Comments on "A new look at low-energy nuclear reaction research"

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Cold fusion researchers have accumulated a large body of anomalous results over the last 20 years that they claim proves a new, mysterious nuclear reaction is active in systems they study. Krivit and Marwan (*J. Environ. Monit.*, 2009, **11**, 1731) give a brief and wholly positive view of this body of research. Unfortunately, cold fusion researchers routinely ignore conventional explanations of their observations, and claim much greater than the real accuracy and precision for their techniques. This paper attempts to equally briefly address those aspects of the field with the intent of providing a balanced view of the field, and to establish some criteria for subsequent publications in this arena.

Introduction

Krivit and Marwan[1] (referred to hereafter as 'K&M') present a wholly positive view of so called low energy nuclear reactions (LENR) research. The history of LENR research, more commonly known as cold fusion research, is dominated by one prevailing fact, irreproducibility. From the very beginning, this has caused consternation and frustration in all those who have attempted to research and/or study the field. In the 'hard' sciences like chemistry and physics, which is the arena wherein cold fusion plays, no significant scientific progress can ever be made without reproducibility. However, K&M gloss over this problem while attempting to promote the idea that significant progress *has* been made.

While some limited progress has been made in the 20 years since cold fusion was first announced, seemingly by simple trial and error methodologies, in general the only thing that has actually occurred is a mutating diversification of what was once the simple Fleischmann and Pons (F&P) electrochemical exper iment. Thus K&M provide lists of experimental methods, trig gering methods, and effects and products. Unfortunately, simply providing lists of anomalies without providing compelling reasoning to interconnect these methods and results is a logical flaw. Anomalous results are typical occurrences in scientific research. Without knowing why each individual anomaly occurred, *i.e.*, without knowing, or at least postulating, the cause of the anomaly, it is impossible to relate that anomaly to any other anomaly by anything other than coincidence. And certainly, one must resolve causes before anomalies can be used as proof of any thesis.

The process of resolving anomalies is what science is all about, since all known processes and effects began as 'anomalies' or unexplained observations to the researcher of that time. In the case of K&M's lists, conventional explanations are readily brought up that have the potential to explain how the anomalous event(s) occurred. But K&M do not bring any of these to light, instead focusing on the presumed nuclear explanation favored by cold fusion researchers, who also generically fail to consider conventional explanations adequately.

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The only appropriate way to discuss the methods and results presented by K&M is individually. However, this produces an excessively long article if attempted to completeness, and this author is not skilled enough in many of the areas needed to address the complete listings. Therefore, we here will address some selected areas in detail, some we will comment on in passing, and others we will not address, all the while attempting to limit the length of this paper, which necessarily may not bring out all the details the reader may have preferred to see.

Calorimetry

The largest single block of results claiming to support the nuclear thesis is measurements of what is commonly known as excess heat. Cold fusion researchers construct calorimeters and place various types of experiments in these calorimeters in an attempt to detect the heat released by these purported unexpected nuclear reactions. Many positive results have been reported with very high signal to noise ratios as normally computed, and therein resides the problem.

In 2002, this author published a reanalysis[2] of laboratory data claimed to have shown unequivocal excess heat[3] (or more correctly, power), wherein a previously unrecognized systematic error was demonstrated to have the capacity to explain the observations without invoking a nuclear reaction. This error was termed the 'calibration constant shift' (CCS). This explanation was challenged twice,[4,5] and responses published,[6,7] although the first challenge was non specific. The second challenge focused on the proposed speculative mechanism for how the CCS might have occurred in F&P type cells rather than on the CCS expla nation itself. The responses clarified the issues and left the CCS unchallenged as a potential explanation of apparent excess heat signals.

The CCS is a fundamental problem that can actually occur any time a calibration equation is used to interpret experimental data, and thus is actually widely applicable and not just limited to cold fusion calorimetry. The basic requirement for being able to successfully calibrate an analytical device (such as a calorimeter or voltmeter) is for that device to be stable for a reasonable period of time. If a calibration is established on a device, and then it changes its condition, the previously determined calibration expressed *via* a calibration equation with constants is suddenly

made invalid. If this shift is not recognized, the experimentalist will apply the prior calibration equation with currently invalid calibration constants to compute the experimental result(s), giving an error. What should have been done was to recalibrate under the new steady state and use the new calibration constants, since the constants have now 'shifted' to new values. This is the genesis of the term 'calibration constant shift'.

In the reanalysis,[2] a simple assumption was made, namely that the system's steady state had changed due to the onset of what was called the non nuclear Fleischmann Pons Hawkins effect (FPHE). This change in steady state was assumed to have been accomplished without the introduction of a new excess heat source. The data was then reanalyzed under that assumption and the 'new' calibration constants determined for each individual run. The calibration equation used in this case was a simple linear one ($y = mx + b$) and the slope term was explicitly examined, with the result that the variation in that term of $\pm 1\%$ (1 σ) was found. This is a common precision level for a good analytical technique, yet it was adequate to explain an excess power signal of 780 mW, which was $\sim$10 times the commonly assumed error level determined by baseline noise fluctuation.

