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Research Article

CMNS Research – Past, Present and Future

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Abstract

As a community, we have invested a great deal of time and money in investigating claims of anomalous heat first asserted by Martin Fleischmann and Stanley Pons in 1989. Despite this effort, we remain unable to specify the phenomenon or phenomena revealed. Although it is clear within the LENR community that the effect is of nuclear origin and is exo-energetic, we have yet to define with confidence the pathway to practical technology that seemed implicit in the original announcement. Bringing this community together largely is the belief that such a path is possible, or even imminent. What is taking us so long?

Keywords: CMNS, Cold fusion, Excess heat, Helium, LENR, Tritium

1. Introduction

This paper deals only indirectly with scientific issues. I do not believe that the problems of Cold Fusion, LENR, CMNS, whatever name we choose, are primarily scientific, political or even financial. I am aware of technologies – or pre-technology demonstrations – that have the potential to change the basic infrastructure of power generation, worldwide. In a very real and somewhat urgent sense, sustainable, large scale, primary energy production that does not contaminate our world or harm species is the primary responsibility that we face as scientists, humans and self-appointed stewards of the planet. The understandable, but regrettably human, response to such an opportunity has been in far too many instances the search for personal, regional or corporate glory, fame, wealth or power. This condition has petrified our progress by stultifying collaboration. I am no less responsible for – or less guilty than any other – of concealing secrets. The barbarians are not outside the gates – they are inside – they are us.

I spoke about this problem last year at Tony La Gatta's ICCF19 in Padua, Italy, and would like to elaborate here using a few personal examples. The primary, perhaps only, problems I see to progress in understanding, amplification and application are those of communication both within and outside the community. Hopefully, we can use these conference proceedings to make progress in this arena. The science is (almost) there and there is no shortage of planetary or financial incentive.

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2. Background

First some mythology and mythological metaphors: Martin Fleischmann – father of this field and one of my earliest mentors – drew parallels between Cold Fusion and Icarus. He tortuously crafted the acronym I.C.A.R.U.S. (Isoperibolic Calorimetry Research and Utility System) for his boiling water calorimeter, first publicly displayed at ICCF3 in Nagoya, Japan, in 1992.

Martin selected the metaphor of ICARUS to bring attention to the perils of flying close to the sun with deficient apparatus and support. Martin told Chris Tinsley [1] and others including me that "*I should have called it Daedalus but I couldn't think of a good acronym.*" Daedalus was the father of ICARUS. Both were imprisoned in the labyrinth that Daedalus had constructed for King Minos to cage the Minotaur – a long story. Daedalus was a technical genius and constructed wings facilitating his and his son ICARUS to escape. Daedalus warned ICARUS that the materials of construction were unsuitable for near-solar excursion. Being young and tempted by the brightest object perceivable in his universe Icarus paid no heed, or could not resist, the temptation – and perished.

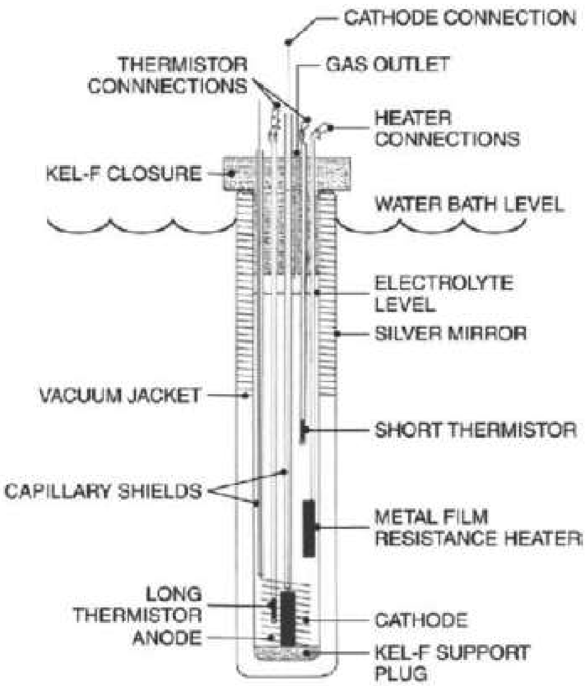
If Martin imagined himself a latter-day Daedalus I will avoid the temptation to speculate who the modern ICARUS, or ICARI, are or were. Martin gave us the tools to fly – or at least ample motivation to try. Many have failed to heed his warnings and have fallen to earth. But perhaps the story of Prometheus is also apt and cautionary. Are we self-censoring the delivery of new fire to mankind because we are afraid of the wrath of contemporary Zeus? Are we guarding our secrets for fear that others will take credit? Whichever or both, it needs to stop. Anyone who believes that what we are working on has the potential to relieve or solve mankind's most fundamental problem of sustainable, benign primary energy production should collaborate, cooperate and communicate. Having spent a career in confidential research I understand the motives for reticence but it is time for a new axiom. An alternative possibility suggested in review is that some scientists may have left details unexpressed simply because they did not know they were important or they thought that they were obvious. It is critical that scientists communicate exactly what they have done; without this replication is impossible and progress incapacitated.

