Fusion: The Evidence Reviewed

Fire from Ice. Searching for the Truth Behind the Cold Fusion Furor. EUGENE F. MALLOVE. Wiley, New York, 1991. xviii, 334 pp., illus. $22.95. Wiley Science Editions.

The author of *Fire From Ice: Searching for the Truth Behind the Cold Fusion Furor* minces no words in his preface:

> After reviewing mounting evidence from cold fusion experiments, I am persuaded that it provides a *compelling* indication that a new kind of nuclear process is at work. I would say that the evidence is *overwhelmingly* compelling that cold fusion is a real, new nuclear process capable of significant excess power generation. The evidence for significant power generation, however, cannot be said to be *conclusive*. . . . There is yet no *proved* nuclear explanation for the excess heat. That excess heat *exists* is amply proved.

Thus does Eugene F. Mallove, at the time of writing his book chief science writer for the MIT News Office, with advanced degrees in astronautical engineering and environmental science, state the point of his book.

On 23 March 1989, in an unforgettable press conference at the University of Utah, two well-known electrochemists, B. Stanley Pons and Martin Fleischmann, reported that electrolysis of a lithium solution in heavy water, with palladium cathode and platinum anode, produced neutrons, tritium, and large amounts of heat, all from the fusion of deuterium nuclei. Within days, other groups performed similar experiments, and many reported confirmation of one or another product. Within a month, a group at Frascati, Italy, reported large numbers of neutrons from "dry fusion," induced by allowing titanium chips or lathe turnings immersed in deuterium gas to warm from liquid nitrogen temperature.

Thus began a frenzy, worldwide, to reproduce, to enhance, and to understand these results, which fit not at all the vast knowledge of nuclear physics and of nuclear fusion. Mallove notes the 1956 demonstration by L. W. Alvarez *et al.* of deuterons fusing in less than a microsecond when bound in a molecular ion some 200 times smaller than normal by the action of a negative muon 207 times heavier than an electron, which is thus able to shield the Coulomb repulsion of the deuteron beyond the tiny Bohr orbit of the muon. With the example of muon-catalyzed fusion, it was natural to express the reaction rates needed to account for the heat claims of Fleischmann and Pons, or the neutron emission rates of Steven E. Jones *et al.*, as corresponding to "heavy electrons" of mass 10 and 5 times that of a normal electron, respectively; but it was clear from the beginning that the "high effective mass" of electrons in some conducting solids was a concept irrelevant to the close binding or shielding of deuterons. If the cold fusion phenomenon were real, its explanation would likely involve some coherent phenomenon that would increase the reaction rate by a factor involving the number of deuterons in some macroscopic region, but no such proposal has persuaded anyone other than its author. Julian Schwinger and Peter Hagelstein (of x-ray laser fame) have been active in advancing theories of cold fusion, Hagelstein "(by his own count) dozens of versions." Whether cold fusion is real or not, almost all of these theories must be wrong; though we should remember that the Schroedinger and Heisenberg approaches to quantum mechanics turned out to be equivalent!

> The priorities were clear: (1) prove that there was indeed a new effect; (2) achieve consistent reproducibility; and (3) discover the mechanism of the phenomenon (a).

Where do we stand on these priorities? Scores of "positive results" are cited in Mallove's book, and many have appeared in the scientific literature; many of those results have themselves proved incapable of replication.

Mallove writes,

> Some theorists refused to believe the results of experiments which their theories could not explain—in particular the finding of excess power and energy without readily apparent nuclear products to account for it.

He quotes Pons: "I think that you need to consider first that the experimental data must be duplicated and explained, and then a theory put forth, rather than saying your data must be wrong because the theory doesn't predict that." And P. W. Anderson: "the most important experimental results are precisely those that do *not* have a theoretical interpretation." Just so; and the problem is that the discoverers have had great difficulty in duplicating their own results, arguing that experiments must be carried out for much longer times and under different (and changing) conditions from those of the first publication. If a scientist fails to achieve "success," it can always be argued that the conditions were not right, or that he or she gave up too soon.

Mallove characterizes me as "a skeptical theorist . . . but technological, not scientific debunking, had been his forte." Actually, my successes have been in experimental physics and instrumentation, and the author has missed the experiments and analysis with my IBM colleague James L. Levine, 1972–1974 that clearly demonstrated that claims of detection of gravitational radiation in a mechanical resonator had been erroneous. The late Bernd Matthias and I had some influence on the experiments at Los Alamos that helped to show that "polywater" was a concentrated solution of impurities in ordinary water—too bad, but it explained how the oceans of the earth had managed to avoid a transition to polywater.

Sometimes theory leads to an advance in understanding, before experiment; but for most of us, experiment is *the* source of discovery and of advance, even when not explicable by theory. I would have no problem with "remote viewing," with polywater of the 1960s and 1970s, or with other astonishing concepts, if only they were demonstrable and durable. But as we have learned the hard way, a researcher must challenge his or her own results, and only if the results persist under every manipulation that "should make no difference" can they be claimed to be correct. Not in this mold, for instance, is the statement according to Mallove:

> Martin Fleischmann told me in 1991, "I'm absolutely 100 percent sure that there was a difference in the gamma-ray spectra between blank and measured, in our measurements. I'm sure that is correct. But *why* that was so is not clear."