This is why the CCS is such an important realization regarding these experiments. It increased the quantitative measure of system noise by a factor of 10. And there is no reason to believe this one case developed with a top line calorimeter (98%+ heat capture efficiency) limits that factor in any way. The CCS is a systematic error, and it is an unfortunate fact of systematic errors that they tend to invalidate all prior work since the error was not recognized before the point where they are delineated. Thus it becomes imperative to evaluate the sensitivity of the calorimetric results to potential changes in the calibration constants in a process knows as sensitivity analysis. To date, no cold fusion calorimetric study has done this, even though this revelation was published 7 years ago.

What is required of cold fusion calorimetric reports is the clear inclusion of the calibration equation and constants used, and some measure of their variability. As well, some sensitivity analysis of the impact of this variation on the results is required, since it was shown in the reanalysis[2] that less efficient calorimeters could be expected to show a greater CCS effect. This is consistent with the idea that the function of the calibration equation is to 'adjust' the signals up to equate to the actual input power (upwards adjustment is required since some heat is always lost). In a less efficient calorimeter this adjustment is larger than in a more efficient one, and thus variation in those constants will have a larger impact on the computed result. In principle, such data exists in laboratory records, but none has been presented to date.

Thus it would seem that cold fusion calorimetry is currently near or at its limits of accuracy and precision. But these limits still allow for large apparent excess power peaks, and thus excess heat measurements must be carefully scrutinized to insure that they are not caused by a CCS. To date this author knows of only one instance of this[2] and that case rejected the claim of true excess power. In the response of 2005[6] this author pointed out that the claims of excess heat in the paper[4] preceding the response could likewise potentially be explained by a CCS, but actual data that would allow such a determination was missing.

K&M go on to discuss a specific type of calorimetric result obtained by F&P commonly known as 'heat after death' (HAD). In the HAD experiment, a F&P electrolysis cell is allowed to lose enough electrolyte *via* evaporation, entrainment, and electrolysis that electrical contact is broken and current flow stops. Such an event is shown in K&M's Figure 4 and 5, where an excess power signal is observed for approximately 3 h after this point is reached.

It is instructive to review this result with the prior discussion of the CCS in mind. Once current stops, the driving force to load the Pd electrode with hydrogen is removed, and the system seeks to obtain equilibrium under the new state by releasing gas, converting the situation from an electrolysis cell to a gas unloading experiment. At the point where electrical contact was broken, the cell gas was very nearly a stoichiometric mixture of hydrogen and oxygen. Significant hydrogen release will occur because the electrolytic loading was equivalent to many atmospheres of hydrogen gas pressure exposure, but the hydrogen present will inhibit complete Pd unloading. The equilibrium plateau pressure of Pd D at $\sim$70 100 °C is $\sim$300 1000 mbar,[8] and the cell pressure is $\sim$1000 mbar since it is an open cell, with at least 2/3 of that consisting of D_2. Thus initial unloading to $\sim$0.6 D M ^{1} units should occur, and not much more, leaving plenty of hydrogen in the electrode as hydride.

With the Pd and Pt electrodes exposed, a metal surface is presented which will catalyze the recombination of hydrogen and oxygen. That process does three things; (a) it reduces the hydrogen pressure, causing the Pd to unload further, and (b) it reduces the overall pressure, causing air to be drawn back into the cell, resupplying the oxygen content of the cell gas somewhat, and (c) it produces heat, which will presumably be detected by temperature sensing devices in the cell. Clearly, the steady state is now *radically* different and the system would have to be recalibrated under the new steady state to translate those temperatures into heats. However, K&M report that the *energy* detected was "far beyond the quantity of possible stored chemical energy". No recalibration was reported in the 1993 F&P paper, so one wonders how this was determined accurately.

At an even more subtle level is the consideration of the *rate* of hydrogen release from the Pd electrode, which would impact the amount of time heat would be produced by recombination. It is well known that surface contaminants affect the ability of Pd to absorb and desorb hydrogen,[9,10] and after being immersed in an electrolyte solution for hundreds of hours, it is unlikely the Pd surface is pristine, and in fact there are many studies showing it is not (see below). Furthermore, crack free Pd is supposedly required to allow high loading to be attained, and high loading is supposedly required to obtain the FPHE. This in turn implies a low surface to volume ratio (SVR) is the desired condition. Hydrogen desorption from Pd is a diffusional process, and the lower the SVR, the slower the desorption will proceed. To maintain, or even slightly increase, the registered temperature would only require enough heat to be supplied to replace that lost from the cell, which is now an unstirred gas cell instead of a well stirred liquid cell.

