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McKubre, M. C. H., et al., *Development of Advanced Concepts for Nuclear Processes in Deuterated Metals*, EPRI TR-104195, Research Project 3170-01, Final Report, August 1994, 128 pages, plus 342 pages on microfiche. $200

Review by Jed Rothwell

Comprehensive, Meticulous and Definitive

This is one of the most comprehensive descriptions of a set of cold fusion experiments ever published. The only reports I know of that rival it are from F.G. Will et al.,[1,2] and M. H. Miles et al.[3] This EPRI book describes the research paid for by EPRI and performed at SRI International between 1989 and 1994 by M. McKubre, S. Crouch-Baker, F. Tanzella and eight other principal investigators. These are among the most careful cold fusion experiments ever done. The results are unequivocal. They were first described in 1991, and summarized in McKubre's 1992 lecture notice at MIT:

> Unaccounted, and statistically significant heat excesses have been observed on more than 40 occasions. The excess energies of these observations are larger than can be accounted for by known chemical or mechanical energy storage processes. Observations of excess power and energy are strongly correlated with the measured D/Pd ratio, to the imposed cathodic current density, and to a third process of unknown origin, with an extended time constant.

There is no room for doubt about these results. They are far above noise levels and potential errors, which were established with almost fanatical precision. (See the sidebar "Fifty Sigma Results.") Figure 1 shows an example in which total uncertainty from noise and potential errors was established at less than 0.1 watts, total electrolysis input (not shown here) was ten watts, and the excess ranged from 0.9 to 1.0 watts for 192 hours (8 days). While this is 'only' 10% excess, the Uncertainty line is far below Excess Power. This was established with exhaustive procedures described in detail in the text. For example, inlet and outlet temperatures were measured with two thermocouples at the inlet, and two at the outlet, with an accuracy of ± 0.001K. Here is how the flow rate was maintained and measured in experiments P12 through P16:

> The heat transfer fluid was pumped from the bath and past the cell inside the calorimeter volume by Fluid Metering, Inc. (Oyster Bay, NY), mode QV-OSSY constant displacement pumps. The mass flow rate was determined by pumping the flow to an auto-siphon device placed on a Setra model 5000L digital balance. Precautions were taken to ensure that fluid was not lost following its transit through the cell and before

flow rate determination.

This method allows accuracy of better than 0.01%; that is, 200 ± 0.01g per 240 ± 0.01 seconds. In a number of experiments rotometers were also employed.

Great effort was devoted to quantifying and reducing already-small errors, like the heat loss from conduction of the electrode and thermocouple lead wires going into the cell. Most workers ignore such factors because they are so small compared to the excess heat.

SRI has an interesting approach to doing control or blank experiments, in addition to the usual use of platinum electrodes:

> In general, two different experiments were performed simultaneously in the same bath the electrochemical cells were connected electrically in series, but the calorimetric systems were hydraulically in parallel. Separate pumps were provided for each calorimeter, and the flows from the two cells were multiplexed to a single mass balance. All measurements were multiplexed to a single multimeter that was periodically interchanged with another precalibrated meter. In this way, one series cell effectively acted as a standard for the other: if P_{excess} was observed not to be zero in one cell while it was zero in another, this difference was unlikely to be an artifact due to current source of voltmeter miscalibration. Further, the current-measuring resistors were interchanged, replaced, and removed and recalibrated during periods of excess power production, a procedure that reduced the likelihood that errors were associated with the measurement of current.

These details give you a sense of how much effort went into the experiments.

The tone of the writing is conservative, emphasizing how much we do not know. It is painfully honest in places. Difficulties and equipment failures are described in detail. You get a sense you are watching over someone's shoulder in the lab, or leafing through a lab notebook. The authors take nothing for granted. Most people simply state the cold fusion reaction must be beyond chemistry because it produces so much heat from such a small mass. In this book they quantitatively prove that assertion, by listing an inventory of chemicals in the cell, every possible chemical reaction, and the mass of all reactants in the cell. (See sidebar "Overkill Example.) They compute total joules from the reactants and show that only 2 to 3% of the excess heat could have come from chemistry. This is overkill; it is proving the obvious in meticulous detail. It seems a little silly. But it is good to see the authors bulldoze away all possible arguments.

