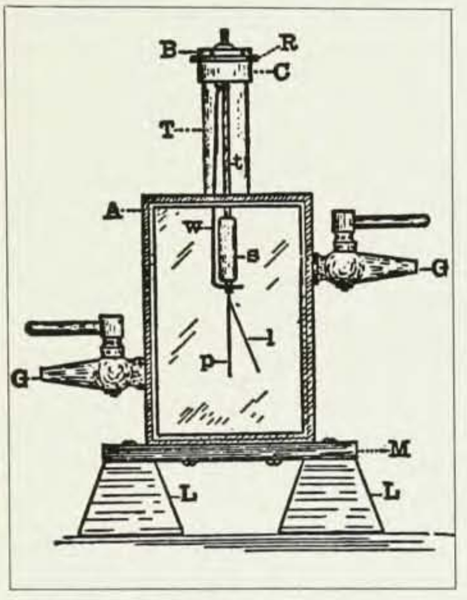
books

American nuclear physics in the early years



Bertram B. Boltwood and his air-tight electroscope for measuring radium emissions. Boltwood (1870–1927) was an American physicist–chemist who proved radium is a disintegration product of uranium and who established the basis for the study of radioactive isotopes.

Radioactivity in America: Growth and Decay of Science

L. Badash
345 pp. John Hopkins U., Baltimore, Md., 1979. $18.95

Reviewed by David L. Anderson

It is easy for most American physicists, if they think at all about the history of their field, to think that physics in America really began in the late 1920's with the brilliant group of young physicists returning from Göttingen and Copenhagen—John H. Van Vleck, Edwin C. Kemble, J. Robert Oppenheimer, Edward U. Condon and others, or in the early 1930's with the work of experimentalists such as Ernest O. Lawrence and Carl D. Anderson. Of course one knows of legendary figures such as Henry, Gibbs, Rowland and Michelson, but they seemed to be singular points. As corrective to this naive view, Lawrence Badash, a young historian of science now at the University of California, Santa Barbara, shows in this excellent book that American research in the three decades after Henri Becquerel's 1896 discovery of radioactivity provided much of the content and the setting for the "coming of age" of physics as a discipline in this country.

One might quibble slightly with the subtitle; perhaps the study of radioactivity as such did "decay," but the subject matter was transmuted into the solid basis for nuclear physics. That subject matter now seems to undergraduates as fairly straightforward, even obvious. Only current physics provides mysteries, confusions, exasperations and exhilarations. But what looks simple in hindsight was terribly confusing at the time. How many naturally occurring radioactive substances were there? What were their chemical characteristics? What sorts of particles or rays did they emit? Where did the energy come from? Why do some newly separated substances seem to grow in activity? Why do some seem constant in activity, while others decay? The Rutherford–Soddy concept of transmutation, dating from 1903, seemed especially bizarre, since it cut at the the root of the very idea of the atom. Yet that concept, together with much patient and ingenious chemical and physical research, finally brought forth the concept of isotopy and made it possible to find places in only three radioactive families for the bewildering array of some forty different activities. Meanwhile, of course, the Rutherford nuclear atom was a fruitful by-product of radioactive research and the stage was set for the dramatic theoretical and experimental development of modern nuclear and atomic physics.

The main title of the book is also slightly misleading. Of course North Americans did significant work in radioactivity, but much of the important work was done in Europe. Badash's point, of course, is that the American work was both quantitatively and qualitatively significant and recognized as such. There was a remarkable amount of transatlantic cooperation by interchange of personnel, journals and letters. (Letters that spent six or seven days on a transatlantic liner in the 1920's reached their destinations in about the same length of time as letters that spend only six or seven hours on a 747 these days. Progress?) Badash's hero is clearly Bertram B. Boltwood of Yale, but many others participated, Haroutune Badourian, Robert A. Millikan, Herbert McCoy, Hermann Schlundt and Theodore W. Richards among them. The author also treats the interactions among the physicists and chemists and their universities, government agencies, mining companies and industrial laboratories. Early medical applications (and charlatanry) fill an interesting chapter.

At times the story of the sorting out of the 40-odd naturally occurring radioactive isotopes gets a bit tedious, especially since the early names for them (Uranium I, Uranium X_1, Uranium X_2, Radium A, B, C, D, etc.), which made sense in the early research, then hung on as albatrosses around the thought-processes of scientists trying to understand the concept of isotopy. The reader may find it helpful to prepare a grid for listing each activity by old and modern nomenclature, with atomic number (and elemental name) on one axis, and neutron number on the other. One can then visualize alpha and beta transitions and appreciate somewhat more why it was such a struggle to sort out the massive confusions.

It is good to have this book available, not only for historians of science, but also for young practicing physicists who wonder how American physical science "came of age."

