BOOK REVIEWS

Cold Fusion Revisited

Bad Science. The Short Life and Weird Times of Cold Fusion. GARY TAUBES. Random House, New York, 1993. xxii, 503 pp. $24.50.

On 23 March 1989 I was drinking beer with a group of electrochemists in a pub at the Massachusetts Institute of Technology called the Thirsty Ear. My companions and I were surprised to see Martin Fleischmann and B. Stanley Pons, colleagues who were well known to all of us, beaming down from their video perch near the bar. The sound on the television wasn't turned on, so we didn't know what they had done that merited two or three minutes on the national news, but we were aware soon enough of their fantastic claim to have produced fusion of deuterium nuclei within the palladium cathode of a simple electrochemical cell. Evidence for cold fusion came in the form of excess heat generation and detection of fusion products: neutrons and tritium. The discovery of room-temperature fusion by scientists whose expertise was well removed from the field of hot nuclear fusion was greeted with admiration and respect as well as skepticism and jealousy. The two camps holding these views came to be known formally as "believers" and "nonbelievers" in the fracas that followed the 1989 press conference. Few members of either group were afraid to expostulate to the scientific journals, newspapers, tabloids, or television news: cold fusion had something for almost everyone. In *Bad Science*, Gary Taubes has written the definitive, if not a fully objective, episodic work on what happened, why it happened, and to whom it happened during a period commencing a few months prior to the announcement of cold fusion and continuing through 2 January 1992 when a cold fusion researcher was killed in an explosion at the Stanford Research Institute.

This is at least the fifth significant publication for the general public spawned by the cold fusion controversy (others being D. F. Peat's *Cold Fusion: The Making of a Scientific Controversy*, Contemporary Books, 1989; F. E. Close's *Too Hot to Handle*, Princeton University Press, 1991; E. F. Mallove's *Fire from Ice*, Wiley, 1991; and J. R. Huizenga's *Cold Fusion: The Scientific Fiasco of the Century*, University of Rochester Press, 1992). It is far and away the best: the others were either rushed into publication and are poorly and hurriedly written or serve primarily as a soapbox. Taubes has done high-quality research into his subject, and he has gotten most of the facts straight and in context. I spoke with ten of the central characters in the book (there were 257 interviews), and all but one thought their opinions were faithfully reproduced: a common statement was "the quote in the book was 80 to 90% correct."

The book begins on 16 March 1989 during a meeting between the president of the University of Utah and his "scientists and lawyers" (always a dangerous combination) where the participants concluded that they had to share the discovery of cold fusion with the world at a press conference a week later. The alternative they feared was to be scooped by physicist Steven Jones, who worked just down the road at Brigham Young University and who Pons and Fleischmann thought had "pirated" their new invention via the peer-review process (this general line of reasoning will be familiar to those who work within the context of peer review). In the meantime, all those present at the meeting agreed to keep the discovery of cold nuclear fusion quiet. Fear and secrecy, which are always anathema to science, are correctly identified in the book as the principal ambience surrounding the birth of cold fusion.

Following the account of the news conference, the book takes us back in time, covering the inception of the idea by both the Utah and the Brigham Young groups, how they came to be aware of each other, how they each dealt with the knowledge that their amazing discoveries not only were not unique but were occurring at institutions separated by only a few miles of the Utah desert. This background material is critical for understanding the next, and most interesting, stage of cold fusion, which commenced with the first shock waves of excitement and continued through the next three months or so of widespread serious research into the phenomenon. This part of the book is the best, and Taubes does an excellent job of immersing the reader in the moment. For example, we are given a good sense of what Pons and Fleischmann thought Jones was doing and thinking during the period just prior to and just after the first press conference, and we are given an equally fair insight into Jones's thoughts. It is at this point that Taubes paints Jones as an innocent bystander and Pons and Fleischmann as overly aggressive and paranoid, characterizations that pervade the rest of the book. This oversimplification of these characters detracts somewhat from the book as a serious historical reference, but it certainly helps keep the reader intrigued: good and evil are clearly defined here.

In the second part of the book, which is aptly subtitled A Collective Derangement of Minds, many events occur quickly and simultaneously. Although Taubes organizes the first part of his book chronologically, he uses a clever literary device in the second part and recasts the account around the principal geographic centers of cold fusion activity. Taubes guides us through the centers of believers and nonbelievers, and we experience how their perspectives change during the months after the press conference. Taubes does an excellent job of describing the defining moments of cold fusion in parallel: we really understand the relationships between these events and the personalities that drove them, and we come away with a sense of understanding who did and said what when. We move between the main players at the big-name institutions (MIT and Caltech) and, as Taubes paints

Prices of Books

Average per-volume prices of books reviewed in *Science* 1988–1993. The average prices per page for the technical books in the natural sciences for the years covered were 16.1¢, 16.9¢, 17.8¢, 17.0¢, 17.2¢, and 17.2¢. (Data are for hard-cover books except where books were available only in paperback.) For earlier data from *Science* and other relevant information see *Science* **211**, 933 (1981); **235**, 95 (1986); **239**, 81 (1987); **243**, 99 (1989).

Category	Price (dollars)					
	1988	1989	1990	1991	1992	1993
All books	54.05	54.58	54.43	54.08	57.58	51.21
Technical books in natural sciences	71.70	73.73	75.57	73.19	76.78	79.00

them, the somewhat suspect universities in the provinces (Utah and Texas A&M). This prejudice may reflect the author's pedigree, which includes Harvard, Stanford, and Columbia. There are also excellent insights into the goings-on at some of the innocent-bystander institutions (the University of Texas, Harwell, and several others). The rush to get details about the cold fusion experiment by phone, fax, and e-mail is captured in prose that definitely gets across the excitement of the times: this frenetic section of the book authentically chronicles the "fog of fusion." The dramatic announcements at Texas A&M, Stanford, and Georgia Tech that confirmed Pons and Fleischmann's results are described in detail.

