A Policy Argument for a Rational Approach to Cold Fusion Research

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Insufficient Progress

Cold fusion research has not progressed at a rate that is warranted by the significance of the developments that Martin Fleischmann and Stanley Pons revealed to the world in 1989, and the extensive research since then. That slow rate of progress is also inconsistent with the global need for a new source of clean energy.

We just recognized the 30th anniversary of the announcement of their work without having answered the two most basic questions in a way that is sufficient for the broader scientific community to be supportive of further investigation. Are the observations of excess heat real? If so, can the phenomenon be made useful? These questions need definitive answers.

A responsible analysis of the history of cold fusion research calls for developing an understanding of why the field has experienced such inadequate progress. The history is complex, and a full analysis would be beyond the scope of this article. However, developing the critical points of such an understanding can inform the path forward. This article considers a few aspects of cold fusion history and other historical examples as part of a policy argument for a prompt and conclusive research effort.

Looking for a Breakthrough

Human civilization has made breakthroughs at crucial moments in its history. One modern example is the development of the Haber-Bosch process that creates ammonia from atmospheric nitrogen to produce nitrogen fertilizer. This development has allowed humankind to avert famine on a massive scale. The consequence of this development can be found in the assessment that as much as half of the nitrogen in human tissues has its origin in the Haber-Bosch process.

In the case of cold fusion, rather than pooh-poohing the idea of a breathtaking opportunity that does not match the conventional understanding of our natural world, we should ask, "What if a possibility such as cold fusion was not revealed? What if science presented us with no possibilities?" As we think about the challenges that advancing and commercializing cold fusion could help overcome, such as climate change, it could be useful to imagine a scenario where human civilization faced an existential threat, but science offered no options. Imagining such a situation, that is well within the bounds of possibility, might give rise to greater openness and appreciation for controversial ideas such as that of cold fusion.

Professor of philosophy at Cambridge University Huw Price says:

> Missing a new source of carbon-free energy might well be catastrophic at this point, and that makes it prudent to investigate even low probability options...

His remarks appeared in the *Financial Times* article commenting about the Google team Perspective on cold fusion, which recently appeared in *Nature*.

Price's comments reinforce the idea of having a focused effort to beat on the subject for answers to its two big questions. This effort could include approaches such as exhaustively traversing the identifiable parameter space, surveying the historical literature for what could be worthwhile entry points and utilizing machine intelligence to analyze and evaluate the literature and experimental data to provide managers with actionable analytics.

The goal of such an endeavor would be to accomplish either a definitive demonstration of excess heat or transmutation, or in the alternative, a reasonable consensus that the effort has left no significant missed opportunity. At the successful conclusion of such an effort, even if the cold fusion phenomena were found to be the result of systemic errors in experimental observations or calculations, the work would nonetheless have been worthwhile.

Accomplishing such an undertaking takes funding commensurate with (a) the potential of this objective and (b) the level of urgency that is warranted to realize it.

Missing Factors in Cold Fusion Research: Urgency and Cooperation

Two of the most significant scientific and technological breakthroughs in the 20th century were driven by urgency and realized by cooperation. The first is the Manhattan Project that led to the development of the atomic bomb, and the second is the human spaceflight program that resulted in the U.S. moon landing by the end of that decade. Both were unprecedented human endeavors, conducted with urgency, which relied on large scale cooperation.

Yuval Harari said on PBS' "Amanpour & Company" (October 4, 2018):

> We are the only social mammals that can cooperate in very, very large numbers and in flexible ways and this is the secret of our success, very simply. It is not something on the individual level, it's the collective level. If you look at any large scale human achievement, whether it is flying to the moon or splitting the atom, or building the pyramids, this is the result of large scale cooperation. And we are the only mammals that can cooperate on a very large scale because we are the only ones that can create and believe in fictional stories.

Urgency and large-scale cooperation have not been significant components of cold fusion research. They should be. To date, systematic research in cold fusion has been minimal in terms of both scale and duration. Fortunately, however, we have the aggregate body of research that has been contributed by hundreds of scientists in dozens of countries over three decades that can act as trail blazes for a second wave.

In terms of urgency, in 1989, the impact of climate change was mostly prospective, and urgency did not play a meaningful role in investigating the effect. Now, thirty years later, the urgency of climate change is much higher, and drives us down the path we should have earlier taken.

