Applying the Scientific Method to Cold Fusion Studies
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This paper is based on a public seminar I gave at the University of California at Berkeley on November 7, 2006, shortly before my early retirement.

I'm pleased to be here again on the Berkeley campus, and what a beautiful day it is!

A number of years ago, I was on this campus visiting Prof. Louis Alvarez, Professor, physicist and Nobel-prize winner. Prof. Alvarez had observed muon-catalyzed fusion experimentally for the first time in a hydrogen bubble chamber. Dr. Alvarez was very interested in my results using deuterum and tritium mixtures. We stopped negative muons in tritium plus deuterium mixtures at various temperatures and densities, in order to produce muon-catalyzed d-t fusion for the first time! And we achieved a record number of catalyzed fusion events during these experiments. Firsts are fun, but experience shows they often prove controversial.

Professor Alvarez was a no-nonsense scientist and a very creative fellow. He and his son came up with this idea that the animal population on the early earth underwent a very major change because of an asteroid striking the earth. This theory was very unpopular when it first came out but it has since been placed on solid ground by means of a number of experimental tests. So it is now widely accepted, but it took a long time to change some scientists' minds – with a lot of empirical data, of course.

This slow process of convincing scientists is reminiscent of history of the plate-tectonics model, which was resisted by geologists for a long time, but now is the backbone-model of modern geology.

Louis Alvarez set that example of not being afraid to voice unpopular hypotheses and then to proceed with experiments while encouraging others to do experiments to check his findings. That's what we do in science, we do experiments. In my view, experiments trump theory every time. And a 100% repeatable experiment which has been independently verified stands as empirical fact, whether or not there exists a solid theoretical explanation for the observations.

The idea of science is free inquiry, free speech and experiments to determine what is correct, what's true. It is really not a matter of what is popular at any given time.

The heart of good science, I would say, is repeatability by the scientist and independent verification by other scientists who scrutinize and test the findings.

I must also emphasize that there is a tendency to label non-traditional science as "pseudo-science" when in fact it may be nascent or "proto-science". Only experiments and repeatability will tell the difference, and this takes time.

My first major publication in which I was lead author was a paper reporting experimental results on muon-catalyzed fusion.[1] Unlike thermonuclear fusion which occurs on the sun at high temperature, this type of fusion occurs at room temperature. The muon, which is basically a heavy cousin of the electron, pulls hydrogen nuclei of the isotopes of deuterium and tritium closely together so that tunneling occurs through the Coulomb barrier leading to nuclear fusion.

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1 S.E. Jones, A.N. Anderson, A.J. Caffrey, J.B. Walter, K.D. Watts, J.N. Bradbury, P.A.M. Gram, H.R. Maltrud, M. Leon, M.A. Paciotti, "Experimental investigation of Muon-Catalyzed d-t Fusion," **Physical Review Letters** 51: 1757-1760 (1983).

Our next paper on muon-catalyzed fusion, published in Physical Review Letters, was strongly challenged.[2] I traveled to UC-Berkeley to defend the collaboration's conclusions in that paper, much as I am doing today. We recorded a very small "muon-alpha sticking coefficient," which had a consequence that a much higher fusion energy yield was realized than had been theoretically predicted. One of the physicists at Berkeley said "you can't possibly be right; you are challenging J. David Jackson"! Now those of you who know Jackson know that he was one of the top theoretical physicists of his day, and he was a Professor of Physics at Berkeley.

Jackson had studied the critical muon-alpha sticking coefficient for muon-catalyzed d-t fusion and predicted that it would be around 1%. Then we did the experiment for the first time and we measured this parameter in a liquid deuterium-tritium mixture, we found a sticking value of about 0.42%, much smaller than predicted by Jackson. We were told by physicists that our measurement couldn't possibly be correct, but we couldn't just back down from our experimental measurements!

So we repeated and extended the experiments and found that our measurement was indeed correct. And our results were 100% repeatable. But it took a subsequent independent experiment to test our results and confirm to many people that we were correct. Now our published value, published in a peer-reviewed journal article[2] is accepted as correct. And it is the theory which was refined.

Again, I'm setting a background -- that experiments determine what is true and correct, not someone's theoretical notions, even someone famous like J. David Jackson.

My next major paper was in Nature, 1986, a British scientific journal.[3] I want to point out that it generally takes several years to go from a conference proceedings or a minor paper to a major paper like this one in Nature. By 1986 I was about seven years into the study of muon catalyzed fusion and received an invitation to write a paper for Nature. By now, I have published three times in Nature, and that itself has been quite an adventure!

My colleague Professor Johann Rafelski and I published a significant paper in Scientific American in 1987.[4] The title was "Cold Nuclear Fusion." We probably couldn't get away with that title today in Scientific American because of the history since then regarding "cold fusion." But this title is referring to muon-catalyzed d-t fusion which by 1987 was not controversial. Our surprising results had been verified in independent experiments; the low muon-alpha sticking coefficient which I talked about was verified and so on. Catalyzed fusion does occur at low temperatures, including room temperature, that fact was now mainstream and accepted.

Based on the strength of this research for which I was a principal investigator, I was invited to speak in Erice, Italy, at a conference attended by top scientists.[5] I later returned to the same venue in Erice, but this time to talk about piezo-nuclear fusion in room-temperature metals. But I get ahead of myself here...

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2 S.E. Jones, A.N. Anderson, J.N. Bradbury, A.J. Caffrey, J.S. Cohen, P.A.M. Gram, M. Leon, R.L. Maltrud, M.A. Paciotti, C.D. Van Siclen, and K.D. Watts, "Observation of Unexpected Density Effects in Muon-Catalyzed d-t Fusion," **Physical Review Letters** 56: 588-591 (1986).

3 S.E. Jones, "Muon-Catalysed Fusion Revisited," (Invited article) **Nature** 321: 127-133 (1986).

4 J. Rafelski and S.E. Jones, "Cold Nuclear Fusion," **Scientific American**, 257: 84-89 (July 1987).

5 S.E. Jones, "Can 250+ fusions per muon be achieved?," Invited talk for Erice School-Workshop, Erice, Italy, April 3-9, 1987, New York: Plenum Press, 1987, pgs. 73-88.

Our next major paper was published in Nature in April, 1989.[6] Talk about controversial! But it passed a rather severe peer-review and was accepted for publication. The paper dealt with our experiments in what is now commonly called "cold fusion" but not the Pons-Fleischmann variety. It was truly deuteron-deuteron (d-d)fusion, because we observed the energetic end-products of d-d fusion, namely energetic neutrons and (later) 3-MeV protons. I still prefer to call our discovery piezo-nuclear or metal-catalyzed fusion.

We had been studying this fusion approach since 1985, at least three years before we heard of Pons and Fleischmann, looking at nuclear fusion catalyzed in metals, and a colleague and I had published on "cold fusion" way back in 1986.[7] We began experiments at BYU in May 1986, and about this time our hypothesis formed: [b] somehow metals will enhance fusion yields between light nuclei, and some metals will enhance fusion better than other metals. And then we designed and performed our own experiments to test this hypothesis, and succeeded.[/b]

As we think about fossil fuels and pollution, we realize that fusion energy offers a lot of hope. Perhaps this is why this particular paper in <u>Nature</u> received a great deal of attention, as seen in the media in 1989. Some people thought we were supporting the amazing claims of Pons and Fleischmann – cold d-d fusion with lots of heat-energy released but -they claimed- essentially no neutrons. But I said our experiments with low-level neutrons were different. Energetic neutrons and protons (that we measured) are signature particles for the d-d fusion reaction, but these are lacking in the Pons-Fleischmann-type "excess heat" experiments. That is, they were claiming excess heat via d-d fusion- without energetic product-neutrons. "Aneutronic" fusion people called it. The paucity of neutrons I personally think was one of their biggest stumbling blocks. Physicists in general just could not accept aneutronic d-d fusion, and for good reason.

In view of our disparate results, I personally encouraged Dr. Fleischmann to drop the term "fusion" and simply call it anomalous heat. But it seemed that he and Pons wouldn't hear of it. By now, I find that many researchers in the field agree -- what they saw may be real, but it is not d-d fusion. Some call it "LENR" (Low Energy Nuclear Reactions), but to me the wisest approach at this stage is to just call it "anomalous heat". I believe that before his death, Dr. Fleischmann agreed that he should not have called (in 1989) his process "fusion". (See "60 Minutes" interview.) It was not fusion.

At the same time, my close friend and colleague Prof. Johann Rafelski urged me to keep an open mind about the excess heat itself. I think I did, although I entertained some doubts. A fact that bothered many scientists about the "excess heat" cold-fusion claims of Drs. Pons and Fleischmann was the fact that their experiments were not even close to 100% repeatable. In any case, I do now have a *very* open mind regarding the anomalous heat or energy, given recent data.

I came to the conclusion in 1989 that the two groups were simply observing two different phenomena. I used an analogy of trains on separate tracks, trying to help people understand. It is quite possible for different effects to take place when one loads a metal with deuterium or hydrogen.

Now, were we wrong in our own claims of low-level neutron production in d-d cold fusion? I suppose I wouldn't be standing before you today if my colleagues and I had been wrong. Please

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6 S.E. Jones, E.P. Palmer, J.B. Czirr, D.L. Decker, G.L. Jensen, J.M. Thorne, and S.F. Taylor & J. Rafelski, "Observation of Cold Nuclear Fusion in Condensed Matter," **Nature** 338: 737-740 (April 1989).

7 C.D. Van Siclen and S.E. Jones, "Piezonuclear Fusion in Isotopic Hydrogen Molecules," Journal of Physics G. Nucl. Phys. 12: 213-221 (1986)

consider the DATA Tables below summarizing experimental results from several independent experiments.[8] Please note the publication dates – it took approximately nine years to get this low-level "cold d-d fusion effect" verified.

Material[4]	U_e (eV)
D_2 gas[1]	25 ± 5
Pd	800±90
Sb	720±70
Pt	670±50
Co	640±70
Au/Pd/PdO[2]	601±23
Tl	550±90
Bi	530±60
Al	520±50
In	520±50
Ba	490±70
V	480±60
Pb	480±50
Zn	480±50
Cu	470±50
Nb	470±60
Fe	460±60
Mg	440±40
Mo	420±50
Mn	390±50
Ni	380±40
Cd	360±40
Ag	330±40
Ta[3,4]	322±15
Cr	320±70
Pd[3]	280±30
Au	280±50
Ta	270±30
W	250±30
Rh	230±40
Re	230±30
Ru	215±30
Sr	210±30
Ir	200±40
Be	180±40
Sn	130±20

Material[5]	U_e (eV)
Pd-Li	**1500**±310
Au-Li	60±150
Li metal	? (large, I expect)

Material[4]	U_e(eV)
Sc	≤30
Al_2O_3	≤30
Y	≤70
Zr	≤40
Lu	≤40
Hf	≤30
La	≤60
Ce	≤30
Pr	≤70
Nd	≤30
Sm	≤30
C	≤60
Si	≤60
Ge	≤80
Eu	≤50
Gd	≤50
Tb	≤30
Dy	≤30
Ho	≤70
Er	≤50
Tm	≤70
Yb	≤40
BeO	≤30
B	≤30
CaO_2	≤50

1. U. Griefe, *et al.*, Z. Phys. **A351**:107 (1995).
2. H. Yuki, J. Kasagi, A.G. Lipson, *et al.*, JETP Letters, **68**:823 (1998).
3. K. Czerski, *et al.*, Europhys. Lett. **54**:449 (2001).
4. F. Raiola, *et al.*, Eur. Phys. J. **A19**:283 (2004).
5. J. Kasagi, *et al.*, J. Phys. Soc. Japan, **73**:608 (2004).

8 Yuki, Kasagi, Lipson, 1998; Czerski, 2001; Raiola, 2004; Kasagi, 2004 – are metal-catalyzed d-d fusion experiments, achieving finally 100% reproducibility. Griefe, et al. (1995) did the d-d fusion measurement in D2 gas- used for comparisons. Note the table has more detailed references, numbered there 1-5 in chronological order.

Thus, experiments in Japan and Europe have been published starting in 1998 and our 1986 hypothesis has been amply verified – that metals would catalyze fusion and that some metals would have a larger fusion-enhancement effect than other metals. These experiments have also achieved 100% reproducibility, which eluded our team's efforts in the 1980's and 90's.

Let me say again for emphasis that 100% repeatability also distinguishes the true d-d cold fusion experiments from the anomalous heat events observed by others (at this time). Another difference, certainly, regards the magnitude of the two (separate) effects. The fusion reactions represented in our Nature paper and in the Data Tables attached produce heat, yes, but at less than one-BILLIONTH of a watt! This is so small that no calorimeter can measure it, and so we use other methods such as neutron and proton detection.

I encourage researchers in the anomalous heat field to keep seeking – just don't call it "cold fusion" unless you start seeing the corresponding fusion products! That has been my concern from the outset.

Note this comment from the 2006 paper by K. Czerski et al.: regarding low-level neutron detection:

"**As shown in [Europhys. Lett. 68:363 (2004)], the screening energy of order 300 eV determined in accelerator experiments can explain the neutron production rate observed by Jones et al.** [***Nature*** 338:737, 1989[9]] **at room temperature**." K. Czerski, et al., Eur. Phys. J. A27:S01,83 (2006)

Here the authors refer back to our 1989 paper in **Nature** -- Czerski et al. state the fusion enhancements in metals which they observe are consistent with and effectively confirm our earlier cold fusion results reported in 1989. Very decent - a scientific courtesy of them to say this.

So we finally have confirmation. But it took a long time to get that confirmation, and even today, it is fair to say that most scientists and the general population are quite unaware that our 1989 paper in Nature has been corroborated and that this true-d-d-fusion effect (which Paul Palmer called "cold fusion" in 1986) is now on firm footing.

A surprising finding in this true-d-d-fusion was reported by Huke, Czerski et al. at the 11th International Conference on Cold Fusion.[9] The experiment involves deuteron-loading into various metals using a low-energy deuteron beam (a method we also utilized), and is briefly described as follows:

"As known for a long time... the d+d fusion reactions have 3 possible outgoing channels, 2H(d,p)3H, 2H(d,n)3He and 2H(d,γ)4He. Two of them mediated by the strong interaction generate high energetic particles with a branching ratio of about ... while the third one is an electromagnetic transition [producing 4He] suppressed by 10^- 4

. ..The experiment has been carried out at a cascade accelerator optimized for low energy beams. The targets were **pure metal disks becoming self-implanted deuterium targets under the deuteron irradiation.** Four Si-detectors at the laboratory angles of 90 , 110 , 130 and 150 degrees were used for the detection of all charged particles, p, t, 3He, of the reactions 2H(d,p)t and 2H(d,n)3He [6, 2]. ...The low energy part of some representative spectra from the 90 -detector is depicted in fig. 1 magnifying the two lines of the recoil nuclei 3He and t. The spectra are normalized to an integral value of one in order to make them commensurable."

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9 A. Huke et al., Condensed Matter Nuclear Science: Proceedings of the 11th International Conference on Cold Fusion, edited by Jean-Paul Bibérian.

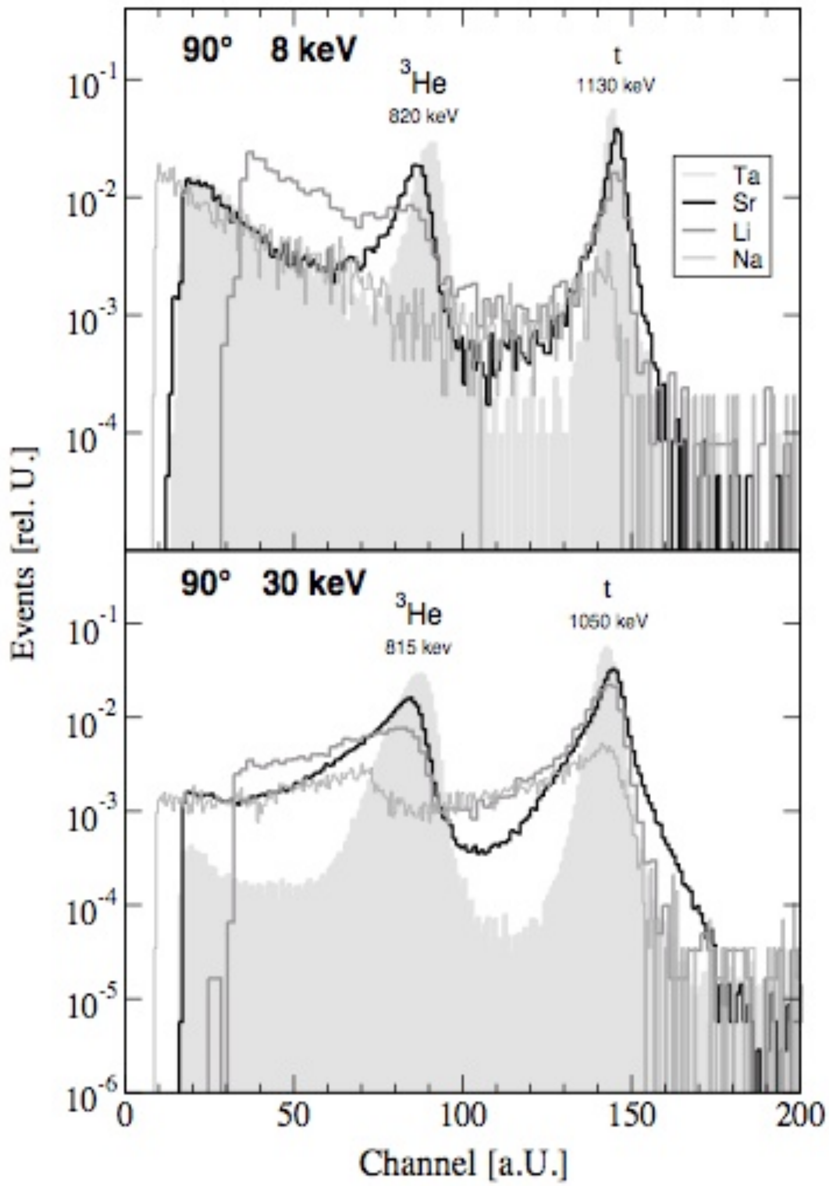

Figure 1, data from Huke, Czerski et al., presented at ICCF-11.

Looking at Figure 1 ,we clearly see peaks representing energetic 3He and triton particles exiting the deuteron-loaded foils. This provides compelling evidence for the occurrence of d-d fusion in these metals. The authors find that the neutron-to-proton ratio decreases for some metals as the beam energy gets lower and lower – a surprising effect which they explain in terms of deuteron polarization.

The Summary Tables display the calculated effective screening potential for each metal from the various experiments– which provide a measure of the effectiveness of various metals in enhancing fusion. The larger the number, the more enhancement of fusion yields one gets with that metal (a non-linear effect). It is this surprising effect, the variation of fusion yields with different metals and the strong enhancement of d-d fusion in the metal-lattice environment, that we independently postulated as we began our experiments back in 1986.

Note that palladium-lithium alloy is by far the best alloy that has been found so far. The number 1500 eV for the effective electron screening potential in the case of the Pd-Li metal alloy turns out to be unexpectedly large in the sense that it appears to be beyond the best theoretical model to date. I expect that lithium metal alone will also provide a large fusion enhancement factor – and we were preparing experiments to test that prediction at the time I accepted "early retirement" (October 2006). Lithium has the additional advantage that, for an impinging deuteron beam, lithium provides both the metallic matrix for enhancing the d-d fusion cross section as well as providing an advanced fuel for deuteron-Lithium nuclear reactions. .

The hope is that if we can understand how the fusion-enhancement effect in metals can be so large (as seen in the attached summary tables), we can further increase the yields from metal-catalyzed d-d fusion. But, to be frank, we have a LONG way to go before commercial energy could be realized by true d-d cold fusion.

Finally, I wish to summarize our hypothesis track-record in this multi-faceted field which includes cold fusion and anomalous heat-energy.

•1. Jones/Van Siclen Idaho Nat'l Engineering Laboratory hypothesis (1985): **"Fusion occurs in the core of Jupiter, causing it to emit heat."** The core of Jupiter is understood to be metallic hydrogen + deuterium. The hypothesis was later applied to the interior of the earth (in our 1989 Nature paper) which contains metals and abundant hydrogen/deuterium." Our peer-reviewed paper on this was published in Journal of Physics G in 1986, years before we heard of P&F. There are further results supporting the notion of fusion in the planets; but I would say the jury is still out.

•2. Jones/Palmer Hypothesis (1986): ***"Metals catalyze nuclear fusion, and some metals will enhance fusion more than others."*** *Paul Palmer called this "cold fusion," I called it "piezo fusion". Verified; see data above.*

3. My colleagues and I learned about Pons and Fleischmann for the first time in fall 1988; this led to the hypothesis: ***"The excess heat (if real) is NOT due to d-d cold fusion" and I stand by that.*** *That was the basis of the famous poll or vote, APS mtg Apr 1989. I have long believed that these are TWO SEPARATE effects.*

4. I have tried meanwhile to keep an open mind regarding the claimed "Excess Heat" whether it is real or not, largely at the arguments of Professor Rafelski.

And since the Berkeley talk in 2006, I have added the following:

•5***.The NRL and some light-water-electrolysis data (including Davey-bell systems) I find compelling, and so some time ago I agreed that there is anomalous excess heat - it is real (but not dd fusion). My hypothesis for some time in this regard is: #4. "The anomalous heat is real, but is not due to hydrogen-isotope fusion.***"

•Meanwhile, by end 2007 with my colleague L. Kraut, I began researching "freedom energy" – with the hypothesis (#5), ***"Yes, there may be something to freedom energy; and we can find***

out by ignoring ridicule and doing experiments." *Freedom energy is characterized by more* ***measured output energy*** *than* ***measured (known) input energy****, i.e., overunity. Note that "overunity" defined properly does not imply a violation of the laws of Physics nor perpetual motion – rather, we mean that a* ***hitherto-unknown or untapped source of energy*** *may be entering the picture.*

•*Soon I postulated: Hypothesis #6* ***"anomalous heat may actually be a manifestation of so-called "freedom energy", an as-yet-untapped source of energy that I and others are studying."*** *I presented this bold hypothesis in a seminar given at University of Missouri, Oct 2012, and I made my slides publicly available.*

•*This lead to my hypothesis 7:* ***"Anomalous heat will be observed in many light-water experiments; and the term "cold fusion" will become totally discredited for the excess heat observations since the heat is not due in fact to fusion."***

•*To which I add #8:* ***"This may be new physics; although anomalous heat observations in light water go back at least 60 years" (e.g., the patent by Peter Davey, 1945).***

I suspect that these latter ideas will also prove controversial, but they, too, can be tested using the scientific method, if we are sufficiently brave.