Why are we here and how did we get to where we are? Through the work of many more able than I, of course, much of whose work was not immediately recognized and is not well remembered today. I would like to focus in a very personal way on the individuals and deeds that motivated me most and inspired what is now more than 27 years of almost constant single focus on demonstrating and thence understanding what we know as the Fleischmann Pons Heat Effect (FPHE) and its much more important generalization Condensed Matter Nuclear Science (CMNS) in which the nature, rates and consequences of nuclear effects in solid and liquid matter differ importantly from those in free space or rarified conditions.

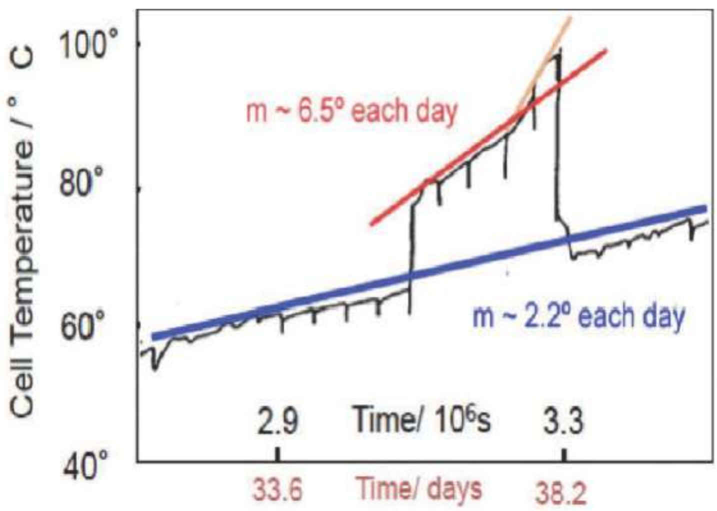
My first hero of course is Martin Fleischmann, closely followed by John Bockris. I met Martin in person first at Southampton in early 1977, John somewhat earlier. But I knew them both as Titans of Electrochemistry from my very first interest in the subject. I have often used Fig. 1 as the convincing argument for a heat effect in the deuterium-palladium system that is incommensurate with any conceivable chemical or lattice energy storage effect.

As you recall, when operated at constant current in their electrochemical cell, the sloping baseline temperature develops with decreasing conductance of the electrolyte, and increasing impedance and thermodynamic voltage at the two electrodes. The downward temperature excursions in Fig. 1 were caused by additions of D_2O every 12 h, to make up for electrolysis loss in their thermodynamically open cell. With one such addition, the temperature rose unexpectedly and apparently spontaneously, the first derivative of temperature increased then increased again also apparently spontaneously, and the temperature of the electrolyte rose to boiling. These increases were not due to increasing cell voltage or resistance, and the increase in temperature could be explained only by the appearance of a new added heat source. Approximately four and a half days later, with another heavy water addition, this new heat source disappeared and the cell resumed its baseline trajectory.

