JJ/Q/216

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION INTERVIEW WITH JOSHUA LEDERBERG at STANFORD UNIVERSITY, PALO ALTO, CALIFORNIA.

Tuesday, 23 August 1977

(This transcript was prepared from a tape recording.)

EE: We will have it done by professional transcribing services in Washington.

JL: Okay.

EE: And you can then correct, amend, add to, do whatever you want to.

JL: Well, there are a number of things I would want to try and get back to, some of the documents.

EE: Right.

JL: Details I haven't -- I did try to collect some stuff on how I myself first got into space, so, I have a little folder, a couple of things like that -- that was like in 1957 but I haven't been trying to aggregate or organize the material. But I think I can find quite a bit of stuff that would be helpful.

I guess first of all I have -- this is a very straightforward outline for the narrative discussion for getting in the basic historical information. I have no comment about that. It looks first rate. Besides that have you yet come into any issues? Is there any -- are there themes that you are interested in exploring? Are you writing that kind of a history?

EE: Yeah. I'm trying to -- I guess what I'm trying to deal with is sort of the intellectual history as much as anything else of what I consider the modern exploration for a phase of Mars -- the spacecraft oriented phase.

JL: Okay.

EE: And I'm really interested in trying to deal with how people were drawn -- especially yourself -- into a discipline such as exobiology when there was so much that could be done in terms of the life sciences here on earth.

JL: Uh-huh.

EE: Why people were drawn into that.

JL: I'm glad you put your finger on that. For one thing, a lot of people were never drawn into it or only very, very reluctantly. And the cast of characters you find among the biologists and the people related to the program are obviously a very small sample and it must be true in some respects -- a peculiar sample although I would be at a loss -- I'm not --

I don't have the detachment to say what it is that made us different from most everybody else.

It would be, I think, an honest history of this development would also include all of the criticism that we were exposed to. Some of it by public and direct people like Abelson and --

EE: Commoner.

JL: -- Commoner and Simpson, who I guess is intellectually the most respectable of that group. And much more of it, as you might expect, it was never expressed in print.

Some of that was directed against the space program as a whole and why don't we rebuild our cities instead of these thrills and toys and some of this was at the level. I think they were contaminated. I found it hard to accept. I was eager to get the straightforward pure critical comment that was directed at the actual issues.

Simpson came the closest and it was just as dogmatic as what he was criticizing. How could one accept his assertions as being any more likely to be true, you know, that life was unique on earth -- that certainly if that was not the case that ultimate intelligence surely was, and so on.

So, when I had to conclude that there were other motives behind their concerns and that they were not based on the scientific substance, the most valid criticism and one that I guess still holds is that perhaps the effort is premature and that the issues we are trying to reach are too difficult for our existing technology quite apart from the scientific interest.

And the answer to that is -- well, maturity is only defined by the kinds of efforts that people make and you build stepping stones. And that a few people have to fail to begin with in any enterprise that's important and complex and that is eventually successful.

Now, I don't know if you will find the attitudes I'm expressing widely accepted among the protagonists. I'm sure there was a good deal of arrogance and a certainty that the dogmatism of equal depths that "By God, there is life on Mars. We're going to find it." Or, "You get your wagon to that star." It almost doesn't matter what's in it, you can get a good ride along the way.

EE: There was that and there was some ambiguity too. I think Horowitz is an example of another never really clearly stating why it was he was going.

JL: It was curious because on contamination questions he would accept a policy that there really can't be life on Mars and certainly it can't be contaminated, and yet he would be enthusiastic and a very skillful party to actually trying to do something about it.

That degree of complexity I think is typical of scientific efforts and it means a suspension of judgement. It means the ability to pursue both sides of the issue as long as there is any possibility of either side being correct. And that I admire, so I have had no difficulty with Norman in that regard.

I think once or twice he went a little further in detail in asserting one side of it to a degree that I then felt was inconsistent with his staying in the program at all. If he really was that sure of the impossibility of the effort then why was he there? As long as he was saying that he didn't put a very high likelihood on the outcome but that small residue of possibility of finding something interesting multiplied by the degree of interest that there would be in it, then you could see that there was some justification for it.

But that's something I don't know that anyone has really properly looked into, and it would be dishonest not to allude to it.

EE: Well, I'm trying to -- I am not writing a technical history or a company history in the standard sense of the word. I want to try to get the human interactions as much as possible because I'm a firm believer that human beings make -- you know, any endeavor like Viking is not -- you know -- is not an agency created thing.

JL: No, those are much harder to get at.

EE: I know.

JL: And you will find those mostly at much more detailed kinds of questions, not the philosophical problems. But if you're talking about the history of ideas, the very tenability of exobiology -- it was frequently called the only discipline that has no content and I couldn't really disagree with that --

EE: Or a discipline in search of a subject then?

JL: Yes, okay. Well, I understood that kind of remark and I couldn't really disagree with it. If someone had asked me what exobiology consisted of I would have said it was a theoretical subject trying to find an experimental framework in which it could operate. That it was a little bit like mathematics looking for computers but without the logical rigor that mathematics had, and that if nothing else it would shed light on the logical structure of our thinking about biology which I think has not been attended to adequately. And that at best you might find something.

But the issue once posed -- what are optimum means of disposing of your resources so as to answer the question raised, there is the actual content of exobiology and to actually land and do try to get some data that you are actually trying to understand. I have some data -- exochemistry is a little closer to the truth then exobiology right now.

Well, now that something is actually happening by way of observational data you don't often hear that remark any more even though the data are very discouraging. But for many laboratory scientists this is an extremely unfamiliar and uncomfortable mode of operation.

They are having to deal at such a speculative, hypothetical level for so many years -- not being able to get your hands on some tangible information. And then to do that within the framework -- you might say the operational framework with which this is done, the NASA colossus, and the committees, the administrative aspect of it, the politics, and so forth.

Well, the two together were much more than most scientists could bear and I can deeply sympathize with that. I don't know how I would explain why I continued to bear it but I might be able to say one thing about it and I wouldn't like to be quoted directly but I'd like to have the idea behind it.

I was interested in planetary exploration since I was a kid. You see, I'm just part of the warp and woof of thinking about biology, by and large I've read Jeans and Eddington plus some of the popular stuff as well. So, when I was learning biology it was in a cosmological framework and Oparin's work was the seminal difference.

That's the point I want to distress to you in your three headings of the sources of ideas. I will elaborate on that in just a minute.

I think that getting the Nobel Prize and the kind of -- what shall I say -- stability that it offers made it possible for me to stay in a non-reputable game. I don't say disreputable, but a non-reputable game. It might have been very, very difficult otherwise and it would be very hard for a capable young scientist whose had a lot of risks to take in his career to hitch it to something as uncertain as exobiology.

I think that's why you may find some dichotomy. That among the more seasoned and older people you will find people with well established reputations who can afford to get into that kind of a game. It's out of the usual competition. It's in a completely different ballpark. But they have a platform that they can stand from.

If you go to the younger people you then find that it is a rather odd group. That many of them don't have any reputation outside the field. They were attracted to it for a variety of reasons. Some of them their own sheer fascination with it; some of them just happened to come along and found something that they could try to do and they didn't have that much better at the time. You know, a whole mix of things might be involved there.

I just thought that was something you might want to take some notice about. I think you do find that to a lesser degree than the other disciplines that are more reputable -- their connections, physics' connections with space exploration doesn't require apologies and biology generally seems to.

EE: Right. Is that to a certain extent a result of the fact that biologists tend to be somewhat insecure in their own discipline? You know, in the late '50s and '60s it was burgeoning and they didn't know what direction it was going to take.

JL: Oh, no. Quite the contrary. That was the heyday of optimism. There were so many things to do and so forth. Things were exploding all over.

But consider what we've already agreed on -- the rather critical attitudes that some of the main-lines of the discipline had about this effort. Well, what kind of person just going into his career is going to buck that kind of current? You will find some real obstinate headstrong ones who may anyhow. But I don't want to overplay it. I may be -- by trying to explain it I may be putting more emphasis on it.

EE: No. I have that feeling from what I've --

JL: There are a lot of people connected with it who might say -- you know -- do not have significant main line reputations. Those are among the younger people.

The older people connected with it are the ones that have already won it and can feel very secure and not give a damn about what the majority of their colleagues felt about it. And that's the distinction.

EE: Can we tick off some of the two groups? I would take it that Carl Sagan would be among the younger group.

JL: Well, he's not a biologist. He's an astronomer and he stands by himself in every respect. He was one of the headstrong young students that was willing to buck it.

As an astronomer the mere notion of going into planetary astronomy at the time he got started was crazy. You know, Kuiper was still busy with it, but he was warned many times that it would doom his career here to remain stuck with that and then have a continued interest on questions of life there. It's really no part of science.

But he stuck with it anyhow and he'd had, as you know, in a sense a rather checkered career. He's made brilliant scientific contributions. He's very critical, very imaginative insight into physical processes that may go on. And he has also published many, many very wild speculations and you have to know his lingo to know how he has stamped them. He generally has but sometimes in the ways that the less informed don't quite see --

EE: Right.

JL: -- where he's going from one to the other. Of course, he also sees himself as having a significant role, which he does, as an apologist for a publicist for science. They build public interest and public base for it. You know, to have it continue to go.

But he's really quite unique in that and his commitment to questions of life on the planets is something that he started with in his career and he is going to go through thick or thin regardless of what it meant.

He did have some knocks. He had troubles getting an academic position on that ground. He was here for a year. He's a visiting assistant professor in this department. I tried to look around here to see what I could do to get a department of astronomy started or connected to the astronomy, and so on. He wasn't respectable enough for that purpose.

He was at Berkeley for a year. He was at Harvard for a year. You know, they were all interested and amused but they weren't really going to commit an academic discipline in that direction at that time. I was delighted when he finally got hooked up with Cornell. I thought he had real trouble getting settled anywhere.

EE: What about a man like Vishniac? Where would he fit?

JL: Well, he's the opposite. He's not -- he was not quite as old as I was, but he was already highly reputable. As a biologist he had been one of Neil's -- perhaps one of his principal protegee's. He really had a very solid reputation as a microbiologist.

In early '58 my main mission was trying to get people interested. I was trying to find other good critical scientists that could lay the groundwork for an effective effort.

Critical one, we had to get the quarantine thing settled first so that it would buy time in order to do the rest and do it right. And I just looked everywhere I could to try to find people whom I regard highly that could get interested in it. You know, I struck out nine times out of ten in terms of the contacts that I made with various people.

It was the ones who agreed -- you know, some of them more or less reluctantly out of curiosity and so on -- that eventually got hooked. Those two committees, the EASTEX and WESTEX were the first nucleus. They were the people who could be found who could take an interest in it. Those are all people of established reputations.

Then after that when it came to trying to develop the nitty gritty of the program and NDBA put out its RFPs and so on, you began to get into a different level of operational implementation. There almost nobody who had a well-working biological research program wanted to drop that and to commit themselves to -- was involved in nursemaiding the instruments through the industrial development.

In fact, you will find that Norm Horowitz did it but only by delegation to Cameron and Hobby. They did almost all the work in terms of monitoring it. By being connected with JPL he was able to have people like that affiliated with him.

In a university that's very difficult. You have professors and you have students and you've got almost nobody else. So, trying to get any organized effort that requires any sort of administrative structure is very difficult at a university. A tenure system is not compatible with that.

EE: I know.

JL: You have to understand that and how that relates to the personalities in every other issue. So, for example, Alex Rich and myself both did not want to be tied down to a particular experiment because -- you know -- that would be years of very detailed commitment to a particular facet of it and we both felt that there was a lot that we could do in terms of the general overview of overall scientific direction in management.

We both refused to be the chairman of the committee for somewhat similar reasons, but we were very interested and wanted to get it done well and so we were there as members without portfolios of that cabinet.....

Wolf had gotten hooked into that in a rather odd way. He had a rather similar role and he was much more effective as the general commentator. But almost out of curiosity he tried to see what he could put together quickly that would simply demonstrate that a fairly compact instrument that could measure some biological signal could be done. So, he slapped together the Wolf Trap.

That wasn't his designation of it either and it was in a sense trivial but someone had done it. And as soon as it was there it became the focus of a lot of attention. That was a proto- type instrument and so on. He got kind of stuck with it. He got dragged into it more and more as his contribution to defending the overall program -- that the first thing he put his finger on happened to get stuck to his fingers.

He became more and more identified with it in the course of time. He was far more useful for his very broad knowledge of bacterial taxonomy and physiology -- just a range of habitats and develop pathways to bacteria, exhibit, and then being on top of a particular instrument.

Those were not especially his skills and I'm not sure that that's an instrument he would have chosen himself if he had taken more time to deliberate on the whole set of alternatives. He is a gifted and creative person but there was just that accident and how that particular approach got connected with him.

The anomaly here is that there was no lack of ideas on things to do. There were, you know, hundreds of possibilities and any one of us could have dreamed up another hundred of them. The question was which one to choose. What criteria to be done, and then how do you then implement that. And that's what you get out of laboratory science and sort of basic science -- you get insight almost immediately.

Then you go into the engineering design issue. Who could foresee what the problems of integration will be when you finally build the spacecraft starting out with three of four basic concepts. You know, their validity and legitimacy have almost nothing to do with how the final machine is going to work in the long run.

You know, by hindsight there were a lot of things that would have been nice to have by way of cross-connected plumbing. On Viking there were some bizarre experiments that might have been possible if we had simply raised them randomly with values at every intersection.

So, even the integration that was accomplished sort of optimized things other than the scientific utility of the individual instruments.

