Book Reviews

An Era at the Cavendish

Cambridge Physics in the Thirties. JOHN HENDRY, Ed. Hilger, Bristol, 1984 (U.S. distributor, Heyden, Philadelphia). xii, 209 pp., illus. $29.

"There have been many periods of excitement in the history of experimental physics, but never has there been anything to compare with that described in this volume." Thus the editor, John Hendry, formerly a historian with the United Kingdom Atomic Energy Authority and now a research fellow at the London Business School, proffers his wares. His introductions and orientations constitute a third of the volume; T. E. Allibone's valuable account of the relations between the Cavendish Laboratory and Metropolitan Vickers makes up a sixth; and 18 pieces share the remaining half. Some of these pieces are too brief to be useful.

The editor has achieved his general purposes: to make available already published accounts that have been difficult of access, and to cause the creation of written recollections by Cambridge men of the 1930's who have not yet had their full say. The outstanding contributions in these categories are Norman Feather's analysis of the discovery of the neutron, which has been buried in the proceedings of the tenth International Congress of History of Science, held in 1962, and Allibone's paper. Hendry's introductions identify the main actors and actions in the great excitement—the discovery of the neutron, the artificial disintegration of the nucleus, the detection of the positron—and in the Cavendish background. He does not attempt to reconcile contradictions among his reminiscers. Although it might have risked the appearance of ungraciousness, an analysis of what Hendry himself calls the "myth of the Cavendish" would have been in order.

One main element in the myth, which Hendry mentions, is that the Cavendish accomplished its wonders on string and sealing wax. Another element, which he does not identify, is that Rutherford inspired or directed the great work. Several of the contributors refer to the reign of string and sealing wax; others emphasize the advanced state of Cavendish equipment, as in Kapitza's expensive big-magnet laboratory, the novel electronics of Wynn-Williams (whose reminiscences Hendry reprints), and the close ties between Rutherford's laboratory and research and development at Metropolitan Vickers. Artificial disintegration and the detection of new particles and reactions were accomplished with this advanced instrumentation. As for Rutherford, Hendry's contributors credit him with genius and stinginess in about equal measure, and several writers offer examples of his bad temper and obstruction to researches that proved important and that he thought unpromising or dilatory.

These discrepancies may be resolved by distinguishing between the work of the younger members of the laboratory, on whom the hand of the laboratory steward, who scrupulously enforced Rutherford's old-fashioned ideas about expenditure, fell most heavily, and the more senior members, who could get what they needed by pushing Rutherford, or by acquiring services and apparatus as gifts from industry. Rutherford was not the leader but a sometimes reluctant follower of the initiatives of his strongest research men: Chadwick, Cockcroft, Blackett, Oliphant (all of whom are among Hendry's contributors). These men felt themselves awkwardly placed by their chief's attitude toward money. All had left the Cavendish for other positions before Rutherford died in 1937.

A useful example of Rutherford's approach and its consequences in an environment for which he was not prepared was his last series of researches, undertaken with Oliphant as a follow-up to the work of Cockcroft and Walton.

Rutherford had called for particle accelerators to disintegrate nuclei but had grudged the initial expense. When Cockcroft argued on the basis of Gamow's tunneling theory that a machine of a few hundred kilovolts would do, Rutherford agreed to its construction. When Cockcroft and Walton succeeded, at 700 kV, Rutherford commissioned Oliphant to make an accelerator giving a maximum of 200 kV: where everyone else wanted to go bigger, he insisted on going smaller, in order to explore yields and types of nuclear reactions near their energy thresholds. It turned out that Rutherford and Oliphant could not provoke reactions in nuclei heavier than boron's; but that was quite enough for the discovery of deuteron fusion and the isobars of mass three. This success helped persuade Rutherford that efforts of E. O. Lawrence and others to build particle accelerators of a million or ten million volts were premature; and he declined to plan to build a cyclotron until 1936. As a consequence the Cavendish, the nursery of nuclear physics, was two generations of machines behind Berkeley at the outbreak of World War II. Rutherford agreed to build a cyclotron when Lord Austin gave tens of thousands of pounds for the purpose. According to Oliphant, Rutherford was upset at the amount of money and had a tantrum over the prospect of spending it. He had complained to Allibone about the price of the 100 kV transformer from Metropolitan Vickers for the Oliphant accelerator. It had cost 85 pounds.

Hendry does not risk an explanation of the fertility of the Cavendish or offer a scale of comparative excitement in the history of experimental physics. It is perhaps unfair to call him to account for his hyperbole. But history is based on comparisons over time and place; and against the discoveries of x-rays, radioactivity, and the electron those of the neutron, the positron, and artificial disintegration do not obviously carry the day. Indeed, on one reasonable criterion the earlier discoveries have precedence, for they brought to light agencies altogether unforeseen, whereas the later discoveries realized theoretical predictions and were readily absorbed into the existing fabric of physics.

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Mathematics as Empirical

The Nature of Mathematical Knowledge. PHILIP KITCHER. Oxford University Press, New York, 1983. xii, 288 pp. $25.

This is a fascinating, sometimes difficult, often contentious book meant to raise provocative questions about the nature of mathematical knowledge, its origins, development, and epistemological status. Kitcher's basic idea is that mathematical knowledge is fundamentally empirical—that the truths and proofs of mathematics are ultimately grounded in actual experience, not in abstract objects. The usual apriorist assumptions of