

CHEMICAL HERITAGE FOUNDATION

JOSHUA LEDERBERG

Transcript of an Interview
Conducted by

Audra J. Wolfe

at

Rockefeller University
New York City

on

18 August 2000

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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Audra J. Wolfe on 18 August 2000

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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

1943-1945 U.S. Navy
V-12 and Hospital Corps; Ens. USNR

1945-1946 Columbia University
Research Assistant, zoology

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

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

1950 University of California, Berkeley
Visiting Professor of Bacteriology

1957 University of Melbourne
Visiting Professor of Bacteriology

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

1978-1990 The Rockefeller University
President
1990- University Professor Emeritus

Honors

- 1957 National Academy of Sciences
- 1958 Nobel Prize for Physiology or Medicine
- 1960 Sc.D. (honorary), Yale University
- 1961 Alexander Hamilton Award, Columbia University
- 1961 Wilbur Cross Medal, Yale University
- 1961 Proctor Medal, Sigma Xi
- 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
- 1988 Distinguished Service Medal, Columbia University
- 1989 National Medal of Science
- 1991 D. Phil. (honorary), Tel-Aviv University
- 1993 Founding Member, Academie Universelle des Cultures
- 1995 Allen Newell Award, Association for Computing Machinery
- 1996 John Stearns Award for Lifetime Achievement, New York Academy of
Medicine
- 1997 Maxwell Finland Award, National Foundation of Infectious Diseases
- 1997 Mayor's Award in Science and Technology, New York City
- 1998 Dr. Mil. Med. (honorary), USUHS

ABSTRACT

Joshua Lederberg begins the interview with a discussion of his involvement in the contamination issues of planetary exploration. As interest in space exploration gained momentum, Lederberg was in the midst of discussion regarding protecting the Earth from possible extraterrestrial contamination. Lederberg felt that more emphasis needed to be placed on building a sound space program, one that focused more on planetary research rather than sending humans into space. Lederberg worked to develop alternatives to the “man-in-space” program, focusing on the importance for international cooperation. Lederberg served on several national committees, including the Space Science Board and the Kennedy Health Transition Team. After receiving the Nobel Prize in 1958, Lederberg joined the faculty of Stanford University, where he continued his life-long research in genetic structure and function in microorganisms. Lederberg continued to be actively involved in artificial intelligence research and in the NASA experimental programs seeking life on Mars. He has also been a consultant on health-related matters for both the U.S. and international communities, serving on the World Health Organization’s Advisory Health Research Council. Lederberg wrote his own column on a wide variety of topics, both scientific and non-scientific. Lederberg concludes the interview with a discussion of the environment at Stanford University during the Cold War, and thoughts on U.S. defense projects.

INTERVIEWER

Audra J. Wolfe received her Ph.D. in History and Sociology of Science at the University of Pennsylvania in 2001. She received an M.A. from that program in 1999 and a B.S. in chemistry and biochemistry from Purdue University in 1997. She was the 2000 summer Othmer Student at the Chemical Heritage Foundation. In addition, she has been the recipient of a National Science Foundation Graduate Fellowship and was named an Honorary Mellon Graduate Fellow in the Humanistic Studies for 1997-1998. She is currently researching and writing a dissertation on the public role of American biologists in the postwar years.

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INTERVIEWEE: Joshua Lederberg

INTERVIEWER: Audra J. Wolfe

LOCATION: Office of Joshua Lederberg at
Rockefeller University in New York City

DATE: 18 August 2000

WOLFE: We're going to begin this interview today, starting with the same place that your involvement with the space program began: the issue of contamination. You've talked with several people about how you became interested in contamination, discussing Calcutta and the possibilities of nuclear radiation and things on the moon. What did you see as at stake in these contamination arguments for scientists or for the public or for national prestige—however you'd like to interpret that?

LEDERBERG: Well, I saw planetary inquiry as a very important scientific domain—all the kinds of questions that have come up and continue to be investigated at the present time. I didn't want to see something as foolish as generating or dropping a load of radioactive waste, which could interfere in all kinds of ways with the issue of doing the science. Then a little less likely, there would be revealed later on, even dropping packages of spores, which is what a decaying organism would represent. If, later expeditions recovered evidence of microbial life, you wouldn't want to be plagued with the concern that they were simply the garbage deposits of the previous expedition. But above all I was perturbed that the whole program seemed to give no thought at all to the scientific aspects of exploration. It was more a circus for amusement. I thought to seek a way of presenting the issue, to try to put some piece of the program on a much sounder scientific footing—make it a serious human effort.

Now the other side was this was a moral equivalent of war; the U.S. and the Soviet Union were determined to show that there were going to be no limits in what they were willing to expend for their security. This was demonstrated, to a large degree by defense expenditures, but also by NASA [National Aeronautics and Space Administration]—and that they each did have the technological capability to ensure mutual destruction. Since that was not so credible without some sort of technological demonstration, better that it be in space than it be another Hiroshima or another Nagasaki. So I saw those as mitigating aspects of what I called the circus.

WOLFE: All right. Did you encounter much resistance in pushing for contamination and, if so, what kind of people resisted that or in sterilizing probes?

LEDERBERG: Well, I was actually surprised at how much consensual support I did get. I would say that 90 percent of the people that I talked to—must have been somewhat selective but

not grossly—caught on right away and were anywhere from lukewarm to warm to very enthusiastic supporters of the names of the groups that we'd gotten on the committees. I guess there were only two outstanding figures that I can recall that were adamantly opposed to putting this up: one was Phil [Philip H.] Abelson and the other was George [Gaylord] Simpson. Phil, I think, was so antagonized by the entire space program that he didn't want to see anything that might seem like constructive interest in it. So he was not particularly interested in making it better. He wanted it to go away altogether.

Simpson was a committed terrestrially bound paleontologist, and he generated all kinds of arguments, none of which impressed me in the slightest: why life was such an improbable event that there was no point looking for it outside the earth. Well, it might have been improbable, but here we knew it existed. So I really couldn't see much sense to the detail of his argument. But he and Abelson did form a—numerically—rather small core of people who poo-pooed the entire notion. But otherwise, I would say it had astonishingly positive support. I was quite startled when I got rather prompt replies back from the higher officials of the National Academy [of Sciences, NAS] and so on.

WOLFE: Were NASA and the military as equally supporting?

