J. Condensed Matter Nucl. Sci. 19 (2016) 183–191

Research Article

Cold Fusion – CMNS – LENR; Past, Present and Projected Future Status

Michael C.H. McKubre*

SRI International, Menlo Park, CA, USA

Abstract

A brief overview of the SRI effort over 26 years is provided as a precursor to suggestions on how we might best proceed to validate the vision of Martin Fleischmann and Stanley Pons, and proceed towards its logical conclusion.

Keywords: Cold fusion, Excess heat, Helium, LENR, Reproducibility

1. Introduction

On March 23, 1989, Martin Fleischmann and Stanley Pons announced their observation of a non-chemical heat source in the D_2O/Pd system. In their initial publication they speculated on the possibility that the effect they had observed may derive from nuclear processes and included the interrogative "Fusion?" in the preliminary title of their submission [1]. While they presented no direct evidence for nuclear or fusion processes, the evidence of a heat effect considerably larger than chemical or lattice storage effects was strong. In their initial Patent submission [2] Fleischmann and Pons reported results from the electrochemical cell and calorimeter shown in Fig. 1 that would be difficult or impossible to explain by conventional chemistry or electrochemistry. Operating at constant current in a thermodynamically open system, the cell was topped up with D_2O approximately every 12 h to compensate for D_2 and O_2 losses via electrolysis (the downward excursions in Fig. 2). As the cell resistance rises at constant current due to loss of electrolyte conductivity and increased anodic and cathodic overvoltages, the power in the cell also rises and so does the cell temperature as shown in Fig. 2. With addition of D_2O at $\sim 3.1\times\ 10^6$ s (after ~36 days of electrolysis) the cell temperature in the example shown abruptly and inexplicably rose $\sim 20°C$, and the temperature vs, time slope changed, then changed again and the cell temperature rose to boiling, all at constant current and sensibly constant input power. Given the power and energy densities of the heat effect claimed by Fleischmann and Pons, only one of two rational possibilities existed in 1989:

- Fleischmann and Pons were wrong in their excess heat determinations.

*E-mail: michael.mckubre@sri.com

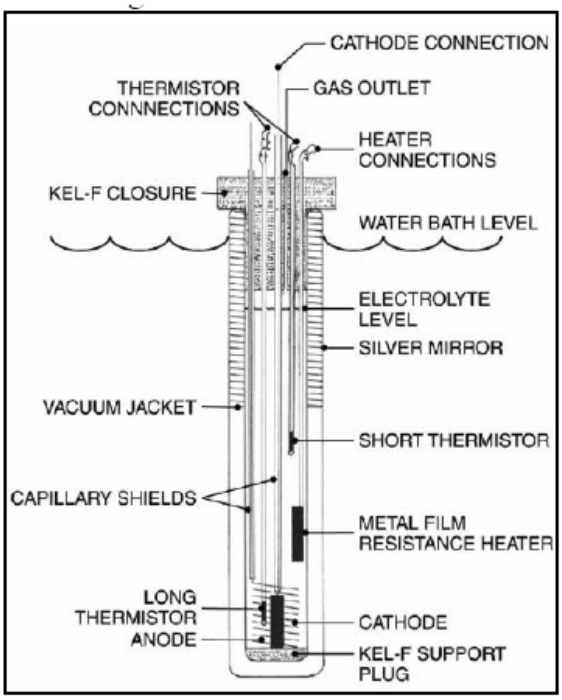

Figure 1. Fleischmann Pons calorimeter.

- Nuclear reactions occur in metallic lattices by mechanisms and with product distributions different from similar reactions in free space.

This opened considerable speculation about the two following questions. Is there such a thing as Condensed Matter Nuclear Science (CMNS)? Does our free-space view of nuclear physics need to be extended in potentially interesting directions?

2. Experimental effort at SRI

Scientists at SRI have engaged in approximately 70 person-years of study stimulated by the original Fleischmann and Pons announcement [3–5]. The resulting conclusion is that nuclear level thermal energy density can be produced from the electrochemical loading of deuterium into palladium under difficult-to-achieve but well-defined conditions.

