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Research Article

Lessons from Cold Fusion Archives and from History

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Abstract

The field is somewhat chaotic. Results are inconsistent and seem contradictory. There is no widely accepted theoretical explanation. History shows that this kind of chaos is healthy in emergent science. In fields such as plasma fusion there is broad agreement and a solid theoretical basis, but not much progress. We should embrace chaos and celebrate intellectual ferment. Despite the confusion, the literature does prove the effect is real, and it teaches how to replicate. The literature includes many failed experiments. There are two kinds: amateur mistakes and noble failures. At Kamiokande they made amateur mistakes such as holding the palladium in their bare hands. To avoid such mistakes you should read textbooks, read the papers at LENR-CANR, and consult with an electrochemist. A noble failure would be Srinivasan spending six months at SRI trying to replicate the bulk nickel-hydrogen excess heat reported by Mills and replicated at BARC. Srinivasan concluded that he had no significant heat, and that the BARC results were in error. Success will only come thanks to failures such as this.

Keywords: Excess heat, History, Materials, Palladium

1. Introduction

There are about 2000 documents at LENR-CANR.org, ranging from the sublime to the ridiculous. What can we learn from all these papers? I have not read them all but I have read hundreds, and I think these are the most important lessons:

(1) Cold fusion is chaotic, and that is a good thing.
(2) The literature does prove the effect is real and it teaches how to replicate. I will point out specific papers that show how to replicate.
(3) This is a multidisciplinary subject. That means you better read the literature and consult with experts, or your experiment will fail.
(4) Finally, the worst error you can make is an unexamined assumption.

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2. Chaos

History shows that chaos, confusion and doubt are normal in emergent science. They are healthy. Here is wonderful description of Hahn and Meitner:

"Their early papers are a mixture of error and truth as complicated as the mixture of fission products resulting from the bombardments. Such confusion was to remain for long time a characteristic of much of the work on uranium." – E. Segré (Quoted by Mallove, p. 22) [1]

We should embrace chaos and celebrate intellectual ferment. In plasma fusion there is broad agreement and a solid theoretical basis, but they have not made much progress. Cold fusion is still emerging. Yes, it has been 24 years by the calendar, but compared to plasma fusion this research is two months old. It has gotten roughly US $50 million since 1989, which is how much the DoE spends on plasma fusion every 2 months.[a]

History books sanitize the past. They hide the mistakes, the disputes and chaos that accompanied the birth of fission, and aviation, semiconductors, the laser, and many other breakthroughs. History books give you what I call the museum gallery illusion that the past was better than the present. When you spend an afternoon at the Metropolitan Museum of Art you may get the impression that all French painters in 1880 were consummate geniuses. For every Degas or Renoir there were dozens of third-rate artists. They never made it into the museums. If you want great art, you must settle for lots of schlock art. If you want great science, you must settle for a lot of second-rate science, and also many odd people.

You need many wannabe great artists because you never know who has talent. The early sketches by van Gogh were skilled but no more creative or memorable than the early work of many forgotten artists. A young person destined to do great work may not be aware of her own latent abilities. Martin Fleischmann, when you and I knew him, was hard-working. Long after he retired he would get up every morning and spend hours in his bathrobe puttering around writing papers and graphing data. He was not always so diligent. He told me once: "When I first arrived in England as a refugee I was the laziest boy you ever saw. The laziest boy in Europe. I found myself penniless and living in an abandoned chicken house. I said to myself, 'if you do not get to work, go to school and make something of yourself you will spend the rest of your life in a chicken house."' I suppose if there had been no war, Martin might have been the indolent son of a rich family all of his life.

3. Cold Fusion is Real

The next lesson from the literature is that cold fusion is real. In September 1990, Fritz Will published a list of 92 researchers with positive results [2]. After people such as McKubre, Storms and Fritz Will himself published in 1990, the controversy should have ended. No other claim in the history of experimental science has been so widely replicated at such high signal to noise ratios yet still not believed.

Prof. Heinz Gerischer was a leading electrochemist and the Director of the Department of Physical Chemistry at the. He reviewed the evidence in 1991 and concluded "there [are] now undoubtedly overwhelming indications that nuclear processes take place in metal alloys [3]." For a distinguished professor this is emphatic. "Undoubtedly overwhelming" is shouting through a megaphone. Most qualified experts I know who have read the literature agree that the effect is real. Most so-called skeptics have not read the literature. I will grant, science is not a popularity contest, but informed opinion should carry weight.

