Book Reviews

Arthur Holly Compton's Work

The Compton Effect. Turning Point in Physics. ROGER H. STUEWER. Science History Publications (Neale Watson), New York, 1975. xii, 368 pp., illus. $25.

Scientific Papers of Arthur Holly Compton. X-Ray and Other Studies. ROBERT S. SHANKLAND, Ed. University of Chicago Press, Chicago, 1974. xxxviii, 778 pp., illus. $27.50.

As Stuewer says, the Compton effect was a turning point in physics. It forced physicists, among them Bohr and Compton himself, who for years had rejected the concept of the photon, at last to take it seriously. The newly respectable light quantum helped stimulate researches which in one direction led to wave and in another to matrix mechanics. The perplexing simultaneity of the discovery of the alternative forms of quantum mechanics may best be explained by their common concern with problems pointed to by the Compton effect.

The familiar picture of x-ray scattering as relativistic billiards was suggested independently in 1922 by Debye and by Compton. Debye, an early practitioner of quantum theory, had conceived, perhaps as early as 1920, that the light quantum might save the phenomena of x-ray scattering; but he did not publish until encouraged by Compton's report to the National Research Council (1922), which emphasized four experimental results irreconciliable, in Debye's opinion, with electromagnetic theory: (i) the intensity of scattered x-radiation was far stronger in the forward than in the backward direction; (ii) the same asymmetry afflicted electrons set free during the scattering process; (iii) the scattered radiation appeared to be softened, likewise asymmetrically, but more backwards than forwards; (iv) the total scattered intensity could fall well below the minimum calculated by J. J. Thomson on the electromagnetic theory.

Compton came to his discovery not via the brilliant and lucky insight of the practiced quantum theoretician, but through the failure of a dogged and resourceful effort to describe the scattering of high-frequency radiations by the ordinary electrodynamics alone. By 1920 Compton knew all the facts—with respect to γ- and x-rays—which Debye was to regard as intractable difficulties. But Compton then still hoped, as he had for several years, to refer item (i) to the interference of secondary wavelets scattered from a fat, flexible electron, either a shell or a ring, the diameter of which he fixed, after laborious calculations, at about 4×10^{-10} centimeter. Ultimately (iv) also came under this program. As for (iii), Compton explained that it concerned not a truly scattered radiation, in which there is no frequency change, but a new "fluorescent" radiation, which he thought was emitted by electrons ejected by the incident beam and broadcasting at diverse frequencies in accordance with Doppler's principle. Compton then came to worry about (ii), which defeated his ingenuity. He retained the Doppler interpretation of the softening, but now (1921) tentatively employed the detestable quantum hypothesis to calculate the kinetic energy of the "oscillating" electron. A stronger form of the same proposal appeared in his National Research Council report: the electron comes away with velocity $h\nu/mc$ and radiates a Doppler-shifted secondary x-ray. Finally Compton gave up the last scrap of electromagnetic theory, cut out the Doppler stage, and introduced the billiard ball collision. As Stuewer points out, the available x-ray data could not distinguish between the final and penultimate theories; the last step was recommended not by Compton's experiments, but by the force of the quantum theory to which he had surrendered.

Stuewer traces in detail the complicated itinerary that brought Compton to his discovery. He has mercilessly limited himself to what historians of science call "internal history": an account of ideas, not of men and institutions. His purview does not even include instrumental improvements; for although he follows others in crediting Compton's success partly to the excellence of his apparatus, he gives no data to substantiate the claim. Within its severe limits, however, Stuewer's book is quite valuable for its courageous grappling with the often difficult physics, for its firm organization of the myriad details of the papers considered, for its occasional use of Compton's research notebooks and other archival material, and for its many illustrations, graphs, and tables.

Shankland's book consists primarily of reprints of selected papers on x-rays; cosmic rays are put off for another occasion (*1*). All Compton's papers analyzed by Stuewer are present; as Shankland observes, they constitute an inspiring achievement, from which one can learn not only physics, but how to do physics. Shankland also contributes a brief introduction and appendices outlining Compton's x-ray work and the research it directly inspired.

Obiter dicta in Stuewer's analysis suggest that Compton's tenacity owed something to his Midwestern religious upbringing and to a felt need to justify his choice of a scientific over a clerical career. It is curious that Compton thought his education typical of the training of American physicists of his generation. Further undeveloped data in Stuewer's and Shankland's books suggest that American mathematical physics was far stronger before 1925 than is usually conceded. A good intellectual biography of the third American Nobel laureate in physics would be most welcome and instructive (*2*).

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References and Notes

1. There exists an anthology of Compton's nontechnical writings: M. Johnston, Ed., *The Cosmos of Arthur Holly Compton* (Knopf, New York, 1967).
2. A start has been made by J. R. Blackwood [*The House on College Avenue: The Comptons at Wooster 1891–1913* (MIT Press, Cambridge, Mass., 1968)].

An Attempt to Limit Research

Antivivisection and Medical Science in Victorian Society. RICHARD D. FRENCH. Princeton University Press, Princeton, N.J., 1975. xiv, 426 pp. Cloth, $20; paper, $9.95.

What role can historians play in the attempt to understand the complex relationships between science, technology, and society in the present world? Richard D. French, the author of this substantial book on the antivivisection movement in Victorian England, has proceeded upon the explicit premise that his historical analysis can elucidate current issues.

