tion imposed by military regulations upon the project. George Kistiakowsky recalls the conflict between little science and big technology in developing the implosion technique, a conflict he was called in to resolve against his better judgment that Los Alamos would make no contribution to victory. The initial intimacy of Los Alamos was eroded by unforeseen growth, which some attribute to the residue of experts who had completed their assigned tasks but could not leave because of security regulations. Still others praise the flexibility of these same experts, which contributed to the solution of new problems that arose in the course of the project and were the real reasons for growth in their view. For many, the contradictions between their ideals of research and the realities of Los Alamos were personified by General Leslie Groves, who even his supporters admit had "an almost *uncommon* facility for saying the wrong thing to a scientist." Yet these same problems have arisen repeatedly in the evolution of Big Science, and Norris Bradbury's account of post-war Los Alamos indicates how he managed them, although not without cost.

Los Alamos was a watershed not only in the evolution of the relationship between science and government and in the development of modern Big Science but also in the individual lives of many of its inhabitants, who remember this period as an unparalleled adventure. No doubt the confluences of the primitive and beautiful environment with the makeshift town, of ancient Indian pueblo life with modern scientific research, and of wartime necessity with the opportunity to mingle with the greatest scientific minds lent excitement, intensity, and romance to the experience. The events at Trinity, Hiroshima, and Nagasaki, however, gave it meaning. Laura Fermi recalls that her younger son felt that his father, Nobel laureate Enrico Fermi, was less important than a playmate's father, who held the exalted rank of captain in the army, until after Hiroshima. For him, as for many veterans of Los Alamos, the experience there is forever illuminated in the light of Trinity. Most of the lecturers represented here defend that experience in the light of the conflict resulting from the use of the bomb.

The editors have attempted to place these lectures in a different perspective by focusing on the prehistory of Los Alamos in their introduction. They have deliberately avoided reconciling conflicts between differing accounts to preserve the spontaneity and authenticity of this historical "source material," but they have gone too far, in my opinion, in preserving some of the stylistic inadequacies of a number of the lecturers, so that one is actually repelled by the prose in some essays. Although a number of the lecturers have told their stories before, there are enough new faces in the book to afford novel insight into the Los Alamos experience and some corrective to the heroic legend.

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Events of Physics

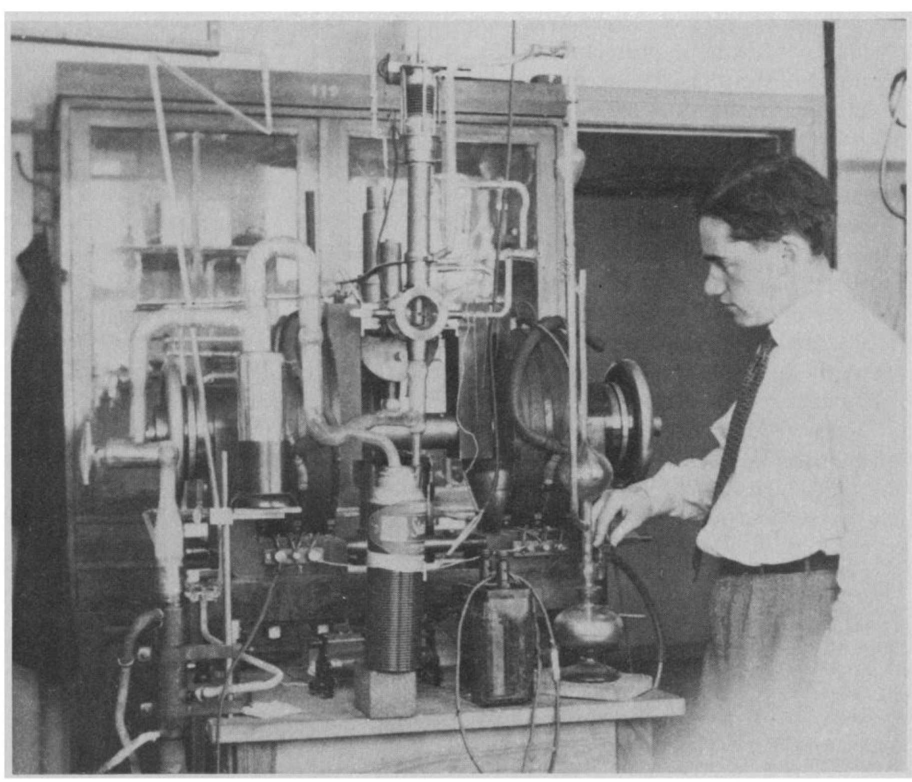
From X-Rays to Quarks. Modern Physicists and Their Discoveries. EMILIO SEGRÈ. Translated from the Italian edition (Milan, 1976). Freeman, San Francisco, 1980. xii, 338 pp., illus. Cloth, $20; paper, $9.95.

