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website [6]. The table of contents and the first chapter of the book under review are on the World Scientific website [7].

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Voodoo Science: The Road from Foolishness to Fraud by Robert L. Park. Oxford University Press, 2001. 230 pp. $17.95 (paper). ISBN-10: 0195147103.

Dr. Robert Park is recognized by many people as an advocate for science and for the integrity of science. He does not like to see it abused and misrepresented by unethical opportunists, and he tries to serve the public good by alerting the public to what he believes is false science. Park is also well-known for using wit, sarcasm and name-calling to make his points.

This review looks only at the parts of Park's book that covers cold fusion. In many sections of this book, Park expresses himself with derision and invective, likening himself to a hostile juvenile in a schoolyard.

The hypothesis of fusion, as the underlying process to the research ascribed to as "cold fusion," is suspect and unproved; it is now and has been from the day Martin Fleischmann and Stanley Pons made their pronouncement in a press conference in 1989 at the University of Utah. On the other hand, proof of nuclear reactions exists and has existed in this field for many years, long before Park published this book in 2001. This review will pick out important sections of his book that mention "cold fusion" and will provide related comments.

On pp. 13–14 Park writes, "Each year at the cold fusion conference there is great excitement over new results that are said at last to show incontrovertible

proof that fusion is taking place at low temperatures. Perhaps it's new evidence of neutrons or gamma rays characteristic of deuterium fusion; or helium ... But by the time of the next meeting, many of these papers will have been discredited or withdrawn."

It is clear that when Park writes this, he is expressing his disbelief. That fact is that each year, there have been new, exciting results. Neutrons, helium and energy gains have been reported – rigorously. As far as his assertion that many of these papers "will have been discredited or withdrawn," Park does not provide any reference for this statement and it is inconsistent with the facts known to this author. Park comments at the end of his book about scientists' responsibility to society. Journalists or people who write about science also have a responsibility to society. Park's comment about these papers indicates a failure on his part to report accurately.

On p. 14 he writes: "Cold fusion is no closer to being proven than it was the day it was announced." If Park is referring to a fusion mechanism, he is correct, that theory is not proven. If he is referring to a novel nuclear reaction, he is incorrect; evidence for nuclear reactions is plentiful and was so at the time he wrote his book.

On p. 14 he writes, "These are scientists; they are presumably trained to view new claims with skepticism. What keeps them coming back each year with hope in their breasts? Why does this little band so fervently believe in something the rest of the scientific community rejected as fantasy years earlier?" And on p. 27 he speculates that they "found in cold fusion relief from boredom."

The questions Park asks are good questions. Unfortunately, he unilaterally divines the answer and assesses the researchers as merely foolish and given to fantasy. This author asked the same question but asked the researchers directly. In general, they responded that they persisted because they saw a positive result in their experiment(s), they checked their instrumentation carefully and they found no source of error. They trusted their methods and instruments, despite their own or others' preconceived notions.

On p. 18 Park makes a snide remark about his presumption that Fleischmann and Pons were ignorant about the fundamentals of their work. "How," Park wondered, "could Pons and Fleischmann have been working on their cold fusion idea for five years, as they claimed, without going to the library to find out what was already known about hydrogen in metals?"

Fleischmann began his long investigation in Pd/D effects when he was 20, in 1947, reading, among other works, that of Percy Bridgman, a Harvard professor of physics and a Nobel Prize winner. Park's comment couldn't be more wrong. Fleischmann spent his entire life studying hydrogen in metals and was awarded nearly every prize in the field. Not only that, but Fleischmann and Pons were working in a regime beyond that which was known. This is what pioneering science is all about.

On p. 18 Park talks about the inexplicable fact that the expected fusion byproducts should have killed Fleischmann and Pons, and there they were,

living proof, in direct contradiction to the presumed theoretical explanation for their results. Park conveys a related dark joke which became known as "the dead graduate student problem."

In many ways, Park is correct. For 19 years, excess heat without harmful radiation has been a consistent fact in LENR research. However, informed scientists no longer consider this a joking matter.

Park writes on pp. 18–19 how he was interviewed by NBC News chief science correspondent Robert Bazell just a few days after the cold fusion press conference. In the interview, he writes, he "summarized why the cold fusion claim had to be wrong." Odds are that Park could have been completely right. The Fleischmann-Pons claim did seem completely unlikely. However, for Park to expound unequivocally that it was entirely wrong was a risk he took in the absence of data, which was yet to be published, and direct experience.