But, it says something about what the role of scientists ought to be in this game, and I've been very puzzled as to what we were doing there on the Viking committee. Now, you have to have somebody who is responsible. Someone that understands what that instrument is supposed to do and can ride herd on the engineering design in its further development.

But I think in a certain sense you need a shop of a kind that's not likely to fit well either in the industrial framework or in the university laboratory. You need a group who is responsible to scientists but knows all the engineering. And that we never really had. You know, a little help from Johnson at Ames. Not a little it was very important, but we really didn't get to that very detailed examination of each instrument.

Not so much to criticize what's being done, that you can track through at the present way, but to think of other things that aren't being done -- to explore all of the missed opportunities that come along. And that's something the scientist is really not very well able to do.

Also, we should have had more than we knew that we were missing -- a much more detailed check out. As the proportion of testing and background operation to final implementations ought to be amplified by a factor of ten -- it ought to look absurd at the time that you are doing it because there is basically no rationale for picking out the right things to do by way of background checking.

So it's really only a huge catalog of those instances that enables you to do it. Well, here your time pressures as much as anything else make that impossible. You really do need to do it on something that is quite close to your final instrument. You don't have those until six months before and that just doesn't give -- you know -- begin to give the time to work with all the opportunities.

Altogether I guess time compressions is what hits us all in the end. You know, something looks like 10 years off, it looks like it is infinitely far away, and until some of the notions are defined it is. There's not much point in testing everything against every instrument.

But from the time that you have a definitive choice of analytical concepts to liftoff it's always too short to do what you ought to do.

Then of course there's the problem of having enough information about your target to make a reasonable selection of things to connect with it. We would make decisions like putting carbon monoxide into the gas mix on very short notice and this is not clear to me -- it is clear to me now that we had not given that as much thought as we possibly should have. We should have had somebody around to do all the chemistry of carbon monoxide before that --

EE: Uh-huh.

JL: -- was laid on. It might have been able to point out -- It might have been able to predict some of the reactions that we ran into.

We checked out empirically and Norm did a very competent job of checking out all the obvious points, but it wasn't that sharp a focus for examining -- you know, what could be the implications of every element that we're putting in now.

I guess we're into a later stage but it's --

EE: It's all right.

JL: The main place where personalities played a significant role was -- I would say within the Viking Committee itself when it came to detailed scientific criticism of what was going in.

There were a lot of things a lot of us had to say about one another's experiments. There were limited resources and --

EE: Double shrinking resources in terms of inflation.

JL: Well, that's true. And time. It's the very critical problem. There's just so much more to do than would ever be possible, and we are working in a different framework than we grew up in as scientists.

In science if you don't like what somebody else is doing, in the most courteous fashion you wait until that's published and then you do something else and you publish a rejoinder. Well, that's not the way the game is played here. There isn't the time for that, for one thing. And the journals wouldn't publish details -- reports on the behavior of some hypothetical test system that is going to be sent to another planet, and so on.

So, it had to be worked out in some other way. I mean, there were lots of conferences and lots of critical discussions. We would all tell Gil Levin what we thought ought to be in his cocktail and he had his own ideas about that.

In the long run unless one of us was prepared to do our own experimentation in order to show -- you know -- when one idea was more right than another one, you couldn't really enforce -- your own -- perceptions of it. You couldn't prove that you were right --

EE: Right.

JL: -- in contrast to someone elses judgment. So, at some point each experiment manager eventually had the options of saying "I know you guys think that way, but I think this way and as long as I'm responsible for this instrument this is the way it's going to be."

That happened quite a few times. And a number of times and ways that I would regret, and a number of times that I would say that I would have done it differently but I don't know whether I'm right or he's right.

And once in a while there was a constructive dialectic and there were changes. But it did not end up being nearly the degree of scientific cross-criticism that should have been there or that I think most people from the outside believe might have been there.

EE: Okay. Let me interrupt you. That raises sort of a complex --

JL: I have one more comment to make about -- just in talking I can hear myself articulate issues I haven't thought much about.

I've asked myself why that's the case. One reason it's the case again is the institutional structure behind it. If I were to face similar issues in the discipline of molecular genetics the way that I would respond to it is through my students. I don't personally respond to every detail of the intellectual discourse I'm involved in -- and that's where the work gets done.

We did not have this effort at this stage of its development -- it was not in that academic framework. We had very few, if any, graduate students directly involved. You know, how in the world could you get a dissertation on a project that's going to take you 8 years, and so on.

So that was one of the reasons for the limitation of intellectual resources. We did not really have the opportunity for the full mobilization of the scientific critical structure because that includes not only the greybeard but the people in the labs and particularly students and fellows. You know, the brighter young minds with a lot of energy and a lot of ideas and a lot of criticism.

In physics you will find it because it's possible to get these on measuring electron densities in different layers of the atmosphere. In the future in biology I think we will have it now that we've got -- you know -- some data. The kinds of experiments that we only think of doing now would be publish- able, would be pertinent to some concrete fact.

But I don't think a graduate student could have gotten the dissertation on a design for a hypothetical mission in Mars unless he was in engineering that I think the times will have changed in a very favorable direction.

I'm just trying to articulate some of the structural institutional background and some of the ways the things actually happened.

EE: Okay. And these are things which have bothered me and concerned me in terms of the intellectual history of space science. It seems to me that one of the great risks that anyone takes entering space science is, as you say, the long gestation period. It is almost eight to ten years, no matter what the experiment is.

And then running the risk that at the last moment it may be bumped off the flight.

JL: Uh-huh.

EE: You know, the experiment got cancelled after the 1972 issue of Icans came out saying that's one of the experiments that's going to fly.

JL: Uh-huh.

EE: And then it's dropped. For a professional scientist this can have -- especially in the academic sense -- a very serious impact on his career in terms of --

JL: Especially if he is not secure yet. If he doesn't have the tenure.

EE: Yes.

JL: Well, Wolf did, so it didn't have that kind of -- but for a younger person it would be fatal.

EE: So what you are saying about a graduate student looking into this area of doing research in it -- he would have to be relatively secure in himself if not in his position before he would dare risk something like that.

JL: Might have to be crazy. If he is not secure in his position and he does it anyhow he is committing suicide.

EE: Okay. That raises a question then of the non-academics.

JL: Uh-huh.

EE: I'm not using it in a majority sense. The non-academics, Vance Oyama --

JL: Uh-huh.

EE: -- Horowitz --

JL: No, Horowitz is a professor at Cal Tech.

EE: But he also has the security of JPL too.

JL: But his home base is Cal Tech. He had some additional resources by being at JPL. But go ahead.

EE: But, you know, for example, Gil Levin --

JL: Yes, that's right.

EE: -- first Hazelton Labs and then Biospherics.

JL: No, it's no accident that you get that kind of involvement. The contract laboratories or the government labs don't face these kinds of hazards. I mean, there is nothing -- there is no risk connected with this versus other alternative programs or connections that they would get into.

Once you have taken the risk of working for NASA in the first place this is a very good thing to do from that standpoint.

So, those were the available resources and what that left out as a result -- and there was no easy solution to that -- I had a few thoughts at the time and I have many more now -- was the critical apparatus that the academic structure does provide. These are basically the graduate students. There is nothing harder than explaining what you are doing to your own students. You know, to do it on a day-by-day basis. They think about it, they come back to you -- a lot of active agile minds. And it takes that.

The individual professor or group leader has always so many other preoccupations. He's only spending part-time in other committee meetings -- express my views on the matter and give a little thought between time. But it wasn't part of my day-to-day critical life.

I have a few people around here that I can talk with about these notions. But as far as Viking biology was concerned there was very little that we were actually doing in the lab that would have borne directly on what was going there.

The main reason for that is that there was nothing there that could bear in a student's framework. Now, as I say, things have changed. There are tangible issues now, the critical modeling of the findings of Viking are still not here and a lot of issues that have more to do with chemistry than biology at this point still need to be resolved.

So I think there are some interesting questions. And we are starting to do some things that bear at least indirectly on that.

EE: Let me go back a stage to what you were saying earlier.

JL: Okay.

EE: Did you ever get criticism from your sort of collegial community based upon the type of issues you are raising here or were they, as you say, perhaps more political in the criticism?

JL: Oh --

EE: The fact that it hadn't been tested out.

JL: No, the private criticisms that I would get were -- you know -- why you were in such a high risk enterprise. Yes, maybe it's possible -- is life there, but if so it will take 20 years to find out. Logically -- well, some say so, and so forth.

My only answer is that my own temperment is to go for big stakes. I get rather impatient with short things that are small issues. That's been true since the start of my scientific career and it paid off. There is something -- reinforcement has something to do with that.

As a medical student I had gotten interested in why does everybody believe that bacteria doesn't have any sexual mechanism and in fact was able to show that they do. So, you know, that's the reinforcement I was talking about.

EE: Well, I was thinking in terms of people like Commoner and Abelson. Their criticism wasn't based upon a critical look at experiments --

JL: No, I --

EE: -- it was a broader.

JL: Oh, yes. No, I'm talking about colleagues and people that might have some tinge of concern as to why the country is spending so much money on space. That's been talked about sufficiently in the frame of Apollo and this is almost a little side on that. But it's something that we need to go into too. I will defer that at the moment.

But, once we got past that the issues were then the scientific credibility of the entire program and there was simply the very high risk and do you really have the resources to be able to deal with it, and does it need someone of my own talents and interests to be able to do it. I have to say yes. If you knew what I knew you would understand it needs a lot of us.

But I couldn't get a Kornberg or a Falberg (phonetic) or David Horowitz -- really gifted colleagues and biochemistry to take very much interest in this. They just got more immediate reinforcements from stuff they were doing -- if something took a year they would regard it as being a horrendously long time.

EE: Okay. Now, there seems to be something implicit in what you have been saying which has intrigued me. And that is the search for life also assumes a negative result as well as a positive result. You may find or you may not find life.

JL: Yes.

EE: And much of what has advanced scientific thought has been negative --

JL: That's true.

EE: -- in it's result.

JL: That's certainly true and I'm -- you are quite perceptive in bringing that out.

I was able to believe that getting conclusive information of a negative kind in this direction would be a very important contribution. Most scientists don't have either that historic or philosophical cast to -- you know, they understand it when you tell them but it isn't what's going to drive them. The notion of spending a lot of time and effort for a negative result would be bizarre.

Now, in many instances that would be totally justified but this is one of such transcendent importance that one might well believe -- look at it differently. And I looked at it differently. But it was hard to move them to take it that seriously.

I would have given a great deal -- you know -- to be able to prove conclusively. But that is no longer an issue. That may well happen and there may 10 or 15 years from now have given a great deal to exactly that -- and I would say as a scientific effort is completely justifiable for that outcome.

I have never been sure that we were pursuing -- Well, I thought out programs of planetary exploration are too intense. That may surprise you, but --

EE: No. I had to hold that same personal view.

JL: I think we would be much better off if we had long range commitment to a slower program.

EE: But the budgetary reality --

JL: Politically, economically, and so on.

EE: And you have to keep the next program coming along.

JL: I understand that very well.

EE: So, this is one of things that has puzzled me about -- you know, that the Mariners and Viking with Voyager originally in between then, the pace was such that you never really understand from the preceding program --

JL: Exactly.

EE: -- it's impact.

JL: That's been my most deep-seated criticism of how we are going about it and it guarantees false starts and errors. I fought bitterly against Viking getting started so soon. Head over heels over Mariner. We didn't get anywhere.

The Voyager was to have come 10 years later. I mean, I wasn't the least bit worried about how long it would take to prepare it, I just wanted to have a good scientific base and a good technical base to do it right when that came along.

Well, that can be argued. From a somewhat longer perspective you might say "Yeah, but you are taking these individual issues too seriously." They had to be touted as being the end or the answer or they could never have gotten off the ground. But they are really nothing but engineering trials -- a feasibility demonstration. Your real program is a long stretched out program and it's just going to cost 20 times more than you are willing to admit and no other way would really have worked.

I can't disallow that perspective but it isn't what the missions are touted to be.

EE: I know.

JL: Well, I --

EE: And that's one of things I want to try to record.

JL: I wondered when you said "Why Mars?" why you did in fact say "Why space?" "Why the planets" as sort of an intermediary question. And although those issues have been explored in some detail elsewhere, and maybe they can just be mentioned by reference, there were certainly things I had to talk fast about all the time to get in when talking to my own colleagues.

I guess there is no point in spending a lot of time on the question. I will just give you -- Well, I thought that space -- and I would have had rather than Apollo but something like

Apollo -- was a rather cheap way of demonstrating mutual deterrence. And I wonder whether just keeping missiles in place without dramatic demonstrations of their technical sophistication would have brought home the reality of mutual assured destruction in the way that indirectly they did and that did in a very peaceful way. In a way -- you know -- it sort of points to the stars.

I guess my own contrafactual prediction is that if we had not had space we would have had other military demonstrations of those weapons. Well, that's just my own position --

EE: I argue that point. In the first history we did for NASA which was the history of the Apollo Soyus test project and in the background sort of argued that it really provided sort of that more of equivalent of war which James talked about long before Jimmy Carter picked it up --

JL: That's a little bit different.

EE: But in the sense that -- you know -- that that competition did give sort of technological credence to --

JL: Okay. That aspect of it. I don't take a position on the Jamesian thing to fight.

EE: Well -- yes.

JL: I don't think you need to and so on, but it may be very true. But there are still people who don't understand MAD and there would be a lot more of them if there weren't the pyrotechnic displays of this kind.

Now, any other kinds of demonstrations would have been very dangerous because this was one where the message was second order, not an instant threat although Sputnik --

EE: Certainly was.