Following the initial excitement, the somewhat sad part of the story begins to unfold. This includes retractions by many of those who initially reported positive results. New negative reports also cast a pall over cold fusion: we hear about a particularly damning report from a group at MIT, published in *Nature* (the unofficial journal of cold fusion) in May 1993, that definitively proved that the neutron experiments originally reported by Pons and Fleischmann were poorly executed and incorrectly interpreted. When confronted by this result in person at a scientific meeting held in Los Angeles, Fleischmann admitted the results were incorrect. Now, without a single reliable report of fusion products and only excess heat to hold on to, many advocates of cold fusion transmutated from believers into nonbelievers: the death of serious, widespread cold fusion research was at hand. Taubes reliably covers this period during which the scientific community came to its senses: enthusiasm turns to anger, and the charges range from ineptitude to fraud.

The third part of the book covers the extended death, funeral, and mourning of cold fusion. Here both the scientific community and Taubes are a bit guilty of piling on after the play is dead: after being effectively immersed in the cold fusion drama, most readers will find the epilogue a let-down.

To summarize, the first 400 pages of *Bad Science* are good reading. In tone Taubes's effort falls somewhere between those of Richard Rhodes (*The Making of the Atomic Bomb*) and of Hunter S. Thompson (*Fear and Loathing on the Campaign Trail*). This book is, in the jargon of pulp fiction, excellent beach reading, and it indisputably proves the wisdom of Andy Warhol, who said, "In the future everyone will be world famous for fifteen minutes."

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Telescope Builders

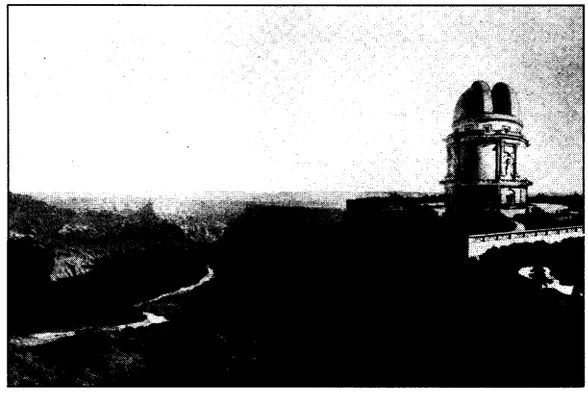
Pauper and Prince. Ritchey, Hale, and Big American Telescopes. DONALD E. OSTERBROCK. University of Arizona Press, Tucson, 1993. xviii, 359 pp., illus. $45.

George Willis Ritchey (1864–1945) is the pauper of the title of Donald Osterbrock's book and George Ellery Hale (1868–1938) the prince. Together they did indeed play large roles in both the building of the biggest telescopes and in defining the early research programs these made possible. Hale's story, however, is much better known than Ritchey's. This book is intended to rectify this situation.

Hale was a rich boy, with all the advantages of privilege: private schools and tutors, spectrographs and telescopes. In his teens Hale became enthusiastic about physical astronomy; his father bought him the very best equipment and built him an observatory. He attended MIT and, before graduation, met the most important instrument-makers and astronomers of his day: John A. Brashear, Samuel P. Langley, Henry A. Rowland, and others. Combining his privilege, ability, and perseverance, Hale readily pushed toward his goal of becoming an astrophysicist.

Ritchey had similar doggedness but lacked the rich father. His family immigrated to America from Ireland in the mid-19th century and made its way by means of farming and small industry in the Ohio River Valley. His father owned a successful furniture factory and then went bankrupt. In and out of factory jobs, Willis Ritchey attended the University of Cincinnati, then free to city residents. He never finished university. Instead, he taught woodworking at a Chicago trade school and made his first telescopes in his basement. In 1891, Ritchey met Hale through the Chicago Section of the Astronomical Society of the Pacific. Within a few years he was on Hale's payroll, funded by Hale's father, as a full-time "optician."

"Carriage on Mount Wilson road, 1907." From left to right are the driver, George Ellery Hale, George Willis Ritchey, and John D. Hooker. [From *Pauper and Prince*; courtesy of the Observatories of the Carnegie Institution of Washington]



"Ritchey's composite drawing and photograph of a fixed vertical telescope at the edge of the Grand Canyon." [From *Pauper and Prince*; courtesy of the Carnegie Institution of Washington]

Why write a book about Willis Ritchey? This is more than just another Horatio Alger story. Osterbrock calls Ritchey "the leader of the revolution in concept and scale" that produced the big telescopes. This is a story of a neglected side of science: the instrumentation that fosters and delimits scientific research. Osterbrock provides one of the most extended examinations to date of an instrument-maker's life and work. To understand the success and character of science in America, we must understand the instrumental infrastructure that made it possible. To know the history of theory or of discovery is not enough on its own.

Pauper and Prince has both strengths and weaknesses. Osterbrock has made extensive use of archival materials and of oral-history interviews. Clearly he also based his narrative on a strong knowledge of relevant primary publications and on his own experience as an astronomer. Discussion of relevant secondary literature is, however, thin. The manuscripts provide a fine-grained view of Ritchey's character and of his approach to the design and construction of sophisticated instruments. Much can