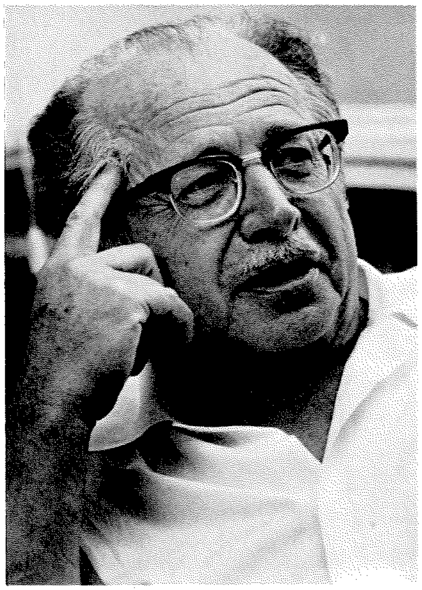
Jesse L. Greenstein — How It Was



Jesse L. Greenstein is the Lee A. DuBridge Professor of Astrophysics, Emeritus, at Caltech, and he was interviewed for the Oral History program of the Caltech Archives by Rachel Prud'homme. Those interviews led to 73 pages of transcribed material, only a small part of which has been excerpted here. We have, for example, touched only lightly on Greenstein's research, though it has been continuous and distinguished throughout his career, as has his service to a multitude of professional and governmental agencies. Retirement in 1980 has changed those aspects of his activities only slightly. We have chosen for this issue of E&S *to concentrate on his story of the history of astronomy at Caltech and of his organizational activities within the Institute since 1948.*

Rachel Prud'homme: You were appointed to the staff of Mount Wilson/ Palomar in 1948 and professor of astronomy at Caltech. What were the Institute and the Observatory like then?

Jesse Greenstein: I was asked by Earnest Watson, dean of the faculty, to undertake the creation of graduate, and some undergraduate, teaching in astronomy at Caltech, to take the lead in acquiring faculty, which faculty would automatically become members of the Mount Wilson and Palomar Observatories staff. On my arrival, Caltech had only one professor in astronomy, Fritz Zwicky. The other people in astronomy were a research associate named Josef Johnson, who taught undergraduate astronomy, and Albert G. Wilson, a senior research fellow; both worked on Zwicky's research projects.

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." There were and are no departments and no heads, of course, but his letter was operationally descriptive. At the rate of better than one a year we began to build up the Caltech astronomy group. The other side of the scientific partnership was the Carnegie Institution of Washington, with offices on Santa Barbara Street a few miles from the campus. As a group, their astronomers had been in Pasadena since 1906 or '07 when George Ellery Hale came to Pasadena. The arrangements that led to the creation of the Mount Wilson/Palomar Observatories were complete before I arrived. The 200-inch reflector had been funded by the Rockefellers, the money given to Caltech, and its construction managed by Caltech. The formal dedication was in 1948, soon after I arrived.

RP: The administrative set-up was complex?

JG: It made my life miserable. Equally well I could say it made my life easy. There were nearby, as colleagues, about 20 distinguished astronomers of varying ages doing research in a wide variety of fields, mainly concentrating on observation rather than interpretation. As a group they had created large-telescope astronomy for the world. They had good operating 60-inch and 100-inch telescopes, solar telescopes, and a spectroscopic laboratory.

I was quite different from them. I had built up a reputation, both of observing and doing theoretical work, at the Yerkes Observatory of the University of Chicago. My thesis at Harvard contained both mathematical theory and observation. The pre-1948 Mount Wilson staff were an incredible bunch of gentlemen-scientists — a breed which doesn't now exist. They didn't share, perhaps didn't approve, my theoretical bent, but they never disagreed with my recommendations in the observatory committee (which was joint between the Caltech and Carnegie institutions) when I suggested appointment of still another young theorist I wanted. In fact, essentially all our early appointments were theorists from Yerkes — who all became very good observers. There was more observational material sitting in Pasadena unanalyzed, and there was more observational opportunity with big telescopes, than anywhere else in the world, even before the 200-inch was finished.

RP: Why did you take the Caltech appointment?