Known critical conditions include:

(1) Maintain high *average* D/Pd ratio (*Loading*)
(2) For times $>$ 20–50 times the deuterium diffusional time constant $\tau_{\mathrm{D/D}}$ (*Initiation*)
(3) At electrolytic current densities[a] i >50–500 mA cm^{-2} (*Activation*)

[a]This current density threshold is reasonably well specified (and large) for wire cathodes with concentric anode geometry but is less well defined (and generally lower) for parallel plate geometries that exhibit far more heterogeneous current distributions.

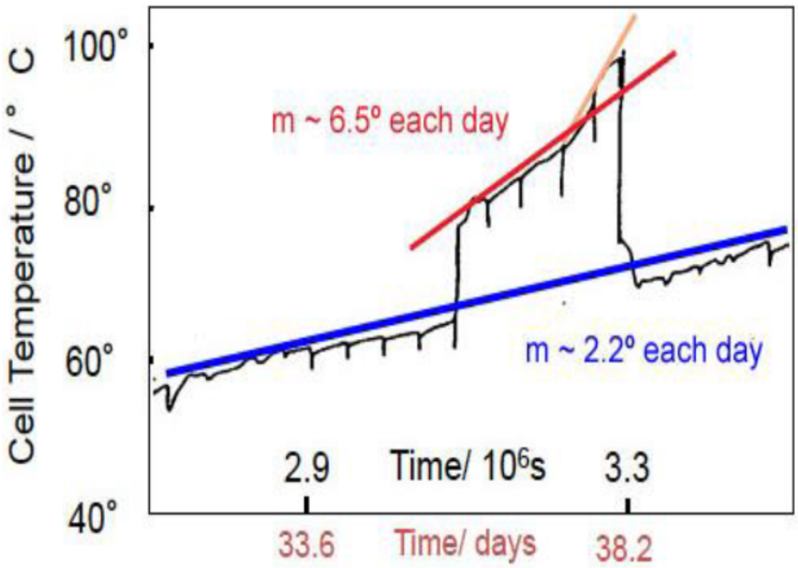

Figure 2. Early reported excess heat excursion [2].

(4) With an imposed or adventitious deuterium flux (*Disequilibrium*)

An empirical "prediction function" was developed for excess power (P_{xs}) as a result of these observations:

$$P_{\text{xs}} = M(x - x^{\circ})^2(i - i^{\circ})|i_{\text{D}}|,$$

where x = D/Pd, x°~0.875, $i = 50$–400 mA cm^{-2}, $i_{\text{D}} = 5$–20 mA cm^{-2}, t >20 $\tau_{\text{D/D}}$.

In addition to helping to define the conditions under which the Fleischmann Pons Heat Effect (FPHE) can be seen and explored, another major thrust of the SRI team was to attempt replication of experiments of other scientists and inventors in the field. A brief itemization follows:

(1) *1989–1991 Fleischmann Pons heat effect*. Initial criticism of the FPHE was that "*it could not be reproduced*". Obviously, given consistent input conditions in non-stochastic processes everything reproduces (particularly systematic error), so this rather unsophisticated criticism can be interpreted to mean one of two things: Fleischmann and Pons did not observe an anomalous heat effect (even as a result on systematic error); the input conditions needed to produce their observation were not reproduced in subsequent attempts. For those who knew Fleischmann even if only by repute, the first possibility was not highly plausible so efforts were undertaken at SRI and elsewhere to understand under what conditions the FPHE could be observed. Once understood we successfully reproduce the FPHE on approximately a hundred occasions with electrodes of various forms.

(2) *1992 – Kevin Wolf gamma activation*. Attempting to reproduce SRI excess heat observations in electrolytic cells with addition of Si and B, Kevin Wolf observed bursts of both neutrons and gamma rays from three out of three cells running under real-time nuclear monitoring. At the end of the experiment these cathodes all were gamma active [6]. Under Wolf's guidance SRI reconstructed his neutron and gamma spectrometers but were not able to recreate the observations of prompt and activated emission in about 2 years of focused effort.

