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Research Article

LENR – What We must Do to Complete Martin Fleischmann's Undertaking

Michael C.H. McKubre*,†

Energy Research Center, SRI International, Menlo Park, CA, USA

Abstract

It is clear to most who have studied the matter carefully that condensed matter nuclear science (CMNS) expresses a real and new phenomenon in physics. The efforts to communicate this reality outside a rather small group have not been very convincing. For something of such potential importance this dichotomy seems strange. How might we improve this situation? A working theory could certainly help, and better correlation between experimental variables, both input and output. A working demonstration that stood alone even as a toy could help facilitate communication directly with influential technical but non-specialist individuals and groups. Some thoughts on the possibilities and constraints are offered below.

Keywords: Demonstration prototype, Fleischmann, Heat effect, Pons

1. Introduction

I would like to preface these remarks with a statement of my position and degree of certainty after having studied LENR for more than 28 years and the deuterium–palladium system for nearly 38. Without a working example certainty is approached only asymptotically. A working theory would certainly help us convince others and advance the field but even well developed models and theories must be anticipated to change and very few laws are so right and rigid as to be immune to change. That being said our failure to communicate effectively outside our community that nuclear processes can and do take advantage of the condensed state is mostly – or initially – due to incomplete or insufficiently well explained experiment, not weak theory. This last statement contains no apology or implied criticism. Much good work has been done in both domains: experiment and theory. Some of the experimental progress is summarized in this paper. More and better experiments should and will be done – and will inform theory.

So how is "asymptotic certainty" based on a preponderance of evidence achieved? Or, better still, working example? I will divide my answer into two parts. My personal confidence derives from tens of thousands of hours spent with my own hands in the company of extremely able individuals in my own laboratory, working with systems as simple

*E-mail: mmckubre@gmail.com.

†Retired

as they can be made, to understand the bases of the effects hypothesized and observed by Fleischmann and Pons, and their extrapolated consequences. The results of this personal effort is reinforced by the works of numerous others – many in the audience – whose skills I have come to trust and whose results form a consistent, if not complete, pattern of understanding.

On the basis of this collective effort I am convinced that condensed matter nuclear science (CMNS) expresses a real and new phenomenon in physics. What we need to do is to communicate this knowledge effectively others. My order of assurance in the various experimental claims of CMNS effects that are deemed anomalous is as follows:

(1) Tritium (and helium-3).
(2) Excess heat at levels consistent with nuclear but not chemical processes.
(3) The production of helium-4 in chemical energy environments ($\sim$1 eV), at levels consistent with the measured excess heat ($>10^3$ eV).
(4) An additional range of condensed matter nuclear effects that are inconsistent with pairwise, isolated nuclear reaction, but nevertheless require nuclear cause.

2. Discussion

Those expecting me to answer the charge implied in the title of this talk will be disappointed – perhaps doubly so – although I suggest you stay to the end. I do not have a clear answer for myself, let alone for the community – the technical equivalent of a "killer app" if you will – that, once accomplished (hopefully promptly), will convince working scientists with relevant skills to add their effort to ours. Over the years the research group at SRI has made several major contributions to the field of research now identified as CMNS, particularly studying the Fleischmann–Pons Heat Effect (FPHE), and I take some advantage from this. These contributions include the following observations and innovations:

(1) Postulate and demonstrate the importance of D/Pd loading in achieving FPHE [1].
(2) Demonstrate and calibrate the utility and applicability of electrical resistance ratio methods to measure D/Pd loading in LENR experiments [2].
(3) Confirm the importance of D/Pd loading and demonstrate a critical threshold onset of the FPHE [3] (jointly with Kunimatsu et al. [4] at IMRA-Japan).
(4) Confirm the existence of an initiation time delay in the FPHE (following Fleischmann–Pons and Bockris).
(5) Confirm the Miles–Bush correlation of excess heat and helium production in the Fleischmann–Pons electrochemical system, Arata–Zhang double-structured cathode and Case gas loading experiments [5].
(6) Demonstrate the critical importance of deuterium interfacial flux in the Fleischmann–Pons heat effect [6] (hypothesized by Hagelstein).
(7) With ENEA (Frascati) demonstrate the importance of metallurgical structure in achieving high D/Pd loading and surface morphology in producing the Fleischmann–Pons heat effect.
(8) With Energetics (New Jersey, USA and Omer, Israel) and ENEA (Frascati) [7] demonstrate the critical beneficial effects of superwave modulated electrochemical stimulus in achieving high D/Pd loading, high deuterium interfacial flux and large power and energy gains in the FPHE (hypothesized by Dardik).

Of crucial and pertinent relevance to my inability to answer the charge in the paper title, if I had "the secret", and presuming for a moment that any one person could conceive or carry such a secret to term, I would very likely be constrained from sharing this knowledge publicly, or perhaps at all with any except the people who paid for my labour. I worked for nearly 40 years in the world of contract research in which individuals, corporations and governmental and neo-governmental institutions provided funds and contractual binding to implement a pre-agreed-on research plan or direction laid out in a formal "Statement of Work". Also and importantly pre-agreed is the ownership of intellectual "property" or "rights" to disburse the knowledge so developed. I have performed and directed more than $50 million of contract research and hold hundreds of "secrets" explicitly or implicitly for former sponsors, or as "non-disclosure" agreements with corporations and individuals.

