to do it, but it was not a very good defense. People got fired anyhow, and the notion that you could somehow escape because you wouldn't be found out because somebody didn't tell on you, I thought was pretty foolish. So it fed into McCarthy. He's one of the vilest people we ever had and I've never had any hesitation in saying that.

BOHNING: Towards to end of the McCarthy era, was the university affected at all?

LEDERBERG: Hardly at all. There were one or two minor skirmishes. When he started going after a couple of Wisconsinites, all the local press who had been sort of divided about him started denouncing him too. It was crazy; when he started going out after [Dwight D.] Eisenhower he was finished absolutely. I can't swear that there was no single person at the university who was somehow singled out or injured, but it was nothing like as big an issue there as it was in California. I don't think the regents took much attention in terms of the symbolic acts of genuflection.

[END OF TAPE, SIDE 13]

LEDERBERG: Generally I was really very much preoccupied with my own research. I took seriously these questions of academic liberties, but they were not in any way at the top of my mind. I was not engaged in consulting in Washington during that era.

BOHNING: I'm not sure if there's anything else about the Wisconsin period. We've talked about getting genetics in the med school finally. But you had some frustrations with that, didn't you?

LEDERBERG: Yes. We got started and there was some ambiguity about what scale it was going to be and what the funding was. That slowed me up in terms of recruiting. We didn't really get very far during that time I was there. I think it really didn't go operational until 1957, so it was about 1955 when it was formally established. We probably did get to where I taught at least one year's medical class. The teaching was the thing that was the operationally unique step. The med school as a whole was undergoing a lot of changes at the time. Things hadn't shaken down very far during the time that I was still there. It did mean that starting from 1955, I did end up being busier and busier with administrative detail, just trying to get things moving. I don't have a clear image of it, but I must have been spending twenty to twenty-five percent of my time just trying to pull all the pieces together, get the funding and that kind of thing. So that was my evolution in that direction. I think up to that point I would have been either in the classroom or the library or the lab. I had no reason to be anywhere else.

BOHNING: Did your graduate student level stay pretty much the same?

LEDERBERG: Yes. I would have two, three at the most, maybe one postdoc at any given time. It was a pretty small lab. I didn't squawk very loudly about it. I didn't realize what influence I might have had. It wouldn't have been easy, but I think I could have swung more space. There must have been some of that getting ready in the medical school, which I don't think I ever got to occupy; there was some new building going on and that was part of what could have eventually have been. Funding was adequate for what I was looking for. NIH was coming into play. My first grant was for three thousand dollars. It started growing after that. I did end up having most of my support eventually from NIH grants. It started out with the Rockefeller Foundation Awards and a little bit of department money, which I mentioned to you. Let's review Wisconsin from the perspective of the thematic issues that I've mentioned to you. First of all, uniqueness. I was a very young fellow, pushing right along, pulling the department together. The issue of precocity becomes less and less as I eventually did get to be twenty-five. [laughter] It became less of an issue. I'd say by that time, if there was anything unique it would have been not a qualitative singularity, just having a very active, intense research program in an area of major interest. But there might have been fifteen or twenty others more or less like that at that point.

In the generic aspects, I think I was the model of an active research scientist. That was my life model at the time. I felt very excited about what was happening from day to day, and I think fairly content. I felt a little bit restless about the sort of the larger philosophical frame; the medical involvement was an issue. We could have done more about other elements of culture. I don't want to depreciate them. Wisconsin was not totally bereft of them, by any means. It wasn't quite the excitement that one experienced either at Berkeley or Stanford. I guess that's true today, too. It's a good school. I certainly don't want to depreciate it, but it might not be at the very top of the list. There were a few outstanding individuals. I was probably as close then to living out my life model as at any time. That was the very core of how I would have viewed myself. I was probably itching for more application. Here I was making an important contribution to the substratum of biological investigation. I think that was both a large and realistic view of how important it was. I don't think I underestimated it in any way. It was wonderful fun being in the middle of the action. I always wanted to keep an eye out for how you can make something useful out of what you're doing, and that wasn't eventuating. It wasn't happening at Bristol. I didn't see too many other avenues for doing too much about it, so I just thought, "Okay, publish your work. Others will pick it up and it will come to mean something someday."