When the authors do not have definitive evidence for something, and they extrapolate, they always say so:

> At very high fugacities . . . a distinct change in the slopes of the resistance/fugacity curves is apparent. This change of slope has been interpreted in terms of the occupation of tetrahedral sites at very high loadings. Firm crystallographic evidence, which has not yet been obtained, is required to substantiate this interpretation. [p. 2-8]

So much care has been taken, and so many doubts have been expressed and then laid to rest, that when they finally get around to making definitive claims you feel they must be on solid ground.

A Triumph, Yet Flawed

In spite the overall technical excellence of this work, it is flawed, for several reasons:

- Since 1989 SRI has made little progress. The cells do not produce more excess heat. Reliability and reproducibility have not improved. SRI is no closer to practical technology than they were in 1989. They do not appear to have made much progress solving the scientific problems either.

- SRI has been slow to try new techniques and materials like nickel or thin film and wire palladium. They have used bulk palladium and heavy water almost exclusively. (They tested loading with wire palladium.)

- These results are definitive, but they are not as dramatic as the boil-off experiments reported by Pons and Fleischmann in 1993.[4]

- The calorimeter has taken so many resources that other aspects of the research, like material sciences, have apparently been shortchanged.

- In a sense, the calorimeter is too good. It is finicky, and difficult to use. It takes a great deal of effort to set up an experiment with it. This does not encourage a spur-of-the-moment test of a new idea. A scientist who once worked with the calorimeter calls it “the great white elephant.” The complexity precludes many interesting experiments. For example, in about five minutes, Storms can turn off an experiment, open up his calorimeter, remove the cathode, weigh it and test it in various ways before it degases significantly or changes in other ways. Some spots in the cathode may even still be reacting after ten minutes, in heat-after-death reaction. Removing the cathode from the complex SRI calorimeter take an hour or more. By the time the cathode can be placed on a scale or in a detection instrument, its composition and properties may change drastically. Any lingering heat-after-death reactions will surely stop.

- As McKubre said in his closing remarks at ICCF6, this calorimeter is designed to maintain a steady state, yet most people think it is essential to jolt the cold fusion cell with rapidly changing conditions. [5] McKubre now questions whether this was a “wise” choice of designs. Other scientists expressed these doubts years ago.

- After the tragic, fatal accident in 1992, the laboratory was arranged with the calorimeter and all instruments in a locked area surrounded by bulletproof glass. During an experimental run, the workers are not allowed to go near the experiment. Scientists from outside of SRI have told me this is impossibly restrictive. Hands-on contact with the instruments is vital.

Some other cold fusion scientists express dissatisfaction and resentment toward SRI. They feel that with all of the resources and talent at SRI, more progress should have been made. They say the research has been misguided, the wrong goals have been set, mistakes like the calorimeter design have been left in place too long. The calorimeter should have been simplified and changed to allow rapid changes. New, more innovative techniques and materials like nickel should have been tried. More attention should have been paid to materials, techniques, and the operating temperature, and less to improving the calorimetry.

Edmund Storms described the root of the problem. He thinks that SRI's research has been too much influenced by the opposition. Skeptical doubts and objections should not be ignored, but they should not be elevated to become the overwhelming design criteria for the experiment. Storms warns of "falling into the tarpit of other people's expectations" in "a research agenda driven by skeptics." The skeptics claimed that extreme precision is needed to prove the cold fusion effect exists. In fact, says Storms, such precision is not needed, and it adds no statistically meaningful confidence to the data. Fifty sigma results are not "more convincing" than 25 sigma ones. He feels that McKubre went too far when he did things like track heat losses via the thermocouple leads. He also believes that it is better to boost the signal by improving cathode materials than it is to relentlessly reduce the noise the way SRI has done. McKubre, upon hearing Storms' comments, protested that they have been trying to do both. It is a question of emphasis, and deciding which should be given a higher priority. Storms and I lean toward boosting the signal. McKubre has not sufficiently explored the kinds of techniques Storms describes. (Or if he has explored them, he did not describe the work here.) In the end, at ICCF6, McKubre agreed that the concerns of the skeptics may have interfered with good experiment design. He said:

> What we have done in part, partly in response to our critics, is to make calorimeters in such a way as to improve the data quality. You improve the data quality by averaging things for a long time, never changing anything . . . In doing that, of course, you maintain the system as closely as possible in a steady state. We have engineered and designed our systems to do that. That is what flow calorimeters do well. I question, now, whether that is wise. [5]

The Establishment Not Convinced

The biggest problem with the SRI results is that they have not convinced the wider scientific community and the scientific establishment that cold fusion is real and important. This, of course, is establishment's fault, not SRI's. These results were duly published in major, peer reviewed scientific journals long ago.[6] The work was described in articles in the Wall Street Journal, on ABC, National Public Radio, and in lectures at MIT, at the International Conferences, and elsewhere. The establishment should have taken notice years ago. But it did not. To say the least, this calls into question the traditional model of scientific progress.

EPRI and SRI have followed the rules. They have done what scientists are supposed to do: they published impeccable, utterly convincing research in top-ranking, peer-reviewed journals. Then they stood quietly aside, politely waiting for applause and recognition. The applause never came, and by now it is obvious that it never will come. McKubre feels that cold fusion will only attract attention when it can be demonstrated as a viable technology. He regrets that he has not been able to do that. In June 1996, during an interview on National Public Radio, he said:

> People's attention needs to be grabbed by something that's simple, unarguable, concrete and rugged, and it has to be simple enough to explain it to the average person or average politician. And it really has to be a lot more robust than anything that we have generated so far.

He is right. We probably do need simple and rugged machines. That is a shame and it is unfair to McKubre and other cold fusion scientists. McKubre's experiment is unarguable. It should not have to be simple, concrete or rugged. These have never been held as standards for believability in science. Things like the Fermilab top quark machine and hot fusion reactors are as far from being "simple and rugged" as anything can be.

SRI could have done more to convince the public with what they have already. Neither SRI nor EPRI has made efforts to publicize the result beyond the usual methods of publishing in technical journals and lecturing at conferences. They have not held press conferences, or gone out of their way to attract media attention. They have made no effort to "explain it to the average person or average politician." SRI's scientific papers are models of clarity and good expository writing, but they are challenging to read. I could easily explain SRI's experiments, their results, and the significance of the results in language that an average person or politician would understand. Anyone with experience writing computer manuals could do it. SRI has never tried.

Why has SRI shied away from press conferences and Sunday Supplement simplified explanations of their research? Part of the reason may be because cold fusion scientists have been attacked because of the perceived fiasco of the 1989 University of Utah press conference. Critics say that cold fusion is "science by press conference." McKubre and others have been careful to avoid that stigma. In fact, the "science by press conference" accusation is gross hypocrisy. Science is always done by press conference, and it always has been. Major experiments at the hot fusion Tokamak reactors are featured on the seven o'clock news and the front pages months before scientific papers are published.[7] Things like the Hubble Telescope; possible traces of life on Mars in Arctic rocks that might have come from Mars; progress in AIDS, and other major science stories are orchestrated and hyped every day in the media and in Congress. There was nothing wrong with holding a press conference in 1989, and certainly nothing unusual about it. They should have held a dozen more press conferences. The only problem with the Utah announcement was that preprints of the Pons and Fleischmann paper were not made available that day, in the auditorium.

Internal politics may be another reason SRI has kept this work obscure. There is a great deal of hostile opposition to cold fusion within EPRI, from people like T. Schneider, as expressed in his Introduction to Hoffman's book *A Dialog on Chemically Induced Nuclear Effects.*[8,9] This internal opposition eventually led EPRI to stop funding most of the research at SRI. The Japanese NHE program is now funding it instead. The supporters of cold fusion within EPRI and SRI should change their political strategy. They have stuck with it long after flaws were revealed, just as they stuck with the calorimeter design and experimental protocols for too long.

Nuclear Evidence

The book contains a short section on the search for evidence of a nuclear reaction. They found no significant helium or transmutations of the host metal, but they did find evidence of radiation in an autoradiograph (Fig. 2).

