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Cold Fusion (LENR) One Perspective on the State of the Science

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Abstract. With recent publicity outside the CMNS field it has become increasingly important to clarify in non-specialist terms what is known and what is understood in the general field of so called Low Energy or Lattice Enhanced Nuclear Reactions (LENR). It is also crucial and timely to expose and elaborate what objections or reservations exist with regard to these new understandings. In essence we are concerned with the answers to the following three questions: What do we think we know? Why do we think we know it? Why do doubts still exist in the broader scientific community?

In this Foreword to the Proceedings of ICCF15 I lean heavily on the experimental work performed at SRI, and by and with its close collaborators (ENEA Frascati, Energetics and MIT) with a view to define experiment-based non-traditional understandings of new physical effects in metal deuterides.

1. Introduction

I was tasked to review the state of the science: at least 1000 man-years worth of work in 30 minutes, and here on a few pages. Of course it is impossible, so what was and is presented here is a very brief and personal view of the state of the science, through time and space constraints necessarily avoiding consideration of many large and important research subtopics.

It is important to understand what we have come together to study. On March 23rd 1989 Fleischmann, Pons and Hawkins [1] reported results of:

- i. an anomalous heat effect resulting from the
- ii. extensive, electrochemical insertion of deuterium into palladium cathodes
- iii. occurring over an extended period of time.

The underlined phrases are important and often forgotten. The effect reported was a heat effect. Calorimetry is the means of studying heat effects. Please note the underlined words: extensive, electrochemical insertion for a prolonged period of time, of deuterium into palladium. The

experiment is electrochemistry, with which very few in the physics community were familiar. And the process occurred with an initiation time many times longer than the time constant of diffusional insertion of deuterium in palladium.

This heat effect occurred at a level consistent with nuclear but not chemical energy or known lattice storage effects, but occurred (*mostly*) without penetrating radiation (α , β , γ , n°) or lattice activation. A remarkable feature of the effect is that a prodigious amount of energy is produced. This energy is not only much greater than can be attributed to chemical reaction processes, there is no physical evidence for such reactions. We have seen this heat effect occurring at hundreds or thousands of times the energy of any chemical reaction. These are the characteristics of the Fleischmann Pons Effect (FPE) and from our present vantage point we can begin to answer some questions:

1. What do we think we know?
2. Why do we think we know it?
3. Why do doubts still exist in the broader scientific community?
4. How do we propose to make progress?

2. What do we think we know?

The existence of an excess power effect is an experimental question, independent of theoretical issues or preconceptions. A great many experiments in which positive excess power results have been presented can be found in the International Cold Fusion Conference series over the past 18 years. Of these the experiments based on those of Fleischmann and Pons are perhaps the most studied and discussed, which makes the FPE of interest to us in our present discussion.

In the studies done at SRI over the years, an effort was made to understand specifically what conditions are required for excess power to be observed in the Fleischmann-Pons experiment [1] (keeping in mind that different requirements apply to other kinds of excess power experiments). A number of such requirements were noted: (i) a cathode had to achieve a maximum loading of about $D/Pd = 0.9$ or higher; (ii) high loading needed to be sustained for 2-4 weeks; (iii) a current density above threshold was required; and (iv) relatively high loading needed to be present for a heat burst to occur. In addition, it was found that changes in the operating parameters could initiate a heat burst, which may be related to a more general correlation between excess heat and a net deuterium flux either in or out of the metal.

The research activity into the FPE at SRI has now accumulating more than 60 man-years of research. We first focused attention on the critical importance of deuterium loading, the role of chemical poisons and additives in controlling the electrochemical interface, in order to achieve and maintain high D/Pd loading. We studied the correlation of excess power production with loading and reported simultaneously with IMRA-Japan [2,3] the threshold onset of the FPE. We designed and built a novel, high-accuracy, fully automated mass flow calorimeter, and set out to perform replication studies of the Fleischmann and Pons heat effect, first to confirm the existence the effect and second to better define the physical conditions under which it can be observed.

As an interim conclusion of these activities we were able to define the parameter space in which one might expect to encounter the Fleischmann-Pons excess heat effect, evaluated as an empirical expression:

$$P_{xs} = M (x-x^\circ)^2 (i-i^\circ) |i_D| \quad [1]$$

where $x = D/Pd$, x° is the threshold value typically ~ 0.875 , the current density threshold i° typically falls in the range $75 < i^\circ < 450 \text{ mA cm}^{-2}$, the deuterium interfacial flux $i_D = 2\text{-}20 \text{ mA cm}^{-2}$. It is important also to recognize a time threshold t° of at least 10 times the deuterium diffusional time constant.

