Miracle at Salt Lake City

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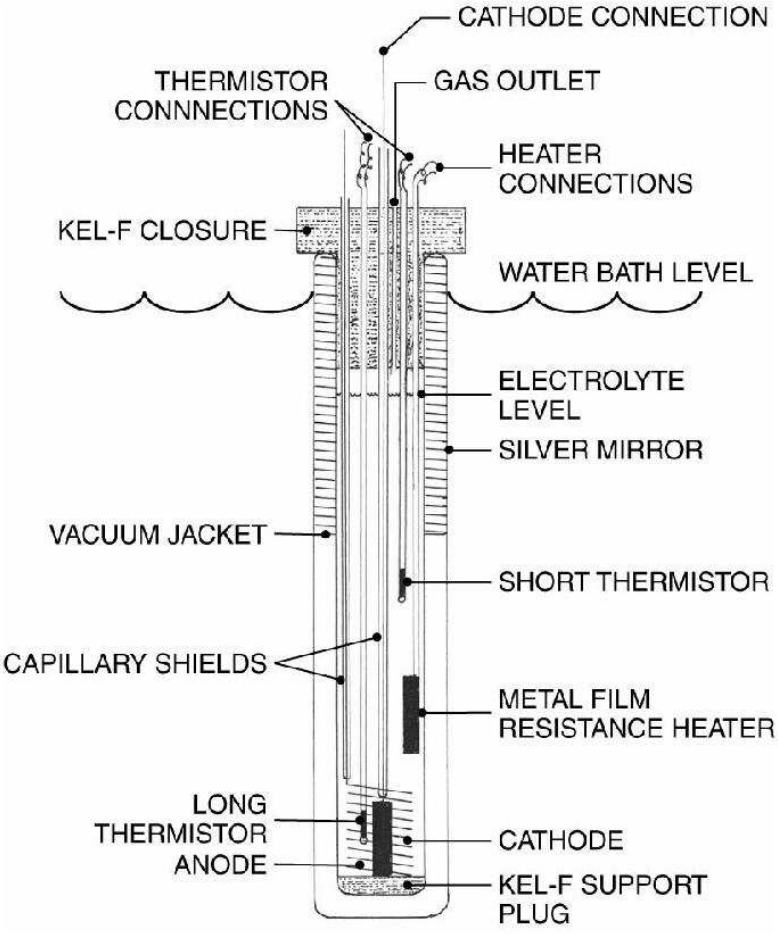
Chapter 1

An astounding announcement

This is a story of great expectations and even greater disappontments. This is a story this is a story for which lots of newspapers have been sold, but very few people can tell that they have understood something about that. This is a story of global opportunities and local selfishness. This is a story where something that appears at a certain time, a little later has disappeared. This is a story where everybody claims to be honest and is ready to point at someone else's mistakes. This is a strange story if you consider that the men involved in it are scientists, but it becomes not so strange if you bear in mind that all the scientists are essentially men. This is the story of the cold fusion.

The story of cold fusion doesn't begin with a vague *"Once upon a time..."* like stories of other kinds, but it has a clear official date of beginning. You can consider in such way the March 23, 1989 when Martin Fleischmann and Stanley Pons, two chemists at the University of Utah, called a press conference in Salt Lake City to announce to the world a seemingly extraordinary discovery. They claimed to have measured an unexpected amount of energy produced in a simple electrolytical cell which could not be explained through some chemical reaction (such as electrolysis), but it could be caused only by an hitherto unknown nuclear process, a sort of fusion which had to be quite different from the ordinary "hot" fusion that happens in the stars, because had to occur at room temperature, so this mysterious phenomenon was soon baptized as *"cold fusion"*. The results achieved by Fleischmann and Pons were quite surprising because the devices they said to have used were simple, well known and not particularly sophisticated from a technological point of view. The device figured in the following diagram is used at the New Hydrogen Energy Institute in Japan [1].

