
Photo by Leigh Wiener

JESSE L. GREENSTEIN
(1909-2002)

INTERVIEWED BY
RACHEL PRUD'HOMME

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Abstract
Interview in three sessions in 1982 with Jesse L. Greenstein, DuBridge Professor of Astrophysics, emeritus. Greenstein discusses his early career at the Yerkes Observatory of the University of Chicago, under Otto Struve (1937-1948), and his arrival at Caltech in 1948 to build an astronomy department in the Division of Physics, Mathematics, and Astronomy. He discusses the early partnership between Caltech and the Carnegie Institution of Washington in running Mount Wilson and Palomar Observatories, the interactions between observational astronomy and theoretical astrophysics, and the rise of radio astronomy. Besides his discussion of his work on stellar composition, the interview contains his recollections of such twentieth-century pioneers of astronomy and astrophysics as Struve, Grote Reber, Gerard Kuiper, Edwin Hubble, Fritz Zwicky, Walter Baade, Rudolph Minkowski, H. P. Robertson, Richard Tolman, and Fred Hoyle—and of various Caltech principals including Lee DuBridge, Earnest Watson, Arnold Beckman, and Robert Christy. He also discusses his service in the 1960s as chairman of Caltech's Faculty Board and member of its Aims and Goals Committee. He speculates about the scarcity of women astronomers and the

difficulties they face. In an addendum to his interview, he discusses in more technical detail latter-day changes in instrumentation, the impact of new and improved detectors, and their contributions to his work on white dwarfs.

Administrative information

Access

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Contact information

Archives, California Institute of Technology
Mail Code 015A-74
Pasadena, CA 91125
Phone: (626)395-2704 Fax: (626)793-8756
Email: archives@caltech.edu

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Jesse Greenstein stands beside the wide-angle camera he designed with fellow Yerkes astronomer Louis G. Henyey in the late 1940s. A spherical concave mirror with 12-inch aperture is surmounted by three struts supporting a multi-component lens. The camera covered a field angle of 140º with critical definition and produced photographs similar to naked-eye observations. Today's "all-sky" cameras are largely the progeny of the Greenstein-Henyey device.*

Jesse Greenstein (identifiable by tie and tweeds) holds the Greenstein-Henyey wide-angle camera, which is being photographed by the person reflected in the mirror. The façade and domes of the Yerkes Observatory appear along the circular rim of the reflection. The camera was used by Donald Osterbrock and Stewart Sharpless in 1950 to produce spectacular photographs of the Milky Way, the zodiacal light, and the counterglow.*****

***** A full description of the Greenstein-Henyey camera is given in Otto Struve, "Photography of the Counterglow," *Sky & Telescope* 10 (July 1951), 215-218. The Archives wishes to acknowledge the help of Prof. Donald E. Osterbrock of Lick Observatory in explaining the construction and use of the camera.

California Institute of Technology

Oral History Project

Interview with Jesse L. Greenstein

by Rachel Prud'homme

Pasadena, California

Caltech Archives, 1983
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PRUD'HOMME: Let's start by talking briefly about your work at Yerkes Observatory at the University of Chicago. When did you get there?

GREENSTEIN: I got to Yerkes in 1937 as a National Research Council Fellow. That was holdable for two years. I stayed through that and then joined the junior faculty beginning in 1939. I left Yerkes in June 1948 to come to Caltech.

PRUD'HOMME: Who were some of your colleagues there?

GREENSTEIN: Well, the most important single figure was the director, Otto Struve, a White Russian émigré and distinguished fourth-generation astronomer from the Baltic German area of Russia and a man of extremely great scientific talent and moral and scientific rectitude.

PRUD'HOMME: He had quite a reputation as a hard and stern leader.

GREENSTEIN: He was a very hard, stern, shy, and very human person whom I greatly admired. He was, in fact, one of the few good leaders I have met in astronomy.

PRUD'HOMME: There were extraordinary developments in the world of physics and astronomy at that time. How did this influence the astronomers working at Yerkes?

GREENSTEIN: Well, probably the largest single invention of the Yerkes period under Otto Struve was the idea of a close collaboration between observational astronomy and theoretical astrophysics. The latter had been a European invention of just the last decade before I arrived at Yerkes, with the development of atomic physics. So that with atomic physics, spectroscopy, and quantum mechanics entering astronomy, what you might call the modern epoch of atomic and later nuclear physics was on us. And Struve's particularly good idea was to bring several distinguished theoretical astrophysicists from Europe.

PRUD'HOMME: Whom did he bring?

GREENSTEIN: The most famous single name is [Subrahmanyan] Chandrasekhar. And he arrived within a year of my coming to Yerkes.

PRUD'HOMME: Where was he from?

GREENSTEIN: He is originally from India, south of India, from a distinguished Brahmin family, and he had graduated from Trinity College, Cambridge, in mathematical astrophysics. By 1934, for example, he had applied the new ideas of special relativity and quantum mechanics to the prediction of the properties of the so-called degenerate stars or white dwarfs, a subject on which I am still working. In fact, the subject was more or less theoretically finished by 1934—not really, but the general outline of the theory was so then derived. A little later, with the refugees, Struve brought in Gerhard Herzberg, who is now in Canada and a Nobel Prize winner. Herzberg built not a theoretical as much as an experimental laboratory for molecular physics, which was also important for quantum mechanics and astrophysics at that time.

PRUD'HOMME: How did the alliance with McDonald Observatory in Texas develop?

GREENSTEIN: That was a result of a gift by a previously unknown wealthy Texas recluse, W. J. McDonald. He left money to the University of Texas, which had no astronomers, to build a telescope which, to quote the will, was to be "powerful enough to look through the gates of heaven." McDonald was an eccentric bachelor, with no immediate family until the will was announced. There were then remote relatives who contested the will. But eventually the University of Texas settled that and made a management agreement with the University of Chicago. I believe that the man who had been a senior administrator at the University of Chicago had moved to be the provost or the equivalent at the University of Texas. And knowing about the existence of the Yerkes group, he got in touch with [University of Chicago president Robert M.] Hutchins, and then [Hutchins got in touch] with Struve. So McDonald Observatory was built with University of Texas money to be used by the Yerkes/University of Chicago staff. It was one of these odd accidents and survived into the 1950s, at which time the University of Texas began to build up its own staff.

PRUD'HOMME: Was this group that Struve had founded what was known as the Midwest astronomers' group?

GREENSTEIN: No, the Midwest astronomers is something I invented, in the sense that I was its secretary. It was an attempt to have regional meetings. Since there were so many state universities within a few hundred miles of each other in the Midwest, we invented a group which I think stretched from Ohio, Indiana, to Wisconsin and sometimes even Minnesota. We had several meetings a year for exchange of ideas, observational programs, and results.

PRUD'HOMME: There's a man named Grote Reber who worked on radio astronomy. Did you know him? Was he involved in this?

GREENSTEIN: Reber was the sole pioneer American radio astronomer after [Karl] Jansky had abandoned the field at the Bell Telephone Labs. Reber was an independent amateur working in a radio manufacturing company near Chicago. He was a bachelor, an eccentric, and a very good designer and scavenger of radio receivers. He built a 30-foot radio antenna in the backyard of his mother's house and did experiments completely on his own, with no contact, until he sent the papers for publication to the *Astrophysical Journal*, which was then edited by Chandrasekhar. Severe questions as to whether the results were faked or not arose.

He was an amateur, but he soon got into moderate contact with the Yerkes people, not only through the publication of his papers but later through friendship between myself, him, and a couple of other Yerkes astronomers, particularly a young American theorist named Louis Henyey. Henyey, myself, and another Yerkes scientist tried to explain both the Jansky and Reber observations of the radio signals from the center of our Milky Way as thermal emission from hot gas, and failed. That was really the first theoretical work in radio astronomy since 1937.

PRUD'HOMME: You worked on the nebular spectrograph with Henyey, didn't you?

GREENSTEIN: Yes. That is a typical Struve idea. Yerkes was, unfortunately, not in a very good climate. I believe that Henyey and I, or I and Struve—I forget which—had a cloudy night assigned with the 40-inch refractor, which is in the same building as the offices at Yerkes. And typically, at midnight Struve was in the library and wanted to talk. He asked, as an abstract question, What was the most efficient possible spectrograph that could be dreamed of? So Henyey and I and Struve dreamed, and within a week or so we had built a wooden assembly of available optical components, which proved to be the most efficient spectrograph possible. It was first tied on the 40-inch refractor and then moved down to McDonald in a modified version. It proved important, because the discovery of interstellar hydrogen clouds called H II—ionized hydrogen—regions was made with it a few years later.

PRUD'HOMME: What kind of war work did you do at Yerkes? And why there? Why did you stay there?

GREENSTEIN: Well, that whole subject is, in fact, discussed in an article by somebody in either *Isis* or *Minerva*—a history of science magazine. It was an article called "The History of the Yerkes Optical Bureau." [See DeVorkin, David, "The Maintenance of a Scientific Institution: Otto Struve, the Yerkes Observatory, and its Optical Bureau during the Second World War," *Minerva* 18, 595-623 (1980).] When war was coming, many a scientist, many astronomers with public positions, like directors or those active in the National Academy of Sciences, worried about what would happen. Struve worried about maintaining a staff against the inroads of what he clearly saw would be largely physics- and engineering-dominated projects. And this article in *Minerva* discusses, from papers available to scholarly research, things that I didn't know, even though I was active in this enterprise.

Struve tried first to build a group of astronomical observatories which would do work for the military and keep the people at home. He tried specifically to collaborate with Harvard, with [Harlow] Shapley, and Shapley had other links already in mind. That didn't work out, so Struve looked for something that his staff might do that would keep people from drifting out of astronomy and perhaps never returning. Struve had a wide imagination on the international role of collaboration, and so he tried various things. I—and Henyey, I'm sure also—had received invitations, for example, to join the MIT group which was going to be [Lee A.] DuBridge's radar group. Another thing in New York was mainly on anti-submarine warfare. Still another group worked on degaussing ships for anti-mine protection. All of these were rapidly being set up, and young people were told that either you were going to be drafted or you were going to work in one of these projects. Well, we were not drafted—though not without some difficulty—and stayed at Yerkes. We had, beyond Henyey and myself, a total of three or four other scientists who worked on various occasions with us. We also developed a shop to build one-of-a-kind military optical devices, at Yerkes.

PRUD'HOMME: Did you do consultant work for the military?

GREENSTEIN: Well, at that time, we were just paid through a grant from the National Defense Research Council to the University of Chicago, one dollar a year or something like that. At various times we were tempted to be lured away. One lure was to what proved to be the Manhattan District at the University of Chicago, eighty miles away.

PRUD'HOMME: In general, the war moved people around. That must have helped the field, in that one couldn't become insular.

GREENSTEIN: Right. Almost everybody found that some scientific specialty had applications and also found that some military devices had scientific promise. Gerard Kuiper, who was almost the founder of modern planetary astronomy, went to a radar countermeasures laboratory from Yerkes. Later, when he returned, he also brought knowledge of the first near-infrared detectors being developed for possible military applications. In fact, he liberated a captured German night-vision infrared device, which he and I experimented with. He also liberated what was called a lead sulfide photoconductor cell. With that, he observed the atmospheres of the planets in the near infrared and was the first to discover that the satellite Titan had an atmosphere. That was done with a formerly classified military device. At Harvard, a man named James G. Baker worked with the air force on reconnaissance technology. He later designed the camera that flew in the U-2 spy plane. And, partly in competition, I went into a very highly classified area after I was at Caltech, which led to the development of what are now called technical means of spying—i.e., reconnaissance satellites, electronic and optical. I stayed in that field till quite recently. There I had to be an outside consultant, because it was work carried on completely outside of Caltech.

PRUD'HOMME: You were appointed to the staff of Mount Wilson/Palomar in 1948, and the following year you were appointed professor and head of the department of astronomy at Caltech. I have basically two areas of questions. One is, what was the observatory like when you came? What kind of equipment was there and who was there using it?

GREENSTEIN: OK, let's do one at a time. The complex arrangements that led to the creation of Mount Wilson/Palomar Observatories were completed before I arrived. I was asked by Earnest Watson, dean of the faculty, to undertake the creation of graduate and some undergraduate teaching in astronomy—to take the lead in acquiring faculty, which faculty would automatically become members of the Mount Wilson/Palomar Observatories staff.

On my arrival at Caltech, there was only one professor—Fritz Zwicky, a remarkable man, who had been shifted from the physics department, where he was a Millikan protégé, to astronomy. He was born in Bulgaria but was a Swiss. He was a son of a consular attaché, I think. And he was so loyal a Swiss that he never took American citizenship. He was a remarkable person, a pain in the neck for me for a generation, but we'll get to that in a moment. The only other person here in astronomy was one of Zwicky's protégés, first a person who taught undergraduate astronomy named Joe Johnson, and then later Al Wilson. Neither of them had regular faculty positions. So when I arrived, there was one professor and myself.

At a rate of better than one a year, we began to build up the Caltech astronomy group. They were all my suggestions. And in a letter from Watson, before I came, he said, “If there were a department at Caltech, you would be department head. And if we create a department, you will be that.”

PRUD’HOMME: You had the reputation of having been the head of the department when there, in fact, was none.

GREENSTEIN: Yes, but there were and are no departments and no heads. But I acted as such; it was operationally true. Now, the other side of the partnership in the observatories was the Carnegie Institution of Washington, who lived in offices a few miles from here and who, as a group, had been in Pasadena as astronomers since 1906 or ’07 when [George Ellery] Hale came to Pasadena.

PRUD’HOMME: Hale got the original money from [the] Rockefeller [Foundation], didn’t he, to build the 200-inch telescope?

GREENSTEIN: Hale raised the money; but Rockefeller would not give it to the Carnegie Institution, of which Hale was an employee. Had it been done, there would have been a simpler administrative setup but possibly less good astronomy.

PRUD’HOMME: The administrative setup was indescribably complex in terms of making decisions.

GREENSTEIN: It made my life miserable. But I could also say it made my life easy. But we’ll come to that. Nevertheless, there were nearby, as colleagues, about twenty astronomers of varying ages who had been doing research in a wide variety of fields, mainly concentrated on observation rather than interpretation. They had created large-telescope astronomy in the world. So you go from Hale to the time I came, which is forty years. You go from a world where the largest reflecting telescope used was a 36-inch at Lick Observatory to a world which, at the end of the Palomar construction, had an incredible number of square meters looking at the sky. Astronomy was the largest privately supported science in the U.S.

PRUD'HOMME: Did they accept your background at Yerkes/McDonald?

GREENSTEIN: I was asked to come, I believe, as their second choice, the first choice being Martin Schwarzschild at Princeton, a very distinguished theorist. It's a hypothesis I have. I had built up a reputation both of observing and doing theoretical work at Yerkes, and my thesis at Harvard had been both in theory and in observation. There weren't too many with that particular combination in the general field and in the right age bracket. I was extremely different from them, on the other hand, as I've said several times. The old Mount Wilson staff, pre-1948, were the most incredible bunch of gentlemen scientists, a breed which doesn't now exist. They didn't agree with my theoretical bent, but they almost never disagreed in the Observatory Committee—which was joint between the Caltech and Carnegie institutions—when I would bring up for appointment the name of a new young theorist I wanted. In fact, essentially all our early appointments were theorists from Yerkes, who all became very good observers. It was my feeling that they would. They were bright enough to do it without any pressure. There was more observational material sitting in Pasadena unanalyzed, and there was more observational possibility with big telescopes than anywhere in the world, even before the 200-inch was finished.

