Fleischmann, M. and M. Miles, *Letters from Martin Fleischmann to Melvin Miles* 2018: LENR-CANR.org

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Letters from Martin Fleischmann to Melvin Miles

Introduction

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This is a collection of letters between Martin Fleischmann, the co-discoverer of cold fusion, and Melvin Miles, who was one of the first to replicate the effect at the Naval Weapons Center, China Lake laboratory. It also includes some correspondence with Stanley Pons and various other people. The collection spans 13 years, from 1992 to 2005. Fleischmann and Miles co-authored several papers, including some with other researchers associated with the U.S. Navy, notably Pamela Mosier-Boss, Stanislaw Szpak and Ashraf Imam. Most of these papers are about calorimetry. They include:

Fleischmann, M. and M. Miles. *The "Instrument Function" of Isoperibolic Calorimeters; Excess Enthalpy Generation due to the Parasitic Reduction of Oxygen*. in *Tenth International Conference on Cold Fusion*. 2003. Cambridge, MA: LENR-CANR.org. http://lenr-canr.org/acrobat/Fleischmantheinstrum.pdf

Fleischmann, M. and M. Miles, *Thermal Behavior of the Polarized Pd/D_2O System* 2012: LENR-CANR.org. http://lenr-canr.org/acrobat/Fleischmanthermalbeh.pdf

Fleischmann, M., et al., *Experimental Evidence of Nuclear Reactions Generated in a Polarized Pd/D Lattice* 2012: LENR-CANR.org http://lenr-canr.org/acrobat/Fleischmanexperiment.pdf

Miles, M., M.A. Imam, and M. Fleischmann. *"Case Studies" of Two Experiments Carried Out With the ICARUS Systems*. in *8th International Conference on Cold Fusion*. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy. http://lenr-canr.org/acrobat/MilesMcasestudie.pdf

Miles, M., M.A. Imam, and M. Fleischmann, *Excess heat and helium production in the palladium-boron system*. Trans. Amer. Nucl. Soc., 2000. **83**(371): p. 72

Miles, M., M. Fleischmann, and M.A. Imam, *Calorimetric Analysis of a Heavy Water Electrolysis Experiment Using a Pd-B Alloy Cathode* 2001, Washington: Naval Research Laboratory. 155 http://lenr-canr.org/acrobat/MilesMcalorimetrd.pdf

Miles, M., M.A. Imam, and M. Fleischmann, *Calorimetric analysis of a heavy water electrolysis experiment using a Pd-B alloy cathode.* Proc. Electrochem. Soc., 2001. **2001-23**: p. 194

Miles, M., et al. *Thermal Behavior of Polarized Pd/D Electrodes Prepared by Co-deposition*. in *The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science.* 2002. Beijing, China: Tsinghua University: Tsinghua Univ. Press http://lenr-canr.org/acrobat/MilesMthermalbeh.pdf

Miles, M. and M. Fleischmann. *Isoperibolic Calorimetric Measurements of the Fleischmann-Pons Effect*. in *ICCF-14 International Conference on Condensed Matter Nuclear Science*. 2008. Washington, DC http://lenr-canr.org/acrobat/MilesMisoperibol.pdf

Miles, M. and M. Fleischmann. *Twenty Year Review of Isoperibolic Calorimetric Measurements of the Fleischmann-Pons Effect*. in *ICCF-14 International Conference on Condensed Matter Nuclear Science*. 2008. Washington, DC

Miles, M. and M. Fleischmann, *Accuracy of Isoperibolic Calorimetry Used in a Cold Fusion Control Experiment*, in *Low-Energy Nuclear Reactions Sourcebook*. 2008, American Chemical Society: Washington, DC. p. 153-171.

Miles, M. and M. Fleischmann. *New approaches to isoperibolic calorimetry*. in *15th International Conference on Condensed Matter Nuclear Science*. 2009. Rome, Italy: ENEA. http://lenr-canr.org/acrobat/ViolanteVproceeding.pdf#page=66

Miles, M. and M. Fleischmann, *Measurements of Excess Power Effects In Pd/D2O Systems Using a New Isoperibolic Calorimeter*. J. Condensed Matter Nucl. Sci., 2011. 4: p. 45-55. http://lenr-canr.org/acrobat/BiberianJPjcondensedc.pdf#page=53

Mosier-Boss, P.A., et al., *Thermal and Nuclear Aspects of the Pd/D_2O System (1)*, ed. S. Szpak and P.A. Mosier-Boss. Vol. 1 A Decade of Research at Navy Laboratories. 2002: SPAWAR Systems Center, San Diego, U.S. Navy http://lenr-canr.org/acrobat/MosierBossthermaland.pdf

Mosier-Boss, P.A. and M. Fleischmann, *Thermal and Nuclear Aspects of the Pd/D_2O System (2)*, ed. S. Szpak and P.A. Mosier-Boss. Vol. 2. Simulation of the Electrochemical Cell (ICARUS) Calorimetry. 2002: SPAWAR Systems Center, San Diego, U.S. Navy http://lenr-canr.org/acrobat/MosierBossthermalanda.pdf

Szpak, S., et al., *Thermal behavior of polarized Pd/D electrodes prepared by co-deposition.* Thermochim. Acta, 2004. **410**: p. 101. http://lenr-canr.org/acrobat/SzpakSthermalbeh.pdf

Most of these letters are discussions of work in progress, such as corrections and suggestions to manuscripts, and detailed nitty-gritty discussions of calorimetry.

You can learn a lot about Martin Fleischmann from these letters. But, if you would like a shortcut to learning what sort of person he was, how he talked, what he thought about cold fusion and various other subjects, you might start with his interview with Christopher Tinsley. [1] He was not shy about expressing his opinions, and he told Chris much of what he says here.

[1] Tinsley, C., An Interview with Professor Martin Fleischmann. Infinite Energy, 1996(11). http://lenr-canr.org/acrobat/TinsleyCanintervie.pdf

A chronicle of frustration and failure

Let me describe some of the events discussed in the letters. The reader may not understand these letters without knowing this background. Many of these events were infuriating. They include: IMRA and the NHE withholding Fleischmann's own data from him; attacks by opponents such as Douglas Morrison (CERN) and John Huizenga (the head of the DoE ERAB panel on cold fusion); failed attempts to get funding; failed attempts to publish papers; and, research programs that cost too much, took too long, and accomplished nothing. Fleischmann's experiences parallel those of other researchers. In my opinion, despite important technical progress, the history of cold fusion has been an unmitigated disaster because of academic politics.

Disputes with NHE

Fleischmann and Miles both worked with the Japanese NHE (New Hydrogen Energy) project of the government agency NEDO (New Energy and Industrial Technology Development Organization). [2] The NHE spent millions of dollars in the 1990s on cold fusion research. It did not make much progress, and in the end the project was abandoned. Many cold fusion researchers felt it was poorly conducted. As you will see here, Fleischmann was very upset with the NHE managers. He felt that they went so far as to lie about his work. Miles spent time working in the NHE laboratory in Sapporo, Japan, using calorimeters developed by Fleischmann. Fleischmann left detailed instructions for how to use these calorimeters. Miles followed the instructions carefully. Miles and Fleischmann feel that the NHE staff did not follow these instructions and did not understand how to use the equipment. In these letters he often complained that they ignored his advice and did not respond to his questions.

Miles has mixed feelings about the project and its managers. On one hand, as he says in one of the letters, they offered him complete academic freedom and they were some of the best managers he ever worked with. (2000-07-20) On the other hand, he was upset with them because in their final report, they did not mention his conclusions about his own experiments.

Miles concluded that he measured excess heat in some tests. The NHE managers disagreed. They did not think his experiments produced excess heat. In their final report, they described only their own interpretation of his results. Suppose the final report had said: "Dr. Miles concluded that he measured excess heat in several experiments, but we disagree. Based on our analysis, the calorimeter was extremely inaccurate, and there was no excess heat." That would be the normal way to describe a scientific disagreement. Instead, the report left out any mention of Miles' own analysis. It did not show his graphs or tables of results.

Miles and Fleischmann might never have known about the NHE final report because it was in Japanese, and no one at the NHE told them what it said. However, as it happened, someone provided a copy of the report to me, and I translated the relevant portions into English. (My

[2] Asami, N., K. Matsui, and F. Hasegawa. *Present Status and the Perspective of New Hydrogen Energy Project.* in 5th International Conference on Cold Fusion. 1995. Monte-Carlo, Monaco: IMRA Europe, Sophia Antipolis Cedex, France. http://lenr-canr.org/acrobat/PonsSproceeding.pdf#page=103

translation is in letter 2000-02-03. See also 2000-01-14.) This ignited a brouhaha between the NHE and Miles. My role in this debacle was described in a message from Eliot Kennel to Miles:

> . . .it seems that Jed will probably allege that there was unethical behavior at the lab and suppression of data, including your experiments. [NHE managers] Matsui-san and Asami-san have indicated to me that they don't care what Jed writes, but they do care about your opinion. (2000-02-03)

This is a comical way to describe the situation. I did not "allege" anything. I translated what Matsui and Asami themselves wrote in their official government final report.

I will describe the technical disagreement between Miles and the NHE below. First let me turn to another sad chapter in the history of cold fusion, which is not widely known.

Why the project in France ended

Fleischmann and Pons worked at IMRA in France. IMRA is a Toyota research company. Fleischmann and Pons made progress, culminating in cells that boiled for more than three months producing excess power from 20 to 100 Watts, at power densities roughly equivalent to a nuclear reactor uranium fuel pellet, as shown in the tables below.

Experiment	1	2	3	4	5	6	7
Cathode	Pd	Pd	Pd	Pd	Pd	Pd	Pd
Rod size, mm	100×2	100×2	100×2	100×2	100×2	12.5×2	12.5×2
Anode	Pt coil	Pt coil	Pt coil	Pt coil	Pt coil	Pt mesh	Pt mesh
Electrolyte: 0.1M	LiOD	LiOD	LiOD	LiOD	LiOD	LiOD	LiOD
Electrolyte, mL:	90.7	90.0	90.6	97.0	97.0	90.4	90.9
Expt time, days	94	134	158	123	123	47	60
Pwr_{excess}/W/4.2hr	-0.1	-0.6	101	17.3	13.8	74.5	39.4
Total energy, MJ	-0.0	-5.5	294	102	0.3	30.5	-7.6
% excess power	0	0	150 (30d)	250 (70d)	0	Variable	~0

Table from Roulette, T., J. Roulette, and S. Pons. Results of ICARUS 9 Experiments Run at IMRA Europe. in Sixth International Conference on Cold Fusion, Progress in New Hydrogen Energy. 1996. Lake Toya, Hokkaido, Japan: New Energy and Industrial Technology Development Organization, Tokyo Institute of Technology, Tokyo, Japan. http://lenr-canr.org/acrobat/RouletteTresultsofi.pdf

	Volume	Operating temperature	Power density by volume	Power density by area
Cold fusion cathode	0.3 cm^3	100°C	300 W/cm^3	16 W/cm^2
Fission reactor fuel pellet	1.0 cm^3	300°C	180 W/cm^3	32 W/cm^2

Roulette *et al.* power density compared to a fission reactors fuel pellet, by volume or by surface area. From the Youtube video "A Brief Introduction to Cold Fusion." http://lenr-canr.org/wordpress/?page_id=1618

While this was extraordinary, it did not mean they were on the cusp of developing practical technology. The reaction was not well controlled, and only two out of seven experiments produced such spectacular results.

A few years after these promising results were obtained, the project came to an end. Fleischmann returned to England. When I visited him in 1997, he told me what happened. He first said that he was not satisfied with this progress. He felt the managers of the program should have given him more leeway to explore the topic, and to try new approaches. He felt the managers were not serious. He said they came to France and played golf instead of managing or learning about the science. But, that was not why the project came to an end. It ended because of a complicated business arrangement and a falling out between Toyota and Johnson Matthey.

This was a joint venture. Johnson Matthey supplied the palladium and then took it back, performing all of the material analysis themselves. They did not share the results with Toyota or with Fleischmann. Edmund Storms also told me this. Storms and I consider this an unworkable way to run a research project. The materials are the most critical aspect of a cold fusion experiment. Some palladium works, but most does not. No palladium works as well as Johnson Matthey's. This was demonstrated most clearly by Miles. [3] So, Johnson Matthey was in a position to know why and how progress was being made, while Fleischmann and Toyota were kept in the dark. This is excessive secrecy. It is one thing to keep results confidential from the outside world, but to keep them secret from the principal researcher will surely stymie progress!

The project made progress despite this weird arrangement, until the managers at Toyota began to sense that it might result in useful technology. Indeed, it was beginning to look like it might result in the most profitable breakthrough in history. Toyota decided to renegotiate the business arrangement with Johnson Matthey. Fleischmann did not tell me the details, but he said Toyota got greedy and demanded 'all the marbles.' They wanted control over the project, and they wanted to give Johnson Matthey only a small fraction of future profits. Johnson Matthey did not agree to this, and they abruptly ended the collaboration. Fleischmann strongly supported Johnson Matthey.

That, in brief, is what Fleischmann told me. I cannot vouch for this account, but I suppose it is true. He had no reason to lie to me. I do not see how anyone might argue that he was covering something up, making excuses, or trying to make Toyota or Johnson Matthey look good. On the contrary, I cannot imagine a worse fiasco! Apparently, two world-class corporations abandoned what may be the most profitable venture in history because of a ridiculous short-term disagreement.

When Fleischmann returned to England after the project, IMRA promised to ship him copies of the experimental data, but they never did. "All of my own data were removed from the material sent back to me from France . . ." (2001-01-29) He was furious about this. He was forced to spend many months trying to re-create the data from the information he carried home. I believe he worked mainly with graphs, drawing lines to see where the data points intersected with the axes. He was doing that when I visited him. I do not know how he managed to re-create data to five significant decimal places from graphs. He describes this tedious work in several of the letters:

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[3] Miles, M. and K.B. Johnson, *Anomalous Effects in Deuterated Systems, Final Report*. 1996, Naval Air Warfare Center Weapons Division, Table 10. http://lenr-canr.org/acrobat/MilesManomalousea.pdf

As you will see, I have now recovered the position we had reached in 1992/93 which was the basis of the design of the ICARUS 1 System. It is all a monumental waste of time! (2002-09-08)

In some of these letters, Fleischmann may have referred to the "NHE" when he meant IMRA. Or, perhaps they were both managing the projects he worked on. I would not know about that.

Attacks by Skeptics

There have been hundreds of harsh attacks from cold fusion and the mass media by prominent scientists. You can see examples in a list compiled by Mallove. [4] In these letters, Fleischmann often mentions four opponents: Douglas Morrison, John Huizenga, Peter Zimmerman and Kirk Shanahan. I will discuss them here.

Douglas Morrison of CERN was a leading opponent in the early years of cold fusion. He posted periodic reports on cold fusion on the internet. His "Cold Fusion Update No 6" is included in this collection of letters (1992-05-05). He wrote a critique of one of Fleischmann's experiments, and Fleischmann wrote a rebuttal. Both were modified somewhat and later published in Physics Letters A. [5,6] The earlier drafts are here:

http://lenr-canr.org/acrobat/Fleischmanreplytothe.pdf

Let me discuss a major problem in Morrison's claims. Regarding the "cigarette lighter effect" (the combustion of deuterium as it degasses from palladium), Morrison cites Kreysa *et al.*, who reported that when a palladium sheet cathode loaded with deuterium or hydrogen was placed on a block of wood, the heat from combustion scorched the wood. [7] He also cited Kreysa *et al.* saying that this effect releases 147.3 kJ per mole D. That figure is correct. It is the textbook heat of formation of water, 285,800 joules per mole, divided by 2. It is divided by 2 because it takes 2 moles of deuterium and 1 mole of oxygen to make up 1 mole of D_2O. However, both Kreysa and Morrison failed to understand the difference between power and energy. So, their statements are meaningless. Based on elementary chemistry they should have realized that even though there was enough power to scorch the wood, the cathode produced thousands of times less energy than Fleischmann reported.

It is easy to estimate the maximum amount of deuterium that could be available. Assume the palladium is fully loaded with deuterium at a ratio of 1:1; with one atom of deuterium for each

[4] Mallove, E., *Classic Nasty, Incompetent, and Stupid Statements About Cold Fusion*. 1991. http://lenr-canr.org/acrobat/MalloveEclassicnas.pdf

[5] Morrison, D.R.O., *Comments on claims of excess enthalpy by Fleischmann and Pons using simple cells made to boil*. Phys. Lett. A, 1994. **185**: p. 498

[6] Fleischmann, M. and S. Pons, *Reply to the critique by Morrison entitled 'Comments on claims of excess enthalpy by Fleischmann and Pons using simple cells made to boil*. Phys. Lett. A, 1994. **187**: p. 276. http://lenr-canr.org/acrobat/Fleischmanreplytothe.pdf

[7] Kreysa, G., G. Marx, and W. Plieth, *A critical analysis of electrochemical nuclear fusion experiments*. J. Electroanal. Chem., 1989. **266**: p. 437

atom of palladium. In practice, it is impossible to achieve such high loading, but this would be the upper limit. The formula is:

$$\text{Moles Pd} = \frac{(\text{Volume} \times \text{Density } 12\ \text{g/cm}^3)}{\text{Atomic mass } 106\ \text{g}}$$

$$\text{Moles H (or D)} \approx \text{Moles Pd}$$

$$\text{Moles water} = \frac{\text{Moles H (or D)}}{2}$$

Kreysa's cathode was a sheet 0.1 cm × 1 cm × 2 cm, which is 0.2 cm^3 volume. That much palladium weighs 2.4 g, which is 0.023 moles. So, it would produce at most 0.011 moles of water, and 3,235 J of heat. That is roughly as much heat as you get from burning 3 kitchen matches. The palladium could not have been fully loaded, so it produced about as much heat as one or two matches. When this much heat emerges rapidly, within about a minute, as reported by Kreysa, it is more than enough to scorch a block of wood. However, energy density is 1,348 J/cm^3 of palladium, whereas Fleischmann's paper reported more than 4,000,000 J/cm^3. That is 2,967 times more energy.

To put it another way, Kreysa concluded: "Assuming that the palladium was loaded with about 80 atom% of hydrogen this corresponds to an estimated heat flow of 35.9 W and a heat flow density of 179.6 W cm^{-3}. This is much more than all the 'excess' heat flow densities reported in Table 1 of ref. 1 [Fleischmann *et al.*[8]]" A heat flow of 35.9 W is power. Kreysa estimated it continued for about a minute, producing ~2,100 J of energy, which is in reasonable agreement with my estimate. Then it stopped. If this had been the same reaction Fleischmann observed, it would have continued for 49 hours.

In his critique, Morrison analyzed another test by Fleischmann and Pons with a boil-off cell. In that test, the cathode was a rod 2 mm in diameter and 1.25 cm long. That comes to 0.03 cm^3 and 0.48 g. Fully loaded, that will produce at most 650 J. Fleischmann described this confusion:

> In the first place we note that the explanation of Kreysa *et al.* could not possibly have applied to the experiment in question: the vapourisation of the D_2O alone would have required ~1.1 MJ of energy whereas the combustion of all the D in the palladium would at most have produced ~650 J (assuming that the D/Pd ratio had reached ~1 in the cathode), a discrepancy of a factor of ~1,700. In the second place, the timescale of the explanation is impossible: the diffusional relaxation time is ~ 29 days whereas the phenomenon took at most ~ 6 hours (we have based this diffusional relaxation time on the value of the diffusion coefficient in the alpha phase; the processes of phase transformation coupled to diffusion are much slower in the fully formed Pd-D system with a corresponding increase of the diffusional relaxation

[8] Fleischmann, M., S. Pons, and M. Hawkins, *Electrochemically induced nuclear fusion of deuterium.* J. Electroanal. Chem., 1989. **261**: p. 301 and errata in Vol. 263. http://lenr-canr.org/acrobat/Fleischmanelectroche.pdf .

time for the removal of D from the lattice). Thirdly, Kreysa *et al.* confused the notion of power (Watts) with that of energy (Joules) which is again an error which has been promulgated by critics seeking "Chemical Explanations" of "Cold Fusion". [9]

Kreysa and Morrison confused power and energy, and they calculated the wrong answer by 3 orders of magnitude (factors of at least 2,967 and 1,700). Quantitative calculations are the essence of science, so that was a grave mistake. I wonder if they even tried to estimate energy. Concepts such as the heat of formation of water and the number of moles per gram are taught in middle school chemistry. A middle school student making such large mistakes would get a failing grade. The reader may feel I am beating a dead horse, so let me point out that many of technical arguments made by Morrison, Huizinga, Shanahan and other leading skeptics are as weak as this. The problems are not usually so apparent, but according to the textbook laws of chemistry and physics, they are wrong.

Morrison was sometimes sloppy. He and other critics often failed to pay attention. They saw problems where none exist. Morrison wrote a paragraph criticizing Fleischmann and Pons for using "a complicated non-linear regression analysis." In his rebuttal, Fleischmann wrote: "Douglas Morrison starts by asserting: 'Firstly, a complicated non-linear regression analysis is employed to allow a claim of excess enthalpy to be made'. He has failed to observe that we *manifestly have not used this technique in this paper* . . ."

Edmund Storms wrote this about Morrison:

> I talked to Morrison on several occasions and got the impression he thought he was doing a public service by forcing Fleischmann to answer questions that needed to be answered more clearly than Fleischmann was doing without this encouragement. In addition, taking a skeptical approach in those days attracted a lot of positive attention, which Morrison enjoyed.[10]

John Huizenga was the head of the first DoE ERAB review panel of cold fusion, [11] and the author of the book *Cold Fusion: The Scientific Fiasco of the Century*. [12] Fleischmann mentions him four times in these letters, concluding that "I cannot believe that he is really as stupid as appears at first sight." Many leading opponents to cold fusion, such as Huizinga, were distinguished scientists. No doubt they made important contributions in their own fields, and as Fleischmann said, they were not stupid. However, I feel that with regard to this one subject, they were not attentive. They were biased. Huizinga concluded his book with a 6-point summation. Points 5 and 6 state that we know *a priori* that all positive cold fusion excess heat results must be wrong:

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[9] Fleischmann, M. and S. Pons, *Reply to the critique by Morrison entitled 'Comments on claims of excess enthalpy by Fleischmann and Pons using simple cells made to boil*. Phys. Lett. A, 1994. **187**: p. 276. http://lenr-canr.org/acrobat/Fleischmanreplytothe.pdf

[10] E. Storms, e-mail, 2018.

[11] ERAB, *Report of the Cold Fusion Panel to the Energy Research Advisory Board* 1989, Washington, DC. http://lenr-canr.org/acrobat/ERABreportofth.pdf

[12] Huizenga, J.R., *Cold Fusion: The Scientific Fiasco of the Century*. 1992, Rochester, NY: University of Rochester Press

5. If the reported intensity of nuclear products is orders of magnitude less than the claimed excess heat, then the excess heat is not due to a nuclear reaction process.

6. Furthermore, if the claimed excess heat exceeds that possible by other conventional processes (chemical, mechanical, etc.), one must conclude that an error has been made in measuring the excess heat."

Beaudette wrote: "what sentences 5 and 6 assert is that nuclear measurements are science, and calorimetric measurements are not science. Throw away their measurements and keep mine. I wonder if there can be found in science a more narrow, a more provincial view of one's professional specialty that is held in these sentences." [13]

Peter Zimmerman was the science advisor to the State Department. He and Robert Park attacked cold fusion during an APS conference. I attended the conference and reported:

> [Zimmerman] said that one of his first official acts was to cancel a meeting about cold fusion . . . and 'that's one of the accomplishments I'm proudest of within the last year.' He announced that he and Park will work to exterminate every trace of cold fusion and all other 'junk science' from the Federal establishment. They will see to it that no other meetings are held anywhere else in Washington, which is a hotbed of cold fusion as we all know. He called upon the audience to join him in this crusade, and to report to the . . . authorities any rumors about unauthorized research and groups of more than three people caught discussing cold fusion. [T]his was met with cheers and applause from an overflow crowd there . . . [14]

Robert Park made similar incendiary remarks. His anger was fresh, as if cold fusion has been announced a week earlier. Park thinks that cold fusion is lunacy and criminal fraud. [15]

Kirk Shanahan wrote a number of papers finding fault in the work of Fleischmann, Storms and others. [16,17,18] Fleischmann considered responding, but in the end he did not. Marwan *et al.* and Storms did respond. [19,20] Shanahan says he does not agree with their conclusions.

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[13] Beaudette, C.G., *Excess Heat: Why Cold Fusion Research Prevailed*. 2002, Concord, NH: Oak Grove Press

[14] Mallove, E. and J. Rothwell, *The pseudoscientists of APS*. Infinite Energy, 1999. 5(25): p. 23. http://lenr-canr.org/acrobat/MalloveEthepseudos.pdf

[15] Park, R.L., BOOK WORLD The Fizzle in the Fusion, in Washington Post. 1991. Quote: "'Was this a delusion, an error, or a fraud?' By the end of the book, it is clear that cold fusion progressed through all three." Park has made similar comments many times.

[16] Shanahan, K., *A Critique of the Student's Guide To Cold Fusion*. 2003, LENR-CANR.org. http://lenr-canr.org/acrobat/ShanahanKacritiqueo.pdf

[17] Shanahan, K., *A Possible Calorimetric Error in Heavy Water Electrolysis on Platinum*. Thermochim. Acta, 2002. **387**(2): p. 95-101. http://lenr-canr.org/acrobat/ShanahanKapossiblec.pdf

[18] Shanahan, K., Reply to *'Comment on papers by K. Shanahan that propose to explain anomalous heat generated by cold fusion,' E. Storms*. Thermochim. Acta, 2005. **441**: p. 210.

[19] Marwan, J., et al., *A new look at low-energy nuclear reaction (LENR) research: a response to Shanahan*. J. Environ. Monit., 2010. **12**(9): p. 1765-1770. http://lenr-canr.org/acrobat/MarwanJanewlookat.pdf

[20] Storms, E., *Comment on papers by K. Shanahan that propose to explain anomalous heat generated by cold fusion.* Thermochim. Acta, 2006. **441**: p. 207-209. http://lenr-canr.org/acrobat/StormsEcommentonp.pdf

In his interactions with me, Shanahan has sometimes said mind-boggling things. I cannot tell whether he is stupid or he is putting me on. Here is one example:

Tadahiko Mizuno reported that he had a cell with a 100 g cathode which produced intense heat after death for several days, with the power exceeding 100 W at first. For a detailed description of this event, see:

Mizuno, T., *Nuclear Transmutation: The Reality of Cold Fusion*. 1998, Concord, NH: Infinite Energy Press. http://lenr-canr.org/acrobat/MizunoTnucleartra.pdf

Shanahan told me he does not believe the heat was real. I pointed out three reasons why it must have been real:

1. Both Mizuno and his colleague Akimoto reported that the cell was far too hot to touch. Mizuno had to wrap it in towels to pick it up and move it to another room.
2. The thermocouple installed in the cell registered over 100°C for the first few days.
3. When the cell was placed in a bucket of water, the water evaporated overnight. Up to 10 liters per night evaporated, and more would have evaporated but the bucket only held 10 liters. This happened several nights in a row.

I asked Shanahan how he might explain these three points. He refused to discuss number 1. Regarding number 2, he said that the thermocouple may have been malfunctioning. I pointed out that it worked before and after the experiment. Regarding number 3, Shanahan claims there is a DoE website about swimming pools which shows that a bucket of water left in ordinary room temperature conditions might evaporate overnight. He did not tell me what website this is. I suggested he try leaving a bucket in a room to see what happens, but he did not respond. He came up with an alternative hypothesis. He believes that the Nuclear Engineering Department laboratory building at Hokkaido National University may have been overrun with "vermin" including rats. He thinks these rats drank all the water from the bucket every night. I pointed out a number of reasons why this cannot be true:

- A rat drinks ~10 mL of water a day, so there would have to be ~1000 rats. They would be lined up in rows waiting their turn to drink.
- A rat could not reach the bottom of the bucket without drowning.
- The water was quite hot, and rats do not drink hot liquids.
- There was no sign of rats in the facility. Large rat infestations are readily apparent from the damage and stench.
- If there had been even a small infestation, the university authorities would have eliminated it, because this laboratory had nuclear reactors, samples of radioactive material, expensive and sensitive instruments and so on.

Shanahan did not respond to any of these points.

Fleischmann thought that Huizinga and Morrison did not really believe what they said. He thought they were too smart for that. If they were smart, I cannot understand why they would make such unconvincing arguments. Perhaps Morrison was trying to fool the public, but it hard to see how he hoped to fool professional scientists, who usually know the difference between power and energy. My impression is that Morrison and Kreysa did not know the difference. Morrison's arguments are so unconvincing I suppose they would persuade many scientists that cold fusion is real. A reader will think: "Come now! Is that the best you can come up with?! You have no case."

Fleischmann's Attitudes

I first encountered Martin Fleischmann when he came to MIT to give a lecture. The public address system was broken. The first thing I heard him say was that he did not need a microphone: "That's alright. I'm quite used to shouting. I've been an academic all my life."

That sums him up. He was feisty, and an academic to the tips of his fingers. After the discovery of cold fusion, he often had to shout, and he viewed the events surrounding cold fusion mainly as an academic dispute. At times I was dismayed by his attitude. I was once commiserating with him about how bad things were. How awful it was that opportunities are lost and people are suffering for lack of energy. I realized that he was complaining mainly about academic politics. I thought that was a crabbed perspective. It was a narrow view of the world unbecoming of a genius who contributed so much to so many fields of science and technology. [21]

Fleischmann believed that cold fusion might become a practical source of energy. He once told me that he estimated that with present supplies of palladium we might generate about a third of all the energy in the world, even assuming that nickel or titanium do not work. He understood how important energy is to the economy, to poor people, and to prevent global warming. Yet he discussed the possibility that cold fusion might become a practical source of energy only a few times in this collection of letters.

Not only was he focused on the academic politics, but often in these letters, and in Tinsley's interview, we see he would have been willing to abandon the research for something more interesting. Or, as he often says, the sake of national security. He wanted to keep the research secret because it might have weapons applications. If the DoD had ordered him to stop the research, he would have done so without hesitation. It seems that in his mind, Cold War competition took precedence over the need for energy. Fleischmann suffered terribly during the Second World War, His father was arrested by the Gestapo and tortured. To everyone's surprise, the Gestapo let his father go, but after the family reached England he died of his wounds. Fleischmann had a dark view of human nature. [22] The Cold War dominated his adulthood. Yet to me, the notion that we would turn our back on cold fusion because it *might* have weapons applications is nihilistic. We must not forget that tens of thousands of people die every week for

[21] *Developments in Electrochemistry - Science Inspired by Martin Fleischmann*, ed. D. Pletcher, Z.Q. Tian, and D.E.G. Williams. 2014: Wiley

[22] See Tinsley interview. On the other hand, Fleischmann had a good sense of humor and a whimsical nature. He once told me, "it has been a lifelong ambition of mine to give a lecture in blank verse."

lack of energy, and that most pollution comes from energy generation, and that global warming is a dire threat to millions of people and to the entire ecosystem. We should not put aside this potential source of energy because it *might* be dangerous, when we know the alternatives *are* dangerous.

His view of this as an academic bun fight sometimes went to extremes, such as when he said his claims about theory might trigger even more opposition than cold fusion: "Incidentally, I predict that this article on electrolyte solutions will raise a worse 'stink' than the C.F. topic mainly because people have developed such entrenched positions." (2000-07-02)

Several times in these letters Fleischmann subscribes to one conspiracy theory or another, about things like D.U. shells (depleted uranium artillery shells), health hazards from cell phones, and most of all, a conspiracy to oppose cold fusion. It is not surprising he believed in such things. He was the object of worldwide acrimony from thousands of scientists. Toyota or possibly some nefarious government agency apparently stole his data. If anyone had a right to believe he was being persecuted by ominous dark forces, Fleischmann did. He knew his own nature. As he wrote in one of his many post-post-scripts: "P.P.P.P.P.S. You may think that I am a very suspicious person. Of course, this is absolutely correct." (1999-11-19)

I do not have access to his papers and I know little about what happened to Fleischmann. On the other hand, I know a lot about the opposition to cold fusion. So, let me speculate about this. Is there a conspiracy? If there is, I would be the last to know. The conspirators will not invite me to their monthly meetings. But I have the impression there is no conspiracy, or if there is one, it has had little effect compared to the open academic politics that have afflicted this field.

The definition of a conspiracy is a surreptitious, organized effort. People opposed to cold fusion are not surreptitious. They are loud. They are in your face, like Zimmerman and Park at the APS. [23] They have widespread support. Hundreds of APS scientists gave Zimmerman and Park a standing ovation. They publish in the *Washington Post*, *Scientific American* and other mass media. They do not seem organized to me. It may seem that I am nitpicking here, or quibbling about the definition of "conspiracy," but if the opposition is not a conspiracy but rather academic politics, we must deal with it as such. (Fleischmann agreed with me about this; see letter 2000-01-10.)

Are there nasty people opposed to cold fusion? Yes, I have encountered many of them. However, I agree with the aphorism that "one should never attribute to malice that which is adequately explained by stupidity." Much of the opposition comes from smart people who make stupid arguments because they are blinded by emotion, such as Morrison and Huizenga. They knew far more about physics than I do, yet I can poke holes through their arguments. It is like poking through wet tissue paper. Their emotional response is understandable, even laudable in a sense, as described in Mallove's coda to his book *Fire from Ice*: [24]

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[23] The only slightly surreptitious thing about the incident was that Zimmerman did not let me see his State Department badge. When I asked to see it, he held some papers up to his chest to cover it. This was hilarious.
[24] Mallove, E., *Fire From Ice*. 1991, NY: John Wiley

"I know that most men, including those at ease with problems of the greatest complexity, can seldom accept even the simplest and most obvious truth if it be such as would oblige them to admit the falsity of conclusions which they have delighted in explaining to colleagues, which they have proudly taught to others, and which they have woven, thread by thread, into the fabric of their lives." – Tolstoy

Here is a less forgiving view:

"The inertia of the human mind and its resistance to innovation are most clearly demonstrated not, as one might expect, by the ignorant mass — which is easily swayed once its imagination is caught — but by professionals with a vested interest in tradition and in the monopoly of learning. Innovation is a twofold threat to academic mediocrities: it endangers their oracular authority, and it evokes the deeper fear that their whole, laboriously constructed intellectual edifice might collapse. The academic backwoodsmen have been the curse of genius from Aristarchus to Darwin and Freud; they stretch, a solid and hostile phalanx of pedantic mediocrities, across the centuries." – Arthur Koestler, *The Sleepwalkers* New York, 1959, p. 427. [25]

Chaotic Work Habits

As you see in these letters, Fleischmann was hard-working. He had amazing powers of concentration and he worked hard every day for as long as he was able. Unfortunately, he was often ineffectual and unorganized, he wasted a great deal of time, and he did not take advantage of computers or the internet. He often lost papers and addresses, and asked Miles to send copies. Many of his friends, including me, offered to help him, but he preferred to work alone using manual methods.

A few examples:

I believe that we may shortly have to communicate rather frequently and I may decide to hitch up my gear to the e-mail. Meanwhile, we will have to rely on the FAX and you may find that it is not possible to reach my number (both my FAX and telephone have developed strange but understandable quirks in recent weeks). 1999-01-12

From 2005:

In this regard, I have noted that the P.C. which controls our e-mail is completely corrupted by various species of spy-ware in spite of our restriction on it's usage. We will try to tidy up the system but, meanwhile we have gone back to using the Fax and snail-mail. Incidentally, I think that I know who planted the spy-ware in our computer system. 2005-07-01

This is an example of chaos and also a manifestation of a conspiracy theory. There may well have been spyware in his computer. It was common in 2005. Spyware was usually installed by hackers who aimed to steal credit card numbers. It is unlikely Fleischmann would have known who did this. If spyware had been installed by a professional, such as Toyota or a government

[25] There are dozens of quotes along these lines, quite depressing, at this web site: http://amasci.com/weird/skepquot.html.

agency, it would not have been disruptive. No one would have known it was there. Spyware that you see is disrupting the computer is installed by amateurs, and it is easily removed. Any local computer consultant could have fixed this problem.

I visited Fleischmann at his house in 1997. I can attest that he spent the mornings wearing a red bathrobe, puttering around, immersed in work. There were piles of paper everywhere. When I was there, he was graphing data by hand, with a pencil on graph paper. I asked him why he did not use a computer. As I recall he said working with pencil and paper gave him a better feel for the data. What I also recall clearly is that he also said he did not trust computer printers because they produce distorted images. He said that a square graph from a computer printer is not square: it is elongated, which makes it impossible to draw lines and intercepts to measure values — a method close to his heart, and mine. He was right about that. Computer printers in the 1980s did have that problem. I saw it even with expensive LaserJet toner printers. However, by the late 1990s, this problem was solved, and long before that he might have used a dedicated computer plotter rather than a printer. Plotters never distorted images.

Fleischmann had a deep understanding of science and technology, and he should have known that although computers have their foibles they can be made to work with help from friendly experts. From these letters you can see that he was also cut off from email, which by the 1990s meant he was cut off from humanity, and from his colleagues. He depended upon fax machines, which were unreliable and a nuisance. As I said, I wish he had accepted more help from Tinsley, from me and from others. One person was able to assist him: Michael Clarke, who lived nearby. He often helped with the computer and various other things. I believe this was on a volunteer basis. We owe Clarke a debt of gratitude.

Fleischmann's Favorite Graph

McKubre pointed out that Fleischmann was a master of theory and mathematics, in ways that people with post-1940s educations seldom attain. Fleischmann would often point to something and say "that is obvious" when it was not a bit obvious to McKubre. Fleischmann's mathematical analysis of calorimetry was far more complex than most people's. He told me he preferred simple hardware and complicated "software" — by which he meant computation; thinking and running equations in his head. Not computers, which, as I said, he distrusted.

Hand in hand with his analytical legerdemain, he strongly believed in simple, direct experiments, such as the boil-off technique and graphs that spoke for themselves. He liked nothing better than an experiment stripped down to its essentials, so that it could not be refuted. The title of his major paper says it all: "From simplicity via complications back to simplicity." [26]

Regarding graphs, he said the eye is the best and most important tool. He often said things like, "one can see that we must have observed excess enthalpy generation of order 10W cm^{-3} just by eye-balling the data." (2003-07-04) He was particularly fond of graphs showing the onset of

[26] Fleischmann, M. and S. Pons. *Calorimetry of the Pd-D2O System: from Simplicity via Complications to Simplicity*. in *Third International Conference on Cold Fusion, "Frontiers of Cold Fusion"*. 1992. Nagoya Japan: Universal Academy Press, Inc., Tokyo, Japan http://lenr-canr.org/acrobat/Fleischmancalorimetra.pdf

the reaction, and positive feedback. There is a splendid example of this in one of these letters, from Miles (2000-10-30). This is from a cell that Miles operated at the NHE laboratory. Here is a heat pulse applied when the cell was not producing excess heat:

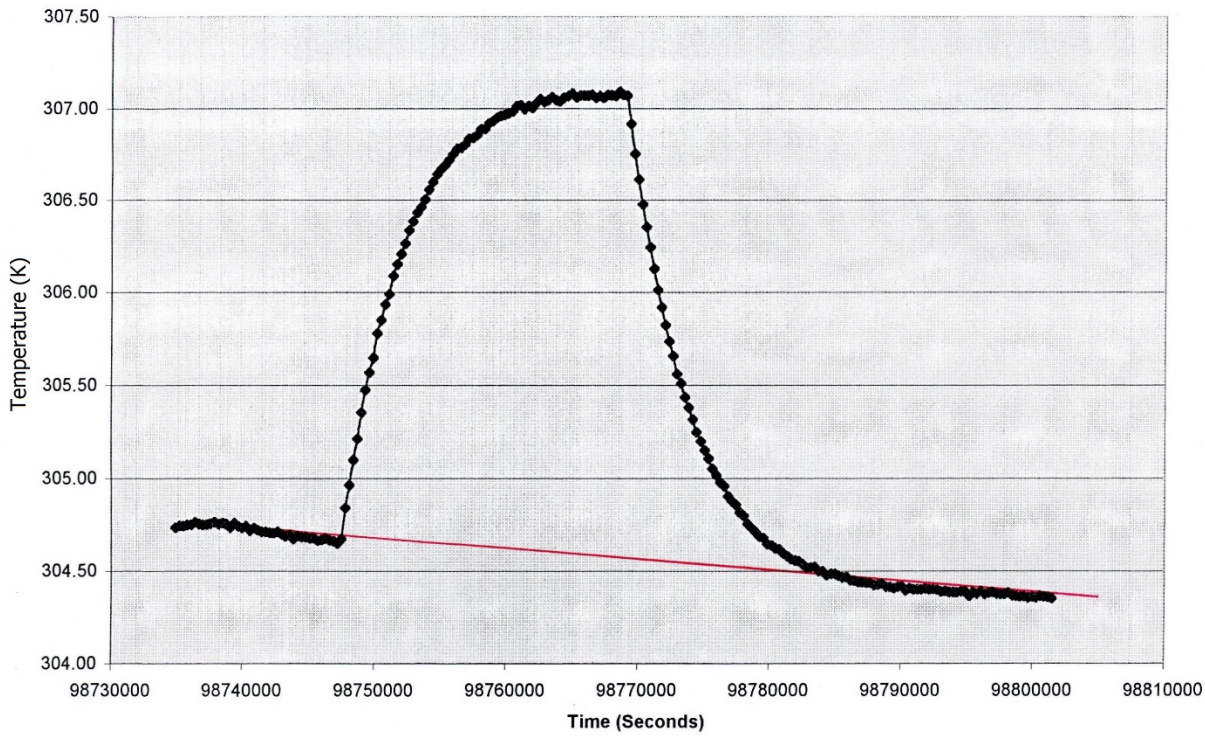

Figure 1. Miles, co-deposition cell A2 second heating pulse. Cell returns to same baseline, with near normal temperature behavior. This shows there is no excess heat.

Here is a heat pulse applied to the same cell earlier in the test, when the cell was producing excess heat:

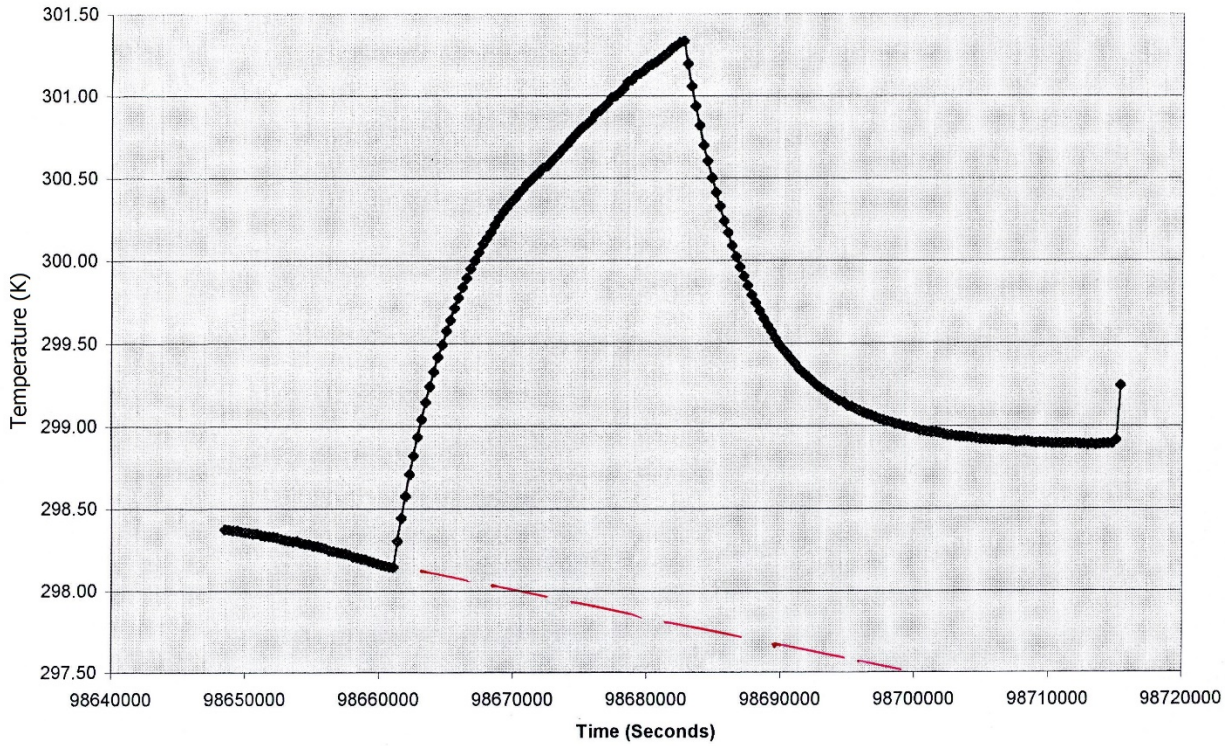

Figure 2. Miles, co-deposition cell A2 first heating pulse. There is a higher baseline after the pulse. The temperature is higher even though the power going into the cell is lower. This shows there is excess heat and positive feedback.

This result led to a dispute with the NHE which is described in detail in the letters. (2000-02-03) Let me summarize it.

Miles assumed there was no excess heat being produced in Fig. 1, and some excess in Fig. 2. He set the "cell constant" for the entire test at the level shown in Fig. 1.

The researchers at the NHE assumed there was no excess heat in Fig. 2. This pulse was applied soon after the test began, so they assumed that excess heat was not yet being produced. This was a reasonable expectation, because in many early cold fusion experiments it took weeks before excess heat began. However, they should have looked more closely at the response to the heat pulse. They would have seen there was already excess heat.

The problem is, setting the cell constant after heat begins makes the constant too high. In this case they ended up setting the constant so that about half of the time the cell appeared to be absorbing heat instead of producing it. On Day 60, when there should have been no heat, there is instead negative 30,000 J. This is impossible. I suppose they concluded that the calorimetry was unreliable, and it was producing large random results both positive and negative. These graphs clearly illustrate the difference between the two cell constants. Here are the results using the cell constant Miles computed:

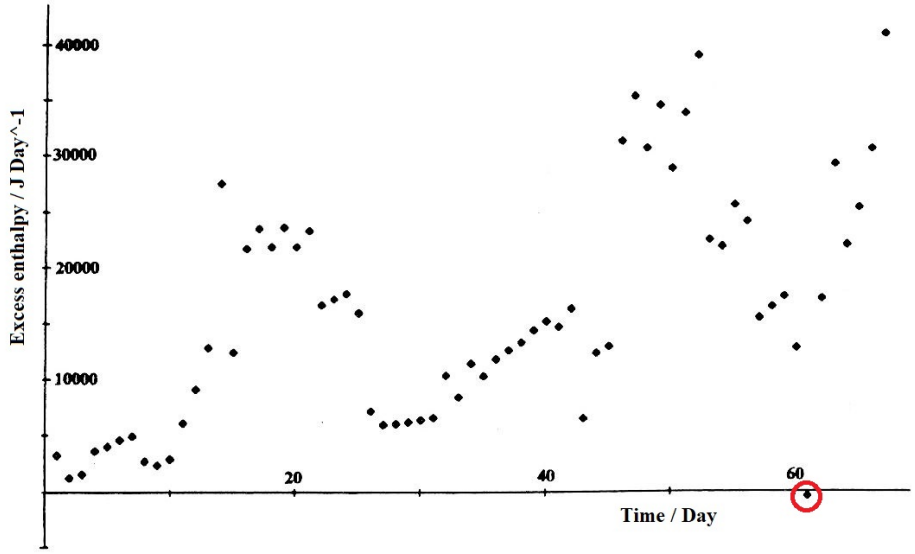

Figure 3. Excess enthalpies using ICARUS procedure that was recommended by Fleischmann. The data point at Day 60 (red circle) shows no excess heat. [27]

And here are the same results graphed with the NHE cell constant. The zero-line (no excess heat) is moved up, leaving about half the results below zero:

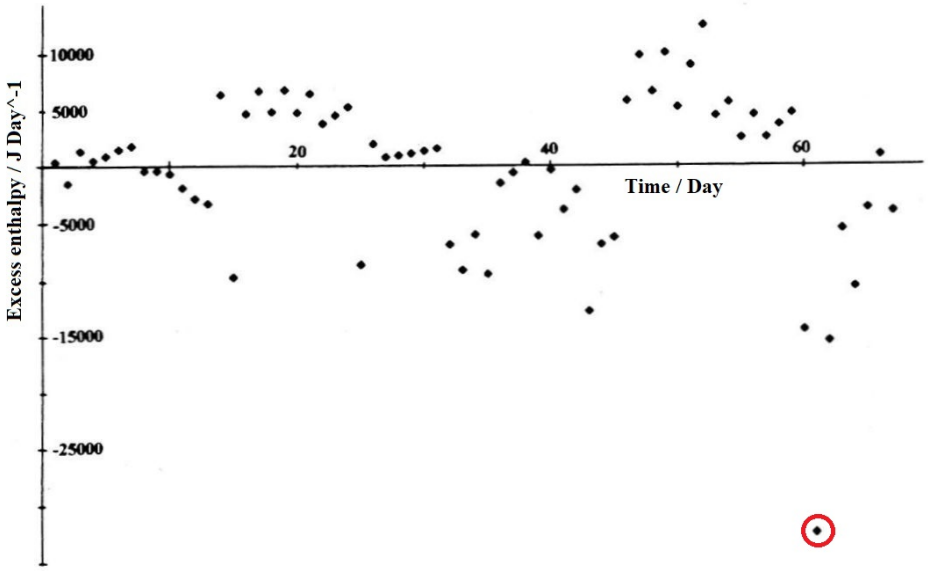

Figure 4. Excess enthalpies using NHE procedure. The data point at Day 60 (red circle) shows negative 30,000 J, which is impossible. The cell constant has shifted the starting point 30,000 J too high. [28]

This mistake, of setting the cell constant after excess heat begins, was also made by Lewis *et al.* at CalTech early in the history of cold fusion. They wrote what I consider an excellent paper

[27] From: Miles, M., M.A. Imam, and M. Fleischmann. *"Case Studies" of Two Experiments Carried Out With the ICARUS Systems*. in *8th International Conference on Cold Fusion*. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy. http://lenr-canr.org/acrobat/MilesMcasestudie.pdf
[28] ibid.

describing their experiments. [29] It has a lot of useful information about electrochemistry and the particular problems encountered in cold fusion experiments. Lewis *et al.* observed what many other researchers concluded was apparent excess heat. But, they reached the inexplicable conclusion that the cell constant had changed, and there was no heat. They do not give a reason why it might have changed. If the cell constant did change, that means the equipment failed for some reason, so they should have done the experiment over again. Fleischmann, Miles and others pointed out this problem, but they did not respond. I reviewed this history here:

Rothwell, J., *How Nature refused to re-examine the 1989 CalTech experiment* 2012: LENR-CANR.org. http://www.lenr-canr.org/acrobat/RothwellJhownaturer.pdf

As Mallove said about the CalTech experiment: "don't stand on the scale when you zero it out."

[29] Lewis, N.S., et al., *Searches for low-temperature nuclear fusion of deuterium in palladium*. Nature (London), 1989. **340**(6234): p. 525.

NOTES ON TEXT

Footnote Attributions

Footnotes and notes in square brackets include the initials of the person who wrote them. Initials are:

RC Robin Carter (a.k.a. “Ruby Carat”)
MF Martin Fleischmann
MM Melvin Miles
MCHM Michael C. H. McKubre
JR Jed Rothwell

Source Materials

Most of these letters were sent by fax machine on thermal paper. This fades over time, so some pages were lost. Fortunately, Miles copied most of the messages onto regular paper. The images were converted to machine readable text by Robin Carter and Jed Rothwell. The quality of the text was poor, so in most cases we were not able to use OCR. We had to either type the text from scratch or use voice input.

Spelling and Punctuation

Fleischmann usually used British spelling such as “programme” and “polarise.” In some of his letters to Americans, he used American spelling. We human editors tried to preserve his spelling in these letters, while Microsoft Word, the OCR programs, voice input and other editing tools relentlessly tried to change it to American spelling.

Fleischmann’s punctuation was often in old-fashioned British style, with a space before question marks and exclamation points. We preserved many examples of this, but not all, again because Microsoft Word and other modern editing tools do not work well with this format. Here is an example of an original fax showing: the poor quality of the text; a space before the question mark, and a space before the exclamation point:

> story). Other people are now also making their way towards the background so is it not time to reveal this ? After all, it is now 14 years since the first announcement so the D.O.E. et al have had plenty of time to tell me of their concerns - if they had any ! I conclude that they either believe or know that there is no such connection or else that they are simply ignorant about the relevant background in Physics.

Sample text from 2003-05-14

Fleischmann sometimes used old-fashioned words such as ‘phone with an apostrophe, indicating that this is an abbreviation for “telephone.” This may seem idiosyncratic but it is

merely old-fashioned. I have seen British and American books and magazines from the 1920s with this spelling.

Fleischmann often wrote in handwriting, adding comments in the margins, corrections, P.Ss, and P.P.Ss and in some cases P.P.P.P.P.Ss. In most cases we show handwritten portions as italic. We preserved a few letters in the original handwriting, such as 2001-08-05.

Bury Lodge heading

Letters beginning with the notation "Bury Lodge heading" had the following heading:

Bury Lodge, Duck Street, Tisbury, Salisbury, Wilts SP3 6LJ
Phone (+44) (0) 1747 870384 Fax (+44) (0) 1747 870845
From Professor Martin Fleischmann, F.R.S.

University of Southampton heading

Letters beginning with the notation "University of Southampton heading" had the following heading:

DEPARTMENT OF CHEMISTRY
THE UNIVERSITY

SOUTHAMPTON
S09 5NH

PROFESSOR MARTIN FLEISCHMANN, F.R.S.

Direct lines (0703) 593371

TEL. 0703 595000
TELEX 47661

FAX 0703 593781

NAWC heading

Letters beginning with the notation "NAWC heading" had the following heading:

Melvin H. Miles, Ph.D.
Chemistry and Materials Branch
Research and Technology Division
Code 4B2300D
Naval Air Warfare Center Weapons Division
China Lake, CA 93555-6100 USA
Phone: xxx
Fax: xxx
e-mail: xxx

(All of the telephone numbers, fax and e-mail addresses in these letters are defunct, so there is no point to including them.)

NAWC fax heading

Some messages from Miles to Fleischmann were written with fax transmission sheets. Here is an example:

TELECOPIER TRANSMISSION COVER SHEET

NAVAL AIR WARFARE CENTER
WEAPONS DIVISION
CHINA LAKE, CALIFORNIA

PAGES: 7 + COVER	DATE TIME: 12 May 1997
FROM	TO
AGENCY: NAWC Weapons Division	AGENCY:
NAME: Dr. Melvin H. Miles	NAME: Prof. Martin Fleischmann
CODE: 4B2300D	CODE: Bury Lodge, Duck Street
TELEPHONE NUMBERS — COMMERCIAL: (619) 939- 1652 — AUTOVON: 437-	TELEPHONE NUMBER: 44-1747-870-384
FACSIMILE EXTENSION: (619) 939-1617 (FAX)	FACSIMILE EXTENSION: 44-1747-870-845
☒ THIS TRANSMISSION CONTAINS NO CLASSIFIED INFORMATION.	VERIFYING EXTENSION:
☒ THIS TRANSMISSION CONTAINS NO RECORDS AS DEFINED IN THE PRIVACY ACT OF 1974.	

NAWC heading

Letters from Miles to Fleischmann beginning with the notation "NAWC heading" had the following heading:

DEPARTMENT OF THE NAVY
NAVAL AIR WARFARE CENTER WEAPONS DIVISION
1 ADMINISTRATION CIRCLE
CHINA LAKE, CA 93555-6001
521 9TH STREET
POINT MUGU, CA 93042-5001

IN REPLY REFER TO:

[kR′] Heat Transfer Coefficient Format

[kR′] often appears in these letters.

Fleischmann used the designation [k_R'] where R is the radiative heat transfer coefficient, with 2 or 3 subscripts: [k_R'] $_{i,j,l}$. The subscripts mean:

i Method of analysis. 1=Differential; 2=Backward integration; 3=Forward integration

j When present: Time period of measurement cycle. When there are only two subscripts this term is not included.

j=5, times somewhat above the origin
j=6, times somewhat above t_1 [application of calibration pulse]
j=7, times somewhat above t_2 [cessation of calibration pulse]
j=8, combination of times for j=6 and j=7

l Indicates 1=Lower bound; 2=True

Thus:

[k_R']$_{11}$ indicates: Differential, Lower bound.
[k_R']$_{262}$ indicates: Backward integration; Time period 6; True value.

A single bar over the k_R' term indicates this is an 11-point average value, where values are measured every 5 minutes (55 minutes):

$$\overline{k_R'}$$

The double bar, used in other documents, indicates a double average; that is, 6 of the 11-point averages combined (6 × 55 = 330 minutes total):

$$\overline{\overline{k_R'}}$$

Source: Miles, M., M. Fleischmann, and M.A. Imam, *Calorimetric Analysis of a Heavy Water Electrolysis Experiment Using a Pd-B Alloy Cathode* 2001, Washington: Naval Research Laboratory, pages 4-5. Averages described on page 12. http://lenr-canr.org/acrobat/MilesMcalorimetrd.pdf

Type A Palladium

Fleischmann often talked about what he called "Type A" palladium. He discussed it in some of these letters. Here are some notes about this, compiled by Jed Rothwell.

For many years Martin Fleischmann has been recommending a particular type of palladium made by Johnson Matthey for cold fusion experiments. He handed out several of these ideal cathodes to experienced researchers, and as far as he knows in every case the samples produced excess heat. Fleischmann and Pons designated this "Type A" palladium. It was developed decades ago for use in hydrogen diffusion tubes: filters that allow hydrogen to pass while holding back other gasses. This alloy was designed to have great structural integrity under high loading. It lasts for years, withstanding cracking and deformation that would quickly destroy other alloys, allowing other gasses to seep through the filters. This robustness happens to be the quality we need for cold fusion. The main reason cold fusion is difficult to reproduce is because when bulk palladium loads with deuterium, it cracks, bends, distorts and it will not load above a certain level, usually ~60%, I think. Below 85 to 90% loading bulk palladium never produces excess heat. A sample of palladium chosen at random from most suppliers will <u>never</u> reach this level of loading.

It seems likely to me that most of the reproducibility problems with bulk palladium cold fusion would have been solved years ago if people had only listened to Martin Fleischmann's advice.

Fleischmann described this material:

> . . . We note that whereas "blank experiments" are always entirely normal (e.g. See Figs 1-5) it is frequently impossible to find <u>any</u> measurement cycle for the Pd-D_2O system which shows such normal behaviour. Of course, in the absence of adequate "blank experiments" such abnormalities have been attributed to malfunctions of the calorimetry, e.g. see (10). [30] However, the correct functioning of "blank experiments" shows that the abnormalities must be due to fluctuating sources of excess enthalpy. The statements made in this paragraph are naturally subject to the restriction that a "satisfactory electrode material" be used i.e. a material intrinsically capable of producing excess enthalpy generation and which maintains its structural integrity throughout the experiment. Most of our own investigations have been carried out with a material which we have described as Johnson Matthey Material Type A. This material is prepared by melting under a blanket gas of cracked ammonia (or else its synthetic equivalent) the concentrations of five key classes of impurities being controlled. Electrodes are then produced by a succession of steps of square rolling, round rolling and, finally, drawing with appropriate annealing steps in the production cycle. [31]

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[30] Saito, T., et al. *Studies on Fleishmann-Pons Calorimetry with ICARUS 1*. in *5th International Conference on Cold Fusion*. 1995. Monte-Carlo, Monaco: IMRA Europe, Sophia Antipolis Cedex, France. http://lenr-canr.org/acrobat/PonsSproceeding.pdf#page=121

[31] Fleischmann, M. *Cold Fusion; Past, Present & Future*. in *The Seventh International Conference on Cold Fusion*. 1998. Vancouver, Canada: ENECO, Inc., Salt Lake City, UT. http://lenr-canr.org/acrobat/Fleischmancoldfusion.pdf

Fleischmann gave me some additional information in 2000, and I wrote the following. He and I discussed buying some of this material, but we could not afford it, and researchers expressed no interest in it, so we did not buy any. See letter 2000-01-07.[32]

The ammonia atmosphere leaves hydrogen in the palladium which controls recrystallization. Unfortunately, this material is very difficult to acquire and there is practically none left in the world, because Johnson Matthey stopped making it several years ago. Palladium for diffusion tubes is now made using a different process in which the palladium is melted under argon. Material made with the newer technique might also work satisfactorily in cold fusion experiments, but Fleischmann never had an opportunity to test it so he does not know. There should be plenty of the new material available, so perhaps someone should buy a sample and try it. Johnson Matthey has offered to make more of the older style Type A for use in cold fusion experiments. They will charge ~$20,000 per ingot, which is a reasonable price. Fortunately, the precise methodology for making the older material is well-documented and an expert who helped fabricate previous batches has offered to supervise production. So, if anyone out there has deep pockets and wants a batch of the ideal material to perform bulk palladium cold fusion experiments, we can arrange it. I do not know any cold fusion research scientists or institutions who can afford $20,000 worth of material, but perhaps several people could get together and pool their resources.

The above description of Type A is not comprehensive. We know little about the material. We cannot explain why it resists distortion and allows high loading. The experts in Johnson Matthey probably know, but they are not talking. When Ed Storms read this description, he immediately thought of a number of important questions about fabrication techniques: "What is the crucible made of in which it is melted? Pick-up of crucible material can not be avoided. How is oxygen removed? Is calcium boride used, which is the usual method? What is the boron content?" Unfortunately, such details are trade secrets which Johnson Matthey will not reveal. Fleischmann does not know the answers. Anyone who has a sample can quickly find out what elements are present in the alloy, in what proportions. But questions such as "How is the oxygen removed?" may not be as easy to ascertain. The trade secrets are not what is in the metal, but how it got there and why it stays.

I asked Fleischmann how confident he is that this material is effective, and how much batch-to-batch variability he observed. He said that since 1980 he has used samples from eight or nine batches. Only one batch failed to work, and was returned for credit.

In general, any material from Johnson Matthey works better than palladium from other sources. The most dramatic proof of this can be seen in M. Miles, "Anomalous Effects in Deuterated Systems," especially Table 10, summarizing the effectiveness of palladium from various different sources. [33] The success ratio with Johnson Matthey material was 17 out of 28 (17/28) compared to 2/5, 0/19, and 2/35 with other sources. Only the alloys fabricated in-house

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[32] See also Rothwell, J., *Lessons from cold fusion archives and from history.* J. Condensed Matter Nucl. Sci., 2015. **15**. http://lenr-canr.org/acrobat/RothwellJlessonsfro.pdf

[33] Miles, M. and K.B. Johnson, *Anomalous Effects in Deuterated Systems, Final Report*. 1996, Naval Air Warfare Center Weapons Division, Table 10. http://lenr-canr.org/acrobat/MilesManomalousea.pdf

by the NRL worked better, with a 7/8 success ratio. Miles tested two samples of Type A palladium supplied to him by Fleischmann and Pons. Both produced excess heat at much higher power density than samples from other suppliers (3 - 14 W/cm^3 compared to 0.3 - 2.1 W/cm^3). Fleischmann reported success with pure palladium, as well as silver and cerium alloys. So did Miles, and he also had good results with boron alloys. The NRL in Washington reported no heat with samples from the same batches Miles tested, but their calorimeter was an order of magnitude less sensitive than his (with 200 mW precision compared to 20 mW), so even if their samples had produced the same level of heat Miles observed, they could not have detected it.

In their Final Report, the NHE claimed that they used "the type of palladium recommended by Fleischmann and Pons" in a series of experiments in the final stage of the project, after all else had failed. This is incorrect. They did not have any of the Type A palladium. Perhaps they used some other Johnson Matthey material instead. They have refused to reveal the batch number or say when or where they acquired the material, but as far as Fleischmann knows, there was no Type A material available at that time. When the NHE program began, Fleischmann supplied them with three Type A cathodes. Two of them produced excess heat, and one failed because of a prosaic problem with the equipment. The NHE disagrees with Fleischmann's conclusion. Based on their method of evaluating calorimetric data, they say all three samples failed to produce heat. They refuse to release detailed data which would allow others to analyze the results. Fleischmann, McKubre and Miles have criticized their methodology, in which a single calibration pulse made a few days after the experiment begins, when low-level excess heat is probably already present. (See the Fleischmann quote above, and M. Miles, "Report on Calorimetric Studies at the NHE Laboratory in Sapporo, Japan.")

The question is: At this late date does anyone care about bulk palladium electrochemical cold fusion? Does anyone still want to try it? Even with the proper materials, this is still a very difficult experiment. Fleischmann and McKubre agree that if techniques can be used, they should be. McKubre said, "the world is fascinated by electrochemistry, except electrochemists. If they can find another way of doing the job they will always choose the other way." Fleischmann believes that the qualities of the palladium material are not be as important with electrodiffusion, which pushes deuterons through the bulk of material rather in through the surface. "Solid-state works better than interface chemistry." (Other people may not find the Italian electrodiffusion results as convincing as he does.) McKubre has successfully replicated the Case experiments using gas loading into commercial catalysts made of palladium on carbon. Researchers may feel that this kind of technique is more promising than bulk palladium, and there is no point to revisiting obsolete, 10-year-old experiments. We may no longer need Type A palladium. We can hardly afford it, anyway.

I once asked Fleischmann how he learned about Type A palladium. He said: "It is very simple. When we began this work I went to Johnson Matthey, I told them what I needed, and they recommended this material." As I said, he has a baroque imagination and he often goes about doing things in indirect, complex ways, but in this case, he used the direct approach.

LETTERS

Letters are listed in chronological order, labeled with the date in year-month-day format.

1989-05-01

[Melvin Miles was one of the first to attempt to replicate cold fusion in 1989. Here is a presentation he gave in May 1989.]

UNCLASSIFIED

May 1, 1989

82-42

COMMANDER'S MEETING SPECIAL TOPIC PRESENTATION

RESEARCH DEPARTMENT

"COLD" FUSION TECHNOLOGY STATUS

by

DR. MEL MILES
Code 3853

1 MAY 1989

001/J0122

UNCLASSIFIED

HEADLINES ON NUCLEAR FUSION MARCH, 1989

MARCH 24 — Scientists claim successful nuclear fusion

Pair Proclaim Nuclear Fusion Breakthrough

Scientists in Utah Say Simple Table-Top Device Produces More Energy Than It Uses in Tests

MARCH 25 — *Excitement and Skepticism*

Fusion Claim Sparks Rush to Duplicate Experiment

Scientists skeptical about duo's nuclear fusion claim

2-F3 N.

APRIL 28, 1989 TLF

FUSION HEADLINES
APRIL, 1989

APRIL 6,7 Clearing Fusion Off the Table Top

Independent fusion test discouraging

Scientists at MIT skeptical of results

APRIL 11 *But Fusion Mystery Only Deepens*

Texans Report 80% Energy Gain in Test

APRIL 18 **Helium Find Gives Lift to Fusion Chemists**

Discovery Backs Controversial Claim of Nuclear Reaction in Simple Experiment

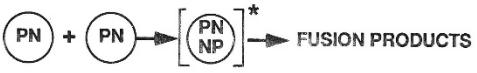
APRIL 19 **Stanford Lab Adds Fuel to Scientific Fire Over Fusion**

APRIL 21 **Scientists Grow Frustrated Over Fusion Claim**

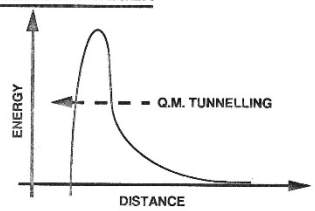
042 - F3 N.

APRIL 26, 1989 TLF

"NORMAL" NUCLEAR FUSION REACTIONS EXPECTED OF DEUTERIUM ATOMS

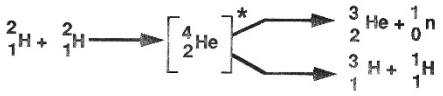


COULOMBIC ENERGY BARRIER



$D + D \longrightarrow {}^{3}He + n + 3.2\ MeV$

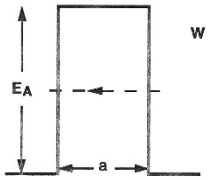
$D + D \longrightarrow T + p + 4.0\ MeV$



2 - F3 N.

APRIL 26, 1989 TLF

NUCLEAR FUSION OF DEUTERIUM
QUANTUM MECHANICAL TUNNEL EFFECT



$$W(E_A, a) = e^{-\frac{4\pi a}{h}\sqrt{2m(E_A - E_O)}}$$

WHERE E_A = 8 MEV (7.7×10^{11} J/MOLE)

$m = 3.32 \times 10^{-27}$ Kg

$h = 6.626 \times 10^{-34}$ J·s

FOR $a = 10^{-10}$ m (1 Å), $W(E_A, a) = 10^{-76,000}$

FOR $a = 10^{-15}$ m (FERMI), $W(E_A, a) = 10^{-0.76} = 0.17$

BOLTZMANN'S FUNCTION (CROSSING OVER BARRIER)

$$W = e^{-E_A/RT}$$

FOR $T = 300K$, $W = 10^{-1.34 \times 10^8}$

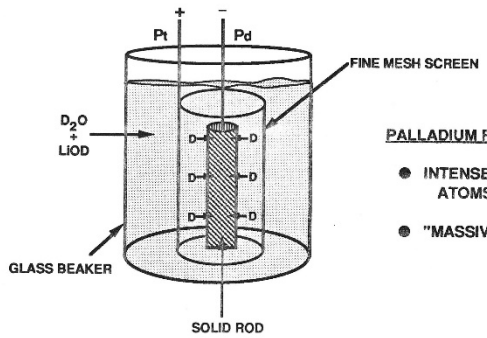
FOR $T = 100 \times 10^6$ K, $W = 10^{-4.0} = 0.0001$

042 F3 N.001

APRIL 29, 1989 TLJ

ELECTROCHEMICAL CELL DESIGN FOR COLD DEUTERIUM FUSION

CELL



PALLADIUM ROD

- INTENSE PRESSURE FORCES DEUTERIUM ATOMS TOWARDS CENTER OF ROD.
- "MASSIVE" ELECTRODE IS KEY!

PALLADIUM ELECTRODE REACTIONS

$D_2O + e^- \longrightarrow Dabs + OD^-$

$D_2O + e^- \longrightarrow \frac{1}{2}D_2 + OD^-$

PLATINUM ELECTRODE REACTIONS

$4\,OD^- \longrightarrow 2\,D_2O + O_2 + 4e^-$

$D_2 + 2\,OD^- \longrightarrow 2\,D_2O + 2e^-$

C0042 F3 N.004

APRIL 29, 1989 TLJ

COLD ELECTROCHEMICAL FUSION OF DEUTERIUM
EFFECTIVE PRESSURE

ELECTROCHEMICAL REACTIONS AT PALLADIUM SURFACE

$$D_2O + e^- \longrightarrow Dabs + OD^-$$

$$D_2O + e^- \longrightarrow \frac{1}{2} D_2 + OD^-$$

$$\Delta G = \Delta G^o + RT \ln (P_{D_2})^{\frac{1}{2}} . a_{OD^-}$$

$$\Delta G = - nFE$$

$$E = E^o - \frac{RT}{nF} \ln (P_{D_2})^{\frac{1}{2}} . a_{OD^-}$$

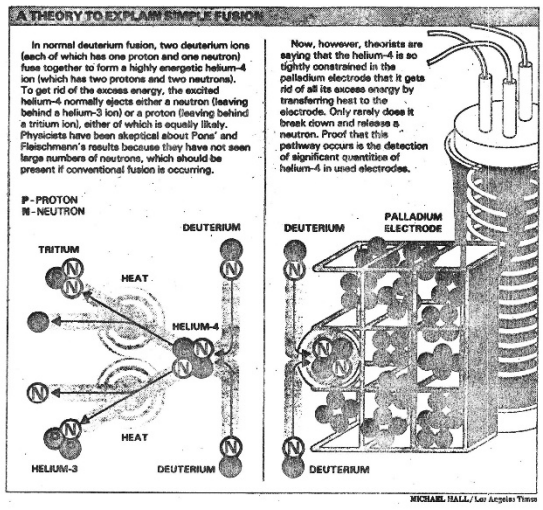
FOR $\eta = E - E^o = - 0.80V$, $P_{D_2} = 10^{26}$ atm

C0042 F3 N.002

APRIL 28, 1989 TLJ

7

DEUTERIUM FUSION THEORY



MICHAEL HALL/Los Angeles Times

C0042 - F3 N.

APRIL 28, 1989 TLF

THE PALLADIUM/HYDROGEN ELECTRODE

α - PHASE

$$Pd + x\,H_2 \longrightarrow Pd\,H_{0.003}$$

- NO CHANGE IN LATTICE PARAMETER
- HYDROGEN ABSORBED INTERSTITIALLY

β - PHASE

$$Pd + x\,H_2 \longrightarrow Pd\,H_{0.6}$$

- 3% INCREASE IN LATTICE PARAMETER
- ALLOY BEHAVIOR
- SEMICONDUCTOR

C0042 F3 N.003

APRIL 28, 1989 TLJ

FUTURE RESEARCH OTHER METALS AND ALLOYS

- METALS THAT ABSORB HYDROGEN
  - PALLADIUM, PLATINUM, IRIDIUM, RHENIUM
  - TITANIUM, TANTALUM, ZIRCONIUM, HAFNIUM
- ALLOYS THAT ABSORB HYDROGEN
  - Pd_4Ag
  - TiFe
  - $LaNi_5$
  - TiNi, Ti_2Ni
  - TiCo

C0042 - F3 N.

APRIL 28, 1989 TLF

ELECTROCHEMICAL DEUTERIUM FUSION
POSSIBLE REQUIREMENTS

- LARGE PALLADIUM ELECTRODE
- PROPER SHAPE OF PALLADIUM ELECTRODE
- PROPER PRE - TREATMENT OF PALLADIUM ELECTRODE
- HEAVY WATER (D_2O) CONTAINING LITHIUM IONS
- PROPER CONFIGURATION OF COUNTER ELECTRODE
- TIME PERIOD FOR ACTIVATION OF PALLADIUM ELECTRODE

"COLD FUSION"
CURRENT BOTTOM LINE

- SCIENTIFIC COMMUNITY STILL HIGHLY SKEPTICAL
- DEVELOPER TO PUBLISH MORE COMPLETE WORK DESCRIBING EXPERIMENTAL PROCEDURES
- DEVELOPERS COLLABORATING WITH DOE SCIENTISTS AT LANL TO DEMONSTRATE "COLD" FUSION

1992-01-21

Miles faxed Robert Nowak as follows:

12 December 1991

FROM:

Naval Weapons Center
Dr. Melvin H. Miles

TO:

Office of Naval Research
Dr. Robert J. Nowak
Chemistry Division

Bob,

Mike Melich called me today and mentioned that you will be seeing Martin Fleischmann soon. Could you ask his opinion on the following factors for getting excess heat:

1. *Heat treatment of Pd Prior to the experiment.*
2. *Anodic treatment of Pd prior to deuterium-loading.*
3. *Current density to use for deuterium-loading.*
4. *Effect, if any, of atmospheric CO_2.*
5. *Effect of D_2O impurities such as Cu, H_2O.*
6. *Effect of impurities in lithium metal such as Hg, Sn, Zn, and Pb.*
7. *Effect of sudden voltage on current changes.*
8. *Effect of bubble pattern at the cathode – standing on random.*
9. *Effect of impurities/additives in Pd such as Ce, Li or Ag.*
10. *Effect of ^6Li vs. ^7Li.*

This may help us to reproduce the excess heat effect more readily.

Thanks,

Mel Miles

Responses from Fleischmann are in the next letter:

University of Southampton heading

Dr. Melvin H. Miles,
Chemistry Division,
Research Department,
Naval Weapons Center,
China Lake,
CA 93555
U.S.A.
MF/ KJW

Confidential

21 January 1992

Dear Melvin,

On my trip through the United States, I was handed a copy of your letter to John Maddox, together with your covering letter. I was very interested to see the points which you have made and you in turn may be interested to know that during the Summer, Stan and I had to deal with one of the official actions of the Patent Examiners. This action was on our patent for a neutron generator and we were very unhappy to do this chore as we had originally made a strong case against taking that particular patent. However, our objections were to no avail! We thought it least likely that we would be able to push the work in the direction required and thus it turned out to be.

However, our need to deal with this matter made us go through the papers cited by the Patent Examiner which included Nate Lewis' contribution. All I can tell you at this time is that Stan and I have many more objections to those papers than you have!

I am also just now going to write to David Williams about various matters in the Harwell paper which need clarification. My correspondence with the various people who said they saw zero effects is due to the wish of various influential people to have all this material re-examined. As you will know, Stan and I only agreed to continue working on the subject providing our own data were independently evaluated and you will also know that this was eventually done by Wilford Hansen because the people who were supposed to do it did not put in the effort required.

The outcome of my correspondence has been that all the groups which I have approached have agreed to let their data be examined and John Maddox has also said that he would welcome such a move! Stan says that it is difficult to make out from my letters whether I am a friend or enemy but in truth, I am quite neutral in my approach. The only person who has so far equivocated is Nate Lewis but I dare say that he will have to follow suit. You will also be interested to know that a number of groups who have not so far published extensively have said that they agree to have their data reviewed. The outcome thus far is much better than I had ever though was possible.

Our thoughts about Cold fusion have naturally been totally over-shadowed by the accident at SRI, all the more distressing for me because I had known Andy Riley for many years now and because I had just visited SRI before Christmas. I dare say that there will be a back-wash from this accident and I let me tell you that I will be very glad to answer any questions which you may wish to pose to me.

When I was in Washington, Bob [Nowak] also handed me a copy of your Fax so let me now deal briefly with the questions which you posed to Bob – I am sure that we will have to cover some of these in greater detail in due course.

[JR The ten questions from Miles asked Nowak to find out about have been added to the text below in blue, followed by Fleischmann's responses.]

1. Heat treatment of Pd Prior to the experiment.

   We do not heat treat the palladium prior to the experiment.
2. Anodic treatment of Pd prior to deuterium-loading.

   We do not polarise the electrodes anodically prior to making calorimetric measurements. However, we have now carried out extensive investigations on the factors which control the deuterium loading and this includes anodic anodic treatment. I wish I could say that we understand the complexities of this topic but in truth we do not.
3. Current density to use for deuterium-loading.

   We always load at low to intermediate current densities, that is, less than 100 mA cm^{-2} before we raise the current density. We believe this to be critically important point and to the best of our knowledge, there are only three groups of people in the world who understand the reason for doing so which are Stan and I, EPRI/SRI and the National Fusion Centre in Nagoya. [34]
   The reason why we have talked about this matter the least is that our protocol is based on a surmise and we do not want to polarise people's thoughts. Equally, it affects the whole of our research strategy.
4. Effect, if any, of atmospheric CO_2.

   I do not know what the effect of atmospheric CO_2 might be. At the very least, it will complicate the calorimetry if CO_2 gets reduced at the cathode.
5. Effect of D_2O impurities such as Cu, H_2O.

   The impurities in the D_2O are probably of key importance and this goes for metals, as well as borates and silicates. I wished I could tell you that we had this sewn up but we have not. However, we do believe that the blocking of the surface either by UPD layers or by insoluble precipitates is very important. [35] As you will see, I am sending a copy of this letter to Bob so I would just expand my comment here by telling you that it is Stan's

[34] MCHM Several reasons occur. I am not sure to which Martin refers here.
[35] MCHM I did not know this opinion of Martin's and needed to rediscover it ourselves. Note later work by IMRA-J and Coolescence, using metallic coatings (Cu & Au).

and my view that as far as the bulk of the palladium is concerned, it is advantageous to produce a set of microelectrodes by suitable blocking of the surface.

6. Effect of impurities in lithium metal such as Hg, Sn, Zn, and Pb.

   The third impurity on your list [Zn] [36] gets in to the D_2O anyway and we believe that this is one of the species which is implicated!

7. Effect of sudden voltage on current changes.

   Sudden changes in the electrochemistry are very important. [37] You will want to know that our strategy in Utah was oriented at developing a reproducible set of electrodes and cells on which we were then going to investigate systematically the effects of perturbations in temperature and perturbations in the electrochemistry. Unfortunately, our work there was terminated because the people at NCFI did not buy any more D_2O. This is one topic which we have to start anew.

8. Effect of bubble pattern at the cathode – standing on random.

   There is clearly a wide variation in the formation of bubbles at the cathode. If bubble evolution is irreproducible then this leads to noise in the electrode potential: the noise levels for different cells is widely variable. [38] You will be interested to know that this affects our data evaluation because the errors in fitting the black box model to the data can become totally dominated by the fluctuations in cell potential, rather than the time dependent change of temperature. We believe that Kalman filtering is therefore a better technique to use than the non-linear regression technique which we have so far used extensively.

9. Effect of impurities/additives in Pd such as Ce, Li or Ag.

   I am sure you will know that our favoured strategy now is to use alloys (which you have listed! - no prizes for guessing).

10. Effect of ^{6}Li vs. ^{7}Li.

    We do not know this as yet.

I would like to make some more general comments with regard to electrode materials. You will know that in our original measurements with palladium alone, we got good reproducibility with 1, 2, and 4 mm diameter rods but never had any excess heat with our original 8 mm rods. We believe that this showed Johnson Matthey that cold working was very important and in fact, we also believe that large diameter electrodes "do not work" because they crack. We have subsequently managed to get excess heat with specially prepared 8 mm rods (I presume that Johnson Matthey changed their procedures so that the rods were prepared in an analogous way to those having smaller diameters but our measurements with these rods proved to be irreproducible).

---

[36] MCHM We did not focus much on Zn but it may have similar character to Pb and Hg.
[37] MCHM This is hugely significant and discovered by us only much later.
[38] MCHM Noise and stimulation (see last Comment).

It is also true that a subsequent batch of 1, 2, and 4 mm diameter rods gave us irreproducibility and low levels of excess heat but we managed to recover more or less our original position with yet further batches of electrode materials.

Our promising results with alloys have been based on single batches of material and in view of our experience with the palladium electrodes, we now feel that we have to carry out a major investigation with repeated batches of alloy electrodes. Bob will tell you that Stan and I are in favour of making these materials widely available if and when we find that they give us reproducible results. *(but the decision is not up to us.)* [39]

There is another important point which I must raise with you and that is most of our results are based on the first charging of the electrode. [40] You may recall that at the Como Conference, I said that people should give this information in future research. As of now, we do not know whether the first charge or any subsequent charge of the electrode will be optimal. This information is obviously critically important but it will take several years to decide this point. I only hope that someone somewhere will provide the resources so that these long term experiments can be initiated and maintained.

Let me also give you some advance notice of extremely interesting new experiments which will be reported this year by other research groups on the generation of excess heat in the gas phase loading of palladium. As far as I can tell, some of the results are really spectacular and, of course, that system will lead to some pretty unambiguous measurements – we have it in mind to use infrared imaging.

I hope that we can continue to keep in touch about this topic.

Best regards for this coming year.

Yours sincerely,

Martin

c.c. Dr. Robert J. Nowak.

[39] MCHM Was this up to Johnson-Matthey? JR Yes, Fleischmann told me Johnson-Matthey controlled the disposition of the palladium materials.
[40] MCHM We almost never saw excess power on "first charging of the electrode". I never discussed this with Martin but clearly they were doing something different to overcome the initiation barrier.

1992-03-09

[JR This letter was entirely handwritten.]

Bury Lodge,
Duck Street,
Tisbury,
Wilts SP3 6LJ

Dear Roger [41]

Re: "Some Comments on the Paper "Analysis of Experiments on Calorimetry of Electrochemical Cells" by R. H. Wilson et al (1)"

M. Fleischmann and S. Pons

(Will be published in August back to back with the GE paper. Please do not quote publicly before publication date + clear any reference prior to that with us as "personal communication".)

I have discussed the text of our paper again with Stan and we feel that there is really very little we can do to soften the tone of our paper. Part of our difficulty is that we still do not have the text of Ron Wilson et al's paper as accepted for publication; the tone of our reply was a response to the version of the G.E. Paper as originally submitted. That version was certainly full of innuendo and very aggressive – our paper is much less aggressive than was theirs.

We agree with the referee that the last sentence on page 15 should be deleted and we have done so; please note also some small changes on page 20.

However, the request by the referee that we should delete the sentence on page 9: "We observe that the results of the independent investigation using Kalman filtering (7) were presented to the group during 1991. Their omission of reference to this work shows that they also reject this method of data processing in addition to the Method 2" should not be followed, we believe. Let me summarise the situation once again:

Wilson et al contend that we did not take into account the changes in the cell potential or evaporative cooling in determining the excess enthalpy. The first is in correct for the results we give in (2). both are incorrect for the results in (7) (inclusion of evaporative cooling would have raised the excess enthalpy for the results in (2)). All this was presented to G.E. in 1991 and was presented at the Como meeting (where Fritz Will was present) and is now published.

We have in fact given Wilson et al a "let-out" in our statement after the semi-colon but the question is whether they will take it? However, what is one to say about a research group which deliberately ignores information presented to the members? They really owe the public some sort of response.

As you know, there is much more to this story, some of which I will tell you "by word of mouth".

---

[41] MCHM Who is Roger?

Yours truculently,

Martin

Some Comments on the Paper "Analysis of Experiments on Calorimetry of $LiOD/D_2O$ Electrochemical Cells" by R.H. Wilson et al ([1])

9 MARCH 1992

M. Fleischmann and S. Pons**

* Department of Chemistry
University of Southampton
Highfield
Southampton, S09 5NH UK

** Department of Chemistry
University of Utah
Salt Lake City, UT 84112 USA

Abstract

We comment here on the title paper and find that it is a series of misconceptions and misrepresentations of previous reports.([2-6]) It is shown that the conclusions reached by the authors lead to gross errors in the prediction of the observed responses of the electrochemical calorimeters described in the original work and that the correct methods of analyses are indeed those we originally described as well as those which have been outlined in subsequent publications. We find that the authors have not validated their own methods and have not provided sufficient information to allow assessment of their work.

1992-03-23

University of Southampton heading

Dr. Melvin H. Miles,
Chemistry Division,
Research Department,
Naval Weapons Center,
China Lake,
CA 93555
U.S.A.
MF/ KJW

Dear Mel,

I am writing about several matters, first of all to check up whether you got my reply to your memo to Bob Nowak? I had to go to France at the beginning of this year, intending to come back here by the middle of February. However, in the event we had so many urgent things to see to that I only came back last week so my correspondence is in its usual state of disarray. Before I went to France, I did in fact write to you with a copy to Bob Nowak so could you let me know whether you received that particular message – I fear it will not have been very helpful because the project we are all working on is characterised more by our lack of knowledge than by anything definite we can say to specify the performance. However, I hope that I did least at least disclose to you our level of ignorance!

Thank you for enclosing your letter to the Editor of Science and the second reason I am writing to you is to ask whether you had any response from him and/or whether they might publish this letter. I find it really remarkable that people like Gary Taubes can have access to such Journals with uncorroborated statement (other might say "lies") while reasoned arguments do not prevail.

The third reason why I am writing to you and this is really a question of urgency is to ask for your opinion on the work carried out in Harwell. In particular, I would be very interested in your comments on the calibration of what they called the FPH calorimeters. For that matter, I would also be very interested in your comments on their improved calorimeter, as well as their isothermal calorimetry. The reason why I would like to have this as a matter of urgency is because I am once again surveying some of the papers which come to the conclusion that there was no excess enthalpy generation in the Pd/D_2O system but as of now, I do not want to tell you my own thoughts because I would like to have your totally independent judgement on these matters.

I hope that you can help me with these questions and do please let me know if you want any further information from me.

Yours sincerely,

Martin

[JR This letter was in response to a letter to the editor at *Science* by Miles, as follows:]

Cold Fusion: China Lake Results

Gary Taubcs, in his article about Martin Fleischmann's cold fusion seminar at the California Institute of Technology (News & Comment, 13 Dec, p. 1582), states that "researchers working with the China. Lake group have said that those observations . . . could be explained by helium-4 contamination from the ambient atmosphere." We are basically a two-man group with respect to cold fusion research at China Lake and neither of us has made such a statement. To my knowledge, neither has anyone else at China Lake made any such statement.

Regarding our report of time correlated measurements of excess heat and helium[42], the simple yes-or-no detection of helium-4 in eight of eight experiments producing excess heat in the absence of helium-4 in six of six control experiments not producing excess heat (one in D_2O, five in H_2O) implies a chance probability of only $(1/2)^{14}$ = 1/16,384 or 0.0061%. Those attributing our results to atmospheric contamination should try to flip a coin until they obtain a predetermined sequence involving 14 tosses. Furthermore, the experiments at China Lake producing the greater amounts of excess enthalpy yielded the larger amounts of helium-4. Our control experiments show that atmospheric contamination is a highly unlikely explanation for our results.

Melvin H. Miles
Weapons Division,
Naval air warfare Center,
China Lake, CA 93555

[42] MM B. F. Bush *et al.*, *J. Electroanal. Chem.*, **304**, 271 (1991).

1992-04-10

TELEFAX TRANSMITTAL SHEET

From: STANLEY PONS
DEPARTMENT OF CHEMISTRY
UNIVERSITY OF UTAH
SALT LAKE CITY UT 84109 USA

TO: Dr. Mel Miles
Naval Air Weapons Center
Weapons Division
China Lake, CA

DIRECT ANY REPLY TO FAX:
801 466-8175 (Utah Office) ___
(33) 93 95 82 25 (France) X

TELEPHONE:
801 486-9227 (Utah) ___
(33) 92 96 03 01 (France) X

DATE: 10 April 1992 TIME: 5:20pm

TOTAL 18 PAGES INCLUDING THIS PAGE

MESSAGE:

Dear Mel:

Martin sent me the attached letter today for me to read and approve. Martin will be busy tonight and this weekend, so I have decided to send it to you immediately since I have no changes to make. Thanks for considering this letter, and we look forward to hearing from you. Martin is in England and it will be best to reply to him there. Best regards.

University of Southampton heading

2 April 1992

To:
Dr, Melvin Miles,
Chemistry Division,
Naval Weapons Center,
China Lake,
Code 3853
CA 9355
U.S.A.
Dear Mel,

Thank you for your Fax of 30th March and the attached copy of your letter to Science. On comparing this with the text of the letter you sent to the Journal on 17th January, I see that all mention of the work at Caltech has been removed. I take it that you haven't changed your mind about any of the content of the original letter to Nature (and Science?) of 5th December 1991? Presumable the Editor will have told you that your reply to Gary Taubes had to be confined to points which bear directly on your own work? Naturally, in this way all the other issues have been neatly "swept under the carpet" but, of course, Gary Taubes will continue to be allowed (or there by encouraged?) to fabricate facts just as he (and Science?) pleases.

When I wrote to you on 23rd March, I asked for your unbiased opinion about the paper from the group at Harwell and, especially, about their measurements with what they called the FPH calorimeters. First of all, thank you for your initial reply about their extraordinary Figs 2c and d. Secondly, I want to explain to you our caution in asking for opinions. Later on in this letter I will outline to you Stan and my attitude to the review of published work in general and the work on "Cold Fusion" in particular (ours included). I will summarise these views here: we are strongly in favour of any review which advances the science (which includes more advanced analyses of data sets which for the bases of published work). However, we are opposed to reviews which will focus on the "political issues" - that will get us nowhere. Naturally, we do not exclude the consideration of these issues but they must be subsidiary to the science (and seen to be so!).

Stan and I have had to make careful appraisals of several of the papers on this topic partly in view of the natural progression of the research but also because of the patent issues which continue to haunt us. I believe that it would be fair to say that this process has led us to a series of question which we are unable to answer in any quantitative fashion. At best we can say: "that was probably alright" or "that couldn't have worked". Our questions have dealt in the main with the calorimetry which we continue to believe is the key signature [43] (together with the associated "nuclear ash" - hopefully only ^{4}He!) and therefore also the key problem area but also with tritium generation, neutron counting, γ-rays etc. In our correspondence with you (and you see that we

[43] MCHM Contrary to David Williams' {DW}assertion that the gamma line compelled Martin most.

wish to develop this) we want to confine attention first of all to the calorimetry but we also want to ask you whether you would be willing to consider/discuss the other signatures?

As you will see we are in favour of reviewing the published material and we do have a set of questions which we believe should be considered. At the same time Stan and I also believe that we should not be directly involved in the review of other people's work. If any such review came to the conclusion that the "negative" outcome of any of these investigations was not justified by the results (or, in the most extreme case, if the conclusion was that the authors actually observed excess enthalpy generation but concluded that the results were negative) then our involvement with that review would surely devalue it as the conclusions would be regarded as special pleading.

I hope that this explains our wish to have your unbiased opinions? However, on further reflection it seems to use that our caution may be somewhat excessive. Perhaps there is no foreseeable difficulty if we simply give you a cross-section of our question? You will probably already have considered most of these and possibly come to the conclusion that the questions cannot be answered or could only be answered if one had access to the raw data. However, it is possible that you have answers and, furthermore, that you have questions (and answers?) which we have not considered. If this is so, then we could appreciate it if you could send these to us.

If you can see that your consideration of our list of questions could pose difficulties for any review which you may be undertaking, then please throw this letter away! Anyway here goes:

As I have already said we want to consider principally some aspects of the calorimetry in the paper from the group at Harwell [(1)] but this also requires us to start with the papers from the group at Caltech [(2,3)]. However, before I consider these papers I want to refer to the approach we used in our work up to October 1989 [(4)]. We have changed our approach considerably since then but we believe that [(4)] will serve to discuss the results in [(1-3)].

I will later also refer to the work from MIT [(5)] in the context of some further comments on our attitude to the review of the published work. We believe that the three sets of papers [(1), (2,3), (5)] were very influential in creating a negative climate of opinion – not because of their excellence but because of the reputations of Caltech, Harwell and MIT! However, in due course we may also wish to consider a number of other papers such as that of Kreysa et al [(6)] which were also influential in creating this negative climate.

I will therefore refer first of all to our own published work [(4)] and I will restrict myself to the temperature-time plots (θ-t plots) and the calibration of the Dewar-type cells. Let me remind you that we did this by applying a constant current to a resistor contained in a separate glass sleeve: the θ-t transients were followed for ~ 6 thermal relaxation times. An illustration of one-calibration set was given in Fig. 5A of [(4)] here reproduced as Fig. 3B. The resultant heat transfer coefficients showed considerable scatter such as that for 33 data sets given in Fig. 3B (this figure was eliminated from our paper[(4)]. However the apparent "errors" were systematic rather than random, Fig. 3C (this was Fig. 5C in [(4)], and superposition at the mid-point calibration gave a residual standard deviation $\sigma_R = 0.155\%$, Fig. 3D (this was Fig. 5D in [(4)]).

The first important set of questions is:

Pons-Fleischmann: Question 1 (I will list the questions according to the principal authors of the respective papers). Could the details of the method of calibration be clearly understood?

Pons-Fleischmann: Question 2 Was it clear that the errors in the heat transfer coefficients were systematic rather than random and that the excess enthalpy and heat transfer coefficients had to be determined simultaneously?

There is a subsidiary question: were the magnitudes of the errors clearly stated?

There is a further question which is almost rhetorical but which is important for the consideration of the Caltech and Harwell papers:

Pons-Fleischmann: Question 3 Was it clear that there is no steady state for the θ-t and Ecell -t profiles and that the heat transfer coefficients decrease ~ linearly with time after the cells have been replenished with D_2O (or H_2O)?

In our first full paper on the calorimetry [4] we used two methods for evaluating the excess enthalpy, that described by "Approximate specific Q_{excess}/W cm^{-3}" and that by "Specific Q_{excess} from regression analysis/W cm^{-3}" e.g. in Table 3 of the paper. The basis of the first method was described in Appendix 4 (except that this illustration used a linearisation of equation (A3.9) to give (A4.2); the evaluation of Q_{excess} used (A3.9); the basis of the second method was described in Appendix 5. We have recently had to reconsider [4] because we have had to reply to a critique of our paper. We will probably wish to ask you a series of questions in due course but at present we would like you to consider the following:

Pons-Fleischmann: Question 4 Were the bases of these two methods of calculation made adequately clear in the paper [4]?

Pons-Fleischmann: Question 5 Was it clear that we considered it essential that both the heat transfer coefficients of Q_{excess} should be evaluated simultaneously for both these methods of calculation?

We would appreciate it if you could send us your replies to these questions and we hope that you will be brutally frank! We shall certainly have to write a further paper on the methods of data evaluation especially in view of the critique and also because we are continuing to develop these methods. If particular points were not clear then we should pay special attention to these aspects.

However, as far as the consideration of the Caltech and Harwell papers is concerned, it is really the need to carry out a simultaneous evaluation of the heat transfer coefficients and of Q_{excess} which is of key importance.

This brings me next to the question about the papers from Caltech [2,3]. I will pose our first two questions while realising that you have answered these question in your correspondence.

Lewis: Question 1 Is it not true that the method of calibration used by the group at Caltech is at best ambiguous and, more likely, completely wrong?

Lewis: Question 2 Is it not true that the time and current density dependencies of the heat transfer coefficients for Pd cathodes polarised in LiOD-D_2O as well as the difference between these heat transfer coefficients and those for Pd cathodes polarised in LiOH-H_2O can be more reasonably explained in terms of excess enthalpy generation in the Pd/LiOD-D_2O system which increases with time and current density?

However, in the light of the answer to Pons-Fleischmann: Questions 2, 3 and 5 we have the follow-up questions:

Lewis: Question 3 In view of the progressive decrease of the heat transfer coefficients with time, is it not true that the method of calibration adopted by Lewis et al [2,3] is subject to unacceptable errors?

We believe that this question applies equally to the work carried out at Harwell[1] (see below). There are also two follow-ups to Question 3:

Lewis: Question 4 Bearing in mind the large calibration errors in the methodology used by Lewis et al (e, 3) and the fact that the heat is it not true that Q_{excess} is subject to even larger errors then is the calibration?

Lewis: Question 5 Are these erroneous methods of calibration/data evaluation the origin of the strange statements that isoperibolic calorimetry is subject to ± 10% errors?

We have been told that Nate Lewis became aware of some of these deficiencies of the work and that he attempted to develop a better method of calibration before and that he attempted to develop a better method of calibration before the publication of the second paper [3]. However, we can see no difference between the publications in Nature and Science except for the Note added in proof in [3]. We therefore have a further question for you:

Lewis: Question 6 Are you aware of any changes made by the group at Caltech to correct for the deficiencies of the calorimetry as described in the paper in Nature[2]?

In the section of this letter which deal with our attitude to the review of the published material (if I ever get that far!) I will deal with our views on the matter of presentation of the results in the various papers and we will certainly wish to address some specific questions to you (also on other topics). However, for the present we will confine our Questions to those matters which are relevant to the way the experiments were carried out.

Lewis: Question 7 Have you any particular views on the exact replication of experiments and, here, especially the comparability of the electrode materials in [2,3] and [4]?

You will wish to know that Johnson Matthey made the electrodes especially for us and undertook to supply these to other research workers. However, the delivery time was always at least 6 weeks so that it is unlikely that even the 1.0 mm wire supplied by Aesar (the U.S., Marketing division of Johnson Matthey) would have been comparable to the material we used. [44]

[44] MCHM Important point.

Lewis: Question 8 Have you any views on the use of Dewars with air at a pressure of 1 at. In the gap? On the feasibility of using vigorous mechanical stirring in a cell contain 30 ml of electrolyte, the cathode, anode and reference electrode, a temperature sensor and an electrical heater? For that matter on a method of stirring which raises the temperature of the cell contents by ~ 0.3°C?

Lewis: Question 9 Have you any views on the duration of the experiments especially on the time elapsed following each change of current density both within and between the calibration cycles?

We would allow between 6 and 7 days between each change of current density to all our excess enthalpy to reach a new steady state [45] (for the diameter electrode used in [(2), (3)]. This is equal to the total experiment time for the results in Table 3 of [(2)].

This brings me to a question which we regard as being very important:

Lewis: Question 10 What is the list of the sum total of calorimetric experiments actually carried out by the group at Caltech?

The authors state that they use 11 different materials for their experiments. Assuming that each material was tested at just two current densities, restricting ourselves to a single sample of measurements with the materials they have listed and to D_2O alone and allowing no time for setting up the experiments, they would have needed 144 days to complete such a schedule (72 days if the work load had been divided between two calorimeters). [46] In our own laboratories we would have set a requirement of 5½ years for the use of a single calorimeter.

There is a further point which is pertinent to this question as well as to Questions 11 and 12 below. At the meeting of the American Electrochemical Society held in Los Angeles in May 1989 it was evident from Nate Lewis' presentation (including the T.V. Conference) that the cell design which had been used in their work up to that time had been dimensioned either from a T.V. News cast or a press conference which showed someone holding a large cell in their hand. This type of cell had been constructed by us for measurements on 2 cm diameter x 10 cm long electrodes. These measurements were never carried out firstly, because we could not afford to do so, secondly, because they would have taken too long and, thirdly, because it became apparent that such large diameter electrodes could not be charged to the point where they generate excess enthalpy (we believe that the principal cause of this is the cracking of the metal: there is much more to this than I can tell you in this letter but some the background may be familiar to you). We used this cell simply as an illustration: it was the most convenient cell for this purpose as it was the largest one available.

It appears therefore that the authors changed their cell designs following the meeting in Los Angeles and according to the text of the paper, all the calorimetric results would have had to be obtained in the period up to 23rd May, the date of submission of the paper. We do not believe that such a schedule was possible allowing time for the construction of the apparatus, setting up, data evaluation and the writing of the paper. We believe that most of the measurements reported

[45] MCHM Whether this is important or not it was certainly "different" – and therefore not replication.
[46] MCHM This is also important and a critical difference.

must have been made using the earlier unsuitable design [47] (not especially the use of a small electrode in a large cell) and/or that the calorimetric measurements reported in the paper are the sum total of those carried out.

I realise that I am polarising your thoughts but we would like your independent answer to the following question:

Lewis: Question 11 Have you any information and views on the total number of experiments carried out and the nature of the cells used for the various experiments?

The reason why we regard this as being so important is that we believe that an independent review of the work carried out should give a precise listing and description of the experiments. We are especially opposed to the making of exaggerated claims be they positive or negative (although we also believe that the people claiming negative results have been especially prone to this habit).

The questions of the cell dimension is also important with regard to a further point.

Lewis: Question 12 Do you believe the results on the temperature distributions reported in the paper?

These are completely out of line with our own measurements. Variations of the magnitude quoted in the paper imply fluctuations in the enthalpy content of the order 8 Joules cm^{-3} when the total power input in less than 1 Watt. We believe that if there is any substance to these measurements, then they must have been made with a large cell containing a small electrode polarised at low current densities (and the electrode was probably incorrectly positioned).

While there are some useful measurements reported [(2)], they are rather peripheral to the main subject in hand.

This brings me to the set of questions which I want to pose to you about the work of the group in Harwell. I will again restrict the questions to points arising from the calorimetry. - even with this restriction the list is by no means comprehensive. You will see from the last part of my letter that I decided during the Autumn of last year to establish contact with the groups at Caltech, MIT and Harwell mainly with the view of getting them to release their raw data for independent review. As part of this process I wrote To David Williams on 22/11/91, he replied on 12/12/91 and we met at the beginning of January to discuss a number of these questions. I wrote to him on 21/1/92 to summarise our discussion and he sent me a preliminary reply on 7/2/92. The various slippages in time have been due to my prolonged stays in the U.S. Last December and in France since January.

I think it would be fair to say that these exchanges have not really led to a clarification of the major issues. I believe that this could only be achieved by re-analysing the raw data but one difficulty with this will be the odd way in which they collected these data (the direct relationship of the thermistor resistance to the enthalpy outputs from the cells using a power series in log R viz $P=a-b \log R + c (\log R)^2$) i.e. it is questionable whether they really recorded "raw data". You

[47] MCHM I agree with this conclusion but was not aware of this detail.

will be interested to know that I believe that Harwell will in fact release these data – I also wrote to David Williams' former boss, the Chief Atomic Scientist, Dr. R. Bullough, and he too would agree to this.

Let us therefore start by considering the calibration of what the group at Harwell called by FPH calorimeters. According to the paper and my discussion with David Williams, the cells were calibrated both before the start of the experiments and also during the experiments immediately after refilling the Dewars with D_2O to make up for electrolysis and evaporation losses: to be specific at "the minimum of the temperature-time curves". We find this procedure hard to understand and we are completely baffled by Fig. 2B of [1]. The temperature-time curves simply do not look like this – a more representative example is Fig. 3C in our paper[4]. We made measurements every 300s and. On this time scale there is a sharp fall in temperature followed by the thermal relaxation of the Dewar contents (the thermal relaxation time for the Dewars used at that time was ~1600s). As the relaxation time for radial mixing is ~3s and axial mixing ~20s there is no mechanism by which one could have a slow drop in temperature leading to a rounded minimum as in Fig. 2B of [1]. David Williams has always been a proponent of using smoothing routines which might explain the recording of such odd data or perhaps the thermistor resistances to the output enthalpies of the cells. This leads to:

<u>Williams: Question 1</u> What is your explanation for the shapes of the Power-time relationships in Fig. 2B of [1] in comparison of the temperature-time relationships such as that in Fig. 3C of [4]?

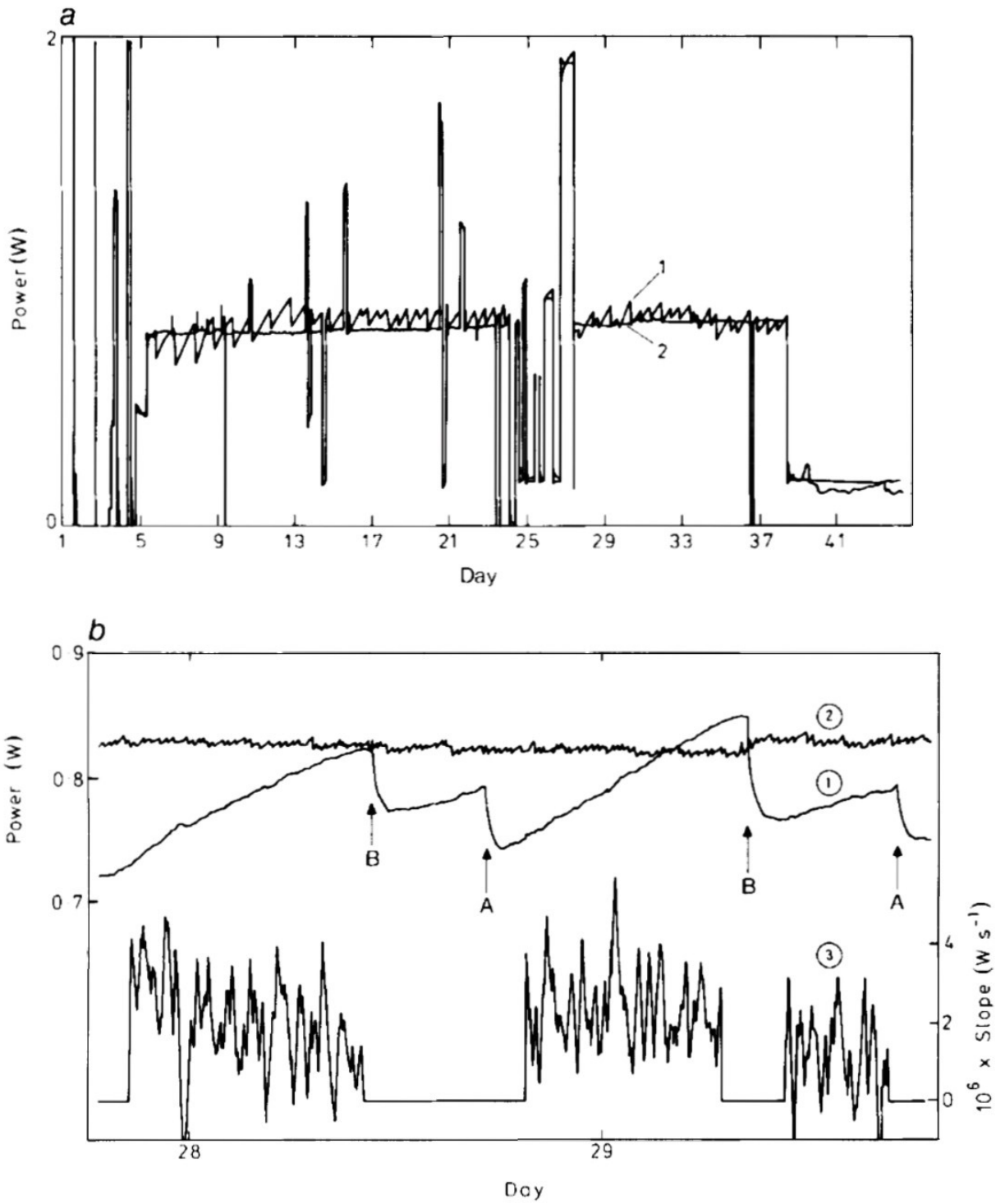


Figure 2 A and B from Williams *et al.* [1] Caption in paper: FIG. 2 a, Raw data from FPH-type calorimeter containing a 4-mm Pd rod (1.5 cm long, Johnson Matthey 'specpure', drawn from sintered stock) in LiOH eletrolyte. Line 1 represents the output power calculated from the thermistor reading and line 2 represents the Joule input power to the cell, $P_{in} = I(V - V_o)$ where $V_o = \Delta H_d/2F$ (1.527 V for D_2O and 1.481 V for H_2O, ΔH_d being the enthalpy of dissociation, for

example, $D_2O(I) \rightarrow D_2(g) + \frac{1}{2}O_2(g)$). The large step variations are calibrations. b, An expanded region of a, which emphasizes the sloping baseline. Lines 1 and 2 are as in a. Line 3 is the gradient of the apparent output power calculated by differentiation of the data using a seven-point Savitzky-Golay routine[23], stepping one point at a time. At points A the calorimeter was topped up to the reference mark with H_2O pre-warmed to the cell temperature. At points B a volume of liquid estimated from the electrolysis rate was added.

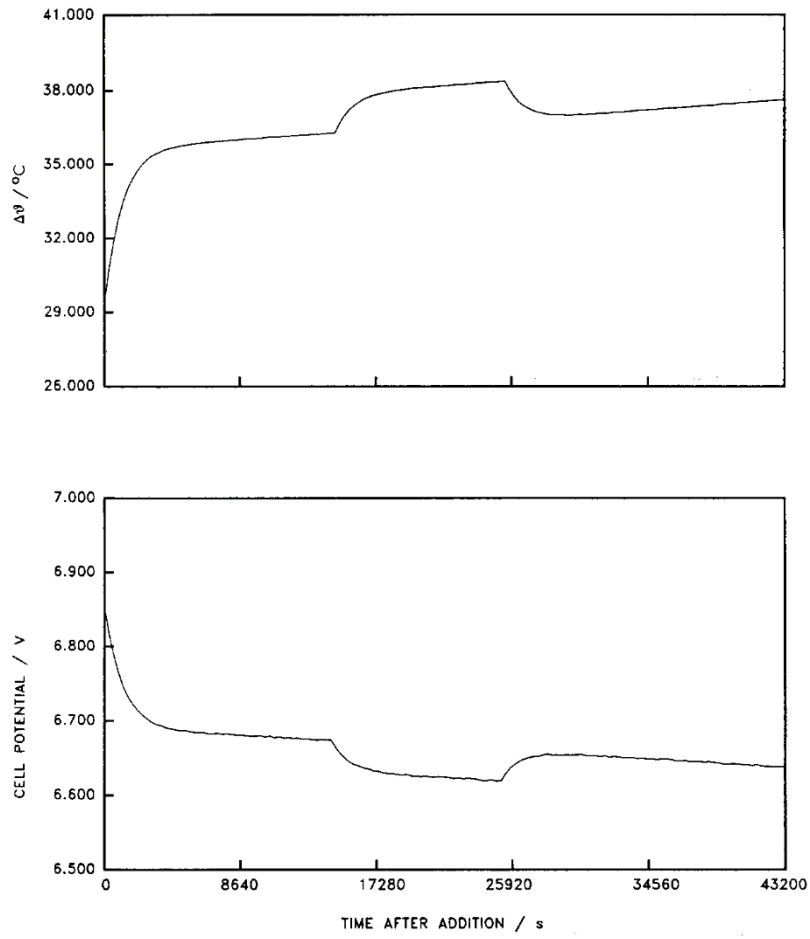


Figure 3C from Fleischmann *et al.* [(4)] Caption in paper: Fig. 3C. Same as Fig. 3A except time of measurement approximately 1.32×10^6 s. Estimated $Q_f = 0.372$ W. Fig. 3A: Temperature above bath vs. time (upper) and cell potential vs. time (lower) data for a 0.4 × 10 cm Pd rod in 0.1 M LiOD solution. The applied current was 800 mA, the bath temperature was 29.87ºC, and the estimated Q_f was 0.158 W. The time of the measurement (taken at the end of the calibration pulse) was approximately 0.45×10^6 s after the beginning of the experiment.

The next point relates back of Pons-Fleischmann: Question 3 as well as Lewis: Question 3:

Williams: Question 2 As there is no steady state in the temperature-time relationships and, more especially, as there is a progressive fall in the heat transfer coefficients with time, is it not true that one cannot establish a unique Power-(log R) relationship which is valid at all times?

Williams: Question 3 If there is no unique Power-(log R) relationship, is it not true that the non-steady state temperature-time variations and the decreases of the heat transfer coefficient are converted into errors in the Power – (log R) calibration used to interpret the measurements in [(1)]?

Williams: Question 4 If the answer to Question 3 is "yes", then is this another origin of the strange statements that isoperibolic calorimetry is subject to ± 10% errors? (see also Lewis; Question 5)

You have shown that the calibration procedure used by Lewis et al [(2,3)] could only detect changes in the rates of excess enthalpy Question 1). The group at Harwell also reduced the current density during the calibration to 0.2 -0.4 of the current density used in the normal electrolysis (although they did not attempt to keep the temperature equal to that in the absence of the input from the resistive heater).

Williams: Question 5 Is the method of calibration used by the group at Harwell also at best ambiguous or at worst completely wrong?

To be specific: If the rate of excess enthalpy generation, Q_{excess}, does not change during the calibration procedure, then Q_{excess} only affects a in the expression P = a-b log R +c(log R)2 (and to a lesser extend will contribute to errors in b and c). Does it not then become impossible to determine Q_{excess} by using the procedure adopted by the group at Harwell?

We come next to the nature of the results obtained by the group at Harwell. Bearing in mind that the calibration of the calorimeters applies to the period immediately following the "topping up" of the cells with D_2O we must ask:

Williams: Question 6 Is it possible for the cells to operate in such a markedly endothermic manner in the initial periods following the "topping up" of the cells as illustrated in Fig. 2B? [48] And you have already referred to the next point but I will include it for the sake of completeness:

Williams: Question 7 What could have been the cause of the unbelievably marked endothermic operation more than 10,000 minutes after the start up of the electrolyses as illustrated in Figs. 2C and d of [(1)]?

The explanation given in [(1)] is clearly inapplicable as the bulk of the palladium charging process will have been completed in say 1,200 minutes. [49] Do these results point to some total fiasco in the data processing?

I would like to address next the question of the accuracy of the measurements if it is assumed that the method of data processing is in fact correct. Bearing in mind that we controlled the

[48] MCHM Physical destabilization of the lower (partially) unstirred and occluded volume?
[49] MCHM Endothermic loading from D_2O? This would also be of completely different scale. See also Willams Question 18

temperature of our water baths to ± 0.01°C overall (and locally to ± 0.003°C) whereas the group in Harwell used ± 0.08°C we have:

Williams: Question 8 Have you an estimate of the likely errors of the measurements in [(1)] in comparison to [(4)]?

In making this estimate one must naturally also bear in mind that the measurement of Q_{excess} in [(4)] was made at the calibration point so that the comparable point for the measurements in [(1)] is immediately following the "topping up" of the cells.

The consideration of Question 8 leads naturally to the next point. The electrodes used in [(1)] were considerably smaller than those used in [(4)] for the comparable current density range (the factor is probably in the range 2-5). [50] We come to:

Williams: Question 9 Bearing in mind the differences in the sizes of the electrodes and the accuracy of the measurements in [(1)] and [(4)] as well as the magnitude of the excess enthalpies for the comparable current densities in [(4)], do you believe that Williams et al would have been able to detect the excess enthalpy using their methodology?

Question 9 leads to a point which has worried us considerably, more so in relation to the measurements with the isothermal calorimeter than with the FPH calorimeters.

Williams: Question 10 Bearing in mind the enthalpy inputs reported for the same solutions and a range of current densities in Table 3 of [(4)] do your believe that the current density range given by Williams et al[(1)] is in fact correct or are the current densities lower than those stated in Table 1 of [(1)]? If they are lower, then how does this bear on the answer to Question 9?

Before dealing with the results obtained with the IHF and isothermal calorimeters, I want to give you some information on the number of experiments carried out. David Williams has confirmed that the results reported in the paper are the sum total of those obtained i.e. there were no duplicates and no tests of the effects of changes in current density. This leads to a question which is rather loaded:

Williams: Question 11 Is an investigation of these systems based on single experiments at essentially a single current density an adequate test of the claims which we made?

I come next to a very delicate matter which you must regard as being strictly confidential. There were in fact further experiments carried out with the isothermal calorimeter and we have an outline of these experiments: they were aimed principally at the calibration of the system and, in our view, would have considerably weakened the reliance on the results obtained with this calorimeter by the group in Harwell if they had been included in the paper. I have questioned David Williams about these results in my letter of 21/1/92 but he has not responded. I do not know how to handle this situation. David Williams has always been a good friend and I would certainly like to work with him on other topics. We believe that this situation points to the need

[50] MCHM What sizes are we talking about (diameter and area)? JR Williams [(1)] 4 mm diameter, 1.5 cm length, 2.14 cm^2. Fleischmann [(4)] 4 mm diameter, 10 cm length, 12.82 cm^2.

to establish an exact listing of the experiments carried out in any comprehensive review of selected papers.

I will turn now to the results obtained with the IHF calorimeter. We have two questions for you:

Williams: Question 12 Do you believe that this device was in any sense an improvement on the FPH calorimeters?

Williams: Question 13 Do you believe that it would have been possible to determine excess enthalpy generation (if any!) using such small electrodes in a large calorimeter?

I have a further question which applies even more so to the measurements with the isothermal calorimeter:

Williams: Question 14 Do you believe that it would have been possible to avoid contamination by H_2O in these two large devices?

I believe that David Williams was disappointed with the performance of the IHF calorimeter and they turned their attention to the isothermal calorimeter. Indeed, they had a pressing wish to put this device to use. It was developed by measuring the Pu content of various materials, a duty which it no doubt achieves very adequately transpires that they did not use auxiliary stirring of the solutions in the electrochemical experiments. The current densities at the anode were so low that the solution in that region would have been unstirred so the stirring was entirely dependent on gas evolution at the cathodes. We observe that the thermal relaxation time was short, possibly of the same order as the mixing time within the calorimeter. The dimensions of the longest lived eddies will be of the order of the calorimeter dimensions. For the stated stability of ± 4 Joules and probably ± 40 Joules. This brings us to the next three questions

Williams: Question 15 [51] Do you believe that mixing within the calorimeter was adequate to eliminate the effects of these fluctuations?

If the answer to Question 15 is "no" then

Williams: Question 16 Would the control system not have acted as a "bang-bang" controller no matter what control strategy was originally chosen? Even if the system did not get into such a regime then bearing in mind the likely answer to Question 13, what would be the expected broad-band noise performance for such a large thermal mass connected across the input of a feedback controller?

If the answers to Question 16 are "yes" and "bad" then

Williams: Question 17 If the system acted as a "bang-bang" controller, then what would have been the effect of the measurement system? Would the non-linearities induced by operating in this mode not affect the accuracy of the system?

[51] JR In the original text, this was number 14, the same as the previous item. I renumbered items 15 through 24 to keep things straight.

In considering the results obtained, we believe that it is probably necessary to discount those for the "cast beads", the "melt spun ribbon" and the "8-mm bar" because of the unknown (and, in all probability, unfavourable) geometry. This brings me to a general question:

Williams: Question 18 Have you any comments on the effects of the symmetry (or rather the lack of symmetry) of the cathodes with respect to the anodes in the various investigations in general and in that of Williams et al[(1)] in particular?

We are reduced therefore to examining the results for the 2 mm diameter x 2 cm rod electrodes. Our first question concerns Fig. 2e. The bulk of the dissolution would have taken place in the first two hours and, if the enthalpy release had been uniform over that time, there should have been an excess enthalpy of ~25 mW. [52]

Williams: Question 19 Why was the excess enthalpy for the dissolution of D in the Pd not seen and what could be the cause of the large negative enthalpy over the first two hours? Does this indicate a malfunction of the instrumentation?

Incidentally, this brings us to another question:

Williams: Question 20 Why was this experiment terminated at such an early stage?

We also have to reiterate Questions 9 and 13:

Williams: Question 21 In the light of the results in [(4)] do you consider it possible that Williams et al[(1)] would have been able to detect excess enthalpy generation on a 0.063 cm^3 electrode? [53]

In answering this question you will want to bear in mind that there is a systematic error in this system of the order of 1% but that the authors quote enthalpy inputs to five significant figures and errors of the order ± 0.1%. This leads to:

Williams: Question 22 What do you believe is the basis of the error limits quoted for the isothermal calorimetry?

Williams: Question 23 If the real error limits are in the range ± 10 to ± 100 mW, then how does this affect the answer to Question 21?

The whole question of the treatment of errors is decidedly odd. You may recall that it is our view that the error limits must be specified for each individual experiment as the variability of the excess enthalpy under nominally identical condition is itself a part of the investigation[(4)]. This leads to

Williams: Question 24 Have you any comments on the treatment of errors in this paper [(1)] and, for that matter, in any of the other investigations?

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[52] MCHM See also Willams Question 7. Is the loading endo- or exo-thermic (up to the maximum)? If the source of loaded D is D_2 gas then this is certainly or exo-thermic. But F&P take full account of the thermoneutral correction (1.54 * I). So, any D loaded into the Pd results in an over-correction (and, therefore, or endo-therm). I have never analyzed calorimeters in this way. What is Martin saying here and in Q7 and Q19 below?

[53] JR The Williams cathode was a rod 2 mm diameter * 2 cm length.

This is our first list of questions about the calorimetry as reported in the paper but there are others e.g. those dealing with the materials. We realise that many of these questions can possibly only be answered if one had access to the original raw data and provided these data are in such a form that one can develop alternative means of data processing. We are therefore in favour of a review which would include such analyses and the prerequisite is that someone should establish a collection of such raw results.

This brings me to a number of further comments about the review processes: our attitude to the review of our won work, the pros and cons of examining published papers, the special position of papers on "Cold Fusion" with regard to such review processes and, finally, the course of events during 1991 which influenced our thinking. I also want to comment on the reasons why a small number of "negative" papers had such a dominant influence on the development of the subject (or rather the lack of development!).

To deal with the first point: as you will have seen Stan and I are strongly in favour of subjecting the papers and research on "Cold Fusion" to independent scrutiny. What is not generally know is that it was a condition of our continuing work on this topic in Utah after October 1989 that our data should be independently assessed. [54] We could foresee that the antagonisms and irrational comments would increase rather than die downward we thought that an independent assessment might serve to counteract this tendency - but perhaps we were mistaken. We certainly did not want to publish anything further arising from the work in Utah unless a cross-section of the data had been independently evaluated.

You may know that the people who were to carry out this separate evaluation failed to do so. However, Wilford Hansen has now made an excellent comprehensive assessment of several of the data sets which we wanted to include in two further papers (the first drafts of these papers have been at NCFI and the Patent Attorneys since September 1990).

Now, with regard to the pros and cons of examining published work in general: we believe that this is normally a waste of time. The effort involved can be as great (or even greater) than the effort of writing the paper(s) in the first place. It is usually adequate to rely on the accumulation of published work to lead to a definition of the experimental evidence.

You will see here that we stress the experimental aspects. We would draw a sharp distinction between experiment and the development of models based on theory. Theory tends to converge to established orthodoxies and inconvenient facts are fitted in with a string of ad-hoc assumptions which are frequently inconsistent with each other. [55] There is therefore a case to be made for comprehensive reviews of established research fields. As far as the present topics are concerned one might well wish to start with the dissolution of H, D and T in Pd and the properties of these systems. Would you be interested in taking part in such ventures?

Next the special position of papers on "Cold Fusion" in such review processes: we have naturally also been aware of the fact that the topic of "Cold Fusion" was unlikely to be a "normal

[54] MCHM Do we have backup for this?
[55] MCHM ☺

case". Thus, we predicted many aspects of the course of events since March 1989 – as is shown for example by our correspondence during 1988. We fully anticipated that there would be a spate of negative results: nothing is easier than the devising of experiments which give nil results and, if this is the expected outcome, then this is the end of the story. It is a relatively straightforward matter to write a paper which look good and imparts this negative message.

As the negative attitudes towards the work built up (and, especially as the negative publicity built up – see later) we came increasingly to the view that a cross-section of the papers and, indeed, of the raw data used in those papers would have to be subjected to independent review. Furthermore, we were of the view that this review would have to encompass a cross-section of both the "positive" and "negative" reports. We also believed that eventually the volume of independent "positive" results would build up so that eventually there would be pressure to subject the "negative" work to independent scrutiny.

I hope that this explains the rather careful attitude we developed towards our own work!

I want to comment next on the course of events during 1991 and the bearing this has on the review processes. First of all, you will see that we did not press for any sort of review (although we wanted to have our own work assessed) and it was the course of events which persuaded us that there would have to be a review and led to our limited intervention in this process (see more about this below). Secondly, you will see that I have not raised any question about the paper from the group at MIT [(5)] although this certainly had at least as great a "negative" impact as the papers from Caltech and Harwell and although it is probably the least satisfactory paper out of this set. The reason is that we could see that the authors could get into serious difficulties (indeed, we wanted to prevent this as far as possible – see below). We believe that it was the deficiencies of this paper, coupled to the excessive "negative" publicity generated by some of its authors, which led to the pressure for inquiries of one form or another.

It was our view that these inquires would develop rapidly into acrimonious debates about personalities, the history of events, politics, etc. when what was really required was an objective study of the methodologies used, the methods of data evaluation, the question whether the results support the conclusion, the definition of the scope of the various investigations etc. I therefore wrote to a cross-section of the authors mainly with a view to getting their agreement to release their data for independent evaluation. I made it clear that Stan and I would not be involved in any such review process. I pointed out that in our view there would be no controversy about the listing of experiments carried out or of the experimental conditions used nor the raw data. Differences of opinion could arise about methods of data evaluation and the conclusion reached but I pointed out that this did not matter as long as the basis for these conclusions were clearly established. I implied that the authors should join in such a review process and pointed out that they would still be free to challenge the conclusions if they disagreed with these.

We hoped that we could in this way neutralise some of the negative aspects of the reviews which were being proposed. One reason for my visit to the U.S.A. last December was our wish to further such a cooperative independent review. You may wish to know some of the outcome: as I have already told you David Williams and his former boss at Harwell have agreed their raw

data (although I believe that they are not very happy about this – who can blame them?). Mark Wrighton also replied in due course in a positive way and Nate Lewis prevaricates. (He was not at Caltech during my visit but wished Gary Taubes on me!) His stance is that he would like to do their own careful calorimetry on our electrode materials. Our view is that he should first release their original raw data. We have also been promised the cooperation of other groups (unofficially) but whether any of this can be translated into action remains to be seen. I have also kept John Maddox fully informed as we did not want to proceed behind his back. He says that he agrees to the procedure but without any great enthusiasm. You will probably see the direction we were trying to head into: a joint publication in Nature (we are gluttons for punishment!). Failing this an independent publication of a paper at the Nagoya meeting entitled "How not to find Cold Fusion".

Unfortunately, because of our many commitments we continue to have severe slippages in time and all of our endeavours may be overtaken by events on the East Coast – it is a pity.

I want to finish this letter with some comments about why such a small number of "negative" papers had such a large influence as this too has a bearing on the review process. It is our view that it was not so much the content of those papers as the press publicity developed by some of the authors which had this devastating effect. The reason why we regard this as being so important is because of its bearing on the Patent situation: the Patent Examiners are quite happy to cite press reports in their Official Actions (rather than the original papers). We feel that this explains in part the press publicity. I should explain to you here that Stan and I had no wish to be involved in Patents (we have made it abundantly clear that we do not want to benefit from this work) but we could see that the university of Utah had no option but to seek Patent cover. Stan and I really do not have much loyalty left to the University but we do have this loyalty to the State – hence our concerns.

We do not believe though that the negative publicity cannot be explained entirely in a simple way. The attitudes which develop can best be described by this statement: "you are wrong and you must stop your research". The normal attitude is to wait for its demise - if the initial results were incorrect. We do not know what one could do to investigate the nature of the press coverage – it is clearly outside the scope of any review of the type we have in mind. Have you any ideas?

At the same time it is clear that a comprehensive review will eventually have to address the questions:

(a) who was responsible for developing these views (the spokespersons are well-known but were other people involved?)

(b) why did they develop these particular views.

I should explain to you that we have already been approached by numerous Social Scientists who are interest in these questions. Should be get involved in such investigations? Will this pressure to investigate these aspects build up to the point where there will have to be formal review?

I promise you that future letters will be shorter!

Best regards,

Yours sincerely,

Martin

MARTIN FLEISCHMANN

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P.S. Stan recently sent me a copy of your letter to Steve Jones (Steve didn't send me his critique of your Seminar). Stan especially like your P.S. but thought that Steve would not appreciate it!

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P.P.S. Your FAX is just now to hand and I see that you have already answered many of my questions. May I ask you to consider the remainder?

P.P.P.S Do you agree that quite irrespective for any of the review there may be of the published work , there should be a published comprehensive critique of several of the past papers (both

positive and negative). Would you be interested in taking part in such a venture and would you like to suggest some further authors?

1992-04-14

Bury Lodge,
Duck Street,
Tisbury,
Wilts. SP3 6LJ
U.K.

Dr. Melvin Miles
NAWC Weapons Division,
China Lake,
California

Dear Mel,

Thank you for your FAX of 13/4/92 and the interesting attachments. I believe that irrespective of any short term objectives, we should prepare comprehensive documentation about past publications. That we'll then give us freedom to decide about future publication plans. As you will realise, Stan and I are rather in the background in all the activity but we have boxes of files!

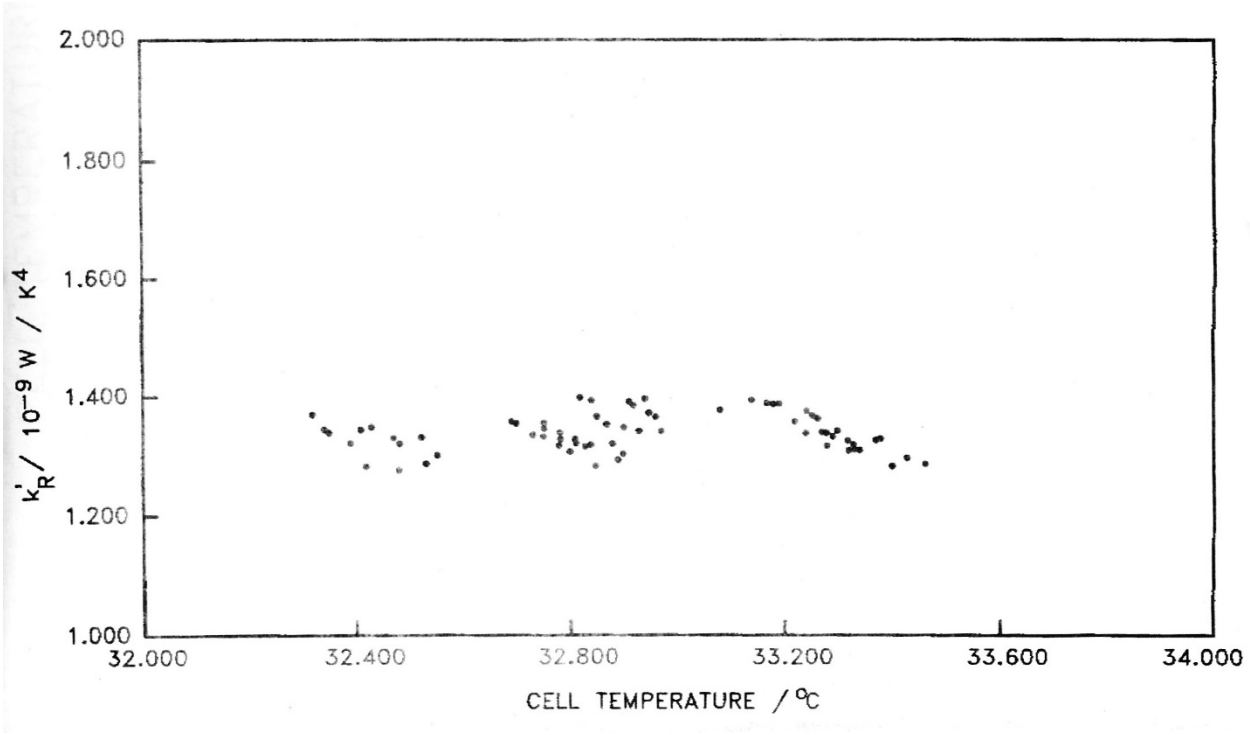
Here with the figures 3A, B, C, D I referred to in my letter. Somewhere in the text Fig 3A was described as Fig 3B but I am sure that you will have spotted this.

Mike will phone you on his return to the U.S.A. I am sure you will have realised that we are concerned at present to sort out the past history but I believe that it would nevertheless be sensible to keep the contact Mel ↔ Mike ↔ Martin ↔ Stan under wraps just at present."

