Introduction to the Cold Fusion Experiments of Melvin Miles

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From 1989 until his funding was terminated in 1995, Melvin Miles performed some of the best cold fusion experiments on record, at the China Lake Naval Air Warfare Center. (He retired from China Lake in 2002.) His goal was to answer two critical questions: Does cold fusion produce helium along with excess heat, like a plasma fusion reaction? And if so, does it produce roughly as much helium per joule of energy as a plasma fusion reaction does? He answers both questions affirmatively. When a cold fusion palladium cathode becomes active, it releases helium into the electrolyte. The helium leaves the cell in the effluent deuterium and oxygen gas. Cathodes that produced more excess heat produced greater amounts of helium. The ratio of helium to energy is roughly comparable to that of hot fusion, within an order of magnitude. This is strong evidence that cold fusion really is some form of nuclear fusion, and not fission, zero point energy, or something else.

Another important lesson from this research is that cathode material is the key to success. This is no surprise to people familiar with the literature, but Miles makes the trend clear. He tested 94 cathodes from 12 different sources. Seventeen out of 28 experiments with Johnson-Matthey palladium produced excess heat. All four of the special Johnson-Matthey cathodes provided by Fleischmann worked. But with 19 cathodes from four other batches, not one worked.

Miles is a professional, and quite willing to share information, unlike some other researchers in the field. He has published fifteen papers in conference proceedings and peer reviewed journals and a 98-page report, which is abstracted in this issue. He has tried to publish more papers and letters, but most peer-reviewed mainstream journals turned him down. His lectures at the major cold fusion conferences have been models of clarity and rigor. He began research on helium detection 1990, in collaboration with B. F. Bush and J. J. Lagowski at the University of Texas. Bush is an expert in dealing with contamination from air, a critical factor in this experiment. Samples of the effluent gas from the experiment were analyzed at the University of Texas. After the experiment had been underway for some time, samples were sent to the U.S. Bureau of Mines, which specializes in gas detection, and to Rockwell International. Similar results were obtained from both laboratories.

Miles was the first to perform a systematic search for helium in the effluent gas. He also looked for helium in the cathode metal, where researchers expected to find it at first. He and others have found only negligible amounts of helium in the metal. This indicates that the reaction occurs near the metal surface rather than in the bulk. After Miles published his findings, other researchers in Italy and Japan also looked for helium in the gas. Results have been mixed, but in recent work using the best, most sensitive equipment yet, Cignini and Gozzi have confirmed Miles' findings. [1]

Miles spent five months experimenting before he saw any excess heat. He was discouraged and ready to quit. Less dedicated people became discouraged after a few weeks of research. People may underestimate Miles, because he never makes a claim without extensive experimental evidence to back it up. He does not make the kind of exaggerated claims that have plagued this field. He has a low key, understated, matter-of-fact tone. He is always willing to discuss his problems and failed experiments. His final report, *Anomalous Effects in Deuterated Systems*, [2] includes a discouraging litany of experiments that produced no excess heat, month after month, years after the first successes. He says this is the most frustrating research he has ever done.

Why Helium is so Difficult to Detect

From the start many people suspected that cold fusion produces helium. J. Schwinger and other theoreticians predicted it should. (Schwinger predicted helium-3; Miles has found helium-4.) Conventional plasma fusion (hot fusion) d-d reactions produce neutrons, tritium and helium. Other hot fusion reactions are aneutronic; they produce no neutrons. When cold fusion was announced, people assumed that it must work the way hot fusion does, so they expected to find the same levels of these nuclear products. But it was soon obvious that cold fusion produces millions of times fewer neutrons. Otherwise, a 1-watt cold fusion reaction would have killed the scientists observing the experiment. Cold fusion sometimes produces a low level of tritium; not as much as hot fusion, or the radiation alarms would go off and the health of the researchers would be endangered. Some devices are optimized to generate tritium, particularly Claytor's device at Los Alamos.

Neutrons and tritium are analogous to the smoke from a fire: some fires produce dense smoke, and some do not. We know how to reduce smoke by ensuring complete combustion. We have not yet learned how to control cold fusion neutrons. Unknown factors cause cold fusion reactions to produced neutrons sometimes, and to become nearly aneutronic at other times, sporadically. It appears helium is always produced in a fixed proportion to the heat.

If helium is always produced, you may wonder why so few other scientists have looked for it. And if neutrons and tritium are rarely produced, why did so many scientists devote such effort to find them? There are two reasons: scientists searched for neutrons out of habit, and because it is easy. Nuclear researchers are good at looking for neutrons and tritium. They all have detectors, and they know how to use them. Hot fusion reactions always produce neutrons in a predictable ratio to the energy. The neutron "signature" (the energy with which the neutrons strike the detector) tells the scientist a great deal about the reaction. Scientists look for neutrons and tritium because they are easy to look for, whereas helium is difficult to detect. Hot fusion produces millions to hundreds of millions of times more neutrons per watt of heat than cold fusion, so the neutrons are easy to detect. You cannot miss them: as I said, the neutron flux from an unshielded 1-watt hot fusion reaction would kill the observer. With cold fusion, "heat is the principal signature of the reaction" (Fleischmann). It makes no sense to look for neutrons unless you are sure a reaction is underway, and the only way to be sure a reaction is underway is to measure excess heat. Scientists who insist on looking for neutrons while they ignore helium and excess heat resemble the drunk who drops his keys in the bushes but looks for it under the streetlight because it is too dark to see in the bushes.