LEDERBERG: This was a strictly a NASA matter because planetary exploration was entirely a NASA mission and has remained so.

WOLFE: All right. This is a little more technical. If you can recall, what were some of the differences between the initial sterilization and quarantine programs? There were different proposals, different levels of protection.

LEDERBERG: Well, they went through a pretty systematic review, on the one hand, trying to estimate what the microbial loads would be, what the chances of survival would be in environments like those of Mars, and then finally putting that together with what was an acceptable level of microbial contamination on the vehicle itself so that further attenuation generated an acceptable risk. Now I don't recall what numbers they finally came down to that fit into those equations, but I can tell you who would be able to provide them, and that's D. Warner North, who did a lot of the risk-assessment work on that. I was in touch with him not very long ago so he is alive and kicking in California.

WOLFE: All right. Initially there was some disagreement among Westex [West Coast Committee on Extraterrestrial Life] members about that. I'm thinking specifically, there seemed to be a conflict between Norman [H.] Horowitz and Aaron Novick about whether you should be asking for quarantine or sterilization. Horowitz, at least, voiced his opinion that discussing this problem too much might turn off the public to the space program.

LEDERBERG: Well, I forgot the latter motive to it. I know that he was probably mostly lukewarm. I think partly his skepticism was about whether there was anything to look for anyhow—he had his own prior views and he's written them up in his book, *To Utopia and Back* (1). You can get his views very clearly in that direction.

WOLFE: All right.

LEDERBERG: To this day he thinks the matter is settled and we know for sure that there is no life on Mars. I've tried to keep a somewhat more open mind about it. I was never sure there was. People have asked me why I've put so much energy into a program where I didn't have a strong conviction, and my answer is it was the question that was important. Until the question was settled it really didn't matter what I believed the answer was; it was important to have a process in place that could provide a credible answer.

WOLFE: Westex had at one point proposed contracting with Fort Detrick to make a detection device. Could you say something about how that idea originated and how it was received?

LEDERBERG: I don't remember the outcome. I recall that as I looked into the literature of decontamination and so on, it seemed to me that most of the practical work in that direction was going on there. There were also very sensitive sensors for detecting microbial activity. I think they had a lot of published information that was useful. I don't believe that they actually ever participated dramatically in the effort, but I can't be sure about that.

WOLFE: Would it have been a problem for NASA, as a civilian agency, to be contracting with Fort Detrick at all, because I know partially you were trying to encourage international cooperation?

LEDERBERG: Well, I may have been naive. But I think I felt that as long as this was an open effort, you go where the expertise lies. So maybe it would have had those problems of credibility and so on. I tended to take people at their words. I knew quite a few of the people at Detrick. A lot of people who had been at Detrick had come to the University of Wisconsin. Its bacteriology department was very close into it. Ira [L.] Baldwin had been a considerable figure on the advisory groups for the BW [biological warfare] program, and that was still the offensive program in those days. He was also the Dean of Agriculture at the University of Wisconsin. While I was less heated than I might have been now, I might have preferred that they were not into an offensive development program, I took them at their words. I thought they were honorable people and they would do the best job they could in fulfilling this kind of requirement. Detrick was an instrument in national policy at that time, and they didn't make national policy.

WOLFE: You said you didn't know the outcome. Do you know if NASA did contract with them?

LEDERBERG: I don't think they ever did contract with them, but I'm not at all certain of that.

WOLFE: Okay.

LEDERBERG: I've never heard it mentioned again, and I've had a lot more contact with the BW program since the late 1960s (with efforts to abjure BW weapons development and to negotiate and ratify the BW disarmament treaty). But I have no guarantee on it.

WOLFE: All right. By about 1962 or so, contamination seemed to be less of an issue that the Space Science Board [SSB] was talking about all the time.

LEDERBERG: Well, it had become accepted. It had become formalized in international treaty, and so forth. The CETEX [Committee on Contamination by Extraterrestrial Exploration] Agreement.

WOLFE: Right.

LEDERBERG: It didn't spell it out chapter and verse, but there was a formal agreement for mutual consultation to take great care. So it was acknowledged at the international level. Now how far that was going to be implemented; we were very curious to know what the Russians attitudes were. Were they serious about it, and so on. I'm sure some were; some weren't. Same is true on our side.

WOLFE: Right. I have a few more general questions about the Space Science Board—a phrase that you used earlier actually, which is “sound scientific program.” And you mentioned that the whole enterprise seemed in some ways like a circus.

LEDERBERG: Still is.

WOLFE: And it still is a circus. [laughter] So what did you have in mind for a sound scientific program, if there was a way that space exploration could be?

LEDERBERG: Well, for one thing, have much more emphasis on the planets, rather than sending people to the moon—sending people anywhere for that matter. Now, some science gets done in support of manned space travel. I can't argue with that. If the decision is made and settled that you're going to be putting people up there, I don't want to put them at risk. I don't want to put the country at risk by that failing. So that needed some scientific support as well. I just personally didn't have any great interest in it or have anything to do with it. So that's the bio-astronautics side of it. But, you know, some good people did good work and tried to understand the space environment and what it would do to people and what would be the life support.

But while I was eager that whatever was done be on a sound scientific basis, I would have preferred a much more vigorous scientific program that would have involved more sampling of material. I had no qualms about return sample of moon material. We can still use more of that in a more systematic role today. We don't need people for it particularly, but it could probably be done with people as an excuse for why you're sending them there. But then even more so when it comes to the exploration of the planets—planetary exploration on the one hand, and then astronomical inquiry too. The Hubble Observatory is a magnificent scientific accomplishment by every account.

WOLFE: What did you see as biology's role in space science?

LEDERBERG: I couldn't see much besides meteorites, comets, and planets, and it would be at minimum better evidence about what primordial organic-chemical synthesis would look like. I got onto the other side of a—I won't call it controversy, but a different light of interpretation with [Harold C.] Urey and [Stanley] Miller, who saw the original organic molecules that were precursors to life, originating by the action of ultraviolet light on a primitive terrestrial atmosphere. I saw an expanding universe, cycles of star formation, heavy element formation, super nova, re-expulsion, developing an inter-stellar plasma that contained mostly hydrogen, a little bit of helium and then down to trace amounts of oxygen, nitrogen, phosphorus, and sulfur, being critical elements. You don't need a synthetic paradigm. These isolated atoms, when they collide with one another, then you've got organic chemistry in space going on on a cosmic scale. And I thought we ought to look there for the origins of organic molecular material. The only question is: how do you get it from space to the surface of the earth? That's still an ongoing issue. But there is cometary. There is meteoritic infall.