Possibly a worse problem than the formal and codified secrecy of information held to be "IP" is the discouragement of research pursuit, analysis and publication. My first and largest sponsor of "cold fusion" research at SRI was EPRI – the Electric Power Research Institute located in Palo Alto, California. I have a long-standing, fruitful and joyous research partnership with EPRI extending back to 1978 – initially of the topic of the palladium-hydrogen and palladium-deuterium systems for purposes other than "cold fusion" [2]. All these works were "semi-public" in the sense that reports were prepared and made publicly available (occasionally behind a pay wall) and open publication was permitted, if not encouraged. EPRI sponsored our experiments that resulted in the first five contributions to my enumerated chronological list above; all of these were published openly. So what is this "possibly worse problem"?

Initially our research focused heavily on one critical research objective – testing Fleischmann and Pons' dramatic claims to determine their merit and possible application and implications for the US utility power industry. So narrow was our focus that we were not able to analyze fully our results and chose to ignore innumerable potentially promising scientific and technologic avenues. On previous occasions I have estimated that only $\sim$10% of the work done by the 10–12 person Energy Research team under EPRI sponsorship at SRI was fully processed to the point that public statement or publication could be made of it. With reflection (and memory sag) I would now rate our early success of converting laboratory results into "known knowns" as $\sim$30% - the majority remaining as "relic knowledge" or intuition in the minds of the principal investigators.

The last potential knowledge problem that we face is "unsound certainty" associated with the unwillingness/inability to reject old knowledge and replace it with new. Josh Billings, an American humorists in the second half of the 19th century, said: "*It ain't what a man don't know that makes him a fool... It's all the things he does know... that just ain't so*". In some ways we are too well trained by and too respectful of our teachers and their teachings. If the observations of any one of a half dozen or more novel phenomena discovered pursuing Fleischmann and Pons elucidation are sound – if nuclear effects take place in condensed matter by means, at rates or with products different from nuclear reactions in free space – then something we have been taught and believe about nuclear interaction is incorrect – something we "know", "*just ain't so*", or something important is missing.

More than 28 years have elapsed since the public announcement by Martin Fleischmann and Stanley Pons on March 23, 1989 of anomalous thermal effects and possible nuclear fusion associated with the super-loading of deuterium into palladium by electrochemical means. Rightly this announcement prompted the redirection of a significant fraction of relevant scientific resource albeit in a relatively small set of nations worldwide: USA/Canada, Japan, Italy, Russia and other former states of the USSR, China, India, and latterly France and Scandinavia. Participation elsewhere was strangely muted. In the "working nations" very few efforts were coordinated with sufficient vision and critical mass of appropriate talent to make progress in what we recognize retrospectively as a very challenging scientific problem.

Initial experimental efforts were not resourced, staffed or focused sufficiently to make reportable progress against this "tough problem". For reasons some of which are now quite well understood, the majority of replication attempts in 1989 and 1990 failed and most experimenters left the field. Cold Fusion (CF) or Low Energy Nuclear Reaction (LENR) became a scientific orphan with no country, institution or technical discipline accepting parentage or responsibility for what might if proven correct, controllable and scalable be mankind's most promising alternative for a secure, sustainable and benign energy future. How can this paradox be rationalized and what must be done to resolve it?

One of two things must be done – probably and preferably both:

(1) Clear scientific evidence must be furnished that nuclear effects take place in condensed matter by mechanisms different from reactions in free space.

(2) Demonstration must be made of a practical use of the energy so created.

Why is this so hard? Why is the bar set so high? The answers lie in history, tone, tradition and cognitive dissonance, none of which issues are we interested in addressing here. Having observed and contemplated this matter for a very long time I assert that "1" without "2" will not work to communicate effectively to the world the reality/importance of "cold fusion". I offer in support the case of tritium that is most certainly produced anomalously in some CMNS experiments. The evidence of this is sufficiently strong, well-published and replicated already to call into question at least an important core of what we believe that we "know" about nuclear processes and nuclear fusion in condensed matter. It is ignored because of our unwillingness/inability as a species to abandon a comfortable paradigm for no perceivable immediate advantage.

It is likely that "2" without "1" will not work either to convince the world of "cold fusion" reality for a different set of reasons. Our efforts to explore and develop Fleischmann–Pons (and other, later) claims have been ridiculed and marginalized. So much so that none but the most courageous dare pursue Martin Fleischmann's undertaking to engineering scale-up and device-production absent certain scientific backup[a]. Our required "proof" need not rise to the level of a fully developed theory. But reactant(s), product(s) and physics-based mechanism will need to be clearly identified before serious investment will be made[b]. Considerable progress occurred with the identification by Melvin Miles in 1990 of ^{4}He as a product, presumably of deuterium reaction, in quantity effectively commensurate with excess heat observation. Although replicated in a number of laboratories around the world the claim of heat-helium nuclear correlation has not produced persuasive effect in the broader scientific community and is challenged even within the CMNS community. If this is to be our "scientific proof" then considerably more and better scientific studies will be needed in this direction, or another.

An important part of the very effective ostracism of CMNS results and research is prevention of publication in mainstream journals. Most recently this has occurred behind an editorial modesty screen of "*we do not think our readers would be interested in this topic*" – even with the failure of review to identify technical error in attempted publications. If we cannot publish then academic pathways are voided and young aspiring academics will have little incentive to join our undertaking. Partly as a consequence of this action – or inaction – we are suffering seriously from a critical "missing generation" in cold fusion research. Fortunately scientist born after $\sim$1980 are largely unaffected by the negativity generated in 1989/90 and remain open and often interested in CMNS. It is time for us to assist and encourage their scientific pursuits.