3. Why do we think we know it?

Evaluation of the terms of equation [1] has been the subject of a number of reports and analyses, authored particularly by SRI, ENEA and Energetics but including data from a wide range of experimenters. This analysis will be discussed in more detail in reference [4] of this Proceedings volume. It can no longer be asserted rationally that there are no heat effect in any of the very large number of experiments reported here and elsewhere [5], or that the effect is the result of (unknown) energy storage or (unseen) chemistry. Also, at this point, any claim that the Fleischmann-Pons Effect is “irreproducible” is not only unsound, it is unscientific. Where and when we are capable of reproducing all parameters critical to the effect, we reproduce the effect.

4. Why do doubts still exist in the broader scientific community?

It might be appropriate to think of this question in terms that apply to parenting. First there was a difficult birth in conditions that while not initially hostile rapidly became so. Second there has been a great deal of poor communication on both sides: an inability to broadcast real scientific progress uncoupled from emotion or ambition; an almost complete lack of willingness on the part of those outside the CMNS community to delve into the work and understand what has been done, and what has changed, in 21 years. Finally, although not critical and somewhat circumvented by imagination, there has been an insufficiency of funding for such a materially complex (and I would argue potentially important) problem. The child, abused at birth and abandoned by most, that Minoru Toyoda helped rescue, now misunderstood and fiscally restrained, has just turned 21. I will not discuss the problem of fiscal constraint, in part because if we solve the issues of hostile rejection and poor communication that will not remain a problem.

Great significance was attached to early negative excess heat results reported by a small number of groups at prestigious institutions. In light of the discussion above, it is useful to see whether these experiments, as well as other early experiments, were operated in a relevant regime. Perhaps the most cited early negative result was that of Lewis *et al* [6] from CalTech in which they reported that “*D/Pd stoichiometries of 0.77, 0.79, and 0.80 obtained from these measurements were taken to be representative of the D/Pd stoichiometry for the charged*

cathodes used in this work.” Also widely cited is the early negative result of Albagli *et al* [7] from MIT who discuss “*average loading ratios were found to be 0.75 ± 0.05 and 0.78 ± 0.05 for the D and H loaded cathodes, respectively.*” The CalTech and MIT negatives are noted in Figure 1 in a histogram illustrating a number of early SRI experiments producing positive excess power results as a function of loading.

Even lower loading results were estimated by Fleming *et al* [8] from Bell Labs in a negative report. In this paper the authors state “*the degree of deuterium incorporation was comparable to that for the open cells for the same time duration. The amount incorporated in longer electrolysis experiments was typically PdD_x ($0.45 < x < 0.75$).*”

