

Regrettably, the book was not well served by its proof-readers. Some names are misspelled and several titles are rendered inaccurately (for example, Flew's first name becomes "Anthony" and the title of a book by Ducasse is repeatedly miscited), there are at least two misquotations, the index contains at least one major error, and there are many other typos.

James M. O. Wheatley
500 Duplex Ave., Apt. 2601
Toronto, Canada M4R 1V6

References

- Almeder, R. (1987). *Beyond Death: Evidence for Life After Death*. Springfield, IL: Thomas.
- Ayer, A. J. (1963). *The Concept of a Person and Other Essays*. New York: St. Martin's Press.
- Ayer, A. J. (1973). *The Central Questions of Philosophy*. London: Weidenfeld and Nicolson.
- Broad, C. D. (1962). *Lectures on Psychical Research*. New York: Humanities Press.
- Edwards, P. (1986-1987). The case against reincarnation. Four parts. *Free Inquiry*, 6:4, 24; 7:1, 38; 7:2, 38; 7:3, 46.
- Edwards, P. (1992). Introduction. In P. Edwards (Ed.), *Immortality* (pp. 1-70). New York: Macmillan.
- Moore, B. N. (1981). *The Philosophical Possibilities Beyond Death*. Springfield, IL: Thomas.
- Penelhum, T. (1970). *Survival and Disembodied Existence*. New York: Humanities Press.
- Stevenson, I. (1974). Some questions related to cases of the reincarnation type. *Journal of the American Society for Psychical Research*, 68, 395.
- Stevenson, I. (1987). *Children Who Remember Previous Lives: A Question of Reincarnation*. Charlottesville: University Press of Virginia.
- Wheatley, J. M. O. (1979). Reincarnation, astral bodies," and "ψ-components." *Journal of the American Society for Psychical Research*, 73, 109.

Bad Science: The Short Life and Weird Times of Cold Fusion by Gary Taubes. New York: Random House, 1993, 503 pp. \$25 (c).

Here's a fifth book in English about cold fusion (see reviews of three others in JSE: 5 #2, 6 #1, 6 #4). What's different, new, or special about this one?

It is considerably longer; but it does not go deeper into the substantive issues of experiment, data, and theory than the earlier books by Close, Huizenga, or Mallove. (The first book, by Peat, is chiefly of historical interest: it was written too soon and published too quickly).

This book is the most recently published, yet it does not bring the story more up to date. The chronological manner in which Taubes tells the story ends in June 1991, though the book was printed two years later. An Epilogue of 4 pages does nothing to make up for that.

With Close and Huizenga, Taubes is quite sure that cold fusion does not exist; but he argues the case less by argument about the issues than by *argumentum ad hominem*.

This book belongs to the "let-it-all-hang-out" genre that places "human interest" clearly ahead of the intellectual issues; a genre that I'm tempted to call scientific pornography because it appeals more to prurient than to scholarly passions. This genre was foreshadowed by J. D. Watson's soul-baring memoir,

The Double Helix, and has brought forth a score or two "docu-novels" over the past decade or so, with such titles or sub-titles as *The Race to Synthesize a Human Gene*, *The Race for the Superconductor*, *The Making of a Scientific Dynasty*, *The Science and Politics of Opiate Research*. Taubes has to his credit — if one cares to call it that — an earlier contribution to this genre in *Nobel Dreams: Power, Deceit, and the Ultimate Experiment*. (For full bibliographic data on these and similar others, see my *Scientific Literacy and the Myth of the Scientific Method*, University of Illinois Press, 1992, p.166.)

This genre of scientific docu-novels is, of course, a sign of our times: the media treat us to "news" about people rather than events, about emotion rather than reason. Television shows endlessly the endless sufferings in Bosnia, Haiti, Rwanda, Somalia; but offers no hard-headed analyses of what feasibly could actually be done about it. In the coverage of sports events we learn much less about the barriers of Nature that humans have learned to surmount with increasing skill than about the "Close-up-and-personal" banalities that reveal champions to be intellectual and emotional babies just like everybody else. Science is just getting the same treatment now as have politics, finance, business and all other spheres of human activity. But that is not a good thing. Society needs heroic role models, *larger* and *better* than life. The monarchy has outlived its usefulness when its members fail to convey superhumanly selfless devotion to duty. The Nobel Prizes become a burden rather than a spur to scientific achievement when Nobelists are seen as schemers and deceivers.