Helium is difficult to detect for four reasons:

1. It is not radioactive. Radioactive elements are easy to detect. You can detect a single atom of tritium at the moment it decays. You measure the solid angle of the detector to determine what percent of the neutrons strike the detector. The neutron signature distinguishes the tritium from other radioactive elements in the background and contamination. Based on the decay rate and the half-life you compute the number of tritium atoms left in the sample. The techniques are not easy to master, but an expert can determine how much tritium a sample has before, during and after an experiment. Helium-4, however, is a non-radioactive, stable atom. No instrument can detect a single atom of a stable isotope. A mass spectrometer makes a relative measurement of helium compared to some other element. You compare helium to a known amount of a carrier gas, usually nitrogen. (In other experiments, argon or krypton may be used as carrier gas, but Miles says they are usually contaminated with helium, so nitrogen is a better choice.) The helium is mixed with the carrier gas and the sample is placed in the spectrometer. The spectrometer registers parts per million (ppm) or parts per billion (ppb) of helium compared to carrier gas. You extrapolate from this to estimate the total helium in the full sample. This method is problematic and indirect compared to measuring radioactive isotopes. Radioactive isotopes are measured in absolute numbers. Non-radioactive isotopes are measured relative to the carrier gas.

Miles also calculates the helium generated per watt of power per second. This is the basis of his claim that the helium is commensurate with a plasma fusion reaction.

2. Helium has nearly the same mass as a deuterium molecule (D_2) (4.02820 amu versus 4.00260 amu). Two thirds of the effluent gas from electrolysis is deuterium gas (D_2). Only 9 parts per billion are helium. It takes a high resolution mass spectrometer to tell them apart. With lower resolution the helium may be indistinguishable, or it might appear as a blurred "shoulder" rather than a separate peak. The effluent gas is processed to filter out most of the deuterium.

3. Helium contamination is ubiquitous. Helium constitutes 5.4 parts per million of the atmosphere at sea level, and it will slowly permeate glass, rubber, plastic and other materials. Contamination cannot be easily eliminated. It must be measured and accounted for in control experiments and runs that did not produce excess heat. Miles' control experiments consistently measured background helium contamination ranging from 3.4 to 4.9 ppb. The mean value was 4.5 ± 0.5 ppb, which is remarkably consistent. In his early experiments, most of this background helium probably diffused through the rubber connection between the cell and the collection flask. (Later, stainless steel tubing was used.) When excess heat was generated, the level of helium increased significantly, always in proportion to the heat. With the cell that generated the most heat, helium doubled, to 9.7 ppb. These numbers are the heart of the studies.

4. Both hot fusion and cold fusion produce fantastically small amounts of helium. Miles has pointed out that a 1-watt fusion reaction would take 73,000 years to produce a single mole (4 grams) of helium. The collection flask takes an hour and fourteen minutes to fill (4440 seconds). In later experiments, using metal flasks, Miles saw only low power, typically around 0.060 watts (60 milliwatts). At this rate the collection flask should have approximately 5×10^{-10} grams of

helium from fusion in it, or 7×10^{13} atoms. After subtracting background contamination, Miles found approximately 2×10^{13} atoms per flask, an extraordinarily close result.

How the Experiment is Performed

Miles uses an open cell with static, isoperibolic calorimetry. This is the simplest possible arrangement. The anode and cathode are placed at the bottom of a tall test tube (the cell). The temperature of the electrolyte is measured with a thermistor. The cell is calibrated by heating the electrolyte with the joule heater at different power levels, and with electrolysis with palladium and light water, or palladium at the beginning of the experiment when no heat is being produced. Miles and Fleischmann think that in the hands of an expert, isoperibolic calorimetry is as precise as flow calorimetry. It has higher resolution; that is, it can show fine gradations of tens of milliwatts with more confidence than a flow calorimeter. Miles says, "it can show a precision of +/- 0.1 milliwatts while flow calorimetry yields only +/- 10 milliwatts (100 times less precise)." [3] It is less complicated; it does not require pumps, tubes, flowmeters, and two thermistors. Fewer things can go wrong. There are no pumps to break, or tubes of flowing water to clog up. The disadvantage is that a static calorimeter is more dependent upon calibrations made before and after the run. A flow calorimeter is a first principle device which is theoretically not dependent upon the calibrations. In actual practice any calorimeter must be extensively calibrated, but some skeptics argue that a static calorimeter might depart from its calibration constant during a run without anyone noticing, and then mysteriously return to it before post-experiment recalibration is performed. This seems very unlikely, although it is conceivable. But the event would be random. It would not correlate with helium production or the choice of cathode material. This is not to suggest that calorimeters never fail. Flow and static calorimeters can drift or go wrong in various well-known ways. The biggest problems with static calorimeters are changes in the bath temperature (or room temperature with an air-cooled cell), and a large change in electrolyte level when gas leaves the cell and the water is not replenished.

