

ago I was going to observe right **after** sundown. The rule of thumb is "Do not start an exposure until 40 minutes after sunset." Experience has told me that you can start when you can see a 9th or 10th magnitude star drift through the field. My way of deciding when to start is to open up and let the telescope sit near the meridian unclamped, *i.e.* not tracking. About 25 minutes after sunset I start to glance through the eyepiece and when I can see an appropriate star drift through I start work. Imagine my surprise when I saw what appeared to be a very faint star just sitting there. It stayed in the field of view and at a declination of about -9° . I had to move off and take my plates on the object I was interested in. As soon as I finished I tried to reset the telescope, drive **off**, on the meridian at exactly the same declination, an almost impossible task since the eyepiece views only a few minutes of arc and the declination scale is only good to a few minutes of arc. Also, the telescope may not have been exactly on the meridian. I never recovered the mysterious star, but I figured out what it must have been—a geosynchronous communications satellite.

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Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor by Eugene F. Mallove. New York: John Wiley & Sons, 1991, xviii + 334 pp.

This is the third, and by far the best book published about "cold fusion"; indeed, it is the only one that can be unhesitatingly recommended. Its **virtues**—absent from the earlier two (by David Peat and by Frank Close)—include that it is well written and produced and that it sets cold fusion into proper contexts: the pre-history of anomalous results with palladium and deuterium, the search for controlled "hot" (thermonuclear) fusion and the need for such sources of energy, and the nature of scientific activity.

One important caveat is that Mallove is an unabashed enthusiast about the reality of cold fusion, and his judgment may not be correct. However, his declared belief does not seem to hinder him from giving a balanced account on the significant points. Sociologists of science may find interest in comparing Mallove's account—that of an engineer turned writer—to that of Close, a physicist who also writes popular science: it is not uncommon for engineers to accept the reality of phenomena that are not yet understood, as it is very common for physicists to disbelieve the reality of phenomena that seem to

contradict contemporary beliefs of physics. Mallove's book emphasizes the positive where Close's emphasized the negative. Mallove writes more sympathetically about Fleischmann and Pons than about Steven Jones whereas Close lauded the latter and stigmatized the former. Close quoted Jones's cautions about commercialization, whereas Mallove quotes Jones's forecast (written in 1987) that muon-catalyzed fusion offers commercial possibilities. Mallove does not give as much detail as did Close about the early contacts among Fleischmann and Pons, the Department of Energy, and Steven Jones at **Brigham** Young University, which help understand much about the initial furor (see my review of Close's book in vol.6 no. 1; this is the only point on which Close is worth reading, by comparison with Mallove).

Unlike the earlier books, this one tells the story in chronological fashion, and it makes interesting and at times exciting reading, after the first two chapters which give a rather slow (but necessarily so) start by setting the context of the need for fusion energy and the history of attempts to harness it.

The next three chapters tell of the press conference and its immediate aftermath. Mallove points out—plausibly and enlighteningly—that Jones, being a physicist, naturally looked to measurement of neutrons as the way of detecting fusion, and he naturally expected muon-catalyzed or solid-state induced "cold" fusion to follow the same mechanisms as fusion under "hot" conditions; whereas the chemists made no such assumptions and simply (or naively or simplistically) thought that the palladium lattice might somehow bring fusion about in which case much energy would be released that could be measured as heat. Mallove gives other examples too that show how significant are the differences between chemists and physicists and how that continues to play a role in this controversy. For example, physicists were prepared to accept the reality of "low-energy cluster-impact fusion" (but not that of electrochemical cold fusion) on the basis of a single report (p.176).

It is reassuring for those who think highly of Pons to be reminded (p. 59) that as early as 5 days after the press conference he had pointed out publicly that he and Fleischmann were postulating not deuterium-deuterium fusion but some unknown fusion process, possibly involving lithium. And enough detail is given (in later chapters too; see p. 174) to make plain how biased *Nature* has been in its coverage and attitude.

Chapter 6 is a concise but very welcome summary of history without which one cannot have a balanced view of the matter. That palladium catalyzes the burning of hydrogen in air allowed one critic to quip that "Fleischmann and Pons have rediscovered a 150-year-old German cigarettelighter". Much more significant is the reminder that in the 1920s Paneth and Peters thought they had fused hydrogen into helium; and how intriguingly curious that Fleischmann and Paneth were in the same department at the University of Durham in the 1950s. (It surely speaks highly of Mallove's research for this book that he knew of this.) And then there was Tandberg's attempt in the 1930s to fuse deuterium. How plausibly Fleischmann came to his ideas is well described, including his work with palladium electrodes in the 1970s. It is also fascinating

to read of the precursors to Jones on muon-catalyzed fusion: **Andrei Sakharov** and F. C. Frank (independently) in the 1940s, rediscovered by Luis Alvarez in 1956.

Chapter 7 explains beautifully for the layman the essential features of the theories proposed by Hagedorn and by Schwinger. Connoisseurs of the resistance by mainstream science to genuine novelty will add to their examples that the manuscripts of the Nobelist Schwinger were rejected by *Physical Review Letters* in terms of such "calumny" that Schwinger resigned from the American Physical Society (some other examples of resistance are given on page 286).

Chapters 11 and 12 are perhaps the weakest in the book. Apparently intended as an interim summary, they argue rather stridently for the reality of cold fusion without the benefit of chapter or verse of evidence. But then chapter 13 again takes up the interesting story and carries it into early 1991. Perhaps the weakest point here concerns funding: Mallove is guilty of special pleading (pp. 255 ff.) as he argues for funds without drawing on any general understanding of how various sorts of scientific research are traditionally and in practice funded. In chapter 18, too, the discussion of funding is not convincing: that one B-2 bomber costs three times as much as the proposed magnetic fusion research is just an out-of-context remark—governments and institutions simply don't compare every single item against every other when drawing up a budget. (On the other hand, perhaps such *ad hoc* comparisons should be publicized more frequently in order that budgets be constructed more rationally.)

The last few chapters seek, rather successfully, to use the cold-fusion episode to illustrate salient features of scientific activity, and here Mallove displays a **gratifyingly** authentic general understanding (which was missing from his discussion of funding). Chapter 17 in particular is accurate about resistance to novelty, the role of authorities in science, the danger of analogies at the same time as history of science is worth knowing, the respective primacies of theory and experiment, the nature of peer review, the fear of error being interpreted as deliberate attempts to mislead. However, the intended meaning of the little sections on Ockham's razor, on vested interests, and on wishful science is obscure; and the discussion of pathological science is a bit **fuzzy**—perhaps inevitably so in such a brief compass.

On the matter of pathological science, Mallove is not above an *ad hominem* swipe at Irving Langmuir's own pet project of cloud-seeding (p.282). Mallove's summing-up argument for cold fusion, that although none of the present evidence "makes an ironclad case. . . . Rather it is the totality of the anomalous phenomena", is uncomfortably reminiscent of arguments for the reality of extrasensory perception or psychokinetic effects, that although every instance has some evidential flaw, nevertheless so many claims have been made that there must be something real behind them. But Mallove is right on the mark in pointing out that elite scientists are so often right about novel, anomalous claims only because they are conservative and, in science, conservatism turns out to be warranted most of the time; it is not that elite scientists are

particularly good at picking those startling claims that will turn out to be warranted, and so their opinions on any given claim deserve no special attention.

This is a meaty book, a well done book—a remarkably well done book to appear so quickly on such a messily controversial topic.

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