



Lew Allen, 1982 Photo by Thiel Photography

LEW ALLEN, JR.
(1925-2010)

INTERVIEWED BY
HEIDI ASPATURIAN

June and July 1991, and
March and April, 1994

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Subject area

Physics, Jet Propulsion Laboratory, U. S. Air Force, national security, National Aeronautics and Space Administration (NASA), space exploration

Abstract

An interview in four sessions, in June and July 1991 and March and April 1994, with Lew Allen, Jr., former director of the Jet Propulsion Laboratory (1982-1990) during a period that included the launches of Galileo to Jupiter, Magellan to Venus, and IRAS, the Infrared Astronomical Satellite, as well as Voyager 2's Uranus and Neptune flybys.

He recalls matriculation at West Point in 1943, receiving flight training, graduating in 1946. Four years in the Strategic Air Command studying nuclear weapons projects, then graduate school at the University of Illinois; PhD in nuclear physics, 1954. Assigned to the Los Alamos Scientific Laboratory (1954-1957); participates in bomb tests at Bikini, Eniwetok, and Nevada. Assigned to weapons development at Kirtland Air Force Base. Joins Office of Space Technology (1961-1965) in the Directorate of Defense Research and Engineering,

under Harold Brown. To Office of Special Projects (again under Brown, now secretary of the air force) in Los Angeles, works on satellites (1965-1973). To CIA as deputy under James Schlesinger; soon appointed director of the National Security Agency. Recalls NSA's attempts to steer clear of Watergate investigation. Becomes air force chief of staff in 1978. After retiring from the air force in 1982, he becomes director of JPL, retiring in 1990.

In this wide-ranging interview, he discusses his nuclear weapons work, his air force career, his service in Washington, his tenure as JPL director, his chairmanship of a controversial NAS panel on export-control laws, and his assessment of the history and future of the U.S. space program.

Administrative information

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Contact information

Archives, California Institute of Technology
Mail Code 015A-74
Pasadena, CA 91125
Phone: (626)395-2704 Fax: (626)395-4073
Email: archives@caltech.edu

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH LEW ALLEN, JR.

BY HEIDI ASPATURIAN

PASADENA, CALIFORNIA

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ORAL HISTORY PROJECT

Interview with Lew Allen, Jr.
Pasadena, California

by Heidi Aspaturian

Session 1	June 10, 1991
Session 2	July 31, 1991
Session 3	March 31, 1994
Session 4	April 4, 1994

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ASPATURIAN: Why don't you start by telling me about your early background and your parents.

ALLEN: Sure. I attended high school in Gainesville, Texas, which was my mother's hometown. My mother and father had separated during the Depression, and she had returned to her parents' home in Gainesville. Gainesville is a small town in Texas that fits the model of *American Graffiti* or *The Last Picture Show* perfectly well. The high school there, while small and somewhat rural, nevertheless provided a good educational background. My class graduated in '42, and the war had begun, which forced all of the boys' attention to military service.

Therefore, when I graduated the options available were to join one or another of the various military programs offering the opportunity to go to college for at least two years. As a matter of fact, I was accepted in the navy program, but about the time that happened I was fortunate enough to receive an appointment to West Point. That opportunity was outstanding enough that I had no hesitation in opting for West Point. I was too young to attend immediately after graduating from high school, so I spent the next year in the local junior college, taking mathematics and science courses.

ASPATURIAN: How old were you when you graduated from high school?

ALLEN: Sixteen. I entered West Point in the summer of 1943.

ASPATURIAN: How early in your education did you decide that science and math were what you wanted to do?

ALLEN: Much later.

ASPATURIAN: But you were taking courses in junior college.

ALLEN: Oh yes. Now, I certainly was interested in science and mathematics during those years but really had no clear view of the future at all. The West Point curriculum was very much truncated due to the war. It had been reduced from four to three years, and at the same time my class was the last of the wartime classes that also included, for those who opted for the air force, flying training during the academic program. So the academic program that I followed was really perhaps a little less than two years, leading to a BS. That made things very intense but also very focused, to put it mildly, when it came to meeting the minimal requirements. Nevertheless, at the end of three years, in June of 1946, I graduated from West Point, standing at about the top quarter of the class. I also graduated with my wings, having spent the spring and the summer of the second year and the spring of the third year in flying training instead of in academics.

ASPATURIAN: Were the courses geared so that the flight training and the academic training complemented one another?

ALLEN: No. They were strictly separate. One simply didn't go to other classes the last semester of the second year or the last semester of the third year. So it was abbreviated. There was obviously an attempt to make up for that by focusing as intensely as possible on fundamentals. And I think in the long run that turned out to be a pretty good foundation. But it's still true that the undergraduate work was quite abbreviated.

ASPATURIAN: Did you have a specific major while you were there?

ALLEN: No. For two reasons. One is that at that time the military academy did not offer majors. The only degree was a bachelor of science in military engineering. In subsequent years, they've

offered majors. However, even if they had offered majors, we wouldn't have been able to take a major, simply due to the absence of time to take any option courses. I mean, we were getting a BS in two years, for all practical purposes, and that meant being focused on core courses.

ASPATURIAN: Which were?

ALLEN: Well, the normal core for any university. There was physics and chemistry and mathematics through differential and integral calculus; and engineering courses, like drafting and map reading, and English courses and military law. All of those kinds of things were requisites and taught briefly.

ASPATURIAN: So you underwent a lot of intensive academic training in addition to your flight time in this relatively short period.

ALLEN: Well, yes, it clearly had to be very intense to meet the requirements in such a short period. After graduation came a period of some transition, training into bombing aircraft, and then an assignment to the Strategic Air Command in the latter part of 1946. But upon being assigned as a pilot in the Strategic Air Command, I was selected rather quickly for specialized training in nuclear weapons, because college graduates, even with a thin education, were a little bit rare in the immediate postwar period. I went to several courses in nuclear weapons projects associated with the Strategic Air Command. After about four years in the Strategic Air Command, there was the opportunity to apply for postgraduate training. I did apply, in aeronautical engineering. It turned out that the air force concluded that they really didn't need aeronautical engineers as much as they needed nuclear physicists. So the air force came back and said, "If you wish to go to graduate school, we would like you to go in nuclear physics." They applied for me to the University of California at Berkeley and the University of Illinois. UC Berkeley rejected my application, presumably due to the fairly skimpy undergraduate background, although actually the letter indicated they were a little confused about all that. But the University of Illinois accepted me, and I entered the University of Illinois in the summer of 1950.

The program I was sent to was intended to lead only to a master's degree. At the time I entered, there was not really any available opportunity to continue for a PhD. So my first two

years of course work at Illinois was based upon the assumption that I would leave at the end of the master's degree. I did reasonably well in that work, and the department recommended to the air force that I continue for a PhD, which the air force allowed me to do. That made the end there a fairly tight period, because it was necessary to complete all the requirements really rather quickly and get in the thesis work with very little delay. Nevertheless it ended up working out all right.

ASPATURIAN: I'm curious about how you found the academic environment at Illinois, compared to what you had gone through at West Point.

ALLEN: The style of teaching was, as you might guess, very different. The military academy at that time, and even more so because of the abbreviation during the wartime years, taught very much by rote; that is, every cadet was required to recite every day and was graded every day on his recitation. It was simply not permitted to fail to do homework. That was a matter not only of a bad grade but of disciplinary action. One recited and was graded on every course every day. That meant that the work was very highly structured, with very little opportunity for any deviation from the prescribed routine, and very much dependent upon the ability to solve the problems of the day. And of course at Illinois it was totally different. No recitations other than occasional exams and end-of-course exams; and with a great deal of latitude and a great deal of attention to fundamental understanding instead of to the qualities demonstrated by daily recitation. Those differences in the style of education were very marked, but on the other hand they were not upsetting. That is, the discipline of the daily recitation had made me, and other students at Illinois in the same category as I was, substantially better than our civilian peers at keeping up with the day-to-day work. Now, the civilians were, in general, much better prepared than we were and had a better fundamental understanding of what was going on than we did, but we compensated for that by a highly disciplined and structured approach to our work and an attempt to keep well ahead of the scholastic requirements. So it worked out fine, and the head of the physics department was quite complimentary about my work and encouraged me to stay. I did a thesis on high-energy photonuclear reactions working at the betatron. My work was part of a series of experiments in different energy ranges done by various students under Professor Alfred Hanson.

ASPATURIAN: He was your advisor?

ALLEN: Yes. And I think the research as a body was very useful, and the work on which my thesis was based was cited rather well for a few years. So all that was generally quite satisfactory, although hectic, given the time pressures—unlike the case with my civilian peers, who could tolerate disappointments. I had to leave Illinois on a scheduled day. But it worked out all right, with a little bit of scrambling at the end to get a thesis finished in the time that the air force would allow me to stay. Immediately following the completion of my thesis, I was assigned to the Los Alamos Nuclear Weapons Laboratory. This was at a time when the development of thermonuclear weapons was reaching a peak, and the Atomic Energy Commission had asked the military services to simply assign them people of scientific training in order to help—that is, just simply as loaned staff members. So for a little over three years I was a staff member at Los Alamos, without really any specific ties back to the air force.

ASPATURIAN: What year was this?

ALLEN: I arrived at Los Alamos in the spring of 1954 and remained there until the summer of 1957.

ASPATURIAN: So the first hydrogen bomb tests had already been held.

ALLEN: Yes. The first hydrogen bomb test was in 1952, although that was not really a weapon but a device that proved the principle of thermonuclear weapons without really having any military utility. The first weapons that really had military utility were tested in '54, coincidentally just when I got there. So I was joining the laboratory in the test division just at the time of the testing of those weapons, which was very interesting.

ASPATURIAN: In what ways, particularly?

ALLEN: Well, because the priorities were indeed very high, and the needs for progress in this area were considered very great. The personnel had changed a good deal from the wartime days of the Manhattan Project, but there were still a number of very competent and good physicists

working at the laboratory, and that made it a very interesting place to be. The early work I did involved participating in certain experiments related to radiation transport and the opacity of materials. Then I was assigned a project of my own, which related to experimental determination of the lethal characteristics of a nuclear weapon used as an antimissile device. This was for the purpose of defining the characteristics of what was then known as the Nike-Zeus system. The work I was doing, in conjunction with a theoretician, ended up defining those various characteristics and the relationships that would allow the design of future weapons for missile defense purposes. So that was all quite interesting and quite satisfying, and we generated several reports, at least one of which proved to be useful at Los Alamos for a number of years. In the summer of '57 I was relieved from my assignment at Los Alamos and assigned to what is now the Air Force Weapons Laboratory at Kirtland Air Force Base. It was known then for its research activity associated with the testing and development of nuclear weapons.

ASPATURIAN: I'd like to go back to Los Alamos for a minute. I think you are probably one of the few scientists now at Caltech who was actually there for an extended period of time after the war, when it kind of assumed a new character. I was wondering what you remembered about the community and the scientific culture there.

ALLEN: Well, to be quite frank, it was a very heady experience for a young and fresh PhD. The group of people was really very good, and the atmosphere was one of great collegiality. Los Alamos at that time went to considerable efforts not to use any titles at all. For example, PhDs were not called "doctor," and no one else had a title. This was all in an attempt to ensure that there was collegiality among the teams working there, and it made for a really very pleasant, very exciting working environment. There were a lot of young people in the groups in which I was working. Because of our constant preparation for testing either in the South Pacific or at the Nevada test site, it was fast-paced and gratifying, in that we obtained experimental results in a reasonably short period of time after preparing for the experiment. So it was really a very satisfying and exciting period.

ASPATURIAN: You mentioned the South Pacific. Were you at Bikini?

ALLEN: Yes. The practice at the time was to have a series of tests in the Eniwetok lagoons in

even years and at the Nevada site in odd years. When I arrived in '54, I went out for the test at Eniwetok-Bikini, but really mostly to get familiar, since I'd so recently reported in. During the summer of '56 I spent the entire test period out there, and some of the experiments I did required me to return again late in 1956. In the years '55 and '57, I participated in the tests in Nevada, which involved several very interesting instrumentation tests of different devices there that were very interesting.

