Review of the book “A Dialogue on Chemically Induced Nuclear Effects – a Guide for the Perplexed About Cold Fusion,” by Nate Hoffman. Published by the American Nuclear Society (ANS) with support from the Electric Power Research Institute (EPRI), 1995.

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OPEN THIS BOOK CAREFULLY

When you take this book of the mailing box, be careful. There is a one-page addendum you might drop on the floor or leave in the box. Don't lose it, because it contains the most important statement in the book:

> “ADDENDUM
>
> Comments were made in this text that the work performed by SRI INTERNATIONAL was difficult to examine in detail because that lab was reticent to share experimental details of a potentially profitable field of research. This experimental secrecy was partially lifted by the following Report to EPRI:
>
> McKubre, M. C. H., et al., 'Development of Advanced Concepts for Nuclear Processes in Deuterated Metals,' TR-104195, Research Project 3170-01, Final Report, August 1994.”

This is a strange little book. It is well written in some parts, with knowledgeable, in-depth, analysis. Yet elsewhere the author makes factual errors that might easily have been avoided. Some of his mistakes are mind-boggling, like his suggestions that chemical supply companies sell used moderator heavy water from CANDU fission reactors, or that no researcher in any cold experiment has ever measured true rms power. The focus of the book is wrong. It covers a few backwater aspects of cold fusion. It describes a handful of unimportant, botched experiments while it ignores the real work. The most important fact about cold fusion is that it produces excess heat beyond the limits of chemistry. As Fleischmann says, “heat is the principal signature of the reaction.” In most experiments, heat is the easiest parameter to measure, giving the highest signal to noise ratio. Yet Hoffman does not discuss any experiments in which excess heat was observed. He censors them out, he pretends they do not exist. This eliminates most of the literature. In the second paragraph of the book, Hoffman says that Pons and Fleischmann claimed excess heat, but that is the last we hear about the subject until the closing remarks. He never says that hundreds of other scientists replicated their findings. He never mentions any particulars about heat. There is no discussion of power; net energy; energy versus mass (megajoules per mole of cathode material); power density; temperature; current density and other triggering mechanisms; or metallurgical conditions and surface treatments required to generate excess heat.

This censorship of anything relating to heat is carried to absurd extremes. A short chapter on calorimetry, titled “Possible Artifacts Associated with Heat Measurements in Palladium /

Deuterium Systems" contains only speculation about hypothetical errors, and no actual calorimetric data from any experiment. Chapters that deal with things like neutrons, charged particles and helium contain references to the literature, samples of data from published experiments, and comments from researchers. But the chapter on calorimetry has no data from any cold fusion paper, even though the majority of papers deal with this subject. Other chapters have extensive bibliographies; this chapter lists four items from outside the literature (including a reference to the Fluke instrument company Catalog), and this whimsical little note:

> "*Note:* Listed bibliography concentrates solely on artifact considerations, not on the numerous studies of heat generation in deuterium / solid systems."

McKubre observed megajoules of excess heat per mole at high signal to noise ratios, using superb calorimeters, so Hoffman censored him. This Addendum lists only McKubre; it should have listed papers from Pons and Fleischmann, Kunimatsu, Storms, Oriani, Gozzi, Aoki, Huggins, Okamoto and dozens of other top scientists who Hoffman ignored. The Addendum of missing papers and data should have been as long as the *Fusion Facts* bibliography (more than 100 pages). On the back page, reviewer Tillbrook of the ANS describes this book as "wide in scope." It is just the opposite. This is a classic case of academic tunnel vision, in which the author refuses to examine 99% of the published papers because he disagrees with their conclusions.

Let us start at the end, and look at this afterthought Addendum carefully, because it epitomizes the whole book.

1. Hoffman asserts that SRI's work has been secret until the Final Report was published in August 1994. This is incorrect. SRI revealed copious details in 1991 at the Second International Conference (ICCF2);[1] at ICCF3 in 1992;[2] at ICCF4 in 1993;[3] in the Journal of Electroanalytical Chemistry article of 1994;[4] and at ICCF5. McKubre and others have discussed this work in formal lectures at MIT,[5] Texas A&M and elsewhere. The Final Report does contain new details, but it does not have any surprises or new information that would impact upon the arguments in this book. Everything Hoffman needed to know was published five years ago. On page 75, he gives one example of one detail that SRI has not shared. Just because SRI did not share one detail he wanted, that does not mean he can justify ignoring the great mass of information they <u>did</u> share.

2. Hoffman asserts that SRI's work was difficult to examine when the book was written. Yet every other review of the field written in the last five years discusses SRI's work, and dozens of other papers reference it.

3. He says that the lab was "reticent to share experimental details" because this field is potentially profitable. He asserts as a general rule that you cannot get sufficient scientific information in areas of research driven by industrial profits. Yet, most research and development

in most fields is for profit. Countless journals, magazines and textbooks are devoted to purely commercial R&D areas like semiconductors and oil refining.

4. This book was published with support from EPRI. The Forward was written by Thomas Schneider of EPRI. EPRI paid for McKubre's research at SRI and Wolf's research at Texas A&M, and it published the Final Report that barely made it into the Addendum. Schneider has been a bitter opponent of cold fusion for many years, but he must have been aware of McKubre's numerous publications over the years, and he must have realized that McKubre's data contradicts Hoffman's conclusions. He should have told Hoffman to include something about McKubre. Schneider and Hoffman want to present a one-sided picture. They want to prove that all positive results are due to "an unidentified error or artifact," as Schneider puts it.