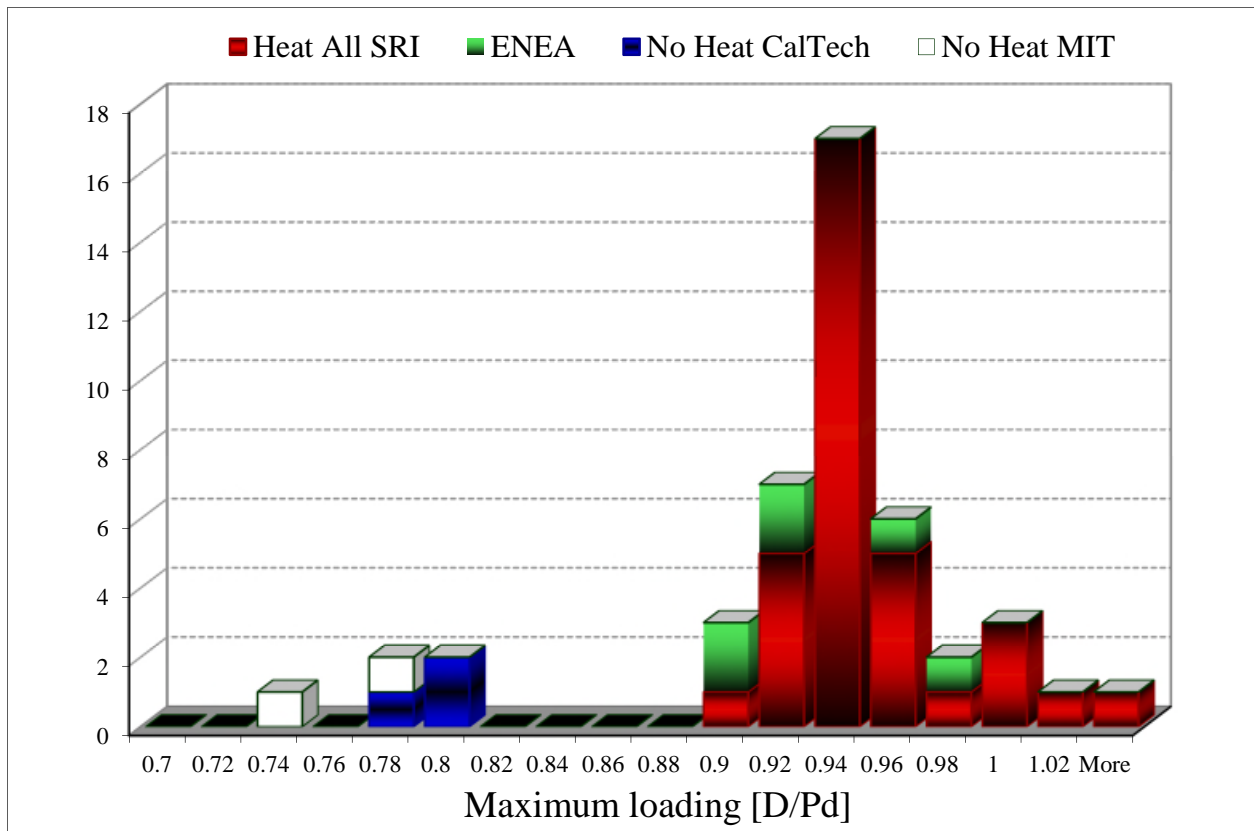


Figure 1: Histogram illustrating the number of early experiments at SRI and ENEA showing measurable excess power as a function of maximum cathode loading. Also illustrated are points for the MIT negative experimental result, with a stated loading of 0.75 ± 0.05 (Ref. [7]); and for the CalTech negative experimental result, with loading measurements quoted at 0.77, 0.78, and 0.80 (Ref. [6]).

From what we know today, and Figure 1 clearly illuminates, none of the cells in any of these cited studies would be expected to evidence any excess heat. Not only for the reasons of a loading deficiency (as stated explicitly): the durations of the experiments were wholly insufficient for a (typical) 300 hour initiation time; the current density stimuli were in the large part too small; the deuterium flux was not measured. None of the criteria of Equation [1] were

shown to be met, at least two demonstrably were not. In hindsight it is evident that the authors were victims of “unknown unknowns”, and perhaps “indecent haste” -- but this is understandable in the circumstances of 1989. What is important is that these experiments be recognized for what they are, not what they are not. They are important members of the experimental database that teaches us under what conditions one encounters the FPE. They are not any part of a proof of nonexistence; absence of evidence is not evidence of absence.

So what are the salient criticisms today? The following attempt is made to anticipate some of the questions and at least point in the directions of resolution. Basic questions:

- *“The experiments/results are not reproducible”*:
 - Some experimental teams see no results
 - Different results are seen in different laboratories
 - Inconsistent results in the same laboratory with similar samples
- *“The results are inaccurate”*:
 - Mis-measurement of input power
 - Mis-measurement of output power
 - Excess power is not outside the measurement uncertainty
- *“The heat is real but is due to unknown or unaccounted chemical effects or lattice energy storage”*:
 - Over-accounting for electrolysis products
 - Chemistry in the electrolyte volume outside the cathode
 - Energy storage and release (small percentage integral excess energy)
 - Hydrinos or other exotic, “high-energy” chemistry
- *“Missing nuclear products”*:
 - Quantitative energetic products not seen
 - Difficulty of measuring ^4He in the presence of D_2 and ambient

“The experiments/results are not reproducible”

First the existence of an apparent irreproducibility is widely recognized and acknowledged, and several papers have been written on this topic [9-11]. What is sometimes forgotten is that the most reproducible effect by its very nature is systematic error. Irreproducibility of results far from being a proof of non-existence argues more the contrary, and simply indicates that not all conditions critical to the effect are being adequately controlled.

Early flippant and intentionally unserious, as well as other claimed serious attempts were made to correlate the appearance of positive FPE results with the record (or existence) of university football teams and with national character. Serious criticisms do exist, however, and it is well recognized that different experiments, even intentionally identical and performed simultaneously in the same laboratory, give different FPE results. These experiments also give different results of much more mundane measurements. In the early days of studying the FPE at SRI experiments were designed to probe the parameters of reproducibility. Sets of 12 cells were

prepared, intentionally identically, and operated simultaneously to monitor the time evolution of electrochemical and physico-chemical parameters believed to be pertinent to the FPE.

A single length of palladium wire was used from a known source and sectioned into 13 identical lengths. These wire sections (typically 3 or 5 cm in length and 1 or 3 mm in diameter) were machined to remove surface damage and inclusions, spot welded with 5 contacts (one cathode current and 4 wires for axial resistance measurement), annealed, surface etched (to remove surface contaminants) and mounted in 12 identical cells. One electrode was reserved for comparative testing. These processes all were performed in the same batch and all by the same person. The twelve cells were filled with electrolyte from a single source and then operated electrically in series (*i.e.* with identical currents) in a 3×4 matrix in the same constant temperature chamber.

