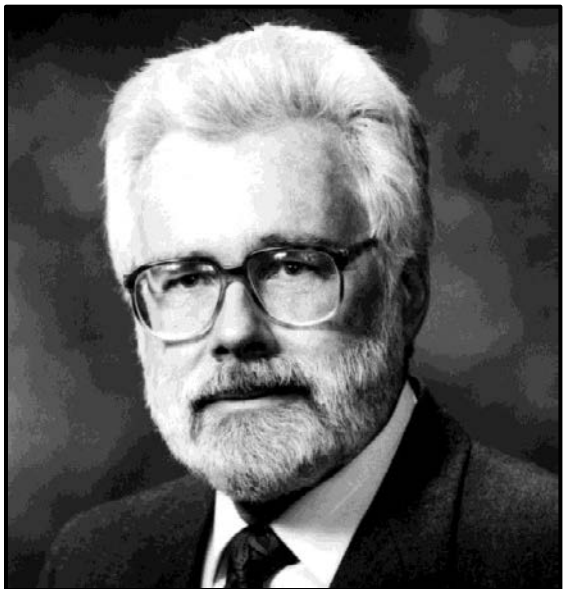

Arden Albee, ca 1992

ARDEN ALBEE
(b. 1939)

INTERVIEWED BY
HEIDI ASPATURIAN

August 16 and 31, and
September 14, 2017

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Geology and planetary science

Abstract

An interview series in three sessions, in August–September 2017, with Arden Albee, professor of geology and planetary science, emeritus, and a key figure in lunar and Martian exploration throughout the ’70s, ’80s, and ’90s. Raised in Michigan, where his early interest in rocks and natural history was nurtured by road trips through the American west, he earned his undergraduate and graduate degrees in geology at Harvard and worked for the US Geological Survey before joining the Caltech faculty in 1959.

At Caltech he pioneered the use of the electron microprobe in petrological studies with colleagues A. Chodos and E. A. Bence, pursued fieldwork in numerous locales, including Greenland, where he and Caltech colleague G. Wasserburg took part in the landmark Oldstone Project to collect and analyze the world’s most ancient rocks, and collaborated in characterizing and dating the moon rocks returned by the Apollo missions.

From 1978 to 1984, he was JPL chief scientist during a transformative period in the lab’s history under successive directors B. Murray and L. Allen. He chaired

numerous NASA planetary exploration committees and served as project scientist for the Mars Observer mission and as mission scientist for Mars Global Surveyor. His lengthy tenure, not without controversy, as Caltech's dean of graduate studies from 1984 to 2000 and his two decades chairing the house committee of the Caltech faculty club, the Athenaeum, are also recounted in this oral history.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH ARDEN ALBEE

BY HEIDI ASPATURIAN

PASADENA, CALIFORNIA

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Arden Albee, ca 1992

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ARDEN ALBEE

SESSION 1

August 16, 2017

ASPATURIAN: This is August 16th, 2017, interview Session Number 1, with Dr. Arden Albee. We usually start by talking a little about your early years—where you were born, your family background.

ALBEE: I was born in Michigan, grew up in Mount Clemens, Michigan, and went to high school there. My father was a math teacher at the high school. My mother was a housewife until the war came along. Then, since she had a normal school degree [essentially the equivalent of today's AA degree], which they had back then and which allowed you to teach, she taught all during the war [i.e., World War II] too, in a rural school. I had two brothers and a sister.

ASPATURIAN: Do you have recollections of how the war affected your family?

ALBEE: We were not that affected because my father was just over the age for military service, so he didn't get called up. We were affected only in the sense of rationing and that sort of thing.

ASPATURIAN: What are the family roots? Where does your family come from originally, to the extent that you know?

ALBEE: I actually looked it up not very long ago. A Benjamin Albee came to Massachusetts from England in 1650. And he got burned out of his flourmill in the King Philip's War [also known as the First Indian War, this was a mid-18th century conflict in New England that pitted Native Americans against a coalition of British colonists and their Native American allies –*Ed.*]. I just found that out a few days ago. It was the start of the King Philip's War, and his older son got killed by the Indians. [AA subsequently added: All eight great-grandfathers came from Great Britain, one as a Scottish rebel prisoner.]

ASPATURIAN: How did you first become interested in science? I assume it was in high school or maybe earlier.

ALBEE: We did a lot of traveling as a family, so I got interested in rocks way, way, way back, and had a museum in my house.

ASPATURIAN: You had a rock collection?

ALBEE: I had some, yes. And miscellaneous things like bird nests and whatever.

ASPATURIAN: Do you recall what first prompted your interest in rocks?

ALBEE: I think it was just traveling and seeing them, because we went west three times when I was growing up. We went west in the days when it was quite an adventure. I had an aunt on my father's side who had moved north of Calgary, Alberta, to marry an uncle on my mother's side who had homesteaded there. So, from Michigan, that was a long way. We went directly across the country, through the Dakotas and Montana, to Yellowstone and Glacier. Up into Canada and so on.

ASPATURIAN: Did you know by the time you were in high school that geology was what—

ALBEE: No. I had no preconceived ideas. I thought of being a lawyer. I don't know why; I didn't know any lawyers. Maybe that's why I thought about it. [Laughter]

ASPATURIAN: I imagine you were a good student.

ALBEE: Yes.

ASPATURIAN: And you went to Harvard as an undergraduate. How did that come about?

ALBEE: I had offers from Harvard and Yale. This was at the time when they, quote, "made them identical." And from the University of Michigan.

ASPATURIAN: Of course.

ALBEE: So, I had to decide basically between Harvard and Yale because they both gave support, and of course we didn't have that kind of money. I didn't have any basis for choosing; I'd never been to either one. I didn't know anything about either one.

ASPATURIAN: You'd never been east?

ALBEE: I'd never been east at that time; we'd always gone west. And I didn't have a clue. I *say*—I don't know whether it was true or not—that I chose Harvard because the sportswriter for our local newspaper was from Yale and let everybody know it.

ASPATURIAN: I see. [Laughter]

ALBEE: And so, I chose to go to Harvard. That's what I say.

ASPATURIAN: You majored in geology at Harvard. What drew you into that field, if you recall?

ALBEE: Well, you had to take various distribution classes. And so, in my first year, I had Geology, and I had History 1, and the contrast was pretty amazing. But I think the reason I finally chose to major in geology is that I saw that everybody else was not planning to get something out of their four years that they could make into a career. They were all going to go to business school or law school or med school, etc. And I couldn't see how I was going to afford to do anything like that. Yes, I liked geology—the introductory course was very, very good. But I think I made that decision because I knew that when I got out in four years, I would be able to get a job.

ASPATURIAN: But you didn't. You went on for the PhD.

ALBEE: [Laughter] I went on for the PhD. Right. I stayed on for a master's, and of course then I was flirting with the draft and everything.

ASPATURIAN: The Korean War?

ALBEE: The Korean War. So, after getting my master's, I went to work for the U.S. Geological Survey [USGS] in Vermont, working on critical elements like talcum powder and asbestos.

ASPATURIAN: Naturally occurring critical elements?

ALBEE: Oh, yeah, we were looking at the supply of them there. Looking at the supplies of talc for Navy paint. And asbestos for fire-fighting suits. So, I did not get drafted because of that. I worked with USGS for what must have been three, four years.

ASPATURIAN: Doing this kind of research?

ALBEE: Right, doing fieldwork. And then I got a National Science Foundation [NSF] scholarship. I saw a little clipping in the Sunday *New York Times* about a new program, so I wrote away. My letter finally made it to the right place, and I got the information, and so I went back to Harvard, with a wife.

ASPATURIAN: On an NSF scholarship. What made you decide you were going to go back for a PhD?

ALBEE: Well, basically it was because I was already working on a potential thesis. I had a quadrangle that I was mapping.

ASPATURIAN: And where was that?

ALBEE: In Vermont. It was part of our USGS work, but I knew it could become a thesis. And Jim [James Burleigh] Thompson at Harvard had lots of students in Vermont and New Hampshire doing quadrangles at that time. So, it was logical to go back to Harvard.

ASPATURIAN: Do you recall what the title of your thesis was? Subject matter, generally? I can put this on pause momentarily.

ALBEE: I can get it. [Pause; recording resumes.]

ASPATURIAN: "Geology of the Hyde Park Quadrangle Vermont, 1957." It comes to 150 pages, with what look like a great many field maps and notes. So, this was your Harvard PhD?

ALBEE: Yes. I went back to Harvard for one more year to pass my orals and all that jazz and then left with an incomplete thesis.

ASPATURIAN: An ABD [All But Dissertation].

ALBEE: Right. I had actually completed the fieldwork, but it wasn't written up and so on. And so, I went back to the USGS, and they sent me to Colorado. I looked for uranium in the Front Range of Colorado and mapped the Precambrian rocks in the core of the range.

ASPATURIAN: The uranium was for defense purposes?

ALBEE: Yes. It was during the uranium rush in the Cold War. I spent a year in Colorado and worked nights on my thesis. Then I got transferred to Maine with the Survey and was given my own project in Maine.

ASPATURIAN: Which was, do you recall?

ALBEE: The Attean Quadrangle. The Survey was trying to find deposits of zinc, and so they were doing a systematic search in some regions. Many new ore deposits at that time had been discovered in Africa, where there had been very large concessions of land [i.e., contractual agreements to develop natural resources], and where geologists had gone in using modern techniques. So, the idea was that since the paper companies had had great concessions in Maine, there were these great swaths of unknown territory to explore. So that's what we were working on.

ASPATURIAN: So, all of this related in some way to what you might call commercial use?

ALBEE: Well, one branch of the Geological Survey is mineral deposits. That's the branch I happened to be in.

ASPATURIAN: How did you end up at Caltech, which happened quite quickly after that?

ALBEE: I was sitting in the office in Beltsville, Maryland, in the winter.

ASPATURIAN: A USGS office?

ALBEE: Yes. Got a phone call from Bob [Robert P.] Sharp [Robert P. Sharp Professor of Geology, emeritus, d. 2004].

ASPATURIAN: The then-chair of the GPS [Geological and Planetary Sciences] Division.

ALBEE: And the voice started out, "Gee, I'm sitting here looking at the mountains and the sun; is it raining there?" [Laughter]

ASPATURIAN: You knew him? Had you met him?

ALBEE: No, no. This is out of the blue.

ASPATURIAN: He cold called you?

ALBEE: He cold called me. They had gotten a recommendation from [Marland P.] Billings, who was one of my Harvard thesis advisors. They were looking for someone to fill in during a sabbatical. So, they invited me out to give a talk. Which I did. Then I stayed for a year and then stayed further. Ian Campbell was the one who was on sabbatical. He was taking a sabbatical to be head of the California Division of Mines, and when he stayed on as head of the division, I stayed in that position.

ASPATURIAN: Was this your first time in California?

ALBEE: Yes, as a matter of fact.

ASPATURIAN: What do you recall the division as being like when you first arrived? You were very young.

ALBEE: All the divisions were very different then. At Harvard and then at Caltech, basically there was a chairman and one secretary, and not much other administrative—whatever you want to call it.

ASPATURIAN: Not much bureaucracy.

ALBEE: Right, right. There were some secretaries to type manuscripts, but not much else. Not many paid employees or many postdocs. It was faculty and students.

ASPATURIAN: Did you decide you wanted to stay, or did they make you an offer? Were there other offers?

ALBEE: Both, I guess. There were no other offers at that time, because I sort of wasn't in the marketplace.

ASPATURIAN: Did you continue working for USGS when you joined Caltech's faculty?

ALBEE: I was working part-time for the USGS, finishing up some manuscripts and some petrological work and so on.

ASPATURIAN: What was your primary research focus when you started; and also, what was the division doing at that time in terms of research, if you recall?

ALBEE: The division at that time had just begun going into isotopic work. And so, the Chicago influx had been, I guess, the year before. [This is a reference to a cutting-edge group of geochemists that R. Sharp recruited to Caltech from the University of Chicago in the 1950s. *–Ed.*]

ASPATURIAN: Oh, OK. So, Sam [Samuel] Epstein [Leonhard Professor of Geochemistry, emeritus, d. 2001]?

ALBEE: It could have been two years before. So [Clair C.] Patterson [professor of geochemistry, emeritus, d. 1995] and [Gerald J.] Wasserburg [MacArthur Professor of Geology and Geophysics, emeritus, d. 2016] and—

ASPATURIAN: Harrison Brown, I think.