McKubre's experiments are among the most important in this field. McKubre has been a keynote speaker at all five International Conferences, three of which Hoffman attended. Yet Hoffman does not even mention McKubre's work. This is not a minor oversight; it is a fatal flaw. Hoffman makes many incorrect statements about calorimetry and experimental technique. If he had taken the time to look carefully at McKubre, or Pons and Fleischmann, Storms, or any of the other mainstream workers, he would have caught these mistakes. The experiments do not fit the patterns he describes. The potential errors he speculates about *could not have occurred.* The equipment and protocols are designed to rule out these errors. In many experiments, the magnitude of the excess heat alone rules out the errors he speculates about. For example, he mentions that bubbles on cooling coils can sometimes insulate the electrolyte, raising the temperature slightly. In leading experiments the temperature rose thousands of times higher than bubble formation could explain.

For people who already know the field, this book is old hat, or a stroll down memory lane. A scientist who is thinking about doing these experiments for the first time who knows nothing about the field will find valuable information, although I recommend reading the original sources instead. Hoffman's writing style is clear and forceful. His own comments are often helpful, and to his credit he has brought good material from wide-ranging sources, like the checklist of potential errors measuring tritium from Carol Talcott-Storms on page 61, the statistical analysis by Tolly on page 89, and the Appendices written by G. Miley and B. Oliver. The bibliography is pretty good too, except, of course, it does not list the major papers about excess heat. (One or two minor papers about heat did creep into the chapter about tritium).

A Cute Presentation Format, But It Gets on Your Nerves

This book is written in an odd but appealing format. It is an imaginary Socratic conversation; a series of questions and answers between a Young Scientist ("YS") and an Old Metallurgist ("OM," Hoffman's alter ego). I have never seen a serious scientific book written as an imaginary conversation. In the chapters on history and the overview this format is effective, but in the technical chapters it becomes tiresome.

The tone of the writing is irritating. It is hypocritical. Hoffman trashes cold fusion by pretending the dramatic successful experiments never happened, and by grossly misrepresenting other experiments. He pretends to be impartial. He even makes a show of being solicitous toward the cold fusion scientists. He uses little of the heavy-handed mockery and harsh language you see in attacks against cold fusion by Huizenga, Taubes or Morrison. Unlike the American Physical Society officials, he makes no accusations of criminality or lunacy, he does not say that cold fusion scientists are half-wit suicidal Branch Davidians.[6] Hoffman criticizes Taubes. Occasionally, he praises a few experiments. In a short chapter titled "Experiments Without Apparent Artifacts" Hoffman says that Chien's experiment at Texas A&M, Claytor's work at Los Alamos, and a few other experiments seem to be free of error. He did not include any that showed excess heat. (He says that Russ George's work at E-Quest is "interesting" but he does not mention that it produces excess heat.) His Mister Nice Guy tone and his occasionally pat-on-the-head praise of a few cold fusion scientists will make a naive reader think that Hoffman really is even-handed, and he really has presented "a broad review" conducted in an "open minded" fashion, as the book jacket blurbs claim. A blurb from Steve Jones says:

> "Dr. Hoffman's treatise on anomalous nuclear effects in deuterided materials has been looked upon as pro-cold-fusion and paradoxically as anti-cold-fusion. It is neither. Rather, I see Dr. Hoffman's book as a significant effort to scrutinize relevant observations with care . . ."

This book does <u>not</u> "scrutinize relevant observations." It buries them! It ignores them, just as Jones himself does. This is a narrow, biased, anti-cold-fusion tract masquerading as a broad review. The relevant data has been stripped out because it contradicts the point of view Hoffman, Schneider and Jones want to foist on the reader. This book demonstrates why you should never depend upon a review of a scientific field, particularly a controversial field. You must also read original sources.

Masterful Analysis of Non-Issues

Many of the topics Hoffman chose to address are non-existent, non-issues. His analysis and presentation of these topics is sometimes masterful. He does a superb job in describing the problems, he includes the details, facts and hard numbers you need when you check the literature to see if these problems marred the experiments. But he does not say that the hypothetical problems he describes do not exist in the real world.

The short chapter about calorimetry "Possible Artifacts Associated with Heat Measurements. . ." and its whimsical phantom bibliography is described above. It is a masterpiece of obfuscation. In it, Hoffman carefully avoids saying anything about any actual cold fusion experimental result. On the surface, it is a technical discussion of hard facts. He talks about the Joule - Thompson effect in detail. Taking a 10.6 gram cathode (which he says is a "typical" mass but which I think is

larger than usual), he says: “The maximum loss of deuterium is from a stoichiometry corresponding to pressures of 0.2 in the shear modulus, which occurs at a loading of about $PdD_{0.9}$, to the stoichiometry of one atmosphere of D_2, which occurs at roughly $PdD_{0.65}$ as one can see from this phase diagram (Figure 6.2).” Work through the diagrams and figures, we find that with 10.6 grams of palladium, under extremely rare circumstances, the Joule - Thompson effect might slowly store and then suddenly release a one-time burst as large as 2.18 kilojoules. With some types of calorimeters, under some circumstances, this might appear to be a positive burst, rather than zero-sum energy storage and release. In many cold fusion experiments, including the ones at SRI, continuous generation from small cathodes adds up to far more than 2.18 kilojoules. Some cathodes have produced a hundred thousand times more energy than this. Therefore, cold fusion cannot be an artifact of the Joule - Thompson effect. Hoffman leaves the reader with the impression that this might be a significant artifact even though his own analysis proves it is not.