ASPATURIAN: Do you have any particular recollections of the tests themselves? We all know, for example, what the Trinity people said and felt when they watched the first atomic blast because it's been quoted so often. You were in a kind of era of discovery.

ALLEN: Well, that's right. I think I was too young to have the moral concerns that some of the scientists working on bombs had immediately following the war or later on. The weapon tests themselves were dramatic in the sense of the particular ones in the Bikini atolls, just because of the enormous power of the devices and the tremendous effects they would have over all of the lagoon in the atoll on which one was working. You prepared experiments and then went off and left them, and they went through tidal waves and radiation blasts and all kinds of things. Then you came back later on and attempted to be sure the instruments were still working properly. That was challenging, interesting, and really rather dramatic. Certainly some of these blasts, even when perceived at quite a distance—such as a blast at Bikini perceived from the Eniwetok, which is several hundred miles away—were of such enormous power that the results were awesome. And it certainly would cause anyone to be concerned about the applications of those kinds of weapons in anger. That would really be very poor.

The work at Kirtland in Albuquerque also related to the testing of weapons, but with the emphasis shifted more to the physics of the weapons' effects. The first couple of years of that effort were dominated by a particular phenomenon. A physicist by the name of Nicholas Christofilos, working at Lawrence Livermore National Laboratory, had recognized that if a nuclear weapon was detonated high enough in the atmosphere, it was possible for the electrons resulting from beta decay to become trapped in the Earth's magnetic field, causing a radiation belt that might have certain military effects. Some ground experimentation was done to confirm the general nature of this effect, and at the time it generated a great deal of concern that there

might be a very potent military effect—and therefore a tremendous crash effort was launched to investigate and test this effect. A series of tests was planned where nuclear weapons were taken onboard a ship to the South Atlantic, where they could be injected into what's known as the South Atlantic Anomaly. Then the weapon was launched to a high altitude and detonated, and the effects were measured in various ways. There were three programs for measuring the effects. One of them was Explorer 4; the experimentation was the responsibility of James Van Allen, and of course JPL [Jet Propulsion Laboratory] was involved in the construction of the satellite. The second was a series of ground-based measurements of artificial auroras—that is, through ground-based radio frequency telescopes, the radiation caused by the electrons rotating in the Earth's magnetic field could be observed. The third part of the program was to send a series of about thirty sounding rockets to 900 kilometers altitude to penetrate the artificial shell that was to be created. I was responsible for that part of the program—or at least for the science part of that program. All of that was done with a very high degree of success, although the weapons didn't go quite as high as they should have and the artificial belts didn't live quite as long as they were expected to, because of the low altitude of injection. But all elements of the program worked quite well. Neither Van Allen nor anyone else had considered that there might be electron belts that existed around the Earth from natural causes, and he had not interpreted the early results from the previous Explorers correctly at all. It was not until recognizing the theory involved behind the artificial belts that he realized that that might be what he was seeing in the natural results. In the earliest papers he released, he was not allowed, for classification reasons, to refer back to the artificial belts, but in some of the papers he did state that the work of Christofilos and the preparation for the artificial injection made him realize what was going on. At that time—and, as I understand it, even today—one has some difficulty accounting for the origin of all of the natural electrons, but nevertheless they clearly were there. So the natural radiation belts ended up being of tremendous public interest.

ASPATURIAN: Did you perceive your own work in those days as being more defense-related or more scientific in its orientation?

ALLEN: Well, I think more defense-related. There was a sense of urgency, because it was thought that this would end up being an effect of considerable military importance. It turned out

that that belief was false. The reason, as is well understood now, was that for the effect to have been of military significance, the density of electrons trapped in the field would have to have been greater than the field itself could have supported. Today we understand that there are instabilities that are generated by a dense electron belt, and therefore one can't get these belts to a level that is of military significance. Now, that wasn't recognized until after the experiment was done; it really took a while to understand all that. So it ended up having a good deal of military significance but not nearly as dramatic nature as was thought at the time.

The Van Allen radiation belt ended up being very interesting scientifically. The results of this experiment were highly secret at the time it was done, but it took only a few months for it to be recognized that they were (a) not of as much military importance as was originally thought and (b) of a great deal of scientific interest. Therefore the entire experiment was declassified, and all of the results were presented at the National Academy of Sciences in a very dramatic presentation about what was certainly the first global geophysics experiment that had ever been done. It was really very exciting. So all that work was really quite interesting.

ASPATURIAN: How did the fact that so much of this was classified affect you and your family or friends? Maybe you couldn't talk to them about what you were doing?

ALLEN: Well, but of course that had been true at Los Alamos as well, so there was no difference. But in general the details of the work at Los Alamos and the details of the work at Kirtland—and, indeed, of much of the work I've done throughout my military career—were not subject to discussion at home or elsewhere. So if you look at it the other way around, when one did have a project like this that could be declassified and talked about, then it was really a particular thrill. So that was a lot of fun, to be able to report on that and generate papers on that.

ASPATURIAN: So were you also writing this whole time, collaborating on scientific papers?

ALLEN: Yes. Not on a large number of them, but the papers describing this experiment were of some significance at the time and attracted a good deal of interest.¹

The work at Los Alamos ended up with a focus on the effects of nuclear weapons

¹ See, for example, Allen, L., Jr., et al. (1959), "Project Jason Measurement of Trapped Electrons from a Nuclear Device by Sounding Rockets," *J. Geophys. Res.*, 64(8), 893-907.

detonated at high altitude, which was ballistic missile defense, and the work at Kirtland continued in that general vein. Following these sets of experiments, we began to look at other effects associated with high-altitude nuclear explosions, and we performed experiments on high-altitude rockets and piggyback experiments on ballistic missile tests and the like, to obtain more space data relevant to the concerns we had about trying to understand how nuclear weapons would operate in that environment.

ASPATURIAN: This was at the height of the cold war, the John Foster Dulles, Eisenhower-Khrushchev period.

ALLEN: I guess you'd say it was at the beginning of the long, high plateau of the cold war. Our attention was largely focused on ballistic missile defense. All of that work, relating to very high-altitude kinds of concerns and some space kinds of concerns, caused Harold Brown—who was subsequently president of Caltech [1969-1977] and whom I had gotten to know some when he was director at Livermore—to ask me to join him when he went to Washington [in 1961] to take over as director of defense research and engineering. Because my mother was very ill at the time, and was in fact dying, I delayed joining his office for six months or so, but then I did join as part of the space technology office that was being formed.

ASPATURIAN: In addition to Brown, I imagine in those ten years of very intensive and exciting work, you must have met some other interesting people. Are there any who stand out in your mind, particularly?

ALLEN: Well, the predecessor of Harold Brown in the Directorate of Defense Research and Engineering was Herb York, with whom I have worked now several times over the years; of course, he was very interesting and remains a very perceptive and concerned individual. With many of the people with whom I worked in all of those years, the paths continually intersected; so there have been any number who were extremely interesting.

The space technology work—we're now in the first years of the Kennedy administration—was a very formative period. This was subsequent to NASA's formation but prior to the decision to go to the moon and the whole decision to do it by means of the lunar-orbit rendezvous. So those decisions were being made during that time, and the defense interests were

in large measure trying to figure out what their role should be in all of that and then also trying to figure out what all of this space activity was going to mean to defense programs. So I was involved in many aspects of that—not only with nuclear issues but also with space power supplies and a little bit on space launch vehicles and some space tracking networks. All that kind of work was associated with those four years working in Harold Brown's office.

ASPATURIAN: Could you talk about that a little more, maybe in terms of some of the politics that were involved?

ALLEN: Well, it was an interesting period, because Robert McNamara was secretary of defense. And he had come into the Defense Department with the intention of rationalizing the budget formation and planning processes by introducing what's called a planning, programming, and budgeting system, using Charlie [Charles J.] Hitch from RAND to be his person to do all that. This was a big change in how one did things, and it was vigorously resisted by the Congress, as well as by all the military services—not all necessarily for very good reasons but only because it was a very big change, and any big bureaucracy reacts poorly to sudden and dramatic changes of that sort. And that had the effect of making life more interesting for people like myself. Simply because the military departments were fighting these changes, they really were not contributing very much to the decisions being made on how the research and engineering programs should be conducted. As a result, Harold Brown and his staff ended up really driving all those things far more than they would have, had the military services not been fighting McNamara on these other issues quite so much. So the result was that, in a much larger way than is true today, the young officers working for Harold Brown ended up having a great deal of influence on how these budget decisions were made. What ended up happening was that since the younger staff people in the military realized they couldn't really work with their own services, because the services were spending all their time fighting the changes, the best way to get anything done was to work directly with people like ourselves. So the work really got done, in a sense, as part of Brown's staff, augmented by those people who were participating in the military staffs while this furor went on elsewhere.

ASPATURIAN: So you had more autonomy and more authority than you might have expected?

ALLEN: That's right. One wouldn't see that much today, for the simple reason that now the changes are all ingrained and everybody is working on it.

ASPATURIAN: What exactly was your role in all this?

ALLEN: Well, there was an office called the Office of Space Technology, and I was one of a couple of people in that office. The research and development programs associated with space—all the budget actions and plans and programs—were our responsibility. The launch vehicles were generally not a part of that small office, but we obviously interacted with that area all the time. And, as I say, it largely related to work on subsystems: Power supplies were a big issue for a long time; energy control systems, radio systems for spacecraft and so on, were all a piece of that. Then there were the general interactions with the other parts of the program, which got into the launch vehicles and other considerations, such as what kind of a manned spaceflight program the military ought to have.

The military agonized over that a great deal. There had been a program called Dyna-Soar, which was a winged vehicle that was to be placed in orbit and then reentered, using a hot-structure technology, so-called, that would end up being very similar to what is being thought of now for the National Aerospace Plane. The program, while interesting technically, had no very easily definable military value, and so as soon as McNamara came he began to wonder why everyone was focusing on it. So he was inclined to cancel it. And the people on Brown's staff convinced McNamara that he shouldn't cancel it without introducing some other program involved in manned spaceflight to take its place. So he did cancel the Dyna-Soar and announced the start of a program called the Manned Orbiting Laboratory [MOL]. And that was very interesting, because we had two things to do. One was help define the characteristics of that laboratory, which was to be based on the Gemini B as the personnel reentry vehicle. The other was to work on the tank structure for the laboratory itself and the type of vehicle that would put it up there. And all of that began coming together pretty well, so the laboratory looked as though it was a technologically sound approach. But the part that didn't come together very well was what in the world one was going to do with it when it got into space. So in struggling with that on several occasions, Harold Brown would assign me tasks to participate in that thrashing about, trying to figure out what the purpose of all this was. And that had modest success. It ended up,

some years later, being canceled, and I was involved in that at a later time.

ASPATURIAN: How about your relationship with the civilian space program? Were you in competition, or was there cooperation regarding budget, information, goals?

ALLEN: There was cooperation in the following sense: There was at that time what was called the Aeronautics and Astronautics Coordinating Board, which was very active and did a great deal of work keeping various technology programs and activities properly coordinated and mutually supported. NASA at that time was moving so fast that they needed to call upon the military services for a great deal of support, in the case of ranges and launch vehicles and all kinds of things. So in that sense it really was not competition at all. On the other hand, the military was really struggling, because they had this inherent gut feeling that there had to be something terribly important about manned spaceflight for them to do, and they couldn't figure out what it was. So there was a great deal of thrashing around, trying to determine that. One of the things we looked at for some time was called Blue Gemini. That was an attempt to build Gemini spacecraft subsequent to the termination of the NASA Gemini program and fly those for air force purposes. We worked all of that to show that the project was technically kind of neat and could be done pretty well, and then one couldn't begin quite figuring out why. So that wasn't done. The MOL—the Manned Orbiting Laboratory program—was the later manifestation of struggling with that, of the military's trying to get a manned program under way without ever, to this day, having solved the issue of why you really wanted to have it manned.

ASPATURIAN: Was part of the concern that if the United States didn't get there first, the Soviets would?

