Book Reviews

Ehrenfest's Earlier Years

Paul Ehrenfest. Vol. 1, The Making of a Theoretical Physicist. MARTIN J. KLEIN. North-Holland, Amsterdam, and Elsevier, New York, 1970. xvi, 330 pp. + plates. $9.50.

In 1912 H. A. Lorentz resigned his professorship at Leyden for a less onerous position in Haarlem. He was then at the height of his profession, the dean of Europe's theoretical physicists, the inevitable chairman at international meetings, the classical scientist *par excellence*: a natural aristocrat, serene in spirit and undeviating in purpose, the author of the crowning synthesis of 19th-century electromagnetic theory. He wanted Einstein for his successor, but Einstein, who had just accepted an offer from Zurich, perforce declined. Lorentz's second choice, Paul Ehrenfest, was a man the exact opposite of himself.

Ehrenfest was born in Vienna in 1880, the fifth son and youngest child of a Jewish grocer. Although the family seems to have been a close and happy one, Paul did not feel a full part of it. He never considered himself either a Viennese or a Jew. Along with his isolation and rootlessness came constant self-doubting, which almost brought him to suicide in his early teens. In this connection, Martin Klein points to Paul's situation as the baby in a busy household filled with much bigger and apparently omniscient brothers. Certainly this circumstance intensified his want of confidence; and it is most striking to find him later explaining his inability to do physics in the style of Einstein and Bohr with an image drawn directly from the experiences of his childhood. "Don't be impatient with me," he wrote Einstein in 1920. "Bear in mind that I hop around among all of you big beasts like a harmless and helpless frog who is afraid of being squashed." To complete the sketch we must add that this frog was an intense creature, far from harmless, who made very great psychological demands on the beasts—both students and colleagues—that befriended him.

Ehrenfest's mathematical ability, which came to light during his last years at the Akademisches Gymnasium, brought him some confidence and his first important friendship (with Gustav Herglotz). The family perhaps thought to make him an industrial chemist, or an engineer like his eldest brother; but after a year at the Technische Hochschule he turned to the University, and the inspiring lectures of Ludwig Boltzmann (1900–1901). As was the custom, he interrupted the course at Vienna with a period of study elsewhere, in his case the University of Göttingen, where he attended the mathematics colloquium of Felix Klein. The interlude proved decisive for his career. He met his future wife, Tatyana Afanassjewa, among his fellow students; he strengthened and broadened his grasp of mathematics; and he impressed Klein, who, as editor of the *Enzyklopädie der mathematischen Wissenschaften*, was to entrust to him the important article on the foundations of statistical mechanics. He returned to Vienna in 1903 to write a doctoral thesis under Boltzmann on an extension to fluid dynamics of the bizarre mechanics of Heinrich Hertz. He finished in 1904, married Tatyana, and remained in Vienna as an informal member of Boltzmann's theoretical seminar.

In 1907, after a year in Göttingen, the Ehrenfests moved to St. Petersburg. Small incomes which they had inherited could support them and their infant daughter until Paul could find a suitable teaching position in Russia. While he hunted he and Tatyana devoted themselves to the encyclopedia article, striving to reduce statistical mechanics to its essentials and to lay bare its doubtful or difficult points. The careful work, which became a classic, was not published until 1911. Meanwhile the Ehrenfests established a circle of young physicists, including their special friend A. F. Joffe, and Paul contributed an occasional paper to German journals. The most striking of these—a paradox in the theory of relativity (1910) and an elaborate analysis of the structure of phase space implied by Planck's law (1911)—brought him to the attention of Einstein and Lorentz. But the Russian authorities overlooked him, and in 1911 he decided most reluctantly to seek a job in the west. A visit to the chief German-speaking universities was not encouraging. He foreclosed the best opportunity, the succession to Einstein at Prague, by refusing to sign a statement of religious affiliation obnoxious to his atheistic conscience. Otherwise he found only a possible assistantship with Sommerfeld at Munich and a position as Privatdozent at the University of Leipzig, which, however, refused to recognize his Viennese doctorate. One can imagine, under the circumstances, the impression made by Lorentz's invitation to stand for the Leyden chair.

Lorentz as usual chose wisely. Ehrenfest quickly succeeded in establishing a thriving school of theoretical physics, something that Lorentz, with his self-containment and acknowledged superiority, had not been able to do. The passionate new professor arranged seminars on the Göttingen model, breathed life into student clubs, opened his home to visiting and native physicists, and constantly directed his associates to the fundamental problems. During the first years of his professorship he made his own most important contribution to the quantum theory, the so-called adiabatic invariants. (These were generalizations of certain quantities studied by Boltzmann to which Ehrenfest was led by his perennial desire to understand how and how deeply the quantum theory affected the foundations of statistical mechanics.) Ultimately the quantization of these invariants gave the postulates of the Bohr theory their most instructive form. They were introduced, however, without reference to that theory, which Ehrenfest initially deplored. "Bohr's work on the quantum theory of the Balmer formula," he wrote in 1913, "has driven me to despair. If this is the way to reach the goal, I must give up doing physics." Toward the end of the First World War Ehrenfest surrendered to Bohr, and in the early '20's (as we will see in the second volume of the work under review) he turned Leyden into one of the few active centers for the study of the quantum theory of the atom. As one might expect, his despondency increased with his success, and with his intimacy with Einstein, Bohr, and Lorentz, against whom he measured himself. "How extraordinarily happy I could be, if only I were not so slack and so *unproductively ambitious*. It is all completely clear to me, but that doesn't help at all. I can obtain as much pleasure as I want out of *everything*, but it's all like confectionery and marmalade—the bread is work that succeeds after exertion, and *there* I fail completely."