The term "open cell" means the effluent gas from electrolysis leaves the cell. Some open cells vent the gas directly to the air, but Miles directs the gas through a bubbler to prevent helium and light water contamination. In a closed cell, a recombiner combines the free oxygen and deuterium gas in the head space to make heavy water again.
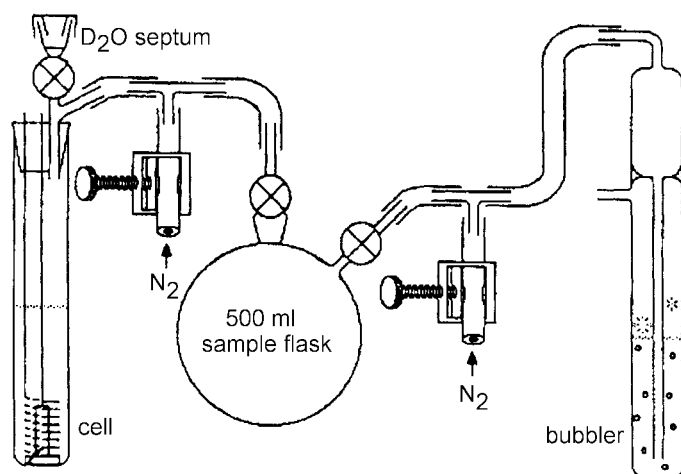
Figure 1. Electrolytic cell with positive pressure gas discharge line used to collect samples of effluent gas.
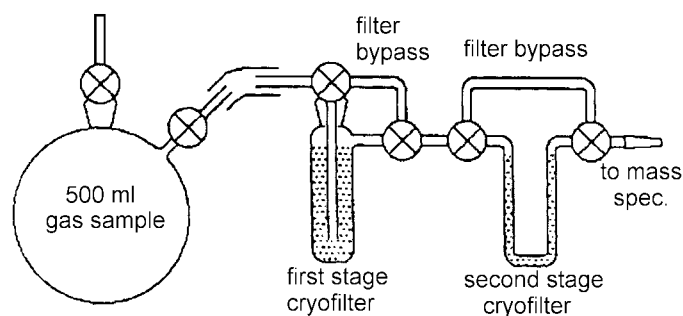

Figure 2. The two-stage activated charcoal cryofilter, designed to remove all gases except helium.

Miles collects the effluent gas from the cell into a stainless steel flask. He ships the flask to another laboratory for analysis. The setup is shown in Figure 1. The flask is purged with nitrogen which has been checked to ensure it does not contain measurable levels of helium. It is attached to the cell and left in place for two days. The system is "self flushing." Effluent gas comes in one side of the flask, fills it, and goes out the other side. It passes through a bubbler, which creates back-pressure. This keeps the gas pressure in the cell, flask, and tubes a little higher than the surrounding air, so that any leaks will go out, not in. During the two days the flask is attached to the cell, enough effluent gas passes through to fill it forty times. Then the stopcocks are turned off. The flask is sealed and removed from the cell. It is transported to another laboratory with a high resolution mass spectrometer. The setup in the second laboratory is shown in Figure 2. The gas is passed through a two-stage filter to remove gases other than helium. The remaining gas

then enters the mass spectrometer where the helium is measured. This description gives you a sense of how demanding the work is: [4]

> Strenuous efforts were made to avoid contamination of the effluent gas from the electrolytic cell with any external source of helium. . . . Two identical systems were always run simultaneously using calorimetric cells as described previously. Connections between the cell, flask, and oil bubbler employed thick-walled rubber vacuum tubing. All connecting lines, as well as the cell, were flushed vigorously with boil-off nitrogen, which contained no ^{4}He (see Table 1), for at least 10 min prior to attaching a gas collection flask. Furthermore, the flasks were generally connected to the cell for at least two days of D_2O electrolysis before removal. The gas evolution rate was calculated to be 6.75 ml min^{-1} at 528 mA (200 mA/cm^2) and 700 Torr assuming ideal gas behavior; thus the 500 ml collection flask was further flushed with more than 19 times its volume of evolving D_2 and O_2 gases per day. Actual measurements of the gas evolution rate by the displacement of water yielded 6.75 ± 0.25 ml min^{-1} for cell A and 6.69 ± 0.15 ml min^{-1} for cell B. All solvent additions were made only after vigorously sparging the make-up D_2O with nitrogen for about 5 min. The D_2O was always added through the septum and stopcock into the cell using a gas-tight syringe (Hamilton No. 1005).

Improved Calorimetry

The paragraph above describes "thick walled rubber vacuum tubing" that connects the cell and the collection flask. S. Jones and others criticized Miles for using rubber tubes in his early experiments, because helium might leak through rubber. Miles soon switched to stainless steel tubing instead of rubber. He improved the experiment in other important ways. He used a double wall isoperibolic calorimeter, instead of single wall version. (See Ref. 2, Fig. 4) Later, Miles collaborated with Bush in another experiment performed at SRI International, using an electronic Seebeck (or Calvet) calorimeter. [5]

Thus, Miles has used three different calorimeter types, and achieved similar results with all three. This is very significant. One of the best ways to confirm that an effect is real is to use a different instrument type, based on different physical principles, such as thermistors and thermocouples. A limitation or inaccuracy in one type may not be a problem in another. An artifact that may distort the output of one instrument type may be impossible in another type. A flow calorimeter may have problems with blocked cooling tubes, pumps and other moving parts. A Seebeck calorimeter is impervious to such problems, because it has no moving parts and no cooling fluid. (Naturally, it may have other problems of its own; no instrument is perfect.)