The variables measured were current (one measurement), cell voltage, pseudo-reference cathode potential, temperature and electrical resistance (D/Pd loading) all being monitored with the same instruments. Intermittent measurements were made of the cathode interfacial impedance. With 12 intentionally identical experiments, every one behaved differently. Not only in terms of their heat production, significant and marked differences were observed in: the current-voltage-time profile for both the cell voltage and reference potential; the ability and willingness of each electrode to absorb deuterium measured by the resistance ratio *vs.* time curve; the maximum loading achievable; the interfacial kinetic and mass transport processes reflected in the interfacial impedance. Every one of these parameters was different for each of the 12 electrodes, in every set tested! This matrix experiment was repeated several times in an attempt to understand the origins of the irreproducibility, and therefore control it. Trace impurity differences were observed to be contributory and there were two sets: deleterious impurities (poisons) that we learned to avoid; impurities that were beneficial to high loading in controlled amounts.

We were not able to control the variability of results simply by electrochemical (and trace chemical) means. The second major factor of experiment variability is the palladium metal cathode: source and condition. Figure 2 plots as a histogram the number of cells attaining the specified loading (whether in a calorimeter or not) varying by metal source or lot #. The first material used extensively at SRI, designated as Engelhard Lot #1 (E#1 on the plot) demonstrated in an astonishing 32% of all experiments a maximum loading $0.95 > D/Pd > 1.0$, with 36% $1.00 > D/Pd > 1.05$, and 14% (3 cathodes) > 1.05 . An electrode capable of attaining and maintaining high loading, is an electrode that is capable of producing excess heat thus a total of 82% of all samples of E#1 material, if properly stimulated, would have been expected to demonstrate the FPE. Unfortunately this apparent success illuminates the problem. Other

materials even from the same manufacturer were far inferior and none yet has been found to approach the loading ability of Engelhard Lot #1¹.

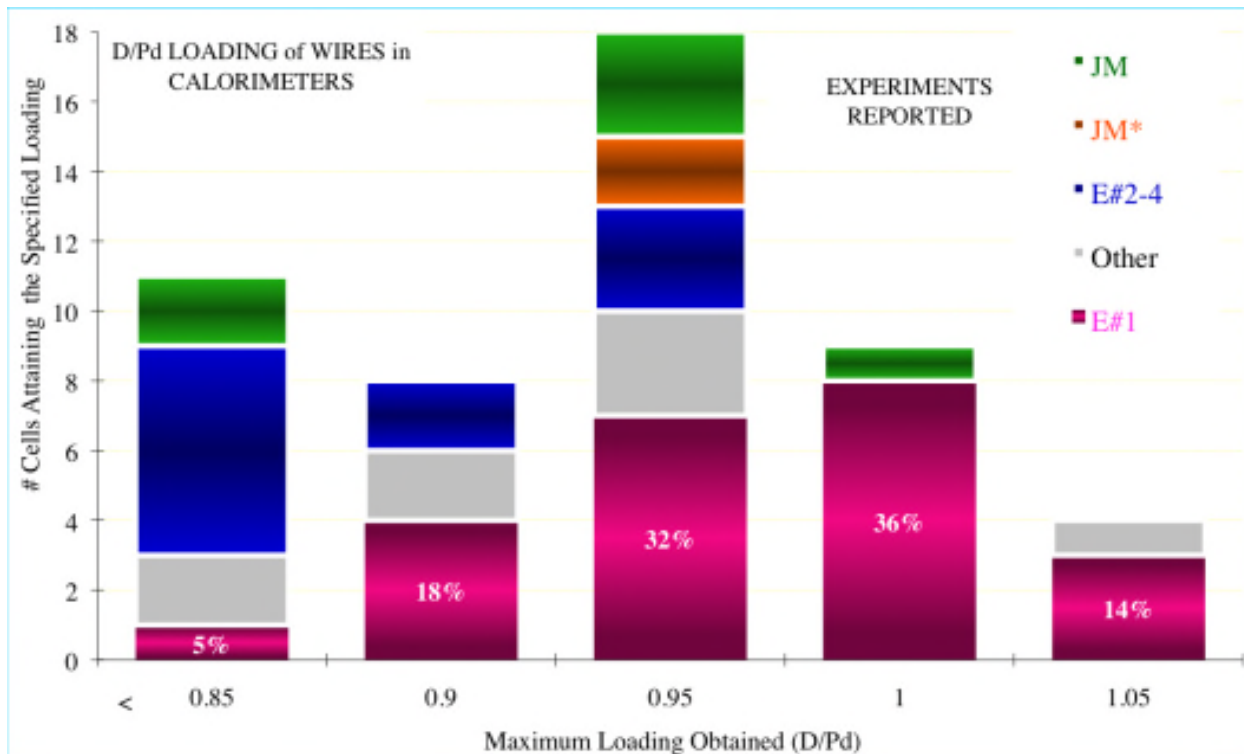


Figure 2: Histogram demonstrating the ability of a Pd cathode to load in 1M LiOD *versus* material source. JM = Johnson Matthey, JM* was a special lot designed to replicate pre-1989 materials, E = Engelhard.