ALLEN: No, that was the motivation on the Apollo program. The military's motivation was that if this is so significant that the president is making a bold decision to go to the moon and develop the capability for manned spaceflight, then manned spaceflight itself must be very important, and we, the military services, ought to understand all that and it ought to be important for us. So that was the thinking that led up to them saying, "This is the way the nation is really going. This is the future. And we need to be a very active part of that. But we can't figure out why!" So that was the tenor of all of that.

ASPATURIAN: It must have been exciting and trying.

ALLEN: Well, it was, except I wasn't in a position of enough responsibility for it to be very trying. I was more in the position of its being an awful lot of fun, to get in the middle of all those arguments and thrash around. And, of course, the situation in a certain sense has not changed to this day.

So there were a number of tasks and activities during that time that were extremely interesting. But at the completion of that assignment [1965], I was reassigned to what was known as the Office of Special Projects, under the Secretary of the Air Force. This was not at Harold Brown's instigation; it was at somebody else's instigation but with Harold Brown's acceptance. It turned out that shortly after I took that assignment in Los Angeles, Harold Brown became secretary of the Air Force. So in that sense, I was again part of his office, although moderately remote.

ASPATURIAN: He was there and you were here.

ALLEN: That's right. But that was a very interesting period. It involved the highly classified programs in the air force, satellites. They've come to be known over the years as "national technical means of verification," meaning observation systems that can be used to verify treaties and the like. And so they had a very high priority and a dramatic rate of accomplishment. My work originally was as head of the research activities in that particular program, and that was very, very interesting and led to a lot of developments, some of which have proved to be very useful.

ASPATURIAN: Was this your first real contact with intelligence gathering?

ALLEN: Yes, right. I worked in those activities then for a long time, first in Los Angeles and then later in Washington, where I came back to be the director of the, if you like, headquarters office, again in the secretary of the Air Force's office. Then I moved back to California for a third assignment, this time as director of the special projects activity in Los Angeles. Those three moves back and forth were in the period when I moved from being a colonel to a major general. So that was all very satisfying, and of course in those days the rate of launching was

very high, by today's standards. **[Tape ends]**

Tape 1, Side 2

ASPATURIAN: While you were at the Space Technology Office, were you involved in satellite development?

ALLEN: Yes. Well, we were headquarters. In the budgeting and programming, yes.

ASPATURIAN: Were these the first years of satellite development for the military?

ALLEN: Yes.

ASPATURIAN: It's come to be such a major and fundamental aspect of military and foreign policy information gathering. I get the impression you were there on the ground floor.

ALLEN: Well, pretty much. I mean the first high-altitude observation system that I recall was the so-called Vela program, whose purpose was to look for nuclear explosions that might occur in space. I worked on that before I left Albuquerque. When I arrived in Harold Brown's office, the evolving programs that were of a great deal of interest were the MIDAS [Missile Detection Alarm System] and SAMOS [Satellite and Missile Observation System] programs, which ultimately became such things as the infrared early warning satellites and meteorological satellites, and so on. The communications satellite program, which was actually the responsibility of other offices, was also a very dramatic development during that time. But our responsibilities to that were related to the technologies that were being supported.

In those days, satellites didn't live very long, so there were lots and lots of them. A launch rate of a few dozen a year overall and a dozen a year at the end of the program was not unusual. And now there's a launch about every four or five years, because the satellites are perfected and live a very long time. So it was a period of high activity.

ASPATURIAN: It was a sort of clean way of intelligence gathering. You didn't have to risk people, for a change.

ALLEN: Yes. That's very nice. I completed the tour as director of the Special Projects Office for the air force in Los Angeles in the beginning of 1973, and then I was assigned for a short period of time to the Air Force Systems Command. But almost immediately, Jim [James R.] Schlesinger asked me to join him when he became the director of the CIA. He had been the person in the Office of Management and Budget reviewing these special satellite programs, and I'd gotten to know him in that capacity. So I moved over there as his deputy for what's called the intelligence community—that is, for the management aspects of director of Central Intelligence, the budgets and programs of the other agencies, not specifically the CIA. That was very interesting, because he was assigned to decrease the size of the CIA significantly, and that, with a number of other corrective measures at the CIA, made it a very turbulent time. Then Watergate began to break, and so he was moved to become secretary of defense in the summer of 1973, after I'd been there only a few months. And then, not very long after he became secretary of defense, he appointed me director of the National Security Agency, which reports to the secretary of defense. Which was just as well, because the CIA then began to be in a long period of unpleasant investigations and siege, which made life a little less pleasant working in those areas. The National Security Agency has responsibility for communications security and such matters.

ASPATURIAN: What does that mean, exactly?

ALLEN: It means the development of codes that secure communications and the development of techniques to solve those codes as they might be used by other people, and the apparatus that collects the foreign communications in order to work on all that. The NSA, at the time I arrived, had not yet been caught up in the investigations of the intelligence communities, which were beginning to affect the CIA—although the NSA also became a focus fairly quickly.

It turned out, fortuitously, that a week after I arrived at the NSA it was concluded, in consultation with the general counsel, that certain NSA activities relating to what are called watch lists—that is, looking for foreign communications involving American citizens, where the names of American citizens are being provided by the FBI for their purposes—could all be considered questionable. And that indeed because of certain recent court cases dealing with wiretaps; some of these activities might be judged illegal. So within a week after I arrived at the

NSA, after consultations with our general counsel, I wrote the attorney general of the United States saying, "I'm not sure whether these actions are proper, and I request that you reconfirm the legality of us responding to these requests from the FBI."

ASPATURIAN: Who was the attorney general at that time?

ALLEN: This was the late summer of '73, and I guess Nixon had just fired everybody, so Richard Kleindienst had left and then Elliot Richardson came.

But in any event, the fact is that when all the investigations then began with regard to whether the NSA might have been improperly involved in domestic surveillance, these concerns were very much attenuated by the fact that it was clear that I had raised the same concerns with the attorney general almost immediately after I arrived. As a result, a number of things were changed, in accordance with the evolving perception of how it all should be interpreted. The next two years at the NSA were dominated to a fair degree by these investigations, and there was a lot of work that had to be done in meeting the demands of Congress, including the necessity for open hearings, which was the first time the NSA director had ever had to appear at an open hearing. But even though it was all tedious, and sometimes painful, it nevertheless came out well, in the sense that unlike the CIA, the NSA was never really hurt by these investigations, and its ability to continue to operate effectively was not very much impeded by all of the thrashing about.

ASPATURIAN: What was your experience testifying before Congress? I recall there was a Frank Church committee, and there was another committee, I think.

ALLEN: The one which you have fortuitously forgotten was the disastrous committee headed by Otis Pike. That was the House side. The Church committee was in the Senate. The Pike committee was a total disaster, because they never behaved responsibly, and the result was that it was very difficult to cooperate with them, and we did not [cooperate] more than we had to.

ASPATURIAN: When you say "didn't behave responsibly," you mean there were leaks and that kind of thing?

ALLEN: Yes, there were leaks. The staff members would not accept the restrictions of security clearance. In spite of constantly making arrangements that certain information would be treated only in closed session, they'd nevertheless turn around and introduce it in open session and cause great embarrassment. Nevertheless, in a perverse way, they were relatively kind to me. The highlight of the Pike committee was when the key staff member called me over late one night and said, "Got a draft, which I'd like very much for you to review. It's a classified draft, but if you see anything in it that's very sensitive, I'll take it out." And so I raced over and late at night went over this whole document, and indeed there were a number of things in there that we regarded as very sensitive, and we expunged them. And the reason all that was significant was because that was the document that was placed in the hands of the *Village Voice* the next day. And so, while it was still damaging, it was a lot less damaging than it would have been had they not let us review it.

ASPATURIAN: Someone from the Pike committee leaked it to the *Village Voice*?

ALLEN: Oh, you bet. Well, I'm not in a position to make allegations, but let me say, as a witness, my suspicions are very high that they not only did it but knew that it was going to be done, and they gave me the option of taking out the most damaging things before they gave it to them. So the result of that is that I have a little more respect for them than I would have had otherwise; but not much, because they were not very responsible.

Now, the Church committee, on the Senate side, was quite different. They were not always friendly and cooperative, but they were very responsible. They did not hold any open hearings until they had worked the issues very, very carefully in closed hearings and understood very well what damage would be done and assessed whether the public good was best served by revealing information in open session or keeping it in closed session. So I have no quarrels at all with the way the Church committee operated. We certainly disagreed on various matters, such as whether certain material should be released or not, but on balance I think they inquired quite responsibly. There were various activities we terminated, but they were for the most part activities that we either should have terminated or which had outlived their technological importance anyway. So the results, as I say, were tedious, painful, but overall not very damaging to the capabilities of the National Security Agency.

So all in all, that ended up dominating a lot of time and effort. I remained in the NSA for a little over four years and found it a very exciting and dramatic place. Schlesinger left after a time. Donald Rumsfeld became secretary of defense, and he was less interested in the work we were doing, but that was, of course, the end of the Nixon administration and the beginning of the Ford administration and all that. We had various involvements in the Watergate affair, but the main niche in our involvement was to be just as careful as possible that we did not get dragged into that morass. There were attempts, for example, to get the NSA to use its very excellent data-processing capability to analyze the Nixon tapes.

ASPATURIAN: Who made the attempts?

ALLEN: The White House. Well, or the prosecutors. I mean sometimes they were working together. The point was not that such a request was improper, from their point of view. The point is that I did not want the NSA to get involved at all, on anybody's side.

ASPATURIAN: Dickey!

ALLEN: Dickey time. So we worked very hard not to be tasked at any assignments, even such things as trying to clean out the noise on all those particular tapes. We were just trying to stay out and point to civilian places where they could do that almost as well as we could, without the risk of our getting dragged into it. So in general that was successful. My predecessor had been involved in meetings that related to "the Plumbers," which was a little embarrassing, but it turned out that he was only involved in getting opinions on the matter and not participating, so it wasn't too bad. And by the time I got there, it was clearly understood by me, and understood a great deal more by Jim Schlesinger, that we should have nothing to do with any of it, good or bad. If we were going to preserve the capability of the NSA, we needed to stay just as far away from that mess as it was possible to stay. So we did, and it worked out all right. We did not get dragged into that. But it made it a very exciting time and a very interesting time. A very fast lesson in civics!

In any event, at the end of my tour with the NSA, I was promoted again, to a four-star general with command of the Air Force Systems Command, which is the research and development command of the air force. I stayed there for a very short period of time—about

nine months—and then became the vice chief of the air force, as just a temporary position for three months, before I became chief of staff, which was my final air force assignment. **[Tape ends]**

LEW ALLEN, Jr.**SESSION 2****July 31, 1991****Begin Tape 2, Side 1**

ASPATURIAN: Let me ask you a question that [archivist] Judy Goodstein wanted me to ask you. Apparently at a dinner that you were both at once, you mentioned that your personal hero was Theodore von Kármán, and she wanted, on the record, a little information about that.

ALLEN: Well, sure. I didn't know von Kármán, but von Kármán in the closing days of World War II was asked by [General] Hap [Henry H.] Arnold to generate a study to define the course of the air force in the postwar period—what they should do about the advances in technology which had become very important in the latter stages of World War II. Von Kármán put together a group of people that ultimately became the first Air Force Scientific Advisory Board, although it wasn't called that at the time. And they generated a document called *Toward New Horizons*, which was a very important document for the air force. Arnold was in favor of the document's recommendations, and his successors were also. And the air force in large measure followed these recommendations. There were a lot of them, but they included creating an organization that would emphasize research and development in the training of officers, up to supporting selected officers through the PhD so that they could be fully up to date on science and technology. The idea was to permit officers to have a career that would involve both line duty and research duties, and many other recommendations. It turned out that those recommendations were being effected in the air force just as I was beginning my career. So in a very large measure, the career that I had in the air force followed the pattern that von Kármán had laid down in that early study. And it was an unusual recipe, not followed by the other services, and one that I think was appropriate for the air force to follow and that did good things for the air force in the early years. Certainly the career pattern I followed was really quite unusual, in terms of normal service careers, but it was in a certain sense directed by these recommendations of von Kármán.