The pendulum's been oscillating from one extreme to the other about how much of the organic molecular burden on earth could have come from external sources. I think it's moving back again towards some credibility that quite a large component could well be accounted for that way rather than atmospheric sources once the atmosphere here has been established. So you might call this cosmic versus terrestrial origin of organic molecules. Well, exploring those contingencies I thought, and think, is still a very important arena for NASA to be interested in. Now there's been microwave studies of organic molecules in space. As methodology gets better and better, more and more complex molecules are being found by that spectroscopic route. I

don't think it's controversial anymore. You have things like comets as reminders of those sorts of aggregates. How to get the organic molecular material on a comet to the earth basically is an issue of absorbing its gravitational energy without burning up everything that's in it. But some of it finally does get through. If you get a meteorite hitting earth that you think came from Mars, then you can get cometary materials.

WOLFE: Right.

LEDERBERG: So there's still a lot of open issues.

WOLFE: Right. Some of the early-proposed experiments—I was really struck by their confidence. For instance, in the design of the—I think it was called—Multivator with the phosphatase.

LEDERBERG: Well, that was out at my lab, you know.

WOLFE: Right.

LEDERBERG: Well, we wanted a uniform platform in which we could plug in a variety of different kinds of experiments. So that's the multi part of Multivator.

WOLFE: Right. Scientifically, for those assays to work, would that require whatever macromolecules that had been out there to be in the same—

LEDERBERG: Well, it depends what you choose. Actually, there is phosphatase activity in quite simple species. As I reflect on it further, in an effort to try to choose the least unlikely substrate targets, we may have been laying ourselves open to a dilemma on the other side. You can get some phosphatase activity out of inorganic salts. So make your choice. [laughter] But when are you going to draw the line? If you had to make a choice—if you stuck to DNA-ase, which might be a preferred assay to answer certain questions, it better be the right question.

WOLFE: Right.

LEDERBERG: If it's a different polymer you might, you know, miss an elephant. [laughter]

WOLFE: Exactly.

LEDERBERG: But we'll say it's a very serious dilemma to know what degree of specificity to be looking for. So phosphatase was one of the choices precisely because there are going to be many more phosphate substrates that the phosphatases we know can handle at some rate or another, then they're going to be something as particular as the deoxynucleotide polymers, which would be a wonderful signal to get. But what are the odds that it's the same kind of polymer? So you have all those tradeoffs.

WOLFE: Did the Multivator actually go up?

LEDERBERG: No. We didn't use that. You know, conceptual analogs of it were the other experiments.

WOLFE: Right.

LEDERBERG: But the experiments that were actually flown—I don't believe there was any single, specific enzyme assay. There was a pretty broad metabolism assay, for which labeled materials, labeled substrates were provided. And if any of them decomposed you'd be able to read the radioactive CO₂ coming off. There again we didn't give enough attention to inorganic artifacts that in retrospect certainly did cloud that issue. And the other was a CO₂ fixation experiment. We assumed that small life could be very near universal. Whether they're colored green or not is another story. But there's a reason it's green.

WOLFE: Yes. You had strongly resisted—in similar administrative matters—combining the exobiology committees and the man-in-space committee. Do you recall that?

LEDERBERG: Well, I thought they were a totally different agenda. I didn't want to be giving political support to the man-in-space program. I wanted us to stand on our own feet and let them do the same. I also thought the talent involved was totally different. So I could see one's physiology; the other's molecular biochemistry.

WOLFE: Right. Was opposition to the man-in-space program fairly common?

LEDERBERG: It's hard to say. I don't know many working scientists who were enthusiastic for it. I think some people bought it as part of a political agenda. There was that small kernel of real space enthusiasts. I think Carl Sagan was probably pretty plus on it. I think he thought it

would excite the public, and he was enough of a cowboy himself that he wanted to be part of it. So it covered a fairly broad spectrum. There was also a lot of opposition. Actually, there's a very interesting reprise. I'm going to try to get this out for you.

WOLFE: All right.

LEDERBERG: But I'm sure there have been more objective surveys on this with people of varying degrees of education and professional interest. I don't recall them, *per se*. But it covered a pretty broad spectrum. The non-scientific motives did appeal to some number of scientists. They're human beings in other aspects too. I think there was a lot of concern about draining budgets away. I also have to say NASA went over backwards to try to win the interest and support of the academic community. It had a lot of very broadly conceived university-centered programs, and so on. Jim [James E.] Webb was all for using spill-over from NASA to improve university academic facilities, and so on. So it had a little constructive aspect, part of a political agenda. For ten years this was Jack [John Fitzgerald] Kennedy's dream. I think his charisma caught up with a number of people. Likewise, you might say, the anti-charisma of Sputnik. We had to do something to show we were up to that challenge. So it was a very complex thing with different motives.

WOLFE: I see.

LEDERBERG: I didn't think we needed men in space. I thought we could accomplish the political objectives equally well. But what do I know? But I did struggle pretty hard to try to get alternative agendas on the table.

WOLFE: Considering this what we needed to accomplish in space is political, in several cases you mentioned the need for international cooperation and that you really wanted to encourage scientific cooperation in space. What did you see as possible?

LEDERBERG: Well, if we were going to do planetary inquiry, first of all I didn't want the competitive spirit to override the cautionary aspects of it—not only with respect to contamination. I wanted the missions to work and I didn't want there to be such heat—“Let's get there before the Russians do”—that we cut corners in the process and then vice-versa. We needed common sense in that regard. I thought it was also a place where—and it was indeed the case—you could have Russian physicists and American physicists talking to each other, and just incidental to that, try to work over other technical contributions to solutions of arms-control programs and things of that sort. I'm sure Jerry [Jerome B.] Wiesner was very deeply imbued with that, being also the science advisor to [Lyndon Baines] Johnson. But when I would bring up with him a manned program he basically said, “You're wasting your time. A decision has

been made. I did my best. I didn't disagree with you, but I've got to follow what the boss says. The decision is settled, so why don't you just go along with it?"

