Physicists and Historians

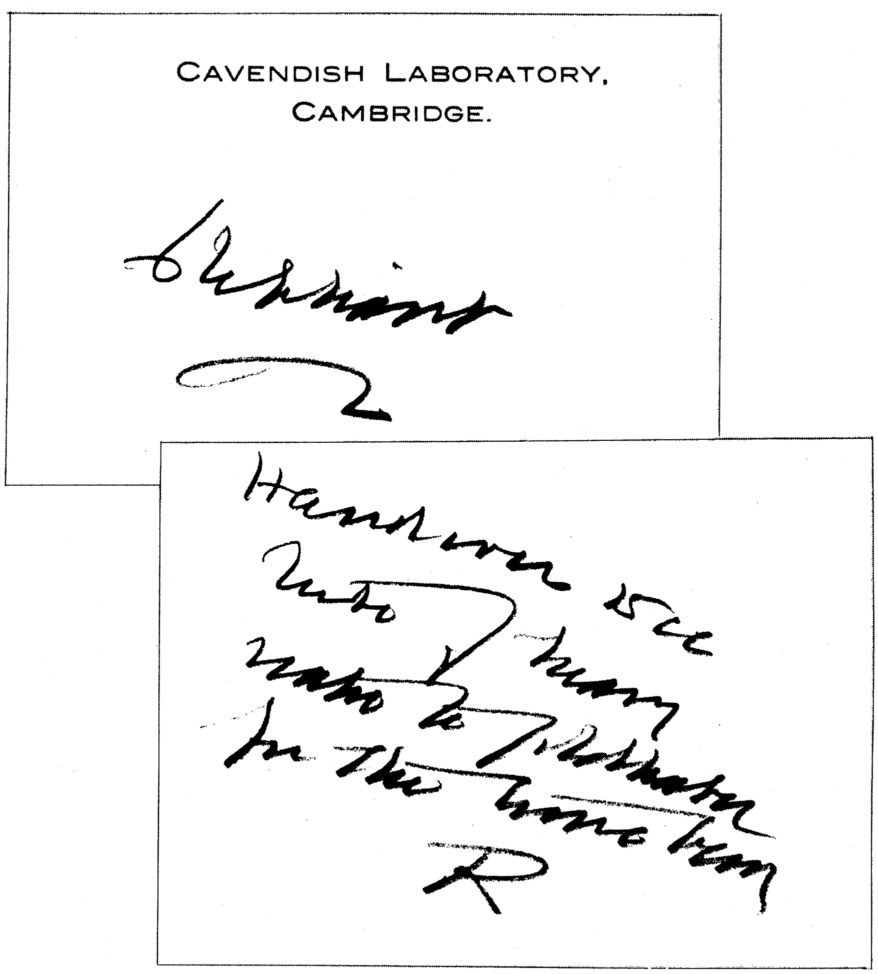
Nuclear Physics in Retrospect. Proceedings of a Symposium on the 1930s. Minneapolis, May 1977. ROGER H. STUEWER, Ed. University of Minnesota Press, Minneapolis, 1979. xviii, 340 pp., illus. $25.

The history of nuclear physics has yet to find its feet. With the sole exception of the experimental and technical developments following from the discovery of nuclear fission in 1938—on which important books have been published by Margaret Gowing, Richard Hewlett and coauthors, and Spencer Weart—no substantial slice or aspect of the history of nuclear physics has yet received monographic treatment. The situation is, to be sure, no better for any other field of physics of the past half century, and would scarcely warrant comment were it not that the past dozen years have seen considerable pump priming in the history of nuclear physics. In particular, there have been at least three conferences throwing historians of physics together with older nuclear physicists in order that those who write history might hear about the early history of nuclear physics from those who made the history.