(3) *1993 Ni – Natural water heat effect*. Considerable interest (and confusion) was introduced into the cold fusion field in 1993 with multiple reports of excess heat production from large area nickel cathodes exercised in light water (typically in carbonate) electrolytes. One highly credible report of such observations was by M. Srinivasan from Bhabha Atomic Research Center (BARC) in India. In 1993 we invited Dr. Srinivasan to demonstrate his experiment during a sabbatical at SRI. He was able to reproduce the effect but we were able to identify an interesting (but mundane) source of the thermal anomaly.

(4) *1993–1996 Mel Miles heat-^{4}He correlation*. In one of the more important papers in cold fusion history Mel Miles presented evidence at ICCF2 in Lake Como [7] that excess heat from Pd/D_2O electrolysis, and ^{4}He, were statistically well correlated nearly quantitatively with a Q value of $\sim$24 MeV/^{4}He atom. One criticism of Miles' technique (later shown to be unfounded) was his use borosilicate glass that is permeable to ^{4}He. To guard against the possibility of error due to ^{4}He inleakage from ambient, SRI used an all metal sealed cell and calorimeter to successfully reproduce the Miles Bush heat-helium results with the experimental assistance of one of Miles' co-authors (Ben Bush).

(5) *1995 Patterson "Light" water excess heat*. At ICCF5 in Monaco in 1995 Dennis Cravens presented an impressive demonstration of a novel excess heat generating device invented by Jim Patterson. The device featured a flow-through, packed-bed of polystyrene latex spheres coated with multiple thin layers on metals including Cu, Ni and Pd. Simple calorimetry was performed by measuring the temperature difference in the electrolyte as it transited a packed bed of coated spheres that was polarized cathodically with respect to a downstream anode. The electrolyte was said to be "water", the demonstration appeared to show energy gains of 3–5 even ignoring known parasitic losses. We attempted to replicate this impressive result with considerable assistance from Dennis Cravens under more carefully controlled conditions at SRI. Although we were able to observe potentially anomalous ∂T's these were nowhere near the magnitude of the effects demonstrated in Monaco. Moreover, whatever excess heat was present if any in the Patterson cell at SRI it was insufficient to demonstrate convincing evidence in a purpose-built mass-flow calorimeter configured to surround the Patterson cell. Only later was it revealed that Patterson employed $\sim$10% D_2O in his "water" electrolyte in earlier apparently successful measurements (including ICCF5). By the time this was known Paterson's original batch of coated spheres had been exhausted; subsequent batches evidenced little of no heat effect.

(6) *1996–1998 Les case heat and ^{4}He.* One of the most startling pronouncements ever made in the cold fusion world was by Les Case at ICCF7 in Vancouver in 1996. After similar pre-treatment in H_2 gas Case introduced D_2 at a pressure of a few atmospheres to a bed of chemical hydrogenation catalyst consisting of "coconut shell charcoal" infiltrated with approximately 0.5% platinum group metals. This was confined in a 1.7-liter stainless steel vessel. In a very narrow temperature range, 150–250°C, and with a few selected catalysts Les was able to produce a 5–35°C temperature rise in D_2 compared with H_2 gas. He also claimed anecdotally to have had a post-test D_2 sample analyzed by mass spectrometry at Oak Ridge National Labs (ORNL) where they observed $\sim$100 ppm of helium-4. Since the concentration of ^{4}He in room air is 5.22 ppm, if verifiable this finding was stunning.

Funded by DARPA SRI mounted a major campaign to replicate Case's reported findings. This effort initially failed repeatedly as we were not able to evidence significant temperature differences between catalysts in D_2 and H_2 or any indication of ^{4}He increase. It was not until we stood beside Les Case and watched his detailed handling of catalyst and gas that we were able to make progress. The differences between the "Case process" and our understanding of the Case process were subtle, but significant, and we were rewarded by an ultimately successful replication of the Case Heat Effect accompanied by some of the largest ^{4}He concentrations so far observed (up to 11.8 ppm) in cold fusion experiments.

