Cold Fusion at Texas A&M

Texas A&M University encouraged open, unfettered investigation of the concepts and phenomenology of "cold fusion" following the original announcement of S. Pons and M. Fleischmann of the University of Utah. Our facilities and faculty in nuclear science, electrochemistry, and thermodynamics have made possible the investigation of many of the conflicting statements and claims relating to the nuclear and thermal events proposed as being associated with these phenomena.

Since October 1989, groups of investigators in separate laboratories at Texas A&M have disagreed concerning results of experiments and their implications, particularly the reported observation of high levels of tritium (10^4 to 10^6 disintegrations per minute per milliliter). Although it was suggested by Gary Taubes in discussion with some of our investigators in the latter part of 1989 that tritium might have been added to the experimental cells by someone anxious to be able to report evidence of a nuclear process, our early and continued investigation of this possibility so far has failed to yield any evidence that this occurred. Currently it is believed by researchers at our Cyclotron Institute that the appearance of tritium arises mainly because of impurities in the materials from which the cells were constructed.

The statement in Gary Taubes' News & Comment article "Cold fusion conundrum at Texas A&M" (15 June, p. 1299) that "the response of A&M researchers and administration to these concerns was limited at best" is not accurate. The administration here has been aware of the concerns expressed by Taubes since mid-1989 and has questioned researchers concerning possible misconduct or unethical behavior. The lack of reproducibility of experimental observations always raises the question of the possibility of fraudulent reporting of experimental data. Most often it reflects inadequate experience, inability to control all the variable factors involved, or malfunction or misreading of instruments.

Meanwhile, researchers at Texas A&M University are being encouraged to continue to question the conflicting data, and each other, as is customary in all professional discourse on matters of importance in science and engineering. In the event that credible evidence arises from these activities and our ongoing review that the integrity of this effort has been breached, the university has established policies and procedures in place to take appropriate action in this circumstance, as it does in all areas of research and scholarship.

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To carry out research on the anomalous effects described by Fleischmann and Pons in 1989 is an endeavor needing much courage. It requires months-long electrolysis at very high current densities and then only a small fraction of the electrodes give the anomalous neutron, heat, and tritium bursts which some interpret as signs of fusion in solid-state confinement. Partly because of the difficulties of reproducing results, and partly because the branching ratio found does not fit the expectations of plasma physics, there is a very high degree of skepticism toward, and indeed hostility to, those who are trying to find the facts among anomalous happenings now reported from no fewer than 60 laboratories in 12 countries.

What was the purpose, then, of printing a gossip-based account which, by strong innuendo, suggests that a graduate student of mine faked his results? And why publish such a damaging account without first asking evaluatory comment on its content by those directly concerned?

1) There might be spot contamination by tritium of some pieces of old palladium, although it is difficult to understand why the metal hydroxide impurity surmised to have trapped the tritium does not yield it up when the hydroxide decomposes thermally during the melting (1552°C) the metal undergoes on the way to becoming a wire.

If any trapped tritium really were present in the virgin palladium, it would still have to undergo the process of anodic dissolution into the solution surrounding the palladium electrode where it is found. (However, the palladium is highly *cathodic*.) Attempts to find it in corresponding light water electrolysis, with the use of palladium later alleged to contain tritium, have failed. Calculations of the rate of anodic dissolution of the tritium show a calculated *rate* many orders of magnitude less than that observed when tritium is produced in palladium–heavy water electrolysis.

Reports of tritium in solution after electrolysis of heavy water on palladium have come from 26 laboratories, including three U.S. national labs. Some have used the so-called fusion palladium from Johnson Matthey, where the customer is the first user. I conclude that the hypothesis that the tritium observed in the so-called "cold fusion" comes from contaminated palladium is less likely than alternative hypotheses.

2) Results in the 10^3 to 10^6 disintegrations per minute per milliliter have been sporadically reported from many independent laboratories around the world. My graduate student would have had to do a great deal of unauthorized travel in order to spike solutions in the experiments of all these groups. It is difficult to believe that each of these organizations has its own secret spiker.

Objective tests for spiking have been devised by E. K. Storms and C. Talcott of the Los Alamos National Laboratory (*1*). They involve comparison between the behavior of the tritium activity with time after production in a purposely spiked solution and one which gets its tritium while evolving deuterium gas on palladium. The behavior we have in our cells, for the 20% which do produce tritium, is unmistakably consistent with the nonspiking behavior.

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REFERENCES

1. E. K. Storms and C. Talcott, private communication to J. O'M. Bockris, 25 June 1990.

I read with considerable interest the article "Cold fusion conundrum at Texas A&M." Several of the statements made by the author warrant comment. My response is focused on those statements pertaining to the research supported by the Electric Power Research Institute (EPRI).