Regards,

Martin

M. Fleischmann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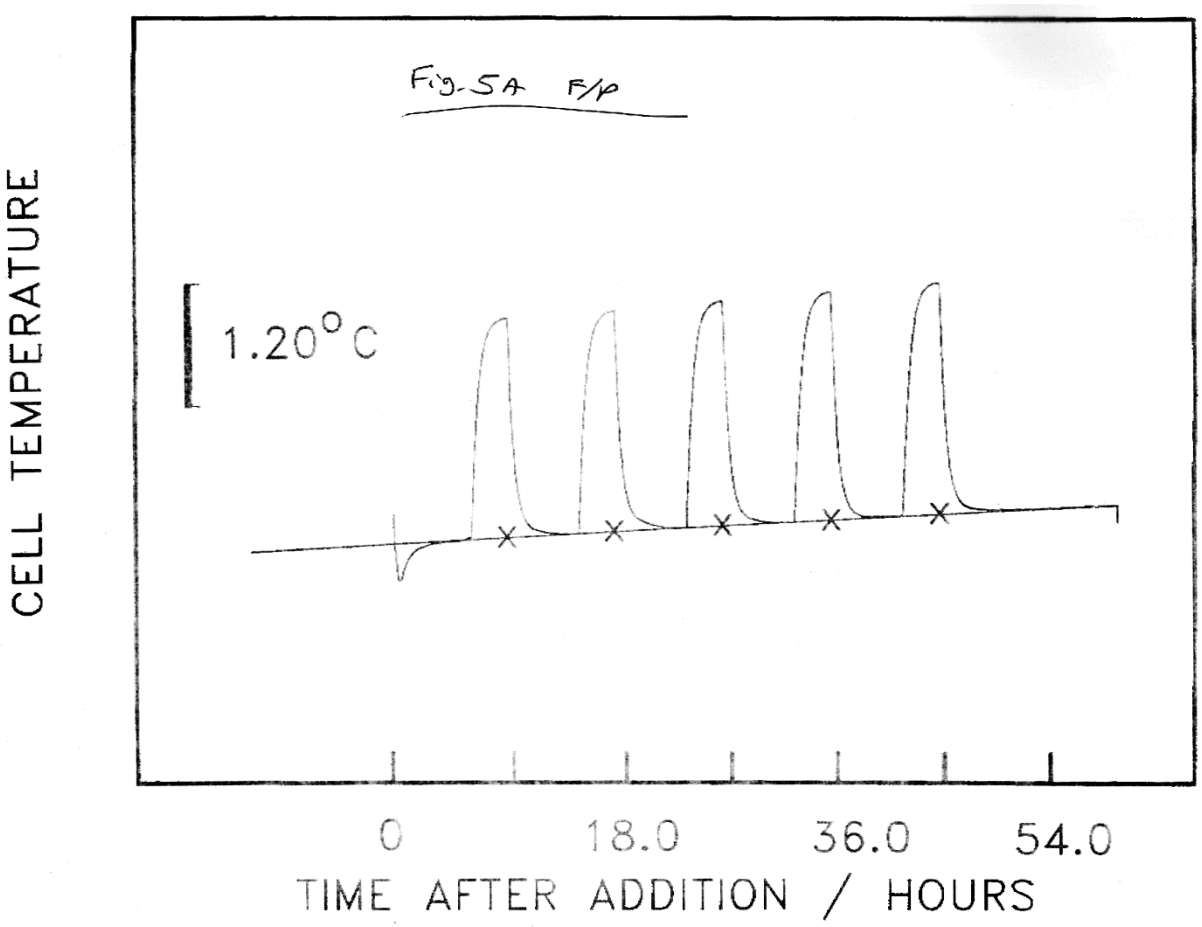
CELL TEMPERATURE / °C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36.0
54.0
TIME AFTER ADDITION / HOURS

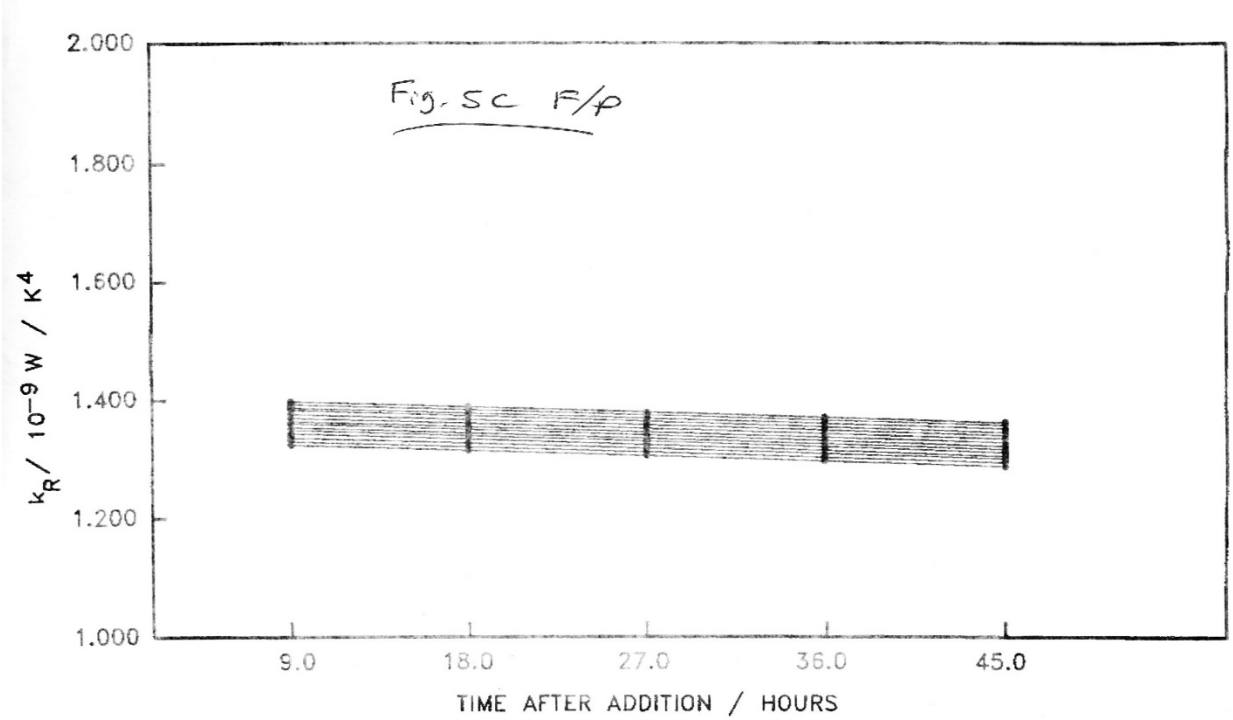
Fig. 5C F/P
2.000
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TIME AFTER ADDITION / HOURS

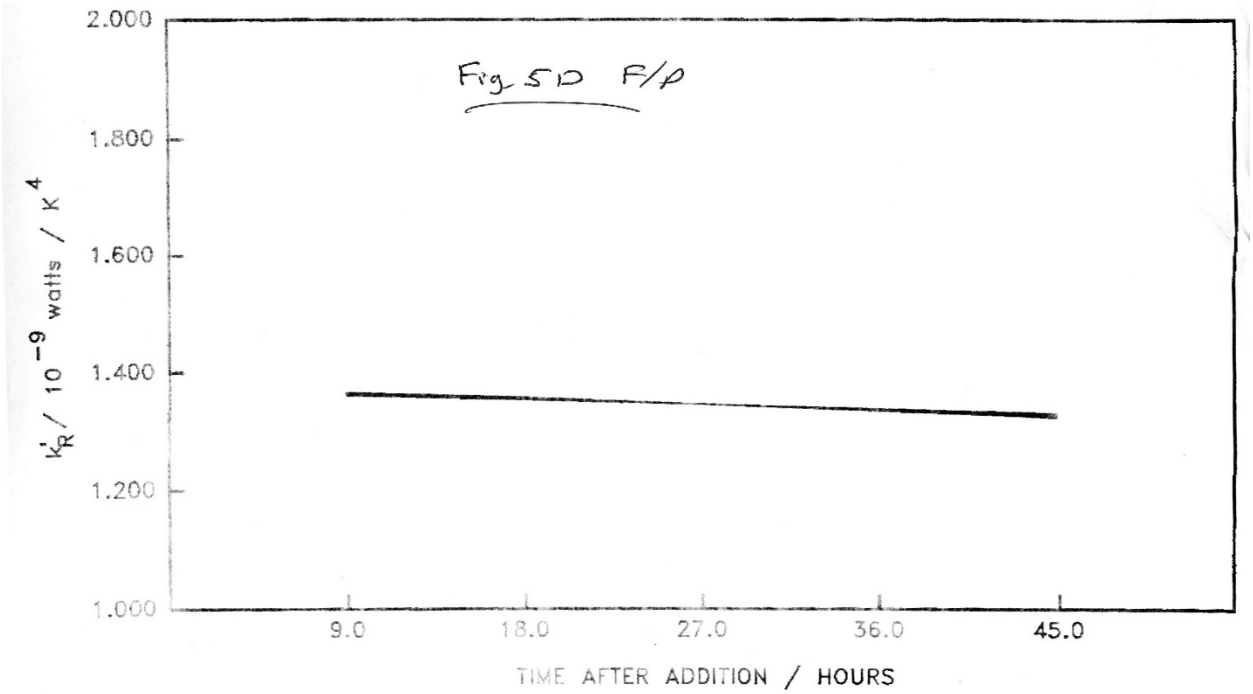
Fig. 5D F/P
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1.000
k'_R / 10^{-9} watts / K^4
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36.0
45.0
TIME AFTER ADDITION / HOURS

1992-**04-21**

University of Southampton heading

Dr. Melvin H. Miles,
Chemistry Division,
Naval Weapons Center,
China Lake,
Code 3853
CA 9355
U.S.A.

MF/KJW

21 April 1992

Dear Mel,

This is just to follow up my letter of 2nd April and Fax of the 14th. It strikes me that the letter forwarded to you by Stan may not have had the final version of page 17 with the two hand-written P.S's. In case it did not have this page, I am now sending it to you.

I see that in my letter of 2nd April I did not in the end explain to you why I restricted my questions to our own, the Caltech and the Harwell papers i.e. I excluded that from MIT. It is our view that the authors of the MIT paper are already in considerable difficulty regarding their actions in converting the raw data into the final text (reference 5 in my list). They are now being quite openly accused of fraud. However, if you, we or all of us were to act on my P.P.P.S. Of 2/4/92, then we would certainly have to consider reference 5. Do you have full documentation on this paper, (including Parker's attempt to disown it, the letter from Mallove to the President of MIT, Charles M. Vest dated 9th February 1992, and the letter of 19th March by Charles M. Vest dated 9th February 1992, and the letter of the 19th March by Charles W. McCutchen to Charles M. Vest). We believe that the paper about the MIT work would make an excellent case study of how not to treat experimental data. [56]

More anon,

Yours sincerely,

Martin

P.S. If you don't have any of these letters and statements, then please let me know. Equally, please send me details of anything I should have sight of. Your full correspondence with Nature and Science would be very useful. Equally, Ed Storms' correspondence with Science and John Appleby's with Nature would be very useful. Can you act on this? I have in mind a further counterblast. More anon.

[56] MCHM ☺

P.S. Stan recently sent me a copy of your letter to Steve Jones (Steve didn't send me his critique of your Seminar). Stan especially like your P.S. But thought that Steve would not appreciate it!

He has a total blockage about the excess enthalpy measurements because his disbelief of our results in 1988/spring 1989 precipitated the unfortunate chain of events leading to the Press Conference. Naturally, if there is excess enthalpy, then this reflects badly on his actions so he strains to disbelieve it. The negative publicity by the low level neutron believers has been a big factor in the lack of development of the subject.

P.P.S. You FAX is just now to hand and I see that you have already answered many of my questions. May I ask you to consider the remainder.

P.P.P.S. Do you agree that quite irrespective of any of the reviews there may be of the published work, there should be a published comprehensive critique of several of the past papers (both positive and negative). Would you be interested in taking part in such a venture and would you like to suggest some further authors?

Do you have Douglas Morrison's "Cold Fusion Update No 6"? If not, let me know and I will FAX it to you.

This our extract from a letter to Stan and Giuliano – the first part is not relevant.

Once again, Giuliano does not know about this correspondence.

[Following is the letter to Stan Szpak and Giuliano Preparata that Fleischmann copied to Miles]

Let me now turn first of all to a possible way of dealing with Douglas Morrison's "Cold Fusion Update No. 6". Some of the other commentaries can be taken on board in a preliminary way in any document we may wish to circulate about this article. This would not stop us from writing further about these commentaries at a later date.

We start with a summary, list of subjects and general introduction written by M.F. This would be written in a very neutral and Olympian way. It would certainly include a statement that this is the first and we hope the last commentary which we will distribute. The reason for this is that we believe that the proper ways for communicating with the general scientific public are the traditional methods: scientific meetings (where it is generally accepted that papers and lectures presented may not be reported except with the permission of the authors) and peer reviewed papers in learned journals.

I would emphasise that our commentary is therefore an exercise in scientific journalism and should be seen as being this. However, we attempt to follow strictly the rules of journalism which we believe are:

i. separation of "facts" from "editorial opinion"; such "facts" include the opinions expressed by other scientists

ii. verification of all "facts" with the people concerned

iii. a clear identification of "editorial opinions".

I hope that this would set the tone for the rest of the article and I would intend to return to the theme in the final section.

Section 1 will deal with Douglas Morrison' commentary on the G.E. Paper and should be written by S.P. This could start with a statement of facts, the main point being that Douglas Morrison's statement that we have not written a rebuttal is incorrect. We can point out that he did not check this vital fact with any of S.P., M.F., Roger Parsons (Editor in Chief) or Ron Fawcett (U.S. Associate Editor).

In addition we have prepared a full commentary addressed to the Editor in Chief and, with his permission, this will be deposited together with our other working papers, data and correspondence on the subject of "Cold Fusion" in an appropriate Library.

A more detailed paper comparing our methods of analysis with that developed by the group at G.E. is being prepared for publication.

S.P. (or M.F.) should ask the permission of Ron Fawcett/Roger Parsons to quote from M.F.'s letter to Ron Fawcett of 26th March 1992 (our latest communication with the Editors about the G.E. Paper) listing our major criticisms. I would prefer it if this permission were refused and that the letters of reply should state that such matters are confidential until after the publication date. This correspondence with the editors could then be published verbatim as a fact in this saga.

S.P., M.F. (or our attorney) should write to Fritz Will to ask him to clarify his position with regard to Douglas Morrison's statement: "The authors include former G.E. Researcher Fritz Will who after the experimental work was essentially completed, became Director of the National Cold Fusion Institute in Salt Lake City". Questions which could be put to him include:

i. what proportion of the work carried out a G.E. on this topic was completed before he became Director of NCFI?

ii. to what extent was he personally involved in this work

iii. if he was not personally involved, then why is he a coauthor of the paper?

iv. if a large volume of work had been carried out, then why did he inform us (S.P. and M.F.) that virtually no work had been carried out?

v. was the outcome of the work carried out at G.E. During 1989 uniformly negative?

vi. if the outcome of the work was not uniformly negative, then how can he justify the content of the G.E. Paper?

vii. if the outcome of the work was uniformly negative, then how can he justify taking up the position of Director of NCFI?

viii. much of the work performed at G.E. during 1989 (such as it was) was carried out by Steve Spacil. Why is he not a coauthor of the paper and why is he not even acknowledged?

We could go on to say: as he is aware, the first drafts of two papers dealing with our work at NCFI were deposited there and at the Patent Attorney in September 1990; this material is contained also in our final report to NCFI which was not included in the printed version; as he knows our agreement to join NCFI in October 1989 was conditional on our data gathered there being independently evaluated but this evaluation was not carried out at NCFI (the parts which are especially critical to the further progress of the work are the data used in the two papers deposited at NCFI in September 1990); this evaluation has now been carried out(reference [7] of our paper) and published, was presented to G.E. We believe during the Spring of 1991 and was presented at the 2nd Annual Conference on Cold Fusion where he was present.

We therefore have the following additional questions:

ix. why were these evaluations, papers and presentations not referred to in the G.E. Paper?

x. which parts of the G.E. Paper does he believe can survive independent scrutiny bearing in mind that these evaluations, papers and presentation were prepared and made prior to the submission of their paper?

We also have the following additional question which we regard as being crucially important although perhaps not directly relevant to his contribution:

x. bearing in mind the uncertainty about the extent, dates, evaluation and interpretation of the work carried out at G.E. does he not agree that preferable all of the prime data gathered there together with all relevant dates and description should be deposited with an independent group (or at least that a representative sample of the work carried out should be so deposited) so that their own measurements can be subjected to an Independent scrutiny and evaluation?

I believe that it is important to draw Fritz into an open-ended exchange. He will probably not reply (the letter should be sent by recorded delivery via Pat Shea) and he should be given a time limit for his reply. Your letter should therefore be written in such a way that we can publish it and should also contain the relevant background information: especially that we were initially denied access to the paper (please put in the episodes with the keys) and that we are still awaiting the text of the final version of the G.E. Paper as accepted for publication and have had to prepare our own paper using the version submitted by them in July 1991 as our basis.

You will see that my main aim is to cut across Douglas Morrison's Section 1 using this correspondence with Fritz will.

Any other ideas?

In writing our reply to Douglas Morrison you might quote his quotes: "Because of the paucity of experimental details in their publication, it has been difficult to determine

quantitatively the effect of calibration errors" e.g. "they have not reported cell temperatures or calibration power" so that pictorial data have been used.

In commenting on this you might say that the authors of the G.E. Paper in fact used our reported temperature-time and cell potential-time data and were well aware of the magnitude of the heater calibration pulse we used at that time (Stan: you will understand why we have to be so absolutely certain about this). A considerable proportion of our paper was devoted to the question of systematic and random errors in the heat transfer coefficients and the accuracy of heat recovery for known inputs. These matters are not referred to in the G. E. paper, nor is that all important Appendix 4 which shows quite clearly what the connections between the different methods of data treatment must be.

You should stress that it is astonishing that bearing in mind the strictures of the authors, there are absolutely no raw data in their paper.

I think that we should write this section (and all the other) in such a way so as to make it clear that we are taking up Douglas Morrison's commentary, not the G.E. paper per se.

There are other matters which have to be taken up and I am sure you both will wish to raise other issues – please list them because, as of now, I believe that I should deal with these in the final Olympian Section.

<u>Section 2</u> will deal with Douglas Morrison's comments on the neutron measurements in the Kamiokande detector. In many ways the ideal person to do this is Hideo Ikegami and I believe that he is really obliged to do so since he spoke at Frascati about this topic.

I wrote to Hideo in late March asking for an update and, as far as I can tell, all of Douglas Morrison's account is wrong or misleading.

If Hideo will not do this then I would be in favour of asking Howard Menlove; failing this Tulio, but I presume that he would have to seek out the relevant information. Whoever does this Section, they should point out that Steve Jones' original result was for a measurement on a single electrode as indeed was ours. If Hideo (or another Japanese) writes this section, they might point out that some astonishment has been expressed in Japan that it was Steve Jones who was asked to prepare this particular electrochemical experiment. It is especially important to contrast Douglas Morrison's statement about count rates with the prime information from the experiment: the distribution of the neutrons in plan and section around the experiment.

<u>Section 3</u> Since this section is supposedly about the work at SRI, we should ask Mike McKubre to comment.

Irrespective of who writes this section, I would be strongly in favour of asking the question: if you read Douglas Morrison's account of the experiments, then can you tell what was done and what the results might be? I certainly cannot.

Instead we have a description muddled up with a reference to the papers by Kreysa, Marx and Plieth and the group at Harwell, the former described as a "thorough experiment". We have a statement attributed to Andy Riley which is intended to throw doubt on the measurements at

SRI (actually this technique has been used since the 1920's) and unattributable statements to research carried out in Harvard at Los Alamos. A description of an experiment should be just that: critique should be contained in a well-defined and separate editorial section.

Section 4 I would suggest that Melvin Miles should comment on 4.1 and he might as well go on to 4.2 as well. Perhaps we would like to question what the "fatal problems with the calibration procedures" might have been in a system which has been so "beautifully instrumented".

I believe that Takahashi could comment on Section 4.3 but Mel could do this equally well. If Mel were to do it he might like to point out that Douglas Morrison doesn't understand the difference between isoperibolic and flow calorimetry. Flow calorimeters are subject to many objections, but not those contained in the G.E. Paper. Another point: Douglas Morrison evidently believes that one can obtain a better heat insulator than vacuum. Come to that, he also doesn't understand that we have two controlled heat flows in series.

Tulio should comment on Section 4.4 nobody on Sections 4.5 and 4.6?

Section 5 I am A.N. Other [57] should state that articles which are sent out on the bit net should have proper references. These would certainly be regarded as being more useful than Douglas Morrison's comment on the dust jacket of the Como Conference Proceedings.

We should then give a list of books with full details. Perhaps it would be in order to add an editorial comment (or rather an editorial question): how can it come about that different authors working with the same set of facts come to so radically different conclusions? We can only recommend that interested readers consult two texts in parallel (say Eugene Mallove and Frank Close) but certainly also read a cross-section of the papers in the Como Conference Proceedings as well as some scientific reviews (list these). We should congratulate him for exchanging information with Gary Taubes notwithstanding the edicts at CERN and suggest that Taubes book when it finally appears should be read in conjunction with his book on Carlo Rubbia.

Section 6 I believe that Tulio should comment on this section. I am intrigued by the fact that a meeting organised by the Turin section of INFN can be described a semi-secret.

When I spoke to you, Giuliano, you said that Tulio had some comments on Douglas Morrison's seminar at Turin. What I find particularly disturbing is that he is evidently going around lecturing about the G.E. Paper before this has been published.

Section 7 I would like to add a Section on "Sins of Omission and Hallmarks of Excellence" which I believe should be written by Melvin Miles.

In the first place we should point out that the negative attitudes which have developed to the subject have been based on a small number of papers to wit:

The papers from Caltech, MIT, Harwell and that by Kreysa, Marx and Plieth. Douglas Morrison has frequently referred to these as being hallmarks of excellence. However, the papers

[57] JR "A.N. Others" is British slang meaning "various other people." From the Cambridge Advanced Learner's Dictionary & Thesaurus, Cambridge U.

from Caltech and MIT seem to have dropped from his view and we have to enquire about this sin of omission?

In preparation for this onslaught someone should write to Nate Lewis, the Parker/Bellinger/Wrighton combination, David Williams and to Kreysa with a carefully itemized list of question along the lines of my proposed letter to Fritz Will. We should list the principal objections which have been made about their work and ask them whether they still stand by the conclusions they have drawn from their results or whether they would like to modify their conclusions in any way?

In our commentary on Douglas Morrison's contribution, we should point out that we are frequently asked whether one can judge a 'negative' paper to be conclusive or inadequate and reply that such a judgement can certainly be made in terms of the ambiguity of the conclusions in relation to the experimental results. Thus Mel has attempted to publish a comment on Nate Lewis' papers (attached as an Appendix) but this has been rejected by both Nature and Science. The same ambiguity applies to the Harwell work (this is all to do with the calorimetry but the same applies to the rest of the measurements). The MIT work can more reasonably be interpreted in terms of excess enthalpy generation for Pd cathodes polarised in D_2O (ditto the Caltech work) but there is great uncertainty about the way the published data were derived from the raw information. Parker now tries to disown the work.

We can then list the replies (if any). If there are none, then we can publish out letter? We can address a further question to Douglas Morrison: why does he no longer cite these papers for their excellence? However, his main sin of omission is that he did not cite Wilford Hansen's review in making his commentary on the G.E. Paper. If he had done so roughly one half of the paper would have been seen to be invalid while the remainder can be seen to be invalid in terms of Appendix 4 of our original paper.

It may well be that Mel will not want to write this section but I believe that he must certainly be involved in its preparation. Perhaps it could be submitted by Gozzi? As of now, I believe that Giuliano should not be involved (i.e. not be named but he certainly must join in the donkey work) because we haven't yet got to the questions of theory. However, perhaps we should add a Section on Lack of Understanding? As you can see, I am presently in favour of restricting this document to issues raised by Douglas Morrison.

<u>Section 8</u> This should be the concluding section and could be another Olympian statement by myself.

The opening gambit could be that Stan and I welcome the publication of the G.E. Paper as long as this is read in combination with our own commentary. It is only by the exchange of properly researched critiques in peer-reviewed journal that the subject can advance. We believe that it is unhelpful to classify all scientists who obtain positive results as True Believers and those who get negative results as Skeptics. In truth we all just obtain results and we must all be skeptics.

By the same token, we are utterly opposed to the unreasoned and unreasonable rejection of critiques of papers which have appeared in learned journals by the editors such as applies to the letter sent by Melvin Miles to Dr. Maddox of Nature regarding aspects of the work of the group at Caltech and rejection by Science of a critique by Edward Storms of an editorial article by Gary Tables in that journal is another example.

We are also utterly opposed to the premature publication of comments about papers and critiques ahead of their publication dates especially when these do not take any account of countervailing arguments. The comments by Robert Park of the American Physical Society and of Douglas Morrison in his Cold Fusion Update No. 6 afford excellent examples of the introduction of bias. We observe that Douglas Morrison did not even extend the courtesy to us to ask whether we had prepared a further paper on the subject let alone ask us for comments on the accuracy of his statements.

One must ask what is the purpose of this negative publicity? Surely, it is completely atypical of the pursuit of Science? The normal pattern is to let a subject be worked out without excessive criticism: it either becomes established or is found to be founded on error (although it frequently does not become established during the first round of research). This is not the situation for the case of Cold Fusion and there can be no doubt that we are in the midst of another attempt to start a negative press campaign. How else are we to interpret headlines such as "Japan, Cold Fusion and Lyndon La Rouche" in the Scientific American of May 1992! (see Appendix 2). And how, pray, doesn't it come about that Douglas Morrison also tries to associate Cold Fusion with Lyndon La Rouche? Is this a matter of pure coincidence? And, pray, does it come about that the Japanese March Edition of the Scientific American contained an article on Professor Takahashi's experiments which was denied to English speaking readers? (for English translation by Jed Rothwell see Appendix 3). Do you believe that this is an acceptable code of conduct?

It is certainly true that the 21st Century was the only English language journal to send reporters to the 2nd Annual Conference on cold Fusion held in Como last year. We leave it to you to decide whether the Fall Issue is a better or worse source of information about the subject than are the Scientific American, Science, Nature, or, indeed, Douglas Morrison's Cold Fusion Updates.

In his latest Cold Fusion Update (No. 6) Douglas Morrison accuses "True Believers" of using the media to propagate their views. Douglas, what have you been doing during the last three years? Do you regard the Bit net as lying outside the province of media communication?

One matter is absolutely certain and that is that the critique of both "positive" and "negative" papers should be carried out in the normal peer reviewed scientific literature so that a sense of calm and proportion can be restored to this subject. It has appeared to us ever since 1989 that an essential prerequisite for this debate is that independent evaluations of data gathered by different groups should be carried out (it is not generally known that a precondition of our own continuation of work on this topic after October 1989 was that there should be such an independent evaluation of our data; the first results of this are to be seen in reference [7]).

So come on researchers all, be you positive or negative; decide on a repository of your data and exact descriptions of your experiments and decide on a mechanism for these independent evaluations.

And come on Douglas: write some papers for peer-reviewed Journals.

All of us who have commented and expressed opinions in this message hope that it is indeed the first and last time that we do so.

So these are my present thoughts. If we are going to do this, then let us do it properly.

Yours,

Martin

P.S. To Giuliano. Could you please arrange to send a copy of the conference proceedings to Dr. M. Mellich, 1224 Meigs Drive, Niceville, Florida 32578-3018, U.S.A. Is it correct that copies were to be sent to all the participants? Even if not, it is important that he should get a copy – I will pay for it if it is required.

[This is Appendix 3. Rothwell translation of the *Nikkei Science* article]

March 9, 1992

The following article appeared in the Japanese edition of *Scientific American.* The title of this magazine sometimes causes confusion; the cover of says "Scientific American" in English and "Nikkei Science" in Japanese. This magazine includes every article from the U.S. edition, translated into Japanese, plus some material unique to Japan. The March 1992 edition carried this two-page description of Takahashi's work on pages 54 and 55, in the "Information" section, which covers current events and fast breaking news.

Information

Cold fusion experiment yields stable heat reaction

"It's a real reaction." "No, it is all experimental error" - the debate about cold fusion goes on. Now, Prof. Akito Takahashi, of the Osaka University Engineering Department has successfully produced a stable cold fusion heat reaction that continued for over a month. Using the now familiar method of electrolysizing heavy water with a hydrogen-absorbing palladium cathode, he reports measuring peak heat outputs several dozen times larger than the electrical input.

Prof. Takahashi's electrolysis device consists of a cathode made of a highly pure palladium plate 2.5 cm square by 1 mm thick; the anode is 0.5 mm platinum wire wrapped in a coil one centimeter from the face of the cathode. Both electrodes are submerged in heavy water.

The current flowing between the electrodes is cycled every six hours. During the first six hours, 0.25 A (low current) is input; during the next six hour segment 4.2 A (high current) is input, and then the cycle is repeated. The experiment began last year on December 15; the heat reaction began about a week into the experiment. The reaction continues as of this writing, at the beginning of February. A peculiar phenomenon has also been noted; the heat output fluctuates in a periodic fashion with each cycle.

The strength of the heat was 50 - 70 W during the low current input, and 200 - 250 W during high current input. During the low period, the output heat energy was several dozen times greater than the electric energy used in electrolysis; during high input, it was 2 to 3 times greater. Subtracting input energy used in electrolysis from the output heat energy left a positive balance averaging about 100 W. Total energy output for the first month was said to exceed 200 megajoules.

The power density was extremely large; depending on how you measure it, power density per cubic centimeter of the palladium cathode was as much as ten times greater than the power density of a fission reactor fuel rod.

A 4 Body Reaction?

Prof. Takahashi proposes a new theory to explain the experimental results, which indicate that a nuclear reaction is occurring in the palladium lattice. According to his theory, 3 and 4 body fusion reactions occur inside the lattice, even though, under the normal density of deuterons packed into the lattice such reactions would be exceedingly rare.

A palladium lattice is a 6 sided face-centered cubic structure. A palladium atom sits in the center of each face, and at each vertex. With this structure, the area between each vertex and the area in the very center of the lattice is called the O site. As deuterons are forced into the lattice by electrolysis they occupy the O sites first. Each cube also contains 8 T sites; when all the O sites are full, the deuterons begin to occupy the T sites. When the deuterons occupy the T sites, they create deeper potential wells than with the O sites.

At this stage, when the deuterons undergo vibrational excitation from the electric current the deuterons in the O site, at a certain probability level, the deuterons begin falling into the T sites. With this lattice structure, there are four O sites around each T site, so if the deuterons in the O sites fall smoothly into the T sites, a maximum of 5 deuterons can concentrate in each T site. Takahashi postulates that in the instant this happens, the deuterons undergo fusion.

A great deal of supporting evidence

According to the proposed theory, the O sites fill up with deuterons when the loading ratio of deuterons to palladium atoms exceeds 0.85. At this point, the deuterons begin entering the T sites, and fusion begins. It has been noted that tritium, which is thought to be a fusion product, starts to be generated after the deuterons pass this level of saturation. This was seen again in the present experiment.

Also, from experiments at SRI in the U.S., and elsewhere, it has been observed that excess heat appears in many cases when the level of saturation goes over 0.9. Takahashi's theory explains this, conjecturing that at the higher loading ratio, more of the 3 and 4 body reactions begin, which output more heat than the tritium producing reactions.

Asst. Prof. Tadahiko Mizuno, of Hokkaido University, Nuclear Engineering Dept., has verified that under electrolysis, the palladium lattice has been loaded with deuterons at a ratio as high as 1.4. He has that verified that even after all O sites in the lattice are filled, additional deuterons can be forced in. Moreover, a diffraction image of a highly loaded palladium lattice shows that the all T sites are occupied, which tends to support Prof. Takahashi's theory.

In the present experiment tritium as well as neutrons were detected; both are considered evidence of a nuclear reaction. A liquid scintillator was used to detect neutrons; this is considered the most accurate instrument for this purpose. A helium 3 neutron detector was employed to cross check this finding. Observations showed, for the first time, that when the heat increased, the number of neutrons tended to decrease. Prof. Takahashi conjectures

that, “this may be due to increased 4 body reactions, which are both aneutronic and highly energetic.”

Replication attempts started

Up until now, many researchers have experimented without achieving stable heat output even though they were using same regime and palladium cathodes. Prof. Takahashi says “I suspect that the shape of the cathodes used previously was not appropriate.” According to Takahashi’s proposed theory, the deuterons loading rate across the palladium must be even, or fusion will not occur. In order to give rise to this condition, deuterons must be forced into all parts of the palladium surface. As Takahashi sees it, in most previous experiments of this type, loading was uneven, and the deuterons escaped from some areas of the palladium surface.

In any case, the only way to verify that Takahashi’s method has truly succeeded in producing a stable cold fusion reaction is to wait for replication from other labs. Already some of the other researchers who attended the January 27 “International Symposium on Nonlinear Phenomena in Electromagnetic Fields” in Nagoya have begun attempting replication. Groups are working at Hokkaido Univ., Tokyo Institute of Technology, and elsewhere.

Prof. Takahashi reports that several teams from the U.S. have also begun attempts at replication. It is said that some overly anxious foreign researchers fear that “the Japanese Government might begin targeted support to dominate the field.”

By J. Takaki, Nihon Keizai Shimbun, Osaka Branch, editorial staff member

Translated by Jed Rothwell

1992-04-24

University of Southampton heading

Dr. Melvin H. Miles,
Chemistry Division,
Naval Weapons Center,
China Lake,
Code 3853
CA 9355
U.S.A.

MF/KJW

24 April 1992

Dear Mel,

Thank you for your Fax of the 23rd April and your very interesting comments on the paper from the group at Harwell. Thank you also for your letter of the 6th April which I have just received through the normal mail. I will write to you further in some detail about the Harwell work, probably during the coming weekend.

On going through my file I see that I have not Faxed you my letter of the 16th April so I am doing this now. I also see that my file is in some disarray and I now do not know whether I sent you my letter of the 14th April with the relevant figures. If you did not receive this letter on the 14th April then could you perhaps let me know when you next write.

As you will have gathered, I feel that there has to be some review of the published work and I agree with you that it is the paper from Caltech, MIT, and Harwell (as well as yours and ours) which should be so reviewed. A paper which you might also include in this list is that by Kreysa, Marx and Plieth. Although this is probably the worst of the bunch it did have considerable influence in creating the negative atmosphere and it is still referred to as being "an outstanding piece of work".

One possibility for publishing this is as you say Fusion Technology. A further possibility is in the Conference Proceedings of the Nagoya meeting and I will also explore with Roger Parsons whether he would accept a critique for the Journal of Electroanalytical Chemistry. However, all in all I now favour an approach to Heinz Gerischer and Charles Tobias, namely for a review to be published in the renewed Advances in Electrochemistry and Electrochemical Engineering. As you know, Heinze Gerischer has somewhat shifted his position and they might now be receptive to such a provocative review.

Giuliano Preparata, Stan Pons and I have been considering a further possibility and that is to put out a compendium response on the Bitnet to Douglas Morrison's latest "Cold Fusion Update No. 6". In order for you to get some flavour of what we have in mind, I am now sending you my

latest letter to Giuliano and Stan: as you see, we have it in mind that you will play a key role and it would certainly be helpful if you could send me your comments on this letter. Let me explain to you that Giuliano Preparata does not know that we are in touch and just in case I did not send you my letter of the 14th April, it is our view that we should keep the interaction Mel ↔ Mike ↔ Martin ↔ Stan confidential at this time.

I am dictating this letter in somewhat of a hurry but I do recall that you mentioned somewhere that it would be useful to involve Steve Crouch-Baker, Wilford Hansen and Richard Oriani. These are splendid people but perhaps we should start with the rather limited cooperation which I have outlined to you. I would suggest that when we have got some way with this, we cold than consider, possibly with Mike Melich, whether we should proceed.

As I am dictating this, it comes to mind that we should probably proceed with all the possibilities I have outlined to you. I am sure it is necessary to try to develop some momentum.

Regards,

Yours,

Martin

P.S. The latest reply you received from Science is much as I expected!

1992-05-05

University of Southampton heading

Dr. Melvin H. Miles,
Chemistry Division,
Naval Weapons Center,
China Lake,
Code 3853
CA 9355
U.S.A.

MF/KJW

5 May 1992

Dear Mel,

As I told you in my letter of the 24th of April I had intended to reply to your Fax of the 23rd about the the paper from the group at Harwell. Unfortunately, because of the usual overload of work I have not been able to deal with this before now and I doubt whether I will get to this point before I have to go to France this coming Sunday, 10th May. I will therefore write to you from there.

It is likely that I shall spend most of the next two months in France so it would probably be best if you were to use our Fax number down there during this time. The number is 33 93 95 82 25. There are very few people who have this number (Mike Melich is one) so may I ask you to keep it secret for the time being. The Fax messages at home are normally read either by my wife or a close friend of ours and forwarded to us but it so happens that this friend is coming with us to France so our home number will not be serviced during this period.

I shall also shortly be taking up again the question of further action with regard to Douglas Morrison's Cold Fusion Update No.6. I am sure that we will be sending you a number of letters about this shortly. When I get to France we shall also started a further major revision of a commentary on our past calorimetric work. I imagine that this will prove to be useful to whosoever may wish to write a critique on the published papers.

Best wishes.

Yours sincerely,

Martin

P.S. The documentation on the paper from the group at MIT was sent to me by various people although I believe that most of it is really relevant to the position and actions of Eugene Mallove. I shall write to him on Friday to list the documents which I referred to in my letter to you of the 21st April. I could ask him either to send them directly to you or else ask him whether I can Fax

them myself to "an interested and responsible person". Could you let me know which of these actions you would prefer – you may not wish him to know of the extent of your interests. As you see, I am very careful!

P.P.S. On checking through the correspondence, I see that you really do need some comments on the calorimetry as a matter of urgency and this will be the first matter I will deal with when I get to France.

P.P.P.S. Herewith Douglas Morrison's Cold Fusion Update No. 6.

Date: Wed, 8 Apr 92 15:07:09 +0200

From: morrison%vxprix.cern.ch@BITNET.CC.CMU.EDU

Subject: Cold Fusion Update No. 6.

To: vincent.cate%sam.cs.cmu.edu@MINT.decnet.cern.ch

Dear Colleagues, 5 April 1992.

COLD FUSION UPDATE No. 6

CRITICISM OF PONS AND FLEISCHMANN'S EXPERIMENTS "DEVASTATING"

JONES RESULTS DECONFIRMED.

PONS, FLEISCHMANN. PREPARATA, BRESSANI, AND DEL GIUDICE SUE FOR 8 BILLION LIRE.

SUMMARY

A group at the General Electric Company, including Fritz Will, have examined the Pons and Fleischmann's analysis and found major errors which make their claims of Cold Fusion unacceptable. Robert Park of the APS describes the GE paper as "devastating".

Steve Jones and Howard Menlove have tried to repeat their experiments claiming small yields of neutrons in the giant Japanese Kamiokande detector and after many months of trying have failed to reproduce their early results.

One asks, with the two experiments that excited world-wide attention in March 1989 now gone, can Cold Fusion survive?

Since the last Cold Fusion Update No. 5 in July 1991 after the Second Annual Conference on Cold Fusion in Como, there has been no major experiment which observed excess heat and comparable amounts of fusion products. There have been a few claims of Cold Fusion effects but the excitement was each time not sustained.

The tragic death of Andy Riley at SRI greatly saddened all who knew him.

The Third Cold Fusion Conference is scheduled to be held in Japan in October 1992. There was a "semi-secret" Cold Fusion meeting in Turin in March.

Funding continues - Stan Pons together with Martin Fleischmann, are working at the Science Research Park near Nice for a Japanese supported company. It is reported that the Electrical Power Research Institute, EPRI, has given further money, $12 million, to the Stanford Research Institute, SRI, for continuing Cold Fusion work. Drs. Pons Fleischmann, Preparata, Bressani and del Giudice are taking legal action against the Repubblica newspaper and asking for 8000 million lire.

SUBJECTS

1. General Electric paper.

   1.1 Background and Introduction

   1.2 Title, Authors, Abstract

   1.3 Critique of Analysis of Fleischmann and Pons

   1.4 Experimental results

   1.5 Reaction of Fleischmann

   1.6 Conclusions

2. Kamiokande experiment for Jones and Menlove

   2.1 Background and Introduction

   2.2 Experimental results

   2.3 Conclusions

3. Andy Riley, SRI and EPRI

   3.1 Introduction

3.2 Andy Riley

3.3 Experiments at SRI

4. Other Experiments

4.1 Helium measurements of Bush et al.

4.2 Mills et al. and Tom Droege

4.3 Takahashi, Osaka

4.4 Bressani et al.

4.5 Cold Fusion in China

4.6 Withdrawal of Cluster Fusion Result

5. Books

6. Meetings, Press Conferences, Legal Actions

7. Conclusions.

1. GENERAL ELECTRIC PAPER

1.1 INTRODUCTION

After the 23 March 1989 press conference, General Electric, like many power companies, signed confidential agreements with the University of Utah. Also then sent people to work with Dr. Pons in Utah and at the same time, and completely independently, started working on Cold Fusion experiments at their Research and Development labs at Schenectady. It is the work of this latter group that will be published this spring in the Journal of Electroanalytic Chemistry. However the major part of their paper is a very detailed consideration of the Fleischmann and Pons work where they find many errors, several of which are serious. The GE experimental work was done on a large scale within the first year, but for a variety of reasons has not been generally available. I learnt of major errors in the Fleischmann and Pons analysis over a year ago and these have been presented to them. Publication was delayed appreciably and the final paper is carefully written. Here the rather kindly abstract will be given and then the

main evidence will be presented so that everyone may judge. The authors include former GE researcher Fritz Will who after the experimental work was essentially completed, became the Director of the National Cold Fusion Institute in Salt Lake City.

1.2 TITLE, AUTHORS AND ABSTRACT

"Analysis of Experiments on Calorimetry of LiOD/D_2O Electrochemical Cells

R.H. Wilson, J.W. Bray, P.G. Kosky, H.B. Vakil and F.G. Will".

Abstract

"In this paper we present a detailed analysis of calorimetry with heavy water electrolytic cells, especially of the type described by Fleischmann Pons et al. in recent publications. We also summarise our own experiments, which involve calorimetry of electrolytic cells of various designs. None of our experiments has yielded any excess heat or radiation products within the detection limits. We evaluate the data and methods of Fleischmann, Pons et al. and, where sufficient data are available, conclude that they significantly over-estimate the excess heat. This is in part because they did not include in their calibration calculation the change in input electrochemical power to the cell resulting from the calibration heater power. An additional significant overestimate of excess energy occurs when the calibration is made at cell temperatures above 60 C, due to the increased evaporation of heavy water during the calibration. Furthermore we find unexplainable inconsistencies in the data on light water controls as reported by Fleischmann and Pons. While our analysis shows their claims of continuous heat generation to be significantly overstated, we cannot prove that no excess heat has been generated in any experiment".

1.3 Analysis of Fleischmann and Pons

A detailed discussion of the open cell used by Fleischmann and Pons is first given and it is pointed out that several terms are not properly accounted for but fortunately do not lead to significant errors. Other potential errors such as inadequate mixing and recombination are believed not to be significant. However the heat loss calibration procedure does lead to important errors, this being established partly theoretically and by experiments with cells similar to those of F&P, thus the heat loss is found to be half by radiation and half by conduction whereas F&P now treat all heat losses as radiative(in their first paper they treated all heat losses as conductive - Newton's law of Cooling). Two calibration procedures are used by F&P, firstly "approximative" and the second a very complicated multiparameter regression analysis which is said to give "exact" excess energies. The calibration depends on giving a brief additional burst of heat to the cell. This temperature change is not taken into account in the first procedure and it is shown that this error substantially reduces the excess heat claimed. Since the second method is claimed by F&P to agree very closely (few milliwatt) with the incorrect first analysis, hence there must be

error(s) in the second analysis. A possible error in the second calculation which would account for this is identified.

When a correct calibration procedure is used, the excess heat claimed is significantly reduced. “Because of the paucity of experimental details in their publications, it has been difficult to determine quantitatively the effect of calibration errors” eg “they have not reported cell temperatures or calibration power” so that pictorial data have been used instead.

Several effects have been neglected by F&P, two of which are important - the reduction in resistance when the cell is heated by the calibration heater and secondly the evaporative cooling of the electrolyte important at higher operating temperatures and which is increased by the calibration heater. The magnitude of the errors caused by these neglects is such that “in some cases the errors are greater than their inferred ‘excess heat’” and “in some some instances excess heat remained after correcting for these errors.”

“The control experiments reported by F&P also pose a dilemma. Using their approximate method to calculate excess heats, they find no excess heat within a few milliwatts. If, however they used the procedures they describe for determining excess heat, they should have obtained significant positive values as a result of neglecting the effects described above. The results they report are inconsistent with the procedures they describe.” Further embarrassing problems are also indicated.

1.4 Experimental Results

A very extensive series of experiments were performed. In one set the cells and procedures of F&P were followed - no excess heat was found. Many small variations (eg different types of palladium and different shapes, different electrolyte) were also tried and also major changes such as thermal insulation to avoid radiation effects, and closed cells with recombination catalysts and a flow cell. The current was varied between a few milliamps to 0.5 amps per cm^2. The length of time was varied. “Within experimental error, no excess energy was found.”

“A few experiments were carefully monitored for gamma ray and neutron production” using good techniques (particularly liked the use of Manganese nitrate solution where the 55Mn captures a neutron to give 56Mn which decays with a half-life of 154 minutes giving a gamma of 0.847 MeV - this is an energy region with little background- this is useful for integration of neutron signals; other detectors were used for direct neutron detection). Activation foils were also used. “Nothing was found above background”. “Many of the electrolytes were checked for tritium build-up. No increase above concentration by electrolysis was found”. “Nor was the concentration of ^{4}He in the Pd rods found to be above background.”

1.5 Reaction of Fleischmann

Business Week of March 2nd reported that “there’s bad news ahead for cold fusion” and then talked about the conclusions of the GE paper - the reporter did not seem to have read the paper himself. The article continued that Pons and Fleischmann asked the journal to let them write a rebuttel - but they have not done so yet. Fleischmann is quoted as saying “Those people have got mental constipation about this thing”. Hope this is not a correct quotation as have always found Martin a charming person, but have found that some people who have no answer to scientific evidence against their work, do react this way. Sad, it would have been pleasanter to see a scientific reply.

1.6 Conclusions

There are two parts to the GE paper. Their experimental results are very extensive, some of them copying the P&F experiments, others are superior; all give no evidence for excess heat or for fusion products. It might be thought that this should be enough convince even True Believers that there is nothing there, but this has already happened with the Harwell experiments led by David Williams who was helped by Fleischmann before the 23 March 1989 press conference, and whose group also did a very large number of experiments, some the same as P&F and others better - and TB’s ignore or discount this work.

However the main thrust of the GE paper is to show that the analysis of the calorimetry had many errors some were so serious that when the P&F data were corrected the excess heat claimed became sometimes a negative effect and sometimes a positive effect so that the conclusion was that one cannot trust the results.

In the Abstract it is written that “we cannot prove that no excess heat has been generated in any experiments”. This statement, which unintentionally, has some legal use, covers the fact that the paper was concerned with the main claim of Cold Fusion, that a steady source of power was possible. The GE paper does not discuss the question of heat bursts. Thus the real question is; “Can the P&F experiments be considered to give trustworthy evidence in favour of the existence of Cold Fusion as a steady source of power?” The GE analysis shows that the P&F work is so full of errors that it is not clear whether they found a positive or a negative effect as is shown clearly in their table 2. In other words the uncertainties are so great that the P&F work cannot be used as a justification for the existence of Cold Fusion.

There is also the embarrassing matter that the control experiments which were said to show no effect, should have shown an effect if they had been analysed the way that P&F said they had analysed them.

The GE authors say that is in principle possible to obtain results on excess heat with the Pons and Fleischmann type cells, but it is complicated and needs to be done properly, which was not the case. Many times it has been suggested that Drs. Pons and Fleischmann do a good experiment with a closed cell and several constant temperature baths surrounding the cell for then the corrections become fewer and small. Also they should much use bigger cells so that the

effects are clear - but they have only reported results from the original small cells which gave excess heat with the errors in calculation.

The GE paper is not a light paper, the appendixes contain very detailed work. The GE authors are major experienced researchers in this field. It is surprising that no response has been made since Drs. Pons and Fleischmann have been aquainted with these difficulties and have been in possession of the GE paper for some considerable time.

The overall conclusion must be that there is no good evidence for useful excess heat or fusion products in the Fleischmann and Pons experiments.

2. KAMIOKANDE EXPERIMENTS OF JONES AND MENLOVE

2.1 Background and Introduction

The Kamiokande detector is a tank of 3000 tons of very pure water in whose walls are many photomultipliers which can detect Cherenkov radiation produced by electrons. The experimental team is large and well-funded. They have done outstandingly good work in neutrino detection. They detected (along with the IMB detector) neutrinos from Supernova 1987A. They have also detected neutrinos from the Sun and have shown that there is no variation with time (in particular not with the inverse of the sunspot number as had been surprisingly claimed by another experiment). Also their measured flux of solar neutrinos is in agreement with Evolutionary model(SSM) calculations of the Saclay group though some other SSM predict higher neutrino fluxes. Thus their experiment is playing a major role in the important question of whether there is a solar neutrino problem or not. The question is important as the solar neutrino problem is the only major result where there may be disagreement with the Standard Model of particle physics.

The Kamiokande detector was off for a year and half to improve and maintain the detector. During this time an installation was made in the centre of the Kamiokande detector where Cold Fusion cells could be installed and surrounded by a sodium chloride solution. If any neutrons were given off by the Cold Fusion cells they would be detected by capture by the 35Cl giving off energetic gammas producing electrons which the photomultipliers would detect by their Cherenkov rings. The system has been calibrated using a 252Cf source and the efficiency for neutron detection is about 20%.

The Kamiokande detector is so big that the Cold Fusion work does not seriously interfere with the Solar Neutrino and Supernova watch activities. For the period January to end May 1991, Kamiokande effectively ran for 99 days for neutrinos and 2 days for Cold Fusion (during neutrino running the Cold Fusion cells were normally running but not interfering with the main work of Kamiokande). It is expected that Cold Fusion measurement will continue until April 1992.

The emission of neutrons from Cold Fusion cells is highly controversial. Most workers did not find neutrons above background but a considerable number of claims were made, several of which have been withdrawn (eg the original claim of 40 000 neutrons/second of F&P). Of the positive claims, some of the lowest rates are from the original 1989 paper of Jones et al. in Nature. However other groups (eg Moshe Gai at Yale, the Frejus/Bugey group) obtain no neutron signal and give upper limits which are one to two order of magnitude lower.

Jones et al. claimed to measure in Run No. 6 a flux of neutrons of

Run No. 6 (4.1 +/- 0.8)*10**-3 n/s above background

Background (1.4 +/- 0.13)*10**-3 n/s.

Later this rate was lowered by averaging over the other runs where no significant effect had been observed and this gives

Average (0.62 +/- 0.1)*10**-3 n/s above background

that is an average which is less than half the background.

As the efficiency was only (1 +/- 0.3)%, this meant that the corrected counting rate was about 0.1 neutrons/second. Kamiokande which has a detector efficiency of about 20%, proposed that with their detector they could obtain a background 10 000 times less. The limit of their neutron sensitivity was expected to be 4*10**-5 n/s with a threshold energy of 7 MeV. Later Steve Jones joined with the Howard Menlove group at Los Alamos and discovered bursts of neutrons. In the summer of 1990 graphs were presented showing bursts of 20 to 149 neutrons observed being emitted in less than 128 microseconds. As the efficiency of the counters was between 21 and 34%, this means bursts of 100 neutrons or more were frequently being observed. Such bursts would be easily detected by the Kamiokande detector.

2.2 Experimental Results

The first experiments were said to be of gas and titanium, but with relative lack of success, and electrolytic cells were tried including some with the “mother earth” type recipe.

First results were presented by Dr. Ikegami at the Second Annual Cold Fusion Conference with a newspaper article claiming that Kamiokande had detected neutrons - this caused quite some excitement but is not included in the published proceedings. However it turned out that these were observed as “bursts” of 2, 3 or 4 neutrons (one neutron is excluded as a “burst”). Now if Uranium (or plutonium) were present as a contaminant in the cell (eg in the Palladium) then as the number of neutrons per fusion can be as large as 6 (or 7), this would account for the effect. The Kamiokande group have already taken enormous precautions to reduce the background from U or Pu and it is sited at a depth of 2700 mwe in the Kamioka mine and is surrounded by a shield of 6 to 7 metres of U-free water so that the background can be as low as one count per year. Quickly it was learnt that neutrons were also detected when H_2O and not D_2O was used which would appear to confirm that the bursts were not from fusion but from

contamination. This is contested by Steve Jones who feels that the data may be significant and one should wait. The prelimainary data that I have seen show that relatively little running was done with H_2O so that the statistics are not very significant.

However after this excitement, the main result tended to get lost that the counting rate was less than one-hundredth of that claimed in the 1989 paper of Jones et al. Thus after more than two years development work, and the insertion of many cells in Kamiokande, the original claim presented in 1989 cannot be justified.

A second main result is that large bursts (here taken as > 27 neutrons) as claimed in the 1990 paper of Menlove et al. cannot be justified as no large burst has been observed.

Steve Jones claims that small bursts (defined as 2 to 10 neutrons) are being observed and the rates are being studied. We shall have to wait and see if some new effect will be claimed at a much lower rate than the previous claims. No statement has been made about intermediate bursts (11 to 26 neutrons).

Have been exchanging many messages with Steve - he is genuinely anxious to find out the truth and discuss in a scientific manner (this does not mean he agrees with my conclusions - his position is unclear to me)

At present some surprising tests are being made using cells filled with concrete. The basic idea is the hope that this would represent in some way what is happening in the earth where Jones et al. claim fusion may be occurring. It is well known that concrete contains radioactive materials, in particular thorium. It is surprising that such an uncontrolled substance is being introduced into Kamiokande which makes such efforts to remove contamination. It is to be hoped that this does not interfere with Kamiokande's main mission to study neutrinos.

2.3 CONCLUSIONS

It must be concluded that the original work on low level neutron counting is not confirmed by a large margin due to the high quality and enormous size of the Kamiokande detector.

This is not the conclusion of Steve Jones who claims some possible effects and that more time and work is needed. What I have seen of these claims makes them look like statistical fluctuations combined with trials of a number of data selections, but there could be other data which has not yet been presented. However whether there are or are not such very low level effects, this does not change the two main conclusions that the level of neutrons observed in 1989 and the level of bursts claimed in 1990, have been disproved by the same experimenters working with numerous cells tested for long periods of time in the Kamiokande detector under favourable conditions.

3. ANDY RILEY, SRI AND EPRI

3.1 INTRODUCTION

The Stanford Research Institute, SRI, does research for agents that give it funding. Thus it is not an academic establishment with a commitment to making available all its results without the agreement of its funding organisations. The Electrical Power Research Institute, EPRI, is the agent of the power companies and has many activities. It has been sponsoring research in Cold Fusion and in particular has been giving appreciable funds to Mike McKubre's group at SRI. In Business Week of 2 March it is written that EPRI will give $3 million to SRI for this year and there is talk of $12 million over three years.

3.2 Andy Riley

Andy Riley was a materials scientist. He was employed by the National Cold Fusion Research Institute in Utah. It was there that I got to know and like him. He was not concerned about the reality or not of Cold Fusion, but was greatly interested in the materials research work that he could do. He was very knowledgable and it was a pleasure to learn from him. He had a great love of the desert and it was he who persuaded me to spend a weekend visiting the Southern Utah desert - he was right, it is splendid.

After NCFI closed down, Andy went to work at SRI. Newspaper reports quote firemen as saying that the explosion which killed Andy was due to the removal of a cell from its container as Andy had found an automatic pressure relief valve had stuck and was trying to open it manually. Happened to be in Palo Alto a week later for a seminar and phoned Mike McKubre who was one of those injured in the explosion. This was Mike's first day back at work and fortunately he was much better. He told me that SRI was now going to start its investigations and the conclusions might be different.

At no time did anyone suggest that the explosion had any connection with the existence or not of Cold Fusion.

It is interesting to recall that in 1989 among the thorough experiments of many German groups (all of which found nothing) was the work of Kreysa, Marx and Plieth of Frankfurt and Berlin who took a deuterium-loaded palladium sheet and placed it on a table where it burnt the table. The point is that considerable energy is stored in the palladium when the deuterium is driven in by electrolysis and when the Pd sheet is removed the energy raises the temperature of the palladium which then becomes an efficient catalyser for hydrogen and oxygen (in the air) to burn. This is the principle of the flameless catalytic combustion of hydrogen which is used in catalytic hydrogen burners. The experimenters then found that if after extracting a D-loaded Pd sheet and placing it on glass rods, a temperature rise of the palladium from 20 C to 418 C occurred within 74 seconds after an incubation time of 15 seconds. There are many reports of Cold Fusion cells exploding and everyone should be aware of the potential dangers and take precautions.

We all grieve for the loss of Andy Riley.

3.3 EXPERIMENTS AT SRI

The fact that EPRI is giving large grants to SRI and that they refer obliquely to results that justify this funding, raise interest in the work at SRI.

In an account of the 2nd Annual Conference on Cold Fusion, 2ACCF, in Update No. 5, it was written that "This was perhaps the most impressive positive result." This has now been published in pages 419 to 443 of "The Science of Cold Fusion" - the 2ACCF proceedings. Again it reads very impressively saying the positive excess heat occurs at high D/Pd loading, greater than one. the loading was measured by the axial resistivity and by volumetric displacement of gas during loading in a closed system at constant volume and pressure. Andy Riley once commented to me that axial resistivity was not a reliable measurement as there was also radial distortion that had to be taken into account.

It seems that the high loading was achieved by using "substantial current Densities (typically 300 to 600 mA/cm^2, but up to 6400 mA/cm^2) for considerable periods of time (typically 1000-2000 hours)" and also with high pressures from 40 to 10 000 psi above atmospheric pressure. The effect of high pressure alone has been tried at much higher values - at Harvard 105 000 atmospheres(1.6 million psi) gave a loading of 1.34 +/- 0.1 and at Los Alamos a megabar was achieved for 14 microseconds, but in both cases negligible numbers of neutrons were produced and at Harvard no excess heat was found with an upper limit of 1.6*10**-8 fusions/dd pair/second. It is of course obvious that very high pressures would not be suitable for confinement of a commercial fusion process as the strength of the walls would decrease with bombardment by fusion products.

The calorimetry used in the experiments was much superior to any other experiment that had claimed excess heat as it used closed cells and insulation and a surrounding isothermal bath. However as there was only one bath, there was needed a "effective conductive loss term, k'. The conductive power loss for the large calorimeters was typically 3 to 5% of the total input power. The accuracy claimed was the greater of 10mW or 0.1%.

While the calorimeters were greatly superior to previous ones giving positive results, they could still be substantially improved by following the Harwell design as used by David Williams. These had the following features; 1. The best measurements avoid corrections by trying to make null measurements as in the Wheatstone bridge. Thus the Harwell calorimeter kept the temperature of the inner isothermal bath constant by varying the input power which then compensated any excess heat produced

2. There were three constant temperature baths

3. Calibration was done by inserting a known source of heat into the calorimeter - this could be a calibrated alpha source.

In view of the substantial sums being invested by EPRI, it is to be hoped that these improvements will be tried.

The 2ACCF paper states that typical excess output power was 5 to 10% with a maximum of 28%. However could not find in the paper the value of the excess power integrated over the whole period of each experiment(which is the number of importance for commercial applications, but at 2ACCF, was told that it was between 1% and 2 to 3%. Such values are too low to be of commercial use which would require more like the values of 300 to 900% originally claimed in March 1989.

4. OTHER EXPERIMENTS

There have been fewer new experimental results published recently. Dieter Britz who now has a total of 688 papers and 96 patents/applications says it is now a "trickle". None of these recent papers are complete experiments or very convincing (apart from the GE complete work and the high quality Kamiokande experiment). However some have attracted attention and will be presented.

4.1 HELIUM MEASUREMENTS OF BUSH et al.

Drs. Bush and Lagowski of Univ. of Texas at Austin have been looking for helium in the electrolysis experiments of Drs. Miles and Ostrom at the Naval Weapons Center, China Lake, CA. They claim to find ^{4}He when excess heat is observed. John Huizenga has criticised the experiments saying the level is very low and helium contamination is a possible explanation - this is strongly denied by Bush et al. The outgoing gasses from the cell are collected for about an hour in a 0.5 litre vessel. It is said that amounts of < 10**12, 10**12, 10**13 and 10**14 atoms of 4helium are found corresponding to no peak, small, medium and large peaks in the spectrometer and there is a correlation with the amount of excess power - the values given for excess power were (0.07 and 0.29 W), (0.12 and 0.17 W), (0.24 and 0.40 W) and (0.22, 0.36, 0.46 and 0.52 W) resp. There is a correlation but it is a poor one and it would appear that the relationship was more of a power law than a linear one as might be expected. If all the reactions were d + d ---> ^{4}He + 23.8 MeV then it is claimed that the highest excess power would yield 5.4 10**14 atoms and "About 10**14 atoms were detected which is within experimental error of then theoretical amount." - this is the nearest one gets to an estimate of errors in the 2ACCF paper.

No ^{3}He was found - this is curious as experiments on dd fusion find that 10**7 times as much ^{3}He should be produced as ^{4}He.

The experiment looks very simple and it is to be hoped that the authors will continue it with better apparatus for a longer time since if their result were correct it should be easily possible to produce large ammounts of helium which would put the matter beyond doubt.

4.2 MILLS et al. and TOM DROEGE

For a while there was some excitement when Mills et al. announced significant excess heat and then Tom Droege with his very high quality calorimeter, confirmed it. But further work by Tom showed there were fatal problems with the calibration procedures or to be more precise an invalid correction. The calorimeter built by Tom is accurate down to near the milliwatt level and is beautifully instrumented as can be seen on pages 243 to 248 of "The Science of Cold Fusion". Tom has a very good reputation in Fermilab, for example for the work he did for the giant CDF detector which one hopes will be able to find the long sought after top quark.

4.3 TAKAHASHI et al., OSAKA

In February Dr. Ikegami gave a talk at Frascati which people who doubted Cold Fusion, found impressive. The highlight of the talk was a description of the results of the group of Dr. Takahashi at Osaka University. They have done four experiments and have presented results on neutrons, tritium and excess heat plus a theory that explains all results. In the early runs peaks in the neutron spectrum were observed at 2.45 MeV as expected from d + d ---> ^{3}He + n, and over the region 3 to 7 MeV which is not expected. In the fourth experiment excess heat of 200W per cc of Pd was observed and more than 100 MJ of heat produced. Tritium was also observed with a ratio n/t of 10**-5. The neutrons were observed at a rate of 1 to 100 n/s/cc. It was claimed that there was a correlation between neutron production and excess heat but the fig. 1 of the paper seems to prove the contrary, the highest neutron rate being near the lowest excess heat claimed. In another transparency it was noted that when the power increases the neutron production decreases - this is contrary to all previous experience of believers and non-believers.

The electrolysis cells have cooling water passing through them and the temperature of the incoming and outgoing water is measured and after calculation the excess heat is deduced. The cells are basically similar to those of Pons and Fleischmann in being open and poorly insulated. Hence all the comments and criticisms of the GE scientists would have to be considered before any claim could be evaluated. In the description available there is not enough detail to follow how all the calibration and heat loss calculations were done, so a serious account of the work, mentioning the GE considerations, is needed before the claims of excess heat could be justified.

A theory is proposed that explains all the results including the n/t ratio of 10**-5 and the n/f ratio of 10**-12 (where f is the rate of fusions expected in the reaction d + d ---> ^{3}He + n). The model assumes "multibody fusions" where not only do d-d fuse but also three and four deuterons, ie d-d-d and d-d-d-d. The four atom fusion is calculated to give a megaWatt per cc while ordinary water would yield a kiloWatt per cc because of the one atom in 6700 which is deuterium. Normal considerations of barrier penetration do not seem to have been considered in this theory which is liable to find few supporters even among True Believers - but one never knows.

4.4 BRESSANI ET AL.

T. Bressani et al. have carried out experiments to measure the energy of neutrons emitted from Ti metal loaded with gaseous deuterium ie following the ideas temperature cycling of Dr. Scaramuzzi. They use a neutron spectrometer with time of flight and double scattering technique. They report a two and a half standard deviation effect corresponding to 1.3 +/- 0.5 neutron per second per gram of Ti - this would correspond to about 10**-12 watts for the reaction d + d ---> ^{3}He + n, they do not report searching for ^{3}He.

The D2 gas pressure used by Bressani et al. was very low with a maximum value of only 1.5 10**3 Torr. They remark that the volume and pressure measurments give a D/Ti loading of only 0.32 which they comment is "totally inconsistent with the Ti-H phase diagram", but the low loading seems consistent with the low loadings for similar pressures used by Steve Jones and collaborators. It seems slightly inconsistent for Dr. Bressani to have employed such a low loading value when he strongly emphasises the necessity of high loadings to obtain the threshold value that is claimed to be necessary to observe Cold Fusion.

Many groups have reported not finding any neutrons when adopting the Scaramuzzi technique and some have upper limits less than one thousandth of the Scaramuzzi claim. As the early reports of Scaramuzzi claimed 5000 neutrons per second using 100 grams of Ti, Dr. T. Bressani must be congratulated for joining the experimental groups that have found different results from Dr. Scaramuzzi - it is surprising that he has not made a comparison of his result with that of the earlier paper which would have allowed him to make a comment himself.

4.5 COLD FUSION IN CHINA

While at the Pugwash Conference talked with Dr. Li who is the leader of the Chinese scientist working on Cold Fusion. He told me that there were very little funds available. The apparatus used is rather primitive and few results are emerging.

4.6 WITHDRAWAL OF CLUSTER FUSION RESULT

In an errata in Physical Review Letters (in press), the group at Brookhaven that had claimed to have observed fusion when using clusters of D_2O to bombard targets, have withdrawn their observation for technical reasons. The problem was light contaminant ions containing deuterium.

This puzzling result which could not be quite explained, was described as cold or lukewarm fusion and was not considered central to the debate about the existance or non-existance of Cold Fusion.

5. BOOKS

Hear that John Huizenga's long-awaited book on Cold Fusion has finally been published. It is an excellent serious book for those interested in Science (and psychology). A must for those involved in Cold Fusion as well as for others.

Gary Taubes phoned me about another subject (neutrinos). His book has unfortunately been delayed until later this year.

Saw Frank Close's book on the shelves in Geneva - good for you Frank!

The proceedings of the Second Annual Cold Fusion conference last June at the Villa Olmo in Como, have now appeared. The dust jacket is very tasteful with delicate images of Volta (a special hero in Como), a cold fusion cell with all the parts labelled, the original 1989 Jones et al. neutron result, Scaramuzzi's controversial plot, something that has a vague resemblence to a Feynman graph but I doubt if he would acknowledge it, etc. The main heavy writing on the cover which stands out well is

THE SCIENCE OF COLD FUSION

and the publishers title. There are 527 pages.

6. MEETINGS, PRESS CONFERENCES, LEGAL ACTIONS

There was a "semi-secret" meeting lasting three and a half days in Turin on Cold Fusion early in March. It was attended by about 40 people, mainly local but some foreigners such as Stan Pons also attended. Was told that there was not very much new. Although the press attended, it was not widely advertised - not even in the University of Turin!

The account of the Turin meeting in a newspaper of 17 March said that it was organised by the Turin section of the INFN and was called "Cold Fusion Three Years After". The participants were described as being under tension and prudent like the adepts of a secret society who are sure of their ultimate triumph. They were particularly encouraged by the new results from Osaka indicating excess heat of 100 Watts per cm^3.

At a press interview, Dr. Pons said there were no more doubts; he took a sabbatical from Utah to open a lab in Nice. There are about 10 people working there with important financial backing from Technova - a company that finances research for some Japanese industrial groups. They have developed a new type of electrolytic cell with Palladium to obtain a kilowatt per cc of electrode. Reproducibility has been attained at 100% and depends on a new type of Palladium alloy. He said that to understand the new phenomenon one must put aside classical fusion reactions in a vacuum and think of alternatives. Preparata's ideas can explain the results. Dr. Pons said the aim was to present to the public a practical application before the end of the year. He also said other labs had prototypes and quoted McKubre at SRI and Bressani at Turin. Prof. Bressani, who was introduced as leader of the Obelix experiment at CERN, Geneva, confirmed that his group had interesting effects from a cell with D2 gas and Titanium of the type proposed by Dr. Scaramuzzi, but said that it would need more time to construct a demonstration cell.

Heard about it only because I was invited to give a seminar at Turin reviewing Cold Fusion and some people were astonished when they were told about the meeting two weeks after it had occurred. The seminar was well received except that a True believer, TB, came forward at the end and made comments of a violence that astonished his colleagues. His essential point was that

I was biased as I had not mentioned recent work including his own, reporting evidence for Cold Fusion. Tried to explain that my talk was based on the one I gave at the Sakharov Conference last summer, consisted of three main parts, (1) a summary of ALL results, positive and null (with upper limits often lower than the positive values), (2) for the period since then, only good experiments with complete, careful calibration and controls, (3) understanding of the results in human terms. As Dr. Ikegami had recently given a lecture at Frascati which was written up in the newspapers on recent research on Cold Fusion in Japan and in particular had spent some time reporting on the results of Dr. Takahashi of Osaka, I showed in reply, 5 transparencies showing his results and explaining the problems that had led me to exclude it from the "good" experiments. The TB was not satisfied and continued in the same violent manner. Afterwards (too late) noticed that he had not questioned anything that was presented in the seminar and in particular not the "devasting" results of the GE analysis of Pons and Fleischmann's work. The seminar finally lasted longer than normally scheduled, two hours but it was observed to me that almost no one left before the end!

Have received an invitation from Dr. Ikegami who is head of one section of the Japanese National Fusion Institute in Nagoya, to attend the Third International Cold Fusion Conference which will be held in Nagoya from 21 to 25 October 1992. "The conference will cover the broadest topics relevant to Cold Fusion phenomena in the research fields including nuclear physics, electrochemistry, and solid state physics". As the organising body is the very serious National Institute for Fusion Science which has done excellent work on Inertial Confinement, etc., it may be expected that this will be a serious conference where the organisers will ensure that the conference will be balanced and that all points of view and both null and positive experiments will be reported and discussed. If these justified hopes are fulfilled, it could be a significant conference and it is to be hoped that many who have worked at some time or other on Cold Fusion will attend. Dr. Ikegami is chairman of the conference. The Fax address is 052 781 9564 and the Email address is

ikegami@nifs.ac.jp

When in Turin learnt that the important newspaper Repubblica is being sued by 5 True Believers for defamation. They are Martin Fleischmann, Stanley Pons Guiliano Preparata, Tullio Bressani, and Emilio del Giudice. This arose from an article in Repubblica where Cold Fusion was defined as a "truffa scientifica" which I am told means "scientific fraud". It appeared on the 20 October 1991. It was said that the results of Pons and Fleischmann cannot be reproduced in any other laboratory. Later another article based on a letter from Believers was entitled "No, we are not False Prophets", but the comment was apparently not withdrawn. Now the newspaper is being sued for a total of eight billion lire which is roughly five million US dollars - this is made up of 2 billion lires for P&F and one billion for Preparata, Bressani and del Giudice plus 200 million for each of the five for repeated violence.

Personally I am against such legal proceedings. It would be much better to wait until the end of this year and see the prototype of Dr. Pons actually giving a kilowatt per cc - preferably a big prototype with many cc giving many kilowatts such as Dr. Pons has been photographed with. Wonder if the Five and Repubblica have read the article in Nature of 19 March, Vol. 356, page

191 where the definition of scientific fraud is discussed. A US senior federal advisory committee has proposed that a strict-constructionist definition of fraud be adopted, namely "plagiarism, the fabrication or intentional falsification of data, research procedures or data analysis, or other deliberate misrepresentations in proposing, conducting, reporting or reviewing research". This report will go to the US Secretary of Health and Human Services, he is concerned mainly with health and biomedical agencies.

POST-SCRIPT

On the SCI_FUSION net, some of the above stories from a press conference in Turin, were "confirmed" by an account with the heading Washington beginning "At a packed press conference today (March 27) in the nation's capitol, leading Italian physicist Dr. Giuliano Preparata announced dramatic new steps forward in the development of cold fusion as a practical, cheap source of clean energy", "Speaking at the National Press Club". Dr. Eugene Mallove, former chief press officer of MIT who wrote the book "Fire from Ice" in favour of cold fusion, also spoke and said he hoped to repeat the results of Dr. Takahashi from Osaka, before the April 15 when Dr. Takahashi is scheduled to give a talk at MIT. "Mallove and Preparata attacked the vicious witch-hunt conducted in the US and Europe against the scientists who had the courage to attest to the reality of this revolutionary new science, and then were subject to persecution similar to that which drove the two pioneers to leave the United States."

The press report finished "You are dealing with a subtle process here which must be explained by real scientific thinking", "For the sake of your children, for the sake of the future of humanity, we must fight this stranglehold on science that affects us all."

Jon Webb then pointed out that "this article was taken from the New Federalist, a publication of the political extremist Lyndon LaRouche." "if the press conference was packed, why haven't there been any other stories about it?" Mr. LaRouche is described in one of the associated magazines as "a political prisoner in federal prison in Rochester, Minn." - others say the long jail sentance has something to do with tax.

POST-SCRIPT 2

Have just received on the net, the text of the proposed change to the Law which would favour Fusion. The phrase "Cold Fusion" is not mentioned, but it is easy to see it would help people doing such experiments. The title is "Replacement of Public Law 96-389, sec 3, Oct. 7, 1980, 95Stat 1540 Chapter 101 -- Fusion Energy Engineering." "(The purpose of this revision of 03/26/92 is to provide small grants to fusion innovators who possess fusion technology patents, allowing them to devote more time and effort in the pursuit of private capital sources)".

It says that preference should be given to aneutronic fusion - which is defined as "any fuel which when burnt in a fusion energy system, produces neutron radiation carrying away less than 10% of the produced energy." The figure of 10% seems very high for an aneutronic reaction which

means no neutrons. It would allow more than 10**16 neutrons per second from a megawatt power plant which would be a major radiation hazard and would damage the materials used in the construction.

"Every US citizen possessing a patent for a fusion energy system is to be provided with full reimboursement of all tax-deductible expenses incurred in the pursuit of the patent, up to a maximum of $100,000"

"(2) to stimulate private sector investment in fusion energy technology by awarding substantial prizes for significant technical achievement and matching private investment with public grants" The prizes are substantial 12 of them each of $100,000,000.

At the Nevada nuclear test range, 100 acres should be made available at a "cost of no more than $1000 per month to lease per acre, including all user fees." This shall "be remote enough that the instantaneous release of 1 gram of tritium gas per month will pose no significant health risk to those outside the test range."

There would be 10 monthly auctions of "10 kilograms of Helium-3". Curious.

7. CONCLUSIONS

The major recent event is that the two original experiments of Pons and Fleischmann and of Jones et al., seem both to have been discredited.

If there was no effect there to confirm, it is not surprising that the majority of experiments found nothing.

The fact that a minority of experiments found some evidence that appeared to confirm the two original experiments, is not unusual in these kind of affairs.

New experiments are decreasing to a "trickle" but it seems the band of True Believers has decided on an active campaign using the media. One wonders if some of them are becoming associated with Lyndon LaRouche or only adopting his style. The well-funded journals, New Federalist and 21st Century Science and Technology, which support LaRouche, have been most generous in their support of Cold Fusion

Expect that the Third Cold Fusion conference will take place. Since it is under the auspices of the very respectable Japanese National Institute for Fusion Reseach, it is to be expected that the meeting will be conducted in a normal scientific manner - that the programme committee will contain both people who believe in Cold Fusion and those who do not. Similarly one can expect invited speakers from the main experiments that do and do not find Cold Fusion effects. It should be an interesting meeting which will do honour to its sponsors.

Douglas R.O. Morrison.

1992-05-07

University of Southampton heading

Dr. Melvin H. Miles,
Chemistry Division,
Naval Weapons Center,
China Lake,
Code 3853
CA 9355
U.S.A.

MF/KJW

7 May 1992

Dear Mel,

While collecting all my bits and pieces together in preparation for going to France, I ran across the attached letter to the Editor of Nature. It was our one and only attempt to get him to moderate his invective and/or try and be factually correct.

Needless to say it was not published.

I dare say that you will by now have had time to digest my letter to Stan Pons and Giuliano Preparata about Douglas Morrison's "Cold Fusion Update No. 6." and that you will soon receive letters asking you whether you will act on parts of this, somewhat along with lines which I outlined in my letter. However, there is one matter which you could perhaps act on immediately if you choose to do so and that is to get the details of Ed Storms' communications with Science re Gary Taubes' assertion that John Bockris' cells had been "spiked" with tritium. If you should this done may I ask you to send me copies to France. There is one other matter which would be work pursuing at this stage and that is John Appleby's correspondence with Nature regarding this paper (with Srinivasan) on the calorimetry. As you are interested in collecting information about all aspects of the calorimetry it strikes me that you would be the correct person to approach John Appleby (at Texas A & M).

As you see, I am spreading the load.

Yours sincerely,

Martin

P.S. I am sending this from France.

There has been much comment recently in the Editorial pages of Nature (issues of 30th March, 13th, and 27th April), about the content of the paper on "Electrochemically Induced Nuclear Fusion of Deuterium" [1] and about our standards of behaviour in releasing the information about this research to the public at large. Many of the comments with respect to the latter are incorrect and have been highly damaging to us. We are therefore addressing this letter to your readership as we wish to give them the facts as we see them and so that they may draw their own conclusions.

In the first place it was asserted in the issue of 30th March that a press conference at the University of Utah on 23rd March was held before any paper had been submitted or accepted for publication. This is incorrect. The Preliminary Note in question was submitted on 13th March and accepted in final form on 22nd March. It is a matter of deep regret to us that many of the corrections which we made were not incorporated in the text released; they can be found in a subsequent issue of the Journal of Electroanalytical Chemistry[2]. The editorials in Nature on 30th March further assert that we informed the press that we had submitted a paper to Nature. This assertion is completely incorrect: we refused to answer questions as to the Journals to which we had submitted our work and we always explained that we refused to do so as reviewers and Editors have to be free to reach their own decisions. This was pointed out to the Editor in a letter dated 7th April from Professor R. Nesbitt, the Dean of the Faculty of Science at Southampton University. In spite of this the editorials in the issues of 13th and 20th April reiterate the statement that we had disclosed to the press that we had submitted a letter to Nature. We believe that the editorial staff of Nature neglected their duty to question us directly about our alleged misbehavior before publishing such assertions.

While this is a serious matter, it is our view that the allegations contained in the 20th April issue that we had not cooperated with the reviewing process are far more serious. We wish to point out to the readers of Nature that we had submitted a letter and not a full article. It was the decision of the Editors and not the reviewers to ask us to submit a fuller version of the article and was our view that we could not comply with this request in the time allowed. We therefore withdrew the letter to Nature but nevertheless sent a revised text and full replies to all of the major points raised by the referees. It may well be that our replies would have satisfied the referees with regard to the requirements for a letter to Nature. However, we will never know whether this is so because, to the best of our knowledge, our replies were not forwarded to the referees.

This matter is all the more serious because the Editorial of the 27th April raises questions which were not posed by the referees such as that of blank determinations and energy storage in the Pd-host lattice. We believe that we could easily have satisfied the referees on such issues if they had questioned us about them but we will now have no redress against such accusations until such time as the detailed papers on our work are published. It is our view therefore that it is not we but the Editorial staff of Nature who have not cooperated with the reviewing procedures. While we agree with many of the sentiments expressed in these editorials, we believe that the editors should have taken more care about the contents; they might well also have had the courtesy to consult us first before publishing such a succession of incorrect statements.

Martin Fleischmann, FRS, Department of Chemistry, The University, Southampton, Hants, SO9 5NH

Stanley Pons, Department of Chemistry, University of Utah, Salt Lake City, Utah, 84109, USA

References:

1) M. Fleischmann, S. Pons, and M. Hawkins, J. Electroanal. Chem. 261 (1989) 301-308.
2) M. Fleischmann, S. Pons, and M. Hawkins, J. Electroanal. Chem. 263 (1989) 187-188.

1992-08-08

Melvin Miles
NAWC Weapons Division
Naval Weapons Center
China Lake, CA

8 July 1992

Dear Mel,

We are keenly aware of your ongoing interest in the calorimetry and apologize for being incommunicado for so long—due to sheer pressure of work here.

We believe that it should be possible to develop relatively simple and accurate models of our calorimeters based on the approach leading to equations (49) and (54) in Martin's notes attached. It should be possible to predict the temperature-time data for whole sequences of measurements (over periods of months) and for various modes of operation (including the Harwell type of experiments). This type of modeling should also greatly facilitate data evaluation and improve the accuracy.

We are working further on this and will keep you posted. Martin or I will write to you sometime soon about the protocols for our experiments.

Regards,

Stan Pons

SP:sdp

Enclosure: Set of M.F.'s notes

A

Reinvestigation of the models of the calorimeters

Summary and further action.

In these notes, I have suggested that we use

$C_{p,H_2O,l} = 75$ Joules Mole^{-1} K^{-1} (?) — a.k. for H_2O

$C_{p,H_2O} = 84.35$ J mol^{-1} K^{-1}

$C_{p,H_2O} = 75.291$ J mol^{-1} K^{-1}

$M^0 = 4$ Moles (the old calorimeters) ($C_p M^0 = 300$ J K^{-1}) — smaller cell

$F = 10^5$ coulombs Mole^{-1} 96,485 As/equil.

$4k_R' \theta_{bath}^3 = 0.11$ watts K^{-1} $k_R' = \frac{0.11 W/K}{4 T_b^3} = 1.0376 \times 10^{-9}$ W K^{-4} (If $T_b = 298.15$ K)

$\frac{\psi I}{\theta_{bath}} = 0.01$ watts K^{-1}

($\psi \rightarrow$ mucl for E–En) (?)

$\lambda = 0.09090909$ ie $0.005/0.55$ (?) error $= 0.09090909$ $\frac{0.05}{0.55} = 0.09090909$ (see p.9) $0.005/0.055$

With these values and the definitions

$$\alpha = \frac{Q_1}{C_{p,H_2O,l} M^0} = 1 \text{ to } 2 \times 10^{-2} \text{ K s}^{-1} \qquad (10)$$

$$\beta = \frac{I}{2FM^0} = 10^{-6} \text{ s}^{-1} \text{ for } I = 0.8 \text{ Amps} \qquad (11)$$

1.0364×10^{-6}

$\frac{A}{(\frac{2eq}{mol})(\frac{As}{eq})(mol)} = s$ (o.k.)

$$\gamma = \frac{\left(4k_R' \theta_{bath}^3 + \frac{\psi I}{\theta_{bath}}\right)}{C_{p,H_2O,l} M^0} = 4 \times 10^{-4} \text{ s}^{-1} \qquad (12)$$

B

$$\eta = \frac{2k_R' \theta_{bath}^3 (1+\lambda) I}{F C_{p,H_2O,l} (M^0)^2} = 2\times10^{-10}\ s^{-2} \qquad (13)$$

units o.k.
My value
$4\times10^{-10}\ s^{-2}$

Please note any changes which we should make to these values.

With these definitions, we can write the equation for the linearised model of the calorimeter as

$$\boxed{\frac{d\Delta\theta}{dt} + \frac{(\gamma-\eta t)}{(1-\beta t)}\Delta\theta = \frac{\alpha}{(1-\beta t)}} \qquad (9)$$

Note that in the attached calculations, the Taylor series expansion has been carried out about the bath temperature (equation (6)).

If we neglect the change of the water equivalent and heat transfer coefficient with time we have

Let $x = \Delta\theta = \Delta T$

$$\boxed{\frac{d\Delta\theta}{dt} = \alpha - \gamma\Delta\theta} \qquad \frac{dx}{dt} = \alpha - \gamma x \Big\} \int \frac{dx}{\alpha-\gamma x} = \int_0^t dt \qquad (14)$$

If C_{pM} and k_R' are constant

with the obvious solution

$$\boxed{\Delta\theta = \frac{\alpha}{\gamma}\left[1 - \exp(-\gamma t)\right]} \qquad (20)$$

Note
$\Delta\theta = 0$ at $t=0$
$\Delta\theta \to \alpha/\gamma$ as $t\to\infty$
$\alpha/\gamma = 25\,K$ (to 50K)

If we allow for the change of the water equivalent with time

$$\boxed{\frac{d\Delta\theta}{dt} = \frac{(\alpha-\gamma\Delta\theta)}{(1-\beta t)}} \qquad (24)$$

Allows $C_pM = f(t)$
$k_R' = \text{constant}$

with the solution

$$\boxed{\Delta\theta = \frac{\alpha}{\gamma}\left[1 - (1-\beta t)^{\frac{\gamma}{\beta}}\right]} \qquad (29)$$

Note
$\Delta\theta = 0$ at $t=0$

If we also allow for the change of the heat transfer coefficient with time, we have to solve (9). In this case we can only obtain a series solution. One possible form is

Eq-49

$$\Delta\theta = \frac{\alpha}{\beta\phi}\left\{\left(\frac{\beta^2}{\eta}\right)\frac{\phi}{(1-\beta t)} + \left(\frac{\beta^2}{\eta}\right)^2\frac{\phi(\phi-1)}{(1-\beta t)^2} + \left(\frac{\beta^2}{\eta}\right)^3\frac{\phi(\phi-1)(\phi-2)}{(1-\beta t)^3} + \cdots\right]$$
$$-\left[\left(\frac{\beta^2}{\eta}\right)\phi + \left(\frac{\beta^2}{\eta}\right)^2\phi(\phi-1) + \left(\frac{\beta^2}{\eta}\right)^3\phi(\phi-1)(\phi-2)\cdots\right]\frac{1}{(1-\beta t)^{\phi}}\exp\left(\frac{-\eta t}{\beta}\right)\Bigg\} \qquad (49)$$

which with reasonable further approximations reduces to

Eq-54

$$\Delta\theta \simeq \alpha\left[\frac{1}{(\gamma-\eta t)} - \frac{1}{\gamma(1-\beta t)^{\phi}}\exp\left(\frac{-\eta t}{\beta}\right)\right] \qquad (54)$$

For $\eta = 0$

$$\Delta\theta = \alpha\left[\frac{1}{\gamma} - \frac{1}{\gamma(1-\beta t)^{\phi}}\right] = \frac{\alpha}{\gamma}\left[1 - \frac{1}{(1-\beta t)^{\phi}}\right]$$

which in turn reduces to (29) for $\eta = 0$.

Action.

We should compare the exact result (20) with a simulation of (14) (just to check up on our simulations)

We should compare the exact result (29) with a simulation of

D

(29) (as a further check on our simulation). We should check up on the differences between (29) and (20).

We should check on the convergence of (49) (by lengthening the series term by term) and compare the result with (54) and the simulation of (9)

Further Action.

We should investigate all the results for expansions about other chosen temperatures.

We should develop the equations for the Harwell and Caltech type operation

We should develop the equations for whole experiment sequences lasting several days.

We should investigate the use of (49) and (54) in regression fitting, filtering procedures etc.

Reinvestigation of the models of the calorimeters.

Objectives of the investigation.

Our objectives could include:

(i) the establishment of sufficiently accurate models which can be described by simple algebraic expressions;

(ii) to derive expressions which describe a variety of operating conditions;

(iii) to use the various algebraic expressions to test the statements which have been made about the validity/accuracy of this type of calorimetry and methods of data analysis;

(iv) to use the expressions to carry out easier and more complete analyses of the raw data;

(v) as an extension to (iv) to carry out analyses of complete experimental sequences lasting typically ~30 days.

We can make a number of statements about this programme at the outset:

ii) the judgement of the adequacy of particular expressions will be based on the comparison of calculated $\Delta\theta - t$ plots with those calculated by simulation of the relevant models;

iii) the expressions derived will include the experiments carried out in Utah and in Harwell

iv) the intention here is to simplify the non-linear regression and Kalman filtering procedures leading on to the design of more sophisticated filters, methods for determining transfer functions and, eventually, control strategies

v) the use of complete experiment sequences will greatly enhance the accuracy and eliminate some of the uncertainties imposed by the experiment design (in particular the errors due to the refilling of the calorimeters). Refilling errors

Some comments on the previous work.

The starting point for our investigation was equation (A3.9) of reference (1): Martin Fleischmann, Stanley Pons, Mark W. Anderson, Liang Jun Li and Marvin Hawkins, J. Electroanal. Chem., 287 (1990) 293:

$(1+\beta)$? $\Rightarrow$ should be $1+\beta$

$$C_{p,D_2O,l}\left[M^0 - \frac{(1+\beta)It}{2F}\right]\frac{d\Delta\theta}{dt} - C_{p,D_2O,l}\frac{\beta I\Delta\theta}{2F}$$

$$= \left[E_{cell}(t) - E_{thermoneutral,bath}\right]I + Q_f(t) + \Delta Q\,H(t-t_1) - \Delta Q\,H(t-t_2)$$

$$- \frac{0.75I}{F}\left(\frac{P}{P^* - P}\right)\left(C_{p,D_2O,g}\,\Delta\theta + L\right)$$

?

$$- k_R^0\,\theta_{bath}^3\left[1 - \frac{(1+\lambda)It}{2FM^0}\right]\left[\frac{(\theta_{bath}+\Delta\theta)^4 - \theta_{bath}^4}{\theta_{bath}^3} \;\; 4\Phi\,\Delta\theta\right] \qquad (1)$$

where the terms are defined in (1). The parameter β allows for a more rapid change of the water equivalent of the calorimeter with time than is predicted by the effects of electrolysis alone. We subsequently set $\beta = 0$ (we shall need this symbol to denote a different quantity in due course).

It is probably impossible to find an analytical solution to (1) and we therefore developed a non-linear regression

procedure which relied on the fitting of the numerical integral of (1) to the experimental data[1]. In order to improve the accuracy and the speed of the fitting procedures, we made two assumptions:

firstly, that we can characterise heat transfer from the cells by radiation alone;

$$k_R^0\,\theta_{bath}^3\left[1-\frac{(1+\lambda)It}{2FM^0}\right]\left[\frac{(\theta_{bath}+\Delta\theta)^4-\theta_{bath}^4}{\theta_{bath}^3}-4\Phi\,\Delta\theta\right]$$

$$\simeq k_R^1\left[1-\frac{(1+\lambda)It}{2FM^0}\right]\left[(\theta_{bath}+\Delta\theta)^4-\theta_{bath}^4\right] \qquad (2)$$

secondly, that we can carry out a Taylor-series expansion of the left hand side of equation (4) about a chosen operating point where $\theta=\theta^0$ and $\Delta\theta=\Delta\theta^0$ so that the shift in temperature from this point is

$$\Delta\theta' = \Delta\theta - \Delta\theta^0 \qquad (3)$$

giving

$$\left[E_{cell}(t)-E_{thermoneutral,\,bath}\right]I + Q_f(t)$$

$$-\frac{0.75I}{F}\left(\frac{P}{P^*-P}\right)\left(C_{P,Dry,g}\,\Delta\theta+L\right) = Q_1' - \frac{\Psi I\,\Delta\theta'}{\theta^0} \qquad (4)$$

The effect of these assumptions is to reduce the number of parameters to be fitted. Note that the representation (2) leads to an underestimate of $Q_f(t)$. As most of the experiment reported in[1] were carried out at temperatures below 50°C, the parameter ψ/θ^0 was determined from the independently measured $E_{cell}(t) - t$ data; more generally, we must also allow for the change in the enthalpy absorbed by evaporation of D_2O with temperature. This has been done in subsequent work:

reference (2): "The Calorimetry of Electrode Reactions", M. Fleischmann, S. Pons, J.L. Li, X. Xu, Y. Qian and J. Pons

reference (3): "Excess Enthalpy Generation in Palladium Alloy Cathodes Polarized in Heavy Water", S. Pons, M. Fleischmann, J.L. Li, X. Xu, Y. Qian and J. Pons

reference (4) "Report to the Utah State Fusion Energy Council on the Analysis of Selected Pons - Fleischmann Calorimetric Data", Wilford Hansen in Eds. Tullio Bressani, Emilio Del Guidice and Giuliano Preparata, "The Science of Cold Fusion: Proceedings of the

I Annual Conference on Cold Fusion" Volume 33 of the Conference Proceedings, The Italian Physical Society, Bologna, Italy (ISBN-887794-045-X) 491 (1992).

The texts of these papers are available. Note in particular that the change in design of the calorimeters (the silvering of the top portions of the Dewars) has reduced the time-dependence of the heat transfer coefficients so that it has been possible to make extensive use of the equation

$$C_{P,D_2O,e}\left[M^0 - \frac{I}{4F}\int_0^t \left(\frac{2P^* + P}{P^* - P}\right) dt\right]\frac{d\Delta\theta}{dt}$$

$$= \left[E_{cell}(t) - E_{thermoneutral,\,bath}\right] I + Q_f(t) + \Delta Q H(t-t_1) - \Delta Q H(t-t_2)$$

$$- \frac{0.75 I}{F}\left(\frac{P}{P^* - P}\right)\left[\left(C_{P,D_2O,g} - C_{P,D_2O,e}\right)\Delta\theta + L\right]$$

$$- k_R'\left[\left(\theta_{bath} + \Delta\theta\right)^4 - \theta_{bath}^4\right] \qquad (5)$$

unknowns
M^0
$Q_f(t) = P_x$
k_R'

which only contains three "unknowns": M^0, $Q_f(t)$ and k_R'. The Kalman filtering procedure reported in [2-4] in particular has made use of (5) as has the point-by-point evaluation in [2,3]. Equation (5) can be used at temperatures up to the

boiling point. However, the application of low-pass filtering normally requires the linearisation of equations about the chosen operating points. Such linearisations were also investigated in [1] as part of the overall modelling procedures. It is likely, in fact, that most data evaluations could be based on such linearisations and we will investigate their applicability.

As a first step, I will assume that the temperature is sufficiently low that we can put

$$Q_1' - \frac{\psi I \Delta\theta'}{\theta^0} = Q_1' + \frac{\psi I \Delta\theta^0}{\theta^0} - \frac{\psi I}{\theta^0}(\Delta\theta^0 + \Delta\theta')$$

$$\simeq Q_1 - \frac{\psi I \Delta\theta}{\theta_{bath}} \qquad (6)$$

i.e. the Taylor series expansion behaves as though it has been carried out about the bath temperature. With (2), (4) and (6) equation (1) becomes

$$C_{P,O_2O_1\ell} M^0 \left[1 - \frac{It}{2FM^0}\right] \frac{d\Delta\theta}{dt} = Q_1 - \left[4k_R' \theta_{bath}^3 \left(1 - \frac{(1+\lambda)It}{2FM^0}\right) + \frac{\psi I}{\theta_{bath}}\right]\Delta\theta \qquad (7)$$

where we omit for the present the calibration pulse $\Delta Q\, H(t-t_1) - \Delta Q\, H(t-t_2)$

In standard form

$$\frac{d\Delta\theta}{dt} + \frac{\left[\left(4k_R'\theta_{bath}^3 + \frac{\psi I}{\theta_{bath}}\right) - \frac{4k_R'\theta_{bath}^3(1+\lambda)It}{2FM^0}\right]\Delta\theta}{C_{p,D_2O,\ell}M^0\left[1-\frac{It}{2FM^0}\right]} = \frac{Q_1}{C_{p,D_2O,\ell}M^0\left[1-\frac{It}{2FM^0}\right]} \quad (8)$$

or $$\frac{d\Delta\theta}{dt} + \frac{(\gamma-\eta t)}{(1-\beta t)}\Delta\theta = \frac{\alpha}{(1-\beta t)} \quad (9)$$

where $$\alpha = \frac{Q_1}{C_{p,D_2O,\ell}M^0} \quad (10)$$

$$\beta = \frac{I}{2FM^0} \quad (11)$$

$$\gamma = \frac{\left(4k_R'\theta_{bath}^3 + \frac{\psi I}{\theta_{bath}}\right)}{C_{p,D_2O,\ell}M^0} \quad (12)$$

$$\eta = \frac{2k_R'\theta_{bath}^3(1+\lambda)I}{F\,C_{p,D_2O,\ell}(M^0)^2} \quad (13)$$

In making subsequent calculations, I will use

$C_{p,D_2O,\ell}$ = 75 Joules Mole^{-1} K^{-1}

M^0 = 4 Moles

F = 10^5 coulombs Mole^{-1}

$4k_R'\theta_{bath}^3$ = 0.11 watts K^{-1}

$$\frac{\psi I}{\theta_{bath}} = 0.01 \text{ watts } K^{-1}$$

$$\lambda = 0.09090909 \text{ ie } 0.005 / .055 \quad \text{note } \frac{0.005}{0.055}$$

$$I = 0.8 \text{ Amps}$$

giving suitable values for the calculations

$$\alpha = 1 \text{ to } 2 \times 10^{-2} \text{ K s}^{-1}$$

$$\beta = 10^{-6} \text{ s}^{-1}$$

$$\gamma = 4 \times 10^{-4} \text{ s}^{-1}$$

$$\eta = 2 \times 10^{-10} \text{ s}^{-2}$$

Models of increasing complexity.

We will examine models of increasing complexity so that we can relate the models to the necessary algebra and to see whether comments which have been made about data treatments are valid.

First, let us start with a calorimeter for which the heat transfer coefficient is independent of time ie $\gamma = 0$. We will also assume that we can neglect the change of the water equivalent with time ie $\beta = 0$. In this case (8) reduces to the simple equation

Assumptions
1. k_R is independent of time ($\gamma = 0$)
2. Change in CPM can be neglected ($\beta = 0$)

$$\frac{d\,\Delta\theta}{dt} = \alpha - \gamma\Delta\theta \qquad (14)$$

or

$$\frac{d\,\Delta\theta}{(\alpha - \gamma\Delta\theta)} = dt \qquad (15)$$

giving

$$-\frac{1}{\gamma}\ln(\alpha - \gamma\Delta\theta) = t + \text{constant} \qquad (16)$$

with the initial condition

$$\Delta\theta = 0 \quad \text{at } t = 0 \qquad (17)$$

$$\text{constant} = -\frac{1}{\gamma} \ln \alpha \tag{18}$$

so that $$\ln\left(\frac{\alpha - \gamma \Delta\theta}{\alpha}\right) = -\gamma t \tag{19}$$

Assume Both both k_R' and C_{pM} are independent of time.

or $$\boxed{\Delta\theta = \frac{\alpha}{\gamma}\left[1 - \exp(-\gamma t)\right]} \tag{20}$$

Result Eq. 20

This gives us the entirely expected behaviour: an exponential relaxation with the relaxation time

$$\tau = \frac{1}{\gamma} = \frac{C_{p,Pt,e} M^0}{\left(4 k_R' \theta_{bath}^3 + \frac{\psi I}{\theta_{bath}}\right)} \tag{21}$$

an initial rise in temperature according to

$$\Delta\theta \propto \alpha t = \frac{Q_1 t}{C_{p,Pt,e} M^0} \tag{22}$$

Initially $\Delta\theta$ is linear with time.

and a final steady state temperature

$$\Delta\theta = \frac{\alpha}{\gamma} = \frac{Q_1}{\left(4 k_R' \theta_{bath}^3 + \frac{\psi I}{\theta_{bath}}\right)} \tag{23}$$

The reason why I am flogging this somewhat dead horse is that this is the only model which can conceivably give a final steady state temperature. However, even for this simple

model, we cannot assume that the heat transfer coefficient is $4k_R' \theta_{bath}^3$! Rather than this simple result we have to allow for the coupling of the change in temperature to the enthalpy input which manifests itself by a change to an effective heat transfer coefficient $\left(4k_R' \theta_{bath}^3 + \frac{\psi I}{\theta_{bath}}\right)$

Let us now make the simple change of allowing for the change of the water equivalent of the calorimeter with time while we still assume that the heat transfer coefficient is independent of time. In this case (9) becomes

$$\frac{d\Delta\theta}{dt} = \frac{(\alpha - \gamma\Delta\theta)}{(1-\beta t)} \tag{24}$$

We first of all integrate this in the obvious way using

$$\frac{d\Delta\theta}{(\alpha - \gamma\Delta\theta)} = \frac{dt}{(1-\beta t)} \tag{25}$$

giving

$$\frac{1}{\gamma} \ln(\alpha - \gamma\Delta\theta) = \frac{1}{\beta} \ln(1-\beta t) + \text{constant} \tag{26}$$

and, with (17)

$$\text{constant} = \frac{1}{\gamma} \ln \alpha \tag{27}$$

(27) with (26) gives

$$\ln\left(\frac{\alpha - \gamma\Delta\theta}{\alpha}\right) = \frac{\gamma}{\beta}\ln(1-\beta t) = \ln(1-\beta t)^{\frac{\gamma}{\beta}} \tag{28}$$

or $$\boxed{\Delta\theta = \frac{\alpha}{\gamma}\left[1-(1-\beta t)^{\frac{\gamma}{\beta}}\right]} \tag{29}$$

Assume R_n' is constant

Eq-29

We now no longer have any recognisable single relaxation behaviour and we certainly do not have a final steady-state temperature. Since βt remains small during any particular day's operation and since γ/β is large, (29) gives an behaviour at short times which resembles (20) – and, actually, at long times $\Delta\theta$ also approaches the value (23) i.e. the fact that the water equivalent changes with time does not have a very large effect on the behaviour.

We will now approach the solution of (24) in a more general way which will be useful for the extension of the modelling. First we rewrite (24) in standard form

$$\frac{d\Delta\theta}{dt} + \frac{\gamma\Delta\theta}{(1-\beta t)} = \frac{\alpha}{(1-\beta t)} \tag{30}$$

(30) gives the integrating factor

$$\exp\int\frac{\gamma\,dt}{(1-\beta t)} = \exp -\frac{\gamma}{\beta}\ln(1-\beta t) = \exp\ln(1-\beta t)^{-\frac{\gamma}{\beta}}$$

$$= \frac{1}{(1-\beta t)^{\gamma/\beta}} \tag{31}$$

Hence (30) can be expressed as

$$\frac{d}{dt}\left[\frac{\Delta\theta}{(1-\beta t)^{\gamma/\beta}}\right] = \frac{\alpha}{(1-\beta t)^{(\gamma+\beta)/\beta}} \tag{32}$$

and $$\frac{\Delta\theta}{(1-\beta t)^{\gamma/\beta}} = \frac{\alpha}{\gamma}\cdot\frac{1}{(1-\beta t)^{\gamma/\beta}} + \text{constant} \tag{33}$$

and (17) gives

$\underline{\Delta\theta = 0 \text{ at } t=0}$

$0 = \frac{\alpha}{\gamma} + \text{constant}$

$$\text{constant} = -\frac{\alpha}{\gamma} \tag{34}$$

Combining (34) with (33)

$$\Delta\theta = \frac{\alpha}{\gamma}\left[1 - (1-\beta t)^{\frac{\gamma}{\beta}}\right] \tag{29}$$

as with the simpler integration procedure.

We now explore the solution of (9) (which is a full expression of

the model for the calorimeter in the linearised version). The integrating factor is now

$$\exp\int\frac{(\gamma-\eta t)}{(1-\beta t)}dt = \exp\left\{\frac{-\gamma}{\beta}\ln(1-\beta t)+\frac{\eta t}{\beta}\ln(1-\beta t)-\frac{\eta}{\beta}\int\ln(1-\beta t)dt\right\}$$

$$=\exp\left\{\frac{-\gamma}{\beta}\ln(1-\beta t)+\frac{\eta t}{\beta}\ln(1-\beta t)+\frac{\eta(1-\beta t)}{\beta^2}\ln(1-\beta t)-\frac{\eta}{\beta^2}(1-\beta t)\right\}$$

$$=\exp\left\{\frac{(\eta-\gamma\beta)}{\beta^2}\ln(1-\beta t)-\frac{\eta}{\beta^2}+\frac{\eta t}{\beta}\right\}$$

$$=\exp\left(\frac{-\eta}{\beta^2}\right)(1-\beta t)^{\phi}\exp\left(\frac{\eta t}{\beta}\right) \qquad (35)$$

where $\phi = \dfrac{(\eta-\gamma\beta)}{\beta^2}$ Note definition of ϕ (36)

We can therefore express (8) as

$$\frac{d}{dt}\left\{\Delta\theta\exp\left(\frac{-\eta}{\beta^2}\right)(1-\beta t)^{\phi}\exp\left(\frac{\eta t}{\beta}\right)\right\}=\alpha\exp\left(\frac{-\eta}{\beta^2}\right)(1-\beta t)^{(\phi-1)}\exp\left(\frac{\eta t}{\beta}\right) \qquad (37)$$

It does not appear to be possible to find a closed form solution to (37) and so we look for a series which converges sufficiently rapidly so that we have in effect a simple algebraic expression. There are four immediately obvious ways for achieving this:

(i) expansion of the term $(1-\beta t)^{(\phi-1)}$ and integration by parts

of the right hand side of (37)

(ii) expansion of the term $\exp\left(\frac{\eta t}{\beta}\right)$ and again integration by parts

(iii) repeated integrations of $(1-\beta t)^{(\phi-1)}$ and differentiations of $\exp\left(\frac{\eta t}{\beta}\right)$

(iv) repeated integrations of $\exp\left(\frac{\eta t}{\beta}\right)$ and differentiations of $(1-\beta t)^{(\phi-1)}$.

There may well be other ways (especially using substitutions or expansions of both $(1-\beta t)^{(\phi-1)}$ and $\exp\left(\frac{\eta t}{\beta}\right)$ but, as of now, I will limit attention to (i) — (iv).

Of these options, (i) will clearly give a poorly convergent series because of the magnitude of $(\phi-1)$ and I will not examine this further at this stage. As against this, (ii) should give a reasonably rapidly convergent series (note: my estimates for γ and η are probably too high).

We have

$$(1-\beta t)^{(\phi-1)}\exp\left(\frac{\eta t}{\beta}\right) = (1-\beta t)^{(\phi-1)}\left[1+\frac{\eta t}{\beta}+\frac{1}{2!}\left(\frac{\eta t}{\beta}\right)^2+\frac{1}{3!}\left(\frac{\eta t}{\beta}\right)^3\cdots\right] \tag{38}$$

and, at least for the first part of the transients, we can truncate the series say for the terms shown. We then get the integrals

$$\int (1-\beta t)^{(\phi-1)}dt = \frac{-1}{\phi\beta}(1-\beta t)^{\phi} \tag{39}$$

$$\int (1-\beta t)^{(\phi-1)}\frac{\eta t}{\beta} = -\frac{\eta t}{\phi\beta^2}(1-\beta t)^{\phi} + \int \frac{\eta}{\phi\beta^2}(1-\beta t)^{\phi}dt$$

$$= -\frac{\eta t}{\phi\beta^2}(1-\beta t)^{\phi} - \frac{\eta}{\phi(\phi+1)\beta^3}(1-\beta t)^{(\phi+1)} \tag{40}$$

$$\int \frac{(1-\beta t)^{(\phi-1)}}{2!}\left(\frac{\eta t}{\beta}\right)^2 = \frac{-1}{\phi\beta}\frac{1}{2!}\left(\frac{\eta t}{\beta}\right)^2(1-\beta t)^{\phi} + \int \frac{\eta^2 t}{\phi\beta^3}(1-\beta t)^{\phi}dt$$

$$= -\frac{\eta^2 t^2}{2!\,\phi\beta^3}(1-\beta t)^{\phi} - \frac{\eta^2 t}{\phi(\phi+1)\beta^4}(1-\beta t)^{(\phi+1)} + \int \frac{\eta^2}{\phi(\phi+1)\beta^4}(1-\beta t)^{(\phi+1)}dt$$

$$= -\frac{\eta^2 t^2}{2!\,\phi\beta^3}(1-\beta t)^{\phi} - \frac{\eta^2 t}{\phi(\phi+1)\beta^4}(1-\beta t)^{(\phi+1)} - \frac{\eta^2}{\phi(\phi+1)(\phi+2)\beta^5}(1-\beta t)^{(\phi+2)} \tag{41}$$

This will obviously give a divergent series because of the magnitudes of the last terms on the RHS of (39) – (41) etc.

We next explore (iii). We have

$$\int (1-\beta t)^{(\phi-1)}\exp\left(\frac{\eta t}{\beta}\right)dt = -\frac{1}{\beta\phi}(1-\beta t)^{\phi}\exp\left(\frac{\eta t}{\beta}\right) + \int \frac{(1-\beta t)^{\phi}}{\beta\phi}\left(\frac{\eta}{\beta}\right)\exp\left(\frac{\eta t}{\beta}\right)dt$$

$$= -\frac{(1-\beta t)^{\phi}}{\beta\phi}\exp\left(\frac{\eta t}{\beta}\right) - \frac{(1-\beta t)^{(\phi+1)}}{\beta^2\phi(\phi+1)}\left(\frac{\eta}{\beta}\right)\exp\left(\frac{\eta t}{\beta}\right) + \int\frac{(1-\beta t)^{(\phi+1)}}{\beta^2\phi(\phi+1)}\left(\frac{\eta}{\beta}\right)^2\exp\left(\frac{\eta t}{\beta}\right)dt$$

$$= -\exp\left(\frac{\eta t}{\beta}\right)\cdot\frac{1}{\beta}\sum_{n=0}^{\infty}\frac{\eta^n}{\beta^{2n}}\frac{(1-\beta t)^{(\phi+n)}}{\prod_{m=0}^{m=n}(\phi+m)} \qquad (42)$$

We next examine the magnitudes of the successive terms in this series. First we have

$$\phi = \frac{(\eta-\gamma\beta)}{\beta^2} = \frac{\dfrac{2k_R'\theta_{batt}^3(1+\lambda)I}{F\,C_{P,D_2O,l}(M^0)^2} - \dfrac{\left(4k_R'\theta_{batt}^3 + \dfrac{\psi' I}{\theta_{batt}}\right)\dfrac{I}{2FM^0}}{C_{P,D_2O,l}\,M^0}}{\left(\dfrac{I}{2FM^0}\right)^2}$$

$$= \frac{2F}{C_{P,D_2O,l}\,I}\left(4k_R'\theta_{batt}^3\lambda - \frac{\psi' I}{\theta_{batt}}\right) \qquad (43)$$

Our results indicate that for the early calorimeters $\lambda \sim 0.05 - 0.1$ while $4k_R'\theta_{batt}^3$ was about 0.1 watts K^{-1} so that $4k_R'\theta_{batt}^3\lambda$ is expected to lie in the range $5\times10^{-3} - 10^{-2}$. On the other hand $\psi' I/\theta_{batt}$ was about 1.5×10^{-2} watts K^{-1} for $I = 800$ mA. This is a somewhat awkward situation because ϕ may well be positive at low I (say 200 mA) and negative at high I (say 800 mA). We must therefore allow for a possible change of sign of ϕ. However,

at present we are interested in the magnitude of ϕ which appears to be in the range $0 - 100$. The effect of the term $\dfrac{1}{\prod_{m=0}^{m=n}(\phi+m)}$ therefore is to make successive terms converge. At the same time, as $(1-\beta t) < 1$, this also enhances the convergence.

We next consider the magnitude of the term

$$\frac{\eta^n}{\beta^{2n}} = \left[\frac{2k_R' \theta_{bark}^3 (1+\lambda) I}{F C_{p,n,\ell} (M^0)^2}\left(\frac{2FM^0}{I}\right)^2\right]^n$$

$$= \left[\frac{8F\, k_R' \theta_{bark}^3 (1+\lambda)}{C_{p,n,\ell}\, I}\right]^n \qquad (44)$$

The minimum value of this will be $\sim (300)^n$ so that the combination of the terms (43) and (44) will make the series (42) diverge.

We must therefore investigate the remaining option (iv). We have

$$\int (1-\beta t)^{(\phi-1)} \exp\left(\frac{\eta t}{\beta}\right) dt = \left(\frac{\beta}{\eta}\right)(1-\beta t)^{(\phi-1)} \exp\left(\frac{\eta t}{\beta}\right) + \int \left(\frac{\beta}{\eta}\right)\beta(\phi-1)(1-\beta t)^{(\phi-2)} \exp\left(\frac{\eta t}{\beta}\right) dt$$

$$= \left(\frac{\beta}{\eta}\right)(1-\beta t)^{(\phi-1)} \exp\left(\frac{\eta t}{\beta}\right) + \left(\frac{\beta}{\eta}\right)^2 \beta(\phi-1)(1-\beta t)^{(\phi-2)} \exp\left(\frac{\eta t}{\beta}\right)$$

$$+ \int \left(\frac{\beta}{\eta}\right)^2 \beta^2 (\phi-1)(\phi-2)(1-\beta t)^{(\phi-3)} \exp\left(\frac{\eta t}{\beta}\right) dt$$

$$= \frac{1}{\beta}\exp\left(\frac{\eta t}{\beta}\right)\left[\frac{\beta^2}{\eta}(1-\beta t)^{(\phi-1)} + \frac{\beta^4}{\eta^2}(\phi-1)(1-\beta t)^{(\phi-2)} + \frac{\beta^6}{\eta^3}(\phi-1)(\phi-2)(1-\beta t)^{(\phi-3)} + \cdots\right]$$

$$= \frac{1}{\beta\phi}\exp\left(\frac{\eta t}{\beta}\right)\sum_{n=1}^{\infty}\left(\frac{\beta^2}{\eta}\right)^n (1-\beta t)^{(\phi-n)}\prod_{m=0}^{m=n}(\phi-m) \qquad (45)$$

The terms in this series alternate in sign for $m > \phi$ so the series must converge eventually. However, we are interested in whether there is adequate convergence if we restrict the series to the first few terms. We note that that $(1-\beta t)^{(\phi-n)}$ must eventually impose a small divergence while $\prod_{m=0}^{m=n}$ imposes a more marked divergence (in modulus). The term

$$\left(\frac{\beta^2}{\eta}\right)^n = \left[\left(\frac{I}{2FM^0}\right)^2 \frac{F\,C_{p,D,n,e}\,(M^0)^2}{2k_R^1\,\theta_{bata}^3\,(1+\lambda)I}\right]^n$$

$$= \left[\frac{C_{p,D,n,e}\,I}{8F\,k_R^1\,\theta_{bata}^3\,(1+\lambda)}\right] \qquad (46)$$

is of order $(4\times10^{-4})^n$. This therefore imposes a strong convergence at the start of the series although eventually $\prod_{m=0}^{m=n}(\phi-m)$ will make the moduli diverge. We are therefore absolutely dependent on the alternation in sign and must investigate this further. However,

for the present, I suggest we proceed empirically and retain just the first few terms of the series. We will compare the solution based on these terms with simulations based on (9). Combining the first few terms of (45) with (37) we have

$$\Delta\theta \exp\left(\frac{-\eta}{\beta^2}\right)(1-\beta t)^{\phi} \exp\left(\frac{\eta t}{\beta}\right)$$

$$= \alpha \exp\left(\frac{-\eta}{\beta^2}\right)\cdot\frac{1}{\beta\phi}\exp\left(\frac{\eta t}{\beta}\right)\left[\left(\frac{\beta^2}{\eta}\right)\phi(1-\beta t)^{(\phi-1)} + \left(\frac{\beta^2}{\eta}\right)^2\phi(\phi-1)(1-\beta t)^{(\phi-2)}\right.$$

$$\left. + \left(\frac{\beta^2}{\eta}\right)^3\phi(\phi-1)(\phi-2)(1-\beta t)^{(\phi-3)} + \cdots\right] + \text{constant} \qquad (47)$$

With the initial condition (17)

$$\text{constant} = -\alpha \exp\left(\frac{-\eta}{\beta^2}\right)\cdot\frac{1}{\beta\phi}\left[\left(\frac{\beta^2}{\eta}\right)\phi + \left(\frac{\beta^2}{\eta}\right)^2\phi(\phi-1) + \left(\frac{\beta^2}{\eta}\right)^3\phi(\phi-1)(\phi-2)\cdots\right] \qquad (48)$$

Combining with (47)

$$\Delta\theta = \frac{\alpha}{\beta\phi}\left\{\left[\left(\frac{\beta^2}{\eta}\right)\frac{\phi}{(1-\beta t)} + \left(\frac{\beta^2}{\eta}\right)^2\frac{\phi(\phi-1)}{(1-\beta t)^2} + \left(\frac{\beta^2}{\eta}\right)^3\frac{\phi(\phi-1)(\phi-2)}{(1-\beta t)^3} + \cdots\right]\right.$$

$$\left. - \left[\left(\frac{\beta^2}{\eta}\right)\phi + \left(\frac{\beta^2}{\eta}\right)^2\phi(\phi-1) + \left(\frac{\beta^2}{\eta}\right)^3\phi(\phi-1)(\phi-2)\cdots\right]\frac{1}{(1-\beta t)^{\phi}}\exp\left(\frac{-\eta t}{\beta}\right)\right\} \qquad (49)$$

This is certainly far from any simple relaxation process.

I return here to the question of the order of magnitude of the terms

in the series in (48). Taking the coefficient of the first terms in the series we see from (12), (13) and (36) that

$$\frac{\beta^2\phi}{\eta} = \frac{\eta - \gamma\beta}{\eta} \tag{50}$$

which will be smaller than λ; if $\frac{\psi I}{2\theta_{bath}} > 2k_R^i\,\theta_{bath}^3\,\lambda I$ the terms in the series alternate in sign immediately. Since ϕ is relatively large, we can put initially

$$\phi \simeq \phi - 1 \simeq \phi - 2 \tag{51}$$

so that the magnitudes of the coefficients are as λ, λ^2, λ^3 etc and the series converges quite rapidly. In fact, to a first approximation, the series are geometric and the sums are

$$S = \frac{\beta^2\phi}{\eta(1-\beta t)} \cdot \frac{1}{1 - \frac{\beta^2\phi}{\eta(1-\beta t)}} \tag{52}$$

and $$S = \frac{\beta^2\phi}{\eta} \cdot \frac{1}{1 - \frac{\beta^2\phi}{\eta}} \tag{53}$$

(52) and (53) in (48) give

$$\Delta\theta \simeq \alpha\left[\frac{1}{(\gamma-\eta t)} - \frac{1}{\gamma(1-\beta t)^{\phi}}\exp\left(\frac{-\eta t}{\beta}\right)\right] \qquad (54)$$

and we should compare (54) with the more exact result (49).

Note that if the heat transfer coefficient is independent of time, i.e. $\eta = 0$, $\phi = -\gamma/\beta$ and (54) becomes

$$\Delta\theta \simeq \frac{\alpha}{\gamma}\left[1 - (1-\beta t)^{\frac{\gamma}{\beta}}\right] \qquad (29)$$

which is identical to the result we have obtained for this simpler model. This suggests that (54) is close to the exact solution.

Eq (54)

$$\boxed{\Delta\theta = \Delta T = \alpha\left[\frac{1}{(\gamma-\eta t)} - \frac{1}{\gamma(1-\beta t)^{\phi}}\exp(-\eta t/\beta)\right]}$$

For $\eta = 0$, $\phi = -\gamma/\beta$

$$\Delta\theta = \alpha\left[\frac{1}{\gamma} - \frac{1}{\gamma(1-\beta t)^{-\gamma/\beta}}\right]$$

$\Delta\theta = 0$ at $t = 0$

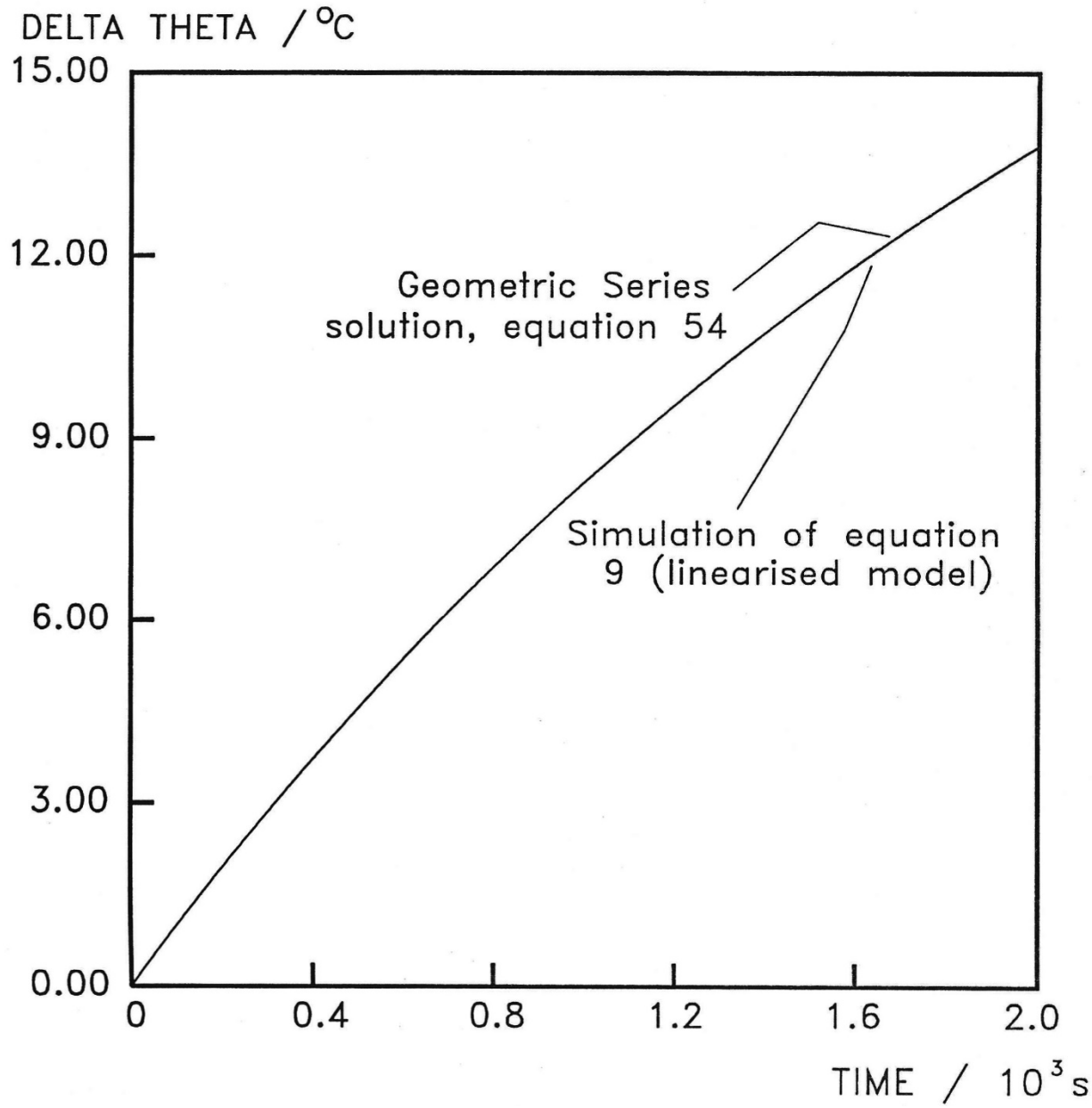
$$\boxed{\Delta\theta = \frac{\alpha}{\gamma}\left[1 - (1-\beta t)^{\gamma/\beta}\right]}$$

$$\alpha = \frac{Q_1}{C_p M^{\circ}}(\approx 10^{-2})\,(K s^{-1}) \Big/ \beta = \frac{I}{2FM^{\circ}}\,(s^{-1}) \quad (\approx 10^{-6} s^{-1})$$

$$\gamma = \frac{4\epsilon\sigma\theta_b^3 + \gamma I/\theta_b}{C_p M^{\circ}}\,(s^{-1}) \quad (\approx 4\times10^{-4} s^{-1})$$

$\gamma/\beta \approx 400$

$\frac{\alpha}{\gamma} = 2.5\times10^{-7} K$


DELTA THETA / °C
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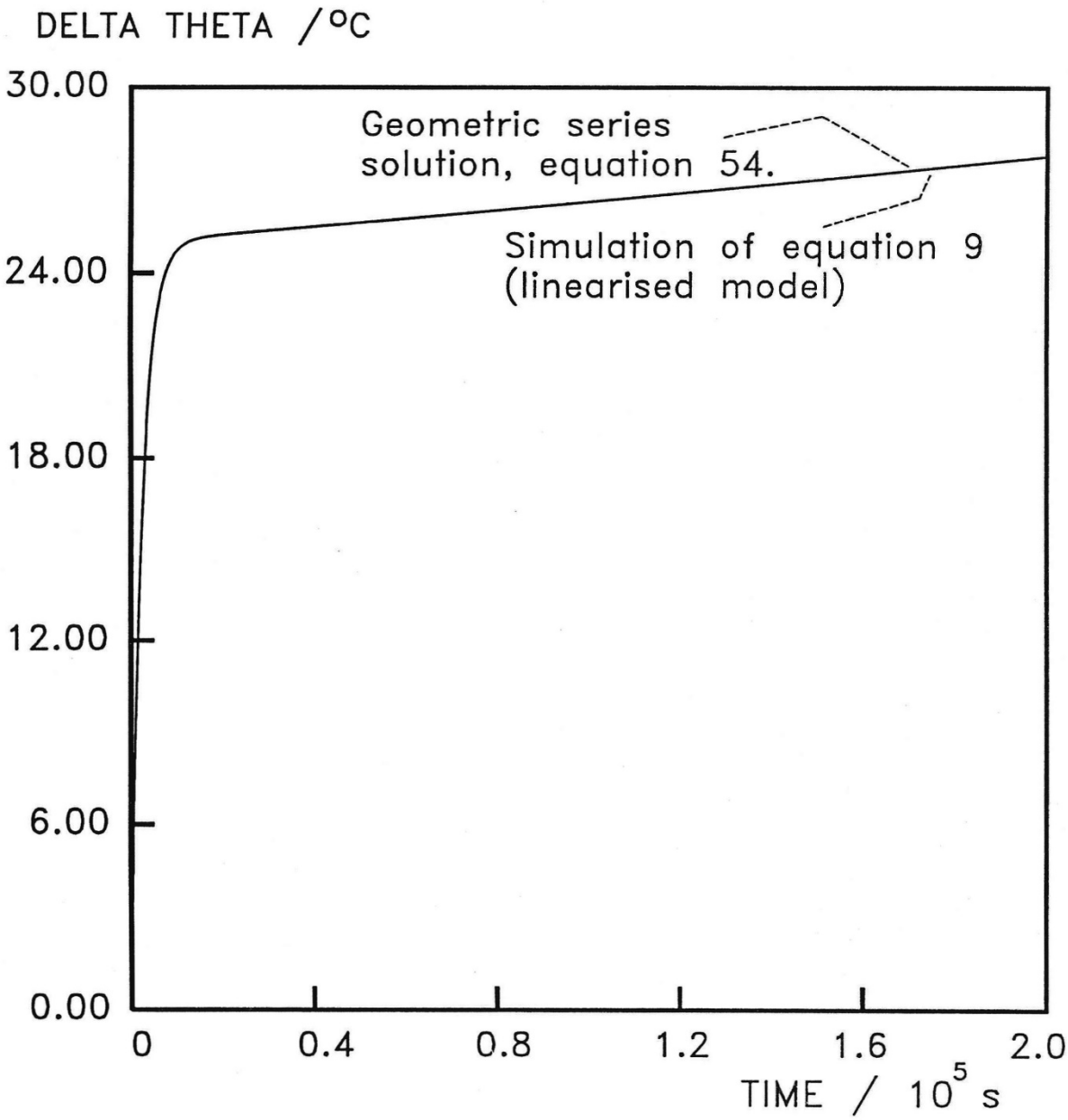
12.00
9.00
6.00
3.00
0.00
0
0.4
0.8
1.2
1.6
2.0
TIME / 10³s
Geometric Series solution, equation 54
Simulation of equation 9 (linearised model)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24.00
18.00
12.00
6.00
0.00
0
0.4
0.8
1.2
1.6
2.0
TIME / 10^5 s
Geometric series solution, equation 54.
Simulation of equation 9 (linearised model)

1993-04-08

TELEFAX TRANSMITTAL SHEET
DR. STANLEY PONS

IMRA EUROPE, SA
220 RU ALBERT, CAQUOT
SOPHIA, ANTIPOUS
VALBONNE 06560 FRANCE

TO: Dr. Mel Miles
NAWC
China Lake, CA

Dear Mel,

Martin and I are waiting a series of paper on some of the criticisms of our previous work. We are therefore, interested in any recent information that you might have regarding the Caltech paper/your own paper, etc. Can you tell me the present status of this matter? Have you submitted it anywhere else? I do have copies of the following correspondence in hands.

Date	Item	Addressee/Sender
5 Mar 93	Letter to "Science"	Miles/Science
22 Feb 93	Letter from "Nature"	Miles/Ball
10 Feb 93	Letter to "Nature"	Editor/Miles
unknown	Manuscript	Nature/Miles
9 Jun 92	Letter	Sailor/Miles
28 May 92	Trip Report	Navy/Miles
6 April 92	Letter	Fleischmann/Miles
5 Dec 91	Open Letter	Scientist/Miles
5 Dec 91	Letter	Maddox/Miles
3 Oct 91	Letter	Jones/Miles

Any other correspondence which you might have would be very useful in preparing these papers. We have heretofore maintained all of your correspondence in strict confindence, and we will continue to do so. Further, anything that we write for publication and which includes any item which are related to your correspondence will he sent to you. We hope all is going well for you, and we would like to know how your present work is going. With my best personal regards.

Yours sincerely,

Stanley Pons

TELEFAX TRANSMITTAL SHEET
DR. STANLEY PONS

IMRA EUROPE, SA
220 RU ALBERT, CAQUOT
SOPHIA, ANTIPOUS
VALBONNE 06560 FRANCE

TO: Dr. Mel Miles
NAWC
China Lake, CA

Dear Mel:

Further to my earlier Fax, I see that I have neither a copy nor reference of your recent paper in J. Electroanal. Chem. Could this be included along with my previous request? Thanks.

Best personal regard,

Yours sincerely,

Stanley Pons

1993-05-10

IMRA

FAX

TO: Dr. Mel Miles
NAWC
China Lake, CA

From: Stan Pons

Dear Mel,

Thank you for keeping me apprised in the saga of your paper. I think what you are doing may expose an incredible example of the suppression of science.

I am finishing a paper that you may enjoy reading regarding the x-ray measurements of various groups. I will send you a confidential copy for your comments, hopefully this week. I do need a reference if you happen to have it on hand. It is a paper of N. Lewis publishing in Analytical Chemistry in 1981 entitled "The Central Role of Analytical Chemistry and Spectroscopy in the Search for Electrochemically Induced Cold Fusion." If you have this, I would appreciate your Faxing it to me.

Best regards.

1993-09-21

[JR This is a fax. The first page is unreadable. Starting from p. 2 it reads:]

1) The temperature in the laboratory we use up to October 1989 was controlled to ± 2°C.
2) The temperatures in the various laboratories we have used since October 1989 have been controlled to ± 0.5°C (we now use to independent control systems) around the temperature of our water thermostats.
3) The effect of 2) is to minimise the effects of any relative changes in the heat transfer to the water bath and the atmosphere.
4) We have independent measurements of the heat transfer to the atmosphere through the tops of the cells (see the Como Conference Proceedings).
5) In our work up to October 1989 we controlled the level of the water in the thermostats to minimise any errors in the heat transfer coefficients due to fluctuations in this level.
6) The effect of 5) is quite negligible in our more recent work using Dewars silvered in the top portion; however, we continue to control the water level.
7) Similarly, the changes in the heat transfer coefficient with the level of electrolyte are now quite negligible when using the partially silvered Dewars.
8) We have obtained a series of solution for the behaviour of our calorimeters which can be summed to give an approximate closed form expression. This expression is current to within 0.01 - 0.1% of the temperature-time transients obtained by numerical integration. We intended to use this expression to evaluate our own results as well as those obtained at Harwell. However, the expression is so cumbersome (combinations of the various coefficients) that this has not so far proved to be a useful approach.
9) In our earlier work we fitted the numerical integrals of the differential equations to experimental transients by using non-linear regression routines. It is a laudable approach but it is vastly expensive - we have been unable to make it "user-friendly".
10) Hansen - Melich have switched to a multi-linear regression fitting of the differential equation to the experimental data. They include the term

$$C_{P,D_2O,\ell}\, M^\circ\, \frac{d\Delta\theta}{dt} \text{ (our notation)}, \frac{d\Delta\theta}{dt}$$

being estimated by forward differencing; they include the second difference (as we do. The problem with this approach (also used by us) is that

$$\frac{d\Delta\theta}{dt}$$

becomes unreliable, just when you really need to know it. The reason is that the rate of data collection is too low (Shannon's theorem) (one reason why we opted for 9).

11) We now integrate everything and fit the formal integral of the differential equation to the experimental data (i.e. we sit somewhere between 9) and 10)). This avoids the need to differentiate experimental data but the main bonus is that we can use simple statistics and plain linear regression. Last but not least: we can get the heat transfer coefficients to about ±0.01%! Of course the whole idea is to raise the statistical significance of our data.

12) We have carried out a reanalysis of the Harwell FP H calorimetry and made a comparison with some of our own work. Our conclusions are briefly as follows:

    a. the general experiment design was very good.

    b. The execution of the experiments was terrible; the errors were horrendous.

    c. There method of calibration was invalid.

    d. Although they correctly identified several of the key characteristics of these calorimeters, the method of data analysis completely ignored these characteristics (it is simply lump them into the errors). In fact there was virtually no effective analysis applied to the measurements made with the FPH calorimeters.

    e. One can nevertheless find ways of calibrating their systems to give lower bounds of the rates of excess enthalpy generation.

    f. They observed excess enthalpy generation in 2 out of their 8 D_2O cells.

All this resides in two pages, submitted for publication but the refereeing held up (by us) to see what Hansen - Melich decide to do - also what Harwell decides to do.

13) We have a third paper on the Harwell reanalysis (on the stocks) but have held this up as it may simply reproduce the Hansen – Melich version.

14) We have reconstructed (the guts) of the Harwell isothermal calorimeter. The outcome is pretty disastrous. The system picks up H_2O they must have finished up with virtually no D in the lattice), the current distribution is completely nonuniform (hence no charging) and the temperature distribution is appalling.

This brings me to the comments made by the referees of your paper:

Referee #1 This is all very general. I dare say you will clarify the text where you can?

Referee #2 Again very general except for Comment 7. Harwell ran their cells for ~40 days. I do not know what you can do about this one except to quote the Hansen - Melich Nagoya Paper. We could send you some better diagrams but this would imply that you have access to the data, and these haven't been made generally available.

If you should want our help in rewording your text, then please send us this and markup where you would like to have suggestions.

Next year Steve Jones saga. Of course he is terrified that people will come to accept heat and ^4He (incidentally, we had our first indications of ^4He in December 1988!). The reason is simply that he messed us up because he wouldn't believe us. Very unwise. What you say about him taking up our work is nearly true. He did do some earlier work but in 10% D_2O - 90% H_2O! He evidently did not know about isotopic separation factors (his electrodes would have had an H/D ratio of about 100). Presumably he also believed that the H-D cross-section would be greater than the D-D one (lots of people believed that, that is why they were so slack about the H_2O content of their systems).

Did we ever tell you what happened when we had the referees comments on our application? I said to Stan: "Hey Stan, this is Stephen Jones and if we answer question X then we will tell him why we believe that there may be fusion in the earth and more likely Jupiter; if we answer question Y, we will tell him how to set it up in the laboratory". Prophetic words.

This brings me to my last but one topic which is somewhat related to your problems with Steve Jones, namely our problems with Douglas Morrison. Following the publication of our paper in Phys. Lett. A there was a crescendo of comments on the E-mail. We do not bother with all that innuendo so it may well be that this commentary started after the Nagoya Meeting? However, from time-to-time other research workers send us hard copies of these messages. As usual, Douglas Morrison was an active participant in these exchanges (it appeared to us that he was trying to polish up his critique) and, in the end, a number of people put pressure on various individuals to get Douglas Morrison to submit his critique to Phys. Lett. A. This he did (see attachment A) and he was clearly very keen to get this published. To this event, he sent it all around the globe, including all the editors of Phys. Lett. Of course, this gave us an opportunity to reply in the proper scientific literature (at long last!), See attachment B. With hindsight the outcome was predictable: the editors refused to accept Douglas Morrison's critique which they regarded as being too polemical and asked him to submit a shortened non-polemical comment. Naturally, this has once again deprived us of a "right to reply".

Douglas Morrison has said that he will produce a critique in a proper style but appears to be finding this to be inordinately difficult to produce. We can well imagine some of his difficulties - for one thing, the disparity between such a balanced account and his normal contributions would be glaringly obvious. Instead, he appears to have reverted to his normal mode of muck spreading.

We thought that you might be interested in these two Documents in view of your exchanges re Steve Jones. He too will find it difficult to be objective and you should in due course draw attention to the disparity between what he finally produces and the E-mail.

However, we thought that you would also be interested in what is becoming known as "Heat after Death". We already knew about this in 1990 (we were looking for it) and this is one of the reasons why we were so opposed to a public scrutiny and review of our work at NCFL. The "cat is out of the bag" now!

The final point is that we are glad to see that you are finally getting ^4He system on stream again. At various times we have discussed (also with M.M.) whether we could get some samples to you cut from the same batch as those where we have obtained high rates of excess enthalpy generation. These matters always drop out of view but your letter, coupled to the fact that I will

shortly have a meeting with J.M. brings us into focus again. Are you still interested in such a venture?

Best regards,

Yours sincerely,

Martin

Martin Fleischmann

Fleischmann attached to this message a draft of the critique by Morrison and the reply by Fleischmann and Pons. These two documents are here:

DEBATE BETWEEN DOUGLAS MORRISON and STANLEY PONS & MARTIN FLEISCHMANN

http://lenr-canr.org/acrobat/Fleischmanreplytothe.pdf

The draft that Fleischmann attached to this message has been added to that document, under "2017 ADDENDUM." It is repeated here:

COMMENTS ON CLAIMS OF EXCESS ENTHALPY BY FLEISCHMANN AND PONS USING SIMPLE CELLS MADE TO BOIL

Douglas R. O. Morrison

CERN, Geneva, Switzerland.

Abstract

Fleischmann and Pons have claimed to have performed a 'simple' experiment and to have observed excess enthalpies of greater than one kWatt per cm^3 of palladium. It is shown that in fact the system they use is exceedingly complicated, is under-instrumented and that they have ignored several important factors so that it is unclear whether they have observed any excess heat.

M. Fleischmann of Southampton and S. Pons of IMRA Europe, have published in Physics Letters A [1], a communication entitled "Calorimetry of the Pd-D_2O system: from simplicity via complications to simplicity". There they claim evidence for the production of specific excess enthalpy of greater than one kW per cc of Palladium in a Pd-D_2O system. They commented that this is comparable with the rates obtained in a fast breeder reactor. They note that the reproducibility is high. In this letter serious doubts are expressed about the justification for this

claim and the methods used to derive it - the experiment is not simple but is exceedingly complex.

Essentially they perform electrolysis in small transparent test tubes which are open so that the gases and vapor can escape freely. The cathodes are rods of palladium of 0.2 cm diameter and 1.25 cm length giving a total volume of 0.039 cm^3. Note that a specific excess enthalpy of 1 kiloWatt per cm^3, would correspond to only 39 Watts for the very small volume of palladium actually used. A thermistor was placed above the level of the top of the palladium rod - this gave the only temperature measurement inside the test tube.

The two cells with D_2O in 0.1 M LiOD solution, mentioned in fig. 8, are considered.

There are five stages:

STAGE 1. For 3 and 9 days, the cells receive a current of 0.2 A and are calibrated/refilled 1 and 7 times, resp. [58]

It was noted that at short times (hours in fig. 9.a) the heat transfer coefficient is markedly negative, that is, there is negative excess enthalpy - this they ascribed to the heat of absorption of deuterium ions entering the lattice.

STAGE 2. The current is increased to 0.5 A in the temperature jump to over 50 C. This stage lasts 16 days minus 14 hours.

The cells are calibrated/refilled once per day, that is about 15 times.

During this time, as shown in fig. 8, the voltage rises at first slowly and then more and more steeply and the temperature similarly rises slowly and then steeply, until the cells are about 85 C (as indicated in fig. 11). This stage ends about 14 hours before the cells boil dry (fig. 11).

STAGE 3. This lasted about 14 hours. It was the time until the cells boiled dry minus the final 600 seconds.

From fig. 11, the temperature of one of the cells (which had 3 days at 0.2 A) goes from 86 to 100 C. There is no clear sign of any calibration/refilling during this time.

From fig. 10b, the specific excess enthalpy derived varies erratically between about 15 and 30 W/cm^3 - since the volume of the palladium is 0.039 cm^3, this means the actual excess enthalpy claimed is only about 0.6 to 1.2 Watts. The calculation is made using a complicated non-linear regression analysis of the system which includes a square heating pulse (from the resistive heater in the cell) and adding D_2O to replace loss of liquid due to evaporation and electrolysis as indicated in figs. 4 and 5 – this heat pulse and its effect are the basis of the calibration.

STAGE 4. The last 600 seconds before the cell is dry.

[58] MCHM "For 3 and 9 days" may be a typo. It is does not make complete sense to me. JR I corrected a few obvious typos in Morrison's message. I left some of them, where I could not be sure what the author intended to say.

The behaviour near and during boiling is observed using a video camera. From this video, the time for the cell to go from about half empty to dry, is taken - more precisely the amount of liquid boiled off is estimated over the final 10 minutes before the test tube was declared dry. A new and apparently simple calculation is made in which the enthalpy input is taken as

(cell voltage - 1.54 V) * (cell current)

and the enthalpy output is assumed to be composed of two terms, the energy radiated and the heat resulting from the vaporization of the D_2O remaining in the cell 600 seconds before it is dry (this latter term is dominant). It is this simple calculation that gives the highest values claimed, namely "the excess rate of energy production is about four times that of the enthalpy input" and that the specific enthalpy is 3.7 kW per cc of Palladium.

STAGE 5. The authors note some further important features. "Following boiling to dryness and the open-circulating of the cells, the cells nevertheless remain at high temperature for prolonged periods of time (fig. 11); furthermore the Kel-F supports of the electrodes at the base of the cells melt so that the local temperature must exceed 300 C". No explanation is given and fig. 10 is marked "cell remains hot, excess heat unknown". From fig. 11 it may be seen that the temperature recorded on the thermistor above the palladium stays just above 100 C for three hours and then falls sharply.

STAGE 3 CALCULATIONS - there are two serious problems.