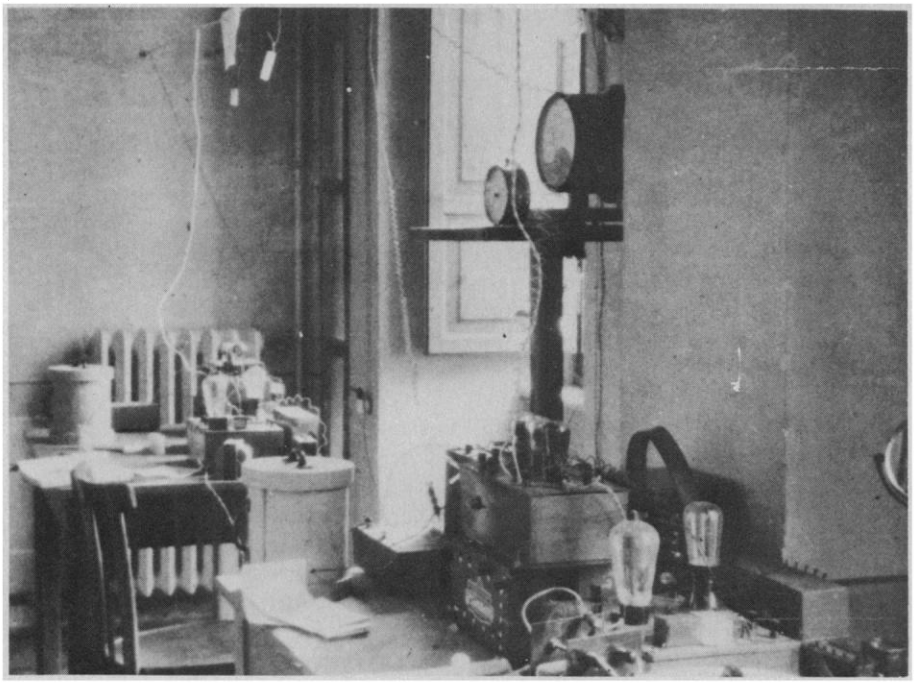
The volume under review is the proceedings of the most recent, and by far the most productive, of these conferences. It was conceived as a sequel to two previous conferences, in 1967 and 1969. In contrast with those two conferences, which consisted solely of round-table discussions and whose proceedings are nearly unreadable and barely usable, the 1977 conference was organized in the conventional manner with each of the eight speakers—all eminent physicists, half of whom had participated in one of the previous conferences—addressing a more or less general aspect of or problem in nuclear physics before the Second World War, approaching it from a more or less autobiographical point of view.

The presentations are uneven in every respect—length, coverage, documentation, distance from mere transcript of oral delivery, organization, coherence, and degree of historicity. Not one of the papers can yet be said to be history—although John A. Wheeler's lengthy contribution comes admirably close. Nor do the speakers, by and large, claim to be writing history; rather, they seem to see themselves as presenting materials for the writers of history. From these materials historians may draw (for further investigation) anecdotes, characterizations of individuals, and generalizations about the conceptual situation and development of the science. And, as most of the papers are generally readable, certainly by any physicist, the volume has value during that interim, in all probability still lengthy, while the history of nuclear physics is set upon proper foundations.

Hans Bethe opened the symposium with a series of disconnected critical comments on the state of nuclear theory in the 1930's and 1940's, making little distinction between historical and scientific correctness. His title, "The happy thirties," thoroughly camouflages the content of his paper and belies its technical character; it is the least readable contribution to the symposium. The following morning, however, Emilio Segrè sounded a new keynote at the other end of the scale. Speaking on "Nuclear physics in Rome," as he has often before, Segrè dwelled almost entirely upon personality. As always, he is highly entertaining, with some new anecdotal gems giving added sparkle to a now familiar story. Next came O. R. Frisch's "Experimental work with nuclei: Hamburg, London, Copenhagen." His interesting account is strictly autobiographical and largely anecdotal; most of what he here writes he has written at greater length elsewhere. Maurice Goldhaber's paper "The nuclear photoelectric effect and remarks on higher multipole transitions: a personal history," is an admirably focused account of the history of a single nuclear process, which integrates his work and personal experiences with the development as a whole. While concentrating on the early '30's, Goldhaber



"Hand over 25 cc tube of heavy water to Goldhaber for the time being," a note from Rutherford to Oliphant, early 1935. Obtaining the heavy water, which was needed for testing the electric dipole character of the photodisintegration of the deuteron, "was quite a business. . . . This was a good fraction of the world's supply of heavy water at that time, and it was carefully handled." [From M. Goldhaber's contribution in *Nuclear Physics in Retrospect*]



Geiger-Müller counters used in Rome around 1934 to detect β-rays emitted by nuclei rendered artificially radioactive by absorption of neutrons. [From E. G. Segrè's contribution in *Nuclear Physics in Retrospect*]

carries the story forward into the '50's. Although brief, this is one of the best papers in the volume.

Edwin McMillan's contribution, "Early history of particle accelerators," is especially noteworthy for the record it brings of McMillan's efforts to do as a historian would—namely to seek for evidence. Appended to his brief paper are informative letters received, in response to his inquiries, from M. A. Tuve, G. Breit, and E. T. S. Walton. Eugene Wigner's slight paper, "The neutron: the impact of its discovery and its uses," is, once again, but random remarks strung together with phrases such as "the next development" and "the next event on my list," which, for all their seeming attention to temporal order, show only a thorough misunderstanding of what history is. Rudolf Peierls, one finds with relief, provides a coherent interpretation of "The development of our ideas on the nuclear forces," showing that (but scarcely how) each of the simplifying assumptions made in the early 1930's has, sooner or later, been dropped. Here again, however, the typology and the causality are less historical than physical or logical.

