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Interview with Dr. Joshua Lederberg
Interviewer: Steven J. Dick
Location: WestPark Hotel, Rosslyn, Virginia
Date: November 12, 1992

Dick: I was just saying that you should feel free to speak freely, because I'll ask permission before I would use any of this, and the transcript will be sent back to you for any emendations and corrections.

Lederberg: Thank you.

Dick: If we could start from the beginning, with some background questions. If you could state for the record your full name. Do you have a middle name?

Lederberg: Joshua--no middle name--Lederberg.

Dick: And your place of birth and date of birth?

Lederberg: May 23, 1925, Montclair, New Jersey.

Dick: And your parents' names?

Lederberg: Zwi Hirsch Lederberg. My mother was Esther Goldenbaum.

Dick: If you could just give me your general educational background, where you went to school.

Lederberg: I moved to New York when I was six months old, and I was educated in the public schools in New York City, attended Stuyvesant High School, which is a school specializing in science by competitive examination, although it is a public school. Then I entered Columbia College. While I was in college, I enlisted in the navy in 1943, and was able to continue my studies in uniform in the V-12 program as a pre-medical student.

I then continued on to Columbia Medical School College of Physicians and Surgeons, had the equivalent of two and a half years of the medical curriculum, then took a leave of absence in order to continue research studies in bacterial genetics in the laboratory of Edward L. Tatum at Yale University. That was intended to be a few months' research leave, but those experiments panned out beyond any reasonable expectation, so in the end I took another year's extended leave, but in the end did not complete my medical studies; instead, qualified for a Ph.D. in microbiology at Yale.

Dick: What year would that have been when you worked with Tatum?

Lederberg: I was in his laboratory from April 1946 until September of 1947. At that point I went on to the University of Wisconsin at a faculty position in the genetics department, which was then in the College of Agriculture, so I was at a cow college for a while, but my medical interests were paramount. A few years later I had the opportunity to start a medical genetics department in the medical school at Wisconsin. I did that starting in 1955, but I left Wisconsin in January of '59 to do something similar to that, start a genetics department at Stanford University in California.

Dick: Okay. If we could go back to Tatum just a moment, could you describe the nature of your work with Tatum?

Lederberg: I go back a further step. While I was an undergraduate at Columbia, I learned from him the art and science of using microbes for genetic analysis--in this instance, the fungus *neurospora*. As a medical student and for quite a number of other reasons, I was also interested in bacteria, and under Ryan's tutelage, designed and began experiments on genetic crossing in *E. coli*. Ryan suggested that I continue that with Tatum. Tatum had been his mentor when Ryan was a post doc at Stanford a couple of years before, and arranged for the possibility of my going to New Haven to do that.

Dick: What is Ryan's first name?

Lederberg: Francis Joseph. It was in Tatum's laboratory that I actually consummated the experiments on genetic crossing in *E. coli*, which then stamped my career ever since, and I am continuing to do very similar research at the present time.

Dick: Was the technique that you used at that time something new?

Lederberg: Well, there were some methodological twists. There's a fairly complicated history to that, and I've written all about it in the memoir that's in the annual review of *Genetics*, but basically I've learned some of the tricks of dealing with mutations that result in growth factor requirements, nutritional mutants in *neurospora*, applied those to *E. coli*, and then, in turn, to the specific question of being able to select for genetic recombination. So it did require some methodological innovation that was really the key to being able to do the experiments. One first had to ask the question, and nobody else was asking it, about whether bacteria had sex. Ever since Leeuwenhoek first discovered bacteria and didn't notice any hanky-panky in his microscopic observations there, in contrast to paramecium, where he was the first to observe conjugation, and I think the idea that there was a scale of life and that lower forms of life were asexual was ingrained at that time and was even embodied in the class name of bacteria. They're called Schizomycetes, to emphasize that they divide just by fission. So it was essentially a unasked question for 300 years.

Dick: Were there other people who were very influential in your early career aside from Tatum?

Lederberg: Francis [Ryan] was much more so. I really began my specific interests in genetics, certainly, learned how to do science. Whatever little discipline I've ever acquired, I owe to his guardianship. It was a very close-knit relationship. Tatum offered me great accommodations. He had offered me something very, very lucky in that he happened to be working with a different *E. coli* strain called K-12. K-12 has become quite famous. It's probably the single biological strain used more widely than any other in all of biology at this time. Fortunately, it's crossable, whereas the strain of *E. coli* that I'd been working before at that point wasn't. If I had confined my studies to what I had in Ryan's lab, possibly nothing would have ever happened. So I can't forget that. But he was very busy organizing the department, and while he was very accommodating, I would only see him at rare intervals. I was always very proud to be able to show him some experimental results and so on, but he did not really take a very active role, whereas Francis

certainly did.

Dick: I want to focus fairly quickly on your interest in extraterrestrial life. You mentioned in one of your articles that you really had had an interest in that subject since childhood. Do you remember what it was that triggered your interest?

Lederberg: I can't focus a specific source of it. First of all, I have to say I've been interested in a great many things. It wasn't that extraterrestrial life was a unique passion, but it was a very intriguing idea, and I was very largely self-taught in biology. I sent you a page or two out of *The Science of Life* by Wells, Huxley, and Wells. Perhaps I didn't indicate sufficiently that that was one of my major textbooks when I was--oh, I would guess, seven or eight years old. I really did study that page by page. It does include some reference to the possibility.

Dick: It has a section on extraterrestrial life.

Lederberg: Yes. And, of course, H.G. Wells and his science fiction had included that. I'd read a little bit of science fiction as I read fairy stories, and I put them to some degree in the same category, and they are. [Laughter]

Dick: H.G. Wells?

Lederberg: I read Wells, sure. Sure. By the time I was ten or twelve, for sure. I don't know if it was quite as early as that. But there was a lot of it. The press had it. I have a recollection of front pages of the *New York Times* describing one of the not closest, but closer approaches of Earth to Mars, and would there be radio signals. The Sunday supplements would have stories of that kind from time to time. I'm sure I read other books that contained that information. It would have been just part of a voracious appetite. But I can't point to a single thing. But I do remember the October 30, 1938 broadcast. What was the title of it again?