PRUD'HOMME: You said they were generous. Were they generous with these materials?

GREENSTEIN: Anything you wanted you would get.

PRUD'HOMME: And you could publish, using their data, if you wanted to?

GREENSTEIN: Yes. Almost always without more than a credit line, not with a joint author's name on it. There were jealousies only within the cosmology area, which has always been riddled with that. The cooperation in spectroscopy is, in a sense, necessary, because one is drowned in large amounts of material. If you had a typical high-resolution Mount Wilson 100-

inch spectrogram, you could literally spend most of a year measuring wavelengths, light intensities, and getting stellar compositions. You selected the most exciting features of anything you observed.

You asked earlier about the relations between the war and astronomy. In the atom bomb project, new elements were produced. One of the fission products was called technetium, which, although it was very radioactive, still has an isotope that lasts several 100,000 years. The moment its spectrum had been analyzed in the laboratory, which I think was done at Argonne in Illinois, the man who did it got in touch with Paul Merrill of the Carnegie staff. Merrill looked at his plates of a certain kind of red giant star called an "S" star and identified the lines of technetium, an unstable radioactive element synthesized in the last 100,000 years, inside that star. Well, that's an interplay between nuclear physics and atomic physics and astrophysics. It was typical. Mount Wilson had had a big spectroscopic laboratory also to analyze the atomic spectra, and that had a big importance to the development of the theory of atomic spectra.

You originally asked about the nature of the group I found. Let me give you a brief résumé. I mentioned the spectroscopists. They're all gone.

PRUD'HOMME: Who are they?

GREENSTEIN: Walter Adams, the director when I was invited, the distinguished spectroscopist. Paul Merrill. Roscoe Sanford. Edison Pettit, who was working in the infrared and solar work also. And then solar spectroscopists—Seth Nicholson, Bob Richardson. I'm sure I'm omitting very important names. Then Ira Bowen, who had been a Caltech professor, became director on my arrival; he was interested in atomic spectra and continued in that area also. He was much in the Mount Wilson tradition.

PRUD'HOMME: Who was using the work, for example, that Adams and Merrill collected?

GREENSTEIN: Well, they published incredible numbers of papers. There was too much almost. There was too much knowledge and not enough interpretation.

PRUD'HOMME: It's interesting. Having the biggest and the best, you really don't care about other institutions. You don't have a rivalry with other institutions then. So it's very free.

GREENSTEIN: It was. And, you see, in a sense a spiritual difference between Yerkes/McDonald and Mount Wilson/Palomar existed, in that Yerkes had quite a good young staff. I was one of many in the early years there. Mount Wilson, when I arrived, had a very old staff. The youngest man there was a few weeks older than I—Olin Wilson, a very good spectroscopist. Many of these people had been acquiring data and then improving instruments and getting better data, so for them it wasn't really a question of having outside competition. They were only competing with themselves. At Yerkes/McDonald, there were many younger people. There was much more emphasis on theory. And one looked more for new discoveries and new interpretations; there was a much more modern—i.e., highly competitive—attitude.

PRUD'HOMME: And not as insular perhaps.

GREENSTEIN: That's right. It was a highly European faculty. In fact, most of the Yerkes people, when I was there, were of European birth or came from India or something; that was Struve's legacy. When I came here, there were essentially no Europeans. I brought in many Europeans, and then the Carnegie Institution also became less insular.

PRUD'HOMME: Had Hale done all the appointing before?

GREENSTEIN: No, Hale had retired in '38. Hale had appointed this older generation, yes. But by the time I came, in '48, there had been Adams's group, which were somewhat younger, with the same tradition. The closest relation between the Carnegie Institution, the Mount Wilson people, and the rest of the world was with the Lick Observatory. And the Lick people were already also working on the redshift problem. The major paper, with big results for the extension of the Hubble velocity-distance relation, was a joint paper between people from Lick and Mount Wilson and our own graduate students—Allan Sandage—the redshifts of a couple of thousand galaxies. They cooperated very well.

When you ask how I was treated when I arrived, they felt that people from Lick, whose largest telescope was a 36-inch, were experienced users of large telescopes. And I, from

McDonald, with an 82-inch, was inexperienced. So it took some getting used to. Just like Caltech itself took some getting used to.

PRUD'HOMME: Why did you take the appointment?

GREENSTEIN: Well, I'm a complicated history. To go back, I had had a little administrative experience in business and enjoyed it. I liked the activity of the war—though not the war, naturally. I liked dealing with the military, for example. And although I went back gladly to science, I could also see problems in Yerkes' future. Furthermore, I was somewhat of a "hot property" at the time, in the sense of having received an offer from the Lick Observatory to come on their staff. I received in total, I think, three [offers at Lick], twice later to be the director.

PRUD'HOMME: And that didn't appeal.

GREENSTEIN: Not at Lick. Everything is complicated. But essentially, I felt that I could build things up here and that I would enjoy it. And I found, in fact, that it was a pleasure. Almost none of the administrative duties are time-consuming or significant except finding interesting people and keeping them happy.

PRUD'HOMME: I want to talk about that. But I also want you to talk about your impressions of the institute and the community when you came. It must have been an extraordinary change from being in the middle of the cold Midwest [Williams Bay, Wisconsin].

GREENSTEIN: Yes. Well, I'd come before that from a big city. I lived in Cambridge [Massachusetts] for seven years. Life was somewhat sophisticated. One of the reasons we never went to Lick was that it was an isolated mountaintop. I came here, and it was a provincial town, and Caltech seemed incredibly small. I remember walking from the Athenaeum, where I stayed—I think, in November 1947—as somebody's guest, to this building [Robinson Hall] and wondering whether they would ever be able to pay my salary, no matter how small the salary. It was a very tiny institution. I had a very happy personal introduction from a man who is I hope not forgotten—H. P. [Bob] Robertson, a physicist in relativity theory. He was an old friend, whom I'd met when I was a graduate student and visited often at Princeton. He was already here, and he, I'm sure, was one of the decisive reasons for my coming. Furthermore, the Carnegie staff was, in my mind, an enormous one, though very different from Yerkes/McDonald. There were two outstanding Germans, Walter Baade and Rudolph Minkowski, with whom it was easier for me to form intellectual links—though they were in cosmology, galaxies, and nebulae—than with the older Mount Wilson spectroscopists. So the attractions were theoretical cosmology, which was Robertson and [Richard C.] Tolman here; Baade and Minkowski, observational cosmology; and of course, finally, Edwin Hubble, who was remote and not well and with whom I had almost no contacts except unpleasant ones. He didn't think much of me; I didn't think much of him. We got along fine.

PRUD'HOMME: Why didn't you think much of him?

GREENSTEIN: Well, I thought he was a stuffed shirt.

PRUD'HOMME: He was very formal, wasn't he?

GREENSTEIN: Yes. And he lived a life much like the one I led later here, not only centered on Caltech but with intellectuals, friends in other areas. In any case, I had written a paper using the nebular spectrograph at McDonald, which I felt was a devastating criticism of an important recent discussion by Hubble and Tolman on the cosmological interpretations of the velocity-distance data. And I told him about it, and he didn't find my result interesting or significant.

But the astronomy community here was rich, for me. Caltech I knew nothing about, except through Robertson and through, of course, Hale's influence. And I certainly had admired Hale, though I'd never met him. Earnest Watson did the negotiating with me for Caltech and told me that I would have to talk to the Mount Wilson staff about their teaching here. With only Zwicky and myself, and Zwicky an irregular type of astronomer, there was no immediate way of giving the formal instruction needed for the PhD.

PRUD'HOMME: You started out essentially with the graduate department.

GREENSTEIN: Yes. Actually, Joe Johnson, or later Al Wilson, carried the only undergraduate course we had, for some years, till I took it over. Under the original agreement between the two institutions, those people at the Carnegie Institution of Washington who were able to and wished to teach were to be available for the graduate program at Caltech. My understanding had been that they would provide the equivalent of a full-time faculty member—i.e., all of them together would add up to one or maybe a little more, and provide a course every term. That proved unworkable. Ira Bowen, who was then director, tried very hard. Every spring, when I called him up about it, he would sigh and say, "Well, I know Jesse. I'll see." The arrangement provided, for example, a dramatic course by Baade, completely informal and ungradable. But it was certainly inspiration to those who were there. And some practical information. Solar astronomy I could get through the Carnegie staff. I felt, from the beginning, that their failure to live up to the understanding on providing astronomy education could be understood. Maybe it gave me a stronger hand at Caltech on new appointments, because soon the administration here realized the arrangement wasn't working.

PRUD'HOMME: Where did the students in astronomy come from?

GREENSTEIN: Well, we started with a very few. It was right after the war, in '48. The first class had people who'd been in the military—you know, only two or three years after the war. I forget the very first, but they were very distinguished. Helmut Abt is now editor of the *Astrophysical Journal*. Allan Sandage, the leading cosmologist. Chip [Halton] Arp, our stormy petrel of a cosmologist, was also one of the early students. We had very good, but very few, students. Three was plenty each year. I think, in general, that those who went here in the earliest years did get help from the Santa Barbara Street people. [Reference is to Carnegie Institution, since the address of Carnegie Observatories is on Santa Barbara St. in Pasadena.—ed.] They certainly got instruction on how to use the telescopes. Of course, it was also the beginning of the electronics era; electronics was nothing much in the older astronomy and was essential in the newer. So, much of the electronics development came from this side, from the Caltech side. But it was applied to all telescopes.

We lived pretty well, I must say. I don't want to emphasize the administrative mess, because, in fact, with a tolerance on both sides, with their soon accepting the idea that theory was important or that Europeans were tolerable, and our accepting the fact that they wouldn't teach and did use the 200-inch telescope, it did work. It's balanced also by the fact that all our students could use the 100- or 60-inch telescopes.

Begin Tape 1, Side 2

PRUD'HOMME: I want to go back to what the community was like—what your social life was like. What your reaction, as basically a New Yorker and a cosmopolitan person, was to the Pasadena community and the Caltech community.

GREENSTEIN: Well, we'd been somewhat starved at Yerkes, living in a small country town for eleven years. My wife had always been interested in the theatre—and she became managing director of a community theatre, which she ran for seven years, even through the war, with fairies for men.

PRUD'HOMME: At Yerkes? Terrific!

GREENSTEIN: Yes. They had to be imported. It was a summer season only; and she had to import 4-Fs. And one of the good ways of staying out of the army was be homosexual. She ran the theatre; she acted. She's an actress at heart and always has been. Anyway, we came out here and we tried to meet some people. I had friends among the Hollywood writers. So we did, in fact, get to know many people in that different community. Further, Caltech was a remarkably sociable place. I'd heard before I came that the parties were extraordinarily good, and it proved to be true. They were slightly alcoholic, but they were always interesting. The physics department had not only Bob Robertson but Charlie Lauritsen and his son Tommy, and Willie Fowler—all of whom were outstanding party people and good scientists. So it was a mixture. And it was a welcome relief. We were fortunate to have a little outside [family] money always. So it was more fun. We'd been in the social world at Yerkes, but it wasn't much. Here it was pretty good. It was much more convivial; a good deal of science—and later, national affairs—was discussed at parties. Robertson had been in Europe. As a graduate student, he'd been in Germany. Then through World War II he'd been in England, and then with the occupying forces. And he was chief scientist later for NATO. So we met a lot of international friends;

people from the British science establishment would appear at Robertson's parties. Sir Solly Zuckerman, for example, was a leading light in British military science. All kinds of crazy people, also. And of course, though I didn't know him as well, [Theodore] von Kármán always had distinguished visitors, including a baseball player, retired, who was partly his bodyguard. It was an interesting and very lively world. Even in the Mount Wilson/Carnegie older staff, there was this German axis, containing Baade and Minkowski, who liked wine and talk, and talk, and talk, and were lots of fun. Baade coined the phrase to explain the spectroscopists, why they weren't invited to parties; he said, "They don't eat; they don't drink; they don't love"—in his German accent, which I can't imitate. But it was an interesting world. And I did like the writers in Hollywood and ballet and music people. Then, unfortunately soon, Caltech, because of the Korean War, got involved in a high-level project with the military which brought an enormous influx of military visitors and friends. The first half dozen years here were rather hectic, I must say.

On the other hand, I also became a member of an infinite number of government advisory committees in that period. Somehow I managed to balance it all.

PRUD'HOMME: Did DuBridge bring in the government people, or did this just happen after Vista?

GREENSTEIN: The government project called Vista, which is still highly classified, was, I guess, suggested to DuBridge by the military or the joint chiefs or the president—I don't really know. He was nominal head of it; Lauritsen and Fowler operated it. It moved off campus, to the Hotel Vista del Arroyo. It led to very interesting discussions; it certainly got me more permanently into the military advisory thing, although I'd already been slightly involved. I recently looked through the list of committees that I'd joined, and I didn't realize I had been doing so much soon after '46 or '47. The first government committees to give money to astronomy were the Office of Naval Research in '47 and, in '52, the National Science Foundation advisory committees—of which I was chairman for three or four years. So after the war a floodgate of government involvement opened—specifically, military, in the early fifties onward.

PRUD'HOMME: Tell me about Lee DuBridge. What kind of person was he?

GREENSTEIN: Well, charismatic, everybody says—square, which is part of his charisma.

PRUD'HOMME: Physically or spiritually?

GREENSTEIN: He is charismatic spiritually, and he has the best normal virtues of our country. Calling him square means that he is really absolutely straightforwardly sincere and conventional and conservative. And yet he will try anything. He is loyal to his friends. And he likes to see the best in people, which is a fine leadership virtue. And it was very easy to work with him. I never had trouble. The only troubles I would ever have—most of these were appointment troubles. They were never money troubles; money seemed to flow like water, when DuBridge was interested.

PRUD'HOMME: He seemed to have a great knack for finding money.

GREENSTEIN: Appointments were always elaborate, because of the Carnegie link and the fact that you had to convince the Caltech physicists. You see, we are in the Division of Physics, Mathematics, and Astronomy. And the mathematicians weren't too interested, but the physicists cared about astronomy. And so you had to fly a person through the astronomy group, the physics group, the Observatory Committee—which meant the Carnegie group—and then through two presidents. Also, in between, there was the division chairman. But the exciting thing with DuBridge was that he would ask questions like "Was this the best possible man?" If he was convinced of it, "What can we do to make sure he comes?" He always loved to know enough about what was going on now, in the last few months, in astronomy. When he gave a talk or had to get some money, he'd know what to say. And he was an extremely quick learner. He remembered and could explain what everyone had done, even though he was far from the actual work.

PRUD'HOMME: This probably explains why he was so good at getting money.

GREENSTEIN: It was an easier time; this place was growing. I kept graphs of five-year projections of how big the astronomy group was to become. Our goal was to add somewhere between a person a year or a person every other year, indefinitely. Of course, that meant a diminishing percentage growth. I think the general projected goal for the permanent astronomy faculty was originally around fifteen or twenty. We never got much above ten. Unfortunately, at the same time the field had broadened into radio astronomy, where we now have three or four faculty. So we are low in numbers of optical astronomers.