In Miles' research, all three calorimeter types successfully detected excess heat in some tests, but not others. It would be suspicious if one calorimeter type always appeared to detect heat in an experiment in which other calorimeter types showed mixed results, or no results.

Advantages of an Off-line Spectrometer

At first glance, collecting the gas in one location and analyzing it another seems clumsy. It invites errors, possible confusion between different flasks, and contamination while the flasks are in transit and waiting to be tested. Gozzi prefers to use a helium detection system attached to the

cell that measures helium as it is generated: on-line, in real time. The off-line helium detection in this study was criticized. Skeptics claim this is a weakness for a variety of reasons, but mainly because helium will seep into the flasks during the delay between collection and analysis. They claimed that helium will readily penetrate glass so the data from glass flasks cannot be trusted. Miles and his co-workers took this criticism to heart. They switched to stainless steel flasks and established that background contamination was accurately measured. They were able to repeat the helium-4 production results using these metal flasks.

Miles was forced to do off-line analysis because the mass spectrometer in his laboratory does not have sufficient resolution to easily distinguish between helium and deuterium molecules. He made a virtue of necessity. He took advantage of off-line detection to reduce costs, improve reliability, and bolster confidence in the results. Cost is reduced because the mass spectrometer is the most expensive instrument needed for this experiment. An on-line spectrometer must be dedicated full time to cold fusion for the duration of the experiment, which may be months or years. When samples are tested off line the spectrometer is only needed for a day or two every few months. Other researchers use it most of the time.

An on-line spectrometer can be a weak link in an experiment. Mass spectrometers are finicky, high tech machines. They are reliable when used in ordinary applications, but in this case they were stretched to the limits of their capabilities. They had to be calibrated and adjusted frequently. Before he tested each new flask, Bush calibrated for helium and fine-tuned his spectrometer. A physics experiment is a running battle with the instruments. Graphs often contain gaps and dotted lines representing times when the data was lost because of one failure or another. Cold fusion itself is a sporadic, unpredictable effect that "usually turns on for two hours starting at 3 a.m. on Sunday night when nobody is around to see it," as Bockris says. When your detection equipment may also drop out of service for hours or days at a time, you have little chance of capturing a complete, continuous heat burst. Either the cold fusion or the mass spectrometer will be broken on any given day, or if they are both working the computer will be out, the thermocouple interface will be flaky, there will be a leak in the hose somewhere, or something else will go wrong. With off-line processing the reliability of the spectrometer does not matter. The flask of sample gas sits on the shelf while the technicians tinker with the instruments.

Off-line processing allows blind testing, which eliminates unconscious bias and bolsters confidence in the results. Miles labeled the flasks with arbitrary codes. (He selected birth dates of his family members.) The people operating the mass spectrometer did not know whether the samples came from control tests, dud runs that produced no excess heat, or excess heat runs. They did not know whether Miles expected to find excess helium or not. His hopes cannot interfere with their objectivity.

Possibilities for Error

When excess heat was generated using metal flasks, the amount of helium generally rose to between 7 and 9 ppb, roughly double the background. The mass spectrometers used in this study can detect ±1.1 ppb, so this is significant. It is 4 to 5 sigma in the best cases. But the error bars are large. The amounts of helium generated by cold fusion are infinitesimally small, as you would expect if it is a nuclear reaction. The data requires sophisticated processing, extrapolation

to determine the helium content of the full sample, and corrections for background helium. The small numbers and complex analysis are unavoidable, but they raise doubts and they require careful study.

Above all, we want assurance that this increase of 4 ppb is not merely contamination. Air contains 5,400 ppb of helium. A tiny bit of air leaking into the cell could easily double contamination from 4 to 8 ppb. Actually, it would probably increase it a hundred or a thousand times. How can we be sure these increases really are correlated with heat? How do we know they are not random fluctuations from contamination? Miles presents a sophisticated statistical analyses. Let us consider this problem from a non-mathematical, common sense point of view. If the helium came from air contamination, the results would not be consistent. The control runs and runs that did not produce excess heat would not all show the same narrow range of helium. Helium would not increase with excess heat. It would not correlate with the level of excess heat. Contamination would produce random levels of helium, probably far higher than what was detected in these experiments. It is difficult to imagine a direct air leak that always lets in a tiny level, at or below 4.6 ppb. An air leak would sometimes let in tens and sometimes thousands of ppb, at random. It is even more difficult to imagine an air leak that doubles when and only when cells produce excess heat. Tiny background levels like 4.5 ppb characteristically come from helium gradually seeping in through rubber or plastic. In the control experiments the helium background always remained within a narrow range of 3.4 to 4.6 ppb. When cells produced excess heat, helium always rose above this narrow range, and it increased in proportion to the heat.

Could the heat itself have caused a tiny increase in diffusion, perhaps by changing the permeability of the rubber? No, because some of the dud cells were run at higher overall power than the heat-producing cells. Electrolysis power levels during dud runs were raised and lowered considerably, from a half-watt to two watts, with no measurable effect on the helium background. Furthermore, the rubber tubing was outside the cell at room temperature.

