Warren Weaver of the Rockefeller Foundation in 1932 and in retirement, around 1970. [From *Partners in Science*; courtesy of Helen Weaver]

foundations had to deal and which determined, more often than not, the success or failure of their efforts. Kohler's descriptions of the convoluted landscape of European science are masterful. Moreover, he tells a good story, especially when his attention shifts to Warren Weaver and the prehistory of molecular biology. Needless to say, there are omissions. Foundation interest in the social sciences has been the subject of extensive study by historians seeking to understand the connections between organized philanthropy and the dynamics of industrialized, capitalistic societies. Kohler admits that he excluded the social sciences from his study largely for practical reasons. That decision lends the book a noncontroversial tone (Carnegie involvement with eugenics is barely mentioned) that suggests, maybe wrongly, that whatever was omitted would have made no difference in any case. Might it be that foundation interest in social order and strategies of social control subtly influenced policies and programs in both the natural and the social sciences? If that is the case, then this important book might be shy a chapter.

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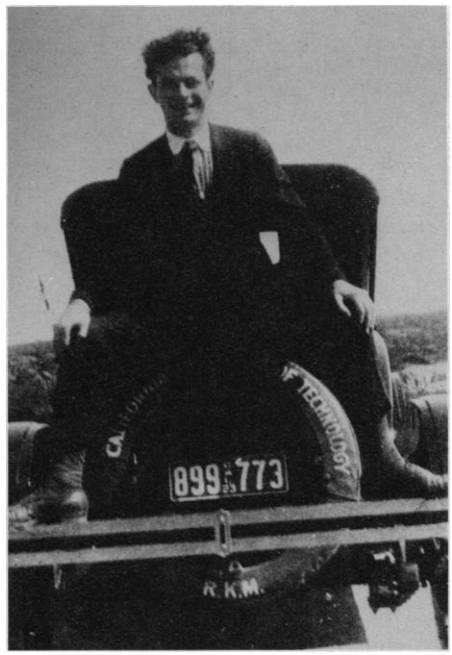
A Center for Science

Millikan's School. A History of the California Institute of Technology. JUDITH R. GOODSTEIN. Norton, New York, 1991. 317 pp. + plates. $25.

Before reading further, put the question to your nearest colleague: what famous American institution of higher education was once known as Throop University? Chances are you will be met by a shrug of indifference or a whoop "*Throop?*"—unless your victim happens to be a graduate of the California Institute of Technology.

If the scores to this spot quiz are as dismal as I expect, they reveal the first (and last) obstacle Judith R. Goodstein must have faced while contemplating her *Millikan's School: A History of the California Institute of Technology*. That is, how can a non-initiate be persuaded to read past the first chapter, or, for that matter, the cover? Two general solutions suggest themselves. Approach 1 is to make Caltech such an exciting endeavor that even a reader who has never heard of the place will be captivated by its saga. Approach 2 is to assume the aura surrounding its very name is so pervasive that any browser who wanders into the book's vicinity will immediately be hypnotized into reading it.

Goodstein, Caltech's former archivist and current registrar, appears to lean toward the second, Calocentric approach. This is the harder route. On the one hand, *Millikan's School*, published on Caltech's 100th anniversary, becomes something of a deserved birthday salute. On the other hand, that evident purpose also invites criticism that the book is little more than a "puff piece"—to borrow a not-so-complimentary term from the magazine industry. Although the sobriety of Goodstein's writing forestalls such criticism, she is treading a fine line. *Millikan's School* is prefaced by no fewer than four "presidential perspectives," penned by past and present Caltech leaders. Ostensibly these essays are to convince the uninitiated of Caltech's importance (a nod to approach 1), but declarations like "There is no place like Caltech" (retreat to approach 2) are unlikely to win converts. Indeed, we learn early on that Caltech is "perhaps the country's leading center for science and technology." "Perhaps" was undoubtedly inserted to mollify MIT graduates; nonetheless one already hears war chants rising from the banks of the Charles.



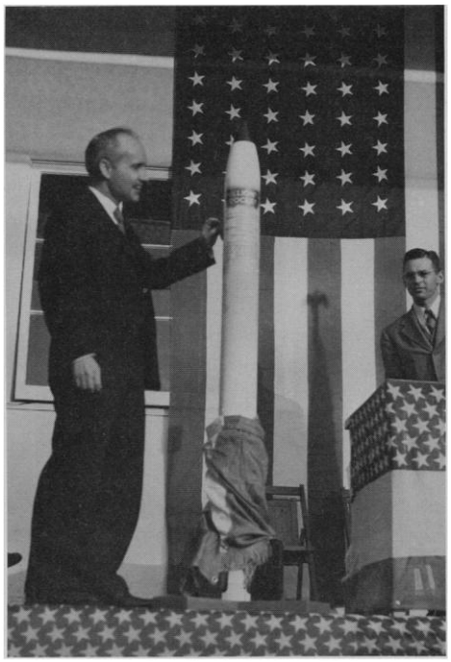
"Linus Carl Pauling entered Caltech as a graduate student in 1922, completing his Ph.D. in chemistry in 1925. Then he sailed for Europe. . . . By the time Pauling returned to Pasadena in 1927, his quest to formulate a quantum theory of the chemical bond had begun." [From *Millikan's School*; California Institute of Technology photo]

The presumption that Caltech is located somewhere near the center of the universe will undoubtedly limit the audience which might have found *Millikan's School* of interest, but it seems to me that Goodstein has problematically restricted herself yet further. Her story is first and foremost the tale of three men: the astronomer George Ellery Hale, the chemist Alfred Noyes, and the physicist Robert Millikan. It is the story of their attempts to transform an undistinguished little school founded in 1891 by one Amos Throop—an intrepid frontiersman, businessman, and philanthropist—into a world-class scientific establishment. During the course of this endeavor we meet some of the men who made Caltech famous: geneticist Thomas Hunt Morgan, aeronautical engineer Theodore von Kármán, and chemist Linus Pauling, among others. Each of these scientists deserves a biography—and many have had them—but because Goodstein's focus is on their contributions to an institution, they do not receive fully developed portraits here.

As an archivist, Goodstein stays close to the sources, holding personal commentary and speculation to a minimum. One gathers that more financial memoranda than scientific records survive, because she devotes as much space to the efforts of Hale, Noyes, and Millikan in fund-raising as she does to science. Again this is a problematic choice: Though the effort required to secure dollars may be an edifying lesson to scientists these days, it does not form the basis of an exciting narrative.

Yet a third difficult decision was to end the narrative with the resignation of Robert Millikan from Caltech's helm at the close of World War II. (A chapter on the Dubridge era is appended.) In one sense this is useful since it preserves traces of a generation now passing over the horizon. But it does mean that those searching for Feynman or Gell-Mann anecdotes will search in vain. Undergraduates themselves do not rate more than a passing mention. Ditch-day antics and other emblems of Caltech cleverness are not part of the story, all of which might lead a potential matriculate to conclude that, in the words of an administrator from my own alma mater, students are indeed no more than "transient parasites."

At the end, therefore, we are left with a



"Ernest C. Watson, administrative head of Caltech's World War II rocket project, with the Institute's one-millionth rocket." [From *Millikan's School*; California Institute of Technology archives]

book that will appeal mostly to Caltech alumni (or perhaps faculty). Even then, *Millikan's School* is concerned with the intersection of the careers of many remarkable men and not with the men themselves. Though it may be true that the whole is greater than the sum of its parts, in this case, certainly, the parts would have been more interesting.

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Condensed Matter Theories

Field Theories of Condensed Matter Systems. EDUARDO FRADKIN. Addison-Wesley, Reading, MA, 1991. xvi, 350 pp., illus. $48.50. Frontiers in Physics.

Field Theories of Condensed Matter Systems—the title alone is enough to awaken suspicion in the community of theoretical physicists of which I am a member. "Field theory," as the phrase is usually used these days, means the techniques of relativistic particle physics theory. This is a realm of science whose inhabitants prefer to breathe only the rarefied air of high mathematical abstraction. In the past they rarely left their world of functional integrals, topological monopoles, and broken chiralities. But a peculiar and rather massive scientific migration has taken place in the last four or five years, during which there has been something of a lull in the appearance of interesting new particle physics data. This, abetted by the discovery of high-temperature superconductivity, has stimulated a number of field theorists to abandon, at least temporarily, their particles and to begin research into condensed matter. The suspicious ones like myself, as the reader may imagine, are the old-fashioned condensed matter physicists, now beset by intruders, who like their theories to have simple mathematics and to be well grounded in experiments. Those woodcuts, so often reproduced these days, of the wary natives greeting Columbus as he steps down on the shore of the New World in his rather ridiculous Renaissance finery just about sum it up.

When I say, therefore, that this is a very good book, it is no empty compliment—it comes from a noble savage. Fradkin has taken the most abstruse parts of the subject and given them the treatment that they have always needed and never received. Take just two examples, the Bethe Ansatz method and topological excitations. It is possible to find clear expositions of both of these topics in the existing literature—possible, but only if you are willing to search long and hard in the library and then sit down for a full week's study of each. By that time you would know far more than was desired. Furthermore, you wouldn't know the proper place of the subject from the point of view of condensed matter physics as a whole. What never existed before but now does is a clear exposition of these topics that is sufficiently detailed to enable a researcher to reproduce the important derivations but not so specialized that a whole new language needs to be mastered. The last point is the most important. Because overspecialization is avoided, these subjects are finally put in context, where their significance can be judged for really the first time.

And are they significant? The complaint about the field theorists has often been that they have produced little that is really new in condensed matter theory but instead have reformulated old theories in new and not necessarily clearer words and mathematics. This book answers that criticism, though in an indirect way. It is true that anyone looking in it for fits of theory and experiment will be disappointed, and nearly all the results have been obtained before by more pedestrian, long-winded, but perhaps simpler methods. The point lies elsewhere,

Vignettes: E. U. Condon and the Bureau of Standards

Edward Uhler Condon was director of the National Bureau of Standards from 1945 until 1951, when repeated subjection to congressional loyalty hearings led him to leave government. In a volume of his Selected Popular Writings *published this year by Springer-Verlag under the editorship of A. O. Barut and others appear the following recollections of his tenure there.*

I was sworn in one afternoon at 4 p.m....and from that moment *was* the director. The next morning I took a cab...and told the driver to take me to the National Bureau of Standards. As we rode out Connecticut Avenue, he remarked: "Bureau of Standards, eh! They should develop some moral standards and some ethical standards." I agreed that they should, but his remark had only heightened the feelings of inadequacy with which I approached my new job.

Some of my most treasured memories of Government service are connected with...appropriations hearings....

On [one] occasion a Congressman was questioning the chief of the Bureau's radio division, who had been talking about the scarcity of space in the radio frequency spectrum for the many needs of communication services. He said: "Doctor, I understand that among you scientists there are two theories: some say space is finite, others say it is infinite. I want to know, where do you stand?" The witness started to explain the limitations of using very low and very high frequencies but the Congressman interrupted him to say, "No, I mean space, you know *space*," making a large and globular gesture toward the part of the three-dimensional continuum in front of him.

The witness squirmed and looked to me for guidance, quite willing to make it finite or infinite for the sake of the budget, but I could only indicate with a gesture that I did not know which was the preference of that particular Congressman. So he gulped hard and said, "I think it's infinite." "Thank you very much Doctor, that's all I wanted to know," replied the Congressman and passed on to another topic.