Dick: "War of the Worlds."

Lederberg: "War of the Worlds." Right. With Orson Welles. And being highly amused at the very thought that anybody could take it seriously, even to the point where that evening, after the Mercury Theater had concluded, and half an hour later there was the break, news item reassurance to people that it was only a broadcast, there aren't really men from Mars, I thought it was all a great big joke. In fact, I thought the news items were part of the broadcast, and I thought, "What a very clever dramatization that is, to coopt the network for the entire evening!" [Laughter] I could not have been more astonished when I read in the next morning's paper that people had panicked. [Laughter] There were so many elements of implausibility about it, including the fact that the whole broadcast took an hour. Oh, well, anyhow . . . I could, in retrospect, regard myself as having already been fairly sophisticated about that issue as of that date. I was thirteen.

I don't remember much more that's very specific in that direction. There were seminars about the possibility of extraterrestrial life at Columbia. I remember Curt Stern, a well-known geneticist, who was visiting Columbia at that time--this was when I was an undergraduate there--and giving kind of a journal club on Spencer Jones' book.

Dick: Early forties or so.

Lederberg: That's right. This would have been probably '42 or '43 that this seminar would have taken place.

Dick: Stern was a--

Lederberg: Curt Stern was a drosophila and human geneticist. I think at that time he was a professor at Rochester, not yet doing human genetics, but very well known in his drosophila work. He may have been a visiting professor for a quarter or something of that sort. So the topic was taken seriously enough to be a subject of speculative discussion, and it was sort of put right next to Oparin and Schrodinger's book. I remember two other books that were--

Dick: *What is Life?*

Lederberg: That's right. As part of that kind of book review.

Dick: And Oparin on *Origin of Life*.

Lederberg: That's correct. Yes. So those were matters that were part of the ambiance of a fairly old-fashioned zoology department.

Dick: Your interest in origin of life, did that evolve out of your work in genetics?

Lederberg: It certainly related to it, and the perspectives I had on the origin of life were informed by genetics. After all, with bacteria having a genetics, they also had an evolution that could be encompassed within the framework of broader biological theory, which was much less true before you had any sense that they had genes and genetic apparatus similar to higher forms and so on. Of course, now they're regarded as prototypes of how DNA works itself out through genes. But even in that statement alone and the fact that I was using bacteria as exemplars for fundamental, but highly generalizable biological issues, is an evolutionary perspective, because bacteria are part of the common theme of biological behavior on the planet. In order to justify that, you have a tacit phylogeny that relates bacterial DNA, DNA of people, DNA of plants, to common origins. So that was not even questioned as part of the premises of this kind of inquiry.

Of course, questions would arise then, is there only a single unique possibility for how life could have evolved? Could there be alternatives? Could there be other genetic materials? And so on. And extraterrestrial life would be the natural experiment to allow one to study that issue. I would not have thought it would happen so quickly, but today we may reach the conclusion that synthetic alternatives that we, by engineering, design will give us a quicker answer to whether there are alternative possibilities than actually finding it by natural observation. That may be coming along pretty soon, finding other replicating molecules and so on. But we had no reason to hope that in the fifties, let's say.

I can recall one other incident that sort of times my involvement with it. This has to have been in 1954. I remember Albert Tyler's young son. He must have been a teenager, a very bright young kid, eleven, twelve years old. I remember at Woods Hole, the marine biological laboratory, having lunch or dinner with the Tylers, and him raising some question about whether there could be life on Mars, and my having a very long and serious conversation with him about that.

Dick: Who are the Tylers?

Lederberg: Albert Tyler was a very well-known embryologist from Cal Tech, I think, was his home base, but I would see him. I went to Woods Hole every few summers in those days, and they were good friends, and he was this very bright kid. I've always enjoyed youngsters of that age.

Dick: What was his opinion on the subject?

Lederberg: I don't think he had one, but his son was very curious about it. I don't remember what Albert thought about it, but I had a very extensive discourse with him about what the possibilities were. I don't remember having had any thought then of the reality of space exploration, that we'd actually be able to test it.

So to come back to your question, my concrete interest in this subject was ignited by the reality of space as an area of tangible exploration. Of course, I'm talking about Sputnik. There I was in an interesting place at the right time. Was it October 6, 1957? Have I got that date right?

Dick: October 4.

Lederberg: I'm sorry. The 4th was Sputnik; the 6th was the day I saw Sputnik. It was, for a variety of reasons, more easily observed in the southern hemisphere than in the northern hemisphere during its first couple of weeks of orbit. So it was quite visible and much talked about, and you could see it. I was in Melbourne at the time. So that's what I mean about the southern hemisphere aspect of it. Also I don't know what the immediate reaction of folks in the States was, but certainly the Australians were sort of asking whether this really meant that the U.S. was very far behind in technological competition and so on. These were issues that were internalized in the States, too.

Then just a few days after that--it was not a few days. It was exactly a month. The date happened to coincide with the anniversary of the October Revolution, which is in November, with the shift in calendar. I had just gotten off the plane in Calcutta on my way back home, stopping for a visit of a week or so with J.B.S. Haldane. So he met me at the airplane. We were riding back, and there was some festival in the streets, and I asked him what it was about. He said, "Oh, there's going to be a lunar eclipse tonight." We got back to the place we were hosted at, at the Indian Statistical Institute, and we had dinner there. We were going to watch the eclipse. Then Haldane remarked--it had been part of the dinner conversation--that he was faintly mocking me as an American for, "Here was something wonderful that the Russians had done." He was quite an avid Marxist in those days, but even more anti-American, I think. That may have been one of the reasons for his--I'll take that back. I was going to say it was a reason for his Marxism, but that's not right.

Then he said, "I wonder if the Soviets aren't going to make a further demonstration and plant a Red Star on the moon during the eclipse as a further demonstration of technical prowess." I don't know how seriously he had entertained it, but my first reaction was to say, "Well, is that technically feasible? Would we see a thermonuclear explosion?" The maximum energy we could imagine if they were, in fact, to do it. We did some "back of the envelope" calculations and decided we just about might. It would be visible from Earth.