PRUD'HOMME: What kinds of people did you look for?

GREENSTEIN: Myself, I looked for people who could understand the physics that I thought was applicable, which was at that time atomic or nuclear physics, and were good mathematicians as far as astrophysics went, and who might be interested in observing. The first staff member appointed was a Mexican, who became an American citizen and is now in Germany. He was Chandrasekhar's best theorist. Guido Munch came. Also Don Osterbrock, who's a theorist, who later became director of Lick. And Art Code, now a leading space astronomer. I tended to feel that the best input for astronomy were ideas of what was going to be important next. And since we had the best instruments in the world, such bright people would undoubtedly be attracted. It's less true now than it was; people have gotten more specialized.

PRUD'HOMME: Did you eventually promote your own students?

GREENSTEIN: We somewhat objected to keeping students, but we did keep some. We had a very close link with Princeton, from where we later got people. Most of the first came from Yerkes. What often happened is that people either from Yerkes, McDonald, or Princeton would come here as postdocs, and some would get into the Carnegie staff, if their dominant interest was observing rather than interpretation. Next, when in the mid-fifties we started in radio astronomy, that was quite different. We then needed experienced engineering types, because there was nobody here.

PRUD'HOMME: Did you feel that the U.S. was behind other countries?

GREENSTEIN: Oh, yes. We were nonexistent.

PRUD'HOMME: There was a big conference on radio astronomy that you partially organized, didn't you?

GREENSTEIN: I was secretary of it. It was organized largely because of Lee DuBridge. Walter Baade, Rudolph Minkowski, and I had been yelling that radio astronomy observation was essential for optical astronomy, because the interpretation of extragalactic radio sources based on the identifications by Baade and Minkowski had shown that we were finding more exciting galaxies, for example, by radio means than by others. The largest redshift found by [Milton] Humason, who had worked on straightforward galaxies, had been 20 percent. But the next one, found as a radio source, 3C-295, was measured by Minkowski at 46 percent. In other words, thirty years of accumulation of redshifts had gotten to 20 percent; radio astronomy made it jump from there to 46 percent in a couple of years. And then when the quasars came, we went over 300 percent.

PRUD'HOMME: Who were the leading radio astronomers then at that period?

GREENSTEIN: Well, some successful work was done with Jodrell Bank. Most of what interested us was actually done by Australians. And DuBridge comes in here, since as head of the MIT Radiation Lab he had had a lot to do with the British. A lot of those Brits went to Australia and stayed, and Australian radio astronomy was blooming. The first radio astronomy head, John Bolton, was a Brit from Leeds, living in Australia, and had identified many of these extragalactic radio sources. The head of the whole radio astronomy research in Australia was a man named Taffy Bowen, E. G. Bowen, who was an old friend of DuBridge's. It was the old boy, old school tie club, except that none of these characters wore old school ties. They were engineering-oriented, very practical. John Bolton could do a better weld than most welders and welded most of our first radio antenna together himself.

We imported several Australians and their friends and Brits—they were Australian Brits. Even now, radio astronomy is heavily linked with Britain rather than Australia. It has younger postdocs from Britain. A lot of the computer wizards are also from Britain. They have a better opportunity to do science here. So we had a good time leaning on the engineering past of the radar establishment, and that helped us get into radio astronomy.

Well, I wanted radio astronomy at Caltech. It couldn't be under the Mount Wilson and Palomar Observatories, since it's too radically a different technique. DuBridge needed a little convincing; he worried whether radio astronomy would run out of problems in five years and we would be stuck with a dead-end science. So it was quite reasonable to have a conference. We had a conference in Washington [1954], which was very exciting. He had no doubts after that. And out of the conference, in fact, came also the idea for a national radio astronomy observatory, which we didn't participate in, since we built our own. Next, the military, the Office of Naval Research, was quite willing to support radio astronomy as a possibly relevant civilian technology to their mission, and they did. I don't know who DuBridge's friends were. I never worried, because he would always have friends.

PRUD'HOMME: I want to go back to the 200-inch telescope for a minute. Who determines who uses it? How much can you consider it a private facility and how much do you feel an obligation that it should be used and be made available?

GREENSTEIN: Well, let's go back. First of all, under the agreement between Carnegie and Caltech, all staff members had equal rights of access to all the equipment on either mountain, and students—our graduate students, specifically—had rights to Mount Wilson or Palomar. And it was understood that we would not have an obligation to students from other institutions, since we were trying to build a local school, which had to fit the Caltech very-tough-man-physics-course syndrome. We had a hard time getting students who could pass the required physics courses and also were interested in astronomy. So we tended to be a little protective against graduate students from other places, unless they came with a guest investigator, as he was called; and that guest investigator would tell us that a student might use the material for a thesis at a rival institution. That worked out, in fact.

At the same time, although it was completely privately supported—Carnegie absolutely private and Caltech till the mid-fifties largely private—the people who'd planned the agreement felt there was a responsibility to the larger community, since we had the best equipment and there was no national observatory. So the observatories instituted a program in which ten or fifteen percent of the time on all telescopes might be made available on application, let's say, roughly a year before you'd start. The block assignment would be given by the year. It proved hard for outsiders to get time on the cosmological problems requiring dark sky, when there's no moon at Palomar, and during the good season, because everybody here also wanted that. The time-assignment committee would judge on scientific merit, purely, if the applicants had an original idea.

PRUD'HOMME: Who made the judgment between who got the gray time and who got the dark time?

GREENSTEIN: We had our own in-house committee.

PRUD'HOMME: And it was a Carnegie/Caltech committee?

GREENSTEIN: Yes. The director was always on that committee and had a little discretion on overriding the committee; the committee was equal Carnegie/Caltech. Since we owned Palomar, we were a little nervous, but it didn't work out badly. Since many of our students and postdocs went to other institutions and were familiar with the telescopes, their coming back was clearly a scientifically good idea. There were no charges made. As long as there was reasonable agreement that there was no direct conflict with the work of a staff member, I would say an outsider had almost as good a chance as an insider, except in the one area—cosmology.

The visitor programs began to taper off as the permanent staffs grew, and certainly when the national observatories grew; it is more or less dead at both institutions now. It's not completely dead in principle. If they go to Palomar, I know we make a charge for accommodations and supplies but no charge for use of the telescope. We would like to make agreements with other institutions that have private money, and good scientists that we like. If they would pay for, say, not a guaranteed set of nights but essentially the out-of-pocket cost of operation for those nights that they got assigned, that would be marvelous. But we haven't reached any such agreement. You see, as it is, there's no money transferred between Carnegie and Caltech. If our people go to Las Campanas and use the Carnegie 100-inch, they have to pay their own fare, that's all. Similarly for use of the 200-inch by Carnegie people.

It's been a slightly dying institution, this guest investigator business, but it was recognition of our national responsibility. We'd been given $6.5 million in good dollars. We owed somebody something.

PRUD'HOMME: When was the Owens Valley Radio Observatory created?

GREENSTEIN: It was soon after, the mid-fifties—I don't know the exact date. The important man was John Bolton, who was on lend-lease from Australia and went back there. He brought a few people with him, and we supplied the graduate students. The students in radio astronomy largely went through the physics curriculum, with some broadening into electrical engineering; they had to learn astronomy as relevant. And at first all courses given were makeshift, until we got trained students. Now in radio astronomy we've kept our own people—not all, but some. All our radio astronomers have PhDs in physics. There's no real difference.

PRUD'HOMME: You have to have people specializing in optics as well.

GREENSTEIN: Yes. Even optics is not a thing just taught in astronomy. Optics is now taught as computing technology.

PRUD'HOMME: So your specialists have to be Renaissance men, in the sense of scientific Renaissance men.

GREENSTEIN: In optical astronomy they have to be, and in each new field of techniques in optical-related radio astronomy, they are somewhat Renaissance. A good fraction of astronomy with the 200-inch is done by people who are physics professors, notably [Robert] Leighton and [Gerry] Neugebauer and their group in Downs and Lauritsen Labs. Tom Phillips, our leading instrumentalist in radio astronomy, is over there. Our theorists in radio astronomy are in Kip Thorne's group. The name "astronomy" or "astrophysics" is meaningless, in the sense that it's just applying physics to the external universe, not only to the atomic, subnuclear.

I haven't mentioned nuclear physics. When I came, my biggest change in life was in 1951, when I became an honorary nuclear physicist and went into stellar abundance analyses and got to be friends with the Kellogg group. Later they hired astronomers. It doesn't matter where people are.

PRUD'HOMME: Where do your students go after Caltech?

GREENSTEIN: Almost all successful ones go to the bigger institutions: notably Princeton, both the university and the Institute for Advanced Study; to the Center for Astrophysics, which is at Harvard; to the National Radio Astronomy Observatory; to Kitt Peak National Observatory; to the Cerro Tololo National Observatory in Chile, whose director is a Caltech student; some to Berkeley/Lick; and some to JPL [Jet Propulsion Laboratory].

PRUD'HOMME: You've put a finger in every pie, in a sense.

GREENSTEIN: Oh, yes. The students from this place have replaced the students from Harvard as the source of the new talent.

PRUD'HOMME: Is this continuing? You implied that your equipment was no longer the best.

GREENSTEIN: It's not exclusive.

PRUD'HOMME: Is there any place in the world that has the biggest and the best and the greatest and the grandest? Is there institutional competition?

GREENSTEIN: Oh, sure, there is. I would say that at the national observatories, all of them…. The National Radio Astronomy Observatory is much bigger than our radio observatory. I think we've got the wave of the future going in what's called submillimeter radio astronomy, which Leighton, Tom Phillips, and others are developing here. Probably the important future rival doesn't yet exist, and it's not one institution. It is a location, on Mauna Kea, in Hawaii, where all the new telescopes are going to be put or have been put. It's a better location for a lot of things. It's very difficult. It's at 13,000 feet; it's hard to work. And the telescopes belong to Canada, Hawaii, France, Great Britain, and the United States. I mean, it's going to be the center. You can't beat the cooperation of everybody.

The nearest rival as far as productivity in current instrumentation in optical astronomy is the University of Arizona, rather than the Kitt Peak National Observatory. But that's just because they've got bright people. You can't rest on your laurels. If you find something now, you don't talk about it as much as you used to, because somebody can publish it first if you do. They can do important work at other places and you have no monopoly. Also, we have reached a stage in radio astronomy where you have to make new developments every few years to keep competitive. Whether a private institution, even with government support, could do that, we don't know. In optical astronomy, we have enough square inches of telescopes in good locations that if we can keep up with current instrumentation I wouldn't worry, as long as we get bright young people. We, unfortunately, having bright young people, have also become a target, losing those people. We've lost some of our best to other institutions. I can't blame them for going; I left Yerkes. And the other thing is if you don't build up young staff continuously, and get self-satisfied, that's a potential danger. We don't have enough money now to add young people as we used to.

JESSE L. GREENSTEIN

SESSION 2

March 16, 1982

Begin Tape 2, Side 1

PRUD'HOMME: I'd like to go back and discuss with you briefly some of the people we mentioned before and perhaps some of the people we inadvertently omitted who were here when you first got to Caltech. You spoke of Fritz Zwicky and said that he was an irregular type of astronomer. What was he like?

GREENSTEIN: Fritz was a self-proclaimed genius, and in many ways he was one. He was a protégé of [Robert A.] Millikan and had not been completely happy as a member of the physics department.

PRUD'HOMME: Why?

GREENSTEIN: Because his opinions in physics and his methods of teaching were both somewhat amusing and controversial. His teaching was directed to those geniuses who would think as he did. And his interests in physics were perhaps premature for the state of physics then; some involved solid-state problems. He was very much alone in the physics group. I gather, since it happened long before I came, that it was Millikan's suggestion that he become an astronomer. He became eventually a professor of astronomy and gathered a small group of people who were personal admirers and who worked with him in pursuit of what he called the morphological approach to science.

PRUD'HOMME: Did he take these people from other departments, or did he bring them in?

GREENSTEIN: The people who worked with him were all non-regular members of the faculty: temporary employees, lecturers, et cetera. He was very much the European professor, the one person with authority and with ideas. He did moderately well before the 200-inch was finished, for example, using the 18-inch Schmidt telescope on Palomar, for whose construction he pressed and of which he was the major user. His major contributions, for which he was recognized well before I came, were in discovery and study of supernovae—violent explosions of massive stars which have been almost the classical prototype of explosive events in other galaxies and possibly quasars. He ran a program for the discovery of supernovae with the 18-inch Schmidt, and various people went down to Palomar and took photographs of the sky. As supernovae were found, they were announced. He followed them up as best he could, with equipment then available.

As another part of his talents, he was in favor of and pushed the construction of our 48-inch Schmidt telescope at Palomar, which was later used for mapping the entire sky. And he used that later himself, for discoveries of clusters of galaxies. He had a feeling that from fundamental, simple, physical principles one could predict the behaviors of large aggregates of stars, of galaxies, of clusters of galaxies, et cetera. And he tried by elementary subjective methods to deduce important conclusions from such mapping programs. He was violently opposed to Einstein's general theory of relativity, and in particular the expansion of the universe. He refused ever to adopt a distance scale to the galaxies or to call the redshifts true velocities.

PRUD'HOMME: So he wasn't very popular.

GREENSTEIN: He was not popular with the establishment, and he was often very wrong. However, in the study of the clusters of galaxies, which he initiated on the 48-inch Schmidt but which was in fact carried out by a young graduate student, George Abell, Zwicky published several catalogs of clusters of galaxies and also discovered galaxies of interesting appearance, which he also cataloged. Although he misinterpreted some of these observations on the basis of his general philosophical theory, a good deal of his factual discovery in cataloging is in fact his largest claim to fame. It is one of the reasons that the younger astronomers in observational cosmology depend heavily on his work and admire his contributions.

He was an extraordinarily original thinker, but he refused to work either with modern technology or with any elaborate theoretical or measuring apparatus. So he was a problem child; since I was nominally running things, I had my problems with him. He was an irregular; he refused to teach in any conventional way. He taught a course in physics for which admission was at his pleasure. If he thought that the people were sufficiently devoted to his ideas, they could be admitted. He taught a classical mechanics course in physics, which was passed with a hundred-percent grade by a nonexistent student, who was a composite of students and professors of physics who wrote the best papers that Zwicky had ever seen. He was teasable, and he was gruff and violent and everything.

One of the worst parts of this situation was that he had collaborated with the great Mount Wilson observer and cosmologist Walter Baade in the discovery of supernovae. He and Baade split violently. They were a dangerous pair to put in the same room; in fact. Zwicky called Baade a Nazi, which he wasn't. And Baade said he was afraid that Zwicky would kill him.

PRUD'HOMME: Very strong.

GREENSTEIN: Yes. He was a very interesting person. I think he'd be worth a good deal of study. His papers and essentially all his letters, I think, are in Switzerland. There is a Zwicky Foundation in Glarus, where he is viewed as a great thinker. He has certain characteristics of nineteenth-century German idealistic philosophy, I would say.

PRUD'HOMME: One isn't necessarily trained to be a collaborator.

