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.

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 bet-

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	

tors 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.

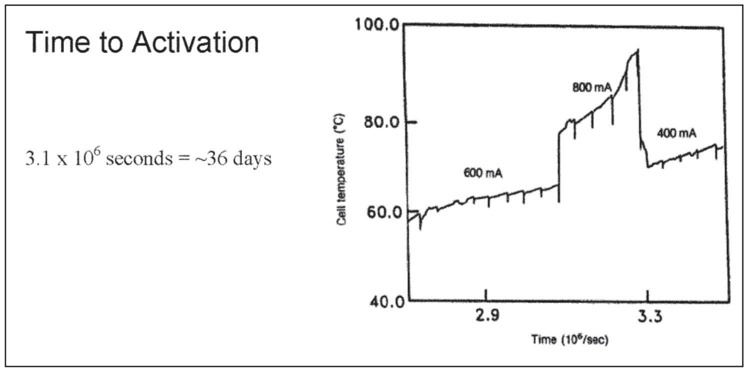

Figure 1. Early excess temperature excursion (Pons & Fleischmann).[1]

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 3.1×10^6 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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Near-Term Possibilities for Advancement of LENR

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1. Introduction

The previous article by Katinsky makes the larger economic and societal case for funding of Cold Fusion, or Low Energy Nuclear Reactions (LENR), as part of the fight against diverse damages caused by climate change. That argument might succeed in motivating funding for scientific understanding and commercial exploitation of LENR. If it does, there remain the questions about what to do initially to advance both the understanding of LENR and its widespread practical use. This article discusses potential near-term actions for the overall advancement of LENR. It deals with three actions. The first is what might be done to engender awareness and acceptance of LENR by the scientific community as a legitimate field of scientific inquiry, funding of LENR research by U.S. government agencies and awareness of the field by the general public, especially those concerned with reducing carbon in the atmosphere. The second topic is a potential study of LENR by the U.S. National Academy of Sciences. The last subject is an outline of potential U.S. programs for the understanding, exploitation and utilization of LENR generators. This article is U.S. centric due to two reasons—the author's familiarity with the U.S. system and the size of that system. Acceptance and funding of LENR in the U.S. ought to have a beneficial effect on activities in other countries.

2. Recognition and Funding

Understanding and exploitation are the two most important goals of research and development on LENR. However, both are impeded by the lack of recognition of the field as a legitimate arena for intellectual inquiry, and the lack of support that is a consequence of that shortfall. Hence, the initial effort must be aimed at getting the field recognized and funded. There are a few ways in which that would happen. They are listed in Table 1. The first three possibilities are similar in that they could happen at any time. However, none of them is under the control of people interested in the advancement of LENR. A replication of the 1985 meltdown experiment in the Fleischmann-Pons laboratory with modern instrumentation would attract a lot of attention. However, we have not found a funding source to attempt that replication. The appearance in stores or on the internet of a power generator based on LENR would also get much attention. The product announced recently by Rossi of Leonardo Corporation is not for sale. However, the use of it for production of warm air can be arranged. It is too early to know if there have been any users of the Rossi system, and what is their quantitative experience. The last means to achieve recognition and funding for LENR is the LENRIA Experiment and Analysis Program (LEAP), now in progress with funding from the Anthropocene Institute. The effectiveness of Phase I of that program will be known late in 2019, but the entire program cannot be completed before late 2020.

Table 1. Possibilities for recognition and funding of LENR.

Possibility	When?	Control?
Clear and Accepted Theoretical Explanation	Could be anytime	None
Strong Demonstration Widely Viewed	Could be anytime	None
Strong Public Backing by Billionaire	Could be anytime	None
Replication of the 1985 Meltdown Experiment	Need two years	None. No funding.
LENR Power Generator on the Market	Possibly Leonardo	Little, if any
Multi-Laboratory Replication Experiment	Need two years	In Progress

3. Potential Study by the U.S. National Academy of Sciences (NAS)

Currently, committees of the Congress will not hold hearings on LENR until it has been recognized by the scientific community in the U.S. That recognition could follow from the results of a study of the topic by the NAS. Similarly, government agencies with responsibilities for and related to energy could be influenced by the outcome of a NAS study that thoroughly investigated the voluminous evidence that LENR is a real and promising source of clean energy. Both of these factors are based on empirical information.

