FUSION facts

A Monthly Newsletter Providing Factual Reports On Cold Fusion Developments

ISSN 1051-8738 • University of Utah Research Park •

VOLUME 2 NUMBER 8 FUSION FACTS FEBRUARY 1991

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COMING IN MARCH 1991?

Fusion Facts will do its best to bring to its readers a **complete description of how to construct a heavy-water cold fusion cell that is repeatable.** The problem of repeatability has been solved by at least four groups of researchers and will soon be made public.

WE MADE A MISTAKE!

In some of our January 1991 issues we listed the forthcoming **2nd Annual Conference on Cold Fusion** to be held in Como, Italy from June 29 to July 24. The dates are June 29 to July 4, 1991. See further information in this issue under section I. Editor.

A. SCIENTIFIC REVIEW OF NCFI

Report of the Committee for the Review of the National Cold Fusion Institute

The Committee conducted a review of the National Cold Fusion Institute on Wednesday, November 7, 1990 at the Institute Laboratories in Salt Lake City. Though we met with the participants in the NCFI programs for but one day, that day was intensive - beginning at breakfast with Dr. Will at 6:45 a.m. and continuing until 5:30 p.m. - and we felt we were well enough informed to allow a useful review of those programs.

However, there was insufficient time for the executive sessions required for that conciliation of the views of the committee members necessary for the preparation of a single, coherent, report. Hence the committee report consists of four separate submissions by the individual members. We believe that little, if anything, was lost in the decision to eschew a manifestly collective report since, as a consequence of the different competencies of the committee members, the four reports largely address different aspects of the Institute. Stated somewhat narrowly and simplistically, those special competencies are, Adair - nuclear physics, Bruckenstein - electrochemistry, Hepler - chemical engineering and calorimetry, and Stein -metallurgy and materials science. The four reports then reflect special emphases on those areas where members were especially competent.

We note that the committee was *not* charged with the evaluation of the probability that cold fusion is real and then has not addressed the question of the wisdom of establishing the Institute. Indeed, all committee members proceeded on the basis that the primary questions addressed by the Institute - Is there cold fusion? And can it be harnessed productively? - are reasonable questions. Conversely, no committee member holds that cold fusion has been firmly established by the NCFI or by others.

The four individual reports were circulated among the members and are endorsed by the whole committee. However, this endorsement is to be read as general, and not particular. Although the opinions of the committee members to not seem to differ strikingly, the four reports

remain as the conclusions of the members who have prepared them and not necessarily of the committee as a whole.

Dr. Robert K. Adair [Physics, Yale]
Dr. Stanley Bruckenstein [Chem., St U of NY, Buffalo]
Dr. Loren G. Hepler [Chem., U. of Alberta, Canada]
Dr. Dale F. Stein [President, Michigan Tech U.]

EDITOR'S COMMENTS

The above committee introduction has been reprinted in full as presented to NCFI. The formal four reports are quoted in part and summarized with editorial comments in the following pages. This presentation is followed by a response to the reports by Dr. Fritz Will, Director of the NCFI.

It must be noted that certain materials presented to the reviewers were done under signed non-disclosure agreements signed by each member of the reviewing team. The value of ensuring that patents are properly filed and that certain information not be publicly disclosed is a decision made by attorneys protecting the potential value of the cold fusion discovery to the University of Utah and to the inventors. Drs. Pons and Fleischmann have been severely criticized for their reticence to provide information and data to fellow scientists. Please note that this withholding of information from the public is not the choice of the scientists but is necessary because of certain peculiarities of international patent law. For more on this subject see the review of Jerry Bishop's report in the Wall Street Journal which appears elsewhere in this issue.

ROBERT K. ADAIR'S REVIEW

Review of the National Cold Fusion Institute by Robert K. Adair, Yale University, December 1, 1990.

ABSTRACT

I report here as a member of a committee asked to review operations of the NCFI.

Taking the effective Institute birthdate as July 1, 1989, the review of the Institute took place 16 months after the Institute commenced operation. The original program of the Institute appears to have been based too strongly on the assumption that cold fusion was a demonstrated fact and that the task of the Institute was to better define the operating parameters and to begin system engineering. After Dr. Fritz Will assumed the position of Director of the Institute Feb. 1, 1990, the Institute programs were correctly redirected towards establishing the existence of the cold fusion -- with a deemphasis of the engineering. Prof. Will understands that the reality of the phenomenon of cold fusion has not been established and the first priority of the Institute is to provide an adequate demonstration of the reality of the phenomenon -- if cold fusion is, in fact, real.

The Institute programs can be divided, somewhat arbitrarily, into two parts. There are *technical programs* directed towards the development of chemical and metallurgical techniques that might elucidate and enhance fusion, and to the development of improved measurements procedures to better detect cold fusion. And there are *search programs* designed specifically to find evidence for cold fusion. The technical programs appear to be well designed and conducted with vigor and intelligence. Although as many as 6 results (according to Dr. Will's fifth quarterly report) of the search programs have been interpreted as indicating cold fusion effects, none are at all definitive. *The NCFI programs have not established the existence of cold fusion.*

In discussing the search program further, it is important to consider the relation of these programs to conventional ideas of the structure of materials and to the nuclear physics of the D+D reaction. The NCFI was formed, presumably, with the goal of further investigating, and exploiting, experimental results that had been interpreted as indicating (1) that deuterons stored in palladium often interacted with each other in exothermic fusion reactions of the form D + D --> XX where XX are the nuclear final states of the reaction. For the most part, the interpretations also required (2) that the reaction products XX, were not approximately equal amounts of ^{3}He + n and T + p, normally seen in D + D fusion. Neither (1) the high probability of the D + D interaction required to explain the experimental results nor (2) the lack of normal fusion products could be expected under conventional theories of condensed matter or nuclear physics. Moreover, attempts to replicate many of the experimental results failed and the conclusion that significant fusion of deuterons occurred in palladium - with conventional or unconventional decay products - had not been generally accepted by the scientific community.

Of the several NCFI programs, only the Bergeson-Barrowes project appears to require only special condition (1). This experiment is a well-defined and well-designed in accord with normal scientific procedures. Any results will be scientifically interesting, positive results will be taken very, very seriously. [See November 1990 issue of FF for a review of the special experiments by Bergeson-Barrowes. Ed.]

The other programs seemed to require two *surprises* (1) and (2) and, consequently, had less well defined experimental goals. Some were interesting, none - yet - had significant results. For the most part, these experiments using improved techniques, plausible and

well-designed technically, are being refined and conducted in systematic ways that show promise of reducing the vulnerability to mistakes of so much previous work and increasing the probability that a positive result can either be replicated convincingly or tossed aside as not validated. However, it is the nature of such programs that a serendipitous discovery of reproducible positive results is possible at any time, but the exhaustion of avenues of possible discovery is far off. Given the mission of the Institute - which includes the assumption that the quite aberrant cold fusion interpretation of previous reports is plausible - my impression is that the Institute is well directed and the programs are conducted with energy and intelligence by able scientists.

Dr. Stanley Pons discussed the progress of the Pons-Fleischmann calorimetry experiments. They have interpreted some of their results as indicating the production of excess energy - though with no indication of fusion products. Important details of their procedures were not made available. As presented, the Pons-Fleischmann results had no connection with fusion. Until their programs are openly described, and independent confirmation is possible, those programs cannot be judged scientifically.

For the most part, the level of cooperation and scientific community among the associates of the Institute seems quite good. The relations of Fleischmann-Pons effort to that of the rest of the Institute appear to be less than ideal - but seem workable.

AUTHOR'S SUMMARY AND CONCLUSIONS

[The following is directly quoted from Adair's final comments.]

The original motivation for modern *cold fusion* experiments seems to have been well-defined scientifically. Pons and Fleischmann considered that they had reasons to believe that under appropriate conditions deuterium held in a palladium lattice would undergo an effective pressure sufficient to drive the nuclei close enough together to produce D + D fusion at a significant rate. They presumed that the D + D reaction induced in the palladium would proceed with branching ratios, and with the energetics known from other low energy experiments. Hence, the experiments that conducted had well defined expectations. The expected (or hoped) to see fusion through the D + D reaction where energy, neutrons, and tritium would be produced in well-defined proportions. [Fleischmann, Pons, and Hawkins in "Electrochemically induced nuclear fusion of deuterium." *J. Electroanal. Chem*., 261, pp 301-308, and erratum, 263, p187 (1989), specifically noted the experimental evidence that classical fusion reactions did not explain the excess heat observed.]

Though these scientifically coherent expectations were not realized, their experiments were interpreted by them as indicating large energy production - without much or any fusion products. As they saw it, the magnitude of the energy production was greater than could reasonably expected from chemical sources - which they knew well - hence they attributed the energy they believed they had seen to nuclear phenomena - which they did not know at all well.

The original experimental design of Fleischmann and Pons seems then to have been driven by their view that conventional descriptions of the properties of deuterium held in a lattice were not quite correct. But they found no phenomena that fit those original expectations. However, they interpret the results of some of their measurements as indicating that excess energy was produced - in the absence of any significant generation of fusion byproducts. To interpret this interesting observations, they concluded that their conjecture that properties of the lattice sharply increased probabilities of the D + D reaction was correct but, further, that D + D nuclear physics was modified by the lattice, a far, far, more radical hypothesis. And that idiosyncratic hypothesis, tailored to fit their interpretation of their results, was only defined negatively and hence was difficult to test. Moreover, by eliminating the requirement of a well-defined structured signal, the new hypothesis, defined only negatively, opened the door to a variety of signals, e.g. uncorrelated excess heat, neutrons, tritium, or helium; and an easy confusion of mistakes with real signals.

Since the origin of the NCFI are to be found in the Fleischmann Pons idiosyncratic interpretation of their results, it is not surprising that the NCFI inherited the problems with that interpretation. Consequently, most of the NCFI programs (the Bergeson-Barrowes experiments excepted) do not test any specific model of cold fusion or require a specific structured signal. Adding the accepted lack of reproducibility that has plagued this kind of inquiry, the NCFI programs are quite vulnerable to mistakes. (Here we differentiate between mistakes and error; mistakes are unbounded.)

Almost all of the members and associates of the NCFI seemed to properly believe in the reality of cold fusion. Properly or improperly, the great majority of the outside scientific community does not. This cleavage has consequences in creating a level of siege atmosphere at the NCFI that may result in science that is less critical than it might be. I felt that very marginal positive results were taken far too seriously by a community - and a Director - who want very much to prove their faith justified. I am concerned that these marginal results might not be reviewed as critically as they ought to be. Ideally, joint participation between NCFI personnel and outside, skeptical, groups should be encouraged.

It is disturbing that the history of cold fusion has been shown too many instances of converts finding positive results and skeptics, making what are nominally the same measurement, finding negative results.

In his last (fifth) quarterly report, Dr. Will refers to approximately 6 results of NCFI programs that have been interpreted as evidence of the generation of cold fusion. Each of these results is idiosyncratic, none of the results fits simple expectations of cold fusion.

These quasi-positive results seem to have been largely held within the confines of the Institute, in accord with the responsible traditions of science and in accord with the position of scientific responsibility taken by the NCFI staff and its Director, Dr. Will. However, this responsibility reflects a reality accepted by the NCFI, at this time: *None of the Institute programs has succeeded in establishing the existence of cold fusion.*

On a more positive note, Dr. Will and, for the most part, the Institute staff and associates, have shown that they understand that the results they have achieved up to this time fall short of proving the reality of cold fusion. Moreover, the present NCFI program is directed towards directions that could results in such proof - if, indeed, cold fusion is real. I judge that an appropriate portion of the Institute effort is directed towards improvements in techniques that should allow more definitive measurements to be made - of heat and of nuclear products. Experiments using these improved techniques, plausible and well-designed technically, are being refined and conducted in systematic ways that show promise of reducing the vulnerability to mistakes of so much previous work and increasing the probability that a positive result can either be replicated convincingly or tossed aside as not validated. However, it is the nature of such programs that a serendipitous discovery of reproducible positive results is possible at any time, but the exhaustion of avenues of possible discovery is far off.

By and large, the NCFI associates seemed to have formed a scientific community where the cooperation of members of that community advances the interest of all. While, regrettably, the Fleischmann-Pons programs seem to be at best only on the periphery of that community, there is still substantial cooperation between the Fleischmann-Pons team and others associated with the NCFI effort. To some extent, the evident division separating the Fleischmann-Pons effort from others seems to stem from patent concerns and legalities connected with those concerns, to some extent personal abrasions may come into play.

EDITOR'S COMMENTS

The above abstract contains more philosophy on cold fusion than an abstract of Adair's comments. His additional 20 pages contain considerable comments. Adair states, "...I have worked in the areas of *classical nuclear physics* which includes the physics of fusion processes, and am probably more familiar with this field of knowledge than the other members of the group." Adair accepts his colleagues' reports on the quality of the chemistry, calorimetry, and material science with qualification.

Adair complains that some data was kept secret but adds, "...I did not find that the material withheld ... much affected my specific criticism of that program inasmuch as that material appeared to concern technical areas outside of my areas of competence."

Adair comments that research work sometimes did not appear to have clear goals based on models of the effect to be tested. Adair states, "Some of that diffuseness of goal appeared to stem from a lack of understanding of nuclear physics that in turn seemed to result in a too-casual disregard of very well founded properties of the fusion reactions." Adair then presents an eight-page tutorial on the "Methods of Cold Fusion Processes" in which he cites the *Cold Fusion Research Report* of the Energy Advisory Board of the DoE.

Adair in his tutorial discussion entirely ignores any of the extensive experimental literature that has found surprises in the tritium/neutron branching ratios. His comments on cold fusion theory ends with the following, "In short, no logically coherent, reasonably complete, explanation of cold fusion has yet been advanced and many feel that no explanation is possible. A very few candidate theories of cold fusion have been suggested but much work must be done on these ideas before they are sufficiently well defined to be considered seriously." These comments were made after Adair had been exposed to the theoretical work of Dr. Preparata which is based on conventional physics and is considered to show considerable promise for explaining experimentally observed facts.