Absent a Leap of Imagination

Soon after the U.S. made its decision to undertake the Manhattan Project, the first human-made nuclear chain reaction took place at the University of Chicago. The Chicago Pile-1 (CP-1), a developmental nuclear reactor constructed and operated by Enrico Fermi and his team, went critical in an experiment they conducted on December 12, 1942. It ran for 4.5 minutes at about 0.5 watts. Further testing was mostly at 0.5 watts.

The first full-scale nuclear reactor after the CP-1, Hanford B, was designed to operate at 250,000,000 watts (250 MW) thermal, a power level over 250 million times that of Fermi's test reactor. Construction of Hanford B began only four months after CP-1 went critical, and its construction was complete 18 months later. Hanford B was later operated at levels above 2000 MW (over two billion times that of Fermi's test reactor) with the only major modification being an increase in its cooling water capacity. Both its design and construction represented a breathtaking leap of imagination. A select chronology for the Manhattan Project is in Table 1.

The developmental history from the CP-1 to the Hanford B represents an example of the possibility of human cooperation coupled with the urgency to overcome an immense scientific and industrial challenge.

Table 1. Hanford site chronology.

Date	Months	Event
Dec. 2, 1942	0	First sustained nuclear chain reaction with the Chicago Pile-1.
Jan. 16, 1943	1.5	Maj. Gen. Leslie Groves selects Hanford site for Pu production.
March 1943	2	Construction begins at Hanford.
Sept. 26, 1944	18	100-B Reactor goes critical. Solve Xe-135 poisoning.
Dec. 26, 1944	2	Startup of T Plant, the first chemical separation plant.
Feb. 2, 1945	1	Los Alamos receives its first plutonium from Hanford.
July 16, 1945	6.5	Trinity test, the first nuclear explosion.
Total	31	

An Economic Argument for Sufficient Cold Fusion Research

For a thought experiment, an estimate of the potential value of energy provided by cold fusion, should it become a ubiquitous source of heat and electricity, can be conservatively set at ~$2 trillion per year worldwide. This value represents one-quarter of the total global usage of energy.

Commensurately, the estimate of the cost of a research program to definitively answer the two big questions of cold fusion—whether the phenomena of cold fusion is real, and if it can be made useful—can be liberally estimated to be $1 billion.

Pursuing a breakthrough requires taking on risk. In science and mathematics, Monte Carlo simulations are often used to model the probability of different outcomes. This thought experiment is going to consider the risk versus reward in the same fashion a professional bettor would evaluate a wager, but without recourse to Monte Carlo or other computations.

The first consideration is to define what it means to win the bet. To win is to receive an economic value of $2 trillion of energy a year from cold fusion for some defined term. The timescale of the return for this analysis shall be set at ten years to err on the side of moderation. Thus, to win the bet equals $2 trillion per year over ten years or $20 trillion of energy value. The wager that must be placed to participate is $1 billion to fund a systematic, urgent and focused research program.

The breakeven calculation is made more understandable by equating the 20 trillion to 20,000 billion. Therefore, to break even on this bet, the probability that cold fusion is a real phenomenon and can be made useful need only be 1/20,000 (a billion dollar bet and a 20,000 billion return). So, any outcome with a likelihood better than 0.00005 or 0.005% or five-thousandths of 1%, and the bet is won.

To put the wager in simpler round terms, if the odds that cold fusion is real and can be made useful as a ubiquitous source of energy is only 1%, then the contemplated $1 billion bet becomes an absolute "no-brainer." A professional bettor with adequate capital would make this bet all day long, and not give it a second thought if they lost any particular instance. The return on the wager at a 1% probability of success would be 200 to 1.

If the probability that the observations of excess heat in the cold fusion experiments are real is 60% or 70%, a figure many of the field's experimentalists would consider a modest estimate, and if the likelihood the effect could be made useful is 50%, the bettors case for making the $1 billion investment on such a research program is more than sufficient from a policy standpoint analysis. The return, in this case, would be more than 6000 to 1.

Despite the billions and trillions of economic value being contemplated, which represent vast and intimidating numbers, the association between the risk and reward remains relative. The laws of probability continue to operate even at this large scale.

It should be noted that the risk versus return analysis for this thought experiment does not consider the value of scientific developments that may arise that are ancillary to the primary purpose of the research effort. Nor does it consider prospective economic benefit of ameliorating climate change that might result from cold fusion successfully displacing a significant portion of the use of fossil fuels for energy generation. This value, translated into economic terms, could eclipse that of energy.