ASPATURIAN: I see. Because your career integrated research and service to a very high degree.

ALLEN: Right. And I ended up in command assignments, although I really spent the majority of my career in research assignments.

ASPATURIAN: Yes. So I can see how von Kármán's recommendations would kind of dovetail with what you found yourself doing. Now, about that list I gave you: Are there any areas there you'd like to talk about?

ALLEN: Well, you ask about the kind of relationship that existed between military intelligence and the CIA. During the really rather brief time I was with the CIA, that was in fact a very touchy issue. The CIA did estimates and analyses of Soviet military capabilities and so did the Defense Intelligence Agency, and they were highly competitive, some of which was healthy, but some of which was not healthy when it went too far. And therefore there was a fair amount of rivalry. In general, the CIA had the better people, because they had a better personnel system. On the other hand, a great deal of military expertise resided in the Defense Department, and therefore the DIA had an easier time calling on that. It was kind of interesting that when I reported to the CIA to be the deputy to Schlesinger, the director, my deputy, in turn, was going to be a West Point classmate of mine who was in the army—Danny [Daniel O.] Graham. He had, just at the time that he came over and joined me at the CIA, published an article that was intended to motivate the DIA analysts to do a better job. The thrust of the article was to say, in effect, the CIA is stealing our birthright and we've got to get back and do a better job of taking these responsibilities for doing assessments of Soviet military capabilities. The article was viewed unfavorably by several people who thought it was really an attack on the CIA; therefore the whole business of my moving under Schlesinger and then Graham coming in was viewed by some as an attempt at a military takeover, at least of those parts of the CIA. And as a result, my promotion to lieutenant general, which was associated with the job and was nominated by the president, was never approved by Congress. The Senate refused to approve my nomination, not because of anything they had against me but because they felt that Graham's coming over was an indication of a sinister plot.

ASPATURIAN: So your nomination got caught in turf politics?

ALLEN: Oh yes. And I was never confirmed. I didn't become a lieutenant general until six

months later, when I took over the National Security Agency, and then the appointment was made while Congress was in recess, so that only the president had to approve it. And they weren't really hung up on me anyway, so it turned out not to be an issue, as I say. I was never confirmed at the CIA.

You asked about the shifts in the governing political parties. Well, of course, when a new president comes in, whether he's in the same party or a new party, he wishes very quickly to make his own impact on the government. Obviously these impacts are much larger if it's a different party. My years in the Nixon and Ford administrations, of course, were years of such turmoil anyway that the Carter administration coming in was bound to be different. Like all presidents, Carter was very anxious to make an early, rapid impact. A president thinks he has only four years, and he can't afford to spend many months trying to get things under way or else he'll never get anything done. So these changes tend to be quite dramatic. And then, of course, between the Carter and Reagan administrations there was a vast difference in political philosophy, so those things were felt very quickly.

I really have nothing to say about detente. It did not have any impact on me, so I really have no comment on it.

You asked about personal interactions with important figures. Well, there are a lot of those; I don't know what's useful.

ASPATURIAN: Things that you think might be interesting from an anecdotal or historical standpoint. [Laughter] I can ask you to think about it. Last time, I think, as we ended, you had just become head of the air force. I was going to ask you to talk about that, and then of course there's the full spectrum of years at the Jet Propulsion Laboratory.

ALLEN: The job of chief of staff of the air force is to organize, train, and equip the air forces of the country for their combat role. The chief of staff of the air force actually does not have a command responsibility, because that's given to the unified and specified commanders. The air force chief of staff sits on the Joint Chiefs of Staff, and the Joint Chiefs of Staff in turn have a command responsibility. It is really the implementing arm of the secretary of defense. The first two years of my service as chief of staff were, of course, with Harold Brown as secretary of defense, whom I had known for many years. But not intimately. [Laughter] Harold Brown is a

very brilliant but often kind of cold person to deal with, and therefore to say that I was a big buddy of his would be an exaggeration. But we had known each other for some time, and I thought he was a very good secretary of defense. He was very loyal to the president, and many times when the president made decisions on which he would have recommended differently, he was always very careful to represent the president's decisions as his own. This brought him a lot of criticism from military people when some of the decisions were ones that were not very favorable to defense interests. That was not really deserved, because he would have wished it to be another way. It took a lot of courage.

During the latter part of the Carter administration, after the invasion of Afghanistan, President Carter changed his views about the defense establishment a great deal and really became very positive about the need for a strong military establishment, and he instituted a large number of programs that involved a buildup of U.S. military capability—which, as a matter of fact, was just beginning to be implemented at the time of the Reagan administration. It's not often recognized, but in many respects the Reagan buildup had really been started by Carter in the latter part of his administration. He was very surprised, and I think shocked, by the Soviet invasion of Afghanistan, because he had had reason to believe—in retrospect, naïvely—that he was making some progress in his relationship with the Soviets, and he did not regard this as a very friendly act at all. So the Carter administration began with quite a negative view on defense from the president and ended really with a very positive view from the president.

Brown was, of course, very concerned about the Strategic Arms Limitations Treaty—SALT—which was never ratified by the Congress but which was supported by the Joint Chiefs of Staff, in a fairly controversial action.

ASPATURIAN: In what respect was it controversial?

ALLEN: Several. One is that a fair body of people really were not enthusiastic about strategic arms treaties with the Soviets at all. Others felt that this particular treaty did not address some of the main issues, which was that SALT created a symmetry between the U.S. strategic forces and the Soviet strategic forces. They felt the U.S. should have stood stronger and gotten a better treaty. And really, the START [Strategic Arms Reduction Treaty] treaty, which has only just now been finally signed, was the attempt to do that, and of course START has been successful in

making a major readjustment in the most threatening aspects of strategic arms on both sides. And SALT did not do that. It placed limits, but it did not achieve any reductions, and the limits it placed were not regarded by many as being terribly effective in restraining the arms race anyway. So there was a good deal of opposition to SALT, and the conservative elements felt very strongly that the treaty really should be rejected and we should start again. We in the Joint Chiefs of Staff concluded that while SALT was modest in its accomplishments, it was nevertheless a useful treaty and represented as good a basis as one could have for starting the next round of negotiations. So we recommended its passage accordingly, which drew some criticism. There was very bitter criticism later on by the incoming Reagan administration. They really took a dim view of that altogether.

The future course of strategic weapons in the air force was the biggest issue that the air force faced, and Harold Brown was deeply involved in trying to formulate decisions on how to go ahead. Carter had terminated the B-1 bomber program and became personally involved in trying to find a satisfactory solution to the modernization of the land-based ballistic missiles. And toward the end of the Carter administration, I think we really had found a basing system that was more or less acceptable. But it was rejected by the Reagan administration when he came in, so it never got anywhere anyway.

The Carter administration, with Harold Brown being quite enthusiastic about it, did begin to emphasize air-launched cruise missiles, which were a significant addition to strategic arms and in many ways did offset the loss of the B-1 program. When the Reagan administration came in and of course restored the B-1 program, it also started the B-2 program. But unfortunately the Reagan administration canceled the basing system that had been worked out for the modernized ICBM [Intercontinental Ballistic Missile], and to this day, ten years later, no satisfactory basing system has been attained.

ASPATURIAN: To your knowledge, was this one of the things that fell under David Stockman's budget ax?

ALLEN: No, this was far more doctrinal. That is, Reagan had campaigned against the basing system that the Carter administration had formulated, and therefore felt obliged to stop it. The trouble with that was that it was stopped without an alternative. And as I said, ten years later we

really have not found an alternative basing system. So that is a very frustrating thing that we can solve now best by negotiating the whole thing away. [Laughter]

But those four years were interesting and challenging years. I was fortunate in that I knew Brown well enough, say, not to be all that chummy but to understand and to respect him, and I think we managed to work together reasonably well. The secretary of the air force, at the time I became chief, did not understand Harold Brown and did not get along with him.

ASPATURIAN: Who was the secretary of the air force?

ALLEN: He was originally John Stetson, and Harold Brown, in his typical way, did not tolerate at all well people who didn't understand what he wanted, [laughter] so he was unmerciful on the secretary, who quit fairly shortly thereafter.

ASPATURIAN: Was the secretary a bit of the "good ol' boy" or were he and Brown simply not on the same wavelength?

ALLEN: It's very hard for me to really judge the merits of people issues like that. Stetson was a successful businessman and believed himself to be very capable of seeing the big picture. Harold Brown is a person who I think does see the big picture but does so on the basis of a capacity to remember, assimilate, and understand incredible amounts of detail. And therefore in any meeting in which the secretary of the air force and Harold Brown would discuss the budget, Mr. Stetson would address global issues and Secretary Brown would cut him short on the global issues and go on to some detail that Mr. Stetson was not prepared to handle. Stetson would get annoyed and would then turn the rest of the discussion over to me. Dr. Brown can be difficult if he wishes to be.

When the Reagan administration came in, they sent an advance party in—a so-called transition team, which let me know that they were not happy with my presumed liberal actions with regard to the SALT treaty and all, and that I would be removed from office very quickly. But it turned out that that didn't happen, because the total amount of destruction this transition team would have caused, had it implemented all the things it really wanted to implement, would have been awesome, and so the actual actions of the Reagan administration were simply more mild. But they did bring about changes and a significant increase in the defense budget. Mr.

[Caspar] Weinberger was very much a different kind of secretary of defense from what Brown had been.

ASPATURIAN: In what ways, particularly?

ALLEN: Well, he was more financially oriented. He'd come from Reagan's budget side. But his main characteristic was really just determined doggedness, in that, having decided with the president what the program should be, he defended that position before Congress and never gave an inch. [Laughter] He was extraordinarily persistent and uncompromising. He actually got away with that pretty well for a while, although of course at the end of his tour I think he had created such animosity with Congress that he wasn't able to do the programs and jobs he could before. Originally, his absolutely unyielding posture had actually worked pretty well.

[Laughter] But he is characterized by a very dogged, very doctrinaire, very great loyalty to Reagan. The Secretary of the air force was Verne Orr, who was a Pasadena person and had been, again, on the financial side of the Reagan governorship. He had been California's director of motor vehicles and served on the Reagan cabinet for a number of years. I got along quite well with him, and we're still friends. And he got along reasonably well with Weinberger—although, again, Weinberger was a very tough secretary.

ASPATURIAN: One thought occurs to me. You said that the Reagan administration originally was suspicious of your ideological leanings. I have to say that you are probably politically quite a bit more liberal than that administration was. Was it difficult for you after they took over?

ALLEN: Yes, initially. The transition team was really slightly to the right of Attila the Hun—they were really very, very conservative. And again, given this normal presidential desire to make an instantaneous impact, we had recommendations that were very, very drastic. That was moderated greatly when the final team actually came on board. Again, they initially had some ideas that I guess were what you could properly describe as conservative, but at least they were different ideas. Some of those were, I think, also a little strange and a little exaggerated to begin with. But again, they moderated some as time went on. But both administrations offered a challenge—Carter being initially really quite anti-defense and Reagan being very pro-defense.

ASPATURIAN: It went from dovish to hawkish, in other words.

ALLEN: Yes. As I say, in a way that's not really exactly right, because Carter was changing a lot and was very concerned about the world situation and no longer believed the Soviets were easy to deal with. So he really became a fair hawk himself.

ASPATURIAN: Did you have enough contact with either or both of these men personally to form a kind of impression of them for the record?

ALLEN: No, I don't think so. I met with both Carter and Reagan on a significant number of occasions, but in all honesty they were fairly formal occasions—that is, where the Joint Chiefs of Staff would meet with the president and present their views on certain issues and the president would respond and there was a certain amount of scripting going around. The president was prepared by his national security advisor, obviously, on what the issues were; a good deal of thought was given ahead of time as to how to present it. These were cases in general where, for one reason or another, the secretary of defense was anxious or maybe sometimes intolerant of having the joint chiefs present their views independently of the secretary of defense. And those are important mechanisms to have, but there's very little feeling of seeing the president.