In preparing this review I have visited several cold-fusion investigators and talked with others, all of them figuring importantly in the book. Earlier, in July 1989, with the DOE Cold Fusion Panel, I visited the Stanford University lab of Robert Huggins, quoted by Mallove as having laboriously recalibrated the cell his group used "for every data point in every sample at every time." I found the data and the data analysis to be far from that standard. In June 1989, I visited the group at Frascati that had on one weekend detected first large numbers of neutron bursts and on the next many hours of nonburst neutron emission from dry fusion; no such significant results have been attained since by that group. More generally, not once in any panel visit were we shown an apparatus that was even claimed at the time to be producing results.

At Los Alamos, in collaboration with the other pioneer of cold fusion, S. E. Jones, Howard Menlove has been attempting to obtain more reliable evidence of cold fusion.

But it is fair to say that two years after his first publication his sample success rate is less than it was a year before, despite many improvements in apparatus, shielding, and (supposedly) understanding.

The book presents results by B. Y. Liaw, P.-L. Tao, P. Turner, and B. E. Liebert on a molten-salt cell containing LiD as providing a power excess of 25 watts for 1.68 watt of input electrochemical power—a 15-fold excess. But not emphasized is the input power of 69 watts to the heater; if that input is taken into account the "excess" is a troubling 40 percent rather than an astounding 1500 percent. I bet against it as a demonstration of cold fusion.

"Tom Droege, a superb engineer who has built state-of-the-art instrumentation for the particle physicists at Fermilab, now . . . perfects an extraordinary calorimeter," Mallove reports. Indeed, and in the process Droege has identified and overcome many problems that must have afflicted less cautious workers. At present, with electrolytic power input of some 1000 milliwatts his sensitivity is about 1 milliwatt, with no clear indication of net excess heat. Those who claim to know how to treat their cathodes to obtain excess heat would do well to adopt Droege's apparatus.

Despite Mallove's contention that cold fusion is not a member of the class defined by Irving Langmuir as "pathological science" and his recommendation that Langmuir's rules for identifying such be "retired to the junk heap," I believe that cold fusion is more likely than not to be an example. Still, if anyone can show me a history and demonstration of strong, reproducible, emission of neutrons, tritium, or heat in a cold (or dry) fusion cell, I will not only urge support but repeat the experiment.

Mallove captures the flavor of the vigorous verbal exchanges at scientific meetings and reports some valuable clarifications. *Fire From Ice* is written in a lively fashion and provides interesting glimpses of the personalities and concepts involved in the cold fusion furor.

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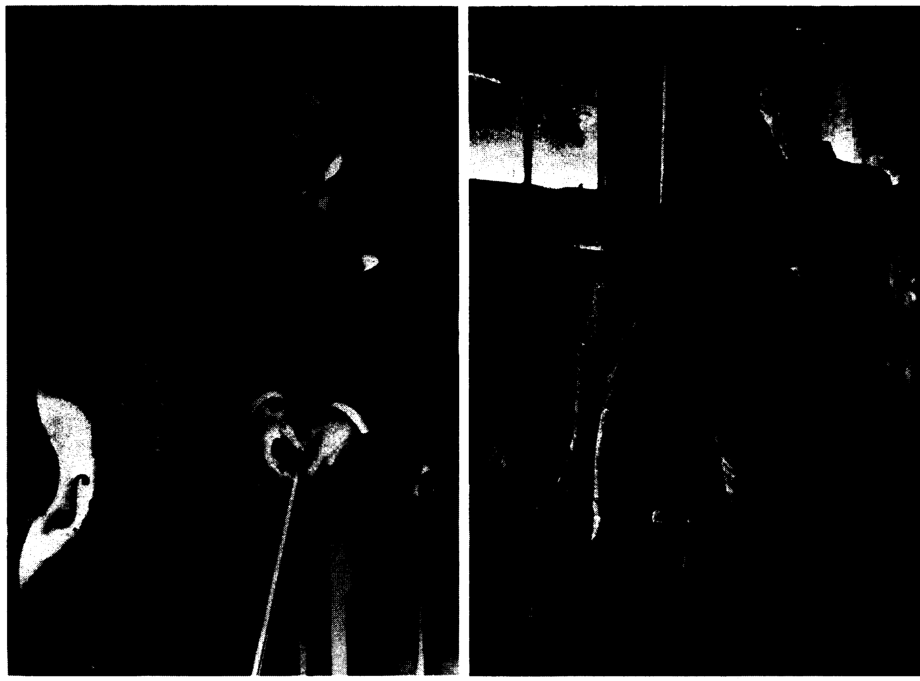
A Pathbreaking

Transforming Traditions in American Biology, 1880–1915. Jane Maienschein. Johns Hopkins University Press, Baltimore, MD, 1991. xii, 366 pp., illus. $48.

Ross Granville Harrison is one of my heroes. He was born in 1870 and in the course of a long life (he died in 1959) he became one of the greatest experimental



Left, E. B. Wilson with cello, 1889 or 1890. [From *Transforming Traditions in American Biology*; collection of Linda Timmons] *Right*, Ross Harrison on a canoeing trip, possibly in Canada. [From *Transforming Traditions in American Biology*; Harrison Papers]

embryologists, developing tissue culture among other contributions; was founding editor of the *Journal of Experimental Zoology* and edited it for 42 years; and chaired the National Research Council through the Second World War. But remarkably, Harrison was not the only American zoologist of such stature in the early part of this century. There were also T. H. Morgan, E. G. Conklin, and E. B. Wilson, who together with

Ross Harrison (second from left) and Thomas Hunt Morgan (second from right) on the way to Blue Mountain, Jamaica, with a Chesapeake Zoological Laboratory group, 1891. [From *Transforming Traditions in American Biology*; Marine Biological Laboratory Archives]