[1] Fleischman — among other achievements — is a fellow of the Royal Society and a Director of the Max Planck Institute in Berlin.

The Rebirth of Cold Fusion: Real Science, Real Hope, Real Energy by Steven **B. Krivit** and Nadine **Winocur**. Pacific Oaks Press, 2004. xxii + 298 pp. $25.95 (paper) (order at http://www.newenergytimes.corn/). ISBN 0-9760545-8-2.

This is an excellent survey of the current status, history, and implications of "cold fusion". Given the range and substance of the book, it is surprisingly easy to read, even a page-turner as some of the stories unfold. For the general reader, the necessarily technical bits of science — which are only a few — are explained lucidly and accurately. For scientists who have not followed this topic, the book tells what they should know, namely, that these extraordinary new explorations have been given too short shrift by the mainstream.

The general problem for "cold fusion" will be thoroughly familiar to anyone who has encountered a phenomenon that seems inexplicable and occurs capriciously. The mainstream discourages rather than encourages risk-takers to explore whether the capriciousness can be overcome, and so it may take much longer than would otherwise be necessary to ascertain whether or not the phenomenon is real. In the case of cold fusion, this book notes how casual yet effective the official discouragement may be: Acknowledgements that some crucial experiments seem sound are not accompanied by any recommendation that further study is desirable (pp. 109, 136–137). The whole scientific community absorbs, through a sort of osmosis, that "cold fusion" is iconic of the unreal (p. 146). Reviewers of manuscripts give silly, invalid, improper reasons why papers should be rejected (for instance, that reported data are not accompanied by a final explanation, p. 124; that a paper on electrochemistry is not "a good match" with the Journal of the Electrochemical Society, p. 128).

The scientific mainstream remains perpetually ignorant of crucial bits of its own history: that the most significant advances are typically resisted or ignored (Barber, 1961; Hook, 2002; Stent, 1972) and that the ability to reproduce an experiment may depend on factors yet to be discovered — it was three decades before the relevant observations of semiconductors could yield workable transistors because the effects appeared only capriciously, owing to the need for an unprecedented degree of purification. Funding agencies — with very few but noble exceptions like the Defense Advanced Research Projects Agency (DARPA)— have not come to grips with the fact that it is precisely the far-out ideas that need to be explored if genuine advances are to stimulated; devoting 5 or 10% of research funding to unorthodox approaches would pay off at least as handsomely as the touted "spin-offs" used to justify otherwise unjustifiable ventures.

For these commonplace reasons, it is taking a couple of decades or more for the several relevant disciplines to become aware that the "cold fusion" announced by

Fleischmann and Pons in 1989 is not only real but actually just an early glimpse of "low-energy nuclear reactions" (LENR): nuclear transformations appear to be accessible by means other than those of high-energy particle physics. *The Rebirth of Cold Fusion* points to the accumulating evidence that deuterium fusion through electrolysis at palladium seems to be a harbinger, a clue, and not a *unique* and therefore doubly unbelievable occurrence. The range of materials that seem able to deliver LENR indicates the need for intensive, wide-ranging studies of novel materials and their properties: alloys, mixtures of metals and non-metals, thin films. Theoreticians need to be closely involved to suggest potentially fruitful follow-up studies guided by the clues offered by experimental successes.

The Rebirth of Cold Fusion begins (Part One) by setting the possibility of energy-producing LENR in context, mentioning fossil fuels and global warming; the "overextended promise" of conventional "hot" nuclear fusion, with a telling comparison of hot and cold fusion as an appendix (p. 258); the fallacy of a "hydrogen economy"; and distant promises of exotic new energies such as zero-point energy. Emphasized is the often-overlooked facts that measuring heat is anything but straightforward (p. 7) and that "a cold fusion experiment is infinitely complex ... [because of] a multitude of electrical, chemical, material science, metallurgical and time variables" (p. 5).

Part Two of the book lends historical perspective. The chapter on "Con-Fusion" shows concisely how the experimental expertise of distinguished and competent electrochemists weighed little as against the authority of theoreticians of nuclear physics. "False Debunking" describes how inadequate, how invalid were the hasty experimental trials that led, within a few months, to the mainstream consensus that Fleischmann and Pons must have been mistaken. When the late Gene Mallove and Mitchell Schwartz took to MIT's president the evidence that data from MIT experiments had actually shown excess heat, the president asked a respected physicist to review it, who agreed that a heavy-water (deuterium oxide) cell had generated more heat than the control, light-water cell; nevertheless, no official public acknowledgement of this fact has issued from MIT (chapter 14).

Part Three, "Discoveries and Mysteries", surveys what has happened in the field over the past 15 years: gains in the ability to reproduce the generation of excess heat and various nuclear phenomena—the production of tritium, helium, neutrons, and a variety of atomic species through nuclear transformations. The scandalous persecution of John Bockris at Texas A&M and his smearing by journalist Gary Taubes are described. A typical anecdote recounts the hubris of the eminent scientist who demanded that his opinion be deferred to because "I am the second most-cited chemist in the world" (p. 235). Being a chemist myself, I can confirm that no scientific discipline has a more naïve view of the significance of citation frequency than does chemistry.

Part Four has some summaries: "Threats to academic freedom", "National security", "Lessons learned", "Speculation about the future". Among these, "national security" seems rather weak: the argument that the United States should do certain science because otherwise it will be left behind by other nations is too questionable to stand as a bald, unargued claim, even though the same sort of bald claim from the government worked quite well during the Cold War. There are a few other weak points in the book, such as inconsistent formatting and other copy-editing glitches in the Endnotes. The only substantive deficiency that I truly regretted is the inclusion of Appendix C, which presents the hypothetico-deductive method as exemplifying how good science is done. Unless it is recognized that this method is a myth, and that science is through-and-through a human activity, one cannot understand how something like the cold-fusion business can come about, still less can one sense how such occurrences might be avoided or ameliorated. But Krivit and Winocur are far from alone in having succumbed to this myth, purveyed as it has been for so long by so many who—since they assume the mantle of knowledgeable—really ought to know better; fortunately, there is nothing in the text of the book itself that hinges on the nature of scientific method.

This very good book by two non-specialists joins two other good books about this business written by non-specialists: Gene Mallove (1991) and Charles Beaudette (2000). All are tributes to what can be accomplished by determined truth-seekers curious about what would seem to many an impossibly technical field to examine. By contrast, the books by physicists Huizenga (1992) and Close (1991) and science journalist Taubes (1993) should be avoided, except by sociologists interested in pathological skepticism and resistance to discovery.

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