GREENSTEIN: No. He was impossible. I collaborated with him once, through all the years we were together. I fought with him perhaps ten times a year. There were always difficulties. His publications often included violent attacks on other people. At one time, the director of the observatories, Ira Bowen, had troubles and tried essentially to institute a censorship of Zwicky. That caused trouble. There was always a certain temptation not to give Zwicky enough observing time with the 200-inch, because so little of what he did with the telescope led to formal publication. On the other hand, if one just thinks of the ideas now prevalent for what's inside active galactic nuclei or inside quasars, Zwicky really thought of them first. If one thinks of neutron stars, Zwicky thought of them actually before [J. Robert] Oppenheimer—in a nonquantitative way. As soon as the neutron was discovered, Zwicky decided he could invent a star made of neutrons. Thus he was a genuinely original person. There's no doubt that he had a mind which was quite extraordinary. But he was also—although he didn't admit it—untutored and not self-controlled.

PRUD'HOMME: Perhaps he flourished better at Caltech than he would have at many other institutions.

GREENSTEIN: Well, he flourished well because one didn't touch him and one didn't interfere with his group activities. And he was a person who could get along that way. He was one of the founders of Aerojet Corporation, which was invented at the beginning of the war. And he had a continuing connection with them as chief scientist—till he was forced out, because of his non-U.S. citizenship, after the war. I am sure he made many original contributions there. My younger colleagues are very fond of him. I am fond of him now that I don't have to fight with him.

I have a scandalous story if you want it. I'll try and tell it quickly. I once went into Zwicky's office to reprove him for having raised the salary of the librarian without consulting me, causing an overrun in our budget. It was my budget and I had its disposal. I was smoking a cigarette. And as I started to yell and scream at him, which was the only way of commanding attention, he began coughing violently and clutching his chest and said at one point, "I was gassed in the war, Jesse." So I dashed out in the hall and got rid of the cigarette and came back all apologies for having upset him. And we got along as we usually did, by my reproving him and he saying he wouldn't do it again till next time. About a month or two later, it occurred to me that he was a Swiss national. He had never been in any army, and I didn't know how he could have been gassed in the war—not World War I or World War II. So I began asking at the Athenaeum lunch table. Finally somebody asked me if I knew about the time that Zwicky had worried about home production of gas masks in case the Japanese bombed Pasadena with poison gas, which he was quite sure would be their first act after Pearl Harbor. He felt this was where the war would eventually be won against them, and that he was their prime target. So he got to the city fathers and offered to try and produce something makeable at home that would serve as an emergency gas mask. Various things were tried—gauze, female gauzes strapped on in an emergency. And he convinced himself that he had a workable gas mask. He called in the Army Corps of Engineers, which had a training truck in which soldiers went, with real gas masks, through the tear gas which was released. This demonstration was held somewhere on the Caltech campus by him. He went in, to come out supposedly in thirty seconds; but he didn't come out. And the gas was pouring in, and after forty-five seconds they began knocking on the walls and there was no answer; they pumped the gas down and opened the thing and there he was, flat out on his back on the floor [laughter]. But that was like him, and that was a charm of his. He would do anything, because he was convinced that he could figure out how to do it. Leverett Davis, whom you mentioned earlier, was one of the students in that final class in the physics department—in which Zwicky was upset by having his impossible problems all correctly answered.

Fritz was also an associate of Clark Millikan's and of von Kármán in Aerojet, and in its founding. Now let me skip to von Kármán, because it does connect.

PRUD'HOMME: You spoke about his having a baseball player bodyguard. I didn't take you up on it last time. What was that?

GREENSTEIN: Oh, yes, that's quite true. Von Kármán was nearly retired from Caltech by the time I came, but was a good personal friend of Bob Robertson's, both active in military things through the war and after. And it was really only socially that I met him. Von Kármán's important role in what's called hypersonic flight—missiles, rockets, et cetera—had been established by the success of the American rocket and high-speed flight programs. Von Kármán was very important in direct advice to the government and direct planning. Eventually, it led to the ICBM [Inter-Continental Ballistic Missile] program.

It is suspected that a man named Moe Berg, who had once been catcher on some baseball team for years, a very famous, large baseball player, was assigned to von Kármán, both as a friend and as a protector. Moe was roughly 250 pounds and 6½ feet. He was also an intellectual and had appeared on one of those 1940-vintage radio programs—*Information, Please*. Moe was one of the *Information, Please* originals, and he was a very interesting and very formidable guy. Whenever von Kármán was walking down the Olive Walk, when he'd come on his rare visits and appear with the Guggenheim people to go to lunch, Moe was walking along, a little bit in front and a little bit on the side, looking around. And he would never deny it or assert it, but it was suspected that Moe had in fact been in the OSS [Office of Strategic Services] and that this was a natural holdover after the war.

Von Kármán was a charmer. He was Hungarian; you don't have to say more than that.

He lived with his sisters, who fed, protected, and pampered him. He was a man whom somebody like Hans Liepmann here would know infinitely more about than I, but whose status in aeronautical science is unchallengable.

PRUD'HOMME: You mentioned Bob Robertson.

GREENSTEIN: Bob was actually the proximate cause of my coming to Caltech, in that I had known him since 1937, when we became personal friends, when I was in the East. He was the leading exponent of the application of general relativity to astronomy. He had been a brilliant applied mathematician, going to Göttingen in the great days of Göttingen. When he came back, he was at Princeton. He was the person who thought through the actual possible observational tests of general relativity, which he helped develop. In particular, he invented a thing called the Robertson/Walker line element, which is the usual description now of the geometry of the universe. In an expanding universe, you describe the way spatial coordinates change with time. Robertson's major contribution was that, and in discussing how general relativity affected the relations between apparent size, apparent brightness, redshift, and distance for various possible universes. So he was perhaps the most important practitioner of general relativity with a special reference to applications of astronomy. When he came from Princeton to here, he worked closely with the observers at Mount Wilson—that's before we started at Caltech—and with Richard C. Tolman, who was another great general relativity expert who had for many years worked with Edwin Hubble.

Tolman was a great man in Caltech's history. He had been heavily involved in the war, was involved with nuclear weapons and their production and use after the war, and was one of the first scientists to recognize the fundamental dangers of nuclear weaponry development. He was a deep, conservative man of dignity, pride, and honesty. He was, I think, one of the first of the modern breed of doubters.

Robertson developed a set of three or four major tests of relativity theory and wrote several papers published in astronomical journals on the progress in understanding how to test general relativity by astronomical objects. He was also one of the most sociable and amusing people I ever met in my life. And it was at parties that he gave that I met von Kármán and Moe Berg, Sir Solly Zuckerman, and other people from England's science establishment.

Bob, I think, had his life career in science ruined more by the war than anybody else. He saw it coming and took on war responsibilities long before we were in the war, and kept them through the rest of his life. For a while he was, after the war, chief scientist of NATO, creating programs for reviving science in European countries. He had, before the war ended, been involved in what was called operations research, and had been one of the people who condemned the American strategic bombing of Germany as a failure in impeding war production in Germany. And after the war he went back to Germany, where he'd been a student at Göttingen. He had many friends there. He helped rescue some of them from the oncoming Russians. He had a great deal to do with the rebuilding of European science, because of his warm contact with the military in the United States. I also met the commanding general of the U.S. Air Defense Command at one time and saw Robertson and Charlie Lauritsen persuading the general that he, the general, had come up with one of the most brilliant ideas all on his own on how to do something. Both of those people and Tolman—Tolman, Charlie Lauritsen, and Robertson—were master science politicians in the best sense.

PRUD'HOMME: You said that finding interesting people and keeping them happy was your greatest administrative headache at Caltech. What made them unhappy?

GREENSTEIN: Well, take as an example just the few people I've spoken about. Robertson gave his course on relativity theory and commuted to Washington and to Europe, and managed to do everything with a smile and to enjoy parties and to like people. He had a remarkable personality. We talked earlier about Zwicky, who had a remarkable personality of a different kind. The only thing one could do to keep Zwicky happy was to give him everything he wanted and keep out of his way.

But most other brilliant people have problems coping either with success or failure—mostly with success—and coping with other people and keeping creative and not becoming sullen or self-destructive. In a certain sense, World War II was a good thing for some of the generation I've been talking of, in that there was something important enough to do so that one survived. There was an external stimulus and an external reward and not the internal self-judging destructive situation which most people find themselves in after they have had moderate success.

Rivalry is the essence of success. In science, one could look back a long time and rival somebody in the far past. You could kill Father, but Father was essentially already dead. You could improve on classical physics with quantum physics, or you could prove relativity better than Newton. But when one goes instead to a level just below that of the greatest discoveries, like relativity or quantum physics, one is often dealing with a contemporary rival and with a pattern in the development of the science in which the very fact that you have started a new subject guarantees that you will not last a long time in it. This is because other people take it up, and, if they have access to reasonably good equipment, they are very likely to do as well or better than you. They may have newer ideas, and you're stuck on an old idea.

One of the problems in an institution like ours, which had the finest instrumentation in the world, was essentially the feeling that one had to live up to it by doing the finest and most original thing, and to do that every day, or every week, or every month. There's never a moment of peace, in which one can say, "I am the leader. I have created this field. My work, which has taken five years, is now coming to fruition, and I am going to answer all the questions."

PRUD'HOMME: You're always surrounded by a pack of wolves, as it were.

GREENSTEIN: Yes. The wolves are not destructive. There may be a little rabbit who comes in with an absolutely marvelous new idea. And what you had wanted to do is often, in time, irrelevant; it's not the major question.

Well, our problem, it seemed to me, was that having the best and working in the forefront of many fields of science, one was always destined to fall off a cliff. It was inevitable that other people would come in, that other colleagues in your own institution would think of a different way of doing it, or that an instrument in which you had pinned your hopes and faith was superseded. It wasn't wrong, it was just not good enough and something new was invented. So that you were always running a race with yourself to be sure you were half happy with yourself, or with others in your own institution who had the same facilities that might be better used. Young people from other places might have an original idea, or you might have to adjust the progress of science, which would say, "This is uninteresting; what you've done is true but makes no difference. How do you explain so-and-so?" Well, just by the nature of the competitive people we had here, and the fact that we were at the top instrumentally, you were always finding people unhappy, miserable, jealous, disappointed, and there was no cure for it.

PRUD'HOMME: So your job as an administrator was then, quite often, to provide solace.

GREENSTEIN: I ran a five o'clock psychoanalytic hour, it seems to me. I'd sit in the office later with the lights off and listen to somebody. And I've done that so much. I'm a frustrated father, I guess—too many scientific children. I had only two real children. When I use the phrase, it's arrogant, because the scientists are not my intellectual children. But many of them are people with whom I've had a somewhat parental relation. They grew up in themselves; they're all brighter than I am, and better, and worked harder, and so forth. But almost always I had to cope with people who were going to leave, people who were going to give it up, people who were going to leave their wives because they were unhappy with their science, or vice versa. But I enjoyed it, and there's no prescription for how it works. Sometimes you make people happy by listening to their troubles. Sometimes you make them happy by denying they have troubles. Sometimes they come up with a new idea, which is deep in their mind and which they've had, which comes out when they're complaining. It's a question of the style of counseling. We don't have professors who are real leaders who tell people what to do anymore. My best leader was the head of Yerkes Observatory, Otto Struve, who never worried about people's personal lives at all, completely unlike me, who worried always about their daily personal problems. Struve would see that people were getting to a dead end in their work, of which he was a good judge. And he would come in and say to me, "Jesse, how about working on so-and-so?" And I'd say, "I don't know what that is." He'd say, "Well, I think it's a very good thing; you ought to look into it." And that was it. No explanation, and very often, very good ideas.

PRUD'HOMME: And were you bound, then, to turn around and do whatever he advised?

GREENSTEIN: Well, it was my own work. But he was an outsider looking both at one's own work and at the whole field. I think part of that was how one helps bright young scientists. By not being the person that they are, doing that work, you can look at it and either help them find their own solution or suggest new things that are relevant. That really has been the greatest pleasure—seeing people grow and change fields. The oddest growth or change we had—almost a ritual—was that almost everybody we brought in, in the early years, was a theorist by background and became a skilled observer and often forgot the theory. Sometimes they became both the theorist and the observer. Almost without exception that was true. And that change was not by my personal intervention but by the existence of the instrument, the best in the world, literally—no rivals for years.

I found a piece of paper with a list of the early PhD theses. I don't know whether this is an impressive list to outsiders—ten years, roughly seventeen PhDs. These are the first PhDs here in astronomy. The first one goes to a person I'd forgotten completely, joint thesis in physics and astronomy, 1950. The next is Eugene Parker, who's one of the greatest experts in plasma physics; he invented the term "solar wind" and is a leader in theory at the University of Chicago. That was 1951. One who is the editor-in-chief of the *Astrophysical Journal* is 1952—Helmut Abt, a good scientist. Nineteen fifty-three: H. C. Arp, against cosmological redshifts, and Allan R. Sandage, pro cosmological redshifts. Next is Roy Gould, head of engineering here—joint thesis, physics and astronomy. Next is a distinguished scientist, John Mathis, of Wisconsin. Next is George Abell, the best textbook writer in American astronomy, now at UCLA. Next is head of the department at Washington. Another is the head of the department at Hawaii. Another is at JPL. Then we had one failure. Then we had Alan Moffet, who was the radio astronomy director, whose thesis was in astronomy and physics. It was a good bunch. The teaching was pretty much done by myself and theorists whom we imported. And when I actually look at it, the theses are more than half theorists'. Of course, that was one strong point, also, in our relation with the physics department; we didn't have a lot of astronomy courses, and the students took both the required physics courses and astronomy courses. And so they turned out to be pretty broad-based.

PRUD'HOMME: Is there anybody else you'd like to mention?

GREENSTEIN: I'd like to mention Earnest Watson, because he represents a lost figure in the structure of Caltech. [Robert A.] Millikan had been a great man, clearly, but he was getting old. And DuBridge had building up to do when he arrived [1946]. But for many years before and after I came, Earnest Watson was in charge of many things. He had been a physicist, a protégé of Millikan's. Earnest had written a book which was a collaboration with Millikan and another.

He served as the dean of the faculty and did much of what lots of different people do now. He worried about the new members of the faculty. Every appointment went through him; all communication was with him. He worried about salaries. I talked to Watson directly about salaries or appointments, immediately after I arrived. He was a bachelor until late in life, over sixty when he married—a widely cultured fellow, a collector of art. Most pictures around the halls of the buildings are from his personal collection. There was also a major collection of Indian miniatures. He knew wealthy people well, and in a sense was easier with them than some of our presidents, because he was fundamentally conservative and lived in Santa Barbara/Montecito. He knew that kind of person. He was a remarkable man, who is rather forgotten here. Where Millikan has all the credit, Earnest Watson did much of the work—or most of the work, in my time at least. A man I really admired—a sort of old-fashioned bachelor, somewhat prissy.

PRUD'HOMME: Did he want any credit for all that he did?

GREENSTEIN: No, he was extremely forthright. If you told him something and he was convinced, he would go do it. He combined half the job, or two-thirds of the job, of the president and the provost and the dean of the graduate school. He did it all, and life seemed simpler. He was an admirable person. He saw Caltech changing from the Millikan era to the DuBridge. When he retired, he became an advisor to the State Department on international relations, specifically with India. He spent two full years in India, advising on the setting up of advanced education for the new government of India.