JG: I had had a little administrative experience in family business and enjoyed it. I liked the activity of the war, though not the war, naturally. I enjoyed dealing with the military, for example. Although I gladly went back to research in 1946, I could see problems in Yerkes' future. I was something of a "hot property" at the time in the sense of having several flattering offers, including one from the Lick Observatory, to join their staff. But I knew that I could build things up here in an excellent place and that I would enjoy building a group. And I found, in fact, that it was a pleasure. Almost none of the administrative duties were seriously time-consuming except finding interesting scientists and keeping them happy. And I could do good scientific work with the 100-inch, and later with the 200-inch when it was completed (in 1952).

RP: What were your impressions of the Institute and the community when you came?

JG: Caltech first seemed incredibly small. I remember walking from the Athenaeum, where I stayed (in November 1947), to the Robinson Lab and wondering whether they would ever be able to pay my salary, no matter how small that salary was. It was a very tiny institution, compared to Chicago.

I had a happy personal introduction through H. P. Robertson, a physicist in relativity theory, who had come to Caltech from Princeton. He was an old friend since I was a graduate student, and often visited. He was one of the decisive reasons for my coming. And there were also two outstanding Germans at Mount Wilson — Walter Baade and Rudolph Minkowski — with whom it was easier for me to form intellectual links, than with some of the older Mount Wilson spectroscopists.

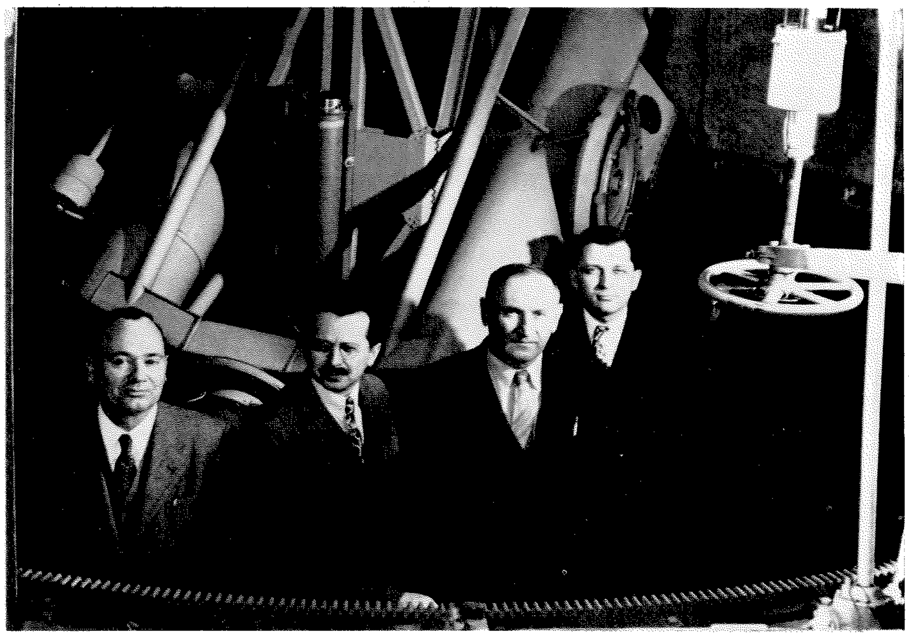
RP: You started out essentially building with the graduate department?

JG: Yes. Under the original agreement between the two institutions, those people at the Carnegie Institution who were able to and wished to teach were to be available for the graduate teaching program and thesis research guidance. My understanding had been that they would provide the teaching equivalent of a full-time faculty member; cooperatively they would provide a course every term. That proved unworkable. Ira Bowen, who was then the observatory 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." I felt, however, that their failure to provide astronomy education could be understood by their years of purely research orientation. It also gave me a strong hand at Caltech on new appointments. But Mount Wilson was an excellent source of thesis guidance.

RP: What about the students in astronomy?

JG: We started with only a few; three good students were plenty each year. I think that those who went to Mount Wilson, in the earliest years, did get significant help from the Carnegie people on Santa Barbara Street. They also got instruction on how to use the telescopes and on what were important outstanding observational programs. Of course, it was the beginning of the electronics era; electronics amounted to nothing much in the older astronomy, but was essential in the newer. Our students used the 100-inch and solar telescopes from the beginning.