Decision points I think we've covered. The main ones—going to Wisconsin in the first place, not being more ambitious about growing and space and funding and so forth, although I also wonder if I might not have overplayed my hand if I pushed it any further. In retrospect I think not; I had more potential power and influence than I understood or acknowledged at the time. It was only when the issue came about my leaving, that I realized that people really would care to that degree. I don't mean that they were careless but there might have been a way to get the deans to try to undertake some special measures. The Enzyme Institute had far better laboratory facilities than I did, and one thing I might have done would have been to try to press for membership in that. It just never occurred to me to ask for things like that. It may seem a strange thing to say, but I was unduly modest on that score. Decisions about trying to set up a medical school—that was getting on the slippery slope, but I can't see how I would have or could have done otherwise. Then eventually deciding to leave there. So I think we've covered the major issues. What else was going on in biology during that decade? That's a lot of homework. There's not much in my oral history, in my recollection, that would add to trying to do a piece of scholarship. But in fact, let me get something out for you, and you take a look at it and bring it back. I'll give it to you before you leave. I've written a history of what I call the vicennium, the period of 1930 to 1950, which I see as the crucial flowering of microbiology, bringing it into the modern era (37). It's not quite the right interval for this one. Certainly the playing out of the DNA story is the big thing that was happening during that time. There was a lot of activity in Paris. There were the beginnings of the messenger RNA, enzymatic induction and so on. That was the real center of the action, at that point, for the details on DNA structure, corroboration, bolstering, nothing as nearly as revolutionary as DNA. You sort of have to get to the end of that decade before you in a sense have the RNA story starting to emerge with the genetic code and so on.

But that is relevant to my own situation, because here I was still doing experiments with the technology of 1946 when the field was becoming increasingly more dominated with biochemical inputs. I was picking up some of them. I wrote a paper on beta-D-galactosidase (38). But I really wasn't adequately equipped to do serious molecular biology. That was harder then than it is now, because you needed ultracentrifuges. We didn't have gel electrophoresis and things of that sort; they're just wonderful further assistance. That is one of the reasons I left Wisconsin; I wanted to be in an environment at least where work of that level and that kind was going on and I had some better chance of participating in it. Arthur Kornberg at Stanford was my attractive magnet from that point of view. I wasn't much involved in popular culture at that time. I would have to look around. I think I did have a letter to the *Bulletin of Atomic Scientists*, in which I said, "You're making a big fuss about radiation damage and let's measure it, and that's okay. But nobody has said anything about chemical mutagenesis." I think that was the first public mention of it, and that was in 1955. I remember some correspondence with H. G. Muller asking him what he thought about that. He said, "Yes, that is an important issue and should we try to take it to the Academy?" He was member of that and I wasn't. Nothing came of it; there was no receptivity. That seems crazy today. [laughter] There are environmental mutagens everywhere.

So I do think that was the introduction to that subject. A couple of people had done experiments with chemicals, as I had done, but not make that extrapolated leap to where these are a public health hazard, no less and no more than radiation is, for comparable reasons. I can't think of any other public issue I came in on that is special to my own science. I had some loose affiliation with progressive politics in Wisconsin. It was an extraordinary state from that point of view. It had the [Robert] La Follette kind of tradition. In civil liberties I've forgotten what the hot challenges were, but I did belong to the ACLU [American Civil Liberties Union] at that time. But those were second-order matters. The lab was really the main focus of interest. Social relations and lab environment. I think I've described that to you as fairly stable in the cast of characters. The names are in the record; I don't remember them nearly as well as I can find the detail. Gatekeepers were pretty friendly. I wasn't asking for very much, so I got all I asked for—grants, papers were published with any problem. Now and then, I started getting a few papers for review. I should look up when I started going on NIH study sections; that would have been my first connections with Washington. It was during that period, probably the early 1950s. I was starting to sit on some of the sections.