Knowing the power required to maintain an elevated cell temperature, even eyeball integration of the positive



(a)



(b)

Figure 1. Average change of calorimetric temperature during steady phases [2].

heat excursion indicates a heat source larger than known or conceivable chemistry by several orders of magnitude. Normalized to the mass of palladium this suggested a nuclear-level, or at least new and very interesting, energy effect. That statement is still true so long as Fleischmann and Pons were not incompetent, delusional or mendacious. I knew them personally and knew them immediately to be expert scientists and experimentalists – in fact incomparably – as well as honest. With my inside knowledge, I knew that this was not a report of bad science or deliberate deceit. But with the ever-present, now-regretted, spirit of non-cooperation, it was not until I saw the Fleischmann Pons Heat Effect with my own eyes, in my own laboratory, that I knew with certainty that they were not deluded either. And this doubt was for a man whose technical prowess I knew and respected more than any other – so great was the stretch of their claim and the damnation distributed generously (if not completely honestly) by doubters and detractors.

Despite 27+ years, many thousands of person-years of work, and thousands of published papers the world today contains two significant groups: those who have seen the FPHE with their own eyes or who are closely associated with such individuals and therefore believe the effect to be real (potentially with real consequences); and those who have not and do not accept the claim.

The former group number in the hundreds or thousands – I count myself as one. We suffer the weakness of having communicated our results incompletely or inadequately. This failure is both accidental and deliberate. By far the worst paper Martin Fleischmann ever published was his first cold fusion paper [3]. The political pressures on Fleischmann and Pons and the University of Utah present in and leading up to 1989 make the technical insufficiencies of reference [3] somewhat understandable, even forgivable. But other factors already were at work in 1989 and remain at work today to suppress rather than fully reveal technical details sufficient for replication and rapid technological advance.

A very similar story pertains to the words and work of a man with whom I had spent more time (until that point) – the father of modern physical electrochemistry and experimental genius, John O'Mara Bockris shown below with his Electrochemistry Group at Imperial College London (Fig. 2) at that time – the very beginning of modern Physical Electrochemistry. There Bockris was advisor to both Martin Fleischman (indirectly) and John Tomlinson, my PhD supervisor at the University of Wellington – both are shown in this photo circled in the back row. Bockris was therefore my "academic grandfather". Incidentally, 1948 was the year Martin first published on the Palladium–Deuterium system, and the year of my birth. This work has been going on for a very long time.

When I first heard what Bockris rightly claimed (latterly), as the first nuclear evidence for cold fusion – the production of tritium – I was extremely skeptical. If tritium were being produced then where were the neutrons? Horror stories circulated about the leakiness of molecular tritium and the ubiquity of "Exit signs" and other potential sources containing huge amounts of this radioactive element – and poised to leak into unwary experiments. Again, I did not believe primarily because, by that time, we had made scores of tritium assays from Pd/D electrolysis cells – many with well-loaded cathodes – in which we had never seen any increase in tritium levels. Indeed, we observed the converse. Tritium appeared to be slightly reduced in the liquid phase after long-term Pd electrolysis experiments. If we did not observe tritium production in numerous well-performed experiments, then why did others?

There were also tales of tritium spiking and scandal concocted by Gary Taubes [4]. These allegations were stated as fact but never proven or technically supported, and were impossible to believe if one knew the individuals that were accused. But they were messy and complicating, and provided sufficient reason to stay away especially if one had no affirming results to contribute. But again, the case for tritium production had already been well made, this time by one of my old heroes, and two new ones.

The tritium results of 1990, first reported to the world at ICCF1 (in Salt Lake City, Utah) were remarkable in that they came to us already pre-replicated. Three groups with impeccable credibility reported, essentially simultaneously, the production of tritium from deuterium–metal systems: John Bockris' group at Texas A&M; Ed Storms and Carol Talcott at Los Alamos National Laboratory (LANL, New Mexico); Mahadeva Srinivasan, P.K. Iyengar and others working at Bhabha Atomic Research Centre, India (BARC, Trombay, India). Bockris I knew to be a superb and honest scientist, and a supreme experimentalist. Although I did not previously know Storms and Talcott, they were very