RP: I want to go back to what the community was like; what your social life was like.



Caltech's astronomy "department" in 1949 consisted of, left to right, Josef Johnson, Jesse Greenstein, Fritz Zwicky, and Albert G. Wilson. One high priority for Greenstein was to build the department up in numbers and diversity.

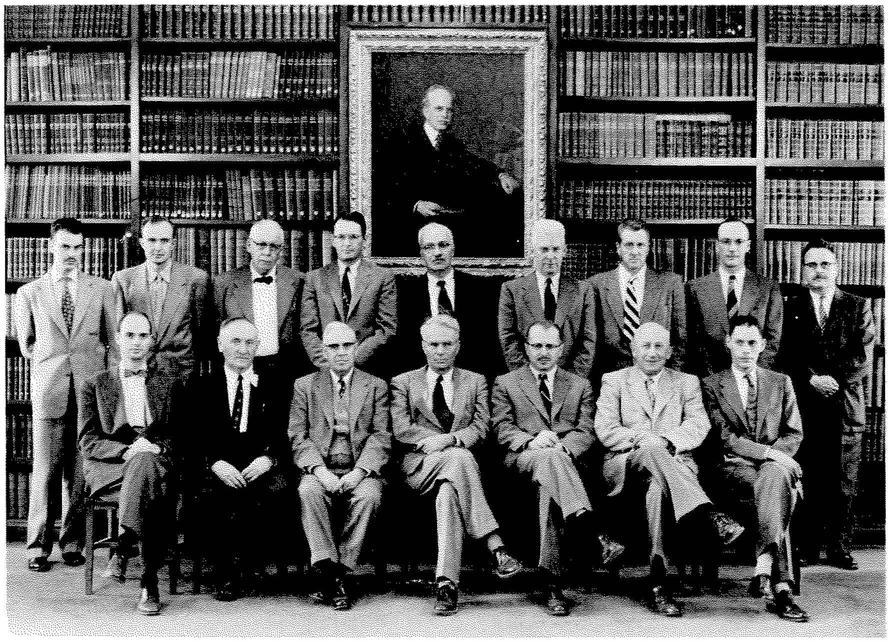
JG: Caltech was a remarkably sociable place. I'd heard before I came that the parties were extraordinarily good; and it proved to be true. The physics department had not only Bob (H.P.) Robertson, but Charlie Lauritsen and his son, Tommy, and Willy Fowler — all of whom were outstanding party people and outstanding scientists. A good deal of science and, later, national affairs were discussed at parties. We met a lot of important people and a lot of crazy people too. Robertson had been chief scientist for NATO and had Sir Solly Zuckerman, a leader of British science, as a frequent visitor. And of course, though I didn't know him as well, von Kármán always had distinguished visitors, including a baseball player, retired, who was his bodyguard. Whenever von Kármán was walking down the Olive Walk with the Guggenheim people to go to lunch, Moe Berg (the baseball star) was walking along, a little bit in front and a little bit on the side, looking around.

RP: Tell me about Lee DuBridge. What kind of person was he?

JG: Well, charismatic, as everybody says; square, which is part of his charisma. 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. The only troubles I had were appointment troubles, never money troubles.

RP: DuBridge seemed to have a great knack for finding money.

JG: Appointments were always complex because of the Carnegie link and the fact that I also had to convince the Caltech physicists. The physicists cared and had opinions about astronomy. And so you had to fly a person through the astronomy group, the physics group (with Bob Bacher's help), the observatory committee (which meant also the Carnegie group), and Watson, and then past the presidents of the two institutions. In between, there was Bacher, the division chairman, later provost. But an exciting thing with DuBridge was that he would ask questions about whether this was the best possible man. If he was convinced, he said, "What can we do to make sure he comes?" He loved to know what was going on in astronomy right now, in the last few months. He'd know what to say when he gave a talk or had to raise money. And he was an extremely quick learner. He remembered and could explain what research everyone had done, even though he was far from the actual work. We were lucky in our administration.