The original cell design was well insulated, implying the natural heat loss rate would be low. It seems eminently reasonable to assume that that amount of heat lost would be small per unit time. The Pd in the 1993 experiment likely evolved hydrogen

slowly, allowing for heat production over extended periods. Thus the HAD event was completely predictable by conventional chemistry. This author knows of no HAD event reports that recalibrate due to the obvious CCS and consider the above concerns. Thus reported HAD events certainly do not signifi cantly advance the nuclear hypothesis. What is required to fully understand HAD events is (a) recalibration (with supplied data/ results as above) and (b) detailed surface examinations of the Pd coupled with gas loading/unloading studies. With that informa tion the above concerns might be addressable.

To summarize then, CCSs and standard Pd hydride chemistry seem to be a crucial part of understanding cold fusion calorim etry results, and some discussion and experimentation aimed at them should have been expected since the 2002 recognition of their possible impact. Unfortunately this is not the case. In fact, exactly the opposite has occurred. In 2007, Dr E. Storms, author of the original reanalyzed data,[3] published a book purporting to review the LENR field.[11] While the book contains hundreds of references, it falls short of being a definitive exposition on the subject for two reasons. First it fails to critically examine the field's papers, instead just providing another uncritical list of what has been published. Second, it seemingly deliberately omits crucial negative publications. Of relevance here is the omission of the 2006 rebuttal[7] to Storms' 2006[5] publication.

A simple omission would potentially have been understand able (except in this case the omitted publication appeared directly after a referenced one), but Storms compounds his error by asserting in the book that the objections raised[2] were adequately addressed.[12] In fact (a) the points responded to by Storms[5] involved *only* the admittedly speculative chemical mechanism proposed to explain the FPHE,[2] not the CCS, and (b) the rebuttal[7] addressed each of Storms' objections in a point by point fashion. Thus the CCS remains the most viable explanation for apparent excess heat events (including HADs) and its potential contribution must be eliminated from any set of results before a nuclear explanation can be put forward as compelling.

It is worth noting that addressing potential CCSs does not supersede or replace the need to watch for other errors as well. For example, the first set of Storms' data presented on the internet in January of 2000 has a severe negative feedback of the input power in the excess power signal which was subsequently traced by Storms to poor grounding. This necessitated rejecting that data. The data reanalyzed was posted in February, 2000, after changes had been made. And, in Figure 3 of the reanalysis,[2] residual error of two types was indicated, severe transient spikes and baseline shifts.

Transmutation or contamination?

As noted by K&M, once calorimetry discussions are over, what is left is an accumulation of results from a variety of methods that purport to show nuclear products, *i.e.* 'nuclear ash', have been detected, thereby proving the nuclear hypothesis. The earliest of these was detection of classical nuclear particles/radiation (alpha, beta, gamma, neutron). This author is not skilled enough in those areas to do more than generally comment that much doubt remains as to the validity of those techniques when sophisticated counting electronics are employed. Some comments can be made about the early use of X ray film regarding a technique from photography known as 'hypering' combined with heat sensitivity of the films. These two things basically cast all X ray film based detection techniques into doubt. But with regards to techniques and methods that purport to detect new elements arising from nuclear reactions such as fusion or fission, much can be said.

Storms' book shows the second most populous type of nuclear ash results is He detection results. Both isotopes of He (^{3}He and ^{4}He) have been supposedly detected at levels that would seem to validate the nuclear hypothesis. However, the issue in detecting the moieties is always: were they generated by a new nuclear reaction or did they arise *via* a contamination pathway? K&M note the Paneth and Peters incident but did not clarify that it was with regards to He detection. Paneth and Peters originally claimed to have detected He formation but then retracted. The basic problem is that normal air has about 5 ppm He in it, and thus air inleakage or just He diffusion through glass can produce trace amounts of He in any experiment. Thus the perennial question noted above.

Storms and K&M try to use reports of He detection as proof of the nuclear hypothesis, but Storms (and K&M) again fails to report crucial negative results. In 2001 3 Oliver, Clarke and coworkers reported a series of papers in *Fusion Science and Technology*. One paper in particular[13] deals with a situation where cold fusion researchers submitted four experimental Arata and Zhang 'double structure' cathodes that were purported to be hydrogen loaded cells where cold fusion had occurred producing ^{4}He to W. Clarke *via* B. Oliver for trace level He analysis. Clarke was a well qualified expert in trace level He detection and discrimination from atmospheric leaks. What Clarke found was that the samples had massive air inleakage, and the He present came from that. Storms fails to mention this paper in his book at all.

Obviously, the problems that plagued Paneth and Peters in 1926 still plague us today. In order to prove reported He results are legitimately assigned to a nuclear origin, the other potential origin (contamination) must be excluded. This is done by reporting on the experimental protocols used to eliminate that option, which must include both proof that the instrumentation used to detect the He is in working order *and* that the process of running the experiment did not allow for air inleakage. While some information is often presented on the first point, routinely no information is provided on the second. This leaves cold fusion researchers technically unable to conclude the nuclear hypothesis is true, which of course does not seem to impair them at all from doing so, as evidenced by K&M in particular.