Cold Fusion began its modern public incarnation on March 23, 1989, six days before Martin Fleischmann's 62nd birthday. Stan Pons was 45 … I was 40. I would argue that Physical Scientists do their best work from their late 30's through their early 60's (Physicists earlier, Mathematician even more so). Stan and Martin were well in this groove – Martin on the "leading edge". I was a youngster in what I now recognize reflectively as my most effective and productive scientific years. After graduating my PhD trained by one of John Bockris' first PhD students (John Tomlinson), I had taken advantage of two years of postdoctoral study at the University of Southampton in England. By 1989 I had more than a decade of "on the job training" at "Stanford Research Institute" (SRI) one of the research powerhouses of Silicon Valley, and the first and (at that time) the largest "not for profit private" research institute in the world. I had a research group of 16 Electrochemists supported by SRI technicians and administrators – I was primed, educated and well-equipped to start a new undertaking.

Twenty-eight years on we are missing precisely that middle generation of trained but energetic scientists who are young enough to contend comfortably with novelty but mature enough to temper innovation with experience. If you

[a]Several offers and claims have been made to produce a "useful", "scaled-up" product but none as yet have succeeded.
[b]Ironically when these three factors are identified most will not be able to invest as the most powerful forces on the planet will have commanded production and controlled IP.

accept my premise then examine the age distribution at our conferences or in our publications; you will find that my core age group is un-represented or at best grossly under-represented. We are obviously heavily blessed with the group 65+ whose access to funding is not closely coupled to perceived reputation in the broader scientific community. We are having some success attracting the <35 groups who avoided the "cold fusion stigma" (except via Wikipedia and the publication embargo). The young and the old have courage to look in new places but scientists of the age and position I was in in 1989 presently are heavily discouraged from entering CMNS. In a world of contract research where reputation is everything anyone except the most courageous seeking to build a research team simply cannot afford to swim against the mainstream – just the denial of publications and patents ensures this. I claim we cannot succeed by diligence and hope alone without this core group of mid-career scientists, and many such groups working well together.

How do we engage the critical group of researchers, giving them confidence that there is a "there there", that it may be of supreme importance to science and mankind, that funding will flow, and that insults will not. I would like to spend the remainder of my time on one thing that I think we can do to effect a complete revolution in thinking in the broader scientific community and dispel instantly the curses visited on us and discussed above. Some have argued for the development and promulgation of a procedure for a "reference experiment" that reliably embodies the elements of "new cold fusion" that produces unambiguous and obvious evidence, and is easily performed by anyone (or any laboratory) with "normal skill in the art". I argued against the wisdom of this at ICCF-14 in 2008 on the grounds of public safety but also made the point [8]: *"If the claim is made that replication is crucial to the development of our field to determine the parameters for advancement, to prove reality to critics, or to uncover systematic error, then it is astonishing that attempts to replicate the FPE have been so few, and methodologically so limited … it must be completely understood that this lack of attention to detail … is precisely the reason that the question of replicability remains on the table".*

The attempts at real replication are few and unconvincing (here I exempt Lonchampt and Biberian [9], some of the efforts at SRI, and few others). Part of the reason for failure is insufficiency of funding and experience. Although large amounts of money were available in 1989/90, and a tremendous amount of talent entered the field (if only briefly), no good procedure had been published (that is still true) and each of us was left alone to decide what it might be that Fleischmann and Pons had actually done. By and large the folk who enthusiastically or otherwise answered Admiral Watkins call [10] to the first ERAB panel on April 24, 1989 to …

(1) Review the experiments and theory of the recent work on cold fusion.
(2) Identify research that should be undertaken to determine, if possible, what physical, chemical, or other processes may be involved.
(3) Finally, identify what R&D direction the DOE should pursue to fully understand these phenomena and develop the information that could lead to their practical application.

… were simply unable to complete their assignment. Even with good will, ability and intent (which was not universal) this charge was impossible. There was no theory and the application of hot fusion concepts were, as Julian Schwinger pointed out [11] irrelevant and misleading. There was very little work complete at the time of ERAB-1, of sufficient duration to overcome what was later discovered to be a long initiation time for the FPHE, performed by people with "normal" (or sufficient) "skill in the art" of electrochemistry. And the work that was done by at least some of the folk who had skill sets pertinent to the experiments proposed and reported by Fleischmann and Pons was being done in secrecy, or at least semi-secrecy, and was not available for the ERAB panel to review in any detail (my work at SRI included).

With my personal knowledge of Fleischmann and Pons, (then) >18 years of training in the Bockris/Fleischmann school of physical electrochemistry, and already >10 years experience with the electrochemistry of the Pd/D system, I and my group were better positioned than most but still we had to guess at a lot of the detail behind what Fleischmann and Pons had done. With detailed ignorance but considerable exposure to Martin's thinking and manner of research we simply set out to test the hypothesis that: "*there is an unexpected and unexplained source of heat in the D/Pd System that may be observed when Deuterium is loaded electrochemically into the Palladium Lattice, to a sufficient degree.*" This hypothesis was tested positively with funding support from EPRI, and several other conditions were discovered that control the FPHE. More conditions remained and remain to be discovered and we are not in a position to completely specify a written procedure that if followed by one of "normal skill in the art" will lead in every case (or in most cases) to a convincing demonstration of condensed matter nuclear effects – of any kind!