In the past, the NAS would do two types of studies. The first is relatively short (a few months) and required about $200K of government agency money. The second was much longer (about two years) and cost roughly $2M, again government agency money. Both were done by an organization built for the purpose, which included appropriate scientific and other personnel. Many of the reports that have resulted from the longer studies are available.[1]

Once the funding is available for a NAS study, there are two initial steps. The first is designation of a chairman for the study, and formation of a committee of experts from the needed disciplines. In the case of LENR, those disciplines would include, at least, solid-state and nuclear physics, electrochemistry, material science, electrical engineering, measurement science and data analysis. The choice of participants is critical, of course. It is best done in a collegial effort by the sponsor of the study and the NAS. Members of the NAS often participate in such studies.

The second early step is development of tasking from the sponsor. It states the desired activities and outcomes. So, an early step in forming a study on the status and promise of LENR would be to provide such tasking. A draft tasking follows. The first two tasks look backward to establish the reality, activities, status and promise of LENR. The next three tasks are forward looking. They deal with potential government programs in the U.S. The final task regards documentation.

Assess the Experimental Reality of LENR

- Review available reports and patents regarding the results of LENR experiments.
- Discuss the status of LENR with scientific and other leaders in the field.
- Summarize and critique the evidence for the production of nuclear products.
- Summarize and critique the evidence for the production of thermal energy.

Review and Summarize Global Efforts to Understand and Exploit LENR

- Review the proceedings of international conferences on LENR.
- Review the national LENR meetings in China, France, Italy, Japan and Russia.
- Summarize and critique current experimental and theoretical research on LENR.
- Summarize and critique current efforts on commercialization of LENR.

Develop and Recommend a National Scientific Research Program to Understand LENR

- Consider the Development of New Instrumentation, Use of the National Synchrotron and Other Facilities, Conduct of Electrochemical Experiments, Conduct of Hot Gas Experiments, Conduct of Plasma Experiments, Conduct of Other Experiments, Material Science and Technology, Data Analysis and Mining, Theoretical Developments and Numerical Simulations.
- Examine the possibility of the National Science Foundation (NSF) leading this program.

Develop and Recommend a National Program for Pre-Competitive Commercialization of LENR

- Consider the Development of Prototypes based on Electrochemical Experiments, Development of Prototypes based on Hot Gas Experiments, Development of Prototypes based on Plasma Experiments, Development of LENR Fuels, Development of Control Systems and Technology-to-Market Projects.
- Examine the possibility of Advanced Research Projects Agency for Energy (ARPA-E) managing this program.

Develop and Recommend a Department of Defense Program for Military Utilization of LENR

- Consider design, testing and production of transportable thermal generators at levels of 10 kW (under 2 m^3) and 100 kW (under 5 m^3), and transportable electrical generators at levels of 5 kW (under 2 m^3) and 50 kW (under 5 m^3).
- Examine the possibility of the Defense Advanced Research Projects Agency (DARPA) managing this program.

Document All Activities and Conclusions Regarding Each of the Above Topics

A study that included these activities and outcomes would involve two major types of activities on the part of the committee members. One would be study of the voluminous published material on LENR. The appendix to this paper contains a listing of most of the major sources of information on LENR. The second committee activity would be to talk to experts on LENR. That could be done by scheduling presentations or in an interview format. The experts could come to the committee, or interact via the internet. It is likely that the committee members would want to visit some active LENR laboratories in the U.S. and, ideally, also abroad.

The impact of NAS studies varies widely. The effects of such studies depend on their content and recommendations, as well as on technological, economic and political factors. So, it is not possible to predict the impact of a LENR study with high confidence. However, it is clear that the world needs sources of clean energy for two major reasons. They are the growing global population, and the increasing *per capita* use of energy as countries develop. Given this need, and growing global concern about the effects of burning fossil fuels on the climate, a NAS study on LENR could get significant and favorable attention in the U.S. and beyond. This last statement indicates that this author believes that the evidence for the reality and promise of LENR is very strong, despite the current lack of understanding and the several challenges of commercializing LENR generators.