In the 1989 DOE review of the cold fusion field, contamination was mentioned as a problem to be addressed, with specific reference to He detection since at that time only He results were being claimed. Today as pointed out by K&M, additional elements are being detected and used to support the nuclear hypothesis. Unfortunately, the contamination question applies equally well to these claims, a fact that was recognized in 1989.[9] This author does not dispute the findings of new elements on electrode or membrane surfaces after use, simply the claim that they arise from transmutation. There are clear cases of data misinterpretation resulting in mis assignment of contaminant identities and a couple of cases where possible contamination pathways were actively identified by additional research that bear consideration.

One of the earliest examples of this was the unpublished work of S. Little of Earthtech International[14] on the RIFEX kit sold by Patterson and Miley in the mid 1990s (details of the business arrangements are not discussed here). The RIFEX kit was a modified F&P cell that used a packed bed of Pd/Cu coated plastic beads as the cathode in a flowing electrolyte system that ostensibly generated significant excess heat. Miley reported[15,16] heavy metal transmutation detected *via* a cornucopia of elements found on the bead surfaces after use (one of Miley's Figures is reproduced as Figure 51 in the Storms book). Little replicated Miley's finding using different analytical methods, but then went further. Little first computed what concentration of contaminants in the cell materials (primarily electrolyte) would represent the detection limit of his techniques. Then he concentrated on locating a potential contamination source of those elements that did not fall below that line. In his search, he found several of the 'new' materials had been leached out of parts used to manufacture the RIFEX cell, thus strongly suggesting that transmutation was not their source.

K&M cite the 2002 report of Iwamura *et al.*[17] on the transmutation of Sr into Mo and Ca into Pr in a process where deuterium was diffused through a complex Pd/CaO sandwich structured membrane. Furthermore, the Mo so detected supposedly had an anomalous isotope distribution. Subsequently, it was reported that the 'Mo' contaminant was actually S,[18] and that a Pr contamination was found in Iwamura's laboratory.[19] It is worth noting that MoS_2 is a common thread lubricant used in ultrahigh vacuum systems (UHV) to prevent thread galling during system bakeouts needed to reach UHV and that the two major techniques used by Iwamura *et al.*, were X ray photoelectron spectroscopy (XPS) and secondary ion mass spectrometry (SIMS), both techniques based in UHV. The apparent detection of Mo by SIMS is probably correct. The primary isotopic anomaly reported for the Mo was a large overabundance of the mass 96 isotope, which could be derived either from an S_3 ion (S is 95% mass 32) or an $FeCa^{20}$ ion.

K&M also present Iwamura's 'temporally correlated growth' results (Fig. 16), however, they (and Iwamura) fail to understand what results would have been anticipated if contaminants were deposited progressively on the membrane surface. XPS samples a semi fixed depth of the surface layers due to the escape length of the ejected photoelectron. Thus, as an element is deposited over the existing surface, it covers the existing surface and increases the depth an ejected photoelectron has to travel through to reach free space. This increases the probability of the electron's readsorption in the solid, which decreases the count of those electrons originating from the original substrate material. Meanwhile the freshly deposited material's signal increases in proportion to how fast it is deposited. Eventually the deposited material would begin to cover itself, decreasing the rate of rise of the XPS signal. Beyond that, the signal from the original substrate would have completely disappeared, assuming uniform coverage. This is exactly what is seen in K&M's Figure 16, and thus does *not* exclusively imply transmutation is occurring.

It should be mentioned that the contaminant elements on electrode surfaces were noted early in the history of the cold fusion story. In 1989, Augustynski *et al.*[9] reported on Pt, Pb, and Zn appearing on Pd electrode surfaces, but nothing was claimed based on this, the working assumption being that these materials were contaminants. It is only after the mutating diversification of the field began that these observations began being used as proof of a nuclear process. In fact, the electrolytes used in most P&F type cells dissolves some metals and these can then be transported to the other electrode and deposited.[20–25] Thus we see in K&M's Figure 9 an EDX spectrum of a used cold fusion electrode showing Pt, Fe and Cr, and some other elements. Pt is attributable to dissolved anode material depositing on the Pd. Fe and Cr are primary constituents of steels. Again, the source of these new elements is unclear, but dissolved or leached contaminants seem more likely than transmutation as the source.

So, all of the above elements have probable or confirmed non nuclear sources in the lab reporting their formation *via* transmutation. Complicating the situation was a failure to consider multi atom ions in the SIMS results (the latter is a common problem in cold fusion research, particularly with respect to metal hydrogen multi atom species.) These problems, as well as failure to understand the impact contamination deposition has on a membrane surface has apparently led cold fusion researchers to invalid conclusions. Bringing the He cases back into the discussion, it is clear that much more work is required from cold fusion researchers than they have reported in order to be able to conclude proof of the nuclear hypothesis has been obtained this way.