Emilio Segrè, codiscoverer of the element technetium and of the antiproton, for which he shared the 1959 Nobel Prize in Physics with Owen Chamberlain, has written a modest and engaging personal account of 20th-century physics, emphasizing the parts he considers most fundamental. By no accident, these include the fields of his major contributions: nuclear and elementary particle physics. His opinion about future trends is that "the possibility of intellectual discovery points to particle physics," as opposed to the study of multibody systems, such as liquids or nuclear matter. Other scientists might well disagree, as he is quick to stress.

Based on lectures given at Berkeley, Chicago, and Rome and "addressed to people who are curious about the physicist's world," the book is a chronologically ordered collection of essays describing the important discoveries and relating anecdotes about the great personalities. A number of these have been told before, and if they are not entirely accurate Segrè is a charming raconteur who tells his stories simply and well.

In the preface is a disclaimer: "The book does not pretend in any way to be a history of modern physics and even less to be a small physics text. It is rather, an impressionistic view of the events as they appeared to me during my scientific career, which started about 1927." (However, the first half of the book is background). Accepting this at face value, I found a lot to enjoy in Segrè's essays.

When he deals with events that occurred on his home ground, whether Rome or Berkeley, where he was a participant or witness, or when speaking of colleagues and personal friends, Segrè



"Otto Stern's laboratory, Hamburg, 1931. O. R. Frisch is shown with the apparatus for measuring the magnetic moment of the proton." [Photograph of Emilio Segrè, from *From X-Rays to Quarks*] speaks with authority. That is so in his account of the prophetic speech of 1929, given by Orso Mario Corbino, director of the Physics Institute of the University of Rome and Enrico Fermi's strong supporter, in which he emphasized the future importance, scientific and technical, of nuclear physics. When the subject is more remote or when failure, not success, is involved, the treatment is much less satisfactory. After a short discussion of the disintegration of light nuclei by alpha particles beginning in 1917, Segrè says, "Rutherford's experiments were repeated in Vienna, and Austrian scientists found more disintegrations than Rutherford did. A lively debate arose, but in the end it was found that Rutherford was right." To one who already knows of the controversy between the nuclear physics groups at Cambridge and Vienna who were using scintillation counting in 1926–27, this offhand remark is frustrating, because an examination of the conflict could reveal much of the nature of experimental physics. (Why did so "simple" a technique succeed at the Cavendish Laboratory but fail in Vienna?)

In painting with broad brushstrokes, it is clear that details must be suppressed. However, why does Segrè, the editor in chief of Fermi's *Collected Papers* and Fermi's collaborator and biographer, say (on p. 144) that Paul Dirac "put Fermi's statistics on a quantum mechanical basis," when the title of Fermi's paper is "On the *quantization* of the ideal monatomic gas" (my emphasis)? And I must include one other caveat against an oversimplification. Segrè states (on p. 245) that Hideki Yukawa's reasoning in proposing the meson theory of nuclear forces involved "little more than an application of the uncertainty principle and of relativity." The type of argument presented as Yukawa's is not to be found in Yukawa's papers, but was given first by Gian Carlo Wick in a letter to *Nature* in 1938, four years after Yukawa's theory was proposed.

