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Cold Fusion Thirty Years Later

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Cold Fusion: 30 Years Later

In March 1989, the claim of a revolutionary discovery in nuclear energy production galvanized the scientific community. It turned into a classic case of pathological science – and a textbook example of the self-correcting nature of science.

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The quest for controllable nuclear fusion as a societal energy source has been multigenerational, expensive, and slow. The benefits of fusion – including a near-inexhaustible fuel source, relatively mundane and non-polluting products, significant amounts of energy produced – are balanced by the technical difficulties and equipment involved. Research into this area is so expensive that support from nation-state entities are typically necessary. It goes without saying, then, that any breakthrough in fusion research, especially one that radically simplifies and cheapens it, would be a major breakthrough indeed.

Such was the setting 30 years ago when a major breakthrough was announced. What followed is now considered a classic case of how science works – and how science isn't supposed to work. Here, I will review what happened, what resulted, and consider how the entire series of events is viewed today.

Announcement: "Cold" Fusion

On 23 March 1989, officials at the University of Utah announced that two electrochemists, Stanley Pons and Martin Fleischmann, "established a sustained nuclear reaction" (Taubes 1993, p. xix). Leaks to the media the day before guaranteed a large crowd at this press conference, and by the next day it was front-page news all over the world.

The press conference was prompted by two issues. First, of course, was the news itself: that apparently fusion had occurred, that it occurred in a relatively simple and inexpensive apparatus, and that one of the Holy Grails of science – if anything can be considered a "holy grail" in science, surely it is sustainable nuclear fusion – was achieved, and by a pair of electrochemists, not nuclear physicists. But second, there were also whiffs of competition – a researcher at Brigham Young University (BYU), Steven Jones, was not only working on fusion via another process, but was apparently beginning to dabble in electrochemistry with his own research group. The two research teams were aware of each other, and each one wanted to be the first to announce success; or at least in the minds of some, announce jointly. Glory, money, and prizes were at stake – and scientists are first, foremost, and (perhaps) unfortunately human.

During the press conference, University officials, along with Pons and Fleischmann, described in general details how the experiments worked: palladium and platinum electrodes (ultimately revealed as pencil-like rods) were immersed in a solution of heavy water[1] (D_2O, D = deuterium = ^2H) and an electrolyte (also ultimately revealed to be lithium deuteroxide; see below regarding concerns about the minimal release of pertinent details). Current was passed through the electrodes, and the palladium electrode apparently absorbed the deuterium atoms and somehow induced them to fuse.[2] The press conference and the accompanying press release (University of Utah 1989) claimed the detection of excess heat, gamma rays, and free neutrons that the electrochemists claimed were consistent with a nuclear reaction occurring inside the palladium electrode.

Because news of the press conference's content had leaked the night before, the place where the meeting was held, the lobby of the Henry B. Eyring Chemistry Building on the University's campus (Taubes 1993, xvii) was packed, and the next morning (a Friday) the news of sustained, so-called "cold", fusion was front-page news. By the end of that weekend, the governor of Utah pledged $5 million from the State for further work in an attempt to break the "Utah effect", a name given by the University's then-president Chase Peterson for the apparent negative attitude of others in the country towards all things Utahn. Peterson envisioned that Utah would becomes the country's, if not the world's, leader in cold fusion technology, transforming the state into the Great Basin's version of Silicon Valley. The university had already taken steps to patent this new technology. There was a lot at stake.

The day before (Taubes 1993, p. 440), Pons and Fleischmann had faxed a revised manuscript to the editor of the *Journal of Electroanalytical Chemistry* (*JEAC*). Copies of the manuscript were also faxed to several colleagues, who in turn faxed it to other colleagues, and distribution of the unpublished manuscript spread rapidly.[3] Electronic mail was also nascent at this time, and news also spread via this electronic communications medium.

What's interesting is that this avenue had been explored before, apparently unbeknownst to the Utah researchers. In the 1920s, German chemists Fritz Paneth and Kurt Peters at the University of Berlin attempted to produce helium by fusing hydrogen, then deuterium, using hot palladium. Their incentive was to produce helium, not energy, because of the lack of sources of domestic helium for airships. They were unsuccessful. In 1927, a Swedish scientist named John Tandberg tried to patent an electrochemical method of producing deuterium using palladium, but it was denied and his experiments are also deemed to have been failures (Collins and Pinch, 1994; Taubes 1993, p. 214; Huizinga, p. 13).

The Evidence

What was the evidence produced by Pons and Fleischmann that fusion was occurring? It was largely based on electrochemistry, which was no surprise; both of them were well-known electrochemists. Fleischmann had recently retired from the Faraday Chair of Chemistry at the University of Southampton after a distinguished career, including development of the ultramicroelectrode (UME) for studying electrochemistry in very tiny volumes, including individual living cells. He also had a role in the discovery of surface-enhanced Raman scattering

(SERS), a technique that increased the sensitivity of a useful method of spectroscopy by a billion-fold, significantly increasing its utility. Although Pons did not have similar landmark events in his career, he was known for his ability to construct instrumentation, going so far as to have his own company providing electrochemical equipment. He was also a very prolific researcher, producing dozens of published papers per year at his peak.