Ironically it was John Huizenga himself, cold fusion bête noire and co-chair of the ERAB-1 panel, who most succinctly identified the greatest barrier to early progress, although he used his wisdom to make the wrong point. Citing his principal reasons for rejecting cold fusion Professor Huizenga stated that: "*It is seldom, if ever, true that it is advantageous in science to move into a new discipline without a thorough foundation in the basics of that field.*" This is useful advice that could be well applied today. But what Huizenga did not note and probably never internalized is that the discipline of the Fleischmann Pons Heat Effect *is Physical_Electrochemistry* and the most pertinent diagnostic tool to study heat effects is calorimetry. Both were new and alien disciplines for most in the nuclear physics community and well out of their fields of competence or even comprehension.

Instead, by couching the argument in the terms of high energy physics, the nuclear physics community moved the debate from our field of mastery into theirs. In doing so they claimed or asserted the intellectual and moral "high ground" and could speak (legitimately) knowledgeably and (mostly) correctly about "absence of neutrons", "insufficiency of tritons", "no gammas", "coulomb barrier penetration" and other paraphernalia of hot fusion. Obviously we were politically outmaneuvered. None of these effects then or now appeared or appear necessarily to be relevant to condensed matter and coherent nuclear reaction. The prince of this field, Julian Schwinger, was at least willing to consider the findings of Fleischmann and Pons to be plausible. The discovery by Mel Miles of a statistically huge correlation between heat and helium production further underscored Schwinger's dictum that "*the circumstances of cold fusion are not those of hot fusion*". However irrelevant, simplistic, outmoded and out-of-discipline the hot-fusion-based objections appear on this side of Alice's looking glass we should pay great attention to the fact that these arguments have not gone away because they have not been effectively rebuffed! What do we need to do? Who is the audience that we need to convince? What is the most effective strategy? What disciplines and skill sets are most appropriate to define CMNS?

I do not think that "merely replicating" will be sufficient. To persuade the people that we need to engage will take more than "science" or "numbers". This has been very well understood by some in the "cold fusion" community and several implied offers or predictions have been made of practical device production on timescales as short as "next year" (see footnote 2). Each visit I have made to the Venture Capital community in the US has resulted in the same response: "*Very interesting; please keep us informed; come back when you have a prototype*". My retort has been the same in each case: "*If I had a prototype why would I need you*?" This glib response ignores the fact that VC's offer more than capital and can support an effective path to commercialization as the success in Silicon Valley shows.

The Silicon Valley model, however, may not be well suited to develop something as technology-intense and critically important as an environmentally benign, universally-accessible, safe and effectively unlimited, large-scale primary energy source. Having watched and participated in the Silicon Valley "green energy revolution" – and shared in its disappointing (albeit predictable) under-success I feel we need another path. To quote myself [12]: "*The results we are seeking will benefit all mankind. Likewise the team of talent and consensus needs also to be multi-national.*" What other models do we have to attempt and succeed in that which Machiavelli [13] warned us so clearly about? "*There is nothing more difficult to take in hand, more perilous to conduct, or more uncertain in its success, than to take the lead in the introduction of a new order of things.*"

Who with the requisite power and influence has this courage, and what do we need to do to attract (and sustain) their attention? I see two levels in this. First while I do not expect the venture community to be likely allies, the people who have become rich through ventures might be. These are technically adroit individuals (or groups of individuals), although none would be considered as having "normal skill in the art" of the FPHE (electrochemical or gas/metal). All such individuals will seek expert opinion. Having served often in a "due diligence" capacity I can confirm that the riskiest thing is to say "yes" to a complex prospect that requires large funding (and time) for completion. The talisman that we create to facilitate communication about CMNS must work on two levels:

(1) It must be sufficiently simple and obvious that no hidden error can possibly exist to negate the result, or leave any doubt. This obviousness must be clear to the potential sponsor so that simplistic negation (or introduced complexity) such as those used in the past[c] carry no weight.
(2) The energy produced must be sufficiently net positive that useful work can be, and is being, made of it.

So now we know:

- *Who*: rich, modern, successful industrialists and their selected experts, and
- *What*: something simple that makes power and thus energy, preferably in electrical form that is easily measured and can be used to supply the conditions needed for control, self-sustainment and utility.

The last sounds like a tall order but it is important to note that our first level prototype does not need to be practical, elegant, cheap or civilian-safe. It needs to be "somewhat reliable" limited by the patience of the reviewers, and "somewhat durable" since it will need to overcome any concerns about stored energy. The technology chosen for the demonstration prototype may have no more to do with ultimate engineering practice than a shared underlying mechanism of power production. Its purpose is to demonstrate that the effect is real and of sufficient scale potential to contribute to a solution of man's oncoming energy deficit. Engineers will use the demonstration prototype (perhaps in second generation form) to explore the parameters of control and scale-up.

It should be noted also that the means of operation of our demonstration prototype does not need a theory or a known and detailed mechanism to be effective. Theoretical help would greatly aid the development of the prototype and help with scale-up, and it would be quite irresponsible to scale a known nuclear effect (and particularly to impose it on society) if the underlying physics is not at least "somewhat in hand". Nevertheless, most technological inventions have been engineered into existence with the science backfilled afterwards. We must anticipate that this will also be the case for "cold fusion" (in inverted commas as we would not know what it really is until we have theory).