Adair reflects on a summary of successes in cold fusion from 92 groups in 10 countries (as presented by Dr. Will) and dismisses all of them with the comment, "However, none of the 92 positive results over the world is definitive and cook-book reproducible." The lack of reproducibility has long been the key problem of cold fusion to be resolved. Elsewhere in this issue, we report on comments made by Dr. John Bockris (Texas A&M) wherein he cites that Kevin Wolf, Thomas Claytor, Michael McKubre, and Glen Schoessow have all demonstrated repeatability in cold fusion experiments of various kinds. To this list must be added the newer claims by Pons and Fleischmann that they have found the "trigger" for obtaining excess heat repeatability.

Adair comments on Dr. Cedzynska's work on tritium measurements, "In general, it seemed that the technique was vulnerable to serious errors." Dr. Will (see below) comments properly on Adair's lack of understanding of Dr. Cedzynska's work.

Adair makes a most interesting observation in the following comments, "While the existence of any significant cold fusion must be a qualitative *surprise*, considering the enormous range of possible barrier tunneling transmissions, a transmission just large enough to barely see, but not so large as to see clearly, would seem to be a kind of extra *surprise* - and a nasty trick of Nature." We share with Adair our marvel at being able to reduce the probability of deuterium fusion by about 70 orders of magnitude and then have great difficulty to achieve one further order of magnitude. However, Liebert and Liaw's invention using a molten-salt electrochemical cell appears to promise another 1 or 2 orders of magnitude in measurable excess heat.

Report of Stanley Bruckenstein, Member National Cold Fusion Institute External Review Committee Review of November 7, 1990.

AUTHOR'S SUMMARY

The integrity of the NCFI scientists and the leadership involved in this program is exemplary. Their desire for a dispassionate review was clear and the Review Committee received the complete cooperation of all scientific, technical and administrative personnel associated with NCFI.

The electrochemical efforts at NCFI are well-conceived and carried out. They address issues central to investigating the electrochemical phenomena occurring in cells used in "cold fusion" experiments.

The development of a "closed system" tritium analysis methodology is significant and should prove to be of considerable value in a variety of "cold fusion" experiments.

Dr. Stanely Pons reported that a solution to the problem of the experimental irreproducibility of "excess heat" production has been found. He has offered to demonstrate his solution and to train others to duplicate his results. His offer should be accepted immediately, since the origin of the excess heat cannot be studied without reproducible conditions. Independent confirmation of Dr. Pons' assertions concerning reproducible production of excess heat, while not proving the existence of "cold fusion", would be exciting.

Other "cold fusion" experiments, both electrochemical and non-electrochemical, are producing interesting results related to detection of particles produced by conventional and nonconventional fusion processes. The work at the NCFI has reached a critical stage and an unambiguous answer to the "cold fusion" question should be possible in the next year.

EDITOR'S COMMENTS

Dr. Bruckenstein suggests that "all future experiments by all groups should involve materials from the same sources having the same history, to the extent that this is possible."

Dr. Bruckenstein cites Dr. K. Cedzynska's et al. work on developing a reliable method of tritium analysis as being a key to the confirmation of tritium production in cold fusion experiments. He states, "...it appears that a precise and accurate scintillation counting technique has been developed."

Dr. Pons told the review committee that he can now reproduce an excess heat event after about 9 to 10 days of electrolysis and that such heat events were large enough to remove any doubts about the production of excess heat.

Review of the National Cold Fusion Institute
By Loren G. Hepler, Prof of Chemistry and Chemical Engineering, Univ of Alberta, Edmonton, Canada.

AUTHOR'S INTRODUCTION

The focus of my report is on the "excess heat" reported by Fleischmann and Pons and others, and disputed by others. "Excess heat" means heat produced in excess of heat that can be accounted for in terms of such known processes as passage of electric current through the cell.

It is my opinion that Pons and Fleischmann and their colleagues have worked diligently and skillfully on a difficult problem, in spite of their initial inexperience with calorimetry. They have provided strong evidence (not proof) in favor of the production of "excess heat". Recent improvements in cell operation make it practical to carry out better measurements that will lead to proof that "excess heat" is or is not a reality. I have suggested one specific approach (already started at NCFI) to such calorimetric measurements.

Production of "excess heat" as defined here is a necessary for the reality of cold fusion, but does not by itself prove the reality of cold fusion. It is possible that "excess heat" is real, but is not due to cold fusion, because there might be some new or overlooked chemical or non-nuclear

physical process in the cells. This kind of non-nuclear process might be scientifically or technologically important.

It is hoped that calorimetric investigations will be continued at NCFI, with appropriate collaboration among researchers in electrochemistry, engineering, and nuclear physics.

AUTHOR'S SUMMARY

Pons and Fleischmann are well-known electrochemists, with little or no previous experience with calorimeters. In spite of their inexperience in this field, it is their reviewer's opinion that they have made reasonable choices about how to proceed with their calorimetric measurements. It is my further opinion that they and their helpers have worked diligently and competently in tackling a difficult problem. Although it is possible that their claims of excess heat production are mistaken, it is neither accurate nor fair to say that they have done bad calorimetry.

As mentioned previously in this review, it appears that recent improvements in cell operation will permit researchers at NCFI and elsewhere to make better calorimetric measurements as soon as patent considerations cease to interfere with transfer of information to other researchers.

Production of excess heat (as defined in this work) does not prove the reality of nuclear fusion because this heat production might be due to various chemical or physical effects that have been overlooked or that haven't yet been discovered. A complete demonstration of the reality of cold fusion of deuterium depends on the production of excess heat and on detection of appropriate amounts of products of the proposed fusion. If the new cells produce more excess heat than was produced by earlier cells and if cold fusion really occurs, it follows that the amount of fusion products in these new cells is larger and therefore easier to measure with the needed accuracy. This connection between the amount of excess heat produced and the amount of fusion products leads to the requirement that reliable calorimetric measurements and reliable fusion product measurements are BOTH needed. Researchers at NCFI recognize this dual need. It is to be hoped that there will be appropriate cooperation between the various groups within the Institute.

EDITOR'S COMMENTS

Dr. Hepler comments, "Calorimetric measurements at the NCFI have not yet established definitely that excess heat is produced or is not produced." Hepler also states, "...it does appear clear that researchers at the NCFI have embarked on a reasonable program of calorimetric measurements..."

Riley and colleagues in the engineering group at NCFI have worked on application of flow calorimetry to investigate seven cells that have been charged for 20 to 103 days. In one case they reported excess heat of 17%, which is considerably larger than their equipment or process errors are deemed to be.

Hepler suggests that thermistors should be used instead of thermocouples because they are more sensitive to small temperature changes. Hepler also reports that Prof. Pons has agreed to tell how the newly-found superior cell operation is obtained as soon as the patent barrier is overcome (which should be soon). This new procedure developed by Pons can show a substantial development of heat in less than ten days of operation. Dr. Hepler strong recommends that flow calorimetry (as developed by Riley et al. of the NCFI) be applied to a few of the new cells.

National Cold Fusion Institute Site Visit
By Dale F. Stein

AUTHOR'S SUMMARY

It was very evident that the leadership of the NCFI was committed to unbiased review of the Institute. The presentations made gave this reviewer a great deal of confidence that the Institute has a variety of people of ability, commitment, and integrity. They were very knowledgeable in their areas of responsibility, they were open to criticism and suggestions, they were properly skeptical and questioning of their results. The constant scrutiny of their actions and experiments is a difficult environment in which to do research at the cutting edge of a field with the many failures and frustrations that are simply the lot of scientists and engineers engaged in forefront research. Expectations had been raised to such a high level that it would have been nearly impossible to meet them. The NCFI should now coolly and purposefully move to determine if the recent results of Dr. Pons are what they appear to be, and if they turn out to be valid, then to move in a focused fashion to expand and improve the process. If they turn out to be invalid then it would seem appropriate that the future of NCFI be evaluated by the appropriate University of Utah and state officials to determine its future.

EDITOR'S COMMENTS

As a part of the metallurgy review, Dr. Stein states, "...it is important that the material has been well characterized (chemical composition, micro-structure, etc.) if the cold fusion phenomena is to become repeatable and controllable. The metallurgy effort within NCFI appears to be appropriate and well done."

The difficulty that has been found in working with cold fusion is emphasized by the following statement by Dr. Stein, "Early experiments in the metallurgy group gave results that indicated a large amount of heat was being generated in the electrochemical cells. Encouraged by these results new cells were designed **with improved features to carefully monitor the experiments.** Since the cells have been changed, no observations of excess heat have been made in some 54 cells". This is perhaps the most frustrating (and costly) byproduct of cold fusion experimentation, the inability to recreate early successes. It appears that with more care there are sparser results. Of course, this is the type of experience that has lead to the denial and/or the abandonment of cold fusion experiments at many research facilities. Fortunately, the problem of reproducibility has been solved and will soon become public knowledge. (See page 13, column 2.)

Dr. Stein emphasized the need to do more work on methods to ensure that cathode D/Pd ratios were sufficiently high -- as it has been indicated that a D/Pd of near 1.0 is required. (See note from Dr. Robert Bush page 24-25.)

Dr. Stein reports, "...it would seem appropriate to focus most of the attention of the metallurgy group on the examination and evaluation of electrodes that have been reported to generate excess heat. Until the characteristics of these electrodes are well documented and the important characteristics identified it is of only limited value to explore for new materials that might be useful in cold fusion experiments. Clearly the NCFI has a gifted group of metallurgists..."

The most important issues, according to Dr. Stein are the following:

"1. The reproducibility of cold fusion results, especially the reported large amounts of excess heat.

"2. The evaluation of electrodes that have been reported to generate excess heat to determine if a product of a nuclear reaction exists in the electrode or if there are signs of some other process that might appear to give excess heat."

In further comments on the problems of reproducibility, Dr. Stein note, "If an independent person can reproduce the results that Dr. Pons showed us and if the analysis of the data is consistent with Dr. Pons' interpretation then a major milestone in cold fusion research has been achieved."

In general, the NCFI scientific review team performed an excellent service. A careful reading of these reports assures one that the new and puzzling technology of cold fusion is being carefully studied at the NCFI.

The National Cold Fusion Institute's Response to the Review Panel's Report
By Dr. Fritz G. Will, Director, NCFI (January 8, 1991)

The Institute appreciates the efforts of the four reviewers who conducted this technical review, as well as the very positive evaluation they offered of most of the Institute's technical programs and its research strategy. The recommendations of the reviewers are well-taken and will be used to further improve and focus our research efforts.

While various manifestations of "cold fusion" reactions have been observed by many groups worldwide, it is agreed that the scientific reality of "cold fusion" has not been firmly established either at the Institute or elsewhere. This would require the reproducible demonstration that the magnitude of the excess heat observed is fully reconcilable with the amount of nuclear reaction products.

However, there is strong and growing evidence for the occurrence of nuclear reactions at or about room temperature, in deuterium-loaded palladium and titanium. The observations in several research groups around the world of the following:

a. neutron emissions with 2.4 MEV energy, sometimes in intense bursts of up to several hundred neutrons in a 1/10,000 second time period,

b. tritium generation at levels one million to one billion times larger than the neutron levels,

c. charged particle emissions, and

d. characteristic X-ray emissions.

These observations defy conventional physics theory and appear to indicate unusual nuclear reactions, possibly "cold fusion."

There also is good evidence for the generation of "excess" heat in "cold-fusion" experiments, conducted by several very reliable research groups. At this point, the relationship between nuclear reactions and excess heat is not clear since the latter is many orders of magnitude larger than can be accounted for by the observed nuclear reaction products.

The general research strategy of the NCFI is to monitor simultaneously as many nuclear byproducts as possible, and, wherever possible, also excess heat.

The normal course of science requires these obviously complex issues to be investigated until they are understood rather than be rejected, only because they are not explainable by conventional nuclear physics theory. After all, the latter applies to gaseous plasmas and should not, a priori, be assumed to apply to condensed matter, such as palladium.

Specific responses to the reviewer's comments are as follows:

The views expressed above differ from those of Professor Adair. His notion of intellectual incoherence in many of the Institute's programs, supposedly resulting from the lack of applying models, is not shared. Professor Adair's categorizing of the NCFI programs into four classes with decreasing merit appears flawed for the following reasons:

1. this approach presumes that the result of an experiment is known before it is conducted, and
2. it uses conventional theory, developed for gaseous plasmas, and applies it to condensed matter, where it is unlikely to apply.

Professor Preparata, a known quantum mechanics theorist from the University in Milan, Italy, was invited to spend a sabbatical at the Institute. He shared his thoughts on new nuclear theories, applicable to condensed matter, and provided guidance to our experimental programs based on new theoretical models.

Parenthetically, the Institute's physics program is, contrary to Professor Adair's suggestion, staffed by one nuclear physicists and two high energy particle physicists. Also, all reviewers were made aware of the need to sign a confidentiality agreement in the letter inviting them to participate in the review.

Professor Adair's criticism, that the NCFI's newly developed tritium analysis procedure is vulnerable to serious errors, appears to be based upon a misunderstanding. It is the open-system procedure, used by others, that is subject to that criticism, not NCFI's newly developed closed-system procedure. The latter receives high marks in Professor Bruckenstein's evaluation.

Lastly, Professor Adair expresses concern that important details of the Pons-Fleischmann work were not made available and that their program cannot be judged scientifically until their results are described openly. This concern is shared by the Institute. As expressed in the reviews of Professors Stein and Bruckenstein, Dr. Pons agreed to have other researchers carry out experiments in his laboratory, independently verifying the generation of excess heat. This demonstration process will be started immediately.

Professor Bruckenstein is very complimentary of the deuterium gas phase experiments, the development of various techniques to measure the deuterium-to-palladium ratio, the new tritium analysis procedure developed at the NCFI, and the novel deuterium pump approach to cold fusion studies. Professor Bruckenstein suggest using the same source of materials wherever possible and to come to agreement with Johnson-Matthey, in particular with regard to relaxing the conditions for analyzing the palladium furnished by them. These and other suggestions are well-taken and will be acted upon.