JL: -- certainly was. And that message was very, very understood. But demonstrations involving actual tests -- actually showing a missile under your nose and -- you know -- planning at your feet and so forth had some of the more ripples in terms of the level of anxiety that they provoked and the kinds of reactions that they would generate it would have been very dangerous.

This I guess was probably an optimum way in which to demonstrate this concept to the people from both sides. I mean, it's almost as if Kennedy and Khrushchev had gotten together. We know the world has changed and that war is no longer tenable on a thermonuclear basis but our peoples don't understand it yet and we have to organize something so that they can. Now, that's not the way it happened.

EE: Let me try an argument that I have developed in this first chapter. I argued that for a number of people in the scientific community -- definitely those in the technological community -- I'm thinking specifically of the Von Braun's and the Ehricke's and people like this. Mars was a much earlier and much more attractive target intellectually than the moon.

JL: Yes.

EE: And I argued that Kennedy interposed the moon in there and that Von Braun wanting to get into space took whatever -- you know -- was offered --

JL: Yes.

EE: -- in terms of big budgets.

JL: Uh-huh.

EE: But that in the back of his mind always was Mars.

JL: Uh-huh.

EE: And so you then in '62, '63 get the Empire Studies in terms of going out --

JL: Uh -huh

EE: -- to the planets Mars and Venus. You get this continuously in the background --

JL: I don't think he really bought unmanned explorations.

EE: No, he never did. Definitely.

JL: Yes.

EE: And that Von Braun was always concerned with showing that superiority over the Soviets. In early '58 - '59 correspondence about the planets within NASA was very heavily laced with the space race.

JL: Uh-huh.

EE: It's very competitive -- you know -- we've got to get there before they send probes out.

JL: Well, that's the Sputnik syndrome and --

EE: Right.

JL: -- I suppose a lot of that is addressed to Congress. And they responded to the various --

EE: But that was I believe at a gut level -- inside the Agency as well.

JL: Yes.

EE: And it was what drove people and it was only after Kennedy gave the Apollo moon goal focus --

JL: Yes.

EE: -- that the planets then began to take a back seat.

JL: Well, the articulation of superiority is a slightly different wrinkle on the functions described here. Maybe that's the way it has to fall out.

Of course, most contemporary commentators on the strategic problem don't understand what superiority means. If you have mutuality in destruction neither side can be superior.

Whether Kennedy ever understood that I don't know. He may have felt he was starting from a clear inferiority which was wrong.

EE: Definitely.

JL: But his misperception about that may have led to the Cuban missile crisis indirectly.

I just wanted to make that distinction. In the demonstration of mutuality both sides have -- really do have the hardware that's talked about. Those are not speculations. They are not hypotheses. Somebody can push a button and land a payload just about where he wants. And it can be your backyard if he wants to. And he will have enough of them left after your first strike that you had better not do that first strike.

That's different than talking about superiority. If you were to substitute the phrase "not untenable inferiority" for superiority you would come close to what I'm driving at.

EE: Uh-huh.

JL: Now, again, that's a question that is even less well understood, and we do have the misnotion that there can be superiority.

EE: Because that's impressive -- inferiority.

JL: Well, the untenable aspect of it.

EE: Uh-huh.

JL: Well, anyhow, whatever the manifest motives of political leaders -- and they had a lot of other problems -- Kenneday had to show that he was a forceful leader and, et cetera, et cetera. It's part of the other game as well, but he had to deal with that domestically.

I think regardless of the manifest motives the outcome is exactly what I have described. That there is a general acceptance of MAD and that may very well not might have happened. People might have talked much more about fighting with nuclear weapons and being able to beat the other guy and not really believe that those Russians have all that it takes -- and vice versa if it hadn't been for the space program.

So I view that as historically the most important justification. Within that framework then -- you know -- once you start a bureaucracy going, once you have a lot of options in terms of the details. There are a mix of things that you can do. You write a charter for the agency. You talk about science -- it does not directly mention the military -- I guess geopolitical is a better statement -- political utilities of it.

You also have the undeniable suspicion that "Well, there are military uses of the technology." Maybe not of this specific spacecraft that they are using here but that gives you the know-how to be able to stay abreast of the alternatives.

The directed energy nonsense that we are hearing nowadays is a spillover of that kind of development and I'm sure many Congressmen believed that they were voting for the military budget and they were voting for NASA -- having something like that in mind. And they were right.

That still could come along as -- not next year -- as something you think about in the competition. There are other applications of space vehicles that are important. Maybe Congressmen didn't know that they were voting indirectly for supporting this technology. You can argue that it would have been more efficient to put it directly into the military program per se and so on, but those motives can't be disregarded.

Then "Why the planets?" Well, I guess the choice is a planet versus the moon. They are the only things around at first order to think about. And, of course, the major decision was the moon.

That was contested. A lot of people wrote to Kennedy almost exactly what you said about Mars being a better target. He made a judgment that you had to have a man there to have the splash -- it was necessary. Whether that is right or wrong I don't know.

I think there would have been the media interest manufactured with equal intensity of a man on the moon -- Well, those are things that can't be changed now.

Within the planetary regimes Mars had simply the longest history of interest. It had not yet been refuted. There was some particular excitement that turned out to be a fallacy on Sinton's discovery of the methane band, or as he thought Mars. You know that story. But that was very much alive during the time that we were starting to do some of the planning.

We didn't know how hot Venus was on the surface at that time although there was some speculation about that direction and that would have been the next alternative. But, you couldn't see anything under it or issue a communication, and so forth, at that time because that was -- you know -- a little less tenable. But it was a closer contender then than it was as soon as there was the first -- of any kind.

Jupiter is still an enigmatic contender. The main reason not to think more about it is a technical one rather than a scientific one at this stage. How the hell do you get some- where that you shouldn't do something about. Subsequently there have been more detailed models of the Jupiter surface and the convection problem seems to be the main one.

That you have a planet with a rapidly increasing density of atmosphere, possibly not a phase boundary -- I don't know what the last word on that is but it may go directly into a liquid phase out of boundary on this particular pressure and temperature conditions. And then knowing more now than we did before, although there is plenty of hints of it about interior -- positive heat loss from the planet -- you know -- convections that must generate in the fluid exterior and therefore you have a region where any organic material has to go to an environment with tremendous fluctuation in temperature which is a hard one to imagine being able to sustain.

If there is life there we will have a lot of technical and scientific issues if we are going to grapple with it.

So, that's where Jupiter seems to be out. I, myself, give up beyond that. There are people who talk about Titan and so on, but I have become exhausted at that point of stretching my imagination.

Well, I think that is why Mars is what is left on the issue. I think that has been gone fairly thoroughly into in the 1965 summer study.

EE: Right. I've used that extensively.

JL: Yes. What happened in the '50s -- the technological capability foremost and bringing some materiality to what had been a Buck Rogers oriented kind of thing. I would give a little more emphasis to the development of evolutionary theory and particularly the line of work that was sparked by Oparin and Miller and Urey.

That was tremendously important to orient people to the cosmological framework of life.

EE: I have the first two chapters with me and I would like to leave them with you.

JL: Okay.

EE: I have worked on that a little bit but I feel somewhat out of my element. My background is in the history of technology rather than --

JL: Okay.

EE: -- strictly the history of science. I have had work in both, so I would like you to read --

JL: Okay.

EE: -- especially what I have said about it and have you make sure I'm right -- on the right track.

JL: Okay. Besides the -- you have to remember Haldane's name in that to make sure --

EE: Right.

JL: Besides the biochemical end of that the notion of a complete theory of evolution was beginning to crystallize. You have the neomendelism, Dobzhansky and Sur (phonetic) Wright and Fisher, and working out a mathematical theory and trying to -- at least having as a specified research program a comprehensive evolutionary framework.

And while that may seem a little disparate from the biochemical foundations it said biology is now addressing less particular questions than it did before. That it is interested in the overall framework in which exists, what are living organisms, how are they distinguished from the nonliving. There must have been a spontaneous generation some time -- it focused attention on the terrestriality of life and that is why one could use a name like exobiology. That was the focus of the exo versus eso -- the double frame from biology might be larger than what we have seen here.

And while you will find that in earlier writers -- in fact, a very good place to see that perspective is in the Wells, Huxley and Wells -- the popular thing on Science of Life, I think it was called. It is subhighschool level but encyclopedic treatment of biology. It was biology in the perspective.

And then -- you know -- a few people are beginning to write explicitly about whether there is life on the planets. Lowell sort of -- Jones, the Astronomer Royal of England wrote a little book on this in the early '40s, I think, which was probably the most respectable treatment of that issue. In fact, I was going over some historical notes on another subject and was sort of startled that Kurt Stern, a geneticist at Berkeley, had given a seminar at Columbia in 1944 which was a book review of Jones' book. That gives you an idea of the kind of interest that there would be --

EE: I like that.

JL: It may not be irrelevant that I attended it. I was an undergraduate at Columbia at the time.

Okay, I think that is a quick runthrough on the overall background. I don't know how much of the story I know in terms of the different facets. I will be very interested to read your history on the early stages of it. I came into it at a particular time from a particular angle and it was obvious that there was somethings already moving, but I can obviously only speak to the things I have my own knowledge about.

First of all -- let's see -- it will help you to understand what and where I was at. I was born in 1925. In 1957 I was 32 years old. I was a professor at the University of Wisconsin. I had a pretty well established career by then on my work on bacterial genetics.

I had had curiosity; these interests about astronomy and the cosmological background of life -- and working on the genetics of bacteria relates to that very closely. To be thinking about bacteria of part of the overall framework of living organisms was at that time a rather curious idea. And in fact they were segregated from other organisms by being asexual. That's part of their name.

So, having discovered that they do what every other organism does sort of brought them into the fold and it ...... [telephone interruption] the discussing of the first two chapters primarily.

Okay. The cart before the horse business is the next thing on my notes here. Yes, I was surprised myself how quickly JPL got organized on tangible missions. I thought we would have 10 years of effort to get some sort of policy that biology was interesting, that the planets were interesting, that we had a reasonable quarantine doctrine, and then almost before we knew it there were these ideas from JPL and Mariner B and so on.

I have forgotten all the details of them. I have no trouble recalling that there was such mission. I have them in my notes in the boxes over there. And we begin to turn our attention -- This is now the EASTEX/WESTEX group and then all the personal connections that were established thereafter. There must have been a dozen different committees mostly out of JPL that I was invited to attend for different sort of planning exercises during that time.

And I got very nervous about the one shot thing and the implications of a negative result and that's where I countered with Voyager. That unlike physical experiments where there was something you knew you wanted to measure that we needed systems that gave you opportunities for sequential decision; that you needed a variety of reagents; that you didn't Step B was going until you had the answer to A; and that there was no way you could plot out all the decisions being advanced.

I guess I still feel that is true. If I calculate what I do in the lab over a period of a week compared to what I think most physicians think we do there would be a sharp difference along exactly those parameters.

So, the idea of artificial intelligence in Voyager was mostly a response to the data communications problems. So, "Could you close the loop closer to Mars? Did you have to send every bit of information back to earth before you decided what your next step was going to be?" And that was more of a probe to "Let's see what we can do?" rather than a premise for the mission. And maybe from what you say it would have been better if it had never been brought up at all if that is what scared people away from it.

Knowing what I know now about artificial intelligence since I got into that game out of this impetus --

EE: Uh-huh.

JL: -- I would agree that it would have been a very risky thing to commit very much of a mission of that magnitude to more than the most elementary pattern of recognition techniques although some things might have been done.

I think I also at that time had more faith in the reliability of computer hardware of any complexity than was justified, and so forth. And people who did know better were right in being very skeptical about the tangibility of that particular approach.

But that was not, to my recollection, one of the major premises of Voyager. It would have been extra gravy to the extent that one could do scene analysis on board to determine where you wanted to go. People are talking about doing that now for the Rover -- for Viking III, and I think it is slightly - -

(end of side 1)

As you well know, the early Mariner proposals never panned out. I'm not -- I can't remember if I really understood what the basic reasons were. Whether they were really insuperable technological obstacles, or that it really was technically premature or not. I just don't know if I'm in position to agree or disagree with those statements, and I would have to look in my own records and see if I had any other attitudes about it at the time.

EE: Basically the big problem was that they just didn't have the launch vehicles at the time to get them to Mars --

JL: Uh-huh.

EE: -- with a large enough package that you could put a landed capsule on the surface.

JL: Well, whether that was due to optimism and existable boosters would do, or to false expectations about the rate of booster development, are some questions I would raise.

But I guess it was in the framework that we were going to go in for a Russian style booster to support Apollo that the idea of Voyager came up. In fact, at that time one could size the Mars mission in terms of what is the most you can do with the available propulsion capability. And the Apollo program seemed to offer that in abundance.

So, I don't think Voyager would have required even as much as Apollo did. Of course, in retrospect it is a damn shame. I understand there are five unused Saturn V's sitting at Cape Canaveral which will be scrapped. What a difference that could have made.

However, there is an interesting point of technological policy that you may have heard about before that I kept running into all the time. And that was that we priced our spacecraft by the milligram and the bigger the spacecraft the more it is going to cost and that is an iron law. And it is not going to save us anything to have more weight, more capability because you scientists are going to think of all kinds of things you want to cram on and we will end up simply just as super sophisticated and expensive in gold plated and miniaturized as ever -- therefore we have to think small in terms of propulsion.

I thought that was mad. I thought it really was possible to impose a mandate that if you can save on your reliability and save in your development costs by using your weight for backup, for more power, for more margin for contingencies that could really be made to work then it wouldn't have to be more expensive.

But that was an iron law at JPL. They had a price in dollars per milligram and you had to size -- you start out with your budget, then you size your spacecraft and then you size your booster to do it and it ends up being a modest -- you know -- medium size booster and not a mega one --

EE: Uh-huh.