Firstly, a complicated non-linear regression analysis is employed to allow a claim of excess enthalpy to be made. This type of analysis by Fleischmann and Pons [2] has been carefully studied by Wilson et al. [3] who state that "they significantly over-estimate the excess heat . . . . . An additional significant overestimate of excess energy occurs when the calibration is made above 60 C". Now stage II is mainly above 50 C and rising to about 86 C. Further Wilson et al. write "Because of the paucity of experimental details in their publications, it has been difficult to determine quantitatively the effect of calibration errors." A reply by Pons and Fleischmann [4] did not address [5] the main questions posed by Wilson et al. From fig. 11, it appears that there were no calibrations in the temperature region of stage 3 - this must be considered a major omission in the design of the experiment. It is concluded that it is not possible to say whether or not there is excess enthalpy production.

Secondly it may be noted in fig. 8 of ref 1, that the cell voltage rises as the temperature rises and that as 100 C is approached, the voltage rises more and more steeply. Experience by the GE group [5] was that in operating similar open cells over many hours, they also noticed arise in cell voltage with time. They attributed this effect as being due to some of the escaping gas is carrying some Lithium salt with them. As the level of the electrolyte is maintained by adding fresh D_2O (but not any lithium salt), the concentration of lithium in the electrolyte decreases with time and the voltage rises. The GE group proved this by atomic absorption analysis. The cell resistance rises (causing higher voltage due to the constant current mode operation) due to loss of lithium salts which was caused by sputtering of electrolytic droplets up the gas outlet tube. This may be considered confirmation that even at moderate temperatures, the outlet stream contains liquids as

well as gases as will be discussed for stage four when the temperature was still higher and the boiling much more vigorous.

It may be concluded that claims of excess enthalpy in stage three, have not been established.

STAGE 4 CALCULATION. This calculation assumes that after a liquid level has been visually estimated from the videos, ALL the liquid below this level is converted into gas. However this neglects two factors:

Firstly the fluid injected from the open cell is assumed to be 100% gaseous. But with vigorously boiling, it normally happens that part of the fluid is in liquid form. This ejected liquid should not then be included in the calculation. This possibility is not considered and no reports of any measurement of the gas/liquid content of the fluids leaving the test tube is described.

Secondly, a further neglect is that as the liquid is boiling vigorously, it must contain gas bubbles and hence the estimate of the amount of liquid below the estimated level, should contain a correction for gas in the liquid - but this possibility is not discussed in the Fleischmann and Pons paper. This problem could have been answered if the enthalpy of the fluid escaping from the cell had been measured, but there is no indication of any such check having been made.

Another important problem is the estimate of the input energy - here the input enthalpy is taken as the current multiplied by the (cell voltage - 1.54V). It is not explained how these quantities are measured. This is crucial as when the cell is boiling vigorously, the impedance must be fluctuating strongly. Thus the current will have both an AC and a DC component. If only the DC component were measured, then the input enthalpy would be underestimated. A detailed description of the current and voltage measuring systems showing their fast response characters is needed, but is not presented, so that although the estimate may be correct, the question is not considered. Also the cell voltage over the last 600 seconds cannot be read from fig. 8 as the bin size is 500,000 seconds and the trace is rising exceedingly steeply - as this is an important question, one would have expected the voltage trace over the last 600 seconds to have been shown in great detail.

A further complication has been noted which invokes the “Leidenfrost” effect which is important with fast reactors (mentioned in the paper). With these reactors, there are no moderating atoms and the heat transfer rates are such that one cannot cool them by using normal water at one atmosphere. This is because of the Leidenfrost effect where the velocity of the water vapour escaping is so great that it stops water reaching the metal surface. It is like the effect observed when a drop of water falls on a very hot stove.

During the boiling in the last 600 seconds, the possibility needs to be considered of some hotspots on the palladium surface (because it is heated by the electrical current but not cooled by contact with liquid and also if the bubble stayed on the surface long enough, some catalysis could take place to heat the spot further). This hotspot would then keep away more liquid because of the vapour layer - so it would get still hotter. The extra turbulence would help to expel the liquid from the small test tube as liquid and not gas. All this is very complex and needs complicated calculations.

The mechanism of bubble formation in bubble chambers was first explained by Frederick Seitz [6]. The important point is that to grow, a bubble needs to be greater than a certain critical radius of about one micron. Below this radius the pressure of the surface tension which is inversely proportional to the radius, is very great and hence quickly kills bubbles whose radius is smaller than the critical radius. The critical radius is reached in a very short time, about a microsecond. Now the Palladium surface tends to be pitted after days electrolysis and would offer a good starting point for the nucleation of the bubble. The bubbles would tend to start again and again in the same favoured place. So it could happen that such a locality becomes quite hot which would generate more of the "Leidenfrost" effect. Initially the gas on the surface of the rod would be deuterium, but once the bubble exceeds the critical size, the electrolyte would also supply the gas in this could contain oxygen and hence permit catalyzed recombination. Another question is what does all this due to the impedance? Better information on this is needed.

The whole effect is very complex and made even more complex than in a fast reactor because the metal, palladium, acts also as a catalyst to recombine the oxygen and deuterium in the gases present! And this would help to host the hotspot still more. And there is also an electric current passing.

Again whether or not there has been any excess enthalpy, cannot be decided from the paper as important considerations are not discussed, information is missing, and proper controls that should have been performed, are not included.

STAGE 5 EFFECTS.

The melting of the Kel-F support below the palladium indicating a temperature of above 300 C, is presented as an "important feature". However there is the "cigarette lighter effect". In the last century, it was difficult to make reliable matches to light cigarettes. A reliable smokeless lighter was invented which consisted of a rod of palladium into which hydrogen had been introduced under pressure. This caused the lattice of the palladium to expand and thus stored energy. To light a cigarette, the top of the rod was uncovered; some hydrogen escaped releasing some of the strains and thus releasing energy which resulted in a small rise in temperature of the end of the rod. Palladium is a catalyst of hydrogen and oxygen which burned to give water plus energy. The palladium now slightly heated, catalyzes the escaping hydrogen and the oxygen of the air and the resulting heat of combustion which is mainly deposited on the surface of the rod, raises its temperature. This temperature rise releases more hydrogen which is catalyzed by the still more efficient hot palladium, and so on until the tip of the rod is so hot that the cigarette can be lit. The reliability of this system is high.

An interesting confirmation of this using electrochemistry was reported by Kreysa, Marx and Pleith [7]. They write "We have to report here that as we removed the deuterium-loaded palladium sheet from the cell and laid it on the table it did burn a scold into the table. One can still argue that this was due to deuterium fusion. Therefore we loaded the palladium sheet cathodic lay with hydrogen using an electrolyte containing only normal water (no enriched heavy water) and laid it on a piece of wood where it also burnt a scald." They say it releases 147.3 kJ

per mole D. [59] "The principle of flameless catalytic combustion of hydrogen" - the official name of the 'cigarette lighter effect' - "is used in catalytic hydrogen burners (D. Behrens (ed) Waserstoffetechnologie - Perspektiven fur Forschung und Entwicklung, Dechema, Frankfurt/M 1986)." To be more quantitative they laid a hydrogen-loaded sheet of palladium on to glass rods and "measured, after an incubation time of 15 s, a temperature rise of the palladium from 20 to 418 degrees within 74 seconds." The 15 second delay is the time during which the gradual escape of hydrogen releases a small amount of energy from the lattice, thus heating the palladium sufficiently for it to become an efficient catalyst. They estimate a heat flow of 35.9 W and a heat flow density of 179.6 W/cm^3".

The dramatic effect of the melting of the Kel-F support cannot be explained by Fleischmann and Pons as being due to electrolysis since there is no liquid no current and no electrolysis. However it is exactly what would be expected with the "cigarette lighter effect" where the hot palladium rod continues to catalyze the interaction of the hydrogen which is slowly escaping from the rod, with oxygen in the cell or from the air.

It might be expected that this effect would occur also with normal water, H_2O, being used instead of heavy water, D_2O, but no report is given in the paper of any results of tests of stages three or four using normal water, H_2O.

Because the volume of the palladium is so small, 0.039 c,3. The heat given out by the burning of deuterium inside it, is too small to account for the maintenance of the cell at near 100 C for three hours - another explanation is needed. It has been pointed out by T Droege [8] that this is a major problem for Fleischmann and Pons to explain why the thermistor records temperatures of remarkable stability, staying within a few degrees of 100 C although before boiling try there is the input electrical energy of 37.5 Watts plus their large claimed excess enthalpy of 144.5 Watts. But after boiling dry and the short-circuiting of the cell, there is still the enthalpy output to ambient (that is radiative heat loss) which they calculate to be 11 Watts. So how can the temperature be constant (or very slightly rising) when there is an 11 Watt loss and no incoming energy?

CONCLUSIONS

A number of effects have been presented which have not been considered before the authors claimed large excess enthalpies. It is not said here these effects necessarily explain everything with conventional (that is well-established) science. Until these effects are properly studied by the authors with a well-designed and well-analyzed experiment with adequate instrumentation (not just a thermistor and a video camera), and until for all five stages of the experiment a full description is given of what occurs when deuterium is replaced by hydrogen, it is unjustified to claim any new energy source.

The experiment and some of the calculations have been described as "simple". This is incorrect - the process involving chaotic motion, is complex and many calibrations and

[59] JR As I note in the introduction, 147.3 kJ per mole is based on the heat of formation of water, and as Fleischmann pointed out in the rebuttal, the cell produced ~1,700 times more energy than all of the deuterium in the palladium could have produced. Morrison should have seen this from the numbers he himself quoted here.

corrections are needed. The calculations have been made to appear simple by incorrectly ignoring important factors. It would have been better to describe the experiments as "poor" rather than "simple". A true "simple" experiment is one where corrections and calibrations can be reduced to a minimum. If one were to insist on using an open cell, then arguments about how much we combination of the D2 and O2 gases occurs, can be avoided by the standard electrochemistry techniques of using a divided cell or an H-cell where the anode and cathode are in the arms of the H so that they are far apart. However simplicity in calorimetry is best achieved by using a closed cell with a catalytic recombiner (e.g. a heated piece of palladium) and by enclosing the cell in a series (e.g. three) baths which are each kept at constant temperature. The cell is kept at a higher temperature than the innermost bath so that if any excess enthalpy is produced inside, the heating of this bath can be reduced to keep a constant temperature, and the excess is measured simply. Since this is a null measurement system, there is little need for complicated calibrations and calculations. It is to be regretted that after nine and a half years (the last four years well-funded) that Fleischmann and Pons say [9] they have been working on this, that they have employed such a simplistic open-cell system. [60]

It is interesting to note that the Fleischmann and Pons paper compares their claimed power production with that from nuclear reactions in a nuclear reactor in this is in line with their dramatic claims [9] that "SIMPLE EXPERIMENT' RESULTS IN SUSTAINED N-FUSION AT ROOM TEMPERATURE FOR THE FIRST TIME" [61] breakthrough process has potential to provide inexhaustible source of energy". It may be noted that the present paper does not mention "Cold Fusion" nor indeed consider a possible nuclear source for the excess heat claimed.

It is a pleasure to acknowledge the help and comments of many friends.

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J. Electroanal. Chem. 332(1992)1.

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[60] MCHM ☺ Obviously closed, not open – and this is a very unwieldy calorimetric dream. It is remarkable that after 9.5 years Douglass remained so ignorant and his comments so naïve. How does he calculate 9.5 years?
[61] MCHM This was, is and will remain the major stumbling block. Sustained N-fusion is theirs… not ours.

[8]. T. Droege, priv. comm.

[9]. Press release, University of Utah, 23 March 1989.

1993-10-22

IMRA

FAX

TO: Dr. M. H. Miles
NAWC Weapons Division
China Lake, CA
U.S.A.

October 22, 1993

Dear Mel,

I fear that I am once again falling behind with my correspondence. A sample of Pd – 10% Ag rod, 4mm diameter by 2 cm length has been sitting on my desk for some time now waiting to be sent to you! This has been cut from a rod another section of which gave reasonable excess enthalpy generation. When I spoke to Mike Melich about this, I suggested that we should send this to you as "Sample of Metal for Testing" and marked "of no Commercial Value". The documentation could state "To be returned to IMRA S.A., France at the completion of testing". Let me say, that we are not bothered by this – it might well turn out that you will wish to have back-up tests made in the U.S.? I have merely suggested this form of words in case sending the sample to you might create difficulties at your end. Let me know whether you agree with this procedure and/or give me alternative suggestions. Also, could you let me know which shipper we should use? Federal Express used to be very good at clearing customs but they have given up in Europe for the time being. However, I believe that we could get this to one of the main airports (where they still have offices) if you want us to use them.

We will dig out the thermal history of the piece we tested here and send this to you in due course. Needless to say, we will be very interested to hear the outcome of your experiments.

How was the meeting in Russia? All news, reports are welcome here?

Regards,

Yours sincerely,

Martin

Martin Fleischmann

1995-02-02

[RC First page of letter is missing; also might be wrongly dated; perhaps real date is 1995-02-10]

Dear Mel

I hope that I'll have time, one of these days, to write to you about the outstanding items!

Regards,

Martin

I am somewhat detached from my records but ……..... you will not be making a presentation?

The reason I am writing to you today (and I know that I have a whole stack of papers and matters on which I owe you replies!) is because I am seeing your advice and help with regard to one part of the session on Calorimetry/Excess Power. Our distinction between calorimetry and excess power arises because, on the one hand, some contributions deal principally with calorimetry whereas the authors did not observe excess power, while on the other hand other contributions deal with the observation of excess power whereas the description of the calorimetry is at best sketchy. We have it in mind that a sub-section of this Section should be devoted to "The Experimenters Regress". This is a notion which is drawn from the sociology of science. In brief at the start of any new controversial field one does not know whether, on the one hand, the "nil result" is correct so that all "positive results" are due to bad experimentation or whether, on the other hand, the nil results are incorrect and point to bad experimentation. The sociologists would contend that one cannot break the experimenters regress but I believe that they are mistaken. At any rate, if one can show that the main accepted negative result was in fact positive, then one can take a major step in breaking the regress.

We would be prepared to give a paper in such a sub-section, a part of which would be devoted to the analysis of the Harwell data sheets. Stan and I have in fact written three papers which, as you can imagine, are stuck in the refereeing process. From our point of view, therefore, it would be quite nice to ventilate this matter at ICCF-5. You will certainly see what is coming: Could you give us your frank opinion about the pros and cons of having such a Session, the pros and cons of discussing the Harwell data sets and last, but not least, would you be prepared to make a contribution to such a Session?

In the case of work on Cold Fusion, there is another very important point and that is that the Interpretation of the results itself shows that the calibration of instruments are subject to errors unless one recognises the presence of positive feedback. This question of the interrelationship of calibration and interpretation is a subject which is simply not recognized by scientists yet it is of critical importance. To me, the cause célèbre has always been the calibration of gravitational antennae. Perhaps it is true to say that Joe Weber himself did not recognise that the Interpretation of his observations showed that the calibration systems used by others (and eventually by himself) were incorrect?

Yours indefatigably,

Martin

Martin Fleischmann

1995-02-23

IMRA

FAX

TO: Dr. M. Miles

Dear Mel,

Please see the attached letter. I have to go to Lourdes (?) and will ask David Thompson to send you a further invitation on the Conference Header Paper.

Regards,

Martin

P.S. I dictated a further letter which I will send later today.

Dear Mel,

I was very pleased to see from your FAX of the 15th February that you are prepared to contribute to the section on "The Experimenters' Regress" which we are organising as part of the session on Excess Power Generation /Calorimetry of the 5th International Conference on Cold Fusion. With this letter, let me extend a formal invitation to you to present a paper outlining your analysis of published data, etc. Let me also confirm that we would appreciate it if you could concentrate on the interpretation of the results obtained in the California Institute of Technology. Needless to say, we do not wish to restrict you to this particular theme.

We look forward to your participation in ICCF-5.

Yours sincerely,

Martin Fleischmann

1995-03-01

IMRA FAX

1 March 1995

Dear Mel,

Many thanks indeed for your abstract which I received this morning. As you will know, I am discussing with Mike Melich how we might structure this particular session. I shall shortly be writing to him because I can detect that he is not keen on what he perceives to be my confrontational intent. I do not really have any such objective but I do believe that it is important at this stage in time to access the significance of all the investigations in as far as this can still be done. I believe that we also have to bear in mind that the trio of results at MIT, Caltech and Harwell still pose a tremendous block to further work. Here I detect a division of attitudes. Those who are able to work in the field (and even those who feel that they may be able to secure funding) want to project a very positive view and are, I believe, wary of developing a critique. Those who wish to work in the field but cannot secure funding would like to see a critique developed – especially if this led to criticism. My own attitude lies somewhere between these two points of view.

It may well be, therefore, that the session will turn out to be somewhat different to what I had intended. Thus, Wilf Hansen, you (and for that matter Mike – if I can persuade him) may well deal with the whole set of questions raised by the need to develop accurate calorimetry. Of course, we are also keenly interested in this topic but I think that I will definitely confine myself to the topic of "The Experimenters' Regress" as I originally perceived it.

I will try to clarify all this with Mike and if needs be, will write to you again. However, as I see it, your own contribution can fit into any chosen scenario.

I was sorry to see from your FAX (and also from the earlier correspondence) that you have some results which you will not be able to present. You will be in good company. I know of several groups who have some fascinating new work which will not see the light of day at ICCF-5. This is a great shame because the overall impression from reading the abstract is that the material is somewhat stale.

Best regards,

Yours sincerely,

Martin

1995-05-16

IMRA FAX

16 May 1995

Dear Mel,

As you see, I am again in the U.K. principally to deal with the next stage in the production of the book of the Conference Proceedings. This brings me to the first important point which is to ask you whether you wish to publish your comments, and secondly, whether you have already sent me a manuscript. According to my own records, we have not yet received this but, as you may know, we have had several prolonged strikes in the Post Office, both in Marseilles and Nice. I know that several papers which have been sent were not received by the time I left Sophia Antipolis last week; Jacques Payet has just been on the phone to me to say that we are now receiving papers at IMRA so it could we be that yours in the "pipeline". We certainly hope that you will produce a ms of your talk.

Another matter concerns your correspondence with Dr. El-Sayed and you will wish to know that I have written to him about Steve Jones' publication. My own view is that the question of whether or not the papers should be published "back to back" is strictly in the hands of the Senior Editor – it certainly is not a question which can be decided by the authors of the papers. You may be interested to know that Wilson et al. tried to keep their critique of our first major paper secret and that we only learnt about it by accident. Following remonstrations by me, it was Roger Parsons who decided that the paper by Wilson et al. and our reply had to appear "back to back" with our reply.

This was by no means a trivial decision because the G.E. Group did not refer to Appendix 4 of our main paper in which we explained how we had calculated the energy balances. Interestingly enough, they referred to everything else in the paper. One cannot help wondering why they did not refer to Appendix 4. We had very good reasons for carrying out the energy balance in the way we described and in point of fact, our scheme of calculation underestimated any excess enthalpy. However, Wilson et al took some raw data, together with the revised heat transfer coefficients and then applied the "correction". This produced a double subtraction error, so much so, that using their revised heat transfer coefficients, the cells would have had to behave as refrigerators – clearly impossible because the endothermicity of the electrolytic reaction had already been taking into account.

We do not know what status the paper by the G.E. Group may now have, but it seems to us that the fact that their critique with our reply appeared "back to back" has made it difficult for people to quote the critique. Needless, to say, the same situation must apply to Steve Jones' critique of your paper. We are also intensely curious to see what Steve Jones' second paper may contain. Presumably it is more nonsense.

More anon.

Martin

1996-12-08

IMRA FAX

Date 8.12.96 (December 8, 1996)

Dear Mel,

When I came back from Sapporo I wrote you a long letter which I then tore up. I then wrote you a short letter which I also tore up and then, in turn, started on a letter even longer than the first one! There are two reasons for these complications: firstly, your experiences with research in the Cold Fusion field are virtually a mirror image of my own; I therefore believe that I may well have some information which may still be useful to you – and, of course, this converse will be equally true. Secondly, I want to explore with you whether we might be able to collaborate to tidy up some loose ends? Tidy up is a misnomer because some of the efforts I have in mind will certainly be extensive!

The reasons then why I went through the sequence long letter/short letter was because I did not know whether I should raise so many issues in "one fell swoop". Furthermore, some of the information is delicate and would lead to even more delicate problems. However, I now believe that there is nothing to be gained by prevaricating so you can expect to get one of my long epistles; I hope this will not spoil your Christmas!

I have to go to Italy tomorrow for extensive discussion with Giuliano Preparata. He has a splendid new laboratory, LEDA (laboratory for advanced electrodynamics) funded by industry. The work is going brilliantly which I view with somewhat mixed feelings because my efforts into those particular lines of research have been uniformly frustrated. I shall take my third draft letter with me and will revise it while I am in Milan. I hope to post it on 18th December.

Until then, very best regards,

Martin

P.S. I should also tell you that I greatly appreciated your present at ICCF-6. Superb work! I also like you meeting report.

P.P.S. In 1989 we were able to make calorimetric measurements with an accuracy determined by errors of 0.1% (or ± 1 mW whichever was the greater). We have maintained this performance although I have been able to push to ± 0.1mW with the full-blown ICARUS -1 calorimetry and data analysis. The precision is determined by errors <0.01%. You may have seen that N.H. E. have managed to convert this to errors of >10%. It is truly mind-boggling. Part of my long letter will deal with the issues this raises.

P.P.P.S. Have you reflected on the significance of 100 kw cm^{-3}?

1997-05-12

NAWC fax heading

Martin,

I hope it can be arranged for me to work in Italy. There is no future for me here at China Lake. [62]

Best Wishes,

Mel Miles

[62] MM No funding for any project was received from Bob Nowak at ONR or other Navy sources following my cold fusion work at China Lake.

NAWC heading

May 12, 1997

Prof. Martin Fleischmann
Bury Lodge, Duck Street
Tisbury, Salisbury
Wilts SP3 6LT
United Kingdom

Dear Martin,

I am very anxious to begin work in Milan, Italy with Professor Preparata's group. I hope this can be worked out with the sponsors. Please give me your personal assessment of the situation for working in Italy.

The enclosed memorandums should clearly explain why I don't want to work here much longer. Younger managers have taken control, and they want me to leave. Furthermore, I feel that I have been unfairly blamed for the failure of the Navy's investment in cold fusion. Defense cutbacks have also made it very difficult for everyone at China Lake.

If I am not going to Italy, then I need to look for work elsewhere in the government or at universities. If at all possible, I would really like to work with you in Italy and experimentally test some of your ideas that we discussed in February.

I have not received any reply to my letter to the FBI concerning the crash of TWA Flight 800 last summer. The FBI, however, recently announced that the crash was apparently due to mechanical failure rather than caused by any missile or bomb.

I am really hoping that I can work on cold fusion in Italy. Please do whatever you can to help.

Sincerely,

Melvin H. Miles

Dr. Melvin H. Miles
NAWCWPNS Fellow

NAWC heading

May 12, 1997

MEMORANDUM

From: Dr. Melvin H. Miles (Code 4B2300D)

To: Dr. Robert W. Gedridge, Jr. (Code 4B2300D)

I have always been a top performer in terms of research accomplishments at NWC Corona Laboratories (1967-1969), at Middle Tennessee State University (1969-1978), and at NAWCWPNS (1978-present). Therefore, your memorandum of 22 April, 1997 leaves me very upset and dismayed. These are indeed very difficult times at China Lake.

My accomplishments thus far for FY-97 include work on 9 publications, 6 presentations, and 3 patent applications (see Enclosure 1). These efforts involve three different research areas (lithium batteries, corrosion, and cold fusion). During this time period, I have also been involved with proposals for four possible sponsors (see Enclosure 2). I will certainly not be this productive while following a rigid work schedule and performing stockroom tasks.

There are several errors in your memorandum that need to be corrected. I was <u>not</u> working on a cold fusion manuscript on Sunday, 13 April and Monday, 14 April as you stated. Instead, I was working on an invited lithium battery paper for Richard Marsh - a potential sponsor for my research (see Enclosure 2). My note to you on 15 April, 1997 (your enclosure 2) made it clear that I was working on a battery paper and not the cold fusion manuscript you cited. The after hours log for the key to the Technical Library documents that I worked 9 hours 44 minutes Sunday, 13 April and 4 hours 43 minutes on Monday, 14 April to complete this battery manuscript. Therefore, I had a total of more than 18 hours of ~~after-hours~~ work one day into the work week. I never imagined that working many more hours than required could get me into this situation. I thought submitting an invited paper to a possible sponsor for battery research was part of my job.

I did not request comp time for cleaning out the office for Pan Carpenter (your Enclosure 3). I was simply documenting my after hours of work as requested by Dr. Nissan. I had to wait for Dr. Kendall Johnson to go through his material in that office before I could clean it out. I completed this job the same day that Dr. Johnson sorted through this material. Pam Carpenter was not concerned about the delay, and she did not move in until the following week.

I have not requested support to attend the IECEC Conference from 27 July to 1 August, 1997. However, I feel that my meeting with Richard Marsh and other battery people at this conference is very important for future research support. I would be willing to pay my own travel expenses to attend this conference and to present two invited papers. It is possible that Richard Marsh, Grover Coors, or Dave Nagel may sponsor my trip. These are possible sponsors for future research.

I have always enjoyed the flexible hours and after hours use of the library allowed in the past by Chemistry. This has contributed to my scientific productivity. No policy should discriminate

against any individual. If I am forced to work a rigid schedule, then everyone else in my organization should have to do the same. I have never abused the system of flexible hours, but rather have worked more hours than required.

Your memorandum of 22 April, 1997 has caused me severe stress and illness. I could not work the rest of that week and had to use sick leave. Your timing was also very poor. Three days after getting married, I received this memorandum rather than any card or acknowledgment of my marriage. It was not a pleasant present.

I cannot fulfill my duties and responsibilities as a Research Chemist for the Navy under the restrictive guidelines of your memorandum. Therefore, in my scheduled meeting with Dr. Ron Derr I will have to request a transfer or some other solution.

Sincerely,

Melvin H. Miles

Dr. Melvin H. Miles
NAWCWPNS Fellow

Copy to:

4BD00D
4B200D
4B2D00D
731000D (Peoples)
4B0000D (Derr)

FY-97 Publications Presentations, and Patents
(October, 1997 - April, 1997)

Dr. Melvin H. Miles

Publications (9)

1. "Heat and Helium Measurements using Palladium and Palladium Alloys in Heavy Water" M.H. Miles, KB. Johnson and M.A. Imam in Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 1, Japan, 1996, pp. 20-28.
2. "Electrochemical Loading of Hydrogen and Deuterium Into Palladium and Palladium-Boron Alloys" M.H. Miles and K.B. Johnson in Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 1, Japan, 1996, pp. 208-212.
3. "Improved, Open Cell, Heat Conduction, Isoperibolic Calorimetry" M.H. Miles and KB. Johnson in Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 2, Japan, 1996, pp. 496-501.
4. "Reply to S.E. Jones and L.D. Hansen concerning claims of Miles, et al. in Pons-Fleischmann-type Cold Fusion Experiments" M.H. Miles in Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 2, Japan, 1996, pp. 524-527
5. "Nuclear products Associated With the Pons and Fleischmann Effect; Helium Commensurate to Heat Generation, Calorimetry, and Radiation" B.F. Bush, J.J. Lagowski and M.H. Miles in Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 2, Japan, 1996, pp. 622-626.
6. "Corrosion Inhibition of Aluminum Alloys Coated with Poly (2.5-bis(N-Methyl-N-Alkylamino) Phenylene Vinylenes" P. Zarras, J.D. Stenger-Smith and M.H. Miles in PMSE-ACS, Vol. 76, 1997, pp. 589-590.
7. "Reply to Examination of Claims of Miles et al. in Pons-Fleischmann-Type Cold Fusion Experiments" M.H. Miles, J. Phys. Chem. (accepted for publication).
8. "Anomalous Heat and Helium Production Using Palladium-Boron Alloys in Heavy Water" M.H. Miles, K.B. Johnson, and M.A. Imam, IECEC-97 (submitted).
9. "The Effect of Passivating Films Involving the Lithium Anode in Thionyl Chloride, Bromine Trifluoride, Molten Nitrates, and Molten Perchlorates" M.H. Miles, DECEC-97 (submitted.)

Presentations (6)

Papers 1 - 5: Presented at ICCF-6, Hokkaido, Japan, 13 - 18 October, 1996.

Paper 6: "Summary of Navy Research on Anomalous Effects in Deuterated Systems" Invited presentation at Pirelli Cavi SPA, Milan, Italy, 25 February, 1997.

Patents (3)

1. “Corrosion Inhibition of Aluminum Alloys coated with Poly (2.5 - Bis (N-Methyl-N. Alkylamino) Phenylene Vinylenes”

P. Zarras, J.D. Stenger-Smith, and M.H. Miles Navy Case No. 78281.

2. “Improved, Isoperibolic Electrochemical Calorimeter” K.B. Johnson and M.H. Miles (in preparation)

3. “Heat Producing Palladium-Boron Alloy Cathodes” M.H. Miles, K.B. Johnson, and M.A. Imam (in preparation).

NAWC heading

12400
4B2300D/101
22 Apr 97

MEMORANDUM

From: Head, Materials Characterization Section (Code 4B2300D)
To: Dr. Mel Miles, Materials Characterization Section (Code 4B2300D)
Subj: Procedural Guidance

Encl: (1) Policies and Procedures Chemistry & Materials Branch Code 4B2000D
(2) Memo of 15 Apr 97 from Dr. Miles
(3) Memo of 16 Apr 97 from Dr. Miles

1. The intent of this memo to clarify Code 4B2000D policies regarding leave, compensation time, and Your tasks for the remainder of FY97 when using the Chemistry and Materials Branch Downtime or other Branch Overhead Job Orders.

2. The Centers basic working hours are 0600-1800. Your Core working hours are 0800-1100 and 1300-1500, which includes a 1-hour lunch. I expect you to be at work or on some type of **pre-approved** leave during each work day. During the 14 April meeting between Yourself, ~~Dr.~~ [63] Robin Nissan (Head, Chemistry and Materials Branch), and myself, you stated that you had worked late Sunday, 13 April, on a manuscript and, therefore, took compensation time (comp time) on Monday, April 14. The manuscript in question, "Heat and Helium Measurements Using Palladium and Palladium Alloys in Heavy Water," is for a scientific project which has not received direct funding since FY95. you were late for the requested 1400 meeting on April 14 and you did not report to work until 1430. You did not notify myself or the 4B2000D office of your absence. As stated in enclosure (1), "..let your supervisor and Pam or Ruth know if you are going to be on leave. This can be done with Qmail, voice mail, or verbally."

3. As a result of the 14 April meeting, you placed a memo (enclosure (2)) in my mail box on 15 April 97 stating, "I spent 9 hours Sunday working on this paper and 5 hours this evening. ....therefore I am taking 5 hours comp-time for that afternoon." A second memo from you (enclosure (3)) was placed in my mail box on 16 April 97 stating, "...I am documenting 3 hours of after hours work on Wednesday evening, 16 April 1997." These memos are not requests for comp time with my prior approval. An employee can request compensation time in writing, Qmail, voice mail, or verbally and only the employee's supervisor can authorize this prior to an employee taking comp time. Furthermore, it should be noted per enclosure (2), that you were working on manuscripts and/or presentations for an effort which has not received direct project funding from the Navy or anyone else to perform since FY95. Therefore, your requests for comp time for working on unfunded projects or cleaning out an office during after hours are denied. As

[63] JR Miles crossed out "Dr." with red ink because, he explained to me: "Robin Nissan was . . . a typical fat government politician rather than a true scientist. Although he liked to use the 'Dr.' title, he did not have a Ph.D degree."

stated by Dr. Robin Nissan at the 14 April meeting, 'There is no authorized comp time for people using the Downtime Job Order who do not have direct project funding to work on a project."

4. Unless you request annual leave or LWOP, your Time & Attendance Card will reflect "Absent With Out Leave (AWOL)" from 0800 until 1430 on 14 April 97. Enclosure (2) request for 5 hours of comp time by you is denied and 5 hours of annual leave or leave without pay (LWOP) may be used on your time card for 15 April 97. Enclosure (3) documenting 3 hours of after hours work by you to clean out an office is not suitable justification for comp time since this work should have been performed during your core working hours. It was requested by Dr. Nissan that you clean out that office approximately three weeks ago.

Subj: Procedural Guidance

5. In accordance with Code 4B2000D policy, the Chemistry and Materials Branch's Downtime Job Order can be used by you and other personnel within Code 4B2000D to write proposals to secure direct project funding if no direct project funding is available for their use. It is inappropriate for you to use the Chemistry and Materials Branch's Downtime Job Order for travel to scientific meetings and specifically the 32nd Intersociety Energy Conversion Engineering Conference in Honolulu, Hawaii, from 27 July to 1 August 1997. You may attend this meeting if direct project funding from a sponsor to support this travel, research and activities has been accepted by Nancy Maegaard (Administrative Officer for Code 4B2000D).

6. My recommendation to Dr. Nissan is to deny you the use of the Chemistry and Materials Branch's Downtime Job Order to work on unfunded tasks or travel to the 32nd Intersociety Energy Conversion Engineering Conference in Honolulu, Hawaii, from 27 July to 1 August 1997 and any other future scientific meetings that do not have direct project funding support.

7. The purpose of the 14 April 97 meeting between ~~Dr.~~ Nissan, Myself, and Juanita Morton (Stockroom Manager), and various personnel from 4B2300D was to identify Stockroom tasks that Code 4B2000D must perform in order to meet Navy requirements for pollution prevention documentation and to match those tasks to individuals who are not funded on direct project dollars but are funded using the Chemistry and Materials Branch's Downtime Job Order. It was stated at this meeting that you, as well as all other personnel using the Chemistry and Materials Branch Downtime Job Order, are to report to Juanita Morton to perform stockroom tasks of chemical inventorying, assist in cleaning the Dallas Hut, or assist in cleaning Building 1 in the China Lake propulsion area unless they are working on direct project funding. Exceptions could be made at the supervisor's discretion for specific proposal writing or support for marketing of proposals.

8. Effective immediately, you will follow the core working hours for 9-hour work days Monday through Thursday from 0800 until 1800 and 0800 to 1700 on non-flex Fridays which include a 1-hour lunch. Any change from this schedule must be received in writing to Dr. Nissan or myself. We will consider your request at that time.

9. For the remainder of FY97, you will report to Juanita Morton at 0800 each work day to perform stockroom tasks or other tasks for the Chemistry and Materials Branch unless you are

working on a project with direct funding available. Any exceptions will be at the discretion of your supervisor. Performing stockroom tasks or other tasks for the Chemistry and Materials Branch when using the Chemistry and Materials Branch Downtime Job Order will be added to your FY97 Performance Plan. You will meet with me on 23 April 97 at 0800 to discuss this and other aspects of your FY97 Performance Plan for the 3rd quarter.

10. Contact me at 939-1648 if you have any questions.

Sincerely,

(signature)

ROBERT W. GEDRIDGE, JR. [64]

Copy to:

4BD000D
4B2000D
4B2D00D
731000D (Peoples)

[64] MM This memo was prompted by Robin Nissan, Head of Chemistry

Postscript to these events, in an e-mail from Miles to Rothwell, 2018:

". . . I was saved from being forced to leave China Lake following this "Stockroom" memo by the head of Research at China Lake, Dr. Ron Derr, who stated: "If this is how we treat our top people at China Lake, then we may as well close shop". Robin Nissan backed off following this, but he never gave me any internal funding. Furthermore, Richard Carlin, of ONR refused to fund me because I had worked on cold fusion."

1997-05-12 #2

Bury Lodge heading

12th May 1997.

Dear Mel,

I am most distressed and dismayed to have your news. My wife has seen some of the correspondence and says that it is all beyond belief!

I know that I owe you some replies on several matters and, also, that I have to raise a number of additional issues with you. I will be writing to you at some length later this week and I hope that I will be able by that time to clear up the position with regard to the Group in Milan, I know that they hit some organisational snags recently and I now need to find out whether all the matters have been satisfactorily resolved,

I think that "Congratulations on your Marriage" fits rather uneasily into this letter but I fear that this cannot be helped. My wife joins me in sending you our Best Wishes!

There are just two more things I want to say today: the first is that I have the highest regard for you as a scientist - I believe that you know this; the second is that you are one of the few people I have met who has total integrity.

Much good this has done you!

Yours sincerely,

Martin

1997-06-17

NAWC fax heading

Martin,

I am still hoping for Italy but may wind up at a university on Pax River in Maryland.

I would like to try your idea $X^- + OH^- \rightarrow Y = +H_2 \uparrow$ *somewhere. Does* X^- *have to be the duty rated form? I am having trouble locating a source for this.*

It was reported on TV last week that the TWA flight 800 crash was due to a fuel-vapor explosion in the central fuel tank – the one likely depleted by the air-conditioning while sitting on the runway.

There has been no response to my letters regarding this TWA crash.

Best Wishes,

Mel Miles

1997-06-18

Martin,

I tried to fax this several times yesterday and today, but it would not go through. . . .

I talked with Emilio and Guiliano this morning. They have lost their support from Pirelli but still hope to arrange for me to work in Milan starting sometime this Fall. I can probably delay any other job decision until then.

Best Wishes,

Mel Miles

P.S. The Patterson cell was discussed on Good Morning America for about 10 minutes last week. Eugene Mallove was on a national late-night talk program for several hours – also last week.

1997-09-10

Melvin H. Miles, Ph.D.
P.O. Box 6202
Ridgecrest, Ca. 93556
760-939-1652

September 10, 1997

Martin,

I have received a six-month fellowship offer from NEDO to work at NHE in Sapporo, Japan. This would begin in October and go until April, 1998 when the NHE program ends. The salary ($58,000/year) would be considerably less than I receive here, hence I am not sure I can afford to go. However, I would enjoy getting back to cold fusion research. My first choice would be to work with you, Giuliano, and Emilio in Italy, if that is still possible. It would also be nice to work 6-months in Japan and then go to Italy, if that could be arranged.

I would like to try Preparata's thin wire design if I go to Japan. However, I would not want to give away anything that is considered confidential. Therefore, I would limit my experiments to concepts that have been published by Preparata. Would this be a satisfactory arrangement?

Please let me know what possibilities still exist for my working in Italy and any comments you have regarding a six-month tour in Japan.

I need to give NHE my decision within a few days.

Sincerely,

Mel Miles

Dr. Melvin H. Miles

1997-09-10 #2

Melvin H. Miles, Ph.D.
P.O. Box 6202
Ridgecrest, Ca. 93556
760-939-1652

September 10, 1997

Emilio,

I will try to telephone you tomorrow at 10:00 a.m. California time using the telephone number 39-2-7060-3324. If this is not convenient, please send me a fax at 760-939-1617 suggesting another time or day.

I have a six-month fellowship offer from NEDO to work at NHE in Sapporo, Japan. This would begin in October and go until April when the NHE program ends.

I would rather work with you in Italy if that is still possible.

I need to give NHE my decision within a few days.

Best Wishes,

Mel Miles

Dr. Melvin H. Miles

P.S. Please give me Martin Fleischmann's fax number.

(MM Postscript added in 2018: *I went to the NHE in Japan. I did not communicate with Martin while I was in Japan.*)

1997-12-01

[RC Many pieces of this are missing.]

From lost NHE notebook

Cathodes for F/P Experiments

(See also p.54 of Notebook #1, China Lake cells)

Note: Repaired anode glass tube _____ epoxy about two weeks ago – looks fine – cell #1

F/P#2 Pd-0.5B (0.5 wt. ___ B) 4.8 x 20.1 mm cell #2

-from NRL

-Polished, cleaned by Mari

Diamond paste (No other polish _____)

x 20.1 mm → V = Pi R^2 L = 0.350 cm^3 A =Pi R^2 _ Pi D L = 3.15 cm^2

F/P #3 Pd-Ce (from M. Fleischmann) cell#1 4.00 x 19.6 mm

→ 3.16 → 19.54

-gave excess heat in China Lake experiment

-polished by mself with Si-C paper

-Dificult to remove long cracks

Polished by Mari to remove cracks

- Final polish by myself using S-C paper (using latex gloves)

3.16 x 19.54 mm → V= pi R^2 L – 0.153 cm^3 A = Pi R^2 + Pi D L = 2.02 cm^2

F/P #1 Pd-Ce-B, NRL sample, 4.4 x 20.05 mm.

-polished only by myself using Si-C paper

-Final polish wearing latex gloves

-No measurable changes in dimensions due to polishing

x 20.05 mm → V = pi R^2 L = 0.305 cm^3 A = pi R^2 + pi D L = 2.92 cm^2

→ Video made for each electrode using microscope (Mari)

Labeled.

Microscope check

Pd-0.5B – highly polished, very shiny. A few holes, scratches are visible

circular polishing lines but very few.

Pd-Ce – Duller finish, longitudinal lines from some Si-C polish

small cracks near ends; a few holes.

3 – Pd-Ce-B – Dull [RC File edge missing; see original paper to finish transcribing]

D_2O Levels (Notebook)

Day 53 →	90.5 cc LiOD (0.2M LiOD	0.500A → 0.600A	??????
Day 54 →	(no addition)		
Day 55 →	90.0 cc	0.600A → 0.800A	+10.0 cc D_2O
Day 56 →	88.0 cc	0.800A → 0.700A	+5.0 cc D_2O
*Day 57 →	94.0 cc over-filled	0.700A → 0.500A	+12.0 cc D_2O
Day 58 →	(no addition)		
Day 59 →	(no addition)		
Day 60 →	90.0 cc	0.500A → 0.900A	+8.0 cc D_2O
Day 61 →	91.0 cc not over-filled	0.900A → 1.00A	+9.0 cc D_2O
Day 62 →	91.0 cc		+8.0 cc D_2O
Day 63 →	90.0 cc		+8.0 cc D_2O
*Day 64 →	97.0 cc over-filled	1.000A → 0.800A	+15.0 cc D_2O
Day 65 →	(no addition)		
Day 66 →	(no addition)		
Day 67 →	89.0 cc	0.800A → 1.005A	+13.0 cc D_2O
Day 68 →	(no addition) → boil off		
Day 69 →	Cell dry		

82 c fills to bottom of silvered _____

+8 cc → normal level

=90cc

Fig. 21

mole Pd = (0.350 cm^3)(12.0 g/cm^3)(1 Mol/ ???????)= 0.0395 mol

-3800 J / 0.0395 mol = -96.2 KJ/mol Pd

(-96.2 KJ / mol Pd)(1 mol pd /1 mole D)(2 mol D / 1 mol D2) = -192 kJ/mol D2

still too high

[RC This is the left-hand side of the paper]

(0.035 W)(130,000 s) = -4550 J

(0.030 W)(130,500 s) = -3915 J

Notebook

I = 150 mA @ 10:00AM

@ 1340 → gassing _____????____

13,200 s to load

(0.05 W)(13,000 s) = 650 J

6505/0.0395 mol = -16.5 KJ/mol pd????

(-16.5 KJ/mol p)(1 mol pd/1 mol D)(2 mol D/1 mol D2) = -33 KJ/mol D2

(0.051 – 0.033) W x 12,000 s = -2345

-5.92 KJ/mol

(-5.92 KJ/mol pd)(1 mol pd/0.5 mol D)(2 mol D/1 mol D2) = -24 KJ/mol D2

[RC This is the right-hand side of the paper]

(0.150A)(?????) / (96.475 ??????) = 0.0395

(0.0395 mol)(96.475 A . s/mol) / (0.150 A) = 25 kWs for ????????????

D_2O + e- → ????????+ OD-

For 13 ??????????, D/P=0.51

15,250s to load PdD0.6

[RC Next Page of lost notebook notes]

15,250 s B+D → endothermic reaction

Jan. 31 -Dennis Peterson

Feb. 1 E. ?????????

Feb. 1 Bill Wittar 253-773-0531

Feb. 4 Iman

Feb. 7 ???????

Feb. 9 Iman

Feb. 11 ????????

Feb. 11 Iman

1998-05-11

NAWC fax heading

Please see letter

I plan to send Dr. Asami an Appendix to my NHE report covering this "heat after death" effect. If you can send me your comments soon, I will incorporate them into my NHE report.

Thanks,

Mel Miles

NAWC heading

DATE: May 11, 1998
TIME: 3:00 p.m.

TO: Professor Martin Fleischmann
FROM: Dr. Melvin H. Miles

Number of Pages including cover sheet: 7

MESSAGE:

Dear Martin,

I enjoyed talking with you at ICCF-7 and hope you had a pleasant trip back home. Professor Takahashi visited me here at the end of April, and we had some good discussions.

I am sending you some material relating to the "heat after death" effect from my experiments in Japan. This is part of a report that I am sending to Dave Nagel of NRL. I am also sending you a print out of my results recorded in my lab notebook. I would like to know how long the cell remained hot after it boiled dry, but this would be the computer acquisition data on the disk that I gave you. Have you had a chance to look at this yet? When you are finished please send the disks back to me. It will be interesting to compare your analysis with the analysis I made for my NHE report. The analysis method used by Japan showed no excess heat, but my analysis showed an excess power in the range of 200 mW for two out of the three cells. The excess power exceeded 10 watts according to my calculations after the cell boiled dry.

I have not heard anything from Italy, but I hope I can find someplace to continue my cold fusion research. I was quite happy with my results in Japan and will send you a copy of my final NHE report in a few weeks. I hope eventually to get some publications from this research.

Best Wishes,

Mel Miles

NOTE BY JR, 2018: This indication of apparent heat after death event turned out to be a mistake. This is described below in the documents under 1998-06-30. We include this letter in the collection partly to show that even a careful professional scientist will make mistakes from time to time. A different analysis by Fleischmann later revealed evidence for heat after death, but this initial indication was a mistake.

5877900

Notebook Data (Japan)

Boil-Off/Japan

t (s)	T2 (°C)	E2 (V)	~~I2~~ I_2 (A)	Tb (°C)	Wt. (g)
5871000	95.10	11.40	0.999	22.06	129.97
5871300	95.22	11.47	0.999	22.07	129.97
5871600	95.34	11.51	0.999	22.06	129.97
5874000	96.51	12.05	0.999	22.06	129.97
5874600	96.79	12.23	0.999	22.06	130.22
5879700	98.14	12.98	0.999	22.06	132.10
5880000	98.82	13.63	0.999	22.06	133.97
5882100	99.30	14.24	0.995	22.06	136.32
5885100	99.83	15.30	0.996	22.06	140.77
5886000	99.98	15.77	0.996	22.06	142.85
5888100	100.27	16.96	0.995	22.06	147.42
5888900	100.38	17.58	0.994	22.06	152.00
5890800	100.61	18.77	0.993	22.06	156.12
5894100	101.01	25.90	0.982	22.07	170.37
5895300	101.36	49.97	0.939	22.06	183.80
5897100	108.41	57.93	0.003	22.06	210.14
5898600	107.67	57.93	0.004	22.06	210.14
5900400	107.88	57.93	0.009	22.05	210.14
5901300	107.34	57.92	0.009	22.05	210.14
5902200	107.20	57.91	0.010	22.06	210.12
	T2	E2	I2	Tb	Wt.
5961300	34.48	108.32	0.006	22.06	210.12
5961900	33.91	107.96	0.013	22.05	210.12
5962500	33.73	107.48	? 0.009	22.05	210.12
5964900	25.57	1.15	0.000	22.05	210.12
5966100	23.21	1.08	0.000	22.06	210.12
5967600	22.46	1.09	0.000	22.05	210.12
5969100	22.26	1.09	0.000	22.05	210.12
5972400	22.16	1.09	0.000	22.05	210.12
5974200	22.14	1.10	0.000	22.05	210.12
5977200	22.13	1.10	0.000	22.06	210.12

($K = 8.112 \times 10^{-10}$ W/K^4)

February 10, 1998

February 11, 1998

Pd-0.5 wt.% B (4.71 x 20.1 mm Rod)

$V = 0.350$ cm^3, $A = 3.15$ cm^2

M.H. Miles

APPENDIX

I conducted one boil-off experiment in the Fleischmann/Pons Icarus 2.00 system at the NHE laboratory. This experiment involved the Pd-0.5B rod prepared at NRL that showed excess heat during my experiment in Japan. The normal NHE treatment of this boil-off data concluded that there was no excess power present. My own experimental observations, however, suggested the presence of excess power and even significant "heat after death" as reported by Fleischmann and Pons. First, there was a furious boiling and swirling action around the cathode during this boiling process. Second, the cell remained hot for several hours after the contents had boiled away despite the sudden drop in the input power to near zero. This effect is shown in Figure A.1. It is quite remarkable that the cell temperature actually increased after the cell boiled dry and the input power plummeted. I pointed this out to Mr. Sumi at NHE, but he passed this off as being typical behavior and did not actually indicate excess heat. This suggests to me that there was also excess heat in previous boil-off experiments at NHE, but this effect was not recognized. I recently shared this data with Martin Fleischmann, and he immediately recognized this effect as "heat after death". More precisely, this means excess heat that continues long after the cell becomes dry and the electrolysis ceases. Both his calculations and mine show the presence of an excess power effect of about 10 W (10.237 W in my calculations). This is by far the largest excess power effect that I have ever observed in any cold fusion studies. Simple calculations yield an excess power density of 29 W/cm^3 for this Pd-0.5B cathode obtained from NRL.

This "heat after death" effect should be compared with similar control experiments using palladium and platinum cathodes that do not produce excess heat. Also the cell should be calibrated when it is filled with gas (mostly D_2O vapor) rather than liquid. Martin Fleischmann told me that this has been done and that the cell constant remains about the same. The cell constant is actually controlled mainly by the vacuum gap across the Dewar cell. The transfer of heat by radiation would still depend only on the temperature inside the cell and the bath temperature, i.e. $P_{out} = K (T_{cell}^4 - T_{bath}^4)$. This is what I assumed in my calculations.

I still need to find time for the completion of my data analysis of this boil-off experiment. This will determine the amount of excess heat during the actual boiling process. However, Martin Fleischmann does not like the Icarus 2.00 system since there is considerable lag time between boiling and the distillate reaching the balance via a long glass tube. The distillate collects and pools in the glass tubing, hence its rate of delivery to the balance does not generally correspond to the power present in the cell. My preliminary calculation shows rather large fluctuations in the excess power calculations during the actual boiling that are likely related to this delay effect.

It is difficult for me to find any other explanation for the cell remaining hot and the temperature even increasing after the cell boiled dry other than the "heat after death" explanation. The only other explanation would be the recombination of the deuterium with oxygen as it exits the palladium. This is not likely since the cell contains mostly D_2O vapor. Furthermore, my calculations show that for PdD_x where x=1.00, the complete combustion of the deuterium in the palladium with oxygen would only sustain 10 watts of excess power for 19 minutes. Therefore, recombination cannot explain why the cell remained at about 108°C for several hours.

The next day following the boil-off experiment, the cell was still warmer than the bath (34.48°C vs. 22.05°C). This effect, however, could be explained by the high rail voltage of the potentiostat (108.32 V) and the small current (0.010 A) though the salt connecting the anode and cathode to give an input power of 1.08 watts. Figure 2 shows how the cell temperature quickly responded to completely switching off the potentiostat (zero input power). If there is no excess heat effect from the cathode, then the cell temperature promptly begins to decrease when there is a drop in the input power to the cell. The immediate response to decreasing the input power is in stark contrast to the behavior shown in Figure 1. This provides further strong evidence for the "heat after death" effect as reported by Fleischmann and Pons. I am preparing an Appendix D reporting my "heat after death" results that I will send to Dr. Asami to add to my NHE Final Report.

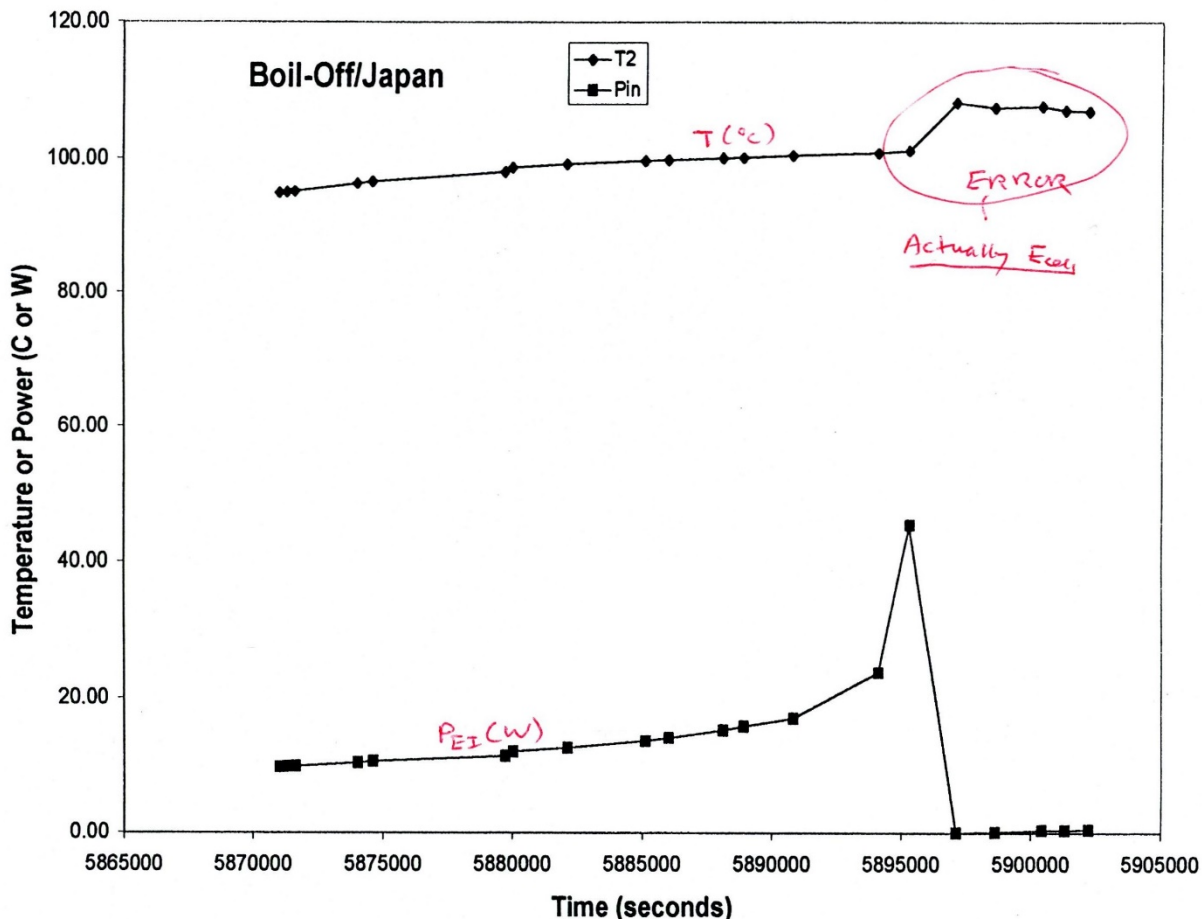


Figure A.1. Cell temperature and cell input power during the boil-off experiment using NRL Pd-0.5B cathode. The cell boiled dry and the electrolysis ceased at 5895000 seconds.

NOTE AT TOP added by Miles later on: "ERROR Actually E_{cell}." This is described under documents 1998-06-30. Mr. Sumi sent data and he explained that Miles made a mistake, confusing channel E2 with T2. See the Memo dated July 23, 1998.

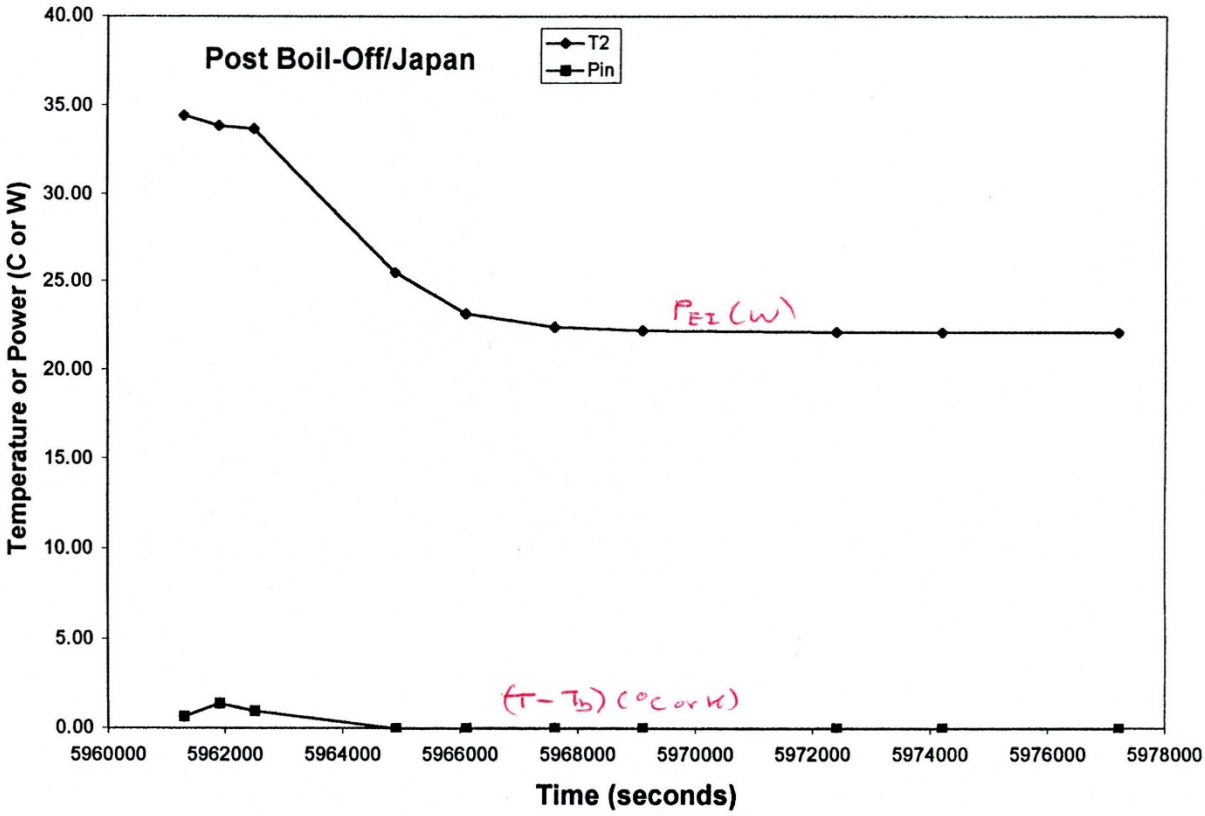


Figure A.2. Cell temperature and cell input power for the day following the boil-off experiment. The power to the cell was turned off at 5963000 seconds.

1998-05-14

Bury Lodge heading

Dr. Mel Miles,
NAWC Weapons Division,
China Lake,
California.
U.S.A.

Dear Mel,

Many thanks for your FAX. I need to write in some detail to you about "Heat after Death" which I have always regarded as one of the critically important aspects of research into "Cold Fusion". However, this will take some time so meanwhile herewith some abbreviated comments.

So far, we have been unable to retrieve the data from your disk. I suspect this is due to the fact that my P.C. does not have Pascal. I need to find someone who has this somewhat antiquated software or else get a disk with the listing. Alternatively, do you have access to some hardware/software which has Pascal installed?

There is a further complication in that the first program on disk 2 is headed XXXX PLS. DAT. This program probably controls the remaining programs of this disk which will read the data from the A-disc to the C-disc. It seems likely that XXXX stands for a password. Is this so? In that case we would not be able to read the disks unless we have the password - it would all be par for the course! You will observe that long and bitter experience has made me ultra suspicious.

If my suspicions turn out to be correct, then we would need expert assistance and perhaps the folks in Virginia could help? As you have observed, it is extremely important that we should retrieve the raw data. Incidentally, are these data the original measurements or have they been reformated to give the time, cell potential and cell voltage series? Did N.H.E. record the cell current, bath temperature and atmospheric pressure? [65] The last is especially important and, if you do not have this, then could you send an enquiry to the Airport in Sapporo - the Airports keep such records.

I have some observations on the "Heat after Death" episode ahead of my next letter. Your data for 9th February indicate that the heat transfer coefficient must have been well in excess of 0.9×10^{-9} WK^{-4}. The lower bound values are usually much less than this presumably because of excess enthalpy generation (as might be expected) - it will be possible to sort all this out once we have the detailed data. Interestingly enough, if we assume that there is no excess enthalpy generation after 5,961,300s, then we also get a heat transfer coefficient of 0.93×10^{-9} WK^{-4} from the data point at 5,961,900s. All this leads to a rate of enthalpy generation of ~10W or ~29 Wcm^{-3} as noted in your Appendix. Giuliano Mengoli gets ~5Wcm^{-3} from an immersed electrode

[65] MM No, they did not.

after polarising this at low current densities. We had about 250 Wcm^{-3} In 1992! This is all very tantalising. In my presentation in Vancouver I was very conservative (at usual) and specified the range 5-50 Wcm^{-3} which could only lead to demonstrations suitable for niche markets. However 250 Wcm^{-3} would just about solve all problems especially when one observes that those electrodes must have been mighty hot inside.

All we have at the present is a lot of loose ends. It is very frustrating especially as the next necessary steps are very clear - and inexpensive.

Yours rumblingly,

Martin

1998-05-17

NAWC heading

DATE: May 17, 1998

TIME: 3:00 p.m.

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

Thank you for your message. My wife and I tried to retrieve the NHE data from the disks while we were in Japan using her Windows 95 system, but we were not able to do this. I thought this was mainly due to the fact that we are not computer experts. I meant to ask Mr. Sumi how to access the data, but I neglected to do this. It was not easy to communicate due to the language barrier. You stated that the program on disk 2 is headed XXX PLS.DAT. Generally last names were used as passwords at NHE. Since these disks came from Mr. Sumi, perhaps the password is his last name. I will contact Dr. Asami and see if he will give me an answer concerning the password.

I was quite surprised that NHE did not record the atmospheric pressure since I knew this would be important. There was absolutely nothing in the laboratory to measure this despite all the other equipment they had available. I will ask both Dr. Asami and Dr. Takahashi to see if they can get me the atmospheric pressure from the Sapporo Airport during this period. [66]

The data acquisition system display on the computer screen gave the time, temperature, cell voltage, and current for each cell, and the bath temperature i.e. t, T_1, E_1, I_1, T_2, E_2, I_2, T_3, E_3, I_3, T_b. There was also a second thermistor in each cell, but this temperature was not displayed on the screen but should be included for the data on the disk.

My wife had all my notebook data on one or two of the disks that I gave you in Excel 97. Were you able to read this data? If not, please let me know and she can make new disk(s) for you.

My computers both here and at work do not have Pascal either. I could try to find somebody in the Navy that could read this. Dr. Dave Nagel of NRL would probably be willing to help with this. I agree with you that it is extremely important that we try to retrieve this raw data. It is my understanding that these disks contain the original measurements.

I find this “heat after death” episode very interesting and don’t see how anybody could refute this evidence for excess power. In retrospect, I wish that I had conducted boil-off experiments on the other two cells. Mr. Sumi tried to discourage me from even doing it on the one cell. I believe that

[66] MM Takahashi sent me this data.

the people at NHE were very weak in understanding your calorimetry. I hope to publish this data, perhaps in Physics Letters B. However, I will have to do this on weekends or evenings due to problems with my present supervisors. I am hoping that I can go to Italy or perhaps to the Naval Research Laboratory, if Dr. Nagel can help me, in order to follow up on these very interesting experiments.

I will soon send you, by mail, a copy of my NHE final report along with a copy of my navy report for Dr. Nagel. I think this will give you some interesting insights on the work at NHE. I am disturbed by various negative comments posted by Elliot Kennel concerning the Fleischmann/Pons calorimetry. [67] It appears to me that he is trying to pose as an expert on all aspects of cold fusion due to the fact that he worked at NHE for 18 months. NOT! People like Elliot continue to make it difficult to get funding for this area.

I will let you know when I find out anything about reading the disks or about the atmospheric pressure at the Sapporo Airport. I hope that we can soon find a way to do the next necessary steps regarding this data. Perhaps we can communicate by e-mail if you would prefer that method.

Best regards,

Mel Miles

[67] MM Elliot Kennel worked at the NHE prior to my appointment.

1998-05-19

NAWC fax heading

Dear Martin,

Thanks for your call. I will call Mike Melich – or write him by email. I am sure he could help us retrieve the raw data from the NHE discs. Dr. Takahashi has emailed me that he will get the atmospheric pressure from the Sapporo Airport.

This fax contains your recent letter plus my reply to Jones. I find it Jones to be fundamentally dishonest it is distortions of facts. I will soon mail you my two NHE reports. Enjoy your trip to Italy and then vacation. I hope things work out for their program.

Best Wishes,

Mel Miles

1998-06-01

NAWC heading

DATE: June 1, 1998

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

Thank you for your fax on the 24th of May, 1998. I had to go to Utah to attend my uncles funeral last week and just returned yesterday. I have mailed you a copy of my NHE Final Report along with my Confidential Report to the Navy (Dave Nagel). Please let me know what you think of this when you have a chance to read it all.

I am faxing you a copy of the e-mail that I sent to Elliot Kennel yesterday. I will let you know of any response that I receive from him. I had access to both Icarus Handbooks, Versions 1 and 2 while at NHE. In fact, Dr. Asami allowed me to keep the Version 2 Handbook, which I have here. This Handbook does contain case studies as a set of appendices. I can send a copy of this Handbook to you sometime.

I will contact Dr. Takahashi regarding the masters thesis relating to the Kamiokande detector.[68] Dr. Takahashi is mailing me the atmospheric pressure data that will be needed when we can assess the raw data.

There is another very important issue of concern. In studying my notebook data of February 10, 1998, it appears that I have transposed some columns. For the data beginning at 5897100 seconds through 5902200 seconds, I am not really sure what is T2 and what is E2. It seems to me that both T2 and E2 were over 100 C or 100 V after the cell boiled dry. The maxi/mini recorded the next day for T2 shows 101.63. This suggests that I incorrectly recorded the cell voltage instead of T2. The raw data will clear this up. I was trying to do too many things that day since my time at NHE was running out, and I still wanted to complete several new experiments. I am faxing you these pages from my notebook and have circled the data in question.

I hope your trips went well. Please let me know if my working in Italy looks promising. I would really like to find someplace to follow up on these experiments.

Best Wishes,

Mel Miles

[68] JR Ishida, T., *Study of the anomalous nuclear effects in solid deuterium systems*. 1992, Tokyo University.

To: Elliot Kennel

From: Mel Miles

Subject: Isoperibolic Calorimetry Cc: Bcc:

Melvin H. Miles, Ph.D.
Chemistry and Materials Branch
Research and Technology Division Code 4B2300D
. . .

Dear Elliot:

I have read your ICCF-7 Conference Report as well as your NEDO End of Tour Report. I am quite concerned about your comments regarding isoperibolic calorimetry and especially your criticism of the Fleischmann/Pons calorimetry. I am convinced that your viewpoints regarding this calorimetry are completely incorrect. I have considered posting my comments on the internet, but I hope we can perhaps discuss some of these issues directly.

The Fleischmann/Pons differential equations have been studied by many critics and have stood the test of time. Despite the many attempts there is not a single scientist, to my knowledge, who has found any errors. I published similar equations relating to my calorimetry (J. Phys. Chem. Vol. 98, pp. 1948 - 1952, 1994), and there has been no report of any errors in my equations presented in that publication. Therefore, why were these equations NOT used by Mr. Sumi and others at NHE? If these equations are correct, then they should be used in any data analysis involving the Fleischmann/Pons cells! I see no basis for your comments regarding mechanical engineers arguing about hair splitting differences between the differential equations. The situation is very simple. The differential equations are correct and they must be used in the data analysis.

I was very surprised that the NHE data analysis of the Fleishmann/Pons calorimetry relied on a single heating pulse very early in the experiment to determine the cell constant. There was a new heating pulse every 24 hours at midnight. Why were these other heating pulses never used to determine the cell constants? If the cell constant is poorly characterized, then a 20% excess heat effect can readily be mistaken for a + or - 10% error in the calorimetry.

The ICCF-7 Conference would have been an excellent place for you to discuss your differences directly with Martin Fleischmann at the poster that he presented concerning the NHE data analysis of his calorimetry and the errors that were made. Did you see this poster and did you discuss the different methods of data analysis with Martin Fleischmann?

I did not follow either the NHE method or the Fleischmann/Pons method in my data analysis involving my use of the F/P calorimeter at NHE. I worked out my own methods starting with the basic calorimetric equations. I found that as I improved my own system of data analysis, this led me closer to the F/P methods as described in the Icarus 2.0 Handbook available at NHE and further away from the NHE method. I found 200-300 mW of excess heat in two cells out of three

while the NHE method showed only large calorimetry errors and no excess heat. It would be helpful to me if you could answer the following two questions: 1. Did you have a central role in interpreting the data sets collected by NHE involving the F/P calorimetry? Related to this is the second question: 2. Have you seen and studied any of the following documentation? a. The Icarus System Handbook, Version 1 (this was at the NHE lab). b. "Report on the First Set of Experiments carried out under the NEDO/NHE Project at the Sapporo Laboratories" June 1994. c. The "Second Report on the Experiments carried out under the NEDO/NHE project at the Sapporo Laboratories" December 1994. d. The further document "The Analysis of Experimental Data Collected with the Icarus System" October 1996. e. The "Icarus System Handbook, Version 2" (this was available at NHE). If you have studied this documentation, then what are your detailed comments about these various items?

I found that Icarus System Handbook, Version 2 was very helpful. The various improvements that I made in my own data analysis always seemed to bring me in closer agreement with the methods described in the handbook. Nevertheless, the NHE method of data analysis completely ignored this handbook. Can you tell me why NHE obtained the F/P calorimetric system, and then completely ignored the handbooks relating to the proper methods of determining the cell constants and also the correct data analysis?

In retrospect, the NHE Laboratory took an accurate calorimetric system and converted it into an inaccurate system by their method of data analysis. This is very similar with what happened with me and the Naval Research Laboratory. They started with my calorimetric design that had an error of + or - 20 mW and made various changes that resulted in errors of + or - 200 mW. This made it almost impossible for them to detect my usual excess heat of 200 - 300 mW.

I am looking forward to your answers to the various questions above.

Sincerely,

Mel Miles

A-2) ! — cloudy
A-3) + ice
medical

No. 55

Date 2/10/98

February 10, 1998 (Tuesday) CELL A-2 BOIL-OFF

Time	$I_1 = 0.374A$ T_1	E_1	$I_2 = 0.999A$ OK T_2	E_2	$I_3 = 0.610A$ T_3	E_3	T_b	T_R
5871000/19571 (+23.9 hrs)	36.64	5.23 (0.374)	95.10	11.40 (0.999)	57.87 (9)	7.27	22.06	22.24
(maxi mini) —	(56.54 36.64)	(7.56 5.01)	(95.10 62.74)	(11.40 7.41)	(59.73 48.76)	(7.90 6.72)		
5871300/19572	36.64	5.23 (0.374)	95.22	11.47	57.99	7.26	22.07	22.24
5871600/19573	36.63	5.23 (0.374)	95.34	11.51	57.97	7.27	22.06	22.23
5874000/19581	36.63	5.22 (0.374)	96.51	12.05 (129.97)	57.97	7.27	22.06	22.28
5874600/19583 (10:04)	36.62	5.23 (0.374)	96.79	12.23 (130.22)	57.98	7.27	22.06	22.23
5877900/19594	36.61	5.22 (0.382)	98.14	12.98 (132.10)	57.97	7.26	22.06	22.19
5880000/19601	36.60	5.22 (0.374)	98.82 (boiling)	13.63 (133.97)	57.97	7.26	22.06	22.15
5882100/19608	36.59	5.22 (0.373)	99.30	14.24 (0.995) (136.32)	57.97	7.26	22.06	22.17
5885100/19618	36.58	5.22 (0.373)	99.83	15.30 (0.996) (140.77)	57.95	7.25	22.06	22.02
5886000/19621	36.58	5.22 (0.374)	99.98	15.77 (0.996) (142.85)	57.95	7.25	22.06	22.09
5888100/19628	36.58	5.22 (0.374)	100.27	16.96 (0.995) (147.42)	57.96	7.25	22.06	22.01
5888900/19631	36.58	5.22 (0.374)	100.38	17.58 (0.994)	57.96	7.25	22.06	22.01
5890800/19637	36.58	5.22 (0.373)	100.61	18.77 (0.993) (156.12)	57.94	7.25	22.06	21.94
5894100/19648	36.58	5.22 (0.373)	101.01	25.90 (0.982) (170.37)	57.95	7.24	22.07	22.03
5895300/19652	36.59	5.21 (0.373)	101.36	49.97 (0.934) (183.8)	57.94	7.25	22.06	22.09
5897100/19658	36.59	5.22 (0.376)	* cell dry 108 108.41	57.93 (0.00.3A) (210.14)	57.93	7.24	22.06	22.10
5898660/19663	36.66	5.23 (0.377)	107.67	57.93 (0.00.4A) (210.14)	57.93	7.24	22.06	22.14
5900400/19669	36.70	5.23 (0.376)	107.38	57.93 (0.009)	57.93	7.24	22.05	22.12
5901300/19672	36.71	5.22 (0.376)	107.34	57.92 (0.00990)	57.92	7.24	22.05	22.04
5902200/19675	36.71	5.23 (0.377)	107.20	57.91 (0.010A) 210.13	57.91	7.24	22.06	22.15

ERRORS

need raw data to check

? (same as E_2? 5/25/98)

* cell A-2 boiled dry, T_2 remain high

Notes cell A-2 cloudy — looks like boiling starting
— heating pulses looked normal last night
E_2, T_2 — on upward climb
cell voltage noise $E_3 \sim E_2 > E_1$ → I_1 E_1 fairly steady

Weight) 129.97 (9:58) → 130.13 (10:02) → 130.22 (10:07) → 132.10 (11:01) → 133.97 (11:37) → 136.32 (12:12) → 140.95 (13:01) → 142.85 (13:17)

pH = 5.99 6.299 5.993 5.983 (−20c)

11:42 — furious boiling swirling around cathode. ±−1" of foam on surface

weight 147.45 (13:51) → 156.16 (14:38) → 170.42 (15:31) → 184.2 (15:51) → 210.14 (16:22) → 210.14 (16:47)

only foam left

No. 56

Date 2/11/98

D2O levels) A-1) +1cc
A-2) Empty, DM
A-3) -½ cc

February 11, 1998 (Wednesday) — Holiday

Time ~10AM +25.1m	I_1=0.371A T_1	E_1	I_2=0.006A T_2	E_2	I_3=0.610A T_3	E_3	T_b	T_R
5961300/19872	36.42	5.18 0.37	34.48	108.32	57.75	7.18	22.06	22.26
5961900/19874	36.55	5.22 0.374	33.91	107.96 0.613	57.76	7.17	22.05	22.28
— (maxi mini) —	(36.54 36.42)	(7.56 5.01)	101.63 (~~110.67~~ 7.44)	(110.67 7.44)	(59.75 48.76)	(7.90 6.72)	—	—
5962500/19876	36.56	5.19 0.375	33.73	107.48	57.77	7.18	22.05	22.27
5964900/19884 11:10	36.56	5.18	OFF ~~25.57~~ I=0	1.15 OFF	57.76	7.17	22.05	22.45
5966100/19888	36.54	5.18 0.373	23.21 I=0	1.08	57.77	7.17	22.06	22.35
5967600/19893	36.51	5.18 0.372	22.46	1.09	57.75	7.17	22.05	22.23
5969100/19898	36.48	5.18 0.373	22.26	1.09	57.75	7.16	22.05	22.22
5972400/19909	36.44	5.17 0.372	22.16	1.09	57.75	7.17	22.05	22.12
5974200/19915	36.43	5.17 0.372	22.14	1.10	57.72	7.16	22.05	22.00
5977200/19925	36.41	5.17 0.371	22.13	1.10	57.70	7.16	22.06	22.06

* All steady state

Notes/

T_2 dropped suddenly overnight, estimated at 7 ~~8~~ PM NOK

T_2 Maxi → 101.63 (?)

Heating pulse normal last night for cells A-1, A-3
none for cell A-2 - (?) - perhaps not visible on screen

Cell voltage noise E_3) $E_1 \sim E_3$

D2O levels A-1) +1cc A-2) Empty A-3) -½ cc

Note) T_2 shows irregular behavior after drop — rises and falls
still more than 10°C above bath temperature

10:55 — Sumi turned off power to cell A-2 ⇒ T_2 drops.

1998-06-03

NAWC fax heading

To: Prof. Martin Fleischmann

Elliot Kennel's reply

(He did not reply to any of my questions)

ICCF-5 cited is

T. Saito, M. Sumi, N. Asami and H. Ikegami, ICCF-5, pp. 105-115, 1995.[69]

What do you make of this paper?

[69] Saito, T., et al. *Studies on Fleischmann-Pons Calorimetry with ICARUS 1*. in *5th International Conference on Cold Fusion*. 1995. Monte-Carlo, Monaco: IMRA Europe, Sophia Antipolis Cedex, France http://lenr-canr.org/acrobat/PonsSproceeding.pdf#page=121

From: Elliot Kennel (6/1/98) To: Mel Miles

Mail*Link® SMTP Isoperibolic Calorimetry

Hello Mel,

Thanks for your letter. I am not sure what has been posted in the various Internet forums, but I have not posted anything there myself, nor have I given anyone permission to publish any of my trip report. I used to post to some of the forums, but after seeing how the non-technical types have degenerated the quality of forum to about the level of professional wrestling, I no longer think it is a good idea. I sent copies of my trip report to about ten people, and did not intend that it should be used to generate negative publicity about cold fusion. I do think that within the community, we should be our own toughest critics, but at the same time it does no good to provide fodder for those who have an axe to grind. My apologies for any harm this may have caused; again, this did not happen with my permission (next time I will put a copyright notice on it, I guess).

The purpose of my trip report is mainly to inform my management about the state of cold fusion to allow the company to make informed business decisions about what technologies to invest in. As a secondary purpose, I also saw fit to share my opinions with others in the field who might be strategic partners some day, or with whom I have a professional relationship. Therefore, I really don't think it should matter too much what I think. If others are citing me as some kind of authority, that would be bad, and not my intention at all.

Anyway, as far as calorimetry is concerned, I don't claim to be any world authority. However, what I can cite is that the guys at NHE were unhappy with the model of Pons-Fleischmann. I think that a paper describing the basic disagreement is contained in ICCF-5, which I don't have a copy of now. Anyway, it deals with a calorimeter coefficient which has a real and imaginary part. As I recall, the NHE guys claimed that sometimes this coefficient had a negative value, [70] which they say violated physics. The phrasing is kind of in Japanese English, but I gather that they felt that the errors could be as high as 30%, so they changed the model.

I don't know who is right and who is wrong. However, I was disturbed by the fact that IMRA-Japan as well as NHE Lab were getting 40% reproducibility with isoperibolic calorimetry whereas with mass flow calorimetry it was 0%. Kubota-san's dual calorimeter was especially depressing, since he obtained excess heat with one calorimeter but not the other. This suggested to me that there may be some kind of false positive that is generated at high loading.

Also, Ed Storms told me he agrees that he has observed some effect that produces artifacts, [71] though he thinks that much of his data is larger than can be explained by non-nuclear effects. However, I don't know the inherent limit for the magnitude of false positives, so I haven't bought off on Ed's argument.

---

[70] MM Only for the "lower bound" constant at the beginning (as expected).

[71] MM Storms ignores the $C_P Mdt/dt$ term.

On the other hand, there are measurements which defy easy explanation. I am sure that scientists such as McKubre, Storms and yourself are very capable and have made very good measurements which are not easily explained. But in addition to a nuclear hypothesis, I think that alternative explanations should be kept open; namely, that perhaps there is some unknown (but not nuclear process) which is produced during high loading which causes certain calorimeters to yield false information. [72] I have proposed one such possibility, but I recognize it is not the only one, and that a nuclear hypothesis still holds out some hope.

However, I think that the most likely payoff is in the area of nuclear accelerator tests, not in electrochemical cells. In that case I think there is a high chance of showing the existence of an anomaly.

Anyway, rightly or wrongly, that is how I have advised my management. Those who disagree should feel free to not buy our stock. Eventually, the picture will become clearer with more experimentation. However, I think that the Internet forums will probably not contribute to any positive developments. I'm sorry if I have inadvertently contributed to negative developments in that regard.

Good luck with your future experiments, and I hope that I will be proven wrong about my judgments on calorimetry!

Yours truly,

[72] JR There is no physical mechanism that would allow conditions in a cathode to affect a thermocouple outside the cathode. Especially not a thermocouple outside the cell in a flow or Seebeck calorimeter. Since Kennel proposed this, he should have suggested a mechanism.

1998-06-09

NAWC fax heading

Martin,

I am sending you a few pages for my ICARUS to handbook regarding Data Analysis. Let me know if this helps in obtaining the raw data from the disks. I can send you the rest by mail (chapter 4).

I am also sending you Kennels PS message. Dr. Asami of the NHE has not replied to any of my requests for my raw data. However, Kennel gave me Dr. Sumi's email address, so I can now contact him.

I now have the Sapporo pressure data thanks to Dr. Takahashi.

Best Wishes, Mel Miles

From: Elliot Kennel (6/2/98)

To: Mel Miles

Hello Mel,

One other point you mentioned in your letter was my comment about the futility of resolving cold fusion with PhDs arguing about differential equations.

I had not intended for this to be a belittling comment.

What I mean by this is that there are two levels of proof that we should be concerned about. The first is whether there is proof of anomalous excess heat in a scientific sense. This point may indeed be resolvable by arguing about the proper equations. However, the second level of proof is convincing people that matter (such as funding agencies). In this case, my view is that even if it can be determined that some of the disputed data is indeed excess heat, the scientific community may ignore it if the proof is too complex, or if there are plausible sources of error which are difficult to rule out. So I am a pessimist concerning the likelihood that calorimetry will ever be used as the proof of a nuclear anomaly, unless the effect is very, very robust and easily duplicated. The heavy water/palladium experiments seem like a very rough row to hoe.

I think that the chances are much better for accelerator experiments, and I hope to have the opportunity to carry them out. If a nuclear anomaly is proven, then I feel optimistic that it will be pursued to the point where we can be sure whether it relates to excess heat or not. Anyway, that's the path I'm pursuing.

Yours truly,

Elliot

[MM ICARUS 2 Handbook, available to the NHE people]

4. Data Analysis

4.1 ICARUS 2 Data Processing Packages

A summary of the data processing package programs, which shows the procedures for (l) preparation of all the input files, (2) which input files are used with which program, (3) what the programs calculate, and (4) the output file names produced by each program, is shown in Figures 4.1-4.4. The output of each program is comma delimited and ready for use, say, in a modern spreadsheet program such as Microsoft's EXCEL 5.0.

The data processing packages for ICARUS 2 are supplied on a separate floppy diskette and are meant to be used on an independent DOS platform PC (386/486/586) machine. The protocol for running the programs consists of:

1. Installing the ICARUS data processing programs from the program diskette onto your PC.
2. Copying the appropriate data files from the data acquisition computer floppy diskettes to your PC.
3. Modifying the CELLDATA.FIL file to reflect the actual operating conditions of each cell.
4. Running the programs.
5. Converting the derived tables to hard copy, and preparation of figures, etc. from the processed ASCII files. From these, refinements to the precision of the heat transfer coefficient may be obtained, and precise and accurate values of the excess enthalpy (when present) ascertained.

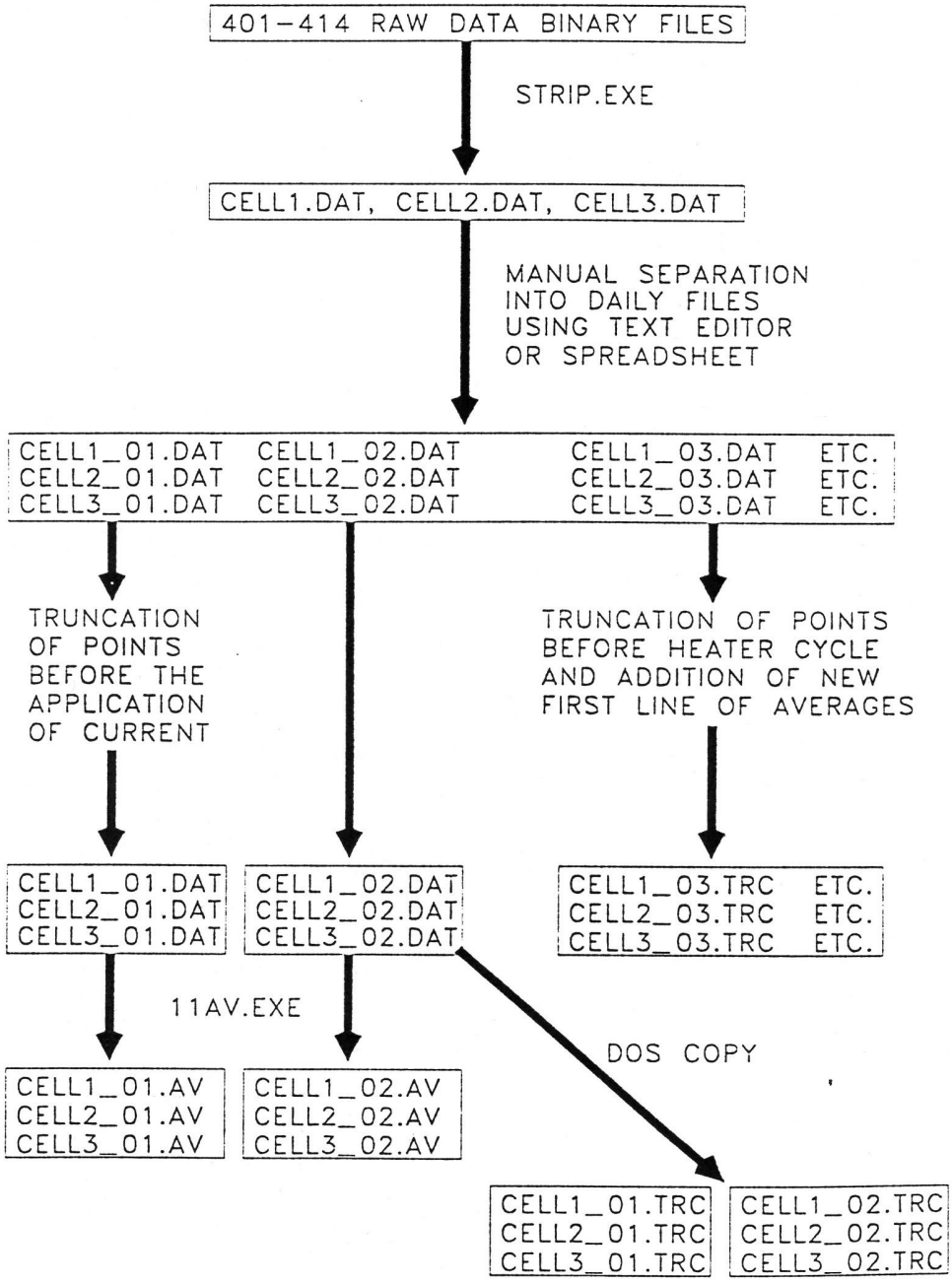


Figure 4-1. Sequential procedure for the preparation of individual data files for analysis by the processing package.

4.1.1 Installation of the ICARUS 2 packages

To install the ICARUS programs on your 386/486/586 DOS PC, first create a new directory C:\SAMPLE on your hard disk by typing the command (at the C:\> prompt)

```
C:\>  MD SAMPLE  ENTER
```

Change to this working directory by typing the command (again at the C:\> prompt)

```
C:\>  CD SAMPLE  ENTER
```

Put the ICARUS 2 program diskette in diskette drive A: and type the command

```
C:\SAMPLE>  COPY A:\*.*  ENTER
```

All of the appropriate files will be copied to your working directory, C:\SAMPLE.

4.1.2 Copying and initial setup of data files

The initial preparation of the data files has been broken down into several small programs. The reasons for this are (1) to determine quickly if any of the acquired data over the past 24 hours have been corrupted for some reason, (2) to increase the understanding of the data manipulation and (3) to allow the user to obtain the raw data for other analyses he may want to undertake.

The diskette that has been written to the floppy disk at midnight by the data acquisition computer has been updated with the last 288 readings taken by the data acquisition system. This represents, therefore, the last 288 x 5 min x 60 min/hour 86,400 seconds (24 hours) of acquisition. Each of the 288 data lines is in 19 byte binary format, as follows:

(Additional pages omitted)

1998-06-22

NAWC heading

DATE: June 22, 1998

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I am in contact with Mr. Sumi by e-mail and hope to get the NHE raw data from him. He has sent me some data from other later experiments. We are having problems since some of the data seems to get converted to an unreadable form. Dr. Takahashi has sent me the atmospheric pressure data for each hour during my experiments at NHE. Where are the disks that I gave you at ICCF-7? Mike Melich is willing to help us read them. Does he have the disks?

I hope we can soon obtain the raw data and complete the data analysis of the boil-off experiment.

Sincerely,

Mel Miles

Mel Miles

I would like the publish paper to show the NHE analysis (no excess heat), my analysis (200-300 mW excess heat, 10 Watts boil off) and the F/P analysis. I would try to publish this in J. Electroanal. Chem. Or Physics Letters A – or should I first try J. Phys. Chem.?

1998-06-23

Dr. Melvin Miles,
Chemistry and Materials Division,
Research and Technology Division,
Naval Air Warfare Center Weapons Division,
China Lake, CA 93555-6100, USA

23rd June 1998

P.S. If you should be able to extract a complete data set as hardcopy from Sumi's data then these would be very useful to start "the ball rolling." Could you ___ sending all this material to me?

Dear Mel,

Many thanks for your fax and I am sorry to have been so long in writing to you: we went to Scotland for holiday after Italy. However, I have just finished a long letter to you + various attachments. All of this is probably ~100 pages total so I'll have to send it to you via SWIFTAIR. Hopefully you should get it soon!

I agree absolutely with your proposed course of action and you will see that some of the comments in my letter will be relevant - especially if the analysis is extended to the experiments in the N.H.E. paper.

I haven't yet sent to the discs to Mike Melich but will do so now together with a letter outlining some proposed courses of action for the future. I'm very glad that you put your "Heat after Death" episode into the Navy System - I shall work on that aspect.

I have located a computer expert who may be able to help us sort out your discs. However, what you say about the "unreadable form" matches what we have found so far and fills me with apprehension. Sumi's latest experiments will probably be useless except as illustrations for the correct ICARUS 1 methodology versus the N.H.E. approach.

More anon, Martin.

1998-06-30

NAWC fax heading

Dear Martin,

Please see my letter and the boil-off data sent to me by Mr. Sumi. Subtracting 91630374.5 seconds from Mr. Sumi's time gives my time as recorded in my notebook.

I hope this data show some excess heat during the boil-off experiment.

Sincerely

Mel Miles

NAWC heading

DATE: June 30, 1998

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

Thank you for your Federal Express package that arrived today. I have looked through most of it but it will be this weekend before I can study it thoroughly.

I am presently faced with an embarrassing situation regarding my NHE boil-off data. Last week Mr. Sumi e-mailed me some of the raw data which I am displaying in the enclosed Figure. This data confirms that I made a serious error in my notebook data. During this boil-off experiment, I encountered values exceeding 100 for both the cell temperature and the cell voltage, which were then confused in my recording of data from the computer screen. As shown in the Figure, the cell temperature dropped below 100 C at about the same time that the cell voltage exceeded 100 V. This is what caused the confusion in my recording of the data. The data from Mr. Sumi shows that the cell temperature dropped rapidly at this time corresponding to the cell boiling dry. Therefore, it does not appear that there was any large "heat after death" effect. Nevertheless, my data shows that there was excess heat produced during the regular experiment and also perhaps during the boil-off period. There are still some things that seem strange as I tried to recall from memory this event at the laboratory. I remember that both Mr. Sumi and I discussed that the cell remained hot after the boil-off, but I guess that we were both looking at the voltage data by mistake. I recall Mr. Sumi's comment that he had seen similar effects where the cell remained, hot, but that this did not indicate any excess heat. Furthermore, my notes of the following day show that I estimated that the cell remained hot for several hours after the cell boiled dry based on the computer screen display of the cell temperature. I have considered that my raw data may have been altered, but I don't really think that is the case. For example, the following day I recorded in my notebook the maxi and mini values displayed on the screen, and these showed that the maximum cell temperature reached 101.63 C and the maximum cell voltage reached 110.67 volts. This agrees with the raw data that Mr. Sumi sent me. The question then, is what should I do now? I have only given this information to a select few people (Dr. Nagel, Dr. Asami, Dr. Carlin, Dr. Li, and you). I plan to send them a retraction. Would this also be your suggestion?

This still doesn't alter my main plan of comparing my data analysis with the NHE data analysis and the F/P data analysis for my three cells. I think that this will show that two of the three cells produced excess heat. I would then proceed with the publication of a paper comparing the different methods of data analysis if you can help me with the F/P method. I am convinced that this is the most accurate method. The data that Mr. Sumi sent me also shows his various

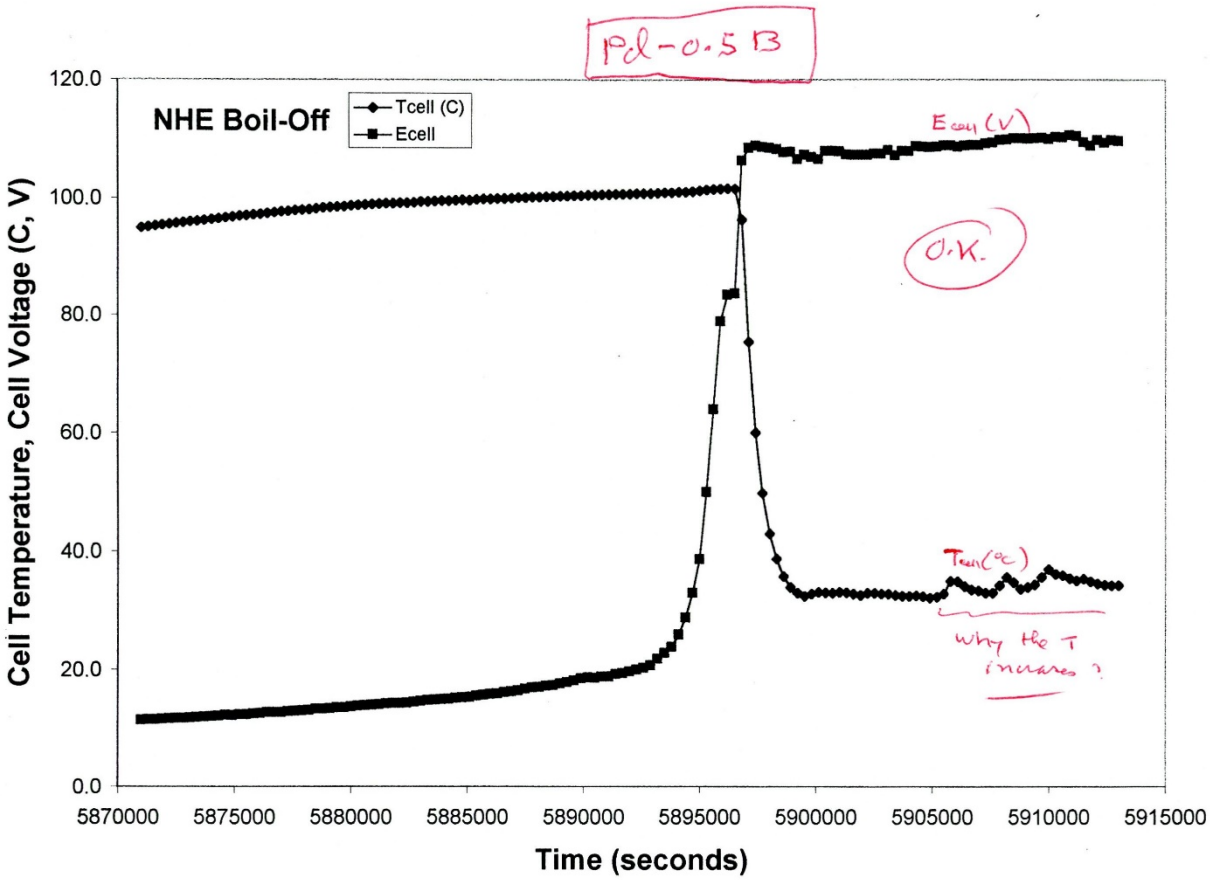
calculations and cell constants that were used. If you could give me an e-mail address I could forward this data directly to you. It is very lengthy, hence I will otherwise have to print it out and send it by mail. Perhaps I can also e-mail the data to Mike Melich as well. I will fax you part of the boil-off raw data as well as the atmospheric pressure data for Sapporo that I obtained from Dr. Takahashi. I will also fax you the weights of the distilled liquid that I recorded during this boil-off period. I hope this will allow you to determine the amount of any excess heat present during the boil-off period.

I will respond to the various questions and requests in the package that arrived today within the next week.

Sincerely,

(signed)

Mel Miles


Pd-0.5 B
NHE Boil-Off
Tcell (C)
Ecell
Ecell (V)
O.K.
Tcell (°C)
why the T increases ?
Cell Temperature, Cell Voltage (C, V)
Time (seconds)
120.0
100.0
80.0
60.0
40.0
20.0
0.0
5870000
5875000
5880000
5885000
5890000
5895000
5900000
5905000
5910000
5915000

NHE, February 10, 1998 (Boil-off)

Time	Weight (D2O)	Clock Time	Weight (D2O)
5874000	129.97	9:58	129.97
5874600	130.22	10:02	130.13
5877900	132.10	10:07	130.22
5880000	133.97	11:01	132.10
5882100	136.32	11:37	133.97
5885100	140.77	12:12	136.32
5886000	142.85	13:01	140.95
5888100	147.42	13:17	142.85
5890800	156.52	13:51	147.45
5895300	183.80	14:38	156.16
5897100	210.14	15:31	170.42
5898600	210.14	15:51	184.02
5902200	210.12	16:22	210.14
		16:47	210.14

Notes

- Clock time was by my watch which was 11 minutes fast.
- 5874000 s was 9:58 by my watch
- There was probably several minutes lag time for the D_2O leaving the cell and collecting in the flask on the balance. This was due to the long glass tubing pathway.

Atmospheric Pressure (From Takahashi)

Sapporo Airport

hour

day

地上気象観測毎時月表［現地気圧］（単位：hPa）

1998, February

地点番号 47412 地点名 札 幌 （石狩支庁） 気象官署名 札幌管区気象台 1998年（平成10年） 2月

	01	02	03	04	05	06	07	08	09	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	平 均	
1	1012.8	1013.5	1014.4	1014.6	1015.1	1015.6	1015.8	1016.2	1016.6	1016.8	1016.9	1016.6	1016.1	1016.3	1016.4	1016.9	1017.4	1017.6	1017.7	1017.9	1017.9	1017.9	1017.8	1017.6	1016.4	
2	1017.1	1017.2	1016.7	1016.7	1016.7	1016.6	1017.0	1017.0	1017.0	1017.0	1016.8	1016.1	1015.6	1015.3	1015.5	1015.7	1015.6	1015.6	1015.3	1015.0	1014.9	1014.8	1014.5	1014.0	1016.0	
3	1012.6	1012.6	1012.2	1011.9	1011.9	1012.1	1012.1	1011.9	1012.0	1012.0	1011.7	1010.8	1009.9	1009.4	1009.5	1009.9	1010.7	1010.8	1011.6	1012.3	1012.2	1011.7	1011.2	1010.3	1011.4	
4	1010.3	1010.6	1010.2	1010.2	1010.4	1010.8	1010.9	1010.6	1010.7	1010.6	1010.3	1009.7	1009.1	1008.9	1009.4	1009.6	1009.8	1010.2	1010.4	1010.4	1010.6	1010.7	1010.8	1010.3	1010.2	
5	1009.9	1009.9	1009.6	1009.2	1009.3	1009.7	1009.5	1009.6	1009.5	1008.9	1008.6	1008.1	1007.8	1007.6	1008.1	1008.5	1008.8	1009.1	1009.2	1009.3	1009.3	1009.2	1009.1	1008.9	1009.0	
6	1008.9	1008.7	1008.9	1008.6	1008.8	1008.7	1008.9	1009.2	1009.2	1009.1	1009.0	1008.8	1008.8	1009.3	1010.2	1011.2	1012.1	1013.4	1014.6	1016.3	1017.6	1018.5	1019.2	1019.9	1011.6	
7	1020.1	1020.4	1020.7	1021.1	1021.3	1021.9	1022.4	1022.9	1023.0	1022.6	1022.1	1021.3	1020.6	1020.3	1020.3	1020.2	1020.0	1019.9	1019.8	1019.1	1018.6	1017.4	1016.3	1015.4	1020.3	
8	1014.8	1014.4	1013.3	1012.5	1011.8	1010.9	1010.1	1009.5	1008.2	1007.0	1006.2	1005.0	1003.0	1001.8	1000.8	1000.1	999.8	999.6	999.3	999.1	999.3	999.0	998.8	998.7	1005.1	
9	998.6	998.6	998.4	998.7	999.2	1000.0	1001.3	1002.2	1002.6	1002.5	1002.7	1002.3	1001.9	1002.0	1002.4	1002.8	1003.1	1003.9	1004.9	1005.4	1005.4	1005.3	1005.6	1005.7	1002.3	L
10	1005.8	1005.6	1005.6	1005.8	1006.3	1006.4	1006.6	1007.1	1007.6	1007.5	1007.7	1007.1	1006.9	1007.0	1007.3	1007.8	1008.5	1009.3	1009.7	1010.4	1011.2	1011.2	1011.6	1012.0	1008.0	
11	1012.2	1013.0	1013.4	1014.0	1014.4	1015.2	1016.1	1017.0	1017.5	1018.1	1018.6	1018.8	1019.0	1019.4	1019.7	1020.0	1020.5	1021.1	1021.5	1021.5	1021.4	1020.9	1020.5	1019.8	1018.1	
12	1019.0	1018.7	1018.0	1016.7	1015.6	1014.5	1013.4	1012.3	1011.1	1010.1	1008.0	1005.6	1003.0	1000.9	999.1	998.6	998.4	997.6	997.1	996.4	996.9	997.5	998.2	999.4	1006.1	
13	1000.8	1003.6	1004.9	1007.2	1008.3	1010.2	1011.4	1012.6	1013.6	1014.2	1014.7	1014.1	1014.0	1014.5	1014.5	1014.8	1015.5	1016.5	1016.5	1016.6	1016.0	1015.3	1014.8	1014.3	1012.5	
14	1013.3	1013.0	1012.0	1011.3	1010.6	1010.1	1009.6	1009.0	1008.4	1008.1	1008.1	1007.3	1006.6	1006.3	1007.5	1008.4	1009.2	1009.8	1010.3	1010.6	1010.5	1010.7	1010.8	1010.9	1009.7	
15	1010.8	1010.9	1011.1	1010.7	1010.8	1011.2	1011.9	1012.4	1012.6	1013.0	1013.0	1012.8	1012.4	1012.4	1012.9	1013.2	1014.0	1014.2	1014.4	1014.9	1015.4	1015.5	1015.6	1015.7	1013.0	
16	1015.7	1015.9	1015.7	1015.7	1015.8	1016.0	1016.4	1016.4	1016.4	1016.4	1016.3	1015.8	1015.6	1015.3	1015.8	1016.1	1016.8	1017.4	1018.1	1018.6	1018.7	1018.8	1018.6	1018.2	1016.7	
17	1018.2	1018.1	1017.7	1017.4	1017.6	1017.8	1018.3	1018.6	1019.1	1019.4	1019.4	1018.8	1018.5	1018.8	1019.4	1019.7	1020.0	1021.0	1021.9	1023.1	1023.9	1024.9	1026.1	1027.1	1020.2	
18	1027.8	1028.2	1028.7	1029.6	1030.0	1030.4	1031.1	1031.8	1032.1	1032.4	1032.3	1031.6	1030.9	1030.4	1030.4	1030.4	1030.7	1031.5	1031.7	1032.0	1032.1	1032.0	1032.0	1031.8	1030.9	H
19	1030.9	1030.5	1030.1	1030.0	1030.0	1029.9	1029.6	1028.9	1028.3	1027.5	1026.8	1025.8	1024.6	1023.7	1022.9	1022.0	1021.7	1021.4	1020.7	1020.8	1021.1	1020.3	1019.5	1019.1	1025.3	
20	1018.5	1018.2	1018.2	1018.2	1018.4	1018.6	1019.0	1019.5	1019.6	1019.5	1019.8	1019.3	1018.8	1018.7	1018.9	1019.3	1019.4	1019.7	1019.7	1019.6	1020.2	1020.8	1020.9	1020.8	1019.3	
21	1020.5	1021.0	1021.1	1021.1	1021.6	1022.0	1022.7	1022.8	1022.8	1022.4	1022.1	1021.4	1020.7	1020.3	1020.7	1021.1	1021.8	1022.5	1023.1	1023.7	1024.6	1024.9	1025.0	1025.3	1022.3	
22	1025.6	1025.7	1025.4	1025.3	1025.5	1025.4	1025.3	1025.4	1025.3	1025.1	1024.8	1024.3	1024.1	1023.7	1023.6	1023.6	1023.7	1023.6	1023.7	1023.6	1023.4	1023.0	1022.9	1022.6	1024.4	
23	1022.0	1021.5	1020.9	1020.5	1020.1	1019.5	1019.1	1020.1	1019.7	1019.6	1019.7	1019.7	1019.4	1019.4	1019.8	1020.1	1020.6	1021.1	1021.5	1021.9	1022.3	1022.7	1023.0	1023.4	1020.7	
24	1023.7	1023.7	1024.2	1024.5	1024.6	1024.7	1025.0	1025.1	1025.2	1025.1	1025.2	1024.7	1024.2	1023.8	1023.8	1023.6	1023.5	1023.8	1023.8	1023.8	1024.0	1024.0	1023.8	1023.3	1024.2	
25	1023.2	1022.8	1022.3	1021.9	1021.4	1021.3	1021.5	1021.2	1020.9	1020.6	1020.4	1019.8	1018.5	1018.1	1018.3	1017.8	1017.5	1017.5	1017.6	1017.7	1018.1	1017.9	1017.7	1017.2	1019.6	
26	1017.1	1016.7	1016.4	1016.2	1016.5	1016.6	1016.7	1016.8	1016.4	1016.1	1015.8	1015.2	1014.5	1014.3	1014.3	1014.4	1014.7	1015.0	1015.5	1015.6	1015.7	1015.7	1015.5	1015.6	1015.7	
27	1015.8	1015.6	1015.2	1014.9	1014.8	1014.6	1014.8	1015.0	1015.1	1014.9	1014.7	1014.1	1013.4	1013.2	1013.5	1013.7	1013.8	1014.0	1014.1	1014.3	1014.6	1014.5	1014.4	1014.4	1014.5	
28	1014.6	1014.6	1014.3	1014.5	1014.5	1014.3	1014.6	1015.0	1014.9	1014.9	1014.5	1014.1	1013.4	1013.6	1013.6	1013.8	1014.0	1014.3	1014.3	1014.3	1014.4	1014.4	1014.4	1014.3	1014.3	

Low= ~~1484~~ 996.4

High= 1032.4

February 9, 1998

	Time	$T_c(S)$	$T_c(L)$	E_{cell}	I_{cell}	T_{bath}		
	97414674.5	341.632	341.673	7.5484	0.804440	295.199	-1.666	
	97414974.4	341.639	341.647	7.5455	0.804440	295.196	2.333	
5784900	97415274.4	341.646	341.673	7.5526	0.804430	295.211	6.666	$T_2 = 68.50$ OK
5785200	97415574.4	341.643	341.668	7.5461	0.804440	295.203	2.166	$T_2 = 68.49$
	97415874.4	341.659	341.666	7.5485	0.804450	295.197	4.499	
5785800	97416174.5	341.670	341.668	7.5367	0.804460	295.204	3.000	$T_2 = 68.52$
	97416474.4	341.677	341.711	7.5400	0.804450	295.199	-1.833	
	97416774.5	341.659	341.695	7.5382	0.804460	295.201	-3.499	
5786700	97417074.5	341.656	341.690	7.5561	0.804460	295.199	1.000	$T_2 = 68.51$
	97417374.4	341.665	341.704	7.5567	0.804450	295.205	6.166	
	97417674.5	341.693	341.725	7.5443	0.804440	295.208	2.166	
	97417974.4	341.678	341.728	7.5405	0.804440	295.205	-1.166	
5787900	97418274.5	341.686	341.687	7.5491	0.804460	295.192	4.333	$T_2 = 68.54$
5788200	97418574.4	341.704	341.701	7.5511	0.804460	295.196	6.666	$T_2 = 68.55$ => Added 13.0 cc D_2O
	97418874.5	341.690	341.745	7.5490	0.804450	295.202	-9.696	
	97419174.5	335.885	335.923	8.3566	0.804450	295.204	-8.366	D_2O addition
	97419474.4	336.671	336.793	8.2676	0.804460	295.198	4.625	
5789400	97419774.5	338.660	338.740	9.3429	1.006500	295.209	6.366	$T_2 = 65.51$
	97420074.4	340.491	340.626	9.2222	1.006050	295.207	5.743	
5790000	97420374.5	342.106	342.277	9.1313	1.005750	295.210	5.072	$T_2 = 68.96$ ~~69.13~~ OK
	97420674.5	343.535	343.652	9.0243	1.005210	295.209	4.523	
	97420974.5	344.820	344.854	8.9743	1.005060	295.212	3.961	
	97421274.5	345.912	346.018	8.8821	1.004930	295.212	3.456	
	97421574.5	346.894	346.976	8.8358	1.005030	295.214	3.098	
	97421874.5	347.771	347.884	8.7767	1.005000	295.217	2.803	
	97422174.5	348.576	348.670	8.7411	1.005230	295.213	2.465	
	97422474.4	349.250	349.324	8.7109	1.004940	295.209	2.143	
	97422774.5	349.862	349.925	8.6735	1.005140	295.210	1.973	
	97423074.4	350.434	350.492	8.6329	1.004700	295.215	1.756	
5791300	97423374.5	350.916	350.974	8.6253	1.004550	295.210	1.481	$T_2 = 77.77$
	97423674.5	351.323	351.381	8.5991	1.004410	295.201	1.330	
	97423974.5	351.714	351.784	8.5696	1.004610	295.211	1.266	
	97424274.5	352.083	352.158	8.5755	1.004200	295.208	1.090	
	97424574.5	352.368	352.467	8.5383	1.004430	295.209	9.283	
	97424874.5	352.640	352.693	8.5353	1.004790	295.210	8.234	
	97425174.4	352.862	352.937	8.5290	1.004220	295.210	7.766	
	97425474.5	353.106	353.145	8.5097	1.004430	295.212	6.766	
	97425774.4	353.268	353.307	8.5037	1.003390	295.215	5.233	
5792700	97426074.5	353.420	353.476	8.4975	1.002720	295.210	5.349	$T_2 = 80.27$
	97426374.5	353.589	353.656	8.5056	1.004090	295.213	5.133	
	97426674.5	353.728	353.826	8.4864	1.003830	295.216	4.766	
	97426974.5	353.875	353.918	8.4955	1.004220	295.218	3.717	
	97427274.4	353.951	353.971	8.4974	1.003500	295.215	3.533	
	97427574.5	354.087	354.146	8.4866	1.003680	295.210	2.882	
	97427874.5	354.124	354.211	8.4968	1.003760	295.207	2.283	
	97428174.5	354.224	354.309	8.4844	1.003580	295.214	3.516	
	97428474.5	354.335	354.374	8.4973	1.003700	295.211	2.633	
	97428774.5	354.382	354.444	8.5021	1.003470	295.213	1.966	
	97429074.5	354.453	354.518	8.4870	1.003640	295.213	2.033	
	97429374.4	354.504	354.570	8.4969	1.004460	295.217	1.717	
	97429674.4	354.556	354.649	8.5056	1.003930	295.216	1.849	

↓

$$\boxed{\text{time} = t(\text{notebook}) + 91630374.5}$$

	Time	Tc(s)	Tc(L)	Ecell	Icell	Tbath		
	97429974.5	354.615	354.709	8.5087	1.003750	295.204	1.700	
	97430274.4	354.658	354.701	8.4885	1.003360	295.212	1.717	
	97430574.4	354.718	354.824	8.5004	1.003640	295.209	1.816	
5800500	97430874.5	354.767	354.818	8.5114	1.003660	295.211	1.400	$T_2 = 81.62$
	97431174.4	354.802	354.888	8.4956	1.003870	295.208	1.066	
	97431474.4	354.831	354.880	8.4964	1.003460	295.213	1.249	
	97431774.5	354.877	354.939	8.4991	1.003540	295.215	1.316	
	97432074.4	354.910	354.918	8.5045	1.003490	295.207	8.333	
	97432374.5	354.927	354.994	8.5023	1.003520	295.205	8.165	
	97432674.5	354.959	354.980	8.5060	1.003780	295.211	5.833	
	97432974.5	354.962	355.034	8.5180	1.003100	295.211	8.000	
	97433274.5	355.007	355.057	8.5070	1.003340	295.212	1.166	
	97433574.5	355.032	355.093	8.5262	1.003540	295.199	7.833	
	97433874.5	355.054	355.158	8.5222	1.003340	295.212	1.400	
	97434174.4	355.116	355.158	8.5328	1.004480	295.214	1.283	
	97434474.4	355.131	355.171	8.5522	1.003880	295.208	7.665	
5804400	97434774.5	355.162	355.250	8.5477	1.003840	295.213	1.133	$T_2 = 82.01$
	97435074.5	355.199	355.207	8.5322	1.003350	295.211	1.100	
	97435374.5	355.228	355.263	8.5568	1.003880	295.206	6.666	
	97435674.5	355.239	355.303	8.5587	1.003840	295.209	9.334	
	97435974.4	355.284	355.334	8.5380	1.003560	295.213	7.666	
	97436274.5	355.285	355.323	8.5726	1.003890	295.198	5.832	
	97436574.5	355.319	355.404	8.5659	1.004220	295.216	1.200	
	97436874.5	355.357	355.405	8.5756	1.003850	295.208	1.166	
	97437174.5	355.389	355.432	8.5655	1.004370	295.209	1.100	
	97437474.4	355.423	355.467	8.5854	1.004610	295.211	1.433	
	97437774.5	355.475	355.481	8.5618	1.004480	295.208	5.499	
	97438074.5	355.456	355.529	8.5572	1.003670	295.204	-3.833	
	97438374.5	355.452	355.511	8.5675	1.003690	295.204	5.167	
	97438674.4	355.487	355.555	8.5730	1.003100	295.209	1.366	
	97438974.5	355.534	355.547	8.5887	1.003620	295.209	8.165	
	97439274.5	355.536	355.590	8.5722	1.002920	295.205	4.333	
	97439574.5	355.560	355.594	8.5817	1.003530	295.206	6.666	
5809500	97439874.5	355.576	355.649	8.5813	1.003750	295.223	7.501	$T_2 = 82.43$
	97440174.4	355.605	355.656	8.5870	1.003690	295.209	7.167	
	97440474.4	355.619	355.684	8.5923	1.002870	295.213	9.165	
	97440774.5	355.660	355.684	8.5976	1.003640	295.210	8.333	
	97441074.4	355.669	355.708	8.6116	1.005430	295.201	8.001	
	97441374.4	355.708	355.769	8.6066	1.004320	295.209	9.833	
	97441674.4	355.728	355.802	8.6102	1.004460	295.205	6.000	
	97441974.4	355.744	355.837	8.6100	1.004150	295.213	8.831	
	97442274.5	355.781	355.832	8.6240	1.003490	295.206	5.832	
	97442574.5	355.779	355.825	8.6152	1.003850	295.208	7.000	
	97442874.5	355.823	355.866	8.6111	1.004510	295.207	1.316	
5812800	97443174.5	355.858	355.903	8.6338	1.004190	295.199	9.833	$T_2 = 82.71$
	97443474.5	355.882	355.903	8.6329	1.004230	295.216	7.167	
	97443774.4	355.901	355.969	8.6483	1.003860	295.203	6.667	
	97444074.4	355.922	356.041	8.6253	1.004170	295.213	1.249	
	97444374.5	355.976	356.012	8.6404	1.005420	295.209	8.998	
5814300	97444674.5	355.976	356.050	8.6642	1.004750	295.213	6.334	$T_2 = 82.83$
	97444974.4	356.014	356.073	8.6427	1.004750	295.213	9.168	

	Time	Tc(s)	Tc(l)	Ecell	Icell	Tb.vb	
	97445274.4	356.031	356.100	8.6378	1.003910	295.214	9.333
	97445574.4	356.070	356.158	8.6533	1.004640	295.202	9.500
581.5562	97445874.4	356.088	356.156	8.6508	1.004520	295.210	8.998
	97446174.5	356.124	356.132	8.6621	1.004520	295.212	6.666
	97446474.4	356.128	356.201	8.6786	1.005050	295.208	8.833
	97446774.5	356.177	356.203	8.6715	1.005230	295.215	1.350
	97447074.4	356.209	356.228	8.6942	1.004740	295.210	2.666
	97447374.5	356.193	356.258	8.6704	1.004590	295.209	6.665
	97447674.5	356.249	356.329	8.6649	1.004630	295.205	1.533
	97447974.5	356.285	356.340	8.7038	1.004850	295.214	1.216
	97448274.4	356.322	356.361	8.6907	1.005110	295.209	1.066
	97448574.5	356.349	356.386	8.7004	1.006040	295.213	8.665
	97448874.5	356.374	356.462	8.6910	1.005880	295.207	8.500
	97449174.5	356.400	356.413	8.7049	1.005940	295.214	1.116
	97449474.5	356.441	356.520	8.7045	1.006120	295.211	1.150
	97449774.4	356.469	356.529	8.7215	1.006050	295.212	5.500
	97450074.4	356.474	356.574	8.7113	1.005550	295.214	6.166
	97450374.4	356.506	356.569	8.7358	1.005740	295.215	1.150
	97450674.4	356.543	356.587	8.7259	1.005770	295.214	1.349
	97450974.5	356.587	356.607	8.7511	1.006040	295.211	7.498
	97451274.5	356.588	356.649	8.7420	1.006140	295.214	1.283
	97451574.5	356.664	356.728	8.7653	1.006110	295.218	3.950
	97451874.4	356.825	356.878	8.7357	1.006440	295.213	5.467
	97452174.4	356.992	357.022	8.7329	1.006540	295.208	5.349
	97452474.5	357.146	357.198	8.7218	1.006540	295.217	4.299
	97452774.5	357.250	357.306	8.7172	1.006450	295.209	3.783
	97453074.5	357.373	357.445	8.7317	1.006500	295.214	3.250
	97453374.4	357.445	357.478	8.7506	1.006420	295.212	2.417
	97453674.4	357.518	357.605	8.7262	1.006480	295.215	2.749
	97453974.5	357.610	357.692	8.7469	1.006470	295.215	2.716
	97454274.5	357.681	357.739	8.7487	1.006600	295.219	2.683
	97454574.5	357.771	357.827	8.7350	1.006590	295.218	2.583
	97454874.4	357.836	357.902	8.7491	1.006520	295.215	2.350
	97455174.4	357.912	357.928	8.7552	1.006680	295.212	2.233
	97455474.5	357.970	358.010	8.7561	1.006000	295.210	1.516
	97455774.5	358.003	358.065	8.7678	1.006130	295.222	1.450
	97456074.4	358.057	358.120	8.7728	1.006040	295.211	2.050
	97456374.4	358.126	358.153	8.7666	1.006220	295.208	1.533
	97456674.5	358.149	358.218	8.7854	1.006010	295.222	1.499
	97456974.5	358.216	358.297	8.7763	1.005340	295.218	1.866
	97457274.5	358.261	358.289	8.7891	1.006180	295.211	1.716
	97457574.5	358.319	358.375	8.7841	1.006090	295.216	1.233
	97457874.5	358.335	358.385	8.7968	1.006310	295.217	1.266
	97458174.5	358.395	358.448	8.8181	1.006350	295.205	1.483
	97458474.5	358.424	358.458	8.7984	1.006380	295.214	8.833
	97458774.5	358.448	358.471	8.8158	1.005690	295.211	3.500
	97459074.5	358.445	358.542	8.7969	1.005930	295.210	6.166
	97459374.5	358.485	358.548	8.8292	1.005650	295.214	1.166
	97459674.5	358.515	358.579	8.8258	1.005570	295.204	9.835
	97459974.4	358.544	358.594	8.8073	1.005650	295.209	1.100
	97460274.5	358.581	358.646	8.8230	1.005420	295.214	9.998

$T_2 =$ ~~83.11~~ 82.94

last notebook entry of day (Feb. 9, 1998)

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Time (s)	Tc(s) (K)	Tc(L) (K)	Ecell (V)	Icell (A)	Tbatt (K)	
97460574.5	358.604	358.664	8.8329	1.005540	295.211	8.000
97460874.5	358.629	358.691	8.8332	1.005790	295.208	1.000
97461174.4	358.664	358.709	8.8686	1.005860	295.212	1.183
97461474.4	358.700	358.762	8.8594	1.005720	295.211	1.383
97461774.5	358.747	358.784	8.8580	1.005850	295.212	1.316
97462074.5	358.779	358.850	8.8628	1.005970	295.211	1.116
97462374.5	358.814	358.889	8.8581	1.005620	295.215	7.501
97462674.4	358.824	358.902	8.8612	1.005680	295.208	5.667
97462974.4	358.848	358.888	8.8680	1.005290	295.211	1.149
97463274.5	358.893	358.911	8.9053	1.005400	295.215	1.166
97463574.5	358.918	358.934	8.8918	1.005460	295.214	7.000
97463874.5	358.935	358.983	8.8923	1.005750	295.202	8.666
97464174.5	358.970	359.027	8.9020	1.005850	295.208	1.316
97464474.5	359.014	359.041	8.8966	1.005530	295.209	1.133
97464774.4	359.038	359.076	8.9041	1.005640	295.204	1.016
97465074.4	359.075	359.139	8.9097	1.005940	295.213	1.216
97465374.5	359.111	359.203	8.9254	1.005830	295.213	1.116
97465674.5	359.142	359.211	8.9447	1.006060	295.208	1.150
97465974.5	359.180	359.204	8.9480	1.004920	295.212	9.166
97466274.5	359.197	359.254	8.9299	1.004990	295.210	1.183
97466574.5	359.251	359.286	8.9595	1.005360	295.206	1.516
97466874.5	359.288	359.344	8.9490	1.005280	295.209	9.166
97467174.5	359.306	359.368	8.9756	1.005310	295.212	1.483
97467474.5	359.377	359.409	8.9827	1.005540	295.206	1.533
97467774.5	359.398	359.457	9.0002	1.005210	295.209	1.866
97468074.5	359.489	359.507	8.9910	1.005540	295.214	1.966
97468374.5	359.516	359.588	9.0078	1.005740	295.206	1.350
97468674.5	359.570	359.656	9.0288	1.005020	295.215	1.850
97468974.5	359.627	359.668	9.0460	1.005110	295.212	1.900
97469274.4	359.684	359.715	9.0455	1.005010	295.207	1.666 → $P_{IN} = 7.56\,W$ → mighnigw
97469574.5	359.727	359.778	9.0514	1.005460	295.211	1.549
97469874.5	359.777	359.830	9.0653	1.005130	295.209	1.333
97470174.4	359.807	359.856	9.0384	1.005060	295.216	1.283
97470474.4	359.854	359.903	9.0766	1.004660	295.211	1.399
97470774.5	359.891	360.001	9.0613	1.004960	295.212	1.516
97471074.5	359.945	360.029	9.0626	1.005410	295.209	1.700
97471374.5	359.993	360.079	9.0867	1.005480	295.218	1.650
97471674.5	360.044	360.089	9.0776	1.004970	295.213	1.733
97471974.4	360.097	360.176	9.0793	1.005310	295.217	1.400
97472274.5	360.128	360.176	9.1067	1.005270	295.207	1.533
97472574.5	360.189	360.224	9.1086	1.005870	295.210	1.583
97472874.5	360.223	360.261	9.1230	1.004490	295.208	1.400
97473174.5	360.273	360.303	9.1010	1.004070	295.217	-9.833
97473474.5	360.164	360.199	9.1542	1.004020	295.214	-3.717
97473774.4	360.050	360.133	9.1690	1.004510	295.211	-2.633
97474074.5	360.006	360.065	9.1939	1.004790	295.207	-1.483
97474374.5	359.961	360.015	9.1851	1.004500	295.207	-1.183
97474674.4	359.935	359.959	9.2203	1.004470	295.205	-7.501
97474974.4	359.916	359.946	9.2260	1.004310	295.211	-6.998
97475274.5	359.893	359.960	9.2281	1.004310	295.214	-4.333
97475574.4	359.890	359.936	9.2392	1.004740	295.220	2.500

97475874.4	359.908	359.978	9.2495	1.004390	295.210	6.998
97476174.5	359.932	359.985	9.2694	1.004980	295.211	5.333
97476474.4	359.940	359.996	9.2968	1.004770	295.207	3.333
97476774.5	359.934	359.998	9.3072	1.004810	295.213	1.166
97477074.5	359.947	360.071	9.3114	1.004780	295.210	5.334
97477374.4	359.966	360.041	9.3285	1.003960	295.214	2.500
97477674.4	359.962	360.022	9.3188	1.004140	295.214	6.998
97477974.5	360.008	360.065	9.3370	1.004240	295.208	1.149
97478274.5	360.031	360.052	9.3587	1.003840	295.212	5.333
97478574.5	360.040	360.092	9.3796	1.003740	295.212	1.000
97478874.5	360.091	360.158	9.3883	1.003680	295.204	1.666
97479174.5	360.140	360.232	9.3838	1.003970	295.213	1.350
97479474.5	360.172	360.237	9.3843	1.003300	295.213	1.583
97479774.5	360.235	360.255	9.4182	1.003560	295.210	1.600
97480074.5	360.268	360.327	9.4383	1.004260	295.210	1.566
97480374.5	360.329	360.398	9.4715	1.004120	295.213	2.283
97480674.5	360.405	360.444	9.4498	1.003840	295.216	2.483
97480974.5	360.478	360.540	9.4893	1.004080	295.213	2.266
97481274.5	360.541	360.598	9.4736	1.004260	295.214	2.116
97481574.5	360.605	360.648	9.4919	1.003920	295.208	2.016
97481874.5	360.662	360.716	9.5290	1.004660	295.211	2.383
97482174.5	360.748	360.827	9.5449	1.004430	295.215	2.733
97482474.5	360.826	360.889	9.5739	1.004540	295.208	2.650
97482774.4	360.907	360.962	9.5518	1.004190	295.209	2.366
97483074.5	360.968	361.009	9.5783	1.003940	295.205	2.599
97483374.5	361.063	361.115	9.5936	1.004360	295.206	2.616
97483674.5	361.125	361.179	9.6028	1.004460	295.208	2.633
97483974.5	361.221	361.227	9.6328	1.004900	295.210	2.383
97484274.5	361.268	361.371	9.6159	1.004510	295.210	2.866
97484574.5	361.393	361.443	9.6620	1.005040	295.208	3.266
97484874.5	361.464	361.496	9.6653	1.004130	295.209	2.383
97485174.5	361.536	361.600	9.7009	1.004380	295.209	2.516
97485474.5	361.615	361.674	9.7081	1.004700	295.215	2.550
97485774.4	361.689	361.766	9.7017	1.004070	295.206	2.316
97486074.5	361.754	361.862	9.7124	1.004570	295.216	2.782
97486374.5	361.856	361.926	9.7253	1.003970	295.216	3.233
97486674.5	361.948	361.973	9.7508	1.004170	295.214	2.733
97486974.5	362.020	362.102	9.7951	1.004280	295.216	2.650
97487274.5	362.107	362.167	9.7861	1.004160	295.205	3.300
97487574.5	362.218	362.239	9.8383	1.004080	295.212	3.300
97487874.5	362.305	362.395	9.8390	1.004290	295.211	2.950
97488174.5	362.395	362.439	9.8615	1.004010	295.212	2.916
97488474.5	362.480	362.543	9.8731	1.004200	295.213	3.016
97488774.5	362.576	362.640	9.8875	1.003880	295.213	3.583
97489074.5	362.695	362.779	9.9195	1.004130	295.207	3.133
97489374.5	362.764	362.861	9.9321	1.003760	295.214	3.333
97489674.5	362.895	362.971	9.9568	1.003840	295.210	3.600
97489974.5	362.980	363.042	9.9888	1.004080	295.210	3.283
97490274.5	363.092	363.168	10.0040	1.004000	295.212	3.633
97490574.4	363.198	363.262	10.0250	1.004040	295.213	3.050
97490874.5	363.275	363.348	10.0382	1.002870	295.214	3.232

	97491174.5	363.392	363.433	10.0719	1.002910	295.212	3.61	
	97491474.5	363.492	363.550	10.0875	1.003080	295.213	3.71	
	97491774.4	363.615	363.658	10.1287	1.003290	295.214	3.66	
	97492074.5	363.712	363.770	10.1387	1.002250	295.207	3.08	
	97492374.5	363.800	363.851	10.1539	1.001200	295.208	3.43	
	97492674.5	363.918	363.980	10.1838	1.002140	295.213	3.53	
	97492974.4	364.012	364.110	10.2186	1.002470	295.208	3.83	
	97493274.5	364.148	364.205	10.2750	1.002370	295.213	4.88	
	97493574.5	364.305	364.332	10.2842	1.001920	295.221	4.31	
	97493874.4	364.407	364.447	10.3191	1.001520	295.212	3.93	
	97494174.4	364.541	364.609	10.3604	1.001710	295.209	4.39	
	97494474.5	364.671	364.735	10.4080	1.001720	295.205	4.56	
	97494774.5	364.815	364.863	10.4262	1.000750	295.207	4.55	
	97495074.5	364.944	365.011	10.4823	1.003260	295.204	4.83	
	97495374.5	365.105	365.158	10.5208	1.003740	295.210	5.10	
	97495674.5	365.250	365.330	10.5558	1.003780	295.212	4.85	
	97495974.4	365.396	365.492	10.5862	1.002890	295.204	5.28	
	97496274.4	365.567	365.643	10.6140	1.003690	295.211	5.36	
	97496574.5	365.718	365.762	10.6680	1.004000	295.210	5.24	
	97496874.5	365.882	365.934	10.7000	1.003720	295.204	5.05	
	97497174.4	366.021	366.116	10.7516	1.003320	295.210	4.96	
	97497474.5	366.180	366.255	10.8015	1.002780	295.198	5.24	
	97497774.5	366.336	366.355	10.8542	1.002250	295.217	4.86	
	97498074.5	366.472	366.537	10.8748	1.002030	295.213	5.10	
	97498374.5	366.642	366.732	10.9697	1.003480	295.204	5.91	
	97498674.5	366.827	366.928	11.0142	1.004460	295.210	5.98	
	97498974.5	367.001	367.070	11.0497	1.002630	295.209	6.00	
	97499274.5	367.187	367.259	11.0953	1.002170	295.210	5.71	
	97499574.5	367.344	367.452	11.1528	1.002340	295.207	5.71	
	97499874.5	367.530	367.596	11.1791	1.000880	295.211	4.98	
	97500174.5	367.643	367.731	11.2407	1.001470	295.216	4.85	
	97500474.5	367.821	367.887	11.2665	1.001620	295.219	5.38	
	97500774.5	367.966	368.039	11.3013	1.001670	295.214	4.98	
	97501074.5	368.120	368.197	11.3668	1.001010	295.214	4.75	Feb. 10, 1998 (October 1)
587,000	97501374.5	368.251	368.298	11.3997	0.999370	295.212	4.23	$T_2 = 95.10$
587,300	97501674.5	368.374	368.429	11.4690	0.999250	295.216	4.01	$T_2 = 95.22$
587,600	97501974.5	368.492	368.598	11.5057	0.999710	295.210	4.20	$T_2 \sim 95.34$
	97502274.4	368.626	368.731	11.5525	1.000940	295.210	4.78	
	97502574.5	368.779	368.863	11.6414	1.000840	295.205	5.04	
	97502874.5	368.929	368.982	11.7221	1.000320	295.212	5.00	
	97503174.5	369.079	369.117	11.7369	1.000460	295.205	4.81	
	97503474.5	369.218	369.280	11.8146	0.999130	295.214	4.73	
	97503774.5	369.363	369.438	11.8886	1.000900	295.208	4.56	
	97504074.5	369.492	369.556	11.9508	0.999610	295.214	4.88	
587,900	97504374.5	369.656	369.701	12.0469	0.998800	295.205	5.23	$T_2 = 96.51$
	97504674.5	369.806	369.852	12.1062	0.999480	295.214	4.70	
587,600	97504974.5	369.938	370.010	12.2304	0.998680	295.206	4.31	$T_2 = 96.74$
	97505274.5	370.065	370.141	12.1879	0.998450	295.209	4.40	
	97505574.5	370.202	370.239	12.2851	0.999060	295.211	4.03	
	97505874.5	370.307	370.362	12.3353	0.999500	295.209	4.08	
	97506174.5	370.447	370.498	12.4565	1.000800	295.214	4.06	

	97506474.5	370.551	370.614	12.5536	0.999340	295.205	4.01E	
	97506774.5	370.688	370.767	12.6606	0.999210	295.207	4.76E	
	97507074.5	370.837	370.934	12.7268	0.998370	295.216	4.13	
	97507374.5	370.936	370.996	12.7180	0.999230	295.207	3.71E	
	97507674.5	371.060	371.131	12.8308	0.999870	295.212	3.76E	
	97507974.5	371.162	371.239	12.9382	0.999550	295.211	3.88	
5877900	97508274.4	371.293	371.348	12.9800	0.999370	295.210	3.36E	$T_2 = 98.14$
	97508574.5	371.364	371.436	13.0382	0.998520	295.219	3.41E	
	97508874.5	371.498	371.542	13.2664	0.998940	295.208	3.96E	
	97509174.5	371.602	371.655	13.3062	0.999150	295.214	3.55	
	97509474.5	371.711	371.763	13.3877	0.998520	295.210	2.96	
	97509774.4	371.780	371.833	13.4814	0.998810	295.220	2.68	
	97510074.5	371.872	371.941	13.4839	0.998770	295.213	3.18	
5880000	97510374.5	371.971	372.017	13.6323	0.998730	295.213	2.95	$T_2 = 98.82$ — cell (boiling)
	97510674.5	372.049	372.120	13.8354	0.998730	295.214	3.05	
	97510974.5	372.154	372.210	13.8447	0.998470	295.209	2.66E	
	97511274.5	372.209	372.281	13.9819	0.998490	295.203	2.11	
	97511574.4	372.281	372.340	14.0200	0.997520	295.217	2.56E	
	97511874.5	372.363	372.421	14.1844	0.998420	295.219	2.01E	
	97512174.5	372.402	372.463	14.2717	0.997410	295.216	1.41E	
5882100	97512474.5	372.448	372.507	14.2373	0.995160	295.210	1.21E	$T_2 = 99.30$
	97512774.4	372.475	372.517	14.3214	0.999170	295.214	1.65	
	97513074.5	372.547	372.633	14.4996	0.997840	295.216	2.74	
	97513374.5	372.640	372.689	14.5917	0.998410	295.214	2.18	
	97513674.5	372.678	372.735	14.7360	0.998310	295.211	1.90	
	97513974.5	372.754	372.814	14.8520	0.998630	295.214	2.18	
	97514274.4	372.809	372.855	14.9063	0.997830	295.213	1.28	
	97514574.4	372.831	372.895	14.9562	0.996140	295.221	1.04	
	97514874.5	372.872	372.930	15.1678	0.996340	295.208	1.86E	
	97515174.4	372.943	372.993	15.2290	0.996900	295.209	1.83	
5885100	97515474.4	372.982	373.017	15.2966	0.996330	295.209	1.56E	$T_2 = 99.83$
	97515774.5	373.037	373.069	15.4777	0.996740	295.213	1.89	
	97516074.5	373.096	373.137	15.6680	0.996320	295.213	1.46E	
5886000	97516374.5	373.125	373.163	15.7720	0.996090	295.212	1.21E	$T_2 = 99.98$
	97516674.5	373.169	373.209	15.9116	0.996900	295.212	1.53	
	97516974.5	373.217	373.264	16.1161	0.996560	295.214	1.60	
	97517274.4	373.265	373.298	16.2655	0.996680	295.217	1.48	
	97517574.5	373.306	373.352	16.4239	0.995970	295.213	1.44	
	97517874.5	373.352	373.403	16.7170	0.994390	295.211	1.58	
	97518174.4	373.401	373.443	16.9188	0.995190	295.214	1.11E	
5888100	97518474.4	373.419	373.467	16.9619	0.994550	295.210	7.66	$T_2 = 100.27$
	97518774.5	373.447	373.478	17.1826	0.994290	295.221	1.15	
	97519074.4	373.488	373.517	17.2700	0.994760	295.220	1.40	
5888400	97519374.5	373.531	373.587	17.5771	0.994370	295.211	1.33	$T_2 = 100.38$
	97519674.5	373.568	373.598	17.7841	0.993280	295.221	1.75	
	97519974.5	373.636	373.682	18.0925	0.993320	295.213	1.56E	
	97520274.5	373.662	373.691	18.4467	0.993570	295.214	1.05	
	97520574.5	373.699	373.737	18.6116	0.993740	295.213	1.30	
	97520874.5	373.740	373.755	18.5255	0.993490	295.215	1.03	
5890800	97521174.5	373.761	373.810	18.7708	0.993270	295.215	6.83	$T_2 = 100.69$
	97521474.5	373.781	373.821	18.7899	0.992180	295.221	1.03	