Other topics discussed briefly include the effect bubble formation on the insulating quality of the cell, and mixing. In any calorimeter, it is essential that you ensure the cooling fluid or electrolyte is properly mixed. Everyone doing cold fusion knows this, and in every experiment I have read about, proper mixing is assured. Mixing is important but so elementary that no scientist would overlook it.

Sometimes he goes to extremes, raising points that he must know are disingenuous. The worst example is at the end of the chapter where the gullible Young Scientist asks: “do the experimenters use rms voltmeters?” The Old Metallurgist answers like a crafty lawyer:

> “OM: To my knowledge, no electrochemist involved in these calorimetric studies uses rms meters. Calorimetry specialists have raised this point many times, but the heat-measuring cold fusion community has rather 'heatedly' replied that rms meters are not required.
>
> YS: On what basis do they justify not using rms meters?
>
> OM: Their claim is that they routinely examine the output signal with cathode-ray tubes and see no signal that is hashy enough to justify rms meters.
>
> YS: Certainly a cathode-ray tube readout should be a sufficient instrument for ruling out the necessity of rms voltmeters, but I'm surprised . . . ”

First of all, this is nonsense. Many cold fusion scientists use rms meters to measure power, as well as oscilloscopes, but more to the point, they all use computer systems with fast data acquisition boards. These not only measure true rms power better than most meters, but they also integrate power to measure energy. Nobody relies upon a meter. Does Hoffman imagine that graduate students are posted to watch a meter 24 hours a day, writing down the power levels and graphing them by hand?

The chapter concludes:

> "OM: Well, that is a quick rundown on possible heat artifacts. In general, these heat measurements are being done by very knowledgeable experimenters who know how to avoid artifacts."

How kind of Hoffman to mention this! As Antony put it: "For Brutus is an honorable man; So are they all, all honorable men . . ."

Mind Boggling Mistakes

Every so often, Hoffman goes off on a tangent and makes wildly incorrect statements. Most of chapter 3, Radioactivity Artifacts, is devoted to a discussion of used moderator water from fission reactors, which he believes may be causing artifacts in some cold fusion experiments. The discussion begins:

> "O.M. There are strong indications that commercially sold heavy water may contain variable contents of used moderator water from either CANDU-type nuclear reactors or Savannah River-type weapons production reactors.
>
> Y.S. What would indicate that?
>
> O.M. The indicator is the enormous variation in the tritium-to-deuterium (T/D) ratio in different batches of heavy water. . .
>
> Y.S. By why in the world would the commercial suppliers mix *any* used moderator water into their feedstocks?
>
> OM: As to why, one cannot just pour used moderator heavy water from these reactors down the drain – the tritium content is too high – and besides, heavy water is not all that cheap . . ."

This is followed by a five-page, in-depth discussion of how used moderator water might affect a cold fusion experiment. Hoffman speculates that uranium from the reactor might contaminate the water. He includes a table of Alpha Emissions for a nanogram of uranium decay isotopes. The first three pages of the next chapter describe how corrosion products from zirconium fission fuel cladding in commercial heavy water probably explain the isotopic and chemical changes seen in palladium cathode surfaces. This is a carefully presented, detailed, professional-looking exposition. It is also a preposterous flight of fancy. The levels of tritium and other dangerous contaminants in used moderator water are millions of times too high for the water to be cleaned up and sold to the public for any purpose. I faxed this section to Ontario Hydro, the company that produces 90% of the world's supply of heavy water for use in their Canadian designed CANDU

reactors. They responded with a detailed two-page fax, in which they show that Hoffman's idea is, quote: “pure nonsense.” They said that the specific activity of tritium in newly refined virgin heavy water varies from 0.05 to 0.17 µ-Ci/kg (microcuries/kilogram), depending on the method of refining and the site of the refinery plant. Heavy water with tritium levels above 2.0 µ-Ci/kg is considered dangerously radioactive, and must be handled by trained personnel wearing appropriate protective clothing, using special equipment. In an operational CANDU power reactor, the moderator water contains 10^7 µ-Ci/kg, and research reactors generally contain 10^6 µ-Ci/kg. Furthermore, this water contains many other contaminants even more dangerous than tritium, including long-lived alpha and gamma emitters. Removing these radioactive contaminants would be far more expensive than refining virgin heavy water from ordinary water. In order to reduce the tritium to safe levels you would have to mix the moderator water with virgin heavy water in a ratio of 1 to 100,000,000. Ontario Hydro concludes:

> “Used moderator water can often be re-sold, but only to other reactor operators. . . . Ontario Hydro dominates the world's nuclear market for heavy water and the world's non-nuclear wholesale market, and we have never attempted to use diluted, cleaned-up old moderator water for our non-nuclear markets.”

The letter points out that some of Hoffman's other speculation about heavy water is also wrong:

> “Theoretically, deuterium gas produced by electrolysis has lower D/T ratio than the source D_2O. However, in practice, there is no difference, as the separation factor is so small, and the operation is done in a single stage process. . . . [Hoffman's] suggestion that commercial D_2 gas suppliers must start with heavy water that is higher in tritium activity than any commercially available product any is also pure nonsense. Ontario Hydro is one of the world's major deuterium gas suppliers and we provide heavy water to one of the world's other major suppliers. The heavy water starting material is our normal virgin product.”