The evidence they claimed was:

1. Excess heat/power. In the course of an experiment in which heavy water was being electrolyzed (ultimately, converting D_2O into deuterium and oxygen gases) by palladium and platinum electrodes, the apparatus exploded while operating overnight, when no one was present. The apparatus was at least partially destroyed and the palladium electrode was partially vaporized. The concrete floor underneath the apparatus was damaged, although reports differed on the details of the damage. This event occurred in the autumn of 1984 or winter of 1985.

To Pons and Fleischmann, the only thing that could have generated enough energy for this to occur had to be nuclear in nature. Indeed, Fleischman had been speculating on this idea as far back as the 1960s, according to the University of Utah press release (University of Utah, 1989). Hydrogen and its isotopes were known to be absorbed into metallic palladium at different rates, so Fleischmann apparently wondered if there were different nuclear effects involved.

The explosion set off a flurry of new studies focusing around the calorimetry of similar electrochemical cells. Calorimetry is the study of the energy changes of processes, and Pons and Fleischmann and their associates (some students, some University of Utah staff) set up a variety of calorimeters to measure the energy changes occurring in these deuterium-palladium cells (See Figure 1).

Pons and Fleischmann reported at, at least in some cases, they had seen temperature spikes in some cells that could not be balanced by the amount of electric energy going in (which was constantly measured, they claimed). It would take days or even weeks to see these spikes, during which time they presumed that the palladium electrode was absorbing deuterium atoms. At some critical juncture, the metal electrode was so loaded with deuterium that the atoms fused – that was only thing, they argued, that could explain the production of so much excess energy in a short amount of time.

2. The release distributed at the press conference implied that the experiment also generated excess neutrons (and tritium; see below), a well-known product of deuterium fusion. The relevant reaction is (Kikuchi et al., 2012, p. 22)

deuterium nucleus + deuterium nucleus $\rightarrow$ helium-3 nucleus + neutron (2.45 MeV)[4]

The electrochemists did not detect the neutrons directly; rather, the neutrons supposedly emitted by the fusion reaction interacted with hydrogen atoms in the water bath surrounding it. Hydrogen atoms are a reasonable absorber (the technical term is “moderator”) of neutrons and can react according the reaction

hydrogen atom + neutron $\rightarrow$ deuterium atom + γ photon (2.22 MeV)

Here, “γ photon” is a gamma-ray photon having an energy of 2.22 MeV. Gamma rays are more straightforward to detect than neutrons, and having a gamma-ray signal at an energy of 2.22

MeV is characteristic of that reaction, and therefore good evidence for the production of neutrons.

When the Pons & Fleischmann was distributed, either by fax or when published, the gamma ray/neutron production evidence they gave was much like shown in Figure 2. While they present this evidence (called a “spectrum”) as support, later investigation found serious problems with the veracity of this evidence.

3. The release distributed at the press conference implied that the experiment also generated tritium. Tritium is also generated by deuterium-deuterium fusion, according to the reaction

deuterium + deuterium → tritium + proton + energy

This reaction occurs about 50% of the time, while the reaction producing helium, above, occurs about 50% of the time, allowing for two obvious methods to detect deuterium fusion.[5] Here, the amount of energy does not matter; what matters is that tritium is an isotope of hydrogen with a single proton and two neutrons (tritium = T = ^{3}H). What is interesting about tritium is that it is radioactive, giving off beta particles (high-speed electrons) which can be measured with a different kind of detector that measures the beta particle energy. The beta particle energy curve ultimately published by Pons and Fleischmann claiming evidence for production of tritium looked similar to that drawn in Figure 3. (It is worth nothing that in the original publication, the two curves looked very similar to each other.)

There's little hint of how long this press conference lasted, in part because it was apparently followed by a tour of some of the lab facilities.[6] But there is no doubt what impact this announcement had on the scientific, political, and even military worlds.

The BYU Contribution

A better understanding of the dynamics of the press conference can be had by knowing that a physicist at nearby Brigham Young University (BYU) was also studying "cold" fusion, but via different mechanisms. However, word spread between the two campuses and concerns of priority and propriety began circulating.

Steven E. Jones was a physicist at BYU whose specialty was nuclear reactions; in particular at that time, the fusion of hydrogen atoms when catalyzed by muons. Muons are short-lived particles identical to an electron save that they have a mass 207 times larger. Because they are so much larger, muons can get closer to a hydrogen atom nucleus, and even induce two nuclei closer together – close enough to fuse, in some cases. This phenomenon was originally discovered in the 1950s (Huizenga 1993, p. 15); Jones and collaborators even published a review of muon-catalyzed fusion in *Scientific American* in 1987 (Rafelsky and Jones 1987). At the time, Jones' work was being supported by the US Department of Energy but there was talk of this support ending, in part because there seemed little future for this process as an energy source, and that programmatic support was only supposed to last a maximum of three years.