All the foregoing contributions together would have made up only a rather slim book; the volume is saved by the symposium's anchor man, Wheeler. "Some men and moments in the history of nuclear physics: the interplay of colleagues and motivations" is a hundred-page scientific autobiography of the first ten years of Wheeler's professional life, culminating in his collaboration with Niels Bohr on the theory of nuclear fission in 1939. The interplay of which Wheeler speaks is between his own motivations and his own colleagues. And the explicandum that integrates the essay conceptually is Wheeler's reorientation in theoretical nuclear physics in which what one might call the Newtonian program, namely "from the motions to find the forces" between the elementary particles, gave way to a view of the nucleus as condensed matter whose collective modes of motion are to be discovered and energetically evaluated. Nor is this merely scientific autobiography; important events in Wheeler's personal life are permitted to appear and the romantic character of the man to stand forth clearly. (With a scant two days in Paris Wheeler found time between his scientific contacts to visit the tomb of Abelard and Heloise.) Although autobiography, Wheeler's essay has breadth of view and depth of research. He has ten times as many footnotes as the next most extensively documented paper (Peierls's) and even cites some of the little secondary literature that exists—a measure of scholarly intent that none of the other contributors share.

Yet it is exactly when reviewing so admirable a piece of recollected history, so bolstered with scholarship, that it is important to emphasize how beguiling is memory, how treacherous its contribution to the historical narrative. This volume, ironically, helps one to bear that in mind by juxtaposing contradictory recollections of different participants. A striking case is Wheeler's concrete and circumstantial description of the seminar in which Bohr, sinking for some moments into trancelike thought, conceived the "compound nucleus" model of nuclear processes—the notion that the excited nucleus has no memory of the process by which it was excited, and that the process by which it subsequently divests itself of its excess energy is but the result of a chance fluctuation in the distribution of that energy over the nuclear constituents. Wheeler confidently dates this illumination in the spring of 1935. Frisch, too, recounts his own vivid recollection of that event, dating it however in the *autumn* of 1935.

Is this discrepancy just one of those inconsequential details in which pettifogging historians delight? Is the editor to be praised for taking no notice of them? Quite the contrary. Experimental discoveries and theoretical considerations bearing on the interaction of neutrons with nuclei were following upon one another so quickly during the course of 1935 that the question of the date of Bohr's insight becomes, in effect, a question of the specific problem situation to which Bohr was responding. On the other hand, the question arises in this reviewer's mind whether Wheeler (who returned to America in June 1935) was actually present at the historic event he describes, whether he has perhaps created this recollection by unconsciously fusing his picture of Bohr's characteristic behaviors with the firsthand accounts of that seminar that he later heard from colleagues and that are now well established in the folklore of the field. Certainly there are elements of Wheeler's account, such as Bohr's immediately drawing the analogy between the nucleus and a liquid drop, that are most unlikely. And certainly it is extremely difficult to imagine that, had Bohr blurted forth this simple yet revolutionary idea in a seminar back in April, it could have remained unknown to theoretical nuclear physicists—as it appears to have done—until Bohr's first public announcement late in January of the following year.

Where then is the history of nuclear physics which all this pump priming is ostensibly meant to stimulate? Why is there yet no steady stream of scholarly historical writings on this subject? One is tempted to say that these conferences have failed because the physicists fail to understand what history is, what historians are trying to do. Certainly many of the contributors to this volume testify eloquently, albeit unconsciously, to this fact (for example: historians "know history and dates and authorships wonderfully," "you get all the little things you can dig up, little obscure publications that there is no evidence anyone ever saw," "the underbrush, all this difficulty of dates, sources, priorities, is straightened out"—a common misapprehension, here voiced by three different physicists). Certainly the failure of the physicists to understand the historians' aims and concerns had much to do with the failure of the conference to produce any effective interchange between the physicists and the historians; the discussion, under the eyes of video cameras, remained almost entirely among the physicists, who rather misunderstood the few questions from historians.

But if nuclear physicists are to be held responsible for the failure of the history of nuclear physics to be constituted as a scholarly field, it is not because they have failed qua historians or qua historical informants, but qua physicists. In contrast with the clarity, simplicity, and completeness of the physics of the extranuclear structure of the atom, a scientific development with a well-defined terminus ad quem and forming a well-established subject of historical scholarship, in the physics of the nucleus there is even to this day no specifiable nuclear force. There is no definitive solution of the problem of nuclear structure and processes, but only a variety of partial, more or less comprehensive, points of view. Because the physicists have not completed their task, the historians remain reluctant to enter upon theirs.

