Book Reviews

Physics in the United States

The Physicists. The History of a Scientific Community in Modern America. DANIEL J. KEVLES. Knopf, New York, 1978. xiv, 496 pp. $15.95.

When early in 1907 Ernest Rutherford wrote to Arthur Schuster concerning the physics chair at Manchester he confessed his eagerness to return to Britain: "We are too near the scientific periphery here, for America as yet does not count very seriously." It is true that taken in the worldwide context American science was hardly significant. There was, however, a general awareness that science in the United States (and Canada) was building for the future. The building process had begun relatively recently (during the last quarter of the 19th century) and qualitatively was proceeding relatively slowly. Between 1873 and 1890 only 22 Americans received doctorates in physics from U.S. institutions, and few of those could boast of their training.

Indeed, training was the focus of early concern. American physics was eventually built upon institutions for the training of physicists. Starting with the Johns Hopkins University in 1876, laboratories for the preparation of physicists were established or reformed at such major centers as Chicago, Cornell, Harvard, Yale, Columbia, Pennsylvania, Clark, Michigan, Wisconsin, and Princeton. Unlike the best of their European counterparts, which were generally organized around research problems and the production of research, the American institutions stressed the production of researchers. The Cavendish Laboratory at Cambridge, for example, was notorious for its lack of concern with institutional arrangements, degrees, matriculation, and the like; the American universities, on the other hand, were in the business of turning out Ph.D.'s. This difference in mental set was laid out as early as 1876 by Simon Newcomb, the well-known American astronomer and mathematician in his dour report "Abstract science in America" (*North American Review* **122**, 117). With respect to organization and quantity of research, Newcomb reported, "We see our science in the aspect best fitted to make us contemplate the past with humility and the future with despair." Newcomb nevertheless held out hope for the future, likening the American scientific community to an army currently "ineffective from the want of leadership and discipline." In the period to follow, from 1876 to 1914, this army was to expand greatly and to become well disciplined. At such universities as Johns Hopkins, Cornell, Chicago, Harvard, and Yale, the largest producers of physics Ph.D.'s, the student was thoroughly trained in basic principles and especially in exact investigation and measurement using refined physical instrumentation, such as Rowland's gratings at Hopkins or Michelson's interferometer at Chicago. If the Cavendish was producing brilliant officers, at least America was developing a splendid infantry. Eventually the United States was to fashion this infantry into one of the world's most vital and productive physics communities.

What is remarkable is that so little is known about this community. We require a better understanding of the training and socialization of the physicists and of the institutions that were their homes during the last century if we are better to understand their choice of research problems and the character of their scientific achievements. We ought to know more of the evolution of their research organization, both academic and—new to the 20th century—industrial; we would like to understand more fully the causes and effects of the transition from little to big science. We need scholars to provide us with an "anthropology" of the physics community, helping us to understand its rituals, myths, and kinship relations, as well as with more conventional examinations of its internal politics and social relations. Finally we must explore the community's "interface" with the larger society—its external politics. It is the last of these aspects that Daniel Kevles has chosen to explore in the greatest depth in his new book.

Kevles's story really begins in 1883 when Henry Rowland, a physicist at Johns Hopkins and one of America's leading producers of physicists, appeared before the American Association for the Advancement of Science with a "Plea for Pure Science." According to Kevles, Rowland's address provided not only a critique and a plea but also a program, characterized by the term "best-science elitism," which advocated the concentration of educational wealth in a few first-class universities and the establishment of a "best-science" network of researchers and students in order to advance the discipline. What was envisaged was a pyramidic structure topped by an elite and committed to knowledge for its own sake.

To this program, Kevles adds the "political elitism" widespread among American scientists and articulated by John Wesley Powell in 1885. This position, which insisted that science should be free from political interference and independent of what the scientists termed "political" institutions clearly "revealed the scientific community's self-conception as an aristocracy of intellect and ideals." This twin program of fostering the best-science elite and demanding a sheltered position of limited accountability was worked out over the next 90 years.

Changing political and social conditions worked to the physicists' advantage. "The more the physicists marched toward the fulfillment of Rowland's best-science program, the more they had to find their support among those with a natural stake in the best science." They found allies in unlikely places: among Progressives, whose interest in reform pushed for an enlargement of the federal role in science, and among industrialists, for whom the scientists trotted out shiny arguments concerning the ultimate technological return of pure science.

World War I accelerated processes begun long before. George Ellery Hale, the nonpareil scientific entrepreneur, seized upon the occasion to fashion new instruments for best-science elitism, among them the National Research Council and the California Institute of Technology. "Hale had done all he could," Kevles writes, "to commit American science through the private, elitist NRC to a virtual cold war abroad and an alliance with the major industries and philanthropic foundations at home." The conservative alliance forged by Hale continued throughout the 1920's, when science became the object of public attention and adulation. That decade witnessed the Einstein boom and marked the time when scientists were consulted as sages on questions as widely disparate as cosmic rays and the national economy. It was also the period in which Rowland's program in the form of "making

the peaks higher'' made substantial progress. The General Education Board, the International Education Board, the Carnegie Corporation, the Rockefeller Foundation, and other such institutions were mobilized to that end. By 1930, Kevles reports, ''The profession had become highly pyramidic in institutional structure.''