Could the helium be random despite appearances? Is it only apparently correlated with heat? Is the range of background values, 3.4 to 4.6 ppb, too wide to be meaningful? The answers can be found in the statistical analyses by Miles. One is particularly simple. Each experiment is reduced to a presence or absence test. Helium below the background 4.6 ppm is "absent," anything above that is "present." In Appendix C, Miles describes the statistical test:

> For our 33 experiments involving heat and helium measurements, excess heat was measured in 21 cases and excess helium was observed in 18 studies. Thus 12 experiments yielded no excess heat and 15 measurements gave no excess helium. If one uses these experimental results as random probabilities of $P_h = 21/33$ for excess heat and $P_{He} = 18/33$ for excess helium, then the probability of random agreement (Pa) for our heat and helium measurements would be . . . 0.512, and the probability of random disagreement (P_d) would be . . . 0.488. The presence or absence of excess heat was always recorded prior to the helium measurement and was not communicated to the helium laboratory. [The "blind test" procedure.] Based on our experimental results, the random probability of the helium measurement correlating with the calorimetric measurement is not exactly one-half. This is analogous to flipping a weighted coin where heads are more probable than tails. The

probability of exactly three mismatches in 33 experiments, therefore, would be . . . 1.203×10^{-6} . . .

The total probability of three or less mismatches in 33 studies would be . . . 1/750,000 . . .

Furthermore, it is very unlikely that random errors would consistently yield helium-4 production rates in the appropriate range of 10^{11} - 10^{12} atoms/s per watt of excess power . . .

Dramatic Differences in Palladium from Different Sources

Miles found that palladium from different sources has dramatically different performance. Cathode material is the most important variable in these experiments. Here is a summary of Table 10 :

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Source                          Success Ratio (excess heat / total tests)
NRL Pd-B alloy                  7/8
Johnson-Matthey (J-M) Pd        13/24
J-M from Fleischmann            4/4
NRL Pd (first batch)            1/2
Tanaka Pd (sheet)               1/3
NRL Pd (another batch)          0/4
NRL Pd-Ag                       0/3
IMRA Japan Pd-Ag                0/2
WESTGO Pd                       0/6
Pd/Cu                           0/2
John Dash Pd (sheet)            0/2
Co-deposition (1992)            2/34

Total:                          28/94
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Most of the samples produced 1 to 2 watts per cubic centimeter. One of the J-M samples produced 15 watts/cm^3, and in 1994 one of the J-M cathodes from Fleischmann produced 14 watts/cm^3. Contrary to the claims of Jones and other critics, Miles observed high levels of heat in the middle and latter stages of his research project, when he used improved, more sensitive calorimeters. Miles observed heat from Johnson-Matthey palladium wire. Samples of wire from the same spool were later tested in the improved China Lake calorimeter developed by Kendall Johnson, and other samples were tested by Wilfred Hansen at Utah State University. Excess heat was confirmed in these tests.

Other researchers who have tested palladium from different sources also report that the best samples come from Johnson-Matthey. Fleischmann says the best palladium is a special grade available from Johnson-Matthey in 2-kilogram lots for $20,000. Miles discusses some of the morphological and metallurgical differences between working and non-working palladium. Storms describes them in greater detail. [6]

Power Density Standard

Miles often quotes the performance of his cathodes in power density: watts per cubic centimeter. He points out that his power density of about 1 watt/cm^3, and many 1989 era

experiments could not have detected such low power in their own cells, given the size of their cathodes and the sensitivity of their instruments. He says that if the calorimeter cannot detect this level of excess power, then the experiment should not be performed. Storms [7] cautions that this standard can be misleading: It might be understood to mean that the reaction is a product of the entire bulk of palladium metal, occurring uniformly throughout the metal. Storms says that was the initial assumption made by Pons and Fleischmann, but it was a mistake, which led to poor experimental approaches. Everyone, including Pons and Fleischmann now believe that the reaction occurs in small active sites in the metal, probably near the surface. The spotting on autoradiographs and the localized areas of transmutation found by Mizuno [8] and Minevski [9] support this hypothesis. Pons and Fleischmann originally used small cathodes with large surface area compared to the bulk. Since the reaction probably occurs near the surface this geometry makes their power density numbers look comparatively good, but the comparison makes no sense.

I do not think Miles intended the power density standard to express a specific hypothesis about the origin of the heat. He meant that given the state of the art of cold fusion research in 1989, and assuming that cathodes in different labs had roughly similar geometry, a researcher should have been prepared to detect the level of heat that Miles, Pons and Fleischmann and others saw at that time: about a watt per cubic centimeter. Miles feels that unless the calorimeter is accurate to ±20 mW or better, it is probably a waste of time to do calorimetric experiments on cold fusion. Miles did not mean that was the upper limit of power. His own cathodes sometimes produced much higher power density after 1989, and so did cathodes in other labs. He knew he was nowhere near the limit of the power producing capacity of the metal. Nobody has ever mapped out these limits. Storms thinks that the power density of the hot spots within the metal might be well over 10^6 watts/cm^3.