4. U.S. National Program on LENR

Whether or not the recommended NAS study were conducted, it is possible to contemplate the organization and activities of programs aimed at scientific understanding, commercial exploitation and military utilization of LENR. Military utilization is included because the U.S. Department of Defense spends enormous amounts of money on energy. Provision of heat and electricity for forward operations and bases is particularly costly. Relatively small and mobile LENR generators, free of the fossil fuel "logistics tail," would be a great advance for the Army and Marine Corps. A national research and development program is outlined and discussed in the rest of this section. It is summarized in Table 2.

The NSF program would have components for the following topics:

Development of New Instrumentation
Use of the National Synchrotron and Other Facilities
Conduct of Electrochemical Experiments
Conduct of Hot Gas Experiments
Conduct of Plasma Experiments
Conduct of Other Experiments
Material Science and Technology
Data Analysis and Mining
Theoretical Developments
Numerical Simulations

The ARPA-E Program would have components on the following topics:

Development of Prototypes based on Electrochemical Experiments
Development of Prototypes based on Hot Gas Experiments
Development of Prototypes based on Plasma Experiments
Development of LENR Fuels
Development of Control Systems
Technology-to-Market Projects

The DARPA Program would have components on the following topics:

Production of a Transportable 10 kW (thermal) Generator Module under 2 m^3.
Production of a Transportable 5 kW (electrical) Generator Module under 2 m^3.
Production of a Transportable 100 kW (thermal) Generator System under 5 m^3.
Production of a Transportable 50 kW (electrical) Generator Module under 5 m^3.

This national R&D program on LENR would follow naturally from the NAS study discussed in the last section. However, as already noted, the program could be organized and funded independent of the NAS study. The latter approach would take fewer years.

The program sketched above would fit the missions of the three lead agencies. It would also recognize the realities of organization, funding and management of R&D programs by the U.S. government. It would not be possible to have a single lead organization, what some people call a "czar," for the overall program. That was done for the Manhattan Project during World War II because of the urgency of the situation. Now, even though climate change requires action, the commercialization of LENR would not, by itself, solve the problem of CO_2 in the atmosphere. Put another way, there is not likely to be the national will for a LENR program similar to the Manhattan Project in the very near future. However, if the growing evidence for the effects of global warming becomes (a) widely known and (b) viewed as a crisis, it is conceivable that an immense national project would follow. The negative effects from the massive burning of fossil fuels include increases in sea level, droughts, wildfires, floods, hurricanes and tornados. They might lead to massive migrations within and between countries, which would dwarf current movements of people due to war and famine. Such migrations would have staggering economic effects, both within nations and globally.

Besides involving the three most appropriate U.S. government agencies, the above program would require the expertise of scientists and engineers in the three major sectors where R&D is performed. Universities, government laboratories and companies all have roles to play in all three goals—understanding, exploitation and utilization. Again, there is precedent from the Manhattan Program—specifically, experimental reactor designs built and tested by Enrico Fermi at the University of Chicago. The Los Alamos National Laboratory was organized as the government laboratory for design and overall coordination of the project. E.I. du Pont de Nemours and Company built the Hanford B reactor. All three types of organizations noted above were coordinated by a single lead organization during the wartime Manhattan Project.

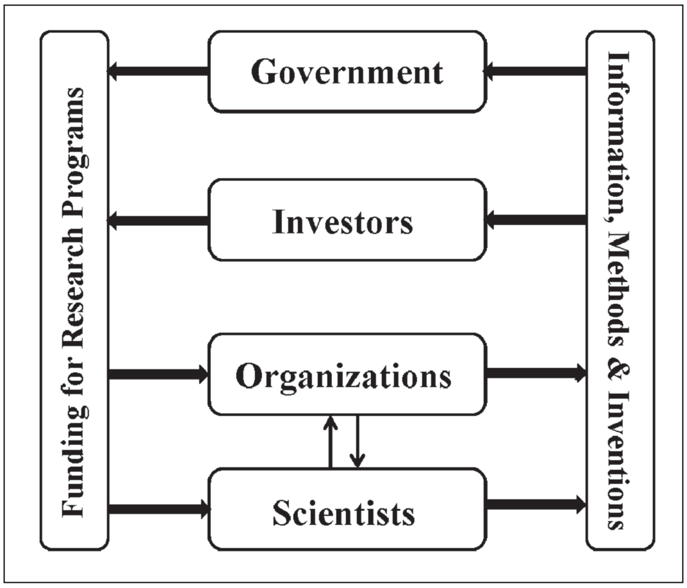
The sizes and durations of the various programs noted in Table 2 are a matter of opinion. Some experienced program managers might prefer programs that are smaller both in funding and duration. Others might want to see larger efforts for longer times. This author has half a century of experience in performing and managing R&D programs. It is his opinion that what is outlined in Table 2 is a reasonable middle ground. Such funding would speed the understanding and use of LENR immensely, compared to the current sparsity of funding. The amount of money involved is not small. The total of the efforts outlined in Table 2 for *five years* sums to \$300M. However, that is approximately the amount that the U.S. government now spends on hot fusion research *each year*.[2]