Contamination will continue to be the preferred explanation for any supposed nuclear ash detected in cold fusion experiments, as long as methods and approaches used to eliminate it as a source term are not described. This issue will continue to raise its head in other areas as well (see below).

Pits in CR-39 plastic

K&M present two more major experimental areas, one being so called 'bubble fusion', which they admit is peripheral. Nevertheless, they spend considerable time on it. This is a typical tactic used by pro cold fusion advocates, namely the uncritical listing of anomalies that, even though clearly unrelated, are presented as supporting evidence. Presumably they wish to convince readers of their primary thesis by a 'mass of evidence' approach, but this is an invalid tactic. We here will neglect the bubble fusion section and move on to the other major block of evidence discussed as proof of neutrons and/or charged particles ('nuclear ash'), pits in CR 39 plastic.

CR 39 has elsewhere been used as a radiation detector in situations where radiation is expected. The basic process by which this works is that a nuclear particle such as a neutron or alpha particle strikes the CR 39 material, which is a polymeric hydrocarbon. The hydrocarbon is damaged by the collision with the particle. The CR 39 is then developed by etching (heating the CR 39 in an etchant solution for varying times). The damaged material with its broken chemical bonds serves as a nucleation site for more rapid etching. Etching removes some of the undamaged material, but the nucleated damage points suffer faster etching. This causes a pit to develop, which becomes large enough with a long enough etch to be seen and counted microscopically. Use of the CR 39 material in controlled situations where only nuclear radiation is expected to expose the CR 39 has proven to be reasonably reliable.

There are some known interfering effects. Background cosmic radiation can induce pitting, but this is normally handled by exposing controls at the same time as the live plates. Bartlett[26] discussed several environmental factors that can produce or alter background pit size, shape, and number distributions. Factors affecting this include, presence of oxygen, heat, humidity, and possible plasticizer affects (originating from storage cases, *etc*.). Duranni[27] implies that these (and other) factors are still impor tant to this day in his section discussing the lack of a theoretical framework for understanding what is to be expected in CR 39 plates after a given exposure, and he discusses the variety of factors routinely studied phenomenologically to measure such effects. Calmosca and Penzo recently published just such a study[28] in the same Proceedings as Duranni. It is reasonable that if such problems exist in the comparably well defined cases studied by those authors, the use of CR 39 in an experimental situation with large unknowns present is somewhat uncertain.

One other important pit inducing process is mechanical damage. It is well known by the cold fusion researchers that scratches can produce strings of pits in developed CR 39 plates. Oriani[29] notes that "We rejected etch pits that occurred in clus ters and those that formed linear arrays fearing that they may have arisen from surface scratches." Little (unpublished[30]) says "We quickly discovered that mechanical damage often leads to round, track like marks after etching." and "We were able to create various marks with sandpaper, needle points and simply by carrying around a chip in a pocket for a day."

Also noted by cold fusion researchers is a vague 'chemical attack'. In 2007, Little[31] posted a document entitled "Initial Report of EarthTech's PACA Experimentation", where PACA stands for protected against chemical attack. (Kowalski[32] attri butes this to Oriani) This author first suggested O_2 attack as a possible pit cause in 2001 on the internet usergroup sci.phy sics.fusion. The SPAWAR group supposedly eliminated this as a cause, along with attack by H_2 or Cl_2[33,34] (however, see below for more discussion). All of these factors can affect CR 39 results in the best situations, but in cold fusion research new conditions are added that aggravate the problem.

There are two basic ways CR 39 plates have been employed in cold fusion research. First, the plates have been placed *in* the electrolyte (or in very close proximity, sometimes separated only by thin layers of materials) of modified P&F type cells, and second, they have been placed *out* of the electrolyte. Pits have been detected in both conditions, but many more pits per square centimetre are detected in plates that have been *in* the electrolyte. The pits in plates located outside of the electrolyte have led researchers to believe they are primarily caused by neutrons, since charged particles, like alpha particles, would not have the range to have reached the plates. Likewise then, the additional pits in the plates placed *in* the electrolyte or near the electrodes are thought to be formed primarily by charged particles like alpha particles or tritons. Plates placed *out* of the electrolyte include those that are separated from the cell and those that have been suspended in the gas space of a working cell (Oriani and Fisher[35]). Extensive studies were conducted recently by cold fusion researchers in what is referred to as 'The Galileo Project', of which only partial publication of results has occurred. Most of the recent publications on CR 39 tracks by the SPAWAR group seem to have arisen from that study or have been strongly impacted by it. The recent literature debate between the SPA WAR group and Kowalski also come out of this.[32,34]

However, what tends not to get published is the negative results that do not support the nuclear conclusion. In 2007, S. Little reported[31,36] that in attempting to replicate results reported by R. Oriani,[29] a radioactive contaminant was discovered in and on the O rings used to seal the Oriani apparatus. So once again, the issue of contamination has been raised. Prior to 2007, adequate testing for such contamination was not conducted, thus all results reported prior to that point must be deemed incon clusive. Any observed pits could have come from an undetected radioactive contaminant. That contaminant could have been introduced as a fixed component of a cell part, such as with the O rings mentioned above, or it could have been leached out of such a component and then transferred to another locale within the cell. Post 2007, some reports have continued to filter in on this, but none of them so far has given anything but passing mention of checking for this problem, primarily as assertions that such checks have been made. Viewed in the light of the Clarke paper,[13] such assertions are unacceptable at this point in time. Actual protocols used must be discussed openly and freely to convince scientists that the contamination issue has been properly handled, which requires publication to reach all potentially interested parties.

