

BOOK REVIEW

Selection of books for review is based on the editor's opinions regarding possible reader interest and on the availability of the book to the editor. Occasional selections may include books on topics somewhat peripheral to the subject matter ordinarily considered acceptable.



A Dialogue on Chemically Induced Nuclear Effects: A Guide for the Perplexed About Cold Fusion

Author Nate Hoffman

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Reviewer Bruce Lewenstein

Toward the end of Nate Hoffman's review of the cold fusion saga, he compares the saga to Edgar Allan Poe's "The Tell-tale Heart" story: The protagonist is sure he has killed his victim and interred the body, but he still hears the beating heart, and it drives him crazy. Critics of the 1989 announcement by Fleischmann and Pons that they had found a method for inducing nuclear fusion at room temperature with tabletop electrochemical techniques must feel the same way. No matter how hard they try to kill cold fusion, they keep hearing its muffled heartbeat. Hoffman's book explains why cold fusion will not die.

Hoffman has had a unique position throughout the saga. An expert on alkali metal technology, he has worked for Rocketdyne (a division of Rockwell International) since 1955. He has acquired substantial experience in fusion and fusion-related issues. When Fleischmann and Pons made their dramatic announcement in March 1989 at the University of Utah, the Electric Power Research Institute (EPRI) asked Hoffman to help EPRI evaluate the various experiments that they subsequently funded. Never directly associated with any of the major research teams in the area, Hoffman was nevertheless privy to much of the detail and data they produced. He has used his unusual level of access to produce an idiosyncratic and enlightening book.

The book is neither a traditional technical explication (which does not exist in textbook form for cold fusion) nor a conventional overview [such as Taubes's journalistic *Bad Science: The Short Life and Weird Times of Cold Fusion* (Random House, 1993) or Huizenga's *Cold Fusion: The Scientific Fiasco of the Century* (Rev. ed., Oxford University Press, 1994),

written by the chair of the U.S. Department of Energy's cold fusion review panel]. Instead, Hoffman's book is modeled on some classic texts in solid-state physics by Hume-Rothery, written in dialogue form. Hoffman has the same "Old Metallurgist" and "Young Scientist" engage in an extended conversation about cold fusion, the technical questions it raises, the available information to resolve those questions, the uncertainties remaining in the field, and various related bits of technical or related information. The result is a fascinating exploration of the development of knowledge in a rapidly moving and theoretically and experimentally challenging field.

Hoffman's chapters cover neutron measurements, natural radioactivity, artifacts associated with changes on the palladium cathode surfaces, heat measurements, statistical considerations, and the overall assessments of "anomalous nuclear effects in deuterium/solid systems." In most chapters, he is primarily concerned with the artifacts that bedevil cold fusion, making clear why any simple statement that a given result is true or false is extremely difficult to make. Most researchers, he makes clear, have simply not considered in sufficient detail all the possible sources of error.

On one hand, such a conclusion feeds the suspicions of most cold fusion critics that the field is riddled with error and should be completely abandoned. But, Hoffman is not so sure, for, as he points out, errors and oversimplifications suffuse the negative experiments and explanations as well. What is more, the continued observation of apparently anomalous results will not go away; scientists have an obligation, he believes, to explore and explain those results. His appendixes help provide background for those explorations, covering the palladium-hydrogen and titanium-hydrogen phase systems, concrete/heavy water systems, nuclear reaction products, helium measurements, heavy water neutron flux background, and branching ratios for deuterium + deuterium fusion at low energies. Several of the chapters and especially the appendixes contain verbatim reprints of memos or reports issued during the saga (1989–1991), an especially valuable technique for historians trying to understand who knew what and when.

While Hoffman's focus is the technical complexity of cold fusion, he also makes clear some of the psychological and sociological conundrums that complicate the field. In the midst of a discussion of radioactivity artifacts, for example, the Old Metallurgist stresses that "Mind-set determines how any scientist accepts data as real." Then he asks the Young Scientist

why he is so positive that the data are unreal. The Young Scientist replies, "That is a very interesting question. I can't say anymore that it would violate my model of nuclear physics reality. But my mind is really set against believing it . . . and I guess that is your point." Indeed it is. Glen, a geophysicist and historian at the U.S. Geological Survey, made a similar observation after conducting detailed interviews with participants in the debates about the causes of the mass extinctions evident in the geological record. He can predict their beliefs based on their prior intellectual commitments, as indicated by their scientific subdiscipline. (See W. Glen, Ed., *The Mass Extinction Debates: How Science Works in a Crisis*, Stanford University Press, 1994.)