Table 2. Organization, goals and programs of a U.S. national R&D program on LENR.

Goals	Scientific Understanding	Commercial Exploitation	Military Utilization
Lead Agencies	NSF	ARPA-E	DARPA
Performer Priorities	1. Universities 2. Government Labs 3. Industry	1. Industry 2. Government Labs 3. Universities	1. Industry 2. Government Labs 3. Universities
Duration of Programs	5 Years	3 Years (Extendable)	3 Years (Extendable)
Average Program Size	\$1M/year	\$3M/year	\$2M/year
Number of Programs	20	10	5
Annual Budget	\$20M	\$30M	\$10M
Total Budget (5 years)	\$100M	\$150M	\$50M

5. Conclusion

The arguments by Katinsky, and concepts for near-term actions based on those arguments, would fix a still broken system for support of LENR. Figure 1 shows the normal flow



of funding into research, and the resulting flow of information and other results from research. Currently, there appears to be more LENR funding coming from investors than from the U.S. government. LENR has been and will remain an active area of science for many years. It is entirely appropriate that the U.S. government provide research and development funding commensurate with both the scientific challenges and practical potential of LENR as a new source of clean energy.

Comparison of the economic case made by Katinsky in the previous article with what is contemplated in this paper makes the near-term program recommendations, which are envisioned here, look somewhat timid. However, they deal only with the situation in the U.S. Similar funding by other large countries, especially those that contribute heavily to CO_2 emission, would result in a much larger global effort to understand and exploit LENR. Whatever the character and magnitude of LENR programs in other countries, the U.S. programs contemplated in this paper might have a significant favorable impact on the availability of clean energy in the coming decades.

Appendix: Information on LENR

The primary topic of the field came to be called Low Energy Nuclear Reactions (LENR), although there are about twenty other names for the subject.[3] The International Conferences on Condensed Matter Nuclear Science have been a primary global forum for the field over the decades since Fleischmann and Pons announced their ability to produce excess heat energy. The meetings were initially known as the International Conference on Cold Fusion, with the abbreviation of ICCF, which has been retained. Links to the voluminous information presented at ICCF21 in 2018 are at https://www.iccf21.com/. Plans for ICCF22 in 2019 are at https://iscmns.org/iccf22/. Links to the proceedings of many earlier ICCFs are on the web.[4] Proceedings of the recent ICCF conferences are published by the *Journal of Condensed Matter Nuclear Science*.[5] An index to the *JCMNS* volumes is available.[6] Proceedings of the annual meetings of the Japan Cold Fusion Research Society are online.[7] Information on many of the twelve International Workshops on Anomalies in Hydrogen Loaded Metals is also on the internet.[8] The 25th Russian Conference on Cold Nuclear Transmutation and Ball Lightning was held in October 2018.

Several websites are devoted to presenting information on LENR. One has a library with thousands of articles, many of which can be downloaded.[9] There have been months when the average rate of *downloading* papers from that site was about one per minute. A 2009 tally of papers by Rothwell, the keeper of the website, is available.[10] There have been over four million downloads of LENR papers from that one website. Many papers are available from the International Society for Condensed Matter Nuclear Science.[11] Other websites are also useful resources on LENR, including the New Energy Foundation,[12] the New Energy Times,[13] Cold Fusion Times,[14] Cold Fusion Now[15] and the Cold Fusion Community.[16] A summary from 2017 of empirical evidence for the reality and potential of LENR is on the website of this author's consulting company.[17] Note that some sites, and even current papers, continue to use the original name of the field, that is, "cold fusion." Whatever the terminology, a large amount of experimental and theoretical literature on LENR is available, and is open to discussion, criticism, and both experimental and theoretical research.

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