If you accept the argument so far, what are good candidates for the demonstration prototype? Which technology? What size? Our object is to make this *as easy as possible for ourselves* (at this stage) – and everyone will have a personal choice based on individual experience and training. At this stage I would like simply to mark out the terrain and see if by discussion and sharing of experience and analysis we can arrive at high probability choices for our demonstration prototype. I restrict attention here to effects explored and discussed under the rubric of "cold fusion" or CMNS (explicitly excluding zero point energy and the "black light" sub-quantum concepts of Randy Mills).

I see two major choices and one hybrid. Each has demonstrated useful characteristics and exhibited potential to conceive our demonstration prototype. This list is restricted to those technologies with which the author has had direct experience. Other approaches are possible and input is welcomed. A short list of proto-technologies that might be used to make a demonstration prototype follows:

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[c] Requirements of an equal branching ratio (neutrons and tritons as in hot fusion); violation of the first law of thermodynamics; non-conservation of baryon number; the "impossibility" of measuring ^{4}He in a D_2 background; and similarly science-sounding but misapplied criticisms. Again for the sake of fairness and completeness it must be noted that legitimate criticisms have been leveled, and questions asked, that have not been fully answered or even addressed.

(1) *Electrochemical PdD/LiOD at elevated temperature* using superwave (or some alternative) disequilibrium stimulus. The progenitors of this avenue are Fleischmann and Pons (electrochemical PdD/LiOD), Fleischmann/Lonchampt/Biberian (elevated temperature), Dardik/Energetics (superwave stimulus). The best example of success is ETI-64 [14] which manifested >30 W thermal output with <1 W average electrochemical input. It exhibited an integrated thermal energy output of 1.14 MJ for an integral electrical energy input of 40 kJ over a 14-h period. This cell boiled the coolant (H_2O at $\sim$1 atm.) during this excursion and then again with greater energy output subsequently.

(2) *Metal–hydrogen gas systems at elevated temperature*. The metal, typically in small dimension form, can be palladium, nickel, some alloy or coating of Pd on Ni, or some other metal having the capacity for high hydrogen permeability. The gas is an isotope of hydrogen: protium, deuterium or tritium – or a mixture. Some observers suggest that protium "works" with nickel, not palladium, and that deuterium is effective with palladium and not nickel. I think it is fair to say that the Ni–H gas system has experienced less experimentation than Pd–D and the findings are more controversial. Nevertheless the claims, if shown to be valid, are stunning: power generation of hundreds of kW, at temperatures above 500°C, sustained for meaningful periods without input power. Certainly this could form the basis of an ideal demonstration or even working prototype.

(3) *Metal–gas modulated plasma*. Somewhere between items 1 and 2 lies glow discharge. This incorporates the advantages of electrochemistry (high chemical potential, high fluxes) and gas systems (short initiation time, low thermal mass, high temperature, low inventory of impurities and lower corrosivity). Representing Energetics Technologies (Incorporated and operating in Israel as ETI but headquartered in New Jersey) Arik El-Boher presented at ICCF10 [15] what was then and remains today one of the most exciting discoveries in Pd–D heat studies. Energetics struck a super-wave modulated glow discharge between thoriated tungsten and a thin palladium coating (on stainless steel) in sub-atmospheric D_2. The experiment produced boiling water with a power gain of 3.88 and an energy gain of 6.72 (because of conspicuous "heat after death") over a period of 10 h. Because the temperature of the plasma was quite high (although unmeasured) one can easily conceive of a demonstration prototype but this experiment has not been replicated to my knowledge despite the best efforts of El-Boher, Energetics and SKINR.

Of final consideration is the size of demonstration prototype. What size is easiest to accomplish with the selected proto-technology and on what scale do we need to operate to engage effectively our selected audience? This latter choice is personal – but critical – and would benefit from wide-ranging discussion. Hypothetically, knowing nothing about cold fusion, LENR or CMNS, having attended none of our conferences or read (or understood) any papers on this topic, what would it take to persuade you that a machine that you are looking at (measurements permitted) is converting nuclear energy to thermal or electrical? The power – in fact the point – of this claim is that the energy density of nuclear reaction is $\sim 10^7$ times that of chemical or mechanical energy storage. Obviously one would need to observe and interrogate the demonstration object and its power production (with full access) for periods sufficiently long to rule out all conceivable potential chemical or mechanical energy storage processes.

It is important to reflect that effects with these characteristics have been shown or claimed before – although not in a demonstration setting – which have so far failed to convince our target audience or the science community at large. Why has what we have done not compelled the acceptance of the FPHE? There are several reasons:

(1) *We have observed no (or few and incommensurate) energetic nuclear products*. In all situations except ours this would be a good thing (provided that the heat was real). A nuclear process without dangerous nuclear products prompt or long-lived, especially from plentiful, cheap and harmless reactants, may have the potential to secure a benign and sustainable energy source for the next millennia. But proving a nuclear effect without energetic nuclear products is challenging precisely because of our distaste for and fear of nuclear processes.

(2) *Calorimetry is not well respected or understood*. Heat is the principal product predicted and observed by

Fleischmann and Pons, and by all serious replicators of their claim (the FPHE). Most such experimenters relied on calorimetry in their cold fusion studies. Calorimetry is an ancient tool, little taught, used or comprehended. The SRI team needed to teach themselves calorimetry in order to interrogate and expand the Fleischmann–Pons claims. Even today confusion and controversy exist inside the field about the "best" – or even acceptable – means of calorimetry. This tool is considered (by some) to be intrinsically inaccurate – this despite the fact that calorimetry forms the basis for modern chemical thermodynamics for which considerable precision and accuracy is required to sustain the entire chemical industry.