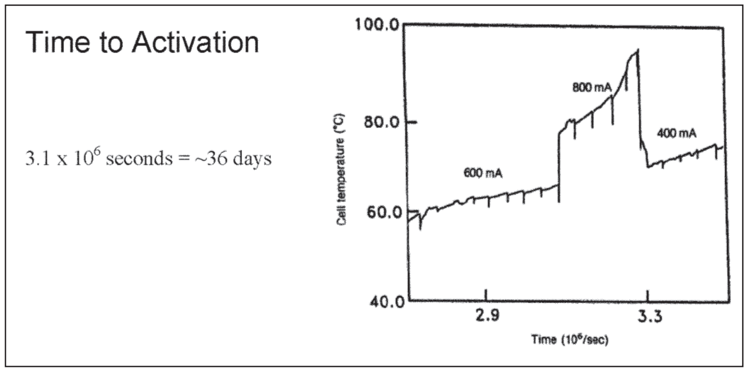

Figure 1. Early excess temperature excursion (Pons & Fleischmann).[1]

Dramatic Differences in Palladium from Different Sources

Miles found that palladium from different sources has dramatically different performance. Cathode material is the most important variable in these experiments. Here is a summary of Table 10 :

Source	Success Ratio (excess heat / total tests)	
NRL Pd-B alloy	7/8	8/9 NHE Japan
Johnson-Matthey (J-M) Pd	13/24	9/10 Ridgecrest,CA
J-M from Fleischmann	4/4	
NRL Pd (first batch)	1/2	
Tanaka Pd (sheet)	1/3	
NRL Pd (another batch)	0/4	
NRL Pd-Ag	0/3	
IMRA Japan Pd-Ag	0/2	
WESTGO Pd	0/6	
Pd/Cu	0/2	
John Dash Pd (sheet)	0/2	
Co-deposition (1992)	2/34	
Total:	28/94	

Figure 2. Palladium from different sources. Based on a table by Jed Rothwell,[2] extrapolated from Miles,[3] with an addition by the author.

Rate of Learning (An Approach to an Accelerated Research Program)

The correlation between the progress in cold fusion research and the duration of the Pd/D_2O electrolytic system experiments, which were used for a significant portion of cold fusion research to date, may be underappreciated. A typical Pd/D_2O cold fusion electrolytic experiment runs for 30 days or more. Understanding the link between the timescale of month-long experiments with low replicability and the current status of research, after 30 years of work by a community of researchers, could help provide insight into designing more efficient and productive pathways for future progress.

In modern commercial semiconductor process development, the systematic and incremental approach employed for solving challenging materials and process problems by companies producing these complex and sophisticated components is driven by the idea of Rate of Learning (ROL):

$$ROL = \Delta K / \Delta T$$
$$K = \text{Knowledge},\ T = \text{Time}$$

Distilled down to its basics, the idea is to achieve the highest possible ROL by maximizing ΔK and minimizing ΔT. Examples:

Baseline	Knowledge = 100	Time = 20	ROL = 5	1 X
	Knowledge = 100	Time = 10↓	ROL = 10	2 X
	Knowledge = 200↑	Time = 20	ROL = 10	2 X
	Knowledge = 200↑	Time = 10↓	ROL = 20	4 X

Among the examples above, we can see that by doubling knowledge and halving time, ROL is increased by 4X.

This construct can be the basis for another thought experiment that explores the idea of ROL applied to historical and prospective cold fusion research. The thought experiment shall analyze three core components of electrolytic cold fusion research: 1) Reproducibility, 2) Time to Activation and 3) Parallel Experiments.

Archetypal electrolytic cold fusion experiments using Pd cathodes are conducted for 30 days or more. The most recognizable graph from Fleischmann and Pons experiments demonstrated a heat burst after 3.1 x 10^6 seconds or ~36 days (see Figure 1).[1]

The author is a member of the LEAP program team, an experimental program being conducted by LENRIA that is based in David Nagel's LENR lab at George Washington University, with the support of the Anthropocene Institute. We are working to replicate the palladium-boron experiments of Dr. Melvin Miles of the Naval Air Warfare Center, China Lake, that utilized PdB alloys created by Dr. Ashraf Imam of the Naval Research Lab, Washington, D.C.

In Mel Miles' PdB experiments at China Lake, at NHE Japan as a visiting scientist and in a later experiment at Ridgecrest, California, excess heat was observed in nine out of ten cases (in the one non-working cathode, visible physical defects were discernible). This represents the highest known reproducibility rate in Pd or Pd alloy electrolytic cold fusion experiments, and shall be used here as a reproducibility example in this exercise. See Figure 2.[2,3] Some PdB experiments exhibited an early excess heat signal as early as 1-3 days.