Professor Hepler suggests the following:

1. the application of flow calorimetry, as otherwise applied on many occasions at the NCFI, to palladium electrodes of Dr. Fleischmann and Pons, and

2. extensive application of the Kalman Filter for the on-line determination of excess heat.

Plans are already in place to carry out the second of these suggestions. There are currently no plans to act on the first.

Professor Stein suggests more work on procedures to achieve large deuterium-to-palladium ratios as one of the more important objectives of NCFI work. This will be done. A decision to deemphasize shock-loading experiments has already been made, in agreement with Professor Steins's suggestion. Also in agreement with his recommendations, the metallurgy group will focus their attention on examining electrodes prior to and after electrochemical and gas phase experiments. In addition to furthering the understanding of these important metallurgical factors, the NCFI plans to do certain key experiments on new materials; the identification of materials other than palladium that would show cold fusion phenomena would constitute a breakthrough, and the NCFI should participate in the search for such new materials.

Professor Stein regards it as very important to design cells that will reproducibly generate excess energy and to identify the possible mechanism. The Institute is in complete agreement. The NCFI regards continued and more sophisticated work on the detection of nuclear particles as exceedingly important. Not only is this work hoped to contribute to the understanding of cold fusion mechanisms, but it is absolutely essential to attaining the ultimate goal of reconciling the amount of excess heat observed with the quantity of nuclear reaction products formed.

Lastly, Professor Stein urges that the NCFI should now "coolly and purposefully move to determine if the recent results of Dr. Pons are what they appear to be." The NCFI and the University are indeed moving aggressively in this direction.

C. NEWS FROM THE U. S.

COLD FUSION'S FATE

Jerry Bishop (Staff Reporter), "Cold Fusion Verdict May Be Delivered Soon", *Wall Street Journal*, February 7, 1991, page B1.

EDITOR'S COMMENTS

Jerry Bishop reports that a showdown on cold fusion is in the offing. Apparently Pons and Fleischmann's lawyers have applied for patents that opens the way for these scientists to disclose a recent discovery that will allow the user to reproduce cold fusion heat events on demand. Similar information and promises to share their newly developed findings were reported to the four scientists in the recent scientific review of NCFI. (See page 1 of this issue).

Dr. Fleischmann reported that two papers are being prepared for publications and that these papers are very important. Therefore, by the end of March, the fate of cold fusion may be decided, reports Jerry Bishop.

As reported in the review of Dr. Bockris's paper recently published in the *New Scientist*, and reviewed under "News from Abroad" page 13, other scientists have found the answer to replication. In addition to Pons & Fleischmann; Schoessow; McKubre; and Claytor as cited by Bockris, we believe that the work done by Yang et al. at the Materials Science Center, National Tsing Hua University, Hsinchu, Taiwan is another important finding in solving the problem of cold fusion replication. *Fusion Facts*, Jan 1991 page 4 selected the work by Yang et al. as one of the top ten fusion results of 1990.

BYU CONFERENCE UPDATES

Drs. Steven Jones of BYU and Howard Menlove of Los Alamos have now begun experiments with Dr. Yoji Totsuka of the U. of Tokyo using the Kamioka, Japan detector facility [I. Amato, "Cold Fusion Still Hasn't Given Up the Ghost," *Science News* 139(3), January 19, 1991, p 37]. As indicated by Dr. Totsuka at the BYU conference in October and by Dr. Jones in the current article, the Kamioka facility's sensitivity for neutrons is 2-3 orders of magnitude more sensitive than previously used detectors, and it is thus hoped will add greatly to confidence in past experimental results.

The paper presented by Dr. Rabinowitz of EPRI analyzing the Beuhler et al. (Brookhaven) and Fallavier et al. cluster impact experiments [see *Fusion Facts* November and December 1990] is now available as Purdue U. preprint PNTG-90-18: M. Rabinowitz (Electric Power Research Institute), Y.E. Kim, R.A. Rice and G.S. Chulick (Purdue U), "Cluster-Impact Fusion: Bridge Between Hot and Cold Fusion?". (Courtesy of Dr. Rabinowitz)

A low-energy resonance is discussed as one cause of the remaining orders-of-magnitude discrepancy between observed and calculated fusion rates in the lowest-energy (10 eV/deuteron) region.

A Naval Research Laboratory paper by Drs. S.R. and T.A. Chubbs, "Distributed Bosonic States and Condensed Matter Fusion," 1990, is available as NRL-MR-6600.

MICHIGAN - DUAL CALORIMETRY METHOD

Courtesy of Dr. Samuel Faile

Frederick T. Wagner, Thomas E. Moylan (General Motors Research Laboratories), Michael E. Hayden, Ulrike Narger and James L. Booth (U. of British Columbia), "A Comparison of Calorimetric Methods Applied to the Electrolysis of Heavy Water on Palladium Cathodes," *J. Electroanal. Chem.* 295, 1990, pp 393-402.

ABSTRACT

The thermal power output of a galvanostatic D_2O electrolysis cell was determined simultaneously by two methods. The first, water-flow calorimetry, gave results rigorously independent of the nature of the heat source. The second, an isoperibolic method involving measurement of temperatures within the electrolysis cells, gave results in agreement with the first when all factors were accounted for. However, neglect of a drop in effective cell impedance accompanying operation of an in-cell calibration heater, or neglect of effects of the dropping electrolyte level, can produce spurious indications of excess heat. This work demonstrates the need for extreme care in application of isoperibolic methods to electrolytic cells.

EDITOR'S COMMENTS

Although negative, this experiment is perhaps unique in that two calorimetric methods based on different principles were used simultaneously on the same electrochemical cell. As stated in the paper, "The first method, here called the ILWFC methodology [U. Narger et al., *Rev. Sci. Inst.* 61, 1990, p 1504], measured the thermal output of the cell as the temperature difference between the inlet and outlet water flows of the ILWFC cavity [in which the cell was mounted], multiplied by the mass water flow rate, the heat capacity of water, and an experimental calibration constant (1.045 ± 0.001) which took into account the ca. 5% of heat which left the cavity via routes other than the flowing water. The second, isoperibolic (i.e., heat transferred to isothermal

surroundings), method measured the thermal output power via the temperature *within* the electrolysis cell under operating conditions relative to that under zero-power conditions. The temperature difference was multiplied by a cell thermal constant kappa determined from the temperature rise induced by introduction of a controlled amount of heat with a calibration heater. This method treats the cell walls, associated boundary layers, and all other thermal paths to the flowing water as a calibrated thermal resistance; the kappa (in W/K) referred to here is the thermal conductance across the calibrated distance."

It was also noted that "since a single glass wall was used in this work, convective/conductive heat transfer dominated; and a linear relation between cell temperature and cell power (Henry's Law) was obtained. More complex behavior would be expected for the more thermally resistive dewar cells used in other work." Use of such a single-walled cell, however, was acknowledged to have resulted in up to 50% variations in the thermal "constant" kappa with time.

Typical power levels calculated from water flow measurements for open and closed cells was 100.3 ± 0.4% of that expected from electrolysis without recombination, for an excess of 0.02 ± 0.03 W, and 100.1 ± 0.2%. The uncorrected value for the open cell isoperibolic calorimetry, however, was 140%. A corrected value of 104 ± 2% was obtained after taking into account drifts in the baseline cell temperature and electrolysis power with time due to changing electrolyte levels, and changes in the cell response during operation of the calibration heater.

Specifically, decreasing electrolyte levels resulted in a drop in cell voltage from 9.56 to 8.60 V as the electrolyte became more concentrated; this was partially counterbalanced by a change in the heat transfer characteristics, such that either temperature increases or decreases in the cell resulted depending on whether the cell was immersed in a bath or flow-through calorimeter. Such varying conditions within the cell were noted to be characteristic of small cells operated at high current densities. The effect of increased temperatures due to operation of the heater may have been either to facilitate the electrode kinetics or to decrease Joule heating through an observed change in the bubbling pattern.

This experiment provides a clear demonstration that different calorimetry methods are sensitive to quite different types of errors. Thus, demonstration of comparable levels of excess power by two such different methods in future experiments (especially simultaneously) could in our opinion be an especially valuable confidence-building tool.

NAVAL WEAPONS CENTER - EXCESS HEAT
Courtesy of Dr. Samuel Faile & Dr. Milica Petek.

M.H. Miles, K.H. Park and D.E. Stilwell (Naval Weapons Center, China Lake CA), "Electrochemical Calorimetric Evidence for Cold Fusion in the Palladium-Deuterium System," *J. Electroanal. Chem.* 296, 1990, pp. 241-254.

ABSTRACT

Several different types of calorimetric cell designs were used to measure excess enthalpy during the electrolysis of LiOD + D_2O using palladium cathodes. Control experiments were run using light water in place of D_2O or using platinum cathodes in place of palladium. Previous experiments using thin palladium wire (d 0.14cm, A 2.64 cm^2) gave no significant differences between the Pd/D_2O cells and the controls. The use of thicker but shorter palladium rods (99.6%, d 0.635 cm, A 2.64 cm^2) with currents of 100 mA/cm^2 resulted in evidence for excess enthalpy in five of six Pd/D_2O cells. These excess enthalpies are significant at the 99.95% confidence level. The excess rate of heating averaged 0.39 W/cm^3 over a 9-day period in one experiment. The total excess enthalpies observed are difficult to explain by chemical reactions. Possible calorimetric error sources also failed to provide satisfactory explanations for the excess enthalpies. There were no significant excess enthalpies in any Pd/H_2O cells. These experiments indicate that a tightly-coiled anode surrounding the palladium cathode is important for obtaining excess heat in a cold fusion experiment.

EDITOR'S COMMENTS

The work described in this paper represents a continuation of research which has previously been presented [M.H. Miles, K.H. Park and D.E. Stilwell, *Proceedings of the First Annual Conference on Cold Fusion*, p. 328; D.E. Stilwell, K.H. Park and M.H. Miles, *J. Fusion Energy* 9, 1990, p. 333; M.H. Miles and R.E. Miles, *J. Electroanal. Chem.* 295, 1990, p. 409].

In the present experiment, excess heat levels of 20-30% were observed after slightly more than a week of electrolysis and maintained for up to slightly more than a week in cells using closely wound spiral Pt anodes and Johnson-Matthey Pd. The average 0.39 W/cm^3 excess in one such episode reported corresponds to 0.14 W. After electrolysis was stopped and restarted, excess heat was lower. Some effects of thermistor positioning and of periodic D_2O additions were noted, but were considerably smaller than the excess heat. Also, D_2O levels indicated negligible recombination, and other experiments involving the same apparatus demonstrated excellent heat balance and agreement with Newton's law of cooling. No tritium enrichment was found.

NAVAL WEAPONS CENTER - NEUTRON ACTIVATION METHOD

Courtesy of Dr. Samuel Faile

M.H. Miles and R.E. Miles, "Theoretical Neutron Flux Levels, Dose Rates, and Metal Foil Activation in Electrochemical Cold Fusion Experiments," *J. Electroanal. Chem*. 295, 1990, pp 409-414.

EDITOR'S COMMENTS

This paper discusses the potential use of neutron activation to verify neutron emission measured directly using neutron counters. Metals best-suited for absorbing thermalized neutrons and emitting detectable gamma rays of specific energies are indicated to be indium and gold. Sample calculations assuming a thermal neutron flux of $1/cm^2$/second gave predicted maximum gamma activities of 3.3 disintegrations/second per cm^3 of In and 5.8 disintegrations/second per cm^3 of Au, although values will take some days to approach these values.

Monte Carlo calculations for the electrochemical cell in the preceding article, assuming a neutron energy distribution giving an average energy of 2 MeV, indicated that an emission rate of approximately 2.5×10^4 neutrons/sec would be required to give this thermal neutron flux at the surface of the cell (i.e. through a thickness of D_2O inferred from a diagram to be approximately 2 cm). It was noted that placement of the foil next to the electrode would be less effective because the neutrons would not have to be thermalized by the electrolyte. Placement too far from the electrode, on the other hand, would also be less effective because the flux per unit area drops proportional to the square of the distance.

Such use of neutron activation of Au and In has in fact been used to confirm high cumulative neutron emission in ion bombardment and plasma focus experiments described by Drs. McKee and Srinivasan at the BYU conference [reported in the November 1990 *Fusion Facts*].

TEXAS - COLD FUSION BUT NO FRAUD
Courtesy of Dr. Samuel Faile

Robert Pool (Science Writer), "Cold Fusion at Texas A&M: Problems, but No Fraud", *Science*. Vol 250, pg 1507-8, 14 Dec 1990.

ABSTRACT

A review has faulted the way research was conducted and reported but found no evidence to justify further investigation.

EDITOR'S COMMENTS

In the July 1990 *Fusion Facts*, page 3, we reported on a defamatory article written by Gary Taubes (a science writer) and published in *Science* in the June 15, 1990 issue. We later heard that this publication of the American Association for the Advancement of Science refused to publish an answer to Taubes written by five Ph.Ds. We are pleased that *Science* has published this article reporting on a four-month-long internal review of the scientific research at Texas A&M. No fraud was found. The original article by Taubes was a deliberate attempt to cast serious doubt on cold fusion by attacking the production of tritium. Ignored was the fact that many other researchers had also reported the production of tritium. So much for the unbiased scientific press.

HISTORICAL - ISOTOPIC EFFECTS IN METALS
Courtesy of Dr. Samuel Faile

R.H. Wiswall and J.J. Reilly, "Inverse Hydrogen Effects in Some Metal Hydride Systems," *Inorganic Chemistry* 11, 1972, pp 1691-1696.

ABSTRACT

Several instances were found of metal hydrides in which replacement of hydrogen by deuterium or tritium resulted in a more stable compound. The experimental evidence consists in measurements of the dissociation pressures of pure isotopic species, and of the equilibrium distribution of hydrogen isotopes between gas and solid phases in systems of mixed isotopic content. Examples are VH_2, NbH_2, $(V,Nb)H_2$, and $LaNi_5H_6$. On the other hand, ternary hydrides formed by the intermetallics Mg_2Ni and TiFe exhibit an isotopic effect which is in the opposite, and more usual, direction.