A portrait of the founder of the Mount Wilson Observatory, George Ellery Hale, is an appropriate backdrop for its staff in the mid 1950s. Front row, left to right, William Baum, Fritz Zwicky, Milton Humason, Ira Bowen, Jesse Greenstein, Walter Baade, and Armin Deutsch. Back row, Guido Munch, Allan Sandage, Edison Pettit, Horace Babcock, Rudolph Minkowski, Seth Nicholson, Robert Richardson, Donald Osterbrock, and Olin Wilson. The observatory celebrated its 50th anniversary in 1955.

RP: What kind of people did you look for?

JG: I looked for people who could understand the physics applicable, at that time atomic or nuclear physics, and were good mathematicians as far as astrophysics required, and who might become interested in observing. 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 be attracted by the opportunity of always doing something new. It's less true now than it was; people have gotten more specialized, are labeled by their subdisciplines. I liked generalists, and tried to get them.

RP: Did you eventually promote your own students?

JG: We did keep some. Also graduates from Yerkes, McDonald, or Princeton would come here as postdocs. Some would stay and some would join the Carnegie staff if their dominant interest was observing rather than interpretation. In the mid-fifties when we started in radio astronomy, it was different. We needed experienced engineering and electronics types, and we had no one on whom to build.

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

JG: I was secretary of the organizing committee in 1954. Walter Baade, Rudolph Minkowski, and I had been yelling that radio-astronomy observations were essential for optical astronomy. The interpretation of extragalactic radio sources based on the identifications by Baade and Minkowski had shown that we were finding more luminous, exciting galaxies by radio means than by others. For example, after 30 years, the largest red shift found by Milton Humason, who had worked on normal galaxies, was 20 percent. But a strong radio galaxy was measured by Minkowski at a red shift of 46 percent. Radio astronomy made that possible in one jump. And when quasars were identified, red shifts passed 300 percent in a few years. That 1954 conference was a decisive turning point in U.S. radio astronomy.

Our first radio astronomy head, John Bolton, was a Brit living in Australia, who had identified many of these extragalactic radio sources. Taffy Bowen, the head of radio astronomy research in Australia, was an old friend of DuBridge's. While it may have been an old-boy, old-school-tie club, none of these characters wore ties. They were engineering-oriented, practical. John Bolton could do a better weld than most welders, and he welded most of our first radio antenna himself. Our radio observatory became successful, and radio and optical astronomy are fully integrated. The present executive officer for astronomy is a radio astronomer, Marshall Cohen. We try to compete on all fronts, we really try.

RP: Is there any place left in the world that has the biggest and best and the greatest and grandest? Is there institutional competition?

JG: Oh, sure, there is. I would say that we compete with the National Observatories, all of them. The National Radio Observatory is much bigger than ours, but I think we've got the wave of the future going in what's called "submillimeter radio astronomy," which Robert Leighton, Tom Phillips, and others are developing here. And also in very-long-baseline interferometry, in which Marshall works. Probably the most important future rival in optical astronomy doesn't as yet fully exist, and that won't belong to one institution. It is a location, 14,000 feet high on Mauna Kea in Hawaii, where many new telescopes are being placed by groups from Canada, Hawaii, France, Great Britain, and the United States. You can't beat excellent seeing weather and everybody cooperating. I mean, it's going to be *the* center.

Whether a private institution, even with government support, can still be preeminent, we don't know. In optical astronomy, we have enough square inches of telescopes in good locations so that, if we can keep up with current instrumentation, I won't worry, as long as we attract bright young people. We, unfortunately, having bright young people, also have become a target, losing those people. I can't blame them for going; I left Yerkes. And the other thing is, there's danger if you don't build up young staff continuously, and get self-satisfied. We don't have enough money now to add young people, as we used to.

RP: You spoke of Fritz Zwicky. What was he like?

JG: Fritz was a self-proclaimed genius, and in many ways he was a real one. He was a protégé of Millikan, and had not been happy in the physics department because his opinions in physics and his methods of teaching were both amusing and controversial. His teaching was directed to potential geniuses who would think as he did. And his interests in physics were premature for the state of physics then, some involving solid state problems. He became 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. His major contribution, well before I came, was in discovery and study of supernovae. Also his early interest in neutron stars and stellar collapse.