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Electricity in the 17th and 18th Centuries: A Study of Early Modern Physics

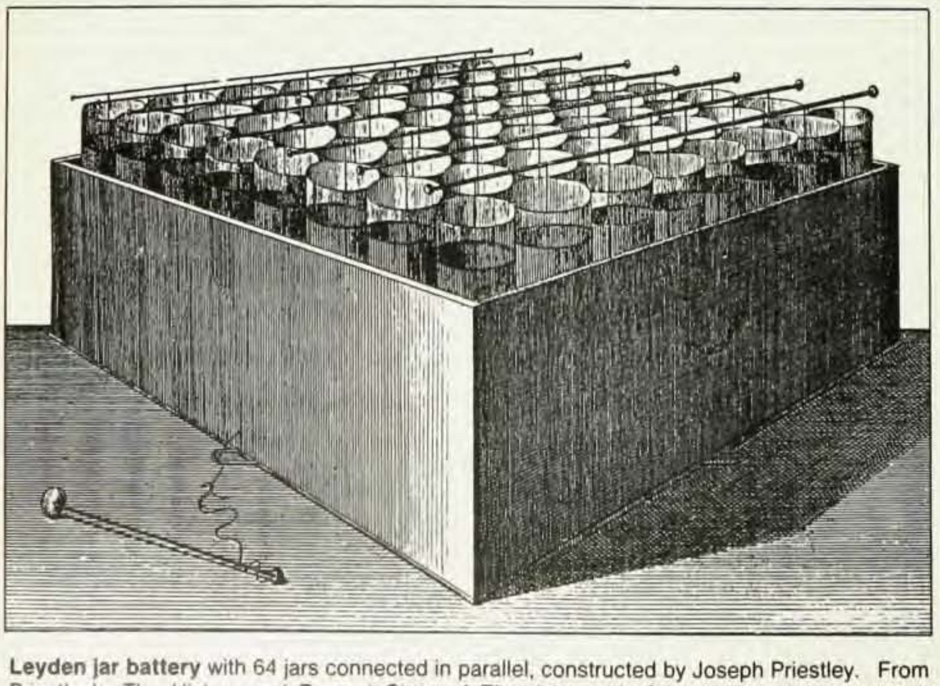
J. L. Heilbron
620 pp. U. California, Berkeley, Cal., 1979. $40.00

Readers of PHYSICS TODAY will find in John Heilbron's impressive work an account of the origins of their profession. He leads us with meticulous care through evidence painstakingly drawn together to show that, beginning in the 17th century, a new group was forming. He counts the numbers and then examines their theories, their societies, their universities, their salaries, the cost of their instruments, their interrelationships, their styles of expression. This is by no means light reading, especially for the non-historian, since Heilbron has the historian's instinct to make available as much of the information he has uncovered as he can. But this also means that the book abounds with intriguing sidelights and commentaries which can make browsing a rewarding experience.

For someone especially interested in electrical instrumentation, like myself, there is satisfaction in reading his view that "new apparatus played a capital role in altering ideas about electricity during the eighteenth century" (page 5). He lays special emphasis on the importance of the Leyden jar. Its "power and unexpectedness . . . the distress, even fear, it initially provoked" (page 307) assured that it would not be ignored. In the end, it revealed inconsistencies in existing theories. "It made a mockery of the confidence of the system-builders of 1745. It created a crisis" (page 316).

Other factors also played critical roles. One of these was the Jesuits. Their extensive educational system was a powerful force, which Heilbron credits with keeping knowledge of electricity alive in the seventeenth century. One of the most important aspects of this system was that members were willing to question Aristotelian doctrines that they were supposedly preserving. Furthermore, a late sixteenth century proposal to develop official textbooks died early in the seventeenth century, a fortunate circumstance since, as Heilbron notes, "Such a text, by freezing curriculum just as the new science began to develop, would seriously have handicapped Jesuit efforts to educate Catholic Europe for survival in the modern world" (page 109).

And there are insights that reveal that parts of the profession have remained unchanged over three centuries. It was often necessary for the successful professor to be a showman, yet to perform in this manner could result in getting students who only attended class to be entertained. To be serious, on the other hand, might drive away the bulk of the students, hence also their fees. This was a dilemma that not all teachers could successfully resolve.



Leyden jar battery with 64 jars connected in parallel, constructed by Joseph Priestley. From Priestley's *The History and Present State of Electricity, with Original Experiments (1767).*

Several times Heilbron treats the various roles of theories in terms that must seem quite familiar to practitioners today. For instance, Gilbert's decision (largely arbitrary at the beginning of the seventeenth century) to treat electricity and magnetism as entirely separate, proved to be very fortunate in terms of allowing him and his successors to define for electricity its own set of problems for which unique solutions could be sought. However, the distinction also introduced difficulties. "For example, the supposed one-sidedness of electrical attraction and two other fanciful items later introduced made it difficult for electricians of the seventeenth century to recognize mutuality in electrical interactions, and practically impossible for them to discover conduction and electrostatic repulsion" (page 176). Thus Heilbron notes, as others have, that Franklin's isolation in America served him well, since he was able to remain ignorant of recent European thinking as he pursued his experiments with a relatively fresh mind (page 330). He goes on to write that some of Franklin's explanations were much too simplistic to be valuable beyond first approximations, yet strength of their statement pressed others to examine and resolve anomalies (page 334).

Heilbron also supports the importance of personality. Thus, when Alessandro Volta traveled to Germany in 1784 he visited G. C. Lichtenberg (of Lichtenberg figures) at Göttingen, who was not particularly well-disposed toward his Italian colleague. "Volta's visit changed that. Lichtenberg could not resist his 'lusty' guest, a genius who swore and crackled over his experiments, guzzled and disputed over his dinner, electrified the ladies . . . and understood more of electricity than anyone else in Europe. After a few days . . . Lichtenberg was prepared to grant him the Newtonship of Electricity," and to promote Volta's views in his writings.

Surely this study of our past tells us something meaningful about our approach to the discipline at present.

BERNARD S. FINN
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Plasma Physics for Nuclear Fusion

K. Miyamoto
620 pp. MIT, Cambridge, Mass., 1979. $50.00

Kenro Miyamoto, professor at the Institute of Plasma Physics, Nagoya University, is well known in the field of stellarator research. He has attempt-