JL: -- as a result. Whether anyone was ever able to get off the ground -- ways of using Saturn V capability for rugged and less expensive spacecraft I don't know. I certainly raised that question form time-to-time but I never made a federal case of it.

And that is something I think might be worth looking into as sort of a guiding principle of program definition.

I don't think I ever understood exactly why Voyager was shot down -- I'll have to read what you say about it -- probably because it would take too long. The impression I had was that it was urgent to get a mission once and for all and get it done with -- that Karth was eager to get something going and that Voyager would take too long, it was too ambitious and that something small had to be done to get it done sooner. And that went against the grain of my perception that we ought to have Mariner cleared up before we even thought about the design -- you know -- in any detail for the next mission, but that was overruled.

EE: Very important is the whole history of proposals for manned missions to Mars. And that competition within the agency always led Karth and Anderson both to believe that the unmanned mission to Mars were really precursors to manned missions to Mars. And in the summer of '67 why --

JL: Over my dead body, but --

EE: But that was their perception. In the summer of '67 then the manned spacecraft center went out with an RFP for a study of a manned mission to Mars.

This was only one of a half dozen or so that had been done previously. But for some reason Karth put two and two together and came up with the fact that Voyager was the precursor to a manned mission to Mars.

And they had been talking about successively more sophisticated Voyager missions ending up with a Rover -- Prospector Rover type of craft.

JL: Yes.

EE: And so it seemed logical. And as a consequence that along with the long-term payoff, as you suggest, led Karth and Anderson and others to reevaluate. And in August of '67 they just decided that the nation couldn't afford Voyager or $100 billion program to Mars for men to go so Voyager got scrapped.

JL: Uh-huh.

EE: And I think it was just a culmination of a long-term--

JL: Yes.

EE: -- dissatisfaction with Voyager and the fact that every time you mentioned Voyager JPL came in with 14 different permutations of how it would be done or how it could be done.

JL: Well, maybe it is right that there should have been a Viking science mission after Mariner and before a large flexible spacecraft. In fact, right now -- if that's what Viking had been and Voyager was still a potentiality I think I would say "No, the time is not right yet."

There are some fairly elementary things about Mars that much cheaper missions can help to establish. The mobility aspect in particular, you need to know where to go. The habitats we have sampled so far are very unpromising. Maybe none exist that we would really want to expend a Voyager on. And, let's invest another 10 or 15 percent of your final Voyager option on the other intermediary missions.

In that sense the decision was correct. What was wrong was to rush it so fast and even that level of expenditure even it was a great deal. Close enough to a billion dollars if it had been counted properly -- not to want to argue about it.

I think it would have been a better mission from a scientific standpoint if it had had another three or four years of digestion of the Mariner outpouring and all the other things we have talked about before.

EE: Okay. That raises a couple of sort of related questions.

The first one -- the question of sampling. Mariner B was going to send a small capsule with one experiment and land it on Mars. That is a very small sample. Viking sending basically three biological experiments to two locations on Mars they are still a relatively soft sample.

JL: I asked you what you thought the amplification factor was. You know, I see like 10 to one, but not a billion to one.

EE: And the question is -- you know -- even if you had gotten reasonably encouraging results without a sort of chemical confusion (interruption) even without the chemical confusion how many more missions would you fly?

JL: Well, that's the problem. Well, you need a theory of the planet before you can answer that question. That is something that Mariner was just beginning to give us.

There is one other premise though -- and that is a good reason not to commit something that is such a big gulp as a Voyager before you understand that question a little better.

The counterstatement to that was that the winds and the dust distribution on Mars cancelled out that problem. And here Carl Sagan and I had long arguments on it. Originally I bought it and my first writing is about strategies for planetary examination that stressed microorganisms will be there no matter what else before, during, and after the evolution of larger forms. They are the primitive organisms, they are the parasites, and they are saprophytes, the scavengers who clean up. The surest thing that you can expect in any planet if there is any habitation are microbes.

And if there are microbes and if there is dust there are going to be scattered out obviously on the earth as a prototype -- as I began to think a little more deeply about it I began to worry about the UV flux hitting the dust. And my own thinking went more towards that on a very dry planet you get the desert phenomenon and you don't have very small plants in the desert. You've got reasonable size shrubs rather widely spaced. But for somewhat different reasons you would need microbes in order to be able to insulate themselves against the UV and in order to conserve the water.

The surface volume ratio is fighting you -- how impermeable a membrane you had, and that therefore in that particular regime maybe airborne dust was not likely to be -- was not as sure a bet as our thinking up to the point had been.

But, you know, that is a very speculative kind of issue. While I think it is very plausible reasoning and until one has at least one sample one could argue that anywhere was as good as anywhere else as long as you got the dust. And that dominated a lot of the thinking about issues of site selection; did it really matter where you went, and so on.

EE: All right.

JL: And Carl stuck to that much longer than I did. In fact, he used that as a argument against gambling on a higher latitude -- that you got the dust no matter where you are at so why risk a lot by going someplace that has a different ground -- that might have a different terrain level.

EE: Okay. Now, sort of related to that -- people seemed to have, especially earlier, have been quick to leap to judgments about whether or not there is life. I am thinking particularly after Mariner IV.

JL: Yes.

EE: Leighton and Murray --

JL: Yeah.

EE: -- in several places --

JL: Like Mars is dead.

EE: "Mars is dead." Right. It is the Moon like ergo -- Mars is dead.

JL: Uh-huh.

EE: What was your reaction to that?

JL: There was a paper that Carl and I wrote on microenvironments on Mars. I'll get that out for you. There were arguments on the equilibrium systems and that while the other data were more discouraging than I would have preferred if I were trying to create a habitat for life I didn't think that they ruled it out, and I thought we had already written a response to it.

Those arguments had appeared before. There had been measurements of the atmospheric pressure in Mars and estimates of the degree of availability before the actual mission. In a way that colored Bruce's interpretation. And I was not thinking that -- you know -- you really have picked an extremely atypical segment of Mars, as in fact turned out to be the case, by what you happen to picture more than -- rather, that even if that were the typical appearances there was still room for microhabitats and we had better keep looking. There were the polar caps. That was positively exciting, it was not conclusive that there was CO2 rather than water, although Bruce at that time leaned to the CO2.

So, it didn't seem to me that it altered that much. There was one other incident which I -- I have to get the right Viking pictures to get cleared up. Mariner 9 didn't quite -- there was one spot that occupied about three pixels that looked like it might be a cloud. That's on Mariner 4. It's a place where you have very high albedo right next to very low so there is the shadow of the cloud and, boy, that would have made a tremendous difference --

EE: Uh-huh.

JL: -- in those appreciations. And I just worked very hard trying to make some sense out of it, get out more data, and was convinced that it was not just the data artifact it was too much correlation by the local areas but it was something every had to shrug their shoulders at. I mean, you can't really make much of a case out of that. The resolution was pretty low.

I looked for that spot again on the Mariner 9 pictures -- had some trouble matching the locations, the geodesy of Mariner 4 is not that wonderful and was never quite sure that I had -- you know -- was able to identify the same place. There was a different resolution, different angles of view, and I had made a note but had never followed through to go hunting through the Viking orbiter pictures to see whether it is credible that there is not only a cloud but it is a permanent one. I would have to go through that again.

That would be only in historical interest because now --

EE: Right.

JL: -- we do know that there were clouds.

EE: Right.

JL: But the fact is that Murray was not about to spend a lot of effort looking for positive evidence that there were clouds there and I stood pretty much alone and very much out of my depth trying to get contrary evidence.

EE: It seemed to me, though, that if you had just the telemetry and not the pictures for Mariner 4 that the prognosis might not have been so bleak. But the craters that showed up even at the low resolution --

JL: Oh, yes. Well, you see, that was an argument not only about contemporary Mars but it was an argument about the history of Mars. And there is still a big paradox there.

Whenever I try to explain away the river features and the presence of those craters I get myself tongue-tied and I don't think we understand that paradox yet either because that argument is still a pretty --

EE: Well, I've heard Hal Masursky step around that one on several occasions. You know, it shows how little we know.

JL: Yes. But I just wanted to stress that what the pictures added besides evidence of the contemporary aridity consistent with the low atmospheric pressures -- the lower range of the ones that were reasonable to expect.

EE: Right.

JL: That says that it has been that way for hundreds of millions of years. That is even more appalling because he extrapolated from that to the origin of the planet that there has never been any significant liquid water and certainly not recently enough that there would be much point in looking for fossils and so on.

EE; Okay. Now, let me raise a philosophical question.

JL: Uh-huh.

EE: And it has to do with doing science. The scientist is only as good as his data. So, Murray made some assumptions that -- I'm not picking on him necessarily.

JL: Yes.

EE: He made some assumptions based upon his data. Later pictures from 6 and 7 and then later yet from 9 -- you know, each time you get new data you sort of have to continue to restructure your thoughts --

JL: Uh-huh.

EE: -- and that again, I guess, brings me back to the same question about the turnaround time necessary to assimilate the implications of the new data.

JL: Yes. Well, a person in the middle of a specialty has trouble sustaining a suspension of judgement. It is not his job to do that. It is his job to work out what you can from what you see right then.

But he ought to know that that phenomenon is there. I am a little more aside from that central issue of the aerological interpretation, and that was something that I would repeat. I said "Look, you guys are pretty sure of it and that means the odds are .9 that you are right." And they would say "You don't really mean it is that low". And I said "Well, you know, look at history, how often did you change your mind before" and so on. And they would -- you know -- sort of grudgingly come around to agree that they were adopting all or none levels of inference on cases where there was merely a high likelihood.

And over a longer perspective you have to give some allowance for new data that you couldn't anticipate and that would be quite bizarre but would revolutionize the interpretation of things that seemed -- you know -- completely solid and airtight. And that was a point of view that I felt somebody had to try to sustain, that maybe our whole perspective would turn out to be completely wrong. I'm sure we were about Sinton and how sure some people were about green patches on Mars, and all that kind of stuff.

That the things that we felt the most sure about left significant likelihoods of alternative views. And that in relation to the importance of the problem that the last ditch alternatives really should not be thrown out as zero. So, I was hanging on to that thin thread for a long time.

EE: I'm thinking also in terms of if you had Viking orbiter resolution photographs the first time how much different the other way on the pendulum decisions might have --

JL: You mean the resolutions or the sites that were pictured because the swath that occurred?

EE: Okay. Perhaps. If you had just had a rough view of the great valley?

JL: One element like that would have said "This is not like the moon like at all. This is a planet that has an enormous amount of crustal activity. So, I think that would have -- even without seeing the rivers or anything else -- would have cast a lot of doubt on the crater thing.

In fact, even -- Well, the volcanoes are interesting too from a biological standpoint but only if they have been active -- you know -- within recent history. That is still a somewhat open question as far as Mars is concerned. But the overall level of activity on Mars is so much greater than on the Moon that I think it would have nurtured some further hopes that you are talking about things that might have stretched through to the contemporary time.

Volcanoes can create fossils -- and the other habitats can be traced.

EE: But at the time of Mariner 4 that was sort of the nadir of --

JL: Yes, it was. That's the exactly the word.

EE: -- of exobiology.

JL: Well, in the sense that many people -- you know -- just accepted it as being the final conclusion and a few diehards talked about microenvironments. They needed some more information.

Actually I believe that that was touched on in the summer study. I believe there was a postcript that we couldn't ignore that data.... Postscript of October '65. Our knowledge has been raised to an entirely new level by the success of the Mariner 4 mission. That sounds like a winner. Well, there are other things we thought we had to back off on that. Our conclusion that the biological experiments of Mars will be a rewarding venture does not depend on the hypothesis of Martian life.

You know, even getting a conclusive negative answer is interesting. We have to think about planetary evolution. The chemical evolution of the planets and so on.

We certainly had to remind ourselves and others that those were the other legs of any argument to continue the exploration. But you can see in this apologia just exactly how we had to grapple with it. We didn't try to challenge them as facts which maybe we should have attempted. I guess we didn't believe it -- you know -- we accepted the factual assertions that Murray offered.

The inferences in the Mariner pictures that Mars is currently very dry only reinforces what we already knew. The only novel feature that might apply is that an extensive drought has prevailed for a very long time. That is historical.

EE: Not millions of years, just "a very long time."

JL: Well, no, we meant --

EE: Right. Hundreds of millions.

JL: "Even here caution is needed. An initial suggestion that present surface features are about two to five billion years old has already been challenged by other estimates of more an order of magnitude smaller...." and so on. The general more important point is that an analysis of these new and complex -- it will be many years before the full meaning becomes clear.

These are desperate people, including myself. But they do not prove that Mars' life is now and do not prove that the early history is incompatible to life's origin.

Well, I think those words speak pretty clearly on where we are at that point.

EE: Let me ask you a question. We are skipping back and forth in time.

JL: Well, let me just add one point. I was down at JPL when the first feature that looked like a river started showing up on the screens and Bruce was standing right next to me. I said "Bruce, that's the river Murray."

EE: I like that. In '62 -- the Iowa summer study -- before that the Ketty Committee had, in '58 -- or was it in '59?

JL: Yes. Uh-huh.

EE: They had said -- you know -- in the biological -- search for biology in Mars is a very important enterprise.

JL: Uh-huh.

EE: And had really given it its first strong official endorsement.

JL: Uh-huh.

EE: In '62 that was reinforced by the summer study. But I get the feeling that by that time there is already considerable momentum favoring that. Was that sort of a way of -- a means of endorsing an already established --

JL: Yes, I think so. One ought to look pretty cynically at the whole Space Science Board set up and the relationship between the academy and NASA. And while I --

EE: I do.