[a]The plasma fusion FY 2012 budget was US $248 million. ITER is now expected to cost US $23 billion. http://articles.washingtonpost.com/2012-06-25/national/35461417_1_nuclear-fusion-iter-fusion-power

Graham Hubler recently said that we need: “either more reproducibility, proof positive that a nuclear process produces the heat, or a viable model, none of which we have at present.” I disagree. I think the literature shows that cold fusion is more reproducible than the transistor was in the early 1950s. The tritium; and the heat correlated with helium; together are proof-positive that the effect is nuclear. We have no viable model, but two out of three isn’t bad. I fear that Graham means to we must start over from scratch with every project to prove that cold fusion exists, like Sisyphus pushing the rock up the hill. If your funding agencies do not accept the proof already published by Mike McKubre, they will not accept your proof either. You will have to wait for Rossi.

4. How to Replicate Cold Fusion

Not only is cold fusion real, but the literature does teach how to replicate. And I am going to teach you how to replicate, in the next five minutes. There are many valuable papers about this. I like these three [4–6]:

(1) Miles, M. and K.B. Johnson, *Anomalous Effects in Deuterated Systems, Final Report*. Naval Air Warfare Center Weapons Division.
(2) Cravens, D. *Factors Affecting Success Rate of Heat Generation in CF Cells*, ICCF4.
(3) Storms, E., *How to produce the Pons–Fleischmann effect,* Fusion Technology.

The Miles paper is a masterpiece. It is famous because it shows the correlation between helium and heat. But, what got my attention is here:

This compares the performance of palladium from different sources. It shows that material is critical to success. The NRL material was pretty good, Johnson Matthey was the best, and palladium from miscellaneous sources at the bottom did not work. In the Johnson Matthey section, the three worked spectacularly well, producing 3–15 W/cm^3, which is about 10 times better than the others:

The notation JM (F/P) means “Johnson Matthey palladium provided by Fleischmann and Pons.” Fleischmann himself handed over these two cathodes to Miles. Johnson Matthey gave Miles the other one from the same stock. Miles told me this was the best palladium he ever used. So, I asked Martin about it. Here is our conversation as I recall it many years later:

JR: “Mel says you gave him the cathodes that work so well.”

MF: “Yes, I handed out many samples, to Mel and to others. Here’s the thing. When Uncle Martin gives you palladium, it works! When other people give you palladium, it doesn’t work. What does that tell you?! Hmmmm?”

JR: “It tells me the secret is in the material. So how did you know what material to use?”

MF: “I didn’t know! I asked my friends at Johnson Matthey.”

Martin told Johnson Matthey he wanted to load palladium with hydrogen to high levels, and they recommended the palladium that they had developed in the 1930s for hydrogen filters. The people at BARC and NASA later used that material as is, inside of Milton Roy hydrogen purifiers. They reported it worked well.

Okay, you start with the recommended palladium. Then you prepare it by various methods such as polishing the surface and doing electrolysis at low power to condition the metal. When Fleischmann and Pons went to France with Toyota, they improved their methods of preparation. The methods were hush-hush, but we know what they are. Because, at ICCF4, Dennis Cravens described his methods. Fleischmann said: “That’s my favorite paper. Dennis revealed our secrets! That’s just what we do!” So, to learn how to prepare the palladium, read Cravens.

This brings us to the third paper, by Edmund Storms. This teaches how to recognize good material. That is, how to winnow it out. This study began when the Tanaka Precious Metals Company sent Storms 90 foil cathodes. Storms devised a series of tests to separate the good ones from the bad ones, and methods to coax them into working better.

These methods include some of the one Cravens recommended" Polish to a mirror finish; wash with acetone; after washing "do not touch with fingers or tissue paper," Load slowly. Storms also described several ways to test a cathode as he prepared it, to predict how well it will work. For example, he wants to know if the palladium is weak, so he looks for cracks, and he uses a handheld micrometer to measure how much the palladium swells up during initial loading. He rejects palladium that swells more than 2%. Storms tested the 90 cathodes and he found 4 that passed all tests. These four produced robust heat. They worked repeatedly. It took Storms about a year to test all 90 samples.

So, this is how you replicate: Ask Johnson Matthey for 90 pieces of hydrogen filter palladium. Master the techniques described by Cravens. Then, spend a year testing them. With luck you will find a few that work. If you don't, get another 90 pieces of palladium and start over.

Fleischmann and Pons' work in France—using Johnson Matthey palladium and the techniques described by Cravens—culminated in spectacular results. In experiment 3 they got 294 MJ of excess energy at 101 W. In experiment 4 they got 250% excess. [7] These results might have been the making of cold fusion. They might have convinced the world that the effect is real. Alas, the opportunity was squandered. This project collapsed, I was told, because of greed and politics.