Ironically, however, in September 1988 Jones' program officer at DOE, Ryszard Gajewski, sent him a proposal from Pons and Fleischmann in which they requested funding to support further research in palladium-induced cold fusion. Over the next few months, Gajewski urged Jones to contact his fellow researchers at the University of Utah and forge a collaboration.

Jones apparently did make contact in mid-December 1988 (Taubes 1993, p. 50). However, though there were several fits and starts, even meetings between the researchers and their respective administrations, attempts to do something collaboratively ultimately failed.

It is worth pointing out that it was very clear to some (including Jones) that Jones' work was not in direct competition to the work of Pons and Fleischmann – for example, Jones did not measure any excess heat, and the number of neutrons he detect numbered in the hundreds at most. However, now that both research groups were aware of each other, the darker side of human nature grew in the minds of several of the people involved – culminating, ultimately, in the press conference of 23 March 1989. BYU followed up with its own press conference, focusing on Jones' less-fantastic version of fusion, on 29 March 1989. Despite the more low-key approach, this announcement served to muddy things further (literally, as is mentioned below).

The Response

Because some national evening news programs carried news of the wonderful scientific advance, reaction by the scientific community was swift and thunderous.

On the one hand, there were immediate hails of congratulations for such a breakthrough – and by chemists with hundreds or thousands of dollars of equipment, not physicists with hundreds of *millions* of dollars of equipment! Even Edward Teller, a giant in the nuclear physics field, called to congratulate Pons (Huizenga 1993, p. 5).

Others, however, were more circumspect, and many of these were scientists who had more experience in calorimetry and nuclear science. Phone calls, emails, and faxes (only recently having become prevalent) were all pressed into service to try to get more details than the press conference provided – because the devil *had* to be in the details.

However, details were difficult to obtain, in part on the desire of the University and the State of Utah to protect its intellectual property. When details were forthcoming – the *JEAC* manuscript was published on 10 April, less than three weeks after the press conference – they were either of limited help or added to the confusion. For example, even though Jones' work was originally presented as not competing with the Pons and Fleischmann claim, attempts to reproduce his work as well – just in case – were stymied by the list of electrolytes in Jones' electrochemical cell: a "Mother Earth soup" (Taubes, 1993, p. 151) – a mix of about 10 metal salts.

In any case, scientists – especially those with expertise in electrochemistry and, to a lesser extent, calorimetry – raced to repeat this work. So much palladium was purchased in a relatively short time that the price spiked (see Figure 4). Almost immediately researchers identified issues with the information released (and then published) by the Utah researchers, including:

- a lack of controls using non-deuterated water;
- an unrealistic gamma ray spectrum supporting neutron production (demonstrated in Figure 2); to wit, a single smooth curve is not a normal-looking gamma ray spectrum, which usually includes other features (including a Compton edge and signals from naturally-present gamma rays);
- a significant lack of neutrons claimed; the amount of excess heat generated should have generated enough neutron radiation to give the researchers radiation poisoning (at the very least), yet the number of neutrons claimed was many orders of magnitude lower than expected;

- a significant lack of tritium, as well as a lack of recognition that by virtue of its production process, heavy water is slightly enriched in tritium as well.

Still, scientists did what scientists should do – try to replicate. What happened?

Mostly, they failed to replicate. Even those few who claimed to replicate (see below) produced wildly inconsistent results.

One of the earliest and most thorough critics of Pons and Fleischmann's work was Nathan S. Lewis, an electrochemist at the California Institute of Technology (Caltech). Lewis, although still in his early 30s, was already a rising star in the field of electrochemistry, having earned his Ph.D. under Mark Wrighton, another well-known electrochemist, at MIT in 1981. Not only was Lewis well-versed in electrochemistry, but he had colleagues at Caltech who were similarly well-versed in detecting neutrons and tritium, measuring gamma rays, and understanding nuclear processes. And, they had the proper equipment and expertise in all these areas. After multiple attempts at detecting all sorts of emissions from their palladium-containing electrolytic cells, they found – nothing.

In the meantime, the American Chemical Society was having its biennial National Meeting in Dallas, Texas, in early April and hastily (and in temporary suspension of certain bylaws) organized a symposium to discuss the Pons and Fleischmann work. The symposium was eventually held in the basketball arena of the Dallas Convention Center, capacity 10,000. The symposium was held on 12 April, less than three weeks after the original press conference. Several speakers preceded Stanley Pons on the podium,[7] who presented essentially the same data that had been previously released. Despite several pointed questions from the audience, he received a standing ovation from the estimated 7000 attendees (Taubes 1993, p. 206; Huizenga

1993, pp. 31 – 4). It might not be improper to use the word "lionized" to describe how Pons was treated by his fellow chemists.