	97521774.4	373.823	373.875	19.2568	0.992140	295.214	1.30(		
	97522074.5	373.859	373.895	19.3422	0.992430	295.215	8.16[		
	97522374.5	373.872	373.916	19.6372	0.991120	295.211	9.33[		
	97522674.5	373.915	373.968	19.9497	0.989940	295.214	8.66[		
	97522974.5	373.924	373.993	20.2270	0.991380	295.215	1.50(		
	97523274.5	373.924	374.029	20.7173	0.989780	295.214	8.66[		
	97523574.5	373.976	374.091	21.8384	0.988800	295.217	1.95(		
	97523874.5	374.041	374.124	22.8179	0.986880	295.223	1.73[		
	97524174.5	374.080	374.148	23.8283	0.985700	295.221	1.95(		
5894100	97524474.5	374.158	374.184	25.8979	0.982070	295.216	1.68[	$T_2 = 101.01$	
	97524774.5	374.181	374.213	28.8106	0.977200	295.214	1.75(		
	97525074.4	374.263	374.299	32.9171	0.969910	295.216	3.08[		
	97525374.5	374.366	374.422	38.6512	0.961250	295.215	4.13[		
5895300	97525674.5	374.511	374.519	49.9746	0.939230	295.209	3.48[	$T_2 = 161.38$	
	97525974.5	374.575	374.673	63.8894	0.902450	295.212	3.06[		
	97526274.5	374.695	374.788	78.8030	0.829920	295.212	3.40(		
	97526574.5	374.779	374.874	83.3207	0.690070	295.213	-3.16[		
	97526874.5	374.676	375.266	83.5818	0.707530	295.206	-8.93[		
	97527174.5	369.416	371.022	106.2749	0.007560	295.207	-4.35		
5897100	97527474.4	348.570	351.586	108.4079	0.002590	295.210	-6.03	$T_2 = 75.42$	cell DAY
	97527774.5	333.218	335.706	108.7809	0.002410	295.210	-4.26		
	97528074.5	322.994	326.702	108.5842	0.002340	295.208	-2.83		
	97528374.5	316.202	320.057	108.4573	0.002310	295.199	-1.83		
	97528674.5	311.996	315.300	108.2089	0.003640	295.201	-1.20		
5898600	97528974.5	308.992	312.072	107.6734	0.004370	295.209	-8.07	$T_2 = 35.84$	
	97529274.5	307.152	310.159	107.7278	0.005310	295.209	-4.72		
	97529574.5	306.160	309.536	106.5184	0.008370	295.204	-2.44		
	97529874.5	305.686	310.339	107.2030	0.008960	295.205	-1.71		
	97530174.5	306.057	310.386	106.9013	0.009200	295.202	1.00		
	97530474.5	306.291	310.226	106.4690	0.006600	295.210	2.58		
5900400	97530774.5	306.212	310.328	107.8791	0.008660	295.201	-1.71	$T_2 = 33.06$	
	97531074.5	306.188	310.285	107.9053	0.007740	295.199	2.40		
	97531374.5	306.356	309.908	107.7940	0.008750	295.208	1.15		
5901300	97531674.5	306.257	310.024	107.3401	0.009030	295.201	-5.65	$T_2 = 33.11$	
	97531974.4	306.017	309.865	107.2287	0.008390	295.196	-7.13		
	97532274.5	305.829	310.155	107.2609	0.009730	295.206	2.03		
5902200	97532574.5	306.139	310.120	107.2013	0.010040	295.207	6.16	$T_2 = 32.99$	(last entry of day 5:30pm)
	97532874.5	306.199	309.991	107.4746	0.010780	295.209	-1.63		
	97533174.5	306.041	309.906	107.4706	0.010510	295.208	-2.55		(Feb.10, 1998)
	97533474.5	306.046	309.694	108.0222	0.008480	295.205	-4.03		
	97533774.4	305.799	309.447	107.1287	0.007830	295.206	-6.20		
	97534074.5	305.674	309.111	107.8874	0.007000	295.196	-2.83		
	97534374.5	305.629	309.020	107.9393	0.008250	295.206	1.03		
	97534674.5	305.736	308.745	108.7188	0.006160	295.205	-1.40		
	97534974.5	305.545	308.677	108.6481	0.006530	295.207	-5.78		
	97535274.5	305.389	307.998	108.5754	0.006830	295.208	-9.83		
	97535574.5	305.486	308.255	108.6483	0.008800	295.209	1.08		
	97535874.5	306.042	308.688	108.8787	0.012960	295.211	4.54		
	97536174.5	308.210	311.042	108.8443	0.012530	295.219	3.38	6:30	
	97536474.5	308.073	310.906	108.6294	0.009800	295.208	-1.50		
	97536774.5	307.307	310.151	108.8838	0.007600	295.209	-2.25		

97537074.5	306.722	309.247	108.9190	0.006950	295.207	-1.29	
97537374.5	306.531	308.670	108.9441	0.008400	295.204	-9.16	
97537674.4	306.172	308.337	109.2650	0.009510	295.208	-5.60	
97537974.5	306.195	308.291	109.3623	0.008910	295.209	2.16	
97538274.5	307.470	309.844	109.8414	0.009740	295.208	4.38	
97538574.5	308.823	310.947	109.9203	0.010300	295.213	8.60	
97538874.5	307.986	312.271	110.1202	0.008740	295.211	-3.32	
97539174.5	306.829	314.649	110.1174	0.007920	295.208	-1.45	
97539474.5	307.114	314.764	110.0447	0.009900	295.205	1.28	
97539774.5	307.599	315.203	110.0994	0.012250	295.209	2.83	7:30
97540074.5	308.813	316.383	110.2117	0.012160	295.205	4.28	
97540374.5	310.169	317.076	109.9826	0.010790	295.208	9.65	
97540674.5	309.392	316.654	110.3555	0.010900	295.210	-1.74	
97540974.5	309.122	316.807	110.2998	0.010960	295.210	-1.27	high
97541274.5	308.625	316.330	110.6657	0.010360	295.211	-1.46	
97541574.4	308.241	318.592	110.5332	0.012990	295.209	-1.33	
97541874.4	308.545	315.086	109.4444	0.019930	295.210	-1.91	
97542174.4	308.126	314.878	108.8565	0.014340	295.211	-1.33	
97542474.5	307.746	313.770	109.8182	0.014640	295.208	-1.05	
97542774.5	307.491	314.590	109.3692	0.012300	295.199	-4.96	
97543074.5	307.448	314.453	109.8605	0.008770	295.209	-1.43	
97543374.5	307.405	313.998	109.6990	0.009280	295.207	-4.56	8:30
97543674.5	307.174	313.581	109.5355	0.010050	295.209	-6.90	
97543974.5	306.991	313.566	109.6865	0.009270	295.214	1.33	
97544274.4	307.254	314.030	109.7257	0.009660	295.203	1.90	
97544574.4	307.105	313.337	110.0179	0.008950	295.201	-4.14	
97544874.5	307.005	314.107	109.8994	0.008050	295.205	-1.49	
97545174.5	307.015	313.810	109.6034	0.008230	295.207	8.66	
97545474.4	307.525	315.540	109.8666	0.011630	295.214	1.71	
97545774.4	308.044	314.850	109.6737	0.011000	295.205	1.28	
97546074.4	308.294	315.033	109.8703	0.009550	295.206	4.59	
97546374.5	308.320	314.989	109.5958	0.016330	295.202	-1.33	
97546674.5	308.286	315.093	109.9204	0.009320	295.210	-3.30	
97546974.5	308.122	315.258	109.7997	0.010550	295.208	-6.11	
97547274.4	307.919	314.541	110.3314	0.007520	295.215	-5.33	9:30
97547574.5	307.802	314.931	110.4460	0.006930	295.213	-5.08	
97547874.4	307.614	314.388	110.2320	0.009500	295.211	-4.06	
97548174.4	307.558	314.932	109.7278	0.006010	295.209	-2.70	
97548474.4	307.452	314.170	109.9162	0.007290	295.205	-3.13	
97548774.4	307.370	314.495	109.5097	0.014580	295.208	-3.08	
97549074.5	307.267	314.622	109.3980	0.013850	295.207	2.00	
97549374.4	307.382	314.684	109.3290	0.008300	295.207	1.96	
97549674.4	307.385	314.426	109.0478	0.011820	295.212	4.99	
97549974.5	307.412	314.600	109.4165	0.010060	295.197	6.00	
97550274.4	307.421	314.702	109.2961	0.011860	295.205	9.50	
97550574.4	307.469	314.668	109.4267	0.007270	295.216	-1.69	10:30
97550874.5	307.319	314.472	109.2235	0.012880	295.201	-2.31	
97551174.4	307.330	314.647	109.2189	0.012220	295.198	-4.33	
97551474.5	307.293	314.064	109.2798	0.014180	295.206	-4.51	
97551774.4	307.059	314.284	109.9572	0.006440	295.208	-4.78	
97552074.5	307.006	313.818	109.9730	0.021720	295.207	-4.39	

97567674.5	310.737	312.019	106.5920	0.006430	295.205	-2.80
97567974.5	308.393	310.404	106.3959	0.010790	295.208	-5.93
97568274.5	307.176	309.477	106.0352	0.011300	295.208	-7.55
97568574.4	307.940	311.279	106.0363	0.013610	295.208	2.97
97568874.4	308.960	311.198	106.5501	0.009020	295.198	8.68
97569174.5	308.461	311.010	105.8673	0.011890	295.203	-1.13
97569474.5	308.277	310.363	106.4454	0.006570	295.204	-2.59
97569774.5	306.906	309.549	105.7399	0.020030	295.206	-2.23
97570074.4	306.935	309.533	106.9023	0.013070	295.210	-3.61
97570374.5	306.689	309.531	107.1126	0.006320	295.206	-6.35
97570674.4	306.554	309.712	106.9048	0.011940	295.213	-3.66
97570974.4	306.469	309.703	106.9348	0.006680	295.198	-1.41
97571274.4	306.469	309.683	106.3381	0.007250	295.205	-9.33
97571574.4	306.413	309.614	106.7334	0.005740	295.207	-2.48
97571874.4	306.320	308.889	106.8240	0.013710	295.208	-4.46
97572174.5	306.145	308.594	107.5320	0.001640	295.204	-1.28
97572474.4	306.243	308.599	106.9481	0.008790	295.197	4.55
97572774.5	306.418	308.986	107.0350	0.012610	295.204	5.05
97573074.4	306.546	309.134	106.1033	0.007710	295.205	-7.83
97573374.5	306.371	308.704	106.8839	0.025900	295.210	1.24
97573674.5	306.621	309.374	107.1598	0.009220	295.205	1.14
97573974.4	307.058	309.989	107.5709	0.004820	295.209	4.98
97574274.4	306.920	309.358	107.3505	0.005330	295.210	-7.26
97574574.4	306.622	309.737	106.5861	0.010030	295.203	-4.84
97574874.5	306.629	309.762	107.1721	0.008480	295.203	-3.04
97575174.5	306.439	309.266	106.9202	0.006550	295.204	-1.55
97575474.4	306.536	308.960	106.7735	0.009950	295.201	-3.55
97575774.4	306.226	309.213	106.6490	0.005940	295.204	-2.88
97576074.5	306.363	309.741	107.0104	0.008460	295.205	2.28
97576374.5	306.363	309.221	107.3373	0.004960	295.203	-1.08
97576674.4	306.298	309.962	107.0592	0.011280	295.215	2.15
97576974.5	307.653	310.326	106.7010	0.010410	295.208	3.30
97577274.4	308.281	311.397	107.2078	0.005930	295.197	-3.38
97577574.4	307.450	309.755	107.3548	0.006940	295.201	-1.88
97577874.4	307.151	310.030	106.7645	0.023380	295.199	-1.83
97578174.4	307.439	310.188	107.2235	0.005930	295.207	6.99
97578474.5	307.193	309.719	106.9069	0.010750	295.197	2.49
97578774.4	308.937	311.652	107.6098	0.004980	295.206	8.96
97579074.5	307.731	310.223	107.0322	0.008700	295.208	-2.67
97579374.5	307.329	309.339	107.0858	0.003810	295.209	-1.56
97579674.4	306.794	308.744	106.9388	0.005700	295.205	-6.55
97579974.5	306.936	308.736	106.8588	0.003160	295.207	-3.83
97580274.5	306.564	308.142	106.8377	0.007230	295.205	-5.20
97580574.5	306.624	308.048	106.5110	0.006050	295.204	5.83
97580874.4	306.914	308.743	106.8972	0.008910	295.203	1.03
97581174.4	306.686	308.074	106.8430	0.013780	295.203	2.91
97581474.4	307.089	308.052	107.2957	0.011730	295.201	1.29
97581774.4	307.461	308.227	107.9831	0.006090	295.210	1.09
97582074.4	307.748	309.150	107.5770	0.010540	295.208	7.81
97582374.5	307.930	309.310	107.7596	0.009750	295.206	3.33
97582674.5	307.768	309.340	107.3322	0.010070	295.205	-1.18

	97582974.4	307.859	309.569	107.5096	0.012170	295.199	-4.10	
	97583274.4	307.522	308.825	108.0622	0.005450	295.209	-6.84	
	97583574.5	307.448	309.402	107.7848	0.009910	295.215	-2.16	
	97583874.5	307.392	309.636	107.8956	0.005240	295.205	-5.76	
	97584174.5	307.102	309.333	107.0380	0.007280	295.208	9.03	
	97584474.4	307.934	309.371	106.9867	0.012160	295.201	1.90	
	97584774.4	308.242	309.768	107.6467	0.008260	295.204	5.48	
	97585074.5	308.263	310.152	109.1355	0.014950	295.205	2.29	
	97585374.5	308.380	310.262	109.5698	0.004510	295.206	-9.10	
	97585674.5	307.717	309.761	108.9927	0.010940	295.208	-1.40	
	97585974.4	307.537	309.586	108.4987	0.040950	295.202	-1.51	
	97586274.5	307.626	310.098	108.5092	0.014230	295.205	2.84	
	97586574.4	309.242	311.794	108.6610	0.009090	295.200	2.90	
	97586874.5	309.368	311.653	108.8214	0.014860	295.205	5.63	
	97587174.4	309.580	311.770	107.3267	0.019970	295.204	1.05	
	97587474.5	310.003	311.952	108.0337	0.017530	295.212	7.23	
	97587774.5	310.014	311.895	107.8658	0.009140	295.208	-4.06	
	97588074.4	309.759	311.571	108.5157	0.007080	295.198	-8.41	
	97588374.4	309.509	311.246	108.3872	0.014940	295.203	1.08	
	97588674.4	309.824	311.570	108.5751	0.015270	295.206	-5.09	
	97588974.5	309.203	310.966	108.4375	0.011920	295.209	-1.62	
	97589274.5	308.848	310.608	108.2012	0.008830	295.200	-1.20	
	97589574.4	308.482	310.255	108.0664	0.009440	295.207	-5.96	
	97589874.5	308.490	310.467	108.2598	0.006420	295.200	-3.61	
	97590174.5	308.265	310.083	108.4423	0.008300	295.204	-7.11	
	97590474.5	308.063	309.936	108.8207	0.006510	295.207	1.83	
	97590774.4	308.276	310.119	108.0568	0.008660	295.211	9.66	
	97591074.4	308.121	309.919	108.3798	0.010680	295.209	-8.63	February 11, 1998
	97591374.5	307.758	309.605	108.4393	0.009710	295.202	-8.18	
5961300	97591674.5	307.630	309.430	108.3189	0.005530	295.206	-8.76	$T_2 = 34.48°C$
	97591974.4	307.232	309.127	107.9981	0.006040	295.206	-9.43	
5961900	97592274.5	307.064	309.172	107.9649	0.013210	295.200	-5.11	$T_2 = 33.91$
	97592574.5	306.925	309.059	108.2332	0.007100	295.212	-3.05	
5962500	97592874.5	306.881	308.846	107.4756	0.008580	295.199	2.68	$T_2 = 33.73$
	97593174.4	307.086	309.065	107.0500	0.022560	295.199	2.18	
	97593474.4	307.012	308.837	107.8636	0.005910	295.201	-1.45	
	97593774.5	306.210	309.306	107.5362	0.006520	295.209	-1.95	
	97594074.4	305.842	309.250	108.0175	0.014090	295.201	-1.90	OFF (potentiostat)
	97594374.4	305.067	308.159	1.3446	0.000000	295.207	-6.073	
	97594674.4	302.198	305.391	1.2464	0.000000	295.207	-8.335	
	97594974.4	300.066	303.186	1.2016	0.000000	295.204	-5.789	
5964900	97595274.5	298.724	301.419	1.1527	0.000000	295.201	-3.760	$T_2 = 25.57$
	97595574.4	297.810	300.036	1.1078	0.000000	295.202	-2.602	
	97595874.4	297.163	298.945	1.0949	0.000000	295.203	-1.853	
	97596174.5	296.698	298.106	1.0848	0.000000	295.202	-1.333	
5966100	97596474.4	296.363	297.461	1.0793	0.000000	295.207	-9.668	$T_2 = 23.21$
	97596774.4	296.118	296.966	1.0720	0.000000	295.211	-7.132	
	97597074.5	295.935	296.587	1.1277	0.000000	295.205	-5.300	
	97597374.4	295.800	296.296	1.1047	0.000000	295.203	-4.000	
	97597674.5	295.695	296.070	1.0938	0.000010	295.205	-3.099	
5967600	97597974.5	295.614	295.894	1.0926	0.000000	295.202	-2.400	$T_2 = 22.46$

	97598274.5	295.551	295.757	1.0924	0.000000	295.204	-1.866	
	97598574.5	295.502	295.652	1.0916	0.000000	295.203	-1.450	
	97598874.4	295.464	295.569	1.0915	0.000000	295.201	-1.150	
	97599174.5	295.433	295.506	1.0914	0.000000	295.205	-9.333	
5969100	97599474.4	295.408	295.456	1.0891	0.000000	295.201	-7.500	$T_2 = 22.26$
	97599774.5	295.388	295.416	1.0886	0.000010	295.202	-6.000	
	97600074.4	295.372	295.385	1.0873	0.000000	295.206	-5.000	
	97600374.5	295.358	295.361	1.0873	0.000000	295.207	-3.999	
	97600674.5	295.348	295.342	1.0864	0.000010	295.210	-3.333	
	97600974.5	295.338	295.328	1.0872	0.000010	295.210	-2.833	
	97601274.5	295.331	295.316	1.0885	0.000000	295.206	-2.167	
	97601574.4	295.325	295.307	1.0885	0.000000	295.201	-2.000	
	97601874.5	295.319	295.300	1.0886	0.000000	295.212	-1.500	
	97602174.4	295.316	295.295	1.0880	0.000000	295.211	-1.166	
5972400	97602474.5	295.312	295.290	1.0886	0.000000	295.203	-1.499	$T_2 = 22.16$
	97602774.5	295.307	295.285	1.0910	0.000000	295.199	-1.166	
	97603074.4	295.305	295.285	1.0937	0.000000	295.206	-8.333	
	97603374.5	295.302	295.280	1.0949	0.000000	295.200	-9.998	
	97603674.5	295.299	295.277	1.0950	0.000010	295.199	-8.333	
	97603974.5	295.297	295.275	1.0952	0.000000	295.209	-3.333	
5974200	97604274.5	295.297	295.275	1.0962	0.000000	295.209	-3.333	$T_2 = 22.15$
	97604574.4	295.295	295.273	1.0975	0.000000	295.203	-5.000	
	97604874.5	295.294	295.272	1.0960	0.000000	295.211	-3.333	
	97605174.4	295.293	295.271	1.0969	0.000000	295.202	-6.667	
	97605474.4	295.290	295.269	1.0975	0.000000	295.199	-6.665	
	97605774.5	295.289	295.267	1.0988	0.000000	295.200	-4.999	
	97606074.5	295.287	295.265	1.0997	0.000000	295.205	-6.666	
	97606374.5	295.285	295.263	1.1006	0.000000	295.202	-3.333	
	97606674.5	295.285	295.264	1.1023	0.000000	295.202	0.000	
	97606974.4	295.285	295.264	1.1031	0.000010	295.205	-5.000	
5977200	97607274.5	295.282	295.260	1.1044	0.000000	295.207	-4.999	$T_2 = 22.13$ (last notebook entry for day. Feb. 11, 1998)
	97607574.5	295.282	295.260	1.1048	0.000000	295.214	1.666	
	97607874.5	295.283	295.261	1.1058	0.000000	295.205	-1.666	
	97608174.5	295.281	295.258	1.1075	0.000010	295.204	-3.333	
	97608474.5	295.281	295.258	1.1080	0.000000	295.207	1.666	
	97608774.5	295.282	295.258	1.1088	0.000010	295.209	1.666	
	97609074.5	295.282	295.258	1.1106	0.000010	295.200	-1.666	
	97609374.5	295.281	295.256	1.1116	0.000000	295.191	-3.333	
	97609674.5	295.280	295.255	1.1121	0.000010	295.196	-5.000	
	97609974.5	295.278	295.253	1.1138	0.000000	295.197	-3.333	
	97610274.5	295.278	295.252	1.1150	0.000000	295.207	0.000	
	97610574.5	295.278	295.252	1.1156	0.000000	295.197	-1.666	
	97610874.4	295.277	295.250	1.1169	0.000010	295.208	-1.666	
	97611174.4	295.277	295.263	1.1124	0.000010	295.202	1.666	
	97611474.4	295.278	295.395	1.1210	0.000000	295.197	1.000	
	97611774.4	295.283	295.447	1.1249	0.000000	295.208	1.999	
	97612074.5	295.290	295.448	1.1280	0.000000	295.205	1.833	
	97612374.4	295.294	295.432	1.1306	0.000010	295.204	1.000	
	97612674.5	295.296	295.409	1.1329	0.000010	295.196	1.666	
	97612974.5	295.295	295.383	1.1354	0.000000	295.203	-5.000	
	97613274.5	295.293	295.359	1.1367	0.000000	295.202	-6.667	

1998-07-02

NAWC fax heading

Dear Martin,

This is the beginning data from Dec. 5, 1997 to Dec. 12, 1997. I will have to fax it in segments since it will exceed the storage memory of our fax machine. There is a total of 47 pages of the data. This includes 6 columns of the raw data and 14 columns of the NHE calculations based on the raw data.

Best Wishes,

Mel Miles

1998-07-03

Bury Lodge heading

Dr. Melvin Miles,
Chemistry and Materials Division,
Naval Air Warfare Center Weapons Division,
China Lake, CA 93555-6100, U.S.A.

Dear Mel,

I believe that I now have all the data sets which you intended to send to me (i.e. pages 1-47 of the start-up and pages 1-15 for the last three days of operation; also the associated pages and your letter to C & E News). Many thanks for your noble deeds! I saw this morning that you had spotted and corrected all the errors in transmission.

Incidentally, my FAX ran out of paper with your page 47, so if you sent anything after this I haven't got it!

It would be very useful for me to have some additional information (and some of this is essential).

1) Could you please confirm the dimensions of your electrode (length, width and thickness).
2) It would be very useful to have the times of refilling of the cells and the volumes of D_2O added.
3) It would be very useful to have the times of application and cessation of the calibration pulses and essential, to have the power inputs for these calibrations (I could backcalculate to get this information but it would be better if you can list this).
4) It would be useful to know the value of the true heat transfer coefficient which N.H.E. used for their calculations and, also, when and how this was determined.
5) I do not understand columns 14 and 18 of the data sets. Presumably these are water equivalents but how on earth were they determined?

Now for some more general matters. I take it that there are data for another ~59 days for this experiment which would be say 360 pages if you use the landscape format. It would be useful to have this format so that I can correlate the new calculations with the ones done by N.H.E. I will definitely have to have this as hard copy at some stage - can you face the task of printing the sheets and sending them to me by Air Mail?

The calculations carried out by N.H.E. are very strange. The charitable view is that they did not understand what they were doing, the uncharitable view is . …… (I will leave that to you!) Also their results are inconsistent with what they said in their paper (reference (23) of my long letter) but much more consistent with what they were told in December 1994 (reference (25)) if they insisted on using their methodology.

There are some very jolly effects in your "Heat-after-Death" episode!

I must send you a list of corrections to my long letter of 18/6/98, also some addenda.

I wrote a letter this week to Mike Melich but I can't get this through to him by FAX in Niceville. I don't want to send the letter to Monterey. Is he on his travels at present?

I am currently in a frantic letter writing mode but as soon as I have got through the worst of the backlog, I will start to process the data. I agree with your scheme of work but the only question is: "have we got the stamina?" Incidentally, one interpretation of the N.H.E. data processing strategy is that the troops on the ground simply couldn't be bothered to do anything properly. But then they did enter into the ICARUS venture on the basis of its specification (see reference (8)) and we did offer to do the data processing for them. In the end I even offered to do this solo and gratis so you will see that I want to stuff the correct version down their throats!

Regards,

Martin

1998-07-03 #2

NAWC heading

3 July, 1998

Dear Martin,

Mike Melich will be in Monterey early next week. I put all the data from Mr. Sumi on a disk using a Zip-Drive and FedEx'ed this to him at Monterey today. Pages 1-47 where the complete data that I have faxed yesterday. I will print out the other pages and send it by airmail. I will also copy my notebook pages that contain data, D_2O additions, and observations.

My response to your numbered questions follow:

1. *Pd-0.5 B, d = 4.71 mm, L = 20.1 mm, A = 3.15 cm^2, V = 0.35 cm^3.*
2. *The cells were always filled at 10:00 AM. I am faxing the notebook page that gives the volume added.*
3. *My analysis of the Sumi data shows that the heating pulse was applied at 7:00 PM starting at 91921974.5 s. This heating pulse was applied for exactly 6 hours. Sumi told me that the heating pulse was 0.25 W.*
4. *The NHE value of the heat transfer coefficient for this cell was $K_R = 0.793504 \times 10^{-9}$ J/K^{-4}. I was told that it was determined by the very first heating pulse and then used throughout the experiment.*
5. *These are the water equivalents, but the values are very strange.*

I am also faxing Sumi's graph for this Pd-0.5 B experiment in cell A-2. One shows the cell temperature and it cell current while the other gives the excess power and cell temperature. Based on the NHE data analysis, the excess power is 0 ±100 mW. My data analysis shows excess power exceeding 200 mW (Fig. 10 of my NHE Report). It will be interesting to compare these two data analyses with your data analysis of the same raw data. As I improved my data analysis, I realized that this was bringing me closer to the methods described in the Icarus Handbook. I don't understand why NHE ignored this Handbook.

I am also faxing the atmospheric data at Sapporo for October 1997 - March 1998. I knew that there was a problem at NHE when they told me that they did not measure the pressure and that it was not important.

The editor of C+EN [73] sent me an email yesterday stating that they would publish my letter.

Best Wishes,

Mel Miles

[73] JR C&EN *Chemical and Engineering News*

P.S. Note that 90 mL in LiOD was added to the cell A-2 on 1/26/98. This increased the LiOD concentration to 0.2 M.

My major goal in any cold fusion work is to find some way to make this field acceptable and respectable to other scientists. I would like to see some experiment that could decisively show that critics such as Jones, Morrison, and others, are completely wrong. How could this best be done? I can't do cold fusion experiments here hence I will have to find some other place to work. The boil-off effect could prove to be this decisive experiment. In my NHE report, I described a "fluidized bed" experiment using small palladium particles that gave excess heat. Perhaps such experiments will prove to be reproducible due to the dynamic conditions. These experiments could then be driven to boiling to give the "heat after death" effect. Another possibility would be electromigration experiments that are driven to boiling. If an experiment can be designed that is readily reproducible at any laboratory, then the cold fusion battle will be over. There would then also be plenty of funding for cold fusion research. I hope this could be accomplished within the next few years. What are your thoughts about these subjects?

I am not allowed to do cold fusion research at China Lake, hence my only hope of contributing to solving the cold fusion problem is to find another place to work. My only hopes at present are Italy, John Dash at Portland State University, or perhaps Dr. Takahashi in Japan. John Dash has been promised funding by a private investor, and he would like me to work with him. It remains to be seen, however, if he will actually receive this funding.

Another reason for my interest in a solution for the cold fusion controversy is that I would like to someday write a book about my experiences with the Navy and at NHE in Japan. I think that you should also write a book. However, any such books will be practically ignored unless the cold fusion problems are solved and a renewed interest in the subject develops. Please let me know also about what you think of this.

This all relates to your question about stamina. I want to continue the battle, but my progress will be slow while I am at China Lake. I think the paper on my NHE data will be very useful and will show the errors of the NHE data analysis. However, it will not change the negative opinion of most scientists regarding cold fusion. My major goal is to find some way to create a major shift of opinion of scientists regarding this subject. I would like to know exactly how this could be done, and how I could help.

Best wishes,

(signed)

Mel Miles

1998-07-10

Dr. Melvin Miles,
Chemistry and Materials Division,
Naval Air Warfare Center Weapons Division,
China Lake,
CA 93555-6000
USA

10 July 1998

Dear Mel,

Herewith just a short progress report, I can now transcribe at least parts of the data sets but I do not know as yet whether we will be able to disentangle the ICARUS 2 software package - the Handbook would be useful! If we can't disentangle this, then we should be able to reimplement those parts which will be necessary but, as I told you, we would like to get at the software because I believe that there are some mistakes and misapprehensions in this, (you will see that this will affect the way in which we may wish to write up the work).

I have started to recalculate some of the data sets you have FAXed to me. In doing this I have used 1.527V as the thermoneutral potential rather than 1.54V. The value of 1.54V has a rather strange history. In March 1988 we were conducting a review of our work while I was in hospital and I tried to correct the thermoneutral potential to 30°C (the bath temperature we were using at the time) and for the fact that D_2 and O_2 were being evolved from 0.1M LiOD rather than D_2O. I made a mistake in a decimal point which I have always said was due to the after effects of anaesthetics! The thermoneutral potential is certainly not 1.527V but it is nearer to that value than to 154V. As everybody else uses 1.527V, I have more recently also used that value. This somewhat increases $(k_R')_{11}$.

The results for day 3 for your second data set are a microcosm of the Cold Fusion Saga and the inanities (or otherwise) of N.H.E. Perhaps nanocosm would be a better description because some of the effects are not as large as can be seen in other data sets. As you know, I start off by plotting the data, see the attached Fig. 1 (this is a reduction to A4 format from the original A3 sized plot). You can see immediately that the temperature-time series has not relaxed completely during the 6-hour calibration pulse, Fig. 2. I told N.H.E. repeatedly that this pulse had to be lengthened to 12 hours and they were explicitly instructed to do this in the ICARUS 1 Handbook[(8)]. (I am using the reference list of my letter of 18/6/98). They ignored all such instructions. However, the failure of the temperature-time profile to relax is not exclusively due to the use of a short calibration pulse because there is also "positive feedback"! You can see this immediately by drawing a straight line through the plot before the application of the heating pulse and extrapolating this to the end of the measurement cycle. Evidently, the application of

the heating pulse has increased a rate of enthalpy generation so that the temperature or time plot does not relax to the sloping base line. The very early onset of "positive feedback" is very interesting - is this a property of the Pd-B systems? *(See Fig. 2)*

By contrast the cell voltage-time plot seems reasonably normal although it is clear once again that the duration of the calibration pulse is too short, see Fig. 3.

We can now attempt to calculate $(k_R')_1$ and $(k_R')_2$. The first step is to compare $(k_R')_1$ to the value of $(k_R')_{11}$ just before the application of the heating pulse, 0.84152×10^{-9} W K^{-4}. *(See Fig. 4)* Evidently, the application of the pulse has decreased the "lower-bound" heat transfer coefficient which can only be due to an increase of a rate of excess enthalpy generation. Of course, we then expect the true heat transfer coefficient, $(k_R')_2$, to be less than [74] the "lower bound value" as is indeed the case! I am afraid that this interesting result has a large penalty: it may prove to be rather difficult to determine the true heat transfer coefficient.

When I started that data analysis, I assumed that the rate of excess enthalpy generation would be rather low when using a cell current of 0.15A. I therefore expected $(k_R')_{11}$ to increase in the initial stages to a value very close to the true heat transfer coefficient as we had seen in our earlier work e.g [(4),(5)] and also in the first N.H.E. data sets[(26)]. However, the early onset of "positive feedback" makes this observation impossible. Nevertheless, you can see quite clearly from the N.H.E. evaluation that $(k_R')_{11}$ increases right up to the time of application of the first calibration pulse. However, the value on day 2 will be even larger than 0.84152×10^{-9} W K^{-4} - I will give you an update in due course but I have already drawn your attention to the fact that the heat transfer coefficient for this cell is rather high.

We must also bear in mind that $(k_R')_{11}$ decreases slightly with time between the periods of refilling the cells due to the falling level of electrolyte (I must write further to you about this) e.g. see Fig. 2 of [(11)]. This effect is probably about 0.8% between the start of the experiment and the time of application of the heating pulse on Day 3. The minimum estimate of the true heat transfer coefficient for the first part of day 1 is therefore ~ 0.85×10^{-9} W K^{-4} not the value 0.793504×10^{-9} W K^{-4} which N.H.E. derived by an entirely invalid procedure!

We can now apply one of the many "sanity checks" on the data which I used over the years. Suppose the appropriate value of the true heat transfer coefficient is indeed ~ 0.85×10^{-9} W K^{-4} we can combine this with the value of $(k_R')_{11}$ derived at the bottom of page 1 of your data sets (Column 13) and f(0) (Column 11) to give a rate of excess enthalpy generation of 0.0637W - nearly double that quoted by N.H.E. We know that the initial enthalpy of absorption of D per atom is about 40 kJ $mole^{-1}$. If the whole of the current is used to charge the electrode, then we would expect a rate of excess enthalpy generation of 0.0622W! You could easily verify that one does indeed use the whole of the current by checking that there is no gas evolution on the cathode in these stages (I have confirmed this in the past but it would be best if you could check this up).

[74] MM Should this be "greater than"?

Of course, this rather large value of the true heat transfer coefficient shows that you must have had quite appreciable rates of excess enthalpy generation even at a cell current of 0.15A.

Clearly, we must analyse everything in great detail. I will resurrect some old data on Pt-D_2O blanks so that we can carry out analyses on your data and the blanks in parallel.

More anon,

Yours,

Martin

P.S. We shall be going to Cornwall on 14th returning the 20th. M.M. [Michael Melich] is probably coming here on 23rd.

P.P.S. The situation is actually more complicated than this. I have carried out a preliminary analysis four days 3-7 and realise that I must actually do such an analysis for the whole data set. I will write to you in detail when we get back from Cornwall. Actually we have a somewhat nasty problem with this data set which will complicate the analyses.

P.P.P.S. I will answer your letter of 11/7/98 when we get back from Cornwall. You can take it that I will do of fair bit of the analysis. I am heavily engaged now in trying to raise funds for the next step of the work!

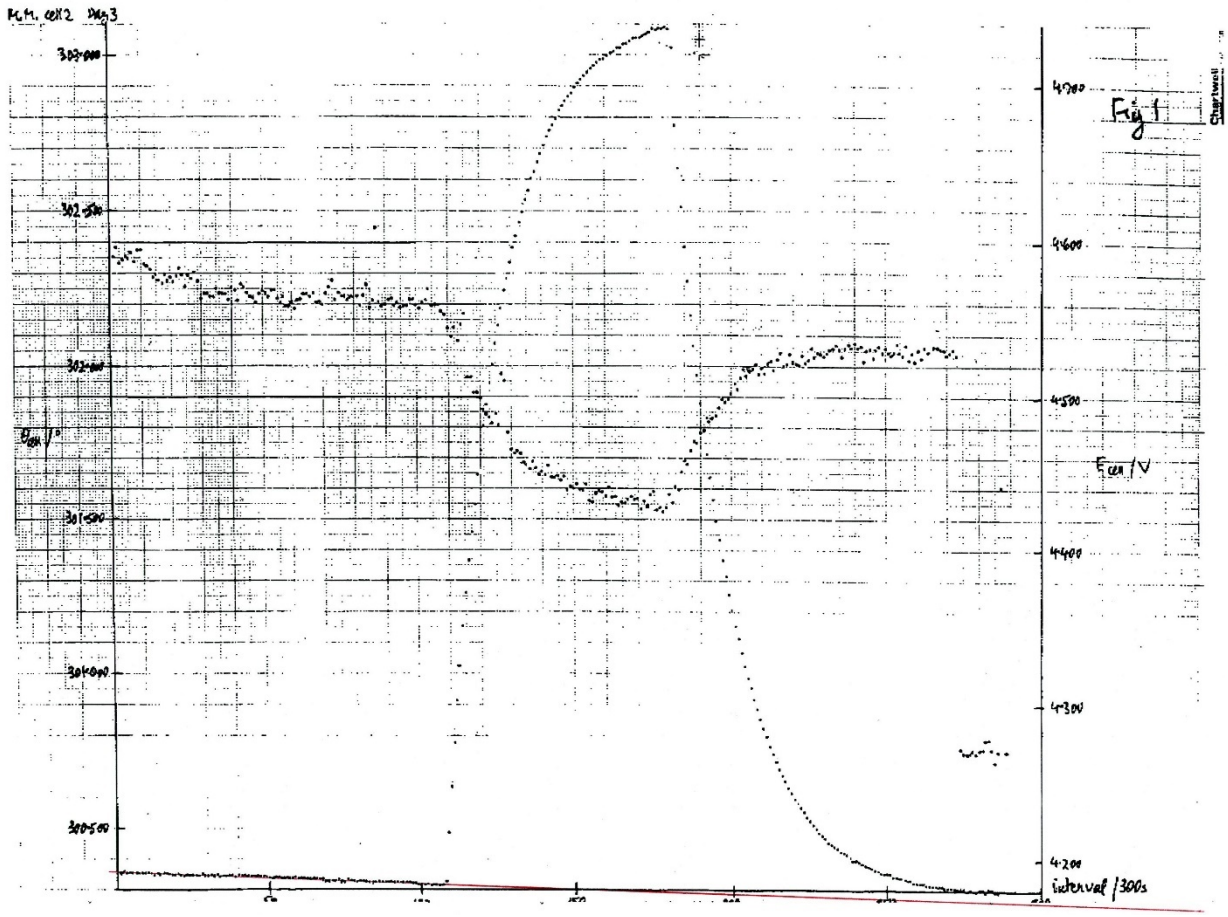


Fig 1
4700
4600
4500
4400
4300
4200
interval / 300s

1998-07-13

NWAC heading

13 July 1998

Dear Martin,

I received your fax today, but I will be on travel for the rest of the week, hence I want to give you a brief reply. I will Xerox of the ICARUS 2.00 Handbook as soon as I return and mail it to you. It contains about 160 pages.

As I expected, your calculations show that the NHE heat transfer coefficient was too small. This leads to positive and negative values for excess heat which they confused with calorimetric errors. The value of ~0.85 × 10^{-9} WK^{-4} means that there was even more excess heat than I reported.

Regarding gas evolution, the Pd-B alloy was slower than the other two cathodes to show vigorous gassing. According to my notebook, the 0.15 A was applied at 10 AM. At 11:38, gassing head become vigorous for the other two cells, but not for the Pd-B. At 11:54, the Pd-B still appeared to be loading most of the electrolyzed deuterium. At 13:40, all three cathodes were gassing vigorously (Page 6 of my notebook).

Sincerely,

Mel Miles

1998-07-23

NWAC fax heading

Dear Martin,

I will mail the pH and weight data from Mr. Sumi today. The pH change does not support the "spillover of electrolyte" proposed by Mr. Moxley in his letter. I did observe about 3 cm of foam in my cell during boiling, but the liquid level was well below the cell top. The loss of liquid by forming may be a problem if NHE ran their cells over-filled as you suggest. It was not a problem in my boil-off experiment.

Sincerely,

Mel Miles

NAWC heading

DATE: July 23, 1998

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I received all of the pH and weight data from Mr. Sumi of NHE concerning my boil-off experiment there. It looks like data was recorded every 14 seconds, hence again there will be quite a few pages of data. I find no pH evidence for the loss of any appreciable LiOD. The pH change was from 7.99 to 11.40 during the boil-off period.

I thought I should reply to some of the items in your letter of 23 June 1998 before I have to leave on travel again. Some of my comments follow:

Item 1 - Yes, you had earlier sent me your ICCF-7 paper. I found this paper very interesting. Thank you.

Item 2 - My notebook shows that I confused E2 with T2 when this value exceeded 100. This was when the cell boiled dry. We now have all the computer data from NHE concerning this experiment.

Item 3 - I copied the complete ICARUS 2 Handbook last weekend and this was mailed to you earlier this week. It should arrive soon.

Item 4 - The ICARUS 14 calorimetry design looks very interesting. I would like to try this design if I can find a place to do cold fusion experiments again. It would indeed be very useful to have a cell design that has an unique heat transfer coefficient. I enjoyed reading the history about the ICARUS 1 and the ICARUS 2 designs. It was the ICARUS 2 that I used in Japan. I was told that the heating pulse was applied at midnight, but the data from Mr. Sumi suggest that it was closer to 7 p.m. I always added D_2O at 10 a.m. To my knowledge NHE never carried out factorial experiments involving calorimetry. In fact, I think that their experiments using the F/P cells were very limited. I was never informed about any controls that were run by NHE before I arrived. I was only shown three F/P cells, and I had to repair the counter electrode cage on one of these. You mentioned that Cell 2 had an unusually high heat transfer coefficient. I really don't understand the reason for this, but I was never shown any other cell that could have been used instead.

I have been studying all your material in detail and will try to write more at a later date.

Best wishes,

(signed)

Mel Miles

P.S. How was your visit with Mike Melich? Perhaps he can help in obtaining Navy funding if you are invited to visit NRL. I think Mike Melich would try to be there if possible. I would also like to be there if this takes place.

1998-08-12

NAWC heading

DATE: August 12, 1998

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I received a telephone call from Mike Melich while he was in England. I am hoping that he can help find some way for you and I to collaborate on cold fusion research in the near future. My work here is keeping me quite busy, but I hope to find time to help in the NHE data analysis. I would like to evaluate the cell constant from heating pulses using your methods.

The ending of the NHE program in Japan has certainly hurt this field and made it much more difficult to obtain funding. Therefore, a publication that clearly shows their errors would be very helpful. The evaluation of my data for the three F/P cells at NHE would be a central feature for this publication.

I have e-mailed a request to Mr. Sumi of NHE to send me the raw data for their ICCF-5 paper. Hopefully he will do this.

Last week I attended IECEC-98 in Colorado Springs. It is ironic that I was not allowed to attend the same meeting last year due to the fact that one of my two papers involved cold fusion. This year I didn't have any papers, but my supervisors wanted me to attend to learn the latest about lithium batteries and fuel cells. There was, however, a cold fusion session with papers by Miley, Muzuno, Swartz, and others. Since Filimonov of Russia was not there to present his paper, I was asked to speak for about ten minutes off the cuff about cold fusion. It was good to see that cold fusion papers were permitted at this meeting. However, I was told that this was not easy and the persons responsible for the session took a lot of abuse from other officials involved with this meeting. It is frustrating that most scientists don't take the time to look at the abundant evidence for this field.

I will be on vacation next week.

Best wishes,

(signed)

Mel Miles

1998-08-14

Bury Lodge heading

14th August 1998.

Dear Mel,

Many thanks for your FAX and I know that I have been incommunicado for some time. This is just to let you know that I am working "flat out" on the data analysis.

I have decided that we need to make a comparison with a data set for a "blank experiment" taken with an ICARUS-2 System and analysed correctly as well as with the N.H.E. Methodology. I have very nearly finished this (but there are some provisos which I will write to you about in due course). I now have to abstract the key information with suitable spread sheets.

All this threatens to become much too voluminous so we may need to publish a paper referring to a further report. Do you think that the Navy would wish to have such a report for their files?

Matters in Italy seem to be progressing although rather slowly. I continue to stir various pots but, as you say, it is not a popular subject!

Have a good holiday!

Regards,

Martin

1998-08-15

Return-Path: <nagel@dave.nrl.navy.mil>
Posted-Date: Sat, 15 Aug 1998
Subject: Finally!!

Dear Mel,

I have to start by apologizing for treating you so badly. There are some people I can ignore without qualms, but you are NOT one of them. Despite working almost every Saturday, I remain buried. I am healthy, but too blasted busy.

7 Jun you sent me a copy of the letter to Matsui. You made a good case for continuing the work in Japan. I suppose that the letter led to nothing.

15 Jun you wrote me about trying to get the NHE raw data, and your interactions with Imam here. Two days later I got your sample of the gobbledy gook from NHE. Then, on 6 July you wrote that the computer data you got from Mr. Sumi showed a discrepancy with your notebook, following the boil-off experiment. Does that mean that you were finally able to read the NHE data files??

21 July you noted Imam's nice visit and told me about the material from Martin Fleischmann. I did get it this week, and have paged through it. Some of the statements are quite interesting, both scientifically and otherwise. I have felt for a long time now that it would be really instructive to have and thoroughly study the work of Martin and Stanley. Getting this material is a significant step in that direction. I have never concluded on my own whether or not the detail to which they go in calibrating and modeling their systems is really needed. If the signals are large, it is not usually necessary to be so sophisticated.

Earlier this week, you told me about the IECEC-98 meeting, and asked my advice about DeCorpo. At this time, I recommend that you go for it! There does not seem to be anything happening on the DARPA front, although I did have some impact on the advisory group with my 20 Feb presentation, according to Bob Nowak. Here, things are in poor shape regarding "cold fusion". This division is fairing quite well overall and producing some nice results. But, Graham Hubler can pay only a little attention to the topic (also). Dawn Domingues is off on other projects, as is Pat Hagans. The retired gent who tried to repeat Tom Claytor's experiment is golfing. George Chambers, you must know, took a job at another Navy facility maybe three years ago. So, if you can either or both (a) keep ONR informed or (b) get some support, I would be delighted.

We will soon lose the very bright young theoretician Rob Rudd to a lectureship at Oxford. Before he goes, he and I are trying to apply the accepted theory of the Mossbauer effect to the problem of coupling the lattice and nuclear levels. Of course, that is the same sandbox in which Peter Hagelstein is playing. However, he has gone off the topic to apply his ideas to the coupling of microwave and optical effects. Jolly good physics, but not on the "cold fusion" target.

I expect to have lunch with Imam, and a visiting scientist (Jim Baird from Yale and the University of Alabama at Hunstville) next week. Also, I think Mike Melich will roll through here this coming week. Imam told me, when I saw him in the cafeteria this week, that an invention disclosure will soon appear before the Invention Evaluation Board, which I chair. Hope it goes!!

Enough for now. I am behind on lots of other emails. Again, thanks for your patience. Hang in there!

Best,

Dave

Dr. David J. Nagel
Superintendent, Condensed Matter & Radiation Sciences Division
Naval Research Laboratory, Washington, DC 20375-5320

1998-08-31

NAWC heading

DATE: August 31, 1998

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I got back last week from my trip to Oregon where I have a cabin and about 40 acres of timber. It was a good change but also exhausting work since I only go there once a year, hence I have to do all the maintenance while I am there. There was a good crop of wild blackberries due to the heavy rains from El Nino last winter.

I am faxing you numerous pages of correspondence involving Mr. Sumi, Mr. Matsui, as well as Dave Nagel. As you can see, Mr. Sumi states that he does not have the data used in the ICCF-5 publication, hence I have requested this data from Mr. Matsui as he suggested. His answer will probably be NO, as you have suggested, but I am still hoping that I will be surprised. I have also asked Mr. Sumi and Mr. Matsui if there were any blank experiments conducted at NHE.

Dr. Tripodi will be visiting me here later in September. We worked together last year at NHE. I am hoping I can find out from him regarding the possibilities of my working in Italy.

Mke Melich called me while he was in England visiting you, but I have not heard from him since. He thought the best plan would be for me to work with you there in England. I would really like to do that if it could be arranged. I will have 25 years in with the Navy by the end of this year and could retire and go elsewhere to work on cold fusion. Do you think it is possible to obtain the necessary laboratory space and equipment for me to work with you in England? Perhaps Mike Melich can find some way for the Navy to help with the funding. Anyway, I wanted to make sure that you know that I am willing to do this if it can be arranged. I find it very frustrating to see the time go by and not being able to carry out further experiments on this topic.

I talked again with Dr. Imam of NRL last week. He is the one that produced the Pd-B materials that have worked so well in my experiments. We decided that it would be very helpful to publish a paper concerning my NHE experiments that produced excess heat for the Pd-B cathode. This paper would also discuss the various methods of treating the data and especially the errors in the NHE analysis. We think that various authors should be included on this paper, and hence lend weight to the conclusion that there is an excess heat effect. Possible co-authors could include yourself, Dr. Imam, Dave Nagel, Mike Melich, Wilford Hansen, and any one else that is interested. I wanted to also include Mr. Sumi but he has declined. Please let me know what you think of this idea.

Unfortunately, I have not made much progress with my own data analysis of my NHE experiments. I tried to plot out a heating pulse using your example but this was too time consuming. I am hoping that Mike Melich can send me the data in Excel format and I could then do the analysis fairly rapidly on my computer. What do you do with respect to the two thermistors in the cell? Do you plot out both temperatures? I imagine that there is really not much difference despite Steve Jones repeated arguments about this topic.

I haven't forgotten your many questions in your long letter and hope to get back to answering some of them. I found that package to be very informative and have studied it through several times. I don't think very many scientists have taken the time to fully understand the various heat transfer coefficients that you have described. I asked Mr. Sumi about the discussion of these heat transfer coefficients in the Icarus-2 Handbook, but he didn't seem to understand this subject. I think the people at NHE were only interested in large excess heat effects and didn't want to make the effort to understand the measurements of small effects.

On another subject, the SRI calorimetry was used extensively at NHE, but no excess heat was ever measured. I even ran two experiments using this calorimetry, using materials that were usually successful, but there was no excess heat produced. I have been reading Mike McKubre's NHE report. He states that over the past several years there have been zero excess heat results despite performing 88 experiments at NHE and SRI. This is certainly frustrating and contributed greatly to the decision to end the program at NHE. In my opinion, this flow calorimetry is too static since the temperature remains constant. Therefore, there is no positive feedback effect. Despite the lack of positive results at SRI, I have heard that they have received $400K in funding from DARPA through Bob Nowak to continue their program. This, of course, bothers me since I have had zero funding from the Navy since 1995.

On a more positive note, I spoke with Stan Szpak last week. He is retired but continues with his cold fusion studies at the Navy laboratory in San Diego. He has some good contacts that are interested in his program and his work is progressing quite well. He has a new infrared camera and his experiments show hot spots on the cathode. He continues to publish these new results.

He also says that SRI will be trying to repeat his experiment showing the emission of X-rays from his cells.

Please let me know what you think about some of the ideas that I have discussed. Please give me an update on your progress concerning the data analysis.

Best wishes,

Mel Miles

svuni@trdc.mhi .co. jp, Re: Cold Fusion Data

To: sumi@trdc.mhi.co.jp
From: el <melmiles@ridgecrest.ca.us>
Subject: Re: Cold Fusion Data

Dear Mr. Sumi,

Thank you for your last message. I have asked Mr. Matsui for the data relating to the ICCF-5 publication. This field is certainly frustrating since the excess heat is often small. Therefore, the determination of the heat transfer coefficient becomes controversial. I think all methods of data analysis would show excess heat if the effect were large.

Can you tell me if any blank experiments were conducted at NHE? This would involve experiments such as the use of platinum instead of palladium as the cathode material. Such experiments would be a big help to me in determining the correct method of data analysis. Please let me know if any blank or control experiments were run at NHE using the F/P calorimetry.

Thank you again for your help.

Best wishes,

Mel Miles

At 11:43 AM 8/18/98 +0900, you wrote:

>I have received your E-mail of 11th August today (18th).
>I have had long holiday.
>I appreciate your kindness but I don't need the name as co-author.
>My company don't please to concern to coldfusion, because my data don't agree with Iwamura results.
>I don't have the data of used in the ICCF-5 publication
>"Studies on Fleischmann-Pons Calorimetry with ICARUS 1" (pgs. 105-114).
>IAE have the data.
>Please ask to Matsui-san.

To: Dr. Matsui

From: el melmiles@ridgecrest.ca.us

Subject: Publication on NHE Work

Cc: Dr. N. Asami

Dr. Matsui,

I would like to publish a paper regarding my NHE results using the F/P cells (Figures 8, 10 and 12 of my NHE Final Report). It seems to me that the various methods of data analysis will all show excess power if the effect is sufficiently large (Px>400 mW). For small effects (100-200 mW), however, the method used to determine the heat transfer coefficient becomes critical. Unfortunately, the effect is usually small (100-300 mW) according to my experience. Therefore, I am trying to find the best method for determining the heat transfer coefficient. I will need data from blank experiments in order to do this properly. Can you send me any data sets from blank experiments at NHE using platinum as the cathode in heavy water? The data sets for NHE experiments 4251 and 4711 presented in ICCF-5, pp. 105-115 would also be very useful since these experiments did not show excess heat. Can you send me the data sets for these two experiments? This would be very useful in my analysis of the F/P calorimetry.

I enjoyed my opportunity at NHE to work with the F/P calorimetry. I hope I can now determine the best method for performing the data analysis.

Sincerely,

Dr. Melvin H. Miles

copy: Dr. N. Asami

Posted-Date: Sun, 12 Jul 1998 18:06:47 -0700 (PDT)

From: "Kazuaki Matsui" <mac@iae.or.jp>

To: "el" <melmiles@ridgecrest.ca.us>

Subject: RE: NHE boil-off experiment Date: Mon, 13 Jul 1998

Dear Dr. Miles,

Sorry for delay in my reply. By the way, Dr. Asami has moved to Tokyo, my office, the Institute of Applied Energy, and his e-mail address is . . .

Regarding the series of boiling-off experiments in NHE Laboratory, we do not have any clear indication of so-called "heat after death" although we know that temperature measurement in the cell after boil-off is not so easy and reliable because of only vapor. General observation with the series, we were able to detect any anomalous heat generation over about 10 mW during normal electrolysis condition, and 3 W during boiling with up to 50 W of input power. However, we could not detect any events of excess heat generation over the instrumentation limitation except some cases with clear explanation such as "spill over of electrolyte".

We know that Prof. Fleischmann may start again speaking about "NHE excess heat with his apparatus", because we have handed out data set of ours at ICCF-7 this April. The discussion with him started from the end of 1994 till ICCF-5 April 1995, and his argument of "excess heat" of very low amount was not accepted by any colleagues here including even Prof. Ikegami. I do not intend to discourage you but this is the things happened here where we know and trace.

Best regards,

Kazuaki MATSUI

The Institute of Applied Energy-

SHINBASHI SY BLDG.

14-2 NISHISHINNBASHI 1-CHOME,

MINATO-KU, TOKYO, 105-0003, JAPAN

> Original Message
> From: el [mailto:melmiles@ridgecrest.ca.us]
> Sent: Tuesday, July 07, 1998 12:04 PM
> To: mac@neon.iae.or.jp
> Subject: NHE boil-off experiment
>
>
> Dear Dr. Matsui,

>
> I have received all of the computer raw data relating to my
> Fleischmann/Pons
> experiments at NHE from Mr. Sumi. It appears that there is a discrepancy
> between my notebook data and the computer data for a short time period
> following the boil-off experiment. It seems that the cell
> voltage exceeded
> 100 volts at about the same time that the cell temperature exceed 100
> degrees C and that these figures were confused. According to the computer
> data, the cell did not remain hot after it boiled dry. I still think that
> there was excess heat produced by the cell during the boil-off experiment,
> but this all needs a re-evaluation. Any cell that remains hot following a
> boil-off experiment would be a clear indication of a large excess heat
> effect. However, this was apparently not the case in my
> experiment based on
> the computer raw data. If possible, I would like to know if there was any
> cell that remained hot following a boil-off in any other NHE experiment.
>
> Sincerely,
>
> Mel Miles

1998-09-09

Bury Lodge heading

9th September 1998.

Dr. Melvin Miles,
Chemistry and Materials Branch,
Research and Technology Division,
Naval Air Warfare Center Weapons Division,
China Lake, CA 93555
U.S.A.

Dear Mel,

I am glad that you have had a vacation in Oregon even though it must have been rather strenuous!

Many thanks for your letter and the enclosures. I need to write to you in some detail about the various points you (and your correspondents) have made and I will do this during the coming weekend. At that time I may explain to you the reason(s) for the delay.

Meanwhile, this is just a short note to tell you that I am currently writing up the extensive preliminaries to the data evaluations. I will send you all of this material in due course although there may be some further delay with regard to the spreadsheets. I have found it necessary to reconstruct some of the calculations I did in 1992 which were the background to setting up the whole ICARUS scheme. It seems like a waste of time and effort but I believe that in the end it will clarify the material. The first batch of "stuff" will cover these calculations as well as the analysis of a "blank" experiment.

Best regards,

Martin

1998-09-10

NAWC heading

DATE: September 10, 1998

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

Several new items have come up that I want to discuss with you. Stan Szpak called me today regarding my NHE report that I sent to him last week. He is convinced that my co-deposition experiments present strong evidence that this method gives reproducible excess heat events. He wants to include my figure showing this in a publication that he is preparing. I am faxing this figure to you. He claims that re-combination can be ruled out as a source of excess heat. From a calculation that I did today, Cells A-2 and A-3 produced 64,000 Joules of excess heat or more. This would be equivalent to recombining the gases to produce 3.6cc of liquid D_2O. That would be outside the error range of my volume measurements, hence recombination could not be the source of excess heat. For these experiments, even the NHE analysis method showed an excess heat effect. Stan Szpak will include my name on this paper, which he plans to submit to Fusion Technology. I will fax you a copy when it is ready to send out. Perhaps we can apply your methods of analysis to these co-deposition experiments at some future date.

Last weekend I plotted out the temperature and voltage over a 24 hr. period for the Pd-B data. This was quite tedious but also informative. I hope to eventually get this data in the Excel spreadsheet format from Mike Melich. Anyway, my plots show that a longer heating pulse such as 12 hrs. would be better for establishing the baseline for the temperature and voltage. This is in accord with what you stated earlier. I hope to do further analysis this weekend that will yield the value for the cell constant. For the data that I used, there was no positive feedback following the heating pulse. I will fax you this plot sometime next week.

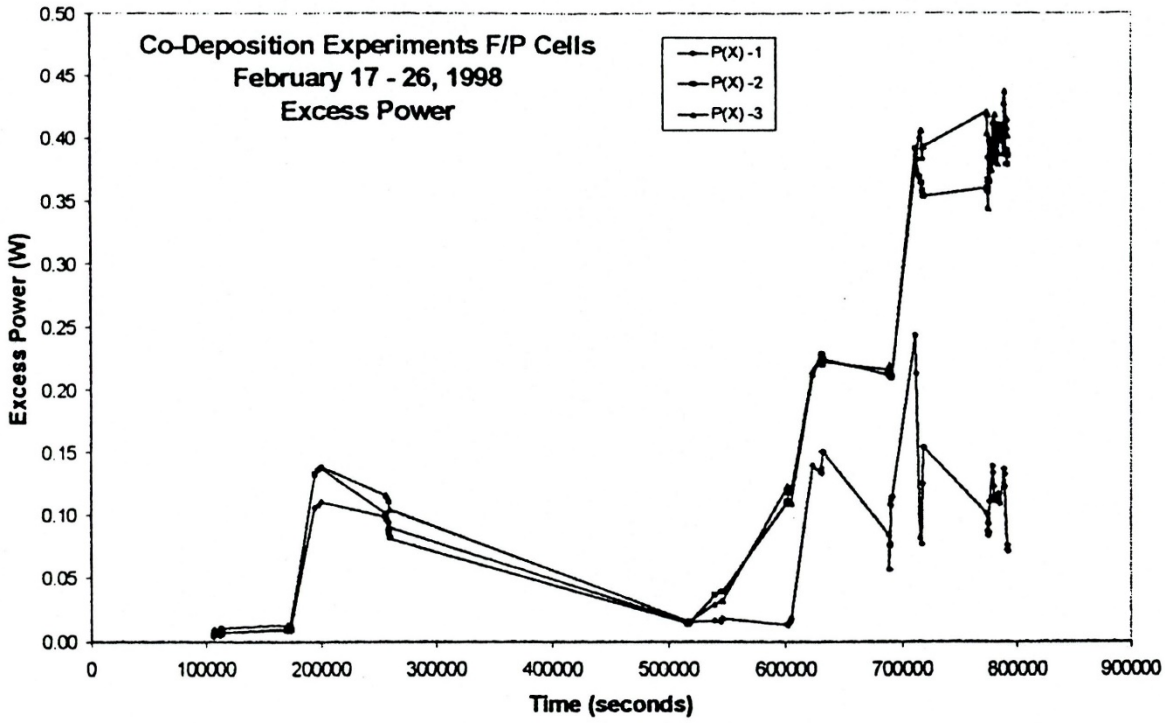
I have a question concerning the Icarus 2.00 Handbook. The values given on page 455 for the heat capacity of D_2O and H_2O vapor seem to be in error. For example, my NBS Handbook gives 34.27 J/mol.K for the heat capacity of D_2O vapor. Please let me know what values that you use.

There is a controversy involving Russ George and Mike McKubre. In an experiment conducted by Russ George at SRI involving the method reported by Les Case, there was evidence for helium-4 production. Russ George has publicized this result and has prepared a paper contrary to Mike McKubre's wishes. Russ George talked to me about this by telephone today. He wanted to fax you a copy of his report, hence I gave him your fax number. Perhaps you have already received it by now. If these results prove correct, it may be a big help in reviving interest in cold fusion.

I will let you know next week concerning the progress of my analysis of the NHE data. I wish I had more time to spend on it, but I have been busy writing proposals for funding for next year.

Best wishes,

Mel Miles



1998-09-14

NAWC heading

DATE: September 14, 1998

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I am faxing you my data analysis for the Pd-B NHE experiment covering the time period of 93531774 s to 93636774 s (December 26 - 27 of 1997). First, the cell voltage fluctuations are only +0.02V, thus the data is good. I graphed this on 25 cm × 38 cm paper which was slow and tedious work. The graph shows that a heating pulse covering a longer time period would have been very helpful in establishing a better baseline. From the graph, I obtained T_2=319.27K, T_1=317.20K, E_2=6.348V and E_1=6.456V. My bath temperature average was 295.205K. From this I obtain $(k_R{}^1)_2=7.712\times10^{-10}$ W/K^4. For my NHE Report (p. 17), I used $(k_R{}^1)_{11}=8.112\times10^{-10}$ W/K^4 assuming no excess heat. This difference could be explained by an increase of excess power of 0.0107 W rather than the assumption that the excess power is constant. This seems logical to me since a large weekend D_2O addition (9cc) plus reducing the current from 0.502A to 0.402A produced considerable cell cooling (negative feedback). Thus the recovery of the excess heat during the heating cycle could have readily equaled the 10 mW. Nevertheless, there is no clear evidence of positive feedback in the graph. It would be interesting to compare $(k_R{}^1)_2$ with the values based on backward and forward integration, i.e. $(k_R{}^1)_{21}$ and $(k_R{}^1)_{31}$.

I plan to do similar treatments to other blocks of data as my free time permits. It would be much easier to have this data in the Excel spreadsheet format and then plot it directly by computer. However, I can see that the analysis of the graphs would then not be as accurate.

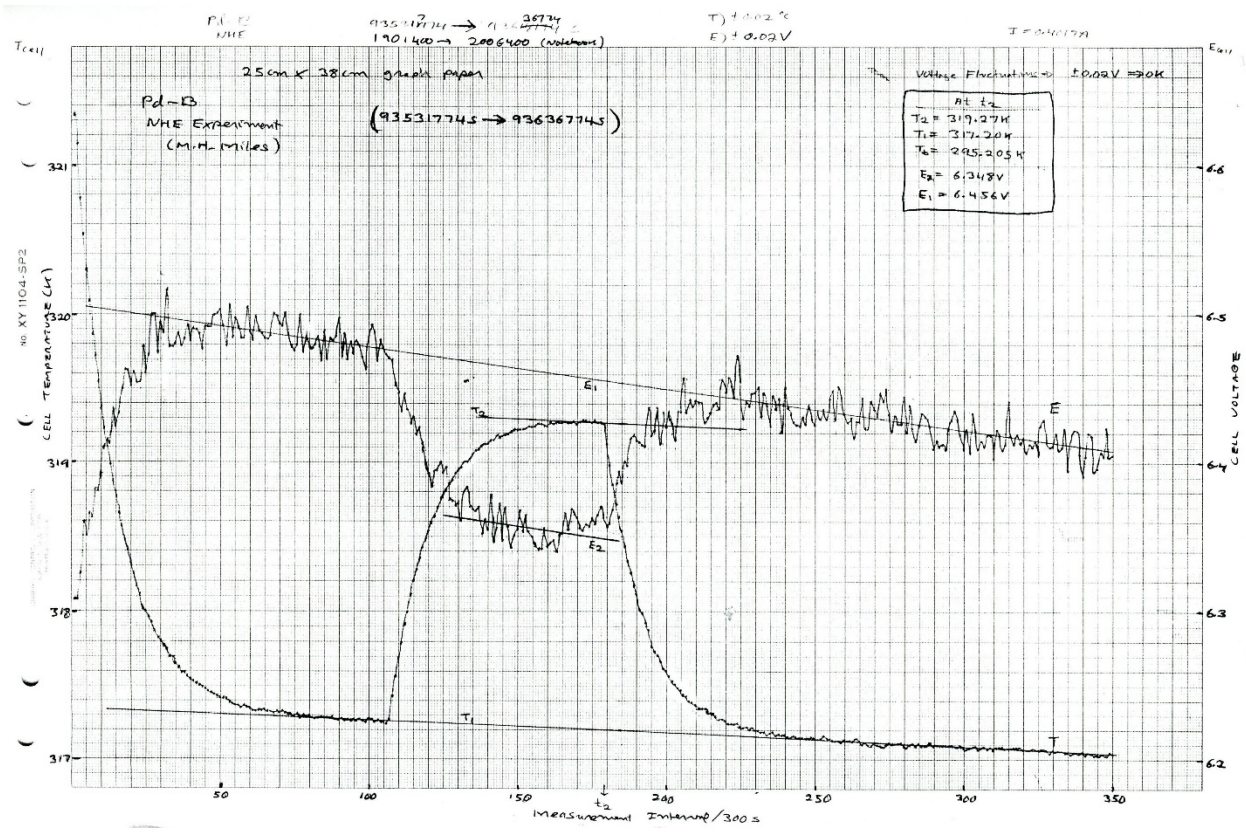
Please give me any comments or suggestions regarding my analysis of this data. I could do an exact treatment by computer using Eqs. 18-22 plus Appendix A in my NHE Report, but I don't think this would significantly change my results.

Best wishes,

(signed)

Mel Miles

P.S. To my knowledge, NHE never performed a single graphical analysis of this type. It seems that they went off on their own and ignored the Icarus Handbook.


25cm x 28cm graph paper
Pd-B
NHE Experiment
(M.H. Miles)
(935317745 → 936367745)
Voltage Fluctuations ±0.02V ≈70K
At t2
T2 = 319.27K
T1 = 317.20K
T0 = 295.205K
E2 = 6.348V
E1 = 6.456V
CELL TEMPERATURE (K)
CELL VOLTAGE
Measurement Interval/300 s
t2
E1
E2
T2
T1
E
T

Pd-B Data Analysis for 93531774 s to 93636774 s

Notebook Times} 1901400 → 2006400 s
December 26 → December 27, 1997

Determination of $(k_R')_2$. See ICARUS 2-00, pp. A-9 – A-11

Graphing Results}
$T_2 = 319.27\,K \Rightarrow \Delta\Phi_2 = 24.065\,K$
$T_1 = 317.20\,K \Rightarrow \Delta\Phi_1 = 21.995\,K$
$T_b = 295.205\,K$
$E_2 = 6.348\,V$
$E_1 = 6.456\,V$

Assume Q_F = constant and $\Delta Q = 0.2506\,W$

From Eq. A-12, p. A-11

$$(k_R')_2 = \frac{(0.2506 - 2.5934 + 2.5500 - 0.01248 + 0.01110)\,W}{(319.27)^4 - (317.20)^4}$$

$$\boxed{(k_R')_2 = \frac{0.2058\,W}{0.026685 \times 10^{10}\,K^4} = 7.712 \times 10^{-10}\,W/K^4}$$

where $C_{p,D_2O,g} = 34.27\,J/mol\cdot K$
$C_{p,D_2O,l} = 84.349\,J/mol\cdot K$
$L_{D_2O} = 41672.60\,J/mol$
$P(\Delta\Phi_2, t_2) = 0.08989\,atm$
$P(\Delta\Phi_2, t_1) = 0.08058\,atm$
$P^* = 1.00\,atm$ (assumed)

Compare with my NHE Report (p. 17)

$$\boxed{(k_R')_{11} = 8.112 \times 10^{-10}\,W/K^4}$$

Lower Bound
Assumes $Q_F = 0$
(no excess heat)

Note} $(k_R')_2 = (k_R')_{11}$ if power (numerator) is 0.2165 W

Date 9/9/98 CO- Deposition

CO- Deposition Analysis

Stan Szpak — recombination can be ruled out based on his studies

$$D_2 + \tfrac{1}{2} O_2 \rightarrow D_2O \qquad \Delta H^\circ = -294.600\ kJ$$

ΔH_f°) 0 0 -294.600 $\left(E^\circ_H = 1.527\ V\right)$

From Fig-13, NHE Report

0.40 W for 100,000 s → 40,000 J
0.20 W for 80,000 s → 16,000 J
0.10 W for 80,000 s → 8,000 J
} 64,000 J excess heat

0.02 W for 300,000 s → 6,000 J → omit

Assuming re-combination

$$(64{,}000\ J)\left(\frac{1\ mol}{294{,}600\ J}\right) = \underset{(0.218)}{0.2\ mole}\ D_2O \text{ produced}$$

$$(0.2\ mol)\left(20.02748\ \frac{g}{mol}\right)\left(\frac{1\ cc}{1.1055\ g}\right) = \underset{(3.62)}{\boxed{3.6\ cc}}$$ → should be obvious by volume measurements

Cell A-1		Cell A-2		Cell A-~~2~~ 3	
Exp.	Theor	Exp	Theor	Exp	Theor
7.4 cc	8.1 cc	7.7 cc	7.2 cc	8.7 cc	8.1 cc
⇒ lowest excess heat					

⇒ Volume of D_2O consumed is close to theoretical.

⇒ Re-combination does not explain the excess heat observed.

1998-09-15

Bury Lodge heading

15 September 1998

Dr. Melvin Miles,
Chemistry and Materials Branch,
Research and Technology Division,
Code 48 23000D
Naval Air Warfare Weapons Division,
China Lake, CA 93555-6100
U.S.A.

Dear Mel,

First of all, many thanks for your FAXes of 31/88998 and 14/9/1998 which I will respond to separately - my replies so far have been confined to saying that I would do so in due course!

The time has now arrived for me to write up and send to you the very first stage of the reanalysis of the ICARUS-2 Systems and data sets and this is contained in this letter. You will see that this deals first of all with the analysis of a data set generated by simulations using the most drastic simplifications of the differential equation representing the calorimeters. Nevertheless, the use of such drastic simplifications allows one to place important restrictions on the methodology of the data evaluations and these restrictions were at the root of the specification of the ICARUS-1 Systems (1992-1993). The second part deals with the analysis of one measurement cycle for a blank experiment (Pt cathode polarized in O.1M LiOD/D_2O) carried out in 1995 with an ICARUS-2 System. You will see that this follows the predictions based on the simulation (as closely as one can expect). The stage is therefore set for the analysis of Pd-D_2O type systems and I will include also some comments on simplified and rapid methods of data analysis which I am sure we will wish to use.

As much of the present letter follows on material contained in my long letter of 23/6/1998, I will number the separate issues raised here using the earlier numbering system (i.e. I will start with **Item 16**) so as to facilitate the cross-referring of the texts.

However, before I start with that item, I want to comment again on the general "philosophy" underlying the research. To some extent this hitches onto the comments made by Dave Nagel in his e-mail message to you of 15/8/98. I disagree completely with the view he has expressed that one may not need to carry out an accurate data evaluation if the effects observed are sufficiently large. This is a view which has been frequently expressed by other scientists (including those who were at the Sapporo Laboratories). It is true of course, that if the effects are large, then they will be apparent even to a cursory inspection - even to an inspection of the temperature-time series as was the case for some of our data sets collected in 1989 [(1)] and to those collected by the Harwell Group[(2),(3)] (although the presence of these effects was not detected by members of the

Harwell Group itself[4]). However, such inspection of the data is inadequate. Surely we should always aim to draw our conclusions at the highest levels of statistical significance readily accessible when using a given piece of instrumentation? This brings in its train the need to determine the "instrument function" of the devices, if at all possible based on models in turn based on the laws of Physics. This has to be followed by the determination of the precision and accuracy of the evaluations.

Once we have carried out such a task we should then use the instrumentation to determine the build up of the generation of excess enthalpy generation with time. Surely it is not very scientific to restrict attention to those parts/events where the excess enthalpy generation is sufficiently large to overcome the deficiencies of inadequate methods of data analysis?

There are other reasons for trying to establish the instrument functions at high levels of precision and accuracy. One such reason is that such investigations point the way to the improvement of the instrumentation (here the projected use of ICARUS 14 calorimeters). A second reason is that this opens the way for further experiments such as the evaluation of the fluctuations of the thermal output - this would be quite impossible if we place arbitrary restrictions on the precision and accuracy. I had several programmes of work in these areas which had to be abandoned.

The situation with regard to the N.H.E. investigations, is, however, more complicated.They were told that the methods they proposed to use were unsatisfactory (indeed, they were provided with sample evaluations to demonstrate this point). Nevertheless, they insisted on using these methods and, moreover, applied this methodology to the data sets [5] which had already been classified as being unsatisfactory [6],[7]. I believe that it is therefore necessary to provide a clear account of at least some of the available methods of data analysis (and this letter is a first step in that direction) and then to use this methodology on suitable examples of Pd based systems polarized in D_2O solutions. You have made much the same points.

I note also once again that the ICARUS-1 System was supplied in 1993 to N.H.E. as a Version 1. Low Power Measuring System for Three Cells (Title page of [3]) with the proviso "The experimental equipment and hardware are similar to those which we currently use. It is envisaged that updates of the software will be provided from time-to-time. The evolution of this software will be dependent on the needs of the laboratories taking part in this research programme" (page 62 of [8]). It is really remiss of the scientists concerned with the N.H.E. programme to change the objectives of the programme (i.e. of those parts concerned with ICARUS-1) and to imply that we ever had any intention of following their particular objectives. Moreover, they have evidently persuaded everybody else that we agreed that their objectives were part of the original scheme of work.

Of course, it is true that my intention was that ICARUS-2 should guide N.H.E. through the study of "positive feedback", to the measurement of higher levels of excess enthalpy generation leading on to the "boiling episodes" and the study of "Heat after Death" with the possible implementation of demonstration devices based on these two phenomena. The principal means of achieving such objectives were to be modifications in the data processing strategy linked to

appropriate "Case Studies". However, I could see no point in doing any of this until the ICARUS-1 programme had been completed i.e. until there was a clear demonstration that N.H.E. could successfully measure low levels of excess enthalpy generation.

As I have already outlined to you, ICARUS-2 did not follow this strategy. Instead it turned out to be a revamp of ICARUS-1 using new hardware, an inclusion of the "Switching Boxes" (with a catalogue of errors) and the construction of the misconceived weighing systems. I did not agree with any of this. [75]

I have reiterated this sad chronicle of events just in case it should prove to be possible for us to follow the original ICARUS-2 strategy (at least in part) using the data sets which you collected during your stay in Sapporo and any such data sets which we may be able to obtain from N.H.E. Please see here my highly CONFIDENTIAL letter which accompanies the present one.

Before I launch myself on the main part of this letter, I want to make one further general observation concerning the measurement of small enthalpy changes. The fact that enthalpy changes may be small does not make them uninteresting. I will restrict myself here to just one historical example drawn from the field of Nuclear Physics. It was the observation of a small anomaly in the specific heat capacitance of hydrogen coupled to the measurement of the rotational fine structure which led to the discovery of nuclear spin (there are many examples of this kind). It is a good job that the scientists in the late 1920's and early 1930's did not believe that only large effects are significant! If we "fast forward" to the 1980's and 1990's, then it might well have turned out that we would always have been restricted to the measurement and explanation of small enthalpy changes. Fortunately, this is not the case. However, I am quite sure that if we restrict the calorimetry to the measurement of large enthalpy changes, we will then miss a great deal of the "new story".

I note that **Items 4, 5, 6, 8, 10 and 14** of my letter of 23/6/98 are relevant to this Introduction.

Item 23 A Miscellany of Further Comments

The most important point under this heading is that most of the background for the previous items is rather old dating back to 1990/92 even though the raw data for the example discussed in **Item 21** were collected in 1995 (this can be seen by comparing **Item 21** with the material in [(9)] which dates from 1992 with the exception of the experiment described in Fig. 10 of that paper which dates from 1994). In consequence, much of the material is "half-remembered" and I am certain that I have left out some of the important points and have belabored other aspects - the account is certainly incomplete. Our intention in 1992/93 was to produce a simplified but adequate method of data analysis as specified in **Item 19** and not an apologia for our approach to the calorimetry (not that the previous items are in any sense such an apologia - that raises quite different issues).

[75] MCHM This parallels our experience with NHE. Great guys (especially Asami) but with VERY fixed ideas. This was a very important opportunity – wasted.

At the same time I believe that it is very important to produce a detailed analysis and account (as far as is possible at this stage) of the methodology which we adapted. This is especially important in view of the misleading comments which have been made about the calorimetry e.g. [(5)]. I would therefore like us to regard the previous items as a "working draft" and would like to ask you for your detailed comments on amplifications/clarifications/contractions which could be used to prepare a second draft - hopefully the final version of this Document. I realise that it would probably be impossible to publish such an account but, at the same time, I believe it is important to publish an abbreviated version which should also contain analyses of given data sets using the misguided approaches which have been adopted. Such an abbreviated version could then refer to the detailed account which should be deposited in a secure but accessible place.

The second important point is that you will note that most of the present Document was written last month but that I have once again held it back. There are several reasons for this delay which I will also touch on in a covering letter. I will comment here on just two aspects. The first is that it has not been all clear what the next steps might be and what function (if any) a reanalysis of existing data sets might have in such future research. I had hoped that all of this would be have been cleared up by the end of last month but it is now evident that this will take even more time. There is therefore now no point in delaying this Document further. The second aspect is that I have had a number of meetings which should influence the way in which the present Document is written. The main factor here is my meeting with Hideo Ikegami which certainly change my point of view and cause me to make some changes in the text of the preceding Items - as well as making me separate the present Item 23 from the rest of the text. In this context I would like to have your corrections to Items 18 – 22 so as to make this text is suitable for sending to him as well as to a number of other people. This list should include Jean-Paul Biberian, Kikujiro Namba, Giuliano Mengoli, Giuliano Preparata (vice Vittorio Violante), Charles Beaudette – any more, any deletions?

My major immediate objectives would be Hideo Ikegami and Jean-Paul Biberian. My reason for choosing Hideo Ikegami is that he has told me that he has graphical output of the data sets collected by the N.H.E. and that this could be made available to me. This raises important questions/issues which I will discuss in a separate letter. I would therefore like to send him a rather "neutral" account even though I note that all the information (and more!) Has been available to him/N.H.E. for quite some time! It is just possible that this might persuade our Japanese ex-colleagues to release some of the data.

My reason for choosing Jean-Paul Biberian is that the Group at Grenobles has data sets which include 7 experiments carried out with Johnson Matthey Material Type A and all of which gave excess heat. I believe that some of these experimental results were very close to the ones which we discussed at ICCF 3 (see also [(9)]). Furthermore, the experiments carried out by the Group at Grenobles complement our work in one important respect and I believe that their data actually may be "cleaner" then our data (less "noise"). It would therefore be very important to analyse these data sets and I note that Jean-Paul Biberian has promised several times to send them to me. I anxiously await their arrival!

There is one further aspect of this part of the saga which is important. The Lonchampt-Bonnetain paper was presented at ICCF 6 by Biberian where he disclosed that the electrode material was of the Johnson Matthey A Type. This important piece of information disappeared from the published text and at ICCF 7, Jean-Paul did not remember anything about this aspect!

The question of whether and, if so, how one might be able to obtain "raw data sets" from the various research groups (including the data sets collected at Salt Lake City and Sophia Antipolis!) Has occupied me intensely. It will be apparent that so far there has been virtually nil return. It has therefore appeared to me that the relevant approaches should perhaps now be made at an official level. This would have the advantage that this would require an official "No" if access is once again denied - rather than relying on not answering letters. I believe that you will see that I have in mind the ways in which official approach(es) might be arranged.

I believe that it is useful to consider somewhat further the execution and analysis of experiments which fall broadly under the "ICARUS Umbrella". I have found this whole saga to be quite staggering and have watched the developments with increasing dismay and despondency. Taken at its face value one must believe that the workers concerned do not understand the difference between differential and integral coefficients, the disadvantages of differentiating "noisy" data as compared to integrating such data, the differences between the precision and accuracy of data evaluations, the recognition of "negative" and "positive feedback", the analysis of cooling curves and much else. The "much else" includes the understanding of relaxation and recognition of the presence of strange attractors and the way in which the effects of such complications can be circumvented. Of course, it is possible that the researchers concerned are so incompetent that they understand none of these matters but what is so remarkable is that they have failed to understand **any** of these topics even when they have been described to them.

I will single out here just one particular aspect which arises from the present Document. One must ask: "how is it possible to observe the behavior of "blank experiments" described by the "lower bound heat transfer coefficient, $(k_R')_{21}$" as illustrated in Figs. 50 and 51 and yet explain the behavior of Pd-D_2O systems such as that illustrated in Fig. 8 of [9]?" The contrast in the behavior of "blank experiments" and those using Pd-D_2O systems has been repeatedly pointed out to N.H.E. it is relevant here to reflect also on the precision and accuracy of the experiments. Of course, if the precision is as high as that shown by Figs. 50 and 51, then there will be no difficulty in interpreting **changes** in the rates excess enthalpy generation as small as 1 mW at the 10 σ level. However, the high precision of the instrumentation (relative errors being below 0.01%) has been converted into a 10% error in the paper from the Group at N.H.E. (5). It is hard to see how anybody could make such an assertion while still keeping a straight face. If the errors were as high as this, then it would be impossible to say anything sensible about the calorimetry - for that matter, it would remove one of the main planks of scientific methodology. It is all rather reminiscent of the sardonic comments made about the Large Electron Positron (LEP) Collider at Geneva. Skeptics say that the acronym LEP stands for Large Errors Pay: one simply decides what one wishes to observe and adjusts the errors to fit in with the conclusions.

Of course, the question of the magnitude of the errors raises three further important questions: (i) what error limits are required so as to be able to detect excess enthalpy generation at an adequate level of statistical significance? (ii) what is the difference (if any) between the experiments carried out with the ICARUS Systems and ICARUS look-alikes and with other types of calorimetry? (iii) how can one assess the error limits of a given piece of instrumentation?

The first question belongs to the category: "how long is a piece of string?". The answer is that one simply stops the development of the methodology when one is able to make an adequate set of measurements. I note here that this particular specification is itself dependent on the physical size of the systems being investigated as well as the chosen operating conditions in our particular investigation the limit was certainly reached when the errors had been reduced to the 0.01% level. Naturally, the first question impacts on the second and I note that it is the use of less precise and accurate calorimetric methods which has bedeviled so much of the research in this field. The reason is that with the use of less precise/accurate methods, it becomes impossible to monitor the build-up of excess enthalpy generation. This then brings us to the third question and my answer to this is: exactly with the methods outlined in the Document at least as far as isoperibolic calorimetry is concerned (although it is not very difficult to specify improvements in those methods!). The answer to this question brings us to very interesting further lines of inquiry which can be summarised by the following question: "why is it that N.H.E. have never made any sets of raw data for blank experiments available for further analysis?" If one considers this question in a naïve way, then one would say that there can hardly be any reason for not releasing data sets which do not show any generation of excess enthalpy! I believe that one should take precisely this line of approaching any further negotiations - as I did in my correspondence with Harwell. However, from their point of view one cannot take such a naïve approach because the release of such data would allow others to establish the error limits and the validity of their various assertions (5). It is relevant that not only were all of the data sets for Pd-D_2O systems removed from the papers sent back to me from France but also data for about 400 calibrations carried out on "blank experiments". Of course, my colleagues may have been concerned that my analyses could establish all of the errors which have been made in setting up these calibrations but what they did not know, was that I had also worked out ways of eliminating the effects of these errors.

Instead of seeking to establish the correct way(s) of calibrating the systems, the Group at N.H.E. use the procedure leading to $(k_R'o)_{362}$, probably coupled to timing errors in the calibration pulse which they did not allow for. Needless to say, this produced non-sensical results which they used as a justification for substituting their invalid methods of data analysis (the fact that this method was invalid has already been pointed out to them [(7)]). Moreover, this invalid method of data analysis was applied to just two experiments, regarded as being typical, although the fact that there were malfunctions in these experiments has also been pointed out [(6), (7)].

I realise that one should not resort to explanations of such behaviour in terms of conspiracy theories - except as a last resort. However, in this particular field (as in other previous examples) people have resorted to do increasingly odd explanations especially to the behaviour of the

dramatis personae. Is it not therefore more straightforward to assume that there is an underlying agenda which seeks to hinder/prevent further work in this field?

I see that I stated in Item 22 that I would specify "short-cuts" to the data evaluations in the present Section. However, these "short-cuts" will emerge more naturally in succeeding Items dealing with the analyses of data sets you collected in Sapporo.

(Following this is a draft of a paper which was later published by SPAWAR. This draft is very similar to the final version, which is here, starting on p. 5):

http://lenr-canr.org/acrobat/MosierBossthermalanda.pdf

1998-10-30

Bury Lodge heading

30th October 1998.

Dr. Melvin Miles,
Chemistry and Materials Branch,
Research and Technology Division,
Naval Air Warfare Center Weapons Division,
China lake, OA 93555-6100.

Dear Mel,

I dare say that you will have given me up for lost but, as you will see, I am still around and reactivated.

I am writing this FAX to let you know that I will be sending you a very long Document tomorrow under separate cover (with a copy to Mike Melich). This Document deals with the ICARUS-Methodology and contains 70 pages of text (widely spaced), 65 figures and 9 spreadsheets. In spite of its length it actually only covers the analysis of a data set generated by calculation as well as of one measurement cycle for a "blank" experiment. This is intended to serve as background for the analysis of data sets for the Pd-D_2O systems. Restrictions of the further analyses to valid methods coupled to the use of short cuts will allow these further analyses to be carried out quite expeditiously.

I should explain why I have considered it necessary to produce this Document even though some of it covers much the same ground as my letter to you of 18/6/98 as well as of the Paper and Poster I gave at ICCF 7. In the first place, I believe it is always essential to determine the Instrument Function (or of a parameter or sets of parameters which define the Instrument Function) and, associated with this to validate the methods of data analysis. I believe that the validation is best done using simulated/calculated data. Secondly, one then needs to see the extent to which "blank" experiments conform to expectations. Thirdly, one needs to investigate the ways in which methods of data analysis may fail. This is especially important in the present example because N.H.E. used precisely the methods of data analysis which had been shown to fail.

In my covering letter to Mike Melich I have also raised, fourthly, the question of whether such a Document (or a revised text) should be deposited in a safe and accessible location so that we could then prepare a much abbreviated version, for publication (ICCF 8?). Fifthly, whether we could use a revised text as a lever to extract data sets from N.H.E., Technova, the Group at Grenoble and one or two further laboratories. Sixthly, and lastly we have to consider/establish what role the analysis of existing data sets may play in the future research programmes.

You will see that the Document I am sending to you was mostly written last month. The reason why I have held It back is because I thought that decisions for further work could be

reached quite quickly. However, it now seems to me that these decisions will still take some time so that it now seems sensible to me that we should at the least start one small ball rolling!

I shall be going to Italy on Monday and I hope that I will be able to reach some positive decisions there. I will write to you again on my return here on 8/11/09.

Meanwhile, best regards,

Martin

1998-11-07

NAWC heading

DATE: November 7, 1998

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

Thank you for your letter of 30 October 1998. I had not given you up for lost since I know you are very busy. Anyway, I have been busy writing proposals in order to stay funded for another year here.

I will be mailing you a copy of a recent paper by Stan Szpak, Pamela Mosier-Boss, and myself that has been submitted to Fusion Tech. This paper involves my co-deposition experiments conducted at NHE in Japan that show a good reproducible excess heat effect. This involves Section 4.2 and Figure 3. I plan on doing a more detail analysis of this data and hope that perhaps you can help. Stan Szpak told me to send you his best regards.

Dr. Tripodi and his friend Dr. Daniele Di Gioacchino visited with me in late September. Dr. Tripodi indicated a possibility that I could work for a few months in Italy next year on cold fusion. This seems to be the only possibility for me to continue work on cold fusion. I hope this works out.

I received an e-mail message from Mike Melich, but it does not seem that he can work out anything for future cold fusion work. Judging from several scientific meetings that I have been to there seems to be a very negative opinion against cold fusion. Dr. Carlin who replaced Bob Nowak at ONR is very negative toward the subject. He seemed interested in funding me for some molten salt electrochemical research until he found out that I had been involved with cold fusion. He has hardly talked with me since. I hope that I can find enough funding for next year at China Lake. It would be nice to have some breakthrough to change this negative opinion towards cold fusion. Perhaps Dr. McKubre's work at SRI will help towards this goal. Today I bought a copy of Wired Magazine that has a nice positive review of this subject, including a picture of you. [76]

I look forward to receiving the long document that you mentioned. I will consult with Mike Melich regarding the best place to keep this document. Regarding the data sets from NHE, I contacted Mr. Matsui and he seemed to be willing to give me this data. However, it has now

[76] JR Platt, C., *What If Cold Fusion Is Real?*, in *Wired*. 1998 https://www.wired.com/1998/11/coldfusion/

been well over a month and I have not received anything to date. I will contact him again and see what will happen.

It was nice to hear from you again. Please let me know how your analysis of my Pd-B results are progressing. I have graphed several more heating cycles, but it seems like my cell constant always come out somewhat smaller than I had hoped. Please let me know what cell constant you are getting.

Best wishes,

(signed)

Mel Miles

P.S. I am also faxing the abstract/title page of the Szpak, Mosier-Boss, Miles paper that I am mailing to you.

Szpak, S., P.A. Mosier-Boss, and M. Miles, *Calorimetry of the Pd+D codeposition.* Fusion Technol., 1999. **36**: p. 234. http://lenr-canr.org/acrobat/SzpakScalorimetra.pdf

CALORIMETRY OF THE Pd+D CODEPOSITION

Stanislaw Szpak and Pamela A. Mosier-Boss

Spawar Systems Center San Diego

San Diego, CA 92152-5000

and

Melvin H. Miles

Naval Air Warfare Center Weapons Division

China Lake, CA 93555-6001

Abstract

Thermal activities associated with electrochemical compression of deuterium produced on electrodes prepared by Pd+D codeposition are discussed. Three cases are considered: activities during and shortly after commencement of current flow, those observed during runs of several days duration and surface temperature distribution recorded by infra-red scanning. Experimental results show excellent reproducibility, high power outputs and the development of thermal instabilities resulting in the formation of local hot spots.

1.0 Introduction

An alternate method for the initiation of the Fleischmann-Pons effect is to employ an electrode prepared by Pd+D codeposition. This technique involves the electrodeposition from a Pd2+ salt solution at cell currents (potentials) so adjusted as to deposit the Pd film in the presence of evolving deuterium1. The effectiveness of this approach with regard to generation of nuclear and thermal events was reported by us in the initial phase of our investigation. One advantage of the codeposition process is rapid saturation with deuterium; atomic ratios D/Pd > 1.0 were measured within minutes[2]. . . .

1998-12-25

NAWC heading

DATE: December 25, 1998

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I have not had time to do anything regarding cold fusion lately. I am hoping to write a paper relating to my work in Japan over the Christmas holidays. My battery work for the Navy has kept me busy writing proposals and papers. I would like to be doing more with cold fusion but it has been difficult to find the time.

Merry Christmas and best wishes for 1999.

(signed)

Mel Miles

1999-01-12

Bury Lodge heading

Dr. Melvin Miles,
Chemistry and Materials Division,
China Lake,
CA 93555-6100
U.S.A.

12 January 1999

Dear Mel,

Happy New Year and many thanks for your letter of 25/12/98 and the interesting enclosures. I think that you have charted some very clear ways forward with the Advanced Batteries Programme and I wish you every success!

Perhaps I should say "Happyish New Year" or at least to express the wish that 1999 will be better than 1998 which was a real "Annus Horribilis".

I can see that you will have had very little time for the C.F. Project in recent months. My own endeavors with your important data set can best be described at being in the nature of a "Curate's Egg" i.e. "Good in Parts". During the autumn I attempted:

i. a complete ICARUS-style analysis of the Days 1-67 of the whole data set.

This study has given some interesting results (including the selection of (vi) below). However, it gradually became apparent that a full analysis might not be possible and, furthermore, that most of the analyses might not be publishable – even if they are feasible. All of this was therefore something of a waste of time.

More recently I therefore switched attention to:

ii. a detailed investigation of the results for Day 3;

iii. a study of the part of Day 68 leading to "Boiling to Dryness";

iv. investigation of the initial part of "Heat after Death" on day 68;

v. investigation of "Heat after Death" on day 69;

vi. investigations of "Heat after Death" for the initial part of Day 26.

This selection of topics has been due partly to my belief that it is the "Heat after Death" episodes (iv)-(vi) which are of prime interest coupled to the event (iii). The main interest in (ii) is the illustration of the mistakes made in the N.H.E. programme/data analysis also shown up clearly by (iii). I have to do some further work on this aspect as well as (I), partly to determine the causes of those mistakes (which link back to the long Document I wrote las year) and partly

because I need to establish the water equivalent for the cell which I need mainly for (iv) – (vi). The recent work on these aspects uses "guesstimates" and this work will need to be revised.

First of all then some thumbnail sketches. The rate of excess enthalpy generation increases to 7 – 10 W i.e. say 20 – 30 W cm^{-3} during (iii) and this rate is maintained in the initial phase of (iv). There is also a small rate of excess enthalpy generation in (v) (not really an "excess" but simply a "rate of enthalpy generation" as there is no enthalpy input!). (vi) gives a clear demonstration of Case 1 of "Heat after Death" using the classification in our paper in Trans. Fusion Technology 26 (1994) 87.

I believe that the time has come for me to write another draft Report which I would like you to consider in detail. There are two reasons why it is only a draft: firstly because some of the material is based on "guesstimates" of parameter (which I have referred to); secondly, because I need your replies to a number of key questions. It will then be possible to revise the draft and, ideally, I would like this final text to be part of your Series of Reports to the Navy (if this is possible). Again there are two reasons for wishing to have this Document part of the Official Report Structure; firstly, for contributing to the "rounding-off" of this part of your research, secondly, because I would like to discuss/determine with you whether we could write an abbreviated /condensed version for presentation, say at ICCF 8. As the extent of the material to be covered is very wide, it would certainly be very helpful if we could refer, where necessary, to a full account given in an Official Report.

May I ask you to give me your comments on all these suggestions?

I will also write to you in due course about he developments in Italy. The effort in Frascati is certainly going ahead but, unfortunately, the scope of the research programme is still not clear. The major thrust of the work will be in electrodiffusion but, as you will surmise, I am also keen that provision be somehow made for further work on "Heat after Death" (if possible based on the implementation of ICARUS-14 calorimeter), the measurement of the accumulation of ^4He in a variety of novel structures etc. etc. Above everything else, it is highly desirable that the work should pass through a further innovative stage which I believe is your forte. Unfortunately, it is clear that the Italian system is highly bureaucratic and decisions are a long time in coming.

It would clearly be very useful if I were able to send a copy of the proposed Report to the folks at Frascati and perhaps you could also let me have your comments on this.

One further matter which we need to discuss is whether we could devise some mechanism for getting access to other data sets, perhaps again using the proposed Report as a suitable lever?

I believe that we may shortly have to communicate rather frequently and I may decide to hitch up my gear to the e-mail. Meanwhile, we will have to rely on the FAX and you may find that it is not possible to reach my number (both my FAX and telephone have developed strange but understandable quirks in recent weeks). If you should find that this is so, then perhaps you could let me know and I will devise other ways of reaching me here.

All the best for 1999!

Yours,

Martin

P.S. I am sending a copy of this letter to Mike Melich as I am sure he will wish to keep posted about these activities.

1999-01-19

ELSEVIER

Journal of Electroanalytical Chemistry 465 (1999) 116

JOURNAL OF ELECTROANALYTICAL CHEMISTRY

Announcement

From Femto to Teraamps – an Electrochemistry Meeting to Honour the Work of Martin Fleischmann; received 19 January 1999

Location: Southampton

Date: 28 April – 30 April 2000

the purpose of the meeting is to explore recent developments in the many and diverse facets of electrochemistry initiated or furthered by Martin Fleischmann, FRS during his long and distinguished career. Hence topics to be discussed will range from fundamental physical chemistry to industrial applications of electrolysis.

Martin Fleischmann will present a Plenary Lecture and, commonly, but not exclusively, other speakers will have had a scientific association with Martin Fleischmann at some time during their careers. In addition to the lectures there will be a Poster Session and contributions are invited from all, whether established scientists or students.

The cost of the Conference will be £270 (including accommodation for Friday and Saturday nights and all meals during the Conference). The organising Committee hopes to have a number of bursaries to aid attendance by students.

Organising Committee

Guy Denuault
Laurence M. Peter
Derek Pletcher
Frank C. Walsh
Carole Chatley

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P11: S0022-0728(99)00042-X

1999-01-28

Bury Lodge heading

Dr. Melvin Miles,
Chemistry and Materials Division,
Naval Air Warfare Center Weapons Division,
China Lake,
CA 93555-6100
U.S.A.

28 January 1999

Dear Mel,

Many thanks for your FAX and I will get myself hooked up to the e-mail sometime soon. My attitude to the Internet, e-mail, etc. is somewhat reminiscent of St. Augustine's attitude to goodness. So I will soldier on with the FAX for the time being.

I will be very glad indeed to expand our circle to include Stan Szpak and I will send a copy of the next draft report to him. However, I will continue to need your input!

Your plan of action regarding your Cells A and B seems eminently sensible. We have indeed seen comparable effects in our own studies. In fact, the increase in cell temperature accompanied by decreases in the enthalpy input was one of the first effects which showed that we had to have excess enthalpy generation. Having started on a note of enthusiasm, I must also sound a word of caution. The effects the Group in Harwell had in their Cells 3 and 4 were somewhat related to the phenomena you want to describe (although, admittedly, they did not observe a decrease in temperature in their "blank cell"). What notice did they take of these effects? What notice has anybody else taken of the effects pointed out by Hansen/Melich and by myself? Stan and I had three papers on the extended analyses of the Harwell data sets. They never got published

Your plans regarding a publication for Cells A and B brings to mind a very important question: which cell have I been investigating when using this terminology? We need to clear up this matter post haste.

One of the other questions I need to clear up with you is that of the lay-out of the cells you used. Could you please send me a sketch diagram showing the approximate positions of the "long" and "short" thermistors, the heaters and the cathodes? Could you also show me a sketch of the anode design - was this a helix of fairly heavy gauge Pt wire or was it a Pt "basket"? [77]

Incidentally, re future publications, it is virtually certain now that ICCF-8 will be delayed until Spring 2000. However, it seems that the Italians have secured strong high-level support so ICCF-8 should be an high profile meeting.

[77] MM It was medium gauge Pt wire.

I also need to deal with two further important matters. The tenth anniversary of March 1989 approaches and there is the predictable increase of interest by media people. One question I get asked is: "is there an English version of the paper which appeared in Journal de Chimie Physique, 93 (1996) 711?" (journalists are not confident with their French). I think that this paper is regarded as our latest publication in a refereed Journal.

There is actually an English version of this paper which dates back to 1994. Actually, there are four versions but none of these got published! Mike Melich and Dave Nagel also asked me about the French paper at ICCF-7 and I told them that it had a very unhappy history and there the matter rested until just recently. It is the questioning which I have been subjected to which made me resurrect the old files which include the English versions of the paper, the correspondence with the Editor etc. etc. I have made a compilation of these Documents and will shortly send one copy to Mike Melich. One matter is extremely important: one of the versions shows exactly the point we had reached in the summer of 1994. It seems desirable to me that I should send some of this material to at least one of the journalists - more about him anon. There is also a covering letter to Mike and I have written to ask him whether I should send a duplicate package to you - possibly also to the journalist with Mike's and Dave's identity deleted. It strikes me that you might like to keep this package as a further illustration to future generations of the collapse of the scientific process.

The particular journalist I have in mind also opened up a line of questioning about Steve Jones' calorimetry and asked me why we had not responded/reacted to his comments. I replied that I could see no point in doing so because you had dealt with the matter and because Steve's points were so self-evidently incorrect. However, it seems sensible for me to send the journalist a further package containing Steve's paper, your various attempts at rebuttals, correspondence with editors, referees comments etc. I believe that I have most of this material but I think that it would be far better if you were to make up a new package and send this to me preferably with a covering letter outlining the history of this episode. If you are prepared to do this then ASAP is the motto: March 1999 is rapidly approaching!

Regards,

Martin

1999-02-18

NAWC fax heading

DATE: February 18, 1999

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I hope you received the outlines regarding the proposed Navy report to be published with Stan Szpak, yourself and me as authors. This fax with answer some of your questions in regard to your 29 January 1999 fax to me.

First, Cells A and B were of the China Lake design. The cell dimensions were 1.8 cm diameter and 15.0 cm in length. The cells were filled with 18.0 cc of 0.1M LiOD. The thermistors were located on the outside of the cells walls at locations 1.9 cm and 4.5 cm from the bottom of the cell. The thermistors were also located on opposite sides of the cell. Despite these differences, the two thermistors showed almost exactly the same temperature changes. I have been working evenings till midnight and on weekends to complete this paper. It is now being typed and I'll send you a copy in about a week for your comments. I am not allowed to work on cold fusion during regular working hours. I hope this paper will be published in Journal of Physical Chemistry. I have been very conservative in my scientific terms and have not used the words cold fusion or discussed any nuclear process. I simply clearly show seven occasions of excess power and that chemical explanations fail to explain the effect. The readers can then draw their own conclusions. By confining the discussions to basic physical chemistry, I hope the reviewers will relent and allow publication.

Regarding your questions regarding the layout of the F/P cells, I will send you a sketch showing the positions of the long and short thermistors. The anode was a helix of Pt wire but this was not of a heavy gauge. It was wound around a plastic cage structure.

I am not aware of an English version in Journal de Chimie Physique, 93 (1996) 711. I don't have a copy, hence please send me one if possible. I passed the French language test for my Ph.D. and think I could read it. I would appreciate receiving the 1994 material that you mentioned. I would like to have this material for my files for some future writing that I may do regarding cold fusion.

Regarding your last paragraph, I will send you a copy of my complete file regarding my correspondence relating to Steve Jones. This will include my letters to J. Phys. Chem. trying to get my reply published. I will also include all referee comments.

I am quite aware of the tenth anniversary of cold fusion and mention this in my latest paper. However, I refer to the excess power as the Fleischmann/Pons effect rather than using the term cold fusion. I thought the progress would be much greater after ten years, but the lack of funding

makes it very difficult. I hope this situation will eventually change. The papers that I expect to publish from my Japan research work may help.

Best wishes,

Mel

CHAPTER 5

FURTHER DESCRIPTION OF THE EXPERIMENTAL EQUIPMENT

5.1 The Dewar-type Electrochemical Cells

The Dewar electrochemical calorimeter cell is illustrated in Figure 5-1. These Dewars are silvered in their top portions so that heat transfer is confined almost exclusively to radiation across the lower, unsilvered part. The reasons for the choice of this type of design are outlined below.

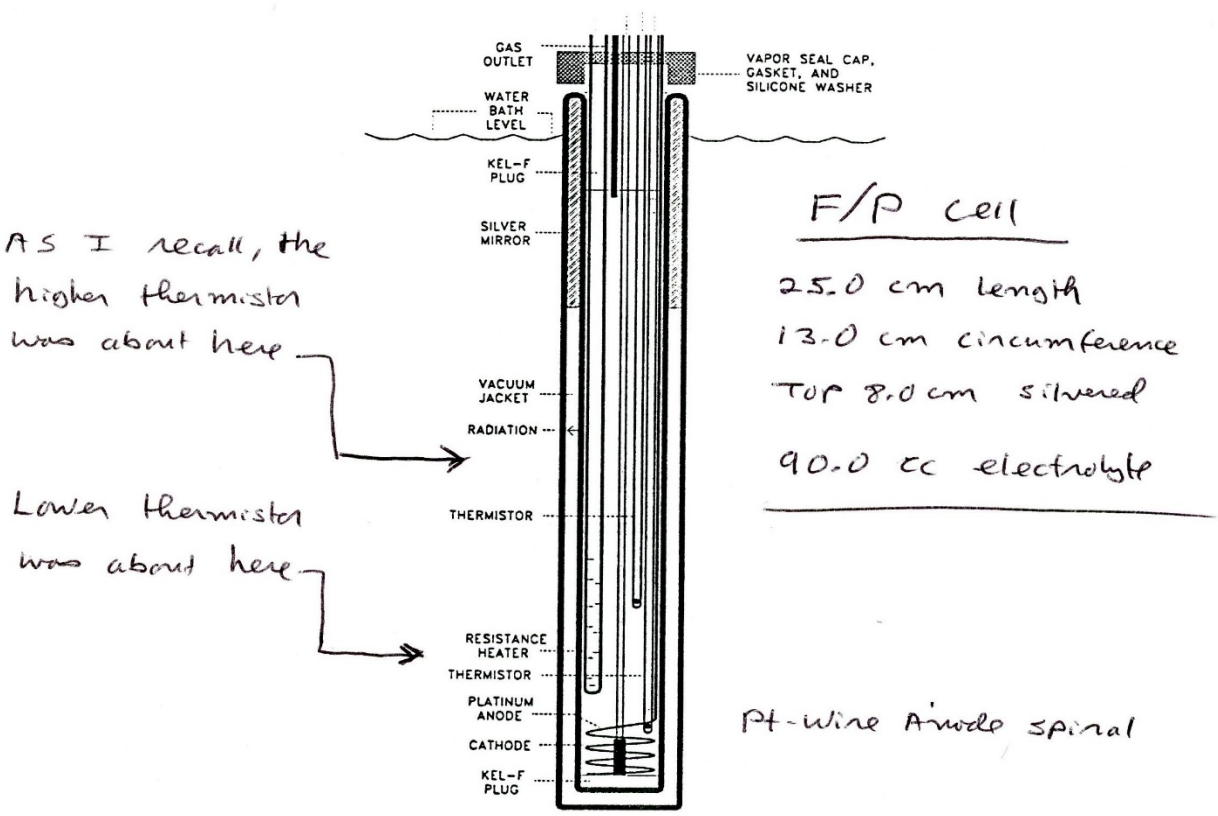

Figure 5-1. Schematic diagram of the single compartment open vacuum Dewar calorimeter cell silvered in the top portion.

The internal components of the Dewar are mounted in the deep Kel-F plug which seals the cell; the top of the cell is further sealed with Parafilm (not shown on the diagram). The Pt spiral anode is supported by a thin Kel-F disk at the base of the Dewar. The cathodes (Pd or Pt as the case may be) are mounted centrally . . .

From my Final NHE Report

Question: Could radiation emitted from the Pd cathode (Fig. 4) affect thermistor T_1 as shown in Fig. 3 ?? Yes - as shown later.

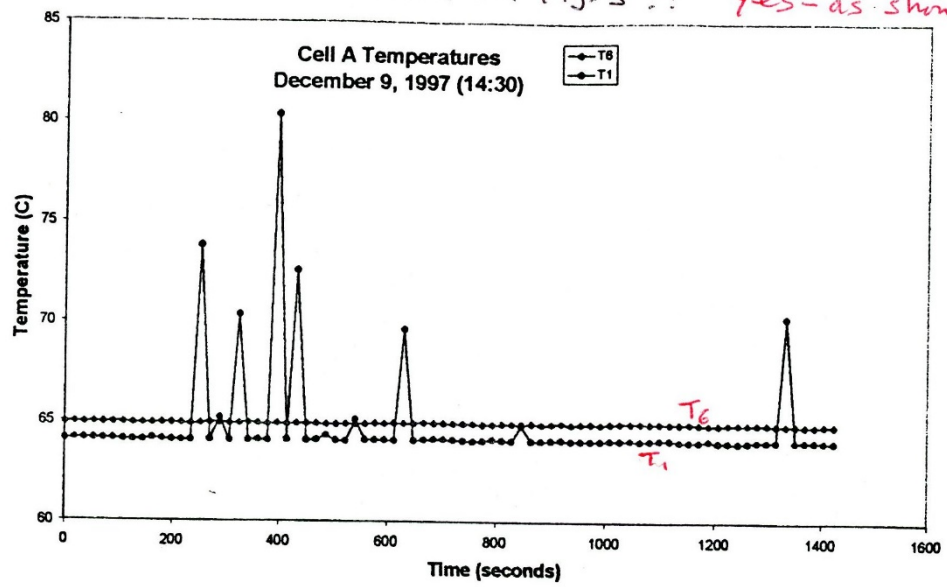

Figure 3. Thermistor readings versus time for Cell A (T_1, T_6).

These temperature excursions quickly returned to normal within the time period of the separate measurements (18 seconds). This suggests that the sudden rise and decline in the temperature readings for thermistor T_1 it is due to electromagnetic radiation from the palladium cathode rather than actual increases in temperature. Thermistor T_1 was located directly in line with the palladium cathode while thermistor T_6 was positioned higher on the outside cell surface. A schematic of the cell and thermistor positions is shown in Figure 4.

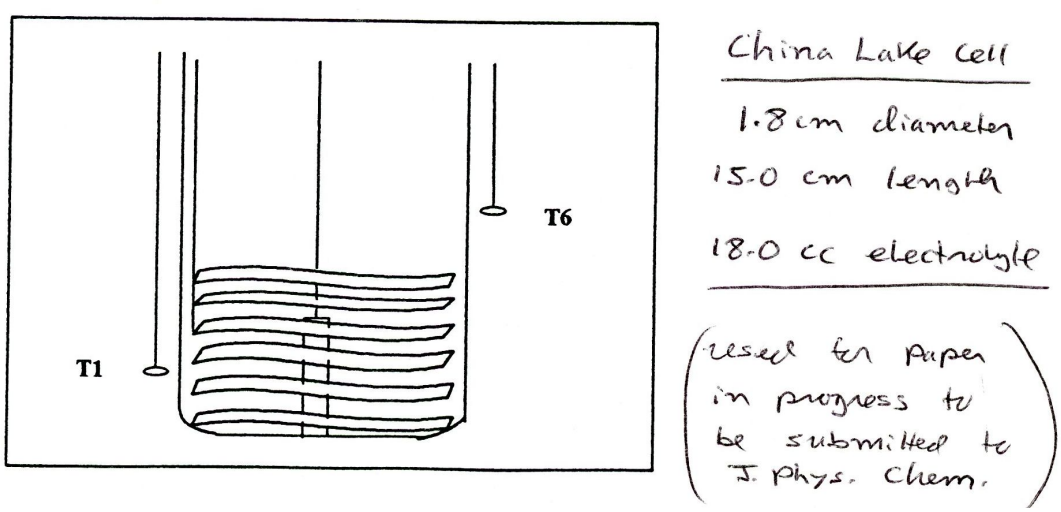

Figure 4. Schematic positioning of thermistor T_1 and T_6 relative to the palladium cathode rod and platinum anode coil.

1999-02-28

DATE: February 28, 1999

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I enjoyed talking with you Friday. I now have all the Jones correspondence collected and I will send it air mail Monday. If you need it sooner please let me know right away and I can send it by Federal Express. I think that you will find that there is much more material than you were aware of. Because of the fact that I am a member of the same church as Jones (Morman [sic]) and attended BYU, I think Jones felt that he could freely criticize my work. I would not have minded this if he had stuck to facts and avoided misrepresentations in his discussions.

Regarding my paper for Journal of Physical Chemistry, Stan Szpak feel that the referees would object to my Figure 8. Both Stan and I think that this figure is very intriguing and the temperature excursions could be caused by high energy gamma rays affecting the resistance of the thermistor. However, I think that I will probably leave this figure out to avoid controversy. My paper then will be based strictly on fundamental principles of physical chemistry. Please let me know what you think.

Best wishes,

Mel Miles

Mel

1999-03-16

Bury Lodge heading

Dr. Melvin Miles,
Chemistry and Materials Division,
Naval Air Warfare Center Weapons Division,
China Lake,
CA 93555-6100
U.S.A.

16 March 1999

Dear Mel,

Just recently, I have had one of my not infrequent bouts of coping with matters which I don't really want to deal with and which distract me from the task in hand to wit: the completion of the analyses of your data sets. However, I have now dealt with most of these "matters" and am returning to the main theme. As a first step to this return to the land of the living, I am answering long outstanding correspondence.

As part of this, I am checking through the letters/FAXes you have sent to me and I will itemise my reply - just in case I have missed some important parts.

1.A) Many thanks for your FAX of 8th March 1999 attaching a copy of your letter of 7th. March 1999 to "Public Forum". Could you please let me have copies of the Journal of Physical Chemistry, 98 (1994) 1949 - I would like to update my Nate Lewis file.

1.B) In 1992 we had to write an extensive commentary on the Official Actions of the Patent Examiner regarding the U. of U. Patent Applications. A part of this dealt with the Caltech work giving also the background as seen from our point of view. Our comments are rather scurrilous - would you like to have a copy?

At that time I tried to persuade all concerned that we had to establish the status of the "negative research reports". The Patent Attorneys did nothing to that event. When they finally came round to our point of view, it was really too late to do anything about this; too late both in the sense of time and also because of shortage of funds.

Our commentary made at that time on the Patent Issues is more than 100 pages long. Would you like to have the whole lot?

Incidentally, with regard to the part dealing with the work at Caltech, there is an interesting angle on the use of "short/fat" calorimeters versus the "long/thin" calorimeters we settled on.

2) Many thanks also for the package of 28th February 1999 dealing with the eventual publication of your reply to Steve Jones et al in the Journal of Physical Chemistry. The question of the validity of the assertions of Steve Jones crops up repeatedly and it strikes me that it might be desirable for me to write a short commentary based on your experiences to use as a hand-out Could you please comment on this suggestion? If I were to do this, then I would have to ask you for some additional material. One reason for writing such a commentary is my belief that interested readers will need a summary and guidance through all of this material.

I would actually like to start with your experiences with the D.O.E. panel so I would like to have a copy of the paper D.E. Stilwell, K.H. Pack, M.H. Miles, J. Fusion Energy, 9 (1990) 333. [78] From our various conversations, I recall that you subsequently wrote to members of the panel to inform them that you had subsequently observed excess energy generation but that they refused to make an amendment to the report. Did you write such a letter and, if so, could I have a copy? If you informed them in some other way, then it would do if you could outline the events in a letter to me which I could cite in my summary.

The next matter is that I would like to have copies of all the other papers cited in J.P.C. B 102 (1998) 3642, [79] the original draft of this paper and the back-to-back comment by Steve Jones et al (also any subsequent correspondence). I probably have much of this material but it is dispersed and your own files will be much more readily accessible than mine are. Furthermore, it would be useful to have copies of any of Steve Jones' e-mail correspondence or pastings on the Internet. I would like to have this as an illustration of the dangers of such publication media.

I know that this will be a great deal of work but I believe that it is well worth the effort involved.

I believe that it is important to consider the "position where Steve Jones came from" (although I will probably not include comments on this in my summary). In some of my recent comments on the events of March 1989 I have summarised our reasons for wishing to delay any publication of the results. I quote:

"I believe that it is important to summarise our reasons for wishing to delay the publication of the results. In the first place, although we had indications for the formation of ^{4}He, these results were not publishable; secondly, we believed that most scientists would judge the work on the basis of the Q.M. paradigm, applied to the collision of two deuterons in a dilute plasma and would therefore conclude that our results had to be false; thirdly, we did not believe that industry would conclude that research in this field (let alone any products based on this research) would be in their short or medium term

[78] JR Stilwell, D.E., K.H. Park, and M. Miles, *Electrochemical Calorimetric Studies on the Electrolysis of Water and Heavy Water (D2O).* J. Fusion Energy, 1990. **9**(3): p. 333
[79] JR Miles, M., *Reply to 'Examination of claims of Miles et al. in Pons-Fleischmann-type cold fusion experiments'.* J. Phys. Chem. B, 1998. **102**: p. 3642

interest although there might well be initial flashes of enthusiasm; fourthly, we believed that those concerned with National Security could hardly be expected to welcome such research in the University Sector; fifthly, we believed that we would lose our freedom of action because research on this topic would become constrained by targets and modes of operation ill-matched to achieving further progress, lastly, we really wished to return to the more general problem of searching for examples of the operation of the Q.E.D. paradigm (see Section 4)".

As far as Steve Jones' comments are concerned, this would appear to place him firmly in the category described here as "secondly" i.e. he is absolutely constrained by his knowledge of "Hot Fusion". (Incidentally, Hideo Ikegami has also always been so constrained). In consequence, he always falls back on arguments rooted in the Q.M. paradigm. That being so, it is really quite pointless to discuss experimental evidence with him which appears to run counter to this paradigm I believe that Steve Jones even lacks the flexibility to invent special arguments which might "save the paradigm" (in the manner of Kim, Vigier, etc), not that I believe in such exercises. As you know, I have throughout approached the topic from the Q.E.D. paradigm.

The question of Steve Jones' "belief system" seems to me to be quite firmly established by his comments which is one reason why I would like to extend the trawl to the e-mail and Internet. It may be appropriate to refer to this in the proposed commentary leading up to the question: "that being so, then why did he ever embark on the research topic in the first place?" In January/February 1989 (when we met him twice) I formed the impression that this was because he had caught sight of some Soviet work which I believe was related to what I have called "fourthly". At that time he told me that the Soviet Scientists had stopped talking to him which I associated with his intention to examine neutron generation from metal/deuterium systems subjected to intense compression. Incidentally, what happened to that original B. Y.U. programme? They had developed a system which was in the nature of a neutron spectrometer suitable for very low neutron fluxes and it would have been a simple matter to follow up the neutron generation saga (it is somewhat related to two-dimensional spectroscopy).

There is a great deal more about these aspects which we should discuss further but I will leave this for another day. I am sure you will realise that my comments are not for public consumption and that we would have to exercise great care with anything which we might decide to say.

3) Your FAX and letter of 2nd February 1999. The paper is very interesting and I agree with Stan Szpak's comment that there is no need to include Fig. 8 at this stage. Inclusion will simply lead to complications.

I realise that one should be able to find the electrode dimensions by reading NAWCWPNS TP 8302 but this will not be readily available to other research workers. Should you include this information in the paper?

I would predict that the referees will ask you to cut down on the number of figures. They will suggest that you give one (or possibly two) illustrative examples and then simply cite the total numbers of such "events" which were seen.

It seems to me that you were operating rather close to the threshold current density and certainly close to the time/temperature for the onset of "positive feedback", hence the fluctuations in excess enthalpy. Cell B never made it to the required temperature.

Should you perhaps have referred to the somewhat similar observations made by the group at Harwell as revealed by the Melich/Hansen and Fleischmann papers and point out that these effects were not discussed by the Harwell research group?

Of course, one would dearly like to calculate the rate of excess enthalpy generation absolutely. You will see the benefits which would be derived by using ICARUS 14 calorimeters in any future study.

With regard to the last paragraph of your covering letter: I would suggest that, when I have written up the analysis of the data set you collected in Japan, we should then decide how best to incorporate it into a report. I can predict that there will be some parts which we will not wish to include. However, many parts of these data sets illustrate some of the key features which I have been trying to point out over the last ten years. Perhaps we should base the report on such an angle? See also (5) and (8) below.

4) My FAX of 28th January 1999 and your replies of 29th January 1999 and 8th February 1999. Thanks for clearing up my questions and misconceptions regarding the Pd-B data sets which you collected during your stay in Japan and which I have been analysing (and continue to do so). The information on page 4 of your FAX of 8th February 1999 is very useful as will transpire from my report. I will comment further on the proposed report in (5) below.

I believe that your question about the position we had reached in June 1994 will have been substantially answered by pages 21 and 22 with Figs. 13, 14A and 14B of Appendix C of my letter of 22nd January 1999. Fig. 14B of the draft Appendix C is identical to Fig. 10 of the paper I gave at ICCF 7, page 119, I believe I have asked you before: given that we had reached this position in June 1994, then what do you make of the paper by T. Roulette, J. Roulette and S. Pons, Proceedings of ICCF 6, Vol. 1 (1966) 85? [80] Can you understand what they may or may not have done?

There are several important points which need to be made about the status of the work reached in June 1994. In the first place we had reached rates of excess enthalpy generation of ~ 60W (specific rates ~ 1.5 kW cm^{-3}) maintained for ~ 50 days. Secondly, this opened the way for the next 3-6 stages of the programme on scale up. The ICARUS

[80] JR Roulette, T., J. Roulette, and S. Pons. *Results of ICARUS 9 Experiments Run at IMRA Europe*. in *Sixth International Conference on Cold Fusion, Progress in New Hydrogen Energy*. 1996. Lake Toya, Hokkaido, Japan: New Energy and Industrial Technology Development Organization, Tokyo Institute of Technology, Tokyo, Japan http://lenr-canr.org/acrobat/RouletteTresultsofi.pdf

10-13 calorimeters were built for this programme but never put into use. Originally, the design limit was an output of 2 kW but this was then raised to 3 kW so as to allow the addition of steam compression (probably a Roots blower in the first step) so as to raise the quality of the steam. If this programme had gone according to plan, then we could have added another 3 stages of scale up to achieve our target of a 10 kW device by the year 2000. Thirdly, the work in 1994 had been carried out with Johnson Matthey Type A Material but our stocks were then exhausted. We were then kept busy with the ICARUS 2 Saga, ICCF 5 etc. but what is most significant, my relations with Johnson Matthey were loused up. Perhaps my colleagues thought that this would not matter - they had large stocks of Johnson Matthey Material Type B. This material was produced because we believed that it would be possible to develop a large collaborative programme involving IMRA Europe, IMRA Japan, Stanford SRI and, perhaps also, N.H.E. The important point about the Material Type B was that it was not expected to give high rates of excess enthalpy generation: the intention was to make systematic changes to the Type B Material to see whether it could be made to converge onto the behaviour of the Type A Material (hopefully, even to improve on this material) and thereby to define at least some of the key processing variables. This programme was hardly started - nobody had the requisite funds as I had repeatedly pointed out to all concerned. Fourthly, and instead of following a logical stepwise development using Type A Material in the ICARUS 10 - 14 calorimeters, all concerned embarked on a massive extension of the ICARUS 9 saga using unspecified materials (although I believe that the electrodes were largely fabricated from the stocks of Type B Material). Fifthly and lastly, all discussion of the situation we had reached in June 1994 was frustrated. I decided that I could not achieve anything further with my existing colleagues. I left the option of my continuation with the programme to my colleagues but I set a precondition on any such continuation. This was that there should be a systematic, comprehensive and comparative evaluation of selected data sets collected by N.H.E., at IMRA Europe and IMRA Materials Laboratory. These requests were *de facto* refused and I severed my connections.

I am sure that there is much more to this story than I have set out here. What should one say:

Grrrrrr!

5) Next your FAXes of 2nd and 3rd February 1999 and to deal with the contents of the proposed report to the Navy. I will be glad to follow any suggestions which you and Stan Szpak may care to make. It strikes me that, as there is potentially a very large amount of material to be covered, we should in the end spend some time collectively to produce a reasonably comprehensive Executive Summary (a stratagem which I have often used). This Executive Summary could simply introduce the component parts and we would then be free to drive the summary to reasonably forceful conclusions. At the same time, we could delineate additional material which it might be necessary to add at a later date?

Perhaps it is appropriate to illustrate this particular strategy? Suppose that my report on the analysis of the data sets for the Pd-B electrode (which you collected in Japan) were to

lead to a requirement that the methods of data analysis should be verified. We could then simply include an abbreviated version of the Report I sent you on 15th September 1998. I believe that such sectioning of the material will greatly simplify the task of producing the Report but, as the same time, we will need to pull the whole thing together. An Executive Summary is the easiest way of doing this.

Of course, I was very pleased (of course, of course and very very pleased) to hear that Frank Gordon will underwrite this venture. Have you and Stan made any sort of budget to see what costs you may incur and which should be charged to the budget? The immediately obvious costs which I can foresee here are those for preparing the final written parts, diagrams of the sections I will be responsible for, as well as costs of duplicating the required number of copies.

However, if there should be some spare funds, then I would recommend that we should also archive some of the key material - even if we do not evaluate all of this in detail. This exercise should aim to produce discs or C.D.'s as well as hard copy (hard copy at least of selected parts). There are many reasons for my wishing to include such hard copy. Magnetic storage is notoriously ephemeral and also subject to changes in technology. I can illustrate this with the example of what I believe to be some prime data sets which I may be able to access. These reside on tapes using technology which is now obsolete. It would be quite a costly exercise to transcribe and format these data onto discs. All of this would have been avoided if the data had been initially stored as hard copy - this can always be scanned into any new computer system.

I believe that we should discuss this question of archiving the available material. There are some further "hidden" aspects to my wish to do so. As you know, just about all the key players have taken steps to deny access to their prime data. I believe there are two principal reasons for this strategy. First of all, they wish to be free to comment as they see fit on the data while denying anybody else the means to challenge their interpretations/conclusions. Secondly, I am sure that all these data have been notarized and archived in case it should prove to be commercially desirable to have such data in reserve at some time in the future. If we were to prepare archived material, then it might just about be possible to winkle out some of the "hidden" data sets. At the very least we could establish who is (and who is not) willing to release then-data sets.

Here again I would like to illustrate these points from my personal experience. I believe that it is still not generally known that Stan Pons and I eventually agreed to join N.C.F.I. (in November 1989) on condition that any data sets which we would gather there would be independently evaluated (in addition to the evaluations which we would carry out ourselves). In the event, the group which was supposed to carry out these evaluations withdrew from the research. However, a limited exercise along those lines was eventually carried out by Wilford Hansen (see Proceedings of ICCF 2 (1991) 491). Mike Melich and I used the fact that such an independent evaluation had been carried out as a means for persuading Harwell to release their prime data sets. It was then apparent that the group at Harwell had not examined their raw data but that excess enthalpy generation was

immediately apparent once this was done (see M.E. Melich and W.H. Hansen, Proceedings of ICCF 3 (1993) 397; [81] M. Fleischmann, Proceedings of ICCF 5 (1995) 152). [82] The fact that the group at Harwell had got it wrong (also the groups at CALTECH and MIT) has had very little impact but it may just be that some extended documentation would have some effect?

Incidentally, I wrote three papers on the reanalysis of the Harwell data sets in the hope that they might get into the mainstream scientific literature but could not get these published. However, some of this material could be used as illustrations in the report. We also made measurements on a mock-up of the Harwell isothermal calorimeter but, following our experience with the first three papers, decided against writing this up. The measurements using this isothermal calorimeter are an object lesson in how not to carry out calorimetry.

A second incidentally: I do not wholly agree with Wilf Hansen's analyses of our data sets and my disagreement is somewhat relevant to the subject matter of the report.

A third incidentally: having looked again at the Proceedings of ICCF 2, I would like to draw your attention to the paper M. Fleischmann, Proceedings of ICCF 2 (1991) 475, especially to Fig. 1. [83] This is relevant to the paper you are currently writing (as are the Harwell data sets).

---

[81] JR Melich, M.E. and W.N. Hansen. *Back to the Future, The Fleischmann-Pons Effect in 1994*. in *Fourth International Conference on Cold Fusion*. 1993. Lahaina, Maui: Electric Power Research Institute 3412 Hillview Ave., Palo Alto, CA 94304 http://lenr-canr.org/acrobat/MelichMEbacktothef.pdf

[82] JR Fleischmann, M. *The Experimenters' Regress*. in *5th International Conference on Cold Fusion*. 1995. Monte-Carlo, Monaco: IMRA Europe, Sophia Antipolis Cedex, France http://lenr-canr.org/acrobat/PonsSproceeding.pdf#page=169

[83] JR Fleischmann, M. *The Present Status of Research in Cold Fusion*. in *Second Annual Conference on Cold Fusion, "The Science of Cold Fusion"*. 1991. Como, Italy: Societa Italiana di Fisica, Bologna, Italy. http://lenr-canr.org/acrobat/Fleischmanthepresent.pdf

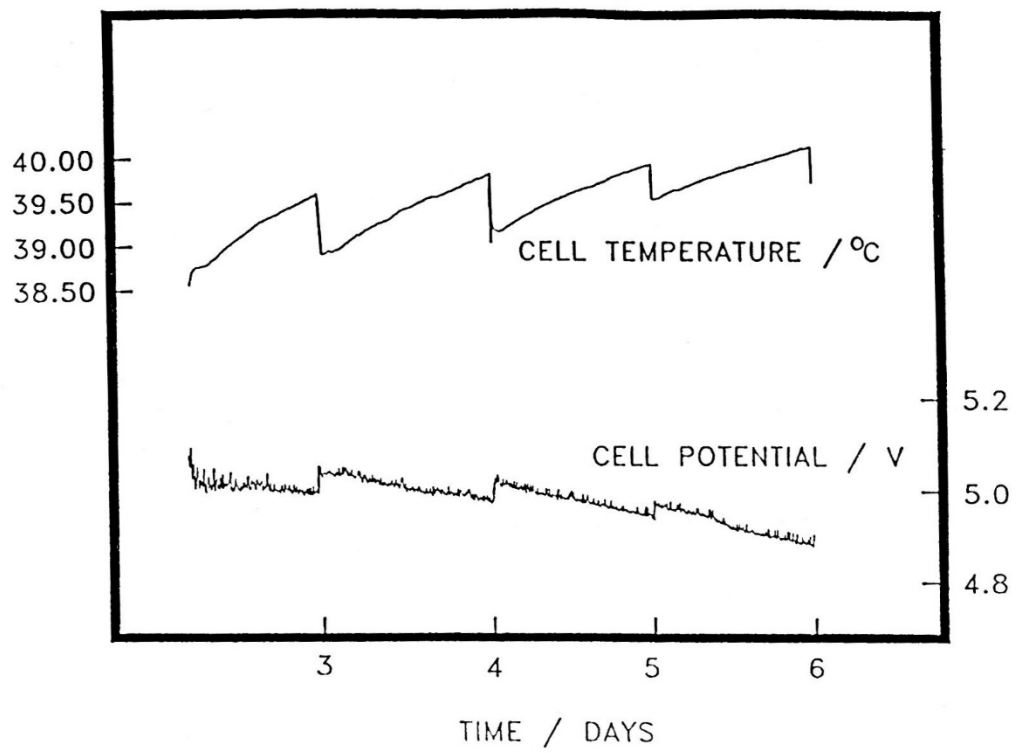

Figure 1 from ICCF2, *The Present Status of Research in Cold Fusion.* Cell temperature (upper) and cell potential (lower) vs. time since cell was started for the electrolysis of D_2O in 0.6M Li_2SO_4 solution at pH 10 at a palladium rod cathode (0.4 × 1.25 cm). The cell current was 400 mA, the water bath temperature was 30.00°C, and the room temperature was 21°C. The rate of excess enthalpy generation at the end of each day was 0.045 W (day 3), 0.066 W (day 4), 0.086 W (day 5), and 0.115 W (day 6). The accumulation of excess enthalpy for this period was on the order of 26 KJ.

A second illustration is the reanalysis of the data sets for the Pd-B system which you collected during your stay in Japan. This is the only example where we have hard numbers for a particular interpretation (the N.H.E. version), the original instruction sets (the Handbooks for the ICARUS 1 and 2 systems) and, finally my reanalysis of the data (which follows the instructions in the Handbooks and then makes extensions to the "boiling episode" and "Heat-after-Death"). The evaluations carried out by N.H.E. are completely incorrect and one can document these mistakes in detail! I believe that this example, in particular, illustrates the need to archive all the available data sets

6) One of the tasks which I have been struggling with recently is the draft of an article which Scott Chubb has been asking for some time now. It strikes me that parts of this article might be suitable for the Report under the heading "Motivations for the Research Project". I will ask Scott whether I might send embargoed copies of this article to you and Stan to see whether some of the aspects covered should be included in the Report.

7) Many thanks also for your FAX of 6th January 1999 which I believe I received as a letter with three pages of enclosures. I was glad to see from the first of these that you are keeping fit! The second and third pages appear to be extracts from a book of Abstracts for

the 12th IUPAC Conference on Chemical Thermodynamics (pages 193 and 194). The Abstract of your talk - fine, but how did you cover all that material in just 20 minutes?

I was rather intrigued by Steve Jones' Abstract. Have you by any chance a copy of the article by Van Siden, CD., and Jones, S.R., J. Physics G: Nuclear Physics 12 (1986) 213. Poor old Steve: does he believe in Cold Fusion or doesn't he? If he believes in his neutron counting results, then the Q.M. paradigm is clearly inadequate. In that case, he should stop trotting out all his familiar objections. He can't have it both ways.

The matter of the neutron counting results in Japan is rather interesting. You may recall that he and his colleagues started off by trying to carry out what was in effect neutron spectroscopy at very low neutron fluxes. This was all very laudable - but why then did he not follow up his original intention of examining the effects of extreme compression? Instead, he and Kevin Woolf tried to measure the positional dependence of neutron generation using the Kamiokande position sensitive detector in Japan. I have been told that these experiments were very bad but there was no doubt that the neutrons came from the experimental volume! What happened to these results? Eventually, I secured a Masters Thesis from Tokyo [84] which contained the results but this thesis was removed from the material sent back to me here from France. Well, well, well!

Instead of following up these two strong leads, Steve returned to measuring the total flux in the deep mine in Utah. Of course, this is a good way of burying any signal in the noise at the low energy end of the spectrum. Well, well, well!

It is of some interest that the three most competent research groups who developed neutron spectroscopy at low neutron fluxes (both proton recoil and time-of-flight spectroscopy) have given up their research after obtaining their first results. The most competent of these competent groups never even published their first data.

All I can say to all of this is to ask you a question: what is one to make of all this?

Having started on a Steve Jones saga, I feel impelled to add a further comment. When the group at B.Y.U. started their first sets of measurements, they used 10% D_2O in 90% H_2O to make up their electrolytes. It seems that they did not know about isotopic separation. The Pd cathodes would only have contained ~1% D^+ in 99% H^+ and, of course, there was absolutely no chance of observing anything whatsoever.

When Stan and I received the first round of the referees' comments from the D.o.E. in the autumn of 1988 (no less than 5 referees!) I recall saying to Stan at his kitchen table; "hey, Stan, this referee is Steve Jones from B.Y.U. He is asking some pretty pointed questions and, if we answer Question X, then we will tell him why we believe that there is some novel fusion process in the Ni core of Jupiter (maybe in the earth as well); if we answer Question Y, then we will tell him how to set it up in the lab." We then discussed the

[84] JR Ishida, T., *Study of the anomalous nuclear effects in solid deuterium systems*. 1992, Tokyo University.

matter and decided that as the "cat was out of the bag" anyway, we might as well answer the questions.

The key step, of course, was to use D_2O with a very low H_2O content but I believe that Steve Jones has never grasped the real reason for using electrolytes based on this composition. However, they restarted their experiments leading up to their questionable neutron spectrum.

I would add here that I have never objected to Steve Jones acting as a referee but I believe that he should have disclosed his interest at the outset. The fact that he did not do so contributed in no small measure to the build-up of distrust. Nor, for that matter, did I ever object to the group at B.Y.U. restarting their experiments once they realised that the electrolyte had to be modified. However, what I did (and still do) object to is their behaviour in January/February 1989.

8) Fred Jaeger sent me a notice of the next symposium to be held in Asti this autumn. Your and my names are not on Bill Collis' circulation list nor is Vittorio Violante, Giuliano Preparata or Emilio Del Giudice nor any of the Japanese. What is one to make of this? Should I write to Fausto Lanfranco? It would be a good opportunity since he was responsible for setting these events in train.

Should we leave these people to their fate or should we prepare at least one paper for them (and then present it at ICCF 8)? We could use the material already to hand including the Poster I gave at ICCF 7.

That's about all which comes to mind at present - have I left anything out?

Best regards,

Martin

P.S. I have just written to Stan Szpak enclosing Section 5 of this letter. Could you please let me have his full addresses and 'phone numbers - that is if he doesn't mind revealing this information.

1999-03-29

NAWC heading

FAX MEMO

DATE: March 29, 1999

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

Thanks for your fax of 16 March 1999. I apologize for the delay, but my wife has been ill and she does the typing. The tenth anniversary of cold fusion has now come and gone. I gave some material to the local newspaper but they evidently had no interest in a story. I was disturbed by a report from Jed Rothwell concerning the American Physical Society meeting. There was an anti-cold fusion meeting and some government official by the name of Zimmerman vowed to stamp out cold fusion everywhere in the government. He wanted it reported if there were three or more people discussing the subject. I've never seen such weird behavior. I will send you a copy of Jed Rothwell's report.

I talked to Stan Szpak and he said that you had contacted him. His home address is as follows:
Szpak, Stan 3498 Conrad Ave. San Diego CA 92117

I have his telephone numbers at work and will write them on the cover sheet tomorrow. I think the main point is that the funding for the government report will expire at the end of the fiscal year which is Sept. 30, 1999. We need to complete the report before then.

I have been spending many hours on data analysis related to my work in Japan. I agree that the process of data evaluation is frequently curtailed as you stated in your paper "The Experimenters' Regress". This has led to extensive revision of the paper I am working on, and I will send you a copy soon. I have followed your suggestions and have referred to similar observations of temperature increases in the Harwell data by Melich/Hansen and yourself.

I will follow your notation in response to the other points.

1 A: My letter to the public forum was recently published. This is a large newspaper serving LA and surrounding areas and is second only to the LA Times in southern California. I will send you a copy of the published letter along with a copy of Journal of Physical Chemistry, 98 (1994) 1949 that you requested.

1B: I would like to have a copy of all the material that you mentioned in this section concerning the Cal Tech work and the patent issues. I would like to hear more about your thinking on the use of the "short / fat" calorimeter versus the "long / thin" calorimeter. The calorimetric constant

would certainly change much more slowly for the short/fat calorimeter but the stirring would be a problem.

2: I would be glad to help you with any commentary concerning the assertions of Steve Jones. I agree that this would be useful. I will send you a copy of the paper "Fusion Energy, 9, (1990) 333. I did write a letter to all members of the D.O.E. panel stating that I have observed excess heat, but this was a year or so after the D.O.E. report had been published. I never received any response from anybody on the panel. I will try to find this letter and send you a copy. I will also send you the original draft of the J. Phys. paper as well as Steve Jones e-mail correspondence. It may take me some time to get all this together. I enjoyed your other comments regarding Steve Jones and his belief system. I hate to say this about a fellow member of the Morman [sic] church, but I found him to be fundamentally dishonest.