Then we engaged in a mutual colloquy of what a tragedy that would be, to provide a circus with that kind of demonstration, that you'd have an irretrievable

contamination with an enormous amount of radioactive fallout on the Moon's surface. It might prejudice all kinds of further scientific inquiry about the elemental composition of the Moon and all the rest of it. And it's undoubtedly true. I'm not sure which of us took the lead on this, but sort of deploring that this is what the world had come to, that in the name of this muscle-flexing and sabre-rattling of military prowess, that this enormously exciting potentiality for expanding our scientific horizons from the Earth to the rest of the cosmos was going to be subordinated. So the radioactive contamination possibility was a metaphor for the subordination of scientific goals to PR stunts. He didn't disagree with me, although I think he would have still been very pleased for the pro-Soviet demonstration that I think he half hoped would actually happen.

Well, needless to say, it didn't. But that set me to thinking that this was something that needed some more attention. I didn't think it likely that many others would be worrying about it in this framework, so as soon as I got back home to Madison, I did a little bit of reading or rereading on rocketry and space flight and some of the background, just to be sure I wasn't totally off base in what were the realms of the possible and so on. It was not that concretely informative. And then started to write a number of letters just asking that the issue be studied about whether there should not be some international convention to prohibit ecologically harmful experiments, but particularly from the point of view of interfering with scientific inquiry. I wasn't expecting it would be poisoning the inhabitants of the moon, but just making it less than a virgin surface.

Dick: I'm not sure I see the connection between your conversation with Haldane and what you just said. What is the connection?

Lederberg: He had provoked the idea that there might be a demonstration of a thermonuclear explosion, and that's generic of the kinds of things that those dumb people who think about what to do with space flight are going to do, rather than think about scientific inquiry. Besides the lost opportunities for doing good science, they might really louse it up. I then generalized from radioactive contamination also to biological contamination. I thought even for the moon, without necessarily having any expectation whatever there were indigenous life there, even the question of what might have been carried to the moon surface by meteorites and so on could be prejudiced if we dropped some of our own.

Dick: But you had no conversation with Holdane regarding biological contamination?

Lederberg: I don't remember whether it came up that quickly or not. It's a fairly quick leap from one to the other, but I just don't recall whether that was in the conversation. It would have been quite likely. It would have been a very reasonable thing to think about.

Dick: By the way, what was the source of his anti-Americanism?

Lederberg: It starts from pro-Sovietism. You might ask what's the source of that. I think he was a premature anti-fascist. That might have been the major aspect of it. But he was also a rebel in every other way. Probably oedipal explanations will do as well as any other. But he also enjoyed being a maverick, and he was in almost every dimension. [Laughter] He did split with the communists, though, over Lysenko, and he had been an editor of the *Daily Worker* and so on. But he couldn't stomach Lysenko, and failing to get any head way on that, he resigned

from the party. He didn't lose his sympathies with socialist doctrine. Then in America he thought it was an imperialist country, and he had no other unfavorable experience of America. But he left London and came to India. That's a long story. It's documented elsewhere.

Dick: I'm interested in how the subject of extraterrestrial life went from a topic of discussion, which is what it was in the forties and early fifties, to a discipline. I take it this is what you're telling me now, once we get into the Space Age, is a big part of the story of how it became a discipline.

Lederberg: Absolutely. It's the opportunity for experimental design that focused attention to what were the problematics of extraterrestrial life. In order to think about designing missions to look for life, you had to ask what were the signatures of life. How narrow or parochial a view do you have of it? Might you have completely different evolutionary patterns from the ones we know on Earth? What kind of net would you cast? I eventually came to the conclusion that nothing could perfectly answer it, but . . .

Dick: So that's one thread. But wasn't there also another thread coming from the aviation space medicine side? This was Strughold, who had already been in the early fifties, before Sputnik, talking about the possibility of life in outer space.

Lederberg: I don't think it has much to do with the medicine part of space medicine; I think it has to do with a space orientation. Once you're outside the Earth's gravitational field, you ask what else is out there. One of the questions you ask of a planet is could there be life there. Planets were among the objects.

Dick: So Strughold just happened to be a person in space medicine who was also interested in life.

Lederberg: Well, being in space medicine, he was a lot closer to the--certainly much more deeply interested in the realities of space flight than others would be. With a medical background, he'd have enough biology to pursue that query. But I don't see a direct relationship with space medicine per se. Although at some point the issue of habitability of planets would come up, and once you start asking can you grow vegetables on a planet, you'd also ask about other indigenous life forms there. So space agriculture, so to speak, might be a necessary reminder about extraterrestrial life per se.

But the issues about is there life on other planets, I think, as a series of questions had very wide currency. Most people thought of it as science fictional, but the people who believe in the reality of space flight take a different view of the matter and think very tangibly that those speculative ideas were going to have to be tested sometime, and could be.

Dick: Did you know of Strughold and his work?

Lederberg: No, I'm not sure I ever met him, or I may have seen him at a lecture somewhere. I think that's about as much contact as I ever had. But I don't think it had any bearing on the history of my own ideas on the subject.

Dick: Do you have any feeling for how important his work was?

Lederberg: I don't think it had much input on exobiology. I think he was one of

the founders of space medicine. I think you'll find that tradition.

Dick: His book, *The Red and Green Planet*, for example, did you know of that at the time?

Lederberg: I must have, but I don't think it added much to what I was already thinking about.

Dick: If we could go on with your story, then, about after the dawn of the Space Age and your idea of biological contamination.

Lederberg: Well, I was pleasantly surprised that this suggestion did take hold fairly promptly. Dick Bronk, the president of the National Academy, who also, by the way, ended up being president of the Rockefeller University--so it's a small world. But whom I may or may not have met before that, and I just don't recall. I might have had some passing contact with him in connection with other Academy affairs.

Up to that time I was not much of a science policy kind of person. I didn't go to Washington very much, and was mostly preoccupied with my own laboratory affairs. But he appointed a committee and asked me to help in the advisory program, and very properly wanted to see what other scientists would think about it.

Dick: This was after you had written these two papers, one on lunar biology and one on cosmic microbiology?