(3) *Heat is ephemeral*. When heat is not present there is little evidence of it having been there unless work has been done with the heat. Failing this we may have indications of prior melting or phase change that only experts can interpret. To "sell" the pre-existence of excess heat one must make convincing argument on the basis of "measurements" and "numbers" alone. While this is a normal case in science many of the people we must convince are not scientists and are not willing to trust the proof of an "impossible" (or at best highly novel) effect to an analysis of "mere numbers", or the testimony of one "expert" over another.

(4) *The effect is "small"; relative*. The set of proto-technologies suggested above were selected precisely for their potential for large (and scalable) effects. Especially for electrochemical technologies the input power requirement is large and of long duration. Loading is achieved and maintained by the imposition of a potential gradient in a low impedance system (the electrochemical cell). The current that flows is large and must be maintained for very long times (hundreds of hours for bulk Pd) to overcome initiation effects while maintaining loading. Except during initial loading essentially all of this current flows to parasitic processes, primarily D_2 and O_2 evolution. For aqueous electrochemical loading this cannot be avoided but means that any excess energy generated by putative nuclear processes must be superimposed on a large input power (IV) and even larger input energy (IVt) because of the integration of energy during the long initiation phase. The maximum excess power typically observed at SRI in $>$100 successful experiments was 3–30% of P_{in}. Although statistically certain this is still a small heat effect relative to input power and is not persuasive to anyone unfamiliar with the experiment and calorimeter details. It is also "not enough" as will be shown later in this paper.

(5) *The effect is "small"; absolute*. In his first book published in 2007 [16] Ed Storms cites 242 successful heat-producing experiments around the world (123 electrolytic). Of these 117 (64 electrolytic) show excess power of 1.25 W or less. His histogram (Fig. 40 in [16] and reproduced as Fig. 1) falls off roughly exponentially and rapidly with maximum produced excess power so that cells exhibiting a maximum excess power of 1.25–2.5 W are only 35 (18 electrolytic) and 2.5–3.75W are only 23 (12 electrolytic).

How much heat is needed to convince a non-expert? It could be argued that since the FPHE cannot directly be seen "the evidence must be palpable to be real". The threshold of tactile perception depends on several factors but one might reasonably expect to be able to distinguish by touch the thermal difference due to switching 10 W thermal into or out of a "small" system. By the time we get to a "modestly robust" level of excess power with range 10$\pm$1.25 W the count of successful FPHE experiments worldwide from 1989 to 2006 reviewed by Storms is 16 (nine electrolytic). But there is some power in the tail and Storms' world count of experiments producing $>$10 W of excess power is 40 (with only 15 electrolytic). This is 17% of all successful heat-producing experiments reviewed by Storms. The $>$10 W level reflects only 12% of electrolytic successes but the rapid drop-off in excess power levels from electrolytic experiments is likely due to the expense (as well as hazard) of large Pd/D_2O electrochemical experiments.

Figure 1 plots a histogram of all successful excess heat production reported in the interval 1989–2006 as reviewed by Storms [16]. The analysis inset in the upper left demonstrates the roughly exponential character of these data. But, as noted above there is "power in the tail" and if we accept 10 W as a "sufficiently robust" level of excess heat production for our demonstration prototype then this should be plausibly achievable. Our demonstration object is, however, required to do more than "feel warm". We need it to generate sufficient electricity to self-sustain for which

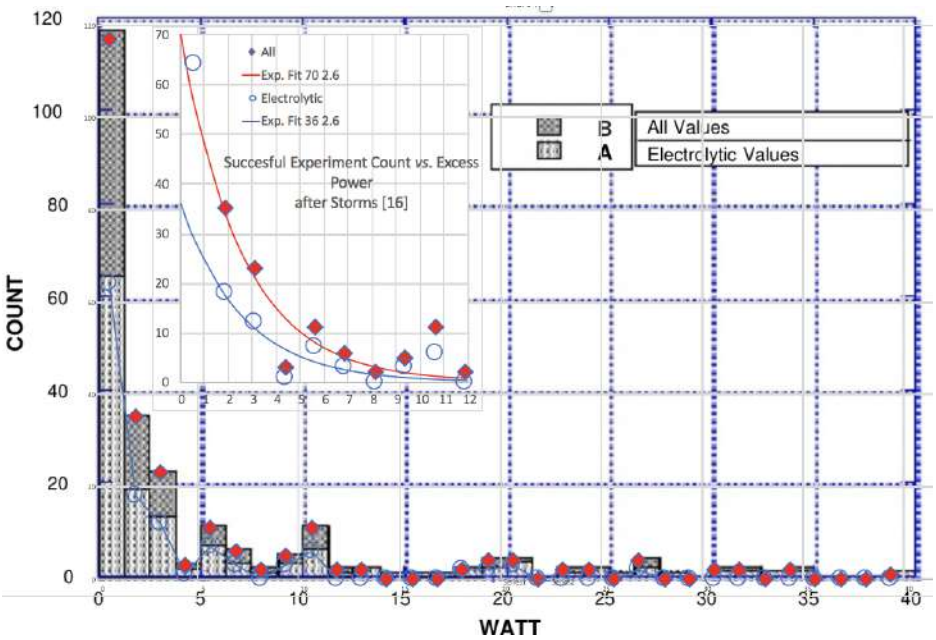

Figure 1. Histogram of successful excess heat production 1989–2006 from [16]. Filled diamonds are all experiments, open circles are electrochemical.

we require energy gain and significantly elevated operating temperature to offset the strictures specified by Sadi Carnot, and worse.