EDITOR'S COMMENTS

In addition to the experimental data presented, the paper is valuable in that it gives extensive citations regarding similar data for numerous other metal hydrides not listed in the abstract. The significance of this type of data is that the two-fold mass difference between hydrogen and deuterium can lead to notable differences in their uptake by metals. As implied in the paper, attainment of a particular equilibrium loading would be at least slightly more difficult for deuterium than hydrogen in most such metals, including Pd, but the opposite is true in a few cases. This would of course be a helpful characteristic in cold fusion.

For example, in the V gas-loading experiment, the gas pressure required to attain a composition of greater than approximately VD_1, and thus begin to form the VD_2 phase, was approximately 1-1/2 atmospheres. In contrast, the H_2 pressure needed to begin forming VH_2 was 4-5 atmospheres, three times greater. The authors note that such stability differences are temperature-dependent; in the case of commercial V, it was calculated that the difference would reverse above approximately 137 C. (The projected crossover point for purer V was not indicated.)

In addition, several other points were noted in the paper. First, changes were also observed during these experiments in the positions of phase boundaries. For example, while a VH_2 phase began to appear when the average loading reached $VH_{0.95}$, VD_2 did not appear until a composition of $VD_{1.1}$. Second, it was found that the impurities in the commercial grade V used were still sufficient to more than double the pressures required, but also increased the speed with which equilibrium was attained. Third, tritium/deuterium separation factors in V at low temperature were found to also be opposite to those in most other metals and quite temperature-dependent.

In addition to stability differences, it is important to note that kinetic differences between H and D may also affect the degree of hydriding/deuteriding attained prior to equilibration, since both diffusion rates and reaction rates may vary between isotopes. (In Pd, deuterium diffusion is more rapid than hydrogen.)

C. NEWS FROM ABROAD

ENGLAND - WHAT HAPPENED TO COLD FUSION?

Courtesy of Dr. Samuel Faile

"WHATEVER HAPPENED TO COLD FUSION?", *New Scientist,* 19 January 1991, pp 46ff.

"INTRODUCTION: The cold fusion saga is one of the most extraordinary episodes in the history of scientific research. The sudden announcement of a potential source of virtually limitless energy turned its inventors into media stars. . . . *New Scientist* presents two opposing views of what cold fusion was, or is, all about: one from a nuclear physicist, and well-known science writer, who insists it is time to bring down the curtain on a fake phenomenon; the other from an eminent chemist [Dr. J.O'M. Bockris, Texas A&M] who does research on cold fusion and believes it is worth pursuing."

COLD FUSION NEVER WAS

Frank Close (Rutherford Appleton Laboratory), "Cold fusion I: the discovery that never was", *New Scientist*, 19 January 1991, pg 46-50.
[At last, the bubble of cold fusion has burst, leaving behind a sticky story of intrigue, false facts and wrong inferences.]

COLD FUSION STILL IS

John Bockris (Texas A&M), "Cold fusion II: the story continues", *New Scientist,* 19 January 1991, pg 50-53.
[Many scientists think that fusion carried out in a test tube is nonsense. But a good look at recent results reveals that the reaction is real.]

EDITOR'S COMMENTS

We applaud *New Scientist* for publishing this pair of articles. The two articles are a study in contrasts. The article by Frank Close is couched in emotional terms, and contains many statements that are known to us to be untrue or, at best, are distorted. The article by Dr. Bockris is unemotional, carefully reasoned, contains experimental data for examples, and is restrained in terms of the recitation of the pros and cons of cold fusion. It appears that one article has an axe to grind and the other is a document written by a reasoning scientist.

An example of Frank Close's writing: "In a single week in April, anecdotal claims by small teams of one or two researchers of having achieved cold fusion flooded the media. This was despite the fact that major research laboratories, which had large teams of scientists with wide ranging expertise and far superior equipment, were seeing nothing and saying nothing." The facts (see *Fusion Facts,* Vol 1, No 3, Sept 1989): 4/2/89 Hungary announces detection of neutrons; 4/5/89 Brookhaven tentatively confirms cold fusion (McBreen has positive results, Weismann has negative results and gets funded later by DoE); 4/10/89 Hall at Texas A&M gets excess heat; 4/12/89 excess heat found in Indira Gandhi Centre, India; 4/12/89 Moscow U replicates CF; 4/13/89 Hagelstein (MIT) announces theory; 4/14/89 U of Wash gets tritium; 4/18/89 Italy's National Agency announce evidence of CF; 4/21/89 Huggins at Stanford, group in Czechoslovakia, and India announce CF successes; 4/28/89 two more national labs report successes but will not publish as yet; 4/29/89 Landau, Case Western reports success. Later, we learned that within three weeks of P-F announcement ten groups in India achieved some successes in cold fusion.

The most telling point made by Frank Close is the following: "**The funding of billions of dollars of research into hot fusion was in the balance; if the claims of cold fusion turned out to be true, the DOE would have to reallocate funding**". Later Close states, "By the end of the year [1989] the experts concluded that there was no evidence for fusion, had identified flaws in several positive experiments and advised against any special funding of the research."

The most obvious distortion of fact by Frank Close is the following: "The research groups for cold fusion tend to be small and tend to be relatively *amateur* compared with the full-time, large-scale teams of laboratory scientists who have, almost universally, seen nothing." These *amateurs* include Drs. Huggins, Bockris, Landau, Srinivasan, Preparata, Appleby, McBreen, Noninski, and many others who are world-famous and/or from institutions as prestigious as Los Alamos or Rutherford Appleton.

The best summary of the article by Dr. Bockris are the pluses and minuses as presented:

The pluses:

1. About 100 laboratories have reported anomalous effects similar to those claimed in March 1989 by Fleischmann and Pons.

2. Mainly of those who have tried and failed to replicate the phenomena have not accounted for the fact that prolonged electrolysis is necessary, and that even then the phenomena are burst-like.

3. The Japanese have made cold fusion one of their national priorities. [So, too, India. Ed.]

4. The theoretical side is not so dark as it seems: some theorists believe they can see a mechanism for cold fusion and one of them is Julian Schwinger, Nobel Laureate.

5. The famous irreproducibility is not universal. A few example of reproducible heat, neutron and tritium measurements are now available.

6. In accounting for the lack of balance between the nuclear particles and the heat, we must remember that it is difficult to find some of the particles. For example, if the phenomena occur at or near the surface, most of the helium4 will escape with the deuterium and then the analysis of, say, 10^4 helium atoms per second mixed with 10^{19} deuterium molecules will be a considerable challenge. But six laboratories have found helium inside electrodes.

The minuses:

1. More than 100 laboratories have failed to reproduce the phenomena.

2. Most nuclear physicists (using the theory of dilute high temperature plasmas) see the reported phenomena as "impossible".

3. The nuclear particles which are reported in bursts do not tie up numerically with the heat output (discrepancy 100 to 1000 times).

4. Until the anomalous effects can be tuned in at will using instruction that can be followed by other researchers, cold fusion is not part of established science.

REPRODUCIBILITY

The most interesting part of Dr. Bockris' paper is the following: "Reproducibility is the key to scientific respectability. Scientists do not accept a phenomenon unless they can demonstrate it at will. It is good to be able to report, then, that people are beginning to claim that they can reproduce results in cold fusion. Wolf in our laboratories [Texas A&M] can obtain the emission of neutrons from a bank of electrodes more or less on call. Thomas Claytor, who works at Los Alamos, can make tritium reproducibly from a 10-layer sandwich of palladium, saturated with deuterium, and alternating with layers of silicon. At Stanford Research Institute [now SRI, International], Michael McKubre and his colleagues have found two electrodes from which they can produce excess heat in their cells at will. Glen Schoessow at the University of Florida claims he also can do it at will." A group at a Taiwan University achieved six out of the first six cells that produced both excess heat and tritium.

Bockris ends with this cogent statement: "... It seems now established that nuclear particles are, under some circumstance, produced in bursts at electrodes in the cold. As to the heat, there is no proof that it originates in a nuclear process, though when it coincides with nuclear emissions, it is difficult to think that it doesn't. No matter what the science journals and what many physicists say, **something is going on."**

JAPAN - MEASURING INSTANTANEOUS LOADING
(courtesy of Dr. Samuel Faile)

Lars Grasjo and Masahari Seo (Hokkaido U, Japan), "Measurement of Absorption of Hydrogen and Deuterium into Palladium During Electrolysis by a Quartz Crystal Microbalance," *J. Electroanal. Chem.* 296, 1990, pp 233-239.

Nobushige Yamamoto, Takeo Ohsaka, Takashi Terashima and Noboru Oyama (Tokyo U of Agriculture and Technology), "In Situ Electrochemical Quartz Crystal Microbalance Studies of Water Electrolysis at a Palladium Cathode in Acidic Aqueous Media," *J. Electroanal. Chem*. 296, 1990, pp 463-471.

EDITOR'S COMMENTS

Both of these new papers discuss in-situ measurements of H or D loading of Pd electroplated onto a quartz crystal. A similar paper, G.T. Cheek and W.E. O'Grady, *J. Electroanal. Chem.* 277, 1990, p 341, was summarized by Dr. Cheek at the First Annual Conference on Cold Fusion. This technique allows the electrode mass, and therefore loading, to be continuously monitored during an experiment by measuring the resulting change in the resonant frequency of the crystal.

Unfortunately, the papers note that other factors such as the stress state of the Pd may alter the usual linear mass-frequency relationship. In addition, the technique uses thin films or foils, and thus limits both the size and geometry of the Pd. Finally, as noted in the literature [e.g. Jean Horkans, "Film Thickness Effects on Hydrogen Sorption at Palladium Electrodes," *J. Electroanal. Chem.* 106, 1980, pp 245-249], hydrogen sorption at thin film evaporated Pd electrodes is quite different from that at bulk Pd, unless annealed; while higher loadings may be achievable, small grain size leads to large numbers of grain boundaries which impede hydrogen diffusion.

JAPAN - THEORY
Courtesy of Dr. Samuel Faile

Setsuo Ichimaru, Shuji Ogata and Aiichiro Nakano (U of Tokyo), "Rates of Nuclear Fusion in Metal Hydrides," *J. Physical Society Japan* 59, 1990, pp 3904-3915

ABSTRACT

Hydrogen nuclei in metal hydrides have a dual character of quantum solids trapped between the metal-lattice sites and of semiclassical itinerant particles diffusive between them. First-principle calculations are presented for the pycnonuclear reaction rates in binary-ionic mixture solids and for the thermonuclear reaction rates enhanced by quantum and classical many-body effects in itinerant hydrogen. The latter effects are elucidated through an elaborate Monte Carlo simulation study for short-range correlations between itinerant hydrogen, interacting via electron-screened repulsive forces, in periodic and aperiodic (due to defects) lattice fields of metal hydrides. We find that the screening potentials and the resultant fusion rates depend extremely sensitively on microscopic details in the lattice fields, corroborating qualitatively the varied results in recent "cold fusion" experiments.

EDITOR'S COMMENTS

Extensive calculations for PdH and TiH_2 suggested that measurable deuterium fusion rates could be possible in such crystals under sufficiently nonequilibrium conditions, such that hydrogen atoms spend less of their time in the preferred lattice sites. While such calculated rates can be very sensitive to lattice defects, the effect in Pd and Ti is calculated to be opposite, with defects enhancing the fusion rate in Ti but decreasing it in Pd.

RUSSIA - COLD FUSION REACTION
Courtesy of Dr. Samuel Faile

V.B. Brudanin, V.M. Bystritsky, V.G. Egorov, S.G. Stetsenko & I.A. Yutlandov (Jt Inst for Nuclear Res, Moscow), "Search for the cold fusion d(d,^{4}He) in electrolysis of D_2O", *Physics Letters A,* Vol 151 No 9, pg 543-546, 10 ref.

ABSTRACT

A search for high-energy alpha-particles from the reaction d + d + Pd |Ti| --> ^{4}He + Pd' |Ti'| has been carried out using track detectors Cr-39 and silicon surface barrier detectors. The upper limit of the investigated partial reaction rate has been estimated per deuteron pair to be less than or equal to 8×10^{-27} per sec.

EDITOR'S COMMENTS

This paper seeks and fails to find specific alpha particles having a specified energy range that may be the result of a deuteron reaction. The paper fails to describe the nature of the experiment so that the experienced reader could determine if the experimenters could expect to achieve a cold fusion reaction. No excess heat was measured, therefore, no nuclear reaction by-products would be expected. Therefore, the paper has little scientific value.

USSR- FRACTOFUSION
Courtesy of Dr. Samuel Faile

B.V. Derjaguin, V.A. Kluev, A.G. Lipson and Yu.P. Toporov (USSR Academy of Sciences), "Excitation of Nuclear Reaction Under Mechanical Effect (Impact) on Deuterated Solids," *Physica B* 167, 1990, pp 189-193

ABSTRACT

Using the analysis data on the characteristics of radiation accompanying failure of solids as the basis, a supposition is made of the feasibility of dd-reactions taking place during fracture of deuterated dielectrics. The experiments carried out prove the feasibility of generation of neutrons at the moment of fracture of D_2O LiD crystals, indicating the excitation of nuclear reaction.

EDITOR'S COMMENTS

The present paper is the latest on a series of fractoemission/fractofusion experiments which precedes the March 1989 Fleischmann-Pons and Jones papers. For instance, a previous paper by Derjaguin et al appeared in *Nature* 341 (1989), p. 492.

In the present paper, neutron emission of 0.34 ± 0.10 counts per shot above background was detected in a series of 75 tests in which LiD crystals were fractured by the impact of a projectile. No such emission was observed for hydrogen control experiments. Emission during fracture of D_2O ice gave 0.16 ± 0.05 counts/shot above background. Efficiency of the BF_3 counter was 1%, and background was approximately 0.25 counts/shot. Emission was suggested to be due to fusion of deuterons accelerated by electrical fields associated with cracking.