3: Thank you for your comments on my paper. I will leave out Figure 8, but I still think that the thermistor closest to the palladium cathode responded to some type of radiation. I have placed my thermistors at China Lake near a radiation source and have seen some strange high temperatures that resulted. I have cut down on the number of figures as you suggested and referenced your comments related to the Harwell data. I am concerned, however, by the extremely strong feelings against cold fusion that were present at the APS meeting as reported by Jed Rothwell. Maybe my new paper will never stand a chance if scientists have that type of attitude.

4: Thanks for the information in this section. I was similarly frustrated by my collaboration with the Naval Research Laboratory. They modified my calorimeter without consulting me and wound up with an error of +/- 200 mW. They could have never detected the level of excess heat that I had measured. I wrote a letter stating this scientific fact, but that only got me in trouble with NRL people and Bob Nowak and Fred Saalfeld of ONR. In fact, I have not received any funding from ONR since. This has made my life at China Lake quite difficult. If I don't find funding soon I may retire sometime next year.

5: Your comments on the proposed Navy report sounds reasonable to me. I think that it would be good to archive any available material that we don't publish in a report. Your three papers on the Harwell data set should be included in this. It is too bad that none of this was published in scientific literature.

6: I would like to receive a draft of the article that you have been working on for Scott Chubb. Some of this could be included in the report in my opinion.

7: Regarding the 12th IUPAC conference, there were a lot of questions and interruption, including some from Steve Jones, hence I never got a chance to cover much of the material. I don't have a copy of the article mentioned but I will try to find a copy. I still need to ask Dr. Takahashi about the master's thesis concerning the Kamiokande experiments. I am not surprised by your comments about Steve Jones. I think he is an opportunist who wants the spotlight whenever he can grab it. He is not content to just work in a laboratory but craves publicity, attention, and feeling important. I would never trust him, especially after what he did at my BYU seminar.

8: I am especially surprised that you were not on the circulation list for the Asti meeting in Italy. I don't understand what they are trying to do by leaving out people like yourself, Violante, Preparata, and Emilio. I think that you should write to them. Dr. Paolo Tripodi who is working with Mike McKubre has told me about the meeting and encouraged me to present the paper that I am writing. He was going to have information about this meeting sent to me, but I have received nothing so far. I would like to attend if possible, but I would have to pay my own way unless there is some financial help.

My present problem is that I have a full time job besides cold fusion. Trying to find funding for next year has been taking much of my time. My weekends and evenings have been used for several months in writing the cold fusion paper based on Japan work. I can easily see where four or five interesting papers could result from my short five months stay in Japan, but when would I have time to write all these papers? I appreciate your help in the analysis of the palladium-boron study in Japan. I hope this will result in a paper for the Asti meeting as well as one for ICCF-8. Furthermore, Stan Szpak is anxious to have the complete data analysis of my three co-deposition studies in Japan that produced excess heat. Almost everything I did in Japan produced positive excess heat results, yet Dr. Asami and Mr. Sumi never seemed to have any interest in these results. I think they would have preferred negative results to justify closing the laboratory. I hope someday the truth regarding all these episodes of cold fusion are realized as being scientifically correct and accepted by most other scientists. However, I have no idea how long this is going to take – probably years. In the meantime we have to keep trying.

Thanks again for your informative fax.

Best wishes,

Mel Miles

Mel Miles

Jed Rothwell
vortex-l @ eskimo.com
Tuesday, March 23, 1999 7:04 PM
vortex-L@eskimo.com
News from the APS meeting

Hello all from the APS meeting in Atlanta, the cheapest conference I have ever attended. I got there for subway fare, got in with a free with journalist's pass, and the APS gave me free fruit and coffee. Not bad!

On Monday there were three good papers on CF but I had to miss them to attend the anti-cold-fusion lynch mob session instead. It featuring Bob Park, Huizenga, Morrison, the Amazing Randi, and a top Federal science honcho named Zimmerman from the State Department Arms Control Agency. It was a trip! Zimmerman is conducting a jihad against cold fusion, vowing to wipe it out in the State Department, the Patent Office (?) and the Commerce Department with the help of the President's Science Advisor. (The Patent Office?!? And they call *us* kooks!) It was a depressing and alarming, yet droll. I will describe it in detail when I get a chance. I must tell about the weirdest part here, because if I do not write it down I might think I dreamed it.

Zimmerman gave a rousing anti-cold-fusion talk, pressing all the buttons. He said that one of his first official acts was to cancel a meeting about cold fusion, and "that's one of the accomplishments I'm proudest of within the last year." He announced that he and Park will work to exterminate every trace of CF and all other "junk science" from the Federal establishment. They will see to it that no other meetings are held anywhere else in Washington, which is a hotbed of cold fusion as we all know. He called upon the audience to join him in this crusade, and to report to the highest authorities any rumors about unauthorized research and groups of more than three people caught discussing cold fusion. Naturally, this was met with met with cheers and applause from overflow crowd there, which was a sort of sci.physics.fusion come to life. I thought people like that only existed on Internet.

Anyway, here is the weird part. After the session I approached Zimmerman and asked him for the exact spelling of his name and his job title. He grabbed some papers and held them to his chest, covering his badge, and he said I'm not going to tell you. He looked like John Cleese in "A Fish Called Wanda" where he is caught naked in a stranger's apartment. He said "I am not officially here." I asked if he planned to publish his remarks in the proceedings and he said maybe in a noncommittal tone (meaning: No Way). Apparently he wants his statements off the record, yet he had made them in front of a cheering crowd and he saw me wearing a press badge, sitting in the front row, taking photographs and recording the lecture on audio tape! Does he think I plan to keep it secret? Does he think I am so dumb I will not look up his name in the conference registry or call State? Let me recap: we have a top science advisor at the State Department - an appointed official of the U.S. Government, making maybe $100 K in the taxpayer's money. His proudest accomplishment so far has been to disrupt a meeting, which tells me he needs to get a life, or get a job. He boasted to 400 people that his policy is to ban discussions about cold fusion, and he wants to instigate purges in other departments, BUT he

will not put this policy in writing, and he will not give a journalist his name. If that is not Alice-ln-Wonderland material I do not know what is. The story is too improbable to sell as fiction.

To me, this proves once and for all that there is no conspiracy against cold fusion. Two reasons:

1. A guy who tries to hide his name tag could not conspire his way out of a paper bag. His "cover up" is literally a piece of paper held to his chest, to cover up. Not James Bond.

2. When you announce it is your policy to oppress a field of science to cheering crowd of 400 people, and you ask them to join you, that is not a conspiracy, folks. Conspiracies are covert, this was overt (except sort of ex-post facto off the record). This was no conspiracy, it was a Movement, a Revival, or a Crusade. It was like nothing I have ever seen before, or ever hope to see again.

And no, I did not snap a photo of the guy holding the paper over his name tag, but by golly I hereby claim the movie rights.

- Jed

1999-04-28

NAWC heading

FAX MEMO

DATE: April 28, 1999

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I have been somewhat depressed and have not made much progress recently on the requests in your last letter. The main problem is that I am almost out of funding for my battery research at China Lake. I have not been able to get any funding from ONR since my cold fusion program was abruptly terminated in 1995 following Steve Jones' critical review of my work. It is difficult to survive here without ONR support. Richard Carlin of ONR seemed friendly and willing to fund me when he took over for Bob Nowak in 1997. His attitude totally changed after I told him that I was going to Japan to work on cold fusion. Last week he formally rejected my most recent battery proposal. I will soon try to write another proposal, but I think I will retire if that does not get funded. I would like to continue work on cold fusion somewhere after I retire.

It is extremely frustrating to see that your excellent papers continue to be ignored after 10 years and that cold fusion is labeled as junk science by many critics. It is also frustrating that the Navy totally ignores my results and that ONR refuses to fund me on other projects. My report and letter pointing out that the NRL work was flawed by a calorimetric error of ±200mW only made enemies and damaged my career. It is ironic that both NHE in Japan and NRL messed up the calorimetry transferred to them to the point that any excess heat could not be detected.

I spoke to Stan Szpak last week by telephone. He wanted me to remind you that the funding for the Navy report will expire at the end of September.

I hope to soon begin my analysis of the co-deposition experiments that I conducted in Japan using the F/P cells and calorimetry. There seemed to be large excess heat effects in all three cells. In fact, the effect was large enough that even the flawed analysis used by NHE showed excess power. You should have this data from Mike Melich, but I can print it out and mail it to you if necessary. Stan Szpak would like to include this analysis in the Navy report.

After I run out of funding for my battery projects next month, I will likely be detailed to work on odd projects elsewhere. I will then try to survive until the end of this year and then retire from the Navy and hopefully find work elsewhere.

Sincerely,

Mel Miles

Mel Miles

1994-04-29

29 April 1999

Martin,

This email today confirms what I thought happened with Richard Carlin of ONR when I went to work on cold fusion in Japan. This is made it virtually impossible to get research funding from Richard Carlin of ONR.

Richard Carlin was calling me for discussion on molten salts and seemed very willing to fund me until I informed him about my going to Japan to work on cold fusion. He has been very cold to me ever since.

My only chance now is to survive until this year ends and then retire.

I hope someday that we will both be indicated for our efforts on cold fusion!

Best Wishes,

Mel Miles

(e-mail)

From: Harris, Dan C
Sent: Thursday, April 29, 1999 10:40 AM
To: Miles, Mel; Lindsay, Geoff
Cc: Nissan, Robin A
Subject: FW: Rich Carlin

Mel and Geoff,

I stopped off at ONR yesterday and talked with Larry Kabacoff (my 6.2 sponsor) and with Kelvin. I mentioned the battery proposal to Larry and he said that Carlin was the only one in the building who could be the sponsor. He thought that if Carlin was not interested, then ONR was not going to fund it. I discussed the proposal with Kelvin and he concurred that Carlin was the guy. Knowing that Carlin has an existing bias, I left a copy of the January 1999 conference publication -- not the proposal -- for Kelvin to take to Carlin to see if there was anything to interest Carlin. The results are stated below in Kelvin's note.

I conclude two things. (1) It is counterproductive to keep beating on Carlin. (2) The lead funding agency for thermal batteries is not ONR. Kelvin thought it was the Army when we talked yesterday. If it is the Army, then they are the agency that needs to be lobbied. We are actively hurting ourselves with a proposal in this area at ONR.

Geoff, I know you are very persistent in your marketing, which is a great strength of yours. However, in the case of Carlin, the persistence is not appreciated once he has made up his mind that he is not interested in a particular topic or proposal.

- - - - - - -

From: Kevin Higa
Sent: Wednesday, April 28, 1999 16:54
To: Dan C Harris
Subject: Rich Carlin

Hi Dan,

Thanks for stopping by.

I took the paper to Rich and asked him if he wanted the paper by Mel. His first response was "No!" I told him it was just informational and he reiterated all the same issues. Navy does not have the lead or the funds. He said the paper looked like it contained the same data he had seen before. His main scientific comment was that the cathode and electrode materials were a serious problem for nitrate systems. The cathodes and electrodes Mel mentioned in the paper are standard types of electrodes. Just before I left, he told me that bring him informational materials was how things should be done, not proposals. Rich reacted just the way I thought he would. **He actually thanked me for not giving him a proposal from Mel.** Rich brought up the issue of

cold fusion again. Apparently, when Mel took off for Japan, he was the electrochemist on Geoff s program with Carlin. Rich said Mel really damaged his credibility - Rich blames Mel for the poor performance on that grant. Mel would have to do some outstanding electrochemistry to convince Carlin to fund him. I sincerely believe Carlin is a lost cause as far as Mel is concerned.

The way to get into Carlin's program is to bring him ideas and data. If Carlin is convinced that you can help his program, he will discuss ideas with you and ask you to send him a white paper or proposal. He hates unsolicited proposals, especially if they are resubmitted time and time again. Geoff does this even after Carlin tells him that he is not interested.

Kelvin

1999-05-28

NAWC heading

28 May 1999

Martin,

I have not heard from you for a while and hope everything is O.K.

Enclosed is my manuscript as submitted to J. Phy. Chem. Thanks for your help and suggestions. I hope the Editor will allowed to be reviewed. I have worked on this manuscript on weekends and evenings since last December. I now hope to start on results using the F/P Calorimetry in Japan. I would first like to analyze the data from the Szpak-type co-deposition experiments in Japan that appeared to produce up to 400 mW of excess power.

I am leaving this weekend for Utah to visit my elderly parents (90+) and then to New Mexico (Albuquerque) where I was invited to present my cold fusion results to the Society for Scientific Exploration next week. John Bockris helped organize this meeting. Ed storms and Mike McKubre will also be there.

Best Wishes,

Mel Miles

1999-06-21

NAWC heading

FAX MEMO

DATE: June 21, 1999

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I haven't heard from you for some time and hope this is simply because you are quite busy. I am aware that you have had some health problems and hope that there are no problems in this regard. I spoke by telephone with Stan Szpak last week. He is still hoping that we can complete a report for the Navy by the end of September while the funding is still available. He suggested that perhaps you can write your own report in any format you desire. We both feel that it is important that you are able to publish your ideas relating to cold fusion where this can be accessed by others and referenced. I would like to see a detailed discussion of the NHE errors in their treatment of the F/P calorimetry. I would also like to see your treatment of my F/P calorimetric data that I obtained in Japan. I preferred, from the beginning, complete freedom for you to write this report in any way that you desired. Stan Szpak wanted to incorporate some of his work, but I now think that he would just like to have you write your own report. This would be a good way for you to document your detailed knowledge of the F/P calorimetry.

I don't see much future for my continuing to work for the Navy. Therefore, I plan to retire at the end of this year. Richard Carlin of ONR refuses to fund me because of my previous cold fusion work. I am considering the possibility of working with George Miley at the University of Illinois as well as working with John Dash at Portland State University. This would allow me to get back into cold fusion research. I think this would be the best way for me to finish out my scientific career. I would also be interested in working in Italy if there is a possibility of collaborating with you. However, it seems like that work is slow in getting underway.

Several months ago I visited Blacklight Power and Dr. Randall Mills. He has an impressive new laboratory and apparently considerable funding. He seems very brilliant in conversations. Perhaps his fractional quantum numbers theory has some validity. He showed us many new compounds involving hydrinos and novel chemistry. This could be a bridge between heavy water and light water cold fusion results. On the other hand, it could also be completely wrong. What do you make of all this?

Please write and let me at least know how you are doing. Also, let me know if you can complete your report so that we can have it published by the Navy.

Best wishes,

Mel Miles

Mel Miles

1999-06-22

Bury Lodge heading

Dr. Melvin Miles,
Chemistry and Materials Division,
Naval Air Warfare Center Weapons Division,
China Lake,
CA 93555-6100
U.S.A.

22nd June 1999

Dear Mel,

It is most remiss of me that I have not written to you for such a long time! First of all then, many thanks for the copy of your paper to J.P.C. Fine, and I will write to you further about this in due course.

Secondly, thanks also for your FAX of 21st June 1999. Yes, I am still reasonably fit and chunter along in my usual way. Temperamentally, I am still fighting fit.

The matter of my contribution to your report is again on course. I have completed all the critically important evaluations although some minor matters will have to be cleared up as I proceed with the writing. At the present time I am dealing with just that and hope to have this finished by the end of June. This will just leave the production of a large set of diagrams and spreadsheets which I had anticipated would be finished by the end of July. However, I have to go to Italy next month so mid-August would seem to be a more realistic timing for completing the whole venture.

I had actually carried out most of the calculations by X-mas but I had got rather "boxed in". I then had to deal with numerous other urgent matters but this was no bad thing because when I managed to get back to the task, I found that I could resolve some of the difficulties.

I think that this time scale will allow us to decide how we might best complete the report i.e. how, many sections we should have and what material we should include – i.e. we could go through one rewrite. I certainly appreciate your offer to give me a free hand but, at the same time, I think that it would be sensible if the Report was in the standard format - as far as this is possible. To get this under way, could you please let me know the page size to be used, the margins, the line spacing, rules about headings, spread sheets, diagrams, references and anything else which comes to mind. Should I try to get hold of an American English spellcheck - there is no point in irritating the readers with English English.

As far as I am concerned, there will be one glaring omission. As I told you, Asami gave me a C.D. in Vancouver which contains one set of data for a Pt-blank experiment, I think that it would be very good to aim for an addendum using an N.H.E. blank just to show that everything

is in fact A.O.K. with the system. The problem here is that these data have to be translated to diskettes and one has to make hard copy of some of it. This would have to be paid for so do we have any spare dollars and, if so, how much?

How, as regards my visit to Italy which does bear somewhat on your second paragraph. The whole business there has made very slow progress because E.N.E.A has been reorganised. Carlo Rubbia has been appointed to be Director but, much to my surprise, he is proving to be very sympathetic. Meanwhile, the people on the ground have got some very interesting results. However, we have one severe problem namely that Giuliano has to have a major operation. I believe though that all will be well.

When I go to Italy, I will raise again the question of your participation - that is if you would like me to do so. Of the two other possibilities you have mentioned, I can see that Portland would be attractive geographically but, of course, Illinois would be better scientifically. Should you perhaps consider dividing your loyalties including Italy in your options? Your news about ONR funding fills me with incredulity.

I really don't know what to make of Randall Mills. The whole thing seems quite crazy but then, who am I to make such statements? As I always explain to people, I am a very conventional scientist but then my conventional science includes Q.F.T. and Q.E.D.

Incidentally, ICCF 8 has been put back to next year. I gather that the meeting will be on the Italian Riviera. Should we work up a joint paper on the results you obtained in Japan backed up with a miscellany of NHE results?

More anon and all the best!

Martin

P.S. May I leave you to tell Stan Szpak about the schedule for ay section of the Report?

P.P.S. I forgot to tell you that your results show all the major features of excess heat (although at a moderate level), "positive feedback" and "Heat after Death" including one new result. [85] *They also demonstrate the strange features of the NHE analyses. What more could we ask for?*

[85] MM Early appearance of excess heat (Day 2).

1999-09-17

Bury Lodge heading

Dr. Melvin Miles,
Chemistry and Materials Division,
China Lake,
CA 93555-6100
U.S.A.

17 September 1999

Dear Mel,

As you will see, I am at long last sending you my analysis of the experiment Mc21 which you carried out during your stay in Japan. I had hoped to complete this by the end of June but I then hit a series of snags!

Although the Report is very long it is by no means complete: I have a great deal of further material which I have excluded. However, I believe that I have dealt with the main issues and I have also written it in such a way that criticisms/requests for further information can be dealt with as addenda to the Report. Unfortunately, because of the time restrictions, this Report is very much in the nature of a "first draft" - if I had had the time to rewrite it, it would surely have been greatly shortened and it would then have had more "bite".

You will see that the report has been laid out so that it could really be reduced from A.4 to Quarto format (do you still use Quarto and Foolscap sizes in the U.S.?). It isn't really necessary to carry out any reduction – it could be Xeroxed directly on Quarto sized paper. The only difficulty could arise with the Figures and Spreadsheets. All the figures and two of the spreadsheets were originally printed in A.3 form and I have then reduced them to the A.4 size. You will have to decide whether they can stand a further reduction to Quarto size. If this is unacceptable, we will try to produce Figures and Spreadsheets in Quarto size.

One of the most important decision which we must reach is what to do about Section E. Footnotes E.11 and E.12 give me much cause for concern and have been in and out of the text several times and in several forms. Footnote E.11 sets the scene for E.12 which is the real problem. Those skilled in the art and who know about the themes of our research (especially the unpublished parts) will be able to guess from E.12 what I really had in mind. However, I believe that some of the key players had guessed this by 26th March 1989 and, indeed, this may be the cause of many of our difficulties. The problem is that I have been somewhat "economical with the truth" and the question now is whether one should not after all reveal something about the original intentions? In brief, I wanted to see whether any detectable C.F. would reveal the correctness of my thinking about a number of other projects whereas I believed that the application would be in the Defence field (by combining C.F. with methodology derived from a preceding investigation). I was also greatly bothered by certain trends which I could discern in

Soviet Research. I would have been quite happy for the topic to be classified and to use our observations as the basis of further work on a range of other topics.

However, the "cookie crumbled" in quite a different way and it appeared that the whole topic might turn out to be very useful in the Civilian Section (perhaps even essential?) and we then had all the trouble with Steve Jones/D.o.E. We must discuss these aspects very carefully when we meet.

There is also the question of whether we should use this Report (and other material I have filed away) for a publication? Bill Collis has at last written to me about the Meeting in Asti. Will you be going to the Meeting? Should we offer them a talk on

"The Analysis of Experiment Mc 21: a Case Study"

and/or should we offer a similar talk for ICCF 8? If you were to give such a paper you would no doubt wish to interweave it with some of your other results; if I were to give it, I would present it in the context of other Calorimetric measurements.

Sheila and I (and her brother) are going to San Francisco on the 23rd for a late holiday ahead of the Meeting in Ontario. Sheila and her brother are returning here on 5th October and I will come on to Ontario – I may go to Washington after the Meeting. As I will have to write to Bill Collis while en route, I will telephone you when we are in California to see whether we should offer a paper to the Meeting. My schedule in October is somewhat complicated by a law suit and the need to go to Frascati. Guiliano is very fit and combative – he still hasn't had his operation but is off to Aszerbaijan! What a man! He has just done a brilliant piece of work on Black Holes and is working his way towards the unified Field Theory. Carlo Rubbia has been appointed Head of ENEA – INFL and is closing down Hot Fusion. The air is filled with vibrations. I must get details from you about all the bad vibes you have had – we have a great deal to discuss.

Regards,

Martin

See P.S.'s on page 3

The most obvious shortcoming of the report is the lack of a more detailed analysis of the behaviour on Day 61 to complement the analysis of Day 3 given in Fig. A 16. I checked up on the performance using the ICARUS-1 Methodology for the calculation of $(k_R')21$, $(k_R')22$, $(k_R')31$ *and* $(k_R')32$ *and everything was in apple pie order. However, the analysis given in Fig. A.16 covers the whole measurement cycle,* $0<t<T$ *rather than* $t_1 < t < t_2$ *and it seems to me that I should make such extended analyses for all the data sets – at least for that on Day 61. We could then draw attention yet again to the fact that N.H.E. Have not provided any blanks so that we have had to use an internal calibration.*

P.P.S. We should look at your Pd-Ce data in Ontario. There is a lot riding on the notion of introducing 4f-states [86]

P.P.P.S. I have enough understanding of the water capillary saga. Again, can we discuss this in Ontario

P.P.P.P.S. Finally, I have a list of expenses which I [incurred]in producing the report. Typing £370.00, Xeroxing £59:70 and preparation of spreadsheets and diagrams £234:00. With regard to the last item, I had to off-load some of the work. The sum total is £672:70. Will the budget stretch to that? In case it should be possible to make such charges, I am attaching all the relevant bills.

[Missing text]

(p. 98 of draft)

. . . determined hitherto (see Footnote E.9). In view of the difficulties which were evidently being experienced in the data evaluation, I have at various times urged that a group should have been set up charged with the task of evaluating the various data sets. The task of the parent groups would then have been reduced to the validation of these evaluations. I have never received any reply to these suggestions. (see Footnote E. 10)

Short of this suggestion, I have urged that the raw data for various selected experiments should be released for further study. However with the exception of experiment Mc 21 (considered in this Report) and the data sets provided by Dr. Asami at ICCF 7, I have again had no response to these proposals. I note that the data provided for me at ICCF 7 did not include any of the data sets which I had previously selected. This selection was based on my wish to restrict data evaluations to experiments carried out prior to June 1994 (with the important exception of experiment Mc 21).

E.9 It would evidently have been sensible to restrict such evaluations to just a few data sets because further work should have been carried out using ICARUS-14 Calorimeters (originally classified as the ICARUS-4 Systems)

E.10 However, I note that neither the original authors nor Harwell commented on the reassessments [(20),(21)] of parts of the original investigation by the Group at Harwell[(22)].

E.11 Furthermore, I had believed for some time that lines of research based on this work could point the way towards aspects of importance to National Security and should therefore be excluded from work in the Public Domain (see also Footnote

[86] MCHM We also worked with some of Martin's Pd-Ce – presumably of the same lot. It was actually Pd-Ce-Sm … I am not sure if Martin knew that.

E. 12). I would emphasise here that the topic on its own does not point to matters of interest to National Security

E. 12 One important consideration for us was that we had previously investigated a prerequisite for the development of certain weapons technology. The fact that we had brought this work to a successful conclusion was well-known to those who had access to this information.

Coincidentally, this work made me aware of the fact that there is an important group of phenomena whose explanation must lie in the field of Quantum Electrodynamics.

It was clear (to me at least) that the combination of aspects of this weapons technology with systems showing evidence of the presence of Cold Fusion was a matter which required careful and detailed examination. This became all the more necessary when it became clear that the systems were subject to "positive feedback". Unfortunately, we could not restrict the dissemination of information about this particular aspect. It also became clear during 1988-89 that we would not be able to prevent publication of the work in progress. It is relevant that our "fall-back position" had been that the consideration of the question of publication should be deferred until September 1990 but we could not even achieve that particular limited objective.

1999-09-20

Bury Lodge heading

Dr. Melvin Miles,
Chemistry and Materials Division,
Naval Air Warfare Center Weapons Division,
China Lake,
CA 93555-6100
U.S.A.

20 September 1999

Dear Mel,

My report should now be winging its way across the Atlantic. When I spoke to you, you said that you would not be going to the 4th Asti Workshop and I believe that I told you that my own plans were conditional on whether or not I could combine that meeting with discussions in Frascati (already much delayed). Following our conversations I checked my very incomplete "Asti File" (I must tell you about this incompleteness when we meet - more dirt I'm afraid) and I see that Bill Collis wrote to me just recently (see FAX attached). The important point to note is that the Italian Physical Society published the Proceedings of the 3rd Workshop so they might well publish those of the 4th Workshop? If so, then this could be quite a significant step in the saga and would give an early opportunity to discuss your experiment Mc.21.

I have written a "holding letter" to Bill Collis to say that we might offer a paper:

"The Analysis of Experiment Mc, 21 : A case Study"

but this is conditional on your approval. I will 'phone you when we get to California to see whether we should commit ourselves in this way.

Regards,

Martin

1999-10-28

NAWC heading

FAX MEMO

DATE: October 28, 1999

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I just returned yesterday from the Asti Conference in Italy. I was hoping to see you there, but I hope you can still write the joint paper on the Case Study for the Pd-B experiment in Japan. My other paper will briefly touch on this and then reference your analysis.

There is a matter of urgency that I need to discuss with you. This relates to my trip to Paris last month and Navy interests in a possible device supposedly developed by the Russians. I mentioned this to you at the Ontario, California [ACS] meeting two weeks ago. There will be a high-level meeting next week to discuss this matter, hence I hope you can reply right away. I cannot visualize how the device would work based on the information from the French contacts. However, I can see that the loading of deuterium into palladium followed by the application of a very high current may trigger such a device. Can you provide me details of how you think this may work? I suppose it would be safe to send this by fax, or perhaps you could telephone me at a set time. I would like to have this information for the meeting next week. I guess there is considerable concern about such a device getting into the wrong hands – if it could actually work. This, of course, relates to your footnotes that you sent me concerning the Case Study of Pd-B. If you think it would be unwise to send details by fax or by telephone discussions, please let me know. However, I can't think of any better way of discussing this with you. Perhaps the appropriate people could contact you directly.

Peter Zarras, co-chair of the Ontario, California meeting needs a copy of your expense receipts in order that he can send you the $500 reimbursement. I guess the ACS needs a copy of the receipts, such as airline fare, for tax purposes. Please fax these to me as soon as possible.

Best wishes,

Mel Miles

Mel Miles

1999-11-19

Bury Lodge heading

CONFIDENTIAL

Dr. Melvin Miles,
Chemistry and Materials Division,
Naval Air Warfare Center Weapons Division,
China Lake,
CA 93555-6100
U.S.A.

19 November 1999

Dear Mel,

After I wrote to you last week I realised that your budgetary constraints will probably only allow you to meet a part of my hotel expenses in Ontario. So let me say that any help which Peter Zarras can give will be greatly appreciated but also that I am very philosophical about such matters!

The main reason why I am writing to you today is to urge you to look at the background of the C.F. saga with more skepticism than you usually show. I believe that this is necessary in view of your developing contacts in Washington, I will illustrate this by referring to the results in the Report which I sent to you in September.

I am attaching a copy of Fig. A10 of the Report on which I have noted the correct value of the true heat transfer coefficient (as substantiated also by the results in Fig. A.20 and Fig. A.21). Now one can ask; how can it be that the Scientists at N.H.E. settled on a value of the true heat transfer coefficient which is less than any observed value of the lower bound heat transfer coefficient? One must bear in mind here that they had been shown several times that such a conclusion is impossible and they had also been shown how to evaluate the results correctly. Furthermore, one should bear in mind that there are ~ 30 ways of evaluating the results only one of which, $(k_R')_{362}$ gives highly error prone and, usually incorrect results. The related coefficient $(k_R')_{32}$ gives reasonable results if the ICARUS protocols are adhered to - which they were not. Now which methodology did they use? We do not know because they haven't told us but, as far as I can tell, it was that leading to $(k_R')_{362}$!

One must now ask oneself: what precisely does this reveal about their intentions and how should one interpret their actions?

When I gave the Seminar in Frascati I did not initially use slides prepared from Figs. A.18 and A.19. However, Paolo Tripodi then said that one must differentiate between the actions of N.H.E. and Mitsubishi. He implied that the group at N.H.E. used a value of the heat transfer coefficient which would make the excess enthalpies straddle the zero line i.e. as in Fig. A.18

leading to the conclusion that the methodology was inaccurate and that there is zero excess enthalpy generation. However, the true behaviour is as in Fig. A19. [87]

At that stage, I showed slides of the two figures. There is a secondary reason why I am outlining this saga to you. There was a Chinese visitor at Frascati at that time who was intensely interested in the topic and asked the people there for copies of my slides. I am making haste slowly and, indeed, I would like to have your comments and any comments your Washington contacts might wish to make on the contents of this letter. Of course, the results *per se* are of no interest as far as National Security is concerned except in the sense that our Japanese Colleagues have been misleading everybody (or else, at least, trying to mislead everybody). Needless to say, one must expect the Chinese in particular to gather intelligence whenever possible. Incidentally, at the time of the Monte Carlo meeting I got some pretty good evidence that they are spying on the Russians.

It is relevant to the saga that our Japanese Colleagues have so far refused to give me any data for "blank" experiments and all the data I had for other experiments were removed, from the files sent back to me from Sophia Antipolis. At least, I interpret their actions as a refusal because they simply do not respond to my requests for data. I interpret this refusal as being due to the fact that if I had such data, I could then show that the instrumentation was working perfectly. The paper which they published in the Monte Carlo proceedings was based on two experiments, the first of which was subject to a fault which we could not identify (but probably due to a bad connection or dry joint) and the second was totally corrupted by noise. This was all spelled out to them in the First and Second Reports which we sent to them in 1994. However, in spite of the high noise levels, I could show that they did, in fact, observe excess enthalpy generation in the second experiment, (the Poster I gave in Vancouver). You can find more about the strange background to these events in our earlier correspondence.

What do you make of all this? Incidentally, the French have also so far refused to give me the data they obtained using J.M. Material Type A electrodes (which I got for them with great difficulty) using an ICARUS-1 look alike.

You will recall that Asami gave me a CD. in Vancouver containing details of 7 experiments. I suspected (and still suspect) that these experiments had been selected to show zero excess enthalpy generation but, of course, they have shot themselves in the foot because I can show that the equipment was working perfectly! Furthermore, there is excess enthalpy generation in one experiment as the system is driven to the boiling point (I believe that they did not know how to analyse these portions of the experiments). I will need your help to get more information about the relevant air pressures from Sapporo Airport.

I think that we should analyse also your Pd-Ce experiment and, at that stage, we should invite Asami, Matsui (possibly also Sumi and Ikegami) to join us in writing a paper. If they decline,

---

[87] MF *One would need an error in the cell temperature of 5 - 6 K to explain away the negative excess enthalpies. This is beyond the bounds of possibility especially as the two temperature readings in the cell agree with each other!*

then we should go ahead on our own and, in this case, incorporate material from the Poster I gave in Vancouver.

When we spoke by 'phone I told you some things which I should perhaps not have said over an "open" telephone. As our conversation was not secure I also stopped short of giving you some information which you may need. One important matter is that I told you that there is some indirect evidence that the specific rate of excess enthalpy generation can reach 1 MW cm^{-3} but 10 MW cm^{-3} would be nearer the mark. This is still very short of the values achieved in fission devices but would nevertheless lead to a mighty bang if the system were appropriately confined - and it is pretty clear how one should do this. Also, the impossible takes a little longer: 1 year, 2 years? How long if one had the N.A.W.C. facilities?

More anon,

Yours,

Martin

P.S. Can you please send me the title, authors and ISBN number of the book on Q.E.D. recommended to you by your friends?

P.P.S. My longer term research plans included experiments to measure the effects of quantum fluctuations. This is especially important in the context of Q.E.D. but, of course, I shall now never be able to implement such work.

P.P.P.S. One crucially important matter which needs to be explored is the effect of electrode geometry within the context of Q.E.D.

P.P.P.P.S. I have some evidence that the Russians concluded that C.F. explained a number of phenomena which had been puzzling them. I do not know which phenomena but I could hazard a guess.

P.P.P.P.P.S. As you know, I have flown a number of kites in connection with S.B.E.R. There was only one response and this was from a member of Baraboshkin's group in Sverdlovsk. Interestingly, he said that Baraboshkin had told him that I had told Baraboshkin . . . . . which I never did! I thought that they were fishing.

P.P.P.P.P.P.S. You may think that I am a very suspicious person. Of course, this is absolutely correct.

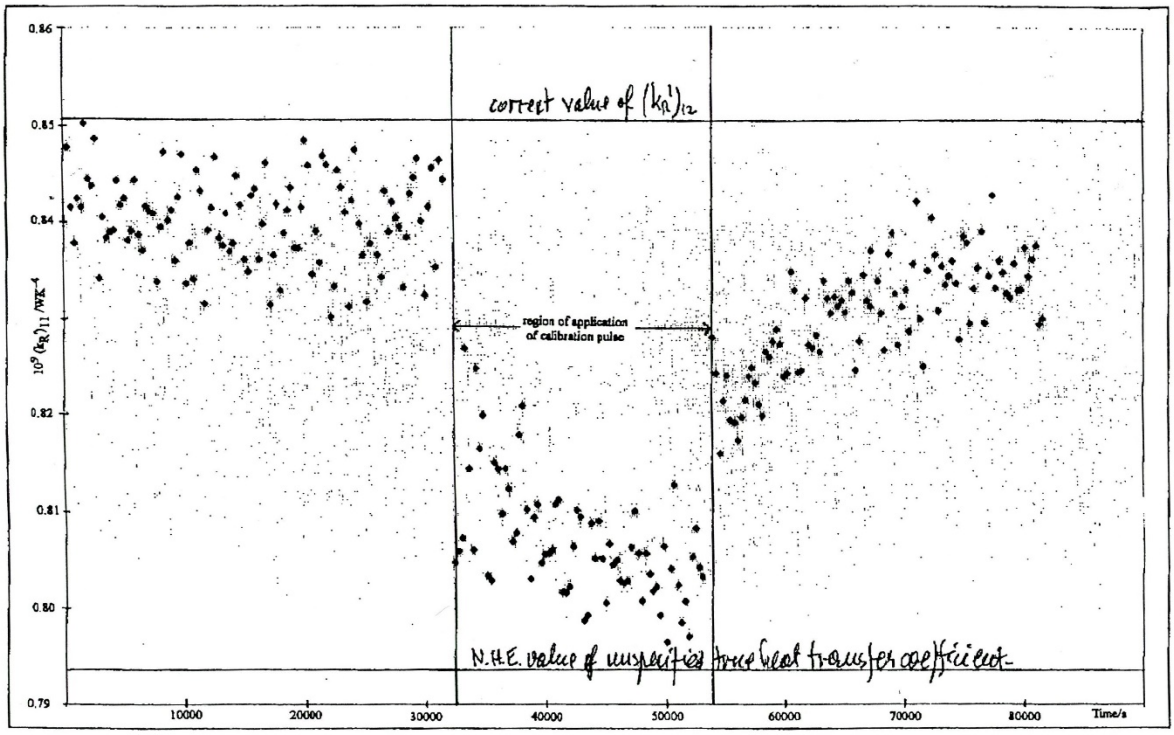

Fig A.10

124

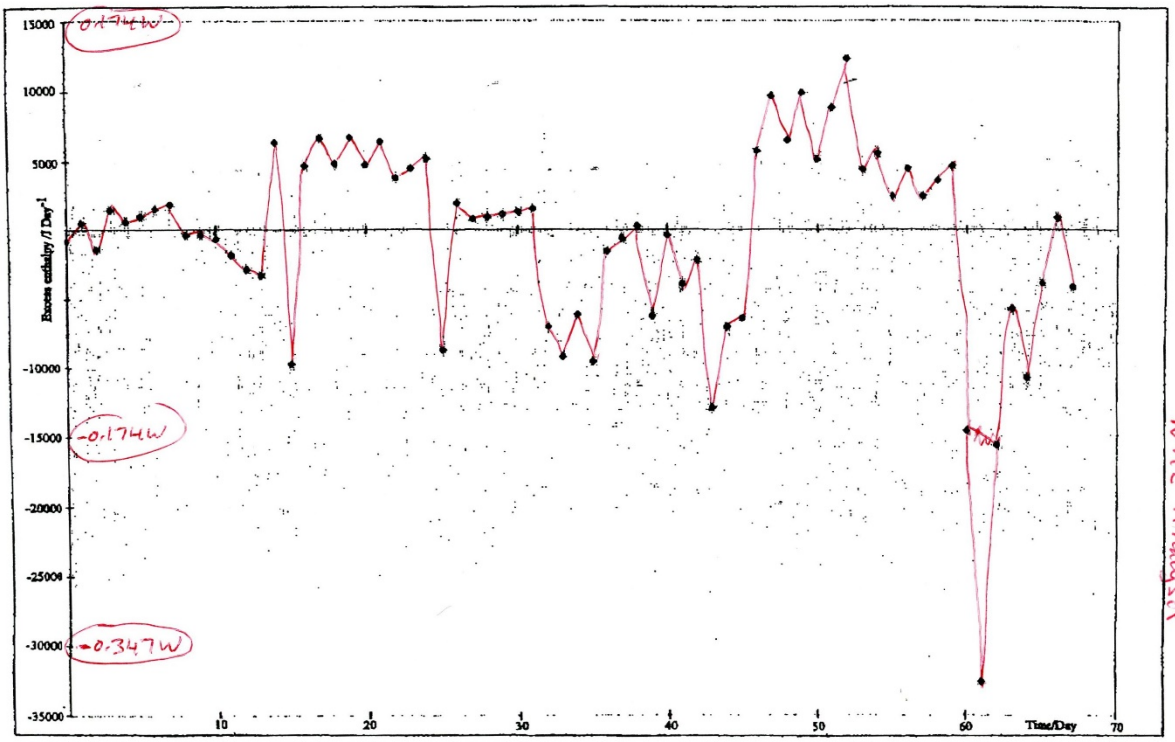

Fig. A.18

NHE Analysis

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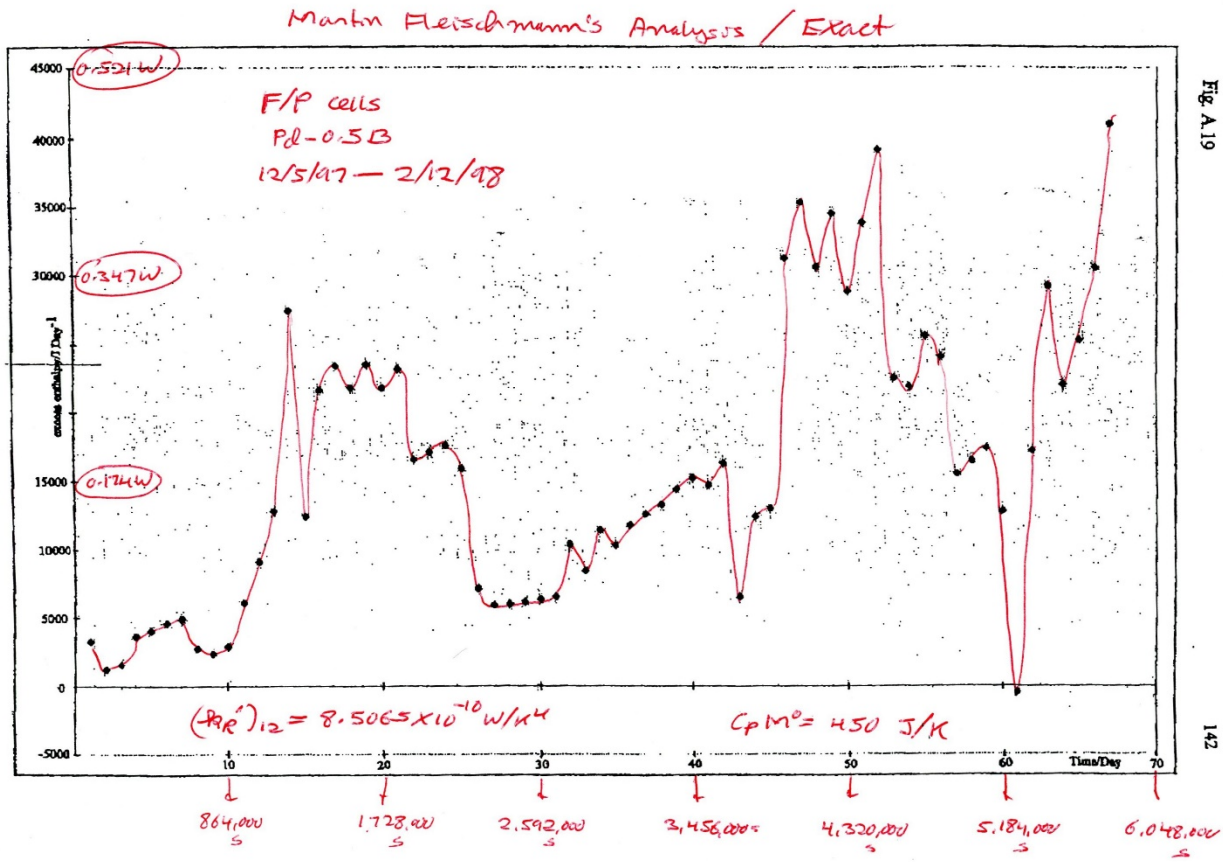
Martin Fleischmann's Analysis / Exact
F/P cells
Pd-0.5B
12/5/97 — 2/12/98
0.521 W
0.347 W
0.174 W
excess enthalpy/J Day⁻¹
45000
40000
35000
30000
15000
10000
5000
0
-5000
10
20
30
40
50
60
70
Time/Day
$(\Phi_R')_{12} = 8.5065 \times 10^{-10}$ W/K⁴
$C_p M^\circ = 450$ J/K
864,000 s
1,728,000 s
2,592,000 s
3,456,000 s
4,320,000 s
5,184,000 s
6,048,000 s

Fig. A.19

1999-11-19 #2

DEPARTMENT OF THE NAVY
NAVAL AIR WARFARE CENTER WEAPONS DIVISION
1 ADMINISTRATION CIRCLE
CHINA LAKE, CA 93555-6100
521 9TH STREET
POINT MUGU, CA 93042-5001

IN REPLY REFER TO:

Dear Martin,

Thanks for your fax. I tend to be too trusting and you could very well be right about the Japanese intentions. More about that later.

I sent your manuscript to Dr. Imam today. Perhaps it can be published in an NRL Report.

For Pd-B, my preliminary analysis in Japan (Fig. 10) shows the basic feature of your exact analysis (Fig. A19). Please note that the Pd-B was a Rod with d = 4.7 mm and L = 20.1 mm (V = 0.350 cm^3, A = 3.15 cm^2). The experiment started at 10:00 AM on Dec. 4, 1997 and ended at 10:00 AM on Feb. 12, 1998. However, the current was turned on at 10:00 AM on Dec. 5, 1997. I doubt if this will make much difference.

My Pd-Ce results are shown in Fig. 11. It will be interesting to see your exact analysis for this experiment. This was also a Rod (the one you gave to me) with d = 3.16 mm, L = 19.54 mm (V = 0.153 cm^3, A = 2.02 cm^2). This was run? with exactly the same time period as the Pd-B rod.

The third experiment used Pd-Ce-B made by NRL. It showed little, if any, excess heat. This experiment is valuable because it demonstrates stable calorimetry

Best Wishes, Mel Miles

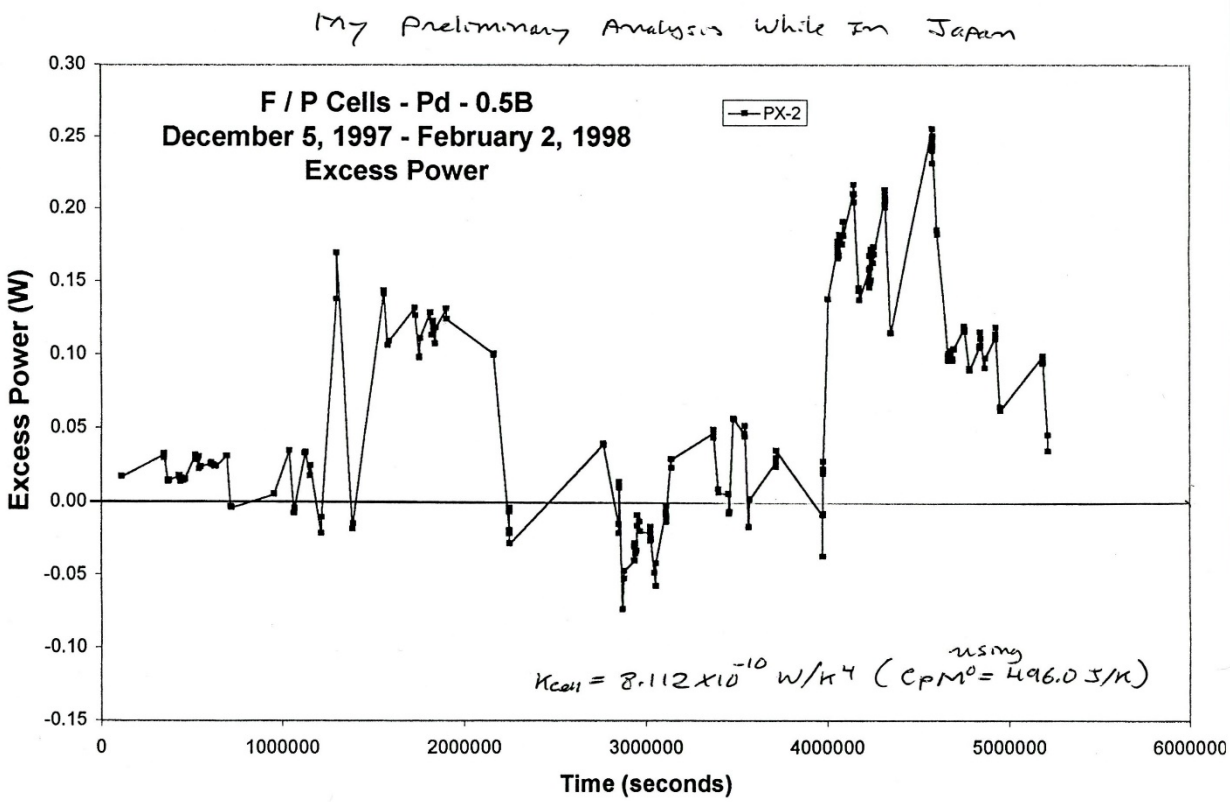

Figure 10. Excess power measurements for the Pd-0.5B cathode in Cell A-2.

Martin Fleischmann's Analysis / Exact

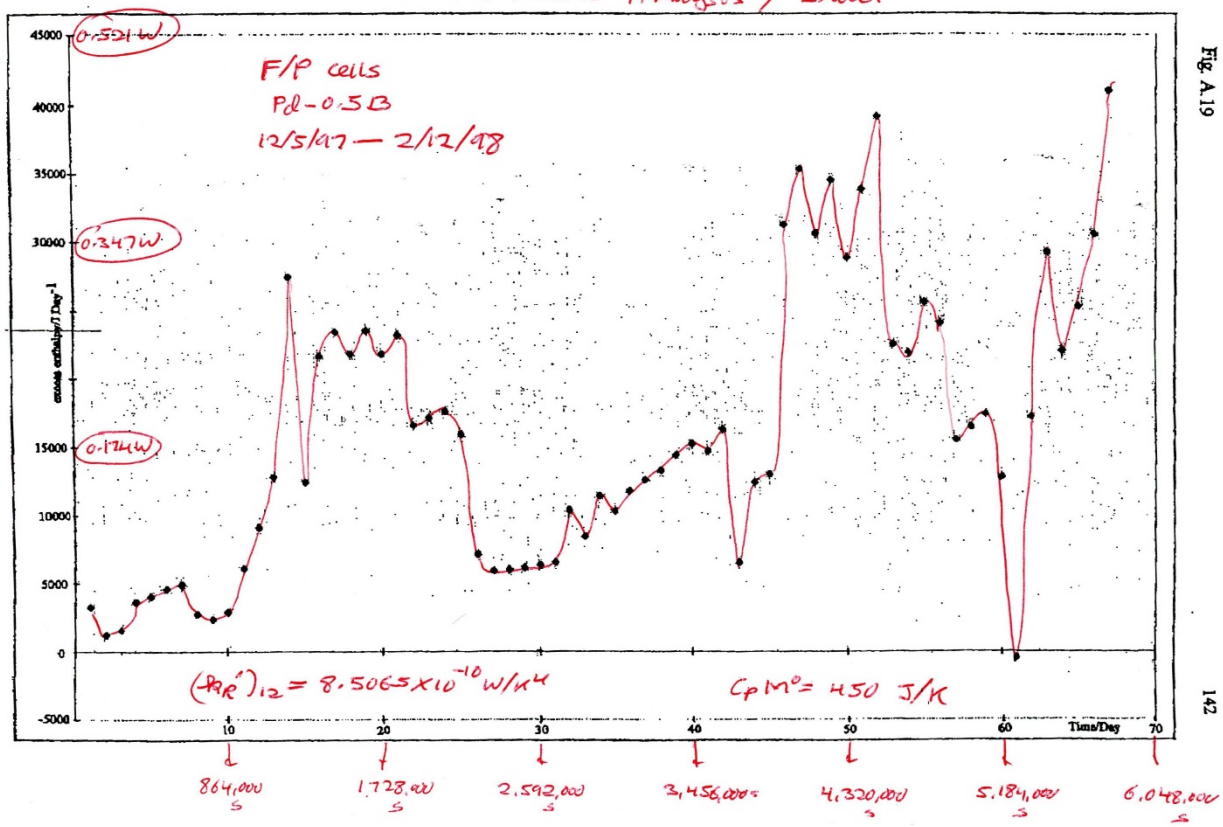

Fig. A.19

142

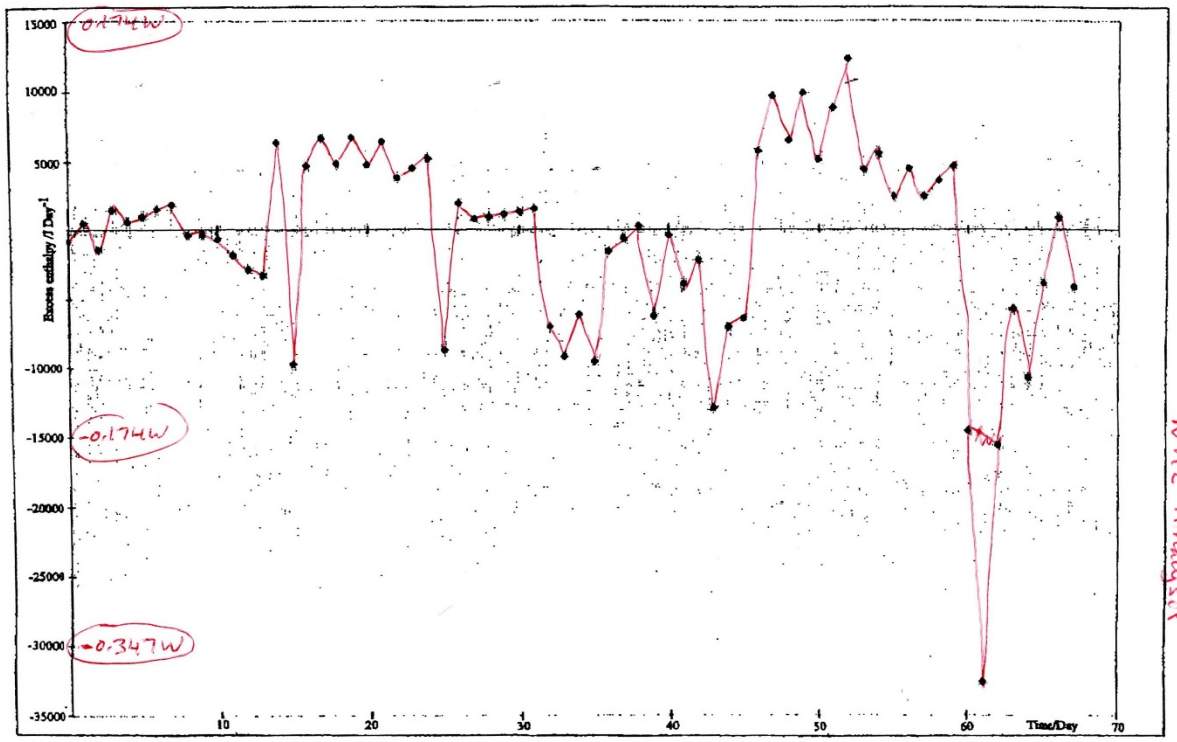

Fig. A.18

NHE Analysis

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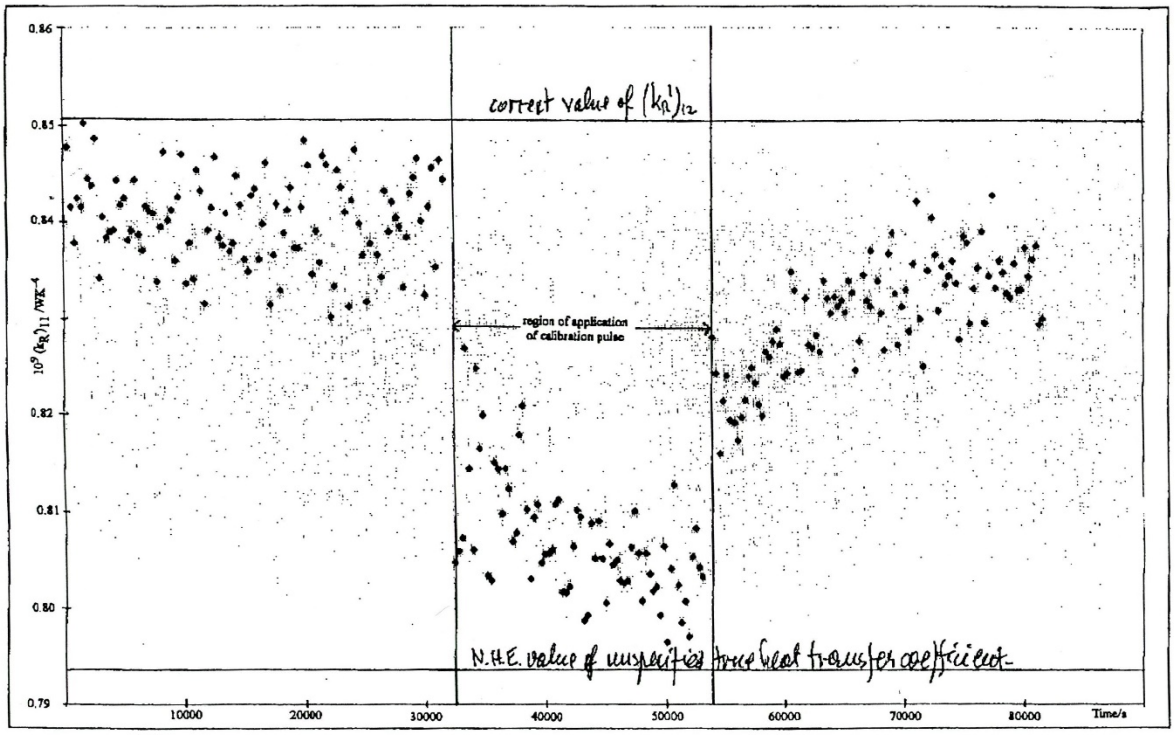


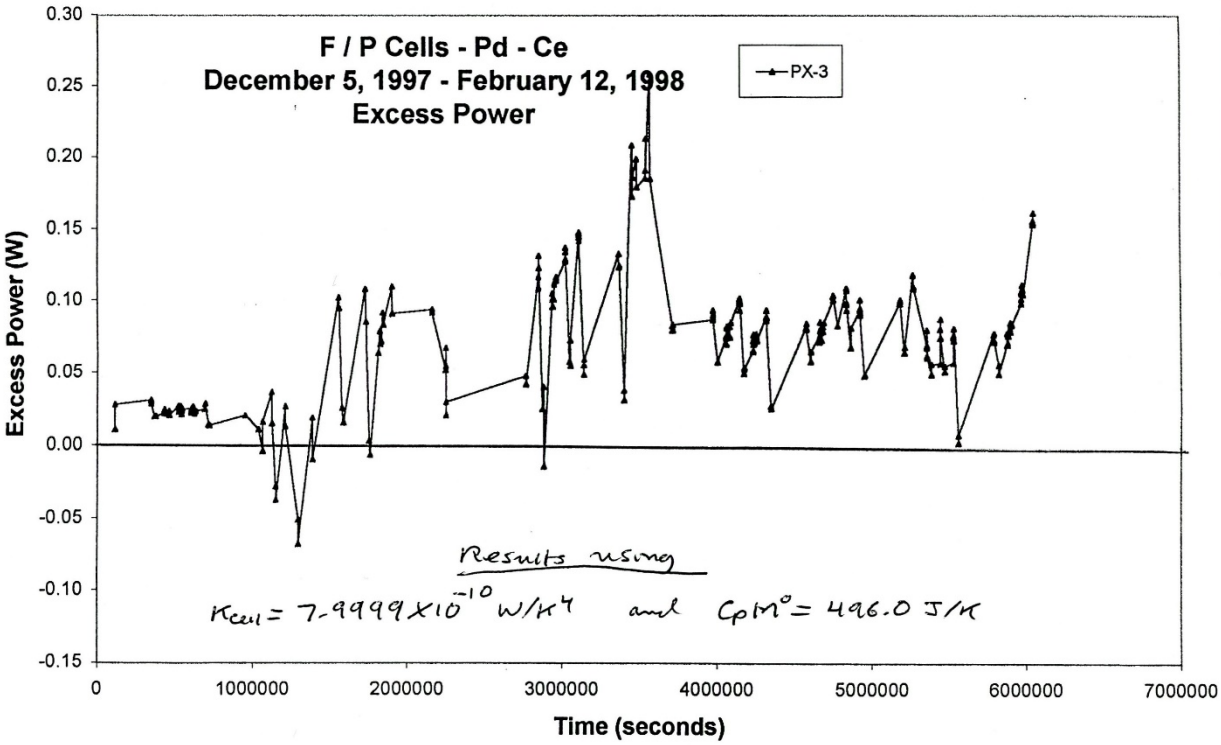


Figure 11. Excess power measurements for the Pd-Ce cathode in Cell A-3.

Analysis of Experiment Mc 21 carried out by M.H. Miles in the Sapporo Laboratories of the New Hydrogen Energy (N.H.E.) Group

Section A.

A.1 Introduction

This experiment, designated as FP2-97120402-M7c2, was carried out by M.H. Miles during his stay in 1997-98 in the Sapporo Laboratories of the N.H.E. Group. The experiment was started at 10:00 a.m. on ~~8~~ 4th December 1997 and terminated at (?) ~~00:30~~ 10:00 a.m. on 12th February 1998 on the ~~69~~ 70th day following the start of the experiment. The electrode used had the form of a ~~parallelepiped~~ cylindrical rod of dimensions 4.71 mm X 20.1 mm ~~X 2 mm i.e.~~ cylindrical rod

Volume of electrode = ~~0.1893~~ 0.350 cm^3 (A.1)

area of electrode = ~~2.886~~ 3.15 cm^2 (A.2)

and was made of 99.5% Pd + 0.5% B as prepared by the Naval Research Laboratories, ~~Arlington, Virginia.~~ Washington, D.C.

The cell used in the experiment was of the ICARUS-1 Type and is illustrated in Fig. A.1 (the illustration is approximately to scale). The cell number was given as 38 and the experiment was carried out in position A.2 (i.e. in position 2 of thermostat tank A) using an ICARUS-2 Type electrochemical polarisation, control and data acquisition system. The electrochemical system consisted of an Hi-Tek DT2101 potentiostat wired up as a galvanostat. These potentiostats/galvanostats are capable of delivering currents of ± 1A at output voltages up to ~ ± 100V. A separate potentiostat/galvanostat was used to deliver constant currents to the resistive heater used to calibrate the cell. The system was controlled by a 486 data acquisition

December 1, 1997

From My NHE Notebook

Cathodes for F/P Experiments

(see also p. 54 of Notebook #1, China Lake cells)

Note) repaired anode glass tube using epoxy about two weeks ago — looks fine → cell #1

2 → F/P #2 **Pd – 0.5 B** (0.5 wt.% B) 4.8 × 20.1 mm [cell #2]
↳ 4.71 mm

- From NRL
- Polished, cleaned by Mari
  Diamond paste (No other polish used)

$\boxed{4.71 \times 20.1\ mm} \Rightarrow V = \pi R^2 L = \boxed{0.350\ cm^3}$ $A = \pi R^2 + \pi D L = \boxed{3.15\ cm^2}$

3 → F/P #3 **Pd – Ce** (from M. Fleischmann) [cell #1] 4.00 × 19.6 mm
↳ 3.16 ↳ 19.54

- gave excess heat in China Lake experiment
- Polished by myself with Si-C paper
- Difficult to remove long cracks
- Polished by Mari to remove cracks
- Final polish by myself using SiC paper (using latex gloves)

$\boxed{3.16 \times 19.54\ mm} \Rightarrow V = \pi R^2 L = \boxed{0.153\ cm^3}$ $A = \pi R^2 + \pi D L = \boxed{2.02\ cm^2}$

1 → F/P #3 **Pd – Ce – B**, NRL sample, 4.4 × 20.05 mm
↳ 4.40

- polished only by myself using Si-C paper
- Final polish wearing latex gloves.
- No measurable changes in dimensions due to polishing

$\boxed{4.40 \times 20.05\ mm} \Rightarrow V = \pi R^2 L = \boxed{0.305\ cm^3}$ $A = \pi R^2 + \pi D L = \boxed{2.92\ cm^2}$

⇒ Video made for each electrode using microscope (Mari) Labeled.

Microscope check

1 – Pd-0.5B – highly polished, very shiny. A few holes, scratches are visible circular polishing lines but very few

2 – Pd-Ce ⇒ Duller finish. Longitudinal lines from final Si-C polish small cracks near ends. A few holes.

3 – Pd-Ce-B ⇒ Dull finish. Longitudinal lines from Si-C polish. More holes than #2.

1999-12-02

NAWC heading

FAX MEMO

DATE: December 2, 1999

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

. . . In reply to your letter of 19 November 1999, I probably am too trusting of other people in regards to the cold fusion "saga". You could very well be right about your suspicions concerning Japan. It is certainly possible that they have deliberately tried to make it appear that there are no measurable effects in cold fusion. I was certainly surprised in Japan at the lack of interest that Dr. Asami and others at NHE showed towards my measurements of excess heat. I don't know if my results appeared in their final reports. I need to check with Dr. Takahashi about this since he received a copy of their report in Japanese. The politics at the time I was there seemed to indicate that they wanted no cold fusion effects that would interfere with their decision to close down the laboratory. Nevertheless, it is also possible that they looked at your complicated equations and tried to find an easier method, which lead to their incorrect data processing. Most of the people at NHE were engineers and not research scientists. Mr. Sumi told me that NEDO was only interested in "large effects" such as 100% excess heat, or larger.

What is your opinion towards Dr. Ikegami? He visited NHE several times and was there at the final review meeting. It seemed to me that he was overly critical of the Fleischmann-Pons calorimetry. Perhaps he had some political motives that I am not aware of. Elliott Kennel certainly was a strong critic of your calorimetry as well as any other cold fusion effects. I wondered whether he was put up to this by somebody at NEDO in order to justify the closing of NHE.

I had a similar experience when I tried to transfer my calorimetric methods to the Naval Research Laboratory. In my case, I think the people involved in the calorimetric measurements were simply inept. They certainly did not have the right background to understand calorimetry. Their own final report gives the calorimetric error as + / - 200 mW. Therefore, they would have missed anything except large effects. In my case, this was not due to politics but instead it was due to poor science. NRL consumed 2/3 of the funding but never produced anything worth publishing. This is the main reason why the Navy gave up funding for cold fusion. The only positive thing to come out of NRL was the Pd-B alloys produced by Dr. Imam. Incidentally, Dr. Imam is interested in publishing your Pd-B data analysis as an NRL report. Do you have this on disk that would make this job easier?

I agree 100% with you that we should analysis also the Pd-Ce experiment. Of course, I won't be able to provide much help since you are the expert at this data analysis. In fact, I am very impressed with your methods. You are certainly better at this than anybody else that I have every met. Would you like me to contact Asami, Matsui, Sumi, and Ikegami to see if they want to join us in writing a paper? Please let me know.

I am presently revising my paper for Professor Parsons and The Journal of Electroanalytical Chemistry. I am also writing a paper for the Asti Meeting. This has been taking up all my spare time recently. I will send you a copy of both when they are finished.

Regarding your first P.S., this book is titled "The Quantum Vacuum - An Introduction to Quantum Electrodynamics" by Peter W. Milonni of Los Alamos, New Mexico. [88] The ISBN number is ISBN 0-12-498080-5. If you obtain this book please let me know what you think and what sections would apply best to cold fusion theories.

I will try to write more later when my wife is not so tired!

Mel Miles

Mel

[88] MM Later loaned to Martin (Never Returned).

1999-12-21

Bury Lodge heading

Professor G. Preparata & Dr. Emilio Del Giudice,
E.N.E.A.
Frascati

21 December 1999

Dear Giullano and Emilio,

I now want to launch myself on the description of one of my "pipe dreams" for the C.F work - see my letter of 1/12/99. In brief this "pipe dream" is the search for the answer to the question:

> "can one devise a system (or systems) in which the rate of thermal output is controlled by the rate of addition of D to the system(s) and nature of the nuclear reaction(s)?"

As I said in my letter of 1/12/99, this idea is not as far-fetched as might appear at first sight and I want to jump right ahead some way along the route of a possible realisation of this "pipe dream" by noting that it is the combination of electrodiffusion with electrolytic charging which could lead to a demonstration of the concept. In effect, we wish to construct a "black box" as illustrated in Fig. 1. The chemical input of D into the lattice is j/F where j is the electrolytic current; the thermal input is I^2R where 1 is the "Coehn" current; the thermal output is $I^2R + j\Delta H/2F$ where ΔH is the energy release per mole of the product formed which I will assume to be ^4He. I have shown that ^4He is released from the "black box" but, of course, it is possible that it will be retained within the box.

Next, I want to put an important restriction on the question which I have posed namely, that we will only investigate the steady state. The question then reduces to looking for the answer to whether or not the rate of excess enthalpy generation is $j\Delta H/2F$. I don't believe that we should ever be drawn into the search of whether the excess enthalpy release can be made equal to that expected for the total amount of D stored in the system. That is an horrendous amount, say 25 MJ for a 10 cm long Pd wire of 50 μm diameter. As I have said on other occasions, there are other people who may be (or are?) interested in this question. I would note here that it is not especially difficult to devise programmes which would provide an answer but I have set aside firmly any attempts in that direction. One reason why I have touched on this side-issue is because I am sure that the question of safety will eventually be raised - especially if the Coehn-Aharonov approach proves to be successful. One benefit of the programme I am outlining here is that the further developments (the various aspects of "Heat after Death") will certainly give *inter alia* important information on the safety aspects.

I want to interject here "an historical note". It had become clear to us by the summer of 1990 that the time had come to switch our attentions to electro diffusion coupled to electrolytic charging. As you will recall, my attempts to start systematic work on this aspect failed for a

variety of reasons - this is a matter which we should consider further on a future occasion. However, eventually, we were able to carry out a small amount of work on the concept described by Fig. 2 i.e. the induction of excess enthalpy generation in a system where a Pd wire is loaded from the gas phase. You will recall that our investigations led to a "parting of the ways" - I was never able to bring this work to the point at which it could have led to publishable conclusions. However, I am reasonably satisfied that we achieved rates of excess enthalpy generation in excess of 10 kW cm^{-3} expressed as a rate for the total volume of the wire. Of course your own work has shown that such rates can be achieved and, am I correct in concluding that you also showed that such high rates are achievable for loading from the gas phase?

One matter which is obviously of crucial importance is for us to discuss to what extent the concept described by Fig. 2 does or does not satisfy the criteria posed by the Coehn-Aharonov effect. However, for the present, it is sufficient to note that high rates of excess enthalpy generation can be achieved with loading from the gas phase.

I want to turn now to another of my obsessions, that of the so-called microelectrodes. Fig. 3 shows how such a concept can be introduced into the investigation of the problem I have outlined (although the dimensions of the present "Coehn wires", $\Phi = 50$ µm, are really above that of the usual microelectrodes). Let us start the consideration of such a system by assuming that $j = 10^{-7}$ A. Then the current density for the $D_2O + e \rightarrow Pd\text{-}D + OD^-$ reaction on the microdisc will be 5 mA cm^{-2}. This leads to an important question: "what are the current densities for the electrolytic current in your present experiments?" However, for the present it is sufficient to note that loading from the gas phase can lead to high levels of excess enthalpy generation when this loading is combined with electrodiffusion. We would therefore expect that the same will apply for loading with an electrolytic current density of 5 mA cm^{-2}. Let us also note in passing that the principal by-pass current due to the reduction of oxygen present at the concentration in equilibrium with air is expected to be 8×10^{-10} A and, of course, this current could be reduced by de-oxygenating the solution (e.g. by sparging with "boil-off" nitrogen). We can therefore expect the discharge reaction to be 100% efficient.

We must next take a "quantum step" in the preconditions for the experiments required to answer the question which I have posed. This is that we can confine the D^+ in the lattice for adequately long wires as in Fig. 3 - say length = 10 cm. We note that we will need say 200 days of charging at 10^{-7} A to charge the whole electrode to a ratio D/Pd = 1. Furthermore we expect the mobility to be $u_+ \approx 5 \times 10^{-6}$ cm $s^{-1}/(V\ cm^{-1})$. We can therefore also expect that me relaxation time for the charging of the wire will be ~ 200 days in the absence of any Coehn-Aharonov effects. Of course, the presence of these effects will markedly reduce this relaxation time and we can also expect that we will not need to charge the wire completely along the whole length in order to achieve a limiting rate of excess enthalpy production. This will also contribute to the shortening of the time scales.

We must ask next: "what is the maximum steady-state rate of excess enthalpy production which would correspond to a charging rate of 10^{-7} A" The answer is ~ 1 W which would require a specific rate of excess enthalpy generation of 5 kW cm^{-3} based on the volume of the whole wire!

So you will see that, if all the preconditions are met, the experiment becomes entirely sensible and doable! Of course, it is extremely unlikely that one will be able to construct a successful experiment at "first go, starting from cold." However, "pipe dreams' sometimes become possible if one can establish that the preconditions are at the very least not excluded.

The next question is: "is it worth-while to attempt such an experiment?" After all, we already know that there is a reasonable correlation between the rate of excess enthalpy generation and the rate of ^{4}He production. However, I believe that the experiment I have described is nevertheless worthwhile because it will lead in turn to a substantial number of other investigations. The first step is to show that any limiting rate of excess enthalpy production becomes independent of the "Coehn current, I," provided this is sufficiently large while at fixed I the rate of excess enthalpy production is proportional to the electrolytic current, j. At a later stage we may wish to delineate the relationships between j, I, ϕ, 1 and θ which will undoubtedly be very complicated; we may also wish to explore the effects of changes in composition of the wire.

With regard to the question of whether we should or should not start such experiments, it is also relevant that the costs will be low and the manpower requirements will be minimal. Of course, at a later date we might wish to increase the effort required to delineate the parameter space(s). This would undoubtedly require the setting-up of experiments in parallel in view of the long duration of each experiment. However, the question of whether or not we should develop a full-blown investigation can be decided at a later date.

A wider ranging investigation might well include the question of excess enthalpy generation in larger sheets, Fig. 4. Such experiments could chart the way towards the scale-up of the measurements and it might then also be sensible to combine the measurements of excess enthalpy with measurements of the rates of ^{4}He [89] generation.

I envisage that the first experiments will be carried out in sealed calorimeters developed from the ICARUS-1 design, Fig. 5, (the system could remain sealed for the whole duration of the experiments). I note that the experiment I have described fits into the dynamic range of this type of isoperibolic calorimetry. It is possible that temperature measurements could be made with accurate thermometers and that there would be no need for computerised instrumentation. It would be necessary, however, to set up an accurately controlled thermostat.

This is probably my last letter before Christmas so very best wishes to you all and let's look forward to the next millennium!

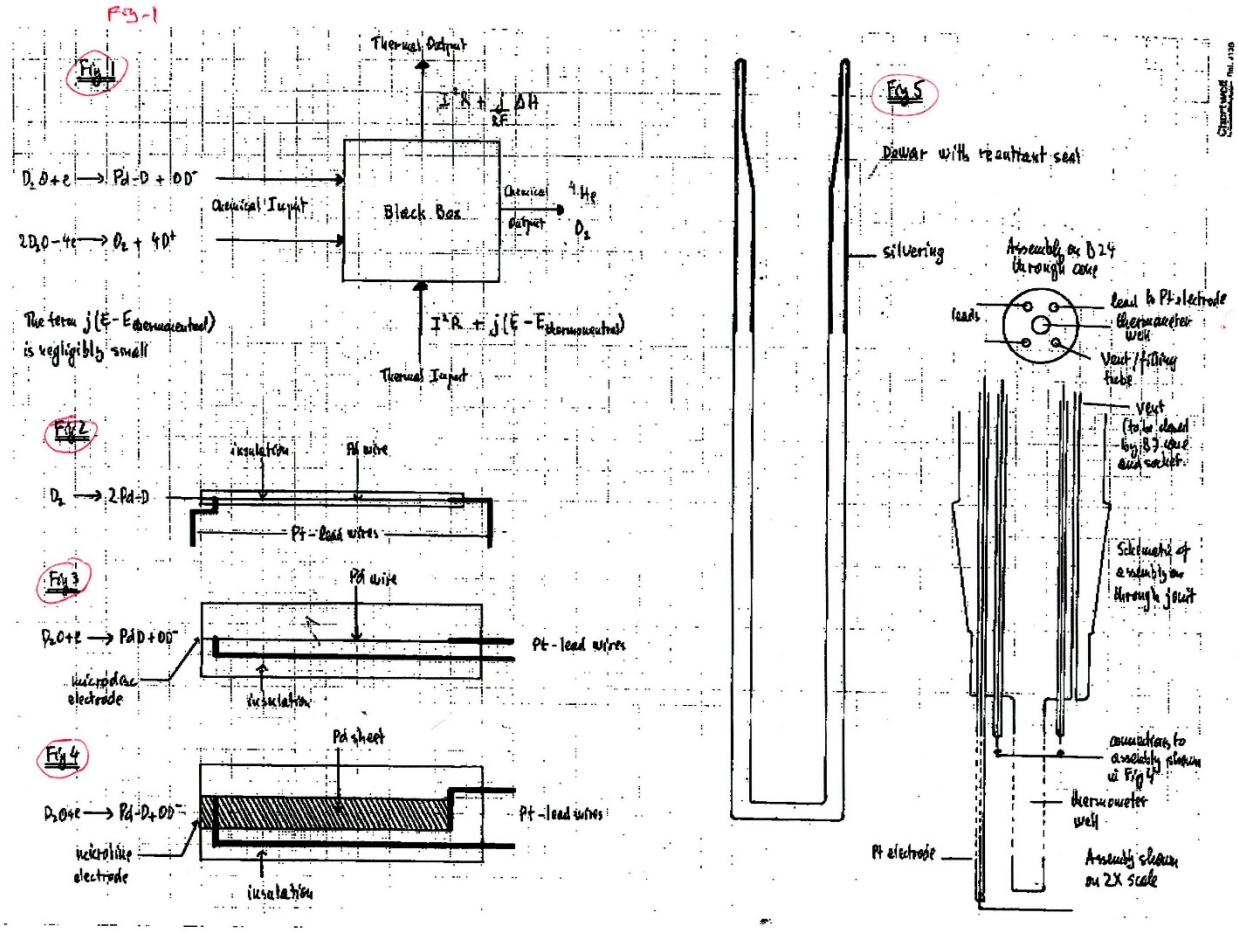
Yours,

Martin

P.S. I can't get this through to your FAX so will leave this till after Christmas. Before then, I will also write to you about our discussions at J.M. which affect the contents of this letter.

[89] JR This originally said ^{4}H; MM noted that it was probably a typo that should be ^{4}He.

P.P.S. it will be clear to you that we can take several steps to shorten the time scales of the "pipe dream" experiments.



1999-12-21 #2

Bury Lodge heading

Dr. Melvin Miles,
Chemistry and Materials Division,
China Lake,
CA 93555-6100
U.S.A.

This FAX summarizes my attitude/points of view 21/12/99. It has somewhat overtaken by your FAX of 22/12/99. I will write to you at length after Christmas.

21 December 1999

Dear Mel,

I hope you haven't given me up as lost! First of all, many thanks for transmitting my receipts to Peter Zarras – I hope that this will produce a payment. Secondly, I am now sending you under separate cover a disk containing the text of the report (using Microsoft Windows, Version 3.1). At the same time I am sending a duplicate disk to Stan Szpak who has started working on his composite report. The text on these disks does not contain the spreadsheets or diagrams which are distributed over various disks with incompatible software. Since the meeting in Ontario, I have had one go at assembling it all using Power Point but I simply have not had the time to finish the job. I have told Stan that it might be best to decide on the text first of all, then pick the relevant spreadsheets and figures and use a scanner to produce a new disk). I have also suggested that it might be appropriate to exclude the spreadsheets but to refer to them and say that they are available on request. Of course, all of this applies equally to anything Dr. Imam may decide to do.

I dare say that you will have told Stan and I made a mistake with regard to the shape and volume of the electrode? This mistake means that the axes of some of the figures will have to be changed (those giving the specific rate of excess enthalpy generation). It may well be also that some of the inconsistencies I have referred to in the report can be explained by the error in the volume of the electrode?

I have suggested to Stan that it might be appropriate to incorporate some of the material contained in my report to you of 15/9/98 in the final report. The data analysis of simulations and of "blank experiments" using the Pt-D_2O system do, after all, explain the reasons for the choice of the particular methods used in the evaluation of experiments carried out with the ICARUS Systems.

I believe that I have told you that I am currently analysing one of the key data sets collected by our ex-colleagues at N.H.E. (using a non-J.M. Electrode). The outcome is disturbing or profoundly disturbing depending on one's point of view. It appears that the power inputs

delivered to the cell (as given by their spreadsheets) have been overestimated. Now what is one to make of that? The consequence is that the "lower bound heat transfer coefficients" are larger than the "true heat transfer coefficients." This has been one of the main criticisms leveled against the ICARUS Calorimetry but, of course, one can now see that the apparent inconsistency has been due either to incompetence or, what is worse, intent.

I hope to finish the analysis in the New Year and I will then look at your Pd-Ce data which will be a more positive exercise! However, I am suffering from time constraints. In your letter you asked whether you should approach Asami, Matsui, Sumi and Ikegami. By all means do so – it will be much better if you do this rather than I. You could tell them that you have a report from me discussing the various ways of analysing the data and justifying the choice of methods used for the ICARUS Systems (illustrated by the analyses of blank experiments and of simulations) and a further report on the Pd-B experiment you carried out in Sapporo. You understand that there will be two further reports, one dealing with your Pd-Ce experiment (also carried out in Sapporo) and the other dealing with an experiment carried out by the group at N.H.E. Some of this material will be publishable (and should be published!) and, as far as you are concerned, you would prefer this to be a joint publication with them rather than a separate effort by M.M. M.F. If we do it this way, then it will open up several ways in which either you or I or both of us can open channels of discussion with them. As I see it, they will either have to back down or else we simply have to say that they were wrong (with the appropriate illustrations).

In your letter you also asked for my opinion about Hideo Ikegami: This is a matter about which I don't want to put pen to paper partly (but only partly) because I have had different opinions at different times. This is a matter which we should discuss when we next meet as it might have some bearing on some of your recent preoccupations.

Christmas approaches so here are my Very Best Wishes and let's march forward into the New Millennium!

Yours,

Martin

1999-12-22

NAWC heading

FAX MEMO

DATE: December 22, 1999

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

First, let me wish you a Merry Christmas and a Happy Millennium New Year.

I hope you received the revised manuscript that I hope will be published by Roger Parsons in the Journal of Electroanalytical Chemistry. I will soon be mailing you a copy of my paper for the Asti Conference that I understand will be published by the Italian Physical Society. I have referenced your proposed paper for ICCF8 to be held in May, 2000. It is my understanding that this will be a shorter version of your paper that will be published as a Navy report. This brings me to a possible problem and I need your advice. When we discussed this Navy report with Stan Szpak, it was my understanding that he wanted a revised and shorter version. Therefore, I contacted Dr. Imam at the Naval Research Laboratory (NRL) to see if he could get it published there as a Navy report. It turned out that he was very interested in doing this since it involves his Pd-B material produced at NRL. Now Stan tells me that he will have the full report published in San Diego also, as a Navy report. Therefore, it looks like we will have two publications of the full report. Perhaps this doesn't really matter since both San Diego and NRL want to make revisions in format including the title. My question is, should I inform each laboratory involved or just let it proceed as scheduled? Another possible solution would be for you to complete the analysis of the Pd-Ce electrode and have Stan published that report instead. This would provide for two Navy publications involving two different experiments. Please let me know what you think.

I noticed that the deadline for Abstracts submissions to ICCF8 is December 15, 1999, which I have already missed. However, I can still submit to the website up to December 31, 1999. I am going to try to get at least two abstracts submitted by next week. I hope you can also submit the Pd-B study that I have already referenced in my Asti report.

I have discussed your letters and our telephone conversation with Pennie Peterson who gathers security information for the Navy. She will pass this on to appropriate people in Washington D.C. Perhaps an agent will want to contact you in England or I will be asked to discuss this with you in person sometime. However, it is also possible that this will simply be ignored although I tried to stress its importance.

I think you will find my Asti report interesting and a good lead-in to your criticisms of the NHE data analysis. Please let me know what you think when you receive this report.

Again, have a good Christmas.

Best wishes,

Mel Miles

Mel Miles

1999-12-27

NAWC heading

FAX MEMO

DATE: December 27, 1999

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I am sending you a copy of the two Abstracts that I recently submitted for ICCF8. I think my paper on the Pd-alloys would be a good lead-in to your more detailed paper on Pd-B. I hope that ICCF8 can schedule them back-to-back. Please send me a copy of your Abstract when it is ready.

Have you made any arrangements for ICCF8? For example, what hotel are you planning to stay at? I need to make my reservations soon.

Best wishes

Mel Miles

Mel Miles

Calorimetric Studies of Palladium Alloy Cathodes Using Fleischmann-Pons Dewar Type Cells

Melvin H. Miles
New Hydrogen Energy Laboratory
3-5 Techno-Park 2-Chome Shimonopporo
Atsubetsu-Ku, Sapporo-004, Japan

ABSTRACT

My first three experiments conducted at NHE using the F-D Dewar type cells investigated the Pd-Ce-B, Pd-B, and Pd-Ce alloy cathodes. Significant excess power was produced from the cells using the Pd-B and Pd-Ce alloy cathodes. The Pd-Ce-B alloy, in contrast, showed no significant excess power effects. Previous experiments at China Lake using similar Pd-B alloy cathodes prepared by the Naval Research Laboratory (NRL) produced excess heat in seven out of eight experiments. The same Pd-Ce cathode that was used at NHE also produced significant excess power in previous experiments at China Lake. Due to the controversy over methods of data analysis for the F-P cells, I developed my own methods while at NHE. As I refined my methods for evaluating the calorimetric measurements, they approached more closely the methods outlined by Fleischmann and Pons in their Icarus systems handbooks available at NHE. It was disturbing that the method previously developed by NHE for the analysis of the F-P cells showed no excess heat for any of these same three experiments. The major problem with the NHE method is that a single calibration was used in determining the heat transfer coefficient for the cell. An incorrect heat transfer coefficient can readily confuse the excess heat effect with the calorimetric error for the system. Calorimetric results for the same experiment using the NHE method, my method, and the F-P method for data analysis will be presented. The fact that the alternative NHE method showed no excess heat for F-P cells illustrates the problem in transferring calorimetric methods from one laboratory to another. The second laboratory often fails to follow directions and makes changes that compromise the calorimetry. Similar problems were encountered in the attempt to transfer the China Lake calorimetry to NRL, hence excess heat was not observed.

Measurements of Excess Power in Pd/D_2O+LiOD Electrolysis Cells

Melvin H. Miles
New Hydrogen Energy Laboratory
3-5 Techno-Park 2-Chome Shimonopporo
Atsubetsu-Ku, Sapporo-004, Japan

ABSTRACT

New experiments in sensitive China Lake type calorimetric cells displayed the characteristics of the excess power effect during seven different occasions. These measurements clearly show the anomalous increase in the cell temperature despite the steadily decreasing electrical input power during Pd/D_2O+LiOD/Pt electrolysis. This strange behavior can be modeled by the use of an anomalous excess power term in the calorimetric equations. Two thermistors used in each calorimetric cell always show nearly identical temperature changes, thus errors due to temperature gradients within the cell are unlikely. The onset of the excess power apparently develops in a gradual manner. There were never any large, abrupt increases in the excess power. The addition of D_2O with its sudden cooling of the cell generally dissipated the excess power effect. No clear triggering events for the excess power could be identified. Several possible chemical processes considered do not explain the excess power. Normal behavior was always observed for a similar experiment conducted as a control. Anomalous thermistor signals suggest the emission of electromagnetic radiation from the active palladium cathode.

2000-01-06

NAWC fax heading

Martin,

This makes me think that you are right about NHE and Japan.

Mel Miles

Melmiles

From: Jed Rothwell <JedRothwell@infinite-energy.com>
To: <ewall@infinite-energy.com>; <Storms2@ix.netcom.com>; <editor@infinite-energy.com>; <jkooistra@infinite-energy.com>; <melmiles@ridgecrest.ca.us>
Sent: Thursday, January 06, 2000 6:40 AM
Subject: The NHE lied about Miles

[Here is the response from Takahashi. This is not a good translation, but the gist of it is, they have totally distorted Mel's results. I have asked Takahashi to fax me the original. I will improve the translation.

NHE
Distorted my results

- JR]

Date: Thu, 6 Jan 2000 12:17:05 +0900
From: "Akito Takahashi" <akito@nucl.eng.osaka-u.ac.jp>
Subject: Re: Request from Mel Miles

Dear Jed:
Happy New Year.
In the final evaluation report of NHE project, the work by Dr. Miles is not mentioned. However, in the research report NEDO-NHE-9701 (June, 1998), in p.120 they very briefly mention about Dr. Miles work at NHE labo. My translation is as follows:

3. Miles' test
Our guest researcher Dr. Miles has done a test experiment at NHE laboratory, using ICARUS-2 installing a cathode which he said to observe excess heat previously. Experimental results are shown in Figs.3.5.9 through 3.5.12. He changed current pattern by his own idea, which was very much different from the current pattern that F/P taught to NHE labo and NHE researchers have tried so. He has added heavy water by watching the liquid level of cell, while the NHE team added heavy water by constant period. Calculated excess heat varied with plus and minus values, so that the observation of clear excess heat can not be claimed. In the experiment M7C2, he boiled the cell at the end of experiment by increasing current to 1A. In this boiling phase, clear excess heat was not observed and feature of calculated excess heat values was as same as we experienced in the past. In the later phase of boiling it looked that the condensation tube tatched and gave fluctuating results.

Martin,
I am quite angry that my hard work at NHE is summarized by this incorrect paragraph.
Mel Miles

So, Jed, this report is not encouraging.
I send copy to Miles.
Sincerely,

Pd-B Experiment

Akito Takahashi
(????)
Professor
Department of Nuclear Engineering,

2000-01-06 #2

Melvin H. Miles, Ph.D.
807 Mamie Avenue
Ridgecrest, CA 93555

January 6, 2000

Dr. Asami,

I have received several reports from Martin Fleischmann discussing various methods of data analysis for the Fleischmann-Pons Icarus Systems. Although I agree that the starting differential equation is correct (but very complicated), there appears to be two major problems:

1. Determination of the correct heat transfer coefficient.

2. Determination of the water equivalent of the cell; C_PM.

These two very important parameters must be determined correctly in order to obtain reasonable calorimetric results. In the interest of science, I would like to publish a paper that clearly defines the correct procedures for determining these parameters. I propose, therefore, a joint publication of this matter including you and me along with Martin Fleischmann, K. Matsui, M. Sumi, and H. Ikegami. This publication would center on the experimental data from the Pd-B experiment that I conducted at NHE (see Fig. 10, p. 17 of my final NHE report).

Please let me know if you would like to be involved as an author on this joint publication. I hope by this joint effort that we can reach a consensus on correct methods for the analysis of the Fleischmann-Pons calorimetry.

Sincerely,

Dr. Melvin H. Miles

Copies: Dr. H. Ikegami, Mr. M. Sumi, Mr. K. Matsui

From: Naoto Asami <asami@iae.or.jp>
To: Melmiles <melmiles@ridgenet.net>
Sent: Friday, January 07, 2000 7:11 PM
Subject: Re: Joint Publication

Dear Miles-san

A happy new year!

How are you and your Linda-san? We are fine. But, Tomoko, my wife is now in Sapporo and I am in Tokyo.

From 28th Dec. to 3rd Jan., this period is Japanese usual holyday as you know, I returned to Sapporo during this holyday and enjoyed winter time of Hokkaido.

Thank you for your e-mail and your kind proposal of joint publication. However, I think it is not necessary to involve my name as a co-author in your publication, because my contribution is nothing to make your report. So, I would like to make a counter propose as follows;

1) To refer NHE reports which had been presented in ICCF-6 to ICCF-8.

2) To express our NHE contribution in an acknowledgement.

I already transferred your e-mail to Prof. Ikegami as your request.

Please remember me to your Linda-san.

Sincerely yours,

N. Asami

2000-01-07

[JR This letter refers to a plan by Fleischmann and me to buy some Johnson Matthey Type A palladium and offer it to any interested cold fusion researcher. No researcher expressed interest. See "Type A Palladium" in the Notes on Text section at the beginning of this document.]

Bury Lodge heading

Dr. Jed Rothwell,
Infinite Energy Magazine,
1954 Airport Road, Suite 204,
Chamblee, Georgia 30341
U.S.A.

7 January 2000

Dear Jed,

Happy New Millennium - although this is really 1 year ahead of schedule!

Many thanks for Sir Arthur C. Clarke's address. I will now write to him to thank him for his dedication. I think that I will also offer to answer any questions be may wish to pose to me, to discuss the strange history of the development (or rather the lack of development) of the subject thus far as well as the way(s) in which this topic fits into the wider question of the next phase of research in the Natural Sciences.

The question of further purchases of rods of J.M.. Material Type A has been quiescent since your visit. You may recall that at our meeting here in Tisbury I needed to stitch together a new consortium to purchase an ingot having a suitable specification and to arrange for this to be processed to produce materials suitable for various investigations. However, there were no takers and it seemed to me that you did not want to pursue the question any further.

If I update these costs, then I arrive at a figure of ~ $20K for the ingot and some minimal processing. You will be interested that I again visited J.M. on 22/12/99 to discuss the production of suitable materials. 1 did this because there is now some prospect of carrying out further research in a stable environment i.e. at .Frascati. The situation at J.M. with regard to supplying a wide range of materials suitable for a forward programme on Cold Fusion has now been complicated by a restructuring of parts of the Company. You may know that parts of the Company have been sold to Allied Signal where they now fly under the banner of "Honeywell". However. I established that it would certainly still be possible to purchase an ingot prepared under suitable conditions. The question of the costs is then dependent on how many different types of material might be required e.g. rods of different radii, sheets of different thicknesses, wires of different radii, targets for evaporation/sputtering. If there is a large diversity, then it

would be necessary to seek out a larger ingot (or several ingots), prepare the required materials and rely on J.M. to process the inevitable residues for other purposes.

The reason why I am outlining this to you is to point our that if you should want just a single rod, say of one of the standard dimensions which we used e.g. diameter = 0.2 cm, length = 1 cm, then the costs will depend on whether or not there are other requirements for such rods. We must also bear in mind that the price of Pd has increased considerably since we last discussed this question (mainly due to increased use of the metal in catalysts for the automotive industry coupled to the instability in Russia). My guess is that if you could "hitch a ride" on the backs of a larger order, then you should budget for $75 - $150. However, this is not a strategy which I would recommend because this material will soon be exhausted! If you should wish to place a larger order, say 10 rods of a single diameter, then the unit cost will be much lower. I am presently waiting for quotations from J.M. which will let me make a much more accurate estimate. However, let me urge you to come to a rapid decision: once I get the quotations from J.M., we will have to come to a rapid decision with regard to any order which we may wish to place.

In your FAX of 23/12.199 you also referred to Ed Storms current concerns. I must say that I find all this (the comparison of isoperibolic and flow calorimeters) very strange and, indeed, I am surprised how frequently and resolutely people get hold of the "wrong end of the stick". We have shown several times now that a **properly designed** isoperibolic calorimeter allows one to reduce the errors to ± 0.01% provided one uses a **properly designed** data processing system. The second proviso is very. important. You may know that I am presently reanalysing several data sets for "old" experiments which include those carried out by Mel Miles during his stay in the NHE Laboratories. As a precondition of these analyses, I sent Mel a report on 15/9/98 which outlined the raison d'être of the ICARUS-style analyses illustrated by the analyses of "blank" experiments (using the Pt-D20 system) as well as the analyses of data sets generated by simulations. This report showed that one can identify about 30 ways of analysing the data. The reason why this is only an approximate figure is because the exact number depends on the way one chooses to define the methodology.

It turns out that some of these methods can be rejected forthwith because they are not sufficiently accurate. Of the remainder some can be shown to be highly precise and/or accurate and, moreover, convenient to use. These are the methods which were incorporated into the ICARUS package. There is, however, one method which is unreliable/inaccurate unless the experiments and analyses are carried out under tightly controlled conditions. Unfortunately, this method was also included in the ICARUS package but with all the necessary instructions to ensure that the conditions for its application would be met.

I think that you will be able to guess what happened, The group at N.H.E. used only the unsatisfactory method of data analysis, changed the experimental conditions and analyses so that this method could not give satisfactory results, muddled up this particular methodology with some of the satisfactory methods (thereby also degrading the satisfactory methods) and then developed a further unsatisfactory method. They regarded this as being original although we had previously used it in 1992 to evaluate approximate rates of excess enthalpy generation. However,

we had shown that it is again necessary to place important restrictions on this method even if one only wants to evaluate such approximate rates.

It is important to realise that all of this had been spelled out to them not only in the ICARUS Handbooks but also in a series of reports dating back to July 1994 (actually these reports dealt inter cilia with the experiments which N.H.E. used later to illustrate their paper!).

Accident or design?

The reason why I am outlining all this to you is because I think it likely that Ed will have been influenced by the N.H.E. investigations so that he will probably be using their unsatisfactory methods of data analysis. It might be desirable, therefore, if Ed were to have sight of my report to Mel Miles of 15/9/98. I certainly would have no objections to this but, of course, the decision as to whether or not he should be able to do so rests with Mel. The only proviso which I would make is that I would like to vet this report before Ed might make further use of the contents. The reason is that the report may contain some uncomplimentary remarks about N.H.E. which I would not wish to place in the public domain!

An alternative would be for Ed to send me pans of his data sets. Time permitting, I would then reanalyse these sets and send him a confidential Report.

It might also be desirable for Ed to have sight of a later Report to Mel in which I have illustrated the influence of "positive feedback" on the calibration of one particular cell, various aspects of "Heat-after-Death" and the analysis of a data set as the cell is being driven towards boiling. The contents of most of this Report will certainly be published but, again, the decision as to whether or not the Report should be released to Ed would rest with Mel. In this case though, I would wish the contents of two of the Appendices to remain confidential to Mel at the present time. The reason why this particular Report may be important is because NHE (in common with other research groups) have consistently ignored the effects of "positive feedback". If this effect is present, then it becomes impossible to calibrate the isoperibolic calorimeters unless one takes special steps to take account of the effects,

I want to qualify now my comments on getting hold of the "wrong end of the stick." I believe that it is very important for research workers who embark on calorimetry (especially of the messy systems which are of our concern) to read some of the standard texts of Chemical Engineering and, especially perhaps, the classical work "Chemical Reaction Engineering" by Otto Levenspiel. This will show them immediately that there are only two "ideal" designs of systems: the "well-stirred tank" and the "ideal plug flow device". Isoperibolic calorimeters satisfy the criteria of the "well-stirred tank" and, in principle flow through packed or fluidised beds could satisfy the criteria of "ideal plug flow" i.e. the Patterson-style cell might fit into this category. However, in practice, such devices show all manner of side effects (e.g. entry effects) so that it becomes virtually impossible to ensure that the conditions of "ideal plug flow" are met. As I will illustrate below this also has an important bearing on the performance of flow calorimeters.

Isoperibolic calorimeters then are “ideal” where by “ideal” we imply that they satisfy the laws of Physics. The same considerations however show that flow calorimeters can never be “ideal”. In the first place, there must always be a change in temperature along the length of the cooling tube so that the contents of the calorimeter cannot be at a uniform temperature. Secondly, the “apparent heat recovery” of the systems is never 100%. What happens in practice is that research workers breathe a sigh of relief once they achieve say 98 - 99% thermal recovery and they then believe that this “apparent heat recovery” must actually he the “true heat recovery”. Unfortunately, this is not correct. The flow in the cooling tubes never satisfies the criteria of fully-developed turbulence (the Reynold’s Numbers are too low) nor of Poiseuille Flow (the Reynold’s Numbers are too high and/or the geometries are unsuitable). In practice, the only significance which we can attach to the change of temperature in the cooling fluid is that it indicates an “apparent heat recovery”. The question of whether or not this “apparent heat recovery” is a “true heat recovery” requires us to establish whether the device satisfies the laws of Physics e.g., that the “apparent heat recovery” is independent of the flow velocity of the cooling liquid. We made several attempts to develop a “First Law Calorimeter” but we always found that our devices did not satisfy the laws of Physics.

Of course, what might happen if we enclose an isoperibolic calorimeter inside a flow calorimeter is anybody’s guess. All that one can say for certain is that the performance of the isoperibolic calorimeter must be degraded. This seems to me to be a singularly pointless experiment. However, if you or Ed would like to send me a diagram of the apparatus, then I will be glad to send you my “first shot” at identifying the problem areas.

Regards,

[signed]

Martin

2000-01-10

[JR This is included in 2000-01-14. Perhaps the early dates in letter should read 1999.]

Bury Lodge heading

Dr. Melvin Miles,
Chemistry and Materials Division,
China Lake,
CA 93555-6100
U.S.A.

14 January 2000

Dear Mel,

The attached letter was written some time ago (before 10/1/2000) but has been held up for a variety of reasons. Since then I have had your FAX of 6/1/2000 and copies of your "Asti" papers (Proceedings and the Italian Physical Society publication) and the joint paper with Stan Szpak and Pamela Mosier-Boss. Jed Rothwell has also written to me on the same matter as that in your FAX of 6/1/2000.

The papers are all fine. I need to write to you at some length about the misrepresentations/mistaken analyses made by our Japanese colleagues and will do so this weekend. I think that they must have regarded you as a "Trojan Horse".

More anon,

Yours,

Martin

[Attached is a letter dated 10 January 2000]

Bury Lodge heading

10 January 2000

Dear Mel,

As you will get this letter after the New Year, let me start by wishing you a happy New Millennium!

You will have realised that my FAX of 21/12/1999 was by way of a reply to yours of 2/12/99 although it really covered some of the points you raised in your FAX of 22/12/99. However, it may be best if I comment further on the issues you have raised in this later FAX? As far as I am concerned, I don't mind in the least if the report I sent to you is published both by NRL and by San Diego. However, I do believe that you have to inform both Stan Szpak and Dr. Imam of the

situation which has arisen. I think that the proposal which I made to you and to Stan on 21/12/99 might be the best way to deal with the situation i.e. for Stan to publish an outline of my report to you of 15/9/98 setting the scene for the further analyses of your experiment on the Pd-B system. Stan's report could then refer to the fact that further detailed analysis of your experiment on the Pd-B system. Stan's report could then refer to the fact that further detailed analyses will be given as addenda to the report and that these addenda will include analyses of your experiment on the Pd-Ce system as well as of an experiment carried out by the N.H.E. Group. Furthermore, he could refer to the fact that a full report on the Pd-B system will be given by Dr. Imam. Of course, this procedure will involve rather more work than the preparation of a report on the Pd-B system alone but I will certainly be glad to help with a preparation of such a composite document (I think that Dr. Imam's task will be more straightforward). I believe that the course of action I have outlined will give us maximum flexibility – which we will certainly need! As I told you in my FAX of 21/12/99, I am suffering from time constraints. I will certainly bring my analyses of the N.G.E. experiment to some sort of definite conclusion but it is not clear at the present time when the material will be suitable for publication in a Navy report – it may prove to be too controversial! In that case, we could fall back on using some of the material contained in my poster in Vancouver. This would have the advantage that it would deal with one of the experiments used to illustrate the publication from N.H.E. (and would drag in the First and Second Reports to N.H.E.!). We also have to bear in mind that I may not be able to complete the analyses of your Pd-Ce experiment in the short term because of my time constraints.

All I can say at the present time is that I will leave you to deal with the situation – I will fall in with any decision which you, Stan and Dr. Imam may reach.

Next, let me confirm that I did indeed receive the M.S. Which you sent to Roger Parsons together with the various addenda. I think that it reads very well and that you made very convincing points. Also, that you had quite sympathetic treatment from the referees (one of whom was probably David Williams!). Of course, it did not come as a surprise to me: as I told you, the increase of cell temperature with decrease of enthalpy input was one of the fist pieces of evidence which showed us that there was excess enthalpy generation. I said something to that effect in a Newsletter which was circulated to the Electrochemistry Group of the Royal Society of Chemistry and published in an amended form in the Proceedings of the Second Conference on Cold Fusion (at the request of Tullio Bresani, Emilio Del Giudice and Giuliano Preparata). Of course, at the time, we had some experiments which showed no excess enthalpy generation and "blanks" using Pt-D_2O systems which were always in thermal balance. Perhaps we should have published such results?

I believe that one could say something further about your data when looking at these with the benefit of hindsight. The temperature of your "Cell B" was consistently below that required for the onset of "positive feedback" when using Pd electrodes. In consequence, one would expect this cell to show at most only low levels of excess enthalpy generation and the rates of such excess enthalpy generation to be relatively constant. On the other hand the temperature of your "Cell A" was consistently in the region of the onset of this "positive feedback". In this region one always sees the onset oscillatory phenomena including the onset of oscillations in the rates

of excess enthalpy generation. These oscillations may be regular or chaotic so that they must be due to the operation of at least one strange attractor. This is a feature of these systems which has not so far been investigated – apart from noticing the presence of such oscillations. [90] We have observed these oscillations in all the systems which we investigated (Pd, Pd-Ag, Pd-Ce, Pd-Rh) and they are especially marked for Pd-Rh. It appeared to us that maintenance of the conditions leading to the presence of prolonged oscillation always reduced the rates of excess enthalpy generation – and even destroyed the effect altogether. I believe it is for this reason that we never observed significant rates of excess enthalpy generation in the Pd-Rh system (although one could no doubt find conditions which would overcome the negative effects of the oscillations).

I also believe that some of the somewhat negative conclusions reached in investigations of excess enthalpy generation (even by groups reporting such excess enthalpy generation) are due to maintaining the operation conditions in the region of the onset of "positive feedback". This is true for example of the work of the Stanford-SRI group. They have struggled long and hard to relate the rates of excess enthalpy generation to the non-stationary behaviour of the systems. This is true, no doubt, because of the operation of some strange attractor(s) but I don't believe that one should seek to interpret the rates of excess enthalpy generation in this way. In our own work, we have simply increased the current densities at suitable times so as to increase the cell temperatures and to "drive" the systems through the regions of instability. [91]

It seems to me that the only group which has taken on board our comments on this topic is that of Giuliano Mengoli at CNR, Padua. The work of this group has indicated that the "performance envelope" is quite different below and above the onset of "positive feedback" - for one thing, there is no threshold current density above the onset of "positive feedback". It also seems to me that the condition of operation of their calorimeters have been chosen so as to ensure continued operation at temperatures above those required for the onset of "positive feedback". Of course, this has led to the demonstration of prolonged "Heat-after-Death" effects, a very interesting result. However, it has also meant that the accessible range of current densities was restricted so that this group has never achieved higher rates of excess enthalpy generation. I note that the systems shift from "primary" to "tertiary" current distribution which leads to high rates of excess enthalpy generation.

It may well be that we will wish to correspond further about some of these matters?

Many thanks also for the Abstracts of the papers which you have submitted to ICCF 8. These are fine (I have no comments at this time) and, as you have said, that on "Calorimetric Studies of Palladium Alloy Cathodes Using Fleischmann-Pons Dewar Type Cells" should serve as a lead in to a more detailed paper on Pd-B. I think that this should be a joint Miles-Fleischmann paper. After all, it is your experiment and I also believe that the participants must be heartily sick of hearing me at these meetings! You will gather from what I have said that I have not so far submitted an Abstract. After some discussions with Giuliano and Emilio, we decided that I should do so after my next visit to Frascati, sometime in January. As you will see,

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[90] MCHM This is very important.

[91] MCHM Martin & I had very different interpretations the role of instability (and, therefore, positive feedback). Ironically, his calorimetry was much better at preserving / capitalizing on such feedback.

I am using my privileged position! May I suggest that I should FAX you an Abstract from Frascati and, when we have agreed the working submit this there and then to the Organising Committee. Of course, they may then say that we are too late with the submission!

I fear also that I have not been able to prepare a paper for Bill Collis because of a total work overload. I was also influenced by Giuliano and Emilio who said that the papers from ICCF 8 would be a more important matter.

The only positive step I have made so far with regard to ICCF 8 is to accept a hotel booking at the Hotel Florida made for me by Signora Patrizia Pasilini. I'm sure that this was made at the instigation of Giuliano or Emilio and I have been too indolent to check up! I believe that the Hotel Florida is close the Conference Venue so this will fit my decrepit state.

I look forward to having your Asti report. With regard to the penultimate paragraph of your letter of 22/12/99: I shall await developments. If there is no follow up, then perhaps we should have a further up conversation at ICCF-8? At the very least I believe that it would be useful for you to know the various strands which led me to investigate this topic. Some of these are well-known abut then these do not add up to the whole story.

I am sure that there are some other important matters I should write to you about. If so, then perhaps you could jog my memory?

Regards,

Martin

P.S. I will get the book "The Quantum Vacuum – An Introduction to Quantum Electrodynamics" and think about the problem which you have alluded to. However, my guess is that my own ideas about the investigation of the effects of Quantum Fluctuations are quite different to those bandied around by other people.

P.P.S. Many thanks also for your kind comment to Roger Parsons about my activities regarding the analyses of data. I think that your assessment is perhaps a little "over the top". There are many people who could analyse data but they don't do so because this activity is so undervalued. [92] Where I do perhaps differ from other people is that I try to take this activity as a starting point for simplifying the experiments. One of the most galling aspects of my involvement with this topic is that I have been unable to develop the ICARUS -14 System and, also, unable to initiate experiments aimed at relating the rate of excess enthalpy generation to the rate of loading. I will tell you about his last aspect when we meet at ICCF-8.

[92] MCHM ☺

2000-01-10

Bury Lodge heading

Dr. Jed Rothwell,
Infinite Energy Magazine,
1954 Airport Road, Suite 204,
Chamblee, Georgia 30341

10 January 2000

Dear Jed,

Since writing to you, I have received a copy of Takahashi's FAX to you of 6/1/2000 (via Mel Miles). Miles is very angry. All this makes me think that you should definitely ask Mel for a copy of my report of 17/9/99 although you should probably also ask Mel for that of 15/9/98. Parts of the report of 17/9199 will almost certainly appear as an Official Publication. It may well be that we should set up a small discussion group to deal with the contents of these two reports?

If (and when?) you read the report of 17/9/997 you should bear in mind that I made mistakes about the form and volume of Mel's electrode. In consequence, there were some anomalies in the derived data. I suspect (but I haven't as yet checked this exactly) that these anomalies will disappear when one uses the correct form and volume.

You may recall that when you and Gene visited us here we touched on the question of "conspiracy theories". You made the point that it didn't really matter whether the events affecting C F. were due to "conspiracies" or "ineptitude" because the consequences were identical. This is certainly true but, if the course of action is due to "conspiracies", then one must also ask: "why were such "conspiracies" put in place?" The answers to this question may produce a profound difference between the effects of "conspiracies" and "ineptitude". (I believe that they do so for the case of CF.) I note also that if people continue to rubbish (trash) all evidence requiring Q.E.D. for the interpretation of the results of experiments, then progress in the Natural Sciences will be halted.

You may know that Asami gave me a C.D. at I.C.C.F.7 containing data sets for 7 experiments carried out in Japan. One of these is especially interesting because the cell was driven to the boiling point. I believe that Asami did this to stop me presenting my poster which contained my analysis of one of the experiments N.H.E. used to illustrate their paper. None of these data sets correspond to the ones I had repeatedly requested (I used June 1994 as a cut-off point).

I am presently analysing the interesting experiment on Asami's disk. As I said in a letter to Mel: the results are disturbing or profoundly disturbing depending on one's point of view

(disturbing = ineptitude; profoundly disturbing = conspiracies). If one accepts “conspiracies”, then one must conclude that the experiments were falsified.

I had hoped to start the Millennium on an happier note.

Regards,

[signed]

Martin

2000-01-14

Bury Lodge heading

Dr. Jed Rothwell,
Infinite Energy Magazine
1954 Airport Road Suite 304,
Chamblee,
Georgia 30341
U.S.A..

14 January 2000

Dear Jed,

Thank you for your FAX of yesterday and the attached translation of the relevant sections of the N.H.E. report. I am flow sending you a letter dated 10/1 /2000 although it was actually written before that date, You will see that this letter is rather relevant to the questions posed in your letter. A letter from me to Mel of 19/11/99 is also highly relevant although the text of this letter needs to be sanitized (see further below)

There are really two separate issues: (i) what should one say about the NHE experiments?; (ii) what should one say about the experiments carried out by Mel in the N.H.E. laboratories?

As far as the first point is concerned, there is really very little which. one can say in the absence of the "raw data". Surely the first correct step would have to be the release of the relevant data sets and documentation so that A. N. Other(s) can study these to see whether the conclusions drawn by our Japanese colleagues are correct/ambiguous/incorrect? As N.H.E. state that they have carried out detailed evaluations, these data sets must be available. Should you not now request the release of the relevant information? This information should contain a listing of the batch numbers of the Pd electrodes for reasons which I will explain below.

My comments on the NHE experiments are therefore necessarily of a very general nature. I have repeatedly requested the raw data for several experiments for which preliminary analyses were given in Reports dated June 1994 and December 1994. I have never been given these raw data nor data for any "blank" experiment. I therefore do not know whether any such "blank" experiment was ever carried out - if they had been it would allow one to establish whether the equipment was working satisfactorily as well as the precision and accuracy of the instrumentation

As far as the analyses of the experiments are concerned, I would draw your attention first to the "sanitized extract" of my letter to Mel of 19/11/99. In developing ICARUS 1 we were much concerned to focus on simple methods of data analysis: **simple** in the sense that they only used linear regression procedures (not multi linear or non-linear regression) and avoided the use of weighting functions. A consequence of this strategy was that there were a very large number

of possible methodologies. Most could be excluded as being imprecise and/or inaccurate. Of the remainder, one could be shown to be unreliable and a further method was only conditionally reliable: conditionally in the sense that this method required that the ICARUS protocols had to be strictly adhered to, in particular, measurement cycles lasting 2 days. You can guess what happened: N.H.E. used the unreliable method and compounded this by inventing an invalid method of their own and using 1-day measurement cycles.

As far as the Pd electrode materials are concerned, I do not believe that with the exception of the initial experiments which we set up for them in December 1993, they ever used Johnson Matthey Material Type A. We only had a very limited supply of electrodes in December 1993, sufficient to give them 3 electrodes. Two of these generated excess heat, one experiment had a self-evident fault. This was all pointed out to their in June 1994. The faulty experiment was one of the two results quoted in their ICCF-5 paper (there is much much more to this story as I will tell you when we next meet). There is evidence that the other two cells were contaminated by light water round about day 16. (strange?).

I believe that the electrodes which they describe as being of the J.M. type were actually drawn from a stock made following requests from the Stanford-SRI group. This required the preparation of a large batch of material which was not expected to give electrodes generating excess beat. We could only afford to buy this batch if everybody "pitched in". The whole idea was to explore the modifications which would have to be carried out to produce satisfactory materials. However, in view of shortage of funds, this programme was never carried out. It is for this reason that it is essential to establish the batch numbers of the NHE electrodes.

As far as the overfilling of the cells is concerned, it has been repeatedly established that the D_2O consumed is given by Faraday's laws of electrolysis and that the refilling can be carried out to ±1%. Mel's results in Japan are again in line with this. However, as far as I can tell: the group at N.H.E. always added D_2O in excess of this volume. This led to the overfilling of the cells which can be easily detected from the time-dependence of the "lower-bound heat transfer coefficient". Actually, overfilling of the cells can be predicted for one day of the experiment which Mel carried out in Japan and which I have analysed: the "lower-bound beat transfer coefficient" shows the expected anomaly.

I do not know the details of any automatic refilling equipment N.H.E. may have devised

I will now deal with Mel's experiment (ii) which I believe will illustrate more fully some of the points I have made with respect to (i). As a matter of fact, I believe that all of this is covered pretty comprehensively by the sanitized sections of my letter to Mel of 19/11/99. This reads as follows:

[EDITED VERSION OF LETTER TO MILES FOLLOWS.]

The main reason why I am writing to you today is to urge you to look at the background of the C.F. saga with more skepticism than you usually show. I will illustrate this by referring to the results in the Report which I sent to you in September

I am attaching a copy of Fig. A. 10 of the Report on which I have noted the correct value of the true heat transfer coefficient (as substantiated also by the results in Fig. A.20 and Fig. A.21). Now one can ask: how can it be that the scientists at NHE settled on a value of the true heat transfer coefficient which is less than any observed value of the lower bound heat transfer coefficient? One must bear in mind here that they had been shown several times that such a conclusion is impossible and they had also been shown how to evaluate the results correctly. Furthermore, one should bear in mind that there are 30 ways of evaluating the results only one of which, $(k_R')_{362}$ gives highly error prone and, unusually incorrect results. The related coefficient $(k_R')_{32}$ gives reasonable results if the ICARUS protocols are adhered to - which they were not. Now which methodology did they use? We do not know because they haven't told us but, as far as I can tell, it was that leading to $(k_R'0)_{369}$!

One must now ask oneself: what precisely does this reveal about their intentions and how should one interpret their actions?

When I gave the Seminar in Frascati I did not initially use slides prepared from Figs, A 18 and A. 19. However, Paolo Tripodi then said that one must differentiate between the actions of N.H.E. and Mitsubishi. He implied that the group at NHE used a value of the heat transfer coefficient which would make the excess enthalpies straddle the zero line i.e. as in Fig. A. 18, leading to the conclusion that the methodology was inaccurate and that there is zero excess enthalpy generation. However, the true behaviour is as in Fig. A. 19. One would need an error in the cell temperature of 5-6K to explain away the negative excess enthalpies. This is beyond the bounds of possibility especially as the two temperature readings in the cell agree with each other.

At that stage, I showed slides of the two figures.

It is relevant to this saga that our Japanese Colleagues have so far refined to give me any data for "blank" experiments and all the data I had for other experiments were removed from the files sent back to me from Sophia Antipolis. At least, I interpret their actions as a refusal because they simply do not respond to my requests for data. I interpret this refusal as being due to the fact that if I had such data, I could then show that the instrumentation was working perfectly. The paper which they published in the Monte Carlo proceedings was based on two experiments, the first of which was subject to a fault which we could not identify (but probably due to a bad connection or dry joint) and the second was totally corrupted by noise. This was all spelled out to them in the First and Second Reports which we sent to them in 1994. However, in spite of the high noise levels, I could show that they did, in fact observe excess enthalpy generation in the second experiment. (the Poster I gave in Vancouver). You can find more about the strange background to these events in our earlier correspondence.

What do you make of all this? Incidentally, the French have also so far refused to give me the data they obtained using J.M. Material Type A electrodes (which I got for them with great difficulty) using an ICARUS-I look alike.

You will recall that Asami gave me a CD. in Vancouver containing details of 7 experiments. I suspected (and still suspect) that these experiments had been selected to show zero excess enthalpy generation but, of course, they have shot themselves in the foot because I can show that

the equipment was working perfectly! Furthermore, there is excess enthalpy generation in one experiment as the system is driven to the boiling point. (I believe that they did not know how to analyse these portions of the experiments). I will need your help to get more information about the relevant air pressures from Sapporo Airport.

I think that we/I should analyse also your Pd-Ce experiment and, at that stage, we should invite Asami, Matsui (possibly also Sumi and Ikegami) to join us in writing a paper. If they decline, then we should go ahead on our own and, in this case, incorporate material from the Poster I gave in Vancouver.

[END OF LETTER TO MILES]

That's about it for the present. In your FAX you said that this material is to be incorporated in an article suitable for laymen. Would it not be sufficient/appropriate to say that the conclusions reached by the Japanese are based on a faulty analysis and to call on them to publish their raw data in toto?

However in confidence me tell you that I believe that the Japanese either decided that it was inadvisable for them to see any evidence for Cold Fusion, or else that they were leant on to reach such a decision. This is very much in line with the predictions which I made in 1989 for the evolution of this research topic.

Regards,

[signed]
Martin

P.S. After I finished this letter, I found that my FAX had jammed. Pages 4 and S of your FAX and the Abstract and page 4 of Mel's paper came through when I cleared the jam. However, I do not believe that these additional pages would need me to change any of my comments.

P. P. S. I think that we should also note that the N.H.E. group have consistently ignored the complications caused by "positive feedback" - see my FAX of 7/1/2000.

From Professor Martin Fleischmann, F.R.S.

Dr. Jed Rothwell,
Infinite Energy Magazine
1954 Airport Road Suite 304,
Chamblee,
Georgia 30341
U.S.A..

14 January 2000

Dear Jed,

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I am attaching a copy of Fig. A. 10 of the Report on which I have noted the correct value of the true heat transfer coefficient (as substantiated also by the results in Fig. A.20 and Fig. A.21). Now one can ask: how can it be that the scientists at NHE settled on a value of the true heat transfer coefficient which is less than any observed value of the lower bound heat transfer coefficient? One must bear in mind here that they had been shown several times that such a conclusion is impossible and they had also been shown how to evaluate the results correctly. Furthermore, one should bear in mind that there are 30 ways of evaluating the results only one of which, $(k_R')_{362}$ gives highly error prone and, unusually incorrect results. The related coefficient $(k_R')_{32}$ gives reasonable results if the ICARUS protocols are adhered to - which they were not. Now which methodology did they use? We do not know because they haven't told us but, as far as I can tell, it was that leading to $(k_R'0)_{369}$!

One must now ask oneself: what precisely does this reveal about their intentions and how should one interpret their actions?

When I gave the Seminar in Frascati I did not initially use slides prepared from Figs, A 18 and A. 19. However, Paolo Tripodi then said that one must differentiate between the actions of N.H.E. and Mitsubishi. He implied that the group at NHE used a value of the heat transfer coefficient which would make the excess enthalpies straddle the zero line i.e. as in Fig. A. 18, leading to the conclusion that the methodology was inaccurate and that there is zero excess enthalpy generation. However, the true behaviour is as in Fig. A. 19. One would need an error in the cell temperature of 5-6K to explain away the negative excess enthalpies. This is beyond the bounds of possibility especially as the two temperature readings in the cell agree with each other.

At that stage, I showed slides of the two figures.

It is relevant to this saga that our Japanese Colleagues have so far refined to give me any data for "blank" experiments and all the data I had for other experiments were removed from the files sent back to me from Sophia Antipolis. At least, I interpret their actions as a refusal because they simply do not respond to my requests for data. I interpret this refusal as being due to the fact that if I had such data, I could then show that the instrumentation was working perfectly. The paper which they published in the Monte Carlo proceedings was based on two experiments, the first of which was subject to a fault which we could not identify (but probably due to a bad connection or dry joint) and the second was totally corrupted by noise. This was all spelled out to them in the First and Second Reports which we sent to them in 1994. However, in spite of the high noise levels, I could show that they did, in fact observe excess enthalpy generation in the second experiment. (the Poster I gave in Vancouver). You can find more about the strange background to these events in our earlier correspondence.

What do you make of all this? Incidentally, the French have also so far refused to give me the data they obtained using J.M. Material Type A electrodes (which I got for them with great difficulty) using an ICARUS-I look alike.

You will recall that Asami gave me a CD. in Vancouver containing details of 7 experiments. I suspected (and still suspect) that these experiments had been selected to show zero excess enthalpy generation but, of course, they have shot themselves in the foot because I can show that the equipment was working perfectly! Furthermore, there is excess enthalpy generation in one experiment as the system is driven to the boiling point. (I believe that they did not know how to analyse these portions of the experiments). I will need your help to get more information about the relevant air pressures from Sapporo Airport.

I think that we/I should analyse also your Pd-Ce experiment and, at that stage, we should invite Asami, Matsui (possibly also Sumi and Ikegami) to join us in writing a paper. If they decline, then we should go ahead on our own and, in this case, incorporate material from the Poster I gave in Vancouver.

[END OF LETTER TO MILES]

That's about it for the present. In your FAX you said that this material is to be incorporated in an article suitable for laymen. Would it not be sufficient/appropriate to say that the conclusions reached by the Japanese are based on a faulty analysis and to call on them to publish their raw data in toto?

However in confidence me tell you that I believe that the Japanese either decided that it was inadvisable for them to see any evidence for Cold Fusion, or else that they were leant on to reach such a decision. This is very much in line with the predictions which I made in 1989 for the evolution of this research topic.

Regards,

[signed]

Martin

P.S. After I finished this letter, I found that my FAX had jammed. Pages 4 and S of your FAX and the Abstract and page 4 of Mel's paper came through when I cleared the jam. However, I do not believe that these additional pages would need me to change any of my comments.

P. P. S. I think that we should also note that the N.H.E. group have consistently ignored the complications caused by "positive feedback" - see my FAX of 7/1/2000.

2000-02-02

This is an introduction to the correspondence with Kennel in the next section.

Melvin H. Miles, Ph.D.
Chemistry and Materials Branch
Research and Technology Division
Code 4T4220D
Naval Air Warfare Center Weapons Division
China Lake, CA 93555-6100 USA

FAX MEMO

DATE: February 2, 2000

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I am sending you an e-mail from Elliot Kennel and my response. A few weeks ago Dr. Asami declined my offer to be a co-author on the Pd-B paper. I also contacted Mr. Sumi and Mr. Matsui, but they have not responded. Therefore, we should go ahead without any of them as authors.

I have been extremely busy preparing a proposal for ONR on supercapacitors. Do you have any suggestions for supercapacitor research? I need to answer your last fax in detail and will try to do this later this week. I have had very little time for cold fusion matters during the past month.

Best wishes,

Mel Miles

Mel Miles

P.S. Dr. M. A. Imam would like to be included as an author on the Pd-B paper. He made the Pd-B material. Is this O.K? He is with the Naval Research Laboratory, Washington D.C. 20375.

P.P.S. On a lighter note, today is 2/02/2000 – the first date with all even digits since 8/28/888 – a long time ago!

2000-02-03

(The e-mails below are shown in the order they were sent, rather than the order they were listed in original document.)

From: Elliot B. Kennel <EKennel@compuserve.com>
To: Miles, Mel <melmiles@riclgecrest.ca.us>
Sent: Tuesday, February 01, 2000 2:53 PM
Subject: NHE Lab

Hello Mel,

Recently I've become aware that Jed Rothwell is planning to write an article concerning the activities at NHE Lab. According to the information I've been sent from Matsui-san and Asami-san, it seems that Jed will probably allege that there was unethical behavior at the lab and suppression of data, including your experiments. [93]

Matsui-san and Asami-san have indicated to me that they don't care what Jed writes, but they do care about your opinion.

It turns out that I am going to Japan next week, and will see Matsui and Asami. If there are some specific questions or concerns that > you feel should be addressed, I am willing to act as an intermediary if you think it would be helpful.

Yours truly,

Elliot

[93] JR Apparently, Kennel, Matsui and Asami refer here to the fact that I translated and circulated their official report. This report came as an unpleasant surprise to Miles, and it upset Miles and Fleischmann, for the reasons described in the Introduction and below.

From: Melmiles <melmiles@ridgenet.net>
To: Elliot B. Kennel <EKennel@compuserve.com>
Cc: Jed Rothwell <jedrothwell@infinite-energy.com>; Dr. Asami <asami@iae.or.jp>; Paolo Tripodi <paolo.tripodi@lnf.infn.it>; <michael.mckubre@sri.com>; Mr. Sumi sumi@trdc.mhi.co.jp
Sent: Tuesday, February 01, 2000 7:21 PM
Subject: Re: NHE Lab

Dear Elliot,

First, let me make it very clear that I am intensely interested and concerned about scientific truth as it relates to cold fusion research. However, I have very little regard or interest in any politics relating to this field. I am a Research Scientist not a Politician! Because of anti-cold fusion politics, I was assigned to report to the stockroom at China Lake, do chemical inventory, and clean out buildings when the Office of Naval Research (ONR) suddenly terminated my cold fusion funding in 1995. Because of my working on cold fusion in Japan in 1997-1998, another funding officer of ONR stopped communicating with me, terminated funding in battery research, and stated my cold fusion work destroyed my credibility as a scientist. Regardless, of what anyone does to me, I will continue to support the scientific truths relating to cold fusion.

My studies at the NHE laboratory produced excess heat in 8 out of 10 experiments - excluding two controls. This is the highest success rate that I have experienced in my cold fusion studies. Excess heat in three co-deposition experiments, using F/P cells at NHE are reported in Fusion Technology, Vol. 25, pp. 478-486, 1999. My China Lake calorimentry used at NHE also recorded excess heat. My manuscript on this has been recently accepted by an international scientific journal.

A joint paper with Martin Fleischmann on the Pd-B rod used at NHE will also report an excess heat effect. There will also be additional papers in scientific journals that will report on my excess heat effects at NHE.

Jed Rothwell has informed me that the Japanese version of the final NHE report does not credit me with any excess heat effects in my experiments. Naturally, this has upset me. Once again it appears that politics are trying to erase scientific truths relating to cold fusion. This I cannot and will not support.

Sincerely,

Dr. Melvin H. Miles

From: Kazuaki Matsui <mac@iae.orjp>
To: Melmiles <melmiles@ridgenet.net>
Cc: <ekennel@apsci.com>; <asami@iae.or.jp>; <h-kamimu©erl.hitachi.co.jp>
Sent: Thursday, February 03, 2000 5:59 PM
Subject: RE: e-mail from Elliot

Dear Dr. Miles,

I am a little bit surprised in the things going on. First of all, we, Japanese side, appreciated your participation to our project in Sapporo, NHE, even though there were some disagreement in understanding and or observation of your experiment. But this is not unusual at all at least to me. I remember that you have reported your results to the committee of which members included Prof. Akito Takahashi, Prof. Ikegami, Prof Ohta and others, with discussion. The reports were drafted by us and reviewed by the same committee. No politics at all. But Dr. Asami and myself plan to see how the reports described, probably in this month for our understanding. I understand and sympathize how you feel to your circumstances, but I believe that it is not necessary to turn your back to your friends because of your grief and anger. It sounds like that someone wants to stir up and create conflicts by making noises. Mr. Kennel happens to come over to Japan for his holiday in middle of February as you know, and we will discuss possible better action by us, but including no action at all.

Yours sincerely,

****

Kazuaki MATSUI mac@iae.or.jp
Director, Research and Development Division
The Institute of Applied Energy
Shinbashi SY Bldg.
14-2 Nishishinbashi 1-Chome,
Minato-ku, Tokyo, 105-0003, JAPAN
Tel: +81-3-3508-8894 Fax: +81-3-3501-1735

--- Original Message

From: Melmiles [mailto:melmiles@ridgenet.net]

Sent: Wednesday, February 02, 2000 12:25 PM

To: Mr. Matsui

Subject: e-mail from Elliot

Mr. Matsui:

Please note my reply to Elliot's e-mail - Mel

Dear Elliot,

First, let me make it very clear that I am intensely interested and concerned about scientific truth as it relates to cold fusion research. However, I have very little regard or interest in any politics relating to this field. . . .

[The rest of this message is above]

[JR Here is the translation of the N.H.E. report that triggered this discussion and the comments by Kennel. See the Introduction.]

Section 3.5 from:

新水素エンエルギー実証技術開発 Shinsuiso enerugii jissyou gijyutsu kaihatsu (New Hydrogen Energy Verification, Engineering and Development), NEDO-NHE-9701 (June, 1998), p. 120

Translation and footnotes by Jed Rothwell. The authors of this report are not listed on the title page. Presumably they included N. Asami and other project leaders.

3.5 Open cell electrolysis excess heat verification experiments

3.5.1 Summary

Starting in 1995 we began a series of tests with an ICARUS-2 open cell electrolysis system acquired from Fleischmann and Pons, however we were unable to replicate excess heat with this system. It had been anticipated that when the ICARUS-1, which only functions up to 70 deg C, was upgraded to an ICARUS-2 which allows operation at higher temperatures in boiling regime, the high temperatures would promote excess heat generation. Moreover, since we did not observe excess heat with the palladium supplied by them, we tried the palladium that was used to produce excess heat in the I/J [Imra Japan] cell, which may be considered a standard, but it too failed to produce excess heat.

Dr. Miles came to the NHE as a guest researcher from United States, bringing cathodes which he reportedly generated excess heat in previous experiments. He installed the cathodes in the ICARUS-2 calorimeter. Both his results and the NHE ICARUS-2 tests are described below.

3.5.2 experimental results

The external conditions and results are shown in table 3.5-1.

1. F/P experiment

After the project began, we reached a stage at which over 100 runs were conducted without replicating excess heat. We decided to start from scratch and perform experiments with Johnson Matthey palladium.

Three experimental runs were performed, but all three cases, as in previous tests, excess heat was not replicated. Sample results are shown in figs. 3.5-1, and 3.5-2. After maintaining a 200 mA current for a period of one week, current was increased 2500 mA for approximately one month, and then the experiment terminated with a boil of intent. Calibration performed with a heater proved to be highly replicable, however when current was increased 250 mA, the excess heat computations showed a shift to the negative side. Based on past experience, we believe this was caused by heat losses from the power leads going into the cell. [1] In the analysis of the boil-off event, when we took into account evaporative losses, a peak value seem to indicate excess

heat, but this was only caused by an overflow, [2] and the actual signal fluctuated around the zero line. However, after amperage was increased to 500 mA the cell rapidly reached the boiling point, in comparatively much less time than the previous low-power stage of the experiment. Results indicate that during the long, low-power electrolysis phase, impurities accumulated on the cathode surface. Furthermore, in experiment 7121, the condenser came in contact with the collection cell used to weigh the condensate, which caused large instabilities.

3. Miles' experiment

Our guest researcher Dr. Miles performed an experiment in the NHE laboratory using an ICARUS-2 calorimeter [supplied by Fleischmann and Pons]. He installed a cathode which he claims previously generated excess heat.

Experimental results are shown in Figs.3.5.9 through 3.5.12. [3] Dr. Miles altered the input current patterns to fit his own ideas about how the experiment should be done, in a complete departure from the protocols which were recommended by Fleischmann and Pons, and which were used in all previous experiments performed by NHE personnel. He has added heavy water by observing the water level of cell, while the NHE team added heavy water constantly and automatically. [4] Calculated excess heat fluctuated between positive and negative values, and the overall data set does not constitute clear evidence of excess heat. In the last phase of experiment M7c2, boiling was induced by raising the current to 1 A. In the boiling phase, no clear sign of excess heat was observed; the heat profile was the same as we saw in previous boil-off tests. Midway through the boil-off test, large temperature fluctuations occurred, perhaps because the condensation tube came in contact with condensate collection vessel. [5]

3.5.3 Conclusions

We performed 100 runs which should have replicated the open cell electrolysis method published by F&P. Finally, as a last step, we performed experiments using the Johnson Matthey palladium supplied by F&P, [6] and based on the comments they made to us, we conducted long-term experiments in which electrolysis continued for a half-year, [7] as well as experiments using cathode materials which previously produce excess heat.

In both of these tests, as in all previous experiments, excess heat was not observed.

<u>Translator's notes</u>

1. They say this was caused by heat losses from the power leads, but I suspect it may be caused by their methods of selecting the zero-point and the size of the error-bars, described below.

2. The English word "overflow" is used here. Based on the NHE claims made during ICCF conferences and in an interview published in the Nikkei, I surmise this means losses due to unboiled fluid leaving the cell in droplets of froth.

3. These graphs bear no resemblance to the graphs shown by Miles at the American Chemical Society or in his Asti paper. Miles is convinced that his cells did produce heat; the NHE authors of this report apparently disagree. He presented his data to the NHE managers along with a comprehensive report, but they did not mention his conclusions in the final report. During the weeks the cells were producing excess heat, Miles invited Project Manager Asami to come and have a look, but Asami politely refused.

4. Fleischmann says that based on an analysis of the calorimetric data, he discovered that NHE staff members always overestimated heavy water consumption and overfilled the cell with their automatic refilling machine, which I gather works something like an intravenous pump. When refilling a cell with any method you must keep track of the actual waterline. McKubre comments:

> . . . I had never focused before on the top-up pump used at NHE. We have used these extensively. For one reason or other, they always go wrong, either over or under watering the cells. We never got any satisfactory results in this mode of operation. If ALL of the NHE F/P experiments were performed in this way, and NONE of Stan and Martin's were, and a contaminant leached from the pump poisoned the cathode.....

[Private communication, Jan. 2000]

5. The Japanese text says only, "the condenser came in contact." It does not say what it came in contact with. In the paragraph above describing experiment No. 7121, they state explicitly that the condenser came in contact with the condensate collection vessel. I assume they experienced the same problem again when testing Miles' cathode.

6. By any reasonable standard, this should have been the very first test, not test No. 101. In the first 100 tests, apparently they used palladium from Japanese sources.

7. Running a cold fusion experiment for six months without results is a preposterous thing to do. I very doubt that Fleischmann and Pons recommended this course of action.

Comments by Jed Rothwell

The conclusions reached in this report are the opposite the ones Miles described in his report the NHE and in his ACS and Asti presentations. Miles did not observe any significant negative heat excursions. Miles explains that they used their own method of data evaluation which they mistakenly believe is based upon Fleischmann and Pons method. Miles knew that the NHE scientists disagreed with his analysis, and they found no heat according to their method:

> The fact that the alternative NHE methods showed no excess heat for F-P cells illustrates the problem in transferring calorimetric methods from one laboratory to another. The second laboratory often fails to follow directions and makes changes that compromise the calorimetry. [94,95]

Miles was unaware of the fact that they did not present his analysis or his point of view in the final report. He was upset by this.

The NHE calorimetry is supposedly based upon Fleischmann and Pons' method. That is what the authors of this report think, and they accuse Miles of departing from the established Fleischmann/Pons calorimetry and electrolysis protocol, but others who have examined the NHE methods think that their method is a departure. On the face of it, it seems to be quite different, for two main reasons: 1. They employ only one calibration pulse; 2. They assume there is no excess heat during this single pulse, even though other methods based on absolute standards sometimes show excess heat is already occurring when this pulse is made. In other words, they define the starting point, or the zero-point, by fiat, which is why they sometimes conclude that cells swallow up energy endothermically. They define an excess heat event as a three sigma departure from this zero-point.

The NHE method boils down to calibrating with a single heat pulse applied on the third day of the experiment. Fleischmann took them to task for this, to no avail. [96] He says they ignore the fact that the cell is already producing excess heat at this time. This can be seen in analyses that do not depend upon calibration, but which are based upon first-principle absolute methods instead. Fleischmann emphasizes two such methods: 1. After a heat pulse, the heat decay curve does not fit Newton's law of cooling. The cell does not cool fast enough; there must have been an extra, unaccounted-for source of energy stretching out the curve. 2. The cell temperature does not "relax" all the way back to the original base temperature where it started before the pulse, because cold fusion positive feedback was already occurring.

Miles published two graphs showing the different outcomes from the two methods of calibration. The first one shows that because they calibrated on Day 3 after the heat began, the N.H.E.

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[94] JR Miles, M., *Report on Calorimetric Studies at the NHE Laboratory in Sapporo, Japan.* Infinite Energy, 2000. **5**(30): p. 22. http://lenr-canr.org/acrobat/MilesMreportonca.pdf

[95] JR See also: Miles, M., *NEDO Final Report - Electrochemical Calorimetric Studies Of Palladium And Palladium Alloys In Heavy Water* 2004: University of La Verne, http://lenr-canr.org/acrobat/MilesMnedofinalr.pdf

[96] JR Fleischmann, M. *Cold Fusion; Past, Present & Future. in The Seventh International Conference on Cold Fusion*. 1998. Vancouver, Canada: ENECO, Inc., Salt Lake City, UT. http://lenr-canr.org/acrobat/Fleischmancoldfusion.pdf See p. 3, ". . . a result which contravenes the Second Law of Thermodynamics!"

concluded that the calorimeter was fluctuating above and below the zero line, which would be a very large error. Miles used the Fleischmann-Pons "lower bound" method to calculate the cell constant. When the excess power is small, this lower bound constant approaches the true constant. He assumed that all of the fluctuations the followed were genuine positive heat excursions.