However, in the weeks that followed, more and more scientists of various nationalities and specialties reported in: no confirmation. What follows is not an exhaustive list of contributors, but should give an idea of the firepower aimed at this issue:

- Nathan Lewis' efforts (see above), including efforts of other collaborators at Caltech, were unanimously negative.
- Steven Koonin, a Caltech theoretical physicist, theoretically modeled deuterium-deuterium fusion and predicted it to occur at a rate approximately 50 orders of magnitude (that is, 10^{50} times) slower than Pons and Fleischmann reported.
- Charles Martin, an electrochemist at Texas A & M University and friend of both Pons and Lewis, initially reported excess heat in heavy-water electrolysis and reported this to both Lewis and Pons. However, a few days later, he also found excess heat in light (that is, regular) water electrolysis, causing him to rethink his findings. After a significant amount of additional work, by early June he concluded that there was nothing unusual occurring and that Pons and Fleischmann were "wrong" (Taubes 1993, p. 300).
- Richard D. Petrasso of MIT did an extensive study of neutron capture by water and demonstrated rather conclusively (even, apparently to the satisfaction of Pons and Fleischmann) that the gamma ray spectrum produced by the two researchers (reproduced in Figure 2 here) was spurious – that is, incorrect.
- The United Kingdom Atomic Energy Authority at Harwell, Oxfordshire, England ("Harwell Labs"), which had a longstanding relationship with Martin Fleischman, worked with Fleischmann to reproduce the work (in fact, after the press conference

Fleischmann went to England for the Easter weekend and then went to work with Harwell scientists to share information about cold fusion). Long story short – all attempts at Harwell came to nothing, and on 15 June 1989 Harwell suspended its experiments in cold fusion, having spent approximately £230,000 (the equivalent of £544,000 as of 2017, or approximately $720,000 [2017 dollars]).

- James Mahaffey at Georgia Tech Research Institute initially claimed the detection of excess neutrons, but retracted that claim after it was found that the neutron detector was heat sensitive – even to the heat of the hands handling them.
- Ed Storms and Carol Talcott at Los Alamos National Laboratory claimed to see increased tritium amounts in several cells, but their experimental technique was a bit of a departure from the norm, as they intentionally poisoned their palladium electrode with sulfur. A larger issue, however, was that they apparently operated 150 heavy water electrolysis cells – but only 4 light water control cells. Even though they reported increased tritium in 13 of their 150 heavy-water cells, their results were viewed as statistically meaningless given the tiny number of controls.

This list is incomplete, as many other researchers and labs were working around the world to confirm the original results.

There were some champions, however (again, this is not an exhaustive list):

- Robert Huggins of the Materials Science Department at Stanford University claimed to have observed excess heat, although not as much as Pons and Fleischmann did. One of his claims was that researchers who failed to replicate the results did not have the requisite materials science background (Huizenga 1993, p. 56). Ultimately he became one of the most resolute non-Utahn supporters of cold fusion.

- John Bockris, an electrochemist at Texas A & M University, a colleague of Charles Martin (see above). Bockris' achievements rivaled those of Fleischmann's, if not overshadowed them. Bockris and his group investigated. On or around 24 April, Bockris and his group got data that indicated a significant production of tritium. So much so, in fact, that it seems to them obvious that fusion was occurring. Several days later, a few other cells showed significant tritium presence. Supporters latched on to this new evidence hard – Bockris included, ultimately becoming a second resolute non-Utahn supporter of cold fusions. Inconsistencies in data and results from other cells suggested that instead of fusion, some of the cells were intentionally spiked with tritium to satisfy the expectations of the research advisor (Huizenga 1993, pp. 114-129; Taubes 1993, pp. 326 – 7).

Later Developments

On 1 and 2 May 1989, there was a special session on cold fusion at the American Physical Society (APS) meeting in Baltimore, Maryland. Unlike the reception at the chemistry society meeting less than three weeks previously, at this meeting there was a lot more evidence – mostly negative evidence – to consider, and the reception towards cold fusion was skeptical, if not hostile. (In fact, neither Pons nor Fleischmann attended, even though both of them had been invited.) Jones from BYU spoke, presenting his results and again reiterating that his results were different than the claims out of the University of Utah. But then Koonin and Lewis, both from Caltech, spoke and delivered what can only be described as a scientific one-two knockout punch. Another standing ovation occurred.