D. SHORT ARTICLES

HEAT ENGINES POWERED BY COLD FUSION

By Dr. Harold Aspden, Department of Electrical Engineering, U. of Southampton, England

It has been suggested [1] that the excess heat generated by cold fusion cannot be harnessed efficiently owing to the limitation set by the Carnot efficiency factor. If the temperature of fusion fuel cells is limited to that of water or even that of the molten salts used in the recent Hawaiian experiments [2], then the conversion of that excess heat into electricity using the conventional heat engine might seemingly have to be far less efficient than that available by burning fossil fuel.

It is, therefore, most important for those who hope to invest in this new technology to realize that cold fusion opens up a whole new horizon of possibility on the power generation front, owing to aspects that can be ignored if we think too much in terms of conventional heat-to-electricity energy conversion technology. The essential difference between the cold fusion heat source and the fossil fuel heat source is that there is, ideally, as far as the heat producing process is concerned, no exhaust gas carrying away the rejected heat. **This is extremely important because it means that whatever heat energy is produced from the nuclear reaction can be converted to electricity with a thermal efficiency close to 100 percent and is no way limited by the Carnot factor**.

The operating temperature as determined by optimal fusion cell design criteria governs, not so much the efficiency of the system, but rather the capital expense involved.

FUSION VERSUS SOLAR

To understand the achievement of high thermal efficiency, imagine that one uses those molten salts as the cold fusion heat carrier to heat a thermally-radiating blackbody surface to a temperature of, say, 400 C. At this temperature the Stefan-Boltzmann radiation constant tells us that 11.63 KW of heat power is radiated from a square meter of surface. Now, there are solar-power enthusiasts who consider it feasible to use heavy parabolic mirrors to track the solar radiation and contend with the odd cloudy day and the cycle of day and night, solely with the object of focusing that radiation on a copper pipe at the linear focus of such a mirror in order to provide heat input to a Stirling-type heat engine. This solar process is limited by the Carnot efficiency because the heat rejected by the engine is expelled to ambient atmosphere. With such a solar-energy collecting system, one can at midday hope to capture 1 kW of solar heat radiation per square meter of the collector mirror and, with appropriate design skill, convert about 20 percent of this heat into output electrical power.

In contrast, with the cold fusion radiant heat source, the mirrors are not affected by the 24-hour cycle nor the weather nor the latitude, nor do they need to be heavy structures which move to track the sun. The mirrors can be enclosed in dust-free cell structures and be constructed of inexpensive light-weight thin-film materials. More important, if the radiating surface is at 400 C and the mirror focuses the radiant heat on a thin copper pipe with a resulting temperature of, say, 600 C, the heat rejected by the engine can be shed at a little above 400 C, the ambient temperature inside the structure, so as to be recycled by heat conduction to the radiating surface. In addition there would be a ten-fold intensification of source radiation power (11.63 kW per sq meter compared to solar 1kW per sq meter) as compared with the solar energy source.

NEAR 100 PERCENT EFFICIENCY

The heat converted into electricity by the engine is sourced in the heat produced by the molten salts cold fusion cell and, assuming that the whole cell system is totally enclosed, uses a closed hot air engine cycle and is in a well-insulated thermal housing, we can expect the fusion heat to be converted into useful power with nearly 100 percent thermal efficiency.

The theoretical Carnot efficiency which is 22.9%, meaning an actual engine efficiency of, say, 17%, merely implies that the heat produced from cold fusion has to make, on average, six passes around the radiation and reflection-focusing loop, before being shed as electrical output power. In other words, the electrical power output will probably be of the order of 1.5 kW per square meter of mirror surface, available on a 24-hour basis.

The power-to-volume ratio of this system may not impress those who design fossil fuel power generating facilities or conventional nuclear-fuelled power generators using steam turbines because the structures for the mirror-concentrator cold fusion system would need to be enormous for a 1,000 Megawatt installation. Conceivably, a structure 25 meters high by 100 meters square could produce 1,000 Megawatts. Since the building would be little other than a cell structure of copper pipes, metal-film reflectors. Flat black body radiating surfaces, with the addition of some fusion cells and heat engine units, could be cost-efficient. The reward is that very high thermal efficiency with no polluting exhaust gas could be achieved.

FUTURE NOT MARRED BY LOW TEMPERATURES

The future of cold fusion cannot, therefore, be decried on the basis of an operating temperature lower than that obtainable by burning fossil fuel. Once the heat generated by cold fusion comes firmly under control then the technology of converting that heat into electricity will be at hand.

Readers will understand that what has been suggested above is that the input heat should be at the *cold* operating temperature of the engine, whilst the mirror focusing is used within the structure to concentrate its radiation to produce a higher *hot* operating temperature of the engine. Some may debate the feasibility of this proposal, just as they debate the feasibility of cold fusion. Yet it is known that solar radiation can be focused in our Earth laboratories to an intensity exceeding that at the solar surface. Also, we need to be mindful of the fact that the second law of thermodynamics has recently come under critical scrutiny [3], even in the journal *Nature*, which has not been at all enthusiastic about the prospect of cold solid-state fusion.

AUTHOR'S ADDED NOTE

Readers may be interested in an issue of the periodical *Speculations in Science and Technology* due to be published in December 1990 (Issue No. 4, Volume 13). This issue is wholly devoted to new energy concepts and includes an article by this author developing the above theme. It also includes an article by a Scottish inventor, Edinburgh scientist J. S. Strachan, describing the discovery of a thermoelectric technique using base metals, aluminum and nickel, rather than semiconductors, which operates as a solid-state device converting heat of water at atmospheric pressure into electricity with an efficiency of 70 percent of the Carnot efficiency. This is the development hinted at in the *Nature* reference [3]. When fully developed into a commercial product, this thermoelectric converter should be eminently suitable for incorporation in cold fusion power systems. Because the device is solid state and needs no routine maintenance its widespread application is expected. Important also is the fact that prototype development shows that it can be fabricated in miniature strip form of millimeter section. This size can greatly facilitate designs with very high parabolic mirror area per unit volume of structure. These thermoelectric strips would allow electronic components and wires to replace the heat engines and their attendant copper pipework in the above design proposal, meaning that pipework would only be needed to input the heat generated by the cold fusion cells.

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[2] B.Y. Liaw, et al., "Excess heat using molten-salt electrochemical cell", *Fusion Facts*, Vol 2 No 4, pp 1-11, October 1990.

[3] H. Aspden, "Correspondence concerning Maxwell's demon", *Nature*, **347**, p 25, (1990).

EDITOR'S COMMENTS

Dr. Aspden uses the Stefan-Boltzmann formula to compute the available energy from a black body at 400 C to obtain a value of 11.63 KW per sq meter. Aspden recalls the fact that a solar furnace can achieve temperatures much higher than the temperature attributed to the surface of the sun. He extrapolates that a properly-designed parabolic reflector could magnify the 400 C heat source to a 600 C temperature by focussing onto an object at the focal point of the parabolic mirror.

Here at FF, we have had a speculative discussion about the need for a temperature amplifier that would be analogous to a voltage or current amplifier in electronics. We found the following: The Stefan-Boltzmann formula is only correct for radiation through a vacuum. We have considered the following *thought experiment*:

Take a perfectly mirrored surface on a parabolic mirror; point it toward a heat source; place a thermometer at the focal point.

Question: **Can the thermometer achieve a temperature higher than the source in any kind of a non-vacuum earth environment?** The answer is not obvious. If we understand Aspden's statement in ref [3], then the thermometer would measure a higher temperature. However, we did find that the computation of heat flow was complex. Work by Josef Stefan (1879), Ludwig Boltzmann (1884), Lord Rayleigh (c1890's), led to the development (1900) by Planck of a radiation law as follows:

The distribution of radiant energy dW in terms of wavelength change dL can be expressed as:

$$dW \quad \frac{8 \pi c h L^{-5} dL}{e^{hc} / L^{kT} - 1},$$

where h is Planck's constant, k is the Boltzmann constant, c is the speed of light, and T the absolute temperature.

A second *thought experiment* is the following: Assume that a perfect parabolic mirror, in a large perfect Dewar bottle evacuated to a perfect vacuum, is properly directed at a radiating body of temperature T and that a thermometer is placed at the focal point of the parabolic mirror all of which are at temperature T as the initial conditions. Question: **Would the thermometer indicate a temperature higher than T?** The answer appears to be that if the thermometer were at a temperature higher than T then, heat would flow from the thermometer to the radiator (heat flows in both directions and is assumed to flow more rapidly from the higher to the lower temperature), thus raising the temperature of the radiating body. The radiator then being at a higher temperature would then raise the temperature of the thermometer. This cycle of results would be contrary to known physics. The answer appears to be that the thermometer cannot indicate a temperature higher than T. [This gedanken experiment also suggests that the statement made in the *Nature* article, "Instead it is sufficient to place a convex mirror located in cavity A and positioned away from but facing the hole. ..." is not a true statement because it ignores the transfer of energy from the focal point back to the mirror and to the cavity A].

How, then, can a solar furnace attain a temperature than we deem to be higher than the temperature of the sun? The answer must be that some wavelengths emitted by the sun have a *nominal temperature* that is in excess of the *average temperature* that we attribute to the sun's surface. In the solar furnace, it may be the concentration of energy at these wavelengths that provide the high temperatures observed.

However, Aspden has presented us with a challenging article and has, indeed, shown us that the Carnot efficiency that we normally associate with upper limits on thermal-mechanical conversion may not apply in a properly-designed thermal-electrical conversion system where a molten salt fusion cell is the source of thermal energy.

Readers, We invite you to comment on the above remarks by Hal Fox, or by the article by Dr. Aspden. Dr. Aspden has been provided with the above comments and replies that he is willing to defend his writing. So shoot away at either one or both of us. Ed.

ZERO-POINT ENERGY AND POSSIBLE APPLICATION TO COLD FUSION.
By Dr. Samuel P. Faile

The Institute for Advanced Studies of Austin, Texas has recently been given wide publicity in their search for new energy sources and new electronic devices. An article in the February 1991 issue of OMNI by Owen Davies titled "Volatile Vacuum" reports that the zero-point-vacuum energy in the form of the Casimir force may allow clusters of electrons to form condensed charges that could be important in energy and electronics research.

In a discussion with Hal Puthoff of the Institute many details were supplied concerning the project that Dr. Puthoff and Ken Shoulders are developing. Experimental findings are that clusters or electron beads are formed when a tip of material is vaporized in an electric arc in a vacuum with a voltage of typically 10 KeV. These electron beads typically consisting of 10^9 to 10^{11} electrons are formed in and separate from the positive ions produced by the electric arc. These electron beads are separated by funneling through a pinhole in a metal disk. The beads can be accelerated to a bead speed about 1/10th the speed of light and caused to impinge on a target metal plate. The results is a micron-wide pit or bomb crater in the metal target.

The shape of the electron beads are apparently a function of the electric fields used in their production. Some "beads" apparently have the form of a necklace pattern about 25 microns wide. If these electron beads are allowed to impact layers of ceramic (1 - 2 microns thick) a hole is formed through the plates. This anomalous effect is noticed even when using many ceramic plates with experimental holes being extending through 800 microns of ceramic.

If the ceramic material is being destroyed explosively, although unlikely, this could explain the observed results. If the ceramic were melted the amount of energy in the electron bead may possibly be enough to explain the results. The third possible case involves vaporization where the energy available under conventional circumstances would not be enough. There are some indications for both melting and vaporization.

Another anomalous effect concerns the lifetime of the electron beads. In a chamber the maximum lifetime is about 40 nanoseconds. This maximum lifetime is only obtained by using a small amount of ionized xenon gas in the chamber. In contrast there is a much longer lifetime if the condensed charge is channeled along a ceramic groove.

Some cold fusion researchers believe that such condensed electron charges could be the source of "particles" that could provide the necessary "mass" to account for the increased probability of "tunneling" through the Coulomb barrier and thus produce fusion of deuterium ions. Such electron clusters could form from dendrite tips on the surface under high potential or form in arcs during internal sub-surface fracturing of the palladium as it is being expanded by the deuterium loading. Such fractures have been shown to be able to develop a few thousand electron volts.

The issue as to whether such electron beads could catalyze cold fusion in a manner analogous to the screening effects of muons is, as yet, unresolved. The electrons in a cluster may be fairly dilute with a first approximation of the concentration of electrons being only 10^{24} per cubic centimeter. If experimental evidence is found for a larger number of electron clusters then the results may become a reasonable explanation for some of the observed cold fusion activity. Further studies would have to be done to determine if such electron-bead promoted fusion could produce processes that will provide further clusters and thus provide a self-propagating process.

There are a few important contributions to the literature that should be studied by those cold fusion researchers or theorists who are interested in this significant new phenomena. It must be appreciated that the formation of stable clusters of electrons does indicate that some physical processes can overcome the repulsive barrier of electron charges to cause the electrons to be held together in beads by shorter-length stronger forces. The literature that has been identified is the following:

H. E. Puthoff, "Source of Vacuum Electromagnetic Zero-Point Energy", *Physical Review A,* Vol 40, No 9, pp 4857-4862, November 9, 1989, 25 references.

H. E. Puthoff, "The Energetic Vacuum: Implications for Energy Research", *Speculations in Science and Technology,* Vol 13, No. 3, pp 247-257, 33 references.

R. W. Ziolkowski & M.K. Tippett, "Collective Effects ...", *Physical Review A*, to be printed in the March 1991 issue.

G.A. Mesyats & D.I. Proskurovsky, Pulse Electrical Discharge in Vacuum, c 1989, Soviet book published by Springer-Verlag. [This book describes the electron cluster phenomena.]

Harold Puthoff, "Everything for nothing", *New Scientist,* pp 52-55, 28 July 1990. [A non-mathematical discussion of some new physics.]