Begin Tape 2, Side 2

GREENSTEIN: One lesson on building staff was given to me by George Beadle, the great biologist. I'd had a very bitter blow, in that two of our professors decided to leave and start a new department at Wisconsin—Arthur Code and Donald Osterbrock. They both told me the same day, and then my personal research assistant, a good young woman, came down and told me she was going with them.

PRUD'HOMME: That's what we call a Job Day, in our house.

GREENSTEIN: [Laughter] The Lord afflicted Job with ten punishments. Well, OK. The next day I was walking back from the Athenaeum and George noticed I was very upset. He said, "What's wrong?" I told him what happened. He said, "Well, Jesse, you can't win them all. You can't win them all."

You work awfully hard to try and attract bright people who would fit in the group. And even after you've gone through all the rigamarole of getting it past your colleagues—the division head and the president, and so forth—they still don't come, or they come and they go. The real thing I guess I've learned is that if you really have good people and have done your best to give them the proper facilities and proper financial support, proper personal encouragement, if in their life's history they go or don't come, it's really all the same. You've done the best. It's good for science in the large. You cannot run a purely competitive institution which consists of leadership in everything. It never works anymore. One must feel the larger life of science, of scientists coming and going and changing, as part of the structure. You have to look back every now and then or stand away and look from a distance at a situation. You encourage people, but at a certain time they do leave. One, they may start something themselves; after all, that's not so bad. Or they may find that what you have is not what they want. There are even people who don't like the smog in Pasadena; somebody left for that reason.

One just has to realize that a scientific institution has got life and death built into it, and the coming and going of people; and just try and survive and still enjoy what's happened, after it's happened. When it's happening, it hurts. The people we've had have a strong common thread in being competitive, almost a Caltech hallmark. Therefore, if they fail, they leave. And if they succeed, they leave. Either way.

PRUD'HOMME: It makes it difficult for you.

GREENSTEIN: Well, it's not that I'm feeling sorry now. You always feel sorry while it's happening. But then the whole life of science is that way; institutions come and go. When I was a graduate student, Harvard was the greatest place on Earth. When I was at Yerkes, it happened to be the greatest place. All these things have a cycle. Harvard went way down. Yerkes dwindled from being the finest group of theorists and observers in the country to one or two people. And then perhaps they begin to come back again. If I ever had time, and if I really knew history, I would like to write about the sociology of success.

PRUD'HOMME: The cyclical aspect.

GREENSTEIN: Yes, the cyclic thing, the periods of good and bad.

PRUD'HOMME: But it sounds as though the people that you've produced have rather populated all the major observatories of the United States.

GREENSTEIN: In that way, we still do well. A last big splash in the newspaper was the giant voids in extragalactic space which a group of four scientists discovered. Three of them were from Caltech, now all at different places. You don't necessarily keep all your best people, though you'd like to keep the very best. I think if we had another Murray Gell-Mann or Dick Feynman as graduate students, we wouldn't let them go. Although in principle we like people to leave and, if possible, come back. The other policy is to recognize people under thirty, because otherwise you must lure them back as tenured full professors, at a high price. There's no way of solving such problems.

OK, you asked me about collaborators?

PRUD'HOMME: Yes. You've had so many well-documented major projects. Would you like to talk about any of them in particular? Or any event in relationship to the people you collaborated with?

GREENSTEIN: Let me just start on a concrete fact. There's an interesting sociological change in astronomy. Years ago astronomy was done by looking through telescopes by astronomers. Later it was done with instruments, some of them invented by physicists and used by astronomers. Now there are instruments invented by physicists and used by physicists who like to call themselves astronomers. There are even physicists who work on astronomical topics from space, let's say, and call themselves physicists. So we've gone, historically, from being an isolated small group to being an integral part of advanced physics in almost every field.

In 1947, I had a spaceflight with a rocket on a captured V-2; it failed. But at least I tried. That's a long time ago, just a year before I came here. I worked with physicists. The man who sponsored this project and gave me $7,000 and a V-2 was named James Van Allen, after whom the Van Allen Belts are named. So I was forced then to realize that astronomy was becoming different. When I came to Caltech, it was clear that it had to be even more different. Although I had a big body of classical astronomy astronomers at Santa Barbara Street, I also had Fritz Zwicky who was god-knows-what—partly a physicist and partly astronomer. But I also had all the resources and potential of the physics department. They were great. It was natural, since the teaching also was interdisciplinary, to look for new areas of astronomy or old subjects that had relation to physics.

The most exciting thing that happened was the de-astronomization of astronomy. New astronomy grew here in this building [Robinson Hall]; we have a large number of people—100, 150, I guess, altogether. But there are colonies all over the campus, and some at JPL—many of our graduate students are there—and some of our faculty work with JPL. We have links with the planetary sciences group. We have links with infrared, which is in the Downs Laboratory, as part of physics. We have the new millimeter-wave astronomy, the principal growing area of our radio observatory, over in physics, with the receiver development centered there. Cosmic ray physics, exemplified by Ed [Edward C.] Stone and Robbie [Rochus] Vogt is in physics; they do experiments in space, studying cosmic rays—where do they come from, and what are they made of? For years our closest link in physics was with Kellogg, the low-energy nuclear physics lab. I had a personal link with Leverett Davis, who later worked on the solar wind and cosmic rays in the solar system.

What it really meant was that I lived through the whole of astronomy as it changed into astrophysics and physics. Each time it was a hell of a mental strain, in that I had to learn a new subject. I never knew anything about nuclear physics; I'd just barely heard of it when I arrived. It was secret, and in any case irrelevant to astronomy. So in 1948 I had to learn low-energy nuclear physics.

When I worked with the physicist Leverett Davis, that grew out of my background. I'd done a thesis on interstellar absorption by small dust particles. I'd also worked on it at Yerkes and made some discoveries. Then a new phenomenon was discovered by astronomers—the polarization of light in space. Leverett was a classical physicist, capable of solving any problem, because that's how Caltech PhDs were trained—to solve any problem. And so we got together. We had a hot race with [Lyman] Spitzer of Princeton, who had a different theory. I think we proved to be right. It's amazing that there is still a correct theory, thirty years old. It has been modified, but I think we didn't make any errors in our analysis. The collaboration involved my ideas on what could be out there in space, and how little particles interact with light, plus Leverett's wide knowledge of classical mechanics.

Actually he was particularly expert on that because of World War II, in which he had been in the ballistics of rockets. He had even written a book on rocket spin and trajectories. Our problem was how little particles in space are spun around and spun up, and how they line up. We solved it. It was largely Leverett's brilliant mathematical and physical insight and my knowledge of the astronomical side of small-particle scattering. Well, that became possible only because we knew of each other; it's a small place; you could go and talk.

The relation with [Willy] Fowler—the growth of the new discipline of nucleosynthesis, the origin of chemical elements in stars—also was based on another older interest of mine. Since 1940, I'd been getting into composition of stars. In 1940, I think, one of the stars that Otto Struve told me to work on really provided me with the first analysis of the composition of a star in which nuclear reactions had drastically altered things. I had done that and developed, as a practical thing, the first method for the mass-production analyses of stars, both of normal and abnormal composition. There was really not much clue at first as to what abnormal composition meant. So I began worrying about what it meant, and it was all nuclear physics. When I wrote my first paper on the the amount of lithium in the sun, I found that Ed [Edwin M.] McMillan, a Nobel Prize winner, had been a graduate student here. With one of the first nuclear reactions produced in the laboratory in Kellogg, he had studied the lithium destruction by proton bombardment in the lab. He'd even mentioned astronomical applications to the Sun in the mid-1930s, when he was a student. It took a dozen years to get back to what McMillan had noted early—that there was something interesting to find out in the stars.

The other seed of nucleosynthesis was Hans Bethe's study of the nuclear reactions producing the energy of the Sun, and the composition changes that would occur in the center of the sun. My first papers on that subject were direct attempts to find the effect of nuclear reactions, without too much understanding of the complications. One of them is in 1950; "The Production of Isotope of Carbon C-13 in the Sun." We looked for it and didn't, in fact, find it.

That was contrary to predictions by Hans Bethe. I looked for helium isotope 3, and lithium in 1951. The first few attempts were without knowledge of McMillan's discussion.

The first paper trying to make sense of it, after getting this surprising information, was given in 1952 at a meeting in Rome—I didn't attend, but sent the paper. In 1952, I gave a lecture both at UCLA and Inyokern, in which I invented a process in which neutrons are produced in stars, important for the production of peculiar elements. Clearly I was beginning to have to understand nuclear physics, and certainly before the lecture on the production of strange elements. So somewhere in there, 1950-1952, it paid off to be at Caltech, a small place, to be able to have a friend that had parties—and they were good parties—so that between raucous jokes, one could say, "Hey, Willie, what's it mean if I find so-and-so?"

Charlie Lauritsen has to be one of the early heroes. In 1951, I published something about the isotope helium-3 in the Sun, and its absence, which I couldn't explain. It was certain that it is produced in the Sun. So after a Friday evening talk over beer in the Kellogg basement, I got up at the blackboard with Charlie Lauritsen and said, "Well, what can we do?" And Charlie invented a nuclear reaction which destroys helium-3 as soon as it's made. He had no knowledge of the subject until he heard me talk. The reaction, helium-3 plus helium-3, had no interest in nuclear physics per se, but Charlie thought of it, and it was correct.

Where does the interplay, the stimulation, come from? It's the small numbers, the fact that the people were not stuck on one big isolated project. In Kellogg, doing all the low-energy, light-element nuclear reactions, they didn't care which. Those relevant to the Sun seemed logical for them to do. The fact that you could go into an experimental group and say, "I don't find helium-3 in the Sun and it should be there," or "Ten percent of the helium should be helium-3, and my upper limit is ten parts in a million, so there just isn't any." And then you'd find an answer the same evening, at the first talk on it.

That is what is good about a smaller institution with good people. If I gave that same lecture at UC Berkeley, very few physicists would have come. In fact none, probably, in those days. If they had come, they would have listened. It happens that Luis Alvarez, at Berkeley, was the first to find that helium-3 existed on Earth as a stable element. And had he heard of my lecture and come, he would have done the same thing, and solved the problem.

PRUD'HOMME: You can't really isolate yourself here. You can't ivory tower yourself, in a sense.

GREENSTEIN: No. Well, those are the merits. The demerits are equal—that everything centers on personality and on a few powerful individuals.

PRUD'HOMME: You worked with Fowler and Hoyle.

GREENSTEIN: I knew Fowler well by then. Astronomy had a genius in England, Fred Hoyle. Fred was a bright person in many ways. He was an original thinker; he was deep and worked problems out, unlike Fritz Zwicky. But he was not accepted by the English establishment. It took something of a revolution even at Caltech for me to get a visiting professor's appointment for Fred Hoyle.

PRUD'HOMME: He worked at the observatory?

GREENSTEIN: No, he never used the telescope. He had an appointment in astronomy as a visiting professor to lecture on his theory of the origin of the chemical elements in stars. That course was in astronomy; it was well attended, and specifically by the Kellogg nuclear physicists. Fred predicted a certain new property of the reaction rates that produce carbon had to exist—there had to be what's called a bound level. This mediated the reaction of an unstable nucleus called beryllium-8 with helium atoms, to make stable carbon-12. He predicted it occurred via the existence of what is called a strong resonance. He said that in his lectures; he had worked it out but did not know enough nuclear physics and certainly did not know whether the resonance existed. Within a few months, the Kellogg group made the experiment and found the energy level right where he said it would be. That, of course, is how science is supposed to work—it doesn't often—and Fred was thus deeply endeared to the hearts of the nuclear physicists in Kellogg. Unfortunately, he was very unendeared to the hearts of the other parts of the physics department.

PRUD'HOMME: Was this because of his reputation at Cambridge?

GREENSTEIN: Well, he is also careless. He wasn't at Caltech. It's also pro-establishment. He had made himself thoroughly disliked in England.

PRUD'HOMME: The fellow Brits didn't like him because of that?

GREENSTEIN: Because of his Yorkshire accent, his low-class origins, and the fact that he was doing things that they didn't yet recognize as science. Partly it was the steady state theory, an arrogant speculation which met with disdain. But really that fight occurred later than this other fight. Fred wrote papers, three or four years before the period about which I'm talking, on the origin of the elements of the stars. I had read those; I was interested in the topic and it seemed respectable science to me. And I wasn't for or against the steady state. So I wanted him to come to Caltech. But one doesn't do anything as a dictator. I had to pass the idea of an appointment through the physics faculty, and first the division chairman. They didn't like Hoyle at the observatory, because they didn't like his opposing the simple, expanding universe. I think I sponsored him twice. Then Kellogg Lab created a permanent visiting professorship, in which he could come whenever he wanted. Moreover, from that visit came the two Burbidges, protégés and admirers of Hoyle. Finally, this led to the codification of the theory of origin of elements in stars, and what's called "B^2FH"—Burbidge, Burbidge, Fowler, and Hoyle. I hired Mr. [Geoffrey] Burbidge, because we couldn't hire a woman in the observatory staff. Kellogg hired Mrs. [Margaret] Burbidge, because they could, since she was supposedly just going to sit and watch nuclear counters count. Although Mr. Burbidge got assigned observing time, she did the observing. He sat in the darkroom and, perhaps, read comics. I don't admire their later progress through life, but they were important because they were very bright. She had done some work on stellar composition and continued to, for a while. And [Geoffrey] Burbidge understood nuclear physics; in those years, he was a quick-thinking, competent scientist. All in all, it was a very good time. Subsequently Kellogg spent some of its federal money for people to work on the evolution, structure, and composition of stars, as well as on nuclear physics problems and their relations to astrophysics.

Bob Christy, for example, worked on the theory of nuclear reactions. He was good mathematically, and he understood nuclear theory better than most in Kellogg. The cross sections measured with a nuclear reactor at a certain energy have to be transformed to what they would be at a temperature in a star. And Christy did that. For some years, we had the best scientists visiting and looking on stellar problems. I was supplying information on the stars of funny composition, a subject which I'd started. Once, only once, I worked with Fowler and Hoyle on a speculative thing, connected with the light elements that I'd studied in 1951, peculiar, and affected by nuclear reactions. I forget what year that was.

PRUD'HOMME: 1960.

GREENSTEIN: Then came my bowing out of the whole subject of stellar composition. From 1957 to 1970 I received a large amount of government money for research. I used it to bring in postdocs, who worked on the composition of stars and nuclear reactions in stars, and on stellar evolution. That was partly a result of my losing personal interest in that subject; next, it was going very fast and almost out of my grasp. One just had to do a great deal of routine but difficult stellar-composition analysis before one understood what were the questions to ask the nuclear physicists and to see if the answers were right. I'm just saying that although the composition project lasted till 1970, I hardly did anything on composition myself after, I would say, '64. Then I began to turn to low-luminosity stars. I kept writing papers on compositions mostly because I had good young collaborators and I had access to the equipment.

PRUD'HOMME: Where do you get your postdocs from, when you say you imported a lot of postdocs?