Hoffman went badly astray here. He made a wild guess that was wrong by eight orders of magnitude. He should have called Ontario Hydro to check out these weird ideas. Why didn't his editor or Schneider catch this? Did Schneider read the book before he wrote the Forward? The text that Hoffman himself wrote (excluding material from other authors) is only 130 pages, and he reportedly worked on it for about a year. In all that time he should have taken a few minutes to call Ontario Hydro. This is not a minor issue. If Hoffman had tucked this idea in a footnote, as a throwaway suggestion, then I suppose his editors and reviewers might have overlooked it. But he devoted eight pages to it! Six percent of the book! He devotes zero pages to excess heat; power density; temperature; triggering mechanisms; current density; cathode testing techniques and other topics most people think are the key to the phenomenon. And that is not the only glaring error in the book. It is the most egregious mistake, but the statements about rms meters and calorimetry run a close second.

The used heavy water idea is pure nonsense for another reason. Researchers looking for tritium always test the heavy water and cell components before the experiment, to establish the baseline of tritium and other radioactive elements. They remove samples of electrolyte during the experiment and test them again, or they use continuous on-line sampling. The graphs in the papers show no significant tritium at first, and then a burst of tritium activity after the cold fusion reaction begins. Obviously, if the heavy water in the bottle was heavily contaminated with tritium or other radioactive elements, people would notice! Any sensible scientist would cancel the test and look for heavy water with less contamination to start with, especially if the contamination was life-threatening, as it would be with used moderator water. In any case, tritium is always measured against a baseline starting point. They do not measure only once after the experiment concludes. Anyone who reads any cold fusion paper will see this.

Hoffman's publisher and reviewers at the American Nuclear Society and EPRI are among the world's leading experts in nuclear fission. EPRI performs the most advanced nuclear power reactor research and development in the world. They spend hundreds of millions of dollars designing new fission power reactors. How could a leading expert at EPRI like Schneider fail to realize that no chemical supply house could sell used heavy water? Did they forget that researchers always establish a baseline before measuring tritium? Jones asserts that Hoffman is "careful." Did Jones read the book? I believe that Hoffman, Schneider, Jones and the others did not overlook these mistakes. They ignored them, because they do not take this field seriously. They do not care whether the arguments marshaled against cold fusion are scientifically correct or "pure nonsense." They will use any tactic to discredit the field. They ignore positive experiments; they pretend that McKubre does not exist; and they toss out scientifically illiterate arguments like "used moderator water." They publish these absurd arguments on Internet and in books like this in order to confuse the issue. They want to drown out reasoned scientific debate with chaotic nonsense and mind-boggling mistakes. They want to lower the standards of science so that instead of looking at carefully derived data, superb calorimetry, and meticulously worked out theories, we go chasing off after used moderator water and other phantasmagoria.

Focus on Botched Experiments

Hoffman devotes much of this book to statements from people who are not qualified to discuss cold fusion. This reaches extremes in Chapter 8, in which he describes a farcical scientific conference held in 1991 called the Half Moon Bay Review, which was billed as "an in-depth review of cold fusion." It was sponsored by Schneider and arranged by Hoffman himself. There were three ground rules:

> "1. No firm 'believers' in cold fusion were to be included.
>
> 2. 'Skeptics' were welcome as long as we felt that such a scientist could be objective about presented data.

3. Each scientist panel member should be among the top few in the technical area for which he or she was selected."

In actual practice, rule 1 meant that no competent cold fusion scientist was invited to join the Review Panel, and rule 3 was ignored. The assembled "experts" specialize in neutrons and high energy plasma physics. Not a single one of them has ever published a paper on cold fusion or performed a cold fusion experiment. EPRI did not even invite their own expert electrochemist, McKubre, who is widely acknowledged as being one of the best in the world. This is like holding a conference on Tokamak hot fusion reactors and inviting only electrochemists.

The conference called in four experimental groups for the Review Panel to pass judgment upon: the Texas A&M cyclotron group (Kevin Wolf); Rocketdyne Helium Mass Spectroscopy Group; the Brigham Young University (BYU) 'Anomalous Nuclear Effects' Group; the Colorado School of Mines. Unfortunately, in 1991, none of these groups had ever done a successful cold fusion experiment. None of them have even done cold fusion calorimetry, which is at the heart of the experiment. The first two groups had assisted others in post-experiment analysis, but they themselves had not done any experiments. The Review Panel might have called in any number of scientists who had done competent experiments and seen positive results, but they decided not to. Theirs conclusion was pre-ordained, given the people they called in: "We were not presented with (nor are we aware of) any consistently reproducible data that shows anomalous nuclear effects in deuterium/metal systems." Perhaps if they had invited a scientist who claimed to see such effects, he might have told them about it.

Since there was no one on the panel qualified to judge cold fusion, and since they only invited scientists whose experiments failed, I think this Conference was a deliberate attempt to discredit the field. If I am right, the review panel will never reconvene and call in the same groups again, because subsequently two of them saw definitive evidence of cold fusion. Kevin Wolf saw dramatic transmutations, which is exactly the sort of nuclear evidence the panel claimed it was looking for. Wolf himself denies that this is evidence of cold fusion; he claims that the transmutations have nothing to do with the fact that the cathodes were in a test tube undergoing electrolysis with heavy water. He thinks that by a fantastic coincidence, the cathode was hit by a WIMP particle form outer space. No other scientist that I have spoken with believes that. Appendix D of this book is the report from the Rocketdyne Group, written in 1993. Long after the Half Moon Bay Review, Rocketdyne saw definitive evidence of helium in three groups of experiments. They dispute the first one, from China Lake (Miles), although their reasons are spurious. They do not dispute Texas A&M (Chien, Bockris) or E-Quest. Hoffman should have said this back in Chapter 8. He should have pointed out that subsequent work from two of these groups negated the conclusions of the panel.