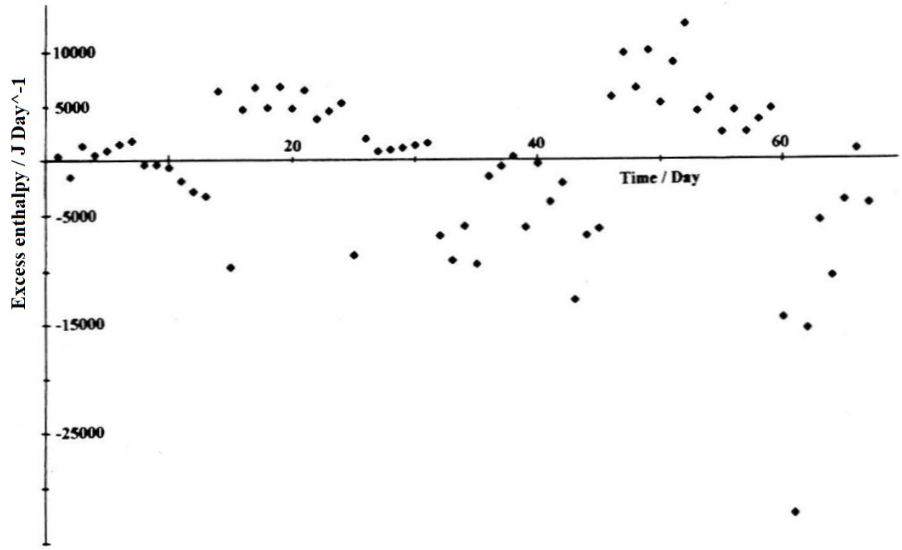

Fig. 20. Excess enthalpies using N.H.E. procedure

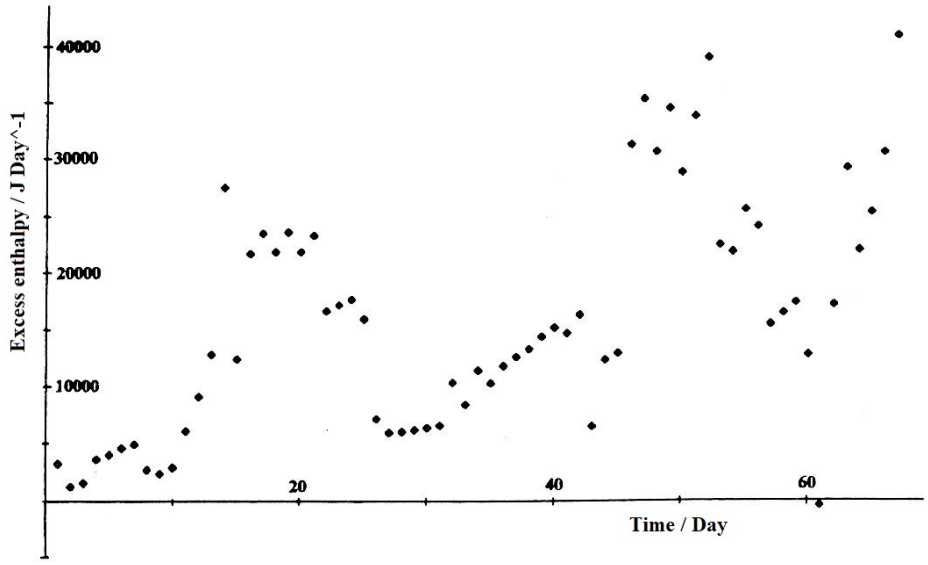

Fig. 21. Excess enthalpies using ICARUS procedure

Figures 20 and 21 from Miles, M., M.A. Imam, and M. Fleischmann. *"Case Studies" of Two Experiments Carried Out With the ICARUS Systems*. in *8th International Conference on Cold Fusion*. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy. These show the

effects of calibrating on Day 3 (top) with the assumption there is no heat at that time, versus calibrating on Day 1 (bottom)

McKubre described the arbitrary zero-point calibration:

> The Japanese retro-analysis method . . . processes all the data retrospectively, and assigns the mean as zero (i.e. net excess energy = 0). Variations, even known systematic variations, are considered as uncertainty (or "error"). Nothing counts unless it is more than 3 times this uncertainty value. This is what physicists do in stochastic system analysis, and chemical engineers when they have no knowledge of experiment details and no absolute calibration. For our experiments and F/P experiments (whether performed by Mel Miles or not) it is just WRONG!!!

[Private communication, Jan. 2000]

"Known systematic variations" would be, for example, complications introduced by changing water levels in the cell. This could be accounted for by making the formulas more complex and adding a term for the water level, but the NHE workers chose to keep the equations simple and to fold all minor sources of noise into one large estimate of uncertainty.

In 2018 Miles described this incident again:

. . . I liked the people at NHE and enjoyed this experience. However, I was very disappointed in how NHE misrepresented my NHE results and my Final Report where nearly every experiment produced excess heat. I think that my showing excess heat effects would not fit with their decision made before I arrived in October 1997 to close NHE at the end of March 1998.

I have the ICARUS manual, "The ICARUS SYSTEMS" written by F-P and published by Technova (1995). It is very detailed - well over 100 pages. I don't think NHE followed it at all. I read sections and found it helpful, but, like NHE, I used my own methods. After I returned in 1998, Martin had me copy it and send the copy to him. Although the main author, Fleischmann no longer had this manual.

The NHE main error was basing the entire experiment on a calibration done on the third day. My Pd-B produced excess heat by the second day, thus NHE based this entire experiment on a cell constant which was much too low. The cell constant was even lower than the constant predicted by the Stefan- Boltzmann constant assuming heat transfer only by radiation. Therefore real excess heat is shown only as fluctuations, positive and negative, around a zero value. The NHE cell constants were even lower for my three co-deposition experiments which produced even more excess power by the third day. I was using the same F-P Dewar cells which should of had about the same cell constants.

My method for finding the cell constant was to use the F-P "Lower- Bound" method to calculate the cell constant assuming no excess power. This cell constant will approach the true value on days where the excess power is small. My constant came much closer to the correct value than

did the NHE value. This all reminds me of Coolescence and Rick Cantwell using a smaller cell constant for my ICCF-20 paper that, as always, will zero out any excess power. Nate Lewis of CalTech was the first to zero out C/F excess power by changing the cell constant in his 1989 paper.

Martin was very unhappy with the NHE paper from ICCF-5 (Monte Carlo, 1995) about the F-P calorimetry. In a very long letter to me, he dissected it almost line-by-line to point out the many errors.

[E-mail, Jan. 2018]

2002-02-15

FAX

DATE: February 15, 2000

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I stayed home from work last week due to the flu but I felt well enough to work on your Pd-B report. I have made a number of changes and sent this on to Dr. Imam at NRL. It would be very helpful for him if he could get this report on a diskette as you mentioned in your letter of 21 December 1999. Did you ever mail this disk? If so I never received it. I have informed Stan Spzak and Dr. Imam about the possible duplicate publications. This does not seem to be a problem for either of them. They will both probably make numerous changes in the manuscript. For example, Dr. Imam wants to change the title to "Analysis of a Heavy Water Electrolysis Experiment Using a Pd-B Alloy Cathode Prepared by NRL". All references to experiment MC21 will be deleted. In my discussions with him, he thinks that you should be the first author followed by myself and then Dr. Imam. I hope this is OK with you. I have also corrected for the shape of the Pd-B rod with dimensions 4.71 mm X 20.1 mm, Volume of electrode = 0.350 cm^3 and area of electrode = 3.15 cm^2.

I have also made it clear that the schedule of D_2O additions and changes in the cell current were completely my own decisions. NHE gave me full control of running the experiment. The application of heating pulse and its rather short time-span was built into their computer program hence I had no control over this. I have made changes in the text to make this clear.

I tried to control the D_2O levels by using the lower edge of the silvered region as a guide. Prior to the experiment I found that 82 cc of the D_2O - LiOD solution filled the cell to this mark. Thereafter, I could extract D_2O temporarily with a syringe to accurately determine the D_2O level in the cell. My notebook measurements of this indicate that the cell was not overfilled on day 61. I added 9.0 cc of D_2O on that day to fill the cell to 91 cc. Hence this was only 1 cc in excess. At the same time I increased the cell current from 0.900 A to 1.000 A. Thus, there was both a cooling effect due to the D_2O and a heating effect due to the increase in current. Therefore I am wondering if there could be some other explanation for Figure A.20 on page 144. The first three points cover the time-span when the cell was rapidly adjusting to the new conditions. The points that follow the line starting at about 15000 seconds are at about the same time as when the equilibrium was reestablished. However, the region of the calibration pulse seems to follow the line reasonably well.

According to my notebook records, there were two days when the cell was overfilled on purpose. This was on day 57 when 12.0 cc of D_2O were added to give a total volume of 94.0 cc. I did this because of the upcoming long weekend. The second overfilling of the cell was on day 64 when 15.0 cc of D_2O was added to give a cell volume of 97.0 cc. This was because of the weekend when I could not enter the lab, and the rather high cell current (0.804 A). The rest of the time I think the D_2O level was under fairly good control. However, I could have made a mistake but I don't think that was likely since I always performed the measurement of the actual level.

On page 8 you stated that the exact volumes of D_2O could not be established with certainty after day 62. My notebook records show the following:

	Date	**Added**	**Cell Volume**
Day 62	Feb. 4, 1998	8.0 cc	91.0 cc
Day 63	Feb. 5, 1998	8.0 cc	90.0 cc
Day 64	Feb. 6, 1998	15.0 cc	97.0 cc
Day 67	Feb. 9, 1998	13.0 cc	89.0 cc

I have some comments about Figure 21 on page 146. First, the corrected volume for the Pd-B electrode will have no effect on your horizontal line since the heat effect should give 0.031 W at 0.150 A regardless of the volume. My notebook data shows that gas evolution from the Pd-B electrodes was low until about three hours and forty minutes later (13,000 seconds). My notebook shows vigorous gas evolution from the Pd-B electrode at that time. This is about the time that a sharp drop in Q was observed. It appears to me that there was a baseline of excess power of about 0.027 W in addition to the excess power due to loading. This baseline of excess power apparently went away at about 130,000 seconds. Does this make any sense to you? It is certainly strange behavior to have actual excess power present during the loading process.

There appears to be a reappearing error due to the heat capacity of D_2O gas. I found this in the ICARUS Handbook, pg. 4-55. This heat capacity of D_2O vapor is given as 44.500 J/mol K. My NBS Tables give 34.27 J/mol K. On page 45 of Pd-B manuscript, this value is given again incorrectly. This term has only a small effect in the equation hence the error is slight.

On page 58 Equation B.19, I think the co-efficient should be $(k_R')_{31}$ furthermore, on page 93, fourth line from the bottom, the coefficients are averages so they need a line across the top – if I am understanding this correctly. Furthermore, the second term should be $(k_R')_{12}$.

Figure A-2 on page 108 needs adjusting to correct for the electrode area.

Figure A-22 on page 148 needs to be adjusted for the actual electrode volume – 50 W / cm^3 becomes 27 W/ cm^3. Back to page 34, my notebook shows 91.0 cc on Day 25 and 92.0 cc on Day 26 hence the cells were not overfilled according to my records.

I really enjoyed studying your report and found it very informative. It will certainly be great for future scientists interested in this field to have this information available as a Navy report. I think that about covers it. I'll look through the report one more time and add any further comments as a post-script, if necessary.

Best wishes,

Mel Miles

Mel Miles

P.S. Dr. Imam would like to attend ICCF-8 but would need his name added to the Pd-B paper to justify this trip to the Navy. Can we add his name to this paper. He was the one that prepared these Pd-B materials.

2000-02-17

Bury Lodge address

17 February 2000.

Dear Mel,

I shall shortly be off to Italy where I will activate some new courses of action and, hopefully, several more which have been pending for a long time! Ahead of my departure, I am having a mini-clear-up of my desk. My correspondence with you though is in much disarray for a variety of reasons which I will explain to you when we next meet. I will try to write to you from Italy or else from Austria where I will go in March for a few days skiing.

Ahead of that time, I have checked through my correspondence and have found that the disk which I was due to send to you (the text of the Report of 17.9.99) still resides here. I am now sending this to you post-haste, also a duplicate to Stan Szpak. Apologies!

Next, by all means let us take Dr. Imam as a co-author and could you please tell him that I hope to meet him at ICCF-8 and that we will be able to have three-way discussion. I will try to draft the paper while I am in Italy.

Many thanks for all the other comments in your FAX of 15.2.00 and for clearing up various inconsistencies in the Report. I realised that this needed some further action and am taking it with me to Italy together with your FAX just in order to see whether I can find some other points which need to be cleared up.

I am still working flat out on the analysis of the data for one of the N.H.E. Experiments contained on the disk Asami gave to me at ICCF 7. This used an electrode made by the IMRA Materials Laboratory and, based on our own experience with these materials, I did not expect that this experiment would show any significant excess enthalpy generation under normal conditions of operation. Thus it has turned out to be so that this experiment can substitute for a "blank". You will recall that N.H.E. never gave me the data for such "blanks" if, indeed, they ever carried out such experiments.

However, the analyses throw up some interesting facts. I have just written to Jed Rothwell about the execution of our own as well as the N.H.E. experiments which is best illustrated by the attached Figures. In the pre-ICARUS and ICARUS-1 phases, the experiments were wired up as in Fig 1 i.e. Using two 4-terminal connections. For a variety of reasons I wanted to use the cell current to also calibrate the systems so in 1994 we investigated the insertion of switching boxes as in Fig 2. this worked fine so long as the wiring was carried out with heavy gauge wire. However, in 1995 I became convinced that the ICARUS-2 systems had been wired up as in Fig 3 using the wires supplied for ICARUS 1. Somewhat later I found out that the system had switching boxes had been promptly eliminated!

I found that the resistances of the leads used in ICARUS-1 was in the range 0.5 to 1.5 Ohms. There are four separate inconsistencies in the data set given to me by Asami which are all removed if one assumes the inclusion of a resistance external to the cell of 0.8 Ohms. This being so, one would expect the "lower bound heat transfer coefficients" to have errors between 2.5-7.5% and all these coefficients to be larger than the "true heat transfer coefficients" by roughly the same extent.* All measurements with ICARUS-2 have therefore been compromised. However, I can cope with most of the induced errors!

Have you any comments?

However, I would expect that such "bad" electrodes would show some excess enthalpy generation as the system is being driven to high temperatures/high cell voltages and this is indeed the case for this particular experiment. There is quite significant excess enthalpy generation at the near-boiling condition and there is also Heat-after-Death in much the same way as in your Pd-B experiment.

What shall we do with all of this?

Regards,

Martin

**As has repeatedly been complained about by the group [?] at N.H.E.*

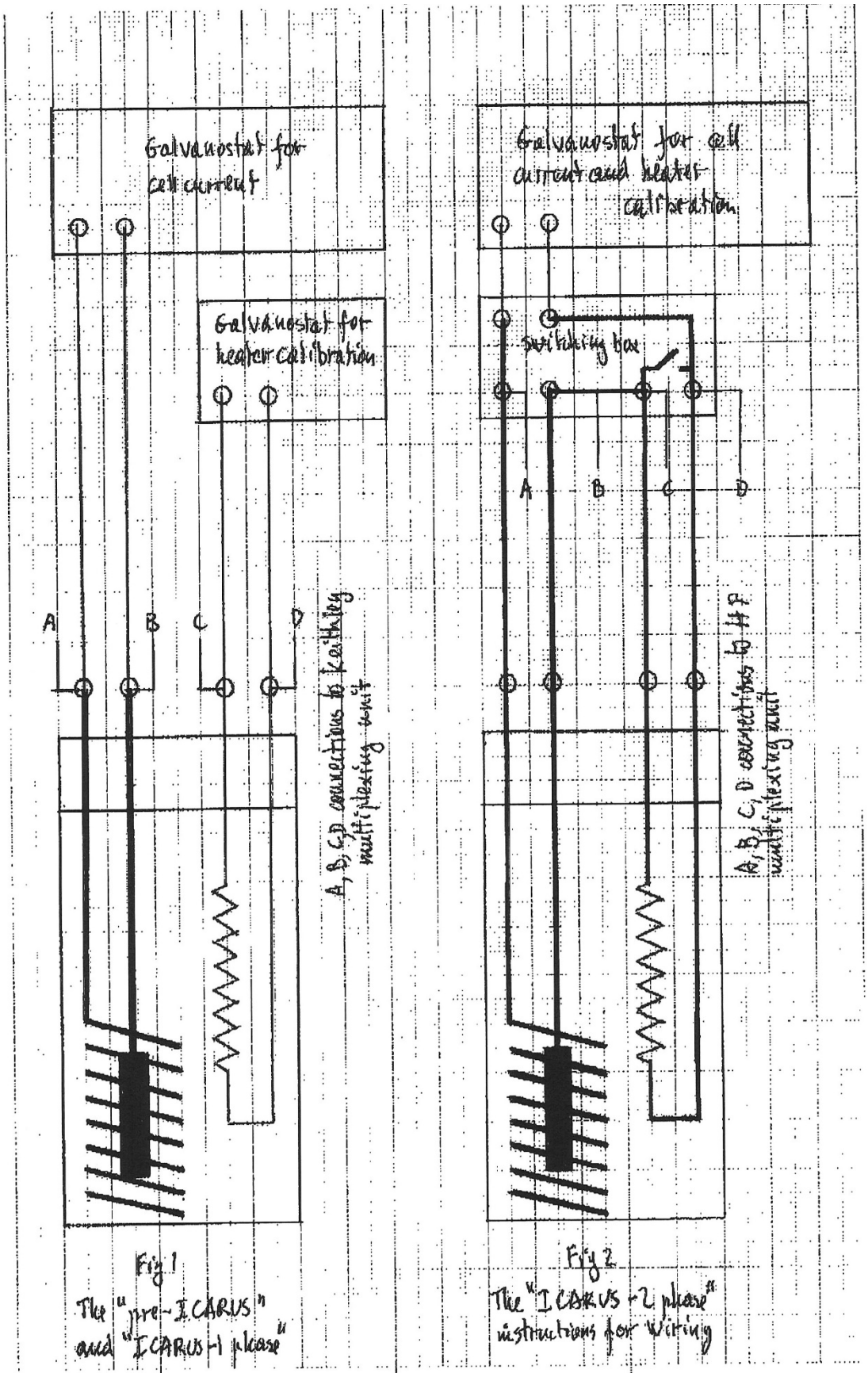

Fig 1
The "pre-ICARUS" and "ICARUS-1 phase"

Fig 2
The "ICARUS-2 phase" instructions for wiring

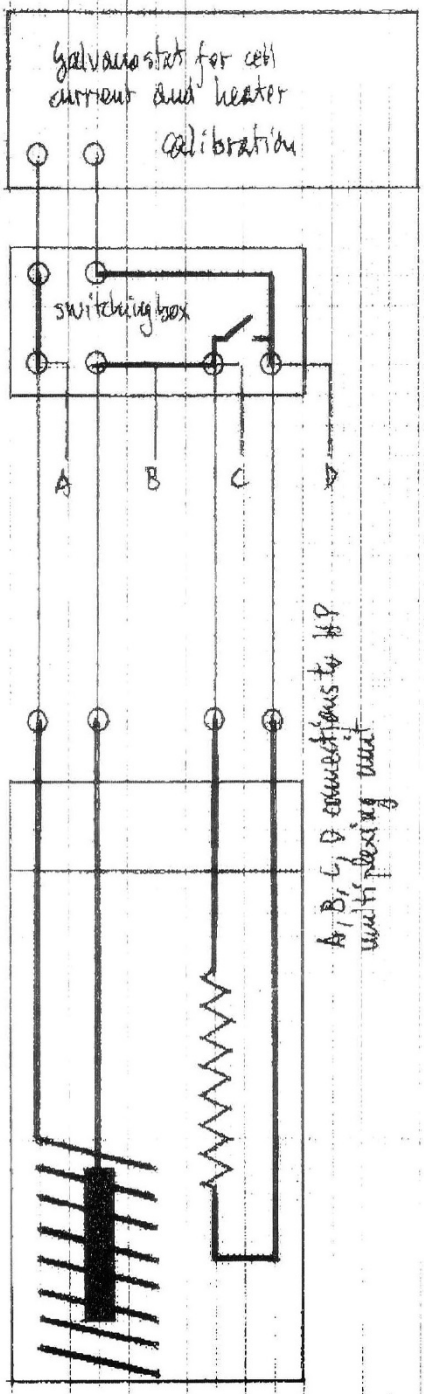

Fig 3
The "ICARUS-2 phase"
Execution of the experiments

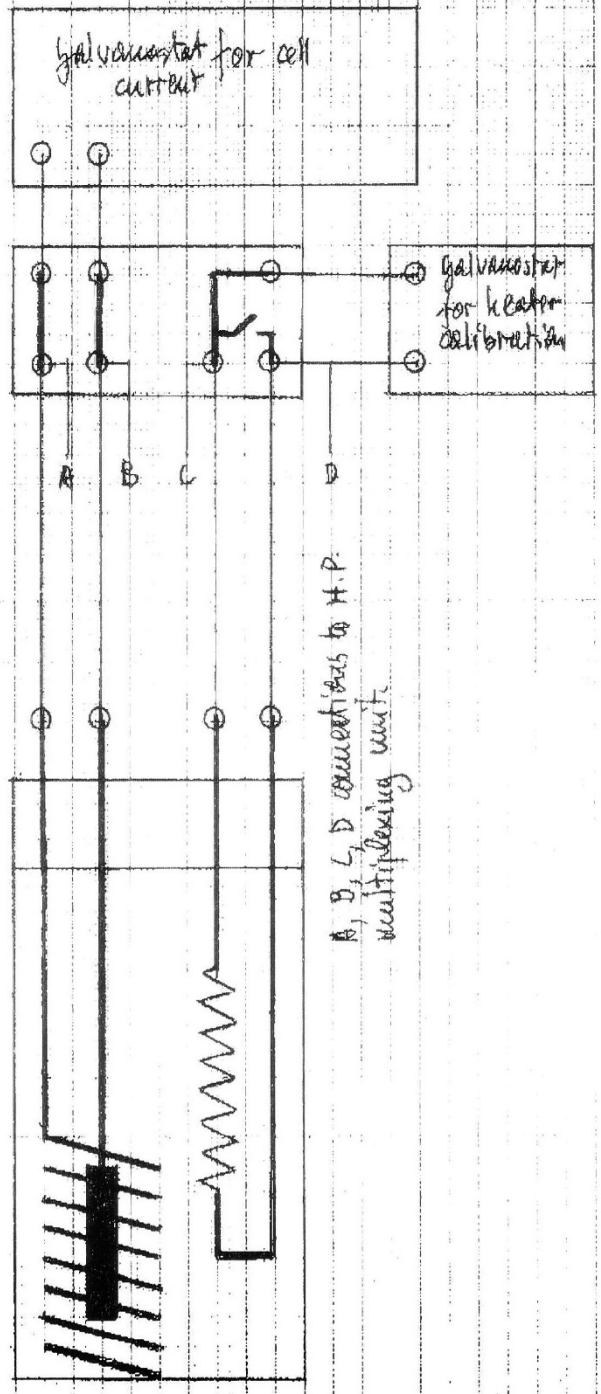

Fig 4
The "ICARUS-2 phase"
Actual experimental configuration

2000-02-29

NAWC heading

FAX MEMO

DATE: February 29, 2000

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I realize that you are probably in Italy at the present time, but I want to keep you posted of my correspondence with Mr. Matsui in Japan. Therefore, I am faxing you a copy of my latest e-mail message to Mr. Matsui. I will have more to write later concerning the Pd-B report and you letter 17 February 2000.

Have fun in Austria with the skiing!

Best wishes,

Mel Miles

Mel Miles

Dear Mr. Matsui,

Thank you very much for your reply of February 3, 2000. First of all, I really enjoyed my experience at NHE in Sapporo Japan. I rate this as one of the highlights of my career. I was treated very well by Dr. Asami, one of the best supervisors that I have ever worked for. It was a real pleasure for me to have the opportunity once again to do cold fusion research - something that I have not been able to do in my present position since 1995. We are all well aware that this is a scientific topic of great controversy. My number one priority is to find the scientific truth relating to this subject. I am certain that the truth will eventually win out as it must when scientific methods are applied.

Regarding the Fleischmann-Pons calorimetry, I was aware of the controversy involving the NHE paper presented in Monaco (ICCF5, pp. 105-115, 1995) that was critical of previous work reported by Fleischmann & Pons. Therefore, I did not side with either NHE or Martin Fleischmann but rather developed my own methods of analysis. It is true that my presentation at NHE in January of 1998 supported the NHE position. From my data for the F/P cells, it appeared that there was no excess heat and large calorimetric errors. However, this analysis was based on the heat transfer co-efficient reported to me by Mr. Sumi of NHE as well as the value of 490 J/K as the water equivalent of the cell. During the further processing of this data in my last few months in Japan, I became convinced that the heat transfer co-efficient used by Mr. Sumi was incorrect. Furthermore, I have done considerable work with this data since returning from Japan and now believe that the water equivalent of the cell is closer to 450 J/K. Both of these values must be accurately established in order to obtain correct results for the F/P calorimetry. There was considerable excess heat in both the Pd-B and Pd-Ce experiments. This will be discussed in two Navy reports that I am working on as well as at ICCF8 and possibly in scientific journals. In my final report to NHE, I made it very clear that these experiments, using the F/P calorimetry, produced excess heat. This creates a problem with my reporting excess heat and the Japanese version of the NHE report claiming that I had no excess heat. I was not aware of this difference until it was reported to me by Jed Rothwell.

I also observed excess heat at NHE using my China Lake calorimetry. In fact, a detailed paper on this has been accepted for publication by the Journal of Electroanalyical Chemistry. I will be glad to send you a reprint of this paper as well as the reviewer comments and the comments by the editor, Professor Roger Parsons. These comments were all quite favorable towards my conclusion of an excess heat effect. It will be very clear from this paper that this work was done at NHE. Therefore, I hope we can both be reporting similar stories for positive excess heat effects at NHE. My excess heat effects at NHE ranged from 50 to 500 mW, thus they are not huge effects, but they are in very good agreement with my previous results at China Lake and well beyond any experimental errors.

Another paper with Dr. Stan Spzak was published in the September issue of Fusion Technology. This paper was also peer-reviewed and reports excess heat for my NHE experiments in F/P cells using the co-deposition method developed by Dr. Spzak. I will also send you a reprint of this publication.

In summary, I think we need to be "on the same page", regarding my excess heat measurements at NHE, to avoid problems. Nevertheless, this does not distract from our friendship and my high regard for both you, Dr. Asami, and all my Japanese co-workers. Perhaps we have scientific disagreements, or different agendas, but I hope that we are all striving for the scientific truths regarding cold fusion.

Best wishes,

Mel Miles

— Original Message —

From: Kazuaki Matsui
To: Melmiles
Sent: Thursday, February 03, 2000 5:59 PM
Subject: RE: e-mail from Elliot

Dear Dr. Miles,

I am a little bit surprised in the things going on. First of all, we, Japanese side, appreciated your participation to our project in Sapporo, NHE, even though there were some disagreement in understanding and or observation of your experiment. But this is not unusual at all at least to me. I remember that you have reported your results to the committee of which members included Prof. Akito Takahashi, Prof. Ikegami, Prof Ohta and others, with discussion. The reports were drafted by us and reviewed by the same committee. No politics at all. But Dr. Asami and myself plan to see how the reports described, probably in this month for our understanding. I understand and sympathize how you feel to your circumstances, but I believe that it is not necessary to turn your back to your friends because of your grief and anger. It sounds like that someone wants to stir up and create conflicts by making noises. Mr Kennel happens to come over to Japan for his holiday in middle of February as you know, and we will discuss possible better action by us, but including no action at all.

Yours sincerely.

Kazuaki MATSUI mac@iae.or.jp
Director, Research and Development Division
The Institute of Applied Energy
Shinbashi SY Bldg.
14-2 Nishishinbashi 1-Chome,
Minato-ku, Tokyo. 105-0003. JAPAN

—Original Message—

From: Melmiles [mailto:melmiles@ridgenet.net]
Sent: Wednesday, February 02, 2000 12:25 PM
To: Mr. Matsui
Subject: e-mail from Elliot

Mr. Matsui:

Please note my reply to Elliot's e-mail – Mel

Dear Elliot,

First, let me make it very clear that I am intensely interested and concerned about scientific truth as it relates to cold fusion research. However, I have very little regard or interest in any politics relating to this field.

I am a Research Scientist not a Politician! Because of anti-cold fusion politics, I was assigned to report to the stockroom at China Lake, do chemical inventory, and clean out buildings when the Office of Naval Research (ONR) suddenly terminated my cold fusion funding in 1995. . . .

[The full copy is under 2000-02-03]

2000-03-14

[From Stan Szpak]

Mel,

1. Concerning Chubb's contribution:

(a) length: 15-20 pages (simple spacing)

(b) type of presentation: written for people interested in cold fusion – not for theoretical physicists.

(c) Emphasis on the evolution of theories, common threads, if any.

Examples: American: Schwinger, Chubb, Hagelstein, Kim, Miley ....

Italian: Preparata .... others?

French: Vigier ... others?

Russian: Tsarev .... others?

Japanese: ... ?

Chinese: .... ?

Dr. Frank Gordon would like to have a copy of your trip report to Black Power (Mills). Please, send him a copy.

His FAX: (619)-553-2951

Regards,

Stan

2000-03-17

Melvin H. Miles, Ph.D.
Chemistry and Materials Branch
Research and Technology Division
Code 4T4220D
Naval Air Warfare Center Weapons Division
China Lake, CA 93555-6100 USA

FAX MEMO

DATE: March 17, 2000

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles TIME: 10:00 a.m.

MESSAGE:

Dear Martin,

Dr. Imam and I are making good progress towards the publication of your data analysis as a NRL report. Dr. Imam wants to include a "Foreword" which I am faxing to you. Please let me know if you have any suggested change. I will then send you the original with my signature for you to sign and mail to Dr. Imam.

I have a question regarding the consumption of D20 during electrolysis. There should be a considerable amount that is evaporated and carried away by the gas stream. Exactly what happens to this portion of the D_2O? I imagine that most of it refluxes with the cell wall or with the glass tubing that was in place to carry the D_2O to the weighing pan during boil-off. This glass apparatus was connected throughout the experiment and probably served as a reflux surface. Nevertheless, my calculations show that 4% more D_2O was lost than expected from electrolysis alone. Is this number reasonable? I think this 4% factor will help to explain our differences relating to the D_2O level.

I hope you had a good trip to Italy and didn't hurt yourself skiing in Austria. What are the prospects of my working in Italy sometime in the future?

Best wishes,

Mel Miles

Mel Miles

FOREWORD

This study involves the palladium-boron alloy materials prepared at the Naval Research Laboratory (NRL) by Dr. M. Ashraf Imam (see NRL/MR/6170-96-7803, January 9, 1996). This new material was developed as part of a collaborative program with NRL and the Naval Air Warfare Center Weapons Division (NAWCWD), China Lake, that was funded by the Office of Naval Research (ONR). Previous studies at NAWCWD showed that the best reproducibility for excess power was obtained using the palladium-boron materials supplied by NRL (see NAWCWPNS TP 8302, September 1996). The new experimental studies described in this report were conducted by Dr. Melvin H. Miles at the New Hydrogen Energy (NRE) laboratory in Sapporo, Japan. Dr. Miles received a six month appointment as a Guest Researcher sponsored by the New Energy Development Organization (NEDO) of Japan. Dr. Miles expresses his appreciation to Dr. N. Asami and Mr. K. Matsui for providing him with this research opportunity. This experiment was conducted in a special Dewar-type colorimetric cell silvered at the top that was developed by Drs. Martin Fleischmann and Stanley Pons. The detailed analysis of the experimental data presented in this report was conducted by Dr. Martin Fleischmann. An independent method of data analysis developed by Dr. Miles while he was in Japan was presented in his NEDO Final Report [97] and shows similar trends for the excess heat effect.

Dr. Martin Fleischmann

Melvin H. Miles Dr. Melvin H. Miles

Dr. Ashraf Imam

[97] MM But not used by NHE.

2000-04-11

Bury Lodge heading

11th April 2000.

Dear Mel,

I have to say once again: I am a very bad correspondent. Anyway herewith now a copy of the Abstract on "Case Studies of Experiments carried out with the ICARUS Systems" which I submitted to ICCF-8 some time ago. [98] The paper has been accepted for presentation at the meeting and we will have to correspond about the details in due course. I take it that it will be sufficent to allow Dr. Imam to get funding for the Conference? Please let me know whether there is anything further which I should do.

I have made much headway in analysing the data set for the experiment carried out the N.H.E. Using the Pd electrode. I can demonstrate all the mistakes which were made by the goup: rewriting of the software controlling the data acquisition computer (which must introduce large errors into the evaluation of $(k_R')31$ and $(k_R')32$ favoured by N.H.E. - although they were told not to do so!), overfilling of the cells with electrolyte/D_2O, disconnection of the pressure sensor, and, last but not least, errors in the wiring up of the experiments which introduced errors in the input enthalpies to the cell. Because of the first of these errors, the analysis has to be based on $(k_R')11$ and we should say that we will return to the evaluations of $(k_R')31$, $(k_R')32$, $(k_R')21$, and $(k_R')22$ in a further paper. Notwithstanding these errors and, notwithstanding the use of inappropriate electrode material (produced by Tanaka Metals) the results show that there was excess enthalpy generation in the region close to the boiling point and during "boiling to dryness" and there was "Heat-after-Death". The analysis of the "boiling to dryness" episode is especially instructive. I believe that in this paper we should ask N.H.E. And A.N. other(s) to verify our analyses and also to ask how they may have reached the conclusions described in their report. Furthermore, we should ask them to release all the raw data for the experiments which formed the basis for their conclusions.

I believe that you will see that this material is very extensive so I think that this paper will have to be confined to the Pd-B and Pd systems leaving the Pd-Ce for another day.

I have been in Italy several times, most recently returning here yesterday.*[99] Giuliano is very ill about which more anon. Their experiments on the Coehn-Ahranov effects are going brilliantly and I think that their work should largely be confined to such electrodiffusion systems. This is a matter which we should discuss at Lerici. However, I have also been concerned to slot some other work around their investigations.

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[98] JR Miles, M., M.A. Imam, and M. Fleischmann. *"Case Studies" of Two Experiments Carried Out With the ICARUS Systems. in 8th International Conference on Cold Fusion*. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy, http://lenr-canr.org/acrobat/MilesMcasestudie.pdf

[99] JR This footnote lost, alas. Fleischmann's footnotes, asides and P.P.P. … S.s were fun to read.

One experiment is "up and running", a second experiment has been constructed and will be operational at the beginning of May: a third experiment will be based on the second and should be operational during the summer.

This brings me to your and George Miley's invitation to the ANS Meeting in Washington. To be quite frank I am quite tired of speaking about the same "old stuff" and travel funds are now a big problem for me. As against that, I should pose you the question: "how important is it for me to go to this meeting?" If you say that I should go, then, of course, I will do so. One possible solution to all of this is for me to ask the ENEA folk whether I can talk about the new work and whether they will pick up the tabs.

Another approach would be for me to describe why we <u>really</u> embarked on this venture. I shall shortly be giving a talk here in the U.D. On "Unfinished Business" which will outline some aspects of our search for demonstrations of the Q.E.D. paradigm finishing up with C.F. It's a risky strategy because such descriptions/revelations may be popular with the Defence Establishments. Here again, I would welcome having your comments.

Yours indefatiguably,

Martin

[P.S.] [100]

The most obvious shortcoming of the report is the lack of a more detailed analysis of the behaviour on Day 61 to complement the analysis of Day 3 given in Fig. A 16. I checked up on the performance using the ICARUS-1 Methodology for the calculation of $(k_R')21$, $(k_R')22$, $(k_R')31$ *and* $(k_R')32$ *and everything was in apple pie order. However, the analysis given in Fig. A.16 covers the whole measurement cycle,* $0<t<T$ *rather than* $t_1 < t < t_2$ *and it seems to me that I should make such extended analyses for all the data sets – at least for that on Day 61. We could then draw attention yet again to the fact that N.H.E. Have not provided any blanks so that we have had to use an internal calibration.*

P.P.S. We should look at your Pd-Ce data in Ontario. There is a lot riding on the notion of introducing 4f-states

P.P.P.S. I have enough understanding of the water capillary saga. Again, can we discuss this in Ontario

P.P.P.P.S. Finally, I have a list of expenses which I [incurred]in producing the report. Typing £370.00, Xeroxing £59:70 and preparation of spreadsheets and diagrams £234:00. With regard to the last item, I had to off-load some of the work. The sum total is £672:70. Will the budget stretch to that? In case it should be possible to make such charges, I am attaching all the relevant bills.

[100] JR This post script was also attached to the letter of 1999-09-17

2000-06-13

Bury Lodge heading

13th June 2000.

Dr. Melvin H. Miles,
Chemistry and Materials Branch.,
Research, and Technology Division,
Naval Air Warfare Center Weapons Division,
China lake, CA 93555-6100, U.S.A.

CONFIDENTIAL

Dear Mel,

SOS!

Have you got the instructions for the submission of the papers for ICCF-8 and, if so, could you please send me the relevant details? I never received the complete documentation for the meeting. I am presently rewriting the paper and would like to prepare the text as far as possible according to the Instructions: margins, paper size, founts, line spacings, style of references, page numbering and anything else which comes to mind.

The main difficulty will be the length of the paper and I am currently vigorously pruning the first draft. This brings to mind that the final text will not be comprehensible to readers unfamiliar with the field and I will therefore shortly also start writing a comprehensive Report which will contain full details - mainly spreadsheets and more graphs. I think that such a text could form an addendum to the earlier Reports and perhaps Stan or . . . Imam could prepare such a Document? In any event, I will send copies to you and Stan.

My attempts to condense the paper also show that there is one aspect which is difficult to cope with, namely the section of the final day leading up to evaporation to dryness. This particular aspect has never been discussed comprehensively - hence the difficulties. At the same time, such episodes afford one of the most simple (and interesting!) means of investigating C.F. I would therefore propose that we make a suitable collection of data sets and start on comprehensive analyses. The simple approach is outlined on the last paragraph of page 6 and the first paragraph of page 7 as well as Fig 19 of the original text. I have in mind that we should illustrate this further and then go on to an extended discussion within the framework of the Colligative Properties of electrolyte solutions. One very important aspect would be the illustration of the complexities of carrying out the detailed analyses when one has relatively low rates of excess enthalpy generation (as contrasted with the case of high rates of excess enthalpy generation).

I have the original data sets which we analysed in 1992 (high rates of excess enthalpy generation), your data set for M7C2 and the Japanese data set FF2-9506203-5661. As I hinted in the original text, the impossibility of arriving at a comprehensive analysis of the Japanese data set muat be due in part to errors in the volume and concentration of the electrolyte. Clearly, the whole exercise would he greatly strengthened if we could get hold of at least one further collection of data sets for experiments carried out in an independent laboratory.

This brings me to the reason why I have labelled this letter as being CONFIDENTIAL. At ICCF-8 I once again asked Jean-Paul Biberian for the data sets which were the basis of the paper by Lonchamp, Bonnetain and Hicter at ICCF-6. These data sets are especially interesting because all seven samples investigated were driven to dryness and the relevant electrodes were cut from Just-one rod of J.M. Material Type A. However, I also told Jean-Paul that I did not believe anything which he had told me (I was pretty forthright with several people at the meeting!) but that I thought that the DSG had instructed him to avoid giving me these data sets. He denied this (but then, he could hardly do anything else) and promised me once again to set about extracting the relevant information. It will be interesting to see what may happen.

All this brings to mind that the archiving of hard copy of the data sets should be a precondition for the publication of Reports and Papers.

Have you any comments on my suggestion that we should embark on such a venture? If J-P.B. comes up with the goods, then he could be included in the list of authors. An alternative option would be to offer to send him a confidential comprehensive report on the relevant data sets.

There are two matters which we should try to clear up at this time. The first concerns the solubility of LiOD in D_2O as the cells are being driven to dryness. Clearly this will depend on the temperature-time history and hence on the atmospheric pressure. A long time ago I found some information on the solubility at 100°C but this may well be wrong. Have you any means of getting at such data? Alternatively, could you set up a simple experiment to evaporate say 100ml of ~5M LiOD to dryness, to measure the temperature-time history and sample the electrolyte at suitable points and determine the LiOD by titration? Saturation should be reached at 8-9M.

The second matter concerns the question Of whether we should make some attempt to consider non-ideality. Do you know of any compilation of thermodynamic data of LiOD/D_2O at temperatures close to the boiling point?

Lastly, some comments on ICCF-8. I found this interesting but depressing. The whole field has now been effectively side-lined to topics which, although very interesting, are hardly life-threatening to the seven sisters and other commercial Interests. Of course the "University-based" research workers can be kept busy indefinitely on such peripheral issues, investigations of the underlying theory can be sidelined into suitably obscure aspects of quantum fluids while other sections of the work can disappear into suitable black holes. Actually, this is very much in line with my predictions.

Yours grittily,

Martin

P.S. I will need some references to your own work - certainly the fact that the ICARUS-style approach gives results similar to those which you have obtained and the fact that Pd-B electrodes had shown excess enthalpy generation at China Lake. Could you please mark up the relevant sheets of my first draft and add the references you would like me to include in the final version?

2000-06-26

Bury Lodge heading

26th June 2000.

Dear Mel,

I believe that the attached FAX of my letter to Franco Scaramuzzi and Antonella De Ninno is self-explanatory? I am now sending you a copy of the paper in the present form. It is still too long but I cannot see how it could be reduced further and still make a meaningful contribution. This is especially true of the number of Figures.

I believe that it would be a rather negative step to split the paper into two parts but, of course, we could do so if all else fails. Could you please comment on this particular aspect?

Of course, there is a great deal more which could be (and should be!) done but we have to ask ourselves the question: is it at all meaningful to do so. What is really needed is a series of papers highlighting various aspects of the ICARUS Methodology. However, I believe that such papers would simply be ignored as has been the case for previous "positive" reports. Would it not be true to say that the subject has now been comprehensively destroyed – at least as far as excess enthalpy generation is concerned. The residual subject matter can be sidelined as I point out in my last FAX.

All the best,

Yours,

Martin

P.S. I am contemplating re-doing the diagrams to get 6/page (contemplating without any enthusiasm). This would reduce the length of the paper to 13 pages. Any comments?

Bury Lodge heading

26th June 2000.

Dear Franco and Antonella,

As is so often the case in my correspondence I must start with an apology. I fear that I am late in writing to you to express my appreciation for your organisation of the splendid Meeting at the Villa Marigola. Quite apart from the Science, Sheila and I thought that Lerici was a marvelous location and we fully intend to return there purely for pleasure!

I am now sending you under separate cover two copies of the joint paper with M.H. Miles and M.A. Imam: "Case Studies of Two Experiments carried out with the ICARUS-Systems". We had actually produced a first draft of this paper ahead of the meeting in Lerici but it was quite clear that this text was far too long for inclusion in the Conference Proceedings. The major difficulty was that the text relied heavily on 35 Figures used as illustrations.

Since returning to the U.K. We have been engaged in trying to reduce the length of the paper and I also decided that we needed to carry out some further calculations to justify some of the statements made in the first version (we have found that all these statements were correct although we have had to make some minor changes to some of the Figures). I should point out here that neither Dr. Miles nor Dr. Imam have seen the version which I am now sending to you. The reason is that, in view of the late date, we feel that we should now send you this text. However, I also believe that Dr. Miles and Dr. Imam would not wish to change the content of the paper. I should also point out to you that I did not receive the instructions for the preparation of the texts as relayed to me by Mel Miles.

I would like to add some comments here on the content and length of the paper. The paper really consists of two parts: the first dealing with Experiment FP2-9506203-5561 carried out by N.H.E. alone; the second, Experiment FP 97120402-M7C2 carried out by Mel Miles during his stay in the N.H. E. Laboratories. Individually, these parts are about of the correct length. However, we do not believe that it would be sensible to split the paper into two parts because they have to be taken together to demonstrate the validity of the ICARUS Systems approach. We find Heat-after-Death, excess enthalpy generation in the temperature region close to the boiling points, excess enthalpy generation in other temperature regions for experiment FP 97120402-M7C2 and "positive feedback". We have also illustrated some new results including the rates of excess enthalpy generation just before and just after the onset of Heat-after-Death and the way positive feedback can be taken into account in the calibration of the cells. Last but not least, we have identified some of the mistakes made by the N.H.E. group in the execution and evaluation of the experiments.

It is this last matter which we believe to be of crucial importance. My own assessment is that if people do not believe in the validity of a phenomenon, then they also believe it doesn't matter how badly the experiments are carried out and evaluated. However, they should not then be allowed to publish any Report unless their conclusions are fully substantiated. A part of such substantiations is the publication of the "raw data". I do not know whether the group at N.H.E.

have ever given such "raw data" to third parties – all I know is that they have never replied to any of my letters asking for this information. However, we do have Mel Miles' data sets and, by a series of accidents (which N.H.E. may believe to be misadventures!) I have details of some other experiments. There is an important point here: it is not really necessary to carry out a very large number of experiments in order to delineate the main features of the phenomenon – of course the systematic exploration of the effects would require such extensive series of experiments.

Needless to say, there is a great deal more which needs to be done even on the data evaluations of just these two experiments. However, for the present, I have a very simple objective which is to try to force the N.H.E. to give detailed justification of their statements and to force them to publish their "raw data". These "raw data" would surely prove to be an invaluable asset. Publication of such data would set a precedent for the publication of other data sets e.g. those which were the basis of the paper by Longchampt, Bonnetain, and Hicter at ICCF-6. It is this consideration in the main that makes us wish to publish our present paper as a single entity.

Very best regards,

Yours sincerely,

Martin

cc. Dr. Melvin Miles

The following paper was attached to this letter:

Miles, M., M.A. Imam, and M. Fleischmann. *"Case Studies" of Two Experiments Carried Out With the ICARUS Systems. in 8th International Conference on Cold Fusion*. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy, http://lenr-canr.org/acrobat/MilesMcasestudie.pdf

2000-06-26 #2

(Handwritten comments by Fleischmann are shown in italics.)

Excellent! An unfulfilled chapter of my life!

PROJECT PROPOSAL FOR THE FY01 NAWC

IN-HOUSE LABORATORY INDEPENDENT RESEARCH (ILIR) PROGRAM

DATE: June 26, 2000

PROPOSED TITLE: HYDROGEN ISOTOPES IN METALS UNDER EXTREME CONDITIONS

PRINCIPLE INVESTIGATOR: Melvin H. Miles LEVEL (MY): 0.24

CODE/PHONE/FAX/E-MAIL: 4T4220D / (760) 939-1652 / (760) 939-1617

ASSOCIATE INVESTIGATOR: Jeffery Davis LEVEL (MY): 0.29

CODE/PHONE/FAX/E-MAIL: 4T4330D (760) 939-3357 (760) 939-2597

ASSOCIATE INVESTIGATOR: Don Thompson LEVEL (MY): 0.15

THIS PROJECT IS: NEW? Yes CONTINUING?

FUNDING REQUESTED /FUNDING RECEIVED ($K)

FY00	FY01	FY02	FY03	FY04
0	120	100	100	0

PROJECT SUMMARY: Hydrogen isotopes in metals produce unusual condensed matter conditions that are not well understood. For example, the palladium-deuterium system yields concentrations of 110 M deuterium, 110 M palladium, and 5200 M for electrons. The confinement parameter – chemical potential – size diagram suggests that D+D fusion reactions are possible in the Pd-D system. There are many reports of excess heat and helium production for Pd-D systems that confirm this possibility. Extreme perturbations of the Pd-D condensed matter system could produce ignition of the fusion reaction and explosive release of energy. Possible perturbations include temperature, pressure, electric field, and magnetic field effects.

Theoretical calculations show that the fusion energy released from 1.0 cm^3 of Pd-D (12.2g) would be 1.30×10^{11} J. This is equivalent to 32.5 tons of TNT (29,500 Kg). Realistically, probably no more than 10% of this explosive energy release could occur before the system would blow apart. Nevertheless, this would still yield the equivalent energy release of 3 Tons of

TNT (nearly 3,000 Kg) from 1.0 cm^3 of materials. The palladium-deuterium-tritium system can be driven to ignition even more readily via the D+T fusion reaction. Other metals that could produce similar effects with hydrogen isotopes include V, Nb, Ta, Ti and U.

Under shock conditions, a solid can be exposed to 10 – 1,000 kBar pressure increase over 100 ns with an accompanying rise in temperature of over 1,000 K. * Pulsing of an electric current or magnetic field can be can be used in conjunction with the shock experiments to increase perturbation of the Pd-D system.

** More with SEMTEX, RDX, HMX, etc. 10 Mbar?*

SECTION 1 — OBJECTIVE: The objective is to explore Pd-D and other metal-hydrogen isotopes for energy release during extreme perturbations of temperature, pressure, and electromagnetic field effects.

Absolutely

SECTION 2 — INTRODUCTION / BACKGROUND: 2.1. This research originates from Martin Fleischmann, who, as early as 1960 concluded that the behavior of hydrogen isotopes compressed into a Pd-host lattice could only be understood in terms of Quantum ElectroDynamics (QED). The behavior of the system must be expressed in terms of interacting fields rather than particles. This led to his view that nuclear reactions of hydrogen isotopes in host lattices could be promoted by coherent processes. Classified research by Dr. Fleischmann in Great Britain on depleted uranium prompted his concern regarding possible weapon applications of metal-deuterium/tritium systems. The observations of positive feedback for the Pd-D system also suggests possible weapon applications. Increasing the temperature of the Pd-D system generally increases the excess heat production.

2.2. To our knowledge, there is no previous work in this area with respect to small scale, fusion energy devices. It is obvious, however, that such work would not be made public. Related work involving this topic includes:

1. M. Fleischmann and S. Pons, "Electrochemically Induced Nuclear Fusion of Deuterium", *J. Electroanal. Chem.*, **261** (1989), pp. 301-308.
2. M.H. Miles, "Calorimetric Studies of Pd/D_2O+LiOD Electrolysis Cells", *J. Electroanal. Chem.*, **482** (2000), pp. 56-65.
3. G. Preparata, *QED Coherence In Matter* (World Scientific, 1995).
4. Y. Fukai, *The Metal-Hydrogen System, Basic Bulk Properties*, (Springer-Verlag, 1993).
5. M. Myers, *Dynamic Behavior of Materials*, (John Wiley & Sons, 1994)

SECTION 3 — TECHNICAL APPROACH: The possibility of enormous energy release will restrict initial investigations to small scale systems. Even 1.0 mg of Pd-D could theoretically release the energy equivalent of 2.4 Kg of TNT. The main approach will be to electrochemically

compress deuterium into palladium for a sufficient time period and then suddenly perturb the system to high temperatures and high pressures by the use of explosives. Variations include using D_2 and D_2+T_2 gases under pressure for loading the palladium prior to the sudden perturbation. Geometric variables will be investigated by using compressed powders, rods, tubes, cubes or other shapes for the palladium. The use of other metals should also be investigated. Measurements will include explosive forces and radiation monitoring.

1) Load palladium and other metals with deuterium

• Investigate extent of loading of deuterium

• Test effect of geometric variables on sample (size and shape)

• Investigate transportation of sample

I would add to investigate $^{238}UD_3$

2. Subject metal — D systems to extreme perturbation and monitor energy release

• Explosive deformation

• Gas gun

• High EM fields

If the particle size is <u>below</u> that of a coherence domain of a relevant system, then the particle will not disintegrate. This suggests that we should investigate powders in highly reducing media e.g. mixtures of alkali metal hydrides or alkaline earth metal hydrides.

SECTION 4. NAVY RELEVANCE / PAYOFF (IF SUCCESSFUL):

4.1. This work has the potential to completely revolutionize all naval weapon systems. Even small, uninhabited aerial vehicles could deliver a major explosive force to the enemy. Guns from Naval ships could deliver massive blows. Torpedoes could easily destroy any enemy ship. The metal-hydrogen isotope systems would minimize weapon size while maximizing destructive power. Also of use for single shot high out put energy storage devices.

I must discuss rocket propulsion with you.

4.2. Many laboratories have reported the small scale release of excess power from Pd-D systems. These effects are generally not reproducible and have not been accepted by most of the scientific community. The sudden, extreme perturbation technique for ignition has not been reported. Thus, this represents a completely new area of research.

4.3. Chemistry has the equipment necessary to electrochemically compress deuterium into various palladium structures. Engineering science has the facilities and instrumentation such as Instron, Split-Hopkinson pressure bar, gas gun, and explosive capabilities to perturb the Pd-D system towards ignition.

4.4. Specific milestones include:

(1) Test electrochemically loaded Pd-D systems under pressure and temperature perturbations.

(2) Test gas loaded Pd-D and Pd-D-T systems under various perturbations.

SECTION 5 — PROGRESS (CONTINUING PROJECTS ONLY):

New Start

SECTION 6 — BREAKDOWN OF FUNDING REQUIREMENTS:

FY01 Total Requested: 120K

Labor: 105K

Other (Specify): Travel: 5K, Materials: 10K

FY02 Total Requested: 100K

Labor: 92K

Other (Specify): Travel: 5K, Materials: 3K

FY03 Total Requested: 100K

Labor: 92K

Other (Specify): Travel: 5K, Materials: 3K

Seems reasonable.

ACTION 7 – RELATED PROJECTS / POTENTIAL TRANSITIONS:

7.1. Identify related projects for which significant interactions with this project will occur.

The nature of this project will require restricted interactions. Several key individuals from NRL and NCCOSC may be kept informed about the results. Consultations with Martin Fleischmann will be essential for the success of this program.

7.2. List projects/sponsors to which the results of this project might transition. Successful experiments on this project should readily transition to funding by ONR and DARPA.

Provide new warhead concept for many of our systems including anti-ballistic missile defense.

Yes!!

SECTION 8: PROJECT INVESTIGATOR(S) INFORMATION:

8.Briefly describe significant technical results achieved from research conducted during the last five (5) years.

8.1.1 Melvin H. Miles

During the last five years has been involved in thermal batteries, corrosion, solid polymer electrolytes, calorimetry, lithium-ion batteries, fuel cells, and supercapacitors. Ph.D. in physical chemistry, NWC Fellow and 26 years working for the Navy. Has more than 160 research publications.

8.1.2 Jeffery J. Davis

Over the last five years has been involved in metal/metal oxide and metal/polymer reactions as well as the investigation of mechanical deformation of materials. Completed Ph.D. in physics in this area. Has worked in the areas of small scale testing of explosive and propellants. Has been employed as a research physicist specializing in shock physics and mechanical deformation for the Navy for ten years.

Initially I would aim for sub- micron size particles. I would aim to include nano [?] dispersions of particles with hydrogen on the open specs.

8.1.3 Don Thompson

Has a background in materials engineering and, over the last five years has been involved with the development of manometer metals and metal oxides powders for use in ordnance applications. During that time he was also the team leader for the Detonation Mechanics Laboratory and was involved with the Sidewinder Life Extension Project and the AIM-9X External Hardware Evaluation Effort. He has a technical background in nuclear engineering, warhead design, and ordnance testing and evaluation.

8.2 List publication references for research documentation that occurred during the last five (5) years

8.2.1 Melvin H. Miles

1. "Improved Thermal Batteries Using Molten Nitrate Electrolytes" M.H. Miles, 35th ACS Western Regional Meeting, 37th SAS Pacific Conference, October 6-8, 1999, Abstract No. 259.

2. "Calorimetric Studies of Pd/D_2O+LiOD/Pt Electrolysis Cells" M.H. Miles, 35th ACS Western Regional Meeting, 37th SAS Pacific Conference, October 6-8, 1999, Abstract No. 255

3. "Calorimetry of the Pd+D Codeposition" S. Szpak, P.A. Mosier-Boss, and M.H. Miles, Fusion Technology, Vol. 36, pp. 234-241, 1999

4. "Calorimetric Studies of Pd/D_2O+LiOD Electrolysis Cells" M.H. Miles, J. Electroanal. Chem., Vol. 482, pp. 56-65, 2000

5. "Report on Calorimetric Studies at the NHE Laboratory in Sapporo, Japan," M.H. Miles, Asti Workshop on Anomalies in Hydrogen/Deuterium Loaded Metals, Conference Proceedings, Italian Physical Society, October 22-24, 1999 (in press)

6. "Calorimetric Studies at the New Hydrogen Energy Laboratory in Japan," M.H. Miles, Bulletin of the American Physical Society, Series II, Vol. 45, No. 1, March 2000, Abstract C32

7. "My Impressions as a NEDO Guest Researcher at the NHE Laboratory in Japan," M.H. Miles, Infinite Energy, Vol. 5, Issue 30, pp. 18-21, 2000

8. "Report on Calorimetric Studies at the NHE Laboratory in Sapporo, Japan," M.H. Miles, Infinite Energy, Vol. 5, Issue 30, pp. 22-25, 2000

9. "Calorimetric Studies of Palladium Alloy Cathodes Using Fleischmann-Pons Dewar Type Cells," M.H. Miles, Proceedings of the Eighth International Conference on Cold Fusion (ICCF-8) May 21-26, 2000 (in press)

10. "Case Studies of Two Experiments Carried out with the ICARUS Systems," M.H. Miles, M.A. Imam, and M. Fleischmann, Proceedings of the Eighth International Conference on Cold Fusion (ICCF-8) May 21-26, 2000 (in press) http://lenr-canr.org/acrobat/MilesMcasestudie.pdf

11. "Lithium Thermal Batteries Using Molten Nitrate Electrolytes," M.H. Miles, Proceedings of the 39th Power Sources Conference, June 12-15, 2000, pp. 560-563

12. "Thermal and Nuclear Aspects of the Pd/D_2O System. Vol. LA Decade of Research at Navy Laboratories," S. Szpak, P.A. Mosier-Boss, M.H. Miles, S. Chubb, T. Chubb, and M. Fleischmann, SPAWARSYSCEN Report (in preparation)

13. "Thermal and Nuclear Aspects of the Pd/D_2O System, Vol. II: Simulation of the Electrochemical Cell (ICARUS) Calorimetry," S. Szpak, P.A. Mosier-Boss, M.H. Miles, and M. Fleischmann, SPAWARSYSCEN Report (in preparation)

14. "Calorimetric Analysis of a Heavy Water Electrolysis Experiment Using a Pd-B Alloy Cathode," M.H. Miles, M.A. Imam, and M. Fleischmann, NRL Report (in preparation)

15. "Excess Heat and Helium Production in the Palladium-Boron System," M.H. Miles, M.A. Imam, and M. Fleischmann, Proceedings of the American Nuclear Society Winter Meeting, November 12-17, 2000 (in press)

8.2.2 Jeffery J. Davis

Jeffery Davis, Philip J. Miller, and Cliff Bedford. "Effect of metal particle size on the detonation properties of various explosives" to be published in the proceedings of TTCP — WTP-4 Technical Workshop, Indian Head MD, April 16, 1999.

Jeffery Davis and Philip J. Miller. "Shock and Impact Initiation of an Incendiary Material", to be published in the proceedings of Joint Army, Navy, NASA and Air Force (JANNAF) meeting, Tucson AZ, December 12, 1998

Jeffery J. Davis, Allen J. Lindfors, Philip J. Miller, Steve Finnegan, Diana L. Woody. "Detonation Like Phenomena in Metal - Polymers and Metal/Metal Oxide — Polymers", to be

published in the proceedings of llth International Detonation Symposium, Snowmass CO, September 3, 1998.

Jeffery J. Davis and Philip Miller. "Reaction of Pyrotechnic Material Under Deformation: Experiments and Modeling", to be published in the proceedings of Joint Army, Navy, NASA and Air Force (JANNAF) meeting, West Palm Beach FL, October 30, 1997.

J.J. Davis and A.J. Lindfors, "Inert Hugoniot for a porous titanium - Teflon mixture: experiment and calculations", Shock Compression of Condensed Matter - 1997, Proceedings of the Conference of the APS Topical Group on Condensed Matter, Amherst MA July 27 - August 1,

1997 Ed. S. C. Schmidt, D. P. Dandekar, J. W. Forbes, AIP Press, (1998) pp. 663 - 666. D.L. Woody, J.J. Davis, J.S. Deiter, "Recovery studies of impact-induced metal/polymer reactions in Titanium based composites", Shock Compression of Condensed Matter - 1997, Proceedings of the Conference of the APS Topical Group on Condensed Matter, Amherst MA July 27 - August 1, 1997 Ed. S. C. Schmidt, D. P. Dandekar, J. W. Forbes, AIP Press, (1998) pp. 667 - 670.

D.L. Woody and J.J Davis, "The Effect of Particle Size and Porosity on Metal/Metal Exothermic Reactions Induced by Low Velocity Impact", in Proceedings of 14th US Army Symposium on Solid Mechanics 16-18 October - Myrtle Beach SC edited by K.R Iyer and S. Chou, Battelle Press (1997) pp. 43-48

H. John and J.J. Davis, "Porosity and Particle Size Effects on High Strain Rate Properties of Metal/Metal Oxide Materials", in Proceedings of 14th US Army Symposium on Solid Mechanics 16-18 October - Myrtle Beach SC edited by K.R Iyer and S. Chou, Battelle Press (1997) pp. 401-408.

H.J. John, Jr., J.J. Davis, F.E. Hudson III, and R.L. Robbs Porosity and Particle Size Effects on High Strain Rate Properties of Fe_2O_3/Al/Teflon, by. Naval Air Warfare Center Weapons Division. China Lake, Calif. Mar. 1997. NAWCWPNS TM 8084.

Could I have a copy of this article?

J.J. Davis, D.L. Woody, and P.J. Miller, "Effect of Shear Rate on Initiation of Pyrotechnic Materials - Experimental Results and Computer Modeling", to be published in Proceedings of JANNAF Combustion Subcommittee and Propulsion Systems Hazards Subcommittee Joint Meeting, November 1996.

D.L. Woody, J.J. Davis, and C.D. Bedford, "Comparison of the Visible Emissions from Energetic Materials Containing Differing Particle Sized Aluminum", to be published in Proceedings of JANNAF Combustion Subcommittee and Propulsion Systems Hazards Subcommittee Joint Meeting, November 1996.

D.L. Woody and J.J Davis, "The Effect of Addition of Metal Compositions to Energetics", Proceedings of JANNAF Hazards Meeting, St. Petersburg Florida, December 1995.

J. Forbes, J. Davis, and C. Wong, "Detonation Synthesis of Nano-size Materials" in Decomposition, Combustion, and Detonation Chemistry, Eds. T.B. Brill, T.P. Russell, W.C. Tao, and R.B. Wardle, Materials Research Society Symposium Proceedings Volume 418, Material Research Society, Pittsburgh PA (1996) pp. 439 - 444.

D.L. Woody, J.J Davis, and P.J. Miller, "Metal/Metal Exothermic Reactions Induced By Low Velocity Impact", in Decomposition, Combustion, and Detonation Chemistry, Eds. T.B. Brill, T.P. Russell, W.C. Tao, and R.B. Wardle, Materials Research Society Symposium Proceedings Volume 418, Material Research Society, Pittsburgh PA (1996) pp. 445 - 449.

D.L. Woody, J.J Davis, and J.S. Deiter, "Plastic Flow Generated Solid State Metal/Metal Reactions", in Shock Compression of Condensed Matter - 1995, Eds. S.C. Schmidt and W.C. Tao, AIP Press (1996) pp. 717 - 720.

J.J Davis and D.L. Woody, "Reactions in Neat Porous Metal/Metal and Metal/Metal Oxide Compounds under Shear Induced Plastic Flow Conditions", in Metallurgical and Material Applications of Shock-wave and High-Strain-Rate Phenomena Eds. L.E. Murr, K.P. Staudhammer, M.A. Meyers, Elsevier, Amsterdam (1995) Chapter 78 pp. 661-668.

8.2.3 Don Thompson

Lead-Free Cartridge Case Primer, Patent # 5717159

TP 8484, SIDEWINDER WARHEAD, (WDU-17/13) SERVICE LIFE EXTENSION PROGRAM FINAL REPORT by D. Thompson & Martin Koca

TM 8234. SIDEWINDER WARHEAD (WDU-17/B) SERVICE LIFE EXTENSION PROGRAM-GROUP I COMPONENT EVALUATION AND TESTING, by D. Thompson, A.

Thompson, D. Wooldridge, Martin Koca, and Chris Aumann TM 8244. SIDEWINDER WARHEAD (WDU-17/B) SERVICE LIFE EXTENSION PROGRAM-GROUP II COMPONENT EVALUATION AND TESTING, by D. Thompson, J. Roquemore, D. Wooldridge, Martin Koca, and A. Thompson

TM 8281. SIDEWINDER WARHEAD (WDU-17/B) SERVICE LIFE EXTENSION PROGRAM-GROUP III COMPONENT EVALUATION AND TESTING, by D. Thompson, D. Wooldridge, Martin Koca, and A. Thompson

TM 8243. SIDEWINDER WARHEAD (WDU-17/B) SERVICE LIFE EXTENSION PROGRAM-GROUP I ARENA TESTING, by D. Wooldridge, D. Thompson, M. Koca, C. Aumann, and A. Thompson

TM 8275. SIDEWINDER WARHEAD (WDU-17/B) SERVICE LIFE EXTENSION PROGRAM-GROUP II ARENA TESTING, by D. Wooldridge, D. Thompson, M. Koca, and A. Thompson

TM 8284. SIDEWINDER WARHEAD (WDU-17/B) SERVICE LIFE EXTENSION PROGRAM-GROUP III ARENA TESTING, by D. Thompson, D. Wooldridge, Martin Koca, and A. Thompson

TM 8242. DETONATION VELOCITY TESTING OF LOS ALAMOS NATIONAL LABORATORY ALUMINUM/MOLYBDENUM TRIOXIDE, by Don Thompson and Christopher Aumann

TP 8467. PRELIMINARY INVESTIGATION OF EFFECTS OF AIM-9X WIRING HARNESS ON WARHEAD FRAGMENTATION, by Don Thompson & Danny Wooldridge

TP 8470. HYDROCODE ANALYSIS OF THE IN1ERACTION OF AIM-9X WARHEAD FRAGMENTS WITH AIRCRAFT STRUCTURES, by Don Thompson & Ed Cykowski PS-TR 123. INVESTIGATION OF EFFECTS OF AIM-9X WIRING HARNESS ON

WARHEAD FRAGMENTATION, by Don Thompson and John Brown TP 8400 SENSITIVITY TESTING OF PBXN-3 MOLDING POWDER, by Christopher Aumann, D. Wooldridge, and D. Thompson

PS-TR 124. WARHEAD INITIATION FAILURE EVALUATION, by Don Thompson, Jack Brown, Danny Wooldridge

JANNAF Propulsion Combustion Subcommittee Meeting. “Recent Advances in the Combustions Behavior of Nanometer Particle Size Aluminum Powders,” November 1996.

Contributions to Vol. 4 of NAWCWPNS TP 6750-30, -31, -32, -33-Air Weaponry Technology Program progress reports.

NAWCWPNS TP 8115. Characterization of Nickel/Aluminum Composites Consolidated From Powders, September 1993.

ADPA Bomb and Warhead Symposium. “Evaluation of Inorganic Energetic Materials as Shaped Charge Liners,” May 1993.

NAWCWPNS TP 8100. Evaluation of an Intermetallic Composite Liner, December 1992. Contributions to Vol. 4 of NAWCWPNS TP 6750-15, 16, 17, 18, 19, 20, 21, 22, 23-Air Launched Weaponry Block Program progress reports.

ADPA Bomb and Warhead Symposium. “Inorganic Energetic Materials for Ordnance Applications,” May 1992.

NWC TM 6854. Advanced Bomb Family Warhead Development Effort-Volume 4: Penetration Analysis, December 1992.

NWC TM 6806. Modern Steel Case Warhead: Design Concepts for AWCCM Under BTI-Final Report, August 1990.

NWC TM 6559. 25-mm Target Practice Frangible Projectile Ricochet Tests, August 1990.

NWC TM 6733. Testing of Potentially Ignitable Fills for Tungsten Carbide Fragments, March 1990.

One of my original objectives was to subject diffraction gratings of Pd wires electrochemically charged with D to intense laser radiation. This project seems to have some resemblance to work on inertial confinement. Eventually all this led to SERS [Surface Enhanced Raman Spectroscopy]. Of course, such gratings could also be subjected to electro diffusion. Pulsed laser on electrochemical or pressure modulation could be employed to a variety of detection methods including neutron spectroscopy to produce two-dimensional spectra – a totally new field of Physics.

Subsequent work with SERS shows that one could use roughened surfaces on ?? Pd electrodes. I gave all this up because it seemed to me that it was necessary to spend a man life of my work on the Photo physics. However, this should not stop one from investigating the weapons potential.

The work in S.L.C. involve the use of propagating struck fronts induced by liquid explosions. i.e. about 50 kbar. Has this work ever been reported? If not, why not? As I recall, Professor Adair said that this work should stop (at the review meeting of the NCFI.) I must talk to you about this.

2000-07-02

2nd July 2000.

Dr. S. Szpak
3498, Conrad Avenue,
San Diego, CA 92117,
U.S.A.

CONFIDENTIAL.

Dear Stan,

As you will see, I am at long last sending you some comments on the draft Sections of the Navy Report which you sent to me in May. The delay in my reply is really quite inexcusable; all I can say is that it is certainly not due to slacking! As you will realise, we ran into the preparations for I.C.C.F. 8 during June and I also had several commitments in Italy following the untimely death of Giuliano Preparata. That was certainly a very sad turn of events and his input and wide-ranging knowledge will be sadly missed.

First of all, I would like to say "Thank you" for the sterling work you have done. I think that the Report is shaping up very well and it is certainly essential that such a full discussion should see the light of day. I have here a general question which bears on the high standard of the text: what software do you use and do you use a single package for the mathematics and English text? Also, how do you insert the Figures and what P.C. do you use? It strikes me that I should try to use the same package(s).

I would like to start with some general comments on the Sections you have sent to me. I have no specific comments on "Events in Polarised Pd+D Systems" (which is why I am not returning this Section to you). However, I wish that you and Pam had dealt with this aspect more fully! As is always the case, the subject matter is familiar to the authors but much less so to the general readers (even if they have some expertise in the fields!) Literature references only help to a limited degree - would it be possible to include a collection of the key papers as an Appendix to the Report, say, as a separate Volume given restricted circulation?

To deal next with "The emergence of Cold Fusion": I am returning this Section with some minor corrections. Everything you have said is quite correct but at the same time it only covers part of the story. Of course, this is inevitable because I have been less than frank about these aspects. As time now marches on, I should perhaps outline some of the missing material to you? As I shall touch on some matters which have never yet been made public, 1 have labelled this letter as being "CONFIDENTIAL". If you should wish to touch on some of these aspects, then I would suggest that you should show the relevant Section(s) to your Security People because I would not wish to release information (even indirectly) which they would prefer to keep under wraps for the present. Incidentally Mel knows about some of these aspects.

As I see it, there are really two separate parts to "The Emergence of Cold Fusion":

(i) How did C.F, fit with M.F.'s wider research, plans?

(ii) Why, exactly, did S.P. and M.F. choose to investigate this particular problem?

Of course, separation of the background into these particular parts is somewhat artificial because they are certainly interrelated.

To "start the ball rolling", I am sending a copy of the Galley Proofs of an article I wrote for the Journal "Accountability in Research". [101] I was persuaded to write this Article by Scott Chubb and, as you will see it has still not appeared so you must consider it to be "embargoed" until the publication date. However, Scott might be quite pleased if you were to refer to this Article?! As a matter of fact, my consideration of the subject matter prompted me to write a Preliminary Document (~ 150 pages!) for Scott in which I revealed pretty well all. However, I thought that this was altogether too much of a "Hot Potato" so I never sent this version to Scott. [102]

You will see that this article for "Accountability in Research" bears on (i) in that our consideration of electrolyte solutions was one matter which convinced us that condensed phase systems have to be understood in terms of the Q.E.D. paradigm. It seemed clear to us that a programme in this field certainly had to include experiments to probe the effects of:

(a) space

(b) time

(c) length

(d) dimensionality

(e) number

(f) structure

(g) energy

Of course, this separation with the headings (a) - (g) is rather arbitrary. Any given research topic will usually involve combinations of the various sub-topics.

In the "Accountability in Research" article, I set the scene by discussing the inadequacy of classical and quantum mechanics for analysing the properties of electrolyte solutions but we did not develop research along those lines when I first started on this venture in the 1960's, To be quite frank: I did not know how we should do so and we have only recently "twigged" how we might set about this task - see the paper on QED coherence and electrolyte solutions of which I

[101] JR Fleischmann, M., *Reflections on the Sociology of Science and Social Responsibility in Science*, in Relationship to Cold Fusion. Accountability Res., 2000. **8**. http://lenr-canr.org/acrobat/Fleischmanreflection.pdf

[102] JR I suppose this 150 page document has been lost, along with much of Fleischmann's other work. This is a consequence of his aversion to computers, his unorganized work habits, and his distrust of people.

enclose a reprint. The preconditions for developing the necessary modelling were not available at that time - a good illustration of the point I made in the paper that the construction of models often follows with much delay on the recognition of the need to use a particular paradigm. Incidentally, I predict that this article on electrolyte solutions will raise a worse "stink" than the C.F. topic mainly because people have developed such entrenched positions. It will also be seen as a great threat to certain emergent technologies. This is not actually the case: what is really required is to take account of Q.E.D. in the understanding of the relevant aspects of the Natural Sciences.

As I have said, we did not confront the problem of the modelling of electrolyte solutions "head-on". Instead, we developed a programme of work on the kinetics of fast reactions in solution (ionic reactions). This convinced me that such reactions are subject to memory propagators - a sure-fire signature of Q.E.D. It is this particular aspect which had such a dusty reception (apart from the one scientist who understood Q.F.T.) and which persuaded me that this research had to follow "an hidden agenda".

Setting Q.E.D. as a precondition for any given research topic would simply have led to instant termination of that particular research!

Following my move to Southampton (in 1967) we developed a number of topics aimed at illustrating the effects of Q.E.D. more directly but relying on "hidden agendas". The students and Post Docs never knew the underlying purpose of the miscellany of topics. What I was really aiming to do was to assemble a series of results for a publication round about my 70th Birthday illustrating the need to use Q.E.D. for condensed matter systems as well as the fact that one can probe such effects using electrochemical methodology.

In due course, we had collected quite a series of results illustrating a)-f) but much of this work was in the nature of deviations from the behaviour one could predict using Classical and Quantum Mechanics. It was clear therefore that one should look for demonstrations of g), demonstrations which should unequivocally require the use of the Q.E.D. paradigm. This was really somewhat naïve: the real outcome was that people denied the reality of the observations (i.e. because we had used C.F. as a vehicle for probing (g)).

There are several reasons for my rehearsing this old story to you. The first is that the C.F. Saga was just one aspect of a much wider programme of work. The only people who ever guessed that this might have been the case were Mike Melich, Giuliano Preperata and Emilio Del Giudice - and G.P. even guessed what some of the other topics might have been. It might well be that you and Pam might wish to mention that C.F. was part of a wider programme aimed at showing that electrochemical measurements could be used to probe the applicability of the Q.E.D. paradigm? If you decide to do this, then you might care to mention that electrochemical methods have the accuracy and sensitivity to probe such effects (the ability to measure small signals for small systems, increases in sensitivity by using modulation methods etc).

A second reason is that I have some residual wish to publish a "70th Birthday Paper". Indeed, some of my feelings of disappointment are due not so much to the adverse publicity about C.F. as the fact that our work on this topic effectively terminated all the other projects

which would have gone into the "70th Birthday Package". Just recently I gave a final final lecture in Southampton which had the theme of "Unfinished Business". It dealt with These Q.E.D. effects but I never got round to dealing with C.F.

However, I should also deal with (ii) namely, the question of why we chose C.F. as an example of g). After all, we could, for instance have extended some work on electrocatalysis which we had started (I am sure that catalysis will turn out to be a prime example of a phenomenon which can really only be understood in terms of Q.E.D.) However, I felt that work on topics of this kind would lead to endless argument and therefore be inconclusive. However, however there was an overriding consideration which is the basic reason why I have labelled this letter as being CONFIDENTIAL. This reason was that once one started to think about processes in condensed matter in terms of Q.E.D., one could see the way in which one might explain, modify or extend various weapons technologies or, perhaps, even develop new technologies. I stress that this is simply a might, by no means a certainty. You will see that I have throughout been less than candid about this aspect which ie one reason why my comments on C.F. are so diffuse.

The problem here is that those who are familiar with the work I have done (especially the unpublished work) will have been able to guess how I might have chosen to develop the topic of C.F. if my concern had been National Security. Incidentally, if they have guessed this, then they should also realise that I have studiously avoided work which would lead in that direction. As against this, it seems pretty clear that A.N. Others have some understanding of what may be involved.

Needless to say, one cannot mention these particular aspects but I thought that you should know that there were good reasons for my wishing to have the project classified. It may well be, therefore, that concern about these matters explains the strange behaviour of the Establishment regarding the matter of C.F.

I want to turn now to the rest of the material you sent to me and, first of all, some general points. Firstly, although you have eliminated the spread sheets, we should perhaps offer to make them available to those interested in checking up/modifying/extending our analyses? An offer to make the spreadsheets and raw data available for study might simplify the text in places?

Secondly, I am concerned about the figures. Clearly, it it is best to give as much as possible of the account by means of such figures but, as they stand, the legends and scales are quite unreadable. Would it perhaps be possible to add a second set of axes on which we could add scales and legends using the type-face of the rest of the articles (Roman 8-point?) I am attaching a sheet by way of illustration.

Thirdly, some specific comments on the Figures. Fig 1 should be replaced by something like Fig 1 of the paper "Case Studies of Two Experiments carried out with the ICARUS-Systems" (the enclosed paper which we gave at ICCF 8). [103] The version which you have used has a "Stan

[103] JR Miles, M., M.A. Imam, and M. Fleischmann. *"Case Studies" of Two Experiments Carried Out With the ICARUS Systems*. in 8th International Conference on Cold Fusion. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy, http://lenr-canr.org/acrobat/MilesMcasestudie.pdf

Pons" feel to it. It is certainly inaccurate: N.H.E. may have followed this design which could explain some of the peculiarities of their results. I note next that Figs 5 and 18 are mirror images of Figs A.4 and A.18 of my 1999 Report and this will have to be corrected. I see also that some of the axes have been lost as I have noted on the relevant figures; furthermore, the legends given for some of the figures are incorrect - I believe that I have noted all the necessary corrections, A major correction which we will need to make is that we need to insert a further figure (which I have labelled Pig A) at the bottom of page 14 of your text. The important point here is that Fig 13 (Fig A12 of the 1999 Report) shows discontinuities in the heat transfer coefficient at t=t, and t=t2 which disappear when one uses the correct water equivalent see Fig A (Fig A.13 of the 1999 Report). Of course, there should not be any discontinuities.

I also believe that we should include a reference to the ICARUS-14 calorimeter and show this as an additional Figure (this is Fig A.27 of the 1999 Report). The important point here is that this design would have allowed the experiments to be carried out with calorimeters having a single, unique, value of the true heat transfer coefficient which would have side-stepped all the tedious arguments. Here I am driven to ask: why exactly was I prevented from developing this aspect of the project?

In a somewhat related way, you will see that I want to extend your reference list to include (2A) under your heading (2) where these two references should be to the <u>two</u> Handbooks for the ICARUS-Systems (References (1) and (2) of the 1999 Report). Related to this you will see that I have given very extensive references in the ICCF-8 paper. The reason is that N.H.E, were repeatedly told how the experiments should be carried out and evaluated: there was really no excuse for all the mistakes which they have made if, indeed, they were mistakes! They also received several letters from me as well as an offer for us to set up a group charged with the task of evaluating the data. The task of the parent groups would then have been reduced to validating such evaluations. However, I never received <u>any</u> reply to <u>any</u> of my letters, not even acknowledgements nor any enquiry about the execution and evaluation of the data.

Strange? Can you be surprised that I concluded that they did not really want to see any positive results? However, can we not go somewhat further in our (private) conclusions? Don't their actions vie-a-vis Mel show that they were actually dishonest?

You will also see that I have noted in places that the axes in some of the Figures will have to be checked/corrected because I made a mistake regarding the volume of the electrode. I believe that you and Mel will be able to sort this out.

Finally (for this Section) I should apologise for the mess I have made in the red-lining - mainly due to my doing various bits at different times. I have kept a copy of Parts I and II and I believe that we can sort out the text in one or two exchanges of letters (or 'phone calls?).

Finally, finally, some comments on Part I (Volume II?). I believe that most of my corrections will be clear from the red-lining. However, I cannot relate this Part I to your list of contents for Volume II. Presumably Sections 5-8 (possibly also a part of Section 4) still have to be added? We certainly need some illustrations to show that the evaluation strategies are satisfactory.

Somewhat related to this aspect is the question of whether we should not mention somewhere (in your Section on The Emergence of Cold Fusion?) that various evaluation methods have been used during the development of the project (eg. non-linear regression fitting) but that the present report is confined to one particular methodology, the ICARUS Evaluation Strategy, developed during 1992. This methodology was included in the ICARUS-1 and -2 Systems supplied to N.H.E.

Onwards, Stan, towards your objectives and, hopefully, my wish to see Q.E.D. take centre stage in this next Century.

Regards,

Martin

2000-07-20

Melvin H. Miles, Ph.D.
Chemistry and Materials Branch
Research and Technology Division
Code 4T4220D
Naval Air Warfare Center Weapons Division
China Lake, CA

FAX MEMO

DATE: July 20, 2000

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

TIME: 10:00 a.m.

Number of pages including cover sheet: 7

MESSAGE:

Dear Martin,

Thank you for your fax of July 16, 2000. It is probably I that owe you replies on a number of issues. I will respond to your previous faxes by their date. First, however, I am faxing you a copy of a recent letter that I sent to Dr. Asami. Second, there will be a meeting today to discuss the audio and video taping subject of Italy. Next week there is a scheduled meeting involving the head of the Research Department. If there is interest in pursuing this topic at China Lake, would you be willing to make a trip here sometime for further discussion and planning sessions? However, I don't know what outcome will result from these initial meetings.

July 17. 2000 Fax

My wife and I have been looking for the Pd-Ce data on her computer, but we have not been able to locate it. We had some repairs done on our computer, hence I hope that it was not lost. We sent the data to Mike Melich on a zip drive, and we have asked him to send us the Pd-Ce data by email. I am faxing two graphs from NHE for this Pd-Ce cell.

July 5. 2000 Fax

I am working with Stan Szpak and MA. Imam on the Navy reports. Stan wants me to write 10-15 pages that will take up my free time for the next week or two.

June 26. 2000 Fax

I would prefer not splitting the ICCF-8 paper into two parts. I assume by now that the Italians have decided to publish this as a single paper. I would agree that the critics have largely succeeded in comprehensively destroying this subject. However, they did this by ridicule rather than by any scientific method. I cannot believe Doug Morrison's report on ICCF-8. It makes me wonder if he is not on a different planet.

June 13. 2000 Fax

I tried to find data on the solubility of LiOD in D20 as the solution is evaporated to dryness - but found nothing. Perhaps I could do the experiment here sometime on a weekend. Our elevation here is 2500 feet, hence the atmospheric pressure is generally 690-700 torr. What effect would this have other than lowering the boiling temperatures?

I hope that Jean-Paul Biberian has sent you the data sets used at ICCF-6. I would favor including him as an author if he delivers the goods. We need to archive hard copies of all data sets. My apparent loss of the Japan data sets make this painfully clear.

I hope this gets me somewhat caught up with the questions in your faxes. I will keep searching for the Pd-Ce data.

Best wishes,

Mel Miles

Mel Miles

Melvin H. Miles, Ph.D.
Chemistry and Materials Branch
Research and Technology Division
Code 4T4220D
Naval Air Warfare Center Weapons Division
China Lake, CA

July 17, 2000

Dr. N. Asami
R and D Center for New Hydrogen Energy
The Institute of Applied Energy
2-5 Nishishimbashi 1-Chome Minato-Ku
Tokyo 105, Japan

Dear Dr. Asami,

I am enclosing a copy of my paper (J. Electroanal. Chem., Vol. 482, 2000, pp. 56-65) based on studies that I conducted at NHE using my China Lake calorimetric cells. I am also enclosing copies of all referee comments, my reply letter, and the letters from the editor, Dr. Roger Parsons. The data analysis is much improved over what I was able to do in my NHE Final Report. You may recognize some of the figures since I showed them to you from the original chart recordings while I was working in Japan. I will be happy to share the experimental raw data with anyone who is interested.

The referees and editor all agree with my conclusions that a real excess power effect was produced in Cell A in contrast to Cell B. This also agrees with my previous experiments using these same two cathode materials at China Lake.

I would welcome any discussions, questions, or criticisms from anyone interested in my results. This is a very difficult field, and I hope we all can be tolerant of criticisms and remain friends as we search for the scientific truths relating to excess heat measurements.

I certainly enjoyed my brief experience at the NHE laboratory in Japan. This was one of the highlights of my scientific career. I found you to be an excellent supervisor, and appreciate the freedom you permitted for my work in Japan.

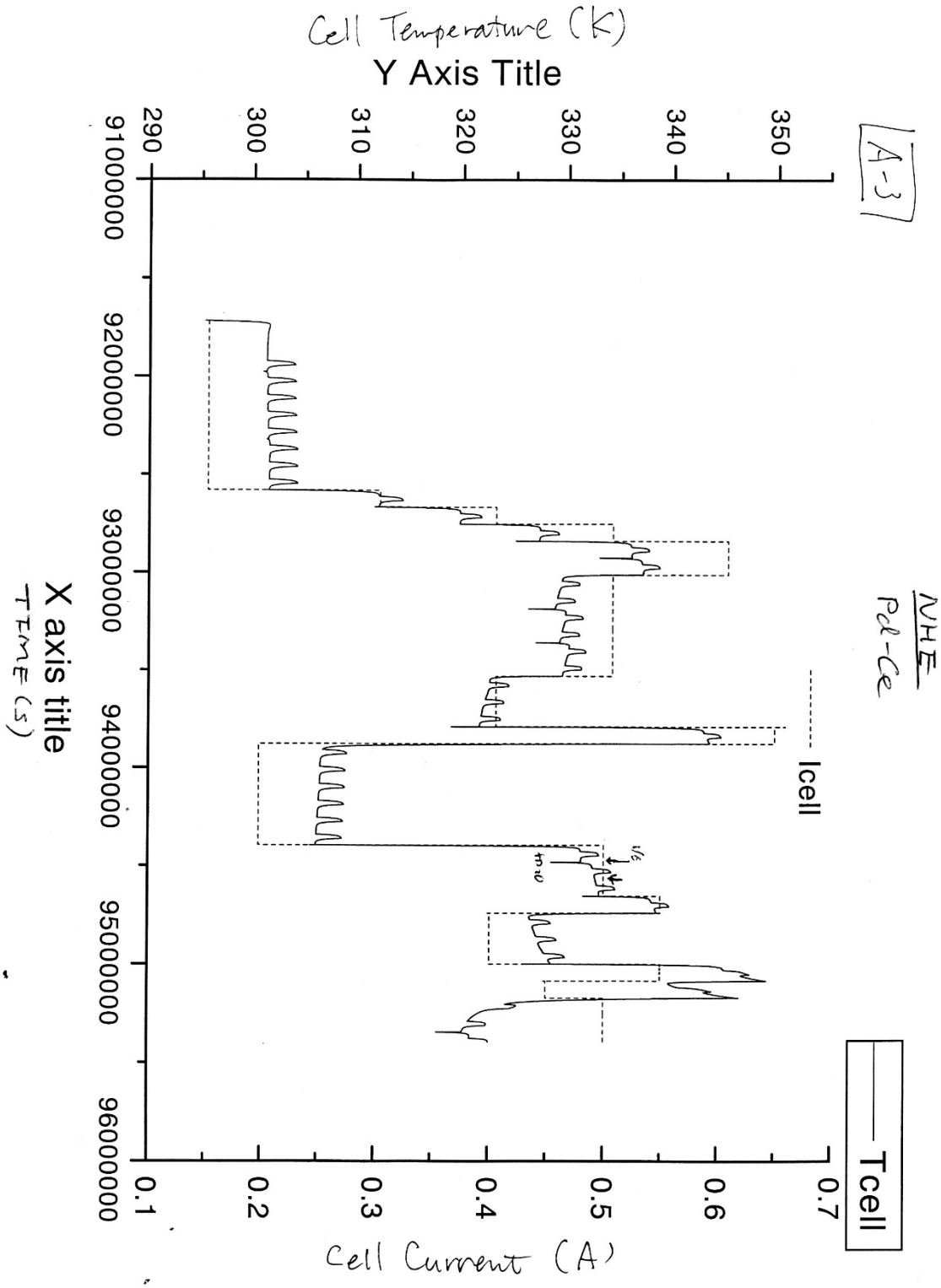
Sincerely,

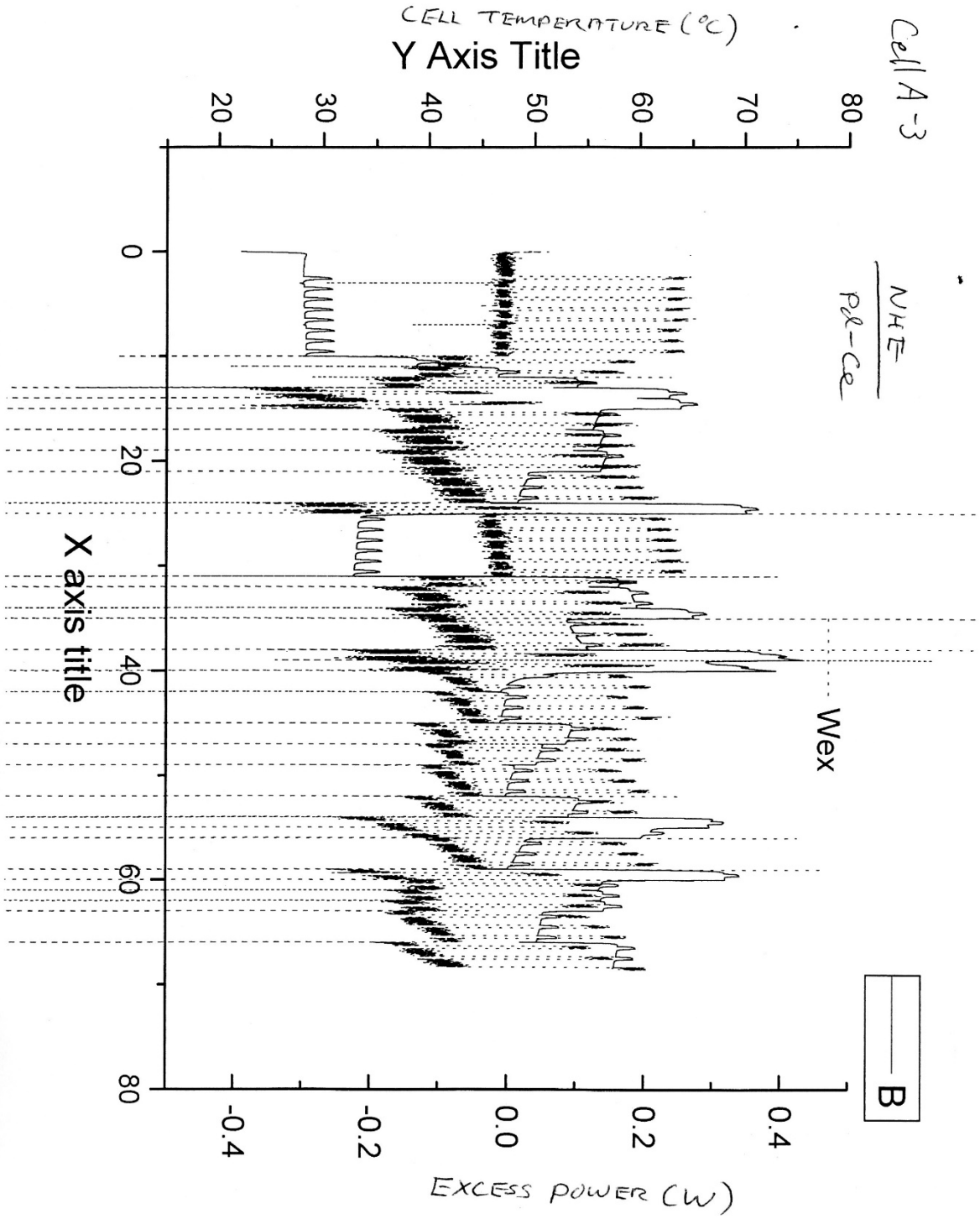
Melvin H. Miles

Dr. Melvin H. Miles
NAWCWD

copies: K. Matsui, M. Sumi, E. Kennel

P.S. Please send Mari Hosoda the extra reprint with my note to her.




Cell A-3
NHE
Pd-Ce
CELL TEMPERATURE (°C)
Y Axis Title
80
70
60
50
40
30
20
Wex
B
0.4
0.2
0.0
-0.2
-0.4
EXCESS POWER (W)
0
20
40
60
80
X axis title

2000-09-27

Bury Lodge heading

27th September 2000

Dear Mel,

Herewith some of the most important pages. We should be able to clean up the rest reasonably quickly when we meet.

I can't find the certificate of posting – 3 weeks seems to me to be an impossibly long time for fast AirMail!

No news or comeback from the M.O.D. Also, to date no Airline Tickets from San Diego.

I am suffering from an attack of black thoughts.

You will need to read the corrections in conjunction with my letter of 5/9/2000

Regards,

Martin

P.S. My application to the M.OD. has now been sent to the International Visits [???] Office, which I believe is back in London. Time's running out!

2000-10-30

Melvin H. Miles, Ph.D.
Chemistry and Materials Branch
Research and Technology Division
Code 4T4220D
Naval Air Warfare Center Weapons Division
China Lake, CA 93555-6100 USA

October 29, 2000

Professor Martin Fleischmann
Bury Lodge, Duck Street
Tisbury, Wilshire, SP3615
Great Britain

Note: Cu rod cathode was 4 mm × 25 mm

Dear Martin,

Enclosed is the data set [on a disk] for the co deposition experiment in Cell A2 that was started immediately following the Pd-B experiment. Since this is the same cell, I think we can assume the same radiative heat transfer co-efficient (0.85065×10^{-9} WK^{-4}) and the same water equivalent for the cell (450 JK^{-1}). The D_2O solution contained $PdCl_2$, ND_4Cl, and ND_4OD. I will copy pages from my notebook that give complete details. The PdCl2 deposits as Pd onto the copper cathode. There were periods of chlorine evolution, but this would minimize the excess heat if we use the thermoneutral potential for D_2O electrolysis only. Even the NHE calculations show excess heat for this experiment. I am enclosing a copy of their results in figure form plus my own figures showing the cell temperature vsrtime and the cell voltage vs. time. This includes the three heating pulses, but only the second pulse'shows anything close to normal. The cell voltage is somewhat unstable since the palladium deposit comes loose causing changes in the electrode area. Therefore, it will probably be a challenge for you to analyze this data. Nevertheless, I am sure Stan Szpak will be very pleased if you can do this, and would probably publish this in another Navy report as well as in some journal. These publications will include your name, Stan Szpak, and myself.

Regarding the Pd-Ce and the Pd-Ce-B data from my experiments at NHE, Mike Melich is suppose to send me this data on a CD. I will also try to get this data to you in printed form sometime in the near future.

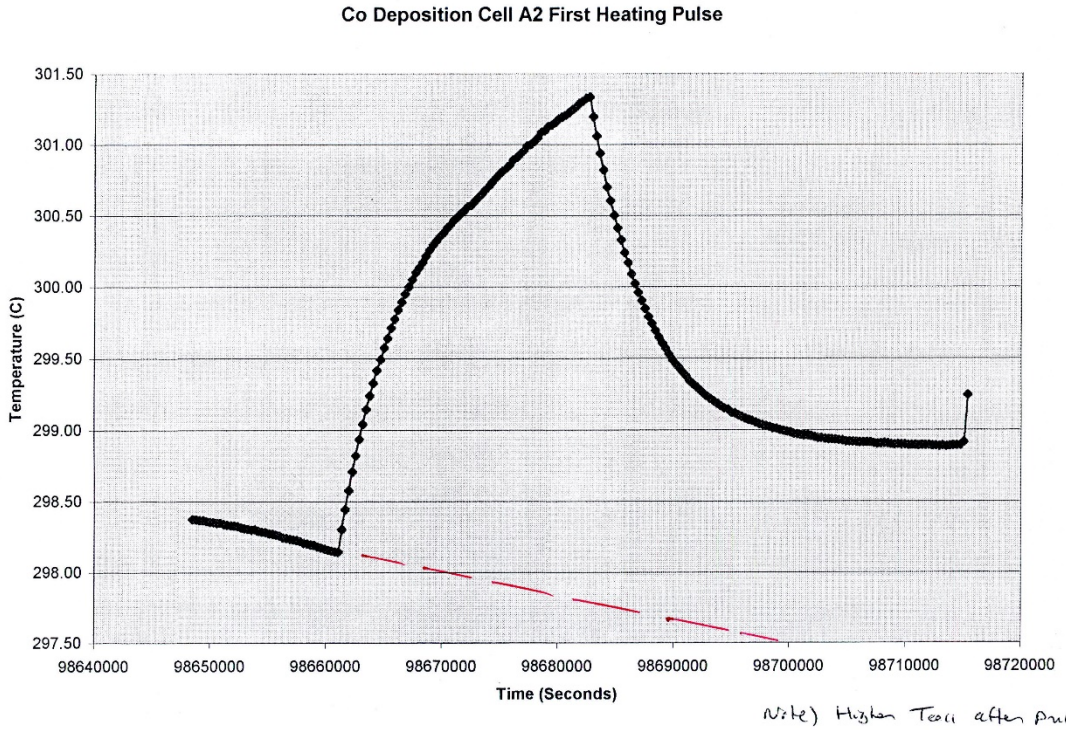
Thanks for your fax from Italy. I hope your health is holding up with all this travel. I would like to do the experiment you suggested, but it will probably have to wait until the two Navy reports are completed. The NRL report is nearing completion, and I will be sending you a copy for any last minute corrections.

Best wishes,

Mel Miles

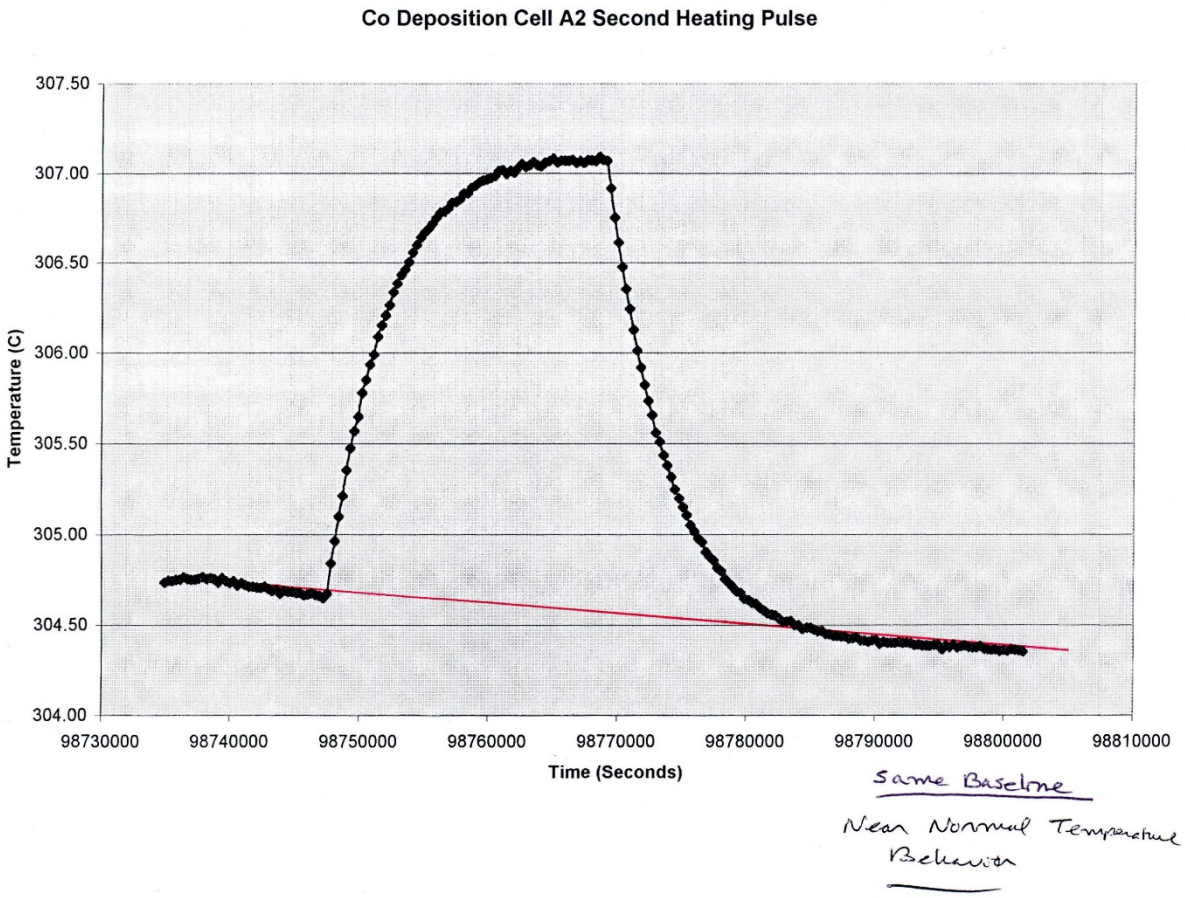
Mel Miles

NHE used this first heating pulse to get their cell constant.
$k_R = 0.699861 \times 10^{-9}\ W K^{-4}$ (NHE)



Higher Baseline After pulse

Note) Higher Tcell after pulse while Ecell is lower.
⇒ Positive Feedback.



[JR Note: In both of these graphs the units in the Y-axis should be degrees K, not C. The Time (Seconds) in the first graph is 98,730,000 s. That is 3 years and 2 months. Miles explained that the computer clock that generated the time stamp on these graphs was never set back to zero. The two graphs are 90,000 s apart, which is almost exactly 1 day.]

2000-11-11

Bury Lodge heading

11th November 2000.

Dr. Melvin H. Miles,
Chemistry and Materials Branch.,
Research, and Technology Division,
Naval Air Warfare Center Weapons Division,
China lake, CA 93555-6100, U.S.A.

Dear Mel,

Many thanks for your FAX of 30/10/2000 and the package of 29/10/2000 which, as you will see, I have now received, I was pleased to hear that Stan is making good progress [104] and please give him my good wishes if you have occasion to speak to him.

The details of the codeposition experiment are very interesting and convincing and I am sure that we will be able to go somewhat further with the analyses of these experiments. I have so far only had a preliminary look at the data sets and the other documentation and I was relieved to see that you addressed the question of the possible recombination of D_2 and O_2 in the relevant section of your report to N.H.E. Evidently, we will be able to assume that to a first approximation we are observing the electrolysis of D_2O with no recombination. However, we should also make a number of other limiting assumptions in these evaluations. I take it therefore that we should launch ourselves on this exercise? As you have said in your letter, it is likely that these analyses will lead to a further paper and I appreciate your suggestion that I should be included in the list of authors!

With regard to the appropriate thermoneutral potential we should note that a little chlorine goes a long way as far as our noses are concerned! The marked drop in the pH of the solution also implies that the dominant anode reaction required to balance the deposition of Palladium is oxygen evolution from D_2O. [105]

As I see it, the main problem with the N.H.E. print out of the data is that the power delivered to the heater has been entered as 0 rather than the actual experimental values. You will recall that this is part-and-parcel of their erroneous data processing strategy which may well have been prompted by a wish to frustrate the accurate ICARUS-type analyses. Do you be any chance know whether the diskette also has 0 entered in column 10 throughout the data sets and/or whether we could retrieve the values of W_{Heat} from the original recordings? Failing this, I could try to reconstruct the original data sets but this would be a labour of love (or hate?). The reason why I would like to have the original data is to demonstrate once again that one should carry out the analysis according to the instructions for the ICARUS Systems. However, as you have said, we do not really need to evaluate the heat transfer coefficients and water equivalents for the cell

[104] MM Szpak had a heart attack. He recovered and lived for many years after this. Stanislaw Szpak (1920 – 2016)
[105] MM $Pd^{++} + D_2O \rightarrow Pd + ½ O_2 + 2D^+$

as the experiment followed immediately on that of the Pb-B electrode. We can therefore use the values for this earlier experiment.

Writing about the Stan Szpak-type experiment brings to mind that we should also be able to take the analyses of the infra-red images somewhat further. [106] Needless to say I am very pleased to see that Lowell[107] endorsed the Spa wars programme. Much of what he said was similar to my own comments on the previous day except that he also suggested that Stan and Pamela should make some fast recordings of optical images. I had not realised that this could lead to diagnostic criteria. Needless to say, this requires new experiments. My own feeling was (and is) that we should also take the analysis of the infra-red images as far as is possible at this stage - if only to define future experiments! You will recall that I believe that these experiments have the capability of resolving the temperature changes due to individual $D^+ + D^+$ fusion events. To this end I would like to examine the sequence of images frame-by-frame and I wonder whether you could discuss with Stan, Pam and Frank whether they could make a video tape (or disc) available (in the 625 line format) so that I can examine this here? However, as I recall, Pam set the cursors on individual spots so as to read off the actual temperatures. What we really need to do is to set the cursors in this way and then to advance the record frame-by-frame. This may require us to use the original instrumentation in which case it might be possible for you to have a go at extracting this information?

It will also be necessary to have precise details of the experimental set-up i.e. the nature and dimensions viewed by individual pixels and anything else which comes to mind. The reason why we will need this information is because we need to set up the relevant model (or models). As I see it the dimension viewed by each pixel will probably be 100 μm, the time required for each data point will be in the range 1-2 μs, and the time lapse between each frame will be 25 ms. The first important question which we should investigate is: what is the time-dependence of the temperature of each hot spot? I expect that there will be considerable variability but some of the hot-spots at least may satisfy the predictions for individual fusion events (or for such individual events coupled to photo fission). Yet again, some of the hotspots may follow the predictions for a defined multiplicity of fusion events.

If we should be able to make some headway with these evaluations, then I will try to set up the appropriate model calculations. There is clearly a large measure of uncertainty with regard to these models: the starting point will be the calculation of the variation of the temperature with time for a source within a crystal light of Pd of given dimensions followed by the variation due to the flux of heat into the surroundings. Clearly, there will be a marked discontinuity in the temperature-time curves. It is for this reason that I want to find out as much as possible about the experiment design; it would also be useful to have any information which may be available about the crystallite size of the Pd deposits.

I should point out here that I believe that experiments using infra-red imaging will be carried out in other research groups in the next 1-2 years. I would therefore like to see the earlier

[106] JR The video and papers about it can be found here: http://lenr-canr.org/wordpress/?page_id=952
[107] MM Lowell Wood

experiments analysed in detail both to "round things off" as well as to set the scene for such further experiments.

This discussion of Stan and Pam's experiments brings me to some comments about the meeting in San Diego. Needless to say I was very glad that the assembled company endorsed the Spawars programme while, at the same time, I was disappointed that this group did not address the issues which concern me - and which were the reason for my coming to San Diego! However, given the composition of the group, the outcome was much as I expected: I always find the matters people evidently don't wish to discuss as interesting as the matters which they do discuss.

Bearing in mind the programme which we wish to carry out, I believe that I should give you some comments about my own perceptions? These comments arise in part from the discussion which we both had and did not have in San Diego.

You know the key factor which persuaded us (me) to initiate the C.F. research. This was based in large measure on my belief that the operation of a certain weapons system can only be interpreted in terms of Q.E.D. A generalisation of such concepts could on the one hand lead to some new technology and, on the other hand could explain some of the strange phenomena which have been observed in developments of Brldgeman's ground breaking work, I knew that the Soviets had been very active in this field - perhaps "knew" is too strong a word; "suspected" might be more appropriate.

This brings me to Mike's [108] first intervention; the request for a thumb-nail sketch of Q.E.D. Quite frankly, I regarded this as a Black Hole from which I would never be able to escape. One can ask: is it possible to give a thumbnail sketch of Cashmir forces of the Lamb shift? Some quite intelligent Physicists even deny that such effects demonstrate that Q.E.D. is the all-singing and dancing paradigm for the 21st Century. However, the critically important point is that most people are completely exhausted by the time they get to the Lamb shift which is just the point at which Giuliano Preparata end Emilio Del Giudice with M.F. in tow, get going. The critical issue here is that the consideration of the effects of the density (note this point!!) Show that the old models of condensed matter need substantial modification (actually, I believe that many of the models are quite wrong). As far as the Pd-D system is concerned, the critical question is that of the formation and behavior of the γ-phase.

You will see that it was not really possible to respond to Mike's question: I had to get to the behaviour of the γ-phase.

This brings me to the "accident" which was clearly of considerable interest. [109] I interpreted this accident in terms of the starting point for our research. This implied that the bit or bits in the concrete would be of no interest whereas the bits embedded in the fume hood would be. [110] Of

[108] MM Michael Melich
[109] JR and MM The Pd-cube meltdown described in Fleischmann and Pons' first paper: Fleischmann, M., S. Pons, and M. Hawkins, *Electrochemically induced nuclear fusion of deuterium.* J. Electroanal. Chem., 1989. **261**: p. 301 and errata in Vol. 263. http://lenr-canr.org/acrobat/Fleischmanelectroche.pdf
[110] MM Because of elemental transmutations?

course, with the benefit of hindsight, we should perhaps have stopped the research at that stage and carried out a comprehensive investigation of the inside of the fume hood. However, we were not prepared for the early demise of the project and we believed that we could return to the "uncontrolled release of thermal energy" at a later date with experiments under more controlled conditions.

The meeting then dealt (in a very wordy way) with Stan and Pam's experiments, and some other matters, and, of course, I was pleased with the outcome. Perhaps we should discuss the "other matters" on another occasion?

Right at the end I tried to pull the discussion back to the matters which concerned me. I started by making a linkage to Frank's [111] own interests although he did not seem to appreciate the point. This was intended to be an entrée to the question of the change in sign of the enthalpy of absorption with increasing state of charge and/or the possible formation of the γ-phase. Of course, I believe that the latter interpretation is correct and the most significant development in recent times has been the understanding of the $\beta \rightarrow \gamma$ transition developed by Giuliano and Emilio. It is this understanding which should make it possible to develop a rationale for the effects of perturbations other than the Coehn-Aharonov effect, as well as the combination of various perturbations.

You may recall the outcome: I was again stopped dead in my tracks but I found this particular intervention very interesting. I said to myself "right, you know all about the γ-phase and you have probably known about this all along. In that case it is unlikely that I can tell you anything which you don't already know." It is relevant here that certain people at the meeting were very well-informed about the research which has been carried out and had a detailed knowledge about my own work. All this strengthened the opinion which I have had all along: the establishment(s) (or, at least certain members of the establishment(s)) have known since 1989 that Stan Pons and I were right (at least in outline) about this research and also, that we were right in having reservations about what we call euphemistically the "uncontrolled release of thermal energy". I do not know whether the establishment(s) can read my mind as to what research I would carry out to further the aspects which I do not want to further. The only person I have met who was ever able to read my mind to that extent was Giuliano Preparata.

I will tell you why I believe this when we next meet.

The final points which I would have liked to make at the meeting were: we have a powerful energy source and, at present there seems to be no obstacle to developing this into viable technologies; this energy source is not threatening to existing interests; more than 50% of the world's oil reserves have been exhausted in the recent unrest in Europe is a good foretaste of what will be in store for us all. It is therefore incumbent on us to develop this energy source. At the same time, I believe that there is a very large flipside to this research. If I am right with my interpretation of the effects of earlier weapons technology, then the political fallout attendant on its use will be an absolute disaster zone. I also believe that I am right with my interpretation, a fact which is likely to be established by the others in the next 1-2 years (my lips are firmly

[111] MM Frank Gordon

sealed, as always). We should therefore take steps to cope with any political fallout and I have my own ideas about what these steps might be. However, it is likely that this research will be described with the usual epithet: too little and too late. So we are left in the same position as always: we must just do the best we can and fill in a few of the blanks.

One of these blanks is certainly the work which you have done on the Pd-Ce system. I was glad to see that Mike Melich is going to produce a CD of this data set and I hope we will soon be able to start work on this venture.

Best regards,

Martin

P.S. I will write to you about your calculations of the enthalpy losses due to evaporation in due course.

P.P.S Before going to Italy I tried to complete the correction of the M.S. we were dealing with in San Diego. However, I got completely stuck with the question of whether we had used the forward or backward integral in deriving the true heat transfer coefficient for the cell in the presence of positive feedback la whether this heat transfer coefficient is $(k_R')_{262}$ or $(k_R')_{362}$. The text is certainly confused about this matter which is probably my fault.

I have to resolve this question this weekend by going through the original calculations and I wonder whether you could tell Pam about this and give my apologies for the delay?

PPPS. You may wish to show the tail-end of my letter to your friends (or even the whole letter).

PPPPS. Your Pd-D co-deposition experiment shows some interesting Heat-after-Death effects. More anon.

PPPPPS. Working my way into this data set persuades me that it would be very interesting to investigate Cu fluidized bed electrodes in the co-deposition version. There is a considerable amount of information available on fluidized bed electrodes (i.e. Cu fluidized beds). The electrode deposits on such beds have good electrochemical stability; fluidized beds are very good heat transfer media; use of such structures would allow one to design in heat-after-death into the mode of operation. More anon.

2000-11-27

Bury Lodge heading

27th November 2000.

Dr. M. H. Miles,
Chemistry and Materials Branch,
Research and Technology Division,
China Lake, CA 93555-6100,
U.S.A.

Dear Mel,

I am afraid that I have been absolutely snowed-in with work during this month so that I am late (as usual) with my reply to your letter of 7/11/2000 and the corrections to the page proofs. In view of the delay, I am sending you the key pages of the red-lined copy via FAX. I am sure that you will be able to incorporate the corrections into the text you will have kept and that you will also be able (if you and Imam so wish) to take account of the two general comments I will make in the next three paragraphs. however, I will also send you today the whole of the red-lined text via Swift-Air.

The first of my general comments is that we have not followed a consistent style in the use of brackets to statements such as (k_R'), (B.11) possibly also to (Footnote 0.6.). This means that other statements in parentheses such as "see below" should be in square brackets ie [see below]. It is then logical to use both types of brackets in statements such as [see equation (B.11)]. I realise that some people would maintain that the style of brackets should follow sequentially as in mathematics ie ([{ etc. In that case we would use (see below) and [see equation (13.11)] . However, it is somewhat marginally important to use a consistent style.

The second general comment is that we have not followed a consistent style in the use of quotation marks, commas and dashes. I believe that we should use quotation marks for statements such as "lower-bound heat transfer coefficient" and dashes for statements such as (k_R'). If you could check the text, then you will see that we have frequently used commas instead of dashes in statements such as (k_R') and, sometimes, dashes in place of quotation marks in statements such as "lower-bound heat transfer coefficient".

I should emphasise that the text is perfectly readable as it stands - and I don't want to give Linda yet more work! However, this Report is likely to be an important Source Document so you and Imam may well decide that we should be rather fussy in the matter of appearance of the text.

Next, the further corrections which are as follows:

page 9 line 29: delete)

page 52 line 32: delete o

page 13 line 34: delete)

Page 14: line 13: add “ (note: quotation marks!) V

lines 40 and 41: add “ (again note: quotation marks).

page 15: line 9: add “ (quotation marks).

page 17: line 41: add s

page 18: line 6 : add “ (quotation marks)

line 23: add “ (quotation marks)

line 25: add “ (quotation marks)

As regards your questions: the text is correct. It is the header for Table A.5. in the Report which is wrong. I am enclosing the corrected version of the header (pages 189 and 190 of the Report). Sce also further comments below.

Page 19: line 17: add “ (Quotation marks)

Again, the text is correct.

[Other corrections omitted]

. . .

As regard to your question, there are admittedly some missing steps between equation (B.19) and the relevant elimination of Q using equation (B.22). I have indicated the relevant equation. However, do we really need to explain this point?

. . .

page 41 equation (B.25A); change $(k_R')2$ to $(k_R')12$

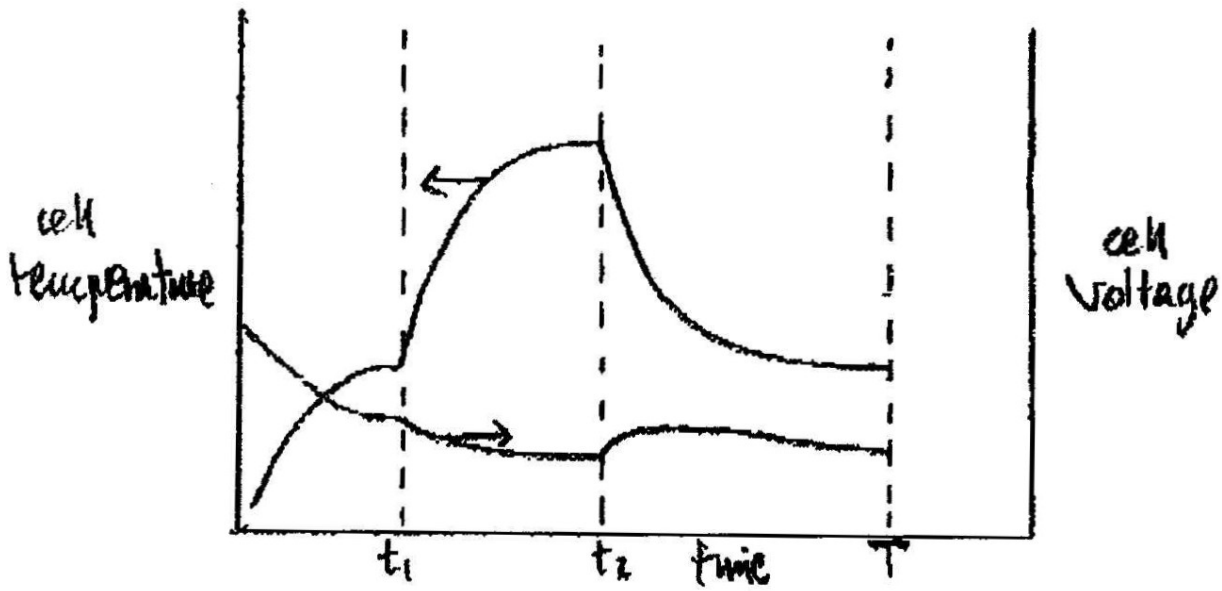
page 42: see my additional comments.

page 60: line 12: change E.10 to B.9.

page 61: add underscores to References 1 and 2.

There are also three general further comments. First of all, I am not sending these pages on which there are your corrections alone. I take it that you have kept a copy of the text? Secondly,

there is a reference in Section B to Figure B.1 but, as far as I can remember, we never produced such a Figure. If you and Imam believe that we should include this, then a sketch would do:



Thirdly, one reason for the delay in my return of the proofs is that I wanted to check that our version of the "true heat transfer coefficient" really was a modification of $(k_R')_{262}$. You may recall that I began to have doubts about this (and these doubts have also held up my vetting of the SPAWAR proofs). I went back to the original calculations and confirmed that we did indeed calculate $(k_R')_{262}$, that columns 14, 15 and 16 of Table A5 are correct and that everything is in apple pie order.

Next, some steps towards the future. First of all, I have made some progress with the analysis of the calorimetric codeposition experiment which you carried out in Sapporo. It seems to me that if will only be possible to investigate the calibration of the cell from the data for Day 6 of this experiment although the apparent simplicity of the temperature-time series is probably partly illusory because of a progressive fall in the rate of excess enthalpy generation during most of the period $0 < t < t_1$. More about this in due course.

Anyway, herewith some results of the reanalyses of the data sets. Fig. 1 shows $10^9\ (k_R')_{11}$ for Day 6 and the N.H.E. value of the "true heat transfer coefficient" which I take to be $10^9(k_R')_{362}$. The value, 0.699861 WK^{-4}, is much smaller than the rock-bottom-minimum value for purely radiative heat transfer and, of course, is also much smaller than the value 0.793504 WK^{-4} which they determined for the Pd-B experiment in the same cell. Needless to say, this did not arouse any suspicions in their minds and, I take it, has not led to any discussion? However, I note that, even with the new value, we would calculate substantial excess enthalpy generation even on Day 6!

I note that $10^9(k_R')_{11}$ is substantially constant in the time region just before and just after the application of the heater calibration pulse at t = t1 which implies that the rate of excess enthalpy

generation is constant (and small) in this time region. We can therefore derive $(k_R')_{11}$ and $(k_R')_{12}$ using this section of the data for Day 6 and Pig 2 illustrates the determination of $10^9(k_R')_{161}$ as well as C_pM. The value of $10^9(k_R')_{161}$ is also shown on Fig 1 - satisfactory? Fig 3 illustrates the determination of $10^9(k_R')_{162}$ - again shown on Fig 1. Note that the value is close to the value which we found in the Pd-B study (as it must be!) In actual fact, one can make a strong argument that one should base this evaluation on the data points for low values of the absuissae which would give $10^9(k_R')_{162} = 0.85573WK^{-4}$, even closer to our previous calibration.

So as you will see, everything is eminently sensible. These values of the "true heat transfer coefficient" give specific rates of excess enthalpy generation in the range 30-35Wcm^3 [112] broadly speaking in line with our first investigation for this range of cell currents! However, I note that the efficiency for excess enthalpy generation ~70% is much higher than in our first investigation (ignoring the "bursts") no doubt due to the higher electrolyte concentration in the codeposition version.

I also note that your data sets show two interesting regions of "Heat-after-Death" and this, together with the relatively high efficiency brings me to the next point and the second set of Figures 1-3. I will explain the reason for the odd numbering later in this letter.

In a P.S. to my previous letter, I mentioned that the time is now ripe ta investigate fluidised bed electrodes and I have made much the same point to Frank Gordon.

Some of the reasons are as follows:-

i. The behaviour of copper fluidized bed electrodes has been extensively investigated:
ii. electrodeposition onto such beds can be carried put from dilute solutions and the deposits have very good mechanical properties;
iii. fluidized bed electrodes can be set up in a variety of configurations depending on the positioning of the "current feeder electrodes" and the relative directions of current and fluid flow;
iv. in relatively dilute solutions, the electrochemical reactions will be confined to special zones of the beds while parts of the beds will be polarized at low current densities;
v. in common with fluidized beds in general, such electrodes have very good heat and mass transfer characteristics.
vi. fluidized beds operate under "plug flow" conditions and plug flow would simplify calorimetric measurements in such flow systems.
vii. fluidized beds give a simple means of obtaining high specific area. electrodes.
viii. certain configurations of fluidized beds are very easy to implement.

Of course (i) and (ii) are relevant to the deposition of Pd onto Cu; (vi) is relevant to calorimetry; (iv) is relevant to exploiting Heat-after-Death; (v) and (vii) are relevant to scale-up and (iii) and (viii) are illustrated by the simple design shown in Fig. l which has already been used in studies of eleotrodeposition. Porous frits can be obtained already sealed into Pyrex and a Competent glassblower could assemble such an apparatus in "a heartbeat". If a glassblower

[112] MF *At a cell current of 300 mA.*

should not be available, then I could arrange for the construction of the apparatus here. If one bases the design on a 2+4cm diameter frit and tubing, then I believe that one would have to budget for flow rates in the range 5-100cm^3 s^{-1} (this affects the range of rotamoters needed).

If one next translates this design to measurements on the Cu-Pd-D system, one arrives at the design shown in Fig 2. I have shown temperature measurements using thermisters but one might well use thermocouples or Pt resistance thermometers (or even thermometers!) Of course, with the use of D_2O based electrolytes, the question of contamination by H_2O becomes important and this will affect the design of the reservoir/heat exchanger. It might well be that this could be based on a reflux condenser. The question of an appropriate sealed pump will be important. I believe that the flow rates will be out of the range of peristaltic pumps but, of course, the chemical industry abounds with designs for pumps. Ingress of H_2O could be reduced by surrounding the key parts by plastic bags filled with dessicants.

Now for the foreseeable Achilles Heels. In the first place it does not follow that this particular design of fluidised bed will be useable at very high current densities because D_2 evolution could well interfere with the operation of the beds. A simple modification could be to place the cathode feeder electrode at the top of the bed. Secondly, it does not follow that such systems could be used for excess enthalpy generation at very high specific rates. The reason is that such high rates appear to be dependent on the deposition of porous resistive layers and such layers are known to impede charge transfer in fluidised bed electrodes. The question of whether or not this will be a limiting factor will depend on whether the collisions of the particles will disrupt the resistive layers sufficiently to allow natal-metal contacts. The behaviour of these systems at high current densities will therefore be highly dependent on particle size and/or fluidisation velocities.

Some further points: calorimetric measurements may require a flattening of the velocity profiles using a device such as that in Fig 3: if fluidized beds prove to be successful, then a device such as that in Fig 2 could open the way to a rapid realisation of water heaters and other utilisations of sources of low grade heat; the effects of the thickness of the Pd deposits could prove to be of special interest; fluidized beds were to have been investigated by our "engineering" colleagues in Salt Lake City and we even procured the starting materials for them but, as usual, nothing happened!; of course, the main aim here is to exploit "Heat-after-Death" but such a study should be accompanied by investigations of conventional electrodes under pulse conditions.

I now return to the reason why these diagrams are labelled 1-3 and not 4-6. Originally I had intended to send them to Frank as part of a follow-up letter but he has written to tell me that he and Stan have in fact discussed the question of using fluidized bed electrodes. Now the point is that I don't want him to think that I want to intrude my ideas on their research programme (nor do I want to intrude on yours!). It also seems to me that they have started to work very intensively on Lowell's suggestions. That is an excellent programme and it might well be that a successful outcome of this programme could change the climate of opinion if sufficiently powerful figures were to "push the boat out". however, I think that the likely consequence would be that the work would be placed in a compartment labelled "a low level signature of no

consequence to the earlier work". There it will join other positive results such as those on transmutations. I continue to think therefore that the only valid course of action is to implement energy sources. I therefore wanted to set down these initial ideas but I also want to ask you whether or not I should include this topic in my next letter to Frank?

Could you please ask Linda whether she knows of a non-prescription medicine to act as an antidote to C.F. How was the meeting in Washington?

Regards,

Martin

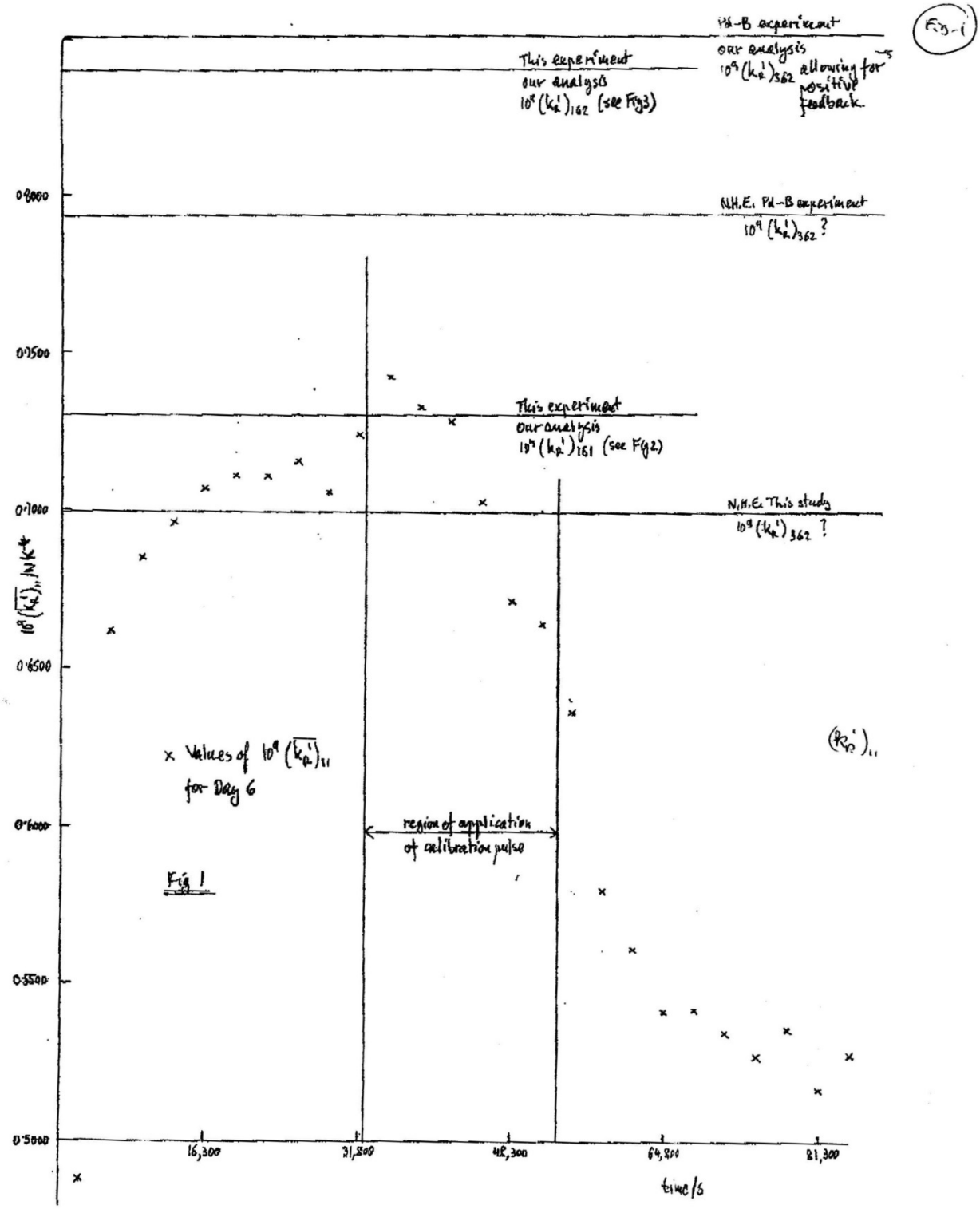


Fig-1
Pd-B experiment
our analysis
$10^9 (k_n')_{362}$ allowing for positive feedback.
This experiment
our analysis
$10^9 (k_n')_{162}$ (see Fig3)
N.H.E. Pd-B experiment
$10^9 (k_n')_{362}$?
This experiment
our analysis
$10^9 (k_n')_{161}$ (see Fig2)
N.H.E. This study
$10^9 (k_n')_{362}$?
$10^9 (\overline{k_n'})_{11}$ /NK⁻¹
0·8000
0·7500
0·7000
0·6500
0·6000
0·5500
0·5000
× Values of $10^9 (\overline{k_n'})_{11}$ for Day 6
region of application of calibration pulse
Fig 1
$(k_n')_{11}$
16,300
31,500
48,300
64,800
81,300
time/s

Fig-2

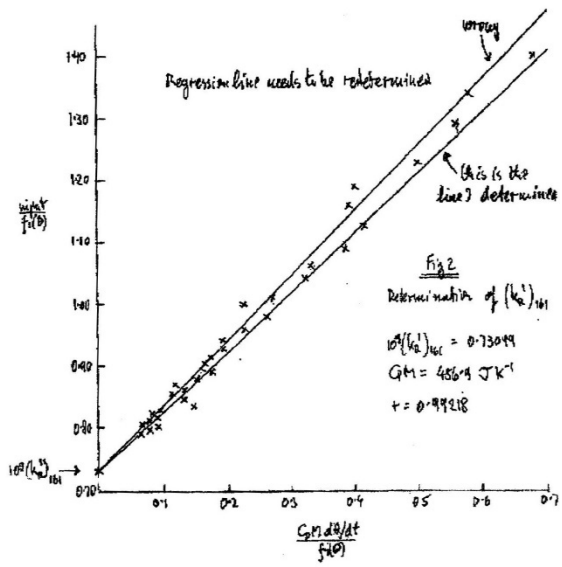

Regression line needs to be redetermined
this is the line I determined
Fig 2
Determination of $(k_2')_{161}$
$10^9(k_2')_{161} = 0.73049$
$GM = 456.9\ JK^{-1}$
$r = 0.99918$
$10^9(k_2')_{161}$
$\frac{\text{input}}{f_1(\theta)}$
$\frac{GM\,d\theta/dt}{f_1(\theta)}$

Fig-3

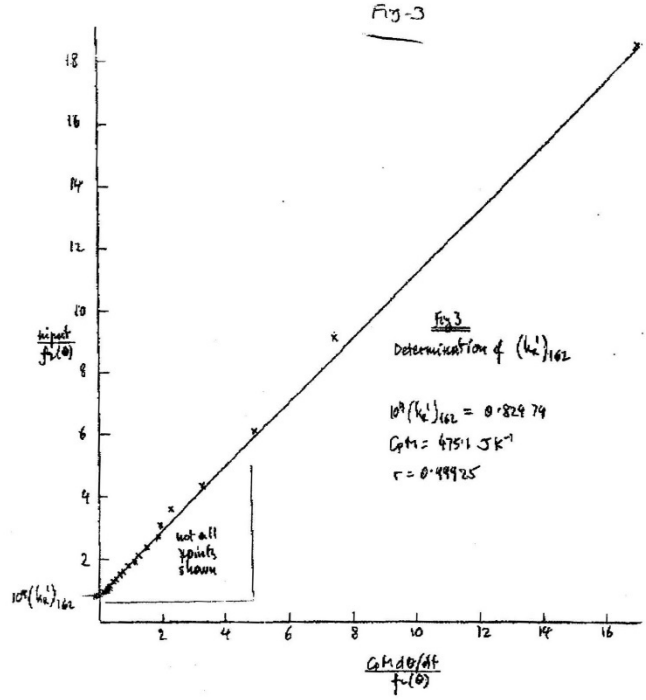


Fig 3
Determination of $(k_2')_{162}$
$10^9(k_2')_{162} = 0.82979$
$GM = 475.1\ JK^{-1}$
$r = 0.99925$
not all points shown
$10^9(k_2')_{162}$
$\frac{\text{input}}{f_1(\theta)}$
$\frac{GM\,d\theta/dt}{f_1(\theta)}$

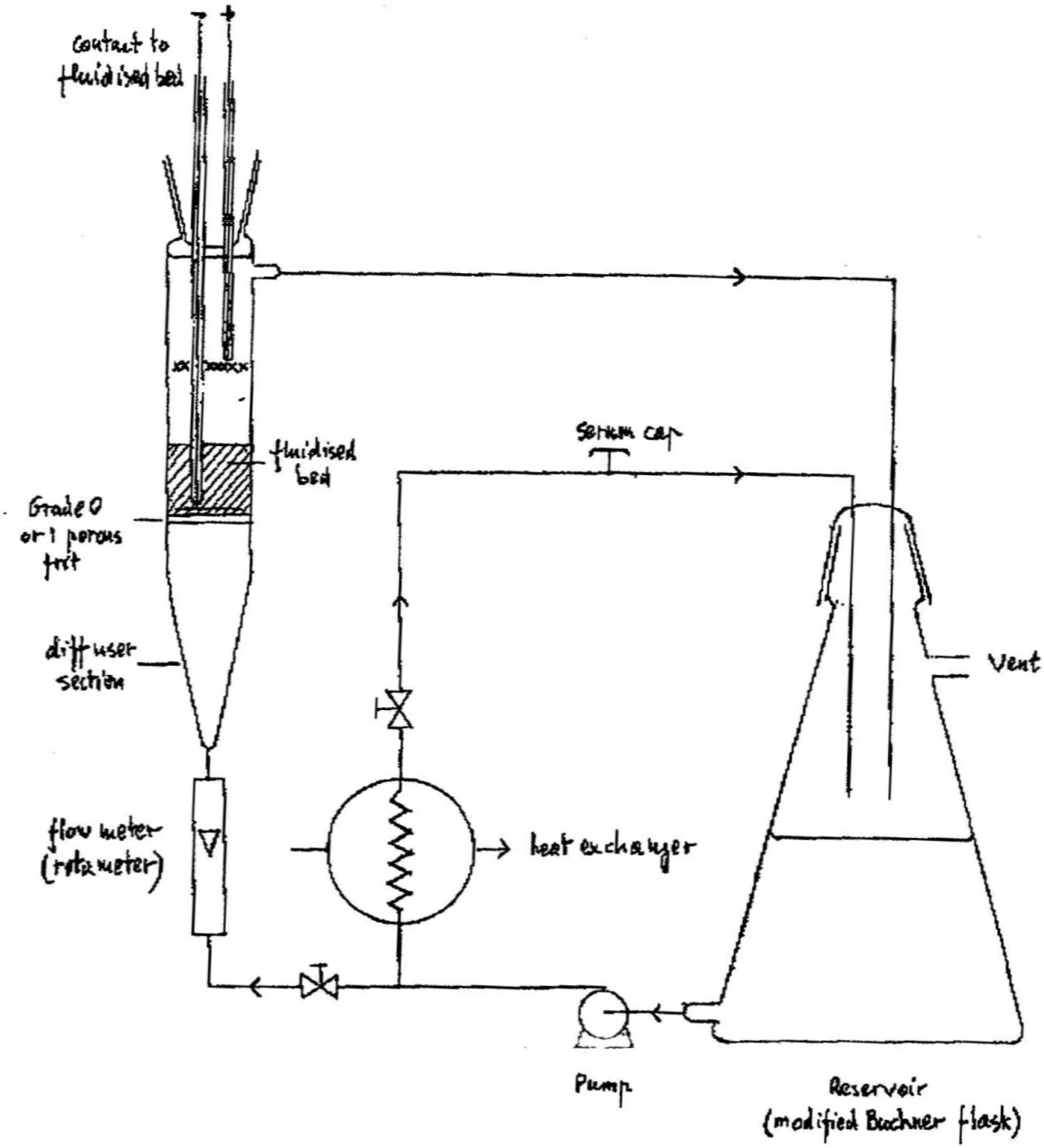


Fig. 1 (reference electrodes not shown)

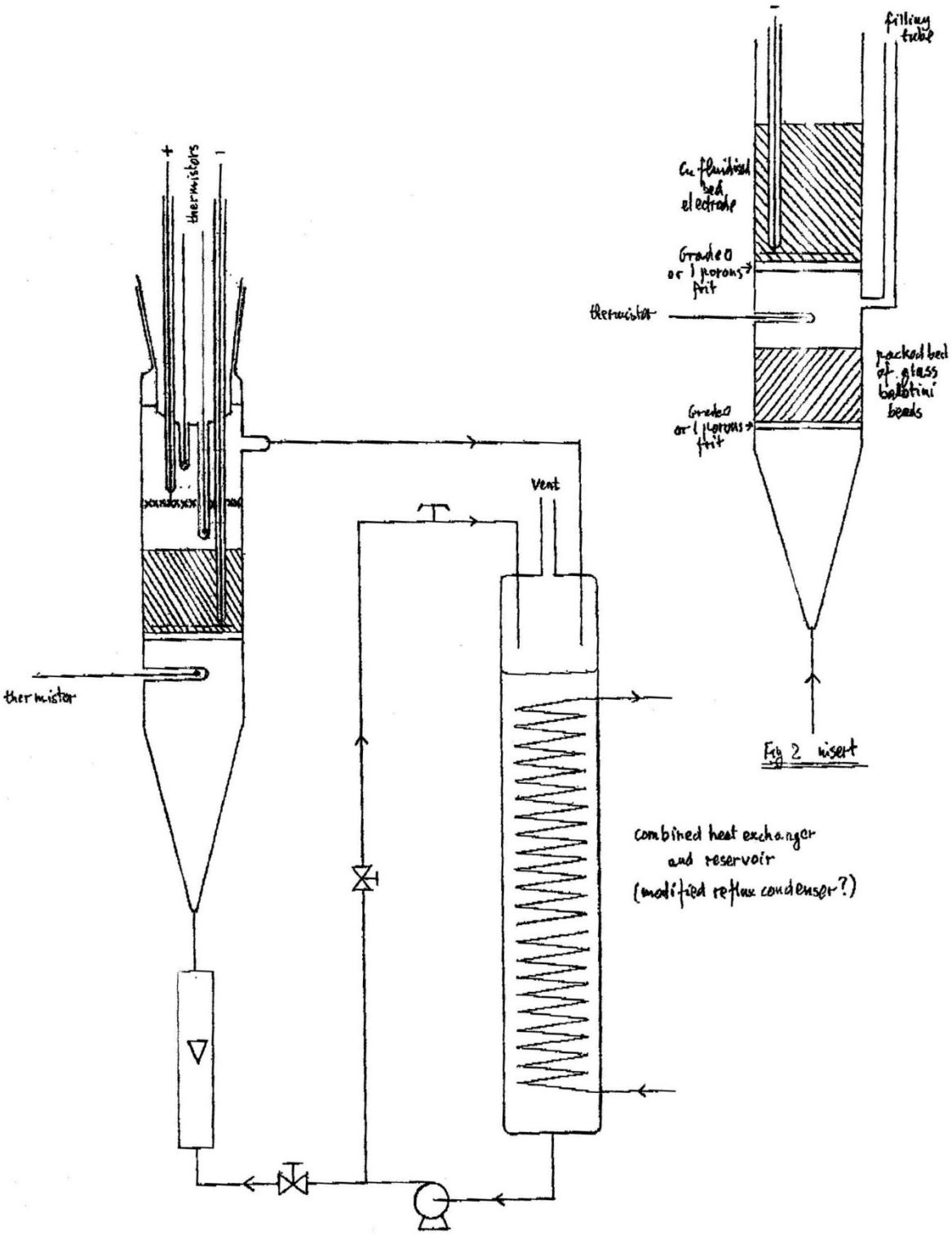


Fig 1 (reference electrodes not shown)
Other components labelled in Fig 1

2000-12-20

NAWC heading

FAX MEMO

DATE: December 20, 2000

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

Thank you for your fax of 27 November 2000. I made all the corrections you suggested and sent it on to Dr. Imam. I hope no new errors creep into the manuscript. This NRL Report has taken much more time than I expected. However, it is now completed, and Dr. Imam thinks it will be published next month. After making use of your fax copy, I received your original sent via Swift-Air. I have used square brackets everywhere as you suggested. It will be nice to have this report published because it should prove to be a valuable reference.