But it wasn't over. Only a week later, on 8 May 1989, the Electrochemical Society (ECS) held its Spring Meeting in Los Angeles. Both Pons and Fleischmann attended, determined to defend their work. Caltech's Lewis also attended. The mood was not as negative as the APS meeting, but the overall tone was still very critical of the original work. There were some supporters of cold fusion at the ECS meeting, however (Huggins and Bockris among them), but their claimed positive results were inconsistent. The fact that Pons and Fleischmann were speaking to fellow electrochemists and apparently failed to win converts was very telling.

About two weeks later was a third conference devoted to cold fusion, this time organized by Los Alamos National Lab and the Department of Energy, this time held in Santa Fe, New Mexico, on 23 – 25 May 1989. This particular meeting was significant because in attendance were members of the Energy Research Advisory Board (ERAB), a committee appointed by the Department of Energy to formally evaluate the cold fusion claims. Again, although invited, Pons and Fleischmann did not attend. It was, as ERAB chairman John R. Huizenga reported, "a most unusual scientific conference where experimental claims were contradictory and chaotic" (Huizenga 1993, p. 75). Most of the attendees reported null or negative results, and those attendees who claimed positive results weren't reproducing each other, a classic sign of negative results.

This meeting did result in ERAB deciding to visit some of the principals of cold fusion, which they did. Ultimately the visits were, in a word, "disappointing" (Huizenga 1993, p. 87), especially given the fact that they were never shown a working cold fusion cell at any visit! After these visits, ERAB met to construct their final report, which was issued in November 1989. It was negative on all counts, and recommended against investing large amounts of money and time on further investigations.

Still, cold fusion lingered. Despite the overwhelming evidence against cold fusion, the State of Utah opened the National Cold Fusion Institute (NCFI) in August 1989 with its promised $5 million. However, after fits and starts, and after all attempts to re-energize cold fusion as a legitimate process, the Institute closed on June 30, 1991. Experiments continued around the world, mostly by believers, but no smoking gun has ever been identified.

Denouement

Currently, the strongly predominant view among scientists is that cold fusion does not exist – at least, not at the level claimed by Pons and Fleischmann or their supporters. However, "believers gonna believe," and even today there is a low level of interest in cold fusion and related phenomena, now euphemized as "low energy nuclear reactions" (LENR). Although mostly relegated to conferences and even journals of their own making, the subject does occasionally appear at an ACS or APS meeting. Publications have dropped off significantly but not entirely as shown in Figure 5, although it could very well be that citations of cold fusion are done more as part of historical background rather than current research.

What happened to the three main characters, Jones, Pons, and Fleischmann? Jones remained at BYU and continued studying muon-catalyzed fusion. After the events of September 11^{th}, he joined the side of the 9/11 conspiracy theorists, claiming that jet fuel fires could not melt the steel beams of the Trade Center buildings, arguing for demolition instead. After investigations into his 9/11 claims at BYU, Jones took early retirement and has apparently dedicated himself to that topic. Blogs on at least one website purported to be scholars searching for "9/11 truth" were still active as of late 2016.

In October 1990, Pons' attorney faxed a request for a sabbatical for his client, but it was denied. Pons retired from the University of Utah effective 1 January 1991, and subsequently moved to France, where he eventually obtained French citizenship. There, he worked with Fleischmann on cold fusion-related work that was sponsored by the Technova Corporation, a subsidiary of Toyota. The work was shut down in 1998, and as of this writing Pons continues to live in France.

Fleischmann moved back to his home in England but continued to work with Pons in the French lab. He remained active as a publishing scientist, collaborating with US and European researchers. He died on 3 August 2012 in England at the age of 85 from natural causes.

Lessons

Despite the low level of continued work in this area (see Figure 5), cold fusion is now considered a classic example of pathological science as defined by Irving Langmuir in a talk given at the Knolls Research Laboratory in Niskayuna, New York (Langmuir, 1953). It has been discussed in detail by Huizenga (Huizenga 1993), so it will not be reiterated here. To those who would argue that the cold fusion affair is an example of what is wrong in science, I would concede that they are partly correct. Space prohibits the inclusion of more details here, but a reading of those details in the several books on the subject (see References) makes it clear that certain obvious deviations of the regular scientific process occurred, leading up to and including the original 23 March 1989 press conference, and continued long after. A lot of things went wrong that should not have, and would not have, had the proper standards of scientific inquiry been followed.

To those who argue that the cold fusion affair is an example of what is right in science, I would also agree (and would, overall, take this side). Despite the obviously unorthodox way the cold fusion events began, scientists ended up doing what scientist do: They tried to understand and replicate the work, and when not being able to do so, said so and even criticized the original results. In the end, *science worked*. Was there the expenditure of a large amount of time and money and effort? Yes, but I would argue that the resources utilized were commensurate with the importance of the claim. We now have a better understanding of what works, *what does not work*, and most of us who lived through the process have moved on. If there is any positive aspect to the entire affair, it should continue to live on as a classic example of What Not To Do In Science.