Hoffman describes in loving detail some experiments performed by Steve Jones to detect low level neutrons. He devotes many pages to a whimsical experiment performed by Jones in which Portland cement was mixed with heavy water. This is interesting science. It sounds like fun. But it has nothing to with mainstream cold fusion. In any case, these experiments failed to produce

definitive results. At ICCF4, Jones retracted most of his earlier claims. Hoffman writes a lot about this sort of marginal, flawed, retracted work, and he does not even mention hundreds of positive, replicated cold fusion experiments. In a short book on cold fusion, why would anyone devote several pages to Portland cement and never get around to mentioning McKubre or Kunimatsu? Hoffman also discusses Jones' palladium experiments, but these experiments prove nothing:

- The experiments were incompetently performed. No attention was paid to critical techniques like keeping the electrolyte and cathode clean. At Kamiokande, the workers were actually shown on Japanese NHK national television touching the metal surface of the cathode with their bare hands.[7] This is appalling sloppiness.

- They did not attempt to measure critical parameters like loading. They performed no pre-testing of cathodes. They made no attempt to raise current density to high enough levels, and they made no attempt trigger the reaction by heating up the sample or with some other well known method.

- They did not attempt to measure excess heat. There is no point to looking for neutrons unless you are sure you have a cold fusion reaction, and the only certain sign of a cold fusion reaction is a burst of excess heat. The heat is <u>far</u> easier to detect than the neutrons, and it is unambiguous. People have argued years about the trace levels of neutrons detected in some cold fusion experiments. Jones himself has retracted some neutron measurements. In contrast, nobody has ever retracted any of the high power calorimetry (power above one watt), and the skeptics have never offered any serious arguments to disprove it. The level of neutrons in cold fusion is millions of times lower than it is for hot fusion, so looking for neutrons without first verifying the heat is folly.

Hoffman and Jones ignore one of the few things we know with certainty about cold fusion: it is nearly aneutronic. It does not produce neutrons commensurate with a hot fusion process. Cold fusion is nothing like fission or hot fusion. Suppose a scientist was given two samples of uranium and told to find out which one was undergoing intense fission. He could try measuring the surface temperature of the metal, but it would be far better to use a Geiger counter, because the principle signature of fission is ionizing radiation. The principle signature of cold fusion is heat. This fact was established during the first five minutes of the first cold fusion experiment that produced measurable levels of excess heat. It is known as “the dead graduate student problem.” If cold fusion was hot fusion, a cell producing a fraction of a watt of heat would generate such intense radiation that in a matter of minutes it would kill the people standing nearby. Yet many observers, including me, have stood next to cold fusion reactions and we are alive and well.

Jones and many others argue that all of those scientists who reported excess heat have made a mistake in their calorimetry. This is the heart of the cold fusion debate. Schneider says that in the Forward, in what I call the “subtle yet massive yet invisible unknown error” hypothesis. He says: “the alternative explanation, that the anomalous heat measurements are not from nuclear

reactions but are the result of an unidentified error or artifact, appears to me to be the only viable explanation of 'cold fusion.' " He does not attempt to guess what error this might be. Schneider knows as well as I do that many cold fusion experiments have yielded very high power, some as high as 180 watts.[8] It takes a gigantic error to produce a false reading of 180 watts. Gigantic errors are inherently easy to spot. They are caught during calibration.* Schneider wants to have it both ways. He wants an error so big that anyone would see it instantly, and so small that several thousand scientists have failed to see it over six years of intense research. The fact is, for the last 200 years, scientists have been using calorimeters to measure multiple-watt power levels. Measuring milliwatts is tricky, but watts are easy. There are few scientific techniques as cut and dry and as reliable as calorimetry in this domain. The basic techniques were described in my daughter's third-grade science textbook.[9] At any large high school science fair you will find experiments using calorimetry accurate and precise enough to measure the power levels observed in the leading cold fusion experiments. (I counted four calorimeters at last year's Dekalb County Fair.)

Schneider, Jones and Hoffman know there are no significant errors in the calorimetry of the mainstream professional scientists working in this field. They pretend there might be an error. They never specify which error, because they know there cannot be one. If the invisible error hypothesis really was "a viable explanation," as Schneider claims, he would list two or three potential candidates that fit the data. When pressed, opponents like Schneider and Hoffman always come up with a list of minor sources of error like the Joule - Thompson effect, which, thanks to Hoffman's own discussion, we see is many orders of magnitude too small to explain the data.

Schneider wants to artificially limit the debate to two options: either cold fusion is a standard hot-fusion style nuclear process, or it is a mistake. He wants to discuss these two hypotheses alone, excluding all others, even though these two were disproved years ago. He does not admit that cold fusion may be something new and different. He ignores the theoretical work of people like Schwinger, Hagelstein, Chubb and Chubb, and Preparata. He wants to limit the debate to two equally impossible, equally sterile hypotheses. Hoffman tries to steer clear of this issue, but he also pretends that Schwinger, Hagelstein and the others do not exist. Occasionally he hints that "a new paradigm" might be needed, but he does not mention any of the new theories. He does not give the name of a single theoretician!

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* Actually, an error of that magnitude would never happen in the first place. If the instruments were broken or installed incorrectly to that extent, they would show nothing. A modern calorimeter uses digital thermometers and computer data acquisition. With such instruments, a 180 watt error is as unlikely as an error that causes a digital clock to run two hours fast one day, and two hours slow the next. When an electronic clock or thermometer breaks that badly, it stops, goes blank, and shows nothing.