Oh, I should bring up something, and that's Gene [Eugene] Garfield. He published a paper in 1955 about the concept of citation indexing, referring to *Shepard's Citations* (38). You know I was already interested in scientific literature quite deeply at that time. The paper made an important impression on me because it was something to worry about following. Two or three years later, I asked myself whatever happened to Gene Garfield's idea? I had not met him. I knew about *Current Contents*. Then it hit me that that was a self-exemplifying question. If there were a citation index I would know how to answer the question and then that sort of thing would come up over and over again. [laughter] So I wrote to him and said, "What's happened to it? What are you going to do about it?" I think I started my relationship with him in early 1958; it might have been slightly earlier than that. He said he didn't know quite where to go. I'm not sure who brought up what first, but the idea eventuated about applying to the NIH for a grant to do a demonstration. I told him I'd be agreeable to being an advisor to his project, so it was submitted to the genetic study section. Since geneticists have an instant reflex about generational relationship of parent and descendent, which is analogous to paper and bibliography, they can understand the idea of citational networking instantly. It's amazingly difficult to explain this to others sometimes. They very promptly supported the concept and ordered a small grant to just demonstrate it and produce a sample genetics citation index. That was done. I was an advisor to it, and it looked like the thing could work. There was a sufficient bulk of exemplary material to give you a sense of just how useful it would be. It was still fairly primitive. It had very abbreviated citations and everything was on punch cards. A little bit of mainframe computing was what was available in those days. It inspired Gene to say, "This thing looks like it might work. There is enough interest in it out in the community; I'm going to see if I can make a business of it." I remember saying, "Gene, don't do that. You're going to lose your shirt. [laughter] You'll never break even on it." [laughter] It also reflects a fairly early interest I had in using computers for some useful purposes in science. But that was the beginnings of the citation index and my relationship with Gene. I think he got started on it and then in the very early 1960s, maybe 1961, he asked me if I would join his board and offered to allow me to make a small investment in the company at that time. As a matter of fact, I think I was his first shareholder. That was the best investment I ever made. Gene did make a profit out of ISI [Institute for Scientific Information].

BOHNING: In spite of your predictions.

LEDERBERG: Yes. That's something I feel pretty proud about, having had some part in this. Then also exobiology started during that interval. We haven't said anything about that yet. Sputnik was the incident of that. I've already done some oral history on that. Steve [Steven J.] Dick is an historian at the U.S. Naval Observatory, and he did an interview with me just a couple of weeks ago focused on just that episode. So I think I won't repeat that here. We'll get the text of it and share it. There's not too much overlap with the other stuff I have here. It relates to how I met Carl Sagan and got him introduced to NASA. It began with Sputnik, which sort of made things possible. I've had a background interest in the question of whether there could be life elsewhere than on earth, but never saw any way to do anything about it. So it was left in the realm of pure speculation, and Sputnik seemed to open an era where than might possibly eventuate. I got to be quite active on that at that point. I haven't said anything about theories of antibody formation. That was also still during my Wisconsin days. I've written a memoir on that (40), titled, "The Ontogeny of the Clonal Selection Theory of Antibody Formation." That's been pretty thoroughly written down, and I can't think of too much that would add to that. The background on that was a Fulbright Fellowship, which I took to go to Australia for a few months, just a little bit of wanderlust. I'd never had a sabbatical up to that point. I was going to work with Mac [Macfarlane] Burnet in Melbourne, ostensibly on genetic recombination of bacteria in the influenza virus. He had discovered a system of genetic exchange there. He was a wizard at flu and knew nothing whatever about genetics. I thought there might be a useful reciprocity in that.

When I got there he told me he had dropped working on that subject and he was working on theories of antibody formation. I have written about this in another memoir in some detail (41). This was while I was still at Wisconsin. I did some experiments with Gus Nossal, who is now Burnet's heir as the director of the Hall Institute in Melbourne. He's been there for some time now. They did support the view that single cells produce only one species of antibody and that an animal making a lot of different antibodies is segregated cell by cell, which is part of the clonal selection theory, which is now universally accepted. Ken Shaffner has written a little bit. Just recently I got a paper from him on the acceptance of the theory, and I think he's done a pretty good job of filling in that detail (42). That's still something I feel I want to go into in a little more detail some day, of the resistance to it. Both Burnet and I faltered for a while. It looked like it was damning evidence and we believed in evidence. We were trying to find some way to rescue it. It still seemed like a very good idea, fundamentally. The evidence—the negative evidence—turned out to be an artifact; it was as good as gold all the way, and is now the common dogma. That was a tiny experiment. It was centered on a distractional experience, but it had wonderful proofs. It may have been the first time I really saw how far one can go with purely theoretical speculation, if you have a good firm background. That's without doing much experimentation. I had done that before, but I'd always done my own experimentation after that.