(1) *1997–1998 Arata–Zhang "DS" cathodes heat and ^{3}He*. Arata contributed one of the more interesting and

reportedly replicable innovations with his novel "double-structured" (DS) cathode. The Arata claim was that every case in which his DS cathodes were exercised in heavy water electrolytes resulted in excess heat and both ^{3}He and ^{4}He in the sealed cathode void space that contained nano-Pd. Blank cells employing H_2O produced null results. Using DARPA funding in a replication effort and trajectory similar to that for the Case effect, SRI with help from the Violante Group at ENEA Frascati, was unable to replicate the Arata DS heat effect without direct assistance from Professors Arata and Zhang. Using their cell and calorimeter design, experimental protocols and a pair of DS cathodes fabricated under their supervision, SRI was able to replicate important aspects of the Arata–Zhang claims. Two identical cells were operated simultaneously, side-by-side, with the same current protocols. The cell and cathode exercised in D_2O produced significant excess power (a maximum of $\sim$10% of P_{In}) and energy (a total of $\sim$100 MJ) while the sister cell and cathode operated in H_2O showed no evidence for excess heat. Considerable effort had been put into planning and preparing for gas phase analysis of the contents of the sealed cathode voids to verify the Arata reported finding that ^{3}He and ^{4}He build up concentration in these voids to very significant levels, possibly commensurate with the excess heat. Due to a "gas handling lapse" by the selected measurement laboratory this particular claim could not be verified by SRI. We were, however, able to prove unambiguously that tritium was created in the sealed void space of the D_2O cathode at some time during the cathodic electrolysis. The initial evidence of ^{3}H creation was a very considerable buildup of the decay product ^{3}He. No ^{3}H, ^{3}He or excess heat were observed for the H_2O cell.

(2) *2003–2011 Energetics "SuperWave" excess heat and ^3H.* At ICCF10 in Cambridge in 2003 Arik El-Boher representing Energetics Technologies Incorporated (ETI) surprised the audience with a report of substantial excess power generation and energy gain at useful working temperatures in both gas discharge cells and electrochemical cells, encountering the heat-after-death phenomenon and the production of tritium. In less than two years of effort Energetics had progressed from uninvolved to contribute what I reported to Martin Fleischmann to be "the best paper of the conference". The secret of Energetics' success appeared to be what their progenitor Irving Dardik called a "SuperWave" – a complex multi-frequency, multi-amplitude driving function designed to stimulate simultaneously a wide range of resonant processes. Of immediate significance to the SRI team was that the "SuperWave" generating function appeared to be capable of engendering simultaneously both high deuterium loading and high interfacial deuterium fluxes in electrochemical systems, both then known to be correlated with excess heat.

With funding from DARPA and significant assistance from the Violante Group at ENEA Frascati, SRI mounted a formal replication effort to demonstrate the efficacy of the SuperWave protocol in creating the conditions necessary for excess heat production. This effort resulted in the most successful campaign of replication so far performed at SRI with one set of Violante-produced Pd foil cathodes and Energetics-SuperWaves resulting in significant excess heat in $\sim$73% of the experiments run. Furthermore, using the criteria of "critical conditions" enumerated above we were able to explain our failures to produce excess heat as a failure to meet one or more critical conditions, in every case except one electrode that produced no measurable heat but considerable tritium [8].

(1) *2012–2015 Brillouin excess heat.* SRI is presently engaged with Brillouin Energy Corporation (BEC) to help them develop and scale up their triggered excess heat effect into parameter space that supports practically engineered heat and/or electricity production devices. The end objective of BEC is (as was ETI) practical technology. The primary experimental driver and foundational IP claim of BEC resides in their use of very fast, closely tailored electrical pulses (that they call a Q-pulse) that is supplied axially along carefully structured cathodes (initially just Ni or Pd wires). This pulse has been shown to trigger excess heat production providing Brillouin with a level of technological control that has not previously been evidenced in the cold fusion/LENR/CMNS world. SRI's engagement with Brillouin is ongoing.