The Rockwell Rocketdyne Report

Appendix D is a 35 page report written by Brian Oliver of Rockwell International, titled "Summary of Helium Analysis Conducted by Rocketdyne Under EPRI Funding." This is one of bright spots in the book. Since July 1989, Rockwell has worked with many cold fusion research labs around the world, looking for evidence of helium in used cathodes and in samples of effluent gas. They tested materials from the University of Utah, ETEC, China Lake, Osaka University, E-Quest, and others. Their procedures and instruments are described in detail.

The Rockwell Rocketdyne facility is widely considered to be the best in the world at measuring helium. The hot fusion Tokamak programs contract with it to do helium analysis. Even John Huizenga, the arch enemy of cold fusion, once said that if Rockwell found helium from a cold fusion experiment, he would endorse those results in writing. At ICCF5 Huizenga said that to Russ George and I. Unfortunately, his resolve did not last long. George and I talked to Hoffman and another Rockwell representative at the conference. We agreed we would find the money to pay for the work, and they agreed to test the George's E-Quest samples. When we told Huizenga, he panicked and reneged, saying he would not endorse the results after all. His exact words were: "I will leave it up to you fellows." Nevertheless, during that hour when he thought there was no chance Rockwell would agree, and no chance we would find a way to pay, Huizenga was confident enough to stake his reputation on Rockwell's work. It is a pity he chickened out because, as this chapter shows, Rockwell did test the E-Quest samples from the Los Alamos experiments, and their tests did prove that the excess heat comes from a nuclear reaction (at least in part). Experiments that did not generate excess heat showed 0.4 ppm helium. Experiments that did generate excess heat yielded helium far above that background level, at levels as high as 552 ppm, 100 times atmospheric concentration. Rockwell also looked at the ratio of ^{3}He to ^{4}He as well as ^{22}Ne to ^{4}He in the samples and found the isotopic ratios prove the helium could not possibly have come from contamination from normal terrestrial helium. This report says that these results have also been confirmed by other helium mass spectrometry at the U.S. Bureau of Mines (Amarillo, TX) and by SRI International. It says reactions in the E-Quest system are "characterized by the production of heat" but it glosses over important details. does not say that the power level is at hundreds of watts, or that the levels of ^{3}He and ^{4}He are proportional to the net energy produced by the device.

Hoffman mentions E-Quest briefly elsewhere in the book but he gives no details, except in one spot. In Table 10.1, on page 130, "Anomalous Nuclear Measurement Examples," he slips in a cryptic reference to E-Quest. He tries hard to disguise it. In this table, the name "E-Quest" is not mentioned; there is nothing about ^{4}He; there is no reference to Appendix D; and naturally there is no mention of heat (a forbidden subject!). But he does include this comment which I suppose must refer to E-Quest:

> "^{3}He; only one questionable measurement of ^{3}He; one gas sample claimed to be from cavitation experiment showed high ^{3}He."

It is hard to know what to make of this. Perhaps Hoffman finds his own laboratory at Rockwell "questionable" for reasons he does not reveal. Nobody else finds it questionable, but perhaps he has some inside information that calls into doubt Rockwell's ability to measure ^{3}He at levels thousands of times above the natural background. From the phrase "claimed to be" I conclude that Hoffman does not fully believe the samples came from E-Quest's experiments at Los Alamos. I cannot imagine where else he thinks they might have come from. Perhaps he imagines that someone else at Los Alamos synthesized ^{3}He in a nuclear reactor, put it in $500 steel collection flasks, and shipped it to Rockwell as a joke.

Oliver's report shows that other tests, on cathodes and gas from other laboratories, were not as definitive as E-Quest, but some of them did show that helium has been created. Even when experiments yielded excess heat, most of the cathodes did not have elevated levels of helium or unnatural isotopic ratios. I suppose that means the reaction usually occurs near the surface, and the helium ash is lost with the effluent gas. Rockwell found clear positive results from two other groups besides E-Quest: China Lake, and Texas A&M.

Melvin Miles at China Lake[10] found helium in effluent gas samples proportional to excess heat. I discussed the Rockwell report with Miles. He pointed out that it is marred by misunderstandings and out-of-date information. Rockwell suspects that the helium may have come from contamination; that is, helium leaking through the glass. The report discusses this at length, but this possibility was ruled out years ago:

- The helium is proportional to the excess heat. There is no mechanism that would allow selective leaking in of helium through the glass weeks after a fraction of a watt of excess heat was generated. When you look a simple presence or absence test, the proof is even more stark. When there was no heat, there was no significant helium; when there <u>was</u> heat, there was easily detected levels of helium. The glass cannot know whether there was heat, and the excess was too small to weaken the glass or chemically change it to leak faster.

- Miles later repeated the experiments with steel collection flasks, and found the same results. Steel is more resistant to helium penetration than glass.

- Miles sent samples in both glass and steel collection flasks to other laboratories, including the University of Texas and SRI. They too confirmed the helium.

- The experiments are continuing, and Miles still observes excess heat and a positive correlation with helium. Over the past year, he has seen as much heat as his best earlier results. He has not yet been able to publish results from the last year.

Rockwell found no helium in the China Lake cathodes. Miles points out that he would not expect to find any, even if it had evolved inside the cathode during the excess heat runs. After every run

with heavy water, the cathodes were electrolysized with ordinary water to recalibrate.* This would probably purge any significant helium from the cathode. Miles told that to Rockwell, but they neglected to mention it in the report.