Lederberg: I don't recall the detailed sequence of events. I think it was before then. I think before I wrote anything, I'd already responded with these committee structures. I think we had Westex going by--no, that would have been early '59. So you're right. I did write in '58. But we had some other things going on in that interval, and Westex was sort of a continued advisory group, and it started to turn to questions beyond contamination, as to whether there were grounds for what I would then have called an exobiological program. As you may recall, one of the things in the summary I gave you, the answer was, yes, that there are valid scientific objectives, that the exploration for life was an appropriate concern, and so on.

Dick: But going back to lunar biology and cosmic microbiology, those were two position papers?

Lederberg: I think they were both from speeches that I gave. No, one of them was, and the one called "Moon Dust"--I have to go back to my files a little bit. I ran into Dean Cowley.

Dick: You co-authored that article.

Lederberg: And co-authored with him. He was a biophysicist. I had started to float these ideas of an alternative to the Urey-Miller Oparin model of the origin of organic material, that maybe this was something intrinsic to the cosmic condensation. This was, for one reason or another, an area he had some interest in, and I was the driver on it, but it was very helpful to me personally, and maybe to the reputation of the article, to have his concurrence on that hypothetical conception. So that was the main thing that that article was about. Connected with it was maybe an examination of the surface of the moon could provide testimony about this

interstellar material, and that was before we had regolith models and understood how thoroughly reworked planetary and lunar surfaces were going to be. It sounds a little bit absurd today, but believe me, there was room for a lot of speculation. I remember Tommy Gold writing a paper, worrying whether we could send a lander to the moon because there might be twenty feet of very loose dust on the lunar surface, that anything we sent would be buried in it. So that gives you some notion of the range of acceptable hypotheses at the time.

Lederberg: One was the COSPAR paper. One was the first COSPAR conference in Nice.

Dick: On this subject?

Lederberg: The Committee on Space Research had annual meetings.

Dick: You mean particularly on--

Lederberg: Do you have my reprints there?

Dick: Yes.

Lederberg: Where is that cosmic microbiology one?

Dick: You mean the "Moon Dust"?

Lederberg: No. You said there was another one. That's lunar biology.

Dick: No, I don't believe you sent me--the first ones that I have are the "Moon Dust."

Lederberg: So where do you get cosmic microbiology?

Dick: That was mentioned in your article in the *Journal of Genetics* here, that you had written two papers.

Lederberg: Let me see. Are there references?

Dick: No. On the lefthand side at the top, you say you had written two papers, one on lunar biology and one on cosmic--

Lederberg: Oh, these are memoranda, not papers. These are unpublished. Aha! Okay. I had forgotten about those. I'll get those out for you. Those are unpublished memoranda which I included in circulating my concerns about contamination, etc.

Dick: I would like to have copies of those, if you could.

Lederberg: Sure.

Dick: So that, then, is what set Bronk and Seitz to setting up these committees?

Lederberg: That's exactly right. Yes.

Dick: Was it difficult to convince them of the importance of this?

Lederberg: Apparently not. I mean, I thought there would be a lot more resistance. In December I was writing those memos. On February 8, the Academy Council formally expressed its concern and [unclear] into it. So they moved very fast. Now, in retrospect, I can see that they correctly saw that space was going to be a big thing, and the Academy better get involved in it. This would be some vehicle that if the issues were in any way legitimate, that the Academy shouldn't be left out. I didn't know then how easy it might be to get the Academy to take an interest in one subject versus another.

Dick: So at this time, then, the biological community for the first time in any significant way, I think, started to have an interest in what eventually became called exobiology.

Lederberg: Yes.

Dick: Can you tell me about how that community was mobilized for this subject?

Lederberg: Well, it was mobilized by the daily papers, above all. [Laughter] I'm sure I was hardly the only one to see the opportunities, although a lot of biologists didn't have the foggiest idea how they would relate to a NASA--no operational precedence for how you would go about doing that. In terms of how we got engaged, I think the Westex and Eastex committees were the first vehicle for that. And not surprisingly, many of the ones who were interested enough to serve on it and were instigated in that direction, did become active participants in one way or another.

Dick: So Westex and Eastex were both set up by the Space Science Board?

Lederberg: I suggested that instead of a lot of travel, that we economize and have two groups, and then we'd coordinate at the end of it. And they accepted that, and it was not a bad idea, as a matter of fact.

Dick: How did you decide who would be on each committee? Were there biologists that you knew in particular?

Lederberg: I decided on the Westex, and whoever was chairing--was it Bruno Rossi who was chairing Eastex? Or was it Luria?

Dick: I think it was Luria.

Lederberg: It was Luria then. But he and Rossi were very close. I'm sure Rossi was his main advisor. So Luria had full cognizance of who his group would be, and I just looked around and tried to find the most appropriate spread of people, then suggested that to Bronk, and then Bronk said, "Fine."

Dick: So were they people you knew would have an interest in this subject?

Lederberg: Yes. Yes. Either would have or should have, would be the way I'd put it. [Laughter] "Should have" is more like it. I mean, I would approach a Roger Stanyer and say, "Roger, isn't it time that you thought about this issue?" If he ended up on the committee or not, I don't remember, but I remember talking about it with him. He was a microbiologist of a very broad interests, and had the kind of talent that I felt would be needed for it, and that reciprocally, there would be some

personal interest into getting into it.

Dick: So there were two things really going on here parallel, weren't there? One was national in the U.S.--Eastex and Westex, and the other was the COSPAR meetings under this term CETEX, Committee on Extraterrestrial Exploration.

Lederberg: ICSU organized CETEX.

Dick: Right.

Lederberg: So that was the international level. Bronk was exactly right in referring it to them so it wouldn't become an American proposal that had to be sold to the rest of the world, that it could actually be worked through on an international basis.

Dick: But did Eastex and Westex make recommendations then to CETEX?

Lederberg: If we did, it would have been through the Academy. We reported to the Academy.

Dick: Let's see. You were in charge of Westex.

Lederberg: Yes. It was very informal. I think I put myself down as rapporteur rather than chairman or "in charge of," but I did organize it.

Dick: And your agenda was entirely to discuss this contamination issue?