Owing to the potential importance of the phenomena that Fleischmann and Pons claim to have observed, EPRI has been supporting a modest program of experimental studies to attempt validation of "cold fusion" claims and to further the scientific understanding of the phenomena.

From the beginning of these efforts, EPRI has been in contact with scientists around the world, including critics and skeptics, to discuss the results and to encourage collaboration in their interpretation. In parallel with our support of research, which includes investigations by three research groups at Texas A&M and by five other contracting organizations, a multidisciplinary EPRI team of electrochemists, nuclear physicists, radiochemists, thermodynamicists, and materials scientists has been examining the experimental designs and results. We are, as a consequence, well aware of the basic questions and specific criticisms surrounding cold fusion, including the startling but elusive observations of substantial amounts of tritium made at Texas A&M by the Bockris group.

EPRI's strategy has been, and continues

to be, the acquisition and documentation of reproducible results to set at least part of the puzzle on firm scientific ground. This approach has led to a current emphasis on observations of nuclear particles and the elucidation of their origin. Included in this work are the efforts of Kevin Wolf at Texas A&M to investigate contamination as a possible source of the tritium. In 1990 EPRI funding to Texas A&M has so far totaled $100,000. Further funding is being negotiated, subject to the preparation of additional experimental protocols.

It is important to note that high-level tritium observations, similar to those of Bockris, have been obtained by various groups at the Bhabha Atomic Research Center in Bombay. The work of E. K. Storms at the Los Alamos National Laboratory on the deliberate contamination of electrolysis cells with tritium produced a pattern of tritium behavior unlike that observed by Bockris. Since the tritium data is at an early stage of scientific investigation, speculation on fraud without additional supporting evidence appears to be premature.

The rationale for EPRI's continued research support is based on the many independent claims of observations of neutrons, tritium, charged particles, and excess heat (including those from five groups at U.S. national laboratories) for which a satisfactory non-nuclear explanation has not yet been found. Our view at EPRI is that the phenomena have not been proved. However, sufficient positive claims exist that further scientific investigation is warranted, as also recommended by the Department of Energy's Energy Research Advisory Board. We would consider it most unfortunate if the *Science* article, because of its limited scope and apparent bias, resulted in a bias against bona fide scientific research to resolve the cold fusion puzzle.

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Response: Comments on the substance of the article were elicited from Bockris on 14 March 1990 at Texas A&M and again on 30 March in Salt Lake City and from Nigel Packham on 13 March at Texas A&M. Both Bockris and Packham were informed during the interviews that an article was in the works and that they were being asked specifically to comment on the information that would be reported in the article. This is standard journalistic procedure. Their responses were then taken into account in the article.

As mentioned in the article, the only other

laboratory currently reporting tritium in the high range found at Texas A&M is the Bhabha Center in India. The rest of the "many" labs have either reported small incremental levels of tritium or have formally reported nothing at all. Scientists can only judge the reliability of data through publication of these data in peer-reviewed scientific journals. What is needed is the reporting of data and experiments that can speak for themselves, and a year and a half after the "discovery" of cold fusion those data and experiments are still talked about but not seen.

As for the work of E. K. Storms and C. Talcott, referred to by Bockris and sent to me as well, these researchers spiked a heavy water electrolysis cell with tritiated water, while monitoring the tritium concentration as a function of time. As expected, what they observed is a constant baseline level before the spiking, an instantaneous rise in tritium concentration (associated with the spiking), and then a linear decay in concentration after spiking, due to the dilution of the tritium by periodic addition of heavy water to replace that lost from electrolysis.

Storms, Talcott, and Bockris argue that Bockris's tritium data first show a rapid decrease in tritium content, and then a linear decrease due to the dilution by subsequent addition of heavy water. They interpret this rapid decrease as the release of deuterium-tritium gas generated in the cell, and the presence of this deuterium-tritium gas as incompatible with the spiking model.

This argument, however, does not appear to be substantiated by the experimental data. Bockris has shown only a single cell for which enough data points exist to define the shape of the tritium decay curve. In that cell, only a single point exists above the linear decay line. It is not scientifically justifiable to use a single data point to claim the existence of an effect, in this case a second decay lifetime. Therefore, using that second decay lifetime, the existence of which has not been established, to further prove that the tritium cannot be due to spiking is even less justifiable. —GARY TAUBES

Viral Etiology of AIDS and the Gallo Probe

The News reports by Barbara J. Culliton (22 June, p. 1494) and Ellis Rubinstein (22 June, p. 1499) contained illuminating, new information about the evolution of our knowledge about the viral etiology of AIDS. As an old virologist who has worked on human viral diseases for almost 60 years, I believe that the issue is not the honesty and