[Laughter]

ASPATURIAN: They don't make for a lot of give and take, no.

ALLEN: No, not really. The chairman of the joint chiefs, of course, has many more interactions with the president; and I think in the instance of the present time, the relationship between Colin Powell and President Bush is quite close. But that was not true of the chairman while I was there. And in fact it wasn't true for the chairman who succeeded him with Reagan, and then I'm not so sure who succeeded him.

ASPATURIAN: Yes, in fact it seems to me that until General Powell, the chairman really hasn't been very visible publicly, either.

ALLEN: Well, in a certain sense he shouldn't be. I'd say he normally should not be. Because it

really is the secretary of defense's job to be the visible party in that total thing. And the national security advisor to the president is quite visible in some administrations and not so much in others. He is quite visible, certainly, in this one—very visible. But I think you'd have to say the present arrangement is a healthy one.

ASPATURIAN: OK, that's an interesting picture. How did the offer to become director of JPL come about?

ALLEN: Well, I don't know. I don't know how the search committee or whatever decided to talk to me and all. But the first inquiry I got was from Harold Brown. He was then a private citizen in Washington. I didn't really know very much about JPL; he described it as sort of scientific and said that I would be contacted and so on.

ASPATURIAN: Did you get the impression that he had recommended you?

ALLEN: I didn't get the impression one way or the other.

ASPATURIAN: From what you say about Dr. Brown, maybe that's understandable.

ALLEN: I just don't know. And then—I don't remember the sequence exactly, but I believe Murph [Marvin L.] Goldberger [Caltech president 1978-1987], whom I had also known for many years, called me after that and discussed it with me. And then I had a visit in Washington from some elements of the search committee. This was all before I had retired from the air force. And shortly after I retired [1982], a formal offer was made.

ASPATURIAN: Had you seen the facility?

ALLEN: Yes. Shortly before I retired, I made a surreptitious visit [laughter] to the lab and met with the executive council of the laboratory and so on. I say "surreptitious" in the sense that I wasn't supposed to know, in theory, that an offer would be made, and in theory they didn't know that I was coming out to visit the lab. [Laughter] In fact, you know, the offer was going to be made, unless everybody decided, after my visit, that it was all a bad idea. And the people at the

lab knew that. One plays the game, in circumstances like that. And it was a very nice visit, and I was certainly very favorably impressed with the laboratory. Apparently they weren't too deeply depressed by me.

ASPATURIAN: It was a match.

ALLEN: It was a match.

ASPATURIAN: What were your initial impressions of JPL?

ALLEN: Well, first of all, the missions at JPL are of course very exciting, and the people at JPL are highly professional, which meant I had absolutely no difficulty in beginning productive interactions with the people here. I was assisted a great deal, of course, by the fact that the deputy at that time was Charles Terhune. He was a retired air force general. You may recall that many years ago—I believe it was way back in the beginning of the Apollo days—NASA had become convinced that JPL was very good at technical leadership but not good at business management and had therefore insisted that William Pickering [JPL director 1954-1976] take as a deputy a retired senior military officer, who would presumably have some management capability. And so that had been a trend up until the time I came. My understanding from the history is that the first one did not work out well, the second one worked out pretty well, and then the one after that was Charles Terhune, who worked out extremely well. He was very much admired by everybody in the laboratory. He and I obviously had great empathy, and he was extremely helpful getting me started, and so we worked it out very well.

When I first came to JPL, of course, there was a lot of concern that the programs were going down, and the budgets were going to be cut and all. And that began to turn around rather quickly. I don't think that was because of me but because of the circumstances of actions already under way. The planetary program, which had for a while looked as though it was going to be in complete disarray, actually was restructured by a committee that had been established with a charter to define a planetary program within certain budgetary constraints that would meet as many of the goals as possible that the Space Science Board had set out. And that committee—which Arden Albee, I know, for one, was on—really did an excellent job and defined lower-cost missions and programs to go ahead and all. And that program was generally

accepted by NASA and so had a significant role in getting JPL back on a solid track.

ASPATURIAN: Going back to your arrival [October 1982], I gather from looking at your file that there was a certain amount of apprehension initially about a scientist who had had a military career coming in to head up the lab. This was written about in the *Pasadena Star-News* and the *L.A. Times*, for example. To what extent were you aware of this, and what steps did you take to deal with it?

ALLEN: Well, even before I got here, they were commenting in the national publications. *Time* magazine wrote about a military man taking over JPL. And so that was really a matter of some concern, and Murph was a little worried about it, but outside of being sensitive about it and taking some care not to do things that would seem inappropriate, it really didn't prove to be much of an issue. The defense work that was under way at JPL was, in any event, growing when I got there. There weren't major new initiatives taken; the ones that were under way already had a pattern of growth to them. And that work was appreciated by most people at the laboratory as being interesting work that helped JPL maintain its vigor at a time when budgets were kind of down. A few people who left the laboratory and the campus felt that the lab shouldn't do any defense work at all, but that issue had already been debated long before I got there. My predecessor, Bruce Murray, had made the basic proposal around 1981 that the laboratory should undertake some higher degree of defense work, and the Caltech faculty had agreed with that, so that issue had already been worked out. So although it was a matter of concern, it did not result in anything we couldn't deal with, one way or the other.

ASPATURIAN: Did you come to JPL with a vision of how you wanted to see the lab evolve?

ALLEN: No, I'm sorry to say that I did not. [Laughter] No, the issue really was to deal with the circumstances that existed at the time. There was a course being formulated, which was really a very sound course, and they had a set of issues having to do with the way the budgets were handled and the way the staff reacted to all that. But as for having a vision, I'd have to say no. [Laughter]

ASPATURIAN: But I imagine that fairly early on you did have a clear picture of what some of the

major challenges were going to be.

ALLEN: Well, sure. At first, of course, the laboratory was still feeling bearish. The budgets were not high, and there had been some layoffs, and as all that began to turn around, the problem really became the other way about—that is, the lab was growing and the facilities were not really suitable to accommodate growth. And there was a desire on my part, and on the part of other managers at JPL, to constrain that growth, but we didn't have all that much ability to do that, because we were, after all, highly motivated to do whatever NASA wanted us to do. So the growth did continue, perhaps somewhat moderated by our anxiety to constrain it to the facilities.

NASA was very good about new buildings. I mean, they built a number of new buildings while I was there. All were acquired in accordance with the master plan, which was very helpful. But the master plan—and, indeed, each of the new buildings—was attained with the understanding from NASA that the aim was not to promote growth but to perform better the things that were being performed. So the buildings improved the quality of the workspace at JPL considerably—the physical plant was still mostly left over from World War II—but they did not provide room for growth.

LEW ALLEN, Jr.

SESSION 3

March 31, 1994

Begin Tape 3, Side 1

ASPATURIAN: Now, when I last interviewed you, almost three years ago, you had just arrived at JPL. When you got there and you began to get the lay of the land, what did the key priorities look like?

ALLEN: My predecessor, Bruce Murray, had left several months earlier. He had left in kind of a huff, because he felt that NASA was not supporting the planetary program with sufficient vigor. He was very disappointed, and he decided he would leave to do other things. So when I came in [October 1982], there was a sense that the planetary exploration ambitions of JPL were not receiving appropriate support and that JPL in fact would need to diversify its activities in order to retain adequate support. As we discussed last time, a decision had been made during Bruce Murray's tenure that the lab would take on a certain portion of work for government agencies other than NASA. Some rules had actually been formulated that specified that between twenty and twenty-five percent of the lab's total work could be undertaken in activities that were not supported by NASA, provided that they were generally consistent with the missions and skills that NASA wished to have developed. NASA would need to approve such projects, because they would be done under the basic NASA contract. And NASA agreed with all that.

So, prior to my arrival, there had been some exploration with the Department of Defense, the Department of Transportation, and the Federal Aviation Administration, and some work was still continuing for the Department of Energy, that all fell into this category. There was a notion that because of my military background I had been drawn in to enhance the Department of Defense work. Of course I was interested in that and did follow it with care, but it was not particularly a priority of mine to increase that.

So the priorities really were to try to get the planetary program established on as sound and orderly a footing as possible and then to diversify as much as was appropriate but no more. The planetary program had been reorganized in accordance with a committee that NASA had put

together on which a number of JPL people participated. It was called the Solar System Exploration Committee, and it had formulated a program for the future of planetary exploration.

ASPATURIAN: This was before you came?

ALLEN: Well, part of it had been done before I arrived, and my recollection is that it was beginning to come into its final stages by the time I got there. My recollection is that shortly after I got there—I don't remember exactly how long, perhaps four or five months—the report of the Solar System Exploration Committee was generally accepted by NASA and provided a strategic plan for planetary exploration over the coming decade. Now, that plan turned out to be reasonably good. It did plan for a scaled-back Magellan mission to Venus, for the Mars Observer mission, and of course for the continuation of Galileo and the start of the Cassini mission to Saturn. It represented a fairly sound plan, most of the elements of which have turned out to be implemented over the decade since it was formulated. And that was really a major improvement in the prospects of the laboratory in terms of its relations with NASA in the planetary area. The plan had been formulated by knowledgeable people, including people from JPL. It was endorsed by NASA and provided a priority list of how to go about these things and, obviously, with normal concerns about funds and location.

So the major priority really became one of trying to begin the work associated with that plan and to encourage NASA to provide the requisite funding and to keep the laboratory focused on those planetary activities. Even though the planetary programs at JPL have decreased over the years and did decrease during the time I was there, in terms of the fraction of the total laboratory effort devoted to planetary exploration, it has always remained the unifying theme of the laboratory and the activity that all the JPL employees, no matter what particular task they're working on, still identify with. They consider JPL as a laboratory to do planetary exploration, even though they may be working on an Earth-observing mission or a Department of Defense mission.

So the priority was clearly to get together as orderly a program as possible along these lines. And that generally has worked out.

ASPATURIAN: Were there specific ways you went about implementing this that worked particularly well or that caused problems?

ALLEN: When I came in, the relations with NASA had been kind of strained, because Bruce Murray was upset that his particular vision of how the planetary program could be conducted was not accepted by NASA. And as a matter of fact, what led to his decision to leave were apparent decisions by the Reagan administration, as it came in, to do very little in the planetary area. It turned out that they weren't really hard decisions, but he was quite convinced that there was a political decision to very much de-emphasize planetary exploration as part of the NASA program.

Now, by the time I got there, the crisis that had precipitated his concern had in a significant measure passed, and the Reagan administration was not so ruthless with regard to planetary exploration. It wasn't a high priority with them, but they still were perfectly willing for NASA to set up a reasonable base of priorities. And NASA itself had a plan that involved somewhat lesser expenditures than had been envisioned earlier but was still a sensible plan that they seemed quite willing to pursue in an orderly way. So establishing good working relations with NASA was an important part of ensuring that this program was reasonably well supported. We all spent a good deal of time working with NASA and being careful that we were seen as a supporter of NASA, as a laboratory that was prepared to establish and implement programs that NASA wanted.

And then other things began to work out very well. Shortly after I arrived, the IRAS [InfraRed Astronomical Satellite] satellite was launched. Now, that was not a planetary program but it was a very exciting astronomical program. JPL had taken it over from Ames Research Center when it had gotten into some difficulty, and that caused a fair amount of controversy, but in fact it worked out very well and the satellite was extremely successful. It operated for about nine or ten months, until its cryogenic fuel was used up, and during that time it surveyed the entire sky in the infrared several times and in several different ways and accumulated a vast amount of data that is still being analyzed, really, a decade later. And the success of that and—as the years went by—the obvious enormous scientific value of the data was very good for JPL's reputation and for NASA. As it became clear that the data was massive and that the scientific value was great, and that it would require a great deal of work to reduce and analyze the data to obtain scientific conclusions from it, a farsighted decision evolved between JPL and the Caltech campus. We suggested to NASA that we move the data-handling to a new facility on the campus and move the employees from the JPL payroll to the campus payroll. The point was that the

center for data reduction would become more of an academic activity and should therefore be structured to be much more friendly to scientists and visitors from other universities who wished to use the data. That was done, and Caltech advanced the money for IPAC [Infrared Processing and Analysis Center] on Wilson Avenue. It was built very quickly, using a sort of prefabricated technique, and it turned out to be very good both for Caltech and NASA. That also went a long way toward helping JPL and Caltech both stay in good favor with NASA.