[1] http://en.wikipedia.org/wiki/Cold_fusion



1.1 Experimental setup

The main component of the experimental setup used by Fleischmann and Pons was a vessel designed to provide thermal insulation (a Dewar flask) containing heavy water (i. e. water where instead of hydrogen you have deuterium, its isotope with one neutron in the nucleus) and an inert palla-

dium cathode. The content of the vessel underwent a process of electrolysis through the application of an external voltage, deuterium and oxygen separated (really, as in every electrolytical process, the presence of a further substance which was able to increase the electrical conductivity of pure water, e.g. some salt as NaCl, was required). The former was partly absorbed by the palladium cathode and partly released in gaseous form, while the latter bubbled and left the cell, which was open on the upper side. The vessel was put in a bath where the temperature was held constant, in order to reduce the effects of external heat sources. The temperature was continuosly measured through a thermistor, a device whose electrical resistance changes because of the variation of temperature. Together with the gaseous products of the electrolytical reaction, a small amount of heat too escaped from the device. In order to counterbalance the heat losses and to keep the reaction active for a long time, an electrical heater was used and heavy water was added at regular intervals. A constant current was applied to the cell continuously for many weeks.
For most of the time nothing strange occurred, the power input to the cell was equal to the power that went out of the cell within measuring accuracy and the cell temperature was stable at around 30 C. But then, at some point (and in some of the experiments), the temperature rose suddenly to about 50 C without changes in the input power, for durations of 2 days or more. The generated power was calculated to be about 20 times the input power during the power bursts. Eventually the power bursts in any one cell would no longer occur and the cell was turned off.
The choice of using a palladium cathode was not casual. It had already been observed from the nineteenth century that palladium and titanium absorb hydrogen (and deuterium also, as was seen after its discovery in 1932) in a very easy and peculiar way. The hydrogen or deuterium disassociate with the respective positive ions, but they remain in a quite mobile state inside the metal lattice, exhibiting rapid diffusion and high electrical conductivity. About this matter it can be nice to remember two curious historical facts that occured much earlier than Fleischmann and Pons story. In 1926, two German scientists, F. Paneth and K. Peters, reported the transformation of hydrogen into helium by spontaneous nuclear catalysis when hydrogen is absorbed by finely divided palladium at room temperature [2]. These authors later retracted their report, recognizing that the helium they measured was due to background from the air.

A year later, Swedish scientist J. Tandberg said that he had fused hydrogen into helium in an electrolytic cell with palladium electrodes. On the

[2]Paneth, F., and K. Peters (1926), Nature, 118, 526

basis of his work he applied for a Swedish patent for "a method to produce helium and useful reaction energy". After deuterium was discovered in 1932, Tandberg continued his experiments with heavy water. Because of Paneth and Peters' retraction, Tandberg's patent application was eventually denied. The research of Fleischmann and his group was guided since sixties by the speculation over the possibility to control nuclear reactions through chemical means. They knew that such a possibility was not allowed by the well tested laws of quantum mechanics, but for some obscure reason Fleischmann thought his speculations could be somehow theoretically motivated through quantum electrodynamics, the quantum theory about interactions between light and matter that takes in account the requirements imposed to every physical theory by relativity principle as formulated by A. Einstein in 1905, namely the universal constance of the speed of light for every observer. He thought that the coherence of the motions of deuterium nuclei in the lattice of the cathode played a fundamental role, in order to overcome the coulombian repulsion that makes the *"hot"* fusion a so difficult process to activate. You'll see later that he was not the only scientist that had some idea in this direction.

Chapter 2

The day after: were there the signatures for a new form of fusion?

