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 many mathematicians are stumbling blocks, says Kitcher, to a proper account of what mathematics is and how it has developed. The early chapters of this book, consequently, are devoted to showing the wrongheadedness of apriorism in a number of guises, labeled broadly within this book "mathematical intuition," either realist or constructivist, and "conceptualism."

For example, one of the major hesitations Kitcher has about whether proofs can establish a priori knowledge is that some theorems require extremely long proofs, and how can these be known a priori? Related to this is Kitcher's interesting variant on an argument of Hume's that no mathematician is ever confident of a difficult proof until it has received the approbation of others. However, anyone who has taught mathematics knows how easily students become confused, making trivial errors or miscalculations. No one would suggest that the uncertainties of students justify the conclusion that mathematics itself is uncertain. Should it be any different with mathematicians?

The real issue here is whether or not the fallibility of mathematicians ultimately affects the epistemological status of mathematics *per se*. Kitcher is content to argue that our knowing ourselves to be fallible and feeling uncertain about difficult proofs means we cannot have a priori knowledge. Clearly, one's reaction to such lines of argument condemning apriorism will be determined by one's own philosophical prejudices.

Kitcher's antidote to apriorism is an unconventional brand of empiricism. In essence, he argues that in the beginning mathematical knowledge was built up from the observation and manipulation of ordinary things. It was the Greeks who began to systematize the practical knowledge of the Egyptians and Babylonians. The knowledge established by individual mathematicians was passed on through a succession of teachers and schools, leading eventually to the modern mathematical community. This community, Kitcher maintains, primarily in the ways it warrants and passes on beliefs from one generation to another, is of *epistemological* relevance. Kitcher calls this his evolutionary theory of mathematical knowledge.

Kitcher maintains that any adequate theory of mathematical knowledge must be psychologistic. He develops this idea in terms of warrants, of which the following will give a flavor of his approach: "X knows that p if and only if p and X believes that p and X's belief that p was produced by a process which is a warrant for it." This is closely related to the subject of proofs, on which Kitcher holds a functional view—to follow a proof is to follow a psychological process.

However, empiricist approaches to mathematics inevitably encounter difficulties. Arithmetic, for example, cannot be limited to physical objects or to operations we actually perform, otherwise Euclid's theorem that there must be an infinity of primes would be meaningless, and mathematics would be unable to countenance theories of the infinite or of infinitesimals. Kitcher deals with this problem by maintaining that arithmetic owes its truth not to the actual operations of real mathematicians but to the "ideal operations performed by ideal agents." This propels his "empiricism" beyond the confines of mathematicians bound by limits of space and time. In fact, set theory involves even greater novelty than arithmetic, for it ultimately requires Kitcher's ideal subject to operate in *super*-time, which is necessary to produce transfinite numbers and the most basic features of transfinite set theory itself—all of which may easily bewilder any conventional empiricist.

Kitcher is determined to account not only for what mathematics is, epistemologically, but for how it changes. In this connection he regards the well-known work of Thomas Kuhn as particularly relevant, but with some important modifications. While critical of certain aspects of Kuhn's thinking (he is particularly hard on the idea of paradigms), Kitcher seizes upon the idea of scientific practice as especially well suited to his needs. Its analogue, mathematical practice, fits well with his evolutionary model, in which the history of mathematics is seen as a sequence of practices. Mathematical practice includes five components: a language, a set of accepted statements, a set of accepted reasonings, a set of questions selected as important, and a set of metamathematical views. It is Kitcher's contention that these components are never in complete harmony, and the attempt to bring concordance generates mathematical change.

These components of mathematical practice are used in the last chapter of Kitcher's book to account for one particularly significant development in the history of mathematics, the development of analysis (the calculus) from Newton and Leibniz to the end of the 19th century. The fit between historical fact and the rational reconstruction that Kitcher offers in this case study is extraordinarily good, but many will feel that the case study only reflects the theory that Kitcher has had in mind all along. It does, however, serve to illustrate his main ideas in a very direct way.

Historians of mathematics will wonder why, in the only deeply historical part of a book that insists on the importance of the history of mathematics, Kitcher has not drawn upon some of the major and most recent works on the subject he is treating. No references are made to the studies of Bos, Baron, Grabiner, Fleckenstein, Hofmann, Manning, Scriba, Westfall, or Whiteside, to mention but a few. Kitcher justifies his disregard of others' work on the grounds of their aprioristic biases, but by neglecting their insights he misses not only mathematical details but some of the rich general historical background as well.

For example, Kitcher suggests more than once that Cauchy's real motive for bringing rigor to the calculus in his famous *Cours d'analyse* (1821) was the problem raised by Fourier's representation of arbitrary functions using trigonometric (Fourier) series. However, little evidence is offered for this conjecture. Insofar as Cauchy's celebrated (and wrong) theorem that the sum of an infinite series of continuous functions is continuous was published in the *Cours d'analyse*, it would seem that in the early 1820's, when he was most concerned about rigor, Cauchy had strong reasons for doubting Fourier's conclusions, if not for rejecting them entirely, as did Lagrange in the most adamant of terms when Fourier first presented his paper on heat diffusion to the French Académie des Sciences in 1807. Nor does Kitcher develop Cauchy's most direct motive for being so careful about rigor in the *Cours d'analyse* (emphasized by Dirk Struik, for one, in work not cited by Kitcher)—namely the demands of teaching the calculus to students at the Ecole Polytechnique. This is especially surprising given the emphasis Kitcher gives elsewhere in his book to teaching as an epistemological vehicle. One might also weigh the importance of Cauchy's own teacher, Lagrange, on the subject of rigor. From the recent work of Judith Grabiner, Lagrange seems a much more likely source than Fourier for Cauchy's interests in bringing "purely analytic proofs" to the presentation of the calculus in 1821.