Then, of course, during all this time, Voyager was continuing to proceed very successfully. JPL's reputation rode very high on Voyager and on Voyager's success at both Uranus and Neptune. During my tenure, there was great publicity and great credit to JPL, and those were obviously very satisfying endeavors.

ASPATURIAN: When you arrived at the lab, you were in a sense an outsider from DC. You had a background in aeronautics and satellite work, but to some extent this was a different area. How did you go about familiarizing yourself with space exploration?

ALLEN: Well, first of all, it wasn't that different. I did know the satellite and the space business. I was certainly aided in the transition a lot by Charles Terhune, who was the deputy director and who also came from an air force background. He had essentially been ready to retire, but he stayed on for another year or two. He'd been at the lab for probably ten years, and so he provided a very nice bridge, in that he clearly understood very well the culture from which I came and he understood the JPL system very well. He was a tremendous help in making the transition seamless. It really didn't cause any trouble.

The JPL senior staff, which I made no changes in for the first year or so, was very competent, good, and easy to work with. I found no impediment to being accepted by them and working with them very closely. Arden Albee was the chief scientist at the time; he was certainly very helpful. He was an active participant in the Solar System Exploration Committee and in the various plans we formulated for the future, and was a good bridge to the campus. I'd known Murph Goldberger for a long time, so I had no problem working with him and the campus environment, so that all was fairly smooth. And while JPL is an organization that does its activities for science, it is primarily an engineering organization. Its business is building satellites and flying them to distant places for the benefit of scientists, only a few of whom are at

JPL and most of whom are at universities. Therefore, the issue for me was not really to become a preeminent space scientist but rather to ensure that I understood the programs and projects under management and the ways in which they were associated. These were things I was certainly familiar with. Cost, scheduling, and engineering problems are the sort of issues that always come up. So the background was not really inapplicable.

ASPATURIAN: Now, one of the things that happened at JPL during your tenure was the increased emphasis on Earth exploration—using satellites to study Earth’s climate, geology, and so on. Can you discuss how that came about?

ALLEN: Well, sure. Number one, there was, as I mentioned, an interest in diversification for the very practical reason of making the JPL program more robust, given NASA’s budgeting constraints. That is to say, the principal program was—and remains to this day—very vulnerable to NASA’s various budget reductions; because NASA does not have the planetary program as its topmost priority. And as interest began to grow in Earth observations, it became clear to us at JPL that the lab’s previous experience had been observing planets, using remote sensing to provide a kind of global perspective of the planets. Those were the techniques and skills that were going to be involved in Earth observations as well. There was really no reason at all why we couldn’t take the skills and techniques from planetary exploration and apply them to Earth. We even tried hard to use language such as “Earth as a planet” and so on, so that NASA would see the connections very clearly, even though the lead agency in this area has always been Goddard [Goddard Institute for Space Studies]. And that had a fair amount of success. JPL had already done some excellent work using aircraft and balloons in Earth observations, and I tried very hard to encourage that to go forward as vigorously as possible, because it was a proving ground for instruments and techniques that would add a great deal of credibility to JPL’s proposals for Earth-observing spacecraft. And then we began to propose instruments for the various Earth-observation platforms that were being developed. One of the first was the upper-atmosphere research satellite, which has been working now for some years very successfully and I think is still in its active phase. One of the major experiments on that was a JPL experiment, a microwave sounder that worked extremely well. So JPL’s status as an Earth-observing agency was certainly enhanced there. We also took an interest in proposing instruments to be flown on

the space shuttle, and several of our instruments were flown on the shuttle in the Earth-observing area. The most significant of those probably is a Synthetic Aperture Radar system, which has been flown several times and will be flown next week again in a much bigger version than was flown before. And the results from those radar flights have been quite spectacular.

But before I got there, JPL had done Seasat, which, although it lived only a little over a hundred days, was quite successful. It got a great deal of data during those hundred days, and at least proved the success of JPL-developed techniques for observation. And Seasat led to the Topex experiment, which is now flying and which is the radar altimetry experiment, which is again, I believe, very successful. All of these Earth-observing activities were very important. They were an adjunct to the planetary missions, quite consistent with them, and really, from the JPL point of view, a kind of seamless part. Not from the NASA view, because they were managed differently. I think they continued to provide a very useful set of scientific work for JPL.

The interest in Earth-observing areas was, like everything, influenced by people. Moustafa Chahine, the chief scientist, is very interested in Earth observation. And Charles Elachi, who became the assistant director for experiments and things, is the principal investigator for the Synthetic Aperture Radar program on the satellites. So the two of them obviously brought a great deal of personal interest and drive and credibility into the field, which caused them to propose an effort with NASA and to work in this area quite vigorously and, I think, quite successfully.

ASPATURIAN: To what extent were you personally involved in planning the Galileo and Magellan missions?

ALLEN: Galileo was all planned before I got there. It was already under construction when I arrived. But over time there were so many changes to Galileo that I don't exactly remember which one happened under my watch and which ones happened earlier. By the time I got there, Galileo had already gone through several changes—that is, it had originally been on an expendable launch vehicle, Titan 3. Then it was at one time going to be launched in two parts, and so forth. But by the time I got there, the Galileo configuration had settled down to the shuttle. Let's see if I can remember this correctly. When I got there, the upper stage for the

Galileo was going to be the Centaur rocket. The plan was that it would be launched out of the shuttle bay and boost Galileo to Jupiter. The Challenger accident immediately caused a reevaluation of all safety aspects associated with the shuttle, and the NASA administrator, in meetings that I attended, made the very traumatic decision to cancel the Centaur on the basis of safety. Using Centaur would involve flying cryogenic fuels in the shuttle bay, and they just didn't want to do that. So that option was canceled; and the alternative was to use what's called the IUS—the Inertial Upper Stage—which had substantially less performance than the Centaur. The only way to get to Jupiter using the IUS was to use what came to be called a VEEGA [Venus-Earth-Earth Gravity-Assist] trajectory—that is, go to Venus and then to Earth, and then to go around the sun once again and come back to Earth, and then finally get enough energy that way to get to Jupiter.

ASPATURIAN: What's called the slingshot?

ALLEN: Right, the slingshot. But this was a triple slingshot—one off Venus and two off the Earth.

ASPATURIAN: Very ingenious.

ALLEN: It was very ingenious, and of course it's on its way to Jupiter now and all that's working quite well. The problem is that it took a great deal of time and involved a lot of changes in the spacecraft, which may or may not have had anything to do with the main antenna not fully opening. In fact, it certainly had a bit to do with it, because on the original trajectory the antenna would have been opened immediately after launch. Under the revised plan, one had to keep the antenna furled and keep sun shields around it to try to control the temperature for, I believe, roughly three or three and a half years before attempting to open it. And when it was opened, of course, it ended up sticking. The sticking was due to a lubrication problem, and I guess we'll never know for sure whether it occurred, because Galileo was carried to Florida and back and then to Florida again, and during all that time a decision was made not to open up the antenna. It was thought that it would withstand all that okay, and the three and a half years in space. It turned out that that wasn't quite right, that the antenna must have lost lubrication somewhere in the process, which caused it to stick.

So Galileo was always bound for the shuttle during my time, but the plan did change from the Centaur to the solid-propellant upper stage and a complicated trajectory.

ASPATURIAN: How about some of the other missions?

ALLEN: Well, of course Magellan has been extremely successful. Part of the Solar System Exploration Committee's activities was to conclude that the Magellan mission should be scaled back. The original plan had been to have a newly developed antenna—essentially, it was going to be the same antenna that was used on Galileo—and to have a fairly complicated spacecraft system that would have a lot of modern capability. And an attitude-control system that would have been really very efficient. When it was decided to scale the mission back, one had to back off from all those things. It was decided to use the Voyager antenna, which was a fixed antenna. It was decided to back way off on the attitude-control system, in such a way that the spacecraft would have to be flown in an elliptic orbit instead of a circular orbit and take data during its periapsis portion and then send it back during its apoapsis portion—which was less efficient but enabled one to simplify the design. And then a decision was made to use a number of subsystems that came from either Voyager or Galileo. So this way, there was a higher confidence in the hardware than there would have been in the newer developments, and we got the cost down. All those things were done, and of course there was a lot of concern that there would be a reduced science return from the mission. But in fact the mission gave marvelous science return, and the inefficiencies have certainly been forgotten. The fact is that it worked very well for a long time, and we got several different surveys. Originally it would have been possible to survey the entire planet in one Venusian year, and this way it took two, but it worked out just fine.

We had troubles during the development of the spacecraft. One of the major problems was that the radar receivers were being made by Hughes Aircraft Company, and they got into trouble. They had tried to use an inexperienced team at Hughes, and they ended up not being able to do the job, so we had to pull all of that work back to JPL and do those things in-house, which turned out all right. Martin Marietta was the spacecraft contractor and did a good job on it, and so overall it was quite successful. But it ended up that JPL did more on the hardware than was originally envisioned.

ASPATURIAN: Was there one mission throughout the time you were there that particularly excited you?

ALLEN: Well, one always has to say that Voyager is kind of the crown jewel. Even after all these years, it still represents an absolutely spectacular achievement, one that is going to be very hard to equal for many, many years. And that's of course because we were going to planets that we had really never been to before, and we were doing it with such a novel and unusual trajectory approach and the system all worked so well. I wasn't there for Jupiter and Saturn, but the encounters at Uranus and Neptune were just absolutely spectacular, and it was a real discovery mission. No one knew much about those planets, and here was the first opportunity to observe them up close. The one thing you can be sure of in all areas of planetary exploration is that there are going to be big and significant surprises, and there certainly were at both Uranus and Neptune. One just has to say that those were very, very satisfying and exciting and really a great thrill to be a part of.

Now, in many ways, as far as scientific achievement goes, IRAS has got to be counted way up there in terms of scientific return, but it was less dramatic, in the sense that one really knew very little about the data during the time of the mission. Because of the way the system was designed, it sort of pumped out this vast amount of data, and everybody was so busy just organizing it and being sure the spacecraft was operating right that nobody had the chance to start exploring the observations until probably a year after the mission was over. We knew it had worked well and we were getting out little tidbits of things throughout the mission, but the scientific reward was really spread out over five or six years, with people just continuously looking at its findings. It was a source of great amazement to everybody how the people at IPAC would get through all of the data one particular way, end up making all kinds of discoveries, and then would refine their analytical tools and go back through the data again a different way and make a whole new set of discoveries. As far as I know, they're still doing that. The data still yield scientific discoveries, even after all these years. But the immediate impact was not as dramatic as Voyager, because it was spread out over a significant amount of time.

Magellan was certainly also very exciting, because we did see things on the Venusian surface that had never been seen before. Again, it was a little unlike Voyager, in that the discoveries were spread out over several years, but it has to come in pretty close to Voyager in

terms of satisfaction.

The radar flights, which were largely Charles Elachi's projects, also turned out to be extremely exciting. The discoveries of the hidden river channels in Egypt were very dramatic and stirred lots and lots of interest. So there were many of these activities that were really great fun and were a great credit not only to the scientists—who were, of course, off-lab exploring the data—but also to JPL for its overall manning and direction of the program.

ASPATURIAN: Did you have a personal philosophy or style of management that you consciously or perhaps over time introduced to JPL?

ALLEN: I don't think I had a style. Well, my style was much different from Bruce Murray's. Bruce Murray was, of course, a renowned scientist with a long academic background, and he operated in a manner that one would sort of expect from that—a lot of interest, a lot of enthusiasm in particular things, but not as orderly a manner as one might have thought was appropriate. And certainly, coming from a different background and being obviously less qualified in the scientific area and all, I was more interested in making sure the organization ran well. So I guess you'd say there were aspects of management that were certainly different under me than under Bruce. I think my aim was probably closer to Pickering's, which was to ensure that one had a good organization and that there was a clear and adequate delegation of responsibilities and authorities to people in the organization to do their jobs efficiently and well. I certainly made it a strong point to work very closely with my assistant lab directors, to know them well, to meet with them privately at least every week, and to understand what was on their minds—what they were doing and why, and supporting what they needed to do. The executive council, which is really a grouping of all the assistant lab directors, would meet weekly and then also have off-site retreats periodically to go over everything.