Still, the human-interest genre can come in different modes: Greek tragedy, say, or comedy in the style of Malcolm Bradbury or Kingsley Amis, or one-dimensional, dogmatically didactic morality play. Taubes' choice is the last of these; and that, in a nut-shell, is what's most wrong with this book. Wrong not only with, but also *in* this book, because the one-sidedness is not true to the facts of the matter and the facts of life, and so readers are not led to genuine understanding of what happened. Thus the dust-jacket repeats the absurd denigration that other non-electrochemists also have committed, that the 1989 announcement came from two "obscure" individuals; and nowhere in the text does Taubes put the reader properly into the picture, that Pons had published much and was quite widely known and reasonably well thought of and that Fleischmann was one of the world's leading electrochemists, at the summit of his career, winner of prizes, Fellow of the Royal Society, long-time Professor at one of Britain's and the world's leading centers of electrochemistry. Where Taubes does refer to Fleischmann's "long and distinguished career" (7), he distorts the significance by falsely describing him as "on the verge of retirement" when he first met Pons *in* 1975. Thus the unwary reader could well think of Fleischmann some 15 years later as an aged has-been; whereas Fleischmann in 1975 was only in his late 40s.

This twisting of the truth is of much greater import than a mere denigration of two people: it also makes it difficult to understand the magnitude of the commotion caused by the 1989 press conference. Imagine that it had featured

Smith and Brown at Utah, later seconded by Jones at Brigham Young. The world of electrochemistry would then cheerfully have joined Close, Huizenga, and Taubes in dismissing it as another instance of the "Utah effect". But no electrochemist could help taking pause over a claim from Martin Fleischmann. Perhaps this *would* turn out to be another of Martin's occasional mis-steps; the claim surely seemed so unlikely, too good to be true. But on the other hand, how little we really know about solid electrodes and solid-state physics; and how much Fleischmann knows about hydrogen electrolysis! No ambitious or simply curious electrochemist, in other words, would shrug it off without at least giving the experiment a try — as they *could* and *would* shrug off the same suggestion from Smith, Brown, and Jones. Just so would astronomers shrug off from Smith, Brown, or Jones an apparently hare-brained suggestion that they would pause over and think about if it came from Tommy Gold. One can ignore the numerous cranks who urge mega-vitamin diets on us, but even scientists eat lots of Vitamin C because when Linus Pauling is convinced about something, lesser mortals are foolish to write it off. Laboratories at Harwell, Los Alamos, Oak Ridge and elsewhere, and competent people at leading universities all over the world, rushed to try the experiments precisely because the announcement had come from Fleischmann and Pons and *not* from two "obscure" people.

Taubes seriously misleads about science and scientists by suggesting that they would be tempted by a Pascal-type wager, for a potential reward against which "a reputation would always seem inconsequential" (93). I don't know of a single scientist who takes that stance, though of course there may be some; but I do know many who value their reputation above any conceivable other "reward". Reputation, in point of fact, is precisely *the* reward traditionally sought by those who go into science.

Real people are a mixture of good and bad. Taubes tells us only of the bad. The lay reader could not conceive that those whom Taubes paints as wrong about cold fusion could at the same time be people with considerable accomplishments to their credit, yet many of them are. Because he can see only one side, Taubes commits such absurdities as citing (275) some physicists' denigration of *Modern Electrochemistry*; an excellent textbook, a *tour de force* when it was first published as the first comprehensive modern text in electrochemistry, one of whose authors happens to be John Bockris of whom Taubes will not speak a single word of good. Then, having apparently been corrected in this misconception, Taubes doesn't drop the misleading characterization of the book from his text, even though he is supposedly trying desperately to shorten it; rather, he adds a disparaging end-note: "Dave Williams, of Harwell, however, said *Modern Electrochemistry* was very good, so maybe one had to be an electrochemist already for it to resonate" (454). In point of fact, of course, the finger of shame should here be pointed at the arrogant physicists who felt free to comment well beyond their competence.

Because things have to be black or white in Taubes' tale, a review panel set

up by the University of Utah must be black. So one of its members is described as "a physicist from Yale, who was renowned in the lay community for his book on the physics of baseball". Another, Stanley Bruckenstein, is merely "an electrochemist from the State University of New York at Buffalo"; yet if you were to talk to electrochemists around the country and ask them for the names of a dozen senior, truly knowledgeable people in the field, of sound judgment and high integrity, Bruckenstein would figure in most or all of them.

When the wrong side goes public, it's bad: "Gai accused Jones of breaking an agreement not to talk to reporters" (469). But when the right side does it, of course it's different: "Gai sent a copy . . . to Huizenga's panel, where it entered the public record. He then had Yale distribute a press release, so that the reporters would at least get the story right" (388); later, "Gai was sending all the copious correspondence . . . to a number of third parties, both scientists and journalists who could serve as witnesses to the crimes being committed against science" (400).