Whatever differences one may have with Kitcher over details of history or interpretation, his overall conception is impressive for its combination of mathematical understanding, philosophical insight, and historical sensitivity. The book he has produced is not always easy to read, but the time devoted to working out the implications of his ideas will be well spent for anyone seriously interested in the epistemology of mathematics and its historical development.

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The Magellanic Clouds

Structure and Evolution of the Magellanic Clouds. SIDNEY VAN DEN BERGH and KLAAS S. DE BOER, Eds. Reidel, Boston, 1984 (distributor, Kluwer Boston, Hingham, Mass.). xviii, 425 pp., illus. $47.50; paper, $24.75. International Astronomical Union Symposium no. 108. From a symposium, Tübingen, Germany, Sept. 1983.

The Large and Small Magellanic Clouds appear to the naked eye of an observer in the Southern Hemisphere as two luminous patches that resemble detached sections of the Milky Way. In fact, the Magellanic Clouds are the nearest major galaxies and have played a central role in the development of our understanding of extragalactic systems and their constituent stellar populations. The significance of the Magellanic Clouds in these regards was especially emphasized by the late Bart Bok, to whose memory this volume of proceedings is appropriately dedicated.

The book is well organized. The papers are divided into seven groups, within each of which there are several reviews and more numerous brief contributed papers. Summaries of panel discussions also are included and cover some of the more controversial topics. The book provides a balanced overview of the present status of research on the Magellanic Clouds.

This certainly was not an easy task. During the decade that has elapsed since the last major meeting on the Magellanic Clouds there has been an information explosion fueled by the construction and instrumentation of ground-based observatories in the Southern Hemisphere and by the advent of sensitive observatories in space. Thus we now have data on very faint stars obtained with large optical telescopes that allow the histories of the Magellanic Clouds to be probed over time scales of billions of years, as well as a variety of x-ray measurements from the Einstein satellite that yield insight into properties of massive, short-lived stars and their violent interactions with interstellar gas. Both topics are well covered in review papers. Some of the newer observational possibilities are still being exploited, but early results are contained in many of the contributed papers. Examples include the important molecular studies that are being carried out in Australia and in Chile by the Columbia-Chile group or the discussions of the nature of a star named R136a, a possible "superstar" or star-cluster powerhouse containing thousands of solar masses in the core of the gigantic 30 Doradus ionized gas cloud. The contributed papers also present interesting new interpretative ideas, such as a study of the spatial distributions of star-forming regions in the Large Cloud by means of pattern recognition analysis.

Do we then have a nearly complete picture of the evolutionary and structural properties of the Magellanic Clouds? The answer is clearly no, although from this symposium it is also evident that we now know where more of the difficulties lie. In principle the evolution of a given galaxy involves a rather straightforward conversion of a gravitationally bound primordial gaseous system into stars, but in reality the processes are anything but simple to diagnose or model. The new studies of the Magellanic Clouds, galaxies that are rather different from our own Milky Way, serve to reinforce this point. As one illustration, recent investigations of microwave line emission from the molecule CO indicate that there might be fewer molecular cloud sites of star formation in the Magellanic Clouds than in the Milky Way, and yet optical and ultraviolet observations leave no doubt that the Clouds are producing many young stars. There is an interesting debate about the degree to which the numerous young stars are due to effects associated with the close passage of the Clouds by the Milky Way rather than to intrinsic variations in modes of star formation between different types of galaxies (the galactic version of the "environment vs. genetics" issue). Similarly, we find that even the well-developed topic of stellar evolution can be a subject for surprises in the attempt to understand the extensive stellar populations of young and intermediate age in the Magellanic Clouds. We also have yet to determine such fundamental points as whether the Magellanic Clouds consist only of normal stars and gas or whether they, like the massive spirals, are embedded in invisible envelopes of dark matter.

We should not be discouraged by these and other loose ends that were brought up throughout the symposium. Rather, we should recognize that the Magellanic Clouds are fulfilling their traditional role as laboratories for advanced studies of normal galaxies and look forward to the next symposium on the subject.

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Invertebrate Vision

Photoreception and Vision in Invertebrates. M. A. ALI, Ed. Plenum, New York, 1984. x, 858 pp., illus. $115. NATO ASI Series A, vol. 74. From an institute, Lennoxville, Quebec, July 1982.

The topics covered in this book range from photoreception by eye spots of single-celled protozoans to vision by 20 million receptor cells of the octopus eye. A third of the book is devoted to the simplest invertebrates, such as protozoans, metazoans, rotifers, and nematodes. Half is devoted to animals that have compound eyes, such as insects and crustaceans. The remainder includes papers on invertebrates that have multiple ocelli, such as spiders and myriapods.

Given its origin in a two-week tutorial workshop, one might expect the volume to be a handbook of papers that survey the field of invertebrate vision. It is not, according to the editor, who faced an unusual problem in organizing the book. Autrum's three volumes of the definitive *Handbook of Sensory Physiology* had recently appeared. Not only do the topics in the two books overlap, a third of the authors who contributed to this book also contributed to the *Handbook*. Ali's response to this challenge was to create not a handbook but a "glorified text-book" meant to be more easily accessible to a larger number of persons. Success in this venture is mixed. Half of the authors wrote tutorial papers that would be useful in a college course. Others wrote for the specialist or limited discussion to their own research. The inflated price of the book limits its utility as a textbook. The quality of the printing is below the standards required by electron micrographs.

The coverage of the lower invertebrate groups includes much comparative description of photoreceptor and pigment