With regards to pits on plates located *in* the electrolyte, a couple of novel and interesting conventional explanations come to mind that have not been seriously examined. First is the idea of oxidative attack on the plates by oxygen bubbles, *i.e.* 'chemical attack'. The argument that F&P cells are well mixed by bubbling action has been presented by Fleischmann and others and was used in the development of the speculative chemical model of what occurs in an F&P cell.[2,6,7] If so, then O_2 bubbles generated at the anode reach everywhere in the cell, including the CR 39 plates (front and back sides). Pure O_2 is highly reactive, and it can be postulated that contact between bubbles and the CR 39 leads to oxidation at the contact point. That oxidized contact point could serve as a nucleation point of a pit. This thesis may have been disproved by cold fusion experiments where thin Mylar sheets were placed over the front of the CR 39 in the electrolyte (ostensibly to modify the energy profile of the alphas striking the CR 39). Pits are still detected but they are modified in nature. Presumably the Mylar would stop the O_2 bubble, but that is also a speculation that would need to be tested. Fortunately, all of this could be potentially tested by bubbling fine O_2 bubbles from a glass frit over CR 39 under otherwise identical experi mental conditions but without ongoing electrolysis and devel oping it normally. This remains to be done at this date.

The SPAWAR group does claim that O_2 does not impact the CR 39 plates based on an earlier set of results,[33,34] but it is unclear if that study fully tested the O_2 theory. In that study, electrolysis with Cu and Ni mesh cathodes was attempted without plating out the normal surface coating of Pd, and no pits or hollows were observed. The actual runs purported to produce cold fusion derived pits ran for a total of ~11.3 days, with the last three days of this being at the current used in the control runs or higher. However, the publication fails to mention the duration of the control experiment which is likely an important factor. Until such important details are published, any conclusion remains elusive.

A second argument is a little more intriguing and difficult to prove. Both this author and the SPAWAR group conclude that hot spots photographed by the SPAWAR group[37,38,39] on an operating cold fusion co deposited Pd electrode represent 'explosions'. The hallmark of an explosion is a shockwave. In fact, Dr F. Gordon reported[40] at a 2009 conference that such pressure spikes had been recorded, so clearly, said shockwaves exist. This author claims such explosions are chemical in nature while the SPAWAR group claims they are nuclear. However, in either case, the resultant shockwaves are potentially capable of doing physical damage which would produce nucleation points for etch pits.

The Mylar covering mentioned before would not stop all the shockwaves, but would modify the number with enough remaining energy to damage the plastic, and perhaps even the shape of the shockwave. (It is to be expected that there would be some anisotropy to these shockwaves since they are produced by an explosion in a bubble that would ignite at the metal surface and travel to the far end, giving the resultant shockwave a directionality.) Also, the shockwave would further disperse as it travels through the CR 39 to the back side of the plate, where upon exiting, if it still had adequate energy, it would also make pits (*via* etching of damaged plastic, not by explosive expulsion of material), which has been observed.[41] Thus, the shockwave postulate seems to fit the observations as well as the emitted particles hypothesis.

An interesting side point can be made based on an observation presented by Williams.[41] In his presentation, one slide stated that "Before etch, electrolyzed CR 39 showed SMALL [author's emphasis] amounts of apparent Pd deposition on the CR 39 plastic track detector." (These were apparently unprotected CR 39 plates.) So the concerns about dissolution and transfer of electrode material noted in the prior section have also been confirmed from the CR 39 studies. What form (ionic, metallic) this Pd is in on the CR 39 is unknown, but if it were in the metallic form it could *also* serve as bubble explosion nucleation points. Such explosions would be physically proximate to the CR 39 and thus would have a large impact with respect to damaging the plastic. Note that the Pd could also deposit on Mylar and if metallicized could foster the same proximal explosions.

More evidence for this thesis are the copious pits detected on CR 39 plates that were immersed in the electrolyte, whose geometric distribution mimics the physical shape of the co deposited electrode. In the conventional FPHE explanation, bubbles of mixed hydrogen and oxygen have to form on the electrode surface so that the electrode surface can initiate the explosive reaction. Thus only bubbles on the surface would ignite and emit shockwaves and their impingement on the CR 39 would mirror their physical distribution as controlled by the electrode configuration. In the end, this mechanism for pit nucleation will be quite difficult to prove out independently. However whenever a rational alternative explanation can be provided for a phenomenon, the issue necessarily remains undecided until one or the other of the alternatives is excluded, regardless of the difficulty associated with obtaining such evidence.