After Martin retired, he and I tried to buy some of the hydrogen filter palladium. Johnson Matthey told us they had changed the method of manufacturing it.[b] They offered to make us a kilogram with the old formula for US $50,000. Martin and I did not have that kind of money. We asked several researchers if they would like to go in with us, but no one expressed interest. By that time Violante was learning how to make reliable cathodes. I suppose he is gradually rediscovering secrets that Johnson Matthey has known for 80 years. Another rock, another hill. Uncle Martin did not get a chance to hand out more palladium, and we never found out whether other people could replicate those spectacular results from France.

The work by Mel Miles also ended badly. When the Navy found out what he was up to, they demoted him from Distinguished Fellow of the Institute to a menial job as a stock room clerk. So he retired [8].

5. Failed Experiments

Continuing our tour of the dark side, let us look at some failed experiments. There are hundreds in the literature. It is not pleasant reading about them, but you should try to understand what went wrong. Let us look at two examples: Srinivasan, and Kamiokande. The people at BARC did many fine experiments. They produced definitive results, especially with palladium. Their results with nickel light water cells are a puzzling mix of success and failure. Srinivasan led three tests:

Here is what else they found in Series 3:

Series 1. BARC (1992). Produced excess heat and tritium

Series 2. SRI (1994). Srinivasan visited SRI and tried to replicate Series 1. After 6 months of effort, he concluded that the excess heat was caused by recombination. He admitted it was a mistake. This is a noble failure.

Series 3. BARC (1996). Back in India, in these tests they made no attempt to measure heat, only tritium. Tritium was again detected, but not at levels as high as the first series.

Here is what else they found in Series 3: The tritium kept vanishing in a "sawtooth fashion." The authors wrote: "A close scrutiny of our sampling, distilling, and counting techniques confirms that the decrease in tritium level is genuine and not attributable to any artifact. We are strongly tempted to suggest that there is an as yet unidentified mechanism periodically 'cleansing' the electrolyte of tritium." [9] These results are as confusing as Hahn and Meitner's were in the 1930s.

[b]They no longer melted the metal under cracked ammonia. Fleischmann and Miles felt that the ammonia might be important. The newer hydrogen filter palladium might work. It has not been tested as far as I know.

The literature is full of weird results like this one. They cannot all be right, but unless you have a magic touchstone, you can not tell which is right and which is not. Theory is no guide. This is frustrating. But I think it is wonderful. You probably do not see things like this at plasma fusion conferences.

Kamiokande. In these studies, the particle detection was superb. Unfortunately, electrochemistry was treated as an afterthought. In 1991 and 1992, they did two sets of tests. They used a total of 50 cells, including gas loaded and electrochemical ones, plus Portland cement made with heavy water. The graduate thesis by Ishida devotes 120 pages to particle detection but only a few pages to the cells [10]. It says "the whole preparation of electrolytic cells was entrusted to groups from BYU and Texas A&M." I have not found a description of the electrochemistry in the published papers. Years after the tests, I spoke with American and Japanese electrochemists who heard about the project. This is what they told me:

- Texas A&M sent instructions along with the cells; i.e. what voltage to use, how to prepare the cathodes. The researchers at Kamiokande apparently ignored these instructions.
- All cells were run on the same circuit, even though the anodes and cathodes were of different sizes and shapes. The cells were wired electrically in series at first, until one cell failed. Then they were rewired in parallel.
- The cells were run at high voltage from the start, because the researchers wanted to get on with the experiment quickly. You are supposed to gradually ramp up, as Cravens and Storms said.
- The researchers were shown on national TV pulling out a cathode with their bare fingers, waving it at the camera, and then putting it back. This contaminates the cathode. The Japanese electrochemists watching this were aghast [11]. Again, Cravens, Storms or any electrochemist would have warned them not to do this.
- They employed no diagnostics other than neutrons; they did not attempt to measure excess heat or loading.
- They did not ask Mizuno or any other electrochemist to assist.

From an electrochemist's point of view, these people were trying to tune a piano with a sledgehammer. McKubre described this experiment as "a profligate squandering of resource and opportunity." He said: "With a little more thought and care, a little less hubris, and the inclusion of just one electrochemist, this could have been a crucial one-off experiment." Ikegami described this as "fishing in a dry hole." It is unlikely any of these cells produced a cold fusion effect.

The key lesson is: read the textbooks, read the literature, consult with experts. Most of the failed experiments went off the tracks because the researchers did not do their homework. It really is that simple.