Lederberg: No, it was broader than that. I think that's manifest in the summary. It was to provide advice to the Academy for the benefit of the government on what the role of biological sciences and space exploration would be, but separating that from the space medicine issues. We did have a very sharp delineation on that question, either because it was already there or because I had no particular interest in it.

Dick: How did your reports feed in to NASA then?

Lederberg: I think there were pretty regular and frequent consultations between Bronk and the administration, and some of this preceded the organization of NASA. When was NASA formally organized?

Dick: October of '58.

Lederberg: Yes. So some of this stuff precedes that. But Bronk was very well connected with government at that time, so he would have been the intermediary.

Dick: But the Westex and Eastex reports were not ever published, is that right?

Lederberg: Not in print, no. They were formally submitted to the Academy. Then later on, the Space Science Board was set up, and I suppose we were subcommittees of the Space Science Board.

Dick: You've given me a copy of the summary of the Westex, and there should be a copy somewhere of the Eastex, probably in the archives of the Academy.

Lederberg: They were fully concurrent. There might have been differences and nuances.

Dick: As we were saying earlier, we were talking about this panel two of the Joint Armed Forces National Research Council Bioastronautics Committee. You don't really remember how that originated. You had no direct dealings with them.

Lederberg: Only via Mel Calvin, and I sent you my correspondence on that.

Dick: Right. Following your Eastex/Westex, which were both relatively short-lived, I take it--

Lederberg: Yes.

Dick: You were at Stanford by then?

Lederberg: Yes. February of '59.

Dick: You set off in another direction, which is with this instrumentation research lab, which actually studied--

Lederberg: Yes. Well, during '59 and '60, I continued to be on these committees. I was on the Space Science Board. When was the Lunar and Planetary Missions Board set up? That was a little bit later. I had a number of meetings on the contamination question. I remember a fellow called Davies was one of the JPL people that we talked with about getting down to particulars on sterilization methodology. Once this went in, it was taken very, very seriously. I suppose I would have thought we were doing very well if we had clean assembly and maybe some disinfectant scrub, but they decided they would do it right and do a complete spacecraft sterilization. I was astonished that it was feasible--politically or technically.

Dick: And economically.

Lederberg: Economically. I don't think it added--it was some small percentage of the total cost, which is still some bucks. But Al Hibbs told me privately that the thing that won it was that it was also a guarantee of reliability, that it was sort of an accelerated lifetime testing all the components, and he wasn't against the decontamination argument, but one of the reasons he was for sterilization was he thought it would actually be a plus in terms of setting very rigorous standards of reliability of operation in a hostile environment. [Laughter] So I'm sure that played into it.

[Begin Tape 1, Side 2]

Lederberg: We're talking '59, '60 now. A fellow called Davies, I think, was the one who prompted me. He worked with Hibbs down at JPL, and he said, "Josh, you know, you've done a good turn. You're providing critical reaction here, but why don't you get with it? Why don't you play a more positive role if you really believe this?" I said, "What do you mean?" He said, "Well, you think we're going to have a Mars mission one of these days, don't you? Why don't you get involved? Why don't you be part of the design?" That hadn't really seriously occurred to me to be other than a committee person.

But meantime, I immersed myself, much more than I ever thought I would, in the engineering aspects of space flight. I read the basic books. I was a little startled to

read Carl's [Sagan] remark in that letter I sent you about my calculating the transfer orbits. My god, did I really do that? [Laughter] That will give you some sense of how deeply I immersed myself in the technology.

So I began to be a little more intrigued about how one might use some of this high-tech stuff with some dual purpose and space instrumentation, doing better for measurements and so on on Earth, so I decided, yes, I'd give it a try.

I forgot at what point I recruited Eliot Leventhal, but it must have been very early in the game, and certainly with the benefit of knowing that he had actually run that lab. I thought this was a no-lose proposition. So we put in a proposal to NASA, and they were very, very generous in those days. So we set up a lab, and [I've] never regretted it.

I'll tell you something. There's one piece of fallout from that lab that has much more than paid for it. I don't know if you've heard something called the FACS machine, the fluorescent assay cell sorter.

Dick: No.

Lederberg: That was a direct byproduct of that laboratory, and it's been an absolutely invaluable instrument. It's one that looks at cells one at a time as they go through a capillary. You can do them at 1,000 a minute. It gets a visual reading on each cell. It can be dyes. You can get fluorescence measurements and so on. With electrostatic deflection of the microdroplet, you can actually sort different kinds of cells into different buckets. So you can have a white cell population that's got sixteen different kinds of T-cells, and actually over periods of a few hours get physical separation of cells according to what antigens they have on the surface and so on. It's become a major piece of cell biology.

Dick: So that was a byproduct of something you were doing at the lab?

Lederberg: Absolutely. A direct outcome of--

Dick: What was the stated purpose, or the general purpose of the work of the lab?

Lederberg: Conceptual design of instruments for a Mars landing. But we were not promised, nor assigned, the job of the final stages, which is to work out proofs of principle for different kinds of automated instrumentation. That's what got me into computing--or vice versa. But anyhow, that became highly convergent around that time and so on.

Dick: So is this where this multivator was originated?

Lederberg: The multivator was our proposed design, and it was a piece of automation that would run a number of samples through a wide variety of tests. It was a rotary cassette, a little bit like a film cassette in a Kodak projector, and various ways of dealing with it. A far more serious investigation of that sort of automation was probably going on even at the time in Technicon and Beckman, but at the time it was at the edge of the art for that sort of automated laboratory operation.

Dick: But the multivator concept was not one of the ones that flew eventually?

Lederberg: Well, not as such. I ended up deciding not to compete for the final instruments. I would have gotten totally enveloped in pushing one instrument through all the engineering efforts. Instead, on the team there were three members with instruments and two of us who were sort of equilibrators and tie-breakers and so on when it came to deciding what would go on and maintain some coherence among the group. I quite gladly accepted that role. The people who had specific instruments were in some ways much more distant from the scientific perspectives because they were so involved with the engineering group that was going to go for every last detail of that particular device.

Dick: So how long did your efforts at that last, then?

