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.

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.

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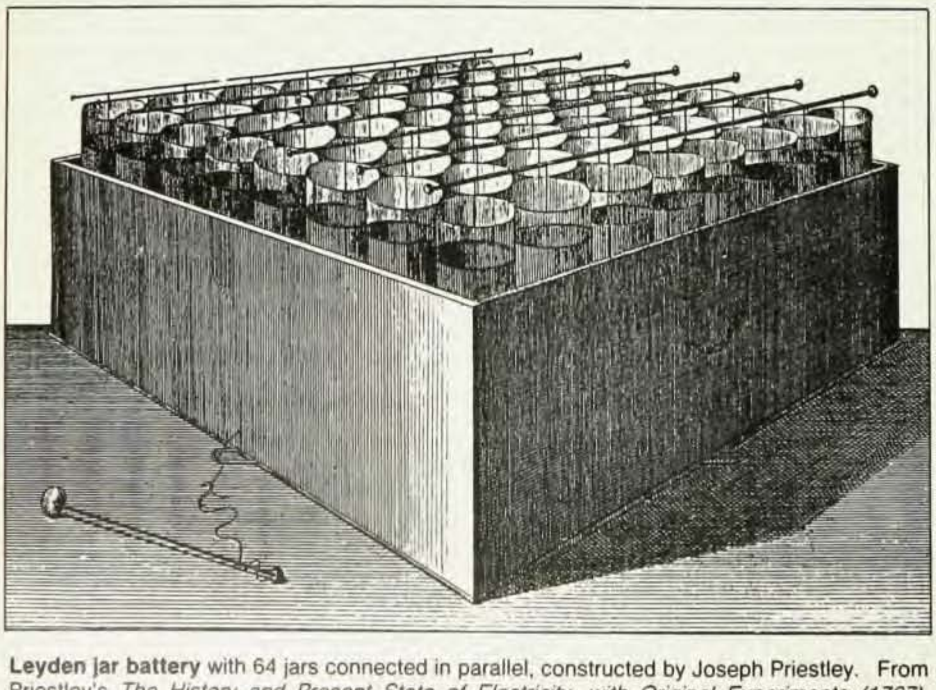

Leyden jar battery with 64 jars connected in parallel, constructed by Joseph Priestley. From Priestley's *The History and Present State of Electricity, with Original Experiments (1767).*

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 attempted the formidable task of writing a graduate text covering the physics of magnetically confined plasmas in its entirety, covering such diverse topics as magnetohydrodynamic equilibrium and stability, microinstabilities, heating and diagnostics. Although necessarily superficial in places, he successfully gives a broad overview of the subject.

This edition represents an English translation of the Japanese original, first published in 1976. Although the translation was performed by the author, he has not used this opportunity to revise and update the 1976 edition. This is particularly unfortunate in those cases in which the accepted wisdom of 1976 has been overturned in the intervening four years—a long time in this rapidly developing field. Typically, this is due to the inapplicability of approximations, such as those used in the derivation of the critical shear for stability of collisionless drift waves in slab geometry and in the critical poloidal β for existence of tokamak equilibrium. Thus, caution must be urged in using such expressions. Also, such recent developments as the theory of ballooning modes in toroidal geometry are not treated.

By adopting a rather terse style, the author has managed to compile a large compendium of useful formulas applicable (with the caveat above) to systems of sufficient complexity to be of interest as prospective fusion devices—tokamaks, stellarators, mirrors, and so on. This pursuit of relevance is rare in an introductory text book and is to be applauded. The other side of the coin, however, is that Miyamoto may have failed to discuss basic principles sufficiently to make the work accessible to students. The author also omits a critical discussion of the domain of the applicability of the approximations and techniques used. For instance, when magnetic surfaces in nonaxisymmetric systems are introduced by the method of averaging, there is no discussion of the fact that surfaces may not exist in the strict sense, nor of the consequences of stochastic field line wandering in the event of surface breakup.

The English is reasonable, though not impeccable, and the number of misprints is not exceptionally high. I found the use of nested parentheses, undifferentiated by size or shape, a feature that made the equations difficult to read. Another habit I found confusing was the systematic omission of the dot between the ∇ operator and the vector in divergences. This is particularly unfortunate as the author occasionally uses dyadic notation.

In summary, I feel that this book will make an excellent reference work, but that it may not prove totally satisfactory as an introductory text. Unfortunately, although there are several good textbooks at a more elementary level, the only comparable text is *MHD Instabilities* by Glenn Bateman, which has a much narrower focus.