Park wrote in his book that he suggested to the NBC cameraman that it would likely turn out to be fraud. Nobody has ever proved that Fleischmann and Pons committed fraud and the people who directly accused them of such – researchers from MIT – quickly retracted their slander in May 1989 when they came around to their senses. Why did Park not remember this when he wrote his book in 2001?

On p. 19, Park derides Fleischmann and Pons for having an incorrect hypothesis. "One reason Pons and Fleischmann had to be wrong," Park writes, "was because the number of neutrons they claimed to see was at least a million times too small to account for the energy they reported."

Fleischmann and Pons were wrong about their theory. They were correct about their main experimental results, the claim of excess heat. Do not be confused by Park's derision. Is it unscientific to be wrong about one's hypothesis? Of course not. Should Fleischmann and Pons bear shame or dishonor for this? Again, no.

"Still," Park continues, "if the experiment produced any neutrons at all, it would be proof that a nuclear process of some sort was taking place." For the record, neutrons were reported in 2007 by the U.S. Navy SPAWAR San Diego group, SRI International and the Russian Academy of Sciences. They were not the sort of energetic neutrons you would expect from thermonuclear fusion but small fluxes of low-energy neutrons, which qualifies for Park's proof of a nuclear reaction.

On p. 24 Park discusses the delicate matter of the light (ordinary) water "control" experiments. He explains their significance quite appropriately.

"Since the hydrogen atoms in ordinary water have no neutrons," Park writes, "they cannot directly fuse to form helium, which needs either one or two neutrons in the nucleus. If something you have been attributing to deuterium fusion is observed with ordinary water, it means you've been fooling yourself."

In response to critics, Pons performed an experiment using ordinary water.

"A few days later when he was asked by a reporter what the result had been," Park writes, "Pons's only comment was a muttered, 'We did not get the baseline we expected.' Apparently the experiment behaved about the same with ordinary

water as it had with deuterated water. Pons and Fleischmann would never mention the light-water experiment again."

Park guesses that Pons saw "about the same" results with the ordinary water. This is a wild and irresponsible guess. In contrast to Park's uncited reporter's response, Nature wrote on April 27, 1989, "Pons tantalized his audience by indicating that preliminary results from just such a comparison suggested an 'unexpected' production of heat in the ordinary cell."

The differences in the two accounts provide three interesting pieces of information. First, they show that Pons had the integrity to report what he knew would be perceived as bad news. Second, they show that there was early information to suggest that a non-fusion process may have been at work. And third, the representation of Pons made by Park significantly differs from that made by *Nature*, displaying a significant journalistic bias.

Park assumes that their hypothesis failed and therefore that their experiment did so as well. But Pons reported the facts as he saw them: an unexpected production of excess heat. Pons remembered the foundation of science (experiment), Park did not. In addition to their fusion hypothesis, Fleischmann and Pons had also speculated, "a hitherto unknown nuclear process." Whether it was belief or open-mindedness, Fleischmann and Pons, and those who followed them, found something, as we now know, something quite real.

Park conveys a dramatic short account of Edward Teller's protégé, Lowell Wood, who was eager to attempt the Fleischmann-Pons experiment. Wood's experiment resulted in an explosion and Park writes that "the blast shattered his apparatus and ended his quest for cold fusion." News update: Wood apparently has resumed his "quest for cold fusion." Wood attended the ICCF-10 conference in August 2003.

Park describes a congressional hearing on cold fusion on p. 94 in which Fleischmann and Pons were called to testify. "Neither admitted to the slightest doubt concerning their discovery," Park writes.

News flash: Fleischmann and Pons were skilled experts in electrochemistry and calorimetry. They knew their apparatus and methods – cold. Of course they were confident about the fundamental aspect of their claim, excess heat.

On p. 97 Park discusses the disastrous American Physical Society meeting in Baltimore, MD, on May 1 and 2, 1989. "It had not gone well for cold fusion," Park wrote, "theorists had reported that cold fusion violated not one but several accepted physical principles; chemists seemed to he able to account for all of the heat without invoking nuclear reactions; elementary flaws in the Utah experiment were laid bare."

Park implies a bastardization of the scientific method; experiments do not "violate" theory. Park, by reporting this, and the theorists, by proposing such, abused their privilege as scientists and weakened public trust. The "chemists" Park alludes to were Caltech's Nathan Lewis and Stanford's Walter Myerhof. In no way did they debunk Fleischmann and Pons' heat measurements. Both, as their main argument, speculated that Fleischmann and Pons failed to stir their

cell, which led them to a mistaken measurement and incorrect interpretation. A week later, at the Electrochemical Society meeting in Los Angeles, Fleischmann and Pons dispelled this erroneous speculation.