The 1930's marked the infusion into American science of Hitler's émigrés, vastly enriching it, and saw as well the beginnings of big science, with its big machines, large research teams, and voracious appetite for money. And here is the crunch. To economic development and progressive reform the physicists added as an ally national security. The days of laissez-faire were over. Vannevar Bush and others attempted to enlist the powers of the Executive Branch on the side of the best-science program and joined battle with congressional adversaries such as Senator Harley Kilgore who resisted the political elitism of Bush and others of the scientific establishment. The Cold War sealed the Faustian bargain between physics and government. This bargain constituted what Kevles calls

> a revolution in the relationship of American physicists to their society and government. Now through the Office of Naval Research, the Atomic Energy Commission, and the National Science Foundation, they were supplied with what they had been seeking for the better part of a century—a system of federal support . . . insulated from political control.

Of course, in Faustian bargains the payment comes due. The 1950's witnessed the Oppenheimer case and McCarthyite incursions; the 1960's saw the rise of pork-barrel science and a severe antiscience reaction. The physicists, it should be noted, provided some of the most outspoken criticism of the science-defense-corporation troika, but nevertheless their community profited from the arrangement. The book ends with uneasy tensions still unresolved:

> How was physics to enjoy sustained support in identification with the needs of economy and defense, yet avoid becoming their creatures. . . ?
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> How was the scientific community's demand for political elitism to be reconciled with the principle of politically responsive public policy?

Kevles's book is a curious mix of peaks and valleys. Overall, scientists, historians, and concerned citizens will find it provocative and informative. Physicists and historians of science may, however, find that it lacks strength when it deals with the ideas and praxis that help define the physics community. Despite hints to the contrary in the preface and despite neatly packaged ''scientific'' chapters, the scientific work of the physics community and the character of the institutions that succored it are not the author's main concerns. The real strength of the book lies in its forthright presentation of the public posture of the community. It is here that the author exercises command of his materials and injects the proper measure of skeptical insight. Kevles is knowledgeable, has mined the important archives, and has presented his work in a clear and readable form. Apart from an overly precious bibliography, he has made an evident effort to write well, for which he should be applauded. One hopes that Kevles's substantial contribution will stimulate historians of science to their long overdue work in providing studies of American scientific ideas and institutions during this century.

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Historical Lectures

History of Twentieth Century Physics. Proceedings of the International School of Physics ''Enrico Fermi,'' course 57. Varenna, Italy, July–August 1972. C. WEINER, Ed. Academic Press, New York, 1977. xvi, 462 pp., illus. $39.50.

In 1972 the Italian Physical Society for the first time devoted its two-week International School of Physics ''Enrico Fermi'' to lectures on the history of 20th-century physics rather than on some area of current research in physics itself. Charles Weiner directed the school and with the help of its secretary, G. Jona-Lasinio, organized the program and invited approximately 80 historians, philosophers, political scientists, and physicists as speakers and participants. The present volume constitutes its proceedings. It is incomplete in at least two respects: Léon Rosenfeld's lectures were lost to the volume owing to his untimely death, and none of the discussions have been included. A further deficiency is also apparent: owing to the long delay in publication, M. J. Sherwin's lecture has already appeared in print as one chapter in his 1975 book *A World Destroyed*, and substantial portions of others are by now well known to scholars. Nonetheless, the volume as a whole can be warmly recommended.

The individual lectures reveal some of the diversity and complexity of developments in 20th-century physics, as well as some substantial differences in the way they may be approached and analyzed historically. M. J. Klein, in tracing the beginnings of quantum theory in the work of Planck and Einstein, and J. Bromberg, in discussing the background to Dirac's first paper of 1927 on quantum electrodynamics and its implications for understanding the wave-particle duality, show the great value of close historical analysis of internal conceptual developments in physics. J. L. Heilbron's lectures on Thomson's, Rutherford's, Bohr's, and Sommerfeld's work on atomic structure between 1900 and 1922 illustrate this same point, but add a new dimension as well in setting this work within the general contexts of physicists' model-making endeavors and the state of the physics profession at the turn of the century. G. Holton, by contrasting the work of Millikan and Ehrenhaft in their pursuit of the electron and subelectron, respectively, establishes the way in which differences in age, educational background, philosophical commitments, styles and methods of research, and similar factors can lead to success or failure in physics research. Y. Elkana draws definite lessons for present-day teaching and research in physics in his deliberately provocative lectures on the historical-philosophical roots of modern physics, in which he advances a theory of the growth of scientific knowledge involving a critical dialogue between competing research programs.

Interspersed with these lectures by historians are ones by prominent physicists who bring their personal knowledge and experiences to bear on their historical reflections and analyses. P. A. M. Dirac and V. F. Weisskopf present captivating recollections of their life, work, and interactions with other physicists. H. B. G. Casimir sketches theoretical and experimental developments in solid state physics and superconductivity, recalls personal experiences he had with Ehrenfest, Bohr, and others, and analyzes in general terms the relationships between science and technology. E. Amaldi recalls in very extensive and illustrated lectures the neutron work of the Rome group in the 1930's and then turns to a discussion of postwar international cooperation in high-energy physics. L. Kowarski gives a closely related account of the origins of CERN and postwar big science. These last lectures are counterbalanced by W. Goldstein's impersonal analysis of postwar science and politics as viewed by a political scientist.

There is, in sum, hardly an essay in this volume that will not repay reading and reflection, and the volume is there-