ALBEE: Harrison Brown and Lee [Leon T.] Silver [Keck Foundation Professor for Resource Geology, emeritus] and [Heinz A.] Lowenstam [professor of paleoecology, emeritus, d. 1993]. That had happened under Bob Sharp's tenure just prior to my coming.

ASPATURIAN: I think Lowenstam referred to them as the "Chicago Mafia" when they came in. And what were you working on?

ALBEE: My thesis was mostly on regional mapping.

ASPATURIAN: In New England?

ALBEE: Yes. I was interested in what we call petrology, which is understanding rocks. In particular, at that time, there was great interest in using the partition of elements between coexisting minerals as they grow in rocks as monitors of pressure and temperature. This technique, of course, has grown. It's standard now.

ASPATURIAN: But it was new then.

ALBEE: But it was new then. And in particular, when I came to Caltech for the year—I guess Sharp must have told me that they had a chemist here—I brought three samples from a particular area in one of the quadrangles I had mapped in Vermont. Not the thesis quadrangle, one to the south. And those three were part of a petrologic package, if you will. And so, I did mineral separations—tediously by hand, with magnetic separation—and got chemical analysis of each of the minerals in these three samples. So that's how I got into that kind of work. That's how I got into the electron microprobe; and then how I got into lunar work; and then how I got into Mars. [Laughter] It was because I was working on the detailed chemistry of the individual minerals that coexist with one another.

ASPATURIAN: I see. And this was far more quantitative, I think, than any work that had been done in this area previously.

ALBEE: Yes. Before then, you could use optical techniques: You'd look at thin sections and use optical techniques to see what they were, but you couldn't do accurate compositions.

ASPATURIAN: What were the early results that came out of this for you in terms of papers?

ALBEE: Well, you know, I had quite a number of publications on this, with students and so on.

ASPATURIAN: What do you recall about Bob Sharp in those days? I've read and still hear quite a lot about him.

ALBEE: I knew Bob for many, many years. I fished with him. Went to Yellowstone with him and Clarence Allen [professor of geology and geophysics, emeritus]. He was an amazing man. He had amazing discipline. When he was division chair, we'd go out to the desert, and he'd sit by the back of his car and write. When we went somewhere on a fieldtrip, he would send postcards to every one of the secretaries. He was just an amazingly focused man but very friendly and concerned about everybody.

ASPATURIAN: He obviously had an eye for talent too, because of the way he rebuilt the division in the 1960s and late fifties.

ALBEE: In my case, JPL [Caltech/NASA Jet Propulsion Laboratory] wanted to get an electron microprobe, and somehow or other Bob persuaded them that they should house it on campus so that it could be used more widely. So, we had one of the very early electron microprobes, and I was put in charge of it.

ASPATURIAN: Where was that technology developed?

ALBEE: It was developed in a number of places around the world; but the first commercial one had just become available from Applied Research Labs, which was out in Sunland, actually. And so that was the start. One of the first things that we did with it was to investigate the so-called purple plague. The gold wires JPL was using in its spacecraft were turning purple and affected conductivity.

ASPATURIAN: What era are we talking about? This must have been a little later.

ALBEE: This was in the early 1960s. So, the initial reason for getting the probe was to try to solve the purple plague problem. And it turned out that they were using gold chloride to make these wires. They used detergent in these dies that forced the gold out to make wires, and it then turned purple and decreased the conductivity, which they didn't want.

So, we worked on some of these early JPL things, but then I started working on the metamorphic rocks and had students doing that.

ASPATURIAN: I have a note here that you were in charge of the electron microprobe chemical analysis facility and that you and a student published a major paper in 1968 [A. E. Bence and A. L. Albee, "Empirical Correction Factors for the Electron Microanalysis of Silicates and Oxides," *The Journal of Geology* 76, no. 4 (July, 1968)].

ALBEE: Right. Ted [A. Edward "Ted" Bence] was actually a postdoc, but we evolved a new technique for doing the analysis. Let me go back a minute, OK?

ASPATURIAN: Sure, please.

ALBEE: The ARL instrument [electron microprobe] worked very much like an X-ray tube: You have an electron beam that excites X-rays off a target. When you have a sample that you want to analyze, an electron beam can focus the beam down to a tiny, tiny spot. So, we could analyze a 1-micron spot using this technique.

ASPATURIAN: Which I'm sure was unprecedented.

ALBEE: Right. And then you set an X-ray diffractometer to a particular wavelength of X-rays. This machine was built with three diffractometers so you could set each of them independently and analyze for three elements—say, iron, magnesium, and manganese. You would lay out a whole day of points for these three elements, on a polished thin section, and you would get data on each of them. Next day you'd reset the spectrometers and get three more elements; the next day, go the same route again and get three more elements—probably nine elements in all, typically. Our output at that time was on-

punch-paper tape— and we went through a long process of upgrading it. I had a mechanical calculator, which had one storage element, so you could reduce data with that, but eventually we converted the paper tape to punch-paper cards and used the IBM 360.

So, this was a laborious, laborious process. From the time you started it, you were lucky if it was only two weeks before you saw the analysis and knew whether it was any good. There was a very complicated way of calculating the corrections for the analysis; it had been developed in England by a group of physicists. So, Ted Bence and I, in this paper you mentioned, evolved a linear approximation and regression of the composition to allow you to make a correction using a small computer, and so on. And much faster. I wrote about that, I think, in *E&S* [*Engineering & Science*, Caltech's research magazine, 1937–2017: Arden Albee, "Rocks—Micron by Micron, *E&S*, November, 1967; http://calteches.library.caltech.edu/2594/1/albee.pdf]. So, in '71, we bought a new probe. It had a separate PDP–8/L computer, which controlled it, and it ran with a high-level language, FOCAL. So, with this, you could sit on a spot and write a program that allowed you to drive the spectrometers, and you could do all the elements with their backgrounds under computer control and get the results out. Then because we had this technique, Ted and I were finally able to get the full analysis before you left the spot. You'd sit there for a minute or so, and you would have a complete analysis.

ASPATURIAN: So, you were working in real-time, for the first time?

ALBEE: In real time. And I'll come back to this, because the same transition happened when you want to talk about the early Mars missions. [*See Session Two*] The transition from Viking to Mars Observer and Mars Global Surveyor was very much like the transition from that first probe to the later one.

ASPATURIAN: It must have been very interesting for you, going from this very laborious hand procedure ten years earlier.

ALBEE: Oh, incredible, yeah, yeah! So, when the lunar sampler program came along— of course, Jerry Wasserburg had been involved in the planning of that for a long time—I

was one of the few labs in the country that had a going electron microprobe. And so, I was funded for the lunar sample program.

ASPATURIAN: You had the technology and the experience to deal with the moon rocks.

ALBEE: Right.

ASPATURIAN: I want to go back to one thing before I forget. In the mid-1960s, you had the seafloor spreading and continental drift theory suddenly becoming widely accepted. Do you recall your reaction, or how that affected your colleagues in the division? That was a real game-changer in many respects.

ALBEE: To a certain extent, there were not that many people working in that area at Caltech. There were a lot of people working on isotopes and more detailed things. But when Kerry Sieh [professor of geology, 1968–2009] came in, he was an advocate for this. He regarded anyone who didn't think it was the greatest thing since sliced bread as feeble-minded. Which I think got antagonism. [Laughter] So, it didn't have as big an impact. It perhaps had more influence eventually in the seismo [seismology] group than in our group, where we didn't have people doing that kind of work.

ASPATURIAN: You mention Jerry Wasserburg. Did you have other colleagues that you collaborated with closely in the 1960s and seventies?

ALBEE: Jerry was the one I collaborated with. We worked together in Death Valley very early. Jerry was a pretty good field geologist, despite all his other skills. He was a good field geologist; he was a good observer; and so, we worked together. He was collecting samples for age dating. As was Lee Silver.

ASPATURIAN: This was in Death Valley?

ALBEE: A lot of it was. I was mapping a region in Death Valley with a student—again it was an outshoot of the work I had done on coexisting phases. We did it over a period of a number of years.

ASPATURIAN: What were you looking at specifically there?

ALBEE: Well, we mapped and published on the Telescope Peak Quadrangle—the lowest point and the highest point. Plus, there were some interesting mineral assemblages in it. It was a totally different kind of metamorphosis than in Vermont.

ASPATURIAN: Yes, I can imagine.

ALBEE: So, I have a number of papers on contrasts between low-pressure, high-temperature and high-pressure kinds of metamorphism. The Appalachians are very different than what we were looking at in California. Jerry and I actually took our families out to Death Valley with us, and I guess you know that Jerry and I worked in Greenland together.

ASPATURIAN: Yes, I was going to get to that.

ALBEE: So, we had our families out there one Thanksgiving, sitting around the fire, talking about the new results on rocks from Greenland. Wasserburg said, "Gee, we should go there. And I said, "OK, let's do it." So, from that conversation around the campfire, we ended up with the Greenland expedition.

ASPATURIAN: What was Jerry like to work with? You hear such mixed reports about him as an individual.

ALBEE: I think I'm the only person who loved working with Jerry. He was honest. What I found was that he respected you for what you did. He might question you about it, but he respected what you did. [*See Session Three Addendum*] So, Jerry and I worked on an awful lot of papers together.

ASPATURIAN: The two of you went to Greenland for something called the Oldstone Project.

ALBEE: Right. Some of the samples from that are on display at the Smithsonian.

ASPATURIAN: So, you were in search of Earth's oldest rocks?

ALBEE: There was a young geologist who lived in Greenland and was doing geology there. He had developed a geological argument that indicated he had rocks that were probably the oldest ones known. A British team had gone out there and collected and dated them, and indeed, they were the oldest rocks known at that time.

ASPATURIAN: About 3.5 billion years, I think.

ALBEE: Right. And so, we went up to Greenland to collect. And what Jerry wanted particularly to do was to get the evolution of lead, which Patterson had worked on.

ASPATURIAN: Clair Patterson. This is the isotopic dating.

ALBEE: Right. We wanted to get the oldest rocks and see how our analysis meshed with Patterson's results. Now outboard motor oil was badly contaminated with lead, and so you had to be sure you collected samples well away from the shore. [Laughter] We took blasting equipment to get fresh rock.

ASPATURIAN: How did you live there?

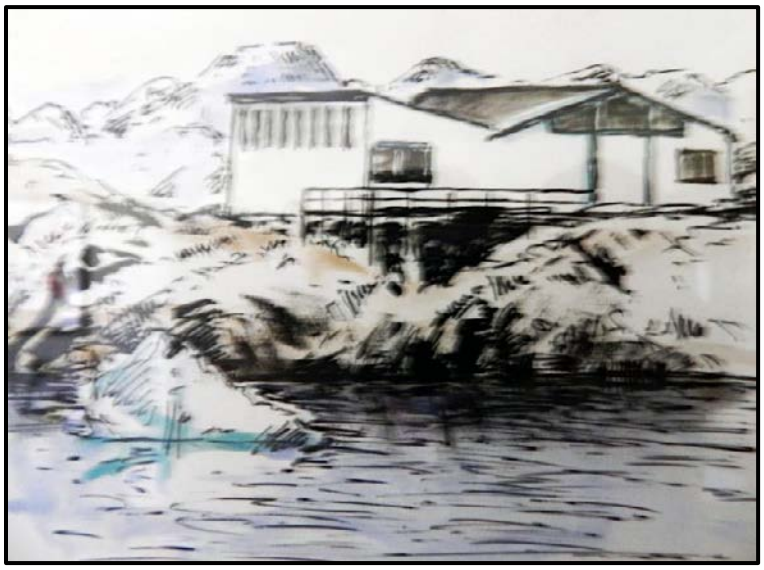
ALBEE: We lived—I'll show you a picture.

ASPATURIAN: And I'll put this on pause. [*Recording is paused; then resumes.]*

ASPATURIAN: OK, you were saying you rented—

ALBEE: We had leased or rented an RCRV [Regional Class Research Vessel], which was an old coastal vessel with a captain, who owned it, and one crewmember. We had one of my sons, one of Jerry's sons, two graduate students, and one undergrad. That was our team. And we kept our gear in a building that was owned by the man. These pictures were done by a local artist—a friend of the captain, actually.

Old Stone Ship



Old Stone Cabin

ASPATURIAN: So, you lived on the boat?

ALBEE: We lived on the boat. Bunks in it.

ASPATURIAN: How did you get to shore? Did you have little dinghies?

ALBEE: We towed a hard boat, and then we had some dinghies.