Lederberg: Forever. [Laughter] Pretty much the time I left Stanford.

Dick: But not on that subject.

Lederberg: Not on that subject, but it became a place--it merged into . . . Mass spec[trometry] was one of the technologies that we got into, and we then started pursuing mass spec for a variety of other purposes in exobiology, in automating it. We were the first people to put together a G.C. [gas chromatograph] mass spec under computer control, and Finnegan brought that out as a piece of instrumentation that they would sell and so on. So there was actually quite a bit of spillover, and it's the kind of thing that we could not get funding for from any other place in government at that time. NIH was not putting out money on this kind of technology development, that's for sure, or very little. Later on, they did more, and when they did, we switched more and more the basis of the lab to NIH.

Dick: You were the person who coined the term "exobiology."

Lederberg: Yes.

Dick: Do you remember sitting down and doing that? At what point was that in your work that you did that?

Lederberg: I don't remember the moment, but I remember thinking that extraterrestrial life was quite a mouthful in trying to explain what subject you were into. Besides, I wanted to define a discipline. Extraterrestrial life was an attribution. So the study of extraterrestrial life is what exobiology is, so it was intended to be shorthand, but as we discussed over dinner, I was also trying to convey the larger conceptual reach of the concept, that esobiology was the antonym, and that had the connotation of something esoteric, something parochial. Exobiology is everything else, and that's a much larger domain.

Dick: You were saying also that the term "exosphere" was in use at the time. So "exo" would mean beyond the Earth.

Lederberg: Yes, if you wanted other provocation, "exo" for "outside." I knew about exoskeletons and a lot of other exos. [Laughter] In fact, I knew the word exoteric, which . . .

Dick: Exoteric?

Lederberg: It's the opposite of esoteric. [Laughter]

Dick: Okay. [Laughter] You mentioned exobiology as a discipline. Can you give me your feelings about when it became a discipline?

Lederberg: Well, I'm not sure it ever did. I mean, it's a gradual transition. Phil Abelson used to mock me by saying it was a discipline without any content. I can understand what he meant. I knew we were on pretty shaky ground. But I would say the turning point for serious systematic study, I would say the Mars conference in '65, those two volumes that the NRC has published.

Dick: Oh, yes. *Biology in Exploration of Mars*.

Lederberg: Yes. I would say by that time there was enough variety of content, calling on information from a variety of places, that anyone writing on the question would have to invoke the body of knowledge that was represented in that book, not necessarily those identical papers, but they would be reflections of it. So at that point there was a common ground of what was agreed upon and what wasn't. I think that constitutes a discipline. Now, it is still a hypothetical one. We don't know if it's an empty set or not, but I think there's much broader--we don't have to start from scratch every time you introduce a subject of, well, what range of elements do you have to consider, and could you have life without water and so on. A lot of the obvious questions have been churned through already. And not that they're answered, but at least the questions have been put in agreeably understood form.

Dick: Would you say exobiology reached a peak with the Viking, or would you say it's a thriving discipline now or a dying discipline or what?

Lederberg: Well, there are very few practitioners of it at this point, on the one hand. On the other hand, if there were to be a renewal of a Mars mission or any evidence bearing on the field, I think it's a dormant sea that could be made to germinate rather quickly. There are a few rediscoveries. People occasionally write papers now and then. I'm tempted to remind them that we went all through that twenty, thirty years ago. But not much. It's in the literature. The literature is accessible. If we wait another hundred years, it will probably all be forgotten and start over again.

Dick: Do you think more attention, more funding should be given to exobiology these days?

Lederberg: Well, that's problematical. I'm not in any hurry to push space. It's not my highest priority. I would think that a space program for science of the order of half a billion a year doesn't give you very many missions. That's probably about the level of priority I would give it. The only other things I would do would be things that had immediate and obvious terrestrial applications. You know, the communications satellites are great, overhead observation has kept the peace. Meteorology is great and so on. But things like the space station or a manned trip to Mars and so on, in my view, they're absolute boondoggles. That having been said, if, in spite of my lonely complaints about them, they go ahead anyhow, then I say, okay, let's use this transport and do at least something useful with it. I first want to see that question settled first.

I do, though--if there's going to be an eventual manned mission to Mars, I would fight against it, at least for the foreseeable future, but if it's going to be there, I really want to see the instrumented ones get there first. One, I think it's necessary

for planning purposes before you risk people, and, two, I am concerned about once you put people on a planet, it's dirty. It can't be avoided. So I'd like a chance to sample it clean.

Dick: So you're still worried about the contamination issue.

Lederberg: Yes, I think so. I don't know to what extremes I would go to insist on it, and a lot of Mars is self-sterilizing. But if you're going to go to an interesting place, by definition it's not self-sterilizing.

Dick: But are you satisfied with the efforts that were put into making sure that the moon and Mars were not contaminated?

Lederberg: Oh, very much so. I doubt if we'll ever reach that standard again.

Dick: So you've got no worries that any of the Russian ones that crash landed--

Lederberg: Oh. Russian ones I don't know enough about. I'm sorry. I'm talking about the U.S. ones. I just don't know. Can't tell.

Dick: Exobiology has had its critics. You mentioned one of them--Phil Abelson.

Lederberg: He didn't know how deeply I agreed with him. He thought I was a champion of the space program in general. As you've heard, I certainly am not. But he did go beyond that, and he thought it was pretty silly to even think about the possibility of life there. He was satisfied a lot quicker than I would be that there was conclusive evidence on the point.

Dick: Who were the other major critics who come to mind?

Lederberg: [George Gaylord] Simpson, although the brunt of his criticism was any thought of--he wasn't thinking so much of planetary as much broader.

Dick: This is the evolutionist.

Lederberg: Yes. Much broader issues--could there be humanoid evolution and so on. But I think some place he's quite skeptical that life could even have gotten started more than once. Am I wrong about that? My memory's a little bit dim on that.

Dick: I think that's probably right.

Lederberg: Those are the two main ones. I recall a few folks were concerned that exobiology would provide authentication of the space program, and I sort of half agreed with them. I had my own concerns about that being a consequence, but I concluded that if we were going to have Apollo with no reference to science whatsoever, that there were some inevitables on the political side, so that was lost. Let's make do the best we can with what's left.