Fortunately there is some consistency of behavior within a consistent set of materials. Electrodes made from the same material lots produce similar excess heat in different calorimeters, in different laboratories. Recently we have been working collaboratively with the Violante team of ENEA (Frascati) and the Energetics team of Dardik, Lesin *et al* to conduct comparative studies on material of similar general form: Pd foils 80 mm long, 7 mm wide and 50 μm thick, designed and produced by ENEA. Figure 3 presents a comparison of results obtained in two different calorimeters, one at SRI and one at ENEA, following Energetics current protocols².

¹ Important but equally confounding, E#1 had the highest levels of impurities of any material we have ever employed in these studies, far higher in fact than the manufacturer's specification of 99.7% purity.

² The unique feature of Energetics' experiments is the use of a fractal sinusoid current stimulus designated by them as a SuperWave™. Alone among all of the current modulations tested at SRI, this waveform is capable simultaneously of supporting high D/Pd loading and high interfacial deuterium flux. In the terms of equation [1], both are needed for excess heat production.

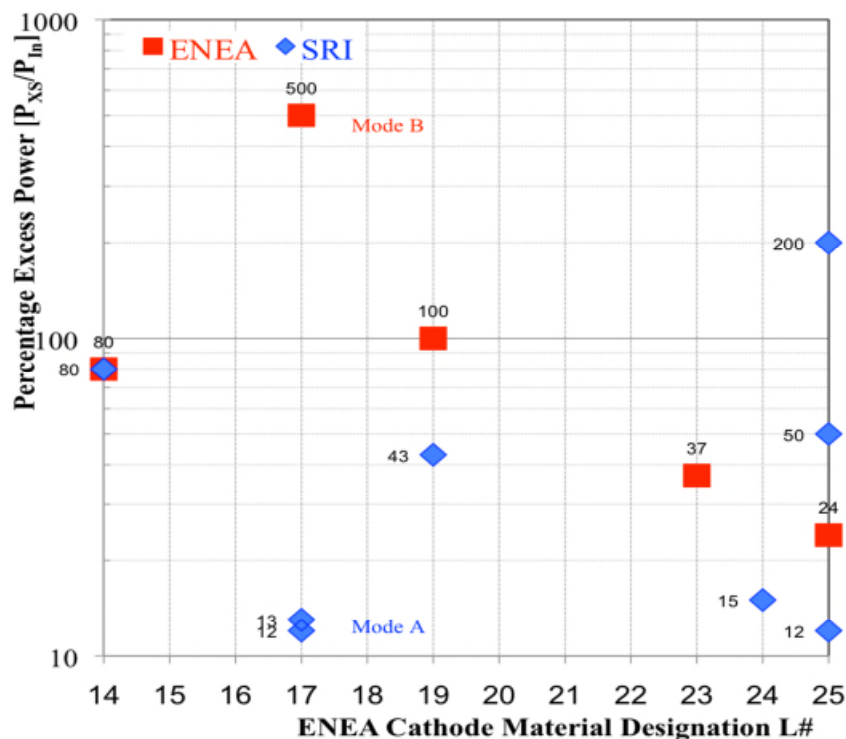


Figure 3: Comparison of results obtained from the same material lots performed independently at ENEA (Frascati) in a closed-cell mass flow calorimeter and SRI (Menlo Park) in an open-cell heat flow calorimeter [12,13].

The different lots of materials reflect different sources, rolling and annealing conditions and are designated by the ENEA “L” number, L14, 15, 16, 17 *etc.* on the horizontal axis. Two experiments with a particular lot of material, L14 run at SRI and ENEA, produced completely independently of each other (without knowledge of the other’s experiment) 80% excess power/input power. With another material, L17, SRI saw 13% and 12% excess power and ENEA observed 500%, but the excess heat production in the two laboratories appeared in different modes. The L19 material showed 43% at SRI and 100% at ENEA. The general point is, that the same lot type of material will give the same approximate level of excess power result in two different laboratories. There is a consistency of behavior, and that behavior varies very much with the lots; the lot numbers without points in Figure 3 produced no excess power at SRI or ENEA (although all lots were not tested at both locations).