ASPATURIAN: What time of year was this?

ALBEE: It was in summer.

ASPATURIAN: How many times did you go, and how long were you there each time?

ALBEE: Well, Jerry and I went once, for about a month, but two of our students continued going and did part of their PhDs up there.

ASPATURIAN: I have a note here—twenty tons of ancient gneisses, brought back to Caltech for geochemical analysis.

ALBEE: [Laughter] And remember, we were packing them out.

ASPATURIAN: Were you involved in the chemical analysis of this, or did those go to your colleagues?

ALBEE: It's the same pattern we followed with the lunar rocks. I did the petrology, the probe work on the samples, and the characterization of the samples, but I didn't do that isotope work. I sometimes joke that I had my name on almost every isotope that's ever been looked at, but I've never used a mass spectrometer. [Laughter]

ASPATURIAN: That's the way these papers sometimes work, right?

ALBEE: Right.

ASPATURIAN: So, we have a number of years of you working on terrestrial geology, and then how did you get involved in the lunar geology? That happened before you went to Greenland. Different planet now.

ALBEE: Basically, NASA set out to recruit those people whom they thought had the best equipment and knowledge and who would be willing to work on lunar samples.

ASPATURIAN: I see, I see.

ALBEE: They had a team of geologists. Yes, you made a proposal, but you sort of knew you'd already been accepted, OK? And you knew that with NASA funding for this research, you could upgrade your labs. So, if you didn't have really good funding support or something that was really your burning desire to work on, you probably signed up to do some lunar work.

ASPATURIAN: It also was very exciting, wasn't it?

ALBEE: Of course. You're trying to see what you can learn about another planet from a little tiny piece of rock. It was an exciting time.

ASPATURIAN: It seems to have coincided with the division's gradually moving into planetary science as well.

ALBEE: The interesting thing, though, is that the division had a very funny split in that those people who viewed themselves as planetary scientists didn't view people who worked on lunar samples as planetary scientists.

ASPATURIAN: Are we talking now about the late 1960s, early seventies?

ALBEE: Right, and even beyond that. I mean I didn't become professor of geology *and planetary science* until many, many years later [1999; *see also Session Three*], and Jerry

never did. No one else did. It was a separate part of the division: It was Harrison Brown and Bruce Murray [professor of planetary science; JPL director, 1976–1982; d. 2013].

ASPATURIAN: And later Peter Goldreich [DuBridge Professor of Astrophysics and Planetary Physics, emeritus]?

ALBEE: Peter Goldreich and so on. So, they did a different brand of planetary science.

ASPATURIAN: You were in on the ground floor very early with regard to looking at lunar samples, I think.

ALBEE: Yes. Starting with Apollo 11.

ASPATURIAN: Did you work with Jerry Wasserburg and his Lunatic Asylum on this?

ALBEE: Again, just the same thing I mentioned when we worked in Death Valley. I actually did not officially receive lunar samples. I worked on the samples that were issued to Jerry for isotopic work, but everyone that he spent thousands and thousands of dollars to do the isotopic work on, we characterized to the extent that they could be characterized. So, we did all the petrology, chemical analyses—everything else on them. So many of the papers about them were joint papers, and others were separate papers, but every lunar rock that the Wasserburg Lunatic Asylum worked on, we characterized.

ASPATURIAN: When you say "we," I have here that you worked with a colleague named Arthur Chodos?

ALBEE: Artie Chodos was a long-term Caltech lab employee in geology. Way, way back in the early days before the probe, he did flame emission spectroscopy. You burn a sample and look at the flame to identify and characterize the elements in it. But Artie was eager to get into the probe research, and so when we got the probe from JPL, he was the one who put it together and worked with it and so on. I guess I could say I was the idea person, and he did all of the work.

ASPATURIAN: Theory and application, working together.

ALBEE: You'll find paper after paper where we're coauthors. He was not a coauthor in the sense that he wrote any of the papers.

ASPATURIAN: But, of course, that's very common. What were some of the key findings to come out of your lunar sample analyses?

ALBEE: One is that they're plain ordinary basalts. There was no hint of water in them. You look at terrestrial rock, and you see a little bit of alteration here and there, no matter how pristine it is, because when it was cooling it had water. And so, in a sense the lunar samples are not that different from what you see here, but you can use them to get information, to understand the geologic history, and so on. Now the lunar breccias, of course, are broken-up rock and they have embedded glasses, and it's a whole other set of problems. But, again, no hint of hydrous alteration.

ASPATURIAN: I have a note here that you found evidence of ancient meteorite strikes in some of these tiny crystals. "Isolation of meteorite particles in lunar samples."

ALBEE: Yes, in the soil samples.

ASPATURIAN: Do you recall how you felt working with this? I mean, you'd had a decade or more doing terrestrial geology.

ALBEE: Well, it was always exciting because you never knew what the next sample would be like. And every mission was different. A lot of it tied in with the isotopic work that Jerry and his group were doing. The lunar rock crystalized over a long period of time, and when that happened, the major elements that go into feldspar and pyroxene got concentrated there, and everything else got left. So, you finally ended up with these basalts with interstices in which the elements are concentrated.

The potassium rubidium strontium isotopic history dominates the lunar soil in many ways, and it dominates it with dates that date back to 3.6 [billion years]. And this

is because you had the concentration in these basalts that got spread out all over. And we had one sample, 12013, which had a huge concentration of those kinds of elements, all the rare-earth elements—uranium, thorium, so on.

ASPATURIAN: This might be what the *New York Times* article I found from 1974 was about? Oldest lunar samples—it quotes you; it quotes Jerry Wasserburg.

ALBEE: No, that was a different one. That was about a dunite fragment inside the breccias. Dunite on Earth is made predominately of olivine. When we analyzed that lunar dunite, in addition to the olivine, it had little grains in the interstices that then gave us the age of 4.6 billion.

ASPATURIAN: Do you recall how you felt when you realized this? The oldest thing in the solar system!

ALBEE: It always comes together kind of slowly, you know.

ASPATURIAN: I know. It's not like in a movie.

ALBEE: But we also, in the 12013 sample that I mentioned, found a single grain of zircon, which we were able to date with the probe to 4.6 billion. This was after the dunite dating—you never knew what you would find around the corner.

ASPATURIAN: You became chair of a lunar research panel in the early 1970s as well.

ALBEE: Right, the Lunar Sample Research Panel.

ASPATURIAN: This was a NASA panel?

ALBEE: Right. Toward the end of the Apollo program, when they were beginning to reorganize, they changed its name to the Lunar Science Review Panel. This panel presided over the funding of all the PIs [principal investigators] working on moon-related

projects. First, it was all the PIs on samples, and the panel pretty much controlled the allocation of samples, but then when we brought in the photography and the remote-sensing people and so on, they were included too. I guess I chaired it for about six years.

ASPATURIAN: And so, into the late 1970s.

ALBEE: And in the process I got put onto—now which one was that? Here, I got it—Terrestrial Bodies Science Working Group. Physical Science Committee. Space Science Advisory Committee—that was ’78 to ’80.

ASPATURIAN: That’s right around the time you went up to JPL.

ALBEE: Right. Then I got onto Mars ’84 Science Working Group. Then the Mars Science Working Group. The Mars Science Steering Group. Mars Study Program. [Laughter]

ASPATURIAN: All Mars, all the time.

ALBEE: So, I got into Mars from the back end. Partly because NASA was interested in doing a Martian sample program.

ASPATURIAN: Same sort of thing. Before I forget, did you have any involvement in training the astronauts to do geology on the moon?

ALBEE: No, no.

ASPATURIAN: I know a couple of your colleagues did; Lee Silver was one of them.

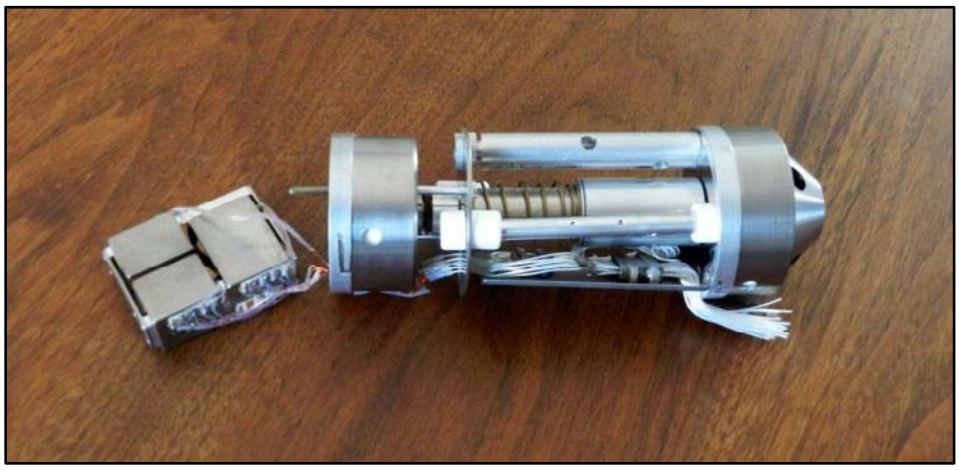
ALBEE: Lee did. One of the two gentlemen I worked with in Vermont also became a major player; Al [Alfred] Chidester was his name. He was one of the main trainers.

ASPATURIAN: So, by the mid-1970s, had your emphasis kind of moved over into what we would think of today as planetary studies?

ALBEE: No, I was still doing both. In a sense, I never did much actual research in Mars science. I worked on instruments, and I chaired committees, etc., but I never called myself one of the Mars researchers—partly because it was a very different environment. I came from the sample world, and I liked being able to get facts. Now where I did do a lot of work on both Mars and Earth was in remote sensing. When I was chief scientist at JPL, I worked on all kinds of remote-sensing projects, including one where I was trying to catch samples from a comet as it passed by. I never really got that one going there but let me show you. How's that?

ASPATURIAN: So, what am I looking at here?

ALBEE: This is a scanning electron microscope to go on a spacecraft to analyze comet dust. And because a spacecraft doesn't like magnetic fields, this one was electrostatic rather than electromagnetic. And because you can't have an electrode that burns out, it has two electrodes, so you can use either one or the other.



Scanning electron microscope

ASPATURIAN: This must have been one of the prototypes? Since it's still here and not up there.

ALBEE: This is the one we got accepted for a comet mission, which was a European mission, but then NASA decided not to fund it.

ASPATURIAN: So, you were left not with a mission but with a prototype. You were also the academic officer for geology for most of the 1970s; did that impact—

ALBEE: More than that, I think.

ASPATURIAN: I have ’71 through ’79.

ALBEE: I guess that’s right. Because when I left JPL, I came back to campus to be Dean [of Graduate Studies, 1984–2000].

ASPATURIAN: Did anything particularly interesting that you recall happen in geology under your watch in that decade, in that context?

ALBEE: Obviously, it was the start of things getting more complicated in terms of administration.

ASPATURIAN: Now Bruce Murray became head of JPL, I believe in 1976, and two years later, you went up as chief scientist.

ALBEE: Robbie [Rochus] Vogt [Avery Distinguished Service Professor and Professor of Physics, emeritus; Caltech provost, 1983–1987] was chief scientist.

ASPATURIAN: That’s right; the first one. He lasted a very short time, though.

ALBEE: Yes, he was there for a short time; and then he left to go, I think, to the PMA division. [Vogt chaired Caltech’s Division of Physics, Mathematics, and Astronomy from 1978 to 1983. *–Ed.*]

ASPATURIAN: So how did your appointment come about?

ALBEE: Out of the blue, as far as that’s concerned. I’d never had a sabbatical. I’d actually arranged to basically take a sabbatical at JPL, partly because of family. I had

four kids and so on. But Bruce, out of the blue, asked me if I'd become chief scientist there.

ASPATURIAN: Had you and Bruce worked together at all? Were you good friends?

ALBEE: No. We actually had carpooled for a while—we both lived in Hastings Ranch. I couldn't say we were close friends.

ASPATURIAN: OK. It came out of the blue, and then what happened?

ALBEE: I became chief scientist. [Laughter] Totally different atmosphere. The first thing I found out was that if I made a casual remark, somebody went off and tried to do something about it.

ASPATURIAN: What did you find when you got into this position? Robbie Vogt was there for such a short time that it occurred to me that basically you kind of defined the job.

ALBEE: The one thing Robbie had started was a program to give some of the senior scientists at JPL special recognition. He had it in mind, or hoped to be able, to give them some financial support independent of missions.