Notes

1. Heavy water is water composed of deuterium atoms instead of hydrogen atoms. Deuterium is an isotope of hydrogen whose nucleus contains one proton and one neutron, as opposed to the single proton only for "normal" hydrogen atoms. Because it has almost twice the mass as regular hydrogen, water composed of deuterium is called "heavy" water.

2. While an unusual effect, the absorption of hydrogen by palladium metal is a well-known phenomenon and has been the subject of much study by various interests, including the development of hydrogen storage devices for fuel cells.

3. My first exposure to the manuscript was as a fax that was received by the research group I was working in as a post-doctoral researcher with at the Lawrence Berkeley (now Lawrence Berkeley National) Laboratory. Fax machines, in addition to email, were only then becoming widespread.

4. MeV = megaelectron volt. 1 MeV = 1.602×10^{-13} J. This may not seem like a lot of energy, but it's enough energy to break about 2 *million* chemical bonds.

5. A third reaction, production of ^{4}He and a gamma ray photon, has a much lower possibility of occurrence (Huizenga 1993) and can be ignored here.

6. A truncated video of the press conference on YouTube is just under 39 minutes long.

7. As a Fellow of the American Chemical Society, I have requested but not yet received any information about that symposium.

References

Collins, H, and T. Pinch. 1994. *The Golem: What Everyone Should Know About Science* Cambridge: Cambridge University Press.

Fleischmann, M. and S. Pons. 1989. Electrochemically induced nuclear fusion of deuterium. *Journal of Electroanalytical Chemistry* 261 (2A): 301 – 8.

Huizenga, J. 1993. *Cold Fusion: The Scientific Fiasco of the Century* Oxford: Oxford University Press.

Kikuchi, M., K. Lackner, and M. Q. Tran. 2012. *Fusion Physics* Vienna: International Atomic Energy Commission.

Langmuir, I. 1953. Seminar at Knolls Research Laboratory, Niskayuna, New York. Transcript available at https://www.cs.princeton.edu/~ken/Langmuir/langmuir.htm. Accessed 5 July 2018.

Rafelsky, J., and S. E. Jones. 1987. *Scientific American* 257 (1): 84 – 9.

Taubes, G. 1993. *Bad Science: The Short Life and Weird Times of Cold Fusion* New York: Random House.

University of Utah Press Release. 1989. Available at https://www.colorado.edu/physics/phys3000/phys3000_sp16/handouts/64_ColdFusionPressRelease.pdf. Accessed June 2018.