The book is not without flaws: There are a number of typographical and minor historical errors, which especially bothered me as I am a historian. I am not a chemist, physicist, metallurgist, or practitioner of any other technical field relevant to determining whether cold fusion is "true," so I cannot identify similar minor errors in the technical material or pass judgment on Hoffman's technical explanations. But, I found his book useful for explaining some of the technical issues in cold fusion. It contains substantially more detail than any of the news reports or other books on cold fusion; at the same time, its dialogue form allows it to provide more background explanation than most technical papers.

Cold fusion is, however, still an evolving field. A small group of researchers continue to explore cold fusion experiments, with some (including Fleischmann and Pons) taking the original heavy water approach and others experimenting with a light water apparatus, that appear (on theoretical grounds) to be even more fantastic than the original Fleischmann-Pons experiments. Hoffman's book concentrates on the early period in the saga (though he has some information through 1995) and predominately looks at the heavy water experiments. Thus, it cannot be considered an absolutely complete explanation of the science of cold fusion. But, it is likely to be useful to those trying to understand the technical issues active in the field in its early years, why a clear answer proved so elusive then, and why some people believe a clear answer remains unavailable today.

Bruce V. Lewenstein is an associate professor at Cornell University in the Departments of Communication and of Science and Technology Studies. He is the director of the Cornell Cold Fusion Archive, a publicly available resource for scholars studying the development of the cold fusion saga.

Author's Response to Book Review

I greatly appreciated Professor Lewenstein's in-depth review of my book. Because he is surely the top authority on "cold fusion" history and because I owe my readers absolute historical accuracy even on minor items, I asked Professor Lewenstein to list all the minor historical errors he found in my book. Here is his list:

"p. 11: Moshe 'Guy'—Usually, his name is listed as Moshe 'Gai.' I realize that part of the problem is a transliteration one, but I believe his technical articles in English use the 'Gai' spelling.

"p. 13: 'several sensational articles' by Taubes. My reading of the evidence says there was only *one* article by Taubes, on 15 June 1990. Also (but this is clearly interpretation and wasn't what I was directly referring to when I said I was concerned about minor errors), I don't have quite as conspiratorial view of Taubes as you do; in his book, for example, he *does* acknowledge that he was the author of the *Science* article (p. 410), although I'll grant that he generally refers to *Science*, not himself.

"p. 15: you refer to Bockris's 'uneasiness' about Champion. Again, this was a matter of interpretation, but my own notes about conversations with Bockris at the Nagoya meeting suggest that he was quite enthusiastic about the possibilities in Champion's work.

"p. 17: you call Frank Close a 'public relations expert.' He's certainly a widely published author (in terms of his 'popular science' works) and, in the small world of British science, knows all the reporters. But, from my position teaching in a department where we send over half our students into the public relations field, he's not a PR expert."

My responses to these questions/comments are as follows:

1. With respect to Moshe Gai, Professor Lewenstein is absolutely correct.

2. With respect to Gary Taubes, his brilliant but dark writings on cold fusion prior to his book, including his input to the Texas universities, make him more than an objective reporter on the subject, in my opinion. I consider him a major player in the cold fusion story. Professor Lewenstein's point that Gary published just one article on "cold fusion" prior to his book is a surprise to me, but his interviews and interactions within the "cold fusion" community were important.

3. Professor Bockris's attitude toward Champion is perfectly described by the word "uneasiness," in my opinion.

4. With respect to Frank Close, Professor Lewenstein may know things that I do not, but to me, this well-known scientist has a remarkable ability to explain science to reporters in a way that allows the information to be transmitted undistorted. He also writes about complex science in a manner that is understood clearly by the non-scientist. This makes Frank Close a public relations expert in my mind, of a type much needed by the scientific community. I suspect the difference between Professor Lewenstein and myself on this issue lies in our definitions of "public relations expert."

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