GREENSTEIN: Well, that's another feature of Caltech—but I guess I learned from Struve and Yerkes. We brought in a large number of Europeans. Europe has always been the center for theory in astrophysics, except in this subject of nuclear astrophysics, where America took the lead. But stellar atmospheres, the study of the composition of stars, the Sun, and stellar evolution really was a European specialty until the big computers came in and everybody could do theory. It became just a matter of computing. Also, Europe, after the war, was beginning to get into modern observational astronomy, in which it had no background and gave no training. So for about thirteen years in which I was bringing people in, half were Europeans or Japanese. Many of them are now distinguished leaders. On that abundance project, I supported a visit by

Wallace Sargent, who had done a thesis on shock waves in the explosion of a nova, nothing to do with composition. Somebody told me that he was good, and that he wanted out of England. Another person I brought in was Leonard Searle, who's now on the Santa Barbara Street staff. His wife is Mrs. [Eleanor] Searle, of the classics department here. He was, I think, a Canadian who lived in England, who didn't have much access to observational possibilities. If you can find good people like that, and if they want to come, and are willing to get used to California and smog, you've got a real treasure trove. I could list more names, but those are two examples of really outstanding scientists we brought in. Americans on it: I worked with George Wallerstein, who was a student here and is now head of the University of Washington's astronomy. I worked even with a couple of Russians, but I don't think we supported them on the air force contract. Many of these postdocs were students we kept on for a year or two, and who then went elsewhere. [Robert A. R.] Parker, who is now a science astronaut, worked with us. [John Beverley] Oke, who's on the faculty here, was on that. [Jun] Jugaku, now head of Japanese astronomy, reports to the minister of education. One of the visitors was a Belgian working on the composition of comets. Bob Kraft, director of the Lick Observatory. Oh, a good woman astronomer, Ann Merchant Boesgaard, now in Hawaii. We broke the woman barrier.

PRUD'HOMME: I was just going to say, she's only the second woman you've mentioned as a colleague of yours in astronomy. Is this peculiar to Caltech? I know that there was a lady at Harvard with whom you worked.

GREENSTEIN: Oh, Cecilia Payne, yes. She was the pioneer in understanding the composition of the stars. It's a tragedy; if you have a place which is so competitive, you cannot—and you didn't have to, in those days—justify getting somebody because it's a woman, especially given the physical difficulties of the process of observing. By the way, observing is not an ideal woman's task. Cecilia Payne could do it. Margaret Burbidge could do it pretty well. But many just cannot. It's a sixteen-hour day and night, and it's hard and it's cold and it's nasty; the primitive nature of observing is really outstanding. There is another woman collaborator with whom I collaborated later—Virginia Trimble, who didn't do any observing; I supplied the material. Most of them are men, alas; and I will be damned and roasted in feminist hell for this.

PRUD'HOMME: Is this changing do you think?

GREENSTEIN: No. We have only one woman on the astronomy faculty. We have no woman student. And my present collaborators are men. I like girls, unfortunately, and I really can't excuse myself. I collaborated, I see from my bibliography, in 1969 with Judith Cohen, who's now on our faculty. In 1967, it was with Virginia Trimble, who is on the faculty of both Maryland and Irvine.

PRUD'HOMME: But this isn't peculiar to Caltech?

GREENSTEIN: No. Oh, I see that my best paper for years, my 300th paper, was written with Anneila Sargent. She worked with me as a research assistant; only later did she go back for a PhD degree, working in the infrared. It's an inexcusable imbalance, but it's true.

PRUD'HOMME: I want to change the subject slightly. You've received many, many honors. Are there any that really meant an extraordinary amount to you?

GREENSTEIN: Well, unfortunately, they're all bitter pills, so I don't like this subject.

PRUD'HOMME: Why do you say that?

GREENSTEIN: My vanity is superb, as you may have noticed, and I know it. I have worked like hell, to put it mildly, as a scientist, for our government, for the military, and for science. I also raised money. And every time anybody's nice to me, it's too late—that is the trouble. I feel that my best science must have been done by 1955 or '60, somewhere in there. That's a long time ago. And I was only elected to the National Academy of Sciences when I had finished doing my best science; in other words, I was elected in 1957.

PRUD'HOMME: Isn't that their general pattern?

GREENSTEIN: Well, I hope not. It's not a matter of principle. What I'm saying is that I would have appreciated being recognized by election to the academy when I was in the thick of it. When I was elected, I immediately plunged into the thick of academy business. I don't think I got any honor; I got just extra jobs with no pay. I don't mean the money. I have never gotten a single medal from the National Academy of Sciences. Science honors are meaningless after a certain point. After I got elected to Caltech, I didn't need these honors anymore. That is really a sad thing. It would be wonderful to win a large money prize, because I happen to love luxuries and money. Now, because I'm retired, I have to beg, in fact. All my work currently is supported by a personal gift from a personal friend whom I've known since I was a boy. I find that rather ironic that having raised many millions from government and others, I have to beg. If I won one of those big prizes that carry a lot of money, mainly it would ease the problems—pay for computer time, research equipment, maybe even a postdoc. But otherwise, honors don't mean much. The Gold Medal from the Royal Astronomical Society was one of the worst ironies, because they gave it mainly because of my work on science planning. That was in their citation—"following a distinguished career of science as head of the Greenstein Committee, the 1970 report of the Academy on the future of science." You see, you get kicked in the teeth if you are sensitive about what things mean. I don't particularly enjoy anything like that.

The only thing scientifically significant, as an honor, is the senior lectureship of the American Astronomical Society, which was rather early, called the Russell Lecture. Well, I see it's not early; it's 1970. I had already passed sixty. Another honor I really cherish, the same year, they made me the DuBridge Professor, because I admire Lee DuBridge enormously. So, when I was busiest, I was getting recognized. But goodness, it doesn't amount to anything. I have six government certificates of, you know, achievement or merit, appreciation for things I can't tell you about. I have a letter—What was it called?—as chairman of the W1-17-L Project. But I can't tell you what it concerned [laughter].

JESSE L. GREENSTEIN

SESSION 3

March 23, 1982

Begin Tape 3, Side 1

PRUD'HOMME: We were discussing before some of the honors that you had received and the committees you had served upon. Do you want to go into that further?

GREENSTEIN: I think that from the point of view of what a scientist actually does, it's of some interest to look not only at honors but at what might be better called responsibilities. It is often an honor to be made, say, an editor of a journal or something like that. And the questions—why people do it, what they do, and how they do it—become a self-propagating activity and are, I'm sure, known to historians of science. But you never know, until it happens to you, that it is going to be quite like it is. Sometime within the last couple of years, I compiled a list of what are called lectureships, visits, and committee memberships. I was startled when I found somewhere that I had given, in the period 1960 to 1975, about fifteen named lectures around the country, and those clearly without the reason of information communication. People didn't really want information; they wanted a name or a person. I made a formal lecture tour which involved twenty-odd lectures more than twenty years ago. I gave a Sigma Xi lecture tour.

The question is, How does one propagate one's enthusiasm for a science? The hope is that by the communication to this audience, somehow something sinks in which in the long run is good for science. One of the typical responses which people think should make a lecturer happy is "Oh, I think that was the best lecture I've heard for years. I can't say I understood any of it, but I just felt marvelous." That is one of the problems with science communication, and with propaganda for science.

PRUD'HOMME: What kinds of lectures? What kinds of places?

GREENSTEIN: Good colleges, good universities. There's a thing called the Pasteur Lectures at Georgetown in Washington, D.C., I remember. I think it was a Catholic university. They were very interested. I remember that particularly because I was very nervous about going into a hotbed of Catholicism and talking about creation occurring fifteen billion years ago.

PRUD'HOMME: The Jesuits might not appreciate you.

GREENSTEIN: Well, it turned out it was a Jesuit who invited me. And it was, in fact, good. That kind of thing has its function in propagation of science, and it's something which I rather enjoy doing. On TV, there have been several major films involving astronomy in which I participated—PBS stuff. I gave a BBC radio broadcast series which I thought was extremely good. I have a transcript; it was one of my best, because BBC radio is at a much higher level than TV. A man named Gerald Leach was the interviewer. I did a film called "The Birth and Death of a Star," which was for KCET, a local PBS station. I was in a Saul Bass film, which still is used in elementary and high schools, which still gives me an audience of twelve-year-olds every now and then. It's called *Creativity* and won an Academy Award.

I dedicated four buildings—new laboratories or observatories. At one time that seemed to be a popular thing. It's a part of the game of interaction with the public; it's something which you have to do without knowing why.

PRUD'HOMME: You said at the end of the last tape that you considered it a tremendous honor to get the DuBridge chair. Was this a first appointment?

GREENSTEIN: Yes. The DuBridge chair was founded at that time. It has an interesting background. DuBridge had already left Washington, in fact. Henry Dreyfuss, one of our trustees, the industrial designer, wanted to do something to honor DuBridge. We spoke about the idea, and he raised a good deal of the money himself. It was one of the first chairs that had well over a million dollars' endowment. Anna Bing Arnold, the lady who endows USC and art buildings at the County Museum, was one of the major pushers for that chair. It was very worthwhile. I was involved a little bit in the money-raising. It had nothing to do with my getting it, I hope. But it was because I had so admired DuBridge, and I hope he liked me. We were both happy with each other, he for having his name on a permanent chair and I having his name on my professorship.

But fund-raising is a different activity, and I think irrelevant. As to other honors, I got a small medal from Belgium and was made a member of their Academy of Sciences. I got a medal, the King kissed me on three cheeks—in Belgium, it's left, right, left; you have to remember the technique.

PRUD'HOMME: You got a gold medal from the Royal Astronomical Society.

GREENSTEIN: That is certainly, as far as prestige goes, the best. It's quite old. It has Newton's face on it. And I got one from the Astronomical Society of the Pacific. The other important awards were the California Scientist of the Year, which I shared with Maarten Schmidt for the invention of the quasars in 1965. And that's a nice thing. And I got various government citations for classified work. The Astronomical Society of the Pacific is the largest gold medal that I have. It is about three-and-a-half inches, well over a quarter-inch thick. And then there are all kinds of odd things. There are other occasions which are more of an honor. You may remember I showed you that big book, from an IAU [International Astronomical Union] meeting held in Rochester; they dedicated the book to me, because I'd helped start the current activity in studying white dwarfs. Those are the things that count, and that was timely, in that white dwarfs were a burgeoning field, and there were many good new people in it. It's fun to be an intellectual grandfather of some kind. If you want what I feel as a meaningful honor, in a current *Monthly Notices of the Royal Astronomical Society* for January, by some people working in New Zealand, starts with, "Although Canopus is the second brightest star in the sky, it has been relatively neglected. Table of line identifications and equivalent work has not been published since Greenstein's 1942 work, nearly forty years ago." When you see yourself enshrined as some kind of dead figure of the past, it's sort of fun. I think the interplay between scientist colleagues, people who come and talk, or don't come but send preprints, that's the substantial way of feeling good about one's work.

PRUD'HOMME: Do you think Caltech still ranks as the leader in astronomy? Or do you think there have been too many developments in too many other areas?

GREENSTEIN: You have to qualify both the words "leadership" and "astronomy." In ground-based optical astronomy, and in areas of radio astronomy, Caltech still qualifies as the leader. I think it has one of the best, or the best, graduate schools and certainly is among the best. Recent PhDs have gone out and seeded the rest of the country. But, again, nobody has the individual honor of that. I guess George Ellery Hale is the only man who comes out with honor. He thought of the 200-inch, and he thought of Caltech.

PRUD'HOMME: But then you built the department.

GREENSTEIN: Well, yes. But the guy who starts the thing is the one that counts. Nevertheless, you have done what you could—not what you should, but what you could—consistent with other life aims and activities.

PRUD'HOMME: You were also involved with administrative committees within the institution. You were the chairman of the Faculty Board. What did the Faculty Board do?

GREENSTEIN: There were two committees—they now may have been changed—that the faculty actually votes on. The rest are appointed. One is Academic Freedom and Tenure, on which I served several terms, and the other is the Faculty Board. Originally, the Faculty Board was a group of people who sat once a month with Earnest Watson, mainly to express their opinions with regard to policy and administrative questions. About two terms before I became chairman, it was made more active and more democratic. My immediate predecessor I believe was either Ernest Swift or [Richard M.] Badger. During his tenure, the trustees felt it important to open a line of communication with the growing and quite changing faculty of the sixties. There should be not just the president and the provost but a broader voice, so that actions taken—including the rules, the policies, and procedures—have some faculty acceptance. I had been a member from '65 to '67. I don't remember when I was president.

PRUD'HOMME: So all the members of the Faculty Board are elected by the faculty, right?

GREENSTEIN: Yes. And of course they include the provost and the president—he's not a voting member, but he sits there—and it has a secretary, Dave Elliot, who writes quite literary minutes. There's an agenda. Changes in courses used to have all to be approved by the Faculty Board.

Originally they just went through an educational policy committee, of which I was once chairman. There was a graduate study committee in which every option had a member. But later, this idea of an elective faculty board, which would voice faculty concerns, became blunted at the most. It was somewhat pro forma until just before I came. But my predecessor came and asked if I would like to be chairman. I said, "How do I get to be chairman?" He said, "Well, we would like you to be chairman." I have no idea how it was decided. But I was more activist. And I did feel that although one still had a fine president, you should make sure that there is no conflict between the administration and the faculty. The faculty at Caltech, largely, doesn't give a damn about how the place is governed—except as it affects their immediate affairs.

PRUD'HOMME: Is there any process by which they could get through to you as a committee? Or was it that once you were elected to the committee, that was it?

GREENSTEIN: No. There were rotating terms on the Faculty Board. There's a nominating committee that attempts to find people who would be interested. They try to balance the different fields—say, engineering versus basic research, biology versus physical science—and age differences. And the board has been a fairly representative group. However, it had never, to my knowledge, taken more than I would say a passive voice in planning future. Things like a fund-raising campaign came down out of the office of the president and the trustees, and the faculty were then told. I think that has been changed; since my period of activity, they added a streamlined steering committee of a half a dozen, to which any faculty member with a current problem can come. In principle, one could have an official ombudsman-type system. If somebody's being passed over, not getting a raise, or feels injustice, he can bypass his option representative and division chairman and go to the Faculty Board's Steering Committee. Then, they filter what is interesting or important, to be handled without making it public, or handled in public.

But to return to the Aims and Goals Committee, I felt there should be a way of getting this extraordinary faculty interested in the future of the institute, in its general planning. I remember mainly that being a nonacademic politician, which meant usually I did what I wanted, I didn't know Robert's Rules of Order. The first meeting I tried to establish a study of what the institute's goals were to be. I brought it up as chairman, but it was not on the agenda.

Somebody pointed out that a non-agenda item cannot be voted on if there is one person in opposition. So one person opposed it on principle—not that he opposed it in fact. Just that it wasn't right. I had decided on it by speaking to people whom I felt to be the young, forward-looking ones in the various divisions. I had even constituted a committee. So then the next meeting I put it on the agenda.

I thought it was an extremely interesting group. It produced a lot of very interesting think pieces, for example, about the role of the humanities at Caltech, or about women in the undergraduate/graduate school. It also produced almost all the recent administrative leaders; in fact, almost everybody active on the Aims and Goals Committee got to be some big shot in the administration.

PRUD'HOMME: Who was on this committee?