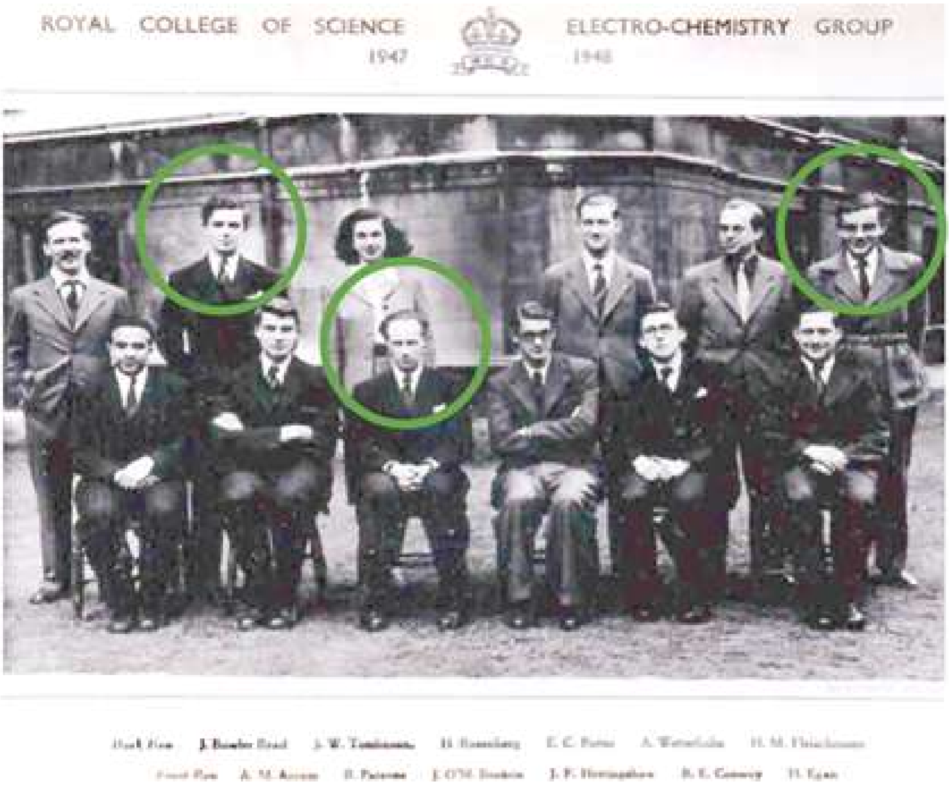


Figure 2. Bockris'group, Imperial College, London, 1947–1948.

impressive in person and Los Alamos had a solid reputation, particularly in the area of nuclear materials. Srinivasan and Iyengar were extremely capable individuals, occupying very senior positions at possibly the pre-eminent nuclear research institution in India – one of the best in the world. By its very nature, the evidence presented (tritium) was clearly of nuclear origin. Furthermore the options for doubt listed above for the Fleischmann and Pons Heat Effect (incompetence, delusion, deceit) clearly did not pertain to the tritium results: incompetence could be ruled out by the nature and skills of the individuals and the institutions they represented; delusion and mendacity could be eliminated because they had reproduced each other's observations by different means and in different experiments. Only collusion survives as a possible explanation, and it seems very clear that each was unaware of the other two groups' activities. To this day, I have not heard a single significant or pertinent criticism of any of these groups' activities or results and we must accept the (now) self-evident truth that tritium is produced *de novo* in deuterated metal gas and electrolytic experiments ... and that Bockris was right! This was the first nuclear evidence for cold fusion and it came to us early, already reproduced and – with the wisdom of hindsight – unarguably so.

Why was this not believed: (A) to be significant; (B) at all? The answer to (A) is easy: there was not enough tritium to explain the heat. Heat appeared when tritium did not, and (apparently) *vice versa*; neutrons, while possibly present at the margins of detectability, did not match the rates of tritium production. It should be noted that this last argument is bogus or at least unscientific since if cold fusion were real at all, it occurred by mechanism or mechanisms unknown and very clearly different from hot fusion, while the formation rates of the various products was (and is still) unpredicted.