He was not popular with any establishment, and he was often wrong. However, from his study of the clusters of galaxies, on the 48-inch Schmidt, Zwicky published several important catalogs. He also discovered and cataloged individual galaxies of interesting appearance, interacting or blue, i.e., hot or disturbed. Although he misinterpreted some of these observations on the basis of his general philosophical theory, in my opinion his factual discoveries in this cataloging are his largest claim to fame. There are good reasons why younger astronomers in observational cosmology depend heavily on his work and admire his contributions. He had an open mind of his own.

RP: You mentioned Bob Robertson.

JG: Bob was the leading exponent of the application of general relativity to astronomy. He was a brilliant applied mathematician. He thought through the possible observational tests of general relativity, which he helped develop, and which guided extragalactic research for years. He invented the "Robertson/Walker line element," the usual relativistic description of the geometry of the large-scale universe. After he came here from Princeton, he worked closely with the observers at Mount Wilson — before we started astronomy at Caltech — and with Richard Tolman, another great general relativity expert. Tolman had, for many years, worked with Edwin Hubble. Tolman had much national influence and was one of the first scientists to recognize the fundamental dangers of nuclear weaponry, and among the first of the modern breed of doubters.

RP: You've had so many well-documented major projects. Would you talk about some of them or any event in relationship to the people you collaborated with?

JG: The most exciting thing that has happened is the de-astronomization of astronomy. We had to keep an open mind. New astronomy grew in Robinson Lab; but has colonies all over campus and at JPL. It has links with the planetary sciences group, and links with infrared, which is in the Downs Laboratory. We have the new millimeter-wave astronomy, the principal growth area of our radio observatory, also over in physics, with the receiver development centered there. Cosmic ray physics, exemplified by Ed Stone and Robbie Vogt, is in physics; they do experiments in space. For years, our closest link in physics was with Kellogg, the low-energy physics lab.

I worked with Leverett Davis in 1951 because I'd done a thesis on interstellar absorption by small dust particles. A new phenomenon was discovered in 1949 by astronomers, polarization of light in space. Leverett was a classical physicist capable of solving any problem, and we got together. We had a hot race with Lyman Spitzer of Princeton, who had a different theory. I think we proved to be essentially right. It's amazing that a correct theory can be 30 years old, improved and modified, but with no serious errors found in our analysis. The collaboration involved my ideas on what could be out there, in space, how small particles interact with light, plus Leverett's wide knowledge of classical mechanics. He was particularly expert because of World War II, in which he had studied rocket ballistics. Our problem was how little non-spherical dust particles in space are spun around by collisions, speed up, and how they can be lined up. We solved it. Such collaboration became possible only because we knew of each other; Caltech is a small place where you could always talk to an expert.

My relation with Fowler on the growth of the new discipline of nucleosynthesis, the origin of chemical elements in stars, began in 1950. This was also based on an old interest of mine. In 1940, one of the stars whose composition I worked on provided the first analysis of the composition of a star in which nuclear reactions had drastically altered things. I had invented a practical method for the mass-production analyses of stars of either normal or abnormal composition. At first, there were few clues as to what abnormal composition meant. I began worrying about that, and it seemed to be all related to nuclear physics. When I was writing my first paper on the amount of lithium in the sun and its isotope ratio, I found that Ed McMillan, later a Nobel prizewinner, had worked on that when a graduate student here. That was one of the first nuclear reactions produced in the laboratory, lithium destruction by proton bombardment. They found that the sun also destroys lithium. It took me a dozen years to start exploring what McMillan had noted — there was something interesting to find out about nuclear physics in the stars.

My first paper trying to make sense of stellar nucleosynthesis, after getting a surprising result on carbon isotopes, was in 1952, at a meeting in Rome. In 1952 I also mentioned a process in which neutrons are produced in stars, neutrons which are important for the production of peculiar elements. Clearly I was beginning to have to understand nuclear physics. So I learned by listening to Fowler, studying the possible astronomical reactions that might occur at various stages of nuclear evolution of the stars. The walls of Kellogg were covered with diagrams of energy levels in the light nuclei, based on laboratory experiments. Nuclei that reacted easily would be destroyed, and therefore be rare. If we could see the central, hot nuclear furnaces, we could test nuclear theory directly, and also our ideas of how hot stellar interiors were. But my composition measurements were only of the surface layers. That is a typically astronomical predicament. We can only measure what nature gives us. Fortunately, in evolving stars, material from the center eventually reaches the surface to show what happened. Some stars explode, others blow most of their mass into space in stellar winds. So I emphasized programs to search for elements or isotopes that would be clues to processes in the core. With many collaborators, with spectra from Palomar and Mount Wilson, we analyzed nearly a hundred stars. I collaborated with Willy only a few times, while he built up a large group of physicists and theoretical astrophysicists in Kellogg. But much of the factual information about the results of element building in the stars came from our, and my, observations of stellar spectra.