Precise Calorimetry Required

Miles uses small cathodes, and they do not produce much excess heat compared to some of the leading cold fusion experiments. He is forced to measure low levels of excess heat, ranging from 50 to 500 milliwatts. He would like to see more power, but these are the best results he has been able to achieve. In order to measure such low power with confidence, he and his colleagues have improved the cell hardware to the point where it is now one of the most precise electrolysis calorimeters in the literature. They can now achieve ±1% or ±10 mW resolution (whichever is larger) over a wide range of temperatures. The best flow calorimeter is McKubre's as SRI, which measures ±25 mW over a narrow range of temperatures. (See "Improved China Lake Calorimetry.") Miles uses sophisticated equations to account for temperature dependencies and room temperature changes. He has written a definitive paper on isoperibolic electrolysis calorimetry. One of the most important lessons from this paper, it seems to me, is that at low power levels the apparent cell constant changes radically. It is not linear. This is shown in Figure 5 from that paper, "Effect of power level on the apparent cell constant (K) and heat transfer coefficient (h)." Miles shows how this led to errors in the negative cold fusion experiment at Cal Tech: "The neglect of the intercept term in eq 4 produces significant errors in the apparent cell constant for power levels below about 0.6 W. All of the calorimetric data reported by Lewis et al are near or below a total power level of 0.6 W; hence his use of the approximate relationship $P \approx$

$\Delta T/h$ is likely a large source of error." I believe this nonlinear response may explain some of the positive results with the gigantic input to output ratios seen with nickel cathodes at very low power. Below a half-watt, a simple linear equation will not work with this type of cell. This does not mean that small gradations in power cannot be detected. With a linear approximation you can measure the difference between 1.010 watts accurately, and you can easily distinguish it from 1.040 watts. At the low end of the scale you will think that 0.020 watts are 0.050.

Political Problems

Melvin Miles is quiet, dignified, and as Martin Fleischmann says, thoroughly honest. He was a university professor before coming to the Navy. He is not a disputatious person. He does not exaggerate or argue for the sake of arguing. He does not enjoy politics and controversy (the way people like the late Giuliano Preparata and I do). However, perhaps because his work is so good, and his critiques are so well crafted, he has drawn fire from the opposition. Over the years he has become embroiled in some nasty political fights with extremists in the Department of Energy (DoE), CalTech, MIT, Harwell, Brigham Young University, and finally in the Navy itself. He was even criticized for working on this final report too long and too earnestly. His managers asked him to wrap it up quickly and drop the subject. As he puts it, he attracts flak for doing the job right.

His political problems began with the DoE ERAB report. [10] This report, compiled in the summer of 1989, concluded that cold fusion is a mistake. It cited Miles' work as an example of a quality experiment that found no excess heat or nuclear products. At the time the report was compiled this was true. Miles worked for five months without success. Later, after he began to observe heat and helium he contacted the ERAB report authors and pointed out that his findings no longer supported their conclusions and their report is now obsolete. They ignored him. There is no better illustration of the fact that the ERAB report began as a rush to judgment and degenerated into a cover-up. Several months before Miles, Bockris, McKubre and other serious researchers finished their first round of experiments, the ERAB report was signed and sealed and the *New York Times*, *Nature*, the American Physical Society and the rest of the scientific establishment already condemned cold fusion as delusion, fraud and mendacity.

Miles wrote one of the best short guides on calorimetry with an electrolytic cell [11] (Another version of this paper is available in the LENR-CANR library.) [12] He discussed many common problems, especially the changing electrolyte levels, when the temperature is measured in the cell. As water is electrolysized in an open cell, the electrolyte level falls and the apparent cell constant increases. A watt of power might raise the temperature of the cell 4ºC when the electrolyte level is high, but as the level falls and the mass of water decreases, and a watt might raise the temperature 5ºC instead. As the water level falls less electrolyte touches the cell wall, so a smaller area of the outside cell wall transfers heat, which also raises the apparent cell constant. Pons and Fleischmann minimized this problem by using a half-silvered Dewar cell, which confines most of the heat transport to a window below the waterline. Miles minimized the electrolyte level effect by integrating the heat in a compartment outside the cell and measuring the cell temperature at that location.

Not only does the apparent cell constant change as the water level falls, but the electrolyte becomes more concentrated, impedance falls, and the cell consumes more power. Every few days the experimenter has to replenish the cell with new heavy water. This causes a sudden jump in impedance and a large fall off in power. Miles has to adjust for these and other factors, like the loss of heat from the top of the cell. As he puts it, "there is no steady state during D_2O + LiOD electrolysis experiments for either cell voltage or cell temperature." He shows how to do an exact numerical analysis to take into account all of the major factors in this changing system. He suggests that if a scientist does not to do this level of sophisticated analysis, he should at least calibrate the cell at different water levels. "Approximate solutions require, at very least, an experimental evaluation of the terms involved in the time dependency of the cell temperature, cell voltage, and cell contents in order to justify the omission of any of these terms." Miles focuses on: "a critical analysis" of "several key publications that have impacted on this scientific topic." In plain English, he describes some large errors in the famous negative cold fusion papers in 1989, from CalTech, MIT and General Electric. He shows that the cells at CalTech and MIT did, in fact, generate excess heat, but the researchers did not realize it because their analysis was faulty.

The errors made by Lewis *et al.* at Cal Tech are basically simple. To start with, Lewis did not calibrate his cell in the normal sense of the word. That is, he did not test it with a joule heater, a dummy platinum cathode, or light water and palladium. Instead, he put the "live" palladium sample that he planned to test for excess heat into the cell, and calibrated with that cathode as the experiment began. He ran electrolysis at a given power level until the temperature stabilized, and he noted the temperature. Then he stepped up the power level and noted the new, higher temperature. If you assume the Fleischmann-Pons hypothesis is correct, and the cathode generates heat, this method should not work. To 'calibrate' means: "determine by comparison with a standard." Lewis did not use a known standard; he compared the palladium with itself. Suppose the "live" cathode began producing heat soon after the experiment began, before Lewis finished calibrating? He would have no way of knowing.