The book has two parts. The first is a narrative account of the antivivisection movement in England from 1870 to the early 1880's. In 1876 the antivivisectionists wrote a bill severely limiting research involving vivisection and brought it to Parliament. Scientists and medical people quickly mobilized opposition that induced government officials to amend the bill in their favor, and it was passed in the

amended form. When administration of the law became oppressive to medical researchers, they reacted by forming a society whose officers negotiated with the Home Office an arrangement that insured unhampered continuation of experiments on live animals. French's account of these developments is dramatic. It is richly documented with vivid quotations from participants in the controversy, and pertinent secondary sources are cited.

The quotations that make French's account so readable may, however, distort the historical situation. The struggle appears to be between equals, but it was not. The antivivisectionists lost abysmally with each major political decision. They had neither the government nor the mass of people nor organized religion on their side. Contrary to the impression conveyed in the more dramatic portions of French's account, the antivivisectionist movement never was effective as a political force.

The book is more than a narrative account of the political and administrative history of the antivivisectionist movement. In the second part French examines social, intellectual, and religious influences in the antivivisection movement. He conducts a sociological analysis of the structure and membership of the movement, including the important role played by women. He also analyzes the objections raised by antivivisectionists to the whole medical profession, the conflicting role of religion in the controversy, and the peculiar anthropomorphic attitude of the British to their pets.

This part deals with very complex issues and is consequently more speculative than the first part. To French's credit, he did not avoid these difficult problems but approached them directly. It is to be hoped that more historians of science will follow his example.

Why does French believe that his historical analysis of the antivivisectionists can help to understand problems of science and society in today's world? He argues justifiably that some of the objections raised by antivivisectionists to organized medicine resemble some objections expressed today. But he also presents two far more ambitious theses for his position. He argues that his case study demonstrates the great complexity of the interaction of science and society, and therefore demonstrates the futility of simplistic or monocausal explanations of such issues. This general conclusion is true and important, but it is already widely recognized by any who study science and society seriously. His second thesis is that today's scientists, by understanding the political tactics of their brethren in 19th-century England, might be able to alter their own tactics to increase political effectiveness. The style adopted by Victorian scientists relied heavily upon direct, nonpublic negotiation with political elites. Although the style has served scientists well, French argues that politicians are now losing "their typical awe of science." He concludes: "Science must begin to develop the kind of bargaining leverage that depends upon the mobilization of individual members of the profession—tactics previously eschewed by science, or at least held at arm's length."

This conclusion suffers from the kind of simplistic assumptions French carefully avoids in his analysis of the antivivisection movement. His argument rests upon the propositions that the West German or United States governments in 1975 have less "awe of science" than did the British Parliament in 1875, that scientists have used the same political tactics over the past century, and that scientists would fare better in the present world if they mobilized and adopted interest group tactics as has the medical profession. Perhaps these propositions are true (I find them dubious), but they surely do not follow from the analysis of the antivivisection movement. Only by assuming such propositions can French make broad connections between his case study and the modern world.

Despite these criticisms, I agree with the initial premise of this book, that historical analysis is essential for understanding the interaction of science, technology, and society at the present and in the future. French chose an inadequate topic for this purpose. As his book demonstrates, the antivivisection movement in England was a special case, more successful than antivivisection movements in such countries as France, Germany, and the United States, but still having only minimal and short-lived effects.

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Behavior of Primates

Primate Aggression, Territoriality, and Xenophobia. A Comparative Perspective. RALPH L. HOLLOWAY, Ed. Academic Press, New York, 1974. xiv, 514 pp., illus. $29.50.

In the introduction to *Primate Aggression, Territoriality, and Xenophobia*, Ralph L. Holloway writes: "We study primate aggression to understand what is and what is not shared by the human animal so that we may have a sane and substantive basis for recommending to society where its programs are inhuman, unjust, stupid, insane, and nonproductive." This collection of review articles and research reports is not going to give anyone a substantive basis for making recommendations to society about anything, however. A few of the papers provide a significant perspective on primate aggression, but most contain very little new or useful information and at worst they degenerate into gibberish.

But should anything more have been expected? Research on aggression in primates is confused, confusing, and often tautological. Both in this volume and in aggression research in general, certain key concepts are treated simultaneously as fundamental principles by which observations are explained and organized and as notions which themselves are in need of verification or explanation. If mating success is used as a criterion for determining a dominance hierarchy in a monkey group, it is meaningless to explain differences in mating success among males on the basis of their position in the dominance hierarchy. Similarly, if the defining characteristic of a "properly socialized" monkey is the formation of stable social relationships, it is tautological to conclude that the function of socialization in primates is to ensure the formation of stable social relationships. The statements in these papers are often worse. What does one make of a conclusion such as "Thus the modification of a display behavior affected the essentially agonistic interactions of the dominance relationships"?

Most of the papers in the first section of the book review aggression in one or more taxa of primates. Sorenson gives a standard species-by-species listing of aggressive behaviors in tree shrews, but does not tell how the catalog of aggressive behaviors was established in the first place. It is an odd state of affairs when behaviors can be described and categorized after the fact but not defined or listed beforehand. Klein and Poirier review aggressive behavior in neotropical and colobine primates, respectively. Both papers, however, contain more assertion than fact, a situation perhaps explained by Klein's conclusion that "quantitative data on any aspect of monkey aggression is lacking despite many hours of observation by many different field workers." It is an open question what kind of data these many observers did collect during their many hours in the field.

A review of aggression in Old World monkeys by Nagel and Kummer is one of the few well-organized and potentially useful papers in the book. The authors state boldly that "aggression in animals is primarily a way of competition, not of de-