3. Present Status

The present situation of the field offers both good news and bad. In the former category it seems apparent that Fleischmann and Pons were correct in more ways than they anticipated The accumulated evidence strongly supports the conclusion that nuclear effects take place in condensed matter by pathways, at rates and with products different from those of simple, isolated, pair-wise nuclear reactions in free space (i.e.two-body interactions). The group of influential physicists self-assigned to prevent or delay the full elucidation of the effect that Fleischmann and Pons reported on March 23, 1989, have largely disappeared. Although there has been terrible attrition from "team CMNS" our critics have suffered much worse and have not been replaced. The "evidence" presented at the May first Meeting of the APS in Baltimore is seen (retrospectively) as essentially irrelevant due to:

- The failure of the group lead by Lewis [9] to anticipate now known threshold conditions: Loading; Current Density; Flux; long Initiation Times.
- The practical irrelevance of the Koonin calculation [10] due to the lack of anticipation of or attention to the massive electron screening effects seen on PdD (and other condensed matter structures [11]), and disregard for the fact that hot fusion products (and thus processes) are clearly not seen and that the D–D spacing in PdD is greater than that in D_2O which does not undergo spontaneous fusion at measurable rates.

As a result of these critical failures it is now very difficult to find a place or person to approach for up-to-date objective criticism. This is mostly not good news, however, as progress depends on constructive criticism.

Other news is not good and there remain a number of critical challenges. Despite considerable effort there is a lack of clear data supporting the existence of an effect and an understanding of what drives the results. In particular there exists no fully documented experiment with raw data set available, and no agreed to theory (despite a plethora of offerings). The confidence that many feel about the reality of the FPHE is largely based on the accumulation and synthesis of the outcome of many experiments (see [12]) rather than the existence of a single clear, simple, unambiguous experimental result. This need could be avoided if there were clear demonstration of technology based on the FPHE but, although many claim to be developing this, none has been brought to the market. In the future, open or communal experimentation may serve as a useful pathway to proof.

The academic stigma is maintained mostly by an inability or barrier to publish in mainstream Journals. The editors of these Journals all are of sufficiently advanced age that they remember the furor and claimed "slam dunk" rejection that attended the May 1989 APS. Because of the paucity of clear results and the experimental complexity, these individuals have not been adequately exposed to the work following May 1998 that largely invalidates the thinking behind that rejection and the global significance of the null results reported at that time. This timidity and unwillingness to re-investigate the data of the FPHE and related effects by editors of main stream Journals, effectively ostracizes the CMNS community from the main body of science and may be the single most effective barricade to progress. Without the ability to publish in recognized journals talented young people with academic aspirations are discouraged from entering the field. Fortunately as positive results persist and improve, and with the passage of time from 1989, this problem is beginning to fade. Innovation, by and large, is for the young, a group essentially unaware and unaffected by the almost hysterical condemnation of Fleischmann and Pons in 1989.

Help presented itself to the CMNS community in a most unlikely manner in the person of Andrea Rossi. Not a recognized scientist, having not presented at or even attended any major meeting in the field, and having published nothing on the topic in a recognized Journal of any type Rossi created what might be called "The Rossi Effect". Using combined showmanship and demonstrated operational scale[b] Rossi brought to the attention of a new generation of innovators the possibility that "cold fusion" might not only be real (on which point the world was largely apathetic)

[b]Rossi's largest claimed result was 475 kW thermal [13].

but also practical. Rossi has inspired a significant number of experimenters to pursue the demonstration of heat on moderate scale (hundreds to thousands of Watts) with significant power and energy gain (2–10) at significantly elevated temperatures (up to and above 1200°C). All of these systems employ Ni (usually in finely divided form, often claimed to be nano-metric), thus obviating the intrinsic issues of cost and scarcity associated with Pd. They also employ natural hydrogen (rather than deuterium) sourced in various forms often from the thermal decomposition of inorganic hydrides. The possibility exists that lithium also may be crucial. To this point the proof of performance remains "tantalizing" but it would be a mistake not to recognize the positive impetus and stimulus that "The Rossi Effect" has had on the CMNS community. Without Rossi (who was not even present) it is unlikely that ICCF19 would have been the largest cold fusion conference to date.