ASPATURIAN: I read part of Bruce Murray's oral history while getting ready to talk to you, and he talks quite a bit about how he had to do a lot of modernizing and shaking up in the wake of [William] Pickering's retirement as JPL director [1954–1976]. What do you recall about this period?

ALBEE: The thing is, Bruce and the lab were in a desperate hunt for another big mission.

ASPATURIAN: This is the late 1970s we're talking about: The Apollo program has ended.

ALBEE: And Viking [1970s Mars missions]. So, they were in a desperate hunt because they were afraid if they didn't get another big mission, how would they support the expertise at JPL and so on. Bruce had a competition to come up with great ideas for projects that would occupy the whole lab.

ASPATURIAN: Visionary projects for the lab?

ALBEE: Yes. And I had gotten involved with a totally different approach at NASA headquarters, which is that we began to look at smaller missions and to look at using commercial spacecraft builders, etc. And so, Bruce and I never saw eye to eye on this. Bruce was never a fan of Mars Observer. In all the time I spent fighting for Mars Observer, Bruce felt that the project would kill the lab.

ASPATURIAN: What did emerge on the front burner in your first couple of years there?

ALBEE: Nothing. There was a big gap in there.

ASPATURIAN: So then what was your portfolio? What were you doing?

ALBEE: Well, lots and lots of things. I sat in on a lot of committees. Fred Felberg and [Charles] Terhune were managers of the lab, and Bruce must have insisted, sort of to my amazement, that they needed a scientist on those kinds of committees. So, I did get involved in a lot of lab operations and how they worked and so on. Learned more than I ever wanted to know, probably. [Laughter] On the other hand, I guess I became an administrator in the process. I wrote the JPL annual report. *That's* one of things that's changed: Now they have a whole big organization to write it. But I did the annual report for the five or six years I was there. I was involved in a lot of personnel committees, trying to choose people and so on. And I was in Washington a lot on these Mars committees.

ASPATURIAN: Did you do anything with regard to expanding relationships between the lab researchers and campus researchers? Was there much of that going on at that time?

ALBEE: There was a fair amount; not as much as a few years later. Yes, we were working on it, but let's face it—money is what makes that happen. This was also a time when Bruce was doing everything he could, to figure out who would fund new lab projects. He was playing with defense, and he was playing with transportation ideas and agencies and so on. And that ended up in some really bad ideas: Bruce wanted to go for a classified mission

ASPATURIAN: Was that the Arroyo Center business [reference is to an army-funded Caltech research center that was proposed but not implemented in the 1970s]?

ALBEE: Yes.

ASPATURIAN: Were you involved in that at all?

ALBEE: I was not involved.

ASPATURIAN: What did you think of that?

ALBEE: I kept uninvolved. [Laughter]

ASPATURIAN: You kept your distance. So, was it under his tenure, though, and working with you that Mars emerged as the next big ongoing project?

ALBEE: No, that was after.

ASPATURIAN: When Lew Allen [JPL director, 1982–1990] came in?

ALBEE: Right. I was spending an awful lot of my time on Mars planning. I led Mars planning at the lab; I led Mars planning at NASA headquarters in Washington.

ASPATURIAN: How did that come about? We should talk about that.

ALBEE: As I said, it sort of happened with going from being chair of that review committee to the space science board, and then I was made representative for terrestrial bodies—for all the missions to the terrestrial bodies. And so, I went up on the Washington side to where I became responsible for an awful lot of Mars planning.

ASPATURIAN: It was sort of an administrative progression that brought you into the science.

ALBEE: Right. [Laughter]

ASPATURIAN: Do you feel that you had any particular accomplishments working at JPL with Bruce Murray?

ALBEE: I think I got to know JPL. It became part of what I did as project scientist for subsequent Mars missions and so on. But as to whether I made different things happen—I had big parties in my backyard, which are well-remembered, apparently. We had a summer faculty program, where we chose faculty from around the country to come and work at the lab in the summer. There was a summer intern program for postdocs and students that I did get going, and so on. They still go on at some level today.

ASPATURIAN: In ’82, Dr. Allen came in. Do you recall how things changed; what went on with him?

ALBEE: I’ll give you an example of the change. When Bruce was there, the guys would come in with a proposal or recommendation, and they’d give their spiels, and then they’d wait for Bruce to make a decision. When Lew Allen took over, had his first meeting—

ASPATURIAN: General Allen. [A four-star general, Allen served as chief of staff of the U.S. Air Force and subsequently as head of the National Security Agency before becoming director of JPL. *–Ed.*].

ALBEE: General Allen. They came in, gave their pitch and waited for a decision. And he said, "What's your recommendation?" I learned that Lew Allen had run research labs. And he was a totally different kind of a person from Bruce. Bruce thought he knew everything. Bruce was a [Donald] Trump, almost. He knew the answer before you gave him the information. And with Lew Allen, it was very different. It very quickly made a difference in the atmosphere in the lab, I thought.

ASPATURIAN: How would you say—aside from having an obviously different personality and management style—that the lab changed under Lew Allen when he came in; and how did that affect what you were doing there?

ALBEE: Well, I think, it obviously got younger: Younger ideas and younger people with digital experience began to come in. We're now coming to that time when there's a digital revolution beginning. The Voyager spacecraft each had one computer, and every instrument on them had to beg for a little bit of help from the computer. So, there are all these instruments out here begging for help. You'll see when we talk about Mars Observer [*Session Two*] that we designed it differently. Every instrument had a computer, as well as the spacecraft. By then you had computers on a chip, and networking had evolved to where you could have a network on a spacecraft. Now that approach was coming into the lab at the time Lew Allen came in because the younger people were coming in. But of course, they had no spacecraft to design, and nothing funded.

ASPATURIAN: So, money was a big concern.

ALBEE: Oh, yeah; that's why Bruce was wildly trying to get JPL involved in defense and civil projects.

ASPATURIAN: Did it become easier once Lew Allen took over? Was there a more successful approach to—

ALBEE: Well, Bruce got to be at odds with headquarters, too.

ASPATURIAN: With NASA?

ALBEE: Yes, they really butted heads a lot. And Lew Allen was a different kind of person.

ASPATURIAN: I have a note here that just around the time you arrived at JPL, Voyager 1 was having its Saturn encounter. Was there any ramping up at that point for outer planetary exploration beyond Voyagers 1 and 2?

ALBEE: No, because nothing was funded. Voyager was kind of it. We kept turning out proposals and so on, but nothing was making it through the grid work.

ASPATURIAN: What would you say, looking back, gave you the most satisfaction out of your five years at JPL?

ALBEE: I guess work on Mars.

ASPATURIAN: What exactly were you doing with Mars during that period, aside from all these committees?

ALBEE: Well, that's mostly what it was. These are design committees. Crazy things. "Mars Balls" —a group of French scientists had the idea of tumbling around Mars in balloons. We had wheeled rovers—every kind of rover you could think of.

ASPATURIAN: All these little models.

ALBEE: All these little models, and the ideas for them had started way back when. Yes, they were committee meetings, but what we were doing was reviewing ideas. They actually made a full-size model wheel too.

ASPATURIAN: What segments of the academic community were serving on these committees? I assume you had geologists, planetary scientists, astronomers.

ALBEE: What sort of happened, by the time we got into—oh, when did I leave?

ASPATURIAN: JPL? 1984.

ALBEE: By about ’76 Viking had ended, and there were a large number of geologists who had worked on Viking and had gotten to know instrumentation. They knew that in their labs they had computers on a chip, and that computationally things were very, very different than what JPL had done with previous missions. They also were frustrated by what they saw as JPL’s size. Everything came out of the central computer on the spacecraft and went to a central place at JPL where it got printed up and then finally got distributed. You couldn’t get your own data. They knew you could do better. These were the people, including a number of Bruce’s and my former students, who were on these preliminary study committees, trying to generate new ideas. So, there were new ideas on instruments, new ideas on detectors, new ideas on wheels and how you land an instrument package on Mars, and so on. Almost all the techniques you see the Mars program using now, we looked at and assessed.

ASPATURIAN: So, all these technologies are rooted in this period?

ALBEE: Right.

ASPATURIAN: Sounds like a very fertile period.

ALBEE: But it all came to naught in the sense that you couldn’t sell these ideas until we finally sold Mars Observer. That was a big, big sales job, selling Mars Observer.

ASPATURIAN: Why don’t we talk about that next time.

ALBEE: OK.

ARDEN ALBEE

SESSION 2

August 31, 2017

Arden Albee in his office with a model of the Mars Observer, 1992

ALBEE: First, let me make a small correction. You asked me [*Session One*] about where the microprobes we purchased had been developed, and I said Europe and other places. I actually made a mistake there in the sense that, yes, microprobes were developed in several places, but the ARL that we purchased was based on a design by Dave [David] Wittry. He was a graduate student in electrical engineering at Caltech, and he became a professor at USC. And so that ARL machine was based on his Caltech thesis. The second one we got was a MAC, which was Material Analysis Company, and it was a Stanford professor who did all the design for it. His name was Vic [Victor] Macres.

ASPATURIAN: These were the two instruments that you started using in the late 1960s?

ALBEE: Right.

ASPATURIAN: I wanted to talk today about the culture and politics and the scientific sociology that went into the two big Mars missions you were involved in, Observer and [Mars Global] Surveyor. I also wanted to say that I listened to your and Glenn Cunningham's '93 and '97 interviews with Larry Mantle on *AirTalk* [a news and commentary interview program produced by NPR Pasadena affiliate KPCC].

ALBEE: No telling what we said. [Laughter]

ASPATURIAN: They were very interesting. And I thought we'd link to both of them within the oral history, and I might bring them up a little later in context. [1993 *AirTalk* interview: https://archive.org/details/capsca_000089; 1997 *AirTalk* interview: https://archive.org/details/capsca_000119] When we left off last time, you had started to talk about the selling process that went into Mars Observer with NASA.

ALBEE: At the time I said that the NASA Mars committees were going in different directions than Bruce. Bruce had this idea about doing ambitious JPL missions, which he called "purple pigeons," and he used to taunt me that the thing about the purple pigeons is that they were salable; they would attract the masses. So, for example, I remember that one of the purple pigeons was a dual Mars rover so that one rover could take pictures of the other, because pictures were what fascinated the public. This was Bruce. As far as he was concerned, Mars Observer was a "gray mouse" that couldn't stand up to the purple pigeon. In his way of thinking, JPL needed a purple pigeon to continue to survive.

ASPATURIAN: But then Bruce left, and Lew Allen came in. [S*ee also Session One*]

ALBEE: Right, right. During that whole process, I had gotten involved with what I'll call a movement of younger people, many of whom who had worked on Viking. They had their own labs; they knew what was happening in the digital world; and they were fed up with the centralization of data from Viking, because the Viking had a single computer—a single data storage, etc., etc. So, it was all closely held in the project. But on all these

NASA advisory committees, you'll see this younger generation trying to think of something that will get us back into the space game and in particular into Mars. There was pressure from industry to use industrial vehicles, so that got all folded into it, and that's why Mars Observer and MGS were industrial vehicles.

ASPATURIAN: MGS being Mars Global Surveyor.

ALBEE: Right. This was a totally different mode than what JPL had been doing, so in a sense, JPL never really accepted Mars Observer or MGS. It saw them as just something that was happening off at the fringes. Viking and Voyager basically had only a single computer, but all of the young people who were chosen to work on Mars Observer were accustomed to working with microprocessors. So, on Mars Observer and MGS, each instrument had an 8086 processor that had some 30,000 transistors on it. It was unthought of a very short time before.

ASPATURIAN: How did you come to be project scientist on Mars Observer?

ALBEE: Well, I'd been working on all these committees. I led the terrestrial bodies committee; I led the Mars studies committee and so on. So basically, I can almost say I made myself project scientist. When I quit being JPL chief scientist and went back to campus—

ASPATURIAN: In 1984.

ALBEE: —Then I made myself project scientist. [Laughter]

ASPATURIAN: Were Mars Observer and the Global Surveyor kind of planned alongside each other?

ALBEE: No. It was a tumultuous back and forth with Mars Observer. It was on again, off again. It was on the shuttle; it was off the shuttle. It had a free ride on the shuttle; it had no ride on the shuttle. Then there was the shuttle accident.

ASPATURIAN: *Challenger*. 1986.