RP: You worked with Sir Fred Hoyle?

JG: Well, only a little; there I served as a catalyst. Fred was a bright person in many fields; he was deep and worked problems out. But he had not been accepted by the English establishment. It took something of a revolution even at Caltech for me to get him a visiting appointment in astronomy to lecture in 1952-53 on his theory of the origin of the chemical elements in stars. That course was well attended, especially by the Kellogg nuclear physicists. Fred predicted a certain new property of nuclei in the reaction that produced carbon — there had to be a bound level. This level mediated the reaction of an unstable nucleus called beryllium 8 with helium atoms, to make stable carbon 12. Since carbon is known to exist, abundantly, in the universe, he predicted the existence and energy of this strong resonance. In advance, he certainly could not know whether the resonance actually 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, theory giving a testable prediction — it doesn't often — and Fred became deeply endeared to the hearts of the nuclear physicists in Kellogg.

RP: Where did your postdocs in astronomy come from?

JG: For the about 15 years in which I was bringing people in, half were Europeans or Japanese. Many of them are now distinguished leaders in astronomy. One was Wallace Sargent, now on our staff, who had done a thesis in England on shock waves in nova explosions. Another was Leonard Searle, on the Santa Barbara Street staff. Such people had previously had little access to observational possibilities. If you can find good people like that, and if they want to come, you've got a real treasure trove. Beverly Oke, who's on the faculty here, was one of them. One Japanese postdoc discovered a rare isotope of helium in stars, helium 3.

Another, George Wallerstein, a Caltech PhD student, was a frequent collaborator. Many postdocs were our own students, kept on for a year or two. Robert Parker, now a science astronaut, worked with us. Kodaira, a leader in Japanese astronomy, was a postdoc. One visitor was a Belgian working on the composition of comets. Bob Kraft, now director of the Lick Observatory, was still another collaborator. And a woman astronomer, Ann Merchant Boesgaard, now professor in Hawaii. We broke the woman barrier.

RP: She's only the second woman you've mentioned as a colleague of yours in astronomy. Is this peculiar to Caltech? I know you worked with a lady at Harvard.

JG: Cecilia Payne, yes. She was the pioneer in understanding the composition of the stars in 1920. If you have a place like ours, which is so competitive, you cannot — and in those days you didn't have to — justify getting somebody only because she's a woman.

RP: Is this changing, do you think?

JG: Not much. We have only one woman on the astronomy faculty. We have no woman student. While my own collaborators were largely men, I collaborated in 1969 with Judith Cohen, now on our faculty; and in 1967, with Virginia Trimble, who is on the faculty of Maryland and UC Irvine. And my best paper for years, my 300th paper, was written in 1974 with Anneila Sargent, Wal's wife. She worked with me as a research assistant; and only later did she go for a PhD degree, working in the infrared. There's an inexcusable imbalance, but it exists.

RP: Do you see a brighter future for women in science at Caltech?

JG: Oh, I hope so. Still, I have a black name because I wrote that I was pessimistic about the opportunities for women in astronomy. When vice president of the American Astronomical Society, I was called on the carpet by women who tried to beat me up, intellectually, while I tried to explain why I had felt it was difficult. There are historical examples within astronomy where women made enormous contributions, possibly because of certain advantageous mental sets they had. The past doesn't mean they lack other abilities necessary for the future. In seeing things in the large, seeing synoptically, getting an idea that comes out of many clues in different areas, I think they've been wonderfully creative. In astronomy, that's what the older generations of women have been good at. Women make up about 10 percent among the few hundred astronomers, with good positions active in research; there are thousands of astronomers with jobs. They do good work in all fields, and several have been elected to the National Academy.

On the practical side, even the best of the women astronomers have had problems because of the requirements of family life. We're still playing those games; a woman often can't be a competitive scientist because she's got to put her husband's job first. I don't think there's a prayer of getting equality in numbers. But while I think we have, as much as possible, created organizational equality of opportunity, the trained people don't yet exist to use it. It will become better.

RP: You've received many, many honors. Are there any that really meant an extraordinary amount to you?

JG: I was elected to the National Academy of Sciences in 1957, which I feel was too late to make me happy. The scientifically significant honor was the Russell Lecture of the American Astronomical Society in 1970. Another honor I cherish is that they made me the DuBridge Professor in 1970. I think that from the point of view of what a scientist actually does, you must look not only at honors received but at what is better called responsibilities imposed. It's often called an honor to be made, say, an editor of a journal, or something like that. But when, five years ago, I compiled a list of lectureships, visits, and committee memberships, I was startled to find I had given, in the period 1960 to 1975, about 50 named lectures or lecture series around the country. Most of those were not really given to communicate — people didn't want the information. They wanted a big name trying to give them a short and painless encounter with current research. The question about honors is how does one reciprocate and fulfill the duty to propagate enthusiasm for science. The hope is that, by the communication to this audience, somehow something sinks in which in the long run is good for science.

As for TV, I participated in several major films involving astronomy, much of it PBS stuff. I dedicated four buildings, new laboratories or observatories, which seemed to be a popular thing. It's a part of the game of interaction with the public; it's something you have to do without knowing why.

RP: You got a gold medal from the Royal Astronomical Society.

JG: That is certainly, as far as prestige goes, one of the best. It has Newton's bust on it. And I got an even larger amount of gold from the Astronomical Society of the Pacific. I shared the California Scientist of the Year Award in 1965 with Maarten Schmidt. And I received various government citations for classified work. Scientists at a symposium of the International Astronomical Union dedicated the book of proceedings to me for my 70th birthday because I'd helped start the modern activity in the study of white dwarfs. But a more meaningful kind of honor is work remembered. In the *Monthly Notices of the Royal Astronomical Society* for 1982, two scientists from New Zealand start their paper with, "Although Canopus is visually the second brightest star in the sky and the brightest supergiant, it has been relatively neglected. A table of line identifications and equivalent widths has not been published since Greenstein's 1942 work nearly 40 years ago." When you see yourself as some kind of not-yet-dead figure from the past, that's fun. But most honors come too late, or for the wrong reasons. Some involve enormous amounts of work — notably the National Academy, for which I ran a major study of required government funding of astronomy in the 1970s. I am told that the cost to the taxpayer of the funded, recommended programs was nearly a billion (1970) dollars. That's good. Honors, track record, past services helped persuade the federal agencies to listen to our recommendations and support a marvelous science.

RP: You were involved with administrative committees within Caltech, and you were chairman of the Faculty Board. What did the Faculty Board do?

JG: Originally, the Faculty Board was a group of professors who met with Earnest Watson to express their opinions with regard to Institute policy and administrative questions. Not long before I became chairman in 1965, it became more active and more democratic. During the tenure of my immediate predecessor, the trustees felt it important to open a line of communication with the growing and changing faculty of the sixties. There should exist, in addition to the president and the provost, a broader faculty voice so that actions taken, including the rules, policies, and procedures, are exposed to and receive faculty acceptance. I did feel that, although we had a fine president, we should make sure that there would be no conflict between "The Administration" and "The Faculty."

I also felt there should be a way of getting our extraordinary faculty interested in the future of the Institute, in its general planning. Being a non-academic politician, which meant usually that I did what I wanted, I didn't know Robert's *Rules of Order.* At my first Faculty Board meeting I tried to appoint a committee to study what the Institute's goals were to be. I had come to the idea after speaking to people whom I felt to be the young, forward-looking men in various divisions. Eventually, that "Aims and Goals" committee produced many interesting think pieces, for example, about the role of the humanities at Caltech, or about women in the undergraduate/graduate school. It also helped produce almost all our recent administrative leaders. I appointed the committee to choose the next president and also the committee to choose the next provost. I worked with an excellent chairman of the Board of Trustees, and many new ideas arose in conversations with Arnold Beckman. All in all, when I was Faculty Board chairman, it 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.