On p. 97 Park says he was interviewed again by NBC's Bob Bazell on the Today program a day after the Baltimore American Physical Society meeting. Park recounts his message to American public in 1989 about cold fusion, re-iterated again in 2001 in his book:

"Cold fusion ... is dead," Park writes, "but the corpse won't stop twitching. Inept scientists who had rushed to report confirmation, greedy university administrators who had tarnished the reputations of their institutions, gullible politicians who had wasted the taxpayers' dollars and careless journalists who had accepted every press release at face value ..."

This would be an appropriate time to mention Sir Arthur C. Clarke's comment in the foreword to "The Rebirth of Cold Fusion":

"[Such] debacles fall into two classes, which I will call Failures of Nerve and Failures of Imagination." In 1989, the cold fusion controversy fitted into the second category, Failures of Imagination, which comes into play when all the available facts are appreciated and marshaled correctly but when the really vital facts are still undiscovered and the possibility of their existence is not even admitted.

"Today, the cold fusion controversy falls into the first category, Failures of Nerve; many vital facts have been discovered, yet sceptics lack the courage to acknowledge them or their immense implications."

"I never imagined," Park writes, "that a decade later there would still be scientists championing cold fusion, or that companies claiming to have developed cold fusion devices would attract investors." News flash: It's nearly two decades later and there are still champions and investors.

Park mocks James Patterson on p. 115 for his claims that his experiment showed the possibility of low energy nuclear transmutations, particularly transmuting radioactive materials to non-radioactive materials.

"That would be a surprise ... to every nuclear physicist in the world," Park writes. "It would, in fact, be a miracle. The only way to 'neutralize' radioactivity is to transmute radioactive isotopes into stable elements, something which, to the extent that it's possible, requires intense neutron bombardment from a nuclear reactor or a powerful nuclear-particle accelerator."

Park reveals an apparent aversion to scientific discovery in this section, as well as an ivory-tower attitude which presumes that "every nuclear physicist in the world" knows all there is to know about the natural universe. Transmutation experiments now show that Patterson was probably onto something.

On Feb. 20, 2004, Lewis G. Larsen of Lattice Energy LLC presented an invited talk at a Department of Energy and Electric Power Research Institute Workshop in San Diego, CA, in front of 80 senior scientists from national laboratories, universities and commercial enterprises.

He said, "You can argue about excess heat measurements and ponder near absence of "normal" nuclear products, but transmutation experiments involving

LENRs are irrefutable." He cited the published transmutation work of Yasuhiro Iwamura of Mitsubishi Heavy Industries showing evidence of low energy nuclear transmutations. Larsen stated that there was no laughter in the room when he spoke.

Park recounts on p. 119 Fleischmann's frustration with his search for a plausible explanation.

"'What else could it be?' Fleischmann asked, repeating the claim that the heat produced was much too great to be due to chemical reactions," Park wrote. "It must be, he said, that two deuterium atoms fuse to form helium-4 by some previously unknown process that generates heat but little or no nuclear radiation."

A scientist must start somewhere, this is the hypothesis. His inquiry was honest. Park's skepticism was not; it was cynicism. Fleischmann's question then, as it is now, was a valid one. To expect a science discovery to arrive pre-packaged with a tidy explanation is unscientific.

Near the end, on p. 212, Park makes his case for his role in the betterment of science and society. "Voodoo science," Park writes, "is a sort of background noise, annoying but rarely rising to a level that seriously interferes with genuine scientific discourse. Something like cold fusion might interrupt the flow of science for a few months, but those who make extraordinary claims must eventually produce the evidence. The more serious threat is to the public, which is not often in a position to judge which claims are real and which are voodoo. Those who are fortunate enough to have chosen science as a career have an obligation to inform the public about voodoo science."

Park's book was published in 2001, 12 years after the introduction of Fleischmann and Pons' claim. Park implies that the cold fusion episode seriously interfered with "genuine scientific discourse."

Certainly the scientific discourse surrounding the cold fusion controversy left much to be desired. Could it have run a better course? Only in hindsight. On the other hand, has Park's failure to do his homework, failure to report responsibly, use of character attacks and defamation benefited the "genuine scientific discourse?" Not in this author's opinion.

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