There is a useful ten-page bibliography that emphasizes biography, and there are ten short appendixes, containing mathematical derivations. Seven of these deal with the thermodynamics of blackbody radiation and quantum statistics; the last appendix (two pages) is called "Quantum mechanics in a nutshell." The appendixes are too brief for anyone who is not already familiar with the subject and too elementary for anyone who is. The illustrations in the text, on the other hand, are very fine, being either reproductions from the original literature or photographs, some of them from Segrè's private collection. Together with his personal observations and attractive style, they help to make this book an appealing one, especially for physicists and students of physics.

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Sources of Inspiration

The Tragicomical History of Thermodynamics, 1822–1854. C. TRUESDELL. Springer-Verlag, New York, 1980. xii, 372 pp. $48. Studies in the History of Mathematics and Physical Sciences, 4.

Clifford Truesdell, along with his distinguished career in the sciences of continuum mechanics and rational thermodynamics, has demonstrated an abiding penchant for the history of those sciences as well. In the 1960's he made significant contributions to the history of rational mechanics, with a special fervor for the work of Leonhard Euler. In the 1970's he turned his linguistic and logical skills to fathoming the major writings in 19th-century thermodynamics, with special favor for the almost unknown work of Ferdinand Reech.

A very short preliminary version of Truesdell's views appeared without his authorization in 1971 under the title *The Tragicomedy of Classical Thermodynamics*. The intervening years have not changed his judgment about thermodynamics as the peculiarly tragicomic science, the science that he says has no peers when it comes to the high "ratio of talk and excuse to reason and result" (p. 3). But he invites thermodynamicists to read his book and to share with him the discovery he unveils there: "*Thermodynamics need* never *have been the Dismal Swamp of Obscurity* that from the first it was" (p. 6).

To reveal his discovery Truesdell, at various stages in his chronological account of the classics in thermodynamics, steps back from the immediate history to cast his net of mathematical logic. He identifies a set of equations that he considers to be the correct mathematical expression of the thermodynamic ideas under discussion. He then proceeds to demonstrate various thermodynamic relations that might have been deduced thereby and asks why the 19th-century thermodynamicists let them slip by.

Truesdell keeps these logical commentaries separate from his historical accounts by placing them in sections labeled "critiques" or, when he cannot resist commenting during an account, by enclosing such paragraphs in brackets. He clearly does not expect that historians of science will approve of this practice and advises any such who might chance upon his work that "he would do well to omit all sections labelled 'critique' and all words confined between square brackets, for in that way he will save himself such pain as my 'ahistorical' approach might otherwise inflict" (p. 5). Actually Truesdell inflicts far more pain by explicitly ignoring almost all that historians of science have written. The real question, of course, is whether he promotes our historical understanding with his claims that thermodynamicists were blind to these supposed relations that they should have seen.

He does and he doesn't. He does give historians some good questions to mull over, but he doesn't give any fully satisfactory answers. He offers two main reasons to explain why thermodynamicists failed to explore fully the logical import of their theories: they did not express their ideas in rigorous mathematics, and they allowed physico-philosophical ideas to intrude into mathematical reasoning. Truesdell often makes additional appeal to the activities of a mythical figure that he variously calls the tragicomic muse or daemon or fury of thermodynamics.

Thus, although Truesdell admires Carnot's remarkable intuitive powers, he regrets Carnot's non-mathematical mode of analysis, chiding him for not seeing a certain simple argument and calling it a failure "typical of the theorist who tries to get along without mathematics" (p. 106). But why did Carnot ignore mathematics except in his footnotes? Truesdell appeals to the muse: "Carnot does not follow the tradition of eighteenth century rational mechanics. . . . Instead, the sardonic muse directs him to write in a medium that anybody can understand" (p. 80).

Truesdell might better have appealed to the fact that Carnot wrote in relation to the thinking of his day about steam engine technology, thinking that viewed pressure and not temperature as the key to power and toyed with ideas that other substances more volatile than water might provide more power. Carnot's axiom, therefore, that all substances would yield the same power between any given temperatures, would have had immediate relevance for practical engineers. Perhaps Truesdell would view that motive with disfavor, because he says, in one of his choice bracketed sentences, after quoting Kelvin's observation in