And all of that was an effort—I think, successful—to build a sense of teamwork and cooperation and working across the various areas and making sure that JPL was functioning as coherently as possible for a lab that was doing lots of different things.

ASPATURIAN: Speaking of lots of different things, you mentioned, in the beginning, Department of Defense work. I was wondering what the feelings were about that.

ALLEN: There was a certain degree of controversy, in that there were some people at the laboratory who felt that it was inappropriate for JPL to do much in the defense area. They brought those feelings with them to the lab when they came to work on planetary programs and said, you know, we chose JPL because we did not want to go to an aerospace contractor in the L.A. area. Now, the fact of the matter is that this was not at all the view that was held by most of the people who had been at the lab for a while, because the lab, of course, began as an army lab. The people who were there in the fifties and the sixties all recall that there had been no problem at all in reconciling work for the Department of Defense with work for NASA. As a matter of fact, they felt they got a patriotic satisfaction from doing work for the Department of Defense. So although there was concern expressed by some, it was by no means a universal feeling. And I think that once the laboratory people as a whole began to get it into their heads that my sense of priorities was not focused on defense but was really focused on the planetary program, and that I intended to support the defense activities as a necessary and appropriate adjunct to the planetary program, the concerns about all that entirely disappeared. In fact, after a year or so, that just wasn't an issue.

Now, the defense work did remain an issue in the sense that the amount one did was always a subject of major debate, because one didn't want to do so much that it in any way competed with the NASA work, which always had first priority. There was a general consensus on that, so one had to debate kind of carefully how one went about seeking defense work, so that it didn't end up in any way limiting our role as a resource to NASA. But as a matter of fact, what ended up happening after I had been there a couple of years was that the problem became not one of keeping the lab alive, which was what Bruce Murray had been so concerned about, but of having more work at the laboratory than we could comfortably do in the facilities that existed. Everyone in the JPL organization had an understandable enthusiasm for doing more work in their particular area, so everyone would propose and advocate as much work as they possibly could, but the result was that we substantially outgrew the facilities and ended up having to use a lot of leased facilities elsewhere in Pasadena. I became convinced, and in working with the other leaders of the laboratory we gained a shared conviction, that we really needed to put constraints on, if you like, the marketing activities of our people, because they were getting out of bounds and there was really no reason for JPL to be bigger than the number of people we could house on the site at Oak Grove.

So one of the great recurring jokes of my last three or four years there is that each year I would set targets for us to come down to and would set ceilings on the number of personnel, and what have you. But I enforced these ceilings in a fairly permissive way, and it turned out that the people at JPL were always so clever that we never got anywhere near the ceilings. So I was forever being teased. The standard executive council meeting was one at which I would issue new ceiling numbers and at which every one of the ALDs [assistant laboratory directors] would say, "Well, there's just no question, we'll get down to those next year, but we do need exceptions this year for doing the work that clearly needs to be done." As a matter of fact, I'm amused that now my successor, Ed [Edward C.] Stone, has found himself facing kind of the same problem, but I think in fact for other reasons he's had to take a more proscriptive approach. So maybe he'll succeed in controlling the acquisitive nature of the JPL people.

LEW ALLEN, Jr.**SESSION 4****April 4, 1994****Begin Tape 3, Side 2**

ASPATURIAN: During our last meeting, you mentioned diversification in a couple of connections. One was with the Earth-orbiting reconnaissance, and another was with the DOD work. I understand JPL diversified into a number of other areas under your stewardship. I wondered if you would talk about those a little.

ALLEN: Well, I'm not sure I know which ones you're speaking of. JPL did some work for other agencies. For example, it did a few tasks with the Federal Aviation Administration when they upgraded the air traffic control system. We did try very hard to increase the support of technology programs, an effort that was helped very much by NASA's decision to construct the Microdevices Laboratory at JPL, which was a true state-of-the-art laboratory for research in microelectronics fabrication techniques. I think that has proved to be a very productive laboratory, and it had been supported by various government agencies.

In the NASA area, of course, there was diversification in the sense that, in addition to planetary research, we were contributing to astronomy. There was IRAS; and then the follow-on to that, which was to be SIRTf [Space InfraRed Telescope Facility]—another large cooled space-based telescope for infrared observations. SIRTf was intended to be one of the four Great Observatories—the other three were the Hubble Space Telescope, the gamma-ray observatory, and the X-ray observatory. But the SIRTf program, like all the Great Observatories, was in the several-billion-dollar class, and as of this date, it has not been funded, although continued technology is being carried out.²

We also got into the Earth sciences, a vigorous program, and then, in association with all of these programs, did much in the way of shuttle flights—some microgravity work, for example, and other projects associated with the shuttle and in anticipation of the space station. JPL did contribute to the early design part of the space station by sending a team into Reston,

² SIRTf was eventually funded and launched in 2003, renamed the Spitzer Space Telescope—ed.

Virginia, to assist NASA in its systems-engineering capabilities for the level-two team that was created there in Reston. We leased that building for them as a convenience for the government. But after several years that came to an end, and in fact that facility doesn't exist anymore. They moved all that work to the Houston facility.

ASPATURIAN: I read in several places about the Microdevices Lab, which I guess occasioned quite a lot of interest and excitement.

ALLEN: Oh yes, it is still a very productive laboratory. In the same vein, in order to ensure that we stayed at the forefront of leading technologies, we made the decision, partway through my tenure, to acquire a Cray supercomputer. There had been a significant amount of resistance in the past, because JPL had gone very much in the direction of being a VAX-oriented laboratory. The individual investigators all liked this, because a VAX minicomputer was a size that they could justify in their budgets and they then had total control over it. So there was a grass-roots enthusiasm for the minicomputer, and getting a supercomputer was not favorably looked upon, because it required some kind of centralized approach and then allocation of time for use. But I finally decided that it was simply necessary to make a move and get a supercomputer; and that if you build it they will come.

ASPATURIAN: And they did.

ALLEN: And they did! So it worked out fine.

ASPATURIAN: You headed up a National Academy of Sciences panel evaluating the Reagan administration's policy with regard to technology transfer.

ALLEN: Yes, export control.

ASPATURIAN: That committee created a certain amount of stir when it issued its report.³

³ National Academy of Sciences, National Academy of Engineering, and Institute of Medicine, *Balancing the National Interest: U.S. National Security Export Controls and Global Economic Competition* (Washington, D.C.: National Academy Press, 1987).

ALLEN: Quite a bit.

ASPATURIAN: I wonder if you could talk a bit about your experiences there.

ALLEN: This was actually a panel convened under the auspices of what's called the academy complex, which represents the National Academy of Sciences and the National Academy of Engineering. The export-control laws were due for a change. There was a good deal of interest in Congress and elsewhere to have an objective study done, because there was a great deal of disagreement within the administration about the policies on export control. The Commerce Department wanted to establish a considerably more liberal policy in export control, and the Defense Department was very, very restrictive. So there was a need for an objective study, which was not in alliance with anybody's camp, to take a fair look at all this, and therefore the academy decided to do it on their own initiative. They started it on their own and funded it to some degree with their own resources, and then solicited funds from various government agencies. It was a self-directed study, if you like. In order to be sure that it was objective.

As we began the study, it was clear that we were going to come down on the side of less control, in favor of more trade, the arguments being in large measure that the very tight export control policy advocated by a certain group in the Pentagon was really not working anyway. There were too many avenues for evasion of those particular rules, and we were not convinced that they were well designed, even so. They were mainly aimed at keeping high technology from the Russians, but they were based on the premise that (a) the Soviets couldn't get this technology elsewhere, which they generally could; or (b) that once they got it, it was going to do them a great deal of good when it required an ability to produce things by reverse engineering, which in general proved very difficult to do. So our general attitude was that a very substantial relaxation of the rules could be made without any harm to national security at all. That was very roundly opposed by the person in the Pentagon responsible at that time, who was Richard Perle. He made many public statements impugning my integrity and that of the panel and all kinds of things, which really made it quite nasty. But in general the conclusions that we made were ones that came largely to pass in the reissued export-control legislation. As events turned out over the next several years, even before the collapse of the Berlin Wall, I think the judgments we made have proved to be about the right ones.

ASPATURIAN: How were you asked to head up this panel? Whose decision was that?

ALLEN: Well, I don't really know. Frank Press, the president of the NAS, asked me to do it, and so I presume that it was his initiative. It could have been Murph Goldberger's suggestion to him.

ASPATURIAN: Sure. How many people were working with you on this issue?

ALLEN: I would think we had a panel of about fifteen. Some very good members.

ASPATURIAN: I can imagine. With an NAS panel, the caliber would undoubtedly be quite high.

ALLEN: People like John Deutch, who is now the deputy secretary of defense; Bobby Inman, who is momentarily the nominee for secretary of defense [laughter]; the former secretary of the treasury, who was very good, and the former secretary of defense—they were all on it. They were very sound members and very helpful.

ASPATURIAN: And this was a fairly unanimous opinion you reached.

ALLEN: It was unanimous in the sense that we agreed on a report. It took a little bit of persuasion of some of the members.

ASPATURIAN: Then there was the "Hubble Trouble" telescope. Can you tell me a little about that also?

ALLEN: Again, I was asked—in this case, by NASA—to head up the investigation into the cause of the Hubble's misshapen primary mirror. There were actually two different approaches to this. One approach, which was undertaken by another group, was to examine the data from the telescope in orbit and try to understand from that data what was really technologically wrong with the telescope. My job was different. It was to go back and review how the telescope was made and see if we could find out what went wrong in the manufacture that would result in such an error. And it turned out that we did. That was really a very interesting examination and one that I really enjoyed, largely because we did find the fault. Otherwise it would have been pretty

frustrating. We found that there had been a particular instrument used to measure the surface accuracy of the primary mirror and that instrument had been made incorrectly. But there had been so much reliance placed on it that its results were regarded as the right ones even though there were data from several other instruments that indicated that there was trouble somewhere.

So it was an interesting situation, one in which our findings were very critical of both the manufacturer and the people supervising the project for having allowed a thing like that to slip through. Subsequently, the contractor did agree to repay a substantial sum of money to the government. Also, the fact that we could diagnose the problem so accurately and actually found the instrument in the same condition that it was in when it measured the telescope meant that we were able to prescribe the nature of the problem to a very high degree of accuracy. That in turn permitted the design of the corrective optics to be made with a very high degree of confidence. The on-orbit measurements were difficult—it wasn't easy to interpret them, but we knew exactly what the cause of the error had been and therefore knew exactly what it was, and that permitted an exact determination of the formula for the corrective optics. So that was really a very interesting and satisfying thing to do.

ASPATURIAN: It sounds very gratifying. Had you ever been involved in that kind of detective-work sort of problem before?

ALLEN: I don't think one comparable to that, no.

ASPATURIAN: Sounds like you might have had fun digging.

ALLEN: Well, it was fun. NASA was very good and they allowed me to put together the committee to do it, with some exceptions; they wanted to be sure a few people on there represented a particular background. But in general I could pick some of the people I knew and had confidence in, and so we got a group together that was really quite effective.

ASPATURIAN: How long did you work on that particular problem?

ALLEN: It wasn't really all that long. I'm guessing a bit now, but I think we really had our conclusions in about six weeks. Then I think it was another six weeks by the time we got the

report out, so probably three months altogether.

It did move fast, and it was interesting, because it was the first activity I'd done like that where the fax was the mode of communication. We found out quickly that we really didn't need to have a lot of meetings, because we could exchange data and views by fax very effectively. And what we ended up doing was prescribing the tests and measurement that we wanted to be made at the manufacturer's plant. We actually hired a young optical scientist to be in residence there to follow what they were doing, and then he would fax us daily about what was coming out on the measurements. So we ended up having a relatively small number of meetings, and yet we were able to conduct a very dynamic and fast-moving investigation.