JL: -- it is generally a good cause, those things are stacked from beginning to end. But it is part of the structure. Who is going to work on those things if they are not interested and enthusiastic? But endorsement is just the right word. They are not critical de novo estimates. They are not unbiased approaches. They are reputable and authoritative and responsible endorsements.

They don't say things that are patently false, they don't say things that are patently ridiculous. They are by and large views of proponents but they achieve two things. One is some scientific respectability by the analysis that it is gone through and some improvement and refinement given the major premises. So, they become used as political arguments in favor of the issues.

Two, and here is the other side of the coin -- you might ask "Well, what do they do in the public interest from the standpoint of some more unbiased view of the matter?" They do make a contribution there because they furnish explicit critical targets. They are public communications. And they are well defined. You don't have a slippery --

EE: Uh-huh.

JL: -- target then and they are things that can be shot down if they -- if that can be done. So, it is a constructive process from that standpoint.

But anyone who believes that these were de novo efforts at primary definition -- is, of course, mistaken. But they do bring things out into the public domain and then whatever further criticisms can be elicited there are fairly soft.

EE: Now, what was the purpose and function of the '65 summer study?

JL: Those two things. I think on a somewhat broader scale was to discover what scientific interest and support there was. To gather what there was and put it together in one place. To get a large degree of internal criticism among the insiders at that level -- sort of a refinement process. To put something out in public that one had some stamp of approval by having National Academy of Sciences written on it and so forth on the one hand. But on the other it then tests the public reaction -- Public meaning scientific public as well.

EE: Uh-huh.

JL: Orr Reynolds who is very central in these developments. Have you had a chance to talk to him at all?

EE: Have not yet.

JL: Okay. I have not seen him in years. I hope he is well. He was on the American Physiological Society the last time I saw him. He felt that he could get nowhere further without having this degree of imprimatur.

You know, that is a kind of peer review. It is probably as good as one can get under those circumstances.

But the main element I want to stress is after the issue kinds of reactions. If it were patently ridiculous we would have heard more about it. And things that are patently ridiculous could be anticipated if they were responsible people putting it together. But it is put together by advocates. [interruption].

A point that needs to be emphasized is that the problem of quarantine was raised first. The conservative aspects of planning before the constructive aspects are laid on. That was perceptions of timing about what would be possible when and then became overtaken when JPL did get interested, did make proposals.

I don't know how deeply we really believe them but we went along and tried to think through what possible models of instruments might be starting in about 1960 whenever a number of people starting to work quite actively to try to do things more constructively. I had just moved here from Wisconsin. I started a committee there among the microbiologists. Some of the people are still in the game. Hal Halvorson is one of the persons I remember -- and Alan Mar (phonetic) -- very distinctly and a couple of people from engineering.

Around that time I met Carl Sagan and discovered what a treasurehouse he was of insight on the astronomical side. He was a graduate student living in Madison. His then wife Lynn was a biology major and he was commuting down to Lake Geneva to the observatory. He worked with Kuiper.

But that didn't really materialize into anything very concrete, and I then wondered and I guess I still do exactly what the role of an academic scientist ought to be in this arena. The notion of actually building instruments in my own lab was something I didn't feel was one where I would be working at my highest competence. I had a lot of scientific ideas and criticism to work out first.

But a lot of those notions were kicked around with WESTEX and so on. I am trying to guess timing just was right. I moved -- when was WESTEX organized? Okay. I've got some of the documents on this -- I will get them out for your reading.

My operational interest began on November 6th, 1957 -- If I have the date right. It's two days after Sputnik was launched.

EE: That would be two days after Sputnik II was launched.

JL: Yes. Well, I believe that was the one we saw then.

EE: October 4th was --

JL: Excuse me. It's October 6th.

EE: Okay.

JL: Yes, October is the November revolution.

EE: Right.

JL: I happened to be in Melbourne at that time on a Fulbright visit -- the University of Melbourne. And the southern hemisphere had a chance to see Sputnik long before the northern hemisphere did. So, you know, fresh on the headlines was the actual event. You could see the thing in the sky. That may have made the thing a little bit more tangible. I certainly noticed it with interest but not much more than that.

Not anything very tangible in terms of what to do about was very much in my mind. A month later I was returning home and went back via India and spent a week at Calcutta visiting Haldane who has had long-life interest in space -- origin of life. I had met him about 10 years earlier and we have had very lively correspondence along the genetic issues.

He had just moved to India from England where he described himself as a political exile and escapee from the American occupation of England. He was, I imagine, as much a thorn in the communist party's side as in everybody elses, but he was describing himself as an active Marxist and so on.

We arrived in Calcutta on the day of -- I think it was November 2nd -- an eclipse and I believe it was also the anniversary of the October revolution. It was a major religious festival

-- you see all this in the streets as we are driving to where he was living.

There was the lunar eclipse coming out that night and we got to talking about Sputnik and so on and he made some rather general remark that he assumed this would put the American myth of technological superiority in its proper place and show that the Marxist doctrine is capable of comparable achievements and in fact he wouldn't be the least bit surprised if they use the occasion tonight to commemorate the revolution by planting a red star on the moon that would be visible from earth. We started scribbling some calculations on what kind of event would be in fact be physically reasonable in that regard. We concluded that it would be a major thermonuclear device.

EE: Right.

JL: If that maybe it could be up in a way to convert more of this energy into light. Maybe it would be just barely possible.

Well, that was all fairly jocular, and so on. But we both in the course of this discussion began to look at each other and realized that here politics was overtaking science, and here we were on the threshold of one of the major leaps of mankind into a new domain and how is it going to be used. It was going to be used as a tawdry spectacle.

I think we would both have agreed with that characterization and the fact that it didn't happen, I think somewhat to his relief. But that is where the jarring conception of what the space program was likely to be about given the competition. And here Haldane and I were in a way representatives of that competition --

EE: Uh-huh.

JL: -- although quite friendly scientifically and certainly in very close exchange in many, many ways, there was a very sharp divergent political views. And we both had the sense that unless really there was a very loud outcry from scientists about the manner of exploitation of their domain it would be nothing but planning red stars on the moon on both sides.

And that was specifically what motivated me to want to worry about the explicit aspects of contaminating the moon for the sake of a mere spectacle. And then the larger issue of was it going to be a scientific enterprise or merely a political show.

So, that's what got me started. As soon as I got home in early December I drafted a letter that I sent around to a number of people, including Hugh Dryden. Sent one on contaminating the moon and shouldn't we be doing something about -- the beginning of cosmic microbiology. I sent one on that there were direct conflicts between some of the efforts that were doubtless being planned and biological interests on the planets. And, by the way, had anybody really given any thought to the planets as sites for biological exploration?

So, that was the start of it. This got to Bronk primarily and he submitted it to the council of the academy and they did pass a resolution responding to my concern that it would be a bad thing to contaminate the planets without studying it and referring the matter in various further ways for more careful consideration. Advocating -- I forget the details of it, but for more explicit examination of it.

Now, this was before NASA was organized but during some of the throws of how it ought to be done, should it be a civilian agency, et cetera, et cetera. I think there already were some National Academy committees connected into NACA. Obviously Hugh Dryden, he was the home secretary of the academy at that time.

So, the Space Science Board was de facto already organized even before NASA was. I don't know if it had that same name or not, but that became the body to which I connected with these concerns.

In due course I got a letter from Berkner inviting me to be a member of it. So that was my early affiliation with that activity.

I guess this must have been simmering during my remaining months at Wisconsin. Not long after that -- just about the same time I started to negotiate with Stanford about coming out here, so I didn't -- it was very little -- there was no point in trying to start at Wisconsin.

I got here in Februrary of '59. There was an incident of a Nobel prize in between. This was quite awkward -- It was a very hurried time being in the middle of moving and all of that. And as soon as I got here one of the things that I planned to get going was something more constructive activity trying to organize the activities of the department.

I had an application into the Rockefeller Foundation to begin some studies of exploration concepts. As soon as NASA got organized I was able to get some money from them to continue that work. So, we got tied into that certainly by 1960. And late that year I was able to include a man called Elliot Levinthal who had been a physicist here in Stanford. He had then gone to Varian and been one of their technical directors when they were just getting started and branched out. He wanted to start his own company.

He had been in that for about 10 years and had gotten to be quite successful, sold it. He got kind of restless and wanted to do something very interesting. He wanted to come back to academic life. So, I asked him to join up as chief research engineer and a biophysicist for our instrumentation activities with NASA support. He has been here ever since in that role.

That is how we were able to build a specific small organization to try to deal with the variety of issues. I can show you the technical reports of our activity. It shows some of the things that we were exploring. You had asked particularly about multivator and about microscope --

EE: Right.

JL: -- and those are mentioned there. On the microscope I had met Jerry Soffen, I think at Ames. I'm not certain -- wasn't he at Ames?

EE: No, he was at JPL.

JL: Well, it must have been there through these WESTEX meetings shortly after I got here. I may have been negotiating for this before I got a little money from the Space Science Board to organize this consulting group. They then decided to have another one on the East Coast because I didn't want to travel that much. I heartily agreed to there being two regional ones but I didn't want to move back and forth over the country all the time.

I must have met Jerry then in the WESTEX context. There were several JPL people, we met at JPL once or twice, who were tied into us. He arranged to let a contract with General Mills to look into mechanization of a simple microscope. I've got their report.

In the meantime I have a couple of things. One is I ran into -- heard about a man called McArthur. I met him in London. He showed this little thing that he built and in fact had marketed it through Cook microscope company. I figured that there really wasn't much more to a microscope than a folded light pad than a rigid structure and you really didn't have to fuss very much. You know, it works as well as any other one.

EE: Uh-huh.

JL: When you stop to think about my scope is nothing but a couple of lenses -- just like Leeuwenhoek had done. You just need a rigid structure to put them in and possibly some focusing mechanism.

It was in the context of looking for automatic focusing that I began to think about computers and more sophisticated electronic devices. But when we started looking at a few soil samples we immediately realized that we would be drowned by the noise that even on the most optimistic assumptions and that we did something of higher specificity, if you are talking about microbes.

When you are talking about intermediate levels of magnification, 10, 20, 30 fold and even up to 100 there are few things that you can pick up there. If you are lucky you pick the edge of a leaf or a piece of moss, or something of that sort. The noise doesn't drown you.

EE: Uh-huh.

JL: A little luck but not a great deal. But if you are looking at individual microbes dispersed among soil particles there are errors of both kinds. There are false positives and negatives. They are abundant if you don't stain, if you don't have more specificity.

EE: Right.

JL: So, the direction that I went into at that time was to see whether we could find more specificity, and in retrospect that was a bit misguided but I thought there would be some other payoffs to it. So I started looking into ultraviolet microscopy which would simplify the kinds of things that Caspersson had been doing -- spectromicroscopy, UV measurements and different wave lengths. But characteristic absorptions might distinguish nucleic acid from minerals that you can bind that with morphology. And maybe get somewhere. And we actually did.

EE: What was that now?

JL: Well, Caspersson is a Swedish biophysicist who made a number of innovations. He opened up ultraviolet microscopy in the the concentrations and abundance of nucleic acids in cells partly using this microscopic technique. So it had actually -- you can show that there is DNA in the nucleus --

EE: Uh-huh.

JL: -- by direct observation. We don't do it that way much any more.

We had given a little thought to electron microscopy and decided that that was technologically not feasible on a small mission. That shouldn't be sneezed at.

EE: Uh-huh.

JL: I think that it requires heavy hardware -- I would imagine. And later on Moran (phonetic) carried that ball a little bit.

So, one of the things we do in our own lab is using the UV microscope trying to mechanize it, make it both to -- well, to try to simplify it. We didn't want to have a heavy micrometer. We wanted to conserve on energy, and so on.

We did a number of things, but maybe the most important side-effect of that was that it was actually the foundation for the cell sorter which Len Herzenberg and his department took up a few years later.

We had the optics and technological basis, thinking about examining cells quickly, and so on. He grabbed on to that and developed an extremely powerful and potent device that is really very important. It's a major contribution in biomedical research.

These are instruments that run for one or two hundred thousand dollars each, and two of them or maybe more per month are being produced and sold now. So, for something of that size you can see--

EE: Uh-huh.

JL: That is an instrument that looks at individual tissue cells and blood from other sources. They are passed through a capillary. They are examined one by one with various optical methods. He uses fluorescence microscopy primarily with fluorescent antibody reagents to give different stains to different cells.

They go to a tiny droplet at the end of the capillary. The droplet is vibrating and is in an oscillatory electrostatic field and the droplet can be deflected into any one of a number of buckets depending on a signal that the optics gave. This can be done at the rate of a thousand cells a second.

So they have sorted -- they have like a McDonald hamburger sign downstairs. I don't know -- they are up to 30 or 40 billion cells that have been sorted now, one at a time.

EE: Yes.

JL: And it has been just terribly useful in a lot of research where you want to get specific cell types when you are hunting for rare cell deviants. It is being applied now in the cancer research, separating out leukocytes that attack cancer cells and the cancer cells themselves and things of that sort.

It has really been a tremendous breakthrough in cell technology. That is one of the spinoffs of this effort.

I don't have much to say about the other things that we developed. I think we eventually gave it up as not -- again, not worth the candle in that context. I would not advocate UV microscope at the present time, but that was the manifest purpose.

The latent purpose was to familiarize ourselves with instrumental methods and with what is involved in space qualification and get in touch with the whole technological side of this enterprise so we could be knowledgeable or useful in other instrumental developments.