When the facts support the wrong side, Taubes finds that surprising: "The evidence, oddly enough, for Pons and Fleischmann's version of the events is persuasive" (37-8). And here again, the bias obscures a vital element of the episode: it was their interaction with Jones that led Fleischmann and Pons to believe that a potential competitor had already *independently confirmed* their wild idea; and this led to going public when they knew it to be still premature, when they greatly wanted to avoid doing so (96-7,250).

So throughout, the book is tendentiously one-sided. To denigrate an article by two people on the wrong side, *Scientific American* is called a "lay publication" (31). Taubes — just acting as a reporter, of course — repeats gossip that Rafelski, holding a responsible position, is "irresponsible and unprincipled", considered by some "an outright charlatan" (30). Unremittingly throughout the book, there are similarly derogatory comments about almost everyone. Many of them are entirely gratuitous; it tells us nothing about Bockris's credibility, for example, that he "appeared in an off-white suit that gave him the air of a plantation owner who had fallen through a time warp" (274). Indeed, Taubes' unbridled derogation of Bockris in so many places may well achieve what perhaps nothing else could, that electrochemists *en masse* will rally to Bockris' support. Fair, after all, is fair; and enough is enough.

Not only are people all right or all wrong for Taubes: even those who are right don't come across as very nice. Allen Bard, universally respected for his statesmanship, is not well served by being quoted as saying things that (if indeed he really said them) he is in the habit of saying strictly in private. Such "heroes" of Taubes' story as Nate Lewis and Chuck Martin are portrayed as foul-mouthed, ruthless competitors whom one would want to keep always at one's front, in full view. As with the films of Tom Cruise so with this book, it seems that the preferred role model for young males is that of the brat: the 33-year-old who boasts that others say of him, "If Nate says it's right, it's right" (119).

That sort of arrogant certitude will be familiar to readers of this *Journal* since it stems so often from those who know that *they* can infallibly spot pseudo-science when they see it. One of the marks of naiveté and lack of understanding in this respect is the referring, as authoritative, to a seminar titled "Pathological Science" given by Irving Langmuir 40 years ago, as one of Taubes' brats does (403). That Taubes fails to correct him for the sake of his readers implies that he too thinks Langmuir's remarks were the last useful words on that matter.

So, I've revealed some serious flaws in the book, and my distaste for it. Yet I read (the first two thirds of it) with utter fascination and found it hard to lay aside. (Why I found Part III less compelling than the first two parts, I'm not sure; perhaps the dosage of *ad hominem* and innuendo and dialogue in quote marks without documentation had become too much.) Taubes can write very well. His descriptions and dialogue ring very true. If only a fraction of his reportage about Stanley Pons is approximately correct, then one would probably not want to buy any second-hand scientific data from Pons.

It may well be that one cannot understand an episode like cold fusion without prying in this manner into people's doings in and out of the lab and what they say in private or in unguarded moments. Certainly the early history that Taubes gives rings very true in its illustration of how science is often distorted by the hopes of Nobel prizes, the greed for patents, and administrators' or institutional ambition. On the other hand, some very substantial works have made rather plain, without *ad hominem*, what went wrong in such matters as the polywater affair or that of N-rays. Moreover, I think it may still be a little too early to conclude that there's nothing substantive at all to the Pons-Fleischmann claim.

One can sympathize with Taubes, that his publisher forced him to cut some 200 pages from the original "700-plus" (xii). Perhaps that's why bibliographic details and attribution of quotes is almost entirely absent. But in that absence, the reader is entitled to wonder just how accurate the supposedly factual material is, let alone dialogue that feigns authenticity through the use of quotation marks: for example (390) what Bockris, Fleischmann, and Pons said to one another in a telephone conversation. The 40 pages of End-notes are *not*, as one usually expects, documentation or citation; they expand on the text, for example: "Nate Lewis, for instance, said the first time he considered the possibility that the cells had been spiked was the first time he heard the results came from Bockris" (460). The lack of documentation is not ameliorated by the list of 257 people who were interviewed (about half of them by phone only), especially since the request of some was honored, that their names be left off the list.

There are a number of errors that ought to be pointed out; because the mentioned lack of documentation precludes efficient checking of what Taubes says, and because his tone of authoritative rectitude would imply that one will not find errors on *his* part. "Poisoning" (55) is *not* "adding metal ions" to "help

