Program Strategy for Low-Energy Nuclear Reactions

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Background

Chemical reactions between molecules, such as burning, are common. They can generally be initiated using modest temperatures. Reactions between nuclei, which have much greater output energies than chemical reactions, usually require correspondingly greater energies to initiate them. Low-energy nuclear reactions (LENR) have the remarkable characteristic that they can be triggered at ordinary temperatures corresponding to less than one electron volt. But, they still output energies typical of nuclear states, one million electron volts or more. In current fusion research, large Tokamaks or immense laser facilities costing over $1B are required to heat nuclear fuels from room temperature to the mega electron volt levels required to produce significant numbers of nuclear fusion reactions. During LENR, the high density of nuclear reactants in a solid lattice results in high reaction rates, which might lead to useful power outputs.

Early reports of LENR (then known as "cold fusion") were flawed and disagreed violently with known solutions of fusion theory. However, in the past 17 years, numerous experiments have obtained anomalous results with high signal to noise ratios. They have involved many methods to both initiate reactions and to diagnose their results. Excess energy and the appearance of new elements due to transmutations were often reported. These and other measurements have produced a robust body of empirical evidence that LENR occur, even though full understanding is not yet at hand. There are over 1,000 papers in the field, many of very high quality. Much information is available on the internet. Two particularly useful sites are www.lenr-canr.org (with a library of articles) and www.newenergytimes.com (with an on-line magazine).

Motivations

The thrust of this article is simple: LENR deserve intense study for two reasons. First, LENR constitute a new area of science, now called Condensed Matter Nuclear Science. The International Society for Condensed Matter Nuclear Science was formed in 2003. See www.iscmns.org. Second, the levels of energy and materials production in LENR experiments indicate that such processes might be useful commercially and otherwise. Remarkably, LENR do not produce significant prompt radiation or residual radioactivity. There is a significant possibility that large and expensive central power plants will be augmented by distributed and safe nuclear energy sources in homes and other locations, as well as in some vehicles.

Given the exciting scientific questions surrounding LENR, and their attractive potential applications, it is time to seriously consider the characteristics of a scientific and technological program on CMNS. Logically, the strategy for such a program is an early consideration, which is addressed here. The details of any program would depend on who sponsors and who does the research and development. However, the overall goals of the program are clear: to acquire scientific knowledge, develop technologies, produce prototypes, and engineer commercial products. Along the way, understanding of LENR would lead to their control and, thence, to their optimization to produce the most energy or materials at the least cost and environmental impact.

Strategy

One view of how a near-term program to understand and exploit LENR can be organized is indicated schematically in Figure 1. The overall effort would have three inter-related layers. They are supporting research and development, primary experiments, and early engineering estimates. The basic layer would involve three topics, (a) refinement and development of instrumentation, (b) the production of new materials and the analyses of many materials with a wide variety of techniques, and (c) continuing theoretical studies, which produce and advance concepts that can lead to computational results. All three of these areas could support all of the efforts in the second layer. The core of the program would be the design, construction, conduct, and analyses of experiments that produce even stronger evidence of LENR, as well as means for their control and optimization. The experiments would be of two general types, replications of work done in other laboratories (old experiments) and experiments that have not been done before (new experiments). Because of the laboratory work already done, and that contemplated in the new program, the characteristics of LENR sources of power and materials would be known. Hence, it would be possible to perform some initial engineering analyses for commercial and other applications of LENR. As time passes and progress accumulates, programs on LENR would evolve to include more engineering, but the initial effort has to be heavily weighted toward science and laboratory measurements. Each of the major facets of the proposed strategy is discussed in more detail in the following paragraphs.

Instrumentation

Contrary to initial beliefs, LENR experiments are not simple. They are intrinsically interdisciplinary, and require skills in diverse fields. These include condensed matter and nuclear

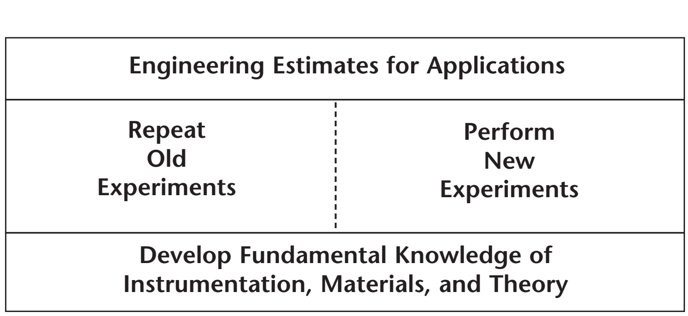

Figure 1. Overall character of a three-tier program strategy for the study and development of LENR.

physics; general and electrochemistry; materials science; electrical, mechanical and thermal engineering; and statistics and data analyses. There are two types of instrumentation requirements. The first is for methods to produce the desired reactions, that is, to initiate the experimental outputs. The second is to diagnose what happens, specifically, to quantify the energies and products of nuclear reaction. For both, there is a need for user-friendly instrumentation to efficiently acquire and archive the often extensive data for later analyses and study. The turning on, acceleration, deceleration, and turning off of LENR experiments are critical to their utility.