GREENSTEIN: Well, for example, Bob Christy was chairman of the committee. I was a committee member and on various panels, but as Faculty Board chairman I had to be a little separated from them. Neil Pings was active in it. He's now dean of deans at USC. And there were people from engineering whom I had hardly known but seemed to be representing the more modern side of what was going to be electronics and information science later. Some of them were active. All in all, it was a breeding ground for potential administrators. It had interesting ideas. It, for example, reaffirmed the Caltech policy on no part-time students; it reaffirmed the idea that the humanities were an essential part of the curriculum, although for many of the members that wasn't clear at all.

PRUD'HOMME: You were more or less taking over the job of identifying the problem areas within the institution. Did the administration resent that?

GREENSTEIN: No, no. Identifying areas of potential growth, and change, really. It was a discussion of needed changes. Some of them were even non-changes. But mostly it was about what might be new. I believe the timing was right. We had an excellent president at the time, in DuBridge. But I know that I appointed the committee to choose the next president. So that was in '67. And also the committee to choose the provost, because that was a newly important office.

The administrative structure had been much simpler. I mentioned how it was under Earnest Watson; later it became more like our present divisional structure. But if you left all power in the division heads, you don't get novelty if one division head wants novelty and the other one wants to make sure that his budget doesn't get cut. You only get novelty really, I'd say, superimposed from above. I, incidentally, served an excellent chairman of the Board of Trustees; much thinking about this arose from conversations I had with Arnold Beckman. Arnold is a reticent and modest man, and I don't believe historically oriented people have given him sufficient attention. Arnold, for example, spoke to me about some slightly more radical changes. One of the questions was whether Caltech should be a large university, covering all fields. That was not favorably viewed. With an idea like that, I probably asked Arnold beforehand what he thought, as chairman of the board and an ex-Caltech professor. When it came to appoint a committee to recommend or evaluate candidates to be the next president, Arnold said it was the job of the Faculty Board chairman to find a committee that would represent the wishes of the faculty, but that the trustees would do exactly what they liked, because it was their job to pick the president. But of course they would like to know what the faculty thought. Well, in fact, they not only got to know what the faculty thought but they interacted fully. There were, I think, three trustees on the trustees' committee to pick the president, who turned out to be Harold Brown. We instituted with that search, the first time, to my knowledge, campus visits by the candidates for presidency.

PRUD'HOMME: So that you could vet them out as well as they vetting you out.

GREENSTEIN: Yes. They visited the various divisions and were on campus and actually lived, I think, in a student house for a day. Why anybody wanted such a job after living with the students, I don't know. But anyway, Bob Sharp, who was the great man at Caltech during my period, being the behind-the-scenes thinker and persuader, was the chairman of that presidential search committee. So, all in all, when I was Faculty Board chairman was a real time of change in the institute's ideals and structure, and in the position of the Faculty Board, now an important part of Caltech.

PRUD'HOMME: This is true across the country; being in the sixties, you had to be more flexible and more receptive to change and to new ideas.

GREENSTEIN: That's right. I think you've got to realize that we are no more receptive to change at Caltech than anyplace else. We are susceptible to scientific change; we're rather tough on organizational change. Other advisory committees in the institute which I served on were innumerable. I was vice president of the YMCA board. The YMCA board was dominated by Jews; it seemed to me a very good idea. It was then our leading ethical center, under Wes Hershey, who is still alive but retired. It was really important for the students, and graduate students, at Caltech. The various divisional committees were set up a little later; that's about '69-'70. They set up faculty committees advisory to the trustees within several of the divisions. And then the trustees had a little parallel structure, where there would be a trustee who would specialize, say, in the physical sciences. I don't know whether that is active as it used to be. I think it was one of the suggestions, in fact, of the Aims and Goals Committee.

PRUD'HOMME: Was there any sort of jealousy of the prominence in the public eye of JPL and of the space program at Caltech? Inter-institutional jealousy?

GREENSTEIN: I'm neither an expert on it, nor do you have the time. The relations between Caltech faculty and JPL are intolerably complicated. There are some people who really depend on its existence completely, and love it, and properly so; it's a great management center for research. And there are others who hate it. I'm unable to go into that. I was on a panel created at JPL—and chairman for four years—to advise them on the use of their radio antennae for radio astronomy specifically. For a while, they gave five or ten percent of their biggest antenna use, so that on that kind of thing the faculty members involved would like it. A lot of the people who had worked as engineering scientists with JPL have positive, but some very negative, points of view. If you want a piece of apparatus designed and built through JPL, and have money, say, from NASA, when you go there you find that you may end with incredible trauma and all your money gone and nothing but a set of plans, because JPL has charged its full management structure costs to your little $50,000 project. So it's a very mixed bag. Astronomers, especially planetologists and the planetary and space sciences division next door are, of course, closer to JPL. I'm sure they're all schizoid in their relations, but they have to work with it. There's a different attitude, even when you have money from the government; if you're a scientist at Caltech, you're a puritan, but not if you're at JPL.

There's the problem of student theses. One of the great developments in planetary astronomy has been the development of radar astronomy at JPL by Dick Goldstein. I believe he was still a graduate student, spending tens of millions of dollars on observing the surfaces of planets by radar from the earth. It was one of these anomalous situations. There is now an external overview committee looking at the relations between Caltech and JPL, and a faculty committee. It's always been a very tender issue.

I'd like to bring up one other general topic. The nicest thing for me about giving advice within Caltech came about because I became in some ways involved with the Humanities Division.

Begin Tape 3, Side 2

PRUD'HOMME: You were talking about the development of the Humanities Division and your relationship with them.

GREENSTEIN: Whatever my reasons. I'm interested in other things than science, and one of the obvious defects of Caltech student life and faculty life is the narrowness of possible interests, cultural distractions or whatever. Fairly early, I got to be personal friends with the then division chairman, Hallett Smith, and afterwards with Bob Huttenback. There was a group of people doing good jobs of teaching recalcitrant students—not necessarily to read or write, but at least the beginnings of it; and for some of them, some interest in it. For example, astronomy required a humanities course for the master's degree long after most people had abolished that humanities requirement. Well, anyway, the Humanities Division had within it economists, people like Alan Sweezy, for example, and Horace Gilbert, who served a useful function in teaching the elements of large-scale economics, let's say. They both have broader interests than just teaching, and had, I think, quite powerful effects on students. Just like the Caltech Y was the ethical, humanizing center, the Humanities Division, including its economics, which at one time was required of every student, was a humanizing and important influence, and I was in favor of that. When I got active in local academic politics, I hoped to see that become more successful. This came just at a time when it was being decided that the Humanities Division should imitate the other divisions, producing advanced scholarship and emphasizing not the teaching ability but the scholarship credentials in its appointees. In particular, they began bringing in mathematical econometric-type economists—no longer the generalists like Sweezy or Gilbert, but specialists. While these are now outstanding scholars and good Caltech people, they're not different enough from their opposite numbers in the physics or biology divisions. They are doing quantitative work; they do some experimental work; they use computers. And I don't think that is what I had been brought up, at least, to view as the humanities.

PRUD'HOMME: Microeconomics is somewhat in fashion, one might say, throughout the country. It isn't just at Caltech.

GREENSTEIN: Yes. Well, I remember being on some committee in which the head of the Social Sciences Council was here to advise on the future growth in the Humanities Division. To get legitimacy, the Humanities Division needed a committee, and I got put on a committee [to see] if I could do them any good. And I was rather shocked to find that what the Social Science Society's—whatever it's called—head was advocating further was growth in mathematical economics, which I felt our students hardly needed. I found myself very much in the underdog position and lost all the fights. When Harold Brown came, which was I guess '67 approximately, I was still an elder statesman and he asked me to come and talk to him about the future of economics. So I went over all these arguments at considerable length. I pointed out that it would cost him nothing at all to create a new division, possibly allied with engineering in the mathematical economics area, but put history, literature, languages and related subjects in a genuine, independent humanities division. They were also looking for a new head. This was the proper occasion that he could seize to make a real change, because he should know that there was a difference between mathematical econometric-type approaches and historical-type approaches. He listened to me, with his typical attention span of around seven or eight flashes of an eye, and he said, "No, Jesse, I won't do it." And that was the end of my attempt to split the two.

I was also on a committee that helped define the requirements for what became the Dreyfuss Professorship in humanities, now held by [Jerome J.] McGann. This was a gift in honor of Henry Dreyfuss by Edward Land, then an anonymous gift to establish a million-dollar fund for humanities. The committee was looking for somebody like Kenneth Clark, or something like that—a generalist in the humanities. It didn't matter whether it was literature or art. I was sort of pro art, just because it might have some more direct appeal. We couldn't find any such person, and eventually the criteria set up were essentially those that would be set up in any good division, say, of English or history, or whatever, at Harvard, which has an enormous humanities effort. You want a scholar who's written the right number of books and who's respected at the meetings of the Modern Language Association, or whatever.

I would say the final result is that it was futile for a scientist to try to change the policies of another division. In fact, it was a failure. But the attempt, by the way, grew naturally out of the Aims and Goals Committee.

PRUD'HOMME: This wasn't just in the economics section.

GREENSTEIN: It was the taking over of what you might call a humanistic tradition by a compromised version of it, which is now called Humanities and Social Sciences. And I have no strenuous objections to social science as a science, but I don't view it as a humanity. And similarly, I tried at one time to persuade people—this, I guess, was when I was still active in academic politics—that it might be a good thing for Caltech to swallow Immaculate Heart College. I was one of the few of the faculty who spoke in favor of it. What other abortive failures can I record? They're all failures. I never won a single one of these fights.

Oh, yes, I thought that another area we might go into—and at one time it had been proposed—that Caltech consider the establishment of a giant new medical school, with Caltech contributing what might be called the engineering and computer technology relevant to the new breed of medical educators and educatees. Perhaps the best thing we could do was hire someone distinguished; there were several possible candidates, one who was world famous in depth psychology. That would have meant not behavioral biology but depth psychology. Again, I was absolutely overwhelmed. We had several visits from the guy who invented encounter therapy, [Carl] Rogers; he went down to La Jolla later—a wonderful man. We had an advisory committee in this area, and with Rogers as a member. And he showed some of the films he used in therapy and in education. And I thought they were both emotionally and educationally extremely powerful; they taught something about psychology, or depth psychology. And Dick

Feynman stormed out of one of the meetings saying that if they appointed a witch doctor of the mind, he would leave Caltech. And Murray Gell-Mann was somewhat opposed, also. So all of us big thinkers didn't agree on the importance of the more traditional, I would call, soul-oriented rather than brain-oriented parts of behavioral biology. I had that out with Arnold Beckman, and it happens he didn't agree with me at all, either. He felt there was a prospect of doing work; the brain study, of course, he was in favor of that, and built the building. That was a thing that could be done on purely quantitative chemical objective grounds. All I can tell you is that as an amateur humanist, I failed.

PRUD'HOMME: It's interesting, because Millikan's theories were that one should have a definite classical background in order to understand the moral implications of the science that you were doing.

GREENSTEIN: Well, we have a professor of law—I think he's part-time—on legal ethics and problems. But it seems to me there has been an evasion, possibly caused by lack of money and the desire not to compete with the other universities. We have resolutely turned away in the humanistic and related areas from generalists toward hard-science-type specialists. And as a hard scientist, you see, I was trying to tell them that hard science was not all there was. It might help ultimately explain psychiatry. I have a good friend, Edward Stainbrook, who was head of psychiatry at USC; he became a great believer in pharmacological psychiatry, although he was trained completely as a talk psychiatrist.

But all in all, my involvement has been time-consuming. Here I am on the godawful Baxter Art Gallery governing board—I've been in and out of that for years. I despise the art, but I feel it's my duty to help it. It's a very distressing world, in the sense that when I grew up in college, art and culture were a part of a scientist. And now it isn't.

PRUD'HOMME: Because science has expanded so much, and there's so much to learn.

GREENSTEIN: If you're a good scientist, you've got all the time in the world. The better the scientist you are, the more time you have. It's just an attitude that hard science has had so much success in the physical and the chemical, biological sciences now, that this example should be passed on to the "soft scholarship" areas of the humanities. I'm glad I said this, because I don't think it's ever going to get recorded. Rod Paul is, to me, one of the most intelligent humanists and human beings and has long had feelings about this situation at Caltech. And I tend to agree with him and not with my physics colleagues.

PRUD'HOMME: You wanted to talk about Earnest Watson.

GREENSTEIN: I just want to mention one thing. I believe that it would be important for your project to dig into the minds of the older of us who remember how much Earnest Watson meant at Caltech. The place had a simple organization, and he was a very busy man who did everything, who had a large amount of available time to think, to talk, to help people on a personal basis. I mentioned Rod Paul; in fact, I have asked him about this. He felt that Watson is one of the least recognized builders of Caltech, overshadowed by Millikan. By the way, Millikan made him a junior instructor before he finished his PhD; he never got his PhD. He never really had an official named position, although for many of Millikan's later years it was Watson who was doing everything, because Millikan was old.

Watson was a man of very wide culture. He had some money, was popular with and on the same wavelengths as the trustees. I would say for the generation that I knew him—twenty years, roughly—he never did a thing on any but the noblest motives. He was a man of incredible moral fiber. It was: "Is this appointment good? Is the man the best? Is this the best appointee possible? If you really need him, we'll get him." He did everything to make it possible. He would do all kinds of little personal things for potential new faculty. And I just have a feeling that his papers—I'm sure they are somewhere—should be found. He's an ideal, single, non-committee academic administrator. And nobody mentions him.

PRUD'HOMME: Was he a warm person? Was he proud? Was he reticent?

GREENSTEIN: He was extremely shy and reticent. But he had strong interests in other things than science. And if you could get him started, he was fine. He was afraid of women; he was shy. I think he went on a cruise when he was sixty or sixty-five and met this wonderful woman—now Jane Watson; Jane Werner her name had been—who was nearly of his age, was editor of the Little Golden Book series for Simon & Schuster, and in fact has the claim of having published more single copies of anything than anybody except the Bible. Eight hundred million copies in Urdu or something. But she was a great woman. They met late in life, and she made him happy. I would really think that Jane should be talked to. Of course, she married him when he was leaving Caltech. Also, by the way, Earnest had artistic sense, a collection of Mogul miniatures which he left the University of Wisconsin, and most of the good science pictures around here were his. He was a coauthor with Millikan of an elementary text. I think that's actually why he didn't get his PhD, because Millikan wanted the text finished and so Earnest wrote it. I don't know enough about him, except that all my relations with him were essentially ideal.

Caltech has been, and is, a great place. Whether it can stay the greatest when research is now done everywhere is harder to say.

PRUD'HOMME: Do you see a brighter future for women in science at Caltech?