ALBEE: *Challenger*. So, Mars Observer got postponed. I'm trying to remember—it went through about four companies. The companies got bought up; we went through a lot of company changes as we went on. We had continual pressure from industry, particularly Hughes Aircraft, who wanted their terrestrial vehicle designs to be used. Mars Observer and MGS were orbital missions, very much like the terrestrial ones. That's why in doing the bidding process, we went out to industry. The idea was that this would save lots of money: It would be a cheap mission, and therefore we'd get it funded. Which we finally did.

ASPATURIAN: How did you feel about that approach?

ALBEE: I was totally in line.

ASPATURIAN: You were in favor of it. You thought it was a good idea?

ALBEE: Because MGS came out of the failure of Mars Observer. So, it basically had the same objectives. Put it into orbit at Mars and have a lot of instruments that would survey the entire surface of the planet. MGS carried through both of those objectives after the failure of Mars Observer.

ASPATURIAN: Who are the key people you worked with on the Observer mission? I know Glenn Cunningham must have been one of them.

ALBEE: Glenn was more important on MGS. We had a couple of different managers on Mars Observer. I can't even remember—I'd have to look them up. But out of the Mars Observer failure, Glenn had emerged as the leader, and then he was the leader for MGS. Most of our team was the same for MGS and Mars Observer.

ASPATURIAN: You said that JPL never really thought of Mars Observer as its mission. How did that affect your ability to guide the mission as project scientist? Were you

largely working from campus? Were you spending most of your time at NASA facilities?

ALBEE: Sort of half-and-half, because during most of that time, I was also Dean [of Graduate Studies].

ASPATURIAN: Yes. That's a separate topic. [S*ee Session Three*]

ALBEE: Right. [Laughter] So, it was sort of half and half or maybe half, half, and half. I was also teaching still.

ASPATURIAN: What were some of the novel aspects of Mars Observer?

ALBEE: The most novel aspect was the distributed computing system. Each instrument was responsible for itself, and so they each had their own computing power. They had their own storage. And there was a bus, which connected all the instruments, plus all the controls and so on, to the spacecraft computer, which then put all the data inputs together and radioed the data back. Now, one of the real problems was that JPL had never allowed scientists that amount of control in operating their instruments. Every command at Voyager and Viking was scrutinized by a group of people for fear that some mistake would sink the spacecraft. But our instruments operated separately from the spacecraft, and as long as the protocols were carried out, the instruments could not damage the spacecraft. So, this was a total change in how you build a spacecraft.

ASPATURIAN: Did you encounter much resistance to some of these innovations that were being introduced?

ALBEE: Well, Mike [Michael] Malin probably got the worst of it, because JPL saw the spacecraft cameras as being theirs.

ASPATURIAN: They'd had done their own cameras always in the past?

ALBEE: On Voyager and Viking and so on, yes. And so, there was an ardent group who thought this was a terrible mistake. They were very much concerned over whether the camera would work. Initially they had basically deleted the camera in the budget process, but it was put back on the mission at NASA headquarters.

ASPATURIAN: So, when you say "they," you mean JPL?

ALBEE: Yes. In terms of the budget process, they just did not support Mike's camera, which was totally different from anything that had ever been built before. It was always called a push broom. In other words, we were using the motion of the spacecraft—we had a line array of sensors; and the motion of the spacecraft then made a picture out of it. This technique had been used in some terrestrial missions, but nothing like this had ever been used in space. The camera had more compute-power and storage-power than all the missions JPL had ever flown before. I think you'll find me quoted somewhere to that effect. But at any rate, it was a revolution in what we were doing on board.

ASPATURIAN: Who brought Mike Malin onto the project? Was that you?

ALBEE: There was a competition—a process of selecting instruments for the payload—and Mike was one of the active people on all these committees we talked about. Now we kept a tally of the total electrical and compute needs and so on of the instruments. They each had to have a low, medium, and high rate.

ASPATURIAN: Of operation?

ALBEE: Of operation. So that they could not trespass upon the needs of the spacecraft.

ASPATURIAN: I see.

ALBEE: So, the rate was partially dependent upon where we were orbitally. This way the PIs could operate their instruments without having to fight overpower with other instruments in the spacecraft and so on. Again, that was a huge transformation. It

became even more so in MGS when we took off two of the heavier instruments and then made it very clear that this would be a mapping mission. For example, the camera didn't have a shutter. It recorded twenty-four hours a day for the whole mission—almost ten years. And so those things were just totally different.

I think it was on Observer that we went to the laser altimeter, because the radar altimeter was heavy. JPL had always used radar altimeters, and of course they didn't particularly like that change either. But Goddard [NASA's Goddard Space Flight Center] had developed a laser altimeter and tested it terrestrially, and the man who was the PI for the radar altimeter that JPL was building was also the PI on the Goddard laser altimeter. And when the radar altimeter got into money problems, we finally changed to the laser altimeter—Goddard put in development money and support to keep it.

ASPATURIAN: Now, Lew Allen was the head of JPL through most of this?

ALBEE: Presumably [Laughter].

ASPATURIAN: That was going to be my next question, as to whether you got any particular support from him.

ALBEE: I don't recall. I mean, he was supportive of everything. He was a very, almost unemotional—not without emotion, but he didn't seem to have particular projects he wanted to get behind.

ASPATURIAN: I gather that there were some unexpected cost overruns on Observer and some other issues not related really to the science.

ALBEE: That's right. First, because of this switching from shuttle to the expendable launch vehicle and back again to the shuttle, and so on. Then we missed an opportunity because of the shuttle problem, so the schedule extended out over a much greater time. So, there were indeed cost overruns, but they were mostly due to circumstances.

ASPATURIAN: For you, as project scientist, did you see this mostly as an exhilarating experience? Was some of it frustrating, given what was occurring?

ALBEE: Well, it was both. We were still working to get that global picture of Mars.

ASPATURIAN: This was the first return to Mars since Viking, I believe. We should establish that.

ALBEE: It's almost twenty years since Viking. That was the whole point of the work all these committees had done—to get back to Mars.

ASPATURIAN: I was going to get into when Observer was lost, but is there anything else in connection with the mission you'd like to talk about?

ALBEE: There's sort of a great story with the camera.

ASPATURIAN: This is Mike Malin's camera?

ALBEE: Yes. Quality control at JPL had had absolute kittens about the camera, because it was being built by a bunch of college students in the basement.

ASPATURIAN: Mike Malin was still a graduate student at that time?

ALBEE: No, he had worked at JPL since graduating, and he was at Arizona State when he won the camera competition. At some point, he got the MacArthur Prize [MacArthur Foundation "Genius" Fellowship], and he left Arizona and set up his own company. Of course, that caused JPL to have kittens, too; "Here's this kid with a company thinking he's going to build a camera." And one of the funny stories is that its memories were built on chips that came from, if you will, a Teddy Bear.

ASPATURIAN: The memory chips for the Mars Observer camera were built on chips—

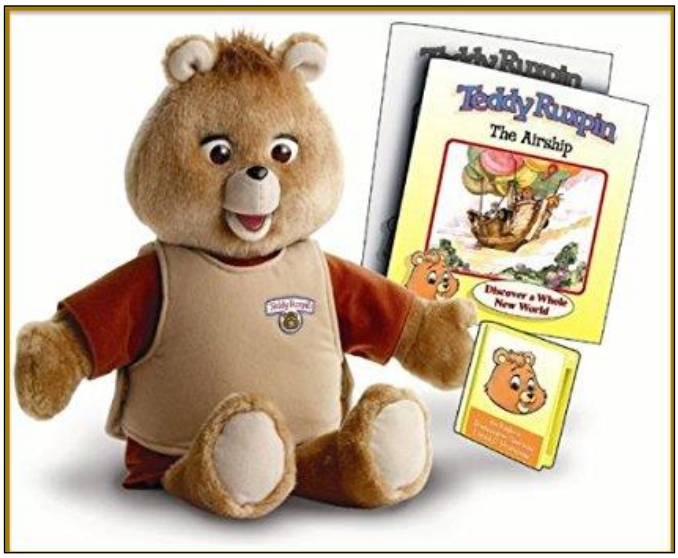
ALBEE: That were designed for this stuffed Teddy Bear [the audio animatronic *Teddy Ruxpin*, originally manufactured and sold by Hasbro –*Ed.*]. Now they didn't actually go in and rape the Teddy Bear to get the chips out. They were able to buy them from the company that had been making the bears because the company had stopped making them.

ASPATURIAN: Who came up with this idea, and why, do you know?

ALBEE: Well, because the chips they needed were unavailable otherwise. This was a particular chip, and they had to get enough of them to do all the testing that JPL was demanding and so on.

ASPATURIAN: I don't think that's a story that's been widely—

ALBEE: Well, that's why I said you ought to interview Mike Malin. [Laughter]



ASPATURIAN: So, the camera was equipped with chips from a Teddy Bear.

ALBEE: Then JPL insisted that they had to build a whole second side for this camera.

ASPATURIAN: A second side?

ALBEE: A whole additional set of backup electronics. So, there was an A-side and a B-side. When we went on to MGS, we never turned on the B-side. So, for ten years, that A- side that they were so concerned about operated.

ASPATURIAN: The B-side was also run on Teddy Bear chips?

ALBEE: Oh, yes. Another thing was that JPL had always built two of each mission spacecraft.

ASPATURIAN: Yes, that's right, a certain redundancy.

ALBEE: To safeguard your investment. What we came up with, from the committee and so on, was that when they'd order the parts and certain assemblies, we would also order backup parts and have them partially assembled and tested, in case we lost a spacecraft. So, when we did lose Mars Observer, we had parts for both another spacecraft and another set of instruments. [On August 21, 1993, three days before Observer was scheduled to go into Mars orbit, all contact with the spacecraft was lost. It remains unclear what happened to it. *–Ed.*]

Now after we lost Mars Observer, industry forced a rebid, again going through Washington. So, there was a rebidding process from industry to propose the spacecraft for MGS. And at that time, Charles had just come up—

ASPATURIAN: This is Charles Elachi [professor of electrical engineering and planetary science, emeritus; JPL director, 2001–2016] you're talking about?

ALBEE: He was not director yet. But he did this whole process.

ASPATURIAN: What capacity? What was he?

ALBEE: I think he was head of space projects [director of space and earth science programs] at the time. But at any rate, he took a personal interest in the mission and so on, and in getting MGS going again.

ASPATURIAN: I'll get to that in more detail in a minute. You referred to college kids building the Observer camera in a basement; was that an exaggeration?

ALBEE: [Laughter] That's a little bit of overstatement—

ASPATURIAN: But not too much?

ALBEE: Right. I mean Mike was a recent graduate. He quit Arizona State, set up his own company early, and so his team were all young.

ASPATURIAN: When you say, "JPL was having kittens." Was that also NASA headquarters? Who was it?

ALBEE: Some people were from headquarters. Some were from JPL. It was a new way of doing things. And this whole use of the network of onboard computers and so on was also a total change. All brand new. But when you look at it, it was exactly like a MAC microprobe. You took a MAC—a thing where you turned dials and so on—and you put on little motors, and you drove them with computers and took data, so everything worked off a network.

ASPATURIAN: Given all the—I don't know if "setbacks" is the right word—but given all the ins and outs and unexpected occurrences with regard to getting Observer successfully launched, did you have any concerns or misgivings about its ability to function at the time?

ALBEE: No; and we still don't quite know what happened. It involved some command or something at the time that we were supposed to go into orbit.

ASPATURIAN: Do you recall when you heard about this—how you felt, what you thought?

ALBEE: Well, there was a press conference, and someone asked me that question; and I said I went home and pulled ivy, which only Californians would understand.

ASPATURIAN: It must have been tough.

ALBEE: It was. You know, again, thanks to Charles, we recouped and re-advertised for spacecraft and made a new choice on spacecraft. Then with our new budget, we ended up taking two of the heaviest and most expensive instruments off. They eventually flew on other Mars missions, but they did not go on MGS.

ASPATURIAN: I have a quote here from your 1993 *AirTalk* interview with Larry Mantle, where you called the loss of Mars Observer a devastating setback for science. Then four years later, you and Glenn Cunningham are back, and Surveyor is already returning data; and Pathfinder is coming up. It was very interesting to hear these two segments back to back.

ALBEE: Well, at that time after Observer, of course, we didn't know whether we'd get a rerun. So, it was indeed devastating.

ASPATURIAN: I'm sure it was.

ALBEE: Even today, we still know the surface of Mars topography better than we know Earth.

ASPATURIAN: I remember you said that on *AirTalk*.

ALBEE: That's a direct quote. Because there are so many oceans on Earth, we just simply know topography better for Mars.