Materials

It is widely believed that LENR depend on special conditions on or within the solid materials, usually metals or alloys, employed in experiments in the field. Both the composition and structure of the materials are considered relevant. But, the characteristics of materials are poorly controlled in many LENR experiments. Hidden variables, such as impurity concentrations, may play a role. Very specific analyses with low minimum detection limits must be employed on materials before and after experiments. This is especially true for experiments that aim to demonstrate the transmutation of elements. But, it is also germane to energy production experiments if low levels of specific elements play a role. The analytical methods necessary to quantify the presence and distribution of impurities are generally developed, but they are expensive to employ. There is considerable evidence that the compositions and structures of the surfaces of electrodes are important in electrochemical approaches to LENR. Further, it appears from some experiments and theories that materials with structures on the scale of nanometers are significant. In short, materials are central to successful experiments that lead to LENR.

Theory and Computations

Theory has two functions, to explain the past and predict the future. Sometimes, theory leads experiments in an area, with plate tectonics being a famous example in geology. Other times, experiments outpace theory, with superconductivity being a well-known case in physics. For LENR, experiments are "ahead" of theory, despite considerable theoretical activity by very capable scientists. More than a dozen theories for LENR have been advanced. Now, all of them are either wrong or incomplete. Few of them have been reduced to numbers for quantitative study. A program on LENR should insure that the major theoretical ideas are fully developed and used for computations that would enable comparisons with the voluminous data in the field. Theories that falter should be winnowed out of the program in favor of theories that offer demonstrable promise in both their computability and results.

Old Experiments

Experimental studies should be the dominant activity in the new program. Reproducibility of LENR experiments, whether within one laboratory or between laboratories, has improved. However, it remains unsatisfactory, which slows progress both on the science and the applications of LENR. Attempts to repeat conditions and results already reported should eventually lead to improved inter-laboratory reproducibility. They would also involve fresh ideas that might improve the output of the experiments. There are at least two dozen reported combinations of input and measurement equipments, materials and protocols that deserve extensive experimental study.

New Experiments

Globally, the few hundred researchers now active in the field of CMNS have many new ideas to achieve and assess the results of the unusual conditions that lead to LENR. It is possible, and maybe even likely, that the best methods and diagnostic combinations are yet to be developed. Further, it is probable that most of the new intellectual property developed in the contemplated program will come from new types of experiments. In both repetitions of old experiments and in the execution of new experiments, modeling of the energy and material flows during the experiment will lead to better understanding and optimization.

Engineering Estimates

All sources of energy and process for production of materials have a set of basic characteristics. They include the rates of the input and output of energy and matter. For example, it is already clear that LENR experiments produce modest temperatures compared to combustion of some gases. The volumes of LENR experiments to date are on the scale of liters. There is interest in developing very small, chip-scale LENR energy sources, as well as relatively compact multi-kilowatt sources. But, it may not be necessary to develop large central power plants based on LENR. Possible applications of LENR power sources should be systematically explored. These range from battery replacements to distributed desalination plants to power modules for spacecraft and lunar modules. The results of this early engineering assessment would identify the more and less likely applications of LENR energy sources. The same kind of analyses for the modification of materials by means of LENR can and should be conducted. For example, can LENR produce useful amounts of tritium, or elements needed for important industries, such as the production of the most advanced integrated circuits? The possibility of integrated sources of energy and materials also deserves conceptual and early engineering.

Funding Sources

The sources and amounts of monies needed to execute the program envisioned here are critical. In the U.S., there are some agencies that could fund the entire program. They include the National Science Foundation, the Department of Energy, and the Defense Advanced Research Projects Agency. That is, the character and possible applications of LENR fall within the charters of these agencies. However, because such agencies have broad science and technology programs and because of the breadth of applications of LENR, it is unlikely that one agency would fund an entire program. This is good and bad news. Shared support reduces the burden and impact on any one agency, but it requires effective interdepartmental coordination, which is usually difficult to achieve. The most critical near-term need is for one of the agencies to begin serious support of work on LENR. Success in such initial work would probably lead to funding by multiple agencies. Such an evolution could lead to adequate support of the field of CMNS. It is unlikely that private investors will fund an early comprehensive program for the understanding and development of LENR. They can and are funding relatively small efforts in particular companies, but such work is not broad and its results are not available. It is necessary for a government in the U.S. or elsewhere to initiate the needed program in the near future.