Oliver describes the analysis of the Chien [11,12] cathodes from Texas A&M in detail. After hemming and hawing, he concludes in italics:

> "Small amounts of ^{4}He were observed in 7 of the 16 specimens analyzed, 5 of these 7 were for specimens that had been etched. Six of the 7 'positive' ^{4}He results were for AM-Pd4. Unlike the results obtained earlier, the results for the present group are more heterogeneous, and also do not show any clear correlation with either sample surface area (Figure D.6) or with etch depth (Figure D.7). *This would suggest that the small amounts of 4He observed in the Chun-Ching Chien cathode material from the electrolysis experiments performed in Professor Bockris's laboratory are indeed heterogeneous and extend to depths greater than ~4 µm.*"

Hoffman Argues With Himself

The saddest part of this book comes close to the end, where Hoffman confronts the most important aspect of cold fusion: the heat. Heat is the principle signature of the reaction, and it is also the best, most iron-clad proof that the effect is real. I found this section heartbreaking. Here is the spectacle of a man who knows how to do good science; a man who knows the facts and the literature, who cannot bring himself to face the truth. Hoffman is stuck in a time warp, arguing about issues that were settled years ago, and issues that make no scientific sense. In the Socratic dialog format Hoffman appears to enjoy playing the devil's advocate and arguing with himself. It is playful. The Young Scientist acts as a foil, allowing the Old Metallurgist to strut his stuff and impress the reader, which he does at times. In this section, the argument gets out of hand. It gets ugly. A split personality emerges, as if he really is arguing with himself. I sense that Hoffman sincerely wants to confront the issue, but he cannot bring himself to do so. He skirts the issue. He approaches the answer, he gets to heart of the matter for a moment, and then suddenly he changes the subject, as if he cannot stand to look at the truth for long. The dialog begins:

> "YS: I guess the real question has to be this: Is the heat real?
>
> OM: The simple facts are as follows. Scientists experienced in the area of calorimetric measurements are performing these experiments. Long periods occur with no heat

* To "recalibrate" means to recheck the equipment after a run and make sure the temperature still rises the same extent in response to a given input power level.

> production, then, occasionally, periods suddenly occur with apparent heat production. These scientists become irate when so-called experts call them charlatans. The occasions when apparent heat appears seem to be highly sensitive to the surface conditions of the palladium and are not reproducible at will."

Instead of answering the question, Hoffman has flown off on a tangent. The correct answer is: "Yes, because the techniques are proven and the signal to noise ratio is very high." Those are the simple facts. Hoffman ignores them as he launches into an irrelevant and factually incorrect discussion. It is true that with some experiments heat is sporadic and unpredictable, but the simple answer remains: "Yes" because calorimeters measure sporadic heat reliably. Furthermore, Hoffman fails to say that Pons and Fleischmann, Patterson, Arata and many others have good control over the reaction. They can trigger it at will, and it continues indefinitely at steady, high power levels with such a large percent excess that there is no question it is real. Calling it "apparent heat production" is like watching a Boeing 747 take off and then saying "apparently it is flying." The conversation goes on:

> "YS: Any phenomenon that is not reproducible at will is most likely not real.
>
> OM: People in the San Fernando valley, Japanese, Colombians, et al., will be glad to hear that earthquakes are not real.
>
> YS: Ouch. I deserved that. My comment was stupid.
>
> OM: A large number of people who should know better have parroted that inane statement. There are, however, many artifacts that can indicate a false period of heat production, as we have discussed. The question of whether heat is being produced is still open, though any such heat is not from deuterium atoms fusing with deuterium atoms to produce equal amounts of ^{3}He + neutron and triton + proton. If the heat is real it must be from a different nuclear reaction or some totally unknown non-nuclear source of reactions with energies far above the electron-volt levels of chemical reactions."

This is the first and only place in the entire book where Hoffman faces the truth. But not for long. The question of whether heat is being produced is <u>not open</u>. It was closed, once and for all, back in 1991 when McKubre and others published their replications of Pons and Fleischmann. Hoffman finally makes this key point: the energies are far about the electron-volt levels of chemical reactions. It is hundreds of thousands of times above those levels, in some cases. Hoffman's statements about "many artifacts" and the heat "still being open to question" are false. Calorimetry at the multiple-watt level is cut-and-dry. It gives an unambiguous answer that no modern scientist can doubt or argue with. It is not complicated; the results are not open to question; the doubts and questions paraded by Hoffman and other skeptics have no scientific merit. The techniques used to measure watt-level heat are so fundamental, so easy, and the results so clear, that for Hoffman and others to question them is tantamount to questioning all of science

back to James Watt. They might as well doubt that Newton's prism experiment proves white light contains the other visible colors.

Other skeptics have attempted to disprove the heat results. Hoffman does not. He does not even dare to mention the actual numbers from the papers. He is a good enough scientist to realize that he cannot challenge them. Instead, he pretends they do not exist. After coming so close to confronting the truth, Hoffman's imaginary dialog fades away in a meandering discussion of marginal experiments. This is typical of the whole sad book:

> "YS: You were supposed to summarize 'cold fusion' for me. Your summary has no punch ending! You agree it is not the normal D-D fusion that was originally postulated. Does any anomalous nuclear effect exist or not? I suspect it does not. You apparently think that the answer is not in yet. Can you at least point to those experiments that you think should be continued?"

No scientist, anywhere, has ever postulated that cold fusion is "normal D-D" fusion. If it was, I would be dead from radiation. That is an infuriating straw man argument.