“The results are inaccurate”

The issue of mis-measurement of input electrical power has been recently raised [14] both generally for dc current stimulus and specifically in reference to Energetics proprietary SuperWave™ modulated current waveforms. It is quite difficult to understand the basis for this

criticism and how and why it persists or surfaces after 21 years. Measurement of current, voltage, resistance, and time are some of the most familiar to engineers and scientists. Industries depend on the accurate measurements of power and energy for waveforms far more complex than any used (so far) in FPE experiments. In general the electrochemical cells are operated under controlled current conditions so that only the voltage varies. Accurate determination of power is thus a scalar, not vector operation and only simple precautions are needed for accurate measurement.

The Nyquist sampling theorem states that one can perfectly reconstruct an analog signal by sampling at twice the highest frequency component. Of primary concern in such measurements therefore is ensuring that higher frequency components are not present unmeasured in the input signal. In general this is very easily arranged by constraining the power bandwidth of the current (and voltage) source. For Energetics' SuperWaves™ that contain deliberate high(er) frequency components the solution to this measurement issue is more complex, but still experimentally quite trivial. Current-Voltage pairs are sampled and multiplied at a 50 k Hz. rate and only then averaged to obtain the input power. Several experimental checks on this procedure have been applied by Energetics, SRI and ENEA [15,16]:

- a. The current and voltage measurement rate (50 k Hz.) used is 500 times higher than that of the highest SuperWaves™ frequency applied.
- b. Ten times higher measurement frequencies (500 k Hz.), have been used with no significant difference observed in the input energy.
- c. A fast commercial power meter (Yokogawa WT210/230) has been used with sampling rate of 100 kilohertz. The results were in agreement within +/- 0.5%.
- d. At SRI and ENEA high frequency oscilloscopes and spectrum analyzers have been used at times of excess power production to demonstrate that the energy of frequencies higher than the Nyquist limit could have no calorimetric consequence.
- e. Following our standard replication protocol [6], experiments set up using Energetic' data acquisition systems at SRI, were completely replaced with an entirely new data acquisition method and system with entirely consistent results [12,13].

Another obvious factor is that calorimeters measure total, absolute energy probably better than any other instrument. Most of the time, most of the calorimeters operate on the thermal baseline with output = input. If the issue were really low sample frequency one would expect to see an error at all times as a systematic effect of the input.

Since many different kinds of calorimeter have been shown to demonstrate consistent effects it seems also very unlikely that significant systematic errors are present in the measured output power and energy. As with the evaluation of input power, the variables needed to resolve output power (mass, time, resistance, temperature difference) are some of the easiest measurements we typically make. It is very hard to sustain rationally any argument that so many people have been mis-measuring these variables consistently for 21 years, with new people entering the field learning or copying the same errors.

A final point arguing against the universal presence of systematic error measurements is the sheer magnitude of the effect. At SRI we have seen an excess power effect at 90σ , ninety times the measurement uncertainty, and have made over one hundred observations of $P_{XS} > 3\sigma$. The effect is not fleeting and persists for hours, days, weeks, in one case longer than 1 month, giving ample time to check the measurement systems. And the output power is not small compared to the power input with power ratios $P_{Out} / P_{In} > 2, 3, 5$, the highest sustained value measured being 25 [17] averaged over 17 hours!

“The effect is due to chemistry or energy storage”

Several factors are often suggested in an argument that FPE excess heat is real and measured correctly but that its cause must be other than nuclear because no such nuclear processes are known. Some of these are:

1. Over-accounting for electrolysis products
2. Chemical reactions involving species in the electrolyte volume
3. Energy storage (slow and unseen) and release (rapid)
4. Hydrinos or other “exotic” chemistry

Is the FPE due to chemistry or energy storage? Simply, it is not! Anybody who has the ability and willingness to undertake simple calculations on the energies of these two different kinds of effects – nuclear and chemical – will easily be able to ascertain that the FPE is not caused by chemistry or an energy storage effect. Furthermore, if it were, that effect would be interesting and potentially very useful. The inventories of chemical species are simply too few. A continuous error such as unwitnessed and unexpected recombination of D_2 and O_2 inside intentionally open calorimeter cells has an energy capacity of the same magnitude as some heat effects observed in them, but this argument fails on two grounds:

- i. the FPE is measured reliably and robustly in closed cells where this effect can play no role, and is similar in form and magnitude to the effect measured in open cells,
- ii. accurate account is easily (and routinely) taken for the amount of water added for electrolyte makeup due to Faradaic loss; prolonged periods of energy excess due to unmeasured recombination would result in FPE cells requiring less³ D_2O (or overfilling).