A further issue involving CR 39 pits is the claim to have observed 'triplets' which K&M bring up. Figure 12 in K&M presents one such triplet (three nearly coincident pits). Unfortunately, these images do not look very much like the triplets that they are supposed to be. Pictures of triplets in ref. 42 45 cited by the SPAWAR group[46] clearly show shape and spatial features absent from the picture in Figure 12. Instead the three pits in Figure 12 seem to be accidentally coincident pits. It is unclear how one could distinguish between the real and acci dental 'triplet' cases. In fact Oriani and Fisher[29] report "Recog nizing that there is an element of judgement in applying criteria for the acceptance of an etch pit as representing a true track, all measurements were made by one of us (RAO) to achieve consistency" for regular pits. If the pits in Figure 12 are in fact from a triplet event, the etching procedure used by the SPAWAR group needs to be modified to show structures developed that mimic those in the references arising in controlled experiments with known radiation sources.

Little[30] has also attempted to replicate the SPAWAR type experiments, which use CR 39 plates *in* the electrolyte (or very near the cathode in one publication[46 48]). While observing pits, Little found a variety of claims made by the SPAWAR group were inaccurate, one even being admitted to during the experi mentation with a resultant change in protocol recommended by the SPAWAR group.[30] It is of note that this particular SPAWAR claim was reproduced in their 2009 publication (submitted Jan. 2009) with no indication of the possible inaccuracy. In the end, Little concluded his experiments "do show that chemical origin is a distinct possibility and therefore that nuclear origin is not a certainty." Unfortunately, this point was apparently lost on the SPAWAR group, as their most recent publication[46] on this topic goes into extensive detail on the analysis of the observed pits *assuming* their nuclear origin without proof, and mentions no serious attempt to test conventional origin theories.

The claims to have detected neutrons and perhaps other charged particles emitted from cold fusion experiments thus remain unsubstantiated. The issues and speculations raised above simply illustrate that pits observed in CR 39 are not conclusive of nuclear particle emission at this point, and may never be. A conclusive experimental protocol aimed at disprov ing one or the other putative sources is as yet unavailable. Until such a separation can be accomplished, the CR 39 pits are not compelling proof of nuclear events.

Temporal correlations and otherwise

We have already discussed the origin of the temporal correlation in Iwamura's data (Figure 16 in K&M). K&M also bring up the idea of using correlated events as proof as well, citing an example of such in Figure 13. Unfortunately this temporal correlation needs to be reproducible as well, and no such bulk of data has been published to date. In fact the reference cited by K&M in the Figure 13 caption is to a presentation and not a publication. Thus, this information is nearly impossible for non attendees to be aware of, let alone to be able to examine in detail.

Figure 13 purports to show a 'triggering' effect, but it is not clear that said event was deliberate or not. The figure seems to imply something happened to induce a neutron emission that also caused a drop in cell potential. These events are correlated in the colloquial use of the term. They are also coincident. The question is whether a cause and effect relationship exists between the plotted variables or not.

Figure 13 also shows that a subsequent sudden increase in cell potential did not affect the neutron counts, that a slight increase in neutron counts was not tied to any change in cell potential as per the prior event, and an abrupt drop in neutron counts did not affect the cell potential. As well at the very beginning of the figure, an abrupt increase in cell potential, a subsequent drop, and a subsequent change in drop rate all did not produce any response in neutron counts. Thus, one can define at least seven events in this trace and only one shows a coincident event. This is not a correlation, this is a coincidence. The only way this event could be used to promote the nuclear proposal is if it were a part of a much larger set of such events. As it stands, Figure 13 illustrates *another* unexplained anomaly in a long line of such.

Another correlation often pointed to is the ^{4}He excess heat correlation presented by Hagelstein, et al[49] at the 2004 DOE Review. The plot, Figure 6 of the report, shows what appears to be a well correlated set of data indicating that as the ^{4}He signal gets larger, so does the excess heat signal. (This plot is constructed from data originated at Dr M. McKubre's SRI laboratory, and is reproduced as Figure 49 in the Storms book.) Recalling the discussions of prior sections, it is imperative to reconsider this plot. If in fact there is no excess heat, then what exactly is being plotted on the Y axis? If there is no proof that the observed He is not from a leak, then how does one know that is not what is being plotted on the X axis? Both 'errors' would accumulate with time, which is probably the interrelating variable in the plot.