H. E. Puthoff, "Gravity as a zero-point-fluctuation force", *Physical Review A,* Vol 39, No 5, pp 2333-2342, 33 references. [ABSTRACT: Sakharov has proposed a suggestive model in which gravity is not a separately existing fundamental force, but rather an induced effect associated with zero-point fluctuations (ZPE's) of the vacuum, in much the same manner as the van der Waals and Casimir forces. In the spirit of this proposal we develop a point-particle ZPF interaction model that accords with and fulfills this hypothesis. In the model gravitational mass and its associated gravitational effects are shown to derive in a fully self-consistent way from electromagnetic-ZPF-induced particle motions (Zitterbewegung). Because of its electromagnetic-ZPF underpinning, gravitational theory in this form constitutes an "already unified" theory.]

H. E. Puthoff, "Ground state of hydrogen as a zero-point-fluctuation-determined state", *Physical Review D,* Vol 35, No 10, pp 3266-3269, 15 May 1987, 20 references. [This paper discusses the interaction of charged harmonic oscillator with zero-point background.]

Lafferty (General Electric), Vacuum Arcs, c 1980, no further identification available at this time.

Patents: Patents have been filed by Puthoff and Shoulders and are expected to be issued by May 1991.

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EDITOR'S COMMENTS

The hypothesized effect of the Casimir force on particles such as electrons ("leptons") is proposed to be similar to that of the strong nuclear force on particles such as neutrons and protons ("hadrons"), allowing stable condensations (analogous to nuclei) to exist in spite of electrical repulsion. Once the Casimir force, which increases extremely rapidly at short distances (proportional to the fourth power of the distance), becomes dominant over the Coulomb repulsion (which is only proportional to the second power of the distance), the electron bead should collapse.

The limits to such a collapse are not immediately obvious, since electrons are commonly treated as point particles without structure or dimensions (unlike neutrons and protons). However, degeneracy (an inability of two particles with the same set of quantum numbers) to occupy the same space) may limit the collapse. The existence of a limit would be consistent with the apparent maximum number of electrons per bead which was reported, in the same way that the heaviest known nuclei (such as ^{238}U) are not stable and spontaneously fission.

A cluster of say 10^{10} electrons with nuclear or subnuclear dimensions and a mass of close to 10^{7} amu (atomic mass units, approximately the weight of proton or hydrogen atom) would of course be an exceptionally good catalyst for fusion reactions: even the subatomic particles known as muons, with 207 times the mass of the electron mass (approximately 1/10 amu) can catalyze over 10^{10} fusions per second, without the atoms needing to have significant kinetic energies as in thermonuclear fusion. Such an electron cluster could conceivably catalyze even fusion reactions involving far heavier elements than hydrogen; all elements lighter than iron could release energy when fused. In addition, however, a surprising number of the other puzzling aspects of cold fusion could also follow directly from such a mechanism.

Even in the d + d fusion reaction, unusually large changes in the branching ratio could be envisioned. For example, the d + d --> p + t branch could be much more strongly favored over the d + d --> n + ^{3}He branch because the orientation of the deuterons as they are accelerated together by the electric field of the cluster would be far stronger than in the conventional Oppenheimer-Philips effect. Other nuclear reaction branches such as d + d --> ^{4}He could also take place without being constrained by conservation laws to be accompanied by gamma radiation. As previously observed by Bockris with charged dendrites, the orientation of two deuterons in the vicinity of a highly charged electron bead could cause the deuterons to be oriented so that the tritium or the $_4$He branch would prevail.

In addition, secondary reactions could be strongly suppressed because any charged particles (nuclei) produced, being positively charged, would be extremely strongly attracted to the cluster by its electrical charge. (Even ^{4}He nuclei produced with 24 MeV of kinetic energy could be expected to be retained by a sufficiently high electrical field.) Thus, for instance, tritium could be formed without the accompaniment of 14-MeV neutrons which would ordinarily result from secondary collisions of the tritons with other deuteriums. Secondary X-rays, ordinarily produced by interactions of any sufficiently energetic charged particles with metal atoms, could also be eliminated. Heating of the lattice would still be expected. The previously reported localized pitting of active electrodes could also result.

Difficulty in obtaining a cold fusion effect would not be surprising in that very specific conditions could be necessary in order to create even one bead in the first place. For example, high loading leading to brittleness, combined with nonequilibrium phenomena (such as hysteresis effects during electrolysis of Pd deuteride, or phase transitions during temperature changes in Ti deuteride), may be necessary to generate beads during cracking. An extremely localized area in which fusion takes place would also be as expected.

The burst-like nature (and the suggestion that some active Pd samples continue to give the best results even if remelted) could also be consistent with the fact that the nuclei produced in the fusions, even He produced with a kinetic energy of 24 MeV, could experience such a strong electrical attraction to the bead that they would remain bound to it and eventually physically shield it. (Again, a similar problem occurs in muon-catalyzed fusion, when binding of helium to the muon neutralizes its charge. In the present case, however, the major impediment would be expected to be physical.)

One final implication of this mechanism is that isolation of the clusters would allow them to be used to catalyze fusion anywhere. Indeed, while particular conditions in metal deuterides may assist in cluster creation, these would probably not be the same as those which yield the greatest fusion rates or the best engineering advantages.

At the Santa Fe Workshop on Cold Fusion Phenomena (May 23-25, 1989), a poster was exhibited with an enlarged photograph of a palladium electrode having small holes. Could an electron bead accelerated by fusion energy have been responsible for "drilling" these holes from the inside of the Pd cathode?

PRIMER: HELIUM AND ISOTOPIC CHANGES
by Michael Dehn, Associate Editor

HELIUM

Two stable isotopes of helium exist, ^{3}He and ^{4}He. The former is formed directly in approximately half of conventional d-d fusion reactions, d + d --> n + ^{3}He, and is also eventually formed by decay of the tritium (halflife 12.3 years) formed in the other main reaction, d + d --> t + p. In addition, approximately 1 in 10^7 conventional d-d reactions forms ^{4}He.

It was initially suspected, therefore, that half of all d-d cold fusion reactions would form ^{3}He. Significant experimental evidence has since suggested, however, that the d + d --> n + ^{3}He reaction is relatively infrequent. In addition, the general inability to measure enough tritium to account for the measured heat generation has raised the question of whether a far greater ^{4}He formation could be occurring, either from d-d fusion or some other reaction. (10^{11} d-d fusions to form ^{4}He would release 1 watt of heat.) Thus, a number of efforts to measure helium have looked for both ^{3}He and ^{4}He using mass spectrometry.

Background helium levels result from both the ^{4}He and ^{3}He contents of air trapped in samples and air leaking into the mass spectrometer. (Indeed, helium leakage is far more difficult to prevent than leakage of O_2 and N_2.) While the average ^{4}He content of air is approximately 5 ppm and that of ^{3}He is far lower, resulting in ^{3}He detection limits which may be several orders of magnitude more sensitive. (However, the ratio between these isotopes may vary considerably in other sources.)

It should also be noted that He levels in the surface 25 microns of at least one batch of as-received Pd rods was found to be 5-10 orders of magnitude higher than the helium content at depth ($<10^{12}$ moles/cm^3), leading to background levels orders of magnitude higher than if this layer had been removed by electropolishing [1].

Helium levels may be reported as either moles or atoms per gram or cubic centimeter. One mole contains 6 x 10^{23} atoms, and one cm^3 of Pd corresponds to 12 grams. The sensitivity of mass spectrometry is relatively low compared with other methods, typically with detection limits on the order of 10^{11-12} atoms/g. Thus, positive results may require palladium samples having a history of significant total heat production.

Some initial experiments, including that of Pons and Fleischmann, concentrated on the measurement of helium in the D_2 gas given off during electrolysis, on the assumption that helium produced by a surface fusion reaction would escape. Various authors have since pointed out, however, that helium formed within metals would typically be retained until the metal was heated to very high temperatures (between 600+ C in the presence of cracks and 1300-1600 C for isolated atoms and bubbles, with the relative proportions depending strongly on metallurgical factors) [e.g. 1]. Other studies have shown a similar behavior in metal hydride [2].

As a result, recent experiments have typically involved outgassing of hydrogen and deuterium from electrodes (to remove D_2 which interferes with helium detection), followed by very-high-temperature treatment or melting of Pd electrodes from electrolysis experiments [2, 3, 4]. To date, the most notable success has been reported in an independent analysis of the Pd electrode from the molten salt experiment of Drs. Liaw and Liebert of the U. of Hawaii [5]. Even in this case, however, ^{4}He levels found [6] were orders of magnitude below the level which could account for the previously measured heat production of 5 MJ.

Although I have not seen this suggestion in print, it seems possible that significantly more helium outgassing could in fact result during ongoing plastic deformation, in the same way that this mechanism can enhance tritium migration [7], but even this mechanism seems unable to release the helium with especially high efficiency. Similarly, I would presume that even helium production by a surface mechanism in which the products are given only very low kinetic energies (so that those emitted inward are not implanted deeper into the sample) would still cause some helium to remain in the sample.

ISOTOPIC CHANGES

Some theories have suggested the possibility that other elements such as Li or Pd are involved in a cold fusion reaction (for example, d+^{6}Li fusion or catalytic transfer of a neutron from d to Pd). For such a possibility to be testable by isotopic analyses (mass spectrometry), significant total heat production would again be necessary. [8] To date, there have been only rare reports of possible isotopic changes, such as a report of depletion of ^{105}Pd and ^{108}Pd, along with an enrichment of ^{106}Pd, at the surface of an electrode [9], although even this has also been suggested to have been due to instrumental error.

In the case of ^{6}Li, it should be stressed that the ^{6}Li content of commercial Li reagents varies several fold [9]. Thus, measurements of altered ^{6}Li/^{7}Li ratios after cold fusion experiments would not be significant unless the initial ratio were accurately established.

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[4] Jean-Pierre Blaser et al., "Experimental Investigation of Cold Fusion Phenomena in Palladium," *Chimia* 43, 1989, pp 262-268.

[5] B.Y. Liaw et al., "Elevated Temperature Excess Heat Production Using Molten-Salt Electrochemical Techniques," *Proceedings of the Special Symposium on Cold Fusion*, World Hydrogen Energy Conference #8, Honolulu HI, July 22-27, 1990.

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[7] Robert A. Huggins, "Fundamental Considerations Relating to the Electrochemical Insertion of Hydrogen and Palladium into Mixed Conductors," *Proceedings of the Special Symposium on Cold Fusion*, World Hydrogen Energy Conference #8, Honolulu HI, July 22-27, 1990.

[8] P. Frodl et al., "Possible Participation of Lithium in Fleischmann-Pons Reaction is Testable," *Z. Naturforsch. A* 45, 1990, pp 757-758.

[9] D. Rolison et al., "Anomalies in the Surface Analysis of Deuterated Palladium Metals," *Proc. First Annual Conference on Cold Fusion*, March 28-31, 1990.

E. BIBLIOGRAPHY UPDATE

BIBLIOGRAPHY UPDATE: THEORY

The following citations represent early theory papers which were not reviewed at the time.

Mark Ya. Azbel (Tel Aviv U), "Possibility of Cold Fusion," *Solid State Comm.* 76, 1990, pp 127-129. This note suggests hypothetical conditions in metals other than Pd or Ti under which significant fusion rates could occur within the Legett-Baym limitations. Very high and ongoing loading, narrow electron bands and high-energy metastable states are required.

T. Bressani (INFN, Torino, Italy), E. DelGiudice and G. Preparata (INFN, Milano, Italy), "First Steps Toward an Understanding of 'Cold' Nuclear Fusion," *Nuovo Cimento A* 101, 1989, pp 845-849. This note represents the first publication of Dr. Preparata's theory.

V. Capek, "Tunneling Efficiency and the Problem of Cold Fusion," *Czech J. Phys. B* 39, 1989, pp 793-795. This paper calculates enormous increases in tunneling at eV energies induced by the presence of the excited states; the tunneling mechanism is one involving interaction with the lattice, and thus does not operate in conventional fusion. Previous papers by Capek are cited: *Czech J. Phys. B* 36, 1986, p 1095; *J. Phys. C* 20, 1987, p 2901; and *J. Phys. (France)* 50, 1989, p 887.

S. Feng (UCLA), "Enhancement of Cold Fusion Rate by Electron Polarization in Palladium Deuterium Solid," *Solid State Comm.* 72, 1989, pp 205-209. This paper indicates that a high dielectric constant resulting from increased semiconductor character of Pd deuteride at high loading can greatly increase tunneling by decreasing the effective width of the Coulomb barrier, although not sufficiently to be the sole mechanism.

A.B. Hassam (U of Maryland) and A. N. Dharamsi (Old Dominion U, Virginia), "Deuterium Molecule in the Presence of Electronic Charge Concentrations: Implications for Cold Fusion," *Phys. Rev. A* 40, 1989, pp 6689-6691. This paper suggests that up to several-fold reductions in the bond length of a D_2 molecule forming in nonequilibrium positions astride a sufficiently large localized electronic charge concentration of high-Z metals such as Pd.

V.V. Komarov (Moscow State U, USSR), O. Melsheimer and A. Popova (Phillips-U. Marburg, Germany), "Does Cold Fusion Exist and is it Measurable?," *Z. Nature. A* 45, 1990, pp 759-761. The model described in this note calculates the existence of a resonant interaction between moving deuterons in a metal lattice, with the resonance energy depending on properties such as the electron density and lattice dimensions. The magnitude of the effect is indicated to be very large (comparable to that in muon-catalyzed fusion).

K. Langanke, H.J. Assenbaum and C. Rolfs (U. Munster, Germany), "Screening Corrections in Cold Deuterium Fusion Rates," *Z. Phys. A* 333, 1989, pp 317-318. This paper calculates increased fusion rates for D_2 in metals of 5-14 orders of magnitude due to screening effects, as well as several orders of magnitude when the molecules are in excited vibrational states. These effects alone are not, however, sufficient to account for measured fusion rates.