Fleischmann and Pons claimed that the only possible explanation of their result had to be based on some mechanism of nuclear fusion. That sounded quite odd to most of the physicists, because the physical processes and the typical energies involved in their experiments were not the proper ones in which nuclear fusion took place. In order for fusion to occur, the electrostatic force that repels the positively charged nuclei must be overcome. Once the distance between the nuclei becomes comparable to one femtometer (10^{-15} m), the attractive stong interaction takes over and the fusion may occur. However, bringing the nuclei so close together requires an energy on the order of 10 MeV per nucleus, whereas the energies of chemical reactions are on the order of several eV. It is hard to explain where the required energy would come from in room-temperature matter. Nuclei are so far apart in a metal lattice that it is hard to believe that the distant atoms could somehow facilitate the fusion reaction: the deuterium nuclei are further apart in a palladium cathode than in a molecule of heavy water. Moreover, when fusion occurs, a large amount of energy is normally released as gamma rays or energetic protons or neutrons: there is no known mechanism that would release this energy as heat within the relatively small metal lattice.
Someome proposed that the excess of heat measured by Fleischmann and Pons could be explained without invoking nuclear reactions: it could be due to some uncontrolled source of heat, some inaccuracies in the measures or

some manipulation of the data. However, Steven E. Jones [1] of Brigham Young University, a cold fusion skeptic, has observed anomalous neutron emissions from electrolytic cells, and said that they result from fusion reactions unexplained by current theories (but 10 orders of magnitude lower than what would be required to explain the excess heat of Fleischmann and Pons) and his claim has never been challenged nor retracted, but confirmed by other researchers.
Soon after Fleischmann and Pons' report cold fusion researchers proposed several theoretical hypotheses to explain the effect. They were partial theories and even today, no complete theory has been found that could explain such a phenomenon. One such theory is based on resonant tunnelling. Quantum tunnelling is an accepted effect by which a charged particle such as a nucleus can "go through" a Coulombian barrier, but it predicts a rate of cold fusion well below what was claimed in the experiments made by Fleischmann and Pons. Resonant tunneling is based on the proposition that the metal lattice can amplify this effect through resonance.

Accordingly to quantum mechanics both energy and matter can behave as particles or waves. There can also be coherent behavior in matter as in superconductivity and superfluidity. The italian high energy physicist Giuliano Preparata, who extensively studied cold fusion both theoretically and experimentally thought that such phenomenon could be explained by coherence effects in matter through the principles and the computational tools of QED. Fleischmann and other cold fusion scientists think that QED can provide a solution. A relevant case of scientist who trusted on the scientific reliability of Fleischmann and Pons' results and thought that they could be explained by that theory was one of its developers, that is the Nobel laureate Julian Schwinger. He believed that "If a proven track record can be established... you have to believe it". He also believed that cold fusion does not violate conventional theory. As he put it, "The defense [of cold fusion] is simply stated: The circumstances of cold fusion are not those of hot fusion".
Fusion or not fusion, the mediatic bomb exploded. The press conference attracted much media attention, the results reported by Fleischmann and Pons were advertized in every part of the world, arousing a lot of enthusiasms and speculations about their possible developments and applications, even in that part of people who is generally not too much excited by the daily work of scientists. The solution of the economic and environmental problems related

[1] A very interesting guy with very broad range of interests. He supports a sort of conspiracy theory behind the collapse of the World Trade Center occurred the September 11, 2001, based on the hypothesis that could have been a controlled event by U. S. government and he tried to prove with archaelogical studies of Maya ruins that Jesus Christ visited the Native Americans after his resurrection, an event chronicled in the Book of Mormon

to global energetic supply seemed at hand.
However, there was something strange in that wonderful discovery.
The public diffusion of a so important result was not accomplished through the ordinary channels of scientific communication, which surely are not so quick as the newspapers, but they assure control and verification by all the expert researchers in a certain area of science. The claims made by Fleischmann and Pons had not gone through the scrutiny of peer review. Before going on perhaps it is useful to try to answer to a simple question: who were these two guys coming from an obscure american University who suddenly became famous in all the world for a little more than fifteen minutes, the time for celebrity foreseen by A. Warhol?
Martin Fleischmann was born in Karlsbad (former Czechoslovakia) in 1927, but when he was eleven, after his father's death caused by Nazi persecution, was taken in by foster parents in the United Kingdom, where he had a brilliant and successful scientific career. When he was forty he was appointed to the professorial chair in electrochemistry at the University of Southampton. About the same time he became president of the International Society of Electrochemistry, and was made a fellow of The Royal Society.

Stanley Pons was born in 1943 in North Carolina, but chose to do his Ph. D. at Southampton, where Fleischmann had acquired an international reputation. By the time Pons received his doctorate in 1979, he was well acquainted with Fleischmann. Later, when Pons became chair of the Department of Chemistry at the University of Utah, Fleischmann was a regular visitor.
It was clear that Fleischmann and his former graduate student Pons were not two extemporary researchers who looked for an easy way towards money and fame. Fleischmann's investigations on chemical-induced nuclear reactions arouse in sixties and when Pons joined to him, after the first tantalizing results, they financed the experiments on electrolysis with their own funds in order to go on with them. In 1988, they applied to the US Department of Energy for funding for a larger series of experiments. Their application was reviewed by several scientists, including Steven E. Jones, who had already started investigating alternative mechanism for nuclear fusion, based on catalysis performed by muons. Both teams met on several occasions to discuss sharing work and techniques, but as they were getting ready to publish their results in early 1989, Fleischmann and Pons unexpectedly decided to report their results without anyone co-worker and through a press conference. It seemed that they wanted to avoid the usual procedure of control by the rest of scientific community and someone accused the two chemists to do science by press release. In a recent interview Fleischmann tried to justify their choice saying that they were almost forced to call for the press

conference by the University establishment, which was interested to obtain patents and grants as soon as possible. From a practical point of view you can understand Fleischmann's choice, in Italy he would have said that "teneva famiglia" (it's a popular expression to say that he had to keep his work as tight as possible, in order to take care of the needs of his family), but it is a curious fact that in the same interview he refused to say publicly something about the collaboration with Jones.

Naturally, not only the attention of newspapers focused on Fleischmann and Pons' work, but also that one of the nuclear physicists' community, that began at once to try to reproduce their results in order to seriously check them. A few months after the press conference the reports about experimental attempts to measure excess of heat in electrolytical cells were published by several important institutions of nuclear reserch, including California Institute of Technology, MIT and the Harwell Center in the Great Britain.

Unfortunately for Fleischmann and Pons, all these works did not find their same results, there was no unexpected production of energy at all. Scientific community pointed out severe critics towards the methods used by the two chemists and the suspect of fraud behind the story of the "cold fusion" began to spread among scientists' and people's minds. A lot of negative articles toward Fleischmann and Pons' work appeared on a number of scientific review, such as *Nature*. The debate became bitter and controversial.

Fleischmann challenged the results obtained by the laboratories that made the checks he said they faked their data in order to have not to admit that something unknown was happening. Someone agreed with him, like Eugene Mallove [2], an MIT-trained engineer working as chief science writer in the MIT news office who at the beginning was skeptical towards Fleischmann and Pons' experiment. Then he studied data from the MIT experiment, and the graph looked wrong to him. He realized that they had moved the baseline to conceal a small amount of anomalous heat, but MIT scientists at once denied it.

The government of the United States assigned a special commission of experts to work in order to definitively understand if the research about cold fusion was worth to be pursued or not. The final report of this commission said that the experimental evidence about cold fusion was unconvincing and this almost put an end to research on cold fusion in the United States.

Everybody who wanted to pursue studies in that field risked at least to be ridiculized or accused of bare scientific seriousness. In June 1990, Gary Taubes, a science writer who had written two books and several articles in-

[2]He was murdered in his house the 14 May, 2004, maybe during a robbery. The case is still unresolved.

vestigating allegations of fraudulent activity in science, published an article in *Science* clearly suggesting that researchers at TexasA&M, whose experiments seemed to suggest that fusion had occured, because of the presence of tritium producted in devices similar to those ones used by Fleischmann and Pons, added tritium to fake their results. After multiple investigations, the University found no evidence of fraud or incompetence. John Bockris, who was then a distinguished professor in physical chemistry at Texas A&M University and a cofounder of the International Society for Electrochemistry, had to appeal to the American Association of University Professors before the harassment stopped.

The University of Utah continued to look for funding to emprove research on cold fusion, but its lab was eventually disbanded in 1991 when it failed to replicate the earlier results. However Fleischmann and Ponswere did not abandon their research on cold fusion and they could continue with that going to the IMRA laboratory, in the south of France, which was created and financed for them by the japanese car company Toyota.

Despite of american refuse of cold fusion, the work on this subject continued in several countries, namely in Japan, France and Italy. But in 1997 Japan's government finally gave up.

In 1998 IMRA was closed, having spent something like 12 millions of pounds on cold-fusion work. Then in March 1998 the University of Utah finally gave up its struggle to obtain worldwide patents on Pons and Fleischmann's work, having been legally bound to pursue patents until the end of the year. The rights then reverted to Pons and Fleischmann themselves, but they were no more much interested in the pursuit of patents. Their research on cold fusion ended when funding from Toyota was cut off. Fleischmann moved back to England and retired, while Pons came back to Southampton ceasing any contact with his colleague from 1995. In the following years the former continued to take part to the periodical meetings organized by the few scientists that still work on cold fusion, while the latter did not speak publicly any word more on that subject.

Chapter 3

The present scenario: what has survived?

Today, a handful of laboratories still pursue cold fusion, but their work remains largely ignored by the vast majority of scientific reviews. If you look on Internet to learn something about cold fusion you often can find jeering comments on it such as: *"Yet the defenders of cold fusion have soldiered on, a number of them merging with a network of conspiracy theorists, psychic spoon-benders, UFO enthusiasts and believers in other exotic physical phenomena outside the ken of science."*
Despite of all the difficulties that characterized the story of cold fusion, researchers in this field organized themselves to periodically meet and discuss their results. They meet at the International Conferences on Cold Fusion, organized almost every couple of years from 1990 and that from 2004 became more properly International Conferences on Condensed Matter Nuclear Science. There are also some specialized journals about topics related to cold fusion and low energy nuclear reactions: "Infinite Energy" (partly supported also by the famous science-fiction writer A. C. Clarke), "New Energy Times" and "Cold Fusion TImes".
In 2004, the Department of Energy of United States (DoE) decided to take a second look at cold fusion. Unlike the authors of recent books on this subject, the official conclusion of the DoE review stated was that there was nothing new in the field since 1989. In the very same document, it was shown that about a third of the review panel members agreed there were anomalous effects, and half of the reviewers found the evidence for excess power compelling. The conclusion written by the DoE did not match the general perceptions of the reviewers and it remains somewhat of a mystery as to why the DoE even bothered to perform this review in the first place.

Regardless, despite the fact that the DoE has not decided to fund cold fu-

sion research at the moment, reports from qualified sources indicate that the review has brought significant attention from private industry and investors to the field. While no details have been made available, rumors indicate that american private industry has decided to take a bet on cold fusion, while the DoE remains on the sidelines.

Chapter 4

This is the end: is cold fusion just only *"pathological science"?*

The press conference called by Fleischmann and Pons to report to the world the results of their experiments was a remarkable error. The haste to advertize (even if under psychological pressure) their work and the delay with which they put it under the scrutiny of scientific community was an error. Their earliest behaviour surely influenced also successive negative positions by "mainstream" scientists. This is particularly meaningful the comment of Edmund Storms, one of the scientist who worked on cold fusion: *"Conventional science requires you to play by certain rules. First, thou shalt not announce thy results via a press conference. Second, thou shalt not exaggerate the results. Third, thou shalt tell other scientists precisely what thou did. They broke all of those rules."*

Many have dismissed cold fusion as an example of *"pathological science"*: in such a science, a scientist, originally conforming to the scientific method, unconsciously doesn't apply any more that method and he begins to read the data of his experiments following more his expectations than objective results. The following characteristics of *"pathological science"* were listed by Nobel laureate chemist Irving Langmuir when he invented the term:

- The maximum effect that is observed is produced by a causative agent of barely detectable intensity, and the magnitude of the effect is substantially independent. of the intensity of the cause.
- The effect is of a magnitude that remains close to the limit of detectability, or many measurements are necessary because of the very low statistical significance of the results.