The answer to (B) is, in my case at least, shameful. More charitably, perhaps, it was due to an excess of caution. But the evidence, when studied and properly assimilated, was clear; the experts and experiments were sound. The fact that tritium could be made at all in what came to be known as "low energy" situations was of vital and critical importance since it challenged what was (then) "known" (or believed) about the "incapacity" of electronic effects to couple to nuclear states and processes. If tritium was produced in an essentially chemical environment – and it clearly had been – it would require a very significant degree of rethinking about the fundamentals of nuclear physics. For many, perhaps most, this was too painful a step and the evidence would be more comfortably ignored. Scientifically, it did not matter that the tritium could not explain the excess heat. What was crucial for science was that tritium was produced at all! In fact, the production of tritium was not only the first solid evidence for cold fusion – it was the first modern evidence of condensed matter nuclear effects.

What did it take for me to believe that tritium atoms can be produced from D, or from D and H, in metals or condensed matter, and thence to believe in cold fusion? The unsurprising but also unacceptable answer is: "I needed to see it with my own yes, in my own laboratory"; in this case, in a completely different experiment. It was not until we (at SRI) observed tritium decay with Brian Clarke [5] by measuring the build-up of helium-3 in our replication of Professor Arata's seminal work with "Double-Structured Cathodes" – in the late 1990s – that I came to accept the tritium evidence as wholly true. I was only then able to incorporate this new fact into my understanding of the mystery that we were studying, and concentrate some effort on trying to comprehend its significance.

Someone said: "*A fool learns only from his own experience*". This is not how science should work, and not how we must make progress. We need to communicate, evaluate, and learn to trust; the literature must be read, understood and respected. Very sadly, the helium-4 story is essentially the same as for tritium. Perhaps even worse, since helium-4 production is claimed to be very closely commensurate with the excess heat, and there is no reason to anticipate the evolution of neutrons (that are not observed), if the primary heat-producing process is, albeit indirectly, $D + D => {}^4He + 24$ MeV (lattice). By way of mitigation, the technical difficulties associated with the measurement of helium-4 are very different from those of helium-3 or hydrogen-3 (tritium). Because of its nuclear stability, Helium-4 is relatively easily measured and resolved from molecular species nominally of the same mass. However, for this very reason, helium-4 is present in the ambient at high levels (5.22 ppm), larger than the expected production in most cold fusion experiments. The primary experimental problem therefore is background interference and suppression.

Knowing this, Melvin Miles designed and implemented an extremely clever deuterium–palladium electrochemical experiment [6], whereby the evolved electrolysis gases, deuterium and oxygen, were used to purge and scavenge helium from his system. Mel's results, reported at ICCF-2 in 1991 in Como, Italy, were stunning. When excess heat was present in the electrochemical experiment, helium-4 appeared approximately commensurately in the evolved electrolysis gas. When excess heat was not present, neither was helium-4. Finally, we had a product that made sense thermodynamically and explained the absence of radiation. This sense was not shared by those with a hot fusion mindset, but if the correlation reported by Miles was real, it proved cold fusion was also, and gave us insight into mechanism. This was a fact that even John Huizenga recognized in his 1992 book [7]. John was counting on non-confirmation – in this, as in other important matters, Huizenga was wrong.

So what was the problem? Why was the significance of Mel Miles' result not well understood or believed at the time? The vessels that Mel used for calorimetry were not helium leak-tight and there was much discussion about the likelihood of helium in-leakage causing or contaminating the result. It took some thinking and calculation to recognize the elegance of the approach that Mel had devised. But most of all, it took the independent replication that Huizenga was counting out, and this all took time. One of the early replicators was SRI so I am quite familiar with the experimental parameters, limitations and consequences of this experiment. In his review of helium-4 and heat correlations in 2015, Abd ul-Rahman Lomax [8] states: "*Miles was amply confirmed, and precision has increased.*

While there are outliers, there is no experimental evidence contradicting the correlation, and only the exact ratio remains in question. In this, we have direct evidence that the effect is real and is nuclear in nature; the mechanism remains a mystery well worth exploration". For an experimental result of earth shattering importance, first reported publicly in 1992, why did it take until 2015, 23 years, for this conclusion to be stated with such clarity and conviction? And even now not every researcher in the CMNS world would agree that helium-4 is the primary product, or even a nuclear one!