I spoke by telephone today with Stan Szpak. He seems to be doing fine and hopes to have his Navy Report published in the next month or two. He is still waiting for Scott Chubb's contribution. He asked me if you had any final corrections. I think he will be contacting you about this.

I found your ideas concerning the copper fluidized bed electrodes very interesting. Since I can't work on it here, I faxed some of this material to Stan. I think they are hoping to try such experiments.

I have not forgotten about the Pd-Ce and the Pd-Ce-B data from my Japan experiments. However, I have simply not had enough time to get this data ready to send to you. I hope to do this sometime next month. Meanwhile, I find your analysis of the co-deposition experiment quite interesting and hope it will lead to a publication.

I attended the ANS Meeting in November and presented the paper based on the Pd-B experiments. Like other such meetings, the cold fusion session was ignored by most other scientists, hence we were mostly speaking to each other once again. I find this very frustrating to go to such effort and then be ignored by most scientists. Another thing that bothers me is that some branches of cold fusion seem too far out and don't even make sense to me. There was one paper based on the poly-neutron concepts that claimed excess heat simply by stirring ordinary water. No palladium or other metal was even necessary. I think these ideas take away our credibility. What do you think? Perhaps I can fax you a copy of this paper. *(Oriani and Fisher)*

Linda and I will be leaving for our Oregon cabin (Grants Pass) tomorrow, and we will spend three weeks there without television or telephone. Therefore you will not hear anything from me until after January 8, 2001. It will be good to get away from everything for a while. I may work

on the Pd-B paper for a journal publication. I am sure that we can publish it in Fusion Technology. The co-authors will be the two of us along with Dr. Imam. I am tempted to submit it to the Journal of Physical Chemistry, but it will probably be sent back. Do you have any suggestions where we may submit it? I will fax you a version before I send it off.

Regarding the "Eagle Project" *(cold fusion weapons)* here at China Lake, we had our funding cut 50%. Therefore, we are trying to figure out what would be best to do with a limited budget. I think we will try to run a series of small tests rather than a few large tests. I have found that there are methods here at China Lake that most people would not know existed. These can be used in a series of small tests. I will try to let you know more later.

Linda and I wish both you and Sheila a Merry Christmas and Best Wishes for the New Year!

Mel and Linda Miles

Mel and Linda

ANS Meeting
November 2000

cess Power and Helium Production During D_2O and H_2O Electrolysis Using Palladium Cathodes," *J. Electroanal. Chem.*, **346** (1993); see also "Anomalous Effects Involving Excess Power, Radiation and Helium Production During D_2O Electrolysis . . . ," *Fusion Technol.*, **25** (1994).

3. L. CASE, "Catalytic Fusion of Deuterium into Helium-4," *Proc. ICCF7*, Vancouver, Canada, April 19–24, 1998.

4. Y. ARATA, Y. ZHANG, *Proc. Jpn. Acad.*, **70B**, 106 (1994); **71B**, 98 (1995); **71B**, 304 (1995); and **75B**, 281 (1999); see also *J. High Temp. Soc.*, **21**, 130 (1995).

4. Anomalous Power Generation Produced by Stirring Water Solutions, *Richard A. Oriani (Univ of Minnesota), John C. Fisher (Fisher)*

Following the report by Fleischmann, Pons, and Hawkins[1] in 1989 that electrolysis of heavy water upon palladium can generate by means of some nuclear reactions more thermal energy than the electrical energy supplied, many experiments have verified that claim,[2] and some have obtained evidence of helium[3] and of tritium[4] generation. However, all such work was done with crystalline materials, which led to the production of theoretical explanations that depend upon the nuclear reaction being enhanced by the crystalline environment. In contrast, the polyneutron theory developed by one of us[5] does not depend upon a crystalline field. One motivation for the present work was to try to distinguish between these two kinds of theories.

Apparatus set up for very rapid stirring of water solutions was of two kinds. The initial experiments were carried out with a commercial plastic impeller mounted on a plastic shaft driven by an alternating-current (ac) motor at 3250 rpm. The liquid in an open beaker consisted of ordinary tap water with Li_2SO_4 or K_2CO_3 in solution. Thermocouples, one dipping into the liquid and the other in the ambient air, served to monitor the relation between the temperature of the stirred liquid and that of its ambient. Several experiments produced sudden rises of temperature lasting several hours at a constant rate of stirring and a constant ambient temperature.

To increase the volume of liquid subjected to high shear deformation rates and to increase the control of the parameters of the experiments, another all-plastic apparatus was constructed. This consisted of five 5-cm-diam perforated disks mounted on a shift to rotate with respect to four perforated disks attached to the containment vessel, with ~1-mm separation between the static and rotating disks. The stirring device was surrounded by light insulation, and the temperature of the agitated liquid and of the surrounding air was sensed by thermocouples connected to a strip chart recorder. The device, driven by an ac motor, was partially protected from air drafts by plastic shields that enclosed also the ambient-measuring thermocouple. The rate of rotation at ~3300 rpm was monitored by a stroboscope or by an optical tachometer.

In a series of 26 experiments, all done with 140 ml of liquid, most with Li_2SO_4 as solute, a very large variation of ΔT ($= T_{liquid} - T_{air}$) was found ranging from 12.7 to 23.6 from one run to another, keeping all parameters constant. One can assume that the minimum ΔT observed in this set of runs corresponds to the temperature difference produced only by the viscous friction of stirring, so that the larger values of ΔT must be caused by some other source of power operating at different levels within the stirring device. This excess power can be comparable in intensity to the thermal power produced by the viscous resistance to stirring.

In addition to the variety of average values of ΔT seen in this set of runs, significant sudden variations of ΔT were observed within an individual run. Increases of ΔT have been documented that were considerably larger than can be explained by the measured changes in (W^2P), where W is the rotational velocity of stirring and P is the electrical power delivered to the motor. We conclude that an additional source of energy can develop and decay in this system. Serious attempts were made to corroborate this phenomenon by Seebeck calorimetry. However, our currently available calorimeter was found inadequate for the purpose.

Our experiments have demonstrated that excess power can be developed in the absence of electrolysis and without any crystalline materials. To the extent that the phenomena displayed by these experiments are accepted as nuclear in origin, as we believe them to be, theories relying on crystal-enhanced deuterium fusion are refuted. The polyneutron theory remains a possible explanation. (?)

1. M. FLEISCHMANN, S. PONS, M. HAWKINS, *J. Electroanal. Chem.*, **261**, 301 (1989) and **263**, 187 (1989).

2. E. STORMS, *Fusion Technol.*, **20**, 433 (1991); see also *J. Sci. Explor.*, **10**, 2, 185 (1996).

3. Y. ARATA, Y.-C. ZHANG, *Proc. Jpn. Acad.*, **70B**, 106 (1994) and **73B**, 1 (1997). Miles/Bush (?)

4. F. G. WILL, K. CEDZYNSKA, D. C. LINTON, *Trans. Fusion Technol.*, **26**, 209 (1994).

5. J. C. FISHER, *Fusion Technol.*, **22**, 511 (1992) and **34**, 66 (1998).

5. Electron Screening in Metal Deuterides, *K. P. Sinha, P. L. Hagelstein (MIT)*

During the last decade, a number of anomalies have been experimentally reported in some metal deuterides. These involve unusual results in condensed matter such as temperature increases, excess power production, helium generation, tritium production, induced radioactivity, and fast particle ejections. A theoretical understanding of these phenomena is still lacking. In what follows, we consider the role of a model in explaining some results.

The crucial parameter in low-energy nuclear reactions involving deuterons is the *d-d* overlap probability. The overlap probability depends on the deuteron-deuteron potential $V(R)$. This potential is not understood well in the active metal deuterides. If the deuterons are located at different interstitial sites of the solid in question, (PdD or TiD), this overlap probability will be extremely small. However, our estimates show that two deuterons occur at the same interstitial site in the active samples. The probability is increased in the very high loading regime, or there are metal atom vacancies and defects. The screening by electrons facilitates two deuterons in coming close together. Yuki et al.[1] and Kasagi et al.[2] have measured protons emitted from targets of deuterides of Ti, Yb, and Pd and estimate Us(Ti) = 19 eV, Us(Yb) = 60 eV, and Us(Pd) = 300 eV, giving the corresponding screening lengths of the order D_s(Ti) = 0.75 A, D_s(Yb) = 0.24 A, and D_s(Pd) = 0.048 A. While for Ti and Yb deuterides, the screening lengths may arise from bound electrons coupled with induced polarization,[3] the situation in PdD requires some additional effects such as nonequilibrium dynamic effects or channeling effects.[4,5]

Here, we focus on induced polarization effects. When we consider electronic orbitals in the vicinity of two deuterons close together, it is obvious that the orbitals would correspond to the molecular D_2, which can be described as the superposition of configurations D-D, D(+)D(−) and D(−)D(+). To this, we must add the interaction with electrons localized around nearby metal atoms.

One deuteron produces an electrostatic field that polarizes nearby electronic orbitals. This induced electronic polarization then produces a field that affects the second deuteron. A Hamiltonian that describes this type of model can be written as

2001-01-11

11th January 2001.

Dr. Stan Szpak.
SPAWAR,
San Diego, CA,
U.S.A.

Dear Stan,

Greetings, Happy New Year and thanks for your FAX of 8/1/01. I was delighted to see that you are making an excellent recovery.

I need to write at least three letters to you of considerable length so this is just to let you know that I will do so during the next few days.

The first letter will start to summarise the results I have been obtaining with the analyses of the "Codeposition Experiment" which Mel carried out at the end of his stay in Japan. This has turned out to be very interesting and I have made a pretty detailed evaluation. The problem now is how to condense the material into a viable set of Tables and Figures and this task will still take me a few days. I also want to "close the loop" to our 1989/1990 investigation and paper. One major outcome is that this experiment points quite clearly to the benefits there would be in using fluidized bed electrodes. It seems to me that it may well prove possible to produce a reliable system generating useful levels of excess power at 10-100 X the energy input. If this should prove to be possible, then this would be an excellent outcome for your efforts in SPAWAR.

Much more anon! I take it that I should also send this letter to Mel?

The second letter will make some comments on the meeting which we had in San Diego. Here, I will just add "just so" to the first paragraph of your FAX of 8/1/01. I think it would be fair to say that my disclosure of the connection between our project on "Cold Fusion" and DU shells "sank like a lead balloon" (except for the intense discussion of our "accident" - but then the uninitiated would not know that this pointed to the strong connection between the two fields of work). You will probably realise that I have spent some considerable time and effort on gathering information - I hesitate to call this espionage - about the efforts and attitudes in various countries around the globe. In this context a lack of response and interest is just as interesting as a positive response: no one actually said "bullshit".

It will be clear to you that I changed my attitude last year towards the disclosure of the connection between the two fields of work: before that time, my lips were firmly sealed. There were two major reasons for my change in attitude. The first was that it seemed pretty clear that the "balloon would go up" about the behaviour of D.U. shells and thus it has turned out to be. Have there been any reports in the U.S. about the furore presently raging in Europe about these weapons systems? I think it can only be a matter of time before people at large make the relevant connections and this will prove to be a singularly difficult and unpleasant development. I had

hoped that we could have spent some time at our meeting to prepare for such an eventuality. The second reason is that some people have got somewhere near the nature of some of the connections and Giuliano Preparata, for one, saw the whole story some time ago. Of course, Giuliano had a bad reception for his ideas in the West but I think that the Russians/Soviets, steeped in the traditions and works of Landau, would have guessed at the connections. Again, much more about this anon!

The second letter will lead in turn to the third in which I will comment on the remaining points in your letter. I think that it is extremely difficult to develop definitive experiments to prove whether "Cold Fusion" is a bulk, near surface or surface effect but I will try to cover this point, however, the principal objective will be to outline the need to explore the presence (or otherwise) of coherent photofission processes and this will make a connection to the second letter. I will also discuss the ways in which your imaging experiments might give some useful information on these aspects.

All the best for 2001!

Yours, Martin

P.S. I am sorry for the continued delay in sending you the red-lined copy of the T.R. As I told Mel, I hit a snag with regard to one particular aspect of the analyses but I managed to resolve all the potential problems quite some time ago - in the end there weren't really any problems but I wanted to be absolutely certain about this. I will give the material I have here a final reading this week and will send you the copy by Express Air Mail on Monday.

2001-01-11

[JR This letter was probably not sent until later, according to a note from Miles. It is from Fleischmann to Miles and Szpak.]

Dr. M. H. Miles,
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N.A.W.C.,
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Dr. Stan Szpak,
SPAWAR,
San Diego, C.A. U.S.A.

[JR No date on this letter.]

Dear Mel and Stan,

I come now to the second of my letters referred to in my letter to Stan of 11/1/2001 which, in turn, refers to the first paragraph of his letter to me of 8/1/2001.

As I explained in my letter of 11/01/2001, it will be obvious that I changed my attitude last year towards the disclosure of the actual and possible connections between "Cold Fusion" and "D.U. shells". [113] Before then, my lips were firmly sealed and I was convinced that any such disclosure would be unhelpful (to say the least!) to both fields of work. As I come to the writing of this letter, I wonder whether my earlier attitude was not, in fact, correct? If that should prove to be the conclusion, then I believe that we should try to "let the whole matter drop."

However, having started on a chain of explanations, it is perhaps desirable to outline, at the very least, both the reasons for my change of mind (already touched on in my letter of 11/1/2001) and the "history" of the interconnections (needless to say, these topics are intimately related).

There are really five major reasons which explain my change of mind. The first is the need for honesty as regards the background which led to the Cold Fusion research topic. As I explained at our meeting (but not in my letter of 11/1/2001), the approach of the end of one's research career requires a "confession time." It will be obvious to you both (and must be obvious to other people as well) that I have posed many restrictions on what I have been prepared to say about the development of the "Cold Fusion" project. In consequence, it is not at all clear as to what may have been the real reasons for the start of the project. Inevitably, therefore, this makes the matter look very much like "Gee Whiz, let's go in the lab and try this out". Stan and Pam

[113] JR D.U. shells means depleted uranium artillery shells.

have struggled valiantly to "read my mind" (in the introduction to the SPAWAR Report) but, as I have said on other occasions, it is premature to attempt to do so – certainly premature in the absence of a detailed knowledge of Q.E.D. I will return to this point later in this letter.

I have already touched on the second reason for my change of mind namely, that I was sure that the "balloon would go up" anyway (see my letter to Stan of 11/1/2001) about the behaviour of D.U. shells sometime during the present year - the ground swell has been building up for some time now. Thus it has turned out to be and, presently, we are being treated to a long chain of dissimulation and misinformation by the Anglo-American Defence Establishments. Of course, nobody believes these pronouncements and all that has been achieved so far is a complete loss of credibility. We are being treated to pictures of soldiers in protective gear sweeping the ex-battlefields with "nuclear detectors" (what, does anybody believe that significant readings of α - particle emission can he detected from ^238U in this way?) Presumably, this is part of an attempt to show that there is no significant increase in radiation. We are being told that six deaths from leukaemia among the Italian military are not statistically significant especially as there is no evidence of kidney damage from heavy metal poisoning (see Footnote 1). However, that ignores the fact that a further ~30 Italian soldiers have the early-middle range symptoms of leukaemia and that is certainly statistically significant. I will return to this question later in this letter.

Other significant developments are the attempt to confuse DU shells with gas guns (or liquid fuel guns), reference to a long list of successful civilian applications of ^238U (keels of yachts, trimming of aircraft, X-ray shields etc.) without any increase of cancer and absence of adverse effects on military testing ranges. The most recent inanity is the attempt to confuse <u>depleted</u> uranium with <u>reprocessed</u> uranium (does anybody believe that reprocessed uranium would have been used in these weapons? If it has, in fact, been thus used, then this would open up a long list of questions). All of these pronouncements are suddenly made by a long list of "experts" and one must ask: where have they all been during the last 20+ years? One must also ask this question with regard to the lack of protective clothing - explained away by the urgency of the military situation, 20+ years is hardly a situation of urgency!

There is one matter which is of special importance and that is that we are now told that the effects of some of these weapons are comparable to those of small nuclear weapons. Of course, the many cynics say that this has been abundantly clear for a long time now and that the effects of D.U. shells are comparable to those of small nuclear weapons because they are small nuclear weapons! The combustion of any conceivable quantity of uranium powder would raise the gas temperature in a tank by no more than 20 - 40 C so how does this explain the melting of the armour and carbonisation of the tank crews?

This brings me to the third reason for my change of mind which is that an increasing number of people believe that there must be a connection between the two fields and are starting to ask leading questions. Needless to say, the absence of any reply is itself fairly revealing and damning (again, see my letter to Stan of 11/1/2001). Here, I will just single out our late friend Giuliano Preparata who could see the whole story "in a flash". I once said to him; "Giuliano, how can it be that you can read my mind?" His reply was to the effect that if there is a number of strange observations and, if there is a single explanation for all these observations, then it is certainly

likely that this single explanation is correct! I will also single out the Soviets/Russians as I am sure that they grasped the connection at the outset. I need to qualify this statement: they may not have grasped that the connection is via QED which I believe is the cornerstone of both these methodologies. However, they did not need to do so - it was sufficient for them to cast the explanations in terms of pressure waves, a matter which goes back to the work of Bridgman [114] in the 1930's, (more about this anon). Some of the leading questions were from Baraboshkin's research group and, indeed, I wonder: how and why did Baraboshkin appear at the meeting in Nagoya in 1992? (see Footnote 4). There are several examples of the extreme sensitivity of the Russians about the general area of "pressure waves" which we may need to discuss. As I have already said: it does not follow that Soviets/Russians have made connections via QED although it seems to me that logical developments of the work of Landau on the superfluidity of ^{4}He would have led them in this direction.

The fourth and fifth reasons are really interrelated. Moreover, the subject matter is already contained in the previous parts of this letter. However, I will reiterate these points as they were part of my "decision tree".

The fourth point is that the explanation of some of the recent work on the generation of transmutation products (and, especially, the strange isotopic distributions) points to a connection between the two topics. At first sight the results reported seem very strange, so strange that I am sure that there are many people who will simply consign them to the category of "pathological science". However, once one interprets the data in terms of a model based on QED, it becomes clear that the results have a rational explanation and it also becomes clear that these effects would be much easier to investigate by using D.U. shells rather than by "Cold Fusion".

The fifth reason is that there is an increasing number of anomalous results in "Cold Fusion" covered by our epithet "uncontrolled releases of thermal energy". I am sure that all the people working in the field of "Cold Fusion" will have had an attitude similar to our own - namely, to avoid such effects if at all possible. However even with such an attitude, the evidence for uncontrolled releases of thermal energy eventually becomes established and, in due course, such effects must be explained.

This brings me to the second part of this letter which you will see is closely related to the material in the first part. I think that it is really most appropriate to write this second part in the form of an "historical account" partly because Stan and Pam are interested in the motivation for this research project.

In the late 1970's we were asked to carry out an investigation which I describe as "a necessary but not sufficient condition for the development of D.U. shells". Coincidentally, this research might also have led to the development of new rocket motors. In view of the source of the request, it was clear that this was a serious concern and we embarked on the research which we brought to a successful conclusion.

[114] JR P. W. Bridgman (1882 – 1961)

It was evident that we were being asked to carry out this work on a "need to know basis". However, it is difficult for Academics to stop thinking and worrying about a problem especially when there is no evident explanation of the facts. The point at issue here was (and is!): How can one explain the disintegration of a solid material into a powder (the explosive disintegration of the solid!) Following intense compression? This is part of the problem which goes back to the work of Bridgman. Of course, one might well be satisfied by the description of the initial and final states (the intensely compressed solid and the essentially "cold" explosion to form the powder) but what about the dynamics of the conversion of the lattice into translational energy? (see Footnote 5).

In the absence of any definitive information (especially as regards the nature and dynamics of the fragments) I had to do some furious guess-work. Clearly, the fragments had to be the final state formed from an actual or virtual intermediate stage whose energy above the ground state has to be in the range of 1 – 10^4 GeV. Terrifying! There was evidently the potential for the participation of all manner of novel outgoing channels superimposed onto the disintegration into powder. I don't think that we have to look beyond the possible participation of incoherent or coherent photo fission processes. The first would produce the fragmentation found in conventional fission (radio-active isotopes + neutrons + γ-rays), the second would produce predominantly stable isotopes (+ neutrons + γ-rays). The yields of such products might be very low (but higher in the case of the coherent processes than in the non-coherent processes) but would nevertheless explain apparently anomalous energy releases. The worrying parts of such scenarios are the statements (+ neutrons + γ-rays). The end product might well be just the expected weak α particle emitter of the oxidised starting material but the transition to this product via the fragmentation of the starting material is another matter.

You will recognise in all of this the reason for my wish to look for evidence of photo fission processes. You will also recognize the reasons for my concern about the rates of incidence of leukaemia (as well as other cancers).

I want to insert here a comment about the moral issues involved in this research. Clearly, there was a "need to know" and to know much more than is apparently the case (see Footnote 7). I would stress here the word "apparently" because the behaviour of DU shells (i.e. the dynamics) may, in fact, be well understood, We "need to know" because we cannot uninvent science. However, the deployment of such weapons is another matter. At the very least I thought it would be unwise because I could see that it might well be used as an argument against the non-proliferation of nuclear weapons. Our knowledge about D.U. shells should therefore have been kept in reserve.

I want to turn now to the aspect which is of prime interest to you, namely, the relationship of my knowledge (and mainly guess-work) about D.U. shells to the start-up of the work on (Cold Fusion). As you know, I had many reasons for wishing to start this work and, in fact, had accumulated equipment for this project, during the 1970's. There were two occasions in that decade when I had come to the starting point of the project but I "backed-off" each time. As far as your interest in the "history" of the subject is concerned, everything I have said is essentially correct but it is certainly incomplete. The importance of the Bridgman- D.U. shells saga (shall

we call it that?) Was that I could see that the insertion of D+ into the Pd was an entirely different way of trying to create weapons and, moreover, it was a way which was open to us without using the facilities of the Defence Establishments. The advent of the D.U. shells was therefore the final part of the jigsaw which persuaded me to start the "Cold Fusion" project. You will see that if this connection could be disclosed, then the whole matter would become much more clear. One could then see that the work of "Cold Fusion" was just part of a somewhat logical progression. However, I have felt throughout that this connection could not be disclosed hence my suppression of this part of the subject.

I should add here that all the work we carried out was subject to the "need to know". I certainly never told any of my colleagues about this part of the prehistory - although they may well have guessed correctly about some of the connections (see Footnote 8).

There is one very important matter which we should consider here. Pd/H+ certainly has never produced an anomalous result; Pd/D+ on the other hand has done so - or may have done so. The worrying effects which have been seen with this system must therefore be classified under the heading "nuclear assisted explosions". Here I must add that the "worrying effects" have never been explored systematically (see Footnote 9). Clearly, the geometry of the system has an important influence which is not very surprising.

This brings me to two very important questions which need to be discussed at this time (and, if possible, resolved) and also some further general comments. The first question is based on the Preparata-Del Giudice inspired work in Italy which has led to a very clear indication that we can arrive at an "equation of state" for D+ in Pd under highly compressed conditions and with the Galvani Potential as an additional state variable (i.e. additional to P and T). I have always thought: splendid, this work must proceed to whatever conclusion may be achievable. At the same time, it seems to me that the stability of the γ - phase (determined by the Coehn-Aharonov effect) is only part of the story. The question therefore is: what else should be done to arrive at a more comprehensive description of the D+ plasma in Pd? This is a matter which I had hoped would be discussed at our meeting in San Diego but this discussion was stillborn. (see also Footnotes 10-12).

The second question is (it is really a set of questions): what do the many critics of research on "Cold Fusion" really believe is going on in these systems and, related to this, how do they interpret the behaviour of D.U. shells? If they ever accept the notion that the behaviour of these systems can only be understood within the Q.E.D. of many-bodies, then what else is "around the corner"? It seems to me that this opens up a vast new vista which we ignore at our peril.

The general comments here are really based on my letter of 29/1/2001 although this is part and parcel of much that has preceded this study. As I have already said, one must ask: How can anyone believe that there is no excess enthalpy generation in the Pd/D+ system? It has always seemed to me that the only reason why one could make such assertions was if one were working to a scenario which requires such disbelief - call it conspiracy if you will! Related to this train of thought, I have always believed that there was only one overriding consideration which justified the continuation of the work in the Public Domain and that was (and is) the development of

sources of excess enthalpy. I think that my letter of 29/1/2001 makes it clear that such sources should now be accessible and, indeed, that such sources would satisfy a substantial part of the existing energy needs. I have felt that sooner or later the benefits of such sources would have to be juxtaposed to the possible disadvantages (which will be clear from the first part of this letter). However, in my bleaker moments it has seemed to me that this juxtaposition already took place in 1989 (perhaps even earlier which is a matter which we might need to discuss) and that the conclusion was that the work on "Cold Fusion" should be stopped or, at the very least, be impeded.

I would add here that if we could be convinced that the disadvantages outweigh the advantages, then we would have to accept this outcome and "put up and shut up". However, for the present, we must carry on!

I have added Footnotes 13-15 which cover some general background material pertinent to the development of "Cold Fusion" and the interface between this topic and that of D.U. shells as well as other matters pertinent to National Security.

Regards,

Footnotes

1) A plausible interpretation of these effects of the use of D.U. shells (assuming that the effects are real) is that the incidence of cancer following the Gulf War conflict is associated with heavy metal poisoning while the incidence of cancer during the conflict is due to radiation damage, possibly because of depletion of the T-cells. The effects of radiation on T-cells is a matter that must have been extensively researched e.g. by NASA.

The possible generation of high levels of radiation during the use of the D.U. shells is related to my understanding of the underlying mechanism of operation of these devices which is discussed briefly in the main text. High levels of radiation would lead to photo fission of the Uranium (also of Tungsten if this is used). This matter would be much more serious if we are dealing with coherent photofission which would lead to the generation of much higher levels of "closed shell" nuclei i.e. of heavy metals with anomalous isotope distributions. You will see the relevance of these comments to recent research on "Cold Fusion". However the participation of photofission would be much easier to detect in the detonation of D.U. shells than is the case in the steady-state operation of "Cold Fusion" devices. All that is required is the collection of the debris coupled to its analysis by some suitable technique (prompt γ-s?).

It is a matter of great regret to me that we did not scrape out the inside of the fume hood following our episode of the "uncontrolled release of thermal energy". We should have collected this material and kept it for later investigation. However, as we all know: hindsight is the only example of 20:20 vision. The circumstances surrounding this "uncontrolled release of thermal energy" was evidently a matter of some interest to participants at the meeting in San Diego. I would add here that this event was entirely in lines with my expectations based on the operation of D.U. shells. This told me that the bits and pieces buried in the concrete would be interesting; the interesting parts would have been buried in the fume hood. It seemed clear that the investigation of such phenomena would require considerable resources which we certainly did not possess at the time - nor have we ever done so since that time (see also Footnotes 2 and 3).

2) It is clear that Giuliano Preparata and Emilio Del Giudice had also concluded by the time of the meeting in Salt Lake City in 1990 that one had to budget for the participation of photofission processes in the "Cold-Fusion" phenomena. At that meeting I attempted to start a discussion with Huizenga about such matters but he just used this as an opportunity to trash the subject further. I cannot believe that he is really as stupid as appears at first sight.

I hoped that we would have been able to start a discussion about photofission at the meeting in San Diego as I had some very specific proposals which I wanted to make. However, once again we would not get round to this point. I concluded about half-way through that meeting that the key participants were following agendas which excluded anything which I might wish to do.

3) There are a number of simple experiments which would allow one to investigate the participation of photofission processes. Some of these would also lead to alternative ways of investigating the "Cold Fusion" phenomena which have a bearing on Steve Jones' original intentions. We should discuss these points in due course.

4) I became very concerned when I learnt that Samsonehko was the co-ordinator of the Soviet/Russian research programme. When I learnt of Baraboshkin's interest, I wrote to Hideo Ikegami to urge him to invite Baraboshkin to the meeting in Nagoya. I did not need to do so; the Russians came "under their own steam" notwithstanding the turmoil in the country.

5) I was predisposed to questions of this kind because I had been concerned with a somewhat related research project in the late 1960's; the kinetics of the unimolecular decomposition of molecule ions in the field free region of mass spectrometers. The results obtained at that time were inexplicable on the basis of any simple model and I thought that this topic might well give an entree to the consideration of the QED of many-body systems (isolated many-body systems!). I had to abandon this topic because it was too far removed from the mainstream of our activities - also we needed a mass spectrometer with an extended field-free region (probably a cyclotron resonance device). I thought that my chances of getting finance for way-out mass spectrometry using way-out methodology leading to way-out interpretations were approximately zero! Now it may well be that this topic has become well-understood (or misunderstood) or even forgotten in the intervening years and I have not checked the literature. However, I thought it worthwhile to mention this field of work because I am sure that the QED of many-body systems will become a central problem of research in this Century.

If this topic were ever reactivated, it would be necessary to check on a very wide field of work including quantum chaos (see also Footnote 6).

6) In due course I started to organise a meeting on "Fractals in the Natural Sciences". You will probably understand the reasons why I wanted to do so (i.e. the hidden reasons!). Unfortunately the organisation of this meeting coincided with my major operation in 1988 (and its consequences) and I could not impose the bias which I wanted to achieve. Although the outcome was very interesting, it did not lead to any discussion of Q.E.D. in condensed phase systems.

I also started to work on the general behaviour of systems having fractal dimensions as well as on light absorption in such systems. Again, you may be able to see the hidden agendas. These are yet again further long stories. I had just got to the point of showing that one can obtain closed form solutions for one set of problems (at that time it was believed that one cannot do so and that may still be the case?) but it was clear that one needed to develop some new mathematical tools. I had to abandon these projects because of the pressures of work on "Cold Fusion".

7) I formed the impression that this work might well be a response to a perceived Soviet threat.

8) As I have already said elsewhere, these connections have been made by other people (including the Russians).

My perceived inability to discuss these matters with other scientists has been one of the major burdens imposed by this field of research. I believe that it will in due course be also seen as a major factor which is held back progress in these topics.

9) I should add here "to the best of my knowledge". The evidence in the Public Domain remains anecdotal.

10) The advent of this work makes it very attractive to resurrect some of the work which we carried out in 1970/71. As I have told you on other occasions we investigated Raman scattering from β-PdH at that time, a topic which in due course led to Surface Enhanced Raman Scattering, (SERS). The underlying idea was to compress H^+ (or D^+) in a diffraction grating using electrodiffusion and then to investigate the effects of laser excitation (which we would now describe as a rather sophisticated subset of inertial confinement). The advent of the Preparata-Del Giudice work indicates that we should investigate Raman scattering from the γ-phase as well as the effects of the β-γ-phase transition induced by the Coehn-Aharonov effect.

11) I also note that I have some well-defined ideas which would allow one to develop the Coehn-Aharonov effect in more robust structures than are currently used in Frascati. I actually started to construct some of the equipment required for this venture but it now seems unlikely that I will be able to execute this programme in Frascati.

As I have always said: the majority of scientists prefer to travel hopefully rather than to arrive - a comment which applies especially to Physicists. However, I prefer to arrive!

12) Towards the end of his life Giuliano Preparata started a programme of work on other aspects of highly compressed plasmas; stellar evolution, supernovas etc. The relevance of these topics to National Security will be evident. I note here that Giuliano Preparata no longer took me into his confidence about these matters - but then there was no reason why he should have done so except that I had told him that I had started to investigate magneto hydrodynamics in the late 1960's. I gave up this project because it was premature. Furthermore, I did not know whether I would be able to execute such a project and I also decided that it would be necessary to first put the Navier-Stokes equation on a sound basis (perhaps even Fick's laws?).

I have listed these points because it might well be desirable to try to reconstruct Giuliano's thinking about these problems?

13) It was evident that the initial development of work in the area of "Cold Fusion" would have to rely on the measurement of thermal balances and the detection of "nuclear ash". The second of these topics was going to be a matter of great difficulty when working with small scale systems - as you well know.

The superposition of the search for neutrons and tritium on the research into "Cold Fusion" was ill-advised as such searches were based on the assumption that "Cold Fusion" had to follow the scenario for "Hot Fusion" - which is not where we started from!

14) Nevertheless tritium and, to a much smaller extent, neutrons were formed in parallel reactions. My general analysis of the information available indicates that these products are formed in a non-steady state i.e. during the transition of the system to the fully coherent state. There is also evidence that tritium is formed following contamination by light water although it is not clear whether this is due to the destruction of the fully coherent state or else the initiation of other fusion channels. Our own work on this topic in Salt Lake City was brought to a halt in 1990 and we were never able to re-implement this work.

It is possible that the generation of tritium could be an objective for “Cold Fusion” research in a number of countries. I drew attention to the fact that the Indian research work in this field had been based on the use of diffusion membranes which maintain the system in a permanent non-steady state.

I have never been asked how I might set about increasing the yield of tritium. I can think of some simple experiments in this area (use of diffusion tubes or other membranes pulse electrolysis, use of D_2O-H_2O mixtures, use of bipolar fluidised beds, dispersion electrolysis: we may need to discuss some of these topics).

15) By the summer of 1988 we had reached a point when we wanted to initiate some work on metal deuterides other than Pd-D^238U and ^238UD3 being of special interest. However, it was clear that we could not start such research unless we could enter into a meaningful dialogue with the Department of Energy. As we could not achieve this objective, our attention turned eventually to the possibility of investigating Na2 ReD9 (although this compound may not exist!) However, there are many other systems which should be investigated.

It was also clear by that time that we were working in the wrong environment. Our wishes to work for a period of time in a National laboratory could not be implemented.

2001-01-29

Bury lodge heading

29th January 2001.

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Dr. Stan Szpak and Dr. Pamela Mosier-Boss.
SPAWAR,
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Dear Mel, Stan and Pam,

I now want to follow up my letter to Mel of 27/11/2000 by sending you a progress report on the analysis of the "Calorimetric Codeposition Experiment" which Mel carried out during his stay at the N.H.E. Laboratories. I believe that Mel also sent a copy of this letter to you, Stan and Pam. This letter is also the first letter I referred to in my letter to Stan of 11/01/2001.

The first important point is that this particular calorimetric experiment was carried out in the same cell, the same thermostatic tank and using the same equipment as Mel had used for the measurements on the Pd-B alloy discussed in the TRS and in our brief paper at ICCF8. It is therefore reasonable to assume that the "true heat transfer coefficient" must have been close to the value found in the previous experiment i.e. $10^9\ (k_R')_{12} = 0.85065 WK^{-4}$. This is of no particular consequence for the first stage of the analysis of the data for the "Code deposition Experiment" (see below). However, we do need to make a reasonable assumption for the water equivalent of the cell and I have used the value $C_PM = 450\ JK^{-1}$ also as found previously. Fig 1 shows the schedule of the cell currents used in this experiment. As for the case of the experiments on the Pd-B electrode we note that the current densities are in the region required for the onset of "positive feedback". We would therefore expect the levels of any rates of excess enthalpy generation to be somewhat variable which, in turn, makes it difficult to achieve accurate determinations of the "true heat transfer coefficients". Examination of the variation of the 11-point means of the lower bound heat transfer coefficient, $(k_R')_{11}$, with time, Fig 2. Shows that the required constancy of the rates of excess enthalpy generation is most likely to have been achieved during the calibration cycles on Days 6 and 7 of the experiment (calibration cycles were applied on Days 6, 7 and 8). This is shown more clearly by the plot of the data for Day 6 on an expanded scale, Fig. 3. We can also note immediately that the fall of $(k_R')_{11}$ at long time due to an increase in Q_{excess} induced by the rise in cell temperature induced by the calibration pulse. It follows that the conditions required for the determination of the integral heat transfer coefficients $(k_R')_{261}$, $(k_R')_{262}$, $(k_R')_{361}$, and $(k_R')_{362}$, were not met so that we have to restrict attention to the less precise and accurate differential coefficients $(k_R')_{161}$ and $(k_R')_{162}$.

We can also note the values of a number of determinations of the "lower bound" and "true heat transfer coefficients" shown on Fig 3. (Some of these values are also shown on Figs 2, and 8). We can see immediately that the value of the "true heat transfer coefficient", 0.699861 X 10^{-9} WK^{-4}, given by the analysis due to the group at N.H.E. is <u>less</u> than the "lower bound heat transfer coefficient which can be determined directly for about 35,000 s of this measurement cycle. As we have noted previously, it is impossible for the "true heat transfer coefficient" to be smaller than the "lower bound" value unless the cell functions as a spontaneous refrigerator! It follows therefore that the N.H.E. calibration is erroneous. I believe that the value given by N.H.E. is based on the "integral true heat transfer coefficient" determined by <u>forward</u> integration of the data set, i.e. $(k_R')°_{362}$ but I have not as yet investigated whether the application of this methodology produces such a low value for any of the measurement cycles on Days 6, 7 or 8.

We can also note that the value $10^9(k_R')°_{362}$ = 0.699861 WK^{-4} is less than the value of 0.72 < $(k_R')_{362}$ x 10^9 < 0.76 WK^{-4} which applies to the cells of this type for heat transfer by radiation alone i.e. the Stefan-Boltzmann value, and evident impossibility because heat transfer must also include a contribution due to conductivity. (We have noted previously that the value $10^9(k_R')°_{262}$ = 0.85065 WK^{-4} for this cell implies heat transfer across the nominal vacuum gap by conductivity due to a "softening" of the vacuum). Furthermore, the value 0.699861 is much smaller than the value $10^9(k_R')°_{362}$ = 0.7935 WK^{-4} determined by the N.H.E. for the execution of the calorimetric measurements using the Pd-B electrode. Perhaps Mel could confirm that the disparity of the two values has never led to any discussion?

I want to return now to a further consideration of the schedule of cell currents, Fig 1. We can see that the duration of the applications of ~ 0.2A and ~ 0.4A on Days 3 and 8 respectively was insufficient to allow us to determine the rates of excess enthalpy generation for these cell currents on these days. We can see next that the reduction of the cell current to ~ 0.02A via a shorter duration at ~ 0.05A on Day 3 allows us to investigate the phenomenon of "Heat after Death" (see below). There is actually a further episode of such "Heat-after-Death") on Day 9 following the termination of the experiment but the spreadsheets indicate that the experiment was terminated in an odd fashion which prevents the analysis of the data. I also note that the duration of the reduction of the cell current to ~ 0.02A on Day 8 is insufficient for any investigation of the phenomenon of "Heat-after-Death").

There are also three further important points which I should have described at the onset of this report. The first is that the evaluations which I have carried out have been restricted to temperature measurements with the "Long thermistor". The reason is that the last significant figure of the measurements of the temperature using the "short thermistor" have been dropped for some of the measurement cycles. Therefore, in order to produce a consistent set of evaluations, only the measurements with the "Long thermistor" had been used. The second point may well be related to the first because it has become apparent that there may well have been truncation errors in the data/results reported by N.H.E.. Thus, although the results have been reported to five significant figures (sometimes even to six!), The evaluations must be carried out to six figures to achieve the results specified for the ICARUS-1 and -2 systems. Failure to do this would certainly affect the precision/accuracy of low levels of excess enthalpy generation and,

furthermore, would lead to erroneous conclusions as regards the precision/accuracy of this type of calorimetry. The third point may also be related to the preceding two points: I have found that the rates of evaporative cooling reported by N.H.E. for this experiment are precisely those given by the methodology specified for the ICARUS-1 system which was not the case for the experiment using the Pd-B electrode. It seems to me therefore that N.H.E. must have used at least two distinct programs for the evaluation of the data. These three points certainly require further separate investigations. However, for the present, I have taken the precaution of carrying out all evaluations "from scratch" as was done for the evaluation of the Pd-B experiment. There is however, an important proviso which may affect the validity of the results: we have to assume that the experimental data have been reported to the required level of accuracy.

I want to consider next the determination of the heat transfer coefficients $(k_R')°_{161}$, $(k_R')°_{162}$, and $(k_R')°_{181}$, as well as of the water equivalent, C_PM,. Thus Fig 4 shows the evaluation of 10^9 $(k_R')°_{161}$ = 0.73099 WK^{-4} (see also my letter to Mel of 27/11/2000), a value which is also shown on Fig 3. The value of C_PM = 456.9 JK^{-1} while the regression coefficient for the plot is r = 0.99218. The use of the plot shown in Fig 4 was specified for the ICARUS-1 Methodology (although the plot for the determination of $(k_R')°_{261}$ was preferred to that for the determination of $(k_R')°_{161}$).

We next examine the determination of the "differential true heat transfer coefficient," $(k_R')°_{162}$. Fig 5, again for the calibration on Day 6 (see also my letter to Mel of 27/11/2000). We obtain 10^{-9} $(k_R')°_{162}$ = 082474 WK^{-4}, C_PM = 475.1 JK^{-1} r = 0.99925. I have noted on other occasions that this determination is unreliable especially as regards to the derived values of C_PM: note the large values of the abscissa especially for the points which have the highest statistical weight. If we therefore restrict attention to the points near the origin, we obtain 10^{-9} $(k_R')°_{162}$ = 0.85573. The derived values of $(k_R')°_{162}$ therefore straddle the value 0.85065 x 10^{-9} WK^{-4} found in the investigation of the Pd-B electrode, which I have taken as a justification for using this value in the further assessment of the "codeposition experiment".

We next examine the evaluation of 10^9 $(k_R')_{182}$ on Day 7, Fig 6. Coincidentally, this figure shows that the evaluation of $(k_R')°_{161}$ and $(k_R')°_{171}$ is unsatisfactory (the right hand and left-hand quadrants of the figure respectively). As I have noted on other occasions, the determination of 10^9 $(k_R')°_{181}$ = 0.61680 WK^{-4}, C_PM = 444917 JK^{-1}, t = 0.99439 is nevertheless satisfactory even though there are doubts about the exact significance of $(k_R')°_{181}$ at zero value of the abscissa.

We next examine the complete data set using the value C_PM equal 450 JK^{-1} and, where appropriate 10^9 $(k_R')_{12}$ = 0.85065 WK^{-4}.

We first re-examined the variation of the "lower bound heat transfer coefficient 10^9 $(k_R')_{11}$" with time. Fig 2 and Table 1. As we have already noted the straight lines on this figure give in succession 10^9 $(k_R')°_{362}$ determined by N.H.E. for this experiment, 10^9 $(k_R')°_{362}$ for the experiment with the Pd-B electrode and our own determination of 10^9 $(k_R')°_{262}$ for that experiment but allowing for the effects of positive feedback. We can see that the use of even the value of $(k_R')_{362}$ determined by N.H.E. will show excess enthalpy generation for most of the experimental time range and even, for most of Day 6. The use of 10^9 $(k_R')°_{161}$ = 0.73099 WK^{-4}

determined as in Fig 6 will show somewhat higher rates of excess enthalpy generation while the use of either 10^9 $(k_R')°_{362}$ determined by the group at N.H.E. for the experiment on the Pd-B system or of 10^9 $(k_R')°_{262}$ determined by ourselves for the same system will show excess enthalpy generation throughout the whole of the time range of the "codeposition experiment".

I note that the use of 10^9 $(k_R')°_{161}$ determined as in Fig 6 follows the strategy which we had first proposed in 1992 i.e. the assumption that the <u>maximum</u> value of the lower bound heat transfer coefficient" gives some estimate of $(k_R')_{12}$ which in turn can be used to derive the <u>minimum</u> value of Q_{excess}.

The values of 10^9 $(k_R')_{11}$ shown in Fig 2 are listed in Table 1. We can note that the use of such tabulations not only decreases the "noise" of the experimental data but also condenses the extensive KR 11 spread sheets into just a single data sheet.

I also note that I have not tried to derive a detailed interpretation for the data on Day 1 during which we observe the co-deposition of Pd onto the Cu substrate as well as dissolution of D in the lattice and, in the later stages, evolution of D_2 gas. The detailed interpretation of the data on Day 1 requires knowledge at the very least of the current efficiencies of the various reaction paths and the thermodynamics of the Pd deposition process(es). However, it appears to me that, if we make various plausible and assumptions, then we must conclude that co-deposition of Pd and D is accompanied by excess enthalpy generation! This is a very important conclusion from the point of view of the science involved and, evidently, it is desirable to develop calorimetric systems of the codeposition reactions.

Fig 7 gives the rates of excess enthalpy generation with the assumption that the "true heat transfer coefficient" is 10^9 $(k_R')°_{262}$ = 0.85065 WK^{-4}. It strikes me that these results are rather similar (but more detailed) to those given in Fig 2, page 2 of the Section "Events in Polarized Pd + D Systems" of the T.R. (see also my recent letter). The results used to construct Fig 7 are listed in Columns 5, 9, 13, 17 and 21 of Table 2 and in Columns 4, 7 and 10 of Table 3.

Columns 4, 8, 12, 16 and 20 of Table 2 and Columns 3, 6 and 9 also give the rates of excess enthalpy generation calculated using the impossibly low values of 10^9 $(k_R')°_{362}$ = 0.699861 WK^{-4} determined by the group at N.H.E. (impossibly low because this value of the "true heat transfer coefficient" is less than the Stefan-Boltzmann value!). It can be seen that even so we must assume that there is excess enthalpy generation (except for a short time region on Day 6).

Columns 3, 7, 11, 15 and 19 of Table 2 also list the "lower bound heat transfer coefficients, 10^9 $(k_R')_{11}$" based on the assumption that there is complete recombination of the electrolytically evolved gases. The comment has often been made that excess enthalpy generation can be explained by such recombination (Mel: I am waiting for you to wax lyrical!) And the SPAWAR effort has been especially singled out in this way on the basis that Pd powder is formed during the electrodeposition stage. The data in Table 2 and Fig 8 however show that such interpretations cannot possibly be correct because the "lower bound heat transfer coefficient" is now larger than the "true value" even if this true value is 10^9 $(k_R')°_{262}$. Here again we would have to assume that the cell acts as a spontaneous refrigerator in order to accept such explanations.

The only region of time in which we could possibly accept and interpretation of excess enthalpy generation in terms of recombination of the evolved gases is towards the end of Day 1. The current density on Day 1 is certainly comparable to that calculated for the diffusion controlled reduction of O_2 and oxidation of D_2 formed in the body of the solution. The low rates of excess enthalpy generation at the end of Day 1 therefore give an upper bound for any such side reaction. This is an upper bound because some O_2 and D_2 will be lost from the solution: furthermore at higher current densities the solution in the vicinity of the cathodes and anodes will be degassed by the electrochemically evolved gases.

I come now to the most important single result shown in Fig 9. The figure shows that in the first place the 6-point average $\overline{\overline{Q_{excess}}}$ of $\overline{Q_{excess}}$ during the last period of operation on Day 2 and the first period on Day 3 immediately preceding the stepwise reduction of the cell current on Day 3. Secondly, it shows the decay of $\overline{Q_{excess}}$ following this stepwise reduction of the cell current. In constructing this figure, I have assumed that the upper bound of any parasitic excess enthalpy generation due to recombination is 0.009 W given by the last value of $\overline{Q_{excess}}$ on Day 1, i.e. the value of $\overline{Q_{excess}}$ shown in Fig 9 are a lower bound. We should note that this is yet a further example of "Heat-after-Death" (though it is not quite "death").

It is results of this kind which persuaded me that the use of fluidized bed electrodes should allow us to reach a demonstration device showing excess enthalpy generation at high power efficiencies e.g. using the system shown in Fig 10. (The Fig 2 of my letter to Mel of 27/11/2000). However, I now believe that the feeder to the fluidized bed cathode should be positioned at the top of the bed. The rate of charging of the Pd coated Cu beads will admittedly be reduced but, at the same time, the disruption of the bed by D_2 evolution will also be reduced.

The basic underlying idea which prompted our first suggestion that the engineers in SLC [115] should undertake this study was that it should certainly be possible to use beds having a volume of beads per unit cross-section of say 10 cm^3 / 1 cm^2 i.e. an effective length of cathode of 10 cm. While it would be quite impossible to charge a 10 cm thick cathode (it would take several years and such a system would not preserve its structural integrity!), It might well be possible to charge a fluidized bed where motion in the D in the lattice is due to the convection of the beads. If the results for Day 7 were to apply, such a system would generate ~ 30 W of excess energy at ~ 0.5 W input all per 1 cm^2 cross-section of the bed. If the results of our 1992 or 1994 studies applied, we should expect to see a ~ 40 kW of excess energy at ~ 50 W input and that would certainly be the end of the beginning!

Of course, there is always the possibility that such an approach might fail - for one thing, we may not be able to charge the beads. The investigation of "Heat-after-Death" should therefore be backed up by the operation of conventional electrodes under pulse conditions. However, I think that it would be well worthwhile to take a chance on achieving a breakthrough when using fluidized bed cathodes.

[115] JR This is difficult to read. I assume it says SLC meaning Salt Lake City.

One further point: the embryonic chapter for the TR (or paper for ICCF-9?) Would be greatly strengthened by making comparisons with "blank" systems. On the face of it, N.H.E. never carried out such measurements (I simply don't believe this - it is much more likely that they did and that these measurements followed strictly the predictions given by the ICARUS methodology. They have therefore refused to give me these data!) And as all of my own data were removed from the material sent back to me from France, we will have to make do with Mel's "near blank". [116]

Onwards,

Yours,

Martin

[116] MM This was with Pd-Ce-B.

Fig 1

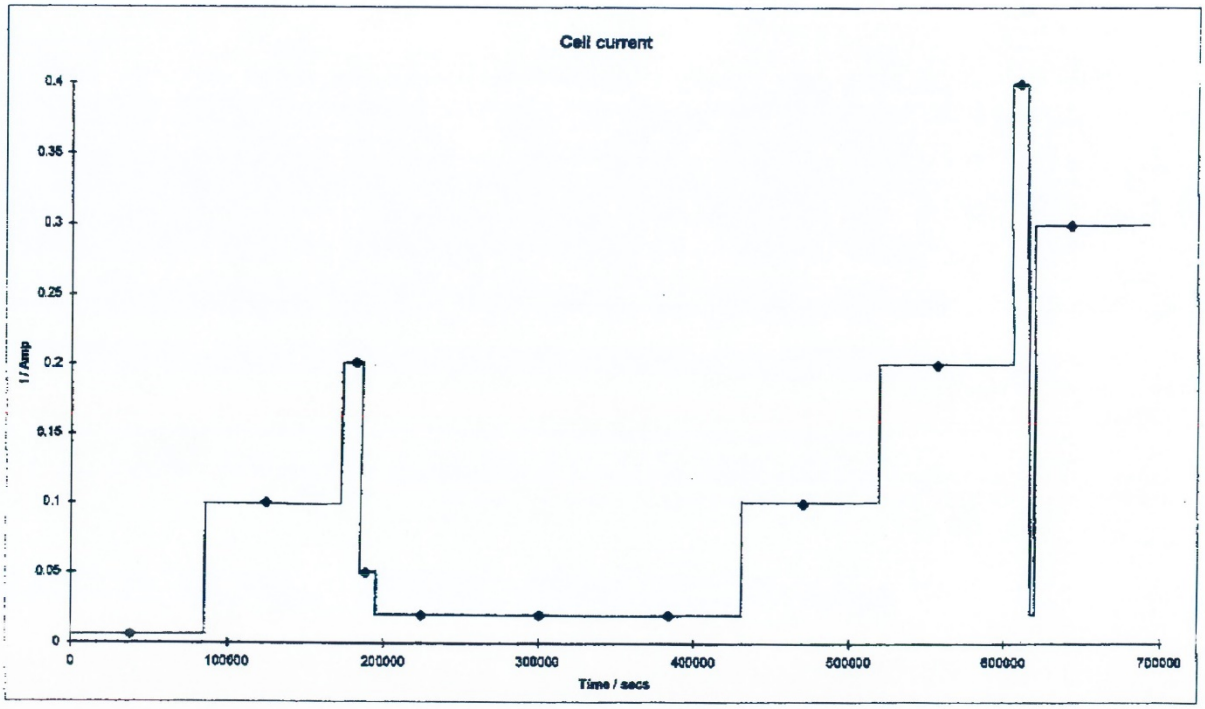


Fig. 2 (assumes no excess enthalpy)

Fig 2

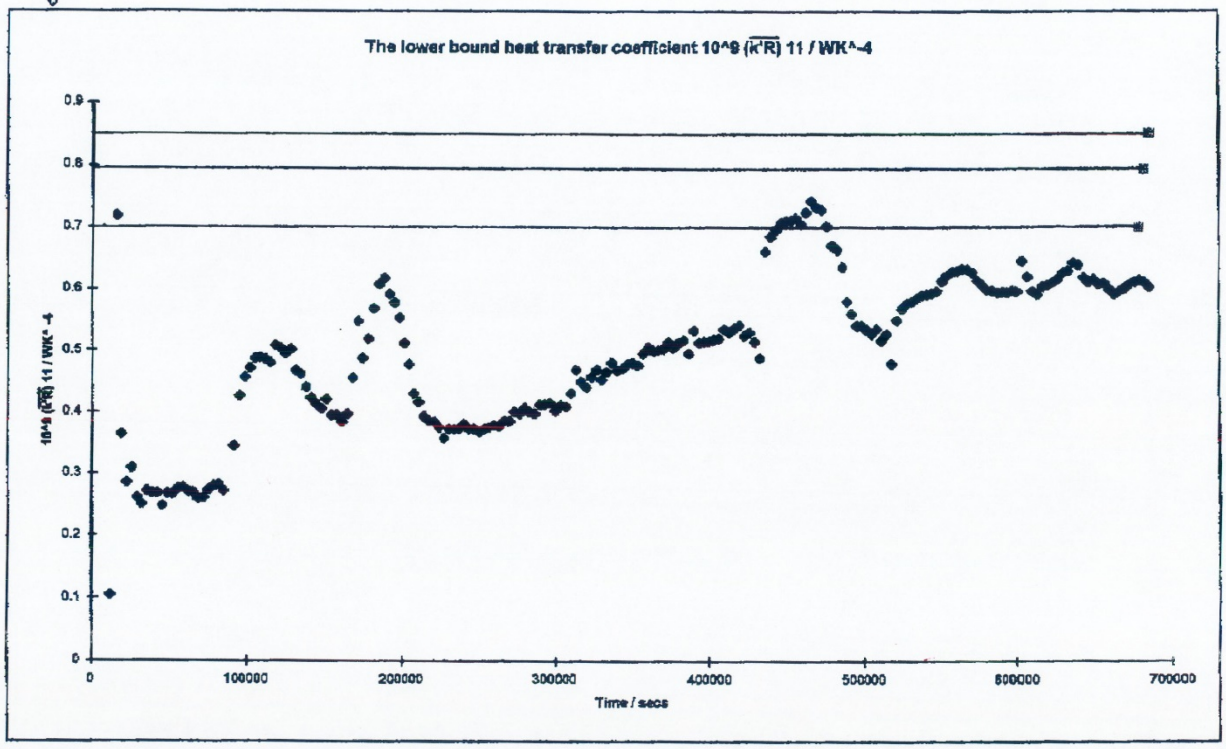


Fig3

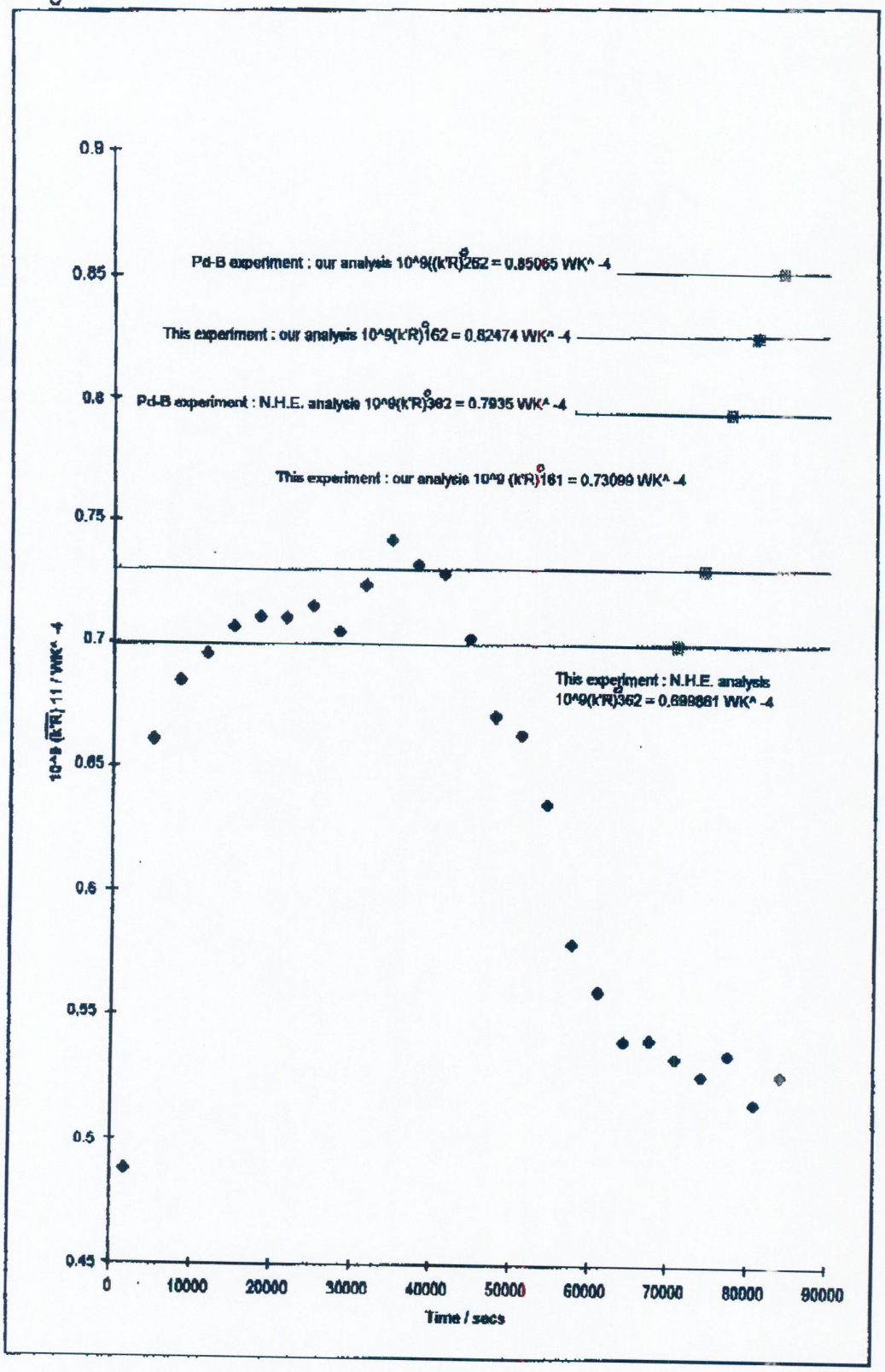

Pd-B experiment : our analysis 10^9((k'R)°262 = 0.85065 WK^ -4
This experiment : our analysis 10^9(k'R)°162 = 0.82474 WK^ -4
Pd-B experiment : N.H.E. analysis 10^9(k'R)°362 = 0.7935 WK^ -4
This experiment : our analysis 10^9 (k'R)°161 = 0.73099 WK^ -4
This experiment : N.H.E. analysis
10^9(k'R)°362 = 0.699861 WK^ -4
10^9 (k'R) 11 / WK^ -4
Time / secs
0.9
0.85
0.8
0.75
0.7
0.65
0.6
0.55
0.5
0.45
0
10000
20000
30000
40000
50000
60000
70000
80000
90000

Fig 4

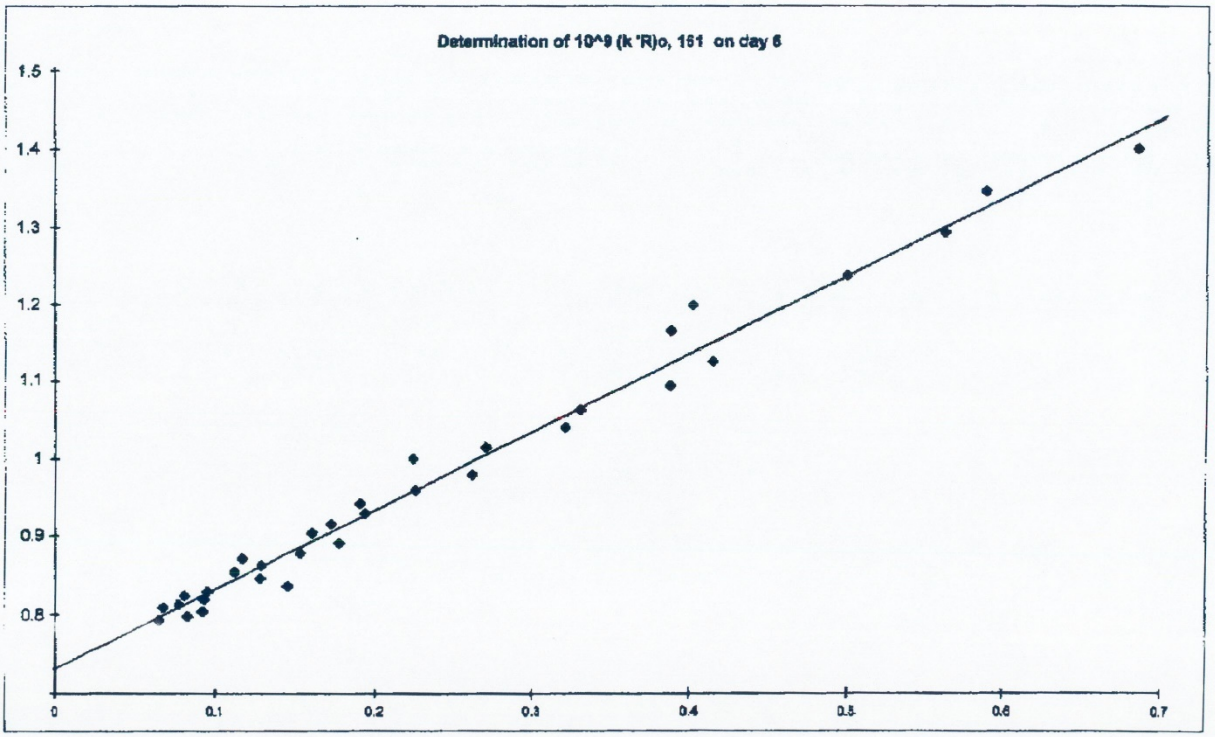

Determination of 10^9 (k 'R)o, 151 on day 6
1.5
1.4
1.3
1.2
1.1
1
0.9
0.8
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7

Fig 5

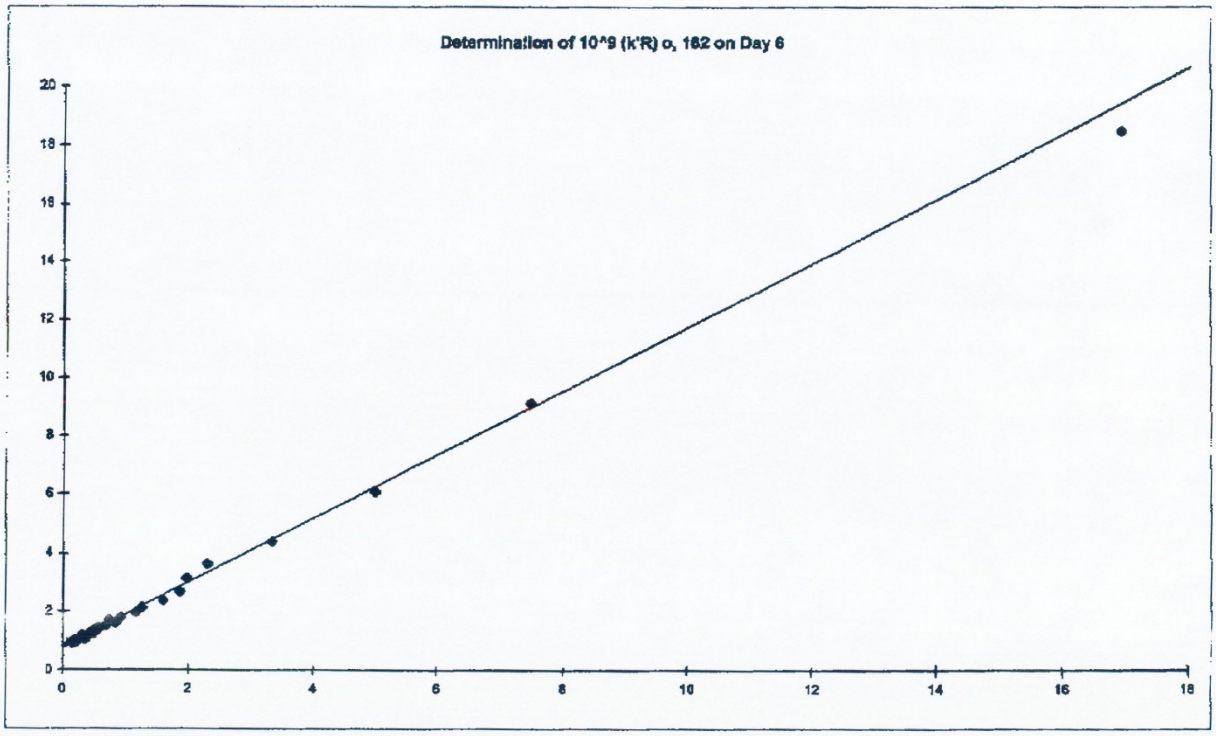

Determination of 10^9 (k'R) o, 182 on Day 6
20
18
16
14
12
10
8
6
4
2
0
0
2
4
6
8
10
12
14
16
18

Fig6

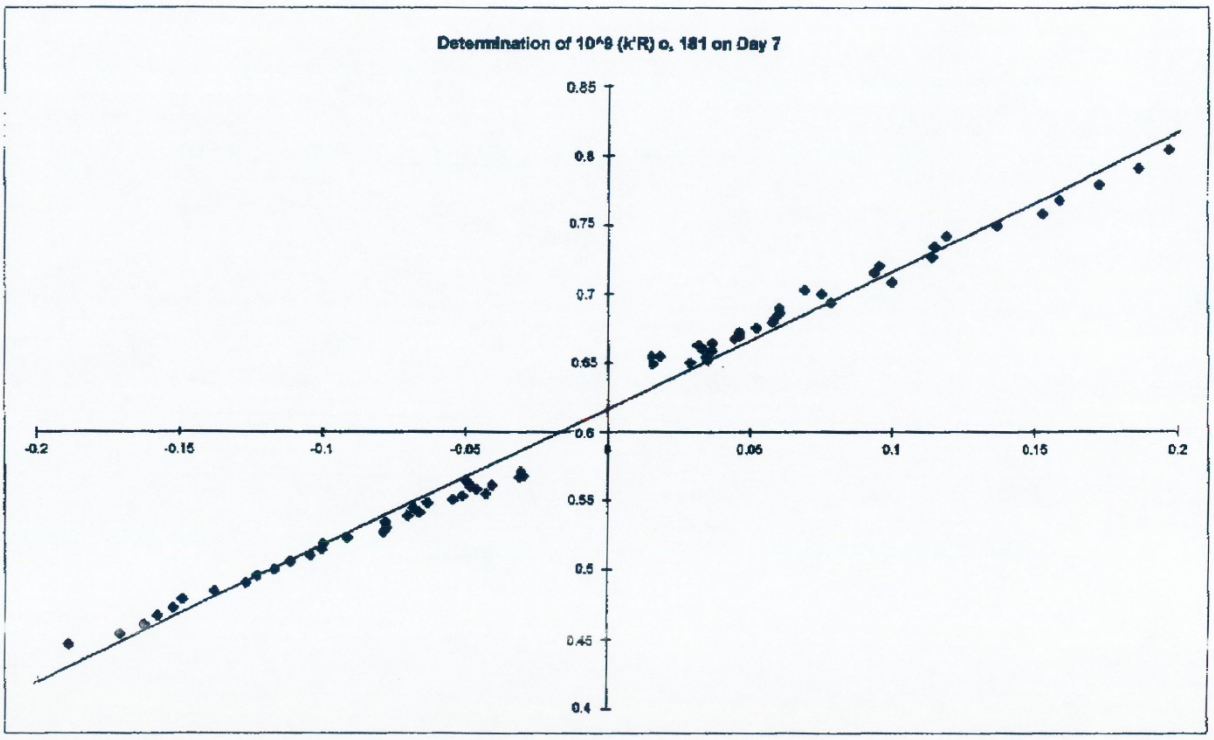

Determination of 10^9 (k'R) o, 181 on Day 7
0.85
0.8
0.75
0.7
0.65
0.6
0.55
0.5
0.45
0.4
-0.2
-0.15
-0.1
-0.05
0
0.05
0.1
0.15
0.2

Fig 7

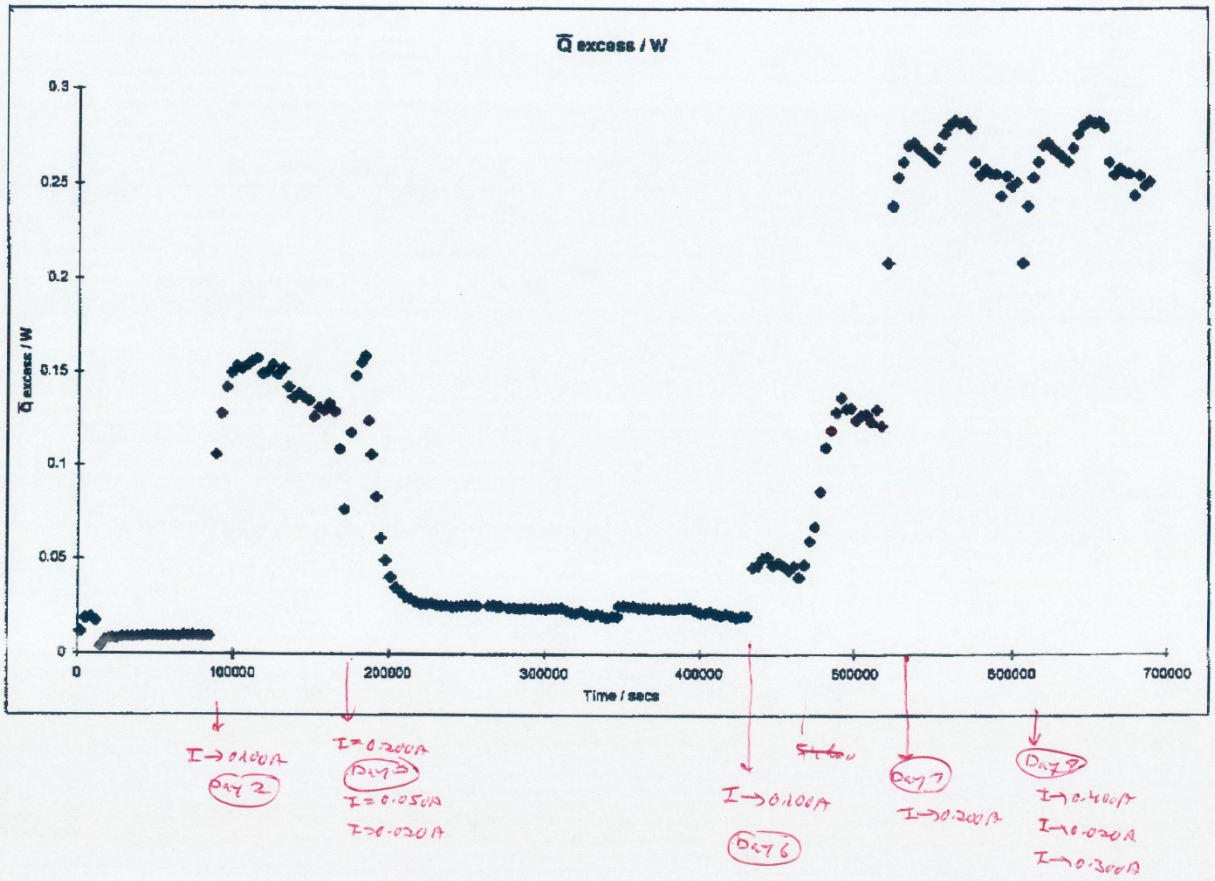

Q̄ excess / W
Q̄ excess / W
0.3
0.25
0.2
0.15
0.1
0.05
0
0
100000
200000
300000
400000
500000
600000
700000
Time / secs
I → 0.100A
Day 2
I = 0.200A
Day 3
I = 0.050A
I = 0.020A
I → 0.100A
Day 6
Day 7
I → 0.200A
Day 8
I → 0.400A
I → 0.020A
I → 0.300A

Fig 8

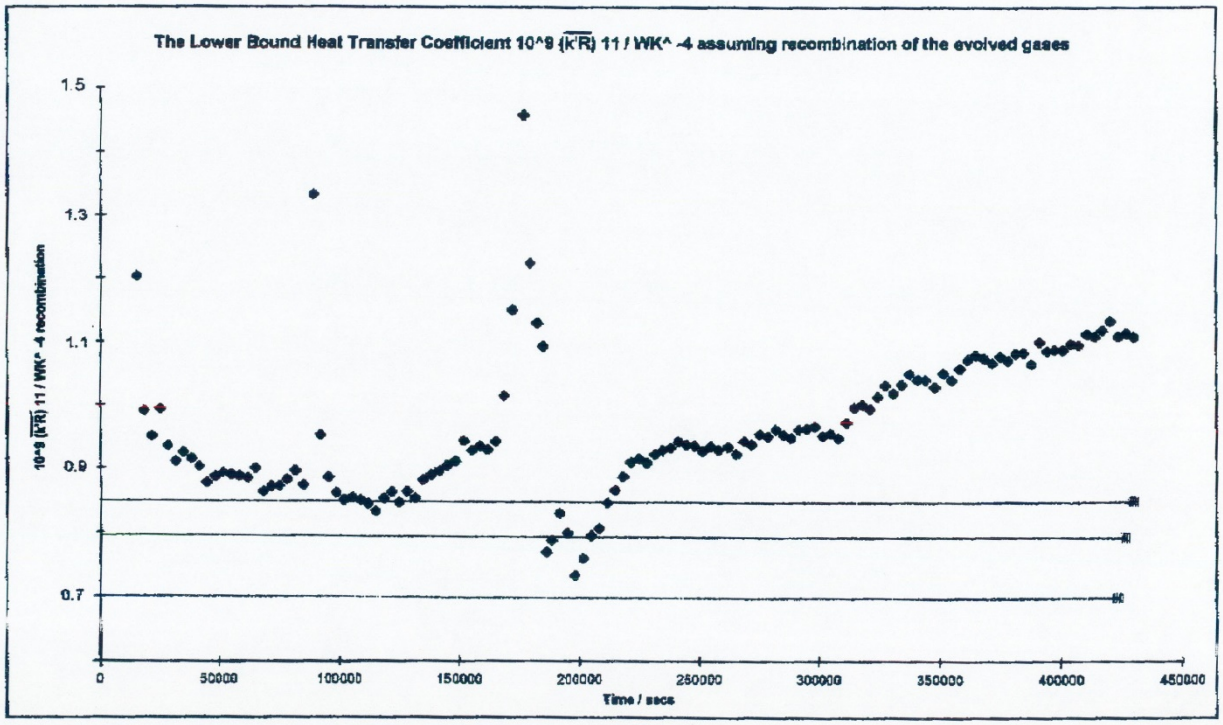
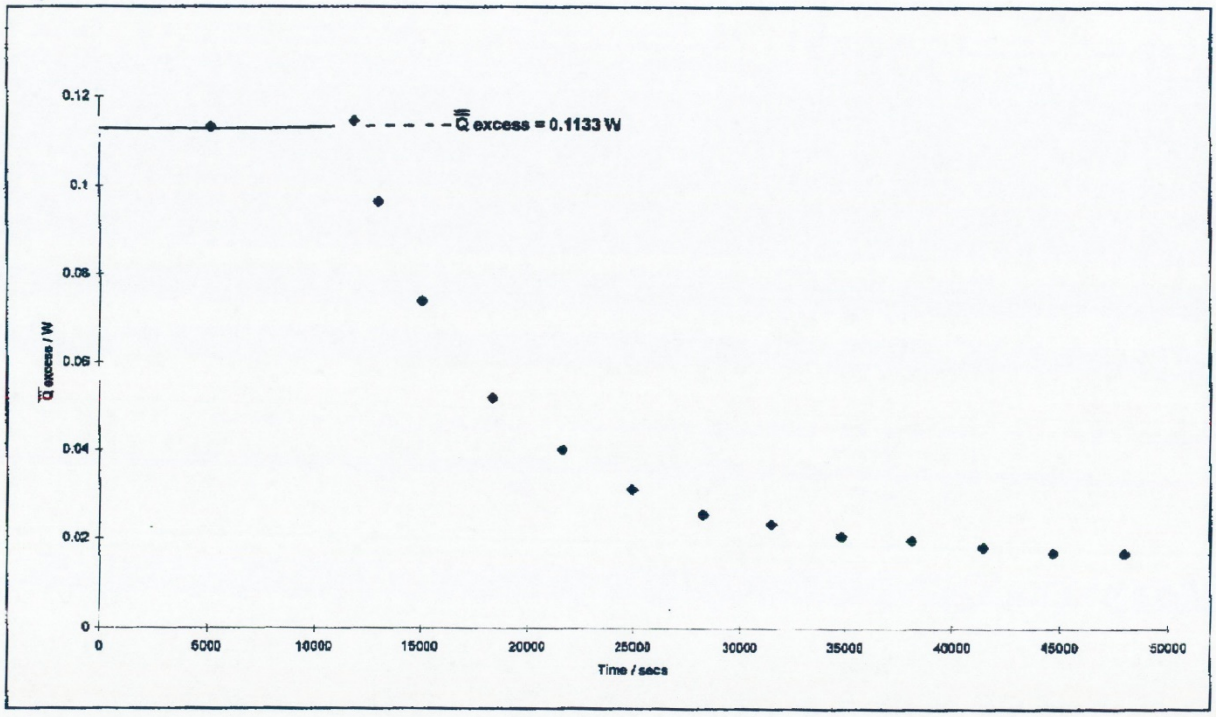

The Lower Bound Heat Transfer Coefficient 10^9 {(k'R) 11 / WK^ -4 assuming recombination of the evolved gases
10^9 {(k'R) 11 / WK^ -4 recombination
1.5
1.3
1.1
0.9
0.7
0
50000
100000
150000
200000
250000
300000
350000
400000
450000
Time / secs

Fig 9


Q excess = 0.1133 W
Q excess / W
0.12
0.1
0.08
0.06
0.04
0.02
0
0
5000
10000
15000
20000
25000
30000
35000
40000
45000
50000
Time / secs

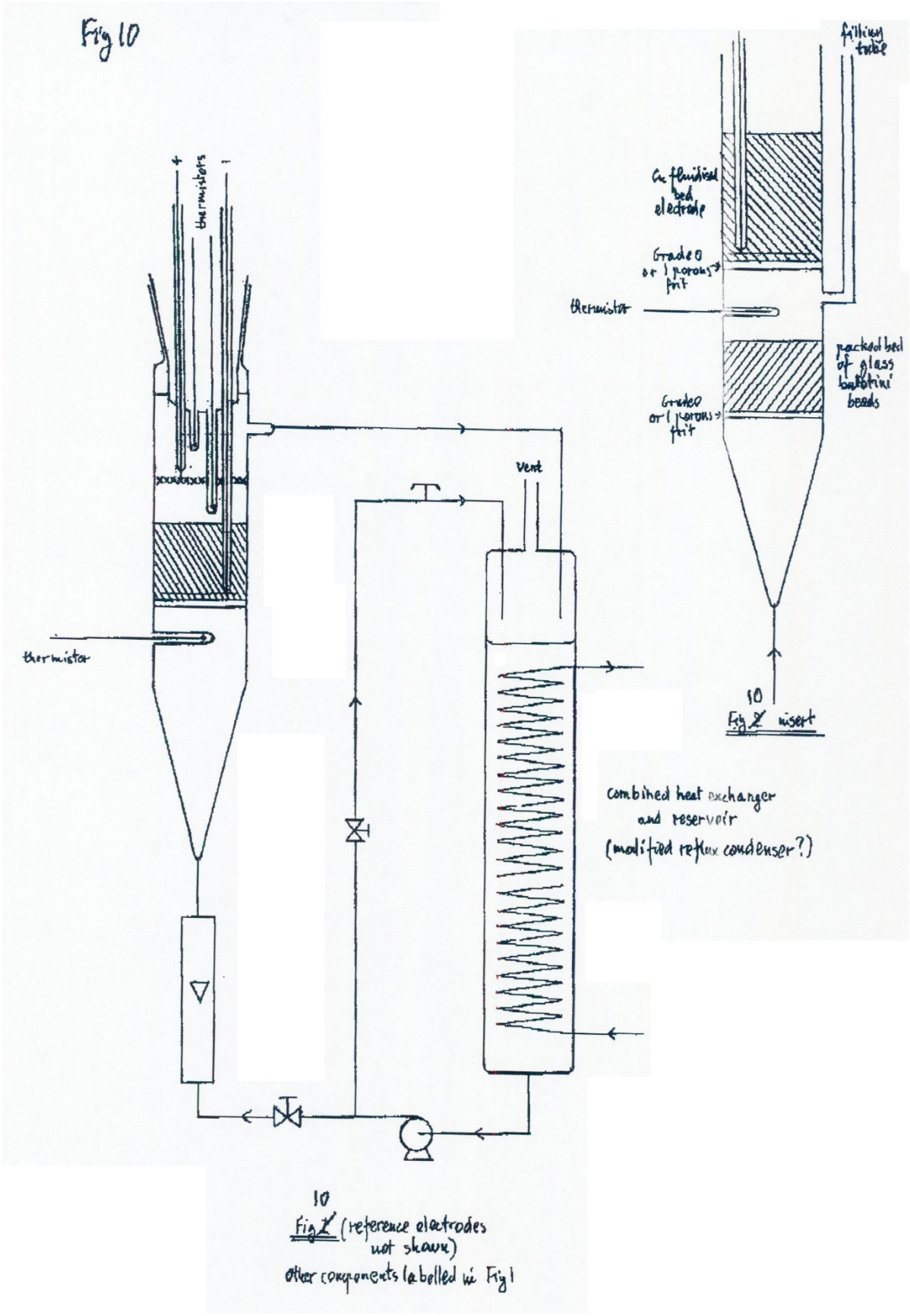


Fig 10
+
thermistors
−
thermistor
Vent
filling tube
Cu fluidized bed electrode
Grade 0 or 1 porous frit
thermistor
packed bed of glass ballotini beads
Grade 0 or 1 porous frit
10
Fig 2 insert
combined heat exchanger and reservoir
(modified reflux condenser?)
10
Fig 2 (reference electrodes not shown)
other components labelled in Fig 1

Table1

The lower bound heat transfer coefficient 10^9 $(\overline{KR})$11 / WK^ -4

Time Day + elapsed time / s	Day 1	Day 2	Day 3	Day 4	Day 5	Day 6	Day 7	Day 8	Calibration
	10^9 $(\overline{KR})$ 11 / WK^ -4	10^9 $(\overline{KR})$ 11 / WK^ -4	10^9 $(\overline{KR})$ 11 / WK^ -4	10^9 $(\overline{KR})$ 11 / WK^ -4	10^9 $(\overline{KR})$ 11 / WK^ -4	10^9 $(\overline{KR})$ 11 / WK^ -4	10^9 $(\overline{KR})$ 11 / WK^ -4	10^9 $(\overline{KR})$ 11 / WK^ -4	10^9 $(\overline{KR})$ 12 / WK^ -4
1800	-1.49344	-0.0858	0.48749	0.3779	0.47856	0.48795	0.47908	0.64577	0.699861
5100	-0.70979	0.34444	0.51924	0.3769	0.48069	0.66113	0.54971	0.62039	0.793504
8400	-0.1475	0.42665	0.56842	0.3857	0.47658	0.68499	0.56902	0.59728	0.85065
11700	0.10495	0.45897	0.60763	0.38513	0.49595	0.69593	0.57723	0.59217	
15000	0.7183	0.4719	0.6177	0.40059	0.50665	0.70689	0.58014	0.60469	
18300	0.36411	0.4878	0.59164	0.397	0.50006	0.71081	0.58526	0.60626	
21600	0.28528	0.48922	0.5781	0.40712	0.50202	0.7105	0.59038	0.61095	
24900	0.30856	0.48677	0.55356	0.40022	0.50282	0.71544	0.59203	0.61694	
28200	0.26101	0.48041	0.51248	0.39695	0.51479	0.70512	0.59315	0.62725	
31500	0.24967	0.50864	0.47774	0.41212	0.50517	0.72403	0.59709	0.63011	
34800	0.2712	0.50441	0.43012	0.41215	0.51357	0.74247	0.61318	0.6435	
38100	0.26742	0.49508	0.41646	0.41505	0.51741	0.7326	0.62329	0.64007	
41400	0.26761	0.50243	0.39305	0.40257	0.49585	0.72861	0.62832	0.61976	0.699861
44700	0.247	0.46767	0.38561	0.41191	0.53356	0.70235	0.63007	0.61181	0.793504
48000	0.26664	0.46198	0.38465	0.40935	0.51358	0.67093	0.63268	0.61681	0.85065
51300	0.26533	0.44048	0.37473	0.43111	0.5147	0.66371	0.63223	0.60868	
54600	0.2753	0.42352	0.3573	0.46939	0.51523	0.63598	0.62664	0.60996	
57900	0.27652	0.413	0.3722	0.44984	0.5181	0.57951	0.61373	0.60061	
61200	0.26939	0.40534	0.37129	0.44095	0.51988	0.56039	0.60734	0.5925	
64500	0.26886	0.42088	0.37269	0.45733	0.53518	0.54096	0.59936	0.59693	
67800	0.25866	0.39459	0.38063	0.47008	0.52991	0.54166	0.59795	0.60128	
71100	0.26025	0.39528	0.37277	0.45343	0.53734	0.53409	0.59504	0.60751	
74400	0.2728	0.38396	0.37248	0.46642	0.54202	0.5271	0.596	0.61177	
77700	0.2796	0.39671	0.36822	0.48096	0.52463	0.53565	0.59509	0.61503	0.699861
81000	0.28216	0.45576	0.37228	0.46812	0.53008	0.51625	0.59824	0.61224	0.793504
84300	0.27082	0.54783	0.37474	0.47155	0.51503	0.52769	0.59676	0.60391	0.85065

Table 2

		day 1				day 2				day 3				day 4				day 5		
Time day	I/Amp	10^9(k'R)11	Q excess	Q excess	I/Amp	10^9(k'R)11	Q excess	Q excess	I/Amp	10^9(k'R)11	Q excess	Q excess	I/Amp	10^9(k'R)11	Q excess	Q excess	I/Amp	10^9(k'R)11	Q excess	Q excess
elapsed		/ WK^ -4	10^9(k'R)12	10^9 (k'R)12		/ WK^ -4	10^9(k'R)12	10^9 (k'R)12		/ WK^ -4	10^9(k'R)12	10^9 (k'R)12		/ WK^ -4	10^9(k'R)12	10^9 (k'R)12		/ WK^ -4	10^9(k'R)12	10^9 (k'R)12
timelapse		recomb	0.69966	0.85065		recomb	0.69966	0.85065		recomb	0.69966	0.85065		recomb	0.69966	0.85065		recomb	0.69966	0.85065
day 1																				
1800		1.86993	0.01074	0.01149		1.33311	0.0863	0.1055		1.45751	0.06863	0.11746		0.93537	0.01721	0.02519		1.03015	0.01153	0.01939
5100		0.1141	0.01653	0.0185		0.9527	0.08956	0.12762		1.22418	0.07987	0.14797		0.92425	0.01715	0.02514		1.05208	0.01106	0.01891
8400		0.35531	0.01631	0.01923		0.88615	0.0915	0.1416	0.201	1.1297	0.07191	0.15503		0.94446	0.01673	0.02476		1.04119	0.01169	0.01956
10600										1.09292	0.056	0.15864								
11700		0.55773	0.01366	0.01713		0.86181	0.09219	0.14945						0.93925	0.01675	0.02477		1.05911	0.01062	0.01848
12900										0.76982	0.03566	0.12356								
15000		1.20315	0.00065	0.00372		0.85094	0.09222	0.1539	0.05	0.78795	0.03703	0.10555		0.55505	0.0159	0.02392		1.07303	0.01001	0.01732
18300		0.99111	0.00466	0.00704		0.8548	0.08862	0.15165		0.83058	0.03455	0.08296		0.95202	0.01818	0.02419		1.0809	0.0104	0.0182
21600		0.9512	0.00597	0.00813		0.85044	0.08946	0.15351		0.79961	0.02499	0.06095		0.96339	0.01569	0.02357		1.0759	0.01016	0.01791
24900		0.99413	0.00552	0.00767		0.84515	0.09121	0.15575		0.73352	0.02422	0.04917		0.95457	0.01593	0.02396		1.06786	0.01029	0.01816
28200		0.9359	0.00635	0.00854		0.83319	0.09182	0.15739		0.76005	0.02217	0.04025		0.94964	0.01617	0.02421		1.079	0.00972	0.01783
31500		0.91152	0.00652	0.00883		0.8533	0.08337	0.14911		0.79574	0.02059	0.03462		0.96275	0.01542	0.02349		1.07202	0.01013	0.01797
34800		0.92503	0.00632	0.00862		0.8635	0.08487	0.15031		0.80718	0.02113	0.03248		0.96444	0.01533	0.02338		1.06285	0.00966	0.01747
38100	0.00632	0.91559	0.0063	0.00857	0.1	0.84726	0.08669	0.15394		0.84704	0.01942	0.02975	0.02	0.9678	0.01519	0.02323	0.02	1.06446	0.00949	0.01733
41400		0.90318	0.00649	0.00877		0.86424	0.08459	0.14852		0.86855	0.01947	0.02867		0.95281	0.01594	0.02402		1.06653	0.01058	0.01839
44700		0.87782	0.00697	0.00929		0.8539	0.09202	0.15194		0.88806	0.01844	0.02729		0.95888	0.01557	0.02373		1.10138	0.00864	0.01847
48000		0.88766	0.00675	0.0091		0.88205	0.08582	0.1412		0.91102	0.01766	0.0261		0.94973	0.01585	0.02406		1.06704	0.00959	0.01734
51300		0.8934	0.00672	0.00899		0.89031	0.08486	0.13628	0.02	0.91547	0.0177	0.0259		0.97419	0.01459	0.02278		1.06913	0.00951	0.01725
54600		0.89035	0.00657	0.00896		0.89654	0.08996	0.13848		0.90987	0.01834	0.02639		0.99735	0.01342	0.02156		1.08864	0.0095	0.01725
57900		0.88824	0.00674	0.00913		0.90443	0.0895	0.13656		0.92378	0.01751	0.02557		1.00285	0.01336	0.0214		1.09814	0.00925	0.01691
61200		0.88534	0.00691	0.00929		0.91171	0.08918	0.13462		0.92976	0.01734	0.02529		0.99615	0.01421	0.02222		1.0962	0.00921	0.01693
64500		0.90073	0.00646	0.00885		0.94416	0.08173	0.12592		0.93355	0.0172	0.02512		1.01491	0.01283	0.0208		1.11369	0.00843	0.01615
67800		0.86425	0.00715	0.00957		0.9287	0.08784	0.13092		0.94378	0.01664	0.0245		1.03273	0.01204	0.01994		1.10959	0.00885	0.01832
71100		0.87189	0.00691	0.00927		0.93581	0.0864	0.12917		0.9385	0.01704	0.02489		1.02014	0.01269	0.02076		1.11949	0.00821	0.01583
74400		0.87246	0.00693	0.00937		0.92968	0.09055	0.13305		0.93728	0.01696	0.02487		1.03398	0.01213	0.01986		1.13364	0.00787	0.01538
77700		0.88269	0.00674	0.00918		0.94329	0.06642	0.1287		0.92898	0.01742	0.02534		1.05093	0.01132	0.01913		1.10956	0.00871	0.01631
81000		0.89624	0.00661	0.009		1.01495	0.06713	0.10848		0.93572	0.0172	0.02512		1.04204	0.01202	0.01984		1.11469	0.00862	0.01624
84300		0.87444	0.00681	0.00934		1.15002	0.03793	0.0784		0.93039	0.01724	0.02523		1.04129	0.01182	0.01962		1.10928	0.01088	0.01828

Table3

	← day 6 →			← day 7 →			← day 8 →		
Time Day elapsed time / secs	I/Amp	Q excess 10^9(K'R)12	Q excess 10^9 (K'R)12	I/Amp	Q excess 10^9(K'R)12	Q excess 10^9 (K'R)12	I/Amp	Q excess 10^9(K'R)12	Q excess 10^9 (K'R)12
		0.59886	0.85065		0.59886	0.85065		0.59886	0.85065
1800	↑	0.04165	0.04552	↑	0.12251	0.20892	↑	0.12251	0.20892
5100		0.01939	0.04651		0.1187	0.2387	0.4	0.1187	0.2387
8400		0.01051	0.05062		0.11751	0.25376		0.11751	0.25376
10800									
11700		0.00483	0.05116		0.11752	0.26196		0.11752	0.26196
12900									
15000		-0.00227	0.04665		0.11974	0.27053		0.11974	0.27053
18300		-0.00159	0.04797		0.11751	0.27213		0.11751	0.27213
21600		-0.00472	0.04843		0.1131	0.26888		0.1131	0.26888
24900		-0.00462	0.04417		0.11123	0.26676		0.11123	0.26676
28200		-0.0011	0.04537		0.1095	0.26423		0.1095	0.26423
31500		-0.00181	0.04042		0.10684	0.26194		0.10684	0.26194
34800		-0.00316	0.04714		0.0971	0.26913		0.0971	0.26913
38100	0.1	-0.00782	0.05977	0.2	0.09247	0.27674	0.3	0.09247	0.27674
41400		-0.01095	0.06732		0.09037	0.28131		0.09037	0.28131
44700		0.00295	0.08619		0.08975	0.28369		0.08975	0.28369
48000		0.02258	0.10967		0.08701	0.28233		0.08701	0.28233
51300		0.02686	0.11907		0.08745	0.28324		0.08745	0.28324
54600		0.02742	0.12875		0.09149	0.28013		0.09149	0.28013
57900		0.05282	0.13647		0.09713	0.26179		0.09713	0.26179
61200		0.05937	0.13059		0.09805	0.25513		0.09805	0.25513
64500		0.06574	0.13095		0.10368	0.25627		0.10368	0.25627
67800		0.06974	0.12444		0.10402	0.25607		0.10402	0.25607
71100		0.0739	0.12662		0.10483	0.25565		0.10483	0.25565
74400		0.068	0.12741		0.10333	0.24425		0.10333	0.24425
77700		0.06519	0.12351		0.10493	0.25465		0.10493	0.25465
81000		0.07105	0.13027		0.10041	0.24927		0.10041	0.24927
84300	↓	0.065	0.12151	↓	0.10205	0.2513	↓	0.10205	0.2513

2001-04-16

Bury Lodge heading

16th April 2001.

Dr. Melvin H. Miles,
Chemistry and Materials Branch.,
Research, and Technology Division,
Naval Air Warfare Center Weapons Division,
China lake, CA 93555-6100, U.S.A.

Dear Mel,

I now want to follow up my letter/FAX of 9.4.2001. One problem caused by my tardy correspondence is that I am bound to forget to answer some of the questions in your earlier letters so could you please draw my attention to any such omissions? I will then deal with these in a further letter.

I will start with a positive item, namely, the progress with the analysis of your Pd-Ce data set. At least this item is positive in the sense that one can follow a defined line of action - it is not positive in the sense that we will necessarily achieve a complete and satisfactory analysis of the data set, as will become apparent in this letter. First of all, I have made graphs of all the raw data (I actually did this quite some time ago before you started to plot the data by computer) and have then produced a listing of $(k_R')_1$ and $(k_R')_2$ which is contained in Table 1. You will see that this table is divided into 4 parts where categories A-C list the results according to the "noise levels" of the raw data and category D gives results for experiments which have identifiable and non-identifiable errors. Clearly, such a division is rather subjective but is nevertheless fairly useful.

You will see that the results show the usual pattern of an increase of "noise" followed by a decrease which we have previously found to be due to driving the system through a regime of "positive feedback". The exception here are the data sets for days 61-63. It is evidently difficult to achieve any sensible evaluation of $(k_R')_1$ and, especially, $(k_R')_2$ for the data in category C. However, the "noise" should not have an undue effect on the evaluation of the integral heat transfer coefficients as I found previously for some of the disastrous N.H.E. data sets (see my Poster at ICCF 7 - I believe you have a copy?). I note here that there is a definite increase in "noise" compared to the data sets collected in SLC and, later, at IMRA, Europe. Some of the causes can be identified e.g, the increase in "noise" of the temperature of the water bath which must be due to inadequate control of the room temperature. Such increases in "noise" could be taken into account by modifying the data analyses but I am not very keen to embark on such a venture. Should we discuss this point?

When I started this particular part of the data analyses, I became concerned that parts of the "noise" might be due to external factors (e.g. inadequate "noise" suppression in the mains: see an earlier letter) but I now think that this is unlikely. Most of the "noise" is in the cell voltage and the cell impedance is simply too low to allow such high levels of injection by external

sources. Comparison with data taken for the Pd-Ce-B electrode at identical times will be useful! More likely, the "noise" is due to fluctuating sources of excess enthalpy generation. In this connection, I note that the variability of $(k_R')_1$ and $(k_R')_2$ is considerably larger then those for correct "blank experiments" (Pt in D_2O) carried out in SLC or at IMRA Europe.

This brings me to an important point also revealed by Table 1. You will see that the "lower bound heat transfer coefficient" is pretty consistently larger than the "true value" and that this disparity increases with the current density. As I see it such a disparity can only be due to two factors: <u>either</u> the presence of "positive feedback" <u>or</u> the inclusion of a resistance external to the cell in the measurement of the cell voltage. I believe that the first explanation is unlikely because the effect is so widely distributed in time (which has not been the case in the previous examination of comparable examples). Of course, if the second explanation holds, then this greatly complicates the interpretations because such external resistances effectively mask the generation of excess enthalpy. In any event, assuming that we can work our way round this particular malfunction, we have to recognise that the significance of any observed excess enthalpy generation will be diminished compared to the situation where there are no such external resistances.

Let me reiterate here: I believe that you were given the incorrect leads linking the cell to the "switching unit" quite deliberately because I was told that the N.H.E. had found this malfunction in January 1995. Presumably, they just wanted to mess up your experiments. Incidentally, the original malfunction was due to my colleagues, as I found out in June 1995. It was one of the factors which led to my "parting of the ways." [117]

In view of this complication, I want to pay special attention to the effects of changes of current density and additions of D_2O. These give calibrations quite independently of the heater calibration pulse i.e. we can compare "like with like". I would therefore like you to check the Table and tell me whether I have correctly identified all the additions of D_2O, see comment r). It is especially important to check whether I have correctly located changes of current density in the absence of additions of D_2O, see comment s) on Days 16, 40 and 48. Incidentally, you asked some time ago whether the addition of D_2O complicates the analyses of the data sets. The answer to this is: not really, once the cell contents are properly mixed as the differential equation representing the calorimeters applies at any given point in time.

Incidentally, incidentally I was able to use the change in currant density in one of the data sets collected at Harwell to achieve a calibration of that cell and I could then show that there was excess enthalpy generation in that cell. Of course, the paper which contained this reinterpretation of the "raw data" was rejected for publication! I believe that M^2 [118] and Wilf Hansen found the identical effects for that particular cell but silence reigns!

I have therefore been much preoccupied with trying to prove the presence of such an external resistance in the current leads to the cell. Originally, I had thought that I might be able to do this

[117] JR Fleischmann's falling out with Pons at IMRA.
[118] JR "M^2" means Michael Melich.

from an examination of $\left(\overline{K_R{}'}\right)_{11}$ and $\left(\overline{\overline{K_R{}'}}\right)_{11}$ over appropriate ranges of the data sets. However, I now think that it is unlikely that this will he possible because the "noise" levels are too high. Thus see for example the results for the data sequence of Days, 4, 5, 6 and 7 following the "topping up" of the cell contents on Day 4. Here, I have corrected one of the N.H.E. inanities by including the calibration pulse, ΔQ, in the relevant time sectors. Fig 1 has been calculated with the parameters used by N.H.E., $E_{thermoneutral}$ = 1.54 V and C_PM = 490 JK^{-1} while Fig 2 has used $E_{thermoneutral}$ = 1.527 V and the "guesstimate" C_PM = 450 JK^{-1}. While Fig 2 is somewhat more satisfactory than Fig 1, I do not believe that we can draw any definite conclusions from such figures. Note in particular that $\left(\overline{K_R{}'}\right)_{11}$ is smaller during the calibration periods than outside these periods while the opposite is true for Fig 2!

Such figures do, of course, lead to some important conclusions quite apart from the presence or absence of external resistances. I will revise these figures in due course and will write to you further about the additional conclusions.

You will see that the variability of $\left(\overline{K_R{}'}\right)_{11}$ is much larger than that which we had previously observed for appropriate "blank experiments", e.g. see Fig 40 of one of the previous Reports (which is also Fig 40 of the SPAWAR Report). The principal cause of this variability is the fluctuation of the cell temperature. It follows, therefore, that the variability can be markedly suppressed by using the various versions of the integral heat transfer coefficients (and further suppressed by using appropriate averages of these coefficients). I am presently much concerned with investigating whether we can use this approach to demonstrate the presence of resistances in the current leads much in the way we did in Fig 7 of the ICCF 8 paper (as well as testing the evaluations of the various $(k_R{}')_{i,j,l}$ and of C_PM. More about this in due course.

However, overall, it is quite clear that the analysis of this particular data set will not give a clear illustration of the validity of the ICARUS Methodology. It is for this reason that I would like to examine the data for the experiment with the Pd-Ce-B electrode. Of course, the proper illustration of the validity of the methodology requires data for an appropriate "blank experiment". On the face of it, N.H.E, never carried out such a "blank" because they have steadfastly refused to reply to my requests for such data. However, I know that the assertion that they did not carry out blanks" is untrue because Stan Pons set up just such an experiment in December 1993, (I even have some graphs for the beginning of this experiment!) Furthermore, the results for several hundred "blanks" were removed from the data sets sent on to me here from France. You will see that it is crucially important to use any "near blank" which we may have, to test the performance of the instrumentation (the instrument function).

This leads me again to the question I posed in my last letter: how much time have we got before the Meeting in Beijing? We now have to assess whether it is possible to complete comprehensive analyses of both the Pd-Ce and the Pd-Ce-B data sets before that meeting - of course, we also have to decide whether a comprehensive illustration of the performance of the ICARUS System would be useful!

Any such analysis of the Pd-Ce-B data will require a set of plots of the "raw data" in the first instance. We will also need to prepare an appropriate set of figures to illustrate the text(s) (plural in case we are able to write an addendum to the Navy Report). In one of your earlier letters you asked how such figures could be prepared using Windows EXEL: it is certainly desirable to present both the Cell Voltage-time and Cell Temperature-time plots on a single figure such as in the example for the Pd-Ce experiment, Fig 3. (these are the "raw data" for Day 68 of this particular experiment). The problem with this software package is that it only recognises one scale for the abscissae. However, we can always work our way round this difficulty as follows: let us start with the cell temperature. We will get the plot illustrated if we construct a new set of abscissae [Cell temperature - 330.000]. In order to get exactly the same scale, we have to add a point 332.800K (a point which we can subsequently delete). If we now wish to add the plot for the cell voltage, we have to construct a second column of abscissae [cell Voltage – 7.050] x 10 where the "10" ensures that the plot is scaled correctly.

The only problem with this approach is that we finish up with a meaningless scale on the y-axes. The easiest way round this problem is to whiten out the original numbers (using white correction fluid) and then to fix on the new scales on the left and right hand sides using invisible write on tape (we use Sellotape here). It is in any case necessary to firm up the axes using a ball-point pen as these are always too faint to print well. We can then Xerox the cobbled-up figures. This approach certainly lacks elegance but it is cheap, cheerful and effective! A*nd fast!!*

Next, could you please vet Fig 4? One of the best places to look for the effects of "Heat-after-Death" is clearly the reduction in cell current on Day 26. We can see such "Heat-after-Death" even when using the faulty N.H.E. analysis (as we did in some of the earlier analyses of other data sets).

Could you also please vet Table 2? Columns 8 and 16 need to be completed. Columns 3 and 11 show changes in times for the addition of D_2O relative to the nominal start of each measurement cycle.

We will certainly have to comment on data such as those given in Table 2. This brings me to a point of difficulty which we will need to discuss. The only really valid way of measuring the current efficiency is to monitor the rates of gas evolution - as we did in our early work. The amount of D_2O added to the cell is certainly always somewhat larger than the amount electrolysed but this balance would disappear if we included the amount of D_2O evaporated at the temperature of the cell contents. Bearing in mind the results for the rates of gas evolution the explanation must be as follows: the rates of evaporation from the liquid contents of the cells are given by the modelling incorporated in the differential equation representing the calorimeters; however, the amount of D_2O leaving the cells is determined by the D_2O content of the gas at a temperature close to room temperature that is, there is condensation in the narrow bore vent at the top of the cells. Such an explanation brings the (amount of D_2O electrolysed + amount evaporated) into close accord with the amount added.

I have hitherto skated around this problem mainly because it complicates the discussion of the "boiling to dryness" episodes. Shall we discuss these points further?

Now for a miscellany of comments.

I have written to Fred Saalfeld but I won't embarass you by sending you a copy of my letter because I have sung your praises. I have done this in the context of my hope that the work can be continued, a matter which I will comment on further below. *Also in the letter I'll write to you next week.*

However, there is one point made in your letter to Fred Saalfeld which needs to be amplified, J.M. certainly always added low concentrations of calcium boride to the melts (at least they did this in the early days). I also got them to prepare the electrode materials under a blanket gas of cracked ammonia. I did this originally as a sort of "belts and braces" insurance policy against oxygen contamination. I realised subsequently that traces of hydrogen remaining in the metal facilitate the annealing process.

Here are two interesting vignettes. When the IMRA Materials Laboratory started to prepare Pd cathodes, they did not know about the calcium boride trick. 31 of the 32 samples they sent to us cracked disastrously. Thus we wasted months of work. The only person I have ever found who understood the importance of deoxygenation was Bill Huggins. [119] We finished up at one of the meetings (one of the meetings in Asti!) discussing the comparable deoxygenation of Cu which used to be done with birch poles - the origin of term "poling".

Next, there is your letter to M^2, Very good but I find it to be mildly threatening. Bearing in mind your video tape, the meeting in San Diego and the fact that certain people now know very well why I embarked on this venture, I would imagine that the Navy would be less than keen for you to work in China. I believe therefore that you should write to M^2 once more to explain that you would like to develop microcalorimetric methods further to see whether one can use such methods to determine the nature of the fusion channels with a possible further extension to look for evidence of photofission. *I will write to you for next week about this in the context of the "yellow letter".*

A second line would be the search for methods which would raise the energy efficiency of the processes so as to produce viable demonstrations (e.g. pulse electrolysis, fluidised beds). Such experiments could use materials prepared at NRL and/or the SPAWAR methodology.

I think you should bear in mind that one of Li's functions must certainly be the gathering of information. You may wish to tell your Intelligence Folks that I did a careful survey of various research projects in this field at the time of ICCF-5. The most definite conclusion which emerged from this survey was that the Chinese were spying on the Russians.

Writing this leads me to think that M^2 and Co, [120] might after all be quite willing for you to go to China but with suitable strings attached.

---

[119] JR He probably meant Robert (Bob) Huggins.
[120] JR "M^2 and Co" probably means Michael Melich and the Navy.

Sheila is exhausted! We are going to Sheffield on Wednesday returning here on Sunday so I will put off writing to you about "tapping the zero point energy" until next week. I will also put off writing to you further about the "Yellow letter" until that time.

Yours also exhaustedly,

Martin

P.S. There is an error of 355s in the timing of the calibration pulses for your Pd-Ce experiment. Now what do you make of that? I will show in due course how this screws up the determination of the heat transfer coefficients based on forward integration of the data, the methodology used by N.H.E.

Table 1 cont.

Table 1		category A		comments	category B		comments	category C		comments	category D		comments	comments
Time /Days	cell current /A	$10^9(k_a)_1$ /WK^{-4}	$10^9(k_a)_2$ /WK^{-4}		$10^9(k_a)_1$ /WK^{-4}	$10^9(k_a)_2$ /WK^{-4}		$10^9(k_a)_1$ /WK^{-4}	$10^9(k_a)_2$ /WK^{-4}		$10^9(k_a)_1$ /WK^{-4}	$10^9(k_a)_2$ /WK^{-4}		
49	0·35086	0·77267	0·75687	j)										j) the data sets indicated in Category D although wise is in Category A. Strange behaviour due to positive feedback?
50	0·50023	0·78597	0·77019	j) r)										
51	0·50024	0·77714	0·75525	j)										
52	0·50023	0·77130	0·75102	j)										
53	0·50048	0·78128	0·77417	j) r)										
54	0·60047	0·77421	0·69836	j) s)							0·77421	0·69836	j) k) o) p)	
55	0·80019			j)							0·78471	0·66757	j) k) p)	
56	0·69989			j)							0·77997	0·70499	j) p)	
57	0·50040	0·78070	0·76543	j) r)										
58	0·50040	0·77422	0·74795	j)										
59	0·50040	0·77287	0·74555	j)										
60	0·50040	0·78360	0·72873	j) h) q) r)										q) some unexplicable changes in cell potential
61	0·60060			j)	0·78268	0·78155	b)							
62	0·60060			j)	0·78440	0·74232	b)							
63	0·60060			j)	0·78496	0·60598	b)							
64	0·49986	0·79395	0·78692	j) r)										r) addition of D_2O
65	0·49985	0·78535	0·77115	j)										
66	0·49986	0·77857	0·76526	j)										
67	0·60998	0·78336	0·76012	j) r)										
68	0·60998	0·77688	0·77225	j)										s) change of cell current without addition of D_2O
	σ	0·00851	0·02098		0·00776	0·01746		0·00992	0·06384		0·00863	0·02766		
	mean	0·77531	0·75793		0·77114	0·78323		0·78324	0·74987		0·78396	0·70778		
	σ/mean	1·10%	2·77%		1·01%	2·26%		1·27%	8·51%		1·10%	3·91%		
Days 3-10	σ	0·01322	0·00492	Days 16-23, 32-35, 42-45, 48-52	0·00848	0·05724	Days 16-17, 20-21, 32-35, 42-45, 48-52	0·00615	0·01843					
	mean	0·76313	0·76078		0·77524	0·75277		0·77592	0·76107					
	σ/mean	0·422%	0·647%		0·836%	7·603		0·870%	2·42%					
Days 3-68	σ	0·00987	0·03829											
	mean	0·77622	0·75786											
	σ/mean	1·27%	5·04%											

Day

Day	I/A	Δt	Aut electrical	Aut evaporative	total heat evaporative	Aut added
1	0·15192		1·23326	0·00904	0·00904	
2	0·15192		2·46652	0·00904	0·00910	
3	0·15192		3·69979	0·00924	0·01254	
4	0·15192		4·93305	0·00924	0·01258	
5	0·15192		6·16631	0·00924	0·01232	
6	0·15192		7·39957	0·00924	0·01256	
7	0·15192		8·62284	0·00926	0·01232	
8	0·15192		9·85610	0·00952	0·03541	
9	0·15192		11·09936	0·00951	0·03795	
10	0·15192	−300	12·32691	0·00950	0·04225	
11	0·30427	−300	14·74179	0·01487	0·05712	
12	0·40542	−300	18·09218	0·03035	0·08740	
13	0·50781		22·21216	0·05681	0·14398	
14	0·60458	+300	27·13945	0·10783	0·25181	
15	0·60458		32·08971	0·11624	0·36805	
16	0·50763		36·19424	0·09497	0·46302	
17	0·50763		40·91696	0·09763	0·55965	
18	0·50763		44·93428	0·08674	0·64639	
19	0·50763		48·55189	0·06985	0·71624	
20	0·50763		52·66454	0·06579	0·78203	
21	0·50763		56·78718	0·06487	0·75790	
22	0·40625		60·08205	0·03386	0·79176	
23	0·40625		63·37693	0·03501	0·82677	
24	0·40625	−300	66·67182	0·03411	0·86088	
25	0·65183		71·95998	0·17873	1·03961	
26	0·19764		73·54810	0·00861	1·04822	
27	0·19764		75·13622	0·00709	1·05531	
28	0·19764		76·72432	0·00724	1·06259	
29	0·19764		78·57243	0·00727	1·06986	
30	0·19764		79·97654	0·00723	1·07709	
31	0·19764	−300	81·58777	0·00723	1·08432	
32	0·50831		86·69532	0·06084	1·14516	
33	0·50831		89·78296	0·03736	1·18252	
34	0·50831	−300	93·87059	0·07583	1·25835	
35	0·53846	−300	98·23529	0·11850	1·37685	
36	0·50834		101·47261	0·09431	1·47116	
37	0·50834		104·71393	0·09474	1·56590	
38	0·50834		107·95524	0·09468	1·66058	
39	0·53815	−600	112·46169	0·17160	1·83223	
40	0·49995		116·98135	0·18561	1·82784	
41	0·50017		120·43886	0·05115	1·87899	
42	0·50017		123·89636	0·05176	2·03075	
43	0·50017		128·25287	0·07965	2·04038	
44	0·50017		132·38938	0·05360	2·09397	
45	0·50017		136·52589	0·05347	2·14744	
46	0·49114		141·26115	0·05585	2·18329	
47	0·50415		144·41790	0·07348	2·25680	
48	0·55986		150·58607	0·05144	2·30844	
49	0·55986		155·05424	0·05083	2·35927	
50	0·50822	+300	159·11306	0·04982	2·40919	
51	0·50822		163·17189	0·04934	2·45853	
52	0·50822		167·23071	0·04935	2·50788	
53	0·60947	+300	172·09898	0·06676	2·57464	
54	0·60947		176·96724	0·06716	2·64180	

Day	I/A	Δt	Aut electrical	Aut evaporative	total heat evaporative	Aut added
55	0·50816		183·49172	0·07228	2·71408	
56	0·61408		188·41666	0·07423	2·78831	
57	0·50839		193·15555	0·07487	2·86317	
58	0·50839		197·89386	0·07495	2·93812	
59	0·50839		203·30217	0·08495	3·02306	
60	0·50917		207·40661	0·08958	3·11264	
61	0·60064	+300	212·07955	0·07773	3·19037	
62	0·50066		217·75147	0·07780	3·26817	
63	0·50766		221·90300	0·07815	3·34632	
64	0·49890	+300	226·40794	0·06785	3·41417	
65	0·49890		230·53173	0·07057	3·48474	
66	0·49890		234·65552	0·07059	3·55533	
67	0·49894		239·59272	0·08403	3·63936	
68	0·49894		244·48312	0·08618	3·72554	
69	0·60973		249·45703			

−3930

Molar Volume of air = 18·16 cu.

Enthalpy of evaporation = 4,672·6 J mol⁻¹

F = 96,484·56 coulombs mol⁻¹

Table 2

Table 2

Table 1 (Start)

Table 1		category A		comments	category B		comments	category C		comments	category D		comments
				a)			b)			c)			d)
Time / Day	cell current / A	$10^9(k_a^1)_1$ / WK^{-4}	$10^9(k_a^1)_2$ / WK^{-4}		$10^9(k_a^1)_1$ / WK^{-4}	$10^9(k_a^1)_2$ / WK^{-4}		$10^9(k_a^1)_1$ / WK^{-4}	$10^9(k_a^1)_2$ / WK^{-4}		$10^9(k_a^1)_1$ / WK^{-4}	$10^9(k_a^1)_2$ / WK^{-4}	
1	0·15192			e)									
2	0·15192			e)									
3	0·15192	0·76067	0·76131										
4	0·15192	0·76222	0·75882	f)									
5	0·15193	0·76508	0·76711										
6	0·15193				0·76232	0·76000	f)						
7	0·15193				0·75785	0·76099	f)						
8	0·15193				0·76492	0·76910	f)						
9	0·15192			f)	0·76585	0·75649	f)						
10	0·15193				0·76242	0·75298	f)						
11	0·50428			f)				0·79228	0·76333	f)			
12	0·40594			f)				0·78828	0·76247	f)			
13	0·50782			f)				0·79019	0·75750	f)			
14	0·60958			f)				0·80890	0·75759	f)			
15	0·60957			f)							0·79795	0·72976	f) g)
16	0·50763			g)				0·78536	0·77161	f)			
17	0·50765							0·77815	0·76931	f)			
18	0·50765			f)				0·77663	0·75167	f) h)			
19	0·50764							0·77684	0·83282	f) i)			
20	0·59765			f)				0·78165	0·75716	f)			
21	0·50764							0·77839	0·76973	f)			
22	0·40626			f)				0·77949	0·75914	f)			
23	0·40626							0·77645	0·76627	f)			
24	0·40625							0·77046	0·77162	f)			
25	0·66104							0·79187	0·73529	f) h)			
26	0·19764			f)	0·77532	0·77800	j) k) l)						
27	0·19765				0·77229	0·77389	j)						
28	0·19765				0·77011	0·77477	j)						
29	0·19765				0·76449	0·76940	j)						
30	0·19766				0·76168	0·75861	j)						
31	0·19766				0·75983	0·75266	j)						
32	0·50932			f)	0·77456	0·77723	j)						
33	0·50933			f)	0·78942	0·78637							
34	0·50933				0·77894	0·77948	j)						
35	0·55016			f)	0·77548	0·78823	j)						
36	0·40085			f)	0·77616	0·77986	j)						
37	0·40085				0·76914	0·79042	j)						
38	0·40084				0·76859	0·80937	j)						
39	0·55009			f)			m)						m)
40	0·49946			g)			m)						m)
41	0·50018			n) f)									
42	0·50018	0·76372	0·70783	j) k)									
43	0·50019	0·77490	0·75931	j)									
44	0·50018	0·77308	0·73570	j) k)									
45	0·50019	0·76913	0·76481	j)									
46	0·60110	0·77403	0·76715	j) f)									
47	0·60111	0·77344	0·75986	j)									
48	0·55025	0·77749	0·75241	j) g)									

comments

a) acceptable noise levels
b) increased noise levels
c) large noise levels
d) identified and unidentified errors
e) no calibration applied
f) note progressive increase in noise
g) unidentifiable fault on polarising circuit and/or measurement of cell voltage or positive feedback?
h) positive feedback?
i) $(k_a^1)_2$ difficult to evaluate
j) note progressive decrease in noise
k) note unusable relaxations
l) change of concentration? delayed response of film resistance?
m) fault on measurement of cell voltages
n) missing evaluation
o) increase in cell temperature with decrease in cell voltage

Fig. 1

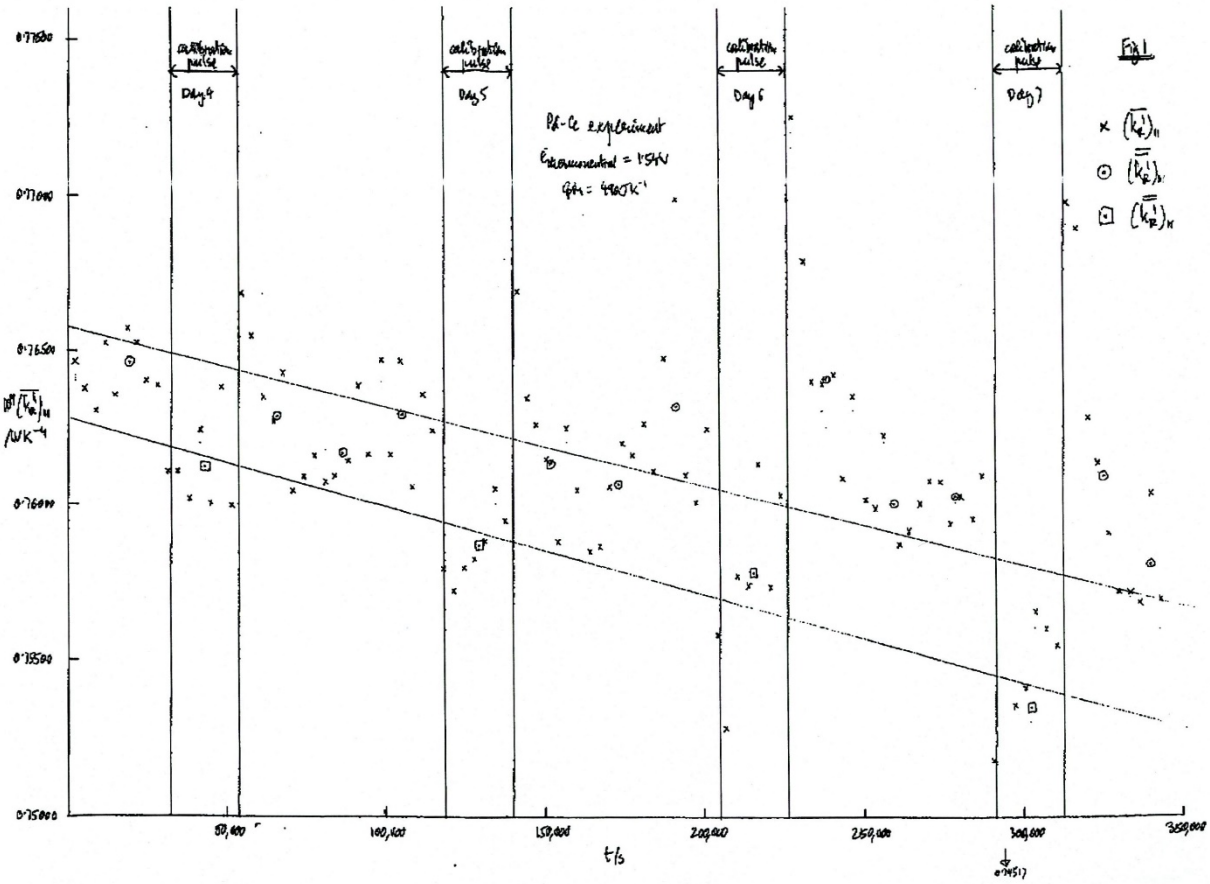

calibration pulse
Day 4
Day 5
Day 6
Day 7
Pt-Ce experiment
0·77500
0·77000
0·76500
0·76000
0·75500
0·75000
50,000
100,000
150,000
200,000
250,000
300,000
350,000
t/s
0·74517

Fig-2

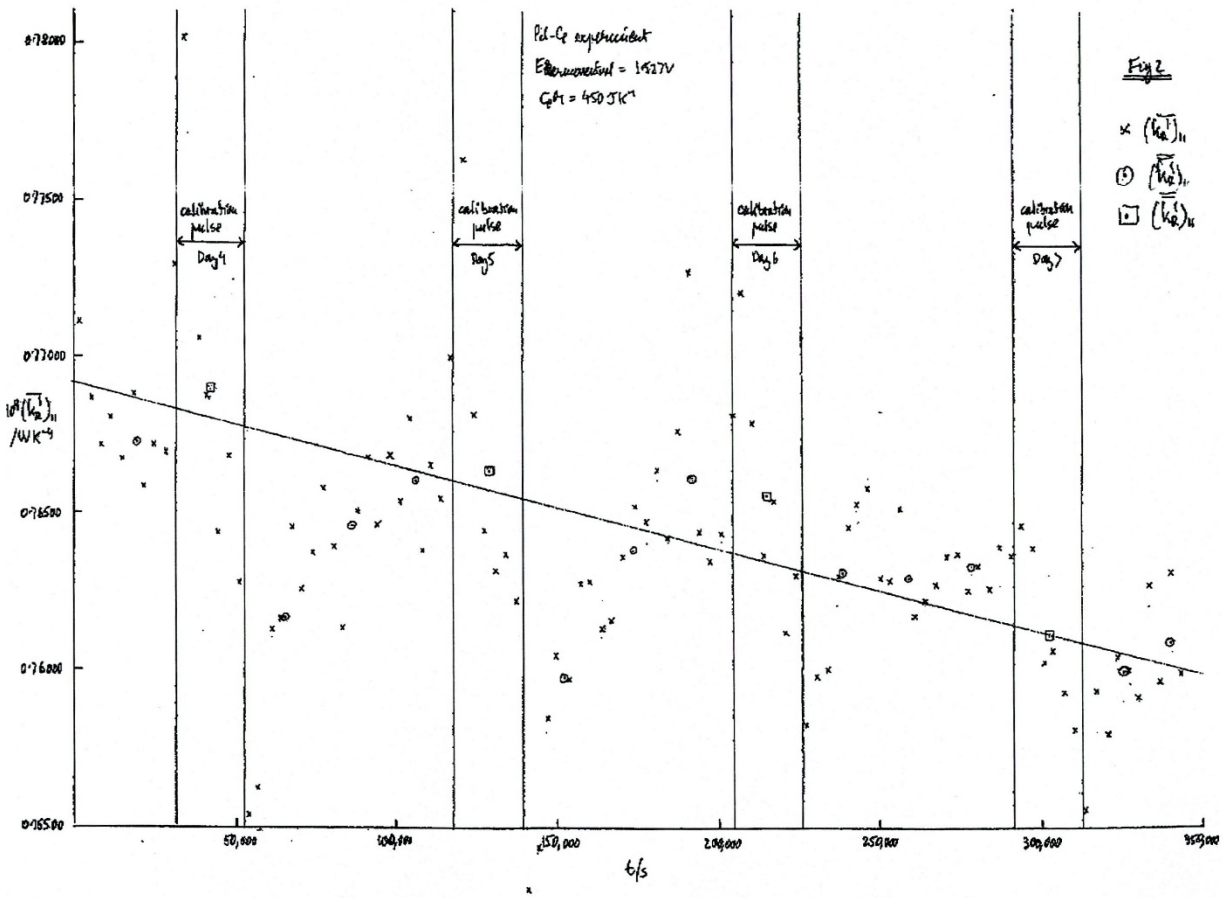

Pd-Cu experiment
Fig 2
calibration pulse
Day 4
Day 5
Day 6
Day 7
0·78000
0·77500
0·77000
0·76500
0·76000
0·75500
50,000
100,000
150,000
200,000
250,000
300,000
350,000
t/s

Fig. 3

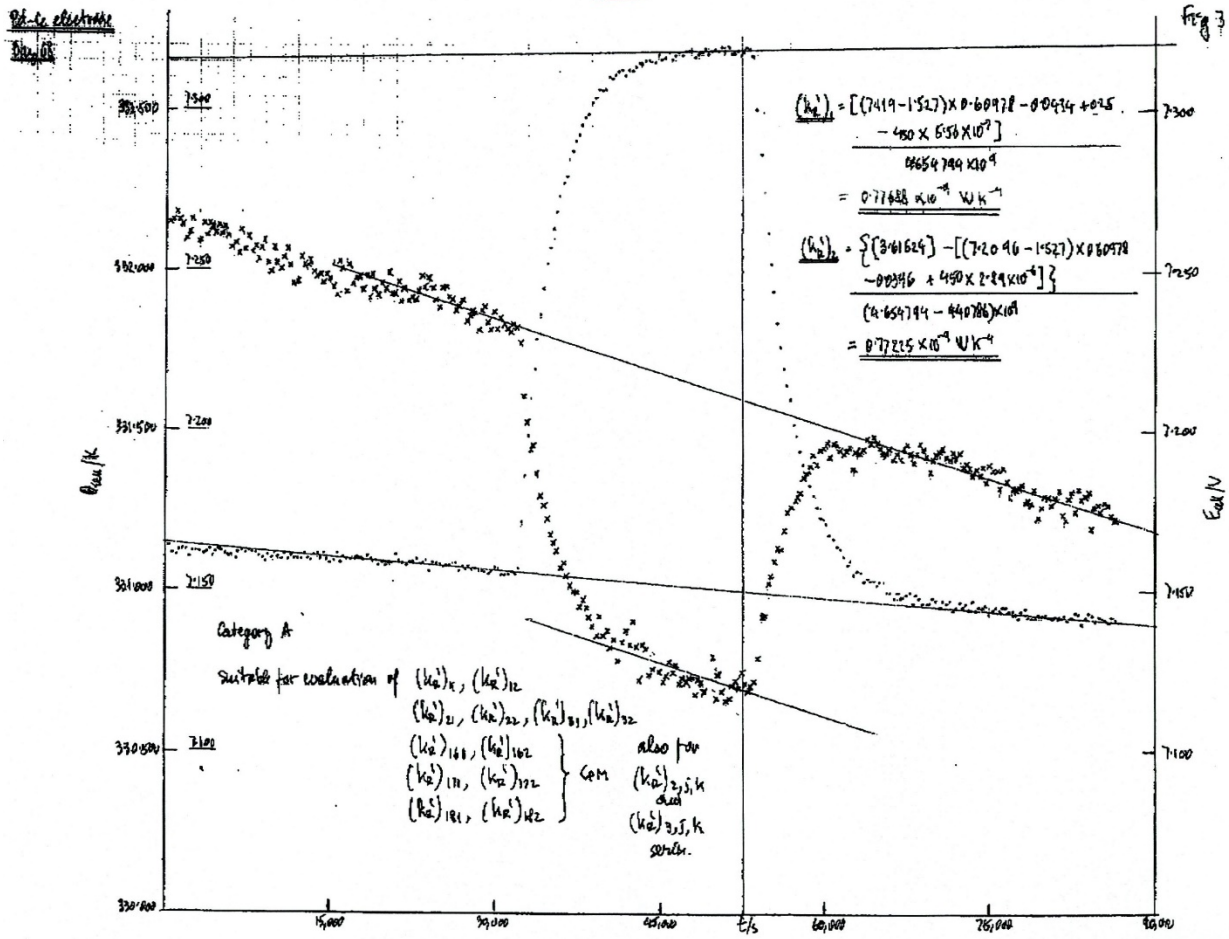
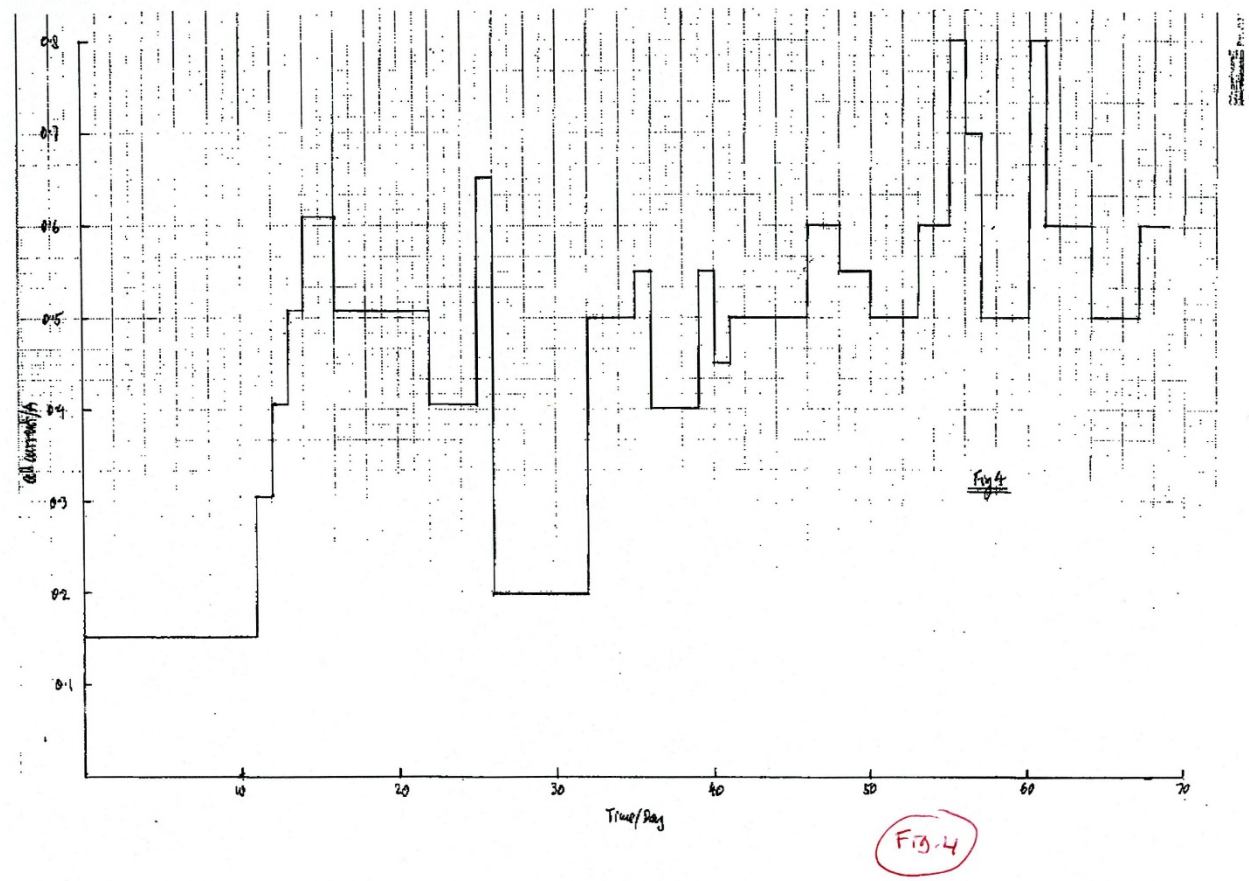


Fig 3
$(k_R')_1 = \frac{[(7.419-1.527)\times 0.60978 - 0.00474 + 0.25 - 450\times 5.58\times 10^{-9}]}{(4.654794\times 10^{9})}$
$= 0.77688\times 10^{-9}\ W K^{-1}$
$(k_R')_2 = \frac{\{[3.61624] - [(7.2096-1.527)\times 0.60978 - 0.00396 + 450\times 2.29\times 10^{-9}]\}}{(4.654794 - 4.40786)\times 10^{9}}$
$= 0.77225\times 10^{-9}\ W K^{-1}$
Category A
Suitable for evaluation of $(k_R')_1$, $(k_R')_{12}$
$(k_R')_{21}$, $(k_R')_{22}$, $(k_R')_{31}$, $(k_R')_{32}$
$(k_R')_{161}$, $(k_R')_{162}$
$(k_R')_{171}$, $(k_R')_{172}$
$(k_R')_{181}$, $(k_R')_{182}$
GPM
also for $(k_R')_{2,5,k}$ and $(k_R')_{3,5,k}$ similar.
θ_{cell}/K
E_{cell}/V
t/s
15,000
30,000
45,000
60,000
75,000
90,000


0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
10
20
30
40
50
60
70
Time/Day
Fig 4
Fig. 4

2001-04-22

Note: This Navy program was canceled a few months later. It was a weapons project at China Lake.