Figure 2 plots the Carnot limit as a solid blue line referenced to the right axis as a function of operating temperature with a heat rejection temperature of 20°C. Also plotted are a family of curves showing the electrical power (in W) "available for use" over the range of operating temperature, calculated for the stated hypothetical gains at a (nominal) input of 10 W. The gains selected are: 2, 3, 5 and 20. Note that Carnot is a limit that cannot be achieved and becomes increasingly difficult to approach as operating temperature reduces. What is clear is that for a gain (heat out/electrical in) of 2, at 10 W electrical power in, one could generate 1 W of power for demonstration above that needed for sustainment, *even in the limit, only above* $\sim 460°$C!

Obviously operating temperature is important and more is better. But gain is even more so. For a gain of 20 (the top curve) we achieve our "extra" 1 W of electrical power for demonstration (in the limit) at only 70°C. By 100°C (the minimum temperature of ETI-64) ~1.6 W of "extra" electrical power would be available for demonstration. Clearly gain is of the essence and the essence of gain is reducing the denominator. Superimposed in Fig. 2 are data points

Table 1. Characteristics of selected cells at SRI and ETI

P_{in} Electric (W)	Cell name	$T°$ C	Net Electric (W)	Gain	Percentage of P_{in} (FM) (%)
2	P19	35	–1.7	2.9	–86
20	P15	65	–16.5	1.3	–83
1.25	L14-2	55	–1.01	1.8	–81
0.093	Pd-C	45	–0.07	3.0	–76
0.55	ETI-GD	**100**	0.24	6.7	44
1.1	ETI-64	**100**	5.3	27.5	490

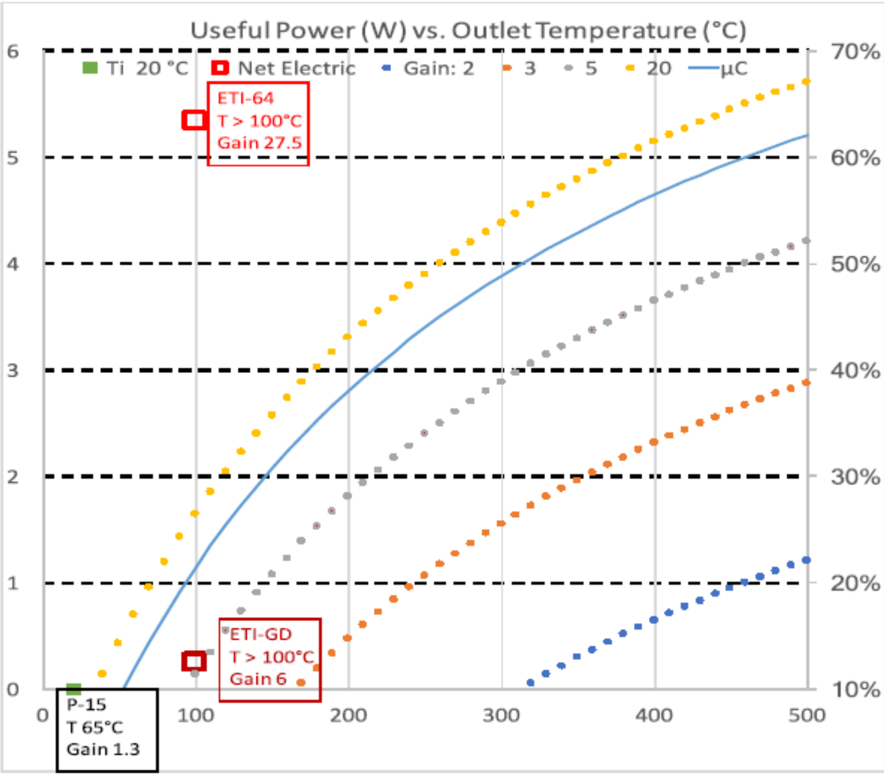

Figure 2. Carnot limitation (solid blue curve referenced to right axis) and hypothetical electrical output for ideal Carnot conversion at the temperatures and gains shown.

from performed experiments: four from SRI and the two cited above for ETI, with characteristics cited in Table 1. Note that the column "Net Electric" is the value calculated for demonstration from P_{in}, T, the measured power gain and Carnot. The percentage of P_{in} is Net Electric divided by P_{in} and can be read as a "Figure of Merit" (FM) for the different experiments. The temperature listed for the two ETI cells is reported as a minimum value and so therefore is the Net Electric Power that might potentially be available.

As clearly seen in Fig. 2 the SRI experiments "fail to register" and have negative FM. Even with perfect Carnot thermal-to-electric conversion the 4 SRI cells all would have produced less electrical output than input because their gains and operating temperatures were too low. This comparison is somewhat unfair in that these experiments were not designed or intended to achieve the conditions for self-sustainment or net electrical production (in fact the design criteria were largely antagonistic to this goal). Nevertheless the experiments cited are "the best" available and yet clearly do not meet the criteria stated as the object of this paper.

Not so for the two Energetics experiments mentioned and listed above, and plotted in Fig. 2. The glow discharge experiment [15] just passes the bar at the temperature limit of the calorimeter (boiling water) but it is certain that higher temperatures were available in the plasma and with different design the square could climb the red dotted curve into "useful" territory. No such extrapolation is needed for ETI-64. With a gain of 27.5 this experiment would have constituted a useful demonstration even at 100°C. This point is made more directly and dramatically in Fig. 3.