Klein has written a most absorbing book, the product of many years' study. He has used Ehrenfest's diaries and correspondence, especially letters exchanged with Lorentz and Einstein; interviews with Ehrenfest's associates and students, and with his widow; and, of course, Ehrenfest's published papers, which Klein has made his own. One can only quarrel with him for not including more from his rich sources, particularly the letters, many of which could have been printed *in extenso* without unduly enlarging the volume. Klein tells his story clearly and straightforwardly, with some repetition made necessary by his convenient (albeit artificial) segregation of Ehrenfest's life and work into separate chapters. One looks forward eagerly to the second volume, and to a fuller understanding of the rootless Viennese, the atheistic Jew, the insightful self-doubter, the pointed paradoxer who was Paul Ehrenfest.

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Public Exposure

'Population Control' through Nuclear Pollution. ARTHUR R. TAMPLIN and JOHN W. GOFMAN. Nelson-Hall, Chicago, 1970. xii, 242 pp. $6.95.

Few people, irrespective of their convictions on atomic energy, will feel any enthusiasm for *'Population Control' through Nuclear Pollution*. The book, which must be characterized as more political than scientific and more emotional than reasoned, is written in such inflammatory language that many readers may simply turn away from it altogether. Perhaps a more serious shortcoming than the style is that the authors sometimes confuse issues in a manner that opens their arguments to substantive criticism. The resulting loss of credibility may do the authors, their cause, and indeed the public more harm than good. As an example of such a confusion of issues, consider their discussion of radiation exposures from nuclear power plants (p. 155):

> What is wrong with nuclear power plants?
> The normal day-to-day operations of a nuclear power plant are regulated by the standards tabulated in Title 10, Part 20, of the Code of Federal Regulations. These are the reactor regulations that are promulgated by the AEC and represent the basis for the licenses issued to the nuclear power plants. As we indicated in the early chapters of this book, the primary standard which sets the allowable level for the radiation exposure of the population-at-large is much too high. We estimate that if the population of the United States were exposed to this guideline there would be an additional 32,000 cancer deaths each year.
> In addition to that, we estimate that the genetic consequences of this could be far greater, leading to an increase of between 150,000 - 1,500,000 additional deaths each year. In addition to these genetic deaths, there could be a 5-50% increase in such debilitating diseases as diabetes, schizophrenia, and rheumatoid arthritis. So far as the secondary standards are concerned, that is the maximum permissible concentrations in air and in water, we demonstrated in this chapter that these standards are essentially meaningless.

There are two issues being discussed here: the FRC recommendations and the possible radiation exposures from reactors. The Federal Radiation Council recommends that for individuals in the general population the maximum allowable exposure should be 170 millirems a year. Since an exposure level of 500 millirems a year is taken as the operational limit at the perimeter of power stations, individuals 10 or 100 miles away would suffer very much smaller doses. Thus even with the present FRC limits, the overall exposure of the population to radiation from reactors would be orders of magnitude smaller than is implied by the authors. Furthermore, the Public Health Service stated in 1970 (*1*) that

> The average annual whole-body dose rate received by individuals living near the site boundaries of 10 of the operating power reactors, based on results obtained from environmental radiation surveillance programs, has been estimated to be generally less than 5 mrem. . . . Preliminary results from a study conducted at the Dresden boiling-water reactor indicate that offside external exposure at this power plant may vary between 5 and 14 mrem per year . . . At the Humboldt Bay boiling-water reactor, the maximum integrated doses above background measured offsite were 50 mrem in 1965 and 35 mrem in 1966.

Possible reconcentration of radionuclides is obviously not included in these estimates. The failure of the authors to distinguish between the maximum allowed individual exposure and the average exposure that the general population could be expected to receive results in such distortion as to damage their credibility.

The authors of course also treat other sources of potentially harmful radiation. For example, they analyze the Plowshare program designed to recover natural gas through underground nuclear explosions. They justifiably call attention to the radioactive contamination of this gas. They say that the Plowshare advocates would respond to their objections with "We won't deliver the gas into homes if it is *too* radioactive" (p. 113). Recent estimates indicate that the potential annual dose of tritium in natural gas resulting from large-scale exploitation is on the order of a few millirems (*2*). Presumably a very large number of people could receive this dose. Who is to decide whether the radioactive natural gas recovered would be an acceptable trade-off for the increased exposure?

The objections that the authors raise to nuclear reactors, Plowshare programs, and other Atomic Energy Commission projects are based on their claim that the risk incurred from exposure to radiation is much greater than was previously believed. They argue that the maximum permissible levels of exposure should therefore be reduced. (The controversy between them and the AEC was reviewed in *Science* 6 Feb. 1970 and 28 Aug. 1970.) It is not our purpose to analyze in detail the calculations on which they have based their conclusions. These calculations can be found in a series of papers presented to the Joint Committee on Atomic Energy at the recent hearings on "Environmental Effects of Producing Electric Power." The reader can find these papers, along with several criticisms, in part 2, volume 2, of the hearings. Even if their calculations overestimate the risks by a factor of 10, the underlying problem they address still remains: Who makes the judgments of what risks are acceptable in return for the benefits of atomic energy? How should such decisions be made?

At the present time the philosophy in setting radiation standards in the United States is that the statistical risks from atomic energy should be no more, and preferably a lot less, than the risks