the palladium absorb hydrogen". Lay readers are poorly served by Taubes' suggestion (134) that cold fusion would contradict "one of the basic tenets of science, . . . that the laws of nature here are the same as the laws of nature elsewhere", implying that "here" could be inside palladium: those very laws might say that fusion can occur in a metal lattice or a plasma though not in a gas. Graduate student Hawkins did *not* have "reason to expect" (142; see also 445) that he would share a Nobel Prize and royalties: graduate students of Nobelists typically do not. The "strongest evidence in a scientific case" is *not* "traditionally reserved" to "figure 1A" (167) of a paper. The geometry of cells *is* a crucial element in much of electrochemistry (186); whether to run cells open or closed is a separate issue. Lay readers might like to be told that "pu" (246) comes from plutonium-beryllium *because* the symbols for those elements are Pu and Be respectively. As a former electrochemist, I found the discussion of "depolarization", cell voltages and the like on p.257 incomprehensible and suggest that non-expert readers skip it. I can't agree with Taubes that (262) "*The New York Time*... traditionally serves as the paper of record for the scientific community"; I don't even know what he means by it. If the temperature gradient in the cells has nothing to do with the stirring of the solution as made visible by dyes (271), then the contention that Fleischmann and Pons had failed to stir their cells adequately (265) becomes a red herring and an invalid criticism of them. Tungsten "bronzes" are *not* (277) alloys, they are electrically conducting ceramic-type tungstates. Somewhere between pages 304 and 309, Taubes forgets that Barry Miller is an electrochemist. There is *no* "natural tritium concentration in heavy water" (444). The technical discussion in note 55 on p.449 is again incomprehensible. The book's index promises more than it delivers. At 20 pages one expects it to be exhaustive, yet it didn't help me locate such significant items as "Utah effect" or "controls".

Dieter Britz, who maintains current a complete bibliography of *scientific* writing about cold fusion, mentions at least a couple more books about the matter in English and one in Russian. I suspect the future holds yet more. Taubes' book tells a lot, most of it probably quite accurate, about many of the characters in the drama and their interactions. It's unlikely to be superseded in this respect, if only because it verges so close at times to libel or slander. The book is, for most of its considerable length, riveting reading. Its major flaw, as I've indicated, is that it is unabashedly one-sided not only about the scientific issues but about the characters as well. Perhaps it's because I'm so much older than Taubes that I should like to have seen more insight displayed into human nature, some acknowledgment that Taubes understands how competent, decent people — like himself, say, or most of the rest of us — can come under certain circumstances to behave in ways that we later regret. Perhaps he might watch the film *Judgment at Nuremberg* and contemplate the tragic dignity of the eminent German jurist portrayed by Burt Lancaster. If indeed cold fusion is as much of a plain mistake as Taubes would have it, then Fleischmann, Pons,

and such others as Bockris would be more authentically portrayed by an ancient Greek tragedist than by the simplistic morality tale of this book.

Henry H. Bauer

Virginia Polytechnic Institute & State University
Blacksburg, VA 24061-0212

Galileo's Revenge: Junk Science in the Courtroom by Peter Huber. Harper Collins, 1991. 274 pp. \$23, (c).

At a recent meeting of the American Statistical Association I bought a T-shirt for my wife that said, "Statistics means you never have to say you're certain." No matter that I hid it out of sight to the extent that I clean forgot about its existence and then overlooked her birthday; the statistical point of the pun is that the same data set can be viewed in many ways.

Yet, despite what my undergraduate students believe, not just anything goes. And not merely because the ruling elite of the academy has imperially decreed what is acceptable and what is otherwise. Nor is there a conspiracy to suppress deviant views. Simply stated, non-sensical procedures and non-sensical results are identifiable and thus can be eliminated from discussion.

Unfortunately, judges and juries, especially when influenced by lawyers and expert witnesses, don't see matters with the same statistical clarity as statisticians do. Peter Huber, author of *Galileo's Revenge: Junk Science in the Classroom*, would change "influenced" to "hoodwinked." His major contention is that malpractice on the witness stand and pseudo-science in the courtroom are rampant.

As a consequence, good products such as the Audi automobile or the drug Bendectin are ousted from the marketplace because unscrupulous lawyers and experts can make a lucrative living litigating endlessly. In twentieth-century America, one can strike it rich on anecdotal rare events just at the threshold of recognition.

Make that very rich. Weak electromagnetic fields, electronic fetal monitoring, cesarean deliveries, breast implants, and AIDS transmission are areas in which clever footwork can convince judges and juries that unlikely doesn't imply absolutely impossible. One of Huber's favorite examples is that of "a soothsayer who decided she had lost her psychic powers following a CAT scan" and "persuaded a Philadelphia jury to award her one million dollars in damages." His other favorite is "the cancer-by-streetcar cases" of trauma: a woman falls off a public vehicle and the injury causes breast cancer.

While not everyone may share Huber's disdain or contempt for soothsayers, astrologers and homeopaths, none would disagree with his scathing critique of entrepreneurial scientific experts who are "saxophones, because the lawyer calls the tune and the expert plays it." Huber juicily quotes a personal injury lawyer: