Contract No:

This document was prepared in conjunction with work accomplished under Contract No. DE-AC09-08SR22470 with the U.S. Department of Energy.

Disclaimer:

SRNL-STI-2012-00678

A Realistic Examination of Cold Fusion Claims 24 Years Later

A whitepaper on conventional explanations for

'cold fusion'

Kirk L. Shanahan

Oct. 22, 2012

A Realistic Examination of Cold Fusion Claims 24 Years Later

Kirk L. Shanahan

Introduction

On March 29, 1989, chemists Martin Fleischmann and Stanley Pons announced they had discovered an effect whose explanation was required to lie in the realm of nuclear reactions. Their claim, and those subsequent to it of roughly similar nature, became known as ‘cold fusion’. Research continues to this day on this effect, but what has become clear is that whatever it is, it is not a conventional fusion process. Thus the ‘cold fusion’ moniker is somewhat inappropriate and many current researchers in the field prefer the term “Low Energy Nuclear Reactions (LENR)”, although other terms have been coined for it as well. However, two salient facts are relevant: (1) these researchers still claim that said reactions do exist, and (2) these researchers claim there is a conspiracy by the scientific establishment to suppress their research. In recent years there have been several efforts aimed at revitalizing the image of the field which have had some success:

- A second review of the field by the US Department of Energy Office of Basic Sciences was conducted in late 2004, albeit with basically unchanged conclusions compared to those presented from the first DOE review in late 1989.
- Numerous new books and survey papers have been published detailing ‘positive’ research results.
- Several government entities have voiced and given partial support for renewed LENR research.

The stakes regarding this field are perceived as being quite high. LENR advocates promise nearly free energy arising from applications based on the effect(s), and thus indirectly promise significant reductions in conflicts due to disputes over limited natural energy resources. Given the USDOE’s responsibilities in the energy and weapons areas, it would seem true that the DOE should have as full and complete understanding of the field as possible.

Unfortunately, another development over the preceding several years has been the disappearance of negative commentary on the scientific basis of the field. This has occurred because the scientific mainline had concluded by c. 1992 that the field was an example of ‘pathological science’. Due to that, research was discouraged in the area and in fact ‘cold fusion’ publications were frowned upon. This only led to the claims of suppression by advocates. In fact there is some limited truth to this statement. This author, being one of the only remaining critics of the field, has suffered the same consequences for attempting to publish articles *critical* of LENR. This is a failure on the part of the mainline establishment, as the results developed out of the LENR research do in fact show something is happening to produce signals which might be interpreted as supporting nuclear reactions (which is what encourages and sustains LENR researchers), but which can also be interpreted via a set of unique and interesting conventional processes. The focus of this document is to describe and address recent objections to such processes so that subsequent LENR research can be guided to develop information that will determine whether either set of explanations has merit. It is hoped that criteria delineated herein will aid the USDOE and other agencies in determining if LENR proposals are meritorious and worthy of support or not.

Furthermore, the specter of a poorly understood process occurring in metal hydrides raises the possibility of said process occurring in the Tritium Facility here at the Savannah River Site, and in the associated research laboratories. A confined, loaded hydride material that suddenly begins self-heating is a potential safety incident waiting to happen. Thus it also behooves the DOE to determine the validity of cold fusion claims for the sake of the employees who might be exposed to an accidental release if the cold fusion process does and would occur in the SRS facilities.

Background – the state of the literature

Currently, the primary objections to LENR arise from this author via a detailed reinterpretation of presented results, which are normally explained in terms of some unknown nuclear reaction that occurs in the solid state at or near room temperature (10-100 ^{o}C). The types of evidence that would lead one to believe nuclear reactions are occurring would be the detection of nuclear reaction products (nuclear ash) and/or energy of levels that can only be acquired via nuclear reactions, either in the form of heat or radiation or both. Knowing this, LENR researchers invariably attempt to present one of these types of evidence. However, their first difficulty is achieving the reproducibility necessary to quantify the controlling factor – result relationship. It is this level of reproducibility that is required to convince the majority of scientists that something has actually been discovered and *understood*. So far, only general levels of reproducibility have been obtained, meaning that effects can be seen often, but they are not reproduced in detail from run to run or researcher to researcher. In other words, the LENR researchers have detailed only the most general level of control over whatever the effect is. Until a recipe for producing the effect(s) is specified in detail with a high degree of reproducibility (i.e. 80% or greater) the mainline scientist is allowed to conclude that the details of the effect remain elusive.

Secondarily, this author has recently published a series of articles[1-4], as comments on other articles[5-8], presenting conventional (i.e. non-nuclear) explanations for the bulk of the LENR claims. The response from the LENR community has been unexpected. In 2002[1], a conventional explanation for the Fleischmann-Pons-Hawkins Effect (as it was termed) (FPHE) was presented where a set of cold fusion calorimetric data posted to the Internet by a cold fusion researcher, Dr. Edmund Storms (subsequently presented at ICCF8[5] as 'proving' excess heat) was reanalyzed under a working assumption of no nuclear reactions. What was found was that minor variations in the calibration constants used to translate the calorimeter signal to the time history of the cell's power consumption/emission was all that was required to explain the obtained excess power spectrum. Further it was shown that there were systematic trends present in the results that suggested a chemical process was at work. The proposal will be referred to as the Calibration Constant Shift (CCS) problem.

Other than derogatory comments posted to the Internet, no response was observed to the proposal until 2004[6], when a research paper was published suggesting, almost as an afterthought, that the CCS proposal was difficult to understand and accept. This author therefore wrote a response to that paper, published in 2005[2], that addressed the concerns of the LENR researchers and showed how the CCS was applicable to their recent publication. Following that, in 2006[7] the original data's author, Dr. E. Storms, published a comment on the CCS that did not challenge any of the mathematical analyses conducted but instead proposed that the suggested chemical mechanism that caused the CCS was not possible and thus the whole CCS analysis was misconstrued. This author published, back-to-back in the same Journal issue, a point-by-point rebuttal[3].

In the meantime, Dr. Storms published a book[9] that purported to survey the field. This book contained tables summarizing the types and magnitudes of evidence produced in LENR research, and included some discussion of the experimental difficulties typically encountered. It did not seem to attempt to qualify any of the research, meaning that all reports were included in the book regardless of the specific study's quality. What was truly shocking however was the presentation of the state of current objections to LENR claims with respect to this author's work. The CCS problem was briefly mentioned and presented as being fully addressed and rejected by the Storms 2006 publication[7]. No mention of the Shanahan rebuttal[3] was made, even though Dr. Storms had participated directly in the peer review of the rebuttal and in extensive email communications on this topic with this author.

The data posted by Dr. Storms in February of 2000 (and used in ref. 1) was one of the best examples of controlled results ever published. In a series of 10 voltage sweeps from 0 volts up to a maximum and back down to 0, excess power signals were produced in nearly uniform proportion to the input power. In other word the data showed that with minor deviation, the excess power was functionally controlled by the input power levels. This would seem to meet the reproducibility requirement noted previously, especially if other LENR researchers could subsequently reproduce the behavior. However, no other LENR researcher has attempted to do so, probably because of one salient fact. Dr. Storms has carefully controlled the excess power production from a platinum (Pt) cathode, instead of the more conventional palladium (Pd) one. Pt has not been shown to hydride under any conditions, thus observing excess power from Pt lead to direct questioning of the relevance of the idea that a certain hydride concentration must be reached before observing excess power. Dr. Storms subsequently abandoned work on Pt in favor of returning to work with Pd, especially 'nano'-Pd.

During this timeframe, at the end of 2004, LENR research was given a credibility boost by the USDOE agreeing to re-review the field. A team of LENR researchers prepared a detailed position paper on the then current state of the field[10] and submitted it in writing for a paper review by a committee of 9 reviewers, and then presented their theses orally to another panel of 9 reviewers over the course of a day, Aug. 23, 2004. The USDOE review concluded (Dec. 1, 2004) nearly exactly what had been concluded from the prior 1989 DOE review of the field. However, it is important to note that both of the review panels were NOT given any of the Shanahan papers/comments, even though this author forwarded such directly to a contact in the DOE Office of Basic Sciences, which was the Office sponsoring the review. In all the comments recorded, only one reviewer was aware of the Shanahan contribution, and he/she recommended that the LENR researchers needed to consider it. To this author's knowledge, the LENR researchers never mentioned any Shanahan contribution.

In the meantime, other LENR researchers had continued their work unabated. In the 2005-7 timeframe, one block of work by a group of researchers led by S. Szpak and P. Mossier-Boss[11-13] located at the San Diego SPAWAR facility became prominent. (The SPAWAR group had been active in the field since the early days.) These workers claimed to have detected nuclear particles being emitted from electrolysis experiments where a dendritic Pd overlayer had been deposited on a metal mesh base and used as a cathode in a classic Fleischmann-Pons electrolysis cell. They used a different radiation detection method from prior studies, namely CR-39 plates. When nuclear particles strike a piece of CR-39 plastic, and the plastic is subsequently etched in basic solution for several hours, pits are known to develop in proportion to the number of particles that illuminated the CR-39. Such pits were found in CR-39 plastic placed near the electrodes of the Szpak F-P cells. Detailed analyses of the evidence were presented in the published literature.

Then in 2009, two authors, S. Krivit and J. Marwan, published a survey paper on LENR in the J. of Environmental Monitoring[8]. (This had been preceded by a shorter survey paper by Krivit in the Indian journal Current Science in 2008[14].) Unfortunately, their survey contained no serious discussion of possible flaws in the work described, so this author published a survey of those issues in 2010[4] as a comment on the 2009 publication. This was immediately answered in a back-to-back publication by a group of 10 currently active LENR researchers[15]. These authors were: J. Marwan, an electrochemist; M. C. H. McKubre and F. L. Tanzella of SRI International, published LENR authors focusing on Pd studies; P. L. Hagelstein, a MIT professor in the Dept. of Electrical Engineering and Computer Science who has authored a LENR theory and was one of the 2004 DOE Review presenters and co-author of the review report; M. H. Miles, a long time LENR researcher and author; M. R. Swartz, a long-time LENR researcher and author; Edmund Storms, discussed somewhat above, formerly a Los Alamos National Laboratory staff scientist and a well-published author; Y. Iwamura, of a Mitsubishi Heavy Industries research group who claimed to have demonstrated heavy metal transmutation; P. A. Mosier-Boss, SPAWAR, San Diego; and L. P. G. Forsley, a LENR researcher focusing especially on radiation detection by CR-39 in recent years. These authors, referred to hereafter as 'the 10 authors', attempted to rebut the 2010 publication[4]. However, they made some extremely serious errors in their rebuttal which negated their points in all but 1 instance.

The 2010 publication discussed 4 general areas of LENR research, following the layout of the 2009 article. First was the calorimetry and the reinterpretation of excess power signals via shifted calibration constants. Second was the claimed heavy metal transmutation and 4He formation results. Third was the CR- 39 evidence, claimed to demonstrate the existence of nuclear particles. And fourth was the perceived correlations between excess power and 4He measurements. The 10 authors began their rebuttal by challenging the CCS hypothesis. Unfortunately, their published response on this proves they had failed to grasp even the most basic point presented, namely that the FPHE in the data showed *systematic* behavior. They refer numerous times throughout the rebuttal to the "random Shanahan CCSH" (they attempted to redefine the CCS acronym to emphasize that it is a Hypothesis and added the 'H' to the acronym). This is a truly astounding error. The title of the 2002 publication[1] begins with "A Systematic Error…", and all four Shanahan publications refer in the text to the non-random or alternatively, systematic, character of the CCS. Especially of note is the Figure in the 2006 paper[3] that explicitly plots this and the use of the phrase 'The CCS is a systematic error,…' in the very paper the 10 authors were attempting to rebut.

The relevant *Webster's New Collegiate Dictionary* definition of 'straw man' is "a weak or imaginary opposition (as an argument or adversary) set up only to be easily confuted [overwhelmed]". Quoting Wikipedia:

"A **straw man** is a component of an argument and is an informal fallacy based on misrepresentation of an opponent's position. To "attack a straw man" is to create the illusion of having refuted a proposition by replacing it with a superficially similar yet unequivalent proposition (the "straw man"), and refuting it, without ever having actually refuted the original position." [emphasis as in original]

In logic terms, what the 10 authors have done in the 2010 rebuttal is establish a 'straw man'. In other words, the 10 authors in fact never address the CCS proposal, but attempt to give the impression they have. They pose the straw man CCSH argument, and then 'destroy' it, 'proving' it invalid. They then use that to claim the Shanahan arguments on calorimetry are wrong. (Those arguments were the basis of the comments on calorimetry and 'heat-after-death' results and on heat-4He correlations. As well, the putative chemical mechanism Shanahan proposed to lead to the CCS was used to provide an alternate explanation for the pits in CR-39.) Clearly however, since the CCS proposal was unaddressed, there is no valid rejection of it presented, and thus the CCS proposal stands as one capable of explaining most if not all of calorimetric data from F-P electrolytic cells and indirectly of CR-39 evidences.

The only part of the rebuttal tied to something besides the 'random Shanahan CCSH' was the attempted rebuttal of the transmutation discussion. In the 2010 paper, Shanahan basically followed the outline established by Krivit and Marwan in 2009, and only made some general observations and comments to point out the omissions in the LENR researchers reports. The primary points Shanahan tried to make in the 2010 paper regarding the heavy metal transmutation results were: (1) there is a good study available[16], albeit unpublished, showing the leaching and deposition of contaminants on electrode surfaces, contaminants derived from the solid cell materials, not just the electrolyte; (2) in some cases, detected 'transmutation products' were later found to be available in the laboratory (admitted w.r.t Pr in ref. 15), suggesting contamination of the experiments could have occurred; (3) isotopic distribution anomaly claims universally made via SIMS analysis methodology are based in data analysis that fails to account for multi-atom ions, or at least offers no information to assuage the question. The unstated precept being applied here is one well-known to chemists: *When anomalous constituents are found in a chemical experiment, particularly at trace levels, contamination is the default explanation, and claims otherwise must eliminate contamination as the source.* The 2010 paper primarily points out that this precept is being ignored by LENR researchers and thus their claims cannot be deemed reliable at this time. The 10 authors' rebuttal does nothing to alleviate this problem and it is useful to examine their points in order to highlight the tactics used to avoid dealing with the real issues possible contamination as an explanation brings up.

In dealing with the Iwamura example, they cite certain experimental facts in support of their points. However, several of these facts are difficult to prove themselves. For example, in dealing with the contention that Pr contamination could have caused the results observed, they assert that because of what Iwamura has reported as the experimental protocol, such contamination could not have occurred. Their logic is apparently that the exposure time was too low and that there should have been contamination in such-and-such a place if contamination had actually occurred, and it wasn't. This is an example of the fallacious tactic of defining a proposed set of events, 'proving' it didn't occur, and then claiming that solves the problem, which is a variant of the strawman approach. While the proposed mechanism may work as advertised, it does not eliminate many other potential ways the end results could be obtained, but is used as if it did.

Normally, if contamination is found in one spot in a lab at a later date, it is *assumed* prior experimental results unexpectedly finding that contaminant arise from the mundane source. To prove otherwise is a monumental problem, usually prohibited by the required workload, requiring specification of how those samples were prepared *in exact detail*, when they were prepared *in exact detail*, who prepared them *in exact detail*, and where they were prepared *in exact detail*. Time sequences for *each* sample must be specified (including controls). The problem is that finding contamination 'wild' in the lab *after* the experiments have been run has allowed time for the contaminant to have moved from place to place either naturally (by drafts for example) or via direct or indirect transfer from human activities (such as lab cleanup or setup of a new experiment), altering its distribution. Thus it is nearly impossible to know the relevant conditions on the day the questioned samples were prepared or what practices experimentalists employed that might have transferred contamination. All one knows for sure is that the contaminant was loose in the lab at some point in the past. The usual response is not to debate what was or wasn't done in the past, but to repeat the experiments under conditions where the contamination potential is eliminated or controlled, and which is proven by exacting attention to detail (such that the above questions could be answered for the new samples with ease).

The 10 authors premise their rebuttal on the idea that the Pr contamination was *only* in the balance, and therefore *must* have contaminated the samples in a *just-so* fashion if it did, but this is a strawman argument. There is no reason to *not* suspect that Pr was in just the right place at just the right time to produce exactly the results observed, no matter how much the experimentalists protest. *This is the fundamental problem with trying to establish scientific principles with trace-level results.* The 10 authors also use this logic with regards to the proposal that MoS_2 contamination could cause the noted results. Another cold fusion researcher has even reported the misassignment of S contamination as Mo[17].

The 10 authors also fail to notice (or note) that Shanahan presented explanations of the time-dependent observations derived from surface science considerations. This illustrates the problem of biased research, namely that pre-defining your results will preclude considering any alternative explanations.

The workload developed to successfully address contamination issues here is normally prohibitive, thus such studies are quite rare (which emphasizes the value of the EarthTech study[16] all the more). If in fact the anomalous results come from an unexpected process, the usual method of proving this is to quantify the production process and thereby establish the reproducibility of the phenomenon. So far, transmutation results are as erratic as the magnitude and time behaviors of excess power results. The 10 authors illustrate this by adding information in their rebuttal about all the *additional* anomalous elements and isotopic distributions detected, *without* addressing the basic concerns of contamination and data misinterpretation. (More bad data never improves a questionable situation.) The 10 authors also cite Mizuno's attempt to alleviate the problem by pre-purifying the electrolyte with a sacrificial electrode. Unfortunately if the contaminants are being leached out of the materials of construction, then no pre-purifying will eliminate the problem. Again, at the overview level, the LENR researchers seem to not recognize the normative precept that must be eliminated in order to drive one to the conclusion that the anomalous elements arise from transmutation. In general this is also a true statement with regards to detection of He 'formation'.

Helium has been claimed to have been detected numerous times by cold fusion researchers. However, there are some fundamental problems with these claims which tend to invalidate them. The primary problem is lack of comparative background sample results. From the beginning in 1989, claims of He formation were plagued by the idea that the He came from atmospheric leaks. Complicating that was the knowledge that 'atmospheric' levels of He in scientific laboratories could vary quite substantially from that of a representative 'typical' atmospheric sample, which is typically taken to contain 5.22 ppm 4He. Furthermore, there was a very telling study done in 2001-2 by W.B. Clarke and coworkers wherein they analyzed 4 gas samples provided by Dr. M. C. McKubre and which were reported to contain LENR generated ^{4}He. Clarke, et al found instead massive evidence of air in-leakage and no ^{4}He beyond what was expected from that. That study re-established that *any* report of analytical He results must also include reports of what else was in the sample and what the background air composition was. To date that requirement has never been met to this author's knowledge. Early studies by Miles, et al, did do some work aimed at addressing these issues, but (a) the reports did not discuss anything but He levels (ignoring the possibility of air leaks that would have been shown by N2 and/or Ne signals), and (b) the work has never been repeated, which fails the detailed reproducibility requirement for conclusiveness.

Most recently, Prof. P. Hagelstein (one of the 10 authors) has posted a progress report for his research facility on his group Web page[18] that contains further derogatory comments regarding the Shanahan propositions, as well as some other interesting observations. In the 'Criticisms' section, Hagelstein begins with a statement that numerous criticisms have been addressed and settled in the past but keep 'reappearing', which insinuates the critics are not cognizant of the current state of the field. Hagelstein then quotes the 2009 Shanahan paper by quoting 3 sentences that stated the observation of poor reproducibility, and then states that the quote is an example of the scientific community discounting the reports because of what is felt to be irreproducibility, with the implication that in fact, it is reproducible. In other words Hagelstein politely accuses this author and the scientific community of fostering the false impression that excess heat results are irreproducible. In this, Hagelstein is playing a sort of 'shell game' where the definition of reproducibility changes according to the purpose of the author. To be clear, this author states that there is *partial* reproducibility in excess heat results, which *implies* a definable process is active, *but* also states that cold fusion researchers have never moved on to *full* reproducibility, where they can specify beforehand exactly how to produce a firm number of watts excess heat. The cold fusion community knows this is true and has said so, but the tactics used by Hagelstein are disingenuous.

What is interesting to note however is a report on their attempt to replicate co-deposition experiments developed by the SPAWAR group (found in the prior "Connection with Fleischmann-Pons and Szpak experiments" section):

> "During the past two years, there was a DTRA supported effort which had as a goal the development of a "lab rat" experiment which could be done quickly and easily, and which would give positive results a large fraction of the time. Part of this effort focused on the Szpak experiment, motivated in part by the shorter cycle time of the experiment (days rather than several weeks), and motivated by a hope of avoiding the serious materials problem of obtaining good cathode samples (which has haunted the Fleischmann-Pons experiment since the early days). Once again, initial attempts to replicate the Szpak experiment seemed to be plagued by a lack of positive results. This was not understood, given the good reproducibility reported initially by Szpak and coworkers. In the experiments of Letts, the initial results were uniformly negative."

This comment was followed by noting that individual contact with the primary researchers was made that might lead to better results. This report clearly illustrates the 'reproducibility problem', and as such demonstrates it is certainly not solved. Hagelstein then goes on to incorrectly conclude that Shanahan feels: "…that all cold fusion effects are not reproducible, and as such they do not belong as a part of science.", which is a gross misstatement of the situation. In fact, the underlying thesis under *all* of the Shanahan publications is that there are interesting results being obtained but simultaneously being grossly misinterpreted. However, it is easier to 'throw the baby out with the bath water' than to give due consideration to the presented alternatives. (To be fair, Hagelstein's collaborator Letts was able to modify the Szpak protocol and make active electrodes. But this just points up the problem with the field, with the relevant question being "Will the next researcher be able to make the Letts modified form work, or the original Szpak form, or will he/she have to do it all over again?")

Hagelstein proceeds to discuss more prior results and then questions another Shanahan quote which suggests that the calorimetric methods are at or near their limit of accuracy. Specifically Hagelstein says in reference to Shanahan:

> "So, how do we understand what this author has written? Seemingly this author is of the opinion that researchers carrying out Fleischmann-Pons experiments are not able to develop and calibrate calorimeters that can get the right answer to within an order of magnitude. The great many control experiments that have been done presumably are to be disregarded, or else the random calibration constant shift proposed by the author must somehow know when a control experiment is done, and know to shift so as to give a positive excess power correlated properly with current density, loading, and whether to shift early if the experiment is a codeposition experiment or with a long delay in the case of a Fleischmann-Pons experiment"

In this quote Hagelstein again repeats the straw man mistake by calling the Shanahan hypothesis random. He further presumes to speak for Shanahan and assigns comments to him that are incorrect and in fact presumptuous and obnoxious. What Shanahan actually says is:

(1) The CCS can induce significant errors in measured output powers, which may result in erroneous excess powers of any given magnitude, dependent on the calorimetric details, *which are normally never published* and thus cannot be quantitatively evaluated.
(2) The control experiments reported are consistent with the CCS problem.
(3) The calibration constant shifts are consistent with the CCS explanation, and do not require pre-knowledge of the intent of the experiment.

(4) The normal calibration methodology obtains calibration constants when no at-the-electrode recombination is occurring. The onset of the FPHE causes an easily understood unidirectional shift away from the norm, completely understandable via the mechanism applied to the calorimeters used so far.
(5) Implicit details of the CCS mechanism are capable of explaining differences in onset lag times, and also suggest potential ways to investigate the problem.

As can be seen, what Shanahan says is diametrically opposed to what Hagelstein says, but the telling point is the repetition of the erroneous "random Shanahan CCSH" straw man argument. Clearly, Hagelstein has failed to understand the Shanahan CCS, and thus is incapable of assessing its value.

Furthermore, Hagelstein ignores another Shanahan proposition which when also considered would tend to invalidate most of the remaining discussion in the "Criticisms" section. Hagelstein spends several more paragraphs discussing how recombination has presumably already been considered. He mentions the fact that in open cells, if the calorimetry is correct, recombination would not only show up calorimetrically, but would show up in reduced off gas products (normally detected by measuring the quantity of water produced by recombining externally). The problem ignored here is the one regarding entrainment of water droplets in the offgas stream. It was noted in Shanahan's 2005 publication that entrainment was the likely cause of the overabundance of water reported in the external recombiner. Thus, water loss rate from the cell (or external water recovered) must consider entrainment, and the effect that subsurface recombination would have on that, but Hagelstein (and others) does not do this.

Hagelstein discusses the apparent excess heat that would be observed in an open cell if 100% recombination were to occur, but this is under the assumption of perfect calorimetry, i.e. if 50 mW of recombination heat were measured, then in fact 50 mW was deposited. However, the basics of calibration point out that for a poorly designed calorimeter, where say, 30% of applied heat is actually not measured, the calibration equation requires that the detected heat be multiplied by 1/.7 to correct Pout to be equal to Pin. Therefore in that calorimeter, 50mW of true heat from recombination would be reported as 71.4 mW. This problem gets less for more efficient calorimeters and larger for less efficient ones (which is in agreement with one of Langmuir's signs of pathological science).

He also cites a recent study by Miles and Fleischmann (M&F) using Pt cathodes which obtained a power balance at the 10e-4 level and thus which would be able to very precisely measure excess heat. (Miles is another of the 10 authors.) However, this paper is another excellent example of how LENR researchers have refused to consider Shanahan's hypotheses. In that paper, M&F make several interesting statements/assumptions:

(1) Pt cathodes are used as they are expected to produce no cold fusion (in opposition to Storms' examples of such)
(2) No entrainment losses are included in their heat transfer model
(3) The electrolysis power term does not include a Faradaic efficiency factor to model recombination (since the *presumption* is that it is 0)
(4) Only electrogenerated O2 reduction (a separate, parasitic reaction which Shanahan agreed was unimportant) is included in their analysis

It should be noted that in general, Pt is known to show less FPHE than appropriately treated Pd, so the likelihood is that the M&F studies did in fact see no FPHE. Therefore, their measure of precision represents a good measure for a non-FPHE system. However, once the FPHE begins to occur, the CCS can likewise occur, and that change will swamp the residual noise factors measured on a non-FPHE system. Thus the M&F paper *does not address* the CCS proposition.

Furthermore, M&F present an equation (eq. 22 in their paper) for a calibration constant kR' which is:

$$kR' = kR + [\ kc\ (T_{cell} - T_b\ ) + RTcell(.75I/F)]\ /\ (\ T_{cell}^{4} - T_b^{4}\ )$$

The prime notation indicates this is an approximation to the real heat transfer constant. By subtracting kR from both sides, and temporarily ignoring the kc term, the approximation magnitude can be directly calculated for their experimental conditions! It turns out to be 3-4 mW depending on the temperature used. Including the kc term would increase that estimate. However, M&F claim their method is accurate to 1 mW, which is inconsistent.

At that point Hagelstein begins to discuss closed cells, where all gaseous products are nominally converted back to water, and concludes that *these* calorimeters are perfect, and since they have detected excess power, it must be real. He concludes by stating:

"Either the author is not aware of, or simply disregards, the work which has laid the issue to rest long ago. In any event, we should regard it as simply in error."

In this instance, 'the author' is the anonymous author of a Wikipedia article on cold fusion. Firstly, to presume that a Wikipedia article is technically accurate is a dangerous assumption. It is the experience of this author that, in particular, the cold fusion article is quite technically lacking. However, Hagelstein is stating the typical LENR researcher position, namely that all critics are uninformed, and usually deliberately so. However, as shown above this is not applicable to Shanahan. (The Wikipedia statement cited *is* technically inaccurate if the Shanahan proposals are included as being covered in it, which is probably not the case, as attempts to get these issues added to the article were blocked by cold fusion aficionados.) Secondly, the Shanahan CCS proposal explains how an inaccurate excess power measurement can occur in a normally functioning calorimeter. This is why the prior quoted statement regarding cold fusion calorimetry being at the limits of its accuracy and precision was made. The error in the computed output power (and thus the excess power) is dominated by the error in the calibration constant determination.

Hagelstein concludes his 'chapter' by discussing a recent review he had received on one of his papers. The reviewer rejected the paper because it attempted to 'rehabilitate' the cold fusion concept, and that concept had been 'disproven' previously. In this case, Hagelstein is right, the mainstream scientific community has adopted that position based on shared mythology and not fact. In fact, Fleischmann, Pons, and Hawkins discovered an effect (the FPHE) but incorrectly explained it. When Fleischmann did the same thing in 1973 for what came to be known as the Surface Enhanced Raman Scattering effect, the scientific community simply discovered the correct explanation. In the FPHE case however, they have opted out and in fact do somewhat suppress the field. This author was not allowed to rebut the paper by the 10 authors in the Journal that published them, and likewise was not allowed to publish a paper critical of the so-called 'Kitamura replication of the Arata work' that appeared in Physics Letter A (see Appendix). In both cases the rejections were based on editorial choice with little to no technical basis. That does represent a situation that is not conducive to good scientific progress. At the same time, the LENR researchers also illegitimately reject propositions that do not contain the word 'nuclear' in them. This also is not conducive to good scientific progress. What is clearly needed is some way to return to the realm of 'good science'. Perhaps an informed re-review of the field could open the way for good criticism of the field to be considered and good research in the area to be published.

Returning now to the paper by the 10 authors, one other rebuttal of Shanahan was included. In the original 2009 Krivit and Marwan article, there was mention of the 1993 Fleischmann and Pons report[20] of a 'heat-after-death' (HAD) event, wherein an electrolysis cell was allowed to boil down to emptiness. In that process it was supposedly noted that the last half of the liquid volume boiled away in 10 minutes, which was far faster what would have occurred if only the applied thermal input was allowed to do the job. Shanahan's primary thrust in the 2009 article was that allowing an electrolysis cell to boil dry was a radical change in cell conditions, and that would most likely results in an equally radical change in heat transfer characteristics, which in turn would necessitate a recalibration, which was never done. This is true, but other problems also exist with the 1993 work.

The 10 authors write:

> "In the 1993 example addressed by K&M and Shanahan, the anomaly observed and reported by Fleischmann and Pons was simply (but remarkably) that the temperature sensed close to an exposed cathode stayed high for a period longer than was previously experienced or anticipated."

This in fact is incorrect. The 1993 paper explicitly calculates the supposed excess energy produced during the HAD event using a '600 seconds' duration of the event. The calculation is based upon evaporating half the cell electrolyte volume in that time period. Fortunately, Fleishmann and Pons also published other information in that paper that allows us to conclude that calculation was wrong. We will examine that now.

Shown in Figure 1 is a 'Photoshopped' version of Figure 8 of the 1993 publication (actual Figures used were from the ICCF3 conference, but they appear identical to the Phys. Lett. A Figures). Originally the Figure 8 was two temperature and voltage as a function of time curves plotted side-by-side (8a and 8b). In Adobe Photoshop, the two halves were overlaid, with 50% transparency used on the upper image. The run with the shorter initial time frame for the operational period where I=0.2 A was translated in the X-direction to match the span of the other run. The two cells had been manufactured to be as identical as possible, and they had been run very similarly. Notations in the paper indicate the cells were filled with approximately 100cc of D2O electrolyte solution at the start of the runs. As can be see, ignoring the fine details of the 'chop' caused by evaporation and refilling of the open cells and calibration heater pulses, the time traces are essentially identical for both temperature and voltage. What is interesting however is that one cell is claimed to have shown a HAD event, while the other is never discussed in that context. It would seem reasonable to expect that such identical performance should have been noted, since it would provide corroborating evidence for the unusual occurrence of a HAD event.

During cell operation, video recordings of the cells were made and the nominal point where half the electrolyte was supposedly evaporated was noted. Stills from the HAD-event cell's video are shown in Figure 12 of the 1993 paper, and it is these stills from which the 600 second number is derived. The excess energy calculation appears in the text immediately below the Figure and uses 2.5 moles of D2O which is one-half of the initial load of 5.0 moles D2O (~100g of D2O or ~100 cc of D2O) and the 600 second timeframe for boil-off of the last half of the D2O. It is the calculation results that suggest a HAD event, having produced a significant apparent excess energy in a short time frame. The claim being

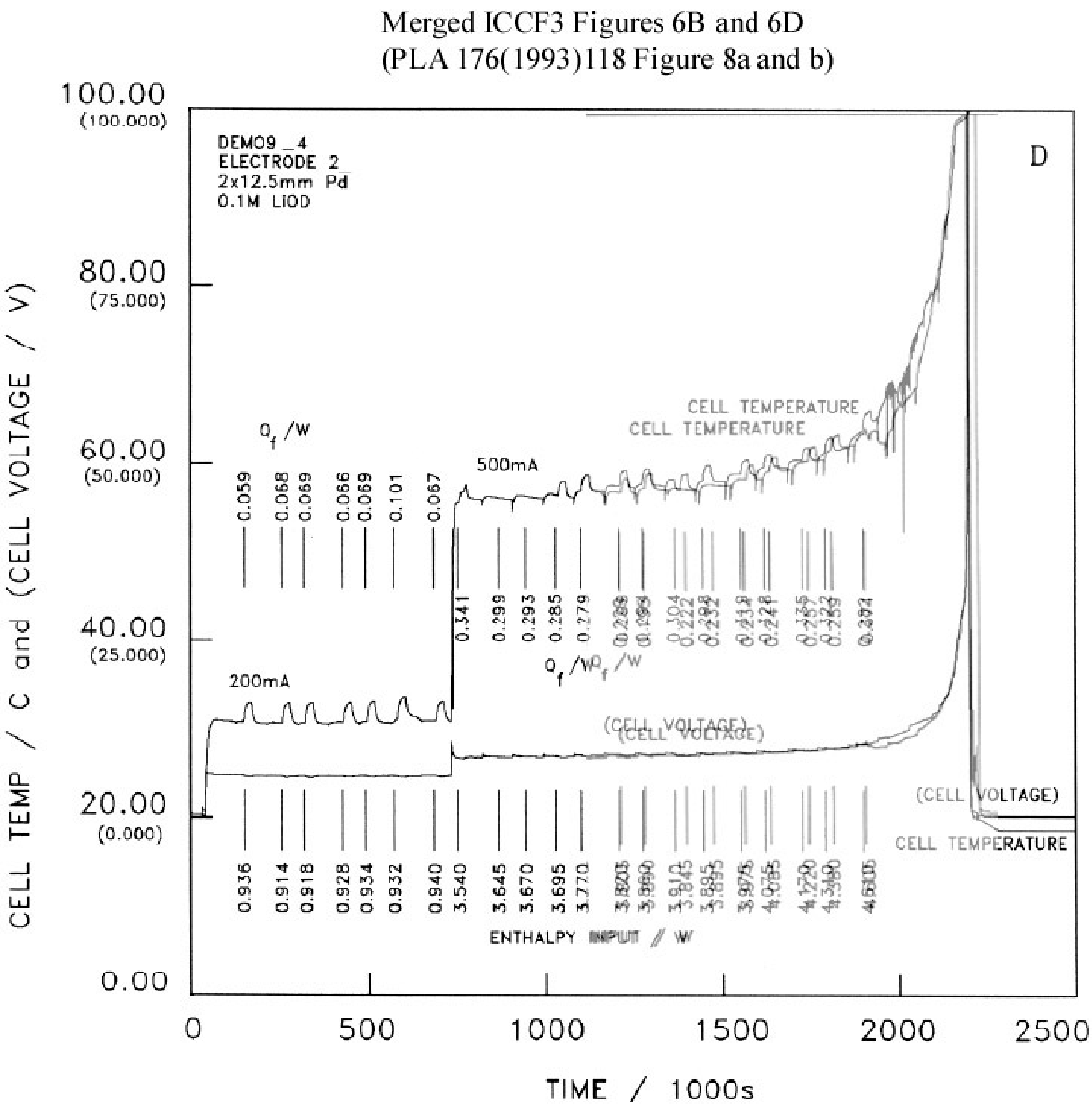

Figure 1. Superimposed run histories from Figures 8a and 8b of Fleishmann and Pons 1993 paper.

made is that ~50cc of heavy water was boiled away in 10 minutes. This is not claimed for the other cell, even though the time-temperature and time-voltage behaviors of the two cells are nearly identical, they were constructed to be identical, and they were filled initially with identical amounts of electrolyte. Surely a HAD event in one cell would have resulted in a different time behavior of that cell. One must assume the non-HAD cell did not show behavior that was considered anomalous. This leads directly to Questions on the validity of using the video-based approach to determine excess energy, thus the net conclusion would seem to be that the 600 second time value is spurious, and thus the excess energy computation was also spurious.

Fleischmann and Pons Calorimetric Method

It has been said that the calorimetry described by the 1993 F&P paper has never been challenged. We would like to do so now. The calorimetry is based upon a lumped-parameter modeling approach where relevant parameters of interest are back-calculated by adjusting their values in a simulation (model) such that the simulation best matches the real world data. The excess heat produced is represented by a single term in the power balance equation (PBEQ), and the details of that term are not defined, unlike what is done for the other terms.

Essentially then, what the excess power term in the PBEQ does is absorb any divergences between the rest of the model and reality. If an identity had not been assigned to that term ('LENR' excess power), it could have equivalently been described as the modeling 'error', and is equivalent to the residual of a statistical model. In fact in chemistry the divergence between real world behavior and ideal (model) behavior is often described mathematically by adding a term to the ideal equation and calling that term the 'excess' property with the intent of defining the mathematical form of the excess term via comparison to real data.

The excess power term (now seen as simply the difference between real and imagined (modeled) cell behavior) can have multiple contributions to it. Three immediately come to mind that do not involve LENR. First, recombination is not considered (neither in the 2008 M&F paper[19]) explicitly as it is done by Miles, et al, as early as 1991, where the PBEQ is modified by adding a multiplicative factor called ϒ, the faradaic efficiency, which ranges from 0 to 1. If recombination occurs, ϒ will be less than 1. Therefore, if any recombination was occurring (not just at-the-electrode in this case, but anywhere in the cell) it would be forced to be accounted for in the excess power term as a positive excess power. This is an interesting assumption in light of the work by Storms, which led to the Shanahan series of papers, who reported excess heat from Pt (Storms actually reporting about 780 mW maximally).

There are discussions elsewhere that indicate obtaining a heat transfer coefficient from this data that is not in agreement with the 'true' heat transfer coefficient would indicate recombination, so we are probably safe assuming in the 2008 paper that no FPHE was active. However, the data presentation therein is somewhat deficient in that they present plots of the L.H.S. vs. the R.H.S. (Left-Hand Side vs. Right-Hand Side of the PBEQ). This method obscures the deviations. Instead the difference of the LHS and RHS should have been plotted vs. either of the 'sides'. This would emphasize any observed patterns in the data, and observed patterns indicate non-random behavior, which in turn indicates a potentially important factor that is working to cause the deviations. If the deviations are of the order of magnitude of the Px, they need to be defined. For example, looking at ref. 19's Figure 4 (p.164), one can note that the line appears to go through the rightmost point, but slightly above the next point to the left, and a little further above the next point to the left, etc. That third point seems to have the largest deviation, with subsequent points seeming to return to the line as they near the origin. This suggests a parabolic shape to the deviation plot, which in turn suggests a non-random factor at work. The maximum deviation seems to be a small fraction of a Watt, but the question is if it is small enough to ignore or not (and whether the factor producing it might give a larger deviation in another experiment). Recall that errors such as this are manifested in the Px term in F&P methods.

Second, in the large multi-term equation presented as representing the enthalpy of the lost gas stream, the PBEQ uses heat capacities of both gaseous and liquid D2O and the heat of heavy water vaporization. This author has never noted the use of a temperature-dependent heat capacity prior to the 2008 M&F paper, but the inclusion is needed, as the Cp of water at a minimum is temperature dependent in the usual temperature regime used. The changes are small but may end up being important because they are multiplied by other terms and thus may be magnified and will again show up in the excess heat term when not accounted for explicitly.

Third, the derived expression for the gas stream enthalpy results in two of its terms being multiplied by the factor P/(P*-P), where P is the vapor pressure of D_2O at that temperature and P* is the ambient pressure of the environment where the exiting gas stream deposits. However, when P approaches P*, which happens when boiling occurs, this term grows infinitely large, and in fact is indeterminate when P = P*. Since it multiplies .75*L (the heat of vaporization at temperature, which is given as 41.7 kJ/mol at boiling), the gas stream enthalpy loss will go infinite, which is clearly unrealistic, but which will result in the 'error' being added to the Px term. Presumably the computer program to analyze the data had some method built into it to avoid this problem, which normally would crash a program. This problem however, will manifest initially as an increasing positive Px (since the enthalpy is expressed as a loss with a negative sign preceding it). The fact that the temperature dependence of L has a minimum at ~333K simply makes the problem less obvious.

Clearly, the simple derivation for the gas stream enthalpy does not apply near boiling. Instead it must be recognized that the equation must instead focus on energy transfer rates as driven by mass transfer rates and power input rate. It is even conceivable that the input power would drive boiling into what is known as the 'film boiling' case, where mass and heat transfer expressions are radically different.

Thus we have concerns that Px will be artificially inflated due to lack of consideration of recombination and (often) temperature dependencies of physical properties. Plus the PBEQ is inadequate in that it does not consider entrainment (which will be computationally *and* experimentally difficult to determine and include). Further, the PBEQ is not a valid equation near boiling.

The PBEQ is not valid under at least two more situations: (1) charging and (2) when part of the electrode has been uncovered (during boiling or just from continued electrolysis). During the initial phase of an experiment, the D_2 gas formed goes into the Pd instead of exiting the cell. This is not accounted for in the Fleischmann PBEQ. Likewise, the gas phase composition exiting the cell during that time frame is not as assumed by the PBEQ ($D_2 + O_2 + D_2O$). Instead it is O_2 from the electrolysis plus D_2O vapor plus whatever was in the cell initially, which is normally air. The heat capacity of that mix would be different from a putative $D_2 + O_2 + D_2O$ mix. Again, the errors here would end up being reflected in Px. Recently several studies of 'co-deposited' systems have been reported, and some claim to have seen Px during the co-deposition. However, these studies use the Fleischmann PBEQ, which is not valid during this timeframe. The reported excess power is most likely just a reflection of this modeling error.

The second arena of invalidity is anytime the cell is simulated when the Pd electrode is partially uncovered. In that case, the uncovered region is not subject to the electrochemical forces that drive the high loading normally obtained, and in fact the Pd would begin to unload. The remaining D in the Pd would then diffuse towards the unloaded region, depleting the Pd still under the electrolyte, so the electrolytic D would now have to replace the D lost from the top region. This process would be controlled by the unloading rate, which for 'good' Pd is supposed to be low, but which in fact is rarely known. This process again alters the base composition of the exiting gas, and thus the gas stream enthalpy expression would be inaccurate. In addition, this D_2 loss pathway would not necessarily allow for equilibrating the D_2 with D_2O vapor. Dependent upon mass transfer rates, this D_2 exits *directly* into the gas space and does not bubble through or grow under the electrolyte as 'normal' electrolysis D_2. This D_2 would dilute the gas space, and thus establish a driving force to evaporate D_2O to reestablish the vapor pressure at temperature, but the question is whether that process would keep up with the electrolysis-driven flow rate. The Pd is normally not run in an uncovered state because of these phenomena, except in boil-off experiments which have been claimed to show large excess heats and heat-after-death. However, the inaccuracy of the PBEQ makes these claims untrustworthy.

These in general are some of the objections that the DOE reviewers of 1989 must have noted when they criticized the F&P calorimetry, along with the idea that isoperibolic calorimetry is a single-point-measurement technique, and such are susceptible to hot spots. The seemingly simple solution to this problem is to close the cell. (At one point Szpak[21] et al, even attempted to bridge the two cases, using a cell that was vented to a recombiner that resided outside the calorimeter boundary.) Then, the gas stream enthalpy term is non-existent, and in a perfect world, the calorimeter/cell could be easily modeled in the lumped parameter approach. However, as was shown by Shanahan, the real world inhomogeneities of experimental apparati lead to the possibility of the CCS problem, which cannot be modeled in a lumped parameter fashion if the cell is treated as a single entity.

In summary then, we find the entrenched rejection of the CCS proposal by the LENR establishment something of a mystery. Perhaps the strongest argument for it is the pictures taken of it in action by the Szpak group using an infra-red sensitive video camera[22]. Yes, they assign the source of the copious hot spots as 'nuclear' explosions, but we simply prefer to drop the 'nuclear' in favor of the more reasonable 'chemical' term. The CCS has strong explanatory power and presents an alternative viewpoint that if followed to the experimental stage might well provide a way to determine controlling factors in these experiments and produce the complete degree of reproducibility that has so far eluded the LENR researchers.

For example, the supposed 'best' way of producing LENR effects today is by utilizing 'nano' powders and/or co-deposition systems. The co-deposition system produces dendritic metal deposits with very high surface-to-volume ratio, which is likewise a feature of nano-sized powders. High surface area would seem to maximize the rate of formation of the special active state that evinces the FPHE. Immersed in electrolyte, these high surface areas serve as very efficient filters which speed up the concentration of the elements necessary to form the active state. The dendritic co-deposition films also serve as very efficient bubble traps simply from their physical structure, which could be thought of as being analogous to the 'filters' stationary sea animals use to strain food from the water.

Calorimetric claims to have observed excess power-energy-heat dominate the claims for proof of 'cold fusion'. But none of the published claims really considers the problems brought out here, and thus they must be held at arm's length until the information allowing them to be judged in light of the CCS problem is published. Likewise, claims to have observed ^{4}He also suffer from failure to present enough information to convince the skilled scientist. These being the two most significant blocks of data 'proving' LENR exist, not fully accepting them *and seriously considering conventional alternatives* forces the scientist to give up on hopes of a Nobel Prize or ridiculously lucrative licensing fees. This is probably the primary motivation for doing what can only be construed as deliberately misunderstanding the CCS proposal. Fortunately, the uncommitted scientist or funding organization is not constrained by this problem.

Summation

At this point, let us recap the primary points of this paper and referenced papers:

Regarding Fleischmann and Pons Lumped Parameter Model Calorimetry

(1) The basic equation used to represent the gas stream enthalpy has multiple flaws
(2) The model cannot be used for charging periods, or during or near boiling
(3) The model does not include entrainment
(4) The model cannot simulate the two-zone model that the CCS uses (at minimum)

Regarding the 'proofs' offered against the CCS model:

(1) The mathematical analysis of the Storms' data has never been challenged, indicating that a minor variation in calibration constants can produce significant excess power signals
(2) The recent response by the 10 authors is fatally flawed and shows their inability or unwillingness to understand the conventional explanations
(3) The idea that 'recombination is routinely searched for and not found' is incorrect. The suggestion here is that nearly every claim to have observed excess anything can be traced to flaws such as those outlined above

Regarding the CCS model/explanation/proposal

(1) The CCS explanation involves a previously unrecognized variation in calorimeter calibration constants that leads to producing apparent excess power signals. 'Apparent' due to the fact their appearance is due to a mathematical misinterpretation of the data, and not to a real, previously unrecognized nuclear process. This variation has strong systematic (i.e. NOT random) character.
(2) The systematic CCS hypothesis presents a potential way to explain any and all excess power results from a Fleischmann-Pons type cell. Given that it is based in the simple fact that calibration constants are experimental values and thus must be considered as variables in error propagation analysis, its precepts may be extensible to other systems, although its details certainly will not.
(3) A 'Special Active Surface' forms on one or both of the electrodes in a F&P electrolysis cell. The SAS formation is a slow process in 'normal' cathode-anode systems, but is greatly accelerated in dendritic Pd from the 'co-deposition process'.
(4) Surface contaminants found on the electrodes in cells or on membranes in gas experiments, or elsewhere, most likely are to be found in the starting materials and are concentrated by the chemical processes occurring in the experimental apparati.
(5) The SAS array is extremely fragile, making it very difficult to study.
(6) 'High' (>.9 D/Pt) loading of Pd only facilitates the formation of the SAS. The SAS may still form on Pd under other conditions. Surface defects may aid in collecting contaminants and forming them into the SAS.
(7) The dendritic structure of co-deposited Pd offers many high energy sites that facilitate the formation of the SAS *and* the capture and holding of gas bubbles.
(8) Hydrogen isotope effects are important. Electrolyte viscosity and bubble adhesion to electrodes will be impacted by them, resulting in different metal surfaces having different results with the different isotopes. (This should be extensible to tritium.)
(9) Hydrogen loading of Pd in either pure Pd or alloy or other complex forms will facilitate impurity migration, which may help the formation of the SAS and/or alter surface compositions
(10) The at-the-electrode, under-the-surface recombination proposed by Shanahan leads to the concepts of pits in CR-39 plates placed in the electrolyte resulting from H2+O2 explosions occurring at favored sites in the SAS array of either contaminants or surface defects or both.

The 'Pathology' of the Field

Part of the fascination with examining this field comes from the attempt to understand how and why 'cold fusion' became known as, alternatively, 'bad science', 'pathological science', 'crackpot science', 'pariah science', etc. In approaching this field, my initial position in 1995 was one of forced neutrality. The field was 'known' to be 'pathological', but my limited contacts with metal hydride chemists and engineers did not produce anyone who could supply a coherent explanation of why this was so. This was somewhat surprising because of the conviction these scientists had in their position. This author was just beginning at that time to personally work with the materials claimed to produce cold fusion, and the simple fact was that if the claims were true, this author's work could suddenly become dangerous. Adding to that the simple intellectual curiosity about how the field got its reputation led to beginning a background study of the field. During the course of that study, several problems, primarily with data interpretation, were uncovered. Initially, cold fusion researchers were contacted with these questions. However, almost universally, once they determined that this author was skeptical, contact was broken off, with one notable exception, Dr. Storms. Through these contacts, those with other cold fusion researchers, those with other scientists involved in documenting and/or rebutting the field, and through the review comments obtained for this author's publications a picture emerged of cold fusion researchers and their critics in general.

In any intellectual arena there will be supporters who put forth claims and evidences that are intended to convince the general scientific community of the validity of their claims. There also will be critics of these claims and evidences, who may present strictly intellectual arguments or who may actually present additional experimental evidences supporting *their* claims and criticisms. This is normal science at work. It has been correctly said that no scientific discovery has been made until it has been reproduced. The difficulty that typically arises in this claim-counterclaim climate is that scientists become emotionally attached to their claims. This is entirely natural and is routinely experienced, but the over-riding requirement is that the scientist makes his or her *best attempt* to remove the emotionalism of the debate and focus strictly on the quality and import of the experimental evidence and theoretical propositions. This is hard to do in some cases, and some people fail outright at this. When that happens, the individual scientist loses the ability to be objective and begins holding beliefs based on personal choice rather than evidence and logic. Note that these beliefs can be either *for* or *against* any given proposition. It is these beliefs that are deemed 'pathological'. Thus we can have either a pathological belief or a pathological skepticism (dis-belief), and *both* are equally detrimental to scientific progress. What tends to be forgotten by people who hold beliefs 'pathologically' is that a perfectly valid position to hold is that there is insufficient data and/or theory to make a singular conclusion at that given point in time. When a field has polarized as badly as the cold fusion arena has, it is a safe assumption that there is quite a bit of 'pathology' being passed around.

It is this author's belief based on his experiences with the cold fusion community that the *primary* cause of this is the unwillingness of cold fusion researchers to consider non-nuclear claims. However, the polarization was exacerbated by skeptics who, having a little difficulty in dealing with the LENR world, likewise shifted to a belief that there were no 'real' results arising from the field, and who in some cases resorted to inappropriate behaviors towards the LENR world. These mainline scientists somehow chose to ignore data that showed *apparent* large S/N ratio signals that indicated 'excess' power/energy/heat was present. That transition was as pathological as the transition to an unwavering belief that the effect must be nuclear. *Both* positions must be quashed if progress is to be made. The apparent high S/N data coupled to the low quality but real reproduction of results indicates something is really there, but the inability to define how to produce these results quantitatively and reliably shows that there is still much to learn. One thing that is always true however is that focusing on trying to control the wrong things will not normally give good results.

This author has presented a consistent conventional explanation dubbed the Fleishmann-Pons-Hawkins Effect (FPHE) for apparent excess heat in Fleischmann-Pons type cells which involves a misinterpretation of experimental data because too simplistic models have been employed. The misinterpretation is to treat the experimental results in a 'lumped-parameter' type of approach that negates the ability to take into account inhomogeneities in the real experimental apparatus. The specific problem was described in 2002 and was dubbed the 'Calibration Constant Shift' problem. It neatly explained how the apparent excess power/energy/heat signals could arise when no true source of excess energy was active.

However, the cold fusion community's response to the proposition can essentially be portrayed as 'denial'. No cogent criticism of the mathematics presented that demonstrated how the CCS would lead to apparent excess heat has ever been published or posited. Instead there have been attacks on the postulated chemical mechanism that might produce conditions leading to a CCS. Even though the proposed mechanism is (a) necessarily preliminary but (b) capable of incorporating a wide variety of experimental results, it has been attacked as 'unrealistic'. Responses to these attacks have been published, but even so, the cold fusion community has pathologically refused to further consider the argument(s). When forced to do so, as in 2010, their response has been to deliberately misunderstand the propositions being presented (a la the 'random' Shanahan hypothesis straw man). Another case in point is the failure of E. Storms to mention in his 2007 book the 2006 Shanahan paper that rebutted Storms' rebuttal. This pathology is equally detrimental to scientific progress.

For real progress to be made, both sides need to investigate the 'mundane' chemical/physical mechanisms being presented to explain 'excess heat' and 'transmutations'. There appears to be some very interesting chemistry at work, that while interesting is unlikely to produce a Nobel Prize or lucrative licensing fees. But that is true of most of the scientific work that occurs and should not be a barrier to proceeding. It might well result in a change in who does the research, since the focus might shift from 'nuclear' to non-nuclear, but that also is normal. In order for this to occur however, the 'mainline' must stop rejecting 'cold fusion' research out of hand, and instead apply standard scientific rigor to the field's submitted publications and presentations. *Likewise* the LENR researchers must *meet* those standards and seek to correct problems pointed out in peer review instead of labeling the critics 'pathological skeptics' and ignoring their comments. Scientific progress comes via finding the keys to 100% reproducibility, and this is most often accomplished collectively.

Conclusion

The current situation can be summarized as a non-optimum situation wherein one highly polarized camp of researchers unwaveringly supports the idea of Low Energy Nuclear Reactions, another highly polarized group of researchers unwaveringly believe the first group is completely mistaken, and the vast majority of scientists believe the issue was resolved in 1992 with the conclusion being that 'cold fusion' is pathological science. This author's position does not fit any of those positions however. This author believes that there are real chemical processes that lead to certain experimental results which are routinely misinterpreted by LENR proponents due to their specific biases but completely dismissed by their counterparts and the general scientific community due to their specific biases. These real chemical processes have certain unique features about them that would warrant some directed research into their characteristics, but they are unlikely to directly lead to anything of major economic value. Scientific information is valuable in its own right however, and new information can always potentially lead to new applications, so well thought out research proposals and investigations in this field should be conducted at the discretion of well-informed funding organizations.

However, because of the extreme polarization of the field and the demonstrated refusal by LENR proponents to consider *any* non-nuclear hypotheses, it is unlikely that any of them will produce well thought out proposals. They will always be biased by their belief that only the 'nuclear' nature of the effects needs to be considered. Similarly, the pathological skeptics will also not produce objections that are unbiased. Therefore, any funding organization desiring to bring clarity to this field needs to exercise caution in whom it picks to conduct research. Unfortunately, the knee-jerk 'pathological science' response of the mainline scientific community makes it equally unlikely anyone else will 'waste' the time required to develop good proposals, which is a lamentable state of affairs, as it basically precludes any significant progress. The proponents of course will continue to conduct biased and misinterpreted work, and make unsubstantiated claims based on that on a routine basis, which will simply fuel a new generation of pathological skeptics who refuse to look past the LENR researchers bias to the raw data (when it is made available).

Current government agencies that are researching LENR directly or funding said research should re-evaluate their activities in light of the evidence for conventional explanations. So far, they do not seem to be aware said explanations exist.

Finally, the USDOE should not be unduly concerned about the safety implications derived from assuming LENRs occur. The evidence to date suggests the proposed 'nuclear' explanations are not valid, and that proposed conventional ones are. While some research towards clarifying this could certainly be supported, the need to do so is not forcibly strong. Furthermore, the USDOE and other government or private agencies should be prepared for recurring claims to have discovered the 'Holy Grail' of LENR in upcoming years, and should exactingly apply normal scientific standards to evaluate both proposals and extant research results.

The case for cold fusion (or 'LENR') stands as unproven today. That fact will remain for all time. If tomorrow, someone discovers the reproducible formula for generating low energy nuclear reactions in or on hydrogen-isotope loaded solids, that fact will not change. The failure of some scientists to obtain the FPHE does *not* prove the FPHE does not occur, because their work can *always* be criticized as being inadequate. Thus, the possibility that cold fusion exists will always be open. The only thing that science can do is show how to reproducibly get an effect. Therefore, it is likely that claims to have discovered *the* way to get LENRs will persist for a long time. However, there is a big difference between *claiming* (or *asserting*) something, and *proving* it.

Acknowledgement and Disclaimer

The author acknowledges the Savannah River National Laboratory for allowing this investigation to occur. The SRNL is part of the Savannah River Site, and is funded under DOE Contact No. DE-AC09-08SR22470. This document in no way represents any official position of SRNL, SRS or DOE. It is the technically-based interpretation of events by this author and any errors in it are his sole responsibility.

References

1.) “A Systematic Error in Mass Flow Calorimetry Demonstrated”, Kirk L. Shanahan, Thermochimica Acta, 387(2) (2002) 95-110

2.) “Comments on "Thermal behavior of polarized Pd/D electrodes prepared by co-deposition”", Kirk L. Shanahan, Thermochimica Acta, 428(1-2), (2005), 207

3.) “Reply to "Comments on papers by K. Shanahan that propose to explain anomalous heat generated by cold fusion", E. Storms, Thermochim. Acta, 2006”, Kirk L. Shanahan, Thermochimica Acta, 441 (2006) 210

4.) “Comments on “A new look at low-energy nuclear reaction research””, Kirk L. Shanahan, J. of Environ. Monitoring, 12, (2010), 1756-1764

5.) “Excess Power Production from Platinum Cathodes Using the Pons-Fleischmann Effect.”, E. Storms, 8th International Conference on Cold Fusion, 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy.

6.) “Thermal behavior of polarized Pd/D electrodes prepared by co-deposition”, S. Szpak, P.A. Mosier-Boss, M.H. Miles, M. Fleischmann; Thermochimica Acta, 410, (2004), 101.

7.) “Comment on papers by K. Shanahan that propose to explain anomalous heat generated by cold fusion”, E. Storms, Thermochim. Acta, , 441, (2006), 207-209.

8.) “A new look at low-energy nuclear reaction research”, S. Krivit and J. Marwan, J. Environ. Monit., 11, (2009), 1731-1746.

9.) “*The Science of Low Energy Nuclear Reactions*”, E. Storms, 2007, World Scientific, ISBN-13 978-981-270-620-1

10.) “New Physical Effects in Metal Deuterides”, Peter L. Hagelstein, Michael C. H. McKubre, David J. Nagel, Talbot A. Chubb, and Randall J. Hekman, manuscript, presented to 2004 DOE Review of Low Energy Nuclear Reactions Panel, August 23, 2004

11.) "Evidence of nuclear reactions in the Pd lattice", Szpak, S., Mosier-Boss, P. A., Young, C., Gordon, F. E., Naturwissenschaften, 92, (2005), 394-397.

12.) "Further evidence of nuclear reactions in the Pd lattice: emission of charged particles", Szpak, S., Mosier-Boss, P.A. , and Gordon, F. E., Naturwissesschaften, 94, (2007). 511

13.) "Anomalous Behavior of the Pd/D System", S. J. Szpak and P.A. Mosier-Boss, Technical Report 1696, (1995), Naval Command, Control, and Ocean Surveillance Center

14.) "Low energy nuclear reaction research - Global scenario", Krivit, S., Curr. Sci., 94, (2008), 854.

15.) "A new look at low-energy nuclear reaction (LENR) research: a response to Shanahan", J. Marwan, M. C. H. McKubre, F. L. Tanzella, P. L. Hagelstein, M. H. Miles, M. R. Swartz, Edmund Storms, Y. Iwamura, P. A. Mosier-Boss and L. P. G. Forsley, J. Environmental Monitoring, 12, (2010), 1765

16.) "Search for Evidence of Nuclear Transmutations in the CETI RIFEX Kit", S. Little and H. Puthoff, http://www.earthtech.org/experiments/rifex/rifex.pdf

17.) "XPS Study on Surface Layer Elements of Pd/CaO Multilayer Complex with and without Deuterium Permeation", 13th International Conference on Cold Fusion, T. Hioki, N. Takakashi, T.Motohiro, Dagomys, Russia, June 25-July 1, 2007

18.) "Fleischmann-Pons effect studies", *Progress Report of the Research Laboratory of Electronics (RLE) at the Massachusetts Institute of Technology* , Progress Report No. 151, chapter 34, http://www.rle.mit.edu/media/pr151/34.pdf

19.) "Accuracy of Isoperibolic Calorimetry Used in a Cold Fusion Control Experiment", M. H. Miles and M. Fleischmann in *Low Energy Nuclear Reactions Sourcebook, ACS Symposium Series 998*, J. Marwan and S. B. Krivit, ed., 2008, p.153

20.) "Calorimetry of the Pd-D2O system: from simplicity via complexity to simplicity", M. Fleischmann and S. Pons, Physics Letters A, 176 (1993) 118

21.) "On the Behavior of the Pd/D System: Evidence for Tritium Production", Stanislaw Szpak, Pamela A. Mosier-Boss, Roger D. Boss, Jerry J. Smith, Fusion Tech. 33 (1998) 38

22.) e.g. "Anomalous Behavior of the Pd/D System". S. Szpak, P.A. Mosier-Boss, Office of Naval Research, 1995, Technical Report 1696; "Polarized D+/Pd-D2O system: Hot spots and mini-explosions," S. Szpak, P.A. Mosier-Boss, J. Dea , F. Gordon, Tenth International Conference on Cold Fusion. 2003. Cambridge, MA, downloaded from http://www.lenr-canr.org/acrobat/SzpakSpolarizedd.pdf; "The Pd/(n)H system: transport processes and development of thermal instabilities", P. A. Mosier-Boss, S. Szpak, Nuovo Cimento, 112A, 1999, 577

Appendix A. Response to Krivit's April 20, 2010 publication

The original reference to this document was:

"Regarding the Department of Energy-Sponsored Comments by Kirk Shanahan", Steven B. Krivit, Editor, New Energy Times, April 20, 2010, published in *New Energy Times, Issue* 35, http://www.newenergytimes.com/v2/news/2010/35/3518responsetoshanahan.shtml

At this date, S. Krivit has just privatized his Web pages at New Energy Times. Thus the Web page his response was originally posted on is no longer accessible without paying for a subscription. The posting therefore is attached following the commentary.

In response to this author's 2010 publication commenting on the 2009 Krivit and Marwan article, Stephen B. Krivit, editor of the New Energy Times newsletter and Web site, published a 'letter'. This letter illustrates many of the flaws outlined in the main body of this report, as well as adding in a few new ones. Therefore, this author would like to respond to the letter. In the following each paragraph of the letter will be addressed. Each response will be preceded by a paraphrased summary of the paragraph's contents that highlights the point or points to be addressed, which will be enclosed in quotation marks. Most of the responses are short. Krivit will be referred to as 'K' below.

The main response body was enclosed in a box on the Web site, page and was preceded by a few paragraphs, which are different from those in the letter. The main gist of those paragraphs in the letter are to suggest that this author has made an inordinate effort to discredit cold fusion aka LENR, even bringing this author's failed attempt to modify the Wikipedia Cold Fusion article into the mix. This is somewhat amusing, as this author's total contribution to the field pales in comparison to those of Fleischmann, McKubre, Miles, and many others. However, any skepticism is too much for a pathological believer.

---- Detailed Comments ----

"Critical and honest skepticism…pathological skepticism…"

No definitions given, apparently these words can be used however one wishes. If a definition was given however, K would have to prove this author's efforts fit.

"three pronged attack…chooses to ignore…cherry-pick…uses distractions to cast doubt…"

K shows his lack of knowledge of how scientific discourse occurs. (A) The 2010 publication was *to be added to* his 2009 publication to produce a balanced picture. (B) Balancing the picture does not *ignore* anything. (C) Page limitations do not allow anything less than selecting the best examples of the point being made, *which K did as well in the 2009 article!* (D) By illustrating problems via 'cherry picking' the *best* examples, *bad techniques, interpretation, and analysis* are clearly illustrated. These aren't 'distractions', they are the crux of the problem, which K doesn't seem to grasp. On the other hand, grasping that would validate them and force K to consider them, a true dilemma for a pathological believer.

"Science asks...but does not require…"

The presented paradigm is incorrect. It is presented as a binary – accept or decline. In fact it is a ternary – accept, decline, be unable to conclude – and unfortunately the latter is the most common occurrence. In fact, by limiting himself to the binary, K misses the whole point of this author's work, namely that *reasonable* alternative explanations exist and thus it is premature to conclude anything. The appropriate next step is to investigate the validity of the alternatives, which the LENR community claims to have done, but which in fact they have not as poignantly illustrated by the 'random Shanahan hypothesis' fiasco.

"no proof or disproof…"

Just lots of insinuations and innuendos…

"erroneous, sweeping generalizations…glosses over….ignores…."

All inaccurate characterizations of what was done. What was actually done was to outline conventional alternative explanations of the data, and point out that they are not addressed by the LENR community. Again, ignoring *reasonable* explanations is the hallmark of pathological behavior. Of course, the LENR community says they have addressed the issues and that this author's propositions are *un*reasonable, but the literature record proves that wrong. The best example (there goes the cherry-picking again…) is the CCS problem, which is a mathematical proof that has never been challenged that ends up requiring all LENR claims to have observed excess power/energy/heat to document the calibration equation variation so that its impact on the excess values can be assessed. That has never been done before, which was not to have been expected formally until 2002, but should have occurred in all subsequent publications (and hasn't).

"LENR researchers…."

Here comes the conspiracy theory… Yes, the author works indirectly for the USDOE, but that doesn't invalidate the claims. The claims are *technical* in nature and can only be addressed *technically*. Whether paychecks or bonuses or whatever were paid is of no importance. If the presented arguments are wrong, show how and they go away. This is the classic conspiracy theorist approach to answering scientific issues though, first attack the issuer as you likely won't have to attack the issue (they think). It fits well with the use of straw men arguments, and *both* are invalid methods.

"Speculate or propagate"

A personal attack of no value. More pathological believer tactics.

"Shanahan tries to persuade…Shanahan does this…" (Combining two paragraphs which probably should not be separate)

The idea here is as stated in this paper's main body. Anytime 'new' elements or molecules are found in an experiment *at trace levels* (which ALL surface science results are) the required initial explanation, settled upon by generations of experience, is contamination. It must be <u>proven</u> they are *not*. This normally is done by establishing quantitative cause-effect relations that allow anyone skilled in the art to easily reproduce the results. *This has not happened!* Therefore, the non-contamination LENR proposal is *unproven.* It is pathological to make a choice here, and borderline pathological to assume the LENR explanation. Furthermore, the CF community is aware of this via the EarthTech studies if nothing else. Every LENR researcher *should* know the general rule of contaminants expressed through the reproducibility requirements, but even if not, the EarthTech results spell it out plainly and clearly: *Contamination is the most likely cause of 'new' elements.* Later K impugns the abilities of the EarthTech researchers, which is nothing more than another personal attack by a pathological believer. Real scientists recognize that EarthTech went to the trouble of proving the old adage *one more time* in a case of 'extraordinary effort', while Iwamura just waved his hands at the issue.

"Shanahan does this with…Kidwell…Shanahan does a disservice" (combining two paragraphs)

Perhaps something can be learned by going through this, let's give it a try… Iwamura reports new elements found. Months later Kidwell goes to Iwamura's lab and finds one of those elements 'in the wild' on Iwamura's benchtop. Iwamura responds by saying "we never got that on our samples". How does Iwamura prove this? Was any cleanup done in the labs in those intervening months? No? Really, do we believe that? They never cleaned up? During the experiments, the lab technicians/grad students/etc. *never* touched any source of contamination? How do you know what they are all? Did you have a special light that caused the contaminants lying on the benchtop to fluoresce so all could see and avoid them? Did you videotape all lab operations during sample prep, analysis, and cleanup? No? How do you know where the contamination was and that it was avoided then? Good lab practice?

No…sorry…the results preclude assuming that. It must be *proved* they arose due to LENR by establishing a protocol that can be easily reproduced anywhere and that provides results that lie *outside* the realm of trace levels. Contaminants were found 'in the wild' (i.e. uncontrolled) in the lab, and the default is always that that contamination was somehow transferred to the samples. The Iwamura response to this is not adequate to overcome the objections. The 3rd choice is still the correct one … unable to conclude. *If* reproducible results are provided, *then* a conclusion may be reached, providing no other *reasonable* explanation is raised to cloud the issue.

"Anyone who…"

Another personal attack implying Shanahan didn't read anything. How does Krivit know this, did he talk to Shanahan? (answer: no) K uses two references here. One is to an EPRI report which this author does not have. It is difficult to get such reports as well, since in principle one must be a member of EPRI to do so. This author did manage to get one such report in the past, McKubre's 1998 report, but the referenced one was not obtained. The other reference is to Miley's results, *which were directly challenged by the EarthTech results!* Do we expect the EPRI report to change this picture? No, but if it is that important to the field, why was it left in a nearly inaccessible place? Surely there are publications detailing the pertinent results that K could have referenced instead of this. But recall that the requirement on a 'good' scientist is to always be ready to abandon closely held theories/laws/beliefs in light of new *good* data. Publish this data and we will all see.

"Readers should consider…"

Conspiracy theory again. Why would DOE care? Good science will trump any such 'cares' anyway.

"Shanahan wants...all excess heats are error"

Shanahan wants all readers to realize there are reporting requirements established by the possibility of the CCS that have *never* been met – *to date!* This means no conclusion can be drawn as to the validity – good or bad- of *any* excess heat report to date. K references 5 papers, 1 of them (Liu, et al) is not on F&P electrolysis cells and thus does not directly bear on this discussion. The rest can be explained via a CCS.

"Consider the…McKubre…Garwin…Lewis"

They did not consider the CCS, which is why it was a publishable paper in 2002…

"The audit team…"

They did not consider the CCS, which is why it was a publishable paper in 2002…
The last sentence of the paragraph is another personal attack on Shanahan. Knowledge of electrochemistry is not needed if all you have to do is 'wiggle' the calibration constants a bit to make the signal go away.

"Shanahan's specious approach…pathological skeptics…Bob Park…."

A mathematically rigorous analysis that clearly demonstrates what is needed to 'zero out' a signal claimed to be significant is not 'specious'. A comment that is not backed by a clear explanation of why it is supposedly 'specious' is however. Bob Park hasn't examined the field as closely as this author.

"Shanahan imaginatively speculates…"

No, this misrepresents completely the extant facts. Again, one more time, Shanahan *shows* that a math trick can zero out the excess heat. Shanahan *shows* the evidence from the data and the data originator's publication that the math 'trick' is entirely reasonable. Shanahan concludes this 'trick' must be shown to have been avoided by any claimant to an excess heat observation. None have done so *to date*. (At least I am 'imaginative'.) Note K's comment "He then simply asserts…" is another straw man.

"To create a stronger foundation…should have first interacted with the principal researchers…"

To put a very fine point on it – why? The data is the data. Talking to Storms (which was actually done by the way) *does NOT change the data! THE DATA showed that it could support the conventional explanation.* In fact, during, before, and after the 2002 publication, *extensive* communication with Storms occurred. In the end, Storms could not refute the analysis or even the speculative mechanism. Storms' 2006 publication was in fact a rehash of those discussions (part of which were posted to sci.physics.fusion with Storms' permission), as was this author's 2006 publication. All that occurred was to put those discussions into the literature, no new information actually appeared for the primary researchers. K's comment here reveals his lack of knowledge arising from the fact that *he* did not contact the primary researchers on this. (pot…kettle…)

"A similar…"
Finally, something that can be agreed to.

"Even McK…"
No comment at this time.

“difference between ‘cold fusion’ and ‘LENR’ was lost on Shanahan…”
No, it was just deemed a tempest-in-a-teapot. ‘Cold Fusion’ is a colloquial name that ‘LENR’ researchers dislike and try to supplant with other names, one of which is ‘LENR’. The facts stay the same.

“Difference far from…”
No, ‘Cold Fusion’ is a colloquial name that ‘LENR’ researchers dislike and try to supplant with other names, one of which is ‘LENR’. The facts stay the same, the theories change, but remain irrelevant if in fact there is no excess heat, transmutation, etc. One needs to confirm the data before building theories.

“The credibility…weakened…neither peer-reviewed nor published…”
Half of the data in the field fits this but this doesn’t stop the LENR researchers. To be specific, the EarthTech work on the RIFEX beads is high quality *negative* results. Negative results rarely get published, but for the record this author encouraged Scott Little to try to do so when the paper first appeared. Not publicly disclosing its funding source makes no difference to the technical content of the paper. The caliber of the other referenced work exceeds that normally found in the field.

“…noise…”

K’s personal opinion, see comments above to assess its validity.

“Clever…who have taken the time to examine…”

A final personal attack implying this author is uniformed. Simple contact with this author would have allayed that fear, but then K would have been forced to consider the ‘alternative side’ - anathema for a true believer, which is illustrated well by the closing lines.

---- FULL TEXT OF KRIVIT POSTING -----
(At this date, S. Krivit has just privatized his Web pages at New Energy Times. Thus the references he includes in this posting pointing to that Web site are unlikely to work.)

Regarding the Department of Energy-Sponsored Comments by Kirk Shanahan

By Steven B. Krivit
Editor, New Energy Times
April 20, 2010

This is a response to a 15-page (8,000-word) set of comments submitted by Kirk L. Shanahan, of the U.S. Department of Energy’s national laboratory at Savannah River,[1] to the 15-page paper by Jan Marwan and me, "A New Look at Low-Energy Nuclear Reaction Research,"[2] in the *Journal of Environmental Monitoring*.
Shanahan’s Wikipedia talk page includes 67,000 words of debate on "cold fusion," and he has devoted six hundred edits on Wikipedia to attacking "cold fusion."

After Harpal Minhas, the editor of *Journal of Environmental Monitoring*, and I made several attempts to agree on the parameters for my response to Shanahan's comment, I gave up and decided to publish a version independently.

Responsibility of Scientific Skepticism

Critical and honest scientific skepticism is essential to the scientific method and to the advancement of scientific progress. Pathological skepticism, however, is used to suppress and discredit competitive ideas, be they ideological, technological or scientific.

In his comments, Shanahan employs a three-pronged attack. He chooses to ignore the preponderance of reliable scientific evidence for nuclear effects in LENR that has accumulated in the past 21 years. He applies highly selective criteria to cherry-pick certain experimental data with potential deficiencies which are vulnerable to attack. He uses these as distractions to cast doubt on the entire large body[3,4,5] of credible LENR data that lies outside the very limited subset on which he focuses his narrow lens.

Science asks but does not require its participants to accept or reject claims; the choice of accepting or rejecting reported results, given accurate information and meaningful analysis, is personal to each individual. Similarly, demand for proof or claim of proof is not a scientific requirement for either skeptic or claimant. Every participant in science has a personal responsibility to directly examine reported claims and data and come to his/her own independent conclusions about its truth and utility.

Our review of LENR was intended to provide readers with a much-needed update to the field. Thus, this response to Shanahan will make no attempt to argue any absolute proof or disproof of LENR claims. That important judgment is the responsibility, as always, of readers.

Specific Remarks About Shanahan's Lengthy Comments

Throughout his comments, Shanahan makes erroneous, sweeping generalizations about LENR researchers. In doing so, he glosses over and ignores wide variances in the quality and thoroughness of reported experimental results among LENR researchers. He ignores the better results and expertise in LENR research. Some of that research is excellent, some is average, and some is inferior and erroneous.

LENR researchers make the best of the limited funding and related resources available to them; the field's longstanding funding problems have imposed particularly severe constraints on access to sophisticated analytical techniques. Ironically, despite the fact that the Department of Energy has spent virtually nothing to support LENR research for 20 years, the department has paid for Shanahan's extensive comments to our paper (under Contract No. DE-AC09-08SR22470).

No single LENR experiment likely will be perfect and definitive in every respect. Calorimetry in one experiment may be weak, but characterization of materials, such as anomalous isotopic changes from that experiment, may be strong. Familiar points of reference, obvious choices for controls and distinct criteria on which to evaluate success or failure do not exist.

Shanahan's method of skepticism is to speculate or propagate insinuations by third parties of things that could have occurred to explain LENR phenomena as ordinary physical processes.

For example, Shanahan tries to persuade the reader that all transmutation products and/or isotopic shifts reported from LENR experiments analyzed with mass spectroscopy or neutron activation analysis are erroneously attributed to LENR, when they are simply the result of some type of contamination.

Shanahan does this with a reference to a conference presentation by David Kidwell[6] (Shanahan ref. 19) of the U.S. Naval Research Laboratory in which Kidwell insinuates that a researcher at Mitsubishi Heavy Industries used "lucky tweezers" contaminated with praseodymium, which, according to Kidwell, accounts for the LENR transmutation results reported by Iwamura.

Shanahan does a tremendous disservice to readers by omitting to mention Iwamura's rebuttal to Kidwell, which immediately followed Kidwell's presentation.[7]

The Iwamura presentation reveals that Kidwell's speculations defy logic and show Kidwell's (and Shanahan's) comments for what they are: pathological skepticism.

Anyone who takes the time to carefully examine all of the field's voluminous experimental data on LENR transmutations should conclude that Shanahan's implication that all such claims are mere "contamination" is patently false.[8,9]

Readers should consider the potential magnitude of low-energy nuclear transmutation research. The implications may be far-reaching as well as a potential embarrassment to federal institutions, such as the Department of Energy, should LENR become a new source of clean nuclear energy and a key national defense application.

Shanahan also wants readers to believe that all excess-heat results in the LENR field are errors. He is correct in saying that some heat measurements in the field may contain large errors. However, that assertion does not hold true for all such data. [10,11,12,13,14,15]

Consider the measurements of excess heat reported by a team led by Michael McKubre at SRI International. In 1993, skeptics visited SRI and carefully audited all the relevant experiments and data. They confirmed, in a written report to the Pentagon, strong evidence of excess heat that "could not possibly be of chemical origin." They considered possible ordinary explanations for the data and found none that was reasonable.[16]

The audit team comprised two of the most outspoken "cold fusion" skeptics ever: well-known nuclear physicist Richard Garwin of IBM (who played a key role in the design and development of the first U.S. hydrogen bomb in the 1950s) and Caltech electrochemist Nathan Lewis. Clearly, if those men had thought the SRI team's excess-heat measurements were erroneous, they would have said so in their report. Given Lewis' long experience, he knows vastly more about electrochemistry and calorimetry issues than Shanahan does.

Shanahan's specious approach is common among pathological skeptics, though in recent years people have been less likely to attempt to cast doubt and uncertainty on the entire set of claims of observed LENR phenomena. Even Bob Park, the previous "arch-enemy" of "cold fusion," has seen the wisdom of treating LENR with more circumspection.

Shanahan imaginatively speculates (or uses citations to third-party speculations) or creatively theorizes about things that could have or might have occurred during an experiment. He then simply asserts with minimal factual justification that the claimed results are better explained by ordinary phenomena and/or are simply a product of compounded experimental errors.

To create a stronger foundation for his comments, Shanahan should first have interacted directly with the principal researchers, discussed his concerns with them, and determined whether his points could be or had been addressed.

A similar responsibility lies with the claimants; if they do not disclose sufficiently detailed background information to provide unambiguous support for their claims, an interested reader is in no position to make an accurate assessment of truth.

Even McKubre, who has a large collection of published reports and data on LENR to his credit, is not exempt from this requirement. A 2000 paper of his that claimed a measurement equating to 24.75 MeV heat per 4He atom without rigorous supporting information does not qualify as a well-substantiated, carefully documented experimental claim.[17]

Concluding Comments

Regrettably, the difference between "cold fusion" and LENR outlined in our article was apparently lost on Shanahan despite our best efforts, although a recent peer-reviewed encyclopedia article[18], presentation to the American Chemical Society[19], and a brief video clip[20] may help clarify this crucial distinction.

The difference is far from semantic; “cold fusion” implies an as-yet-mythical process in which like-charged atomic nuclei overcome the Coulomb barrier at room temperature to produce nuclear energy, products and effects. LENR implies the reality of the same nuclear effects without the presumption of such an as-yet-mythical process.

The credibility of Shanahan's criticisms is further weakened by his citation of and reliance on four references from a single source of allegedly scientific research that are neither peer-reviewed nor published. For 21 years, this shadowy source of private remarks about LENR — Earth Tech International, Inc. of Austin, Texas — has made strenuous efforts[21] to experimentally discredit new-energy technologies and does not publicly disclose its source of funding.[22]

For the most part, Shanahan’s blanket criticisms of the field of LENRs are akin to what he asserts about excess-heat measurements: noise, in this case sponsored by the U.S. Department of Energy.

Clever skeptics can always find ways to theorize and imagine alternate explanations for anomalies in controversial science. For other people, who have taken the time to examine the accumulated body of anomalous experimental data with an open mind, the preponderance of evidence that energetic nuclear phenomena are truly occurring in LENR experiments is abundant and clear. These people will lead science and technology to provide solutions for society.

1 Kirk L. Shanahan, “Comments on ‘A New Look at Low-Energy Nuclear Reaction Research’,” *Journal of Environmental Monitoring*, (in press)

2 Krivit, S. and Marwan, J., “A New Look at Low-Energy Nuclear Reaction Research,” *Journal of Environmental Monitoring*, **Vol. 11**, p. 1731-1746, 2009, DOI:10.1039/B915458M

3 http://newenergytimes.com/v2/reports/SelectedPapers.shtml

4 http://www.lenr-canr.org/

5 http://www.dieterbritz.dk/

6 K. Grabowsky, D.A. Kidwell, C. Cetina, and C. Carosella, “Evaluation of the Claim of Transmutation of Cesium to Praseodymium with the MHI Structure, ” Proceedings of 15th International Conference on Condensed Matter Nuclear Science, Rome, Italy, Oct. 5-9, 2009, (in press) http://www.newenergytimes.com/v2/conferences/2009/ICCF15/Pres/b24-S3_06_Kidwell-EvaluationClaimTransmutation.pdf

7 Iwamura, Yasuhiro, Comments on Evaluation of the Claim of Transmutation of Cesium to Praseodymium with the MHI Structure, Proceedings of 15th International Conference on Condensed Matter Nuclear Science, Rome, Italy, Oct. 5-9, 2009, (in press) http://www.newenergytimes.com/v2/conferences/2009/ICCF15/Pres/b25-Iwamura-CommentsOnNRL.pdf

8 Bush, Ben F. and Lagowski, J.J., "Trace Elements Added to Palladium by Electrolysis in Heavy Water," (Albert Machiels, Thomas Passell, Project Managers) EPRI TP-108743, November 1999

9 Miley, G., Narne, G., Woo, T.,"Use of Combined NAA and SIMS Analyses for Impurity Level Isotope Detection," *Journal of Radiological and Nuclear Chemistry*, Vol. 263(3), p. 691 (2005)

10 Fleischmann M., Pons, S., Anderson, M. W., Li, L. J., Hawkins, M., "Calorimetry of the Palladium-Deuterium-Heavy Water System," Journal of Electroanalytical Chemistry, Vol. 287, p. 293-351, (July 1990)

11 Wilson, R.H. et al., "Analysis of Experiments on the Calorimetry of LiOD-D2O Electrochemical Cells," *Journal of Electroanalytical Chemistry*, Vol. 332, p. 1-31, (1992)

12 Fleischmann, M. and S. Pons, "Some Comments on The Paper 'Analysis of Experiments on The Calorimetry of Liod-D2O Electrochemical Cells,' R.H. Wilson et al., *Journal of Electroanalytical Chemistry*, Vol. 332, (1992)," *Journal of Electroanalytical Chemistry*, Vol. 332, p. 33 (1992)

13 Li, X.Z., et al., "Correlation Between Abnormal Deuterium Flux and Heat Flow in a D/Pd System," *Journal of Physics D: Applied Physics,* Vol. 36, p. 3095, (2003)

14 Kainthla, R. C., Velev,O., Kaba, L. , Lin, G. H., Packham, N. J. C. Szklarczyk, M. Wass, J. and Bockris J. O'M., "Sporadic Observation of the Fleischmann-Pons Heat Effect," Electrochimica Acta, Vol. 34(9), p. 1315-1318, Sept. 1989 doi : 10.1016/0013-4686(89)85026-1

15 Oriani, R.A., et al., "Calorimetric Measurements of Excess Power Output During the Cathodic Charging of Deuterium Into Palladium," *Fusion Technology*, Vol. 18, p. 652, (1990)

[16] http://www.newenergytimes.com/v2/reports/GarwinLewisReport/garwin.shtml
[17] McKubre, Michael, Tanzella, Francis, Tripodi, Paolo and Hagelstein, Peter, "The Emergence of a Coherent Explanation for Anomalies Observed in D/Pd and H/Pd Systems; Evidence for 4He and 3He Production, " Proceedings of 8th International Conference on Cold Fusion. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy.
[18] Krivit, S.B, "Cold Fusion - Precursor to Low-Energy Nuclear Reactions, " *Encyclopedia of Electrochemical Power Sources*, Vol 2, Juergen Garche, Chris Dyer, Patrick Moseley, Zempachi Ogumi, David Rand and Bruno Scrosati, eds, Amsterdam: Elsevier; Dec. 2009. p. 255–270, ISBN 9780444520937
[19] Krivit, S., "Low-Energy Nuclear Reaction Research – 2008 Update," American Chemical Society, Philadelphia, PA, Aug. 20 2008
http://newenergytimes.com/v2/library/2008/2008-Krivit-ACS.pdf
[20] http://www.youtube.com/watch?v=7i5MXRitINU
[21] Little, Scott, "Null Tests of Breakthrough Energy Claims, " Proceedings of 42nd AIAA/ASME/SAE/ASEE Joint Propulsion Conference, AIAA 2006-4909 (2006)
[22] http://www.newenergytimes.com/v2/news/2007/NET22.shtml#earthtech

Appendix B. The comment on the Kitamura replication

Kitamura, et. al published a short paper in Physics Letters A in 2009 (A. Kitamura, T. Nohmi, Y. Sasaki, A. Taniike, A. Tahahaski, R. Seto, Y. Fujita, Phys. Lett. A 373 (2009) 3109), which this author felt contained several errors and inconsistencies. A manuscript Comment was prepared and submitted. However, the Phys. Lett. A editor rejected the paper based on two supposed points, as best I can interpret them: (1) PLA doesn't want to publish cold fusion papers, (2) my paper had no new data in it. I responded by pointing out regarding (1) that the paper had already been published, and regarding (2) that the paper *did* have new data in it, a Figure I included to demonstrate the potential impact of spillover.   After some delays, the editor suggested I shorten the paper. I did so by cutting out the Figure and discussion and trimming a little further, and resubmitted it. After several status requests, I have never received a reply to my resubmission.

Attached is the original version of that manuscript for those who are interested.

SRNL-STI-2012-00678

Comments on “Anomalous effects in charging of Pd powders with high density hydrogen isotopes”

Kirk L. Shanahan

Savannah River National Laboratory

Bldg. 999-2W

Aiken, SC 29808

kirk.shanahan@srnl.doe.gov

A manuscript for submission to

Physics Letter A

SRNL-STI-2012-00678

Comments on "Anomalous effects in charging of Pd powders with high density hydrogen isotopes"

Kirk L. Shanahan

Savannah River National Laboratory

Aiken, SC

Abstract

In Kitamura, et al[1], Pd-containing materials are exposed to isotopes of hydrogen and anomalous results obtained. These are claimed to be a replication of another experiment conducted by Arata and Zhang[2]. Erroneous basic assumptions are pointed out herein that alter the derived conclusions significantly. The final conclusion is that the reported results are likely normal chemistry combined with noise. Thus the implied claim to have proven that cold fusion is occurring in these systems is both premature and unlikely.

Introduction

In Kitamura, et al[1], (hereafter referred to as 'Kitamura') two Pd powders and a PdO/ZrO_2 powder are exposed to protium (H_2) and deuterium (D_2) in a flow calorimeter setup. Ostensibly the intent was to duplicate experimental results of Arata and Zhang[2] (hereafter referred to as 'AZ') that purportedly prove a nuclear process is active in deuterated Pd, the classic cold fusion argument. However, several confounding issues are present in Kitamura that are addressed in this comment.

In Table 1 of Kitamura, fourteen experimental sequences on the three types of Pd-containing powders are summarized. Kitamura arbitrarily divides the absorption process into two phases with the first phase ending when the cell pressure begins to rise from near zero. Most of the tabulated results refer to this first phase. Eleven of the experimental sequences described represent the results of exposing these powders to hydrogen isotopes for the first time, typically called the 'first cycle'. Three results for subsequent exposures (2 'second cycle' and 1 'third cycle') are also reported. Kitamura does not explicitly state at what temperature the loadings are conducted. One possibility is that they conducted the hydrogen exposures at room temperature, which will be the assumed protocol herein. However, another possibility is that they maintained the reported bake-out temperatures during loading, but if true, that simply worsens their case, as discussed below.

Issue #1 – Pd loadings

In 9 of the 11 first cycles, loadings with H/M or D/M (generically referred to as 'Q'/M) ≥0.79 at the end of the first phase are reported. In the case of the palladium black runs, one sample that reportedly loaded to Q/M=0.79 in the first cycle is also reported to have in subsequent second and third cycles only loaded to ~0.24 Q/M. Three runs with 0.1 micron Pd particles, one a second cycle, were reported to have loaded to Q/M~0.44.

The high reported loadings are inconsistent with known bulk Pd chemistry. Pd-hydrogen isotherms are shown in Figure 3.4 of Wicke and Brodowski[3], and at 293 K and ~1MPa, an H/M value of ~0.78 is obtained. The Figure also illustrates that hydrogen content decreases with increasing temperature. Lasser and Klatt[4] present data for all three hydrogen isotopes. Since D is less soluble in Pd than H at a given loading pressure, the D/M value obtained will be lower, and D/M likewise decreases for a given pressure as the temperature increases. If Kitamura actually exposed their samples at the bake-out temperatures, expected loadings would be < .78. To obtain loadings of >0.78 would require either cooling below nominal room temperature, which is not indicated in the paper, or much higher loading pressures.

As well, Kitamura's loadings are for the first phase only. At the end of the first phase, the cell pressure is still very low or zero and has not approached the final pressure. Therefore the final loading of ~0.78 would not have been obtained, yet many values equal to or greater than this are reported specifically for first phase loading. Actual loadings that would be anticipated under equilibrium conditions at the end of the first phase would be closer to those expected at the plateau pressures (at best, ~0.6 for a nearly complete loading at the low pressure). (The estimated plateau pressures of H and D in Pd at 293K are ~7.3 kPa for D and ~0.8 kPa for H, based on the Lasser and Klatt's[4] van't Hoff data.) The lower loadings reported for the 0.1 micron Pd particles and the second and third cycle of Pd black are more typical of loadings that remained in the plateau region, i.e. quarter- to three-quarters-loaded.

The loadings reported may be indicative of inadequate Pd activation, which would affect the results in two ways. Inadequate activation may greatly hamper absorption kinetics and potentially ultimate loading level, and may cause unexpected heat from side reactions. As received Pd normally has significant surface contamination (typically O, C, or S), and this contamination typically hinders the absorption of hydrogen into the Pd[5,6]. The usual procedure when studying hydrogen isotope chemistry in Pd is to 'activate' the material by cycling several times (often >3) in hydrogen to remove these contaminants, complete activation being indicated by rapidly obtained loadings (as expected from surface-to-volume ratio considerations, foils load more slowly than powders) that are in agreement with literature values. Normally, the first few absorption/desorption cycles are not studied due to known difficulty with reproducibility. The differences reported by Kitamura between first cycle results and second and third cycle results are indicative of this problem. Any excess loading obtained in a first cycle by Kitamura above what is subsequently obtained in second and third cycles is more probably indicative of the extent of chemical reaction with these expected surface contaminants during the first cycle than indicative of cold fusion. (Note that the cycling conducted by Kitamura may not assure second and third cycles would load fully.)

It should also be noted that the value of 0.2 eV/atom H obtained in run H-PP2#1 is in agreement with the plateau value obtained by Flanagan, et al [7] and Luo and Flanagan [8] (~19 kJ/mole $1/2H_2$ = 0.20 eV/H). The increase to 0.25 and 0.26 when using D in runs D-PP1#1 and D-PP1#2 was suggested to be due to an isotope effect, but the reported value for the Pd-D system is lower than that of the Pd-H system [7] (~17 kJ/mol D = 0.18 eV/atom D, note that mole H = ½ mole H_2)). In fact, Kitamura's errors listed suggest there is no statistically significant difference between their values and they are not precise enough to distinguish between D and H.

In the case of the PdO/ZrO_2 powder additional complications arise. Kitamura correctly notes the PdO reaction with hydrogen to form water, but incorrectly assume "It is difficult to assume large contribution of ZrO_2 to these quantities." A significant complicating factor with metal oxides is the observation that absorbed hydrogen can migrate onto the oxide, ZrO_2 in this case, to form surface hydroxylated material, i.e. ZrO_2H_x, where x in indeterminate and difficult to control, in a process typically known as spillover. Kitamura rejects this possibility but without specifying why. PdO/ZrO_2 has been specifically studied in this regard and spillover noted[9].

As an illustrative example, this laboratory has measured hydrogen uptake on a powder consisting of well-activated ~5nm Pd nanoparticles supported on α-alumina, and we have detected significant spillover as indicated by the initial portion of the absorption isotherm where all hydrogen introduced is absorbed (see Fig. 1, the early flat region is not observed on unsupported Pd). Only after completing the spillover reaction is the typical Pd isotherm observed, now displaced on the millimoles absorbed axis by the spillover. Note that the amount of H absorbed by the alumina in this case was approximately equivalent to that of the Pd. This means that in the 'first phase' region, this material would produce approximately twice the amount of heat expected from Pd hydriding alone. Failure to consider these processes lead Kitamura to partition the absorbed hydrogen incorrectly, obtaining loadings much too large for the conditions used, but consistent with those presumed to be required to obtain cold fusion.

The observed heat will be the combination of the heat of hydriding and any hydrogen reactions with contaminants, plus surface oxide reduction and whatever heat of reaction with the support is to be expected based on the chemistry. These reactions can potentially continue into the second phase depending on the quality of the activation process used. (Heat could also continue to be generated by hydriding reactions during the second phase depending on the actual loading level at the arbitrary end of the first phase.) The large E_{1st} values reported in Kitamura's Table 1 for materials which presumably loaded to greater than Q/M=0.7 seem too large due to these considerations. The standard considerations noted above should be the first assumed causes for any real excess heat over that expected from simple Pd hydriding chemistry.

Issue #2 – Baseline noise vs. signal?

Kitamura presents graphical results for their powders in Figure 3, showing the output power obtained as a function of time while loading these samples. Both the 0.1 micron Pd particle sample and the Pd black sample evidence baseline deviations in excess of the residual baseline noise occurring after the hydriding reaction heat has subsided (the second phase) that Kitamura also consigns to noise (Figures 3a and b). In Figure 3c they present results for the PdO/ZrO_2 samples which they conclude represent anomalous power and thus replicate the AZ results. However, they also acknowledge that there has been a thermocouple malfunction in the hydrogen trace of Figure 3c, resulting in a negative baseline shift. This has the immediate effect of making the baseline deviation in the deuterium trace in Figure 3c look more significant, since now it is unobscured by the hydrogen trace as in found in Figures 3a and 3b. In the D_2 trace, the period running from ~ 200-500 minutes seems relatively flat and offset positively by approximately the same amount that the hydrogen trace is offset negatively. Furthermore, there appears to be a large and abrupt baseline shift at t~1350 m., where the baseline now goes negative. The maximum deviation of the deuterium trace from the offset baseline of 200-500 m. (before the negative shift) is only about 0.1 W, which is equivalent to those observed in Figures 3a and 3b, where it is assigned to noise. The conclusion would seem to be that the deviations in Figure 3c should likewise be assigned to noise, but instead Kitamura claims they are true excess power signals and indicate a replication of the AZ work, i.e. cold fusion. This seems unlikely.

Issue #3 – Replication or not?

The underlying intent of the Kitamura paper seems to be to declare that a replication of the AZ results has occurred. However, this is only true in the most general of senses, as the two experiments produced significantly different *classes* of results. In AZ, material was loaded and the resultant heat followed as it slowly flowed out of the system and the temperature returned to ambient. The internal temperature of the experimental apparatus evidenced a continuous and uniform positive offset from the external temperature over a span of 3000 minutes, where the presented data ceases. In the Kitamura case however, the deviations in the output power are erratic both in duration and in intensity, sometimes even going to negative values. The negative apparent energy indicates that heat was flowing *into* the cell during that time period, which was never observed in the AZ case. Thus the extent of the Kitamura 'replication' seems to be that anomalous (but different) signals were observed in similar experimental configurations, which is a typical of results in cold fusion research. Unfortunately, the reasons for the anomalies in both cases are unknown and cannot be equated without independently determining them, and attempting to do so represents a fundamental logical flaw. One anomaly does not <u>replicate</u> another *different* anomaly unless the causes of both are known, and the differences can be analytically explained. True replication requires the ability to nearly exactly reproduce the behaviors noted.

Conclusions

The Kitamura paper represents an archetype of cold fusion research results, with poor understanding of the related chemistry and directed assumption-making aimed at confirming unlikely explanations as the norm. As well, the idea that any anomalous result confirms another, even when grossly different in behavior, is a rampant problem in cold fusion research. Because of the issues brought up herein, the conclusion that observation of anomalous energy emanating from a Pd-containing material seems premature. What seems more likely is that activation issues caused significant underloading of the Pd materials, and side reactions caused significant heat generation in several cases. Failure to recognize this caused misassignment of Pd loading levels, which in turn lead to incorrect energetics calculations. The actual signals presented as true anomalous excess power are more likely just baseline drift and the behavior of the baseline drift is significantly different from that observed by AZ. Thus no replication of AZ has occurred, leaving the AZ result as another unsupported anomaly claimed to prove cold fusion.

Acknowledgments

This document was prepared in connection with work under Contract No. DE-AC09-08SR22470 with the U.S. Department of Energy. Helpful conversations with Dr. D. Kidwell, Naval Research Laboratory, Washington, DC, and Prof. T. Flanagan, U. Vermont, Dept. of Chemistry, are gratefully acknowledged. The Pd on alumina material used in our work was prepared in the laboratory of Prof. M. El-Sayed of the Georgia Institute of Technology.

References

[1] A. Kitamura, T. Nohmi, Y. Sasaki, A. Taniike, A. Tahahaski, R. Seto, Y. Fujita, Phys. Lett. A 373 (2009) 3109

[2] Y. Arata, Y. Zhang, J. High Temp. Soc. 34 (2008) 85

[3] E. Wicke, H. Brodowsky, in "Hydrogen in Metals II", G. Alefeld, J. Volkl (eds.), Topics in Applied Physics, vol. 29, 1978, Chapter 3.

[4] R. Lasser, K.-H Klatt, Phys. Rev. B, 28 (1983) 748.

[5] T. B. Flanagan, W. A. Oates, Ann. Rev. Mater. Sci. 21 (1991) 269

[6] W. Oates, T. B. Flanagan, Nature, 231 (1971) 19

[7] T. B. Flanagan, W. Luo, J. D. Clewley, J. Less-Common Met. 172-174 (1991) 42

[8] S. Luo, T. B. Flanagan, J. Alloy and Cmpds. 330-332 (2002) 29

[9] L. F. Chen, J. A. Wang, M. A. Valenzuela, X. Bokhimi, D. R. Acosta, O. Novaro, J. Alloy and Cmpds. 417 (2006) 220

Figure 1. 353K Hydrogen Absorption/Desorption Isotherm from Nanoparticulate Pd on Alumina (prepared by El-Sayed group at GIT, data collected at SRNL)