> Clear evidence of some type of ionizing radiation is observed. The points of light with diffuse halo exposure suggest that some type of radiation may be coming from point sources within the metal and being scattered by the lattice structure. p. 3-19

Approximately 10% of the total mass of some cathodes, including parts of the surface and bulk, where analyzed by Rockwell International for helium. No significant levels of helium were found. Based on the work of Miles and others, it seems likely that if helium was generated by the reaction, it would have escaped into the electrolyte. The surface and near surface were subjected to laser ionization (SALI) to look for evidence of fission transmutation: changes in isotopic composition and expected elements. None were found. It is a shame they did not look more deeply, where Mizuno and Minevski found transmuted metal. The authors do not think they can confirm or deny the nuclear hypothesis. They write:

> We can conclude that if a nuclear process gives rise to the heat, it does so largely by the production of stable nuclei which so far have not been detected but have not yet been seriously sought.

According to Tom Passell of EPRI, not "seriously sought" means that helium has been detected but sources of contamination, like helium leaking in from the air, have not yet been rigorously excluded.

After the book was published, Passell began a series of experiments looking for transmutations in the used SRI cathodes, using prompt gamma activation analysis (PGAA). He believes he has found an ~18% reduction in the ratio of boron-10 to palladium-105 in a cathode that generated excess heat at SRI.[10] This work is still underway.

The Report Summary makes a bold claim about nuclear evidence:

> Although nuclear reaction products commensurate with the excess heat have not yet been observed, small but definite evidence of nuclear reactions have been detected at levels some 40 orders of magnitude greater than predicted by conventional nuclear theory.

Passell explained that the "40 orders of magnitude" refers to research in other labs, particularly in neutron detection.

Confusion about Percentages and Electron Volts Per Atom

In two or three places, the book repeats a statement summarizing results. This was also included in the 1991 ICCF2 paper:

> For the thermodynamically closed and intentionally isothermal systems described here, output power was observed to be as much as 300% in excess of the electrochemical input power or 24% above the known total input power. When excess power was present, it was more typically in the range 5%-10%, in a calorimeter that was accurate to better than +0.5%. The largest excess energy observed corresponded to 1.08 MJ, or 45.1 MJ/mol, or ~450 eV/atom normalized to the Pd lattice or to the deuterium in the palladium at a loading of ~1.
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> p. 1-5

Variations of this 5% to 10% number appear. A table (3.1, p. 3-3) shows Total Energy Excess in megajoules and percent for 15 experiments. The percent numbers range from 0.24 to 3.88%. These are meaningless averages. The 0.24% represents the total excess heat generated by the cathode compared to all sources of energy that went into the cell during the entire experimental run, including the initial incubation period, which sometimes lasted for weeks, starting with one week preparation at low current. Unfortunately, Morrison and other members of the opposition frequently quote these averages out of context, spreading doubts and confusion. They say that a mere 0.24, or 3.88 or 5% excess must be close to the noise. This like asserting that I drive 10 miles per day, so my car goes an average of 0.42 miles per hour during the 24-hour day, so it would be faster to walk. When the car is moving, it goes much faster than 0.42 mph. When the SRI cells produce excess heat, they usually produce much more than 0.24%.

The average is much lower than it would be in most cold fusion experiments, because of the design of the SRI calorimeter. SRI employs a compensation heater to keep the temperature in the cell the same throughout the experiment. When the experiment begins, the compensation heater is turned on high; power from electrolysis and excess heat is zero. When electrolysis goes on, the compensation heater automatically decreases to balance. Heater plus electrolysis power together keep the cell at the same temperature as the heater alone did at the beginning. When excess heat turns on, the heater power decreases again. When excess heat fades away, the compensation heater turns back up. The point is, this 0.24% excess represents of the excess energy compared to all of the energy input into the cell from all sources, including the compensation heater. In most other experiments, there is no compensation heater; cell temperature is allowed to increase and decrease freely.

This confusion about percentages is compounded by another pernicious “skeptical” argument: the stored energy hypothesis. It has often been asserted, even by distinguished scientists like Frank Close, that a cold fusion cell is some kind of energy storage battery. The idea is that the electrical energy input during electrolysis is stored in chemical bonds (electron bonds), and then released later in the form of heat. David Williams, Professor of Chemistry at University College London, believes in the battery hypothesis. He calls cold fusion “an inefficient, unreliable, dangerous and expensive energy storage method.” If cold fusion is anything like a battery storage system, it must be millions of times more efficient than any other form of chemical storage, and it violates the established laws of chemistry.