GREENSTEIN: Oh, I think we better, or unless they change the law, we'll all end up in jail. In my own science, I have a black name, because I have written, in fact, somewhere that I was pessimistic about the opportunities for women in astronomy. I was called on the carpet at a 7:00 a.m. breakfast meeting when I was vice president of the American Astronomical Society by a gang of women who tried to beat me up, intellectually, while I tried to explain why I felt it was difficult. I think there is historical example that in certain areas—specifically, within astronomy—women have made enormous contributions because of certain mental sets which they have. Now that doesn't mean they will lack the others necessary for the future. I think women are probably very good in computer programming, and if that's the way science goes, then they will do fine. In seeing things in the large—seeing synoptically; an idea that comes out of many clues in different areas—I think they're just wonderful. Historically in astronomy that's been what they've been good at. The most famous woman astronomer is Annie Jump Cannon, who worked at Harvard, classified the spectrum of 300,000 stars, and invented the process of spectral classification. She looked at tiny little smudges on a plate with half a dozen features, from different chemical elements which were recognized, and she could see all of this and then remember what another one looked like, and say this is to this as this is to that—she saw a sequence, a simple evolution of characteristics. So she invented what's called spectral classification, much like systematic botany. That was done 1900 to 1930. She got a half a dozen honorary degrees, but she never got a salary commensurate with her achievement. I wrote an obituary for a very great woman, Cecilia Payne-Gaposchkin, who was British and came in 1923 to Harvard and in 1925 wrote a thesis—a book now, in a sense—the introduction to the quantitative analysis of the composition of stars and determining their temperature and pressure. She didn't develop any of the original theoretical work in it, but she applied it from work then going on in Great Britain and Germany, largely. She made many brilliant discoveries. She was censored several times by her boss, Harlow Shapley, who would ask some distinguished older man spectroscopist, who would say, "Oh, no, that can't be." And Cecilia wasn't allowed to publish her discovery. So there's a terrible injustice. She died only a couple of years ago at the age of eighty. I was terribly fond of her. And she had the same interesting characteristics; she was very much like Annie Jump Cannon in being able to see a lot of things at once and remember them, but she was also much more mathematical. So there you go, in twenty years from a pure systematic description to application of atomic theory. There are a few outstanding women right now. Up at Lick Observatory—we tried to steal her several times—a woman named Sandra Faber, who is as good as anybody else in observational cosmology. But you can number them, nationally. There are maybe ten outstanding women astronomers out of maybe a few hundred total, and there are thousands of astronomers.

PRUD'HOMME: Your implication is that women are not very good at making, developing, a new original theory.

GREENSTEIN: So far, at least in astronomy, they've been good at synthesizing ideas. One of my own students, Virginia Trimble, who is one woman I know who has two professorships, one at Maryland and one at Irvine. Her husband commutes with her six months at each place. Anyway, she's one of the best people to invite to a scientific four-day symposium, because she'll put it all together and get everything right, and credit the right people and know what it meant. But as independent creators, women's contributions have been still mild. This is a psychological bent, maybe linked to sex roles. On the practical side, even the best of them have gotten into troubles because of society's partiality to family life. We tried to inveigle Sandra Faber down here—gave her eventually a full professorship offer, although she was very young, and offered access to the biggest telescope. Her husband was a lawyer in San Jose, and his firm was anxious not to lose him. I think they even decided they'd even open a Southern California office if she came down here. Then at the last minute, I guess, they either decided to offer him a full partnership up there or more money. And she called me and said, well, she wasn't going to come because her husband really wanted to stay in San Jose, although he had originally offered to follow her. This was only a year or two ago. So we're still playing out the old games: A woman can't be a scientist because she's got to get the husband a job. I'm very pessimistic about it. Certainly for getting equality in numbers, I don't think there's a prayer. I think we have, as much as possible, organizationally, created equality of opportunity, but people don't use it.

PRUD'HOMME: It's not being accepted. I think you're right.

GREENSTEIN: It's a rather tragic note to end. I'm very sorry about it.

Begin Tape 4, Side 1

GREENSTEIN: This tape contains supplementary thoughts by Greenstein, [recorded on] November 8, 1982, concerning the material in the Archives set of Oral History interviews.

On rereading the transcript of my interviews, I felt strongly that while it contains much personal material, it gives an incomplete picture of how astronomy developed at Caltech after the opening of the graduate department in 1948 and subsequent arrival of new faculty.

The opportunities provided by the 200-inch telescope at Palomar were of the most challenging. The instruments designed to go with the telescope were the creation of Ira S. Bowen, the director of the Mount Wilson and Palomar Observatories. Formerly a Caltech professor, he had a strong interest in optical design and a practical working knowledge of what was possible and desirable. When the telescope went into full operation in 1951-1952, a fundamental set of instruments had been designed and were available. At the [prime] focus there were available a conventional plate holder with offset guiding, a photoelectric photometer for the standard UBV magnitudes, and a nebular spectrograph designed by Bowen and with solid Schmidt optics built by [Don] Hendrix. The latter was used mainly by Rudolph Minkowski and

Milton Humason, experienced observers familiar with work on faint objects, including invisible ones. At the coude focus, there was available one of the most elaborate spectrographs built for astronomy, using Schmidt cameras to give spectra of dispersions from 2 to 18 A/mm, and a special aplanatic camera, which gave 38 A/mm, all of good resolution. These instruments were built in the Central Engineering Shop at Caltech, the optics supplied by Mount Wilson. In general charge of mechanical design was Bruce Rule, whose office was in Robinson Lab. Rule worked with Greenstein and Bowen on the Palomar budget and personnel on the mountain, and with Greenstein on refurbishing Robinson Lab, which had been used for other purposes for fifteen years. Also at Palomar were the 18-inch Schmidt telescope, used extensively by Zwicky, and the 48-inch Schmidt telescope, which after preliminary tests was allotted to the National Geographic Society / Palomar Schmidt Sky Survey. The latter was managed from Caltech and carried out by night assistants on the mountain with general supervision by an astronomer from Caltech. Minkowski examined each pair of plates as they were produced, to decide whether they were good enough to accept or needed repetition. Soon after full operation started, a 20-inch reflector was moved from Mount Wilson to Palomar to provide a smaller photoelectric telescope for more routine uses than the 200-inch.

The available equipment was as good as could be conceived at that time; however, the electronic revolution had not fully taken place. It was an important, long development to bring new auxiliary instruments to the 200-inch telescope. Another milestone was the construction of a 60-inch special-purpose reflector at Palomar. The Carnegie Institution of Washington obtained the funding of about a million dollars from the National Science Foundation, and it was Caltech's responsibility to provide the dome, building, and auxiliary instruments for the new 60-inch. The telescope was a design of Bowen's, which made possible work not requiring the 200-inch. The 60- and 100-inch telescopes at Mount Wilson were being seriously impeded, in work on faint objects, by the existence of the city lights of Los Angeles. Funding for the construction of the 60-inch building was provided by the Oscar Mayer Foundation. Lee DuBridge had met Oscar Mayer Jr. on a Washington committee, found him interested in astronomy, and suggested a visit to Palomar, where Greenstein made an excellent contact. The Mayers eventually gave the funds required for the rather expensive building and have also supported work at the Owens Valley Radio Observatory by providing buildings there. At the time of the dedication of the 60-inch, a Carnegie Institution of Washington trustee, Crawford Greenewalt and his wife, of the

DuPont family, were much impressed by a talk by J. B. Oke. Oke made it clear that with the new electronic equipment envisaged, the 60-inch would soon rival what the 200-inch could do when it was first commissioned. This proved an important seed dropped, because Crawford Greenewalt eventually gave money [during his lifetime] for the Irenee Dupont 100-inch at Las Campanas, Chile. Carnegie eventually invested about ten million dollars in developing that excellent site and provided modern electronic instruments.

Returning to the question of more modern equipment for the 200-inch, it was early realized that electronics would eventually rival and replace photography as a fundamental technique. It was therefore essential that at Caltech some of the new faculty have ideas on the plausible improvements. In many cases, they designed and, when necessary, obtained federal funding for such new equipment. It would be impossible to list fairly all of those involved. On optical problems, they worked closely with Ira Bowen, but it was an individual effort in most cases. For example, J. B. Oke wanted quantitative information on the radiation received from stars and galaxies at many different wavelengths. He first designed a device which scanned the spectrum of the object across a photomultiplier, using part of the prime focus spectrograph, adding the electronics and scanning features. Bob Leighton was on a government committee to advise ARPA [Advanced Research Projects Agency], which wanted to support modern optical devices. He told Greenstein about it and in a few days, Oke had submitted a plan, which was funded for about a quarter of a million dollars, to build a scanner which used thirty-two photomultipliers simultaneously to look at thirty-two parts of a spectrum and quantitatively record the output. This is perhaps the first "modern technology" device on the 200-inch. It worked successfully and is still in occasional use after nearly twenty years.

Another area of improvement was in the use of image intensifiers, originally military devices to assist vision in the dark. These had been classified; as they became available, they were applied where possible, either for photography of the sky, which proved hardly necessary, or more usefully on the various spectrographs in the coude focus of the 200-inch. These provided real gains of a factor of five over photography and an extension of spectral range to the near infrared, at much greater gain. Greenstein obtained money from NASA for one of these, and eventually for another on a higher-resolution camera. At the coude, it was also necessary to provide an extra image-tube device to see faint objects which could now be worked on with the image tube in the coude cameras. To work on still fainter objects, notably galaxies and quasars, a spectrograph was designed and built with an image intensifier tube to be used at the Cassegrain focus. At first, the scientist worked in the cage at the bottom end of the 200-inch telescope in a swiveling chair. Later, a television repeater was used to pipe the view of the sky from the cage down to what was called the data room. There control and data recording were carried out on computers, with remote TV guiding. With regard to these ideas, Bev Oke, James Gunn, Maarten Schmidt, and Steve Shectman should be mentioned. The data room has now been enormously expanded, the telescope is completely television-guided, and almost all data is taken . . . [tape ends]

Begin Tape 4, Side 2

on computers, discs or tapes, for further processing in Pasadena. Nevertheless, the observer remains necessary, to judge the quality of the data, whether the object is interesting or not, and so the problems of the scientist-computer interaction had to be studied. The elaborate computer programs for reduction of electronic data have the same goal as those at the Jet Propulsion Laboratory for processing electronic images returned from *Voyager* probes past the major planets. As these instruments were developed, earlier versions of them were transferred to the 60-inch at Palomar, which has been largely used by graduate students. Many important results have been obtained with the 60-inch, since more time on it was available. The other important novelty was that graduate students who had been brought up in the computer age found it easier to adapt to the new instruments, to design the analysis programs, to handle the data, and to provide experience to make these complex instruments essentially astronomer-proof. The last step at the 200-inch has been the development of the double spectrograph at the Cassegrain focus. This was thought of nearly ten years ago by Oke and Gunn. A light beam is divided into two, to take advantage of the highest efficiency detectors usable in the red and in the blue, separately and simultaneously. While the instrument was still in plan, an important new technological development become available. This arose because James Westphal, of planetary science, was a superb technician and familiar with the development of new detectors; he had worked on the use of television-type devices, image intensifiers, and so on. When the space telescope concept was being developed, a new detector called the charge-coupled device was being developed, for use in the space telescope. Westphal and Gunn became the principal investigators for what is called the Wide Field Planetary Camera for the space telescope, which uses these CCDs. From this developed the possibility of putting a CCD sensitive in the red, and another sensitive in the blue, in the new double spectrograph. This is at present the most superb spectrograph in existence, making the spectrum observable from the near ultraviolet to the near infrared and having nearly perfect efficiency in the red. The overall gain is hard to quantify but is at least a factor of five in resolution, four in speed, over the Oke multichannel spectrophotometer. It is now possible to obtain spectra of objects that had never been seen on any photograph, even with the 200-inch. At the prime focus, a device called the Prime Focus Universal Extragalactic Instrument, with the lovely acronym of PFUEI, has been developed by Gunn. It is being extensively used by him, Oke, and Schmidt, to obtain spectra of very faint galaxies, clusters of galaxies, and quasars. Clusters of galaxies to redshifts of a hundred percent are now routinely observed and photographed with the CCD. The CCD pictures are among the most beautiful ever taken of the sky. Just before Gunn left for Princeton, he began plans for what is called the four-shooter, a camera containing four CCDs, which will record the information from about three million spots on the field of view at the prime focus of the 200-inch. This data is fed into a computer, and the observer can immediately view the picture, after about a fifteen-minute exposure, and see objects ten times fainter than ever had been photographed; they decide to observe one, move it into the center of the field automatically, and then convert the device to a spectrograph to get the redshift.

While all this may seem overly technical, it is necessary to realize that such devices have revolutionized astronomy in perhaps ten years. Their cost is not small, perhaps a quarter of a million dollars each. But the telescope that would do the equivalent cannot be built on Earth, and if it could, would cost many billions of dollars. The interaction between technology, an alert faculty, and the availability of the 200-inch, has made it possible to take advantage of the full power of the computer to handle data. A large computer, called a VAX, has recently been funded and installed in an annex of Robinson Lab, giving us two VAXes, one for radio astronomy and one for processing the images produced by the new technological devices.

Greenstein feels strongly that this new world should be appreciated; the pioneers in the development of technological applications in science are enormously important. How does this reflect in his own work? Perhaps the easiest thing is to realize that with photography of spectra with high resolution, coude spectrographs were the only possibility. It was a triumph to work on stars visible to the naked eye. The first object of peculiar composition was Upsilon Sagitarri, analyzed in 1940, at the 82-inch of the McDonald Observatory. It was a star with an unusually high helium-to-hydrogen abundance, the first graphic evidence of the nuclear furnaces in its core converting almost all hydrogen into helium. Having a constellation name means that it was seen by the ancient Egyptians. At present, quantitative analysis can be carried out on stars 10,000 times fainter, at high resolution. Greenstein has worked for about thirty years at the 200-inch, starting his study of white dwarfs at the coude, with a magnitude limit near twelve. He then started to use Humason's prime focus spectrograph, and with it observed some 100 white dwarfs' spectra, and many of the very first spectra of quasars, before their nature was recognized. Exposures as long as five hours were necessary; one could not work fainter, because the sky drowned the faint starlight. With the multichannel spectrograph, although at lower resolution, Greenstein observed an additional 400 white dwarfs and first explored the various types of composition present in their atmospheres. For such work, the Oke multichannel spectrophotometer permitted determination of the temperatures as well as compositions, and typical integration times were less than an hour. In 1982, in the summer, with the double spectrograph, Greenstein observed approximately 150 white dwarfs in eight nights; the resolution was equal to that at the prime focus, the wavelength coverage was about three times as great, and the signal to noise enormously improved, with a maximum integration time of about fifteen minutes. Discoveries made are hardly digested, but indicate important new problems connected with the atmospheric compositions of the white dwarfs.

At age seventy-three, it is somewhat odd to be faced with a device that produces millions of numbers in fifteen minutes at the telescope, and to have to learn to use a very large computer to manipulate these numbers in Pasadena. Yet the results are already exciting. Under the intense gravitational field in a white dwarf, where gravity is 100,000 times that on Earth, heavy elements settle out of the atmosphere. Lines are enormously broadened by pressure, which is many times that of Earth's atmosphere. The double spectrograph permits determination of both temperature and composition, and in much more detail than did the multichannel. It has been possible to find in spectra of white dwarfs in which no lines had ever been seen before that weak features exist. The theory of the settling of heavy elements out of the atmosphere had been developed, and apparently explained most of the peculiarities in compositions seen before. But new peculiarities, not yet explained or understood, now appear. Certain stars which were supposed to have no hydrogen have, indeed, weak hydrogen lines. Others in which helium probably dominated but showed no lines, now show some helium lines, and notably bands or lines of carbon. Carbon is one of the elements created by nuclear synthesis in the cores of stars, as is helium. Somehow we now see samples of material from the dense core, in spite of the enormous pull of gravity, which should make heavier elements settle out of the atmosphere. Thus, at the end of a career, one opens, with new technological devices, as many new questions as one had solved before.