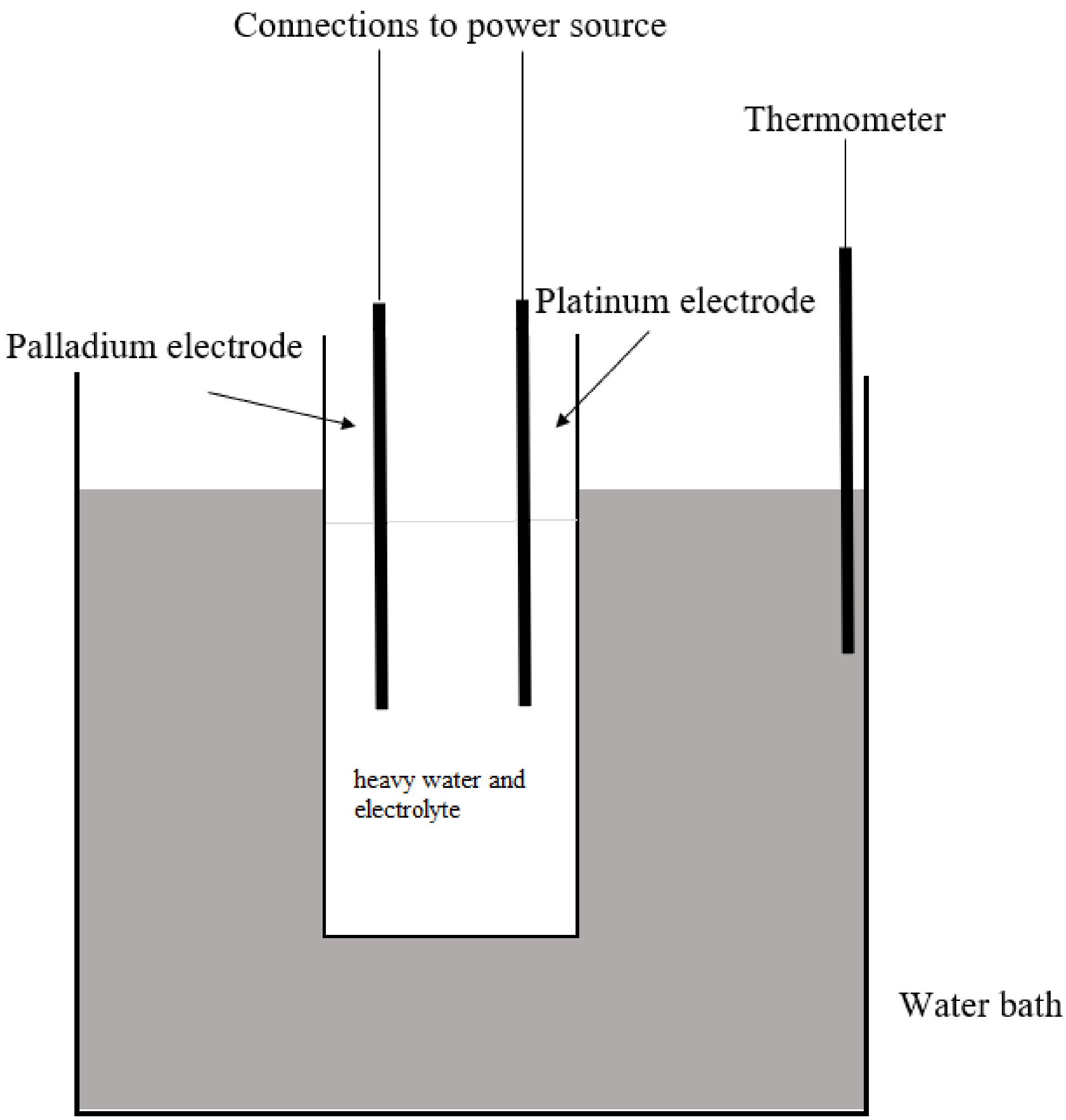

Figure 1. Rough diagram of the Pons-Fleischmann electrolytic cell.

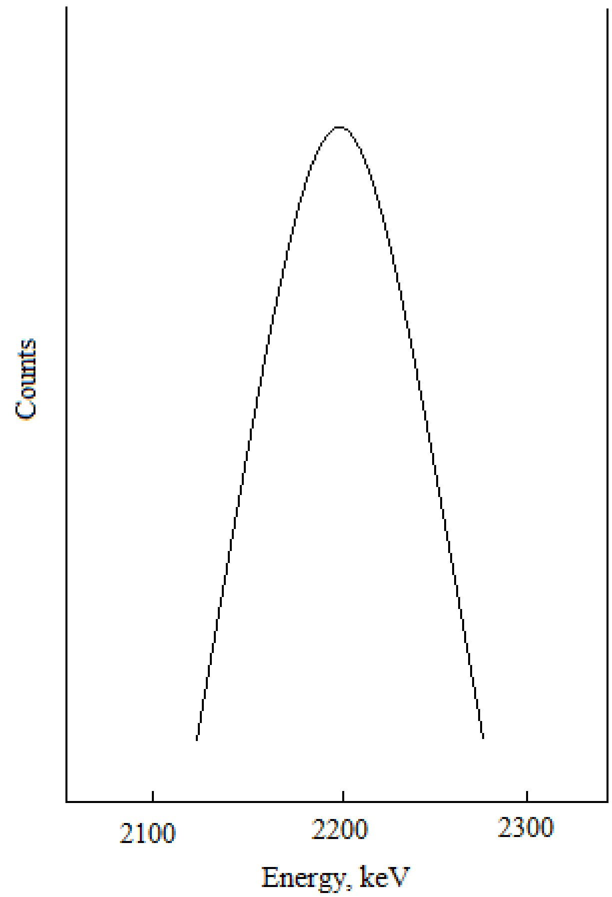

Figure 2. A representation of the neutron-induced gamma ray spectrum published by Pons and Fleischmann. Adapted from Pons & Fleischmann 1989.