Dick: Let's talk a little about Viking. What was your role in Viking?

Lederberg: I've already mentioned the most part. I was part of the Viking lander biology experiment team, and that was to provide general guidance. And then as experiments began to be specifically selected, [my role was] to be part of the inside

body of critical examiners, about how it was going, how the results would be interpreted, and so on. It was pretty intense involvement. There was a committee meeting every couple of weeks, a trip down to JPL equally frequently, and as we got close to the event, it was sort of every other day and so on, going on for years, though. I was also on two or three other advisory groups that fed into the same posture at some of the more strategic levels. There was a Lunar Planetary Missions Board. That was at the Academy, as I recall, and there was something else in NASA likewise, so I was feeding in at a variety of places. I would voice my complaints about the space program as a whole. I'm not too inhibited about that. That didn't totally deter them from calling me in. [Laughter] But I was certainly no toadie.

Dick: Did your expectations for life on Mars change during the sixties and seventies as various ground-based observations changed? For example, in the early sixties, when you were doing Westex, there were Sinton bands which could have been due to organics, a major argument in favor of life on Mars.

Lederberg: Well, I never swallowed it, but in the paper I wrote with Carl Sagan, we did quote them as potential evidence. They were hardly conclusive. The answer was yes. Was it Mariner 9, Mariner 10, those fly-bys? I was down at JPL when the pictures were coming in, and I remember looking over it with Bruce Murray. I do remember seeing a wiggle on the screen there, and I said, "Oh, there's the River Murray, Bruce." [Laughter] And he knew what I was teasing him about. It turned out that they probably were rivers after all, but fossil ones. But those pictures, at first sight, looked very, very discouraging. The original implication was that there could never have been any water in the planet. They really didn't see, with the exception of that one wiggle--I wonder what ever happened to that, riverine. I'm trying to remember exactly where it was.

I also recall going over the pictures very carefully to see if there was any evidence for clouds, and I had a "Eureka" at one point where you could barely see it on the pictures. Went back to the digital data and, by God, there were two pixels adjacent to one another, one of which was high luminance and the other was dark. I queried could that have been an instrumental artifact. The answer is probably not. I said, well, there has to be a cloud in that shadow. That was pooh-poohed, but I tried to arouse some interest in it, but nobody wanted to take it any further. I think maybe reasonably there wasn't anything more you could do about it to validate it one way or the other. I don't recall if there was ever a more thorough systematic search for those kinds of features which you could have run through the computer and tried to look for them.

I remember the later missions did show places where there might have been clouds of one kind or another, and I've tried to see if we could correlate that spot to the later data. I've never been able to get a good mapping of it. So I may or may not have seen that on the Mariner 9. You know, that would have been some mitigation of the model that Bruce was throwing on. And the answer is, yes, it got to be more and more discouraging. I still felt that it was not so conclusive that we ought to give up. But I certainly had to accept the way the evidence was starting to come in was--

Dick: Were you surprised then with the Viking results or not?

Lederberg: Yes, I was surprised. I hadn't taken account that the ozonosphere would be right on the ground. The thing that was a surprise was Claus Biemann's

finding, but thank God for it, because the other experiments ended up being rather less conclusive than we thought. We thought we'd have a problem getting any kind of a signal. It turns out that we got signals from two out of the three experiments. The question was how to interpret them.

Dick: The surprising thing was no organic molecules.

Lederberg: That settled it. That said you'd better think very carefully about nonbiological interpretations of the others. Then as we understood a little bit more what an iron-containing soil would be like if insulated, with a tiny amount of oxygen, some of it going into ozone, what a heavily oxidizing surface that would be, we began to see that--well, we certainly could have designed the experiments differently if we had anticipated it, and would have certainly, even without knowing the carbon result, if we had been able to model the Mars surface on the basis of that kind of insight, we would have designed experiments to clarify that chemistry. That would have been the most important, and certainly been not very optimistic that much would be in there.

Dick: So you put no credence in Levin's claims that there might be a biological cause.

Lederberg: No. Gil thought he saw green leaves on one of the stones. You know that story?

Dick: Yes. You don't believe it.

Lederberg: He just didn't understand color. [Laughter]

Dick: Does that fall in the category of scientific attitudes toward hypotheses of low probability? [Laughter]

Lederberg: Well, it's one thing to say, "I'm going to risk doing an experiment." It's another to go out and say, "I've proven something," when, in fact, you haven't. Those are not the same ball game.

Dick: Right. Some general questions I'd like to ask. These days SETI is a bigger area than exobiology itself. What is your feeling about the SETI enterprise which is now being undertaken by NASA especially?

Lederberg: Well, it's certainly one of the great long shots in the world at a reasonable price. I'm not going to turn things like that down, and I think the price has come down to where it's quite reasonable. I was not too much of a fan of it ten, fifteen years ago when I think the data were otherwise. I think the level of technology that was affordable was very primitive compared to what we have right now, and my view is it's reached a crossover point. My admonition then was to wait until now. Well, now's happened. I don't have the foggiest idea what the outcome's going to be, and I think even the experience of looking is going to inform us, that we will have more tangible thinking about what we're up to. We'll refine the way we view the problem, and we'll be better off on that account, quite apart from actually having attempted to gather some data. I think a lot of other ideas will come along about how to economize still further.

That table I discussed with you, that note I put into that paper for the SETI conference at Yeravan about looking for information in the frequency domain rather

than the time domain, and I'm sure there are a lot of other thoughts of that kind about how one would most efficiently store information to accommodate to having very low signal-to-noise ratios, how the only thing basically we can do about that is integration over a long time. Other concepts of what to look for will come along.

I mentioned a worry about Doppler shifting over long time spans, but we, roughly speaking, would know how to compensate for that. So we could add that into our integration mode and it would make it--in fact, if you got something that was a signal after Doppler adjustment and not before, you'd be pretty sure you had something.

Dick: How about the various factors in the Drake Equation, the probability of the origin of life and all of that?