Detailed energy balance can be complicated in FPE experiments because these occur over long periods of time with no energy excess, and may have many and varied energy inputs. Although many have been accomplished with absolute statistical certainty, the early numbers were not very satisfying and (for example) do not suggest a basis for a useful energy source. This question of energy balance was put finally to rest resoundingly by the Energetics team in experiment L64 [17] about which there has been much comment [5,10,12-14]. This experiment lasted a relatively short time, there was very little time before the excess power burst was

³ The amount of water needed to refill an open electrochemical cell can be readily and accurately calculated using Faraday’s Law that relates the moles of species consumed by electrolysis to the total charge passed.

achieved, and the energy out was markedly greater than the energy in. There was no time for energy storage in this process.

Energetics experiment L64 using a 7 mm × 80 mm × 50 μm Pd foil from ENEA (Frascati) and SuperWaves™ current stimulation demonstrated a maximum output power >34 W twice in the first 20 hours of the experiment, with an input electrical stimulus less than 1W. The energetic response was even more startling with 40 kJ of input energy in that first 20-hour period, 1.14 MJ of energy out, 1.1 MJ of excess energy. A factor of 25 times more energy coming out as heat than was input electrically. For this first heat burst alone the energy was 4.8 KeV/Pd atom, thousands of times more than can be accounted for by known chemistry. A second burst produced boiling in the electrolyte and at least⁴ 3.5 MJ more energy, a total of more than 20 KeV/Pd atom. Similar but slightly less impressive results have been obtained on several other occasions by Energetics.

Missing nuclear products

Initially applied as a “*where is the beef?*” denunciation, the question “*where is the ash?*” was posed (or supposed) to refute the existence of the FPE on the grounds that the only products⁵ possible were energetic and therefore easily observed (and even hazardous). At SRI we have made efforts at varying levels to search for a very wide range of potential nuclear products and ash.

Some salient criticisms are listed below followed by comments:

1. The expected energetic radiation does not accompany heat production
2. The nuclear products claimed cannot account for the excess heat
3. The claimed quantitative product (⁴He) is:
 - a. Impossible to produce
 - b. Difficult to measure
 - c. Not found in sufficient quantity

The first question was first and most directly answered by Julian Schwinger in 1989 [18]: “*The circumstances of hot fusion are not those of cold fusion*”. By this he suggested that quantum coherent superstructure of the Pd(D) lattice might be expected to change the reaction mechanism, the rate, and the product branching ratios. At present there is no consensus among those in the field as to what physical mechanism is responsible for the effect although many propositions are under active discussion and significant progress is being made [for one proposed

⁴ This amount is under-estimated as the heat of vaporization of D₂O was not included in this energy total.

⁵ The term “ash” in “nuclear ash” is a technically inaccurate analogy to chemical ash. In chemical combustion, the ash is left-over material that does not participate in the reaction. It is the residuum of non-volatile oxidized and pre-oxidized materials. In the field of cold fusion, the term “nuclear ash” has come to mean the reaction product. This is equivalent to describing the chemical combustion products CO₂ and H₂O as “ash,” which is incorrect. Thus, in cold fusion helium is sometimes referred to as “nuclear ash” but it would be more accurate to call it a potential product of nuclear reaction.

pathway see 19,20]. Potential products are therefore equally obscure but no rational basis exists to deny the existence of the FPE on the grounds of non-observation of a hypothetical product.

Some nuclear products of FPE reactions clearly exist sub-quantitatively with the excess heat. Tritium and ^3He are produced in FPE experiments, under special circumstances, largely asynchronous with the excess energy [5]. Claims have been made for “massive transmutation” at (or above) the levels needed to account for measured excess energy [5]; these have yet to be verified.

For some time at SRI we have been performing experiments to test the hypothesis that the quantitative product of the heat producing reaction is ^4He that evolves primarily without associated energetic byproducts. This hypothesis did not originate at SRI. As early as 1991 Miles and Bush [21] developed an ingenious self-sparging helium sampling system using electrolytic evolution of D_2 and O_2 to purge out atmospheric ^4He . They obtained a seemingly unassailable statistical correlation between heat and helium production computing a 1:750,000 chance that the correlation was random.

Miles and Bush also obtained a very impressive early quantification of a reaction Q value. Compared with the value predicted for an overall reaction of the sort $\text{d} + \text{d} \Rightarrow ^4\text{He} + 23.8 \text{ MeV}$ (*lattice*), (yielding $2.5 \times 10^{11} \text{ } ^4\text{He} \text{ s}^{-1} \text{ W}^{-1}$ of excess power), Miles and Bush measured an average value of $1.4 \pm 0.7 \times 10^{11} \text{ } ^4\text{He} \text{ s}^{-1} \text{ W}^{-1}$, 54% of the hypothesized value. Later in a study to replicate this work at SRI Bush [22] measured an average $1.5 \pm 0.2 \times 10^{11} \text{ } ^4\text{He} \text{ s}^{-1} \text{ W}^{-1}$ (58% of the “expected” value).