G.H. Lin, R.C. Kainthla, N.J.C. Packham and J.O'M. Bockris (Texas A&M), "Electrochemical Fusion: A Mechanism Speculation," *J. Electroanal. Chem.* 280, 1990, pp 207-211. This note discusses the possible role of deuteron acceleration by high electrical fields associated with dendrite formation during prolonged electrolysis. Similar presentations can be found in several of Dr. Bockris' later papers and conference presentations.

S.Y. Lo (Institute for Boson Studies, Pasadena CA), "Enhancement of Nuclear Fusion in a Strongly Coupled Cold Plasma," *Mod. Phys. Lett. B* 3, 1989, pp 1207-1211. This note presents an early calculation of the large enhancements in tunneling rates possible due to screening at sufficiently high loading.

J.L. Russell Jr, "Plausibility Argument for a Suggested Mechanism for Cold Fusion," *Ann. Nucl. Energy* 17, 1990, pp 545-548. This paper suggests that the ground state of the deuterium nucleus may contain a very small admixture of a virtual state consisting of two neutrons plus a neutrino (resulting in the occasional momentary disappearance of the Coulomb barrier), and that this process is favored in a metal lattice. Resulting H-D fusion would generate tritium without significant heat or secondary reactions, since the neutrino carries off nearly all of the energy. In D-D fusion to form helium, the electron would carry off the reaction energy, generating heat with small amounts of secondary products.

W. Schommers (Kernforschungszentrum Karlsruhe, Germany) and C. Politis (U of Calif. San Diego), "Cold Fusion in Condensed Matter: Is a Theoretical Description in Terms of Usual Solid State Physics Possible?," *Mod. Phys. Lett. B* 3, 1989, pp 597-604. This model calculates short-range attractive forces between deuterons in appropriate solids. (A second citation, *Mod. Phys. Lett. A* 4, 1989, p 1187, is merely an extended abstract of the preceding paper.) A response to this paper is given in V. C. Sahni, "Comment on 'Cold Fusion in Condensed Matter: Is a Theoretical Description in Terms of Usual Solid State Physics Possible?'," *Mod. Phys. Lett. B* 4, 1990, pp 497-498. This paper indicates an error in the preceding calculations.

Russell Seitz (Harvard), "Fusion in from the Cold?," *Nature* 339, 1989, p 185. This letter suggests that localized hot spots or cracks created by decomposition of oversaturated Pd deuteride may result in momentary hot fusion.

S.B. Zhu, J. Lee and G.W. Robinson (Texas Tech U), "Kinetic Energy Imbalance in Inhomogeneous Materials," *Chem. Phys. Lett.* 161, 1989, pp 249-252; and S.-B. Zhu, J. Lee and G.W. Robinson (Texas Tech U), "Nonlinear Effects on Thermonuclear Reaction Rates," *Phys. Lett. A* 144, 1990, pp 361-364. These notes discuss deviations from the Maxwell-Boltzmann distribution of deuteron velocities as a result of nonlinear effects in the presence of much heavier atoms such as Pd. Furthermore, the occurrence of a fusion event could create feedback to create local hot spots in which further fusions could take place.

Various authors have also cited the existence of theoretical papers on other possible low-temperature reactions prior to March 1989.

Richard L. Liboff, "Fusion Via Metallic Deuterium," *Phys. Lett. A* 71, 1979, pp 361-362, suggests the possibility of d-d fusion in liquid metallic deuterium at low temperature, due to a hypothesized large deuteron wavefunction overlap in such a boson fluid.

J. Rand McNally Jr, "Cold Fusion and Graser Prospects," *Fusion Technology* 7, 1985, pp 331-333, discusses the prospects of various other reactions which could possess significant extremely-low-energy resonances, and their potential use in a nuclear-powered gamma-ray laser.

A variety of related papers have also appeared:
E.W. Becker (Germany), "Triple Collision Reaction of Deuterons as a Possible Explanation of Cold Nuclear Fusion," *Naturwissenschaften* 76, 1989, p 214. This note was one of the first to point out that reactions involving three deuterons would be more likely in a cold fusion cell, and that this could allow the formation of ^{4}He without the gamma radiation in the d+d--> ^{4}He reaction, since the energy could be distributed between the He and the third deuteron.

James S. Cohen (Los Alamos National Lab.) and John D. Davies (U of Birmingham, UK), "The Cold Fusion Family," *Nature* 338, 1989, pp 705-707. This note offers an introduction to tunneling and to various possible phenomena such as resonance, fractofusion and catalysis by heavy charged particles.

W.N. Cottingham and D.A. Greenwood, "The Fusion Rate of a Confined Deuteron Pair," *J. Phys. G.* 15, 1989, pp L157-161. This letter calculates that a d-d separation of less than 0.2 A would be needed to reach Jones-type rates.

Vitalii I. Goldanskii and Fyodor I. Dalidchik (USSR Academy of Sciences), "Mechanism of Solid-State Fusion," *Nature* 342, 1989, p 231. This letter contains early suggestions of enhanced tunneling due to resonance transparency in the presence of multiple Coulomb barriers, and also mentions the possibility of fractofusion.

Charles J. Horowitz (Indiana U), "Cold Nuclear Fusion in Metallic Hydrogen and Normal Metals," *Phys. Rev. C* 40, 1989, pp R1555-1558. This early study of the implications of cold fusion resulting from conventional tunneling points out that the achievement of measurable rates by reducing the D-D separation would require distances of roughly 0.1 A, and would strongly favor p-d over d-d fusion. Cold fusion rates due to pressure and screening effects in Jupiter are estimated to be small. Unusual reactions such as p+d--> ^{3}He+e^++e^- or electron capture are also mentioned.

J.C. Jackson (U of Cambridge, UK), in "Cold Fusion Results Still Unexplained," *Nature* 339, 1989, p 345. This letter points out reactions involving neutron capture by palladium and deuteron breakup by neutrons or gamma radiation.

Jin Shangxian, Ding Yibing, Liu Yongzhen, Wu Bailu and Yao Decheng (Academica Sinica, China), "The Possibilities of Cold Nuclear Fusion of Deuterium," *Chinese Phys. Lett.* 7, 1990, pp 28-31. This paper (courtesy of Dr. Jin) estimates very large screening effects (due to correlations between ions, etc.) of up to 106 orders of magnitude, although this mechanism by itself is insufficient to yield measurable fusion rates under equilibrium conditions.

P. Nordlander (Rutgers U), J.K. Norskov (Technical U of Denmark), F. Besenbacher (U of Aarhus, Denmark) and S.M. Myers (Sandia National Lab.), "Multiple Deuterium Occupancy of Vacancies in Pd and Related Metals," *Phys. Rev. B* 40, 1989, pp 1990-1992. This paper calculates that in each transition et al. considered, sites at which an individual metal atom is missing can accommodate six or more deuteriums, but that only in Pd is multiple occupancy favored over single occupancy. However, this decreased separation by itself would not be sufficient to account for observed fusion rates.

O.E. Rossler, J. Becker, M. Hoffmann and W. Nadler (U of Tubingen, Germany), "Fermi Gas Like Hypothesis for Fleischmann-Pons Experiment," *Z. Nature. A* 44, 1989, p 329. This note was one of the earliest to point out the possibility of bosons such as deuterons being delocalized in a Bloch state. It also suggests that in such a case alignment of the deuteron spins using a nuclear magnetic resonance technique will quench the reaction.

Johann Schneider (Ispra, Italy), "How a Rectangular Potential in Schrodinger's Equation Could Explain Some Experimental Results on Cold Nuclear Fusion," *Fusion Technology* 16, 1989, pp 377-378. This note was one of the first to point out the possibility of greatly enhanced tunneling in a situation in which the Coulomb barrier has a rectangular rather than a Morse form. (A similar phenomenon underlies the transmission resonance model of Dr. Bush.)

Isao Shimamura (Institute of Physical and Chemical Research, Hirosawa, Japan), "Intramolecular Nuclear Fusion in Hydrogen-Isotope Molecules," *Progress of Theoretical Physics* 82, 1989, pp 304-314. This paper considers fusion rates in isolated molecules and ions, and rate increases of a few orders of magnitude are calculated for vibrationally excited states. An approximately five-fold increase in the electron mass would be needed to reach Jones-type fusion rates.

Zdenek Slanina (Czechoslovak Academy of Sciences), "Towards Molecular-Thermodynamic Aspects of Postulated Pd/D Low-Temperature Nuclear Fusion: A Useful Example of a Failure of the Conventional Translational Partition Function," *Thermochimica Acta* 156, 1989, pp 285-290. This paper suggests an increase in kinetic energy of particles confined within small spaces, which could help to increase the fusion rates of deuterons confined at sites in a metal lattice.

Leaf Turner (Los Alamos National Lab.), in "Thoughts Unbottled on Cold Fusion," *Physics Today*, September 1989, pp 140-141. This letter contained a very early suggestion that enhanced tunneling of diffusing deuterons (transmission resonance) can result from the presence of a regular series of Coulomb barriers. Sample calculations indicated the enhancement in Pd to occur near room temperature, and the narrowness of the resonance was also suggested to lead to difficulties in replication. Such a model was later developed in detail by Dr. Bush.

Among the negative papers were:

C.J. Benesh and J.P. Vary, "Fusion Rates of Squeezed and Screened Hydrogen Nuclei," *Phys. Rev. C* 40, 1989, pp R495-496. This note finds fusion rates to be extremely sensitive to both d-d separation and screening, and notes that such effects may vary from site to site in the lattice, but concludes that the necessary conditions to yield measurable fusion rates under equilibrium conditions would be unreasonable.

L. Bracci et al, "Nuclear Fusion in Molecular Systems," *J. Phys. G* 16, 1990, pp 83-98, concludes that large-effective-mass electrons alone, while able to cause large changes in tunneling rates, cannot provide sufficient screening.

O.B. Christensen et al, "H-H Interactions in Pd," *Phys. Rev. B* 40, 1989, pp 1993-1996, concludes that the high electron density in Pd metal does not lead to smaller d-d separations than in D_2.

R.S. Fishman and G.D. Mahan, "Binding of Charged Particles in Lattice Defects," *Phys. Rev. B* 40, 1989, pp 11493-11495, calculates the effect of enhanced screening on the separation between two deuterons occupying a single lattice defect, but concludes this effect is still far too small to generate measurable fusion rates.

William A. Fowler, in "Cold Fusion Results Still Unexplained," *Nature* 339, 1989, p 345, indicates in this letter that the rate of the reaction $d+d \rightarrow {}^4He+e^+e^-$ would be less than that of the ordinary $d+d \rightarrow n+{}^3He$, p+t, or ${}^4He$+gamma reactions.

Vitalii I. Goldanskii and Fyodor I. Dalidchik, "On the Possibilities of 'Cold Enhancement' of Nuclear Fusion," *Phys. Lett. B* 234, 1990, pp 465-468, concludes that electron screening, large effective electron mass, resonance, neutron transfer, and radiation-stimulated fusion are unlikely, although a nonequilibrium mechanism involving fractofusion (deuteron acceleration during cracking) may be possible.

Z. Henis et al, "Cold Nuclear Fusion Rates in Condensed Matter: A Phenomenological Analysis," *J. Phys. G* 15, 1989, pp L219-223, calculates that the necessary enhancement in the fusion rate solely through increased screening and decreased deuteron separation would not be expected.

A.J. Leggett and G. Baym, "Exact Upper Bound on Barrier Penetration Probabilities in Many-Body Systems: Application to Cold Fusion," *Phys. Rev. Lett.* 63, 1989, pp 191-194, is a companion to the authors' *Nature* 340, 1989, pp 45-46 paper calculating that enhancement of tunneling under equilibrium conditions, due to effects such as screening, is limited by the experimentally measured behavior of He in ordinary metals, including Pd.

Feng Liu et al, "Nature of Short Range Interaction Between Deuterium Atoms in Pd," *Solid State Comm.* 72, 1989, pp 891-894, not surprisingly finds no equilibrium d-d separations shorter than in D_2, and also notes that D_2 pairing in Pd has not been seen in NMR experiments.

Lawrence L. Lohr, "Electronic Structure of Palladium Clusters: Implications for Cold Fusion," *J. Phys. Chem.* 93, 1989, pp 4697-4698, reaches a similar conclusion.

Yale Jay Lubkin, in a letter in "Thoughts Unbottled on Cold Fusion," *Physics Today*, September 1989, p 144, suggests that destruction of Pd-Pd bonds by hydriding is the source of excess heat.

J.W. Mintmire et al, "Chemical Forces Associated with Deuterium Confinement in Palladium," *Phys. Lett. A* 138, 1989, pp 51-54, finds no equilibrium d-d separations shorter than in D_2.

Linus Pauling, in a letter "Explanations of Cold Fusion," *Nature* 339, 1989, p 105. This letter suggests that decomposition of a deuteride with an especially high D/Pd ratio could account for heat bursts.

Rebuttals of Gerald Rosen (Drexel U), "Deuterium Nuclear Fusion at Room Temperature: A Pertinent Inequality on Barrier Penetration," *J.Chem. Phys.* 91, 1989, pp 4415-4416 (suggesting high tunneling rates of vibrationally excited D_2 in metal lattices) appeared in *J. Chem. Phys.* 93, 1990, by John D. Morgan III, pp 6115-6117; Francesc Mas et al, pp 6118-6119; and Jean Durop, p 6120.

BIBLIOGRAPHY UPDATE - METALLURGY

F. Dalard (Domaine U, France), M. Ulmann, J. Augustynski and P. Selvam (U de Geneve, Switzerland), "Electrochemical Incorporation of Lithium into Palladium from Aprotic Electrolytes," *J. Electroanal. Chem.* 270, 1989, pp 445-450. This note describes experiments confirming the deposition of Li metal can occur on a Pd cathode during electrolysis of a Li-containing solution, as suggested in J. Augustynski, *Chimia* 43, 1989, p 99. Formation of up to four Li-containing phases ranging from incorporation of a few percent Li in Pd and ending with pure Li are inferred. In D_2O electrolysis experiments, Li incorporation may begin at an overvoltage of 0.6 V. Owing to the slowness of Li diffusion in Pd, alloying is expected to occur only within the surface few microns even near the D_2O boiling point, but the incorporation of even 1% Li on the Pd surface is nevertheless noted to significantly alter its electrochemical behavior.