Funding Amounts

There are alternative approaches to estimating the amount of money required to prosecute the contemplated program. One is to use experience with the cost of setting up and performing LENR experiments. Given the rates for salaries and overheads, the cost of research projects is not very different at laboratories in academia, industry, and government laboratories. Because of student involvement, universities tend to require lower funding than the other two sectors, but the difference is not critical to the overall scale of the program. Similarly, the costs of theory, computations, and engineering estimates are lower than those for experiments, including instrumentation development and materials studies. To make an estimate of the total program cost, take the average annual cost of an experiment as $350K, and the similar cost of the other activities as $200K. If there were to be ten old and new experiments, ten instrumentation and materials experiments, and ten theoretical, computational, and early engineering studies, the annual program cost would be near $10M. Of course, funding such numbers of projects at higher levels or starting more projects of such costs would increase the program cost. This would speed progress, but might make it harder to convince the leading agencies to start providing significant support for LENR studies in the next year or two.

Although LENR reactions are much more diverse than fusion, it is instructive to compare funding needed for a program on LENR with the support of nuclear fusion energy research. Figure 2 shows the funding history for hot fusion research in the U.S. during the second half of the twentieth century. The total is well in excess of $10B in FY2000 dollars. A great deal of good science and engineering has been done as a result of that support. However, it is widely accepted that the fusion energy program will not have a near-term impact on the energy needs of the U.S. or the world. The amount of money for the recommended early program on LENR is about 5% of the current expenditures on fusion energy.

Program Content

Viewpoints on what specific projects should be funded in a new program on LENR vary widely. It is the opinion of this author that most of the major ways to produce interesting conditions should be explored experimentally. These include gaseous, plasma, and beam means of loading materials with hydrogen isotopes, in addition to electrochemical processes. The materials should include nanometer and micrometer sized particles, in addition to thin films and bulk geometries. All of the major diagnostics should be exercised. These include heat measurements, analysis for "nuclear ash," that is, the products of LENR, measurements of energetic photon and particle radiation, and imaging of materials surfaces both during and after experiments. Processes that can be accomplished relatively quickly, and those that take longer times, both deserve attention. Adequate funding of theories that have prospects of explaining the diversity of effects from LENR experiments is needed. Prospects for their reduction to quantitative predictions should be a prerequisite for their funding. Initial assessments of possible applications of LENR should both provide motivation for funding additional research and lay the groundwork for industrial development and sales.

Program Management

How programs are managed varies from agency to agency and country to country. Besides the need for inter-agency cooperation already noted, it will be necessary to employ astute technical managers. They must have broad backgrounds in science and technology and significant familiarity with the field of CMNS and its results. Given the small total number of researchers already working on LENR, and the fact that most of them are in the latter parts of their careers, new people will have to be recruited to manage the requisite program, whatever method any agency uses. It is also clear that many new researchers must be attracted to perform the actual work. This should not be a problem, if money becomes available. There are many scientists and engineers in universities, companies and governments, who have backgrounds and skills germane to research on LENR and the development of their applications.

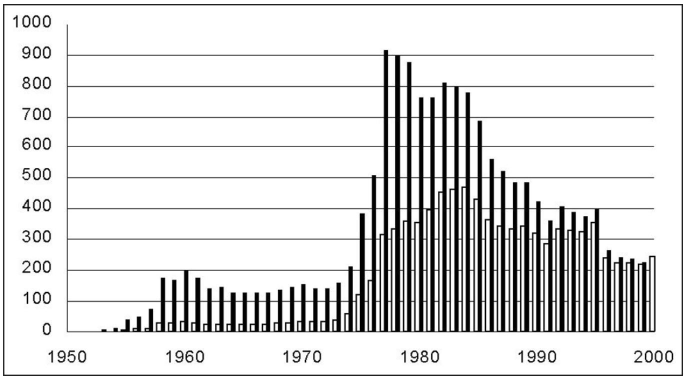

Figure 2. History of funding of hot fusion research by the U.S. government. The scale on the left is millions of dollars. The shorter bars are the amounts expended in the indicated years. The longer bars are adjusted for inflation to the value of the dollar in the year 2000. Source: Department of Energy.

Conclusion

The program on LENR being recommended here may not have a major effect on world energy production in the next decade. The transition from science to technology to engineering and commerce, first, takes time, even in fields that turn out to be important and, second, depends on funding. There are significant differences between (a) current fission and anticipated hot fusion physics and applications, and (b) what appears to be active in LENR experiments and could be possible in their applications. Fusion, like fission sources of energy, will require large capital investments, central power plants and distribution systems that are expensive to maintain, whenever it is ready for utilization. Fusion can produce energy, but does not offer the possibility of useful modification of materials. In fact, like fission reactors, fusion reactors would produce significant nuclear and materials waste. By contrast, LENR offer the possibility of small distributed and maybe even mobile sources of nuclear energy that might be safe to operate and have relatively little deleterious waste. The possibility of having dual-purpose LENR sources, which produce both energy and useful elements, is qualitatively new and enticing.

About the Author

Dr. David J. Nagel is a Research Professor in the Department of Electrical and Computer Engineering of The George Washington University in Washington, D.C. He managed multimillion dollar research and development programs during the thirteen years, when he headed the Condensed Matter and Radiation Sciences division of the Naval Research Laboratory.

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