- There are claims of great accuracy.
- Fantastic theories contrary to experience are suggested.
- Criticisms are met by *ad hoc* excuses. item The ratio of supporters to critics rises and then falls gradually to oblivion.

However, after reviewing the story of Fleischmann and Pons may we conclude that it has been only an unpleasant story of scientific fraud or incompetency at least ? I don't think so and I can't say without any doubts that they made some mistakes or they faked their results. However I can't either say that someone of the scientits that challenged thei results was lying or unqualified.

What I learnt doing the present work is that a number of people in different parts of the world began to work on matters related on cold fusion after Fleishmann and Pons' claim. Several scientists spent time and resources, often compromising personal reputation and possibility to carry on their researches in fair conditions. Probably some of them published really faked data, surely someone made some odd claims. Surely someone magnified too much some still uncertain experimental data, driven by enthusiasm or ambition or also fear of radical changes in the established knowledge. If you suddenly realize or you just only have some clue that the matter on which you have worked for all your life and that all the society retains worthful (e.g. *"hot"* fusion) should be left because something else reveals itself cheaper and more hopeful for the needs of humanity, probably you don't take it easy.

Fleishmann and Pons' haste to advertize their work was surely an error, perhaps one of the least disputable in all the story of cold fusion. However, may we be sure that we don't make the same mistake if we completely put aside the research on cold fusion and we don't come back to consider it as a genuine scientific question that can be studied with the means and the resources of all the official scientific establishment? When I say "means" and "resources" I don't refer just only to well equipped laboratories and substantial fundings, but also to free and neutral communication of the results achieved by the studies in the field of cold fusion. That implies that all the major scientific reviews that now sistematically refuse every article on cold fusion and related matters should sweep out most of their bias and select them with the same criteria used for articles on other subjects. Only the free exchange of ideas, the open debate and the complete access to every experimental method and result make truly possible the development of scientific knowledge.

In order to defend the dignity of their field of research, some scientists that work on cold fusion quote often another case of unexpected scientific discovery that could not (and probably still can not) be clearly explained

with established knowledge, that is the high-temperature superconductivity. I think that the two events were quite different and they can not put on the same ground.

Experiments about cold fusion, if they gave positive results at all, gave them only sporadically and unpredictably. Fleischmann and Pons (and also other researchers that came later) did not report the results of any control experiment useful to understand what conditions were necessary in order to obtain the anomalous excess of heat. The story of high-temperature superconductivity went quite differently.

When Bednorz and Mueller announced their discovery in 1986, no one carped about control experiments, because, once the recipe was known, any competent scientist could make a sample and test it and it would work immediately. If, at their press conference, Pons and Fleischmann had given a dependable recipe for producing excess heat, they very likely would be Nobel Prize winners now (as Bednorz and Mueller are) rather than social outcasts from the community of scientists. The essential key to the return of cold fusion to scientific respectability is to find the missing ingredient that would make the recipe work every time. At the same time "mainstrem" scientists must be ready to listen to whom that has something to say about this quest.

I. Langmuir defined as *"pathological science"* an area of research that simply will not "go away" - long after it was given up on as 'false' by the majority of scientists in the field. I don't agree with people who wants to see conspiracy behind every political or social event, but at the same time I don't think that the opinion of a majority can't be reasonably analysed and even opposed by a single individual, just only because of its own nature of majority.

At last I stop a while and think of the problems due to possess and distribution of energetic resources. I mean the number of wars and more or less local conflicts, the social and economic iniquities and the critical environmental damages that every day take place in the world. So I think maybe we are right if we put aside research on cold fusion, but maybe not. In the latter case we are making a mistake. Let's hope that it shall not be one of the last.

4.1 References

Most of the material collected for the present work, including quotations, has been taken from the Wikipedia pages on cold fusion and on the related controversy: http://en.wikipedia.org/wiki/Cold_fusion and
http://en.wikipedia.org/wiki/Cold_fusion_controversy. Other references that

have been used are:

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