Reed A. Howald, "Calculations on the Palladium-Lithium System for Cold Fusion," *CALPHAD* 14, 1990, pp 1-10. This paper presents updated information on the various phases formed by incorporation of Li into Pd, as calculations are indicated to have shown that deposition of Li metal on a Pd cathode is considered to be possible at the current densities used in cold fusion experiments (400 mA/cm^2 in the example cited). It is noted that eight different Li-Pd alloys are known.

L.E. Murr, "Palladium Metallurgy and Cold Fusion: Some Remarks," *Scripta Metall.* 24, 1990, pp 783-786. This note stresses the expected importance of understanding the role of Pd metallurgy, and especially the microstructure, on cold fusion through such techniques as electron microscopy. It is pointed out that high-temperature superconductivity is similarly sensitive to materials science details, and that fabrication and loading affect dislocation densities, grain size, etc. Finally, it is pointed out that solid-solution alloys with Co, Ni, Cu, Rh, Ag, Ir, Pt and Au is possible and allows considerable manipulation of the microstructure.

M. Sluiter, D. De Fontaine, X.Q. Guo, R. Podloucky and A.J. Freeman, "First-principles Calculation of Phase Equilibria in the Aluminum Lithium System," *Physical Review B* 42, 1990, pp 10460-10476 (courtesy of Dr. Milica Petek). Those interested in replication of the molten-salts method of Drs. Liaw and Liebert (U. of Hawaii) may wish to take note of updated information on the Al-Li alloy formed at the Al electrode. This paper also includes other recent empirical phase diagram data such as from A.J. McAlister, *Bull. Alloy Phase Diagrams* 3, 1982, p 177.

F. LETTERS FROM READERS

PALLADIUM/DEUTERIUM RATIO - THEORY
Courtesy of Dr. Robert T. Bush

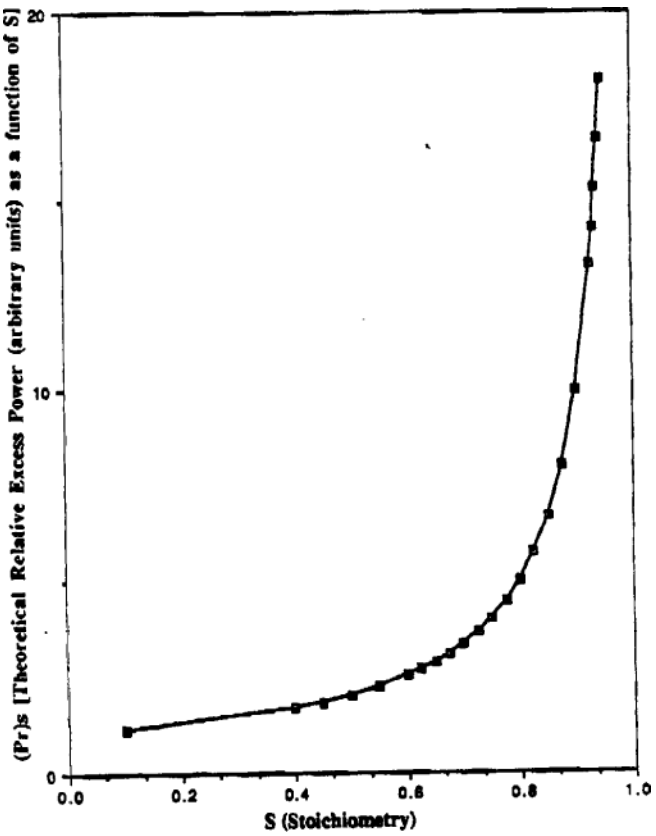
We are indebted to Dr. Bush, Cal Polytechnic Univ, Pomona, Calif. for the following notes and graph:

"Enclosed is my contribution accepted for the BYU Conference Proceedings. This paper is a new piece of work quantifying the role of stoichiometry in the TRM" [Transmission Resonance Model]."

Dr. Bush provides the following graph which depicts the theoretical **relative** Excess Power (in arbitrary units) versus the ratio of D/Pd [y-axis]. Note that the curve is quite flat over the range over the D/Pd ratio of 0.1 to about 0.7. Thereafter, the expected relative excess power rises remarkably with increases in the D/Pd ratio.

See the graph on page 25 column 1.

Following graph shows theoretical Relative Excess Power plotted against the D/Pd ratio:
Dr. Bush lists the following implications of this addition to his TRM model:



1. "Failure to achieve a high enough value of S [D/Pd ratio] should lead to failure in attempting to observe the Fleischmann-Pons effect of excess heat. In the absence of knowledge concerning the actual nuclear cross sections, however, the model does not precisely indicate what "high enough" means.

2. "Gentle loading, i.e., loading at current densities less than 50 mA/sq cm, seems likely to produce a more even loading of the D's into interstitial sites, implying the formation of longer unbroken chains [of PdD]. While I know of no extensive systematic study of this aspect, anecdotal information in the field appears to support this loading technique.

3. "Dislocations, such as those produced by work hardening of a cathode and grain boundaries would tend to produce local regions of relatively high D/Pd ratio by forming natural barriers to back up the interstitial D's in chains, but would, overall, tend to diminish average chain length. Thus, we might expect to observe an excess heat effect earlier in the loading of such a sample as opposed to the case for a well-annealed sample. However, the latter should produce a stronger effect when it is seen, all other factors being equal, due to the reduction in dislocations and grain boundaries. Again, this feature appears to be supported by anecdotal information.

4. "The development of cracks in the sample via the process of hydrogen embrittlement might very well be the limiting factor that would eventually end the ability to observe the excess heat effect with a particular cathode. The effect of crack formation would be to reduce the average unbroken chain length, since cracks will cut across chains and create dumping zones for D's that would otherwise be located at interstitial sites.

5. "The process of heat production is somewhat self-limiting, since the production of heat in a region of the cathode disrupts the chains in that region. Only after recrystallization of the local lattice and the enhancement of the local stoichiometry [D/Pd ratio] can this region again be productive. However, the thermally-induced diffusion of D's into neighboring areas of high average chain length [of PdD], produced by this local meltdown is capable of setting them off by virtue of providing diffusons for transmission along the chains in those neighboring regions.

6. "Triggering of the excess heat phenomenon by effecting changes in the energy shift at the surface of the sample (associated possibly with activation of concentration overpotentials) via current density changes or temperature changes has been discussed in previous papers by the author. However, we should note that perturbations, such as that produced by changing the current density might also operate to provide D's for transmission along chains. In this same vein, an AC signal superimposed on the steady current might increase the efficiency of the excess heat effect by producing backwards transmission along the chains.

7. "The achievement of relatively high D/Pd values [above about 0.65] might, indeed lead to the ignition phenomenon referred to by Fleischmann and Pons.

8. "Since the ratio of D/Pd moves inward in the Pd cathode [with time and electrolysis] from the surface to the interior, it makes sense that there is a surface phenomenon of excess heat as reported by Appleby et al. Thus, we would anticipate being able to observe the excess heat effect when the PdD chain length, near the surface has reach a high enough value. Lithium may play an important role in this effect. The effect of the lithium in the D/Pd ratio may account for why it takes a relatively long loading time to observe the Fleischmann-Pons Effect, as compared to the known diffusion times for D.

CONFUSION IN A JAR - NOVA PRODUCTION

After viewing the Nova production on cold fusion, "Confusion in a Jar", we issued a press release pointing out the disappointment that such a prestigious group as NOVA would interview mainly the scientists who are negative about cold fusion. We received the following comments from Evan Hadingham, Nova's Science Editor:

"... We were interested to read your comments on the show, but I disagree with your belief that the show presented the public with a biased and unbalanced view of the facts.

"... the commentary in the actual show makes it clear that some labs have continued to come up with positive results; however, because of the extreme inconsistency of these results, their interpretation remains controversial. This is what our commentary said in three separate places:

Of the two signs of fusion -- heat and nuclear products -- some groups saw one and some saw the other. Very few labs reported they'd seen both. This was partly because it was very difficult to detect the two at the same time, but also because they required sensitive instruments that were subject to error...

... Most skeptics have long since abandoned cold fusion research. But work has continued at the NCFI in Utah and in a handful of labs around the world. Some researchers consistently detect small amounts of fusion products. Some detect releases of heat. Some occasionally report detecting both at the same time...

...What's been overlooked amid all the controversy is that there probably is an unusual process taking place inside the test tube. But whatever it turns out to be, it has by now lost almost all serious claims to being a major source of energy. Eventually, painstaking scientific research will reveal the whole truth. But until then, what's going on in there will remain a matter of opinion...

"... I agree that it would be a good idea, if we'd had time, to look at some of the efforts going on in Pacific Rim labs, and to include the molten salt fusion experiments. Indeed, I'd appreciate a Xerox describing the Hawaii research so that we can evaluate whether we should add to or change our commentary in this respect. (However, even the account in your own newsletter implies that these experiments suffer from the same reproducibility problems as nearly all the other efforts in the field.)

"While the appreciation of your input is genuine, my confidence in your criticisms is shaken by the statement in your letter that you only believe in publishing confirmatory results in your newsletter, *Fusion Facts.*
Near the start of our film, Fleischmann himself emphasizes the importance of continuing efforts to try to disprove his theory. I'm bound to conclude that your publication is not only one-sided but has little to do with the scientific values endorsed by Pons and Fleischmann themselves".

"Yours sincerely, Evan Hadingham, Science Editor."

[Mr. Hadingham misinterpreted our publication policy. We find that with the enormous progress made in solving the problems of reproducibility that citing negative works can only bring embarrassment to those scientists who were not able to replicate cold fusion. By giving these scientists the opportunity of using the soon-to-be-published recipe for cold fusion replication, they will be able to correct and republish their findings. We regret that NOVA did not interview some of the many scientists (over 120 groups) who are achieving positive results. We will be pleased to furnish NOVA with the names and addresses of the increasing number of scientists who are achieving dramatic developments in cold fusion. We wish to thank Mr. Hadingham for taking the time to write to us. We still think highly of the NOVA series and trust they will do a better job on the next cold fusion production. Ed.]

I. CONFERENCES & CALL FOR PAPERS, ETC.

2ND ANNUAL CONFERENCE IN ITALY IN 1991 CALL FOR PAPERS

Courtesy of Dr. Fritz Will, Director, NCFI

The Second Annual Conference on Cold Fusion will be held in Como, Italy, June 29 to July 4, 1991. The General Chairpersons are Prof. Tullio Bressani (Experimental Physics, U of Torino) and Prof. Guillano Preparata (Theoretical Physics, U of Milano). The International Advisory Committee has the following confirmed members: John O'M. Bockris, Tullio Bressani, Martin Fleischmann, Hideo Ikegami, Xing Zhong Li, Mike McKubre, Howard Menlove, Stanley Pons, Renato Anfelo Ricci, Carlos Sanchez Lopes, Joseph Santucci, Francesco Scaramuzzi, M. Srinivasan, Akito Takahashi, Fritz G. Will. The third annual conference will be held in Japan, India, or China. Further details will be published here as soon as they are available.

CALL FOR PAPERS

Until further notice, urgent inquiries should be addressed to:
Professor Giuliano Preparata
Departimento di Fisica
Universita' di Milano
Via Celoria 16
20133 Milano, Italy

NEXT ANOMALOUS NUCLEAR EFFECTS CONFERENCE TO BE HELD IN ITALY IN 1991

Professor Steven Jones, BYU, announced that the next Anomalous Nuclear Effects In Deuterium/Solid Systems Conference will be held in Italy in the fall of 1991.
Fusion Facts will publish further information about time, location, and call for papers as soon as that information is received.

JOURNAL CALLS FOR PAPERS

Courtesy of Subbiah Arunachalam, Editor, IJT

The Indian Journal of Technology invites papers on cold fusion. Both original research papers and critical review articles in all areas of cold fusion are solicited.

Manuscripts may be sent, in duplicate, to Editor, Indian Journal of Technology, PID, Hillside Road, New Delhi 7110012, India.

TECHNICAL NOTES IN *FUSION TECHNOLOGY*

Fusion Technology has initiated and is continuing a very successful section for "Technical Notes" on cold fusion. This section in intended for fast publication of important papers on new directions, innovative ideas, and new results. Over the past year over 48 papers on cold fusion have been published, making *Fusion Technology* one of the premier professional journals covering this area.

Technical Notes do not have a page limit but they typically run 2-4 journal pages (1 journal page approx. = 3 double-spaced typed pages). A brief abstract is required. ASCII format computer media can be accepted.

Technical Notes will be reviewed but the process stresses rapid response. **Reviewers are instructed to consider Technical Notes as speculative, sometimes incomplete work that should be judged on the basis of innovation, originality, and importance to fusion power development. Appropriate citations to prior work are also essential.**

Deadlines for future issues are as follows:
August 1991 issue: February 20, 1991
September 1991 issue: April 2, 1991
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December 1991 issue: July 2, 1991

Send manuscripts to: George H. Miley, Editor, *Fusion Technology*, Fusion Studies Laboratory, University of Illinois, 103 S. Goodwin Avenue, Urbana, IL 61801. Fax (217) 333-2906. Phone (217) 333-3772.

CONFERENCE PROCEEDINGS AVAILABLE

Copies of the Proceedings of Anomalous Nuclear Effects in Deuterium/Solid Systems Conference, Oct 22-24, 1990 can now be ordered. The estimated publication date for the conference proceedings is currently March 1991. Copies can be ordered by sending a check for $55, payable to S & J Scientific Co., to:
Prof. S.E. Jones, Physics Department
Brigham Young University
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The First Annual Conference on Cold Fusion Conference Proceedings is now available. The proceedings include the papers presented March 28-31, 1990 at the conference. Send check for $55. Order from:
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The following publications have been helping to inform the world by publishing articles on cold fusion:

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