Lewis' method worked, although it is unconventional. He succeeded in "calibrating on the fly" with live palladium. Some people, including Storms, [5] even recommend this method today, because when you replace a dummy platinum cathode the slight change in position may affect the cell performance. Storms, however, has the advantage of hindsight. We now know that palladium - heavy water cold fusion does not generally produce heat for the first few days of the experiment. That was not a safe assumption to make in 1989. In any case, Lewis managed to record a good calibration. Furthermore, he soon recorded evidence of excess heat (although he does not think so). When the experiment began, input was 0.464 watts and the ΔT was 6.5ºC. The cell constant was 14.0ºC per watt. Five days later Lewis increased electrolysis input power to 0.595 watts. The ΔT should have been 8.4ºC, but it climbed to 9.4ºC, an entire degree hotter than expected. (One degree Celsius measured with laboratory grade instruments is a large temperature difference.) There must have been 0.07 watts of excess heat. Yet Lewis reported there was no excess, and he still maintains there was none. If there was no excess heat, the cell constant mysteriously changed from 14.0 to 15.8º per watt. As Miles puts it: "This 26% increase in heating coefficients, based on our experience, is highly unusual. Closer examination, however, shows that Lewis et al. erroneously define the heating coefficient at $h = \Delta T/P_T$ where the total power (P_T) is the sum of the electrolysis power and resistor power. According to Newton's law

of cooling, the temperature difference, ΔT, defines the total power from the cell to its surroundings; thus any excess power (P_X) must be included in defining the total power. This neglect of P_X by Lewis et al. in the equation defining h would lead to an increase in the heating coefficient as the excess power increases." [9]

Lewis never gave a reason for this alleged change in the heating coefficient. He never said it was caused by thermocouple drift or lower electrolyte level, or some other large instrument error. It would have to be a large error; a subtle one would not cause such a large discrepancy. He never described why he thought the cell constant changed. In fact, he will not even admit he claims the cell constant did change. *Nature* asked him whether they should publish a critique by Noninski & Noninski explaining his mistake. (*Nature* allows cold fusion skeptics to accept or reject publication of letters criticizing their work. This may be the first scientific debate in history where one side gets to choose what the other is allowed to say.) In a somewhat muddled private letter to *Nature* editor Lindley, Lewis explained:

> While it is true that our open system measurements were not sensitive to any current-density independent excess heat (as clearly stated in our original *Nature* manuscript), Pons and Fleischmann clearly stated in their work that they only observed current-density dependent excess heat. Our original experiments would have readily detected this heat; we clearly did not see such excesses . . . N&N have now also proceeded to invent a new hypothetical type of heat which they claim could have been missed in our experiments, and which was not reported by Pons and Fleischmann in their experiments. . . .

Actually, the Noninskis and Miles claim the heat was missed in his *equations*, not his experiments. The heat showed up in the experiments with a clear, one degree difference. Perhaps Lewis means that if the excess had been exactly proportional to current density alone, and completely controlled by current density, then he would have detected the heat, so he does not have to worry about apparent changes in the cell constant (or even apparent violations of Newton's Law of Cooling) which he thinks are characteristic of "a new hypothetical type of heat."

Prominent establishment skeptics still say the negative experiments at the "big three," CalTech, MIT and Harwell, were the nails in the coffin of cold fusion. Yet Miles, Noninski, Swartz, [13] Fleischmann, Melich [14] and others have demonstrated that all three of these experiments produced excess heat. Since the experiments were performed by people who wanted to kill cold fusion as quickly as possible, and who later obfuscated and denied their own results, in sense you can say these experiments were the best proof from 1989 that cold fusion was real. (Experiments from later years were far better than anything in 1989.)

Miles also pointed out that Lewis attempted to detect helium from the reaction with a mass spectrometer that was only capable of measuring parts per million, not parts per billion. Any production of helium would not likely be detectable at that sensitivity.

Miles pointed out several problems with MIT, especially the fact that they ignored changes in electrolyte level.

Political Problems with Steve Jones

Steve Jones invited Miles to lecture at Brigham Young in 1991. When Miles tried to present his experimental results, he was repeatedly interrupted. His 50 minute presentation took three hours to complete. Jones has had a vendetta against Miles ever since. He wrote letters to journals and Internet news groups repeating the same misinformation and unscientific claptrap again and again. Miles was finally forced to send a formal objection, because, he explained, he had an obligation to defend his reputation and the quality of work at a U.S. Navy laboratory:

> Professor Steven E. Jones of Brigham Young University has been aggressively attacking my scientific publications involving the Fleischmann-Pons effect (cold fusion) for almost two years and has recently accused me of rejecting heat/helium data points and fudging my results. I highly resent such unfounded attacks since they damage my professional integrity as a scientist as well as reflect on the quality of research conducted at Navy laboratories. Navy scientists are required to submit their work for review and clearance before any manuscript can be released for publication. [15]

Here are some examples of the objections raised by Jones. He says that with better calorimetry and higher resolution mass spectroscopy, the excess heat and helium evidence disappeared. The published experimental data show this is not true. Jones also claimed that data was deliberately withheld, despite the fact that Miles has explained repeatedly that this supposedly "withheld" data was accidentally left out of one early paper and published in all subsequent papers. Jones claims the excess heat was caused by recombination of the effluent gas. Yet, Miles measured the effluent gas flow carefully, and saw no sign of significant recombination. The effluent gas was the main focus of the experiment, so it was measured repeatedly. Miles also points to extensive studies in the literature that show "no recombination (depolarization) effects were observed with fully submerged palladium cathodes run at high currents."