As I mentioned, Fleischmann asked Johnson Matthey to recommend palladium. John Bockris once said to me: "I am not an expert in calorimetry, so I scouted out the best expert in Texas and asked him visit our lab. He came, looked at the apparatus and the data, and then he laughed and said: 'You do not need me; anyone can measure that much heat!"' If Fleischmann and Bockris felt comfortable asking others for help, you should too.

6. Unfounded Assumptions

Finally, let us look at the unfounded assumption. The most egregious error is not doing your homework. The most pernicious error is the unfounded assumption. That is, an assumption so widely held and taken for granted that no one notices it. Here is a fascinating example from the history of biology: the half-century struggle to determine where the genetic code is stored.

By 1900, people knew the laws of genetics. They learned them from observations of fruit flies and other species. Quoting a textbook from 1916: "we may reasonably suppose" that "in the chromosomes is found the material basis of every inherited character." Elsewhere it says "we assume" that a particular characteristic is governed by two different genes which may be in different chromosomes[c] [12]. They knew a lot, and they also knew the limits of their knowledge. Textbooks emphasized that they did not know the physical nature of the gene, and they were speculating about it.

The first question was: Are genes stored in proteins, carbohydrates, or the nucleic acid? Most experts concluded it must be in proteins. Why? Because protein is complex and varied. The acid was ruled out because it seemed, quote: "too simple" [13],[d] and too "boring"[e].

To put it in modern terms: people thought that complex information has to be stored in a complex data storage medium. From our point of view, the notion that the acid is "too simple" sounds comical, because we are used to binary data storage in simple repetitive structures. We live in sea of information. Our computers store more bytes of data than there are grains of sand on all of the beaches of the earth [14].

There was some mechanical data storage back then, such as IBM punch-cards. Most people never encountered such things and had no feel for them. I doubt it would have occurred to a biologist in 1930 to consult with IBM engineers. Wide ranging cross-disciplinary collaboration is a great thing, but you have to have some idea who you want to collaborate with, and why. I do not think we know that either. Other people know the answers to our questions, but we do not know who they are, and they have never heard of us.

This "complicated storage" hypothesis had no basis in biology. It just drifted in through a window and settled in people's minds. Are there phantom unfounded notions holding back the development of cold fusion? John Bockris said: "The Coulomb barrier is a shibboleth! A myth!" The reader can be the judge of that. Many skeptics believe that all cold fusion reactions must produce a huge flux of neutrons. This belief is well-grounded in plasma fusion theory, unlike the "complicated storage" idea. Well-grounded or not, once the experiments proved there is no giant flux of neutrons, everyone should have put that belief aside. It has become an unquestioned assumption. A shibboleth. Are we making similar dogmatic mistakes? I can't tell; I would be as blind to them as you are. I can only caution you to be careful.

Let me close with the history of biology. By the mid-1940s they concluded that the gene is in nucleic acid. They were still floundering around trying to figure out the structure of DNA when along came James Watson, another lazy young man like Martin Fleischmann in the chicken house. By his own account, Watson was always ready to cut corners and goof off. He described a conference in Italy in 1951, a year before he discovered DNA. This is a picture of emergent science and the many odd people it attracts. It may make you feel better about our conferences:

"Much of the talk about the three-dimensional structure of proteins and nucleic acids was hot air. Though this work had been going on for over 15 years, most if not all of the facts were soft. Ideas put forward with conviction were likely to be the products of wild crystalographers who delighted in being in a field where their ideas could not be easily disproved. Thus, though virtually all biochemists . . . were unable to understand the arguments of the X-ray people, there was little uneasiness. It made no sense to learn complicated mathematical methods in order to follow baloney." [15]

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[c]Castle: "One mechanism will now suffice for all kinds of inheritance, this mechanism being found in the chromosomes. In them, we may reasonably suppose, is found the material basis of every inherited character. When the inheritance is of the simplest kind, involving presence or absence of color or some similar character, we assume that a genetic change has occurred in a single, definite locus in a particular chromosome, and that this single change is responsible for the observed inherited variation. Other characters depend on two or more genes, which may lie at different loci in the same chromosome, or even in different chromosomes. . . ."

[d]"Too simple" Asimov, I., A Short History of Biology. 1964: Natural History Press. ". . . it was taken for granted that the nucleic acid was subsidiary and that the protein was the thing itself. . . . Not only was faith in the protein molecule unshakable but, through the 1930s, all evidence seemed to point to the fact that nucleic acids were quite small molecules (made up of only four nucleotides each) and therefore far too simple to carry genetic instructions. The turning point came in 1944 . . ."

[e]"Boring" http://pablosorigins.blogspot.com/2009/11/history-of-dna-part-ii-proteins-vs-dna.html

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