We didn't know whether there would be any opportunities to put that particular instrument on a mission, but we wanted to know what making these instruments would be like. And a good experience in that it dissuaded me from wanting to be too closely connected with the nuts and bolts. The nuts and bolts will be built some ways that are totally different from what you set out. And I am against laboratory scientists trying to make space prototypes.

In this particular arena I think they ought to do their work on the bench and develop the specifications, their own devices, and then when they know what it is they need to measure you can do the translation job at the state of the art at the time instead of 10 years earlier --

EE: Uh-huh.

JL: -- which doesn't do much good anyhow. But then while thinking about the morphological approach was also concerned about what are some of the fundamental criteria of life. I suppose you couldn't recognize them by shape or by chemical characteristics that might identify them.

Here enzymatic activity of some kind seemed like the best bet. We wanted signals that could be amplified in some way. If you have to do a direct analysis you have to be very lucky to be able to recognize a substrate. You see it one molecule at a time. The enzyme is there and it has a turnover number so you have the inherent application of the enzymatic activity and some selectivity that goes along with that.

We toyed with the idea that this is just the time that people are beginning to replicate DNA, could you detect -- Arthur Kornberg's down the hall -- a single molecule of DNA by using it as a template for further reproduction. That is a hard thing to do on the bench and decided it is not feasible to do in the lab although it might be worth thinking about again.

But then that raises the question, "Now, are you sure it is DNA you are looking for and not XNA."

EE: Uh-huh.

JL: And so it has almost too much specificity for a first set of missions.

But we all agreed that specific catalysis was an inevitable attribute. Although growth was the fundamental definition of life, that the highest specificity connected with that was in specific catalysis. So, if you could get a variety of metabolical or enzyme assays you would have the greatest flexibility, and we wanted to think about an approach that let you wait until the last minute to decide exactly which enzyme you are going to look for.

It is a sort of generalized approach that you then plug in a particular substrate and you add it to the product. That was what Multivator was all about. That was just a multiple sample test device. And it's a -- you know -- how else could you do it? It was a rotating wheel with a number of chambers in it. So in a certain sense it looks exactly like a biology instrument although the details were changed a lot.

So our own efforts there were concentrating on the development of very sensitive substrates, and we decided on fluorogenic substrates thinking that fluorescense could be measured more sensitively than anything else. And that is essentially true with the shady exception of radioactivity and we thought that would be instrumentally a little bit more complicated although the general concepts are easily interconvertible. But in that particular arena we thought we could go further with fluorescent substrates.

We did a fair bit of work on that, and I think that was quite useful. The thing that kept that from ever getting on to a mission was our own sterilization criteria because -- and this is where the radioactive substrates ended up having their unique advantage.

All of the things that we put together are inherently labile. They have a fluorescent group attached to some other molecule and it is the splitting off of the fluorescent groups that then fluorescecse. That's what you sense.

EE: Uh-huh.

JL: Once you do that you can get very high sensitivities. They do compete with radioactive measurements.

But the fact is you have a labile bond there that is going to be amendable to catalytic attack. At one temperature you start going into sterilization -- at 250 degrees. Your very sensitivity is what knocks you out. It doesn't take more than a few tenths of a percent of decomposition to totally kill you.

We saw that that was going to happen. At some point we thought that perhaps there would be a regime of separate sterilization of modules -- could cold sterilize the chemicals and then sterile transfer in a chemically protected environment. But when they came out with this simple rugged -- that's the only thing that would work -- protocols for sterilization by heat on the stand we knew that this wasn't going to be flyable in that framework so we turned it off as a contending instrument itself.

EE: When did that go? About '65 or somewhere in there?

JL: I would guess that's about right. I would have to look at our reports. There were a number of other applications of these substrates and so on that we all looked at, but we knew that they weren't going to be flyable.

Around that time I began to say that it was no longer the mission of the lab to think of developing instruments. That it was important that we had that know-how and experience but that there were really other people better able to do that and there were other things that we could.

We got a license from NASA to go into much more fundamentally oriented lines of research at around that time. So, it has been so ever since. For 10 years we have not tried to compete as instrument developers.

During that same interval when I was thinking about analytical approaches I looked into mass spectrometry, became completely captivated by it as a methodology. We did a lot of work here. I got in close rapport with Carl Djerassi of the Chemistry Department. He joined us as a co-investigator on this program.

We did a lot of preliminary work on what the mass spectrometry of the soil would look like. We were just trying to -- not building an instrument -- we had a two ton instrument with big magnets and so on -- but just to authenticate what a sensitive methodology it was.

But then again on the doctrine that we weren't actually building anything I tried to locate other people who might want to get into it. I introduced Klaus Biemann to NASA, as someone who was a very bright young guy, just getting started at that time. He turned out to be just right for doing exactly that. So, he's been the operational effector of mass spectrometry for Viking and all its predecessors ever since that time.

I, myself got very much interested in biomedical applications of mass spectrometry for other purposes and am still pushing that very strongly, but also in the problem of data analysis. Here the mass spectrum, although not as rich in data as a picture, you have a thousand and a rich spectrum of high resolution -- maybe then, maybe a hundred thousand words of information can be communicated. Most of which are not very interesting but a few are quite critical. We began wondering --

Oh, I have to tell you something else. We also had a rather special variety of mass spectrometer called a time of flight machine. I don't know if you know of that particular device but this is one which bombards a sample with a pulse of electrons, gets the puff on ions that are then generated -- collates them --

EE: Uh-huh.

JL: -- then puts them through uniform electrical acceleration. That means that a uniform pulse of ions are now spread out according to their M/E ratio. So, the light particles go faster than the heavy ones. It's a time of flight instrument because you have a drift tube once they have a uniform acceleration a couple of meters long and then at the end of that you have a sensor and you pick up the ion current at the very end.

The ion current is the mass spectrum of your original source. The light ions get there first, the heavy ones later, and the intensities are related to what comes in. You can make corrections in the sensitivity of the detectors as a function the mass of the ions. But one thing I have to tell you, it does this 10^4 times a second. So, everything I've described goes very fast.

EE: Yes.

JL: And there is an enormous data output. In fact, there is still no very good way to handle the pulse-by-pulse data that comes out of a TOF machine. It still overwhelms any of the systems we now have. So they usually use an averaging mode. Usually you have repeated pulses that are superimposed on one another and you get a little higher precision in that way.

You can get time resolved mass spectrometry by focusing on a given ion and saying "How does that ion current?" And one or two change with time. We have about 10,000 data points in the spectrum and they are coming out 10,000 times a second.

Getting the full use of that information is a formidable data reduction problem.

EE: Uh-huh.

JL: -- it really hasn't been solved. It's a faster input than any computer can handle right now. It is in very short bursts.

So we wondered a little bit on what we could do about that in terms of real time management of that information, and so on, and so forth. And ended up with no solution that really got all the efficiency of that kind of machine. So, the bottleneck there is in fact the data analysis, not the generator of the information. And it is still the case.

But we thought that this might be turned into a mass microscope and if you were to have the ion beam -- if you had an electron beam and later we turned to an argon ion beam -- to scan across the specimen and as it is moving each pixel is a pulse of gas, a pulse of ions that reflects the composition of the target.

You could get an enormous amount of information about the chemistry of the target as you scanned it back and forth.

So, we did spend two or three years looking through that concept of analysis and not worrying whether the mass spectrometer per se would be flyable, believing that the art would develop to give you other approaches. And this has been the case although none of the alternatives really have -- but the specific issue does.

I got a lot of interesting things out of it but decided that it would take another several million dollars to actually develop this machine itself. There are just a lot of problems in details as you go into it further.

It turns out that heating a specimen is -- getting enough energy into an individual pulse -- to turn it another way, blasting very tiny holes without destroying the surrounds is something we know how to do but the way we do it -- the ways we do it are not easily compatible with the other capabilities.

We felt we had a flow diagram about a lot of things that were conceivable, almost all of it attainable, but we added up the bill for what would be required, the entire project, and decided that it was probably not worth pursuing any further so we turned it off. And pursued other elements of mass spectrometry instead.

The data analysis parts to it, though, were very intriguing and they were the start of an honest to goodness artificial intelligence project we've had here for about 10 years. This in collaboration with Carl Djerassi and also with Ed Feigenbaum of the computer science department.

We set ourselves the task of building programs that would be smart enough to interpret the mass spectral outputs and deliver the information that the chemists gets out of them in terms of what chemistry there is behind them. And did that a little bit with the motive on what you might want to do on board a planetary mission some time.

But that has reacted as not really being a critical problem that needed to be solved but primarily as being a good challenge for AI in that it was real world oriented, was connected to scientific data, and had all the advantages of the constraints of a domain of scientific inference which is much more narrow minded thinking than common sense thinking is.

A smaller set of rules very rigorously applied rather than the loose thinking that involves knowing everything --

EE: Uh-huh.

JL: -- that being able to walk down the street entails. And most of the work in artificial intelligence until that time had solved the problem of restricting the domain so that a problem was doable by working on games of checkers and chess which was the main thing that AI people had been working on until that time.

We felt that didn't really give; who the experts on chess. Were they scientists? No, they were intuitive types. People not able to communicate the rules of inference that they using -- that alone being it, besides the actual structure that they were working on.

The work we did with this domain has been a prototype. Our assumptions have borne that out. They have been quite successful both as a project and as a pathfinder for work of this kind. It was a very modest effort in a very well field using a high level of specialized expertise and that's where all the success stories in AI are these days. They are very similar to these kinds of projects.

But that has come to be one of my major scientific interests. How do we take the first step towards the mechanization of scientific thought? How far can it go and can we improve the efficiency of scientific progress by getting better use of machines as backups, keeping us honest, and all the rest of it, and in the process of understanding a little bit of what we are doing here ourselves?

So, that is why this is here. The machine we talk of is a resource that is in fact used nationally for congeners of similar projects. The guy I was talking to over the phone is our project operator.

[interruption - end of Side 2]

EE: Well, let's go back to --

JL: Yes.

EE: -- the early days and let's talk about --

JL: Let me get out the EASTEX and WESTEX notes.

EE: Yes, I would like to look at those.

JL: Do you have questions?

EE: Well, let me ask you a few questions about Gulliver and some of those things.

JL: Yes.

EE: I take it, then, that one of the reasons that Gulliver or labelled release --

JL: Yes.

EE: was flyable was the fact that --

JL: These are radioactive substrates.

EE: -- survives.

JL: That's right. Rugged substrates survive heat sterilization. There you are not relying on a single metabolic reaction but a whole chain of them. Therefore, they are not labile bonds within the molecule that would fall apart readily with heat.

You would put in something like lactic acid or alanine and there actually several steps involved in it's metabolism and then picked up by the radioactive release. That is adequately -- you know -- a very sensitive method.

There were problems with it that are -- Well, the method is not as sensitive as one might think if you take the noise into account. It does take a very substantial sample to give a result. That's been fudged over by talking about experiments involving growth.

We were trying to get down to methods that could pick up single cells by their biochemical activities. The methods we had in hand would pick up a few dozen or a few hundred -- maybe a couple more [?] away from that target, and good enough for any practical task that you would want to address. They did suffer from the lability of the substrates but are not useable there.

The radioactive release -- the problem there is not the sensitivity. If you had zero background you could pick up one count. The trouble is there is always the finite background that you've got. And in particular a problem that ended up not being a serious one on Viking because there was never any problem about a signal on Viking. Every sample gave a very strong signal so you didn't have to worry about signal noise consideration. --

EE: Uh-huh.

JL: -- as you have to do with the authenticity of the signal. But the long storage of a radioactive substrate at high concentration on its way to Mars meant you had a considerable amount of radiochemical decay.

The data particles that are emitted during the mission also cause the destruction of other molecules that has to be purged out of the system. The ability to purge is an asset, the fact that you could clean out the baseline signal by gas purge.

But the converse of that, of course, is how perfect it is, and do you really get out your full baseline, is it going to have residual gas that's going to be problems also, and so on. There is still a certain amount of argument about the interpretation of the initial rise on the Gulliver experiment for that reason. We don't know how much of that is initial gas, how the purging was, and so forth. And in the present context it doesn't matter very much --

EE: Uh-huh.

JL: -- because the strong. If we had had more ambiguous signals there would have been some difficulty there. We would have had to try to fight through (?) senior kinetics inference against the very substantial background.

EE: Now, Levin says that the pyrolytic release was sort of spinoff from Gulliver. I don't know -- I guess I'm going to have to talk to Horowitz about that.

JL: I think they are both spinoffs from Calvin. You know, the notion of using radioactive substrates doesn't require any justification of local passing of ideas. I mean, that's the first thing any biochemist thinks of in this arena. I don't know the historical sequence but that sounds really implausible to me.

I think Norm had given a great deal of thought to some of the fundamental attributes of life thought that photosynthesis was necessary, and in a system that was lying around for a while you had to have some way to drive them in the cycle. And knowing that there was CO2 and then later CO in the Mars atmosphere made that kind of approach very practical.

I don't recall it. It may well be in my correspondence but I can't recall it. I can't tell you any more about that.

EE: Okay.

JL: And I just don't remember off-hand either just how Gil got into the loop. Anyway, so many things had happened before the official REP's and experiment selections, and so forth, I think that you really have to go a little earlier.

EE: I'm going to go through his records.

JL: I'll see what I have here that might be around. But he had certainly been given development contracts by NASA long before then. I imagine at one point he was simply asked to build something that had been designed elsewhere. Hadn't he?

EE: I don't know. I think he -- I'm not sure. I would have to look at my notes. I don't remember off hand.

JL: And the third experiment was Oyama's on the --

EE: Right.

JL: -- gas exchange. That got in because he was working at Ames and Ames was the laboratory, the center for all this stuff. I must say I'm very grateful to Vance for having had this experiment in there. If he hadn't seen that oxygen coming through as one of the first things we would probably still have been confused about interpreting the other outcomes.