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Thermodynamics of Irreversible Processes

B. H. Lavenda
182 pp. Halsted (Wiley), New York, 1978. \$27.50

This is a scholarly work, and the author, an erudite theoretical physicist, has read widely and deeply. It is not a textbook. You won't want to pick it up to find out what "irreversible thermodynamics" is all about. If you do, you won't even find out to what physical systems it might be relevant. This book is for the specialist. It may be viewed as a sequel to P. Glansdorff and Ilya Prigogine's *Thermodynamic Theory of Structure, Stability, and Fluctuations* (Wiley-Interscience, New York, 1971). Bernard H. Lavenda, the author, is a product of Prigogine's Brussels school, and he has adopted its style. The Preface sets the tone: "This book is formalistic rather than applicative in character." It sets its goal as making "a definitive statement in regard to the present-day status of linear and nonlinear thermodynamics." This includes resolving "the ambiguity in the meanings of 'dissipation' and 'irreversibility'," as well as in the term "nonlinear" in thermodynamics. Indeed, Chapters 5 through 9—half of the book—deal largely with nonlinear systems and how their thermodynamics differs from the linear. The task of the reviewer here is surely not to criticize the goal. The task is rather to express an opinion on whether the work is correct, whether it is readable and whether it is important.

A quote from the last chapter, "Continuum Thermodynamics," may give a feeling for style and substance:

The reduced power equation then takes the form (Lavenda, 1974)

$$\pi = \frac{1}{\rho}\nabla\cdot(\mathbf{J}_S - \mathbf{J}_D) + \dot{\eta} - 2\phi,$$

$$\dot{\theta} = 0, \ \dot{\mathsf{F}} = \mathbf{0} \qquad (9.2.20)$$

Where we have defined the dissipative flux vector $\mathbf{J}_D$ as

$$\mathbf{J}_D = -\frac{1}{\theta}\mathbf{C}^{(i)}\mathbf{J}_i \qquad (9.2.21)$$

Noting (9.1.9), (9.1.15) and (9.2.3), we can also write the power flux $(\mathbf{J}_s - \mathbf{J}_D)$ as

$$\theta(\mathbf{J}_S - \mathbf{J}_D) = \rho_i\epsilon_i u_i - \mathbf{t}_{(i)}\mathbf{u}_i = \mathbf{J}_E \qquad (9.2.22)$$

This permits us to write the reduced power equation (9.2.20) in the more lucid form

$$\pi - \frac{1}{\rho}\nabla\cdot\mathbf{J}_E = \dot{\eta} - 2\phi \qquad (9.2.23)$$

This is the expression for the thermodynamic principle of the balance of power for the continua under isothermal and isochoric conditions. Expressed in words (9.2.23) states that the absorbed power less energy which flows across the surface of the system appears as the time-rate-of-change of the entropy less the energy which is dissipated in the system.

The reader who can not insert the missing temperature factor spontaneously will have trouble with the rest of the book too.

The early chapters review the Onsager reciprocal relations, including the attack on them by Bernard D. Coleman and Clifford Truesdell. Is the ultimate resolution of the (semantic?) conflict to be found here (page 34)? Writing *homogeneous* instead of *isotropic* doesn't help. Adherents of the school of "rational thermodynamics" would answer no, although their subject gets a whole chapter. Contrasting it with the "Generalized Thermodynamics" (chapter 4) of the Prigogine school is useful, and such a discussion is probably not found elsewhere. The early history of reciprocal relations should surely have cited K. G. Denbigh's little book, *The Thermodynamics of the Steady State* (Methuen, London, 1951). Actually, Denbigh is more successful in imparting physical intuition—the kind that enabled Lord Kelvin to relate the Seebeck and Peltier coefficients eighty years before Onsager.

When a reviewer snipes, a natural question is, how generously does the author cite the reviewer's own work? Here the author gets orchids. He is very generous throughout in giving credit to the two 1953 Onsager and Machlup papers (Phys. Rev. **91**, 1505 and 1512, 1953).

The meat of the book is, of course, the nonlinear thermodynamics. Here the author integrates his own contributions and adds new connections. It took me a while to realize that "half-degrees of freedom" (page 86) are Hendrik B. G. Casimir's α-type variables, and "single degrees of freedom" are the β-type. The next stumbling block was (page 100) the distinction between free and constrained variational principles. Here Lavenda faults Onsager for concluding "that defects that alter the heat flow can only decrease the entropy production under stationary flow conditions, or cause no change." Surely a "crack in the crystal" can decrease the entropy production! What is being held constant in the variation?