There is an additional problem regarding the span of data chosen to construct the plot as well. The data in Figure 6 is also presented as time plots spanning 45 and 20 days in Figures 12 and 13 of that report,[49] respectively. It seen in Figure 12 that at day 20 the 'SC4.2' run's He values begin to decrease, and that at approximately day 30 the 'SC2' run's He values also begin to decrease. The question raised to this is 'Why?'. The 'Case cell' is a closed cell and no dilution should be expected. Has the cold fusion reaction now reversed? Or is there perhaps some unknown and therefore uncontrolled systemic error in the mass spectrometer results?

One factor that must always be kept in mind in these kinds of experiments is that the laboratory air may not be at the nominal recognized standard He concentration due to the periodic and uncontrolled use of He in the entire laboratory complex. Vagaries of the heating and ventilation systems can cause He released in leak testing or liquid He studies to be transported into other labs, and the day to day or hour to hour values may fluctuate because of this. This requires that researchers sample their laboratory air and analyze it as well during these experiments, but what is shown in Figure 12 is the nominal 5.22 ppm line derived from 'outside' air values. Without lab air data, one can draw no solace from the fact that numerically the experimental He concentration is above that of normal air. This is a requirement that must be met *in addition to* proving that the hydrogen handling system does not leak and that the mass spectrometer is functioning properly.

Storms[11] also presents another heat He plot as Figure 47 in his book. However, this plot shows no correlation such as presented by K&M or Hagelstein.[49] In fact, digitizing the data of Figure 47 and neglecting the one obvious flyer at the lowest excess power value produced a correlation coefficient of 0.0995. This is a highly statistically significant number indicating strong confidence that in fact *no* correlation exists. Including the single flyer produces $R = 0.38$, which is indeterminate as to whether a correlation exists or not. This plot was constructed from data from two different laboratories, one from 1998 and the other from 2003. Apparently, it depends on where and when one gets the data as to whether or not a correlation is observed. This is a typical problem observed when one attempts to plot two truly uncorrelated variables in a correlation plot.

Conclusions

This letter has attempted to provide some balance to the wholly positive K&M report by presenting several criticisms of cold fusion researcher claims. The overarching one is that cold fusion researchers refuse to consider conventional explanations of their experimental results. This clearly limits any validity of their conclusions. However, their standard approach is to ignore such issues and jump to the conclusion that some new and completely unexpected nuclear event has taken place. This is exactly what K&M do, and in this they are simply typical of the field, but the educated reader will understand that fallacy now that the problem has been pointed out.

Beyond that however, cold fusion researchers seem to have been trapped into believing that their signals are well out of the noise level, one of Langmuir's indications of pathological science. This paper has discussed several examples; calorimetry 'signals' that would seem to be noise, misunderstanding the mechanism of contaminant transfer (He and other elements) and its impact on experimental results, and claiming that 10 'triplets' out of many thousands of pits indicates fast neutrons for example. Then there is just the wishful thinking evidenced by claims based in misinterpretation of instrumental results, such as the various cases of selective interpretation of SIMS results. Because of their beliefs, conventional explanations remain unexplored, and these researchers' strongly held conclusions unfortunately fall under the definition of pathological science.

It bears repeating that the primary problem of cold fusion research is irreproducibility. But a further subtlety of that issue is the lack of critical review to which such reports are subjected. Typically, cold fusion researchers simply accept with little serious analysis any results claiming to support the nuclear hypothesis. This is illustrated by the uncritical listing of results found in the Storms book, but again Storms is merely typical of the field. Each individual report must first be analyzed independently of all others in a critical and careful search for errors, and if some are found, that report must be shelved until those errors are addressed instead of being added to a long list of other positive reports as some sort of proof by sheer numbers. Critical analysis of each report usually leads to finding one of the common errors discussed above, and thus will greatly reduce the number of papers that might be considered truly anomalous. In many cases, the number is reduced to so few that one can do little but agree that a dedicated researcher might be encouraged. There needs to be a body of replicated results present before one can proceed to any level of understanding of anomalous effects, and replication must be in detail, not simply in finding another but different anomaly in systems with some similarities.

It is certainly true that the cold fusion researchers have accu mulated a large body of anomalous results. Further, it is reasonable to assume these results are real (even if they would indicate equipment malfunction) and therefore that their causes are potentially knowable. But it is in the economic consideration of that knowledge that leads one group of researchers to fanat ically pursue the purported nuclear event, and other more prevalent numbers of researchers to abandon the field as a waste of time. Tracking down anomalies is a laborious process, and the failure of the cold fusion researchers to provide enough evidence to eliminate conventional explanations is probably a direct result of this workload.

Until such time as the conventional explanations are adequately addressed, and such studies published in standard peer reviewed journals, the cold fusion field observer is fully justified in rejecting unsubstantiated claims of novel nuclear reactions occurring in the many situations cold fusion researchers present as definitive. It should be said however that there remains one way to conclusively prove cold fusion is true, and that is to bypass all the scientific debate about questionable data, experiments, and interpretations, and just jump to a working device powered by cold fusion. As Douglas Morrison was wont to say (in paraphrase), "Where's my cold fusion brewed cup of tea?"

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