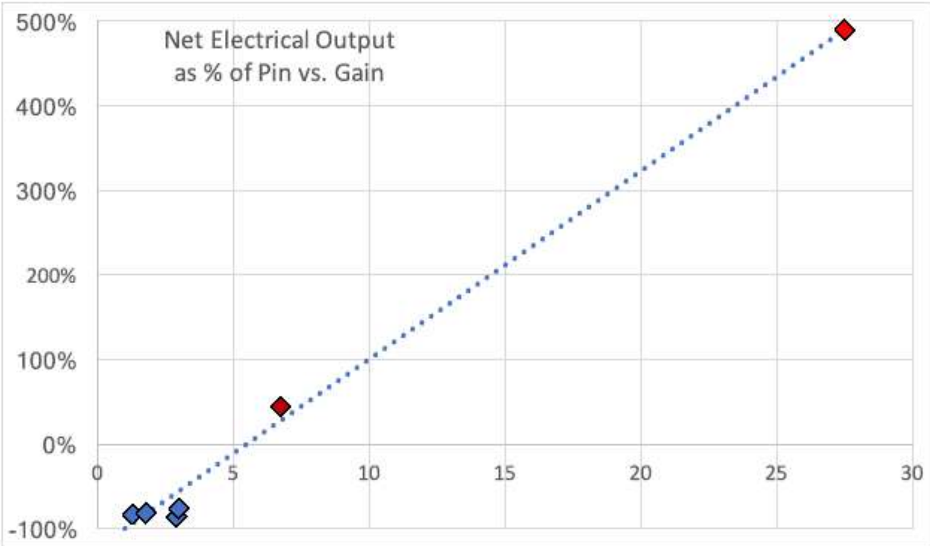

Figure 3. Net Electrical Output for ideal Carnot conversion as a percentage of P_{in} (FM) as a function of experiment thermal gain, for the experiments summarized in Table 1.

The SRI experiments (blue) all fall below zero with gains <3 and FM<0. ETI-GD comes in above the line and if repeatable might easily be "improvable" with increased operating temperature; I still regard this as one of if not the most important experiments ever performed in LENR. But based on the metric FM plotted in Fig. 3, and the goal elucidated in this paper, the star performer is ETI-64 that I have spoken of several times before – if not explicitly in the present context. If we are to approach the goal of self-sustainment with thermal output >10 W with some Watts electrical left over for convincing demonstration then we need to address the question of why ETI-64 "worked" and essentially all others did not.

3. Conclusions

(1) "Loading" (chemical potential) is important at least in creating the conditions in the lattice suitable for condensed matter nuclear processes.
(2) Flux is critical. This may be deuterium flux, electrons, or phonons (or other?). Static, equilibrium conditions do not result in the FPHE.
(3) Theory alone may not allow us to achieve our goal but will be needed for commercial acceptability and to win the help of other working scientists.
(4) To gain acceptance we must be able to demonstrate – more or less on demand – that novel nuclear effects take place in condensed matter and create net energy.
(5) Demonstration must be made of a practical use of this energy; for this we require a Demonstration Prototype
(6) To make effective demonstration, operating Temperature is important but *high gain is crucial to accomplish our goal.*
(7) Gain is more easily affected in the denominator than the numerator – our goal is to create the excess heat effect with low input electrical power stimulation.
(8) Multi-dynamic, multi-resonant processes appear to be involved or crucial in producing the FPHE. Irv Dardik

and Energetics have been demonstrated to be right in concept. To make progress we must better understand this teaching.

Acknowledgments

By "Figure of Merit" the "best" SRI experiments fail to meet my asserted demonstration criteria and were not intended to. These experiments nevertheless were crucial in establishing a basis for understanding and I would like to give special acknowledgement to the early core SRI/EPRI team who achieved this: Steve Crouch-Baker, Andy Riley, Romeu Rocha-Filho, Stuart Smedley, Fran Tanzella, Tom Passell, Joe Santucci, Sharon Wing and Susan Creamer. Particular credit needs also to be given to the Energetics team who worked so hard and succeeded so dramatically, sustained by the vision of Sidney Kimmel and Irv Dardik: Herman Branover, Arik El-Boher, Alison Godfrey, Ehud Greenspan, Shaul Lesin and Tanya Zilov. For special credit I would like to single out five individuals who have contributed immeasurably to my critical understanding: Trevor Dardik, Martin Fleischmann, Peter Hagelstein, Paolo Tripodi and Vittorio Violante. Thanks and credit is given also for valuable input to this paper from Peter Hagelstein, Abd ul-Rahman Lomax, David Nagel, Fran Tanzella and Vittorio Violante.

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Addendum

Not discussed at Asti and added in review is the possibility that new or more complete data on heat–helium correlation might provide a suitable basis to complete the initial phase of CMNS research and answer the question: *"is it real?"*, to allow us to proceed to phase 2: *"what is it good for?"*. Having pursued this experimental question intensively for several years my caution and condition is as follows. Helium (specifically ^{4}He but also ^{3}He and preferable both) will become much more easily measurable and convincing when the FPHE is triggerable more or less on demand. The issues of leaks and pre-occurrence of these isotopes can be effectively allayed if measurements can be made from samples taken from the gas phase immediately before and after a triggered heat event.