There was a limit to how much one could do in one's own lab. I had my first brush with computers at Wisconsin in the early 1950s, 1953 I would guess. I took a course on the plugboard programmed, card machines, and then decided that although you could do standard deviations with a whole pack of cards, it wasn't worth it and there didn't seem much more to do at that stage. I was just keeping an eye out for what computers might be useful for some day and sort of kept it in reserve. My first contact with those machines was in 1941, when I was a high school student and attending American Institute Science Laboratory.

[END OF TAPE, SIDE 14]

LEDERBERG: The American Institute Science Laboratory was a predecessor to the science fair programs. This was then sponsored by Westinghouse and IBM. IBM lent some space that we used as a lab, and we could tinker with what we wanted to do with some very remote supervision. They had one of their advanced card calculators on display at that time. I didn't do very much with it, but I was able to see what the best of the electronic art was as of 1941, and thinking, "Well, that's a machine that's going to be interesting to biologists someday. It sort of emulates what organisms can do in some very, very crude way, but it's not worth investing as yet." When I got to Stanford, that changed that (43). I can't think of any other themes for that particular decade, so maybe that does wrap that up.

BOHNING: That's a good point to close in the day.

LEDERBERG: Okay.

BOHNING: Again, I appreciate the time you spent with me.

[END OF TAPE, SIDE 15]

[END OF INTERVIEW]

NOTES

1. M. Bodansky, *Introduction to Physiological Chemistry* (New York: John Wiley & Sons, 3rd ed., 1934).

2. Richard A. Balford, "Remembering Early Chem. Labs," *Chemical and Engineering News*, 70, No. 23 (8 June 1992): 3.

3. See Russell E. Marker, interview by Jeffrey L. Sturchio, 17 April 1987; The Chemical Heritage Foundation, Transcript #0068.

4. E.B. Wilson, *The Cell in Development and Heredity* (New York: MacMillan, 1925).

5. H.G. Wells, Julian S. Huxley, and G.P. Wells, *Science of Life*, 4 volumes (Garden City, New York: Doubleday, Doran & Co., 1931).

6. Paul de Kruif, *Hunger Fighters* (New York: Harcourt, Brace and Company, 1928); de Kruif, *Microbe Hunters* (New York: Blue Ribbon Books, 1926).

7. Bernard Jaffe, *Crucibles* (New York: Simon and Schuster, 1930).

8. H.G. Wells, *Chemical Pathology, 5th ed*. (Philadelphia: W. B. Saunders Company, 1925).

9. Joseph Needham, *Chemical Embryology* (Cambridge, England: The University Press, 1931).

10. E.B. Wilson, "The Physical Basis of Life," in Jerome Alexander, ed., *Colloid Chemistry, Vol. II* (New York, The Chemical Catalog Company, 1928); pp. 515-524.

11. R. Chambers, "The Nature of the Living Cell as Revealed by Micromanipulation," in Jerome Alexander, ed., *Colloid Chemistry, Vol. II*. (New York: The Chemical Catalog Company, 1928); pp. 467-486.

12. *World of Tomorrow* (N.Y. World's Fair 1939). Produced and Directed by Lance Bird and Tom Johnson. In the series "The American Experience," issued by WGBH Educational Foundation, 1989. Disseminated by PBS. Aired May 6, 1992 by WNET. See also: Gelernter, David Hillel. 1939, *The Lost World of the Fair*. New York: Free Press, c1995.

13. Aldous Huxley, *Brave New World* (Garden City, New York: Doubleday, Doran & Co., 1932).

14. [IBM Think story on the American Institute of Science Laboratory] "Think" Sept/Oct 1979. "The Year of the Gifted Children," v 45(5): pp 12-17.

15. [Film on AISL, Note 14]

16. H.C. Eyster, "Enzymes and the Law of Mass Action," *Plant Physiology*, 17 (1942): 686-688; Eyster, "Enzyme Action," *Science*, 96 (1942): 140-141.

17. J. Brachet and R. Jeener, *Macromolecular Cytoplasmic Particles Rich in Pentosenucleic Acid. I.* "General Properties, Relation to Hydrolyases, Hormones, and Structureal Proteins," *Enzymologia*, 11 (1944): 196-212.

