

interest to aficionados of cold fusion and analysts of such controversies. For a wider audience, however, it is unsatisfactory on several counts: it virtually ignores developments over the last couple of years; it is entirely dogmatic and one-sided about the merits of the case (and too often *ad hominem*); its discussion of cold fusion in the context of accepted scientific practice is partisan and shallow; its reasons for consigning cold fusion to the category of pathological science are uninformed.

Those latter faults, it might be noted, often characterize attempts to debunk anomalous claims.

### *Dogmatism and Argument Ad Hominem*

Huizenga points out that the "history of science contains numerous examples where scientists have embraced a favorite but discredited idea for far too long. . . . When convincing evidence is forthcoming, scientists must be willing . . . to change their position" (p.217). He fails to recognize that what is "discredited and "far too long" to some is not so to others; that what is "convincing evidence" for some is not so for others; that many people fail to share Huizenga's belief that cold fusion should have been assigned to the dustbin no later than November 1989, when his panel's report was released.

Fleischmann and Pons are criticized for not consulting physicists (p.11); yet the Vice President for Research at the University of Utah, who played a central role, was a physicist. The co-chair of Huizenga's own panel, a physicist and Nobelist, insisted against Huizenga's wish on a preamble to their report that said, it is too soon to give a categorical yes-or-no judgment (p.91). Huizenga deplores the silence of internationally respected scientists (p.198) even as he criticizes Schwinger and Teller for publicly lending provisional credence to cold fusion. So "physicists" for this author means only those physicists who hold the same view as he does, and circumstances alter cases: those who agree with Huizenga are right and those who disagree are wrong, without benefit of impersonal, general criteria for assessing right and wrong. Thus Huizenga declares of scientific interest Jones's claim of a 40-magnitude enhancement of fusion but ridicules the 53-magnitude increase that (he calculates) is implied by Pons and Fleischmann (pp.66-67); why? Surely 40 magnitudes is in practice as incredible as 53, or 53 as plausible as 40, given that neither observation has been reproduced or rationalized and that both are magnitudes of astonishing size. Again, Huizenga claims that "statistically meaningful results required comparisons made of like numbers of cells of both types" (p.97). Where did he pull that one from? Here and in other places, Huizenga makes up general rules or principles to fit his particular purpose.

When argument is so partisan, there is likely to be verbal trickery too. "Conventional nuclear physics was declared invalid in metallic lattices by fiat" (p.x), Huizenga says. Not so: theorists just speculated about new nuclear phenomena that might occur within solid lattices, if the experimentalists' reports were to be believed and explained.

One trouble, says Huizenga, is that Pons and Fleischmann were "outside their field of expertise" (p.xi): but is electrochemistry really less pertinent here than nu-

clear chemistry or physics? Is mastery of the necessary experimental art less germane than mastery of the existing theory that might construe the experimental results? Is Huizenga, the Tracy H. Harris Professor of Chemistry and Physics at the University of Rochester, more expert than Fleischmann, a Fellow of the Royal Society and Professor of Electrochemistry at the University of Southampton, one of the leading dozen-or-so centers of electrochemical research in the world?

Having criticized others for venturing outside their field of expertise, Huizenga himself reveals a few points of ignorance. Discussing the interpretation of the Nernst equation, which is derived for equilibrium conditions, he talks about the overpotential term in it (p.33); but "overpotential" means the difference between what the electrode potential actually is and what it would be if the system were at equilibrium. (For a discussion germane to the point Huizenga addresses here, see Henry H. Bauer, "Physical interpretation of very small concentrations" *JSE*, 4 (1990) 49–53.) According to Huizenga, "electrochemistry . . . should be familiar to most chemists and is taught in college freshmen chemistry" (p.32); but I teach freshman chemistry almost every year, and have scanned innumerable texts for these courses, and can testify that there is little if any pertinent electrochemistry in them, quite possibly less than there is nuclear physics, as a matter of fact.

In too many places, Huizenga's criticism becomes offensively personal: believers "resorted to pseudo-science" (p.x) and they are compared to users of laetrile and followers of Lysenko (p.95). Bockris, about as well known as any other living electrochemist, happens to take cold fusion seriously, so Huizenga describes him as a "long time friend and acquaintance of Fleischmann" (p.114)—totally irrelevant except as innuendo; and what makes it necessary to add that a friend is also an acquaintance? Jack Simons and Cheves Walling, well respected theoretical chemists, wrote an early paper suggesting how cold fusion might occur; subsequently, Simons was elected to the Henry Eyring chair, for distinguished work over a long period and having nothing to do with that paper; Huizenga compares that (p.208) to the prize awarded, early in this century, to René Blondlot after his erroneous claims of N-rays had been disproved. A mistaken claim made in another field years ago by another person had been dubbed the "Utah effect" and Huizenga recalls that solely to pronounce cold fusion a repetition of such pathology (p.224).

### *Lessons About the Nature of Science*

The last chapter of this book adduces lessons about such matters as premature publication, reproducibility, handling far-out ideas and claims, and the like. The generalities here are unexceptionable but side-step the complexities and nuances that could make the discussion interesting and pertinent. Consider again the comment that the "history of science contains numerous examples where scientists have embraced a favorite but discredited idea for far too long. . . . When convincing evidence is forthcoming, scientists must be willing . . . to change their position." Much has been written by historians and philosophers of science about why some people found evidence convincing that others did not, about discredited ideas that turned out to be valid, about the impossibility of applying generalizations neatly to specific contemporaneous instances; all that is ignored here.

## Bias in Reviewing

Highly favorable reviews of Huizenga's book have appeared: in *Science*, written by a physicist who works at conventional, "hot" fusion<sup>4</sup>, and in *Nature*, written by Frank Close<sup>5</sup>. The latter is not only a flagrant instance of conflict of interest but is amusingly naive about the nature of science; thus it talks "about how scientific [sic] scientists [sic] should handle their publications" and refers to the National Institutes of Health as not a scientific institution.

Not that this reviewer claims to be unbiased: I did research in electrochemistry for a couple of decades, knew Fleischmann as an accomplished practitioner, have read fairly widely about the nature of science and of pseudo-science, and become angry when critics commit intellectual sins quite as grievous as the ones they complain of.

On the other side, Mallove has circulated an intemperate critique of Huizenga's book. Less committed is Ron Dagani, a science writer, who gives Huizenga's book quite a respectful review while admitting his curiosity "whether there isn't some nugget at the core of the phenomenon that is scientifically valid and interesting"<sup>6</sup>.

## A Verdict on Cold Fusion?

Pons does seem to have been deliberately evasive and even untruthful at times, perhaps even with Fleischmann; but that doesn't in itself disprove the substantive claim. It remains possible that some yet-unidentified factor, or some esoteric combination of two or even more factors, governs actual cold fusion; credible people have rather often reported short bursts of heat or neutrons. Yet the weight of opinion is coming more toward the conclusion that the original findings of Pons and Fleischmann were in some way artifactual.

Close and Huizenga may well prove to be right. But if so, then they will have been right for wrong reasons; and one might sometimes prefer to be wrong for right reasons than to be right for wrong reasons.

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<sup>4</sup>Stanley C. Luckhardt, "A vanishingly small case" *Science*, 257: 560-61.

<sup>5</sup>Frank Close, "The cold war remembered" *Nature*, 358: 291-92.

<sup>6</sup>Ron Dagani, "A chemist debunks cold fusion", *Chemical & Engineering News*, 25 May 1992, 23-4.