ASPATURIAN: When you took over JPL from Bruce Murray, did he have any departing words of wisdom for you?

ALLEN: Well, not really. Actually, he left around May of '82 and I didn't come until October. So he'd really been gone a while by the time I arrived. We certainly chatted, and he certainly offered me his perspective on the lab's history and on some of the people there whom he'd found to be particularly good and particularly helpful to him. All that was very helpful. But, as I say, he'd been away for long enough that there wasn't really a transition.

ASPATURIAN: When you look back on your tenure at JPL, is there something you are proudest of and perhaps something particular that you would have liked to have seen done differently?

ALLEN: As to the first, I enjoyed the whole sequence of things very much while I was there and I'm quite proud of it all. But I think, really, the basic answer is that the strength of JPL is a lot of very, very good people, and so the high points were working with them and having the opportunity to interact with them, and from time to time they'd even provide a little of the guidance as they moved along. So I think rather than incidents or events, which of course are high points and fun, the thrill of being at JPL really is the thrill of day-to-day interaction with a very, very good group of people.

I'm not sure what we would have done, with the benefit of hindsight, differently [Laughter]. I think we did things about as well as they could have been done. There were certain things that were frustrating, but that's always the case. Obviously the Mars Observer

failure has to be attributed to work that was done and decisions that were made largely on my watch, and in retrospect that was a troubled program, although the failure itself was not a direct result of that. The Mars Observer originated under a kind of faulty premise by NASA as to how it should be done, and that approach led to the project getting overextended in several areas.

ASPATURIAN: What was the premise under which NASA was operating?

ALLEN: There were really two things. NASA wanted to go to Mars using a type of spacecraft that had been used frequently in lower Earth orbits and therefore had a production record and history behind it. The idea was to use that as a means of saving money. That turned out not to be nearly as good an idea as one might have thought. To make it suitable for the Mars mission the spacecraft had to be modified so much that it ended up having very little legacy from the equipment that really mattered from the Earth-orbiting spacecraft. Now, I think the only thing we could have done about that, to have made it more responsive to NASA's desires, was to have used it with fewer modifications that would have held the costs down, but unfortunately that would have also had the effect of creating less confidence in the reliability of the spacecraft. So I don't think it ended up working very well.

The second thing was that NASA concluded that by making this decision about the spacecraft, they would then conduct their instrument selection process in such a way that the scientists designing and building the instruments would be obliged to make their instruments fit the spacecraft. This was a major departure from nearly every previous mission, where the spacecraft was, to a significant extent, designed around the instruments. That idea didn't work, either. NASA started out that way, but as soon as they found themselves with very good proposals for instruments that would do really marvelous measurements but required a modification of the spacecraft, they opted for the good instrument, even though it meant making the modification. So the spacecraft ended up being, again, significantly modified to accommodate the instruments rather than the other way, as NASA had originally intended. So that didn't work very well.

The results of all that were that the whole program was more expensive than it was expected to be; plus all those modifications to the spacecraft. And it is probably true that because those modifications were rather substantial, the spacecraft manufacturer did not really

bring any special skills to what he actually built, because he wasn't carrying over much from the many other spacecraft he had built. Then with the pressure to restrain costs, we were constantly having to work with that contractor to keep costs under control, and the results perhaps were that the spacecraft had some flaws in design and testing. Now, as is widely known, the cause of the failure of the spacecraft has never been well identified, but the failure review board on that one was critical of the process used in manufacturing it that could have led to a catastrophic failure.

ASPATURIAN: Was the back-and-forth that went on with NASA over Mars Observer representative or typical of dealing with the agency during your directorship?

ALLEN: Well, I'm not sure it was typical, because Mars Observer was one case. The problem that JPL has always faced in planetary spacecraft is that the spacecraft are so special, to do such long-distance and long-term missions, that they inevitably require very special care in their manufacturing and testing, which ends up making them quite expensive. And the trend from the early years, when the missions lasted just a few days at the planet or were just a fly-by, to later years, when the missions were of very long duration, has been for the cost of the missions to go very high. Galileo was an extremely expensive mission, as it finally turned out. NASA, or anyone reviewing the planetary program, always is hopeful that one can find a way to build smaller spacecraft to take advantage of more production and newer technologies and thereby get the cost of the missions way down and offset the concerns about reliability with multiple spacecraft. And so everybody who has ever reviewed these programs has that as a kind of going-in idea of what would be the best way to conduct the planetary program.

It almost never works out that way, for several reasons. The first one is that these small spacecraft with small instruments on board still end up requiring a fairly substantial booster to get to the great distances, so they may end up maybe being cheaper, but not cheap. And then you face the prospect of doing a relatively small amount of science, because the total scientific capability of the spacecraft has been compromised by downsizing the instruments. The result in general has been that after having gone through this exercise, one becomes unconvinced that the smaller, cheaper spacecraft is really the best way to go. So almost every mission has gone through a history of growing beyond projections and getting too expensive, then being cut back in various ways; and then provoking a very large argument about how to finally end up.

Magellan was scaled way back and ended up working out fine. Mars Observer was started as a scaled-back project and that didn't work out very well. Galileo was not scaled back, and it has worked fine except for the one glitch on the antenna. Cassini has been under constant pressure to scale back because of cost, and it is very hard to scale back, because it's a very ambitious mission. But some scaling back has been done.

Then the current NASA administrator, Daniel Goldin, has very strongly advocated faster, cheaper, better kinds of ways of doing spacecraft. The Pluto fly-by mission, which is currently under examination at JPL, is an attempt to do that. It's an attempt to do a very lightweight, very high-technology spacecraft, while cutting back on many of the other costs. It may be that it will be the one that works. Certainly the work they've been doing with it is very imaginative and creative, and it may just be a way to get to Pluto, which is so hard to reach with an affordable kind of mission.

ASPATURIAN: Do you think that "faster, better, cheaper" is viable, or is that to some extent an oxymoron, given the delicacy and complexity of what is involved here?

ALLEN: Well, I think the Galileo and Cassini missions have gotten so big and expensive—as did Hubble—that future missions are very unlikely to be repeated on that scale. Hopefully, Cassini will stay alive, but it will certainly be one of the last of those very expensive missions. And the rest of the missions that NASA has had on the books—for example SIRTf, and even AXAF [Advanced X-Ray Astrophysics Facility], have all been judged as too expensive, and it is necessary to find some way to do the science less expensively. So I don't think one can really fight that trend. That is, one is simply going to have to find ways to fly missions less expensively, and that will mean doing less science. But there are ways to do credible science using the most modern technologies and very clever people, and that will lead to some cheaper missions.

ASPATURIAN: Well, that's an optimistic and, I hope, accurate projection. One question that I wouldn't have asked you two years ago, because we had a different administration: If you were to be called in by our current Mr. Science, Al Gore, and asked—looking back over your history with JPL—what advice you would give on the future direction of America's space program, and what the benefit of your experience has been that you could share with him, what would you tell

him?

ALLEN: Well, I think that's very difficult. I mean, I am not a supporter of the space station—not for reasons that are critical of what it would intend to do, but just for very practical reasons, in that I think it has gotten too expensive and inadequately defined in an engineering sense. Its costs are not going to be under good control, and I think it's just an inappropriate program in the scale it's been planned for. So I certainly would not advise, frankly, continuing the space station. I think there are probably alternative approaches, which include the extended-duration orbiter and some more Spacelab kinds of things, using the shuttle bay, that I think are more appropriate to do and that can keep that kind of science moving along well. I think NASA would be well advised to move in that direction, to emphasize more strongly its aeronautics and science programs and just kind of get out from under what is, I think, the crushing burden of the space station.

At the moment, the idea of making it a joint program with the Russians has given it a new lease on life, in the sense that now it is attractive enough to people that they are less critical of the space station. But I think that's still not a very good bet and that there are probably other ways to find cooperative endeavors, and that this would just make it too expensive. And once again, the new collaboration takes a design that had perhaps begun to converge a little bit and shifts it to a higher orbit and makes the shuttle loads more problematical. I just can't believe that the costs for all that are all understood.

ASPATURIAN: What about the other areas of space science?

ALLEN: I think there are many areas of science to be done in space. The Earth-observing system to look at global climate change is a very intriguing program. I think it got out of hand, in that NASA had originally had it tied to the shuttle and the space station program in a way that turned out to be really quite inappropriate. It had a hard time getting back out of the very large scale of spacecraft platforms all that led them into. I think one can do the science in smaller, more frequently launched platforms, which I think goes to the thesis that smaller, better, cheaper, faster would work well. But there will be a need for good, long-term climate observations, and they can be best made from space. There is still planetary exploration being done that is very exciting, and there is still astronomy to be done that is very exciting. So there is a lot of space

science.

ASPATURIAN: What are your feelings about a manned mission to Mars, assuming the unmanned mission gets there successfully?

ALLEN: I think it's fine to have a dream of a manned mission to Mars. My reservations, again, only have to do with the scale of the endeavor. I think it is just such a massive endeavor that it's really quite unreasonable and inappropriate to begin the commitment to it in, let's say, the foreseeable future, and by that I mean the remainder of this decade.

But to have it as a vision is fine. It's an exciting thing to do, and there may be a point in the future at which it is appropriate to talk about a mission of that scope. It would have to be multinational and involve major financial commitments by other nations. It would clearly be an endeavor that would require a lot of time to muster the support and the financial commitments to get a significant program under way. So I don't look for that anytime soon and wouldn't put a whole lot of effort into preparing for it, other than the unmanned missions to Mars, which can well provide a scientific basis for understanding that planet in a better way over the next decade and would lay the foundation for what one could do for a manned mission to Mars.

ASPATURIAN: All right. Anything else?

ALLEN: Well, one thing you didn't ask much about was the people at NASA. James Beggs was the administrator during the early part of my tenure at JPL, and I enjoyed working with him. He was good. I think in some ways working with Mr. Beggs did lead to the decision to put the space station engineering effort originally at the Johnson Space Center, and I participated in convincing him that that was the right thing to do. And that was an error. It did not work at all there and has gone through two iterations since then, one of which is creating a systems-engineering center in Reston, Virginia, and the other is moving back to Johnson, where it might work better the second time around.

Mr. Beggs, of course, left under a cloud, due to his being accused of incorrect dealings in his pre-NASA role as a contractor. That was a bad rap and was certainly known by me and others to be a bad rap at the time it was made. It was a tragedy that a really distinguished public servant went through this—nearly two years of being under a very severe cloud while an

indictment was held over his head. Finally the Justice Department basically apologized and the indictment was dismissed, but by that time he had spent two years without any ability to clear his name. It was a very bad sequence of events and very poorly handled. After he left, under a cloud, the question of his successor at NASA was hotly debated. I was asked to be that successor by the White House and did not wish to be. Whether I would have been selected or not if I had indicated interest I don't know, but anyway I declined, and Jim [James C.] Fletcher, the former director, was pulled out of retirement to come back, which was all right. The shuttle accident occurred during the interregnum, when Bill [William R.] Graham was acting administrator. Of course, that was all very unfortunate, and Mr. Graham took a fair amount of criticism for his handling of all that. And then Fletcher received the assignment to rebuild the shuttle program, which ended up going very well. Admiral [Richard H.] Truly replaced Fletcher and now, of course, Goldin has replaced Truly. Working with the various administrators has always been interesting. But in fact each of the NASA administrators has had the manned spaceflight programs as his primary concern. In ways, that has been good, in that JPL has a good reputation with NASA and is highly regarded by NASA. As long as JPL doesn't get into too much trouble, the NASA administrators are mainly interested in working in the manned flight areas, and JPL is fairly well treated, and that's been the case. Len [Lennard] Fisk was the associate administrator for space science and applications. I felt he did a marvelous job, but of course he and Mr. Goldin didn't hit it off well, and he left very quickly. But he was pleasant when I was there. **[Tape ends]**