McKubre specifies the order-of-magnitude energy release per atom of the cathode material as 240 eV (electron volts) per atom of cathode material. Pons and Fleischmann and others also use this standard. They have seen much higher energy releases, thousands and even hundreds of thousands of eV per atom. McKubre's statement "~450 eV/atom normalized to the Pd lattice" should make all skeptics realize that chemical energy storage is impossible, but even after six years the arguments continue, so I will point out some basic physics that McKubre does not spell out.

The largest amount energy that can be stored in any electron bond has long been established by both theory and experiment. Most high energy density reactions, like burning coal, release 2 to 4 electron volts (eV) per atom. Some exotic reactions might theoretically produce as much as 18 eV per atom, but this has not been observed.[11] Therefore, a release of hundreds or thousands of eV per atom is far beyond the limits of chemistry.

Some skeptics have said that the energy release should be normalized against all of the hydrogen atoms in the system, not just those in the palladium lattice. There is far more hydrogen in the water than in the lattice. This makes no sense. The heat originates inside the cathode. The hydrogen in the water is chemically inert; water does not burn. Some new hydrogen from the water is pushed into the cathode from electrolysis, but the cathode is already supersaturated. The new hydrogen merely displaces some of the old hydrogen, which bubbles out, rises to the surface, and is recombined with free oxygen. This mechanism alone would not produce any excess heat. When an atom is pushed into a lattice and it displaces another, there is no net gain or loss of chemical energy, because no new chemicals are formed. Some skeptics have suggested that the new hydrogen atoms entering the lattice "surrender their energy," whatever that might mean. Then they leave, to be replaced with others, which surrender more energy. No known mechanism would allow this to happen, and even if it did happen, it would only act as a heat pump, cooling the electrolyte and warming the cathode, with no net excess heat from the cell.

Finally, it should be noted that energy is not stored up during periods when there is no excess heat. Except for the initial loading period when the palladium hydride is formed in a few hours, input is balanced by output as closely as can be measured. If energy was being stored up in a battery, the reaction would be endothermic: the heat energy output would be less than the electrical energy input. But when a cold fusion cell does not produce excess heat, all of the electrical energy that goes in comes right out again in the form of heat. Some energy is first converted to chemical energy in the form of free oxygen and deuterium gas, but in the closed SRI calorimeter, the gas is recombined back into heavy water, and the energy is released in the form of heat. The skeptics counter that perhaps the energy is stored up very slowly, at tiny power levels that cannot be detected. During the two-week incubation period that proceeds the onset of excess heat, the skeptics claim that the cell is absorbing a few milliwatts of heat, at power levels too small to be detected by the calorimeter. This might be the case with ordinary calorimeters, but the SRI calorimeter is so sensitive it would detect the imbalance. The negative (endothermic) power levels preceding the largest bursts would be well above the noise level.

Current Reversal Technique

The book describes an interesting technique which has not been highlighted in other publications or lectures. To help keep the cathode clean, the workers periodically "strip" the cathode by reversing the current and making the cathode into the anode and vice versa. (That is, they make the palladium electrode positively charged, instead of negative, so that it attracts oxygen instead of hydrogen.) This was described in a paragraph that gives the reader a sense he is looking over someone's shoulder, watching a group of skilled people make mistakes that lead to fruitful discoveries.

> 3A.3.4 Period 4
>
> Unaware of the possible existence of excess power within the calorimeter, the operators prepared to anodically strip the cathode surface at a low current density by first reducing the current to 30 mA (~10 mA cm^{-2}) and then reversing the current. This sequence led to extraordinary observations of resistance and thermal anomalies, shown respectively in Figures 3A-4B and 3A-4f.
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> p. 3A-8

Passell explained that by making the anode a cathode for a while, you dissolve some of the metal surface. He describes this as a "major technique" to boost loading.