4. What is the Future?

The years 2015 and 2016 will not be "business as usual" for the CMNS field – we do not want it to be. Things have changed fundamentally although the extent and direction of change is not obvious to all. We are watching the CMNS field convert from "resource limited" to "talent limited". The capable experimentalists who have brought the field to the point that it is – always in short supply – have diminished with age and attrition and have not been replaced at anything like a steady state rate. Partly inspired by "The Rossi Effect" but also by the weight of positive CMNS results, the time of study (26 years and counting), and the increased awareness that cold fusion may have the possibility to obviate malignant effects of conventional nuclear (fission and hot fusion) and fossil fuel combustion as a primary energy source, significant commercial and governmental interests are directing attention to the questions: "*Is cold fusion/LENR/CMNS real?*" and "*Can it be scaled up to make a difference for the planet?*"

How we proceed will depend on how we convert this "once in a quarter century" opportunity. It is time for us to "*get real*"! Take this opportunity and proceed with due deliberation towards a goal that is not just good for us but may offer real potential for mankind. We have all been drawn to this field at different times and with different motivations but I want to offer a challenge to this community that "business as usual" has not taken us as far as we needed to go. We have contributed mostly as individuals or as individual organizations. We get together every 18 months or so – have been doing that for 26 years – and yet the whole had not exceeded the sum of its parts. Are we too comfortable with this situation? We complain about the lack of respect and resources, how do we really feel about the entry of new people and new money into the field? We would have a lot more competition, for starters. This is coming, which forces the question: *what do we want the future of this field to look like in 5 years*? Do we want to see this field expand and make the world a better place? Think about it. The reflex answer is "*Yes, of course*", but too many (possibly all of us) want to do this "*my way*" using the medium of advancement most familiar to us (business, academic, etc.). How do we escape the myopic trap in which we seem to be ensnared?

We have a tremendous opportunity to come together as a community and expand the field, by making it easier for more resources to participate. Let us draw consensus around our best experiments and have them widely replicated. That would improve the quality of the dataset and improve the science. It would make it harder for critics to deny the verity of LENR, and it would make it easier to encourage students to study it. The goal of any community is to make itself stronger. Those of us who have worked in this field for decades now have a duty to teach. It's time to give back, so that we may all move forward. An understanding of LENR will shock the foundations of science and will leave a legacy for generations. After 26 years and approaching retirement I *really* want to see this. Let's focus on proving to the world we are right. Do we really want to compete amongst ourselves? Do we want to keep keeping the same secrets from each other? I have watched this happen now for nearly all the 26 years I have been engaged in this pursuit. It is time to stop hiding information and start helping each other. If any of us "wins" we all do, and so potentially does the broader community from which we must draw our resources and support.

I personally believe that LENR is too big for any one person or company, or country, to control. Everyone who has contributed to the development of the field is a pioneer. We are well-positioned for glory and recognition, and possibly wealth if it comes. The notion of competition – other than friendly competition among like-minded individuals – is absurd in this field. A monopoly is not possible or productive. If LENR is what I believe it may be it has the potential to change society in the way that Newcomen's steam engine did in England in 1712 – or before that the discovery of fire. It would be ludicrous and anti-societal to attempt to assert exclusive or proprietary "ownership". Even the argument sometimes made that the free enterprise system is the best way to advance such a radical change fails to appreciate the scope and importance of the potential change. Having studied cold fusion/LENR/CMNS now for 26 years (and before that a further decade studying the D/Pd system) I still have discovered no intrinsic reason why the effect cannot be utilized to create a primary energy source that is: essentially unlimited (whether H or D are employed as fuels); accessible to all mankind irrespective of nation or social class; considerably cheaper than existing power sources (especially if Pd can be employed only as a catalyst or avoided altogether); environmentally benign; operable on the scale of single humans or small communities (kW – tens of kW) rather than massive centralized operation with the attendant disbenefits of large capital cost, institutional or governmental control, and the needs and weaknesses of distribution and reticulation.

I know this sounds too good to be true – but I think it may be true and would like to prove it. What single entity should or could control such a boon ethically? Which one of us or which of our institutions? I sincerely believe that a monopoly is not possible or productive. We are the leaders of this field, so let us take that responsibility seriously. The faster and more broadly that LENR has a positive impact, the better off we all are. Think about the commercialization of the transistor. Bell Labs invented most of the enabling technology, and then licensed it broadly and relatively cheaply creating the communication and computing environment we all enjoy today. It would be hard to estimate the number of industries that derive from that decision and the rapid advancement in quality of life that has resulted. We can contrast the licensing of the transistor with the development of power by nuclear fission. In the latter case the technology is controlled by a few large companies and is terminally distrusted by the public. If we want to optimize for impact, the former path is preferred.

