Correlation and cold fusion

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To improve collective understanding of cold fusion, we must recognize and understand the history. Cold fusion was called, by John R. Huizenga,, "*Cold Fusion: The Scientific Fiasco of the Century.*" He was correct. Gary Taubes titled his book, prominently, *Bad Science,* and then in much smaller text, *The Short Life and Weird Times of Cold Fusion.* Taubes was a writer facing no income until he finished the book. The 1989 DoE panel was charged with finding a quick answer in a field where quick answers did not exist.

What the Panel reported was not wrong, in that the evidence at that point was not conclusive. They correctly suggested further research. Taubes, in his rush to complete, did not consider Miles' heat/helium work.

Just as premature rejection led to a widespread belief that cold fusion was found to be a mistake "long ago," so too, reaction to that belief damaged our own work. We believed that it was necessary to produce 'better' results to convince skeptics, "better" usually meaning substantial and reliable heat. Yet the most significant missing evidence from the original work was not heat, it was the reaction product, and especially a correlated nuclear product.

Until this shifts, it is a confirmed characteristic of cold fusion that heat is erratic, not reliable. Steady, reliable results may be a sign of possible artifact … or possible fraud. That may change some day, but the tragedy is that the intense search for "better" results damaged the scientific study of the known effect. Miles showed the way in 1991 with heat/helium.. This considered astonishing by Huizenga "if confirmed," did not depend at all on reliability. It used the variability in heat as "self-control."

There were shortcomings in the Miles work, some of them addressed later, and all addressable by replication with more data from more samples, with increased precision and clearly-defined protocols. Basic confirmation of Miles took many years, and the best result so far, an individual test instead of a substantial series, still had an estimated precision of ten percent.

In the rush to confirm cold fusion, to vindicate Pons and Fleischmann, we lost the scientific method, in which one seeks to prove one's own ideas *wrong*, diligently.

Pons and Fleischmann had an idea that the effect they discovered was a bulk effect. If the reaction were taking place in the bulk, and if helium were the product, it would remain in the bulk, yet when the bulk was studied, helium was not found there. So they failed to release the promised helium analysis results from Johnson-Matthey, and deprecated helium measurement as too expensive, hence their heat-after-death work in France did not apparently include helium measurements. The net result was the waste of millions of dollars, and years of delay.

We need the mainstream. Running with one leg tied until we have communicated the realities of cold fusion to the scientific world, we need the interest of genuine skeptics, those who will actually investigate and sanely criticize what is claimed. That interplay is necessary for science.

We will explore the history (including recent), draw practical conclusions, and discover possibilities.

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