WOLFE: How did you end your involvement with the SSB [Space Science Board]?

LEDERBERG: I don't recall. Is it still in existence? I mean I think I sat in it through any number of terms. And the structure has changed a bit.

WOLFE: Yes. I was really struck by something. In 1962 there was a change in leadership in the NAS as [Frederick] Seitz came in instead of [Detlev Wulf] Bronk. And I think [Harry H.] Hess became the new chair of the SSB instead of [Lloyd] Berkner. But it seemed like there was a changing of the guard. A lot of people were sent letters. From the perspective of looking at it now, it's kind of ambiguous whether all of you felt you had spent your time or if they wanted a different crew.

LEDERBERG: Oh, I had no problem with it. I had spent my time, and I didn't lack for influence if I needed it.

WOLFE: Right. Okay.

LEDERBERG: So, no. I certainly at no point felt abused or left out. And I was on any number of direct NASA committees, and so on. I had more than my fill. [laughter]

WOLFE: I'm sure.

LEDERBERG: So that wasn't an issue. And I was an active participant in the Viking project.

WOLFE: Right. So besides the Space Science Board, I have a list of some of the other early federal advisory boards that you served on, including the Kennedy Health [Transition] Team. Of the other advisory boards that you were serving on in these years right after you went to Stanford [University]—immediately after the Nobel Prize, including the Kennedy Health Team and later in the 1970s with the National Mental Health Council and various others—any that you find are important—which ones did you feel like were the most important of all of your early committee and federal-policy work?

LEDERBERG: Well, it varies with epoch. That episode I told you about in 1950 in the Genetic Society, I think, was my first venture into political territory. That was pretty early in my career actually. I was astonished. Here I was, twenty-five years old. I was three years into my first academic job. And I was being, you know, put up against [H. J.] Muller on a Genetic Society Committee. But I guess I had spoken eloquently enough, and so on, that I persuaded a number of people that I was speaking the voice of reason. It was sort of painful for me to do that; I so deeply admired Muller, and he'd been so kind to me personally. So there was no personal rancor of any kind—not even remotely so. He had buzzed on a particular matter. Then I guess I'm answering a somewhat broader question. But I'll see if there's a reasonable chronological framework. In the mid-1950s I began to be missing my medical roots. I had made a wrenching decision about not finishing medical school. I had completed a Ph.D. sort of *ex post facto*. My work at Yale [University] with Ed [Edward L.] Tatum. Is this familiar to you?

WOLFE: That part about the medical work.

LEDERBERG: Then I took a job—as an alternative to going back and getting my M.D. at Columbia—at [University of] Wisconsin in the Genetics Department, which was a wise choice. But it was in the School of Agriculture. There was no genetics in that or any other medical school at that time. So as I settled in at Wisconsin got my lab going and my teaching going, and so on. The opportunities for being part of a medical search enterprise were expanding as the field was growing. I then, in a certain sense, got to be a little more political and policy-oriented by moving towards the ensconcement of genetics in the medical curriculum and to getting it organized as a self-standing department. And that was I think pretty much of an innovation in schools at that time. So that's 1955, 1956. That coincided with the arrival at Madison of John [Z.] Bowers as the new Dean. He'd been running some of the major radiobiology programs of AEC [Atomic Energy Commission] up to that time. So he was an animal biologist, you know, a beagle radiobiologist by background—biological effects of radiation. But this fit in with his plans where he wanted to lead his medical school. So it sort of happened against a great majority vote of the medical faculty. They had to scrounge around looking for the money for it, and so on, but it looked like it was going to be settled. Anyhow, that was sort of one of the first major policy issues. I got to be head of the committee and the department recruiting people, and so on—extra laboratory commitments.

Then I ended up being a little restless with the somewhat limited horizons at Wisconsin, particularly in the medical direction. A brand new medical school was at Stanford, and Art [Arthur] Kornberg beckoning—going out there himself. So I decided to go out there, and had quite a misfortune. Just as we were packing up to leave from Madison, the Nobel Prize comes in. Not good timing. [laughter] I mean it just doubled the kind of ambivalent feelings about turning my back to my friends, going somewhere else. People were getting it all mixed up, saying, “Stanford has recruited another Nobel Prize winner.” So, you know, sort of nobody being given the credit for any part of this. Anyhow, there it was. There's no doubt that a lot of the public attention that I got after 1958 was connected with the Prize. But remember the fuss I made about planetary quarantine and so on? That was before the Prize.

[END OF TAPE, SIDE 1]

LEDERBERG: With the Prize, there's no question that it added to the automatic credentials. I suspect that when I was introduced to the Kennedys and asked to serve on the health team that it did have something to do with it—that I represented scientific medicine on a health team; I guess I was the person to do that. So there's a kind of self-fulfilling aspect to that. You get more experience—hopefully you do a good job and be noted for other such opportunities, and so on, and you start paying more attention. I felt I needed to enhance my policy education. I can't actually describe how I did that. But I would pay close attention in seminars and sociology, history, political science, economics, and so on. It's not that my reading in those fields began at that time, but it was certainly accentuated as part of that process. I'm kind of approaching an answer to your question, but I'm trying to fill in a little more of the background. In 1961 I was invited to a symposium of the Ciba Foundation on man's biological future. Don't take offense at the word man's—it meant human.

WOLFE: Of course.

LEDERBERG: That set me thinking for the first time a little bit more systematically on what was going to be the human impact of the new biology that I was one of the creators of. I tried to systematize my thoughts in the paper that I gave at that time (2). One of my conclusions was the future was going to be richer than the past. Don't try to settle everything today. The technical environment will be totally transforming. And don't try to fix every problem today. We don't know that much diagnostically and the therapeutic modality will be infinitely greater. So that was my rebuttal to the eugenicists. I mean, you've got a very clumsy methodology. You're not really sure what you're talking about. There are issues about trying to forestall, you know, which became the negative eugenics, "Let's forestall the deleterious mutations and try to prevent malformations." I couldn't argue against it. But then that says let's focus on the individual baby. I don't want deformed babies born. But that's my objective, rather than purifying a gene pool.

WOLFE: Now was that the conference where you introduced the term—is it euphenics?

LEDERBERG: Exactly.

WOLFE: Is that where you introduced it?

LEDERBERG: That was my counter slogan.

WOLFE: Okay.

LEDERBERG: And in a way, it is a stalling action. Let's deal with today's phenotypes. We'll worry about tomorrow's genotypes tomorrow. Muller, I guess, was not there but read a paper. Julian Huxley stood for that movement and he was still talking rather vehemently about the eugenics side. The extent of the debate was with Huxley, that's what framed that. Anyhow, that got me. I started to think a little bit more deeply and I began bringing that into some of my teaching. Nobody else was talking about cloning or genetic engineering. So these were some of the very first discussions of those issues. And I, to some degree, got caught up in it myself. So from 1961 to roughly 1967 it partially overlapped. When I started writing my column I was pretty much pre-occupied with those kinds of issues. Just as the word dogma is a joke, I used the term algeny for genetic alchemy. Both—what's his name—

WOLFE: [Sheldon] Krimsky?

LEDERBERG: Krimsky, yes—at Tufts [University] and Jeremy Rifkin, of all things, had taken it much too seriously. They had no sense of humor whatsoever. [laughter] It was kind of poking a little bit of fun, but still saying there are things to be thinking about and not wanting to keep any secrets about what was going on. So I spent those five years catharcizing myself about what I really felt about those matters. I'd say from about 1967 on, I then I got kind of tired of it, and didn't feel there was that much more to be said. I was going to say the same things over and over again. So I published a few things in that genre and then said that was my last word (3). So it more or less is. Now I can pull out these publications that I still stand on as being surveys. So if you'd like that I'll identify it for you.

WOLFE: That might be helpful.

LEDERBERG: Okay. You were asking about more public involvement with committees and so forth.

WOLFE: Which ones did you feel were the most important with respect to science policy?

LEDERBERG: Well, certainly the Kennedy Health Transition Team. There were a few things there. There were first of all some specific policy recommendations in bolstering Medicare—support for biomedical research as a built-in part of the health agenda. There was a call there. I don't recall if it actually survived the final report to the President. It's one of the versions but not the other, so I guess the President didn't accept it as a formal recommendation. But I called for establishing a National Academy—I think I called it a National Academy of Medicine—to be a

counterpart to the National Academy of Sciences to provide the same sort of expert advice on health policy matters. Well, in one way or another, that was the precursor to the Institute of Medicine, although only a few people knew about it and it was an idea that was revived in any number of forms. But I think I can say it was part of the history of ideas of that particular enterprise.

Now, that was a pretty high-leveraged unit. I mean that was reporting directly to the Chairman, then directly to the President. I decided to go into writing columns. For one thing, I just got sick and tired of travel. I was doing so much of it for NASA that I reacted. So between 1961 and 1965, I think I actually succeeded—in 1962 to 1966, something like that—in not traveling cross country at all for that interval. [laughter] So that says I wasn't on any national committees and I was fighting them off. I wanted to do my own thinking. I wanted to have other avenues of influence. It's a complicated story, but instead I got into writing a weekly column. And that brought me up to 1971. And there I declined service on the President's Science Advisory Committee. I thought it would be a conflict of interest to be somebody writing all and telling all on the one hand, and receiving confidential information on the other. I was probably using that as an excuse to get off the hook. But there was something to that. I couldn't. If I was using all the information at my disposal, which I assume a writer could have probably—particularly a regular columnist and expected to do—it could be a little hairy if you hear things in confidence you then can't talk about.

So I stayed off [national advisory groups]. It wasn't until the end of the 1960s that I started going on. Then they were mostly not high-policy matters. They were tactical things—NSF [National Science Foundation] panels, NIH [National Institutes of Health] panels, study sections—things of that sort.

WOLFE: At some point in the 1970s you became involved in—

LEDERBERG: I'm trying to remember what committees I was in.

WOLFE: Well, this was just kind of a fragment from one of your outlines. You mentioned something about national security involvement in the 1970s. How did you get involved in that?

LEDERBERG: Oh, yes. That was the big thing. Well, one of the consequences—and it did have big repercussions—of some of the things I was writing in the column is that the direction I was taking aligned with what the [Richard Milhouse] Nixon administration decided it was going to do about biological weapons arms control. And that was a separation of the BW and CW [chemical warfare] issues. Now the moral stance on BW was so much more compelling that a lot of my colleagues regretted my severing them because they thought they could use that as leverage to get a more comprehensive agreement. I felt the moral stance of BW so compelling that we had to get that at all cost. I thought it would be a long time before we'd get a CW arms control agreement because it was an established weapon. It was something that had been used

widely in World War I. The military was uncomfortable with it, but they knew what to do with it. They were going to squawk about giving it up.

And then there were the quasi-CW going on in Vietnam, which brought in a wholly different set of controversies, which greatly clouded it. They may have been the main motive for some of the other folks. It was a way to do some Nixon bashing. But I wanted to solve the BW problem. It wasn't that I was very fond of Nixon, but I really was quite fixated on that. So I suddenly got a call from the Arms Control Agency asking if I would advise them in Geneva during the negotiations and maybe speak to the Committee on Disarmament at the UN [United Nations] in Geneva. That was a totally new level of territory for me. I obviously, hastened to say yes, and was quite deeply involved in that process as a result. Two or three impacts. We had a 30th Anniversary—that event not long ago—the negotiation of the treaty. I had a couple of people who had been there in Geneva who came to this UN meeting and said, "Dr. Lederberg, you don't realize what an impact you made with your talk when we diplomats were trouncing around. And the picture you painted if we didn't come to terms was really so horrifying, it really helped beat us into shape." I didn't know that it had that deep an impact. I've had that testimony in several places.

So I get some credit for the actual bringing to terms of the treaty. It's a flawed treaty, but it's the best we're ever going to get. I think it's been a very important step in de-legitimizing BW. Not that it's easy to enforce it, but at least that's the status of the law at this point. So I put that tops on the list and it's right at the beginning of that sequence. Then a little bit after that I started getting nibbles from the Defense Science Board, from other defense-oriented places, the intelligence community. Precisely because most of the academic world was boycotting those agencies at that time, I felt a moral obligation to come to terms with it. If they didn't hear some, what I would call, "voice of reason" from in-house, then they weren't going to get it anywhere else. And there was just going to be such mutual polarization that it would end up being much worse.

WOLFE: So sort of working within the system approach.

LEDERBERG: Exactly. So I realize that's working with the devil, but I made that decision.

WOLFE: Okay.

LEDERBERG: That more or less coincided with my ending my public persona in writing columns. That was coming to an end anyhow. So I felt able to go into getting clearances, dealing with classified information, and so on, if I was no longer writing the weekly column. So I think that until recently my not-very-visible contributions have been in the national security side—some degree of sanity, what we need by way of weapons programs, what chances we should be taking in arms control agreements, and pressing for international convergence in a wide variety of area. And I continue to do that. I was a very lonely character for some time

during the height and ongoing, you know, raging about the Vietnam War. I never gave it any support of any kind. I would chide myself for not having been more openly and aggressively against it. People like [Robert M.] McNamara said how totally wrong it was to have been a proponent. I will say it was wrong for me to not be more aggressively opposed to it. I didn't think it was all one-sided. I didn't think much of the benevolence of the North Vietnamese army or the Vietcong. They were just as imperialistic as the other side. But, it's perfectly obvious that it would have been a better outcome for everybody if we had not supported South Vietnam now that we know the outcome.

WOLFE: Right.

LEDERBERG: What an enormous human cost it entailed. What was flawed was the Domino Doctrine. That can be traced to [Joseph] McCarthy's attack on fire speech. That seems to be going back a few years. But he expunged every trace of expertise on East Asian and Russian politics. Our State Department and our security organizations, while they undoubtedly were some people who had communist commitments—I mean much more harm than good came out of McCarthy's rampage. And this is one of the proofs: to have such a one-sided view of the dynamics of the Communist systems.

So that takes us to the early 1970s, and so on. Most of what I do and have done in connection with DOD [Department of Defense] is not very public, but it isn't that discreet. It's very hard to point to one single thing. But I think a lot of what's happened in those agencies would be even more insane if they didn't have a few folks, like myself, willing to listen, willing to talk back, and willing to do it on a close-in basis. Subsequent to that, I've become more concerned about enforcement of the BW treaty now that the gross violations have shown up. That does involve our national security doctrine to a substantial degree. And then the bio-terrorism questions, if there is further manifestation of it. Those are things that are working, things that are happening as a result of my involvement there. But in parallel, since about fifteen years ago, infectious disease is a much broader agenda. They've been working mostly through the Institute of Medicine [IOM], the National Academy of Sciences, the CDC [Center for Disease Control]. Some inter-departmental governmental agencies have been trying to put aside the complacency about infectious disease. The AIDS [Acquired Immune Deficiency Syndrome] epidemic should have settled that once and for all. But even at that, it's amazing how few people have been able to conceptualize the generalization that if something like AIDS could come up out of the blue so will a dozen other things. And they will. And they are. Today's concern is West Nile Virus. So the IOM report on emerging infections has had enormous impact. Since 1992, it's going to go into another edition before long. I think it has reset actually the global agenda to some degree. I've been on WHO [World Health Organization] committees with the same objectives. Sorry it's such a long-winded answer, but it's not a simple question.

WOLFE: It's extremely helpful. It's actually a very helpful way for you to answer it. If we could actually move on to the columns now for a little bit.

LEDERBERG: Yes.

WOLFE: In Harriet Zuckerman's book she quotes you as saying that you started the columns partially as a way to protect against distortion (4).

LEDERBERG: Is that in the *Nobel Elite* [*Scientific Elite*]?

WOLFE: Yes. That's in there. The idea that if you spoke for yourself, at least you would get it right.

LEDERBERG: Yes. That was my immediate motive, and mostly before then it [journalism] was various kinds of space stuff. They never got it right.

WOLFE: So if you could be a little specific. What kinds of experiences have you had that led you to this—just the space stuff?

LEDERBERG: Well, I don't recall. Well, there must have been some stuff about the agency as well. I'm almost certain it was. But, I'm sorry, what the provocations were have kind of faded away. [laughter]

WOLFE: That's fine.

LEDERBERG: But I recall those attitudes, and Harriet has documented it from pretty early in the game. She must have done that in the late 1960s. It couldn't have been much later than that. When did her book come out?

WOLFE: I think maybe 1979?

LEDERBERG: What's amusing is that she had solicited me for an interview when she was putting it together, and I had declined. Then some years later I got to know her and Bob [Robert K.] Merton very well. We've become best friends and written a couple of things together, and so on (5). So I was trying to guess when that interview might have been. But I saw a great deal of her during the year they were out at Stanford in 1974. So maybe that was the occasion.

WOLFE: Right. In large part, do you think that that strategy worked?

LEDERBERG: Oh, yes. I was able to speak with my own voice—and not just distortion but compression. So many of the things I want to talk about are so nuanced that the interesting questions all are a little bit of this, a little bit of that. And I may be very frustrating to some readers in that I tend to complicate most questions, not simplify them. [laughter]

WOLFE: Right.

LEDERBERG: And that just doesn't come across in somebody else's recordings.

WOLFE: So did you do many TV or radio spots?

LEDERBERG: No. I've pretty much systematically declined them for exactly the reason that I do my own column.

WOLFE: Right.

LEDERBERG: I have good control in my column and I have no control whatever in what appears on a TV show.

WOLFE: How common was it for you to get requests to do say television interviews or radio interviews?

LEDERBERG: I'd say I'd get one or two a month over the years.

WOLFE: All right.

LEDERBERG: You know, they tend to come in clusters when there's some hot event. Lately it's been about bio-terrorism or something of that sort. I think they've probably mostly gotten pretty tired of asking. I've made one or two exceptions. There was a BBC [British Broadcasting Corporation] series on emerging infection where I thought they were trustworthy. And they were. That came out okay. There was one other. But it's just a medium I detest for communication. I think it's great for entertainment—period. [laughter]

WOLFE: Okay.

LEDERBERG: It's not accountable.

WOLFE: No, it's not.

LEDERBERG: You know, somebody—even if it's somebody else's report—if they really get it wrong they can be held to account and have to respond to a demand for a correction. You can't do that with TV.

WOLFE: Right. It seems to be getting worse as well. In many of the columns the content seems to have little to do with your scientific expertise—the one, say, on voting age, or our involvement in Vietnam.

LEDERBERG: Yes.

WOLFE: How did you pick what you wanted to write about?

LEDERBERG: As we got to arms control I felt that although I had entered into the field from a specialist background of a microbiologist, I'd spent a lot of time continuing to work—wait, no the timing's wrong. That only started in 1970 and I was well into my column. I'm putting the cart before the horse. I was still in touch with a lot of scientists who in turn were connected with arms control, Leo Szilard, outstandingly. I'd heard a lot of the debates, so I didn't feel like I was a total dummy. I certainly felt I was the match of any of their other political columnists in dealing with those kinds of issues. I tried to stick to things where there was some special scientific angle, but certainly spilled over that—most notably on internationally security matters. I don't know how else to answer your question. The things I ended up feeling the most strongly about—and then I'd be chided now and then by the editor that, you know, "We have much more high-priced columnists than you to deal with those matters." [laughter]

WOLFE: What about other scientists? Did it bother your colleagues whenever you would write about non-scientific things?

LEDERBERG: No, on the contrary. I thought I would get a certain amount of joshing about trying to grab the headlines of something of that sort, which was not the case. I didn't get that

kind of feedback at all. If anything, they would say, you know, “We’re glad you’re doing this for us.”

WOLFE: Somebody has to do it.

LEDERBERG: That was their general attitude. On specific cases, what I enjoyed the most were people who would tell me, “You didn’t get it quite right. Here’s chapter and verse on the detail, and do better next time. For a columnist, you did okay.” [laughter] So when I could engage in more substantive discussion on things, I opened up. I welcomed it. It’s pretty hard for me to think of anything that I got into hot water about with my scientific colleagues. I think there were some other public issues—I guess abortion is one. That got more angry comments than most anything else.

WOLFE: To your knowledge, were there many other scientists that were doing the same thing in other newspapers?

LEDERBERG: To my knowledge, nobody was doing anything quite like that in terms of a regular series.

WOLFE: Right.

LEDERBERG: Now there were occasional contributions to various places. Hal [Harold J.] Morowitz wrote a monthly note for the general public. That was more of a physician audience. I guess Steve [Steven Jay] Gould probably comes as close as anybody. Here again that’s somewhat more narrowly framed. The National Academy has tried to encourage not single columnists, but it will syndicate a variety of pieces from various sources. So that’s probably the nearest next example.

WOLFE: All right. One that I know of is in about 1965 Salvador [E.] Luria organized a group of people to do a column in the *Boston Globe*.

LEDERBERG: Really?

WOLFE: So for several months someone contributed. It wasn’t the same person.

LEDERBERG: Gee, I don’t even remember that.

WOLFE: Yes. They were kind of similar to man-in-the-future type things. They were about science and the future of mankind. But from that, I didn't know if lots of people were doing this in the 1960s?

LEDERBERG: No. It's very rare. I could see it did peter out—the thing that Salva did. I don't remember ever hearing about that. Is it in his book (6)?

WOLFE: I've seen it at the APS [American Philosophical Society]. They have some of his correspondence about it and some of his correspondence with the editor.

LEDERBERG: It's a little surprising since that was later on. I may have just totally forgotten about it if it was not a longstanding enterprise.

WOLFE: Yes, right.

LEDERBERG: It's not an easy thing to do. But I'll tell you what got me in the end, where I couldn't do it indefinitely, is that I'm just much more of a scholar than a journalist. It ended up that I have twenty cabinets full of ancillary material based on the content of the columns, you know, filling in the notes. If I ever turn to this topic again, the initial discussion, and so on, it just got to be more than I could handle. I couldn't forget about things that I'd written about. But I decided it was time to bring closure to it.

WOLFE: Right.

LEDERBERG: On one or two occasions I was going to republish them as a collective works with some critical commentary. That ended up being my current archival project. And I will annotate the columns *ad lib*, but I'm not under any deadline to do so. So, it's just as things come along.

WOLFE: I'd like to talk just a little bit about what it must have been like to be at Stanford—one of the quintessential Cold-War universities—during the 1960s.

LEDERBERG: Why do you say that?

WOLFE: Because of their physics labs and their electronics labs. In the historiography that's how they've been portrayed. Perhaps it's not so much for the biological sciences but for some of their other work. Historians of science have jumped on them as kind of coming out of the Cold War years.

LEDERBERG: Well, they certainly had no weapons laboratories. They had a major atomic energy department installation, the SLAC [Stanford Linear Accelerator Center] facility, which had absolutely nothing whatsoever to do with weapons. Most of the folks involved with it were ardent proponents of arms control. Some of the most successful were Sid Drell, Pief [Wolfgang K. H.] Panofsky. Pief was the Director of SLAC at one time. SRI [Stanford Research Institute] was an affiliate of Stanford, and it deserves most of the credit for what you're describing. They had innumerable contracts, including the classified contract. There was a student reaction about offing them, and they were off. They were disestablished. They were a totally independent activity. It was a medium-sized handful of professors who joined appointments in not physics—that would be the last place—but electrical engineering.

WOLFE: Right. The electronics.

LEDERBERG: And there wouldn't be anything like advanced radar, and so on.

WOLFE: Well, I think it's getting the reputation actually because it's just behind MIT [Massachusetts Institute of Technology], actually, in the amount of defense funding that it was getting for a large period—I think throughout the 1960s. Because at that time SRI was still considered part of Stanford.

LEDERBERG: Yes. Well, maybe even more importantly, you had the Department of Energy, which makes bombs, which also paid for all of SLAC, which has a budget probably comparable to Stanford University.

WOLFE: Right.

LEDERBERG: So I mean if you do the bookkeeping that way, it may seem so.

WOLFE: Right. I'm sure it might have. But in the biology, in genetics it didn't.

LEDERBERG: But right across the board for the whole university, you know, the mentality of its faculty and the mentality of its administration is not anywhere what you'd call Cold War-ish. That's what I'm trying to say. Oh! There's another reason though. The Hoover Institution.

WOLFE: Okay.

LEDERBERG: That was the bastion of the Cold War side. [laughter] They're all in one place. They're all in one building.

WOLFE: All right. So it was quarantined away from the rest of you. [laughter]

LEDERBERG: Well, yes. They were regarded as pariahs by most of the faculty.

WOLFE: So you said you tried to avoid the classified and clearance and Department of Defense things when you were writing your columns. Did you have any defense contracts, even for any of the information sciences material you were working on?

LEDERBERG: Oh, yes. We had DARPA's [Defense Advanced Research Projects Agency] support for working artificial intelligence, which had no military justification whatsoever. It was what was eventually terminated.

WOLFE: Right.

LEDERBERG: And there were quite a few—DARPA was inventing the Internet. And 80 percent of what it was supporting can arguably be said was fed into that development. So that was the great Cold War contribution that that made.

WOLFE: Right. Did you ever see yourself as either a Cold-War or an anti-Cold-War scientist?

LEDERBERG: Well, I thought those were simplistic classifications. I'm sure you knew that would be the answer. I'd had a jolting experience in 1962 during the Cuban Missile Crisis. It's paradoxical because I shrugged it off. Leo Szilard had called me—and this is in [William] Lanouette's book (7) about Szilard, by the way—and said, "Josh, where do you think we ought to go?" I knew what he was talking about. I said, "I'm not going to go anywhere. Nothing's going to happen here. And if it does, there's no place to hide." I was much more naive then than I am now about how things can go wrong during times of crisis.

[END OF TAPE, SIDE 2]

LEDERBERG: It was my reflections that I had been unduly complacent about the matter that, you know, over the few years following, it made me take stock. I did take the hazard of things getting out of control quite seriously. I also thought that there was a lot of game playing going on, but they were playing with fire. So I knew that we had to engage in measures to lower those risks to temper it. I didn't think the numbers of weapons *per se* were the most critical concern. Because it was just on the overkill. If we got a hundred times more weapons it would kill us all anyhow. What difference does it make at that level?

WOLFE: True.

LEDERBERG: I thought rather of trying to get institutional frameworks that would lessen the likelihood that any of them would ever be used. Here I was, a little bit at odds with the other arms controllers because that, to me, meant, "Let's slow down the rate of technological innovation. Let's go in for test bans as one means of controlling that, and then maybe that'll give us enough time to work out what we're going to do." If the technology is changing every five years—and that's what was happening—it just takes the ground from under your feet as you try to work out new agreements. So a little bit quizzical about numerical arms control—not that I wouldn't have liked to have it happen, but everything was going into that. I felt that it might even have the paradoxical consequence that if all the hard bargaining was going over how many silos have you got and how many launching platforms have you got, they would be given a value beyond their real value because they become part of the bargaining chips, and so forth.

So that was one of the complications. But getting more discourse, including the military, techno military, and political leaders seemed to me very important. What do we really have to argue about? Taking more positive attitudes, trying to find confidence building ways of reassuring the Russians that we were not going to hurt them—even though containment was necessary. We didn't want them spread further over Europe. That was kind of the ideology that I was trying to promote there. So it was kind of a middle ground. It was not an automatic condemnation of the Defense Department. I was very grateful we had a plausible deterrent. I didn't think enough of the stability of the Russian regime to think that we could safely leave matters only in their hands. I didn't think we'd reached the matters safely in the hands of our own leaders. So a balance of terror was the best we could do during that interval. Does that answer your question?

WOLFE: That does answer my question. That's helpful.

LEDERBERG: But it had to be modulated by common sense and by some sense of what was going on, and especially trying to guess how could this thing go wrong, how could it end up with catastrophic consequences? So things like being sure that there were protective action links that would control the launch of missiles from forward positions, being sure that there were regimes of crisis stability and against instability. I thought that was the most important intellectual contribution of the Cold-Warrior academic complexes—working that through in some detail that would give you the latitude of waiting to see whether something was really coming in on you. Else you'd be in a position where you'd say, "Use it or lose it." So those are grossly destabilizing matters. In 1962 I shrugged off the idea that we'd ever be brought to the point that anybody could ever dream of launching. As time went on I became more concerned about the psychological frailty of our leaders under stress.

WOLFE: You briefly mentioned earlier the student protests against the classified research at Stanford. The main one at Stanford I think was on April 3rd, 1969. But on March 4th there was another big grad-student and faculty strike. Did you participate in any of those?

LEDERBERG: I spoke at the amphitheater. [Stanford President] Dick [Richard Wall] Lyman had asked me to do that—in which I said, "Don't be co-opted by violent, radical actions. They're trying to get you to do things that will get you beaten up so you'll be forever co-opted. Use your own heads, and get out the vote. If you want to change policy in Washington, your marching in the streets isn't going to do it. Getting a vote out will."

WOLFE: Okay. I think somewhere it says later you did some teach-ins against biological weapons.

LEDERBERG: Yes. I didn't think that there was that much going on at Stanford. We weren't teaching in against local programs.

WOLFE: Right.

LEDERBERG: But that's part of a public education issue and trying to put some pressure on Nixon to not use chemical weapons, which I think at some points he might have been tempted to do. I don't think BW utilization was a live matter, but it's hard to determine.

WOLFE: Yes.

LEDERBERG: But I was not an activist trying to lead the students on. I was trying to lead them into more responsible roles and using the electoral process.

WOLFE: All right. That actually covers I think all the prepared areas that I wanted to touch on. Are there any other elements of your public persona that you'd like to address or bring up?

LEDERBERG: Well, it's inexhaustible.

WOLFE: I'm sure it is. [laughter]

LEDERBERG: It's all out there in the profile.

WOLFE: Of course.

LEDERBERG: So I'll just throw it back to you.

WOLFE: All right. Well, I thank you for your time.

[END OF TAPE, SIDE 3]

[END OF INTERVIEW]

NOTES

1. Norman H. Horowitz. *To Utopia and Back: the Search for Life in the Solar System* (New York: W.H. Freeman, 1986).
2. Joshua Lederberg, "Biological Future of Man" in: G. Wolstenholme, ed., *Man and His Future*. Ciba Foundation Symposium 1962 (London: J. A. Churchill Ltd.; Boston: Little Brown Co., 1963), 263-273.
3. See for example:
Joshua Lederberg, "Experimental Genetics and Human Evolution." *Amer. Naturalist* 100 (1966): 519-531. Also in: *Bull. Atom. Sci.* 22, no. 8. (1966): 4-11. Also pp. 670-688 in R. Schwarz, (ed.) *Menschliche Existenz und Moderne Welt Berlin*. Gruyter (1967).
4. Harriet Zuckerman. *Scientific Elite: Nobel Laureates in the United States* (New York: Free Press, 1977).
5. See for example:
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