Heat Promotes the Cold Fusion Reaction

The book ends with an all-too-brief discussion of the role of heat in promoting the reaction, a topic critical to the science of cold fusion:

> The apparent discrepancy between our observation of a roughly linear dependence of excess power on current density and that of Fleischmann and Pons of a roughly second-order dependence may be resolved by understanding the influence of temperature. We have designed our experiments, as closely as possible, to decouple the influences of temperature and current density; calorimetric experiments are performed under constant input power conditions, approaching the steady state. Our calorimeters also have large heat conduction so that, even in the condition of substantial excess power, the cathode temperature is not raised appreciably. Under these conditions, we observe an approximately linear response of excess power with current density.
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> In the experiments of Pons and Fleischmann, the current density is raised without reducing the power in a compensatory heater, in a calorimeter with very low heat conduction. For this reason, the cell temperature rises markedly with increasing current density, and the temperature increase is exacerbated by the onset of any excess power production. Under these conditions, Fleischmann and Pons observed a second-order or

higher power dependence of excess power on current density, with no observed maximum.

Taken together, these results suggest that the rate of excess enthalpy production increases at least transiently, with increasing temperature. Such a degree of autocatalysis would be useful in achieving large power gain; the apparent positive derivative with temperature suggests that it may be possible to produce heat efficiently at a usefully elevated temperature.

p. 5.5

McKubre did not always succeed in decoupling the influence of temperature. In my review of ICCF6, [12] I described an incident cited by McKubre in which a heat burst was caused when the flow was accidentally blocked and the cathode temperature rose higher than planned. The event was shown in two graphs from the ICCF4 proceedings (Fig 3, 4). [13] The flow was blocked just before hour 1952 for four hours, when a piece of black electrical tape got into the pipe. This was discovered just before hour 1956, the problem was corrected, and the excess heat began to drop, fading an hour later. Total energy from the burst was 368 kilojoules. If I had been in charge of the research, I would have immediately throttled the flow again to see if that caused another heat burst. Many important scientific discoveries have been made by following up such fortuitous accidents. Yet this incident is not described in the *Final Report*. I see no evidence that the people at SRI followed up on it. As far as I know, nobody working with the flow calorimeters IMRA or the NHE lab has tried throttling the flow.

Fifty Sigma Results

The introduction to this volume leaves no doubt that authors believe they have fully reproduced and confirmed the cold fusion effect. This is an unequivocal endorsement of the claims of Pons and Fleischmann.

EPRI PERSPECTIVE This work confirms the claims of Fleischmann, Pons, and Hawkins of the production of excess heat in deuterium-loaded palladium cathodes at levels too large for chemical transformation. However, the phenomena were obtained in only about half the cells. From the conditions of loading, initiation time, and current density on the successful observations of excess heat, it is understood why the phenomena are so difficult to attain. The conditions in the successful cells were not entirely under experimental control because the closed cells slowly leach silica and other materials from the anode and its supports as well as from the cell walls . . .

The primary objective of further work on this subject will be to demonstrate which nuclear reactions, if any, are generating the excess heat. The only way to do this is to observe in at least roughly quantitative fashion the nuclear reaction products or "ashes." At this time, it is thought that the most likely ashes will be helium of mass 4 observable in the vapor phase of closed cells. The reaction producing helium needs to be known in order to maximize this excess heat phenomena for practical uses in the nuclear power industry.

- Introduction

We do not claim to have examined all possible sources of systematic error in our calorimetry. However, highly instrumented and monitored experiments, using calorimeters of considerably different design and principle, have resulted in qualitatively and quantitatively similar results, i.e., apparent excess heat bursts outside the standard deviation of the random errors by factors up to 50.

p. 3-23

Contents of Book

This book consists of ten major sections; seven printed on paper, three on microfiche. It is too bad the entire book was not produced on CD-ROM and made available via the Internet World Wide Web. It would be much easier to use in electronic form; the page number is confusing, and there is no index. It includes 60 illustrations, graphs, charts and schematics; and five tables. This is a comprehensive, expensive book. It is somewhat repetitive. Much of the text was previously published in conference proceedings and peer-reviewed papers. The text not as repetitive as it first appears. The equipment was modified and improved before each set of tests. It is described again in each section, using text that has been copied and modified.

This book does not cover all of the SRI work on cold fusion. It does not include anything about the loading studies with palladium wire and other topics covered in some of McKubre's lectures. This work will be covered in another volume, Research Project 3170-01.