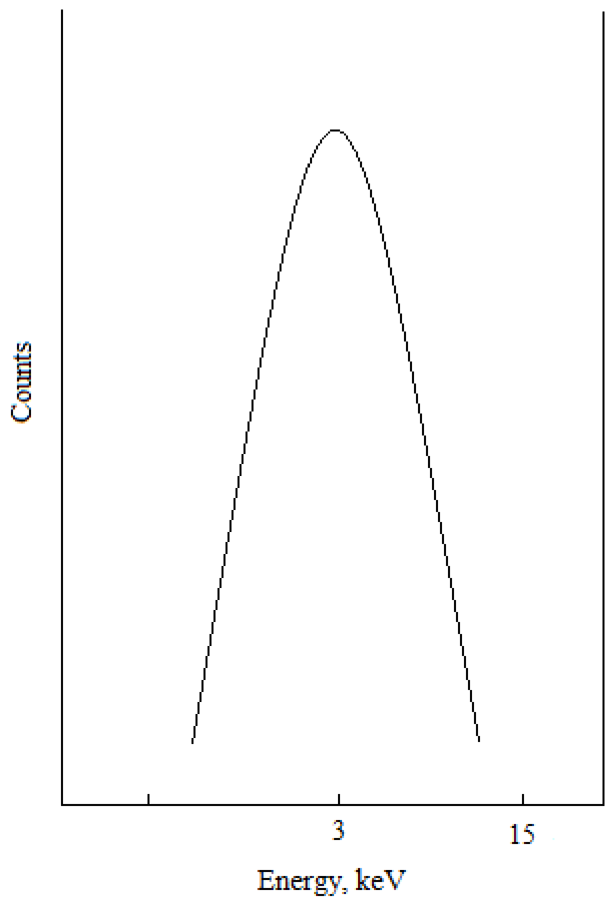

Figure 3. A representation of the beta particle energy spectrum published by Pons and Fleischmann claiming the presence of radioactive tritium. The first tick on the x-axis was unlabeled. Adapted from Pons & Fleischmann 1989.

Figure 4 – Price of palladium ($/oz) between March and September 1989. Source: APMEX, Inc. Graph generated at www.apmex.com. Accessed 3 July 2018.

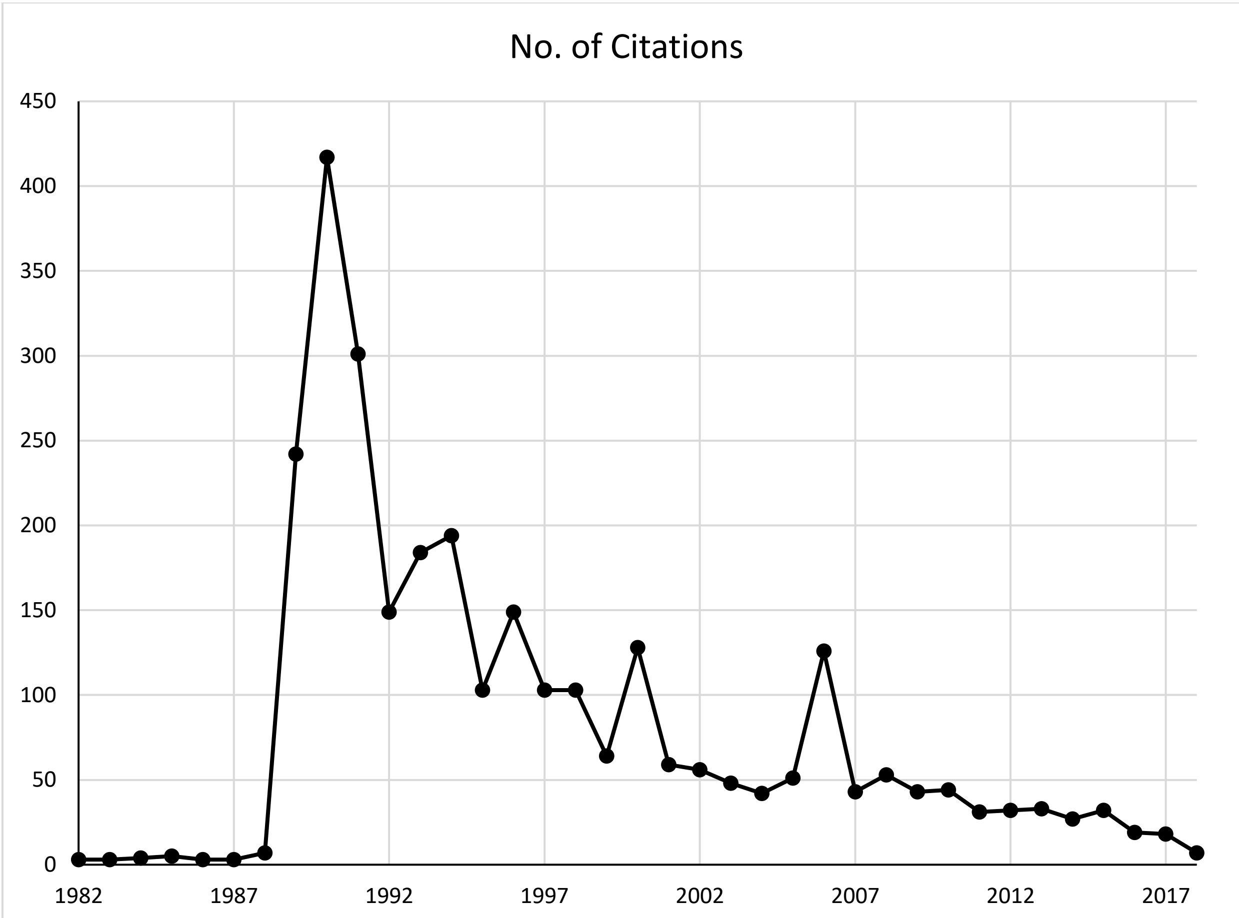

Figure 5 – Number of citations in the Chemical Abstracts online database using "cold fusion" as a keyword. The peak occurs in 1990. The reason for the blip for 2006 has not been investigated. Data for 2018 is year to date.