des Sciences (1966), with which it demands comparison, it is a work well done, simple and sober. Its failings are those of virtually all such publications to date; they are meant for the coffee table rather than the study.

HARRY WOOLF
Department of the History of Science, Johns Hopkins University, Baltimore, Maryland

A Mathematical System

A History of Vector Analysis. The Evolution of the Idea of a Vectorial System. MICHAEL J. CROWE. University of Notre Dame Press, Notre Dame, Ind., 1967. xviii + 270 pp., illus. $12.95.

The evolution of the idea of a vectorial system is one of the most interesting and spirited segments of the history of mathematics. Few areas of mathematics have given rise to such ardent partisanships. The dialogues between advocates of one type of vector analysis over another often reached heated and vituperative levels. Even today the matter of vector notation is a quarrelsome subject among vector analysts. Since the story has not previously been fully or accurately told, students of the history of mathematics owe Michael Crowe a debt for his scholarly and painstaking narration. In following the tale the reader will encounter a long roster of great and not-so-great mathematicians and physicists, among whom are Leibniz, Wessel, Gauss, Argand, Buée, Mourey, Warren, Hamilton, Möbius, Bellavitis, Grassmann, Saint-Venant, O'Brien, Tait, Benjamin Peirce, Maxwell, Clifford, Schlegel, Cayley, Gibbs, Heaviside, Wilson, Burali-Forti, and others. In Crowe's book one finds much biographical material about these men, and the treatment of such principals as Hamilton, Grassmann, Tait, Gibbs, and Heaviside is really superb. The book is developed in strict chronological order, up to the year 1910, and each of the eight chapters concludes with a valuable collection of notes.

It was in 1830 that Hamilton began his search for a three-dimensional vectorial system, and in 1832 that Grassmann got his first ideas for his calculus of extension; in 1843 Hamilton discovered his quaternions, and in 1844 Grassmann published his *Ausdehnungslehre*. Crowe's story is largely about the fate and influence of these two great achievements. Because of similarities in the Hamilton and Grassmann systems, either one could have led to modern vector analysis through a process of simplification, but the "capital" of Hamilton's personal fame as opposed to the anonymity of Grassmann caused the quaternions to play the more influential role in the subsequent development of the Gibbs-Heaviside system. The quaternions, which were originally heralded as among the two or three truly great achievements in mathematics, are now largely regarded as a museum piece. But two worthy credits to quaternions still remain—they led ultimately to the highly versatile vector analysis of today, and they (along with Grassmann's calculus of extension) first opened the floodgates of modern abstract algebra. For the discoveries of Hamilton and Grassmann played a role in the history of algebra very much like that played by the discoveries of Lobachevski and Bolyai in geometry. Just as the latter led to the new non-Euclidean geometries, the former led to the new nontraditional algebras, and both, in turn, further led to the development of formal axiomatics.

Crowe's book purposefully concentrates on the more fundamental aspects of vector analysis, with the result that certain parts of the history of the subject receive little or no attention. Thus, though much is said of vector algebra, little is said of vector calculus; the del operator is scantily considered; and the history of notational squabbling is omitted. Closing the story at the year 1910 has led to the omission of the history of such allied subsequent developments as tensors, vector spaces, and linear algebra. But within his prescribed framework, Crowe tells his story completely, with scholarship, and magnificently—sometimes in almost majestically structured sentences.

HOWARD EVES
Department of Mathematics and Astronomy, University of Maine, Orono

Memorial to a 20th-Century Figure

Oppenheimer. I. I. RABI, ROBERT SERBER, VICTOR F. WEISSKOPF, ABRAHAM PAIS, and GLENN T. SEABORG. Scribner, New York, 1969. x + 92 pp. + plates. $5.95.

In 1967 the American Physical Society devoted a special session of its spring meeting to a memorial for J. Robert Oppenheimer. At that session, four of Oppenheimer's colleagues reviewed his several careers and his contributions to science and society. Their speeches reflected their close personal connection with Oppenheimer, and so conveyed impressions of the man, as well as of his achievements. These talks have now been collected into a book, together with a brief introduction by I. I. Rabi. There is in addition a very good glossary of the scientific terms used by some of the speakers, which could serve as a model for books of this type.

Oppenheimer was a scientist, a teacher, the director of the atomic bomb project, an influential government adviser, and an expositor of science to nonscientists. The speeches printed here touch on all of these activities—most successfully, I think, on his work as scientist and teacher. Oppenheimer's greatest contribution to science in America was not in any of his papers, important as some of them were. It was rather the example of his dedication and the keenness of his critical insight, which, by inspiring his students and colleagues, raised theoretical physics in America to its present position of leadership. These aspects of Oppenheimer are movingly recalled in the speeches of Robert Serber, who deals with the prewar period, and Abraham Pais, who covers the postwar period in which Oppenheimer was director of the Institute for Advanced Study. It took Oppenheimer's special abilities to remain abreast of the many seemingly disparate developments in fundamental physics in the latter period and point the way to finding unexpected relationships among them. The speed and precision with which he was able to do this were apparent to anyone who ever attended a seminar at the Institute.

Oppenheimer's directorship at Los Alamos is recounted by Victor Weisskopf, who stresses how remarkable an institution that laboratory was. This may be seen not only from its inanimate products, but also in its effect on the lives of those who worked there. Again, it was Oppenheimer's genius for grasping all aspects of a complex problem and his ability to inspire the work of others that gave Los Alamos its special character.