Lederberg: Well, I'd prefer to have--no, I know the right coefficient to use. It's 0/0. I think those are indeterminate. To assign a value of 1 when you don't know what it is, has no particular legitimacy. You could pick any number and it would be equally legitimate. Well, even that would say one-half is what you come out with. [Laughter] I'm not sure I agree with that. So any number isn't quite right. I think there is something--it's not mathematics to write down an equation of that sort and multiply the units in that fashion.

Dick: It began as an agenda for the meeting in Greenbank, and I think he never intended it to be considered a real equation.

Lederberg: I hope not.

Dick: It certainly has caused a lot of discussion. It's an interesting way to discuss the factors.

Lederberg: It's a way to explore what you think, but you see, there's an answer other than a numerical coefficient in a case like that, and that's the agnostic one. Now, do you replace Ignosco with .5? I'd have to stop and think about that. If I'm going to use any other number, it means I know more than I said or I have lurking in my unconscious some impression other than what I just said. I think generally I'd be inclined to be more conservative at the argument that because we're here, life must be prevalent, doesn't have to obtain. We just don't know if we're a typical sample. But the net result is that it's not a function that has the kind of predictive power that you're looking for in functions of that sort. It's a way of dissecting your unconscious, and that part I would agree with. So the unit coefficients, yes, they make it a worthwhile subject of discussion, but I don't think you get much out of multiplying the terms together.

Dick: So you are not confident that given the proper conditions, that life would originate on another planet?

Lederberg: Not at all confident, with one exception. Part of the appeal that my own cosmobiological model has is that it says this is not a decision that's been made totally independently planet by planet. It may be built into the cosmic condensation. There may be more homogeneity to what other planets could experience in primordial chemistry than the atmospheric chemistry model would furnish.

Dick: Is this something you published?

Lederberg: That's the paper on "Moon Dust" and that appendix that I wrote a couple of years ago on that. If there is a more homogeneous process occurring on a very large scale, it may be a quirk of how carbon and nitrogen and oxygen and hydrogen atoms collapse, starting from a plasma, does have a common rule in that purines and pyrimidines and some sugars may, in fact, bounce out of that. In that case, the probabilities from one place to another are not independent of one another if we have that common heritage. That's the exception that I would still want to pose, and I think there's a lot worth testing for. It's one of the rationales for going more deeply into the molecular chemistry of comets. I think most people's expectation is one comet is going to be rather like another one in this regard, and that they are quasi-samples of interstellar matter. So we may be not too far from getting a little better handle on what a universal principle of atomic condensation would give you, and there may be some surprises. Things may fall out of that, that fit rather neatly into the pattern of evolution we've seen on Earth. If there is that connection, then the likelihood of similar kinds of evolution is much greater.

Dick: Do you have an opinion on Fred Hoyle's idea about bacteria in space?

Lederberg: Oh, that's absolute nonsense from beginning to end. [Laughter] Where would the bacteria have come from in that abundance? You've got a spaceful of bacteria? I shudder about Hoyle because he's taken an idea similar to what I just said and carried it to an absurd extreme. I don't think bacteria are going to self-organize out of a cosmic condensation.

Dick: Is there anything else that we haven't covered that you would like to put on the record regarding your involvement in exobiology?

Lederberg: Well, I think you've teased out my basic attitudes toward it, but you haven't asked me about what next. There are habitats on Mars that have not been explored yet, so I think there is still an outside possibility that we could still find life there. The equatorial zones are out. We've only sampled a rather narrow band. It's interesting that the two sites we have been at are very similar to one another. You could go to the zones that do show seasonal fluctuation. Those are too cold for most of the systems we can think of, but with a very thin atmosphere, just putting up a vertical plane would capture enough energy that locally you'd get a drastic change in temperature. So you might have oases on that ground, you know, at just the bottom of a cliff or something of that sort. You don't have a uniform regime by any means.

I used to think that you needed a way to connect solar radiation as energy inputs with sources of moisture. You might have those in a permafrost. As you go further down, maybe there is some still liquid moisture at lower layers. Then you'd have to have singular events the equivalent of a fumarole or something of that sort with an energy flow sufficient to keep an area both moist and warm at the same time. That's hard to do with a very low density atmosphere.

But with the thermal vents that we've been seeing, maybe you don't need the sunlight. You could have a living system which is driven entirely by chemical energy inside the planet, which essentially is what the thermal vents do at this time. We mostly don't believe that's where life originated, that it's a later habitat, but we're not even sure of that.

In the case of Mars, the second dilemma is what about 300 million years ago when

those rivers were being carved out. That's a very exciting question, and that's no lose to try to get enough information in the Mars Rover missions that might elicit more of that, that would tell you more about the history of the planet in those dimensions. Anything's possible there.

Dick: Wouldn't a manned mission, where they can poke around in various cubbyholes and different environments, be even better?

Lederberg: I think for the price of a manned mission, you could send ten Rovers, so it's not better. I think man can do just as well from the ground in directing and interpreting those data. The cost of putting the man there and bringing him back is absolutely horrendous.

Dick: But don't you think in the future, if not the near, at least the far, that it's sort of an imperative that people will go to Mars?

Lederberg: I don't see any imperatives in it.

Dick: You don't see a sort of exploration imperative?

Lederberg: I see an exploration imperative, but for somebody else to walk there doesn't satisfy my libido. My participating in an enterprise whereby we have figurative extensions of our own personality. I'd rather 1,000 people had a chance to be at the control levers of a teleoperator than to have one person be my proxy and walk there. With all the constraints there are, it's a rather encumbered walk. So I think for the foreseeable future, man is an encumbranced exploration, not the purpose of it. But people have to look at it that way to agree with it, and many of them don't.

Dick: But you would like to see at least a Mars Rover funding?

Lederberg: If I scale this on the priority of scientific missions to be accomplished, I don't place it extraordinarily high, but I think the order of 500 million a year would be rather reasonable for this level of inquiry, and I think at that rather modest level the other technological drives that it satisfies, sort of the secondary or non-scientific spinoff is enough to make it at least competitive with a lot of other big science projects.

Dick: We're very close to the end of the tape.

Lederberg: I think that's an opportune time.

Dick: Thank you very much.

[End of interview]