For a shorter look at the SRI experiments, see the article in the *Journal of Electroanalytical Chemistry*. [6]

Here are the section numbers and titles:

1. Introduction
2. Degree-of-loading Studies
3. Calorimetric Studies
3A. Experiment P19: A Comprehensive Data Compilation
3B. Experiment P1b
4. Nuclear Detection Experiments
5. Summary and Discussion

APPENDICES (Microfiche)
A. Lockheed Report - Nuclear Detector Experiments
B. Patents Disclosure
C. Accident Investigation Report

Item C. refers to the tragic accident on January 2, 1992, in which Dr. Andrew Riley was killed. This was also described in: S.I. Smedley et al., "The January 2, 1992, Explosion in a Deuterium / Palladium Electrolytic System at SRI International," *Frontiers of Cold Fusion; Proc. 3rd Int. Conf. Cold Fusion, Nagoya, 1992,* Universal Academy Press, Tokyo, 1993

Overkill Example

When a cold fusion cell produces megajoules of energy per mole of cathode material, most scientists consider that proof of excess heat "beyond the limits of chemistry," like a match that burns for an hour. The authors of other cold fusion papers have never bothered to quantify this assertion; they merely list the total energy output and leave to the reader to draw the obvious conclusion. In the SRI book, no assertion is left uncorroborated, even one as indisputable as this. They take the "skeptical" argument seriously, long enough to dispose of it. Experiment P19 produced 790 kJ of excess energy, mainly in three bursts. An inventory of chemicals in the cell; all known chemical reactions; and the total energy these reactions could produce are shown on pages p. 3A-14 through 3A-16. Here is the inventory:

		Species	Mols
Solid Phase *	Cathode	Pd	0.024
		D	0.023
	Anode	Pt	0.016
Solid + liquid phase		B	0.0004
Liquid phase		Li	0.02
Gas phase		D_2	0.002
		O_2	0.001
		D_2O	0.024

* A footnote here shows the level of detail taken into account: "Other solid parts are Al_2O3, SiO_2 and PTFE, which are considered in this analysis to be nonreactive.

Reactions and (approximate) energy releases are:

[1] $PdD_x \Leftrightarrow Pd + D_2$ ~1 kJ mol^{-1}

[2] $D_2 + 1/2\ O_2 \Leftrightarrow D_2O$ ~290 kJ mol^{-1}

[3] $2\ Li + 1/2\ O_2 + D_2O \Leftrightarrow 2\ LiOD$ ~683 kJ mol^{-1}

[4] $2B + 3/2\ O_2 \Leftrightarrow B_2O_3$ ~127 kJ mol^{-1}

An in-depth discussion of these reactions shows they are extremely unlikely because, for example, there is no "unlimited supply of oxygen" in the cell. If we assume oxygen does somehow get in, the four reactions would produce a total of 7 kJ, less than 1% of the total, or 3% of the energy produced by the larger continuous heat bursts.

Acknowledgment

Dr. Tom Passell of EPRI was most helpful to me in my quest to understand this EPRI publication. He answered numerous questions with enthusiasm, panache, and outspoken honesty.

Footnotes

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3. M. H. Miles, "Anomalous Effects in Deuterated Systems," Naval Air Weapons Center (NAWCWPNS TP 8302), 95 pages

4. M. Fleischmann (Univ. Southampton), S. Pons (IMRA Europe), "Calorimetry of the Pd-D2O system: from simplicity via complications to simplicity," *Physics Letters A*, 176 (1993) 118-129

5. M.C.H. McKubre, J. O'M. Bockris, "ICCF6 Summary and Reviews," *Infinite Energy* #10, p. 25

6. M. C. H. McKubre et al., "Isothermal flow calorimetric investigations of the D/Pd and H/Pd systems," *J. Electroanalytical Chem.*, 386 (1994) 55-66

7. For example: J. D. Strachan et al., "Fusion Power Production From TFTR Plasma Fueled with Deuterium and Tritium," PPPL-2978, March 1994. The experiment was trumpeted in the mass media when it was performed, five months before this first short paper was published.

8. N. Hoffman, *A Dialog on Chemically Induced Nuclear Effects*, American Nuclear Society (1993)

9. J. Rothwell, "REVIEW 'A Dialogue on Chemically Induced Nuclear Effects,' *Infinite Energy* No.3, July/August 1995

10. T. Passell, "Search for Nuclear Reaction Products in Heat Producing Pd," 6th Internat. Conf. on Cold Fusion, paper O-035

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