3. Redemption?

What is wrong with this situation, or with us? Is it that experiments are difficult to reproduce, contain uncontrolled critical variables, or lack theory? I have often argued that theory, if not absent in this field, is certainly lagging, but this is not a failure by theorists to be imaginative, productive or rigorous – most are. Instead, I would argue that this is another failure of experimentalists to fully and clearly communicate our results. This failure has denied us the full use of scientific method wherein theory-derived hypotheses are tested experimentally. But absence of theory is not, I believe, a primary or legitimate reason for our failure to advance or convince. I have had very good and productive interactions with enormously capable theorists who became friends. Primarily and initially these included: Giuliano Preparata; Peter Hagelstein; Talbott and Scott Chubb; Akito Takahashi.

Theory is important, in fact crucial, and it will come into its own when we do our experiments better and communicate our results more completely and effectively. But it was the support and interest of two giants in the early days who gave us intellectual cover that maintains today despite their deaths: Julian Schwinger and Edward Teller. Both had a sustaining interest in cold fusion from the very earliest days. If individuals of this caliber believed that solid state, or coherent effects, might modify the trajectory of nuclear interactions sufficiently to generate the heat, tritium, and (later) helium-4 effects, who was I to doubt this possibility? Who was anybody? So the argument that cold fusion was theoretically denied – made mostly by "not-anybodies" – was never serious or important, even though it has had serious and disruptive consequences.

It would take a very long time to mention everyone who has taught me in this field – and how. My principal debts of gratitude for their work and influence are to Martin Fleischmann, of course, being my initial and greatest influence in this field, and to my SRI colleagues – first and most enduring Fran Tanzella, and to Tom Passell – my EPRI Program Manager from 1978 until 1995. I have had and continue to have an extraordinarily fruitful and joyful collaboration with two good friends, Peter Hagelstein and Vittorio Violante – both have done what I could not and taught me about things and places I would never have dreamed exploring. The two last I would like to mention are Ed Storms and Irv Dardik. When each of them see these words they will be annoyed with me for not having mentioned them in my list of "personally influential theorists". But each has influenced me profoundly – Ed with his huge body of work and synthesis of understanding – Irv with his focus on the wider significance and power of multi-frequency resonant effects – a largely untapped field of research that greatly benefited the work at SRI, Energetics and ENEA.

4. Where do We Stand?

I would like to conclude with a personal message and a grand challenge. The only place I ever worked was SRI – for 37 years since emigrating to the USA from my native New Zealand *via* postdoctoral studies in England. As many of you know, I retired from SRI in 2016 and returned to New Zealand to be closer to my family. After nearly four decades of contract research for others, I am now a free man (or more nearly so) with significant degrees of freedom.

But my work in the field of CMNS – our work – is not complete. After more than 27 years I still do not know the basic cause of the effect we are studying. I would like to – I would like us all to. To do so in a rational and timely manner, I believe that we need to collaborate and cooperate more effectively as a community than we have. All I can do is exhort. But I think it should and must be done, and I would like to encourage discussion about how we might begin to treat our individual and shared observations of condensed matter nuclear effects as a potentially real solution to the greatest pending problem of humanity, rather than attempting to co-opt it for personal advantage.

Here is a summary of my major observations.

(1) Collectively we have the answer, individually none of us does! We have to stop trying to be heroes and begin playing together as a team. I continue to believe that this is a game worth winning – and that it can be won - and that we can do this if we change the way we approach the solution. I have contributed 27 years to this task – I can give a few more – so long as I see a reasonable trajectory towards resolution.

(2) As a community and individually, we did not pay sufficient attention to the significance of the work of others. Personally, I took a long time to be persuaded by the evidence of: Heat; Tritium; Helium – and only came to accept these as real when I saw evidence with my own eyes and under my experimental control. For whatever reasons, these evidences now fail to persuade, and fresh replications of the best experiments are necessary. We must do the work ourselves.

(3) We need fresh data. We cannot keep pointing out 20-year-old results as the best evidence of an effect.