18. O.T. Avery, C. M. MacLeod, and M. McCarty, "Studies on the Chemical Nature of the Substance Inducing Transformation of Pneumococcal Types," *Journal of Experimental Medicine*, 79 (1944): 137-158.

19. Joshua Lederberg, "Genetic Recombination in Bacteria: A Discovery Account," *Annual Review of Genetics*, 21 (1987): 23-46, and references therein.

20. Joshua Lederberg, "Bacterial Variation Since Pasteur," *ASM News*, 58 (1992): 261-265.

21. Scott F. Gilbert, ed., *A Conceptual History of Modern Embryology* (NY: Plenum Press, 266p, 1991).

22. Joshua Lederberg, "What the Double Helix (1953) Has Meant for Basic Biomedical Science," *JAMA*, 269 (1993): 1981-1985.

23. Joshua Lederberg, "A View of Genetics," *Les Prix Nobel en 1958* (Stockholm: Almqvist & Wiksell, 1958).

24. L. Luca Cavalli-Sforza, "Forty Years Ago in Genetics: The Unorthodox Mating Behavior of Bacteria," *Genetics*, 132 (1992): 635-637.

25. A. Boivin, "Directed Mutation in Colon Bacilli, by an Inducing Principle of Desoxyribonucleic Nature: Its Meaning for the General Biochemistry of Heredity," *Cold Spring Harbor Symposia on Quantitative Biology*, 12 (1947): 7-17.

26. Jan Sapp, *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics* (New York: Oxford University Press, 1987).

27. Doris T. Zallen and Richard M. Burian, "On the Beginnings of Somatic Cell Hybridization: Boris Ephrussi and Chromosome Transplantation," *Genetics*, 132 (1992): 1-8.

28. Lily E. Kay, *The Molecular Vision of Life* (New York: Oxford University Press, 1993).

29. Ephrussi-Taylor, “H. Genetic Aspects of Transformations of Pneumococci,” *Cold Spring Harbor Symposia on Quantitative Biology*, 16: 445-455, 1951.

30. L. J. Cole and W. H. Wright, The Application of the Pure Line Concept to Bacteria. J. Inf. Dis. 19: 209-221, 1916.

31. Norton Zinder, “Forty Years Ago in Genetics: The Discovery of Bacterial Transduction,” *Genetics*, 132 (1992): 291-294.

32. J. Sapp, *Evolution by Association. A History of Symbiosis* (New York: Oxford University Press, 1994).

33. Joshua Lederberg, “Biological Future of Man,” in G. Wolstenholme, ed., *Man and His Future* (Boston: Little Brown Company, 1963).

34. Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon and Schuster, 1986).

35. John C. Sheehan, *The Enchanted Ring: The Untold Story of Penicillin* (Cambridge, MA: MIT Press, 1982).

36. Curt Stern, *Principles of Human Genetics, 3rd ed.*, (San Francisco, California: W.H. Freeman and Company, 1973).

37. Joshua Lederberg, “History of Microbiology, 1930-1950,” *Encyclopedia of Microbiology* (San Diego: Academic Press, 1992).

38. Joshua Lederberg, “The Beta-D-galactosidase of Eschericha coli, Strain K-12,” *Journal of Bacteriology*, 60 (1950); 381-392.

39. Eugene Garfield, “Citation Indexes for Science,” *Science*, 122 (1955): 108-111.

40. Joshua Lederberg, “Ontogeny of the Clonal Selection Theory of Antibody Formation,” *Annals of the New York Academy of Sciences*, 546 (1988): 175-187.

41. Joshua Lederberg, “Ontogeny of the Clonal Selection Theory of Antibody Formation,” *Annals of the New York Academy of Sciences*, 546 (1988): 175-187.

42. K. F. Schaffner, “Theory Change in Immunology. Part II: The Clonal Selection Theory,” *Theoretical Medicine*, 13(2): 191-216, June 1992.

43. J. Lederberg, “How DENDRAL Was Conceived and Born,” In *ACM Conference on the History of Medical Informatics*, pp. 5-24, (Association for Computing Machinery, New York., 1987). Held: National Library of Medicine 11/5/87. Also 14-44 in Blum, B. I.

and K. Duncan, (eds). *A History of Medical Informatics* (ACM Press & Addison Wesley New York 1990).

INDEX

C

D