Many of the experimentalists who pursued the $Pd\text{-}D_2O$ electrolytic experiments chose not to, or were unable to conduct multiple simultaneous experiments. The increased costs of such experiments often were beyond their budgets. However, conducting parallel experiments can be very conducive for maximizing ROL. The running of multiple simultaneous experiments mitigates time when experiencing low reproducibility. It also allows the faster exploration of variations in materials or of an experimental arrangement or protocol. An urgent, well-funded systematic research program

Table 2. Historical parameters compared to prospective parameters.

Parameter	Historical	Prospective	Improvement
Time to Activation	30+ days	1 - 3 days	10X
Reproducibility	1 of 10	9 of 10	9X
Parallel Experiments	1	10	10X
			Total 900X

may have dozens or hundreds of experimentalists, and hundreds of experiments being conducted at any one time. For the purpose of this thought experiment, ten parallel experiments shall be chosen as our baseline.

In Table 2, typical historical parameters of Time to Activation, Reproducibility and Parallel Experiments are compared to prospective parameters.

As we increase the performance of each individual parameter, such as Time to Activation, Reproducibility and Parallel Experiments, we experience the result, conceptually, not as the sum of their respective increases of performance, but rather, as the multiplication of them. The potential Rate of Learning improvement is conspicuous.

A question this thought experiment asks, but cannot answer, is whether having available an experiment that reduces the Time to Activation from 30+ days to 1-3 days could reduce the time that was necessary to achieve 30 years of research to three years? Similarly, if researchers were able to replicate the original form of cold fusion experiments nine out of ten times, rather than less than one out of ten, by focusing on the most promising materials and techniques, could this have increased progress by a factor of nine? Had the funding and urgency been present to enable and execute a systematic and persistent approach, and if each researcher or team had simultaneously ran ten parallel iterative experiments, rather than one, could our understanding of these materials and systems be ten times further along than they are now? Importantly, what might be the aggregate effect of the combination of these factors?

If we had accelerated our learning, might we have by now discovered that Pons and Fleischmann's original experiments contained some type of unexpected endemic error in observation or calculation, or conversely, would we now have an accepted theory and significant expertise in developing energy and other systems based on the acquired knowledge? Would we already have a grasp of the tools that we shall use to redress the unnatural accumulation of carbon dioxide in our atmosphere?

The acceleration of advancement of learning suggested by this thought experiment could probably not be fully realized. There is also acknowledgment that the approach of the thought experiment is imperfect. Nonetheless, the opportunity to revise our approach to cold fusion research and rapidly increase the rate of its advancement exists. The above scenarios do not take into account the increased experimental throughput that could be achieved by being able to abandon non-working experiments earlier because of shorter expected activation times (and the ability to start new ones). Also, they do not consider the knowledge obtained from systematically characterizing vastly greater numbers of materials and systems of non-working and working experiments. The resulting data warehouse containing these results could represent a game changing resource that we have not quantified here.

The Path Forward

An argument has been posed for an urgent, cooperative, accelerated and economically rational program to determine whether the phenomenon of cold fusion revealed by Martin Fleischmann and Stanley Pons in 1989 is a real effect, and whether it can be made useful. The conclusion of this article is that an organized, systematic, sustained, focused, managed, well-funded cold fusion research program is long overdue and should begin at the earliest possible time.

Pressures on human civilization such as climate change, the growth of per capita energy usage, deforestation, access to fresh water, population growth and other challenges are problems that a low cost, non-polluting energy source such as cold fusion, if it were successful, could help mitigate.

Policymakers should be vigilant not to miss an opportunity that could offer a breakthrough that comes just in time. This is true even if it presents itself in a form that Huw Price has coined "low probability options," that in actuality might have a much higher probability. Yuval Harari informs us that attaining the pinnacle of complex human achievement relies on our ability to work collectively and in flexible ways, and is possible only because we can create and believe in fictional stories. And, history offers the insight that urgent scientific and industrial challenges such as splitting the atom and flying to the moon have in no small part been defined by breathtaking leaps of imagination.

We must ask ourselves: are we prepared, as a nation, or even more broadly as a civilization, to place an uncertain but well-reasoned bet, that is the equivalent economic value of two AirBus A380 jetliners, for the possibility of a breakthrough that could define the next period of human development and pay off in time to avoid irreversible damage to our biosphere? It is time for policymakers to approve and fund a rational cold fusion research program.

In the next article, David Nagel envisions potential near-term actions for the overall advancement of cold fusion research.

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