(4) As a consequence we have limited shared experience. We have not paid sufficient attention to the teaching that does exist, and there exists no consensus around an agreed set of facts. Everyone has a different set of facts. Everyone is taking their own journey. We are not standing on the shoulders of giants, we are standing shoulder to shoulder in the (metaphorical) muck, diving for (real) coins.

(5) The giants that I spoke of before started the job, but they did not complete it. If the likes of: Martin Fleischmann; John Bockris; Fritz Will; Giuliano Preparata; Andrei Lipson, Julian Schwinger – the best I ever knew – could not complete the task that Martin set us, what hope do we have working alone? We must work together!

(6) Despite the negativity expressed both inside and outside our community, there is intense and increasing interest in our field. With the "statute of limitations" expiring on our "glorious transgression" of questioning "established expectation" 27 years ago – talented, young, fresh thinking, imaginative and energetic researchers are entering the field in significant numbers. 30 year-olds do not care what happened 27 years ago – 40 year-olds do not either. These people are our future. Let them learn and do, and watch and help them win.

(7) Our field is not limited by money or constrained by government. Essentially every successful businessman and entrepreneur on the planet is aware that securing a benign primary energy source that is unlimited in power (supply), energy (duration) and availability (location) – is one of the greatest existential planetary questions over which we have any control. Many or most are keen to help. Our job is to make it easy for them. "*Fund me and my idea*" is not helping them or us.

(8) The results we are seeking will benefit all mankind. Likewise the team of talent and consensus needs also to be multi-national. The host nations of ICCF-20 and its satellite, Japan and China, have made significant and conspicuous progress towards fulfilling the potential of Martin's dream, for which we are all extremely grateful. So have the Europeans (especially Russia, Italy, France), and the North Americans (particularly the USA). These groups and nations need to continue to work together – perhaps even better – and we need to expand "the club".

(9) We are presently at the level of building science not technology. In this regime there is no need – or excuse – for competitive instincts or actions. Just as cold fusion or LENR must be a coherent process, so must its solution.

5. Grand Challenge

Now allow me to pose a "call to action", what others have called a "Grand Challenge". I posit that our most urgent goals as a community are to:

- Produce fresh experimental results of non-chemical anomalies.
- Replicate these in multiple laboratories.
- Communicate results clearly via technical articles and presentations.

We all know the level of scrutiny this will face, and we must prepare accordingly. As a community, and to make progress on what I (and our funder friends) consider to be a "reasonable timeline", we need to identify the best experiments, rally around them, replicate them, and publish the results. With science established, technology will flow. Although as yet undecided, ICCF21 will occur most probably in the USA, likely in early 2018. If the organizers will allow me, I propose there and then to report on performance against the challenge to:

- Identify what we consider to the best experiments (three or four).
- Recruit multiple laboratories to work on them.
- Write clear scientific papers including multiple authors from the multiple labs. Let us do our own peer review first.
- Publish these papers in our own Journal of Condensed Matter Nuclear Science (JCMNS) or other peer reviewed scientific journal.
- Present the work at ICCF21 in a special session having focus on these replications.

My singular goal, which I invite you to share is to understand for certain the basic cause of the effect we are studying and have assembled here to discuss – by 2018. All I can offer is my help and a threat. In Padua at ICCF19, I delivered a lecture discussing what I believed to be the need and the time for the senior CMNS generation to teach the next. Some listened. Preparing here for ICCF20, my thoughts have circulated around our need to learn what has already been taught, to collaborate broadly and without ego, and to achieve consensus on a limited set of critical experiments that can be performed soon and – if performed successfully – would make a difference. If you accept the proposal of the Grand Challenge, then we have 18 months to produce results that will be evaluated at ICCF21 and illuminated for the world to see!

Do we accept the opportunity given to us – which is huge and critical for mankind? Or do we fail in what I believe is the greatest challenge and opportunity we as scientists will ever face. I promise you, I will be at ICCF21 to help you all to make that call. If by that time we have not coalesced as a community to take full advantage of our collective wisdom then I suggest that the organizers and Chair(s) of ICCF21 convene a panel and discussion on that occasion to identify a best path forward. Our time is limited and the problems that we hope to solve are increasing as our global energy resources diminish and by-product "ashes" increase in the environment. It is time for community action.

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