Oppenheimer's advisory work for the government is described by Glenn Seaborg, who also mentions some of his efforts to promote a common understanding among men who live by thought. That both of these efforts were not entirely successful is perhaps more of a criticism of us than of Oppenheimer.

The contributors to this book, probably by choice, hardly touch upon the unspeakable hearings of 1954, at which Oppenheimer's services to his country were rewarded by his condemnation as disloyal, a procedure which reminds one of the Athenians' ostracism of Miltiades after his victory at Marathon. It might have been appropriate to include in this book the stirring speech about this injustice delivered by George Kennan at Oppenheimer's funeral service in Princeton. But perhaps it is better to simply describe Oppenheimer's achievements, and let each reader recognize the worth of the man we were privileged to have among us.

GERALD FEINBERG
Department of Physics,
Columbia University, New York City

Quanta and Ontology

Quantum Physics and the Philosophical Tradition. AAGE PETERSEN. Published in cooperation with the Belfer Graduate School of Science, Yeshiva University, by M.I.T. Press, Cambridge, Mass., 1968. x + 202 pp. $7.50.

Niels Bohr was convinced that the development of quantum mechanics contained a lesson, "an epistemological lesson with bearings on problems far beyond the domain of physical science." Although he wrote many essays on this theme in his later years, Bohr never attempted a full-scale discussion of the philosophical implications of quantum physics. Philosophers have taken up some of the issues raised by the statistical nature of quantum mechanics, but the subject as a whole has never attracted their attention in the way relativity did.

In this book Aage Petersen, who served as Bohr's assistant for many years, has undertaken an analysis of the relationship between quantum physics and traditional philosophy. Petersen considers *the* philosophical tradition to be an inquiry into the structure of being or the nature of reality, culminating in the work of Immanuel Kant. Classical physics is consistent with this ontological mode of thought, as he calls it, but quantum physics is not; it represents something new. In order to analyze how and where quantum physics departs from the tradition, Petersen has chosen to emphasize the concept of correspondence, and not the concept of complementarity, which Bohr himself stressed in his later writings. By giving the idea of correspondence such a prominent place, Petersen calls attention to the ways in which quantum mechanics is a rational generalization of the older physics. For, as Bohr put it: "The correspondence principle expresses the tendency to utilize in the systematic development of the quantum theory every feature of the classical theories in a rational transcription appropriate to the fundamental contrast between the [quantum] postulates and the classical theories." Petersen follows Bohr in stressing the indivisible nature of a quantum phenomenon, which requires the specification of the whole experimental arrangement for its definition. He, too, sees the goal of physical theory as unambiguous communication rather than intuitive understanding.

I am not sure that Petersen's concern over the relationship between quantum physics and the ontological mode of thought will be widely shared. Twentieth-century philosophers hardly seem to have been bound by one set of categories, anyway. We even know that Bohr read William James and profited from his reading, which suggests that Petersen may not have used the most relevant philosophical starting point.

Petersen draws an interesting parallel between the philosophical impact of quantum mechanics and that of the calculus, with its ensuing debate over the nature of continuity and limits. This debate was settled by a rigorous theory of limits within mathematics, and not on philosophical grounds, and Petersen suggests we may well see a similar outcome to the unresolved questions about the interpretation of the quantum theory, where we are still in the "pre-Cauchy" stage of the discussion. This is typical of Petersen's attitude. He does not pretend to have settled the difficult questions over which Bohr and Einstein struggled for a quarter of a century. On the contrary, it is his view that "our present understanding of the topic is much more primitive than is usually believed," an opinion in which Bohr and Einstein might well have concurred.

MARTIN J. KLEIN
Department of the History of Science and Medicine, Yale University,
New Haven, Connecticut

An Achievement of Magnitude

The Stanford Two-Mile Accelerator. R. B. NEAL, Ed. Benjamin, New York, 1968. xiv + 1170 pp., illus. $35.

When I see one of the world's great suspension bridges, or when I see, on television, the Apollo spacecraft going into orbit, I feel proud to belong to the same human race as the men who conceived these projects and brought them to fruition. I get the same feeling when I look down upon SLAC, the two-mile-long accelerator at Stanford, from Skyline Boulevard, where it appears as a long line upon the landscape, and I get the same feeling as I read this book. There is a difference between SLAC and a suspension bridge. The beauty and usefulness of a bridge are apparent to most of us without special training, whereas one needs considerable technical knowledge to appreciate the beauty and usefulness of SLAC. This knowledge can be increased by reading this book.

SLAC is at present not only America's longest particle accelerator but also its most expensive; it cost $114 million exclusive of considerable preconstruction, research-and-development, and preoperation funds. It is therefore of interest to all taxpayers to see how this money was spent. It takes a book of 1170 pages to tell the story. The development of particle accelerators can be measured in several ways: A description of the first betatron was, I believe, published in two papers in *Physical Review*, one theoretical and one experimental. Most of an issue of the *Review of Scientific Instruments* was devoted to the Cosmotron. Hansen's first traveling-wave linear accelerator, the Mark I, a forerunner of SLAC, was described in 1948 in a single paper with three authors. This book is the work of 90 authors. This comparison is a familiar one at SLAC, and 10 pages are devoted to the history of accelerator projects at Stanford. I found particularly appealing a photograph, which is by now famous, of the Stanford Mark I linear accelerator being held up by four physicists headed by Bill Hansen, who remains a legend at Stanford.

With such an increase in the size of a project comes increasing complexity, and most of the problems of SLAC are problems of this complexity. For example, special techniques, using laser light, were developed to align the accelerator to unprecedented tolerances. These techniques, described in 23