PAUL FORMAN
National Museum of History and Technology, Smithsonian Institution, Washington, D.C. 20560

Gravitational Waves

Sources of Gravitational Radiation. Proceedings of a workshop, Seattle, July 1978. LARRY L. SMARR, Ed. Cambridge University Press, New York, 1979. xiv, 506 pp., illus. $19.95.

Gravitational waves are propagating ripples of tidal gravitational stress. In Einstein's theory of general relativity (and nearly all other plausible theories of relativistic gravity) the waves are generated by rapidly changing accelerations of massive bodies. Asymmetrically imploding and exploding supernovae will do the job nicely. The waves traverse empty space at the speed of light, their energy becoming diluted by the normal inverse-square law. Gravitational waves, even as weak as they are when they reach the earth, can in principle be detected by the small accelerations that they give to one free mass relative to another that is separated by some distance or by the small stresses that they induce in elastic solids.

There is a certain genteel omission in the title of this fascinating and important book of symposium proceedings. The subject of *detecting* gravitational radiation is one of the book's main concerns. But that subject, some people think, is still slightly disreputable after the much publicized—alas, erroneous—"detection" of gravitational waves a decade ago. The book ought to do much to dispel such notions of disreputability. It is crammed full of hard laboratory information and (albeit not so hard) theoretical calculations. Theorists and experimentalists in this field are learning to speak the same language, and they are plotting their results (best estimates of source strengths from astrophysical sources and best estimates of achievable sensitivities in detectors, respectively) on the same graphs. Today the sources are below the sensitivities, but the gap is rapidly narrowing.

There is a growing consensus that two sorts of gravitational wave experiments are worth pushing hard. The first (and the longer recognized) is to detect kilohertz signals from collapsing supernovae in galaxies out to the Virgo cluster. The event rate ought to be on the order of one a month. The techniques of choice are ground-based (read "NSF-supported"), either with large monocrystal bars of sapphire or silicon or with multipass and high-power laser interferometry. The second experiment is to detect waves of much lower frequency, millihertz, which may have been produced by the collapse of massive black holes in the centers of quasars. One might "see" these collapses out to cosmological distances and therefore back in time to nearly the beginning of the universe. This frequency band and required sensitivity can only be obtained with a space-based (read "NASA-supported") development program. The techniques under consideration are precision Doppler tracking of already planned spacecraft, but with two-frequency up- and down-link telemetry and a dedicated mission with an onboard hydrogen maser clock.

A broad and interesting mix of physics and astronomy goes into the theoretical calculations of wave sources, and this variety is ably represented in the volume. Arnett's review of gravitational collapse in evolved stars does a particularly good job of demythologizing a subject that is usually adrift in a sea of numerical simulation, and (on the other hand) Wilson gives a clear exposition of how relativistic numerical hydrodynamics is actually done. At least half a dozen other papers could be singled out for their high quality and general interest. Two papers are edited transcripts of discussion sessions. Far from the dry pontification that usually dominates such sessions, these discussions—at least as edited by Epstein and Clark—read as free-wheeling brainstorming sessions that stand as remarkable portraits of an evolving and eclectic subject.

WILLIAM H. PRESS
Center for Astrophysics, Harvard University, Cambridge, Massachusetts 02138

Island Study

The Terrestrial Ecology of Aldabra. Papers from a meeting, March 1977. Royal Society, London, 1979. 264 pp., illus. + index. £21.65. First published in *Philosophical Transactions* of the Royal Society of London, series B, vol. 286.

Aldabra is an atoll in the Indian Ocean and the home of giant tortoises. About 15 years ago the British Ministry of Defence planned to build a military air base in an area of the atoll now known to support 60 percent of the tortoise population. The plan encountered serious opposition from conservation groups, and the Royal Society of London, while pressing for the abandonment of the air base scheme, sent a major scientific expedition to the atoll in 1967. Before the end of the year the scheme was indeed abandoned, and the Royal Society then switched its plans from short-term fact-finding to more comprehensive studies. Ten years later the Society held a two-day discussion of research progress. This book is a collection of the 23 papers presented at the discussion.

The major shortcoming of the volume is a lack of integration, which reflects the rather fragmented and uncoordinated nature of some of the research it reports. Nevertheless, there is no better source of information about Aldabra and its inhabitants. There are papers on geological and recent history, rainfall, soils and soil organisms, vegetation, seed dispersal, insects, crabs, land and sea birds, and turtles and tortoises. Of particular interest from the point of view of equilibrium island biogeography theory, which is frequently referred to, is the discovery of