April 22, 2001

Dear Martin,

I urgently need your suggestions for our Pd/D studies. I am trying to load small Pd disk and then electro deposit copper, gold on some other metal to maintain the loading. We need about 30 minutes to transfer the Pd disk from the cell to the device we are testing. How can this best be done? We have very limited funding, hence I need an answer soon. My tests in 0.1 M NaOd with $CuSO_4$ *and* $HAuCl_4$ *don't seem to give the metal film that I desire. We could place the disk in liquid nitrogen to slow the de-loading. Any other suggestions?*

Thanks for your Fax of 16 April 2001. I will have to answer that later. Today I have been working on Stan Szpak's draft of a paper for Electrochimica Acta involving your analysis of the Pd+D co-deposition study. He will send this to you later for your comments or changes.

As part of this work today, I finally looked at your report on this dated 29 January 2001. I find this to be Excellent! It should be published as another Navy report. I may ask to see if we could publish it here at China Lake. It should be noted that the co-deposition studies in Cell A-1 and Cell A-2 give even much larger excess enthalpy. This data also needs your analysis. The Pd-Ce was performed in Cell A-3, hence the same cell constant should apply. My laboratory notes comment on the Pd-Ce cathode vibrations since it was near the bath stir. Perhaps this caused excessive noise in the voltage trace. . . .

[an e-mail sent to an ACS editor]

From: Melmiles
Sent Wednesday, April 18, 2001 8:11
To: chemistry@acs.org
Subject: Letter to the Editor

Editor, Chemistry

Letters To The Editor:

Your recent anniversary issue (pages 42-49) contained an article by Lynn Teo Simarski titled "Eureka or Oops?" that requires a few comments. The cold fusion claims in 1989 were obviously overstated for an effect that proved to be difficult to reproduce. However, the main weapon used against cold fusion has been ridicule - and this is not part of the scientific method. My work has shown a reproducible excess heat effect for a palladium-boron material prepared by Dr. M.A. Imam of the Naval Research Laboratory (NRL), Washington D.C. This is documented in a recent report, "Calorimetric Analysis of a Heavy Water Experiment Using a Pd-B Alloy Cathode" authored by Drs. M. Fleischmann, M.A. Imam and myself (NRL/MR/6320-01-8526, March 26, 2001).

My challenge for all critics of cold fusion is to use the scientific method and either accept the results of this report or clearly identify the scientific errors that invalidate this claim of an anomalous excess enthalpy effect. The actual raw experimental data is available in this detailed report (155 pages).

For those scientists who desire a more accurate version of the cold fusion story, I recommend "Excess Heat: Why Cold Fusion Research Prevailed", Oak Grove Press, 2000 by Charles G. Beaudette.

Dr. Melvin H. Miles
Naval Air Warfare Center Weapons Division
Code 4T4220D
China Lake, CA 93555

2001-06-03

NAWC heading

FAX MEMO

DATE: June 3, 2001

TO: Professor Martin Fleischmann

FROM: Professor Martin Fleischmann

MESSAGE:

Dear Martin,

We will be going to our cabin in Grants Pass for a month leaving on June 8 and returning on July 8. Our mailing address there is: General Delivery, Wolf Creek, Oregon 97497. We will have no telephone, email or fax at the cabin.

The Pd-B paper with you and Dr. Imam as co-authors was accepted for an oral presentation at the Electrochemical Society Meeting in San Francisco, September 2-7, 2001. I would be happy to have you present that paper if you want to attend this meeting. I have three papers that have been accepted for this meeting that will be included in this fax.

I believe Stan Szpak has sent you a copy of a paper that he has written involving your analysis of the co-deposition experiment in Japan. It is my understanding that the Navy report being prepared by Stan will be completed in a month or two.

My letter to the editor of the quarterly publication by the American Chemical Society has never even been acknowledged. I will email this letter to them again. It would be a good idea if you sent them a letter also. I plan to also let Jed Rothwell know about this situation.

The ICCF-9 Cold Fusion Conference in China will take place in May, 2002. You should have received the announcement. I am hoping that we can present a paper involving the Pd-Ce data or the Pd-Ce-B data or both.

Dr. Wilford Hansen of Utah State University called me several weeks ago. He has received a copy of the NRL Report and is very interested in looking at the data himself. I will send him all of the NHE data on a CD for his analysis. He will probably start with the Pd-B experiment. It will be good to get a completely independent analysis of this data. He would like to present his findings at ICCF-9 in China as well as publication in a reviewed journal. Dr. Hansen has been away from science the past two years because he and his wife were serving as Morman [sic] missionaries in New Jersey. He seems eager to get back into looking at cold fusion.

I hope this covers most of the matters of importance. I will contact you again by fax when I return from Oregon.

Best wishes

Mel Miles

Mel Miles

Note

0.01 M $CuSo_4$ + 0.1 M NaCN (1.0 mL of each) added to 19.0 cm^3 of 0.1 M LiOH gives reasonable plating at 50 mA/cm^2.

2001-06-06

Bury Lodge heading

6th June 2001.

Dr. M. H. Miles,
Chemistry and Materials Branch,
Research and Technology Division,
Naval Air Warfare Center Weapons Division,
China Lake, CA 93555-6100,
U.S.A.

Dear Mel,

Many thanks for your FAX of 3rd June and the various attachments. As I recall I last wrote to you sometime before going to Italy and I also collected the diagrams for a progress report on the analysis of the Pd-Ce data sets. However, I couldn't complete this report before leaving here and the whole matter has now been overtaken by the work I have been doing on the Pd-Ce-B data sets. These data sets are much more suitable for assessing the performance of the ICARUS-2 systems than are the sets for the Pd-Ce electrode. It strikes me, therefore, that I should write a single report on both these experiments because we could use such a report as a framework for an addendum to the Report NRL/MR/6320-01-8526 and/or a further paper (for ICCF 9 in the first instance?). I continue to believe that we should evaluate the performance of the instrumentation using the Pd-Ce-B data sets as a "near blank" (with suitable comments on the alleged absence of "true blanks"!) and then to show that deviation from this behaviour are due to the presence of fluctuating sources of excess enthalpy generation. This will allow us to "recycle" the Pd-B data and we should also discuss the possible widening of the authorship to allow the inclusion of comments on the calorimetric code deposition experiment.

I have been rather unwell recently and have had some further checkups which have led to the conclusion: "wait-and-see". However, just at present I need to avoid long journeys so I will definitely not be able to go to the ECS meeting in September - tempting though it is! Many thanks for the details and I take it that you will be going to San Francisco and will present the joint paper? Please let me know whether you will need any help in preparing this presentation.

My present state of health also casts some doubts as to whether I will be able to go to ICCF-9. If I have to have some further surgery in the late autumn/early winter, this would make May 2002 a somewhat borderline date for a return to the fray. However I do believe that it will be important to present a set of papers on your work in Japan. Will you be going to Beijing? Am I correct in my recollection that you have two further data sets which should be evaluated?

This brings me to a most important point: when I last wrote to you did I thank Linda for her sterling work on the hard copies? If I did not do so then ashes and sackcloth on my head and please give her my thanks. If there are two further data sets, then future help will be invaluable!

When I next write to you, I will include comments on the data evaluation and graph plotting - there are some hidden pitfalls in this story.

It strikes me that it would be best if I were to write some comprehensive letters for you to reach China Lake by 8th July. I dare say that you will not want to consider the various items during your vacation at Grants Pass?

Your contacts with Wilford Hansen cheered me up no end. He is an excellent scientist and, what is more, is scrupulously honest. One should not need to say this but I fear that intellectual honesty has been a casualty in the modern world. I met a Physicist recently who has written a book which includes comments on C.F. and I decided that there was no point in being mealy-mouthed about the subject. I said: "that is all wrong and many of the statements you have used are quite fraudulent." I also asked "why didn't you ask me?".

Your contacts with the editor of the A.C.S. Quarterly Publication are pertinent here. In days gone by the Editor would have been obliged to send the article by Simarski at the very least to me but almost certainly as well to other scientists who have worked in this field. The outcome would almost certainly have been that the article wouldn't have seen the light of day. Could you please send me a copy of Simarski's article? I feel inclined to make a fuss! Could you also please send me the name and address of the Editor in Chief of JACS and the name and address of the sub-editor of the Quarterly Publication?

I will also have to send you a commentary on the subject matter of the "Yellow Letter". That "Yellow Letter" was written in January but I still cannot decide what to do about it. However, was I not right in my belief that the excess enthalpy generation had to be established before the "flipside" of the research became the deciding factor? If the research had proceeded along these lines, this would have allowed a juxtaposition of the advantages (civilian) and disadvantages (military) and separation of the responsibilities into the research/power generation and political arenas. However, evidently, I was unsuccessful but I do ask myself whether the whole subject area has not been manipulated to ensure failure? What is your view about all this?

Have a good holiday!

Regards,

Martin

P.S. I owe Stan Szpak a letter. If you should have an opportunity to talk to him, then could you please give him my apologies and tell him that I will be writing soon!

2001-07-10

NAWC fax heading

(We are back from Oregon)

Martin,

this is the first draft of my viewgraphs for the ECS Meeting in San Francisco. There is only 20 minutes for the presentation. Please let me know regarding any suggested changes.

I talked to Stan Szpak today. The San Diego Navy report is about ready.

There has been no response to any of my letters to Michael Woods, Editor of the Chemistry Quarterly.

Best Wishes, Mel Miles

①

CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING A Pd-B ALLOY CATHODE

M.H. MILES
New Hydrogen Energy Laboratory, Sapporro-004, Japan

M.A. Imam
Naval Research Laboratory, Washington, D.C. 20375-5320

M. Fleischmann
ENEA, Frascati Research Centre, 00044 Frascati (Rome), Italy

2001 Joint International Meeting
The Electrochemical Society/The International Society of Electrochemistry
San Francisco, California
September 2-7, 2001

②

THE Q.E.D. PARADIGM

(The Need For Quantum Electrodynamic Applications)

THE Q.E.D. PARADIGM

Is it possible to devise electrochemical experiments which demonstrate the need for this paradigm?

Effects of time } → **space-time**
Space

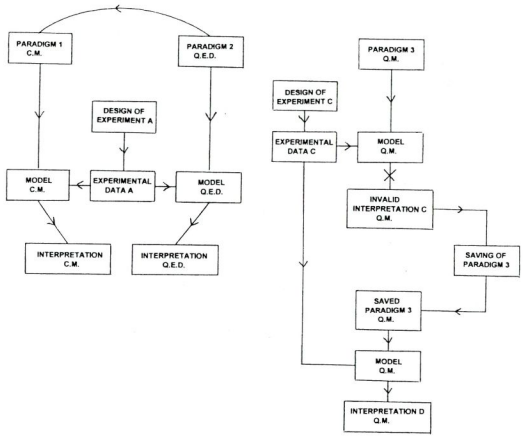
Length
Dimensionality
Number
Structure
→ **"Conventional" Systems**

Energy → **The link to nuclear processes**

Questions Posed By Martin Fleischmann Prior To The Development Of Work on Cold Fusion

CLASSICAL, QUANTUM ELECTRODYNAMIC AND QUANTUM MECHANICAL PARADIGMS

(3)

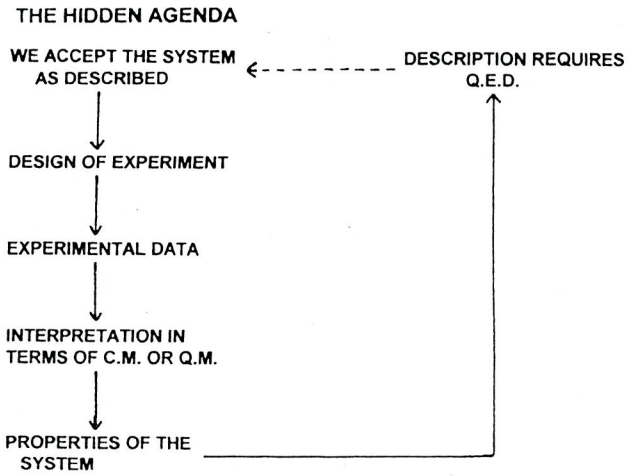


Martin Fleischmann's View Of The Status Of These Three Paradigms
(M. Fleischmann in *Proceedings of the 8th International Conference on Cold Fusion*, F. Scaramuzzi, Editor, Italian Physical Society, Lerici, Italy, 21-26 May 2000, pp XXIII – XXXIII).

(4)

THE HIDDEN AGENDA

(Design of Experiments That Require A Q.E.D. Description)



Martin Fleischmann's Hidden Agenda
(M. Fleischmann in *Proceedings of the 8th International Conference on Cold Fusion*, F. Scaramuzzi, Editor, Italian Physical Society, Lerici, Italy, 21-26 May 2000, pp XXIII – XXXIII).

(5)

GIULIANO PREPARATA / MARTIN FLEISCHMANN INTERACTION

BOOK

"QED COHERENCE IN MATTER" by Giuliano Preparata
World Scientific (Singapore, New Jersey, London, Hong Kong), 1995.

Chapter 8 Towards A Theory Of Cold Fusion Phenomena

- High Deuterium Loading Necessary ($D/Pd \approx 1$)
- Helium-4 Is Main Product
- High Excess Power Density Predicted
  ($P_{ex} \approx 1.25$ kW/cm^3)

(6)

NAVY RESEARCH AND REPORTS

Naval Research Laboratory (Washington, D.C.)

D.D. Dominquez, P.L. Hagans and M.A. Imam, "A Summary of NRL Research on Anomalous Effects In Deuterated Palladium Electrochemical Systems", NRL/MR/6170-96-7803, January 9, 1996.

- Pd-B Materials Produced

Naval Air Warfare Center Weapons Division (China Lake, CA)

M.H. Miles, B.F. Bush and K.B. Johnson, "Anomalous Effects In Deuterated Systems", NAWCWPNS TP 8302, September 1996.

- Excess Heat/Helium-4 Correlation

Space and Naval Warfare Systems Center (San Diego, CA)

S.J. Szpak and P.A. Mosier-Boss, "Anomalous Behavior of the Pd/D System", Technical Report 1696, September 1995

- Co-Deposition Method
- Tritium Production

Naval Research Laboratory
Washington, DC 20375-5320

#7

NRL/MR/6320--01-8526

Calorimetric Analysis of a Heavy Water Electrolysis Experiment Using a Pd-B Alloy Cathode

M.H. Miles

Chemistry and Materials Division, Research Department
Naval Air Warfare Center Weapons Division
China Lake, CA

M. Fleischmann

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England

M.A. Imam

Physical Metallurgy Branch
Materials Science and Technology Division

March 26, 2001

(8)

ELECTROCHEMICAL CALORIMETRIC CELL

(Fleischmann-Pons Dewar Type Cell)

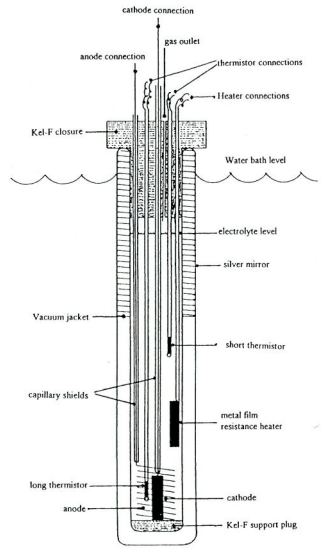


Fig. A.1 Schematic diagram of an ICARUS-1 Type Electrochemical Cell used in the Pd-B experiment. At NHE the long thermistor was positioned somewhat above the anode spiral. The NHE cell was 25 cm in length (with the top 8.0 cm silvered) and 2.5 cm in diameter.

(9)

CALORIMETRIC EQUATIONS AND MODELING

$$P_{calor} = P_{EL} + P_X + P_H - P_{out} - P_{gas} \quad (1)$$

where

$$P_{EL} = [E(t) - \gamma E_H]I \quad (2)$$

$$P_{out} = k_R (T^4_{cell} - T^4_{bath}) \quad (3)$$

$$P_{gas} = (\gamma I/F)\{[0.5\, C_{P,D2} + 0.25\, C_{P,O2} + 0.75\, (P/(P^* - P))C_{P,D2O\,(v)}]\Delta T + 0.75\, (P/(P^* - P))\, L\} \quad (4)$$

$$P_{calor} = C_{P,D2O\,(l)}\, [M^\circ - (1+\beta)\, (\gamma I t/2F)]\, (d\Delta T/dt) - (1+\beta)\, (\gamma I/2F) C_{P,D2O\,(l)} \cdot \Delta T \quad (5)$$

$k_R = 0.85065 \times 10^{-9}\ WK^{-4}$

$M^\circ Cp = 450\ JK^{-1}$

(NRL/MR/6320-01-8526, March 26, 2001)

(10)

POSITIVE FEEDBACK

(Cell Temperature and Voltage For Day 3)

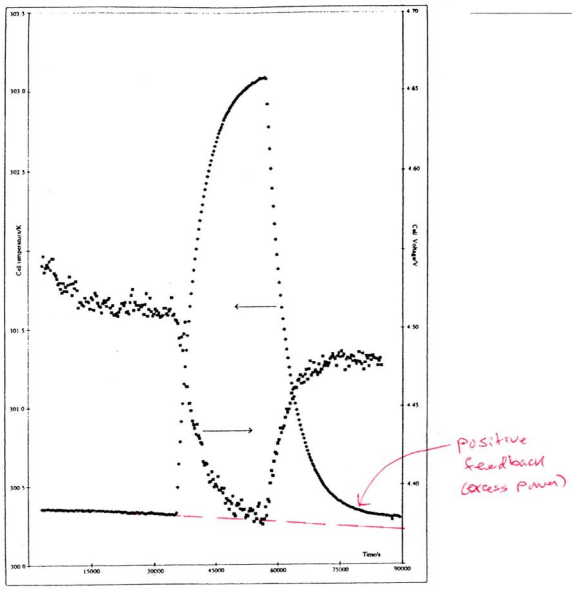
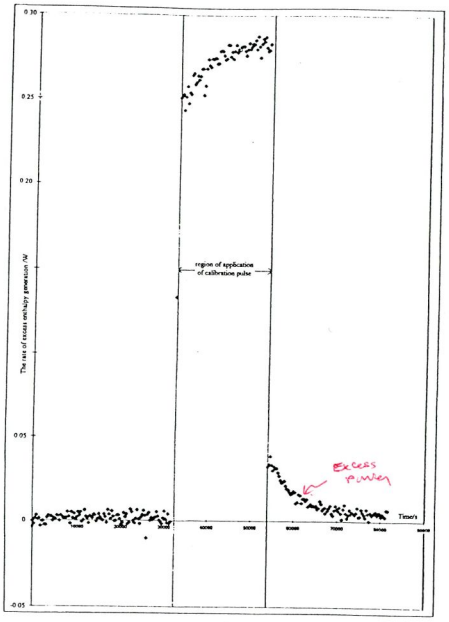


Fig A.7 The "raw data" (the cell temperature and cell potential as a function of time) for the third measurement cycle of the Pd-B experiment.

(11)

RATE OF EXCESS ENTHALPY GENERATION FOR DAY 3

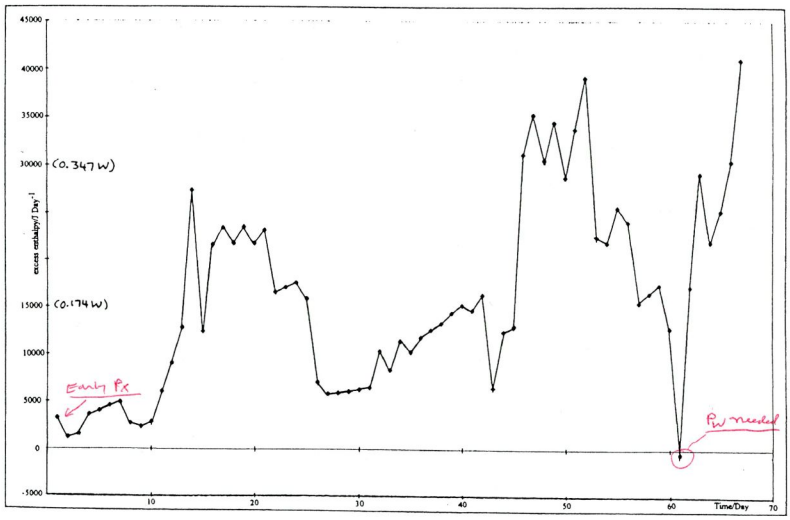
(Inclusion of Calibration Pulse, ΔQ=0.2500W)



(12)

EXCESS ENTHALPY GENERATION PER DAY FOR THE Pd-B EXPERIMENT

(Martin Fleischmann Analysis)



(13)

EXCESS POWER GENERTION FOR THE Pd-B EXPERIMENT

(Miles Analysis: k_R=0.811x10^{-9} WK^{-4}, M° Cp=490 JK^{-1})

Too Small (Negative Px results in places)

Too Large (450 JK⁻¹)

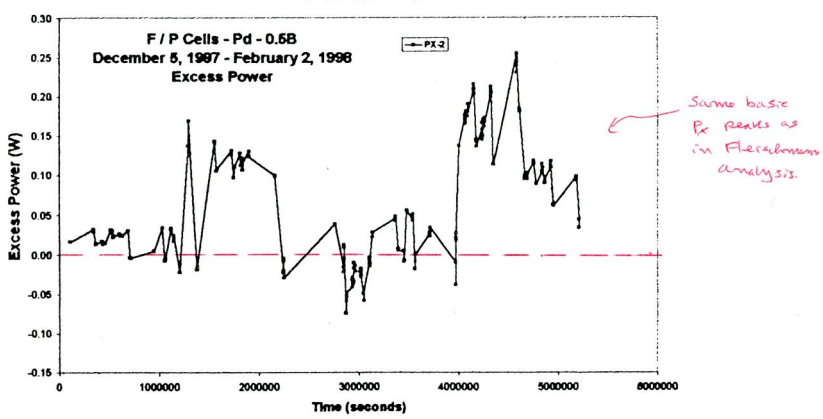


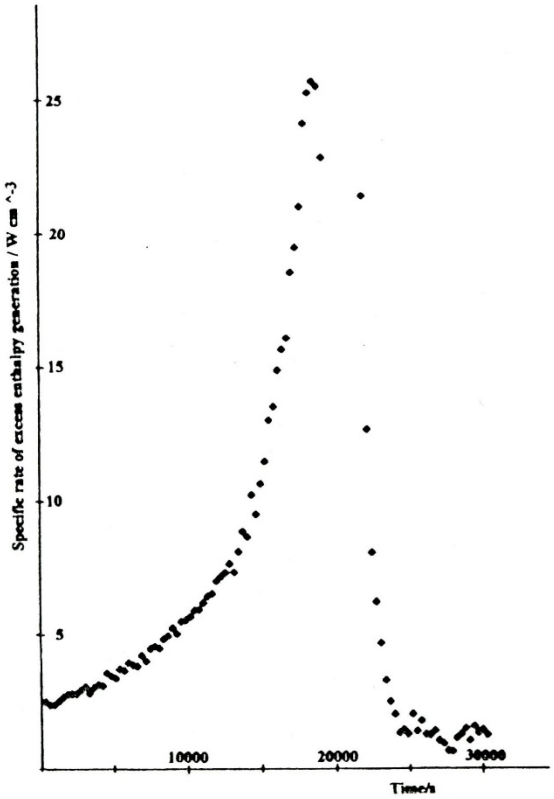
Figure 2. Excess power measurements for the Pd-B cathode.

(M.H. Miles in *Proceedings of the 8th International Conference on Cold Fusion*, F. Scaramuzzi, Editor, Italian Physical Society, Lerici, Italy, 21-26 May 2000 pp. 97-104).

(14)

RATE OF EXCESS ENTHALPY GENERATION DURING BOIL-OFF

(Day 68)



(15)

POSSIBLE ROLE OF ADDED BORON

(0.75 Weight Percent Boron)

- Increased Palladium Hardness
- Deuterium De-Loading Much Slower
- Oxygen Removal During Arc-Melting Process

(16)

SUMMARY OF Pd-B RESULTS

- ✓ Early Appearance of Excess Heat Effect
- ✓ Positive Feedback Observed
- ✓ 700 kJ of Excess Enthalpy Produced
- ✓ Cell Boil-Off Produced Largest Excess Power ($27\ W/cm^3$)
- ✓ Excess Enthalpy Effect Is Reproducible Using Pd-B Materials (8 out of 9 Experiments Produced Excess Power)

Cold Fusion Experiments Initiated by Martin Fleischmann as Test For Q.E.D. Paradigm

2001-07-13

Bury Lodge heading

13th July 2001.

Dr. M. H. Miles,
Naval Air Warfare Center,
Weapons Division,
China Lake, CA 93555,
U.S.A.

Dear Mel,

Greetings and welcome back from Grant's Pass. I trust that you had a good holiday!

Many thanks for the draft of your presentation to the meeting in san Francisco: splendid! I feel a heel for not going to the meeting but I know that I have to ease off somewhat as I will explain to you when we next meet.

My only comment on the content of your lecture is that I think that you will be hard put to present all of this material in just 20 minutes. However I know that you are very experienced in these matters and I am sure that you will be able to decide where to "skate" and, maybe, even what to leave out. Of course, it would be good to start to present some of the background material to show that Cold Fusion was part of a much wider programme, but do you really think that you should include your page 3? It may well be that any explanation such as a flow chart will simply take too long.

Do you think that you should include the ISBN numbers for the Conference Proceedings of ICCF 8 (pages 2, (3), 4, 8, 10, 11, 12, 14) as well as references where appropriate to the report NAL/MR/6320-01-8526? This would counteract the statements frequently made by others that this information is not available in the literature (especially if you make copies of your viewgraphs available at the meeting).

Having suggested that you might wish to eliminate page 3, let me also suggest that you should add figure A-14 of the NRL Report (page 74) after page 11 of your draft. I think that Fig A14 gives a very clear demonstration of the effects of "positive feedback" and of the persistence of this effect following the cancellation of the perturbation which in turn points to the presence of "Heat-after-Death". I think that such an inserted figure could be explained very rapidly.

As the E.C.S. is linked to J.E.S. and I.S.E. is linked to Electrochim. Acta, should we explore whether there are any plans. to publish the papers presented at the San Francisco meeting? It might be an interesting exercise to see whether these Journals would ever accept a paper dealing with "Cold Fusion" and/or how they would set about rejecting such a paper. This brings to mind the truly amazing gyrations which followed my attempt to have a paper published in the Acta following a meeting in Edinburgh notwithstanding the fact that such a publication had been

promised ahead of my decision to take part in the meeting. Would you like to have details of this sordid story to add to your collection?

It strikes me that I should send you some further details to illustrate your page 2. Last year I used this diagram as a framework for a lecture on "Unfinished Business" (which I gave in Southampton). I have in mind giving you illustrations of "space-time," "number" and "structure" which to a greater or lesser extent also illustrate your page 4. Of course "energy" was a really longshot: the real bomb-shell would be the revelation that the deciding factor here was the advent of the D.U. shells! I realise that we simply can't do this and I haven't as yet sent you the letter which I wrote in January.

The analysis of the Pd-Ce-B data sets is progressing and I will shortly send you a progress report on the first stage of this analysis. The outcome is that the ICARUS system in Sapporo behaved exactly as specified in the Handbooks. I believe that this is very important because it was, after all, always possible that there were some malfunctions. Do you think that it would be possible to prepare a Navy Report giving say pages 1 – 58 of the raw data and outlining the analysis with an invitation to A.N. Other(s) to point out to us the error of our ways? This could then form an essential preliminary to a further invitation to analyse the data sets used in NRL/ MR/6320-01-8526. Failing any such analyses, the statements made by other people should be seen to be discredited. You will see that I have it in mind to mount an assault on the Editor of Chemistry Quarterly.

More anon and regards.

Martin

P.S. My FAX is playing up again. As you will see, I got yours of 10/7/2001 but it seems to have given up the ghost since then. Could you use the number of Wood & Co for the time being?

2001-08-05

This is a 3-page handwritten fax, shown on the following pages.

Following the note is part of the rough draft of a paper, probably this one:

http://lenr-canr.org/acrobat/MosierBossthermaland.pdf

Bury Lodge, Duck Street, Tisbury, Salisbury, Wilts SP3 6LJ

Phone (+44) (0) 1747 870384 Fax (+44) (0) 1747 870845

From Professor Martin Fleischmann, F.R.S.

5th August 2001

Dr. M. H. Miles,
Chemistry and Materials Branch,
Research and Technology Division,
NAWPC,
China Lake, CA 93555-6100, USA.

FAX: 001-760-931-1617.

Dear Mel,

As you will see, my FAX is operational again.

It was good to have your news and I must again apologise for being such a bad correspondent. I was glad to see that you are only severing your connection with NAWPC temporarily and, as always, I hope that the funding situation will turn around sometime soon. More about this anon.

The draft of the paper you sent to me on 20/7/2001 will do very well – in fact I would like to nominate you as the author of all our joint publications! It seems to me that papers "mature" with age. The representations are always better than the original. I only have extremely minor corrections on pages 3, 4 and 5 which I am attaching to this FAX. As far as the modelling of the cells is concerned, your equations (1) – (5) make all the essential points and are certainly easier to incorporate in the text than would be equation (B.1) of the Report. I wrote the equation in that form for "psychological reasons". However, if you prefer that form to your own equations, then by all means use it.

As far as the third sentence in the first paragraph of your page 3 is concerned, you might wish to consider the following wording:

"The thermodynamic quantities were referred to the temperature of the water bath with the exception of the vapor pressure of D_2O (P) which was calculated from the Clausius-Clapeyron equation applied at the measured cell temperature".

As I told you in an earlier letter, there is a grey area here: this methodology certainly applies to "blank cells" but I believe that the evaporated D_2O is recondensed and returned to the cell at low to intermediate temperatures. However, evidently, the evaporated D_2O is removed as we approach the boiling point.

Again, as I told you, Emilio was visiting us here last week. They had just reached the point of making you an offer (in the LEDA days) when the industrial sponsors pulled the plug on the whole operation. I'll tell you about all this when we next meet. The recent results at Frascati are quite spectacular and Emilio and I have reached an understanding on the basis of QED Coherence. It is interesting how much we are conditioned to think about science in terms of models — even people like myself and Emilio who are always in the Baconian tradition: giving precedence to experimental information.

As I have said, these results are spectacular but also give much cause for concern in the sense of National Security. This leads me back to our meeting in San Diego. It was evident that L.W. and M.M. did not want me to ramble on about the wider issues and I for my part was very glad that they did at least endorse Stein's programme. I think that this is what Frank was really wishing to achieve although I would push Stein's approach in a somewhat different direction (measurements of the heat release in individual events possibly coupled to the development of laser thermometry). A discussion of these topics could have led to the need to examine Coherent Photofission where I would have proposed the setting up of two teams

to develop the subject: Phil Ratcliffe, Emilio Del Giudice in Italy possibly with a third member familiar with all the complexities of nuclear processes (Talio Bressani?) and Scott Chubb and Peter Hagelstein in the U.S. with an adventurous Quantum Mechanician, Alan Widdom. L.W. would be a useful member but I doubt whether he would wish to be so closely identified with the subject? Do Peter Hagelstein and Alan Widdom know each other? Do they dislike each other? (they are both in the Boston Area)

Emilio told me that Alan Widdom has been hired as a consultant by Pirelli. Emilio also told me that Pirelli is now the largest firm in Italy (bigger than Fiat!) having bought Olivetti which has, in turn, bought Italian Telecom. Tronchetti Provera must now be the richest man in Italy but will this do us any good? He seems to be preoccupied to pre empt all our research – hence Alan Widdom (who is a very smart fellow).

The Pd-Ce-B saga is progressing apace and I will send you the long promised progress report to Tennessee. As I told you in my last FAX the results follow the ICARUS model but I can also prove that they gave you at least one ICARUS-1 type lead so all the energy inputs were overestimated. Now what do you make of that? Presumably they just wanted to mess up your results but, fortunately, they did not understand the difference between precision and accuracy – but then few people do!

Regards and more anon,

Martin

2001-08-06

NAWC Fax heading

6 August 2001

Last Fax from China Lake

[MM Moved to Tennessee to teach at MTSU. Lack of C/L funding for me.]

Martin,

Thanks for your fax. I made the corrections you marked.

Please check with Italy and let me know if they will fund either of us to present this paper at San Francisco.

Talbot Chubb does not want to present it — but he will try to find Navy money to pay my way. [121]

Best Wishes,

Mel Miles

[121] MM I finally received ONR funding for the trip.

2001-08-14

Melvin H. Miles, Ph.D.
Department of Chemistry
Middle Tennessee State University (MTSU) P.O. Box 68
Murfreesboro, TN 37132 USA
Phone: 615-893-9910 (home)
Phone: ?????????? (work)
Fax: 615-898-5182 (work)
e-mail: ?????????????(work)
e-mail: melmiles@bellsouth.net (home)

FAX MEMO

DATE: August 14, 2001

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

We arrived in Tennessee Saturday afternoon and have settled in to an apartment here. I have been assigned an office but have no phone yet (or computer). Classes begin next Monday, August 20, 2001, and I will be teaching the Physical Chemistry course here. Once I get settled in, I hope to have time to examine the cold fusion data from Japan.

I am still wondering about what to do about the Pd-B paper for the San Francisco meeting in September. Have you heard anything from Italy regarding possible help in covering this trip for either you or I to attend?

I am on Leave Without Pay (LWOP) from China Lake and possibly will go back to work there in December (semester break), if funding is available, to test your ideas. I think they will try to proceed with some of the tests while I am away. That's about it for now.

Best wishes,

Mel Miles

2001-08-15

Bury Lodge heading

15th August 2001.

Dr. M. H. Miles,
Department of Chemistry,
Middle Tennessee State University (MTSU),
Murfreesboro, TN 37132, U.S.A.

Dear Mel.

Welcome to Tennessee!

When I checked my FAX this morning, I found your FAXes of 9/8/2001 and 14/8/2001 buried beneath a heap of junk mail. This junk mail is a new phenomenon here in the U.K. One gets all manner of offers, inquiries etc, which at first sight seem perfectly reasonable. However, I am pretty sure that the senders have some sort of deal with the telephone companies to finance this advertising by overcharging on the FAX replies - just as is the case for scratch cards. Do you suffer from this also in the U.S.? I am mentioning this sordid story because it will explain the delay in my letter to Dr Pearson.

Everybody appears to be on holiday in Italy which is rather like France in that labs are virtually shut down in August. Of course, it would be much the best solution if you were to give the paper in San Francisco. Could you please let me know a.s.a.p. how much support you would need? I have given the gist of the content of the paper to our friends and have told them that it is highly appropriate for you to raise the question of the applicability of the Q.E.D. paradigm with a general audience. I am sure that you have it in mind to "duck" awkward questions (if this should prove to be necessary) by saying "that's what the man says; if you want to know more, ask him."

I hope and trust that you will be able to initiate some of the new work in China Lake come December. Stan Pons and I had reached the point of wanting to switch attention to a metal higher up in the Periodic Table during the Summer of 1988 but, of course, we could not do this without the blessing of the D.O.D. However, all our (my) attempts to enter into a meaningful debate have come to nought which I find to be rather interesting. More about this when we next meet.

The tail end of the 10MW cm^{-3} episodes (bangs) seem to produce Ni but, of course, all of this would be much better carried out with a metal higher up in the Periodic Table.

I have started a renewed onslaught on your Pd-Ce-B data sets. This is all turning out along the lines of "how long is a piece of string" but I believe that it is worth doing just to set the record straight with regard to the way the experiments and data analysis should have been carried out.

Best of luck with the Physical Chemistry Course! It will make quite a change from your usual duties and I am sure that you will have an excellent rapport with the students.

Regards,

Martin

2001-08-24

Melvin H. Miles, Ph.D.
Department of Chemistry
Middle Tennessee State University (MTSU) P.O. Box 68
Murfreesboro, TN 37132 USA

FAX MEMO

DATE: August 24, 2001

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

Your letter of 15 August 2001 got mixed in with letters of recommendations that were faxed. *[found it yesterday]* Yours was also received and I thank you for doing this. I may apply for the permanent position when it is advertised next year, but that will depend on the funding situation at China Lake. Thus far, it has been a pleasant change from my worries about funding at China Lake.

Regarding the San Francisco meeting, Talbot Chubb sent an email to Fred Saalfeld, head of ONR, and I promptly received a telephone call from Richard Carl in of ONR with an offer of $2K to cover my travel expenses to San Francisco. [*Thanks to Talbot*] Therefore, I will attend the meeting and present all three of my papers. As you mentioned in your letter. I will have to refer any questions about Q.E.D. back to you.

Regarding the Eagle Program at China Lake last year, they delayed releasing the money until January 2001 because they wanted a detailed plan. My main work was finding a plating method for containing the hydrogen in the metal for the 5 minutes necessary to set up the test. A plating of copper proved to be better than silver or gold – at least in my experimental setup. In order to conserve money for the actual testing, I took leave without pay and spent a month (June) in Oregon at the cabin. The other people wanted to start the actual testing in July. However, when I returned I found that Robin Nissan, head of chemistry, had spent all this money on other programs, thus we could not carry out the testing. It was at this point of frustration that I receive the offer to come here to MTSU to teach. Don Thompson, my co-worker on this program, has continued to try to get funding. We have applied for a second year to test these ideas. However, it will be a few weeks before we will know of any decision. If funding is received, I would like to work on this in December when I return to China Lake during semester break. However, it

does seem like there are people who don’t want us to test these ideas. I would not be surprised if a black program already exists involving such experiments.

Thanks again for your help. Note new address, email address, and phone numbers above.

Best wishes,

Mel Miles

Mel Miles

2001-09-17

Melvin H. Miles, Ph.D.
Department of Chemistry
Middle Tennessee State University (MTSU) P.O. Box 68
Murfreesboro, TN 37132 USA

FAX MEMO

DATE: September 17, 2001

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

We are trying to get back to normal after the tragic terrorist attacks last week in New York and the Pentagon. Linda and I were on American Airlines just a few day earlier to San Francisco and back.

The general attitude at San Francisco was surprise that anyone was still working on cold fusion. My trip report contains details of questions that were asked. One scientist insisted on explaining the "heat-after-death" effect by reaction with oxygen from the atmosphere. There were no questions involving Q.E.D.

I am mailing you the final copy of my Pd-B paper submitted for publication in the Symposium Volume. I would like to submit this paper directly to the Journal of Electrochemistry for publication also. Please let me know if this is O.K. with you. Also, please send me any suggested changes before I submit this paper.

I plan to attend ICCF-9 next May in China. Perhaps the Pd-Ce-B data could be presented there or the Pd-Ce data. The co-deposition experiments are also a possibility. I doubt that China Lake would publish any of this as a Navy Report - but I am sure that Frank Gordon/Stan Szpak would publish this as a Navy Report. Another possibility is that Dr. Imam might publish the Pd-Ce-B data as a NRL Report since he made this material.

Please let me know what you think.

My last Fax received from you was dated 15th August 2001. Is this correct? I want to make sure that your faxes to MTSU are getting to me correctly.

Best wishes,

Mel Miles

2001-09-18

Bury Lodge heading

From E.N.E.A, Frascati, Italy *18th September 2001*

Dr. Stan Szpak,
SPAWAR
San Diego, U.S.A.

Dear Stan and Pamela,

We are all intensely depressed by the terrible news which we have been receiving from the U.S.A. It does not seem to me that we can take any effective action against such insane terrorism except to raise our own security measures. They are much in evidence in the U.K.

As you will see, I am writing to you from ENEA, Frascati. I had intended to write to you at some length from the U.K. before I left (a long overdue reply to your letter of the 30th April 2001!) But, unfortunately, I again ran out of time. One consequence is that you will have to put up with this handwritten letter – I hope that you will be able to make out what I will say.

I believe that it is essential that we should speak/write to each other very frankly because it seems to me that there is a divergence in attitudes between you, Pam and Mel on the one hand and me on the other. Unfortunately, I have not brought with me the text of the letter/report which I sent to you on 24/01/2001, i.e. the report on the experiment which Mel carried out during his stay in Japan (the calorimetry of the co-deposition methodology). However, I believe that I can remember the gist of that report sufficiently well for the purposes of this letter. Perhaps it would be best if I start by commenting on my attitudes and the causes of these attitudes? With the passing of the years I have become increasingly convinced that <u>either</u> the Japanese scientists concerned with this research programme were completely incompetent <u>or</u> that their statements were fraudulent. A key factor here is that they have apparently never carried out a "blank" experiment although they were repeatedly asked to do so and that the Pt/D_2O system was recommended to them for this purpose. Furthermore, they were given the results for such experiments carried out in Sophia Antipolis. Now I actually do not believe that the Japanese scientists are incompetent – there is much too much evidence that they are actually highly competent. Equally, I do not believe that they have never carried out a blank experiment. The reason why they have never given any details of such blanks is because these details would have given the "instrument function" of the calorimeters and would have shown that these devices work exactly as we had specified in various reports and two Handbooks. That being so, they would, of course, have had to interpret deviations from the "blank behavior" observed in the Pd/D_2O system.

The conclusion I draw is that our Japanese colleagues just did not want to observe the phenomenon of "Cold Fusion" although one could speculate at length why this may have been

the case. Needless to say, there are numerous people in Japan who have been quite open minded about C.F. and who have made only valuable contributions to the subject.

Where does all this place us with regard to the analysis of the Pd/D co-deposition experiment which Mel carried out in Japan? I will number the key issues as I see them.

(i) N.H.E. reported various values of the heat transfer coefficient. What is especially noteworthy is that the value quoted for the Pd-B experiment is vastly different from that quoted for the Pd/D codeposition experiment. These two experiments used the same cell and other instrumentation. Moreover, the latter value is actually less than that which one can calculate from the Stefan Boltzmann coefficient and the radiant surface area! All this passed without comment – expletives deleted.

(ii) By examining the general properties of these two experiments I managed to get reasonable consistency into the values of the derived heat transfer coefficients.

(iii) All of this is really rather by the way because one obtains an excess energy production with any of the quoted heat transfer coefficients, even the one which is less than the "Stefan-Boltzmann value"!

Other important points are

(iv) recombination of D_2 and O_2 cannot explain the excess enthalpy production.

(v) There is positive feedback.

(vi) There is a very significant episode of Heat after Death.

All of this is quite a mouthful and certainly more than sufficient for a paper on the topic.

This brings me to the content of your paper. It seems to me that you have "written out" all notions of conflict and (i), (ii), (iii) and (iv) have been excluded. An unkind way of putting this would be to say that the evaluation has been "Mormonized" (I say that because Mel is not a typical Mormon in that sense!)

(To comment further on this point, I believe that the time for being mealy-mouthed is well and truly over). Instead you have introduced statements about increases in excess enthalpy production with decreases in enthalpy input. I know that Mel is keen on pointing this out. It has been done before (including by myself in 1990). It has had absolutely no impact and I could predict that it will have no impact now. [122][Of course, I think that it is valuable to point this out (see your page 1 lines 22-28) but not at the expense of the other points.] Instead people will continue to tittle-tattle behind closed doors about (iv) coupled to the notion that one cannot guarantee that there will be no powered Pd in the system]. They do not really need anything more than that to "rubbish" (trash) the paper.

[122] MF Of course, I think that it is valuable to point this out (see your page 1 lines 22-28) but not at the expense of the other points.

I can see where you are heading in your text – you wish to close the loop to the "Hot spots" and, maybe, to introduce the notion of "Gorsky factors". However, these particular notions do not follow from the material presented in the paper and should, I believe, be presented separately.[123]

The discussion of the thermodynamic analysis of the system is valuable but again is somewhat out of place in this paper. It certainly breaks the flow by being placed in the middle of the text (your pages 3-6). Could this material be presented in an Appendix with a reference in the text to that Appendix e.g. to say that equation (6) follows for the particular case of calorimetry in the co-deposition experiment?

Now for some specific points:

a) *I am making a mental note to talk to you about calorimetry in the ??? in the context of your comment in 2 of p. 1.*

b) *I am also making a mental note to talk to you about the material and thermal balances in the cell (your (ii) on page 5). This introduces notions of where one should put the boundaries in the system.*

c) *Figs 1 and 7 are missing from the text. However, it is clear what Fig 1 would be and I can guess that Fig 7 would show.*

d) *Page 7 line 23 delete "the", line 26 delete "and"*

e) *Page 8 line 2 delete "excess." line 3 replace "It" by "We" line 4 delete "be"*

f) *Page 9 line 5 "indicates" line 16 add colon after "to quote"*

g) *Last paragraph. Chaotic behavior is actually observed in pretty well all the systems (especially for Pd-Rh alloys). See our paper to the Maui Meeting [ICCF-4].*

h) *Some of diagrams are rather confusing. They may be less confusing in their final form but I think that you and Pam should ask yourselves whether they are not rather overloaded with information?*

*Now for some trivial points: you ask whether Electrochimica Acta might be a suitable journal? My feeling about this is that it might be interesting to try to publish there but I do not believe that they will accept any paper dealing with this subject. I speak from bitter experience (very bitter experience) which I can tell you about when we meet next – it would require a letter longer than this present one to deal with this matter! ****

You also asked whether you could add my name as a co-author. I feel somewhat ambivalent about this. If you wish to do so, then by all means do, but I would have it in mind that we should write a further paper dealing with my points (i) – (iv). As it stands, my contribution is somewhat marginal and could be covered by an acknowledgment about the content of Fig 5. Of course, the production of the report of 29/01/2001 required some quite intense effort but, as I have already

[123] MF i.e. page 10, lines 5-29.

said it, I believe that my objections were rather different to yours. But then I was born with a machine gun in my hand.

Regards,

Martin

**** Mel told me that you had intended to submit this paper to Fusion Technology which, I believe, would be more straightforward choice.*

P.S. I will write to you again about one specific point when I get back to England.

2001-10-04

[From Stan Szpak to Martin Fleischmann]

San Diego, 4 Oct. 01

Professor Martin Fleischmann, F.R.S.
Bury Lodge, Duck Street, Tisbury, Salisbury, Wilts SP3 6LJ
United Kingdom

Dear Martin,

The 11 Sept. 01 was the day that was! I watched the morning news and saw the second plane plunging into the building. Instinctively, I thought of another September day, the 1 Sept. 39. Hopefully, this time it will be less destruction and misery. I take comfort in recalling that the attack on western civilization was stopped in the past, in XIII century, in XVII century and there is a little doubt that will be shopped in the XXI century.

Returning to your Fax of 18 Sept. 01, I understand your frustration with regard to complete honesty of the various people involved. We have a very similar case in our laboratory. In the early stage of our work we wanted to do a very careful calorimetry of ceils employing electrodes prepared by the co-deposition process. To do this, we engaged a very competent physicist. He devised an elaborate and very exact procedure, did the experimental work carefully but, when it came to the data analysis, he, guided by the establishment, used all sort of arguments that negated the excess heat generation. And, as others have done, the data were no longer available. As I understand, now in retirement, after the many years he is still analyzing the "lost" data to convince himself that there was no excess heat generated although the data indicate otherwise.

In the draft; of the paper "Thermal behavior...", we did not elaborate on the points that you have raised. This will become clearer as I give you a brief history. Shortly after we published Mel's analysis of the co-deposition experiment [Fusion Technology, 36, 234 (1999)] Mel sent us the raw data. When we plotted the E(t) and T(t) curves it appeared to us that the behavior of the co-deposited electrodes in many ways is no different than that of solid Pd rod. After receiving your communication of 29 Jan 01, we thought that, after extracting selected parts, we would have enough information to write a short paper showing the similarity (positive feedback, life after death) and the advantages (e.g. no waiting period, excess heat at low cell currents) of the co-deposition process. In this connection and to avoid complications, we thought that it was not essential to show how the correct heat transfer coefficient was obtained - rather to state that once it is determined for a given cell and operating procedure, it can be used for data evaluation.

As a first choice, we selected Electrochimica Acta. Its American editor Elton Cairns, a chemical engineer, might have a different attitude than his European counterpart. In any event, in our letter of submission we would request that, if the journal policy is not to publish topics involving cold fusion, he should return the submitted paper without comment. In our attempt to change the

attitude of editors of scientific journals, we (Pamela, Scott Chubb and I) prepared a brief note entitled "Cold fusion: a discovery or pathological science?" (copy attached) and sent it to Chemical and Engineering News". Result – unknown at this time. However, the editor, Ron Dagani, expressed interest in seeing it.

Incidentally, George Miley is stepping down as the editor of Fusion Technology and his successor may, or may not be interested in cold fusion. As I understand, Miley is now chairman of the board and scientific advisor for a newly formed corporation "Lattice Energy LLC", located in Chicago. Its CEO is Mr. Lewis Larsen who claims availability of sufficient funds. I do not know whether or not funding is available. May be a new era is emerging. Recently (11 July 01), Bockris in a memo addressed to Storms, McKubre and myself wrote: "All three of you have communicated with me recently and spoken about reproducible experiments. Surely, once we get the reproducible experiment, we don't need some man going around and collecting money for us. I suspect strongly that in Houston, alone there is $10M ready to be put into anything which has the potential of chemically activated nuclear reactions so long as they can be demonstrated on demand".

Pam and I feel that the points (i)-(iii) that you have raised in FAX dated 18 Sept. 01, should be addressed in a separate publication, using as a background information contained in the shortly forthcomming Technical Report (TR). What we have in mind is to start with the definition of the various forms of the heat transfer coefficient and their use in data interpretation. Then, using selected sets of data (experiment Mc-21, the Pd/B electrode and the co-deposited electrode) we can clearly show the effect of an inappropriate use of the heat transfer coefficient. We will send you an outline for such a paper shortly. Currently, we are revising the draft of "Thermal behavior..." to be more in line with your comments.

Regards and all the best

2001-10-08

Melvin H. Miles, Ph.D.
Department of Chemistry
Middle Tennessee State University (MTSU) P.O. Box 68
Murfreesboro, TN 37132 USA

FAX MEMO

DATE: October 8, 2001

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I hope everything is going fine with you. Stan Szpak called me a few weeks ago and said he had received a fax from you regarding a joint paper. The San Diego Navy Report should soon be available since it is now at the printers.

I received a message from China Lake a few days ago that the "Eagle Project" would not receive any ILIR (internal) funding. I would certainly return to China Lake if I could work on that program, however, ~~Dr.~~ [124] Robin Nissan is up to his usual self and took the funding away for some other purpose before we had a chance to test anything. This fall he has refused to support this program. Thus, I am not very excited about returning to China Lake. I would rather stay here if I could get the permanent position. I could then probably work with George Miley on his program. I could also get some students involved in some cold fusion research. I am already planning an experiment for my physical chemistry class that would investigate the boiling points of $LiOH-H_2O$ solution. I would then write a short paper involving the students names as well for ICCF-9. Can you give me any suggestions on what concentrations we should look at? I plan on looking at both dilute solutions and solutions of high concentration. Next semester, I will do similar studies using $LiOD-D_2O$. Please let me know what you think.

I would like to submit the Pd-B paper soon to The Electrochemical Society. Please let me know if you have any suggested changes.

Best wishes,

Mel Miles

Mel

[124] JR As noted above, Miles crossed out "Dr." because Nissan does not have a Ph.D and because he was ". . . a typical fat government politician rather than a true scientist."

2001-10-26

Bury Lodge heading

26th October 2001.

Dr. Melvin R. Miles,
Department of Chemistry,
Middle Tennessee University (MT5U),
Box 367,
Murfreesboro, TN 37132,
U.S.A.

Dear Mel,

You will recall that you wrote to me last month! I had intended to reply immediately but, unfortunately, my correspondence has again been delayed because of much urgent business. This pressing business included a visit to Italy and will continue until the end of the month. At that time I will take up again the final steps of the analysis of the data sets of the Pd-B-Ce experiment which you carried out in Japan. More about this below!

We too, over here, have been greatly preoccupied with the tragic events in New York and Washington and, more recently, with the threat of biological weapons. This latter aspect goes back a long time and is a matter which I will tell you about when we next meet. The question is: what are we to do in this situation? At times it seems to me that we are in a "no win" position.

The first and most important point is the text of the paper which you intend to submit to the Journal of Electrochemistry (sic - do you mean the Journal of the Electrochemical Society? Or has it changed its name?). I thought this text was absolutely splendid and I have 0 comments - I think that it makes a well-rounded study. I would anticipate that you may get some challenges on the question of the general applicability of the Q.E.D. paradigm but we can deal with those if and when they arise (or we can change the text?). The question of the interpretation of "Heat-after-Death" (reaction with oxygen - see the comments you had at the meeting in San Francisco) may also arise again. It so happens that the magnitude of the effects in your experiments makes it difficult to exclude the possible participation of this phenomenon in the interpretation of Fig 4 of your paper. From this point of view Fig A 26) of the report NRL/MR/6320-01-8526 is more diagnostic: the cell is still filled with D_2O and the current density after the reduction in cell current is still above the limiting current density for the reduction of oxygen i.e. oxygen cannot get at the deuterium in the electrode. The paper by S. Pons and M. Fleischmann in Trans. Fusion Technol., 26 (1994) 87 (or the equivalent paper in the Conference Proceedings of the 4th International Conference held in Maui ie Paper C.1.1) is much more diagnostic: the total enthalpy is far above that which can be attributed to the oxidation of deuterium in the lattice.

I was relieved to see that you plan to attend ICCF 9 in China next year, especially as I am now 99% certain that I will not go. [125] The major reason is that my present state of health would make it very difficult for me to tackle a long journey coupled with the uncertainties of the accommodation and pressures add to the Conference etc. which there would undoubtedly be (based on my previous experience, our Chinese colleagues would expect me to fulfill a rather long itinerary!). All of this therefore brings to mind the paper which you might wish to present? The co-deposition experiment certainly requires two further airings, one as a Navy report and one as a paper in the literature (more about this below). This matter is somewhat related to the analysis of the Pd-Ce-B data and the paper which Stan Szpak wishes to publish - inevitably the issues are rather complicated!

You will have gathered from the first paragraph of this letter that the analysis of the Pd-Ce-B data set is again "on hold". However, you should have a report on my activities by Christmas of this year!

Next, I think that it is appropriate to make some comments on the paper which Stan Szpak wishes to submit for publication. He actually sent this to me on 3/05/01 but for a multitude of reasons I only sent him my comments on 18/09/01 (while I was in Frascati). Did Stan and Pam send you a copy of the draft and do you have any comments on this version? Of course, this paper is perfectly fine viewed from the perspectives of a group of people who wish to continue to work in the field and who may wish to maintain "good relations" with the Japanese research group(s) (except that I do not believe that the last three paragraphs of the Discussion follow from the material presented in the paper; furthermore, the Section 2.1 "Thermodynamics of Electrochemical Cell Calorimetry" breaks the flow of the presentation. I suggested that this could be placed in an Appendix). however, I crossed that particular Rubicon quite some time ago and I believe that the analysis which I, carried out on the codeposition experiment summarised in part in my letter of 29/01/01 to you, Stan and Pam) coupled to your own experiences demonstrates that the interpretations provided by our Japanese colleagues are simply incorrect. I believe also that one has to be on one's guard with respect to the exact reasons why these colleagues may have wished (and continue to wish) to provide such incorrect interpretations. (You will have realised from our various conversations and the comments I made at the meeting at SPAWAR that I have always "looked over my shoulder" to try to see whether other people's research may not have a bearing on National security or, indeed, whether it may not have been carried out precisely in such a context!)

In my letter to Stan and Pamela I have made the following points:

(i) N,H.E. have reported various values of the heat transfer coefficient. What is especially noteworthy is that the value quoted for the Pd-B experiment is vastly different from that quoted for the Pd/D codeposition experiment although these two experiments used the same cell and other instrumentation. Moreover, the latter value is actually less than that which one can calculate from the Stefan-Boltzmann coefficient and the radiant surface area, a matter which passed without comment!

[125] MM Fleischmann did attend this conference.

(ii) By examining the general properties of these two experiments one can get reasonable consistency into the values of the derived heat transfer coefficients (contrast (i)).

(iii) (i) and (ii) are really rather by the way because one obtains an excess energy production with any of the quoted heat transfer coefficients, even the one which is less than the "Stefan-Boltzmann value"!

Other important points are:

(iv) Recombination of D_2 and O_2 cannot explain the excess enthalpy production.

(v) there is positive feedback.

(vi) there is a very significant episode of "Heat-after-Death".

As I said to Stan and Pamela, this is quite a mouthful and certainly more than sufficient for a paper on the topic. I noted that Stan and Pamela introduced statements about increases in excess enthalpy production with decreases in enthalpy input - and I pointed out that you are quite keen on this observation. Furthermore, I pointed out that this has been observed before (including by myself in 1990) but that this had had no impact. Of course, it is valuable to reiterate this aspect but not at the expense of other observations. As I said to Stan and Pamela, people continue to tittle-tattle behind closed doors about (iv) (coupled to the notion that one cannot guarantee that there will be no powdered Pd in the system). It seems to me that they don't really need anything more than that to "rubbish" (trash) Stan's and Pamela's work.

The question therefore is: should we do anything further about the content of my letter of 29/1/01? (See also the last paragraph). It may well be that Stan and Pamela have gone ahead with the submission of a paper based on the text of 30/4/01 in which case I think that it would be desirable to produce a further paper based on the points (i) - (vi) above, some results for the Pd-B experiment and, furthermore, relevant results for the Pd-B-Ce experiment which you carried out in Japan. As I have already said, you should have a report on this by Christmas; the key result is that you were given the wrong leads for connecting the cell to the switching box. As a result there was power dissipation external to the cell and both the sets of (k_R') and C_pM are high; furthermore, the precision and accuracy of the measurements is destroyed. However, if one is aware that there was this error in connecting the cell to the switching box, then one can get reasonably accurate values of (k_R') C_pM and Q_{excess}). This is the final step with which I am still much concerned (especially the evaluation of Q_{excess}). You will see that these evaluations also link back to the content of part of the paper we presented at ICCF 8.

The production of such a paper will be difficult and, at the very least, we should present the first version at ICCF 9. No doubt, once this written text of a paper for the "normal" literature and/or for a Navy Report will become reasonably clear. So shall we make a start on this paper using (i) – (vi) as the key elements? I believe that we should stop well short of accusing N.H.E. of misdeeds although it should be apparent that this is what we believe. As I pointed out to Stan and Pamela, the time for being mealy-mouthed is well and truly over.

When I wrote to Stan and Pamela from Frascati, I told them that I would write further about one specific point when I got back to England, which I have not yet done. This matter concerns the question of whether we should do anything further about the letter I wrote to you, Stan and Pamela on 29/1/01. Of course, that letter brings in its train the evaluations of the Pd-B and Pd-B-Ce experiment which you carried out in Japan. You will have realised from our various conversations and the meeting in San Diego that I have been throughout "looking over my shoulder" to try to establish the motivation(s) for research in this field by other research groups. I think that it also became pretty clear in San Diego that some of the participants shared some of my concerns (especially with regard to research in Japan?). I believe therefore that it might be sensible for you and/or Stan to send a copy of my letter of 29/1/01 to J.C. and P.A. together with the offer to discuss the contents at length with them. Thera are also a number of further matters which I should tell J.C. and P.A. about. [126] It would only take about 5 minutes but I am reluctant to put pen to paper. It may well be though that the system is already well-informed about activities worldwide?

How are matters shaping up in Tennessee? Does the notion of an Academic Career attract you or is this beginning to pall?

All the very best to you and Linda.

Martin

[126] MM JC = Jim Corey and P.A. = Pamela Anderson

2001-10-27

Bury Lodge heading

27/10/01

Dr. Melvin R. Miles,
Department of Chemistry,
Middle Tennessee University (MT5U),
Box 367,
Murfreesboro, TN 37132,
U.S.A.

Dear Mel,

You must have been somewhat puzzled by the letter I sent you yesterday. When I sent this letter I realised that there was something wrong with my fax and, in due course, I found that the memory was jammed up with letters including yours of 8/10/01 and one from Stan Szpak of 4/10/01.

I find your news depressing, but at the same time, it was not unexpected. I think that there is a powerful group of people who simply want to stop the research on C.F. As you know, I believe that one should only invoke "Conspiracy Theories" as a last resort. However, it also seems to me that the same group are hell-bent on frustrating all research which demonstrates that Q.E.D. it is the necessary paradigm for condensed matter systems. Tough luck: our recent research in Italy deeply confirms this with simple experiments. It will be interesting to see how any such group will cope with that one.

I was interested to see your comments on the elevation of the boiling points for $LiOH/H_2O$ and $LiOD/D_2O$ solutions. Exact measurements of elevativities of the boiling point are quite difficult. One missing piece of information is the solubility of LiOH (or LiOD) at the respective boiling point of the solutions. This should be accessible by careful measurements of boiling points and appropriate sampling and titrations?

Regards, Martin

2001-10-29

NAWC heading

October 29, 2000

Professor Martin Fleischmann
Bury Lodge, Duck Street
Tisbury, Wilshire, SP3615
Great Britain

Note Cu Rod cathode was 4 mm × 25 mm

Dear Martin,

Enclosed is the data set for the co deposition experiment in Cell A2 that was started immediately following the Pd-B experiment. Since this is the same cell, I think we can assume the same radiative heat transfer co-efficient (0.85065×10^{-9} WK^{-4}) and the same water equivalent for the cell (450 JK^{-1}). The D_2O solution contained $PdCl_2$, ND_4Cl, and ND_4OD. I will copy pages from my notebook that give complete details. The $PdCl_2$ deposits as Pd onto the copper cathode. There were periods of chlorine evolution, but this would minimize the excess heat if we use the thermoneutral potential for D_2O electrolysis only. Even the NHE calculations show excess heat for this experiment. I am enclosing a copy of their results in figure form plus my own figures showing the cell temperature vs. time and the cell voltage vs. time. This includes the three heating pulses, but only the second pulse shows anything close to normal. The cell voltage is somewhat unstable since the palladium deposit comes loose causing changes in the electrode area. Therefore, it will probably be a challenge for you to analyze this data. Nevertheless, I am sure Stan Szpak will be very pleased if you can do this, and would probably publish this in another Navy report as well as in some journal. These publications will include your name, Stan Szpak, and myself.

Regarding the Pd-Ce and the Pd-Ce-B data from my experiments at NHE, Mike Melich is supposed to send me this data on a CD. I will also try to get this data to you in printed form sometime in the near future.

Thanks for your fax from Italy. I hope your health is holding up with all this travel. I would like to do the experiment you suggested, but it will probably have to wait until the two Navy reports are completed. The NRL report is nearing completion, and I will be sending you a copy for any last minute corrections.

Best wishes,

Mel

Mel Miles

2001-12-12

Melvin H. Miles, Ph.D.
Department of Chemistry
Middle Tennessee State University (MTSU) P.O. Box 68
Murfreesboro, TN 37132 USA

FAX MEMO

DATE: December 12, 2001

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

MESSAGE:

Dear Martin,

I received your faxes of October 26 and October 27 but preparing lectures and grading papers is more time-demanding than I would like. In that sense, I am tiring somewhat of teaching.

Linda and I are flying back to China Lake on Saturday, December 15 where I will work for the Navy until January 4, 2002. We will then return to Tennessee where I will teach for the next semester.

My physical chemistry class tackled the boiling point elevation of $LiOH/H_2O$. I will fax you the resulting abstract that I have submitted for the March 2002 meeting of the American Physical Society as well as for ICCF-9 next May in China. Do you have a paper ready to submit for ICCF-9? I will be happy to present this paper if you cannot attend. If the paper is on the Pd-Ce-B data, Dr. Imam of NRL would like to be included as an author so that he can obtain funding to attend this meeting. He could also get this data published as another NRL report. Another possible paper would be the co-deposition experiments that I conducted in Japan. The stated deadline for submitting a paper for ICCF-9 is December 31, 2001. Information is available at the website: http://iccf9.global.tsinghua.edu.cn

I submitted the Pd-B paper to The Journal of The Electrochemical Society. That has been the correct name for as long as I have been a member. I mailed you a copy of my letter and submitted paper. I have not yet received any response. If they reject the manuscript outright, then I will resign my membership in this society.

[MM Both happened – the manuscript was rejected and I resigned.]

Jim Corey of Sandia visited me here on November 14, 2001. He has promoted your ideas to the CIA and this was ranked for possible funding. The ranking was too low for any funding soon. Perhaps it is already a black program, thus they won't fund it. Jim Corey would like to meet with you directly sometime in England to discuss this topic. Incidentally, I am not sure who you mean by J.C. and P.A. in your fax of October 26, 2001. Perhaps J.C. is Jim Corey, but who is P.A.?

You can fax me in California, 760-939-1617 during December 17 and January 3.

Merry Christmas and Best Wishes for 2002.

Mel and Linda

Mel Miles

Experimental data for LiOH/H_2O system

Molality (moles/kg H_2O)	*0.508*	*1.0*	*2.0*	*2.998*	*7.31 (Saturated)*
$\Delta T_{Exp.}$	*0.55 K*	*1.2 K*	*1.5 K*	*2.0 K*	*6.0 K*

Distilled Water, bp = 100.0°C @743 Torr.

Question: For LiOH yielding Li^+ plus OH^-, perhaps OH^- is too similar to H_2O to yield full effect on boiling point. Thus, experimental ΔT is somewhat low. In contrast, NaCl gives larger ΔT than expected.

[JR An abstract and a short paper were attached to this message. Here is the abstract:]

Miles, M., et al. *The Elevation of Boiling Points in H2O and D2O Electrolytes*. in *The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science*. 2002. Beijing, China: Tsinghua University.: Tsinghua Univ. Press. http://lenr-canr.org/acrobat/MilesMtheelevati.pdf

[JR Here is the paper, which has not been published elsewhere as far as I know:]

CRITICAL ANALYSIS OF COLD FUSION CALORIMETRIC DATA REPORTED BY CALTECH SCIENTISTS

Melvin H. Miles
Chemistry Division, Research Department
Naval Air Warfare Center Weapons Division
China Lake, CA 93555

Most scientists have dismissed cold fusion as a pathological science or as another polywater episode (1), yet the third international conference on this subject will take place in Japan later this year. Furthermore, considerable experimental evidence has quietly accumulated that supports the occurrence of nuclear reactions in a metal lattice near room temperature (2).

The presentations and publications by N. Lewis et al. (3,4) were obviously major factors in turning the scientific and public opinion against the concept of electrochemically induced cold fusion (5). Ignoring any debate about cold fusion, there are apparently major flaws in the calorimetric experiments reported by N. Lewis et al. (3,4) that have been brought to my attention by V. Noninski and reported in detail elsewhere (6). These apparent errors need to be presented to the scientific community and considered in any cold fusion discussions.

The fundamental error in the calorimetric experiments of N. Lewis et al. (3,4) was the variation of both the electrolysis and resistor power while maintaining a constant total power. This error can readily be seen by the following simple algebraic notation:

$$P_T = P_{EI} + P_X \qquad \text{(when } P_R = 0\text{)} \qquad [1]$$

$$P'_T = P'_{EI} + P'_R + P'_X \quad \text{(when } P_R \neq 0\text{)} \qquad [2]$$

where P_T is the total power, P_{EI} is the electrolysis power, P_R is the resistor power, and P_X is the excess power (if any). Thus

$$\Delta P_T = P_T - P'_T = P_{EI} - P'_{EI} - P'_R + P_X - P'_X \qquad [3]$$

If the cell temperature is kept constant, $\Delta P_T = 0$, hence

$$P_X - P'_X = P'_{EI} + P'_R - P_{EI} \qquad [4]$$

The experimental observation by N. Lewis et al. that $P'_{EI} + P'_R - P_{EI} \approx 0$ can only prove that $P_X - P'_X = \Delta P_X \approx 0$, i.e., the excess power (if any) did not change significantly with the change in P_{EI} (current density). It definitely does not follow that P_X or P'_X is zero. Contrary to the claims of the authors (3,4), a study of this nature is completely incapable of proving that no anomalous power was produced.

The increasing heating coefficients (h.c.) reported by N. Lewis et al. (4) from 14.0 to 15.9°C/W (Table 3) suggest an excess enthalpy effect of over 13% in D_2O/LiOD. The heating coefficient can be expressed as h.c.

$$\text{h.c.} = \frac{\Delta T_J + \Delta T_X}{P_T} \qquad [5]$$

where ΔT_J is the Joule heat contribution to the cell temperature change produced by $P_{EI} + P_R$ and ΔT_X is the cell temperature change due to any excess power (P_X). In H_2O or early stages of D_2O experiments, $P_X = 0$, thus $\Delta T_X = 0$ and the true heating coefficient is obtained. If excess power is present, $\Delta T_X > 0$ hence a larger heating coefficient is obtained. This error is due to the neglect of the unknown amount of P_X and using only $P_{EI} + P_R$ to determine the total power. Recalibrations with the load-resistor method used by Lewis et al. (3,4) would not be valid when an unknown amount of anomalous power is present. If heating coefficients in H_2O/LiOH are used (Table 3 of Reference 4) where no anomalous power is expected, then an even larger excess enthalpy is suggested for the D_2O/LiOD studies. The largest amount of excess power suggested by Table 3 is 1.1 W/cm^3 at 140 mA/cm^2. This is in excellent agreement with results reported by M. Fleischmann et al. (7).

A closer examination of Table 2 of Science (3) and Table 3 of *Nature* (4) shows that the sum of $P_{EI} + P_R$ required to maintain a constant cell temperature in D_2O/LiOD is always greater for the experiment at the lower current density. Although this effect is small, it is consistent with the presence of an anomalous power that increases with current density as reported by M. Fleischmann et al. (7).

Possible calorimetric errors due to D_2-O_2 recombination or due to the rate and/or form of gas evolution proposed by N. Lewis et al. (3,4) are simply not valid. Many recent studies (2) have shown that D_2-O_2 recombination is insignificant for flooded Pt and Pd electrodes as used in the Lewis experiments. Recent studies in our laboratory show essentially the same cell temperature versus applied heating power relationship for electrolysis power as found for resistor power where no gas evolution occurs. Gas evolution, therefore, does not significantly affect the heat transfer coefficients in electrochemical calorimetric cells as suggested by N. Lewis (3,4).

Finally, the 1 ppm detection limit for helium measurements in the effluent gases reported by N. Lewis et al. (4) is far too insensitive to measure the ^{4}He yield from the $^2H + {}^2H \rightarrow {}^4He + 23.8$ MeV fusion reaction. Assuming an excess power of 1 W/cm^3 (Pd volume was 0.31 cm^3) would yield only 0.043 ppm of ^{4}He in the effluent gas for the N. Lewis study at 64 mA/cm^2. The detection of ^{4}He in the effluent gases from cells producing excess power has been reported for cold fusion studies using more sensitive methods (8).

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2002-01-04

Bury Lodge heading

4th January 2002.

Dr. Melvin H. Miles,
Department of Chemistry,
Middle Tennessee State University (MTSU),
Murfreesboro, TN 87132,
U.S.A.

Dear Mel (and Linda),

First of all, very best wishes for the New Year and welcome back to Murfreesboro!

My correspondence with you has been in abeyance (though not forgotten) and today I have been trying to "clear the decks" before heading off to Austria for some skiing. I shall be staying from 5th-19th January at the

Hotel-zum-Hirschen,
A-5700, Zell-am-See,
Austria.

I haven't got the FAX or 'phone numbers but will 'phone these through to Sheila as soon as I arrive in Zell-am-See.

You will recall that you wrote to me several times towards the end of last year. I put all these letters aside intending to write to you comprehensively before Christmas. You will be able to guess at what happened: I now cannot find this collection of letters. However, as I recall I thought "fine" about all the points which you raised specifically your publication plans and proposals to include Imam in the various Abstracts. I now have the text of the paper which you sent to the ECS and will take this with me to Austria. I will write to you from there if I have any comments (which is unlikely).

Having said "fine" I must, of course, exclude your own situation with regard to the work on C.F. - a tragedy in several acts. I think that you are quite right in your assumptions about the continuation of the work "under wraps". I have throughout thought this was highly likely (ever since March 1989) and I must give you an update sometime of the tape we recorded in Lerice - it would be more correct to describe this as Part II of the tape. I think that there is really nothing which we can do about this situation.

As I recall you questioned me about J.C. and P.A. in one of your letters. Yes, JC = Jim Corey and P.A. = Pamela Anderson (although I may have made a Freudian mistake in recalling her name!)

One problem with the missing correspondence is that I now cannot find the papers dealing with the elevation of the boiling point of D_2O - LiOD solutions. Could you therefore please send me duplicates?

Now for the real reason for the delay in my correspondence. In October of last year I found that I still had to do ~ 4 weeks work before I could write to you about my analyses of the Pd-Ce-B experiment, FP 2-97120401-M7cl. I was therefore well on course with my plans to send you a Report by Christmas 2001. Unfortunately, I then had to put all of this aside to deal with a number of urgent matters so the situation today is still the same: I need to do ~ 4 weeks further work. I shall be taking the relevant material with me to try to "break the back" of the various problems working in the mornings and evenings. I therefore hope to write to you extensively at the end of this month. Ahead of that time you will wish to know that I have been able to show that our Japanese colleagues wired up this experiment incorrectly. It doesn't really matter because one can compensate for this mistake as long as one knows that it has been made - they will be very annoyed to see the failure of their deception (unless this was really just incompetence).

Work in Italy continues in its usual confusing way but this year I have initiated the construction of the rigs to investigate Szpak-Mosier Boss type systems in fluidised bed electrodes. This is the ninth time of trying to push this through. Wish us luck, it really could prove to be the first step in implementing larger scale devices.

More later this month and

All the best, Yours,

Martin

P.S. I have today also written extensively to Wilf Hansen to give him a list of data sets which I have here and which we could analyse further. You may wish/need to discuss with him the options for further work.

P.P.S. I believe that I did tell you that I would not be going to China. My state of health simply does not allow me to make such long journeys at this time. The U.S. is a different matter because I can recover my equilibrium once there!

2002-04-04

Bury Lodge heading

4th April 2001.

Dr. M. H. Miles,
Department of Chemistry,
Middle Tennessee State University (MTSU),
Murfreesboro,
TN 37132, U.S.A.

Mel: Copy of the fax which we had to send to you from Wood + Co!!

Dear Mel,

I need to write to you at length about your various tribulations which I was truly sorry to learn about. However, they were certainly familiar to me and it seems that all my various efforts have come to nought.

As I have said, I need to write to you at length but I will not be able to do this until after my return from Italy on 13th April (I shall shortly have to leave to go to Padua/Rome). However, ahead of my next letter, I thought I should write to tell you that I have finished my first analysis of your experiment FP2-97120401-M7cl. I say that I have finished although this is by way of the question; "how long is a piece of string?" I am sending you the present version of a Report and you will see that the analysis is incomplete in two important respects (see page 18 of the Report). I am taking the relevant material with me to Italy (that dealing with allowance for a 0.5 Ohm resistance in the current leads) and I believe that I have also located the spreadsheets which relate to the Report I sent to you on 15/09/98. I hope to be able to plug these two gaps by mid-April.

You will also see that the six Tables and the Figures are hand-written. It will be possible to produce proper versions of the Figures once the Tables are set up but Figs 18-23 will require further Tabular material which I could send to you. It seems to me that the material in this Report should somehow see the light of day. An ideal solution would be to change the text where necessary and produce an addendum to NRL/MR/6320-01-8526. Would this be possible? I think that speed is of the essence and I wonder therefore whether you (and Linda?) could produce the Tables and proper Figures.?

You will see that the Report contains two sets of Footnotes, those labelled A which are for public consumption and those labelled B which may have to be restricted. The Footnote B 18 is an especially "hot potato" and I will be very interested to see how you react to this. I, personally, feel that any alternative explanation is quite untenable.

There is one urgent and important matter which I hope you can deal with ahead of my next letter. It seems to me from what you have said in your recent letters that you may not have received all the letters I wrote to you last year and especially those I have written to you since January. Could you therefore please list the dates of my letters; it would be helpful also if you

could list the dates of your letters. If you reply by FAX, you may find that my FAX is non-operational, a problem which developed after I sent FAXes to Li and Tian. Well, well, well - its not the first time! If that is so, you may need to use the FAX of Wood and Co, 0044-1747-871241. There is one further important matter: we should shortly be able to start work with fluidised bed electrodes (my 9th attempt) and, as far as I can tell, we will need to use 500 ml of solution. I would therefore welcome having your comments on the composition of the solution which we should use (or compositions if there are several solutions). Also comments on the schedule of experiments also any comments which Stan and Pam may wish to make about these experiments. We are planning to use ballotini glass beads coated with copper but, clearly, it would be much easier to produce silver coated ballotini beads. Do you/Stan/Pam think that such silver substrates would be satisfactory?

I went to my doctor yesterday for a further check up. The medication which I have been taking has had a pretty striking effect so, as of now, I am planning to go to China. If I go, then I will give a pretty general lecture which will deal mainly with water. I think that you should therefore decide on what you would like to cover and let me know just in case I should prepare the way. One option would be for you to recycle some of the material in NRL/MR/6320-01-8526 seen in the context of the Report I am now sending - especially the two figures which I will send to you later this month!

Yours neverendingly,

Martin

2002-04-22

Bury Lodge heading

22nd April 2002.

Dr. M. H. Miles,
Department of Chemistry,
Middle Tennessee State University (MTSU),
Murfreesboro,
TN 37132, U.S.A.

Dear Mel,

It was good to talk to you! I'm glad that the report on experiment FP2-97120401-M7cl has finally arrived and I trust that it is reasonably comprehensible. I was beginning to have dark thoughts about my inability to communicate with you! However, the delays in the Post are really a little extraordinary as is the behaviour of my FAX - I will tell you about this in due course. You will see that I am sending you this PAX from Wood & Co, FAX No. 0044-1747-871241.

I have now rewritten Section 5 of the Report which I am sending attached to this FAX together with the additional Figures now numbered as 22 and 26 as well as amendments to Table 4. You will see that the changes run on from the bottom of the old page 17. Of course, this rewrite means that many of the old Figure numbers have had to be changed and we have also used the need to make these changes as an opportunity to make some corrections to the remainder of the text. I shall therefore be sending you this updated version later this week. You will see that Fig 25 is to follow if and when I decide to do the additional calculations. However, ahead of that time, you may well decide that you may wish to use the existing Fig 26 so I am attaching a tabulation which will allow you to produce a suitable Figure.

I believe that all of the other Figures can be produced from the Tables with the exception of the old Fig 24, now Fig 27 of the new text. I think that this Figure is very difficult to discuss but we will definitely need it if we should decide to produce an Official Report. I am therefore also sending you the necessary tabulations required for this Figure. I think that all the other Figures can be produced from the Tables which I have already sent to you - please let me know if this should not be the case.

The meetings in Italy were quite phenomenal. Our Italian colleagues have now completely nailed-down the Coehn-Aharanov effect which they are proposing to call the Preparata effect. As I always said: this was an entirely new idea and this work had to go ahead even though it was not clear how a scaled up effect could be engineered. I did not have the heart to tell them that it was also not clear how the "Preparata Effect" would be related to all the other measurements. That involves some further novel thinking and I am beginning to feel my age!

I was actually rather unwell in Italy and had more or less decided that I would have to "chicken out" of the proposed trip to China. This week I have been waiting to see whether I would recover some sort of equilibrium which, thankfully, now seems to be the case. I will decide today whether I will be able to go - Li and Tian must be absolutely fed up with me. As Sheila says: I give the impression of a fit person young for my years but, unfortunately, the reality is far removed from the impression!

The success of the work in Italy means that that particular group of colleagues is beginning to think of the wider implications of "Coherence" for research in the Natural Sciences. I had always thought that this stage would not be reached within my lifetime! However, this view of the subject is rather "back-to-front" as far as I am concerned because I reached the decision to investigate "Cold Fusion" via some much more general problems in the Natural Sciences. This is one of the major reasons why I may decide to go to Beijing as this would give me the opportunity to start on "closing the loop".

More anon and regards,

Martin

P.S. I have held this letter back because I thought I might be able to include Fig 25 but I have still not completed the calculations. I will send you Fig 25 next week + a revised table 4 + a further revision of section 5.

P.P.S. some back of the envelope calculation suggest that our friends have reached 1 – 10 MWcm^3. It does not make us very happy - as at least it will actually complicate our lives.

[JR Missing pages]

. . . result is therefore no peculiar artefact of the methods of data evaluation. We also note that all the heat transfer coefficients are anomalously large which is true of both the "lower bound" and the "true" values. Furthermore, all the values of the water equivalent of the cell are anomalously large. This is a further reason prompting the conclusion that the execution of the experiment was subject to an artefact. In the design of the ICARUS systems it was considered that such artefacts would be revealed most readily from a consideration of the "lower bound" heat transfer coefficients, a consideration which should always be interposed between the preliminary examination of the data, section 3, and the accurate assessment as outlined in this section. As has already been pointed out, this detailed examination of the $(k_R')_{11}$ -spreadsheets is outlined in the next section.

Section 5 Consideration of the $(k_R')_{11}$ - spreadsheets

It has been pointed out in the previous section that the $(k_R')_{11}$-spreadsheets provide an effective means of monitoring the performance of the instrumentation. The reason is that the values of $(k_R')_{11}$ are determined by the local behaviour. Although the heat transfer coefficients derived by forward or backward integration are much more precise (or accurate) than the $(k_R')_{11}$ and $(k_R')_{12}$ values, they inevitably average out all localised changes over the evaluation interval. It is therefore better to investigate localised changes by considering the $(k_R')_{11}$-spreadsheets.

The importance of the experiment FP2-97120401-M7cl lies in the fact that the measurements on Days 3 - 10 were all carried out at the same, low, current density. One would therefore expect that excess enthalpy generation (if any) would be at a low level and that one could carry out averaging procedure over the 8 successive measurement cycles. This is the modified version of $\left(\overline{\overline{k_R'}}\right)_{11}$ produced from $\left(\overline{k_R'}\right)_{11}$ which itself is the usual 11-point mean of $(k_R')_{11}$ which has been used in the experimentation with the ICARUS – systems. It follows that "noise" in $\left(\overline{\overline{k_R'}}\right)_{11}$ $(k_R')_{11}$ will be reduced by a factor of ~ 9 by using these modified $(k_R')_{11}$ values. These values should therefore reveal the "instrument function" and the adequacy (or otherwise) of the modelling of the calorimeters (see Footnote A.12.)

The evaluation of $(k_R')_{11}$ requires valid estimates of the water equivalents of the cell and we therefore obtain a set of spreadsheets corresponding to the assumptions made. Table 4 contains a summary of these spreadsheets. We can see that the use of the "guesstimate" $C_pM = 450JK^{-1}$ shows that the derived values of $\left(\overline{\overline{k_R'}}\right)_{11}$ are markedly perturbed by the application of the calibration pulse, see Fig. 18. The magnitude of these perturbations is reduced by using the value $C_pM = 490JK^{-1}$ (column 3 of Table 4 and Fig. 19) which is closer to the mean value of the derived water equivalents of the cell (see Footnote A.13). Use of the actual values of C_pM derived for each respective measurement cycle gives the values listed in column 4 of Table 4 and plotted in Fig 20. The use of the further refinement of adjusting the mid-point values of C_pM to allow for the effects of electrolysis, column 5 of Table 4 and Fig 21, does not lead to any further marked change as compared to the use of ICARUS - 1 values alone, Fig. 20 (see Footnote A.14).

The most striking feature of the results shown in Figs. 19 - 21 and in Table 4 is that the imposition of the calibration pulse leads to a discontinuity in the derived values of $(k_R')_{11}$. It is evident that the powers delivered to cell and heater are not consistent. We note here that the possibility of a maladjustment of the powers delivered by the two galvanostats used in ICARUS - 1, Fig. 3, would have been avoided by using the wiring scheme developed for ICARUS - 2, Fig. 4. However, the actual scheme used in ICARUS - 2, Fig. 6 reintroduced the possibility of the effects of a mismatch in the power levels delivered to the cell and heater. Fig. 22 illustrates the effects of an increase of 1% in the power levels delivered to the calibration heater (i.e. 0.25250 instead of 0.25000W see also column 8 of Table 4). We can see that such an increase removes the discontinuities seen in Figs. 18 – 21. We also note that this increase would bring the "lower bound" and "true heat transfer coefficients", Table 3, into much closer accord. At the same time, we cannot see any clear cut reason why the power levels delivered to the calibration heater should have been so far in error. We therefore believe that an error must have been made in the wiring of the cell to the ICARUS - 2 system. The most obvious error is that one (or more) of the leads used for the ICARUS - 1 system, Fig. 3, was (or were) also used for the ICARUS - 2 version, Fig. 6 (compare (2)). The median value of these lead resistances was 0.5 Ohms and Figs. 23 and 24 show the effects of including respectively 1 and 0.5 Ohms in the external wiring for the measurement of the cell potentials (see also columns 6 and 7 of Table 4) i.e. either 2 or 1 incorrect leads. The effects of the calibration power on the derived values of the heat transfer coefficient now have the opposite sign to those seen in Figs. 18 - 21. Evidently, external resistances of 0.5 or 1 ohm are too high and Fig. 25 and column 9 of Table 4 show the effects of reducing this resistance to 0.25 Ohm (such a low resistance implies the connection of the cell to the switching box using either a lead supplied with the ICARUS - 1 system but which had subsequently been shortened out or, else, a short replacement for one of the leads originally supplied).

Mel: I fear that we have a further situation best characterised by the question "how long is a piece of string?" It seems to me that we should aim for a further publication a major part of which would be a comparison of Fig. 26 with Fig. 20 or, better, a revised Fig. 26 (see below) with Fig. 21 together with comments about Fig. 22. What do you think about this? The comments that I made in version II of Section 5 remain valid but we now know that there was a mistake in the software (at present I can at best make a guess about the nature of this mistake). We therefore have to recalculate these earlier results. However, most of Fig. 26 is valid and I shall therefore send you a tabulation which will allow you to plot a proper Figure.

Fig. 26 shows the plot of $(k_R')_{11}$ versus time determined for a genuine "blank" experiment (Pt cathode polarised in 0.1M LiOD in D_2O) using an ICARUS – 2 calorimeter, an ICARUS - 2 system and the experimental protocol specified for these systems, (measurement cycles of 48 hour duration). The conditions used in this experiment are otherwise comparable to those used for the derivation of the data in Figs 18 - 21. Comparison of these Figures with Fig. 26 immediately shows a number of important differences. In the first place we can see that there are no discontinuities which could be attributed to the application of the calibration pulses. This confirms that the execution (or evaluation) of experiment FP2 - 97120401 - M7cl must have been subject to one or several errors. Secondly, the decrease of $(k_R')_{11}$ with time over the

measurement cycle adjusted for the effects of the differences in the cell currents used in the two experiments (~0.2A and 0.15A respectively) shows that we should have observed a decrease of ~0.0013×10^{-9} WK^{-4} in $(k_R')_{11}$ over the one - day measurement cycles. The experimentally observed decreases were ~0.0044×10^{-9} WK^{-4} and, evidently, the silvering in the upper part of the Dewar cells was ineffective in reducing the heat transfer across this part of the cell. A logical interpretation of this observation (combined with the anomalously high heat transfer coefficients) is that the vacuum in the cells had "softened" so that there was an appreciable conductive contribution to the heat transfer coefficients. The silvering in the top part of the Dewar cells would not be effective in reducing any such conductive contribution. A third observation is that the results in Fig. 26 show that there is no effect which could be attributed to "positive feedback" (in contrast to the results for experiment 1P2 - 97120401 - M7cl).

We conclude that the vacuum in the cell used in experiment FP2 - 97120401 - M7cl had softened and that the most likely explanation of the discontinuities in the heat transfer coefficients seen in Figs. 18 - 21 was that a mistake had been made in the wiring of the cell to the switching box of the polarizing circuits.

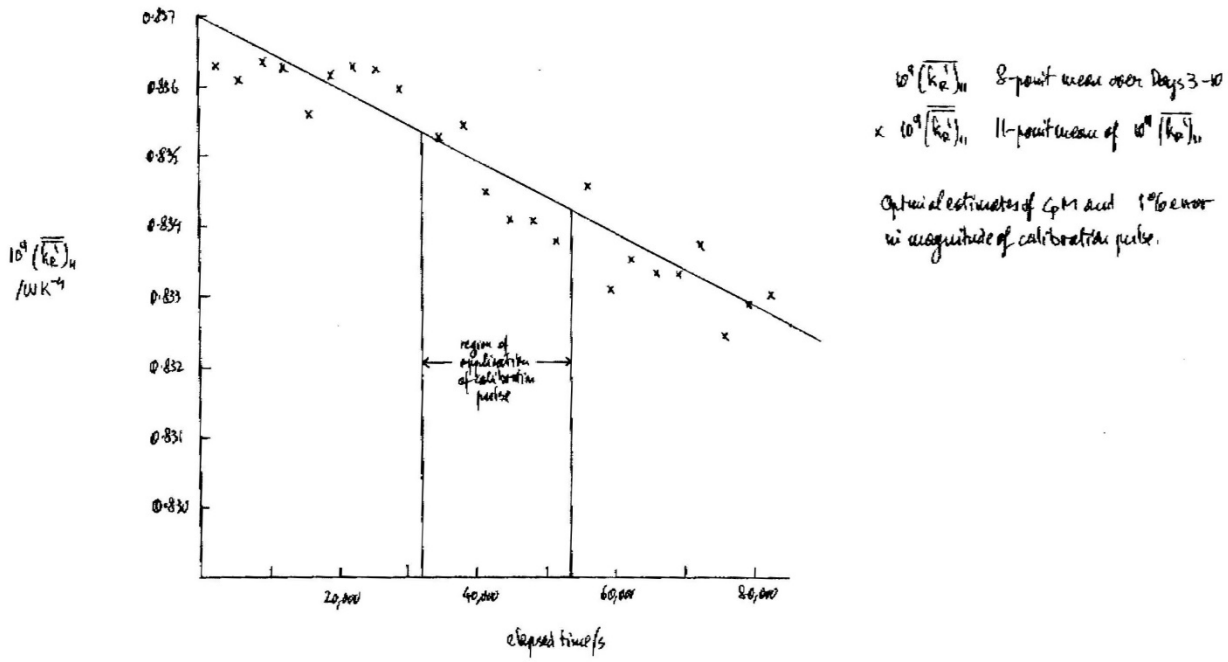
Mel: I fear that we have a further situation best characterised by the question : "how long is a piece of string?" It seems to me that we should aim for a further publication a major part of which would be a comparison of Fig. 26 with Fig. 20 or, better, of a revised Fig 26 (see below) with Fig 21 together with comments about Fig. 22. What do you think about this? The problem now is that the assumptions used for deriving the data in Figs. 18 - 25 (Fig 25 to follow) are not identical to those used for Fig. 26 but, clearly they have to be made consistent. An obvious starting point is the use of identical thermoneutral potentials. Furthermore, the water equivalent used to devise Fig 26 is evidently too large (349 JK^{-1}). I recall writing to Claudia Bartolomeo in August 1995 to tell her this (I had done some preliminary work on the $(k_R')_{21}$ -spreadsheets), see relevant comments about column 3 of Table 6). All of this work is now "lost". It seems to me that $(k_R')^\circ_{261}$ and the associated water equivalents must be re-evaluated and the water equivalents must be used to derive a revised version of Fig. 26 using the "optimal values". It is a major undertaking! In the meantime you may wish to use Fig. 26 as it stands and I shall therefore send you a tabulation which will allow you to plot a proper Figure.

Section 6 Miscellaneous Comments

In addition to the evaluations described above, the present investigation has covered many other aspects. These include the evaluation of $(k_R')_{11}$ for the complete data sets and the derivation of $(k_R')^\circ_{151}$, $(k_R')^\circ_{161}$, $(k_R')^\circ_{171}$ and $(k_R')^\circ_{181}$ together with the associated values of C_pM (see below); furthermore, the integral heat transfer coefficients $(k_R')21$ and $(k_R')31$ have been evaluated and the relevant spreadsheets have been used to devise $(k_R')^\circ_{251}$ and $(k_R')^\circ_{252}$, and $(k_R')^\circ_{261}$, and $(k_R')^\circ_{261}$, and $(k_R')^\circ_{271}$, and $(k_R')^\circ_{272}$, and $(k_R')^\circ_{281}$, and $(k_R')^\circ_{282}$ as well as the heat transfer coefficients for forward integration $(k_R')^\circ_{361}$, and $(k_R')^\circ_{361}$, and $(k_R')^\circ_{371}$, and $(k_R')^\circ_{372}$, and $(k_R')^\circ_{381}$, and $(k_R')^\circ_{382}$.

As the precision and accuracy of the heat transfer coefficients based on the backwards integration of the data is markedly enhanced compared to those of the . . . [discontinued]

Fig 22 $10^9\left(\overline{\overline{k_R^{\,1}}}\right)_{11}$ versus time; GpM = optimal values, 1% error in calibration pulse



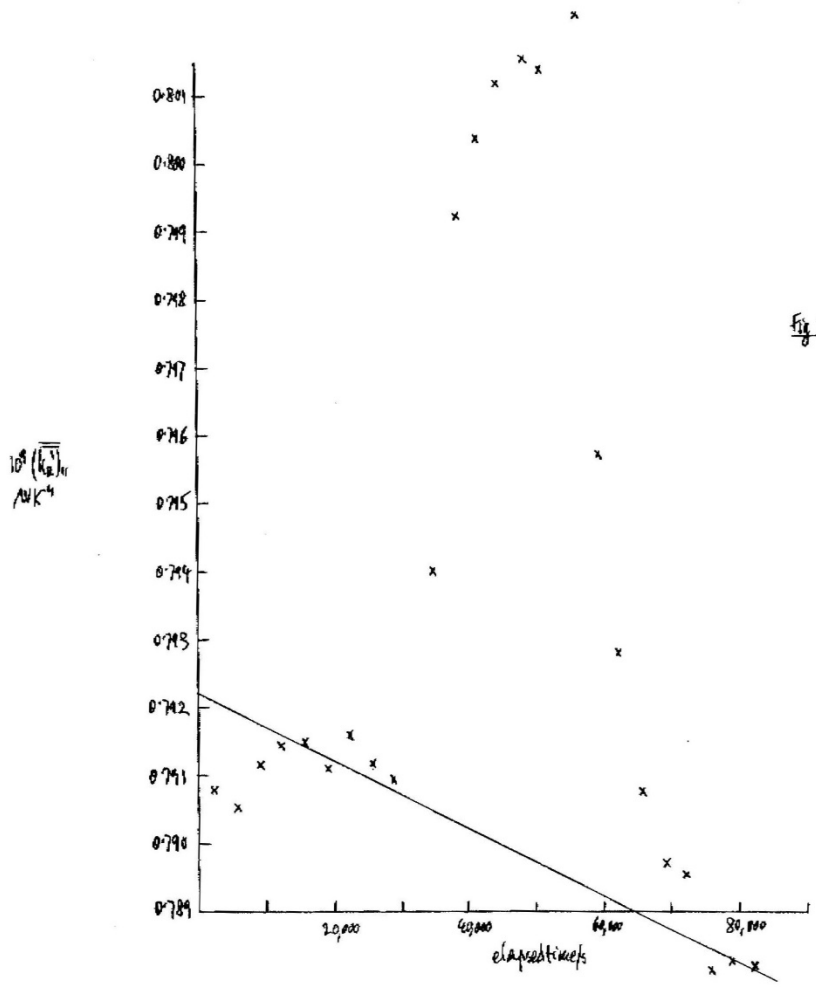


Fig 23 $10^9 \left(\overline{\overline{k_R'}}\right)_n$ versus time; $C_p M$ = optimal values
1 Ω series resistance in current leads

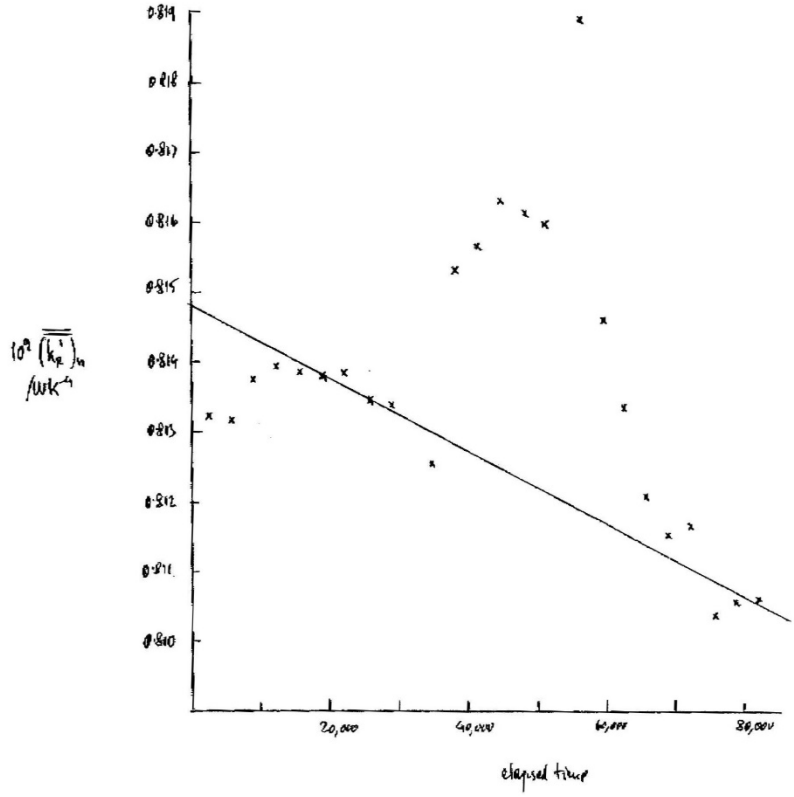


Fig 24 $10^2 \overline{\overline{(k_R^1)}}_n$ versus time; Gpt1 = optimal values
0·5 Ω earth resistance in current lead.

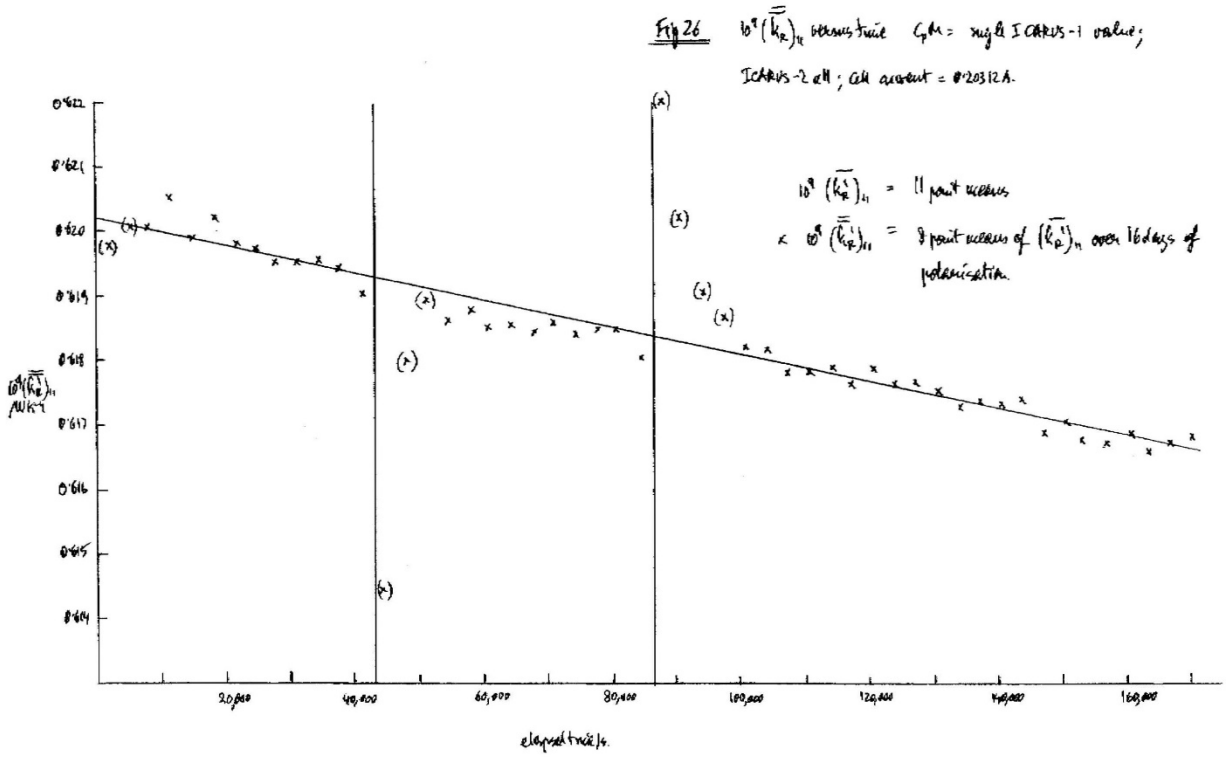


Fig 26
$10^9 (\overline{\overline{k_R}})_{11}$ versus time
Cycle = single ICARUS-1 value;
ICARUS-2 all; all current = 0·20312 A.
$10^9 (\overline{k_R'})_{11}$ = 11 point means
$10^9 (\overline{\overline{k_R'}})_{11}$ = 3 point means of $(\overline{k_R'})_{11}$ over 16 days of polarization.
$10^9 (\overline{\overline{k_R'}})_{11}$ / MKS
0·622
0·621
0·620
0·619
0·618
0·617
0·616
0·615
0·614
20,000
40,000
60,000
80,000
100,000
120,000
140,000
160,000
elapsed time / s.

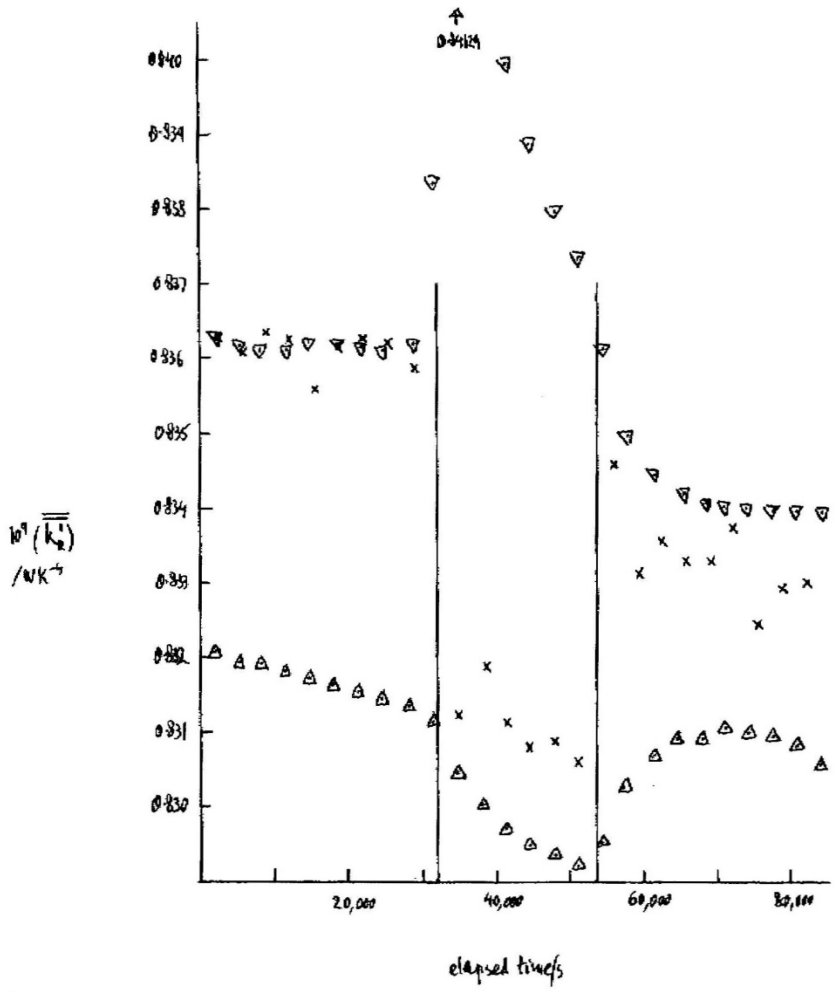


Fig 27 Comparison of the Differential and Integral Heat Transfer Coefficients

× $10^9 (\overline{\overline{k_2'}})_{11}$

▽ $10^9 (\overline{\overline{k_2'}})_{31}$

△ $10^9 (\overline{\overline{k_2'}})_{21}$

Table 4 The double mean Lorock bound heat transfer coefficients $10^4 (\overline{\overline{k_p'}})_{11}$ /WK^{-4}

column	1	2	3	4	5	6	7	8
	elapsed time/s	GM = 450JK^{-1}	GM = 490JK^{-1}	GM = ICARUS-1 values	GM = optimal values	GM = optimal values 10Ω in current leads	GM = optimal values 0.5Ω in current leads	GM = optimal values 1% error in calibration pulse
	2400	0.83705	0.83626	0.83602	0.83628	0.79078	0.81922	0.83628
	5700	0.83631	0.83598	0.83624	0.83607	0.79053	0.81316	0.83607
	9000	0.83645	0.83640	0.83635	0.83635	0.79117	0.81375	0.83635
	12300	0.83637	0.83626	0.83629	0.83625	0.79144	0.81392	0.83625
	15600	0.83654	0.83646	0.83645	0.83558	0.79148	0.81385	0.83558
	18900	0.83621	0.83616	0.83617	0.83617	0.79108	0.81379	0.83617
	22200	0.83617	0.83604	0.83604	0.83628	0.79160	0.81385	0.83628
	25500	0.83602	0.83604	0.83602	0.83624	0.79119	0.81346	0.83624
	28800	0.83583	0.83586	0.83586	0.83586	0.79093	0.81334	0.83586
	34800	0.84496	0.83486	0.83160	0.83122	0.79400	0.81253	0.83525
	38100	0.84025	0.83123	0.83153	0.83188	0.79923	0.81537	0.83944
	41400	0.83511	0.83088	0.83084	0.83112	0.80039	0.81569	0.83428
	44700	0.83311	0.83065	0.83071	0.83080	0.80118	0.81629	0.83407
	48000	0.83128	0.83082	0.83061	0.83087	0.80154	0.81613	0.83409
	51300	0.83129	0.83054	0.83059	0.83059	0.80140	0.81549	0.83578
	56800	0.81709	0.83672	0.83511	0.83458	0.80239	0.81882	0.83458
	59100	0.82272	0.83428	0.83335	0.83310	0.79573	0.81454	0.83310
	62400	0.82480	0.83439	0.83386	0.83354	0.79280	0.81335	0.83354
	65700	0.83015	0.83274	0.83341	0.83328	0.79076	0.81207	0.83328
	69000	0.83169	0.83347	0.83336	0.83328	0.78971	0.81193	0.83328
	72300	0.83282	0.83383	0.83374	0.83375	0.78953	0.81165	0.83375
	75600	0.83220	0.83244	0.83266	0.83244	0.78811	0.81035	0.83244
	78900	0.83258	0.83285	0.83282	0.83289	0.78826	0.81057	0.83289
	82200	0.83209	0.83302	0.83316	0.83301	0.78820	0.81063	0.83301

← shift column 8 to follow column 5 and remember

82100s ↑

region of application of calibration pulse

↓ 53400s

Data required for Fig 26

Time/s	$10^4(\overline{\overline{k_2}})_u$	Time/s	$10^4(\overline{\overline{k_2'}})_u$
1800	0·61978	87600	0·62204
5100	0·62006	90900	0·62020
8400	0·62007	94200	0·61902
11700	0·62053	97500	0·61865
15000	0·61989	100800	0·61820
18300	0·62021	104100	0·61819
21600	0·61979	107400	0·61781
24900	0·61973	110700	0·61782
28200	0·61952	114000	0·61789
31500	0·61953	117300	0·61766
34800	0·61955	120600	0·61787
38100	0·61943	123900	0·61766
41400	0·61944	127200	0·61769
44700	0·61971	130500	0·61751
48000	0·61800	133800	0·61721
51300	0·61874	137100	0·61738
54600	0·61864	140400	0·61732
57900	0·61875	143700	0·61742
61200	0·61851	147000	0·61684
64500	0·61855	150300	0·61704
67800	0·61824	153600	0·61676
71100	0·61859	156900	0·66672
74400	0·61837	160200	0·61688
77700	0·61829	163500	0·61658
81000	0·61851	166800	0·61673
84300	0·61849 0·61949	170100	0·61683

Data required for Fig 27

Time/s	$10^4(\overline{\overline{k_2'}})_u$	Time/s	$10^4(\overline{\overline{k_2'}})_u$	$10^4(\overline{\overline{k_2'}})_u$
2400	0·83623	1800	0·83207	0·83627
5700	0·83607	5100	0·83192	0·83617
9000	0·83635	8400	0·83191	0·83612
12300	0·83625	11700	0·83183	0·83613
15600	0·83558	15000	0·83173	0·83623
18900	0·83617	18300	0·83165	0·83620
22200	0·83628	21600	0·83154	0·83616
25500	0·83624	24900	0·83145	0·83611
28800	0·83586	28200	0·83135	(0·83622)
34800	0·83122	31500	(0·83114)	(0·83836)
38100	0·83188	34800	0·83045	0·84266
41400	0·83112	38100	0·83001	0·84124
44700	0·83080	41400	0·82970	0·83996
48000	0·83087	44700	0·82949	0·83886
51300	0·83059	48000	0·82934	0·83799
55800	0·83458	51300	0·82921	0·83736
59100	0·83310	54600	(0·82953)	(0·83668)
62400	0·83394	57900	0·85025	0·83499
65700	0·83328	61200	0·83069	0·83447
69000	0·83328	65500	0·83091	0·83422
72300	0·83375	67800	0·83092	0·83408
75600	0·83244	71100	0·83106	0·83404
78900	0·83289	74400	0·83100	0·83391
82200	0·83301	77700	0·83095	0·83398
		81001	0·83084	0·83396
		84300	0·83176 (0·83095)	0·83345

2002-05-03

Bury Lodge heading

3 May 2002

Dr. M. R. Miles,
Department of Chemistry,
Middle Tennessee State University, (MTSU),
Murfreesboro,
TN 37132, U.S.A.

Dear Mel,

I have finally, finally decided to go to China. More about this below. I feel somewhat better but I have decided to "chicken out" of all commitments other than ICCF 9. I have explained this to Li and Tian and they have accepted the situation with regrets.

Many thanks for your letter of 23 April 2002 and the details of the solutions and Stan's comment about the silver coated ballotini. I would like to discuss some next steps regarding the "E.C.S." and "Science" with you in Beijing. Could you please bring copies of the papers you submitted (the same paper?) with you so that we can discuss these next steps?

You will have noticed that I did not send you the complete version II of the whole Report following my letter/FAX of 22nd April 2002. I am also now attaching version III of section 5 of the Report but I will once again <u>not</u> send you the complete text. The reason is that, in the meantime, I have made <u>some</u> progress in revising Fig 26 which I am now attaching together with the tabulation needed to-produce this Figure. Incidentally, I noticed that the last number in Column 2 of the tabulation I sent to you on 22nd April should have been 0.61849 not 0.61949 as given in that tabulation. I also noticed that I did not send you Fig 25 which I am now attaching. Enough said?

The reason why I have only said that I have made <u>some</u> progress with the old Fig 26 is because the corrections I have made are based on a partial analysis of Day 2 of the measurement cycles carried out in 1995. I have ignored the corrections due to the change in the thermoneutral potential (which would displace the whole plot) and have simply corrected the mistake in the software (see my letter of 22nd April and I have also corrected the water equivalent. This correction is satisfactory adjacent to $t = 0$ and $t = t_2$ but the complete correction at $t = t_1$ will require the re-evaluation of the complete data set (which is why I have still bracketed the first three values of L after $t = t_1$). I believe that we should discuss all this in Beijing: the whole data set is somewhat less than satisfactory as I will explain to you,

Now, as regards China: I would like to include some reference to Stan Szpak's experiments in my talk on 20th May. A diagram of the apparatus and an illustration of the "hotspots" would be appropriate. My facilities for producing slides are now zero and I wonder therefore whether you could get these two slides from Stan or Pam and bring them with you to Beijing? Incidentally I will be arriving on the morning of the 18th May.

See you in Beijing, and regards,

Martin

[JR Here is a sample of the corrected manuscript that follows.]

. . . result is therefore no peculiar artefact of the methods of data evaluation. We also note that all the heat transfer coefficients are anomalously large which is true of both the "lower bound" and the "true" values. Furthermore, all the values of the water equivalent of the cell are anomalously large. This is a further reason prompting the conclusion that the execution of the experiment was subject to an artefact. In the design of the ICARUS systems it was considered that such artefacts would be revealed most readily from a consideration of the "lower bound" heat transfer coefficients, a consideration which should always be interposed between the preliminary examination of the data, section 3, and the accurate assessment as outlined in this section_ As has already been pointed out, this detailed examination of the (k_R')ii -spreadsheets is outlined in the next section.

VERSION III

Section 5 Consideration of the $(k_R')_{11}$ spreadsheets

It has been pointed out in the previous section that the $(k_R')_{11}$-spreadsheets provide an effective means of monitoring the performance of the instrumentation. The reason is that the values of $(k_R')_{11}$ are determined by the local behaviour. Although the heat transfer coefficients derived by forward or backward integration are much more precise (or accurate) than the $(k_R')_{11}$ and $(k_R')_{12}$ values, they inevitably average out all localised changes over the evaluation interval. It is therefore better to investigate localised changes by considering the $(k_R')_{11}$-spreadsheets.

The importance of the experiment FP2 - 97120401- M7cl lies in the fact that the measurements on Days 3 - 10 were all carried out at the same, low, current density. One would therefore expect that excess enthalpy generation (if any) would be at a low level and that one could carry out averaging procedure over the 8 successive measurement cycles. . . .

[the rest omitted]

2002-06-27

Dr. M.H. Miles
N.A.W.C.
China Lake

27/06/02

Dear Mel,

Herewith a delayed letter and some enclosures. When I 'phoned your old house number, I found that it had been disconnected so it is good to have your present coordinates! Unless I hear from you to the contrary, I will send the letter package to 807 Mamie Avenue.

No doubt you will send me your Mamie addresses when you go there in August?

Regards,

Martin

Bury Lodge heading

21st June 2002.

Dr. M. H. Miles,
Chemistry and Materials Branch,
Research and Technology Division,
N.A.W. C.
China Lake,
CA 93555-6100,
U.S.A.

Dear Mel,

Greetings after this somewhat long delay. I have been rather unwell since coming back from Beijing [ICCF-9] (no great surprise as I thought that this was rather likely!) but I am now at last again picking up the various threads. One consequence of this interregnum is that I have "lost" the piece of paper on which you outlined your plane for this summer. However, as I recall, I believe that you said that you might be going back to the laboratories at China Lake to carry out a further battery project? I will telephone your home number later today to see where I might be able to reach you. I need to send you this FAX and also a letter package so I have to get your various coordinators.

You will recall that I told you (In my letter of 3/5/2002) that I was not sending you version III of my Report on the Data Analysis for days 3-10 of Experiment FP2-97120401-M7cl, because I made some progress with the revision of Fig 26. However, it now strikes me that you should keep a complete record of the various stages of my analysis so I want to send you a copy of version III - this is to be in the letter package.

I believe that I enclosed a copy of Fig 26 of Version III with my letter of 3/5/2002 together with the revision to Fig 26 (labelled Fig 26 revised). I am now again attaching those two Figures; you will see that, in calculating these results, I used the ICARUS-1 value for the water equivalent at $t = t_2$ together with adjustments of this value to allow for the effects of electrolysis to recalculate the first two points adjacent to the $t = 0$ in the first three points adjacent to $t = t_1$. More about this below.

However, as I told you in Beijing, I found that there was an evident mistake in the ICARUS-2 software so that it was necessary to recalculate the whole data set. I have now carried out this recalculation and have also used this as an opportunity to set the thermoneutral potential at 1.527V rather than the 1.54V used in the previous calculations. In this recalculation, I have used the ICARUS-1 values of the water equivalent determined at $t = t_1$ and $t = t_2$ to calculate the first 6 values of the heat transfer coefficient adjacent to these times and the value at $t = t_1$ adjusted for the effects of electrolysis to calculate the first 4 values adjacent to $t = 0$; otherwise, I have used

$C_pM = 349JK^{-1}$ given in 1995 for this data set. This value is too large but, in the remaining time regions, it does not really matter which value we use. I am attaching Fig 26 recalculated which summarises the results together with the tabulation needed to produce this Figure.

You will see that the use of the more or less correct values of the water equivalent removes the marked perturbations of the heat transfer coefficient adjacent to $t = t_1$ and $t = t_2$ and the smaller perturbations adjacent to $t = 0$. Of course, if one uses a completely incorrect value of C_pM (as was done by the N.H.E.), the effects of these perturbations are extended on the time scale. If one then reduces the measurement cycle from 2 days to 1 day, one will inevitably draw completely incorrect conclusions about the precision and accuracy of the calorimetry.

However, the main point about Fig 26, Fig 26 revised and Fig 26 recalculated is that the imposition of the calibration pulse does not lead to any discontinuity in the variation of the heat transfer coefficient with time of the kind seen in Fig 20 of the Report (or the related Figures). This confirms my belief that such discontinuities may be explained by errors in the instrumentation (they can hardly be due to errors in the data analyses).

The disparity between Figs 26 revised or Fig 26 recalculated and Fig 20 has prompted me to examine some of our earlier correspondence with the N.H.E. (as I have always said "correspondence" is an incorrect description because they never replied to my letters) and, in particular, the First Report [(1)] on their experiments. I find that a substantial section of that Report [(1)] was devoted to tests of the kind delineated in Versions I, II, and III of the present Report. It was shown that there was no discontinuity in the heat transfer coefficients due to the imposition of the calibration pulses for two Pt/D_2O "blank" experiments carried out at the beginning of 1994. The Report also contained instructions as to how further experiments were to be carried out! As you know, NHE never carried out such further "blank" experiments or, if they did so, they have never released the relevant data.

I have at times wondered whether I should send you a copy of this first Report [(1)]. I have decided in the end against doing so because the Report is in a sense Confidential to the N.H.E. However, this whole project was to be carried out in the Public Domain so there cannot really be any fundamental objection to my distributing this Report more widely. What do you think I should do? The reasons for my wishing to send this Report to you are quite wide-ranging and include:

(i) historical accuracy

(ii) the importance of carrying out "blank" experiments

(iii) an additional methodology for assessing whether or not the imposition of a calibration pulse leads to changes in the heat transfer coefficient (i.e. additional to the method I have used in the Reports I have sent to you).

(iv) additional methods for assessing the adequacy of the procedures; these are based on a) averaging of the cell potential before evaluating the data b) averaging the cell temperature before this evaluation c) averaging both the cell potential and cell temperature before the evaluation d) comparison of c) with the usual method of

determining $\left(\overline{\overline{k_R{}'}}\right)_{11}$ (there is no difference hence the nonlinearities of the Calorimetric equation do not affect the evaluation).

(v) application of the maximum value of $(k_R')_{11}$ (as a measure of $(k_R')_{12}$) to evaluate the rate of excess enthalpy generation as was done at ICCF 3 (2)

(vi) comments that the N.H.E. experiment 4251 was unsatisfactory so that this experiment could not be evaluated.

There is also the matter of the Second Report which is important because it showed that the N.H.E. experiment 4711 was unsatisfactory and again could not be evaluated. However experiments 4251 and 4711 turned up as the prime and only examples in the N.H.E. paper for the ICCF5 Conference Proceedings although it was not presented at the meeting. The authors also reinvented (v) above (but incorrectly!) and pretended that this was a methodology devised by them because the ICARUS procedures did not work. This prompted me to reanalyse the N.H.E. experiment 4711 and I presented this reanalysis as a Poster at ICCF 6. I believe that you have a copy of this Poster? One important additional point is that this Poster was the calculation of the upper and lower tail distributions (copy of the diagram is attached) which pointed clearly to the benefits of using the integral heat transfer coefficients.

What should we do? Herewith a suggestion: we should work up a paper on "Penultimate Comments on Isoperibolic Calorimetry Applied to the Pd/H And Pd/D Systems" for ICCF 10. If we do this correctly, we should be able to recycle extracts from your, my and the N.H.E. data and try to set the records straight. When I survey the past investigations, I inevitably come to the conclusion that we were meant to fail although the detailed reasons for this wish are very obscure. I think that we made virtually no progress after 1992, a matter which was rather forcibly pointed out to me by Giuliano Preparata. As he said: "you have been put in a gilded cage". My attempts to break out of this were repeatedly frustrated; my final attempt led to the "parting of the ways" which should have taken place in 1994 and not 1995.

This brings me to ICCF 9 which turned out to be rather more interesting than I had expected. It prompted me to make a listing of items which I perceive to be of importance to National Security and I came up with 13 major topics which, of course, translate into some 20-30 research projects (it depends on how you define the projects). What is so interesting about this list is that 10 of these items were on my list in 1988/89 - the remaining 3 would have followed fairly naturally from the first 10 if a reasonably comprehensive research programme had ever been put into place. Perhaps you could tell Jim Cory that it is my view that we will therefore rather inevitably wake up someday to a nasty surprise - that is if the 13 projects are not already parts of adequate "Black programs". You could also tell him that I will certainly be glad to meet him on some future occasion to "chew the cud".

With regard to the meeting, I would classify 2½ of the contributions as being relevant to my additional 3 topics. The reason for saying 2½ and not 2 or 3 is that one of the relevant research groups does not really understand what it is doing - we can discuss this some other time.

I think that one of the most important contributions was that by E. Del Giudice, Antonella De Ninno, A. Fratolillo, M. Porcu and A. Rizzo. Conservative estimates indicate that they must have reached rates of excess enthalpy generation in the range of 1 = 10MWcm^{-3} but I think values in the range of 10-100MWcm^{-3} are more likely – and they haven't even tried to jack up the rates. Grrr! Are you surprised that this topic has made it to my list? Of course, this investigation is a triumph but, having said this, I must also say that I have considerable reservations about the approach which they used. The problem with a heavy emphasis on "bustrophedic" thin films is that it effectively drives the project to a full stop and it also obscures the connection to the other investigations of the generation of excess enthalpy. In point of fact, the generation of D+ in an unique Quantum system has nothing whatever to do with the "bustrophedic" structure - that is just a convenient way of demonstrating the effects. I pointed out several times to Giuliano that I wanted to implement some further experiments around their core design but he did not want to know. As you know, I am a fairly charitable person but, if I wanted to be less charitable, I would begin to question whether my colleagues do not want to create some Clearwater between their research and M. F.'s view of Alfred Coehn? Ah well!

Back to Jim Cory etc. I think that you should regard the Report on the Soviet work which he gave you as being semi-classified. It clearly is not absolutely classified as it is based on published information but it would be best to be cautious about any reference to this work. Yes, I know something about this work but not nearly as much as I would have liked. However, the little bit which I knew was quite sufficient when juxtaposed to the D.U. shell problem. So that is more or less the whole reason for the start of the C.F. saga.

Now here is a thought for you: it is fairly well-known that the application of high pressures to solids can lead to the formation of new phases having surprising properties. So now we see that electrochemical compressions form an interesting sub-set of the high pressure systems, a subset which should be interpreted as being due to the formation of the system having a single unique wave-function. Should we not investigate the application of Q.E.D. to the whole field of the compression of solids?

Finally I have a request for help! I have been trying to write up my lecture in Beijing which is singularly difficult because of the large number of diagrams. Clearly, some of these have to be eliminated and I have decided to cut out those dealing with Stan Szpak's and Pam Mosier-Boss' endeavors by referring to your paper. However, I would also like to refer to Stan and Pam's work in the text. The problem is that I do not have the most appropriate references to this work. Could you please let me know the reference(s) you will be using? If there is more than one, could you tell me which one is most appropriate?

I trust you are having a good vacation and getting ready for Maine! The fall in Maine is splendid!

Regards,

Martin

P.S. In the discussion of any further paper(s) which we may right, we should explain that our comments are "Penultimate" because an "Ultimate" set of comments would require access to the raw data for various investigations. That would allow us to point a very grubby finger at the various research groups.

PPS I should have pointed out that it would certainly be possible to improve the calculation of the data shown in Figs. 26, Fig 26 revised and Fig 26 recalculated. However I believe that the data as they stand are adequate to illustrate the points which we need to make. More exact evaluations should really be based on the integral heat transfer coefficients. Should we launch ourselves on this task?

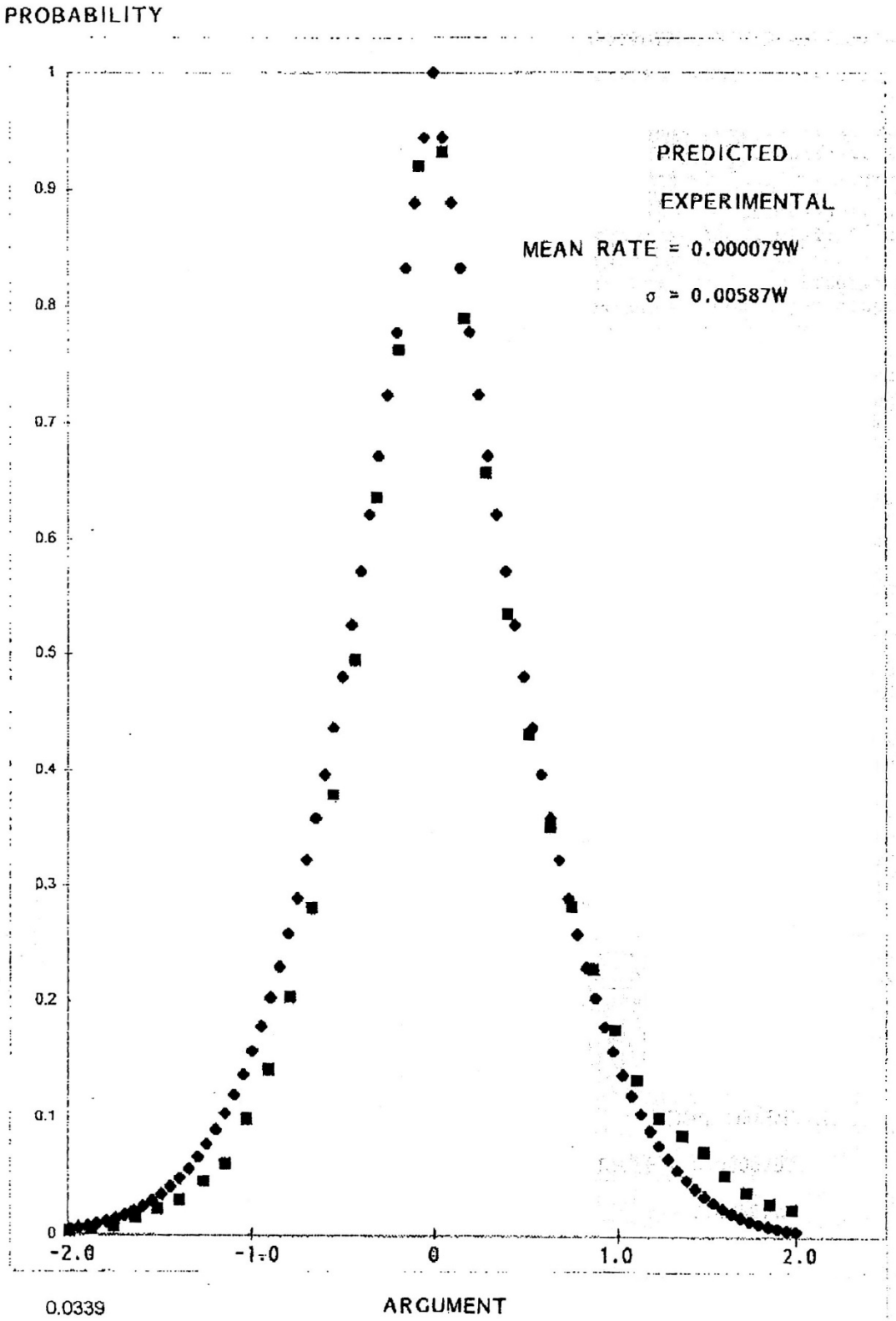
References

1) Report on the First Set of Experiments carried out under the NEDO/NHE Project at the Sapporo Laboratories, June (1994).
2) M Fleischmann and S Pons, Proceedings of the 3rd International Conference on Cold Fusion, editor H Ikegami, universal Academy Press, (1993) 47; ISBN 4-946443-12-6.
3) Second Report on the Experiments carried out under the NEDO/NHE Project at the Sapporo Laboratories, December (1994).
4) Toshiba Seito, Masao Sumi, Naoto Asami and Hideo Ikegami, Proceedings of the 5th International Conference on Cold Fusion, (1995) 105.

P.P.P.S. Who was the gentleman from San Diego who objected first to my use of Stan and Pam's slide and then objected to your use of these slides? He apparently thought that the "hotspots" were somehow connected to the Quantisation of the signal. However, such quantisation can only affect the lowest level signals not of the highest levels measurements. Could you discuss this whole question with Stan and Pam?

Did I ever tell you that I went to our Signals R + D Establishment at Malvern some years ago to look at their gear. I decided that it would be possible to determine the Q values of the processes in thin Pd films but later on came to the conclusion that laser thermometry would be even better.

P.P.P.P.S. I believe that there are some very interesting new possibilities for inserting D^+ into the tetrahedral sites. Will you have any facilities for doing experimental work in Maine and, more importantly, if you will have such facilities, do you want to do anything further? If so, then I will tell you about these possibilities which link back to the Frascati work. It is really a question of doing some careful cyclic voltammetry possibly coupled to the production of some novel structures.

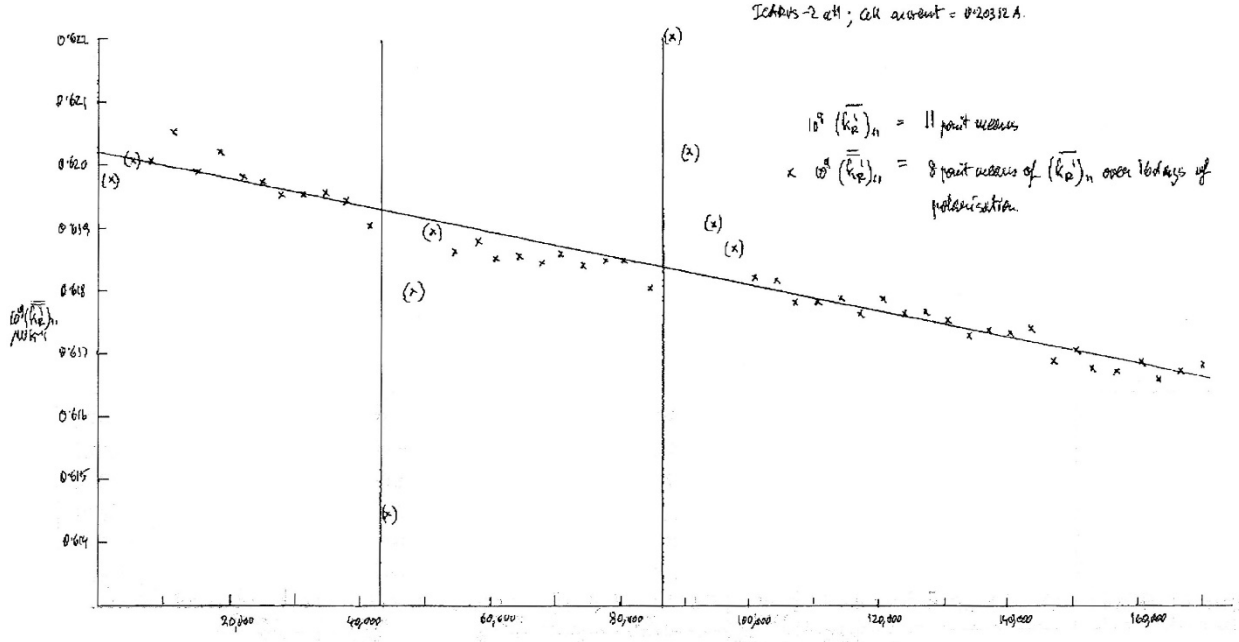


NORMALIZED UPPER AND LOWER TAIL DISTRIBUTIONS; Pt electrode

Table 4 (revised). The double mean Rosenbrock heat transfer coefficients $10^4\,\overline{\overline{(k_R')}}_n$ /WK^{-1}

column	1	2	3	4	5	6	7	8	9	
	elapsed time/s	Gth = 4500 JK^{-1}	Gth = 4900 JK^{-1}	Gth = ICARUS-1 value,	Gth = optimum values,	Gth = optimum values, 1% error in calibration pulse	Gth = optimum values, 1 Ω in current leads	Gth = optimum values, 0.5 Ω in current leads	Gth = optimum values, 0.25 Ω in current leads,	
	2400	0.83705	0.83626	0.83602	0.83628	0.83628	0.79078	0.81922	0.82468	
	5700	0.83631	0.83548	0.83624	0.83607	0.83607	0.79053	0.81316	0.82445	
	9000	0.83645	0.83640	0.83635	0.83635	0.83635	0.79117	0.81375	0.82501	
	12300	0.83637	0.83626	0.83624	0.83625	0.83625	0.79144	0.81392	0.82518	
	15600	0.83654	0.83646	0.83645	0.83558	0.83558	0.79148	0.81385	0.82510	
	18900	0.83621	0.83616	0.83617	0.83617	0.83617	0.79108	0.81379	0.82479	
	22200	0.83617	0.83614	0.83614	0.83628	0.83628	0.79160	0.81385	0.82513	
	25500	0.83602	0.83604	0.83602	0.83624	0.83624	0.79119	0.81346	0.82478	
	28800	0.83583	0.83586	0.83586	0.83586	0.83586	0.79093	0.81334	0.82459	
	34800	0.84446	0.82986	0.83160	0.83122	0.83525	0.79400	0.81253	0.82249	32100 s
	38100	0.84025	0.83123	0.83153	0.83188	0.83544	0.79923	0.81537	0.82354	
	41400	0.83511	0.83068	0.83084	0.83112	0.83448	0.80039	0.81569	0.82326	region of application of calibration pulse
	44700	0.83311	0.83065	0.83071	0.83080	0.83407	0.80118	0.81629	0.82345	
	48100	0.83128	0.83082	0.83061	0.83087	0.83409	0.80157	0.81613	0.82344	
	51300	0.83124	0.83054	0.83054	0.83059	0.83378	0.80140	0.81549	0.82332	
	55800	0.81704	0.83672	0.83511	0.83458	0.83458	0.80239	0.81882	0.82474	57400 s
	59100	0.82272	0.83428	0.83335	0.83310	0.83310	0.79573	0.81459	0.82403	
	62400	0.82480	0.83434	0.83386	0.83354	0.83354	0.79280	0.81335	0.82352	
	65700	0.83015	0.83274	0.83341	0.83328	0.83328	0.79076	0.81207	0.82269	
	69000	0.83161	0.83347	0.83336	0.83328	0.83328	0.78971	0.81153	0.82234	
	72300	0.83282	0.83383	0.83374	0.83375	0.83375	0.78953	0.81165	0.82267	
	75600	0.83220	0.83264	0.83266	0.83244	0.83244	0.78811	0.81035	0.82160	
	78900	0.83258	0.83285	0.83282	0.83289	0.83289	0.78826	0.81057	0.82176	
	82200	0.83289	0.83302	0.83316	0.83301	0.83301	0.78820	0.81063	0.82196	

Fig 26 $10^4\,\overline{\overline{(k_R)}}_n$ versus time Gth = single ICARUS-1 value; ICARUS-2 off; cal current = 0.20312 A.



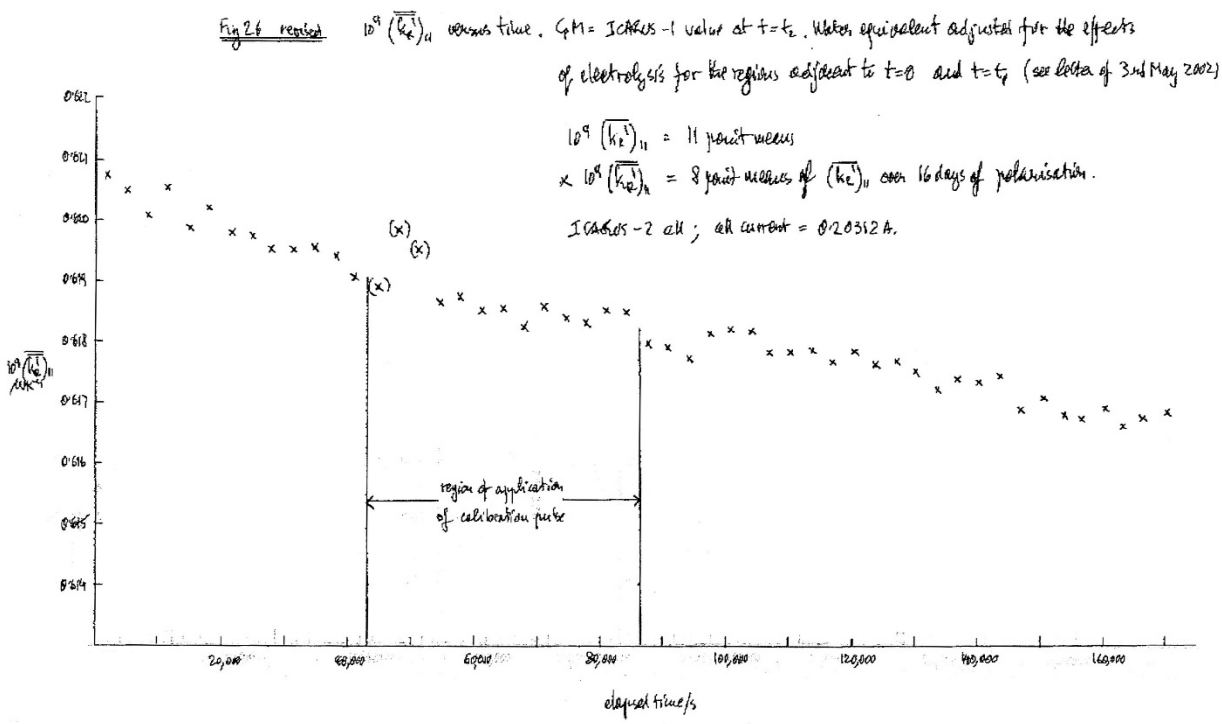


Fig 26 revised
$10^9 (\overline{\overline{k_2^1}})_{11}$ versus time. GM = ICARUS-1 value at $t = t_2$. Values equivalent adjusted for the effects of electrolysis for the regions adjacent to $t=0$ and $t=t_1$ (see letter of 3rd May 2002)
$10^9 (\overline{k_2^1})_{11}$ = 11 point means
x $10^9 (\overline{\overline{k_2^1}})_{11}$ = 8 point means of $(\overline{k_2^1})_{11}$ over 