I think that -- Well, he is not a model of clear thinking in scientific terms and he has come out with some of the absurd kinds of suggestions and detailed interpretations. We have really felt sometimes that we should just grab him by the coat tails and make him sit down and spin off some of his ideas.

He has gone to great detail and specificity to try to account for some of the findings when the evidence was very thin -- spinning structures out of thin air more recently. But, he has been very dogged about his instrument, the way he has pursued the working of his instrument is really admirable. I separate that from his theoretical conceptions which are different -- and they paid off.

EE: Okay. Now I'm going to ask you a question and you may not be able to answer -- you may not be able to help me in this realm.

In '63 to '65 -- maybe even a little bit later than that -- they keep changing their names -- I believe it was Philco Aeronutronics at the time.

JL: Yes.

EE: Now Ford Aerospace --

JL: Yes.

EE: Communications Corporation. They got a contract to develop the automated biological laboratory.

JL: Yes.

EE: That was going to be -- it could either be hard landed or on early mission, or it could be landed as part of Voyager.

JL: Yes.

EE: Not, what I have yet to be able to pin down anywhere in the documentation is why when Viking came along STL got a contract instead of Ford. Do you have any --

JL: I don't have the foggiest idea.

EE: I guess I will have to go and ask some of the people.

JL: It was never my perception that there was that uniqueness of interest of delegation that I know of. You had said "the contract," I would have said got "a contract" because --

EE: Well, for several years it was the biological instrument of the contract.

JL: Yes.

EE: In terms of -- you know -- an overall integrated package.

JL: Well, you have better perspective on the management and how the different elements of it were supposed to be functioning than I did. I guess I didn't take it that seriously as the definitive mission at that point.

EE: Well, that may be the perspective I got out of the JPL document which may not be accurate.

JL: Well, I'm just not familiar with how those things were handled.

EE: Okay. That's one of the things I'm going to have to pursue with Dick Young and people like that.

JL: I'm sure they would be much more knowledgeable about that than I. You know, I certainly never played any role in source selection or anything remotely resembling that. You would hear every now and then that such contract had been let to do that and we would talk to the people involved, and so forth.

I guess until we had the Viking mission laid on and then we knew we were talking for real -- the instrument, the things just struck me as being study contracts of one sort or another -- to some extent busy work.

EE: Okay. Now, once you get down to Viking were there any serious contenders besides the labelled release, pyrolytic release, the gas exchange, and Vishniac's light scattering experiments at that point?

JL: Well, I don't know about contenders. I think there were a couple of other things that we talked about including that got stripped off a little later on. I would have to go through our proposals of the different stages to see what they were.

I would be surprised if we had not put in a bid to include the pH measurements and conductivity measurements. For instance on that point, I know that I tried to get -- it's what I call microscopy -- not very high powered but the equivalent of 20 or 100 full magnifications laid on and it didn't fit anywhere. It didn't fit on a land camera, we couldn't build another one to put in our own systems so some got lost there. In a sense that was a contender.

We talked about more LGC for a wider range of things and we talked about other ways of coupling the mass spectrometry and at one stage they were going to integrate mass spectrometry into the biology package, I think. And then there was more discussion about how to possibly have some more cross connections into that. I forget at what stage it was decided to separate that as a separate experiment.

There was a lot to do about needing an integrated experiment and how we were going to do that, what did that all mean, and so on. We ended up with integrated sampling and sample handling and very little else. Well, at least that was very necessary.

At various stages some of the people threatened to quit if they didn't have more control over their own local area. I know Norman was very uncomfortable about the idea of having to justify his ideas to Vance and Gil. I can understand his feelings on that matter. I know that they are not anywhere nearly on his own intellectual caliber on this point but I also felt that separately things would end up being very difficult to integrate into a system, or nothing would work.

You have to have a minimal level of system integration or nothing would work. But, anyhow, that was imposed on us whether we liked it or not.

As I recall there were two stages of review, or even two proposals. The first set were looked at and then sort of discarded and then a new round -- two rounds were examined together.

But, there again, I wish I had some more of the records. I'm sure I would remember more if I had some prodding. I'm sort of lost in recollection at the moment. Maybe we can dig some stuff out that would help.

I was not as systematic about trying to record things. And in particular, I know I didn't record my own perceptions about what was going on in the way I would have liked. It was just a hassle. So I sort of made a conscious decision I was not going to be the historian in this event.

EE: Right. One of the things -- one of the reasons --

JL: I sort of saved all the pieces of paper that came across my desk --

EE: Right.

JL: but I didn't add to them.

EE: One of the things that I am pursuing in my own mind and have not yet got into the documentation deeply enough myself as yet was one of the reasons that the experiments that were flown selected the fact that they were sort of the survivors and that they had been around? Or was it simply that those seemed to be the most reasonable and most logical experiments to fly?

JL: I know certainly their degree of space readiness must have been a major consideration. And they were as good as anything else. That is, none of the other concepts was so plainly advantageous that one would discard them for that reason.

I recall having my own doubts about whether we really did have the very best package compared to others and since I was not responsible for a given instrument I could be a little more detached about it.

And I wasn't sure that requiring the groups to be preassembled was really the best idea. I think if I had my druthers I would have first had a competition for individual experiment concepts and then organized the teams from the survivors, and that is not the way NASA did it.

That is, at least formally it is not the way it did it. I think they looked at it more or less the same way.

The one thing I have to refresh my memory on most specifically, I know Bessel Kok had already had his designs fairly far along and he may have been one of the more disappointed people at the final selection. But I'm not sure I'm not comparing that with a little later on.

EE: Why did they finally drop Wolf's experiment?

JL: Well, there are two issues. Why was an experiment dropped and then why was that chosen as the one to be dropped? The former one were engineering cost considerations. I can't really quarrel with that. I don't know much about it. I was just told that we had to come back with a trimmed down experiment. It was a painful decision to have to make but an effort to sustain all four would probably prejudice the entire mission. And they just came to the conclusion that one had to go.

I have no reason to challenge that that was a reasonable decision in that framework. They should have had a larger (?) to begin with and maybe it wouldn't have come down to that in the very end.

The procedure by which this was done was a very painful one, and in the end it must have been Naugle and Young who came to the committee and then Chuck Klein who was the Chairman of the committee came to Alex Rich and myself and said "Look, it's obvious you two are going to be the swing votes. You are the only ones who don't have an obvious and selfish interest in this regard. You two had better advise us on what to do and then maybe we can get a team consensus."

If you threw it out to the team obviously everybody would vote to keep his own --

EE: Uh-huh.

JL: -- and it would just give rise to more cliques about what else to do. It is not a very pleasant experience to have that responsibility but I figure that in a certain sense that was what we were there for.

We were there for those kinds of hard decisions. We had several rounds on it but Alex and I very quickly agreed on this and there was not much dissent.

There were two arguments -- and we wrote a letter to Naugle. I don't know if you have that.

EE: No, I don't.

JL: -- about the reasons for the choice. Here is one case where hindsight has thoroughly been reinforced. There was a question. One was that it was a ery un-Martian environment.

The whole experiment depended on growth in a liquid medium at temperatures with 15 degrees centigrade. There is no place on Mars that has that habitat. So, that was what we felt that bugs would be drowned literally. They would not in many millions and millions have seen that quantity of water at that temperature, and so on.

The other was the liability to artifacts since it was only a turbidity measurement and it relied on organisms being able to be separated from the soil and the dust particles that were in it. We believe those particles to be very small and this is correct. You can only have a fairly porous -- the bugs need a fairly large pore size filter plate to get through. Any jostling of the instrument would raise turbidity and you would end up with having to depend on really dramatic exponential kinetics starting out with a very small sample to separate that from other sources of turbidification.

We just didn't feel comfortable in that that experiment could be well enough controlled. That the turbidity per se would be of a reliable result. We felt that it had a very high chance of being a false positive.

We felt that of all of the errors to make that was the worst. It would not be responsible to go into a mission that had an uncontrolled opportunity for a false positive result.

And while we would have been delighted to have that as corroboration of the others we felt that standing on its own that it was the most vulnerable.

Wolf sort of had to agree with it. You know, he was not very happy about it, but it was true. I think he accepted it as an honest conclusion.

There were a lot of other criticisms that could be made in detail about the implementation of the other experiments, and there was no question of kicking him off the team --

EE: No.

JL: -- and so on. So, he went along with it. So, that was -- I've given you 99% of the arguments.

EE: I get the feeling that he was a very constructive member of the team just generally speaking. He sort of kept people together and in line and he got -- well, working together.

EE: Oh, yeah.

JL: Well, he was the most dedicated scientist on the team from the point of view of being deeply committed to the project having a wide base of expertise in it, not having the kinds of ambiguities that Norm did in this direction. It was a great loss.

I think that his thinking about ecosystems was more important than that particular experiment, and that he had an opportunity to use himself to a better advantage not having this experiment hooked into it.

I'm sure a lot of people have wondered if there is any connection between his disappointment and then his accident in Antarctica. I really have no reason to believe that this was the emotional state of the person as far as I knew. But just in case anyone raises those questions I thought I ought to say something.

EE: Yeah. Nobody has ever indicated that to me.

JL: He also did some very important work in Antarctica and here is where Norm really went off the deep end. He and Cameron concluded that there was a habitat on earth much less harsh than Mars that was sterile -- the dry valleys.

Do you know the story?

EE: No, I don't know that story. I've heard it alluded to but I don't know the story.

JL: Norm didn't think there was enough moisture on Mars and here was a habitat much more earth-like than anything on Mars, and this is a place where -- Oh, for about 50,000 years -- it's in very high altitude, it's an anomaly, it never gets any snow or very rarely. The air above it is so cold that it kind of dried out the soil. So it is as possibly as arid a place as you can find on earth. Deserts, even the deepest of them get rained on once a while. That's apparently not the case here.

So, he did some microbiology in that area that I have always believed to be faulty, and I think is now the general consensus that it is and that he believes it contained no indigenous organisms. You could find a few airborne contaminants on top of it and if -- even with all the richness of life on earth couldn't get through the barrier of aridity there....

The argument was more about contaminating Mars --

EE: Uh-huh.

JL: -- how could a very small sample of organisms planted on Mars fight through the hundred fold more intense barriers there, therefore, are we worried about contamination.

Those are the arguments he published on that score. Wolf looked at this through a microscope, found indigenous colonies of microorganisms that you could see there. It is unlikely that those are airborne -- in place and was collecting further evidence of an indigenous habitat.

And there is a paper that came out somewhat more recently by Friedman demonstrating the algae that grow in the surface rocks in the dry valleys. A very intersting habitat. They were actually able to sit in very tiny clefts -- you know -- in the cracks and crevices in the rocks about a millimeter from the very surface. Encapsulated by granite. They get the sunlight and they grow a little vivarium for themselves. So, he has identified the tiny producers down there. We have opened that issue up.

EE: Mrs. Vishniac is putting together some of the -- final report or something on this.

JL: Uh-huh.

EE: I had not heard that. Is he rather upset -- Norman?

JL: I haven't asked him whether he has accepted that finding. We didn't talk about it.

EE: Okay.

JL: He had used it mainly as argument about sterilization. He thought that would --

EE: Well, I guess I'm going to ask Novel and Young and some of those people about -- you know - the selection process in detail and see what they --

JL: Yeah, I think you would have to. Chuck can probably tell you a good deal -- Chuck Klein. He is somewhat more in the middle on that, and I'm on one side.

EE: I've got quite a bit of documentation but it is rather confusing and the documents always reflect the decisions after they have been made, not while they are being made.

JL: I remember being bewildered myself about just exactly what was going on and I didn't know whether the whole selection thing was a charade and that this group had already -- you know -- who else had been previously funded. This is the group that had Norman -- they had to but they had to comply with some procedure.

It had the advantage, at least, of exposing it to alternatives. I've articulated something I hadn't thought about before about this process, call it the Space Science Board syndrome -- decision making by authoritative advocates. It may not be as bad as it can be painted.

EE: Uh-huh.

JL: As long as the outcome to the public -- even if there are mistakes, even if there are conflicts of interest and so forth, there is still accountability. It's more retrospective but we are working within a framework which is very sensitive to colleague criticism.

For that reason the fact that the outcomes are published is an important criterion. And it really is less important than in a court of law, that it be -- you know -- at the time that you make the award or the decision --

EE: Right.

JL: -- that you have everything completely laid out -- you know -- we know every system has its little flaws in it too.

EE: Right.

JL: I'm not saying that it is a paragon of justice, but it's not totally lacking it either. I guess I haven't really given that view of it. I would write a critical view of the academy -- for many years -- I did not get deeply involved there for the most part. I've been a member since '55 or

For some of these kinds of reasons I thought it would put itself out as a source of ultimate authority and wisdom that it had no right to have and in a sense didn't want to cooperate with that. I did with the Space Science Board for a while but with some ambivalence.

I'm just now beginning to see that there was another side to this coin.

EE: Okay. Now, sort of post-Viking.

JL: Yes.

EE: I don't see anyone criticizing -- you know -- the results. Abelson said "By going looking for life on Mars we are likely to be viewed as the greatest Simple Simons of all time". You know, he's been saying that since 1965.

JL: Uh-huh.

EE: No one is making those criticisms today.

JL: No. I think the main reasons are the channels. That Mars has ended up being a much richer planet in every other way, regardless of life, than anyone then believed. It was really believed that it would be just like the Moon. And that you would learn nothing that you couldn't learn from the Moon.

And that, by God, we know better now. So, I think more than anything else that's the answer to that. That Mars has had a very active history and the fact that we don't understand the history of our own planet. The question of life has been a little shifted to the side a little bit.