NBC's program "The Immense Design" in the early 1960s used three distinguished Caltech scientists (above) — William Fowler, Greenstein, and Allan Sandage. Below, a 1969 meeting of the "Aims and Goals" committee brought out (from the left) Thayer Scudder, Robert Christy, David Smith, Thomas Lauritsen, C. J. Pings, Rochus Vogt, Herbert Keller, Rodman Paul, Norman Brooks, and Fred Anson.

RP: That was true across the country; in the sixties, you had to be more flexible and more receptive to change and to new ideas.

JG: That's right. Caltech people are

Greenstein provides a group of schoolchildren with an expert's guided tour of Palomar Observatory.

susceptible to scientific change, but rather conservative on organizational change.

One of the interesting things for me about giving advice within Caltech came from involvement with the humanities division. I'm interested in other things than science, specifically art, music, and literature. One obvious defect of Caltech student and faculty life is the narrowness of cultural distractions. Humanities contained people doing good jobs of teaching recalcitrant students — not necessarily to read or write, but at least beginnings, and for some, active interest. The humanities division also had within it economists, people like Alan Sweezy, for example, and Horace Gilbert, who served the useful function of teaching the elements of macro-economics. They had broad interests and powerful effects on students. (So does the Caltech Y, on whose board I served, which has long been the ethical, humanizing center for undergraduates.) The humanities division was an important humanizing influence. When I was active in local academic politics, I hoped to see that influence expanded.

I was still an elder statesman when Harold Brown came as President. He asked me to discuss the future of humanities and economics. So I went over the 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, and put expanded and scholarly work in history, literature, languages, psychology, and related subjects in an independent humanities division. 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." I had many other losing causes. I also tried to persuade people that it might be a good thing for undergraduate life for Caltech to swallow Immaculate Heart College. Only a few of the faculty were in favor of that. I thought among the humanizing things we could do was to hire someone distinguished in depth psychology. I was again absolutely overwhelmed. Our committee had several visits from Dr. Carl Rogers, a wonderful man who invented encounter therapy. But all of our big thinkers didn't agree on the importance of soul-oriented, rather than brain-oriented, parts of behavioral biology. As an amateur humanist, I never won a single one of these fights.

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

JG: Most brilliant people have problems coping either with success or failure, mostly with success, coping with other people, keeping creative and not becoming self-destructive. In a certain sense, World War II was a good thing for some of us in the older generation, providing an external stimulus for leadership rather than the internal, self-judging, destructive situation in which most people find themselves after they have had some success.

Rivalry is an essence of success. In science, you can look back a long time and have as a rival somebody in the far past. You can kill your spiritual father, but that father is already dead. You improve on classical physics with quantum physics, or you prove relativity better than Newton. But, now at a level of scientific achievement below that of the greatest discoveries, you deal with a contemporary rival, a nearby father. You face a pattern in the development of science in which the fact that you created a new subject guarantees that you will not be a leader for long. This creates personal stress. Other competent people take up the subject. If they have access to reasonably good equipment, they are likely to do better than you.

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

JG: It seems to me that I ran a 5 p.m. psychoanalytic hour. I'd sit in the office with the lights off and listen to somebody. I'm partly a frustrated father, I guess, with too many scientific children. When I use the phrase, that's arrogant, because those scientists were independently good, and not really my intellectual children. But many were people with whom I've had a somewhat parental relation. 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. There's no prescription for how such help works. Sometimes you make people happy by listening to their troubles. Sometimes you make them happy by denying they have troubles. I think part of how one helps bright young scientists is that — by not being that person or doing that work — you can help them find their own solutions or suggest new things that are relevant. That really has been the greatest pleasure — seeing people grow and change.

We work awfully hard to try and attract bright people. And after all the rigamarole, they still don't come, or they come and they go. The real thing I've learned is that if you really have good people, and have done your best to give them facilities, financial support, and personal encouragement, if in their life's history they go or don't go, it's really all the same. You've done your best, and their work is good for science in the large. You must feel the larger life of science, of scientists coming and going, changing, building their own thing, as part of the structure. You just have to realize that a scientific institution has life and death built into it, with the coming and going of people. You try to survive and enjoy it happening. □