I suggest the following as a practical pathway of the joint solution to the very welcome problem of making LENR practical. Let us work together to make each other and the field stronger. I encourage the preparation of more publications with author lists from multiple institutions. Let us reinforce and reference each other's best work and work as a community. Far too many of our publications in our field cite primarily the author's own work. Whether from ignorance or arrogance this needs to stop. Let us make a commitment to learn from each other. From direct observation I know that huge resource has been squandered by failing to take advantage of knowledge earlier hard won in our tiny community. "*Not invented here*" needs to stop and our literature (fortunately fairly well codified in the ICCF proceedings volumes and now the *Journal of Condensed Matter Nuclear Science*) needs to be read, understood and respected, prior to serious experimental effort.

Most importantly we need to invite and include a new generation of researchers hoping they will be able to see farther than we have or can. The future is owned by the young. In our community this is even more true as only those with established reputation in 1989 (and thus seniority) could afford to enter the field. We are all, those who have survived, now 26 years older. Young scientists, unaffected by the academic trauma of the birth of cold fusion are interested and enthusiastic to enter a field with so many open questions and so much potential. It is our duty to help: to teach, to train, to mentor, to lead – if needed – but I expect it will not be. I would like us all to come together now and put our best ideas forward. The opportunity? – obligation? – destiny? – of this community is to birth a new science!

References

[1] M. Fleischmann, S. Pons and M. Hawkins, Electrochemically induced nuclear fusion of deuterium, *J. Electroanal. Chem.* **261**: 301 and errata in **263** (1989).

[2] S. Pons, M. Fleischmann, C. Walling and J. Simpson, International Patent Publication No. 90/10935 (1990).
[3] M. M.C.H. *et al*., Excess power observations in electrochemical studies of the D/Pd system; the influence of loading, in *Proc. ICCF8, Frontiers of Cold Fusion*, Nagoya, Japan, *Universal Academy Press*, Tokyo, Japan, 1992.
[4] M.C.H. McKubre, F.L. Tanzella, P. Tripodi and P.L. Hagelstein, The emergence of a coherent explanation for anomalies observed in D/Pd and H/Pd systems: evidence for ^{4}He and ^{3}He production, in *Proc. ICCF8*, Italy, 2000.
[5] M.C.H. McKubre, J. Bao and F.L. Tanzella, Calorimetric studies of the destructive stimulation of palladium and nickel fine wires, in *Proc. ICCF17*, South Korea, 2012.
[6] T.O. Passell, Charting the way forward in the EPRI research program on deuterated metals, in *Proc. ICCF5*, Monte-Carlo, Monaco, IMRA Europe, Sophia Antipolis Cedex, France, 1995.
[7] M. Miles, B.F. Bush, G.S. Ostrom and J.J. Lagowski, Heat and helium measurements in cold fusion experiments, in *Proc. ICCF2*, Italy, 1991, p. 363.
[8] M.C.H. McKubre *et al*., in *Low-Energy Nuclear Reactions Sourcebook*, Jan Marwan, Steven B. Krivit, (Eds.), Vol. 998, American Chemical Society, 2008.
[9] N.S. Lewis et al., Searches for low-temperature nuclear fusion of deuterium in palladium, *Nature* **340** (1989) 525–530.
[10] S.E. Koonin and M. Nauenberg, Calculated fusion rates in isotopic hydrogen molecules, *Nature* **339** (1989) 690.
[11] J. Kasagi, Screening potential for nuclear reactions in condensed matter, in *Proc. ICCF14*, Washington, DC, 2008.
[12] E. Storms, *The Science of Low Energy Nuclear Reactions,* World Scientific, Singapore, 2007.
[13] M. Lewan, *An Impossible Invention*, M. Lewan, Sweden, ISBN/EAN13 , 2014.