Jones raises a host of nit-picking objections, which often backfire on him. Here is part of a debate between Jones and Miles:

> Jones Query 2(a): Lee Hansen noted that RTV (silicone rubber) which you used to seal the calorimeter against moisture is not a reliable barrier. (Reference: Polymer Permeability, 1. Comyn, ea., Elsevier, 1985.) He also notes that vermiculite used as insulation is unusual and may itself contribute to moisture problems in the calorimeter. If the moisture content in the insulation changes, or moves, this will affect thermal conductivities and flow paths, in turn changing thermal "constants," and decreasing the reliability of your calorimeters.

> Miles Response: Any penetration of moisture into the insulation of our calorimeter would produce an increase in the calibration constant (WPC). No such increase has been observed experimentally. Our calibration constants in Table 3 show no time relationship over a one-year period.

> In an early experiment, water was accidentally injected into the insulation. This produced a marked increase in the calibration constant that made this calorimetric cell unusable. Although our sealant is not in direct contact with water, it is the same that is used as caulking in bathrooms and showers to seal against moisture.

The use of this moisture argument by Lee Hansen to explain our excess heat effects would require the insulation to become drier during the experiment while the cell is submerged in the water bath. This violates common sense as well as the Second Law of Thermodynamics. [16]

Like many skeptical objections, this one:

1. Cannot happen;
2. Did not happen, as the data shows;
3. If it did happen, it would be the skeptic's own problem; it would bolster the cold fusion claim.

In another example, Jones brings up the issue of helium diffusing through glass. He asks Miles to review the data for different glass flasks and compare it to the duration the flask was stored. Miles points out that the flasks which sat in storage longest were among those that showed the least amount of helium.

Termination of the Navy Program and Political Fallout

The Navy program ended in 1995 because the reproducibility problem could not be solved and because of the lack of any positive results at the Naval Research Laboratory (NRL). Miles is always polite, but he does not sugarcoat his evaluations. He got in trouble with the management at the Navy for criticizing the NRL in statements like this in his final report:

> The second approach that hindered progress was the extended focus on NRL Pd-Ag and Pd cathodes. The deuterium loading was poor for these materials, hence excess-heat production was not likely. This is exactly what was observed by our calorimetric experiments. Success with NRL materials was finally achieved for Pd-B alloys, but these results apparently came too late to save this program. Even the NRL Pd-B materials often showed folded-over metal regions or other flaws that would likely act as cracks and prevent high loadings. Our calorimetry generally yielded excess enthalpy when palladium samples that yielded excess heat at other laboratories were tested and never yielded excess heat when samples that contained flaws or that showed poor loading characteristics were tested.

> Some comments are necessary concerning the major disappointment that no excess power was measured in experiments conducted at NRL (Reference 36). The calorimeters at NRL were modified into a larger design to permit loading studies. This obviously changed the scaling of the cell, which is an important factor in these experiments (Reference 5). Furthermore, the room temperature control was very poor in the laboratory at NRL. Finally, the data acquisition system used at NRL did not average the cell potential. This large error source (±5%) is discussed on page 9. These factors led to a reported error of ±200 mW or larger (±10%) in experiments at NRL (Reference 36). Any excess power effect at NRL would have to be at least 600 mW to be clearly beyond experimental error. The excess-power effects observed at China Lake during this program would not have been detectable at NRL.

These statements do not strike me as strident or insubordinate. Nobody has denied the technical accuracy of the statements, and nobody can deny that they describe serious problems. Voltage changes rapidly in an electrochemical cell, mainly because of bubbles. The cell voltage

increases as bubbles collect on the electrode, then drops when bubbles disengage. Thus, the cell voltage is constantly fluctuating. At the NRL, voltage was read once every six minutes. This means they took 240 random samples per day. The standard technique for measuring a rapidly oscillating value like the voltage in an electrochemical cell is to take hundreds or thousands of samples over a minute, average them, and store this average value once a minute or every few minutes. I do not understand how a scientist could overlook the averaging technique. It is built into the canned software that comes with data collection instruments and add-on computer boards. It is described in textbooks, user manuals and even magazine advertisements.

His willingness to speak the truth got Miles into trouble with management. He feels this was precipitated by his criticism of the NRL experiments. Scientists at big laboratories sometimes feel hey have little to learn from people at smaller labs, or retired scientists like Storms who work at home. (Actually, Storms has state of the art calorimetry and high-end data acquisition equipment, better than used at NRL.) [17] The NRL made many changes in their calorimetry system without consulting with Miles, hence their calorimetric error became very large. Is Miles too outspoken? I find nothing objectionable in this report. No scientist should get in trouble with management for pointing out things like a 5% error caused by not averaging voltage, or errors caused by large fluctuations in ambient air temperature. It is the professional responsibility of a scientist to point out such problems.

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