ASPATURIAN: What kind of soul-searching and investigation was there in the wake of Mars Observer?

ALBEE: There was a failure board, and it produced a great thick manual. And then there were a couple of devastating things out of the economics department at Caltech, which I did not think were fair.

ASPATURIAN: What happened there?

ALBEE: Well, there was a graduate student who was studying the failure, and he took, I thought, a very jaundiced view of it.

ASPATURIAN: What was the transition from the loss of Mars Observer to ramping up for Global Surveyor like? Was it difficult to convince NASA and JPL that it was worth trying again?

ALBEE: The thing was we had to turn it around between the time of the loss [August 1993] and the spring budget; we had to get all these things through, or we'd lose an opportunity. So that's why, as I said, Charles was just incredibly important. We had to go out to industry for competitive bidding, and we had to reselect the PIs, and all of these things had to be carried out and a budget approved by NASA by—it must have been May or something.

ASPATURIAN: You and Glenn Cunningham were asked to stay on the project?

ALBEE: All of our leaders stayed on. All the PIs stayed on, except two who fell off in the budget process.

ASPATURIAN: With the instruments that were taken off Observer and not restored on Surveyor?

ALBEE: Right. And in the process, MGS turned out to have advantages over Observer.

ASPATURIAN: Yes, would you like to talk about those? One of the things I think is interesting from a historical perspective is how seasoned scientists rebounded from a catastrophe like this and went on to achieve a large success four years later.

ALBEE: All of the PIs had their electronic parts. They varied; some of them were already pretty well-assembled. So, they just dug in and started working. They were already funded, so they could start almost immediately.

ASPATURIAN: How hard did you have to work to convince NASA that this was worth trying again?

ALBEE: It seems to me it was easier than the first time in that I think people wanted to be seen as bouncing back from a failure. But, again, a lot of that was due to Charles's work. He was well respected at headquarters. And I think that helped—it put JPL behind it.

ASPATURIAN: Did the two of you work fairly closely together on certain aspects of it?

ALBEE: Well, Charles worked very closely with all of the projects. Glenn and Charles were the ones—

ASPATURIAN: Who really shepherded it through?

ALBEE: Right.

ASPATURIAN: Ed [Edward] Stone [Morrisroe Professor of Physics; JPL director, 1991–2001], I think, became head of JPL right around this time as well. What changed at the lab in that regard with his coming on board? Do you have any recollections?

ALBEE: No, because Ed was very, very much involved with Voyager, and these other missions. Yes, he was supportive; he understood; he listened; and so on. But I wouldn't say it was his prime focus.

ASPATURIAN: I see. So, as you began planning for Surveyor, what happened to Observer must have been in the forefront of everybody's thinking.

ALBEE: In a sense we never knew. It could have been as simple as a mis-command. We don't actually know whether it sailed on past Mars or crashed.

ASPATURIAN: Dan Goldin was quoted in *Newsweek* around the time of the Pathfinder mission as triumphantly telling a group of NASA or JPL people that the loss of Observer

was the greatest thing that ever happened to the Mars program. Are you familiar with this?

ALBEE: I don't think so.

ASPATURIAN: Because now, he said, a new way of looking at missions could commence. I just wondered what your thoughts were about that.

ALBEE: Well, he was a funny— [Laughter]

ASPATURIAN: Did you know him?

ALBEE: Yes. He was sort of Trump-ish. He was on-again, off-again. You never quite knew what to make of him. He was the one who had—what were the three things?

ASPATURIAN: "Faster, better, cheaper."

ALBEE: And everybody said you could only do two of the three.

ASPATURIAN: Pick two?

ALBEE: Pick two.

ASPATURIAN: But I imagine that must have somewhat informed your planning with regard to Surveyor.

ALBEE: Yes, that's why two instruments went off; it was a lighter spacecraft we chose—a simpler spacecraft that had to have fewer moving parts. So, I guess he can at least take credit for that. [Laughter]

ASPATURIAN: Did you find working on Surveyor a more enjoyable experience than Observer?

ALBEE: I don't honestly know. It didn't seem that different, I guess.

ASPATURIAN: What were the six instruments that went on Surveyor?

ALBEE: The camera, the thermal emission spectrometer, the laser altimeter, the magnetometer.

ASPATURIAN: Right, that one was very important.

ALBEE: What am I missing?

ASPATURIAN: I can look it up. [Ultra-stable oscillator; Mars relay.] Which two were thrown off?

ALBEE: An atmospheric instrument [pressure modulator infrared radiometer]; partly that went off because the thermal emissions spectrometer could do many of the same things in a different way, so we did not lose climate; and then—the one that required a big boom and cooling of the sensor. I can think of it. I can see it [gamma-ray spectrometer].

ASPATURIAN: It'll come back to you. One of the things that Glenn Cunningham told Larry Mantle was that Surveyor was on track to do about 80 percent of Observer's projects at roughly a quarter of the cost. Is that an accurate assessment?

ALBEE: Um-hum. A quarter of the cost. We were inheriting stuff that had already been built.

ASPATURIAN: That's what he said; that a lot of that money had already gone into Observer, and MGS was reaping the benefits.

ALBEE: That's right. We were recovering a portion of it, which had been planned with foresight in case you lost the mission. If that happened, you had a lot of backup parts, and everything had already been purchased and tested.

ASPATURIAN: Did you have students of yours or postdocs of yours working—

ALBEE: No.

ASPATURIAN: So, it was independent from that.

ALBEE: But almost every member of the team was pretty young and a lot of them had been students at Caltech or students of students from Caltech. Phil [Philip] Christensen was the student of a Viking PI who was a Caltech-er. It was a fairly small group. Then the Goddard team was an excellent team, and Dave [David] Smith and Maria Zuber were an incredible addition to that team.

ASPATURIAN: Had Goddard been the industry point team on Observer as well?

ALBEE: Well, they had built that instrument [laser altimeter]. And then the magnetometer was also out of Goddard. Now the magnetometer on Observer had a long boom. We didn't put any booms on MGS, but the magnetometer was situated on the outer side of the solar panels.

ASPATURIAN: Who built the spacecraft?

ALBEE: Martin Marietta.

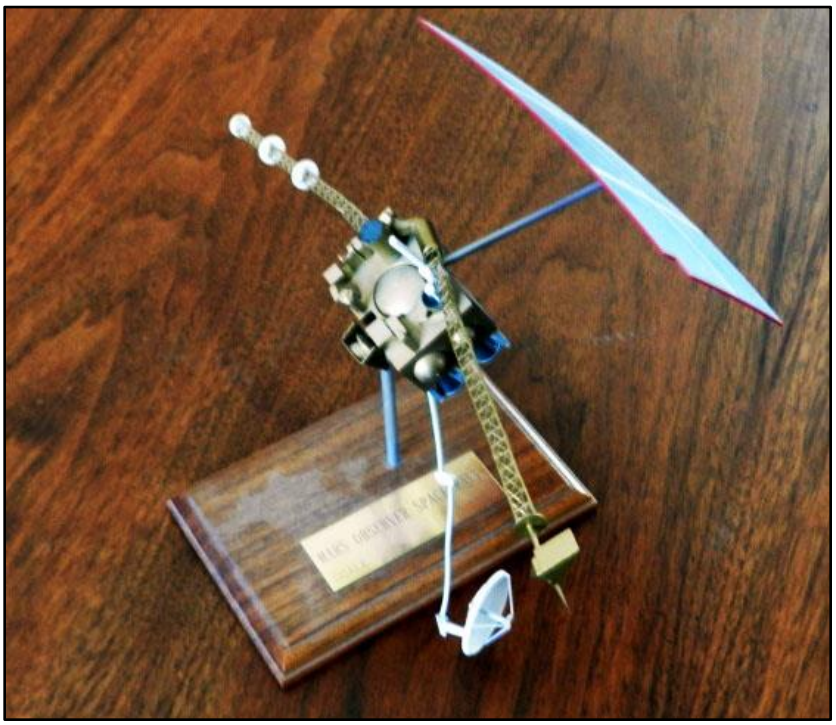
ASPATURIAN: Had they been commissioned to do it the previous time as well?

ALBEE: Correct. They were finally chosen after another industry competition. Partly because they had the parts.

ASPATURIAN: And I guess the know-how, because they'd done it before.

ALBEE: Turn that off for a minute.

ASPATURIAN: I'll put it on pause. [Recording is paused; then resumes.] We're looking here at a model of the Observer.



Model of the Mars Observer

ALBEE: You can see there's one boom out here for the magnetometer and one boom way out here for the pressure modulator infrared radiometer.

ASPATURIAN: It's a bit more cumbersome than the little model of the Surveyor I see sitting behind it.

ALBEE: And then here is the antenna, which is articulated so it points to Earth, and here is a solar panel.

ASPATURIAN: It's a larger, much heavier-looking piece of—

ALBEE: Larger, much heavier, and had the extra instruments on it.

ASPATURIAN: And here we have Surveyor. These are the solar panels?

Model of Mars Global Surveyor

ALBEE: These are the solar panels; MGS was always kept facing the sun. The magnetometer was out here on this little projection.

ASPATURIAN: At the edge of the solar panels. It looks like a more streamlined design in many respects.

ALBEE: And then the antenna, again, is articulated; so, it was always pointing to Earth. So as the spacecraft went around, the planet spun under it: There were no moving parts in the instruments, which had been one of our goals. They were basically on all the time, but you couldn't collect and transmit data all the time.

ASPATURIAN: Did this design kind of emerge out of the planning for the mission, or was it established early on that you wanted it to look like this?

ALBEE: We had learned from Observer. We knew what the instruments were like, and this was a spacecraft designed to fit that set of instruments.

ASPATURIAN: In retrospect, do you think it would have been better to go with a design like this to start with?

ALBEE: Probably, but it wouldn't have accommodated all the instruments.

ASPATURIAN: I see. One of the reasons you were able to do this is—

ALBEE: We took off the two heaviest and most expensive.

ASPATURIAN: I have a note here that you had a launch window in '94, but decided to pass on that. Why?

ALBEE: Probably we weren't ready. I just don't remember.

ASPATURIAN: Maybe after what happened with Observer, you wanted to be absolutely sure.

ALBEE: Probably.

ASPATURIAN: So, do you recall your feelings as the time for Mars insertion with the spacecraft approached?

ALBEE: Well, obviously, you're on edge. And we did have a nail-biting time after we got into orbit; remember, we had to use aerobraking.

ASPATURIAN: I've been reading about that.

ALBEE: Less propulsion and so on. We were using the solar panels for aerobraking, which again was a new idea, and the hinge on one of the panels got damaged as it opened out, and so we couldn't go into orbit as fast. We wanted to achieve circular orbit. We ended up—

ASPATURIAN: Was it more elliptical?

ALBEE: Yes, and I'll show you. We were going to use the aerobraking to come on down real quickly and go into a circular orbit. When this problem with the hinge happened, we slowed up the aerobraking, and came down more slowly so we wouldn't break the "wing." And so, we eased down in an elliptical orbit. It took almost half a year before we got into the circular orbit way down here.

ASPATURIAN: It took longer than you had originally envisioned.

ALBEE: Yes. But it tremendously increased the value of the mission.

ASPATURIAN: How so?

ALBEE: Because in the elliptical orbit, you get lower.

ASPATURIAN: Ah-ha.

ALBEE: So, for example, one of the major discoveries the magnetometer made was these magnetic anomalies. And we would have gotten very little information about them had we gone immediately into our circular orbit.

ASPATURIAN: How come you didn't decide then to stay in an elliptical orbit?

ALBEE: We wanted the circular orbit for the mapping—for the systematic mapping, which was the primary objective.

ASPATURIAN: Got it.

ALBEE: So these were, if you will, secondary objectives; and during this period, we got a lot of atmospheric information because we could use the deceleration through the atmosphere [i.e., the aerobraking] to measure the density of the atmosphere and so on. And because the strength of the magnetic fields falls off with distance, when you went down close you got much higher resolution on them. And fields and particles—it was

like getting two missions in one. There was actually an elliptical mission proposed in competition with Mars Observer, and so by good luck we actually got all of this as add-ons to the objectives of MSG's circular mission.

ASPATURIAN: Were you working at all with the Pathfinder team? Obviously at some level there was a certain complementarity—but was there a conscious complementarity to your efforts?

ALBEE: We were getting data for them, of course; and a lot of the Pathfinder design came from this same set of committees. Remember that crazy little rover I showed you? So, the same people worked in the same groups, but we didn't have joint meetings. They ended up on our review boards; we ended up on their review boards.