Numerous others have made measurements of gas phase ^4He during or immediately following FPE heat excursions [5]. In general the amount of measured helium lies between ~50 and 75% of the amount⁶ predicted for a net reaction⁷ $\text{d} + \text{d} \Rightarrow ^4\text{He}$. Important experimental and theoretical issues attach to the question: “is there missing ^4He ?”, and, if so, “why”? If the net reaction were as written, and occurred in a skin layer close to but below the Pd cathode surface, then one might crudely expect ~50% of the ^4He to leave the cathode while the rest goes deeper to be trapped. Lending some weight to this hypothesis 15 studies have found unexpected ^4He in metal cathodes after FPE energy production [5], although in no case was the amount of ^4He measured sufficient to account for the gas phase deficiency.

In considering the possible fate of ^4He it is important to recall that the surface of a heat-producing FPE cathode is not well-crystallized Pd, even if it started as such. After extensive

⁶ One result in the early Miles Bush work measured ^4He at greater than $2.5 \times 10^{11} \text{ } ^4\text{He} \text{ s}^{-1} \text{ W}^{-1}$ but was attributed to experimental error [23]. In the published literature only the work of DeNinno and coworkers offers evidence [24] of super-quantitative ^4He .

⁷ For reasons involving local energy and angular momentum conservation it is clear that, even if this is the net process, this reaction does not occur in a single step as written without the intimate involvement of other bodies. Since thermodynamics is path independent, however, we can calculate accurately the energy of the overall exothermic process, without knowing the pathway.

electrolysis in LiOD (for example) the cathodic surface will have incorporated significant Li, and the electro-active metallic surfaces become covered with a many-micron layer of hydrated oxy-hydroxides incorporating adventitious (as well as deliberately added) elements from the electrolyte, and leached from cell walls and parts, and from the two electrodes. In particular this “sludge” layer will act to restrain or delay ^4He release and it is reasonable to anticipate that work must to be done to disrupt this layer to approach an accurate mass balance. Since 100% of the helium can never be recovered, this balance will underestimate the total, but as recovery techniques improve it will asymptotically approach the true mass balance.

Limited resources have restricted to only 2 the number of successful heat producing experiments in helium leak-tight calorimeters for which effort was extended to scavenge ^4He held up (by whatever means) in the cell volume. Of these one performed at SRI [25] and the other at ENEA (Frascati) [15], both yielded a total mass balance of ^4He produced within approximately $\pm 10\%$ the $2.5 \times 10^{11} \text{ } ^4\text{He s}^{-1} \text{ W}^{-1}$ value, supporting a claim for an overall reaction Q of $\sim 24 \text{ MeV}/^4\text{He}$ atom produced. This is an important result that needs further verification.

5. Summary and conclusions

On the basis of the evidence and arguments presented here, and far more extensively and compellingly elsewhere [5,10], it is apparent that the Fleischmann-Pons effect is a new effect in physics. It requires a new mechanistic description and explanation. This new effect is very likely to be associated with a significant number of other condensed matter nuclear processes that await exposition and development.

I predict that once explained, the underlying effect will not appear strange at all. It will seem, in retrospect, that it was quite clear that we should have understood it all along. It is a heat producing reaction, consistent with nuclear but not chemical effects, both temporally and quantitatively accompanied by ^4He . This new effect, the Fleischmann Pons Effect, can be accompanied by nuclear “ash”, ^3H and ^3He being important. Strong evidence for other isotopes exists [5]; more may follow.

How do we make progress? We make progress through theory: quantitative predictive fundamental physics descriptions. We will continue to make progress best by using the scientific method. To do so we are going to have to engage the broader scientific community. We simply can't sit here secure behind our walls and talk in closed groups, we need to invoke enthusiasm in the broader scientific community. The organizers of this conference [ICCF15] are to be commended for recognizing this need and furthering that process.

Another way of making progress is by engaging in the process of creating a product. Here we might take advantage of the growing public and political interest in real alternative energy solutions. The FPE produces real and useful energy, process heat. In Energetics experiment L64, in a single burst, twenty five times more heat was produced than entered the cell as electric

power. This heat was produced at temperatures sufficient to boil water. Such an effect has practical value. Obviously taking an experiment to the market as a product requires several steps that are non-trivial. This exercise however may be an effective means of gaining an engineering understanding of the effect even before the scientific.

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