So, you know, one could argue if that is sound. In the course of looking for life you find something that is even more important, regardless whether the outlook is there or not.

EE: I was in Pasadena for most --

JL: But you know, that justifies Mars 65. I mean, it is just exactly what we --

EE: Right.

JL: Go ahead.

EE: I was in Pasadena during both of the landings, and I think the general astonishment of everyone associated with the project in terms of what Mars looked like at the Viking resolutions both --

JL: The orbiter gave you that. The lander didn't really show up so many surprises. It's the Orbiter pictures --

EE: Right.

JL: -- that had given us that perspective. A lot of that was already in Mariner. I mean, you have a Mariner map up there --

EE: Uh-huh.

JL: -- that shows most of the features. What Viking did was to show even more so, and that those things that looked like river channels, by God, there was no other way to interpret them when you can see them in more detail and see how they cut through ridges and so forth.

I just wanted to push that back a step.

EE: Uh-huh. Okay. Should we look at some documents?

JL: Yes -- About the Viking and what it was painted to be. Mariner was touted despite its obviously limited capabilities more than it should have been as being able to detect life -- you know -- there were a lot of cautions about how little you could see from those resolutions. Carl wrote this paper -- you know -- that Earth had life on it and so on.

I guess it worked out all right. But there had been so many burns on both sides from overplaying with the missions could give. There was Pimentel disaster during Mariner. Do you know about that one? Okay. Maybe it is just as well it happened because --

EE: Go ahead and tell me --

JL: Well, he had been up for 36 hours running without any sleep and saw some blips on the IR spectra and there was a news conference coming up. Without thinking he sort of blurted out that he was pretty sure that they had detected methane and ammonia in the Mars atmosphere.

It sort of got out before his colleagues could shush him and that got the headlines that organic chemicals in Mars and evidence of life, and so on. The whole thing was just a fluctuation, it had no real substance whatsoever.

It certainly has done him no good. I think he has more less climbed out of it but it was an aberration without a doubt. In fact, it was one that led -- there was a fairly formal program of psychiatry surveillance during Viking in the background there. It was counselling and having access to resources cause people were under tremendous stress --

EE: Right.

JL: -- a lot of times. I think that was recognized. I think there were other incidents I don't know about on the operational side, but you can really imagine how people make some horrendous mistakes under those sorts of conditions.

So we wanted to be more careful than ever that that wouldn't happen. We had all kinds of agreements. They were more explicit during Mariner than they were about Viking about not running off to the press. That our discussions would be collective ones.

That sort of broke down during Viking. But there was enough of it there that when the first LR results came through and Gil was up in orbit -- about having discovered life there that it was possible [SIDE 4] -- as well as common sense. You know, not speaking out about something they will very well regret later on.

One was able to point the finger at this as a disaster story to be avoided there.

That is something that Jerry can tell you about. And I think that if you are going to talk about the rationality of scientists under stress and so forth there are some very interesting issues there.

EE: We've talked a bit about that, but we need to talk about it some more. We had talked just primarily in terms of the landing selection --

JL: Uh-huh.

EE: -- but I haven't talked to him about in terms of science which I need to do.

JL: But certainly the immediate events after the first signals started coming through and help people using that information -- how they were dealing with it.

In fact, if you could get it out -- I don't know how well documented it is -- in the next two weeks -- how the first Viking lander data were interpreted is a very interesting story. I was up here at the first instant. Elliot was down there. He sent me a message about some of those first numbers coming through.

I got the LR results and the Oyama results simultaneously and immediately worried like hell that somebody was going to go half cocked on the one side and saw that the oxygen release was really the key to this story and so frantically telephone back to hold your horses about a lot of these interpretations, and so on. What I don't know is who else was saying the same kinds of things and when, and so forth.

EE: Uh-huh.

JL: Then word got across over to the chemistry group and I am sure it was Klaus who picked up the same notions almost immediately and provided a lot of tempering on that, not going off half cocked on a biological interpretation.

The release of oxygen was such a critical finding that it had to imply that there was either peroxide or another oxidant around -- when you get another oxidant around you count for LR immediately. You had to wait a while for the PR results but at least they made you reexamine the kinetics of the LR more carefully and so forth.

It could have been a serious credibility disaster.

EE: Recently -- I believe it was in Harpers, maybe Atlantic -- a fellow has written an article, not very clear, but he has written an article anyway saying that life was discovered on Mars. That scientists just aren't admitting to it.

JL: Well, Carl unfortunately is providing some ammunition to that.

EE: I know.

JL: If you look carefully at what he says it's okay, but he sure twisted that. I wish he wouldn't.

EE: It seems to me the name of the game is maintaining your credibility.

JL: Oh, yes. Absolutely.

EE: And Horowitz tried to write a rejoinder to it which I think was terrible.

JL: I haven't seen that. Well, we ought to get off the dime and do a better job of resimulating those findings. We do not have a working laboratory model that exhibits all the detail there. That is, UV irradiating a soil sample and getting out the kinds of results coming through.

There are bits and pieces of it but it hasn't been put together. So, one can argue that we have not yet succeeded.

There is nothing implausible about it. People talk about that as exotic chemistry. I have repeatedly criticized that phrase or misusing the terms. They are in different frame reference. It is exotic from the standpoint that there is no soil sample on the surface of Earth that you could pick up right now that would show that result.

It is not at all exotic in the sense that I can contrive a laboratory simulation in a different atmosphere and different pieces of which simulate everything that has been found there. It is unearthly as a planet but it is not -- Oh, excuse me. It is unearthly in terms of soil --

EE: Right.

JL: -- because t isn't exposed to those conditions, it always has so much moisture around that it would discharge anything that Vance saw.

But that play on words has caused a lot of trouble it seems to me.

EE: This is one of those cases where you have to be very precise in what you say otherwise --

JL: I think we have to lean over backwards. I mean, you are going to be more than precise in that regard and that people are going to hang on your every word. Even if you are precise you have to think how they are going to interpret what you say.

So, I think it goes even a step further than that.

EE: I'm sure that is going to be one of the issues that at the end of the book I will have to deal with.

JL: Where are we now.

EE: Where are we now. And how people have either wanted to find or not find --

JL: That's right.

EE: -- but want a definitive answer where there may not be a possibility of giving a definitive answer.

JL: Well, it is part of the story that I advocated get written although it is too late to get accurate reflections on it without going into the records. That first paper had to be composed in that atmosphere.

Dick Young did a beautiful job. He wrote -- out of the different reports that were there -- we just sat down and for about 36 hours just kind of got it down all in one piece and then passed drafts around for peoples' criticisms.

I was opposed to even writing the paper at that time. He felt that he had a very good chance of having a more detailed corroborations that might say "yes" or "no" and finish the PR, we would have the controls and that kind of stuff. I thought to put out a cliffhanger like that at that stage would just open us up to more criticism, and I was a little afraid that it would have a cast of misleading -- being precise but still misleading people in terms of giving options to their being a biological interpretation which I was sure would be closed off very rapidly.

Partly that had to do with what objective facts could be displayed and partly it had to do with accustoming people like Gil to the bitter reality that you had just better face up to the way things are. Our hopes are not what we wished they were. I thought that would take a little longer.

The paper came out all right. Dick was right and I was wrong. It was possible to write a paper that is a pretty fair reflection -- I don't think we have anything to apologize for in the way that is written. Including the perceptions that were aroused as well as precise statements. I wonder about -- I attribute that to Dick's skill. I went over it very, very critically and worked very hard to smash down every imputation that I thought would be misleading.

EE: I wonder about the pressure of -- you know -- especially I'm thinking of just the general articles that came out after the first landing where you -- since the landing got delayed the time for writing the article was all the more short..

JL: No, that was a scheduled issue --

EE: I know.

JL: -- and you have to meet that deadline. My vote was to bypass it. For those reasons we were not far enough along. In fact, we have a disclaimer at the beginning of the article that we would not customarily write an article at this stage of an experiment. That was Alex Rich's specific wording. But the events being what they were we thought we had better share what we had at this point although it was somewhat inappropriate.

EE: Another one of the time pressures of the space program.

JL: Well, and everything else that surrounds it. It is not as if the stuff hadn't been all through the newspapers too including graphs of Gil's experiments, and so forth.

EE; How well do you think the newspapers interpret something like that since that's where the man on the street is going to get his information?

JL: I'm not the best one to judge that obviously, but I would say that they try hard to find hooks that they could honestly use to make more spectacular stories. They didn't find them and they didn't invent them. So, I really can't fault the reporting. On the contrary, I think they did very well -- certainly some of the questions were. You know, there are good reporters and bad ones.

Some of them at the press conferences were very able. Able to smoke out -- and preferred not to talk at that point, and so on. So, as far as I can judge there were one or two mistakes like this exotic chemistry which are our own fault. They are not serious ones in the total outcome. I think they ended up giving a pretty fair picture of what the mission did. The reporting was pretty extensive.

Now, I'm judging it primarily through the San Francisco Chronicle and David Perlman who is a --

EE: Uh-huh, Right.

JL: -- who is a superb writer. You know, probably the best in the field on this particular subject. His paper gave him a lot of coverage.

So, I don't have the perspective to say what came out elsewhere. I think that Chronicle dealt with it better than the Times did, but those feelings are based on the papers I was reading on the subject.

But I can't say that I was dissatisfied with the final outcome. I want to come back to a more general point though. And that is what was Viking purporting to be?

EE: Right.

JL: One of the things I was most nervous about in the Karth model of it was that somehow this reduced mission was going to be the definitive one and that because they had said so there was never going to be a mission after this. This was going to be the final answer on that question, and that was the word we were given.

I tried very hard to maintain the response that in no way that was scientifically possible. That if we had to promise that -- you know -- bet our reputations on that, that we couldn't.

Somehow during the years that got to be understood. The next year's statements about Viking were more cautious and began edging around that it was the probe mission, that it was being trimmed down, that we had to qualify what it might be able to do. A couple of articles were written and inspired about possible limitations of the scientific foundations of Viking. I felt very strongly about that -- not even yet having the Mariner data in the earlier part and then not being able to use the Mariner data to a very limited degree at the later part except when it came to the site selection.

So, I was pleased at the tone of NASA's PR on Viking and that it did begin to be more cautious and that it was less committed to its being the ultimate experiment and capable of doing the impossible.

It still had some of that in there. It still had to be justified. So, I ended up being not outraged but just moderately uncomfortable with the degree of commitment that it seemed to have. And I made it my own business to be sure that Fletcher understood mine and my colleagues views on that just so that he didn't get out on the limb himself. And I think he understood that point -- that it had finite capabilities and that if we were lucky it could pick up something definitive and that a lot of options we wouldn't be able to hit.

The more we learned from Mariner the more evident that became. We had a symposium down at JPL on after Mariner and on what is ahead for Viking, and I talked to that theme and then Carl urged me to write that up for Icarus and I was just too busy to do it but then agreed that if he would join me on drafting it that I would.

So, we had Icarus on -- effectively what were the limitations of Viking, and that is where we discussed the un-Mars qualities of the environment that we were creating for the experiments. It was not long after the Wolf Trap decision had to have been made, so that was kind of fresh in mind and there was quite a justification for that choice.

So, that was there and of course --

EE: Can you pin that article down for me?

JL: I'll get it for you. Here's the reference to it. I don't have the --

EE: Okay. I may already have that.

JL: Let's see, Icarus 28 --

EE: Prospects for life on Mars.

JL: This was in December of'73 -- there was a big national colloquium on Mars at the Jet Propulsion Lab. This is called -- the original manuscript was a post-Mariner 9 assessment and it took a couple of years to get around to publish it.

EE: So it becomes a pre-Viking assessment. Okay.

JL: But that was a good staging point of the symposium itself. Sort of collected what we were thinking. I think several of the articles did appear.

We were very nervous that more might be claimed for than it could deliver and that it would lead to a kind of backlash and all kinds of criticism on what we really couldn't do and so forth. I think that came out all right.

The main criticisms that can be offered about the Viking choices are roughly in the direction that we ourselves were worried about. That we were so tuned in to an earth-life model and got a lot of water in the system that we hadn't really given enough thought to how dry and how heavily radiated it was and what got all the chemical reactions we are seeing now and could you have thought about them sooner.

The fact is that they were thought about. Some of them were collectively examined. A lot of testing work was done, but, as I mentioned before and particularly in retrospect and not on any theoretical grounds, you have to do 10 times as much as you think is necessary and is possible to really cover it all. There was neither the motive nor the resources nor the time to do it. So, I think we came out relatively lucky. It could have been a worse disaster than it was.

There were a lot of times then that the instruments were very precarious. We were astonished that they worked as well as they did in terms of how things were going just before they were sent to the Cape. And you will hear more about that again from Chuck --

EE: Right.

JL: -- Dick Johnson --

EE: Right.

JL: -- he had good reason to anticipate lots of problems that never eventuated.

Okay. I think that covers most of the questions I had in mind. I guess if I were thinking of themes of this kind of intellectual history I would ask myself the kinds of questions we started out with. Who got into it and why and against what resistance and what reinforcements, what was in it for the, and so forth.

EE: Okay.

JL: The particular constellation of interests that got connected here. I don't know if that is something you completely address within a written story, but it would be --

EE: I'm going to try to as much as --

JL: -- as much as you could get. This is a very good group -- the people who are involved here. Many of them are only marginally involved operationally -- but are tuned into it scientifically in a number of ways. I think they might well be able to offer some very interesting comments.

EE: Okay.

JL: I brought up one box of things. There are just a few things I want to sort out of it that literally don't belong here.

[end of recording]