> "OM: It's not an exclusive list -- and I'm talking about anomalous nuclear effects now, not heat. Obviously, the concrete setting experiments are interesting. The Los Alamos alternating disks of palladium and silicon with submicrosecond bursts of kilovolt charge are most intriguing. The cavitation experiments, which involve the creation of enormous pressures as bubbles collapse, seem worthwhile at this time.
>
> YS: Well, if you can't be more definitive on the reality of anomalous nuclear effects in deuterium/solid systems, at least you know what needs to be pursued.
>
> OM: Is there anything else you'd like to know about the anomalous nuclear effects in deuterium/solid systems? . . ."

So . . . the heat drops out of the discussion, having lasted for only two paragraphs out of the entire book. Yet heat is the single most important aspect of cold fusion. It proves the three great essentials:

1. That cold fusion must exist, because the calorimetric signal to noise ratio of these experiments is so high and the techniques so reliable.

2. That it cannot be a chemical effect.

3. That it may well be the most important discovery in history, because pollution-free high density energy is the most vital resource on earth.

Hoffman sweeps the significance of the heat under the rug along with all heat experiments and data. Hoffman and the other skeptics want to talk about neutrons and Portland cement. They want to change the subject. They will never admit the heat is real, because that proves they have been drastically wrong from the beginning. Hoffman, Jones and other hard-line opponents would dearly like to put a stop to all experiments that show excess heat. Hoffman wants to do Portland cement, he wants Los Alamos to continue looking for tritium. Cold fusion is fine as long as it is "not heat." He wants to talk about E-Quest, but he dares not mention that they observe hundreds of watts of heat, and he deliberately leaves out their best helium results too, in Table 10.1, because he does not want to lend too much support to any aspect of cold fusion. The theme of the book, introduced in the Forward and repeated throughout is that all cold fusion results are marginal and probably mistakes. To prove that contention they censor out the dramatic successes like E-Quest's helium-4, even though Hoffman's own lab confirmed it, and even though it slipped into Appendix D. Above all, Hoffman and Schneider avoid giving any support to heat, no matter how dramatic and clear-cut the evidence for it is. They pretend that most of the top cold fusion papers do not exist. Yet heat is the key to cold fusion, and it is the key to mankind's long term survival.

Footnotes

1. M.C.H. McKubre, R. Rocha-Filho, S.I. Smedley, F.L. Tanzella, S. Crouch-Baker, T.O. Passell and J. Santucci, "Isothermal Flow Calorimetric Investigations of the D/Pd System," The Science of Cold Fusion, Proceedings of the II Annual Conference on Cold Fusion, Como, Italy, June 29-July 4, 1991, pp 419-443

2. M.C.H. McKubre, S. Crouch-Baker, A.M. Riley, R.C. Rocha-Filho, S.I. Smedley, and F.L. Tanzella (SRI International, Menlo Park, CA, USA), "Excess Power Production in D_2O Electrolysis Cells: a Comparison of Results from Differing Cell Designs," presented at the Third International Conference on Cold Fusion, October 21-25, 1992

3. M.C.H. McKubre, B. Bush, S. Crouch-Baker, A. Hauser, N. Jevtic, T. Passell[1], S. Smedley, F. Tanzella, M. Williams, and S. Wing (SRI International, Menlo Park CA, [1]Electric Power Research Inst., Palo Alto, CA), "Calorimetry Studies of the D/Pd System," presented at ICCF4

4. M. McKubre et al., "Isothermal flow calorimetric investigations of the D/Pd and H/Pd systems," J. Electroanal. Chem. 368 (1994) 55

5. M.C.H. McKubre (SRI International), "Observations of Heat in Electrochemical Studies of the Palladium/Deuterium Systems Employing an Isothermal Flow Calorimeter," M.I.T. EECS-LECTURE, Sept. 24, 1992

6. F. Slakey (Science Policy Administrator, American Physical Society), "When the lights of reason go out," New Scientist, Sept. 1993.

7. F. Nakano, “The Reality of Cold Fusion Can No Longer Be Denied,” Bungei Shunju, Sept. 1991.

8. 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

9. M. Cohen et al., “Discover Science,” (Scott, Foresmann), pp. 158 - 159. (Note: While the principles described in this textbook are correct, the experiment as described does not yield the results shown. The authors are so certain that calorimetry works, they did not bother to verify that the particular setup they illustrate will work. I tested it and found that it does not work.)

10. M. H. Miles, R. A. Hollins (Chem. Div., Research Dept., Naval Air Warfare Center, China Lake, California), B. F. Bush, J. J. Lagowski (Dept. Chem., Univ. of Texas), and R. R. Miles (Nuc. Safety Dept., EG&G, Colorado), "Correlation of Excess Power and Helium Production During D_2O and H_2O Electrolysis Using Palladium Cathodes," *J. Electroanalytical Chem.*, vol. 346, nos. 1-2, 1993, pp 99-117

11. Chun-ching Chien (Chem. Div., Inst. Nuc. Energy Res., Taiwan), Dalibor Hodko (on leave from Rudjer Boskovic Inst., Zagreb, Croatia), Zoran Minevski and John O'M. Bockris (Chem. Dept., Texas A&M Univ., Texas), "On an Electrode Producing Massive Quantities of Tritium and Helium," *J. Electroanalytical Chem.*, vol. 338, 1992, pp 189-212

12. Chun-ching Chien and Theresa Chen Huang (Inst. Nucl. Energy Res., Chem. Div., Lung-Tan, Taiwan), "Tritium Production by Electrolysis of Heavy Water," *Fusion Technology*, vol. 22, no. 3, 1992, pp 391-394