ASPATURIAN: I understand. Going back to this interview with Larry Mantle in '97 shortly after Surveyor went into its elliptical orbit, you and Glenn Cunningham sounded so different from how you had sounded four years earlier. Just the tone and the bounce in your voices. What was the feeling once it became pretty clear that this one was going to succeed?

ALBEE: Oh, obviously, great, yes. And, of course, we lasted ten years, short a week.

ASPATURIAN: What do you think were the major findings to come out of Surveyor? What surprised you the most?

ALBEE: Well, I had been very strong in keeping the magnetometer on. Even when we moved it to the end of the solar panels. My reason was that people looking at the history of Mars had said, "Here are all these volcanoes and so forth, way back in early Martian history. There were these great catastrophes back in the planet's early history, and then it became sort of dead"—that was the brief look at it, if you will. And the question was then, if the core had solidified, magnetic fields would have stopped, and so we could have portions of the planet that were old and portions of the planet that were young. And so, having the magnetometer looking for magnetic anomalies and demonstrating that these

anomalies were in the older portion of the planet, gave us a date, if you will, for the cooling of the planetary interior and the collapse of the global magnetic field.

ASPATURIAN: I see; when things began to shut down.

ALBEE: Around 3.7 billion years or so ago. That was the important finding to come out of this—that the magnetic anomalies were restricted to the old terrains, which by comparison to the moon dating, we could date to roughly 3.7.

ASPATURIAN: What else gave you the most satisfaction to discover at that time?

ALBEE: Well, the systematic mapping photography.

ASPATURIAN: I remember some of those images.

ALBEE: The thermal emission spectrometer's systematic mapping of the atmosphere of the weather—of clouds, of dust storms—doing all of that over a ten-year period produced a history that is invaluable. And if you're planning to do aerobraking at Mars, you need to know when the storms are coming and what they do to the atmosphere. So, this gave us the basic atlas of information on Mars.

ASPATURIAN: And the first one as well.

ALBEE: Right.

ASPATURIAN: Any other thoughts on Surveyor?

ALBEE: Well, I thought it was an incredibly great mission. If you look at my article in *Scientific American*, you'll get a late, late summary.

ASPATURIAN: I think that's behind a pay wall.

ALBEE: I can give you one.

ASPATURIAN: I'd like a copy. I did read your article about Surveyor in *E&S* ["Mars Global Surveyor: A Success by Any Measure": http://calteches.library.caltech.edu/4035/1/Mars.pdf].

ALBEE: But in that *Scientific American* one, I include global images from all the instruments, which is an atlas for the planet for the first time. That's what the achievement was—to give an atlas for the planet, in terms of surface, in terms of weather, in terms of clouds, all of these things.

ASPATURIAN: Following the success of Pathfinder and Surveyor, there were a couple of very sad flameouts again [Miles Climate Orbiter and Mars Polar Lander]. Do you have any thoughts on what led to those?

ALBEE: I just wasn't involved. You can only do so many things. [Laughter]

ASPATURIAN: I was wondering, were you working regular forty-hour weeks on these projects?

ALBEE: Well, I sometimes said that I was half-time on this, half-time dean, and half-time teaching. I had students, and I was teaching classes.

ASPATURIAN: Anything else?

ALBEE: I don't think so; not unless you want to go to other things.

ASPATURIAN: I think I'd like to reserve that for our third session.

ARDEN ALBEE

SESSION 3

SEPTEMBER 14, 2017

ASPATURIAN: Today we're going to talk about your tenure as Dean of Graduate Studies and your very extensive role over the years at the Athenaeum. So, my first question here is what led you to take on this dean's job in 1984?

ALBEE: When I was chief scientist at JPL, I got to working quite closely with Fred Felberg, who was an assistant laboratory director. For whatever reason, he had put the chief scientist onto his organizational committees. So, I learned a lot about the mechanics of the lab in that process. At any rate, when I went back to campus, Fred had been Chair of the Athenaeum House Committee for I don't know how long. And he was about to retire, and so he thought, "Ah-ha." [Laughter] So, that's how I got in there.

ASPATURIAN: How about the Dean of Graduate Studies?

ALBEE: I can't remember exactly whether I knew I was coming back to campus to be dean.

ASPATURIAN: Apparently, the previous dean, Neal Pings, had left, and for the next two or three years, Jim [James J.] Morgan [Goldberger Professor of Environmental Engineering, emeritus], the V.P. of Student Affairs, was also the acting graduate dean.

ALBEE: Right. And I think Jim did that knowing I was coming back to campus. I think it must have been the fall of '84.

ASPATURIAN: Do you recall how they persuaded you to do this?

ALBEE: I don't know. [Laughter] I guess I'm easy to persuade. I had been academic advisor for geology. In fact, I had invented that term. As things got more complicated, almost all the divisions got an academic officer. At the time I got here, Bob Sharp was

doing everything with one secretary, and he had gradually given me some assignments with respect to advising and so on. Pretty soon I was doing almost all the student stuff; and finally, he said, "We should formalize this," and I became the GPS division's academic officer. I think I was the first academic officer on campus. And it was a way that the chair could shift off a whole area.

ASPATURIAN: So, you felt becoming graduate dean was kind of a natural progression from that?

ALBEE: I think so.

ASPATURIAN: Do you recall what kind of challenges you were confronted with? You were in that job for a long time.

ALBEE: Sixteen years, I think. Well, unlike all the other major schools that we compete against, we don't have a graduate school. We have a vertical structure. As a result, the treatment of graduate students differed greatly from division to division—in terms of support for the students, in terms of admissions policies. Everything. So basically, the first thing I tried to do was get those policies unified—to get the graduate office to function as the graduate school that we don't have. There were many, many things—questionnaires or polls where you rank people, all these things—that were falling through the cracks because we didn't have a graduate school. So that's what I tried for some time. Then, when I started going to meetings of graduate school deans, it almost immediately became very, very clear that there was a strong focus on recruiting minority graduate students.

ASPATURIAN: This was in the mid to late 1980s.

ALBEE: Which Caltech was doing nothing about. And so, I began working very much on that aspect. Then we had the problem with an influx of foreign graduate students, and so we set up a system in India where we simply told the Indian applicants that they had to have an interview with one of our people—former students or former faculty—which cut

down the flow tremendously. Then, because of the increasing number of our foreign graduate student applicants, I went on the graduate study board for the GRE [Graduate Record Exam] and on the board for the testing of English as another language. Because one of the problems we were having at Caltech——and other schools were having the same problem—was getting students here who really couldn't hack it linguistically.

ASPATURIAN: Since Caltech has six divisions, and each division is kind of its own territory, what sorts of challenges did you face in trying to unify the approach to graduate students?

ALBEE: There were very, very different patterns of support and so on. At the time, there were almost no benefits for graduate students.

ASPATURIAN: You mean financial benefits?

ALBEE: No, like health, spousal benefits, all those things.

ASPATURIAN: Nothing?

ALBEE: There was almost nothing. At that time, benefits were basically offered at the whim of the faculty member. And so, I was basically trying to make the grad office like a graduate school. For example, we don't have a department of economics, so our graduate economics program never got ranked. Well, if you went to enough effort—listing all the theses, all the faculty members and their research and so on—we could very quickly get that done, and I got us ranked second or third in the country. That was just a question of something falling through the cracks. Nobody was protecting Caltech as a graduate institution.

ASPATURIAN: Did you get more support and buy-in from some divisions than others as you attempted to do this?

ALBEE: Chemistry was sort of the hardest. Chemistry and biology. They had graduate teaching assistants because they had big classes and in the jostling for who got how many TAs [teaching assistants] from the Institute, chemistry had done very well. But they were using them in ways that other divisions couldn't. For example, all new chemistry graduate students had to be a TA for Chem 1, and they didn't do anything else the first year. They actually sort of took the class. So, there were a lot of inequalities of that kind across the divisions.

ASPATURIAN: When you came in, Jim Morgan was the VP, and he was succeeded by Gary Lorden [professor of mathematics, emeritus]. What were they like to work with for you?

ALBEE: A little bit different. Morgan was hands-off. Lorden was more hands-on about budgets and so on.

ASPATURIAN: Who else did you work with? I think Jeanne Noda was Gary's assistant vice president for a while. There were several different deans of students, and Lyman Bonner and then Judy [Judith] Goodstein [faculty associate in history; University Archivist, emeritus] was the registrar.

ALBEE: One of the things that we did do very early in my career as dean was that I hired two young ladies just out of high school and secretarial school to assist in the office. Rosa is now at City of Hope, and I'm drawing a blank on the second name.

ASPATURIAN: Natalie [Natalie Gilmore, Assistant Dean of Graduate Studies]?

ALBEE: Natalie is still there. She still is basically running the graduate office. What's left of it. But we put in a computer system for all the graduate students and put as much data into it as we could find from the old cards at the Registrar's Office and so on to build up a database on the graduate school. This was long before the campus-wide automation that Caltech went through, and so we had very difficult problems because of a lack of continuity between systems and so on.

ASPATURIAN: How much dealing did you have directly with the graduate students and the Graduate Student Council [GSC]?

ALBEE: A lot. Because I very actively promoted the graduate student council, and I actually married a couple of the graduate students.

ASPATURIAN: Literally?

ALBEE: Yes. I got one of these—

ASPATURIAN: Credentials?

ALBEE: Yes. It doesn't matter what their names are; they were two very active members of the GSC; they had both been its chairs. The young lady was Jewish. Her parents were very Orthodox Jewish, and the young man was a Christian Texan. They wanted to get married, and they figured they couldn't satisfy either of their families, so they asked me to marry them. I said, "I can't do that." They said, "Yes, you can. We sent away and got you a—"

ASPATURIAN: Are you kidding me?

ALBEE: [Laughter] So subsequently I married a couple of other couples.

ASPATURIAN: You officiated.

ALBEE: I officiated, yes.

ASPATURIAN: This was a real period of transition, I think. You mentioned the underrepresented minorities and new influx of foreign students.

ALBEE: And the continued growth in the number of women at the same time.

ASPATURIAN: That was the next thing I was going to say.

ALBEE: Somehow that got handled better than the minorities. I think that we received applications from women and saw they were good, and we just couldn't even get applications from minorities.

ASPATURIAN: Did you have a sense of why that was?

ALBEE: Well, the Eastern schools were working very, very hard. And they had recruiters for minorities.

ASPATURIAN: When we say minorities, we mean underrepresented?

ALBEE: We had lots and lots of Asians; that was not a problem. The underrepresented part—I think it was Morgan who eventually got us an under-dean, who basically became an undergraduate recruiter; and she was with us for about three years.

ASPATURIAN: And that was basically her portfolio—to encourage graduating seniors to consider Caltech?

ALBEE: Right. And so, she toured a lot of the black schools in the South. But we still couldn't really build up much of a good stream. One of Caltech's lawyers came out of that program. She was recruited, and after getting her biology degree, she got a law degree, and then she came back and now works for Caltech in the patent office. I see her in the Athenaeum every once in a while.

ASPATURIAN: Did you work closely with the division chairs on this?

ALBEE: No. By then there were academic officers in almost every division. And they were the members of the advisory committee. I was working with them.

ASPATURIAN: How about the president and the provost? Was there much weigh-in or oversight from them?

ALBEE: No.

ASPATURIAN: Not much at all.

ALBEE: And when Ed became provost—

ASPATURIAN: Ed [Edward M.] Stolper [Leonhard Professor of Geology; provost, 2007–17; interim president, 2013–2014].

ALBEE: Which is after I had stopped being dean. He basically—in an economy move—put it almost all the way back to the divisions again.

ASPATURIAN: The graduate recruitment.

ALBEE: All the graduate control.

ASPATURIAN: I see. So, he decentralized things again. Do you think this was a good move?

ALBEE: I don't know. It seemed to me at the time that the graduate students were being totally ignored. Everybody felt it. Caltech was an undergraduate college, and you just happened to have a few worker bees in your lab. The thought that it was an actual graduate school was slow-moving, I think.

ASPATURIAN: What other challenges do you recall dealing with?

ALBEE: Well, as I said, each of the divisions had ways they had handled things. [Richard P.] Feynman [Institute Professor of Theoretical Physics; 1965 Nobel laureate in physics; d. 1988] and [Murray] Gell-Mann [Millikan Professor of Theoretical Physics, emeritus; 1969 Nobel laureate in physics] each always had a full-time TA. [Laughter] So, there were things like that.

ASPATURIAN: Aside from physics, how did the other departments—

ALBEE: I developed an algorithm of the number of students in a class, how many hours the class met, and whether or not it had labs. And I assigned TA support on that basis. That encountered considerable resistance.

ASPATURIAN: How about your home division—geo and planetary—were they more receptive to some of what you were doing?

ALBEE: I think so. Because I had been doing it there for years anyway.

ASPATURIAN: What do you regard, looking back, as your primary accomplishments in this job?

ALBEE: Well, I have to ask whether there was an accomplishment in the sense that Ed took it all away. What parts of it are still there now, I don't know.

ASPATURIAN: How about those things that worked while you were there?

ALBEE: I think the rights of the graduate student became much greater. That was the period in which we also put the graduate students on the staff benefits program.

ASPATURIAN: Was some of this in response to federal law, or did Caltech do this all on its own initiative?

ALBEE: The staff benefits was, if you will, a loophole that allowed us to get support for our students.

ASPATURIAN: Did the number of graduate students accepting offers from Caltech increase during your deanship?

ALBEE: In total or underrepresented? I think we got more underrepresented students. I think the graduate school was growing in prestige at that time. So, it's hard to know. But

if you didn't offer an economic package comparable to other schools, you didn't get the students.

ASPATURIAN: How about your own graduate students at this time? You were teaching and doing research the whole time.

ALBEE: I always taught almost a full teaching load.

ASPATURIAN: While doing all of this, really.

ALBEE: And of course, all the time that I was dean, I was Observer's project scientist.

ASPATURIAN: One of the things I've noticed about Caltech faculty is that a lot of them seem to get by with relatively little sleep. You were holding multiple portfolios. Are you one of these people, based on what you're telling me?

ALBEE: Not anymore, I'll tell you that. I think I just worked hard at it.

ASPATURIAN: What have you taught over the years that particularly stands out in your mind?

ALBEE: I've taught various kinds of field geology over the years. And then the course I taught regularly over all the years was petrology and microscopic techniques—X-ray, everything having to do with studying rocks. That was the course I taught every year, all the time. And then, when I started working as chief scientist at the lab and for twentyish years after, I taught a spring-vacation remote-sensing field course, and a lot of JPL-ers took it.

ASPATURIAN: What did that entail?

ALBEE: Well, basically, JPL people had been working on remote sensing for years.

They had done pilot studies throughout the desert to test new instruments—different kinds of radar, spectroscopic instruments, and so on. And so, this class would go out to these test sites that JPL scientists had originally studied with photos. They hadn't had the occasion to go out and see what these places looked like in the field and how you interpret them.

ASPATURIAN: Oh, I see.

ALBEE: And so, I ran that course—usually with about twelve, fifteen students—for twenty years every spring break.

ASPATURIAN: So, before you started doing this, they had not had this exposure?

ALBEE: No. Charles Elachi taught a course in remote sensing, which was taken by a lot of JPL-ers, and then I taught it. And then I taught this spring-break course, which used his course material in the field.

ASPATURIAN: I see. Hands-on.

ALBEE: And then as time went on, we had digital media as well as photographs. They teach a remote-sensing course now at UCLA, because the guy who teaches it took our course.

ASPATURIAN: I see. And they're still teaching it?

ALBEE: I believe so.

ASPATURIAN: Is anything still being done at the lab along the lines of what you introduced?

ALBEE: Oh, yeah, there's a lot of remote-sensing work that is still carried on at the lab. What I was doing was word of mouth: Lab people could sign up for Caltech courses, and

so a lot of them signed up for that. A lot of the foreign engineering students were going to be stuck here over the spring break anyway. So, they got the credit during the spring break for going out in the field. So, I had lots and lots of foreign students.

ASPATURIAN: Where did you go, exactly?

ALBEE: Well, dozens of places. Throughout the Mojave. These are all test sites that have been used.

ASPATURIAN: It sounds like it was a very popular spring-break activity. Let's talk a little about your years in the Athenaeum, which started around the same time you took the graduate deanship

ALBEE: I was just short of twenty years as chair of the house committee.

ASPATURIAN: You must have worked with Abel Ramirez [Athenaeum manager] for a number of years.

ALBEE: You see, when he left with problems, we had to take care of them, during which time I was executive manager, and then we had another manager who left and who we had to replace. The chair of the house committee is also in charge of hiring chefs, which we always did with a food committee and so on, and we went through a couple of chefs, too. At any rate, I effectively managed that house committee for nineteen years.

ASPATURIAN: At one point you actually became Athenaeum general manager.

ALBEE: At three points I did. The relationship between Caltech and the Athenaeum was not well—

ASPATURIAN: Defined?

ALBEE: Defined at that time. The chair of the house committee was basically where the manager reported, OK? So, over those years, I became acting executive manager three times while we hired new managers. I would be doing that for six months or nine months or whatever. And then the last time, which was after I retired by a week, I was standing out in the backyard and the phone rang. It was Dean Currie [Caltech Vice President for Business and Finance, 2005–2016]. He said, "Well, we've had to suspend the manager."

ASPATURIAN: Why was the manager suspended?

ALBEE: Ah, well, they were having—

ASPATURIAN: Irregularities?

ALBEE: Irregularities. They were having protests by employees. OK. And so, I ended up running it. And after about two months it was clear that we weren't going to solve all these problems because we couldn't hire a new manager until we resolved the whole business, so I told Dean, "I think you're going to have to hire me back," and so I formally became the executive manager for a year and a half.

ASPATURIAN: Do you have a particular interest in food?

ALBEE: No. I think I enjoyed management. That last time I took over for a year and a half, I realized that we had great discrepancies between personnel regulations and rules and the way we were managing our timecards. So, I had to educate all the sub-management about overtime rules and so on because we were breaking them right and left.

ASPATURIAN: I see.

ALBEE: I had to become familiar with California labor law.

ASPATURIAN: Did you spend a lot of time comparing the Athenaeum to other faculty clubs at peer universities?

ALBEE: Well, when I was on the dean's committees, I ate at an awful lot of faculty clubs because they would hold these meetings at various places. So, it became very clear that we have an absolutely unique situation and that no one else has really done what we have in turning the Athenaeum into a high-class restaurant. You see before Dean, at the time of David Morrisroe [Vice President for Business and Finance, 1969–1995, d. 2002], the Athenaeum had a substantial subsidy.

ASPATURIAN: Sort of an informal endowment?

ALBEE: A subsidy out of the budget. And basically, we made an agreement that over a five-year period we would get rid of the subsidy. So that was the time when I had to think about growing the membership—getting grad students as members, increasing the number of community members, and increasing revenues to get the Athenaeum to the level it had been where it was still being subsidized.

ASPATURIAN: What was the feeling about that on campus?

ALBEE: Oh, it was terrible.

ASPATURIAN: What happened?

ALBEE: It was the same as when we introduced new graduate student things—"That's not the way it used to be."

ASPATURIAN: And what did you say?

ALBEE: I just explained that we had to work to get rid of the deficit. We changed the whole dues structure, too.

ASPATURIAN: Were you running the committee when the Ath underwent its major renovation?

ALBEE: The first one—well, all of them, I guess. Except this last one—the one outside.

ASPATURIAN: The very comprehensive one where they opened up the ceiling and brought in new carpeting and all of that?

ALBEE: Well, there were several renovations along the way.

ASPATURIAN: I remember that one.

ALBEE: We had another one where we gutted the kitchen because we were getting too big for it. There was no more space, and so we decided that the only way to do it was to take a summer to gut and remodel the kitchen and put in totally new equipment.

ASPATURIAN: Did you also oversee the accommodations, the rooms and so on, all of that?

ALBEE: Yeah. And we put in the pattern of people being able to "buy" a room, designate a room name.

ASPATURIAN: These were all fundraising initiatives. What else do you recall about that period? You are the only professor I've interviewed who was also in this type of role—basically running an exclusive club.

ALBEE: It's a challenge. But in all of these, the pattern throughout is, I have some fondness for management. I want to make things work, figure out how to make people work with me and so on, and that shows up in all the things I've done.

ASPATURIAN: And you clearly like managing when things are in transition and new initiatives are being undertaken.

ALBEE: Or else I undertake them.

ASPATURIAN: That too. That seems to be a common theme.

ALBEE: For a year and a half, after I retired, I was manager of the Athenaeum. And during that same period of time, we were also still running MGS. I went emeritus in 2002, but my ties to JPL went on longer. And after I finally got that done, I didn't have anything to do, so I became a dollar-a-year business manager for Westminster Presbyterian Church over on Lake [Lake Avenue in Pasadena] for six years. Basically, that was being a building manager because the church was broke in an operational sense, so they had to use the facility to raise revenue. I brought in tenants, and so on, there to help its budget.

ASPATURIAN: And they changed your job title a few years before that—you became a professor of geology and planetary science.

ALBEE: I had been working on that for a number of years. I mentioned in one of our earlier interviews [*see Session One*] that the sample people in geology were never quite part of the planetary group.

ASPATURIAN: That's right.

ALBEE: I became chief scientist of JPL, and I was still professor of geology. And when I left and became project scientist for two missions, I was still— So I finally said—I don't remember who was division chair then—"This is ridiculous." I was teaching a remote-sensing class; I had been teaching the spring-break one for years; and I was also now teaching a planetary science class. So, I finally said, "I think it's time to get someone to make a change in my title."

ASPATURIAN: I think the division chair would have been Ed Stolper—the late 1990s?

ALBEE: I guess it was Ed. I think I complained to Ed.

ASPATURIAN: He was responsive, and they changed it?

ALBEE: Well, Ed would have done anything to jab somebody. People talk about Wasserburg being hard to get along with; I think Ed is the harder one. [Laughter]

ASPATURIAN: Did you have anything in particular to do with him while he was provost?

ALBEE: No.

ASPATURIAN: As division chair?

ALBEE: Not really.

ASPATURIAN: But he apparently did put through the change in your job description. Is there anything else you'd like to put into the record?

ALBEE: I don't think so. Oh, there is an interesting one.

ASPATURIAN: Sure.

ALBEE: The vice president who came from Chicago—the lady vice president?

ASPATURIAN: Margo Marshak? [Caltech VP for Student Affairs, 2002–2006]

ALBEE: Right. She had become enamored of the chef—I don't mean enamored, wrong word choice. She had hired a chef for the campus food services; he was very ambitious and took over the food services at JPL; and he was being supported by the president.

ASPATURIAN: The president of Caltech? That would have been David Baltimore [Caltech president, 1997–2006; 1975 Nobel laureate in physiology or medicine].

ALBEE: Um-huh. And Baltimore stuck his finger in and stirred it around in a way that he should not have. I have not spoken to Baltimore since, actually. We ended up with a

committee, and then this chef tried to hire away our chef from the Athenaeum, and the Office of Student Affairs wanted to take over the Athenaeum. It finally ended up with a trustee committee that held hearings, and the Athenaeum was saved because the committee supported the current structure for the Ath. Then the chef left; he's down in San Diego now.

ASPATURIAN: I think I heard a little bit about this.

ALBEE: But Margo was just bad news for Caltech.

ASPATURIAN: I've heard this from other people. Did you work with her?

ALBEE: Only when she came in and stuck her nose in the Athenaeum's business.

ASPATURIAN: You were no longer in the dean's office, of course; you were long gone from there. Well, that was an unfortunate thing.

ALBEE: But that would have been during the year I was the Athenaeum manager.

ASPATURIAN: How long did this go on, this back and forth?

ALBEE: Almost a year. I ended up writing all sorts of things and giving presentations. I just felt David Baltimore acted inappropriately, and that Margo definitely acted inappropriately.

ASPATURIAN: Anything else?

ALBEE: [Laughter] No.

ASPATURIAN: Thank you very much.

ADDENDUM

ASPATURIAN: This is an addendum. Same day, same time, with Dr. Albee.

ALBEE: This has to do with Jerry Wasserburg because Jerry was outspoken, and a lot of people, I'm sure, have said in their interviews that he was a terrible person; they hated him, and so on. A lot of it goes way back to the early days of the development of the mass spectrometers when there was not enough distilled water; and that turned into a fight between the Wasserburg students and the Lee Silver students over the distilled water. And this two-camp feuding has carried on, and I suspect in their oral histories a lot of people will say that Jerry was very difficult. But Jerry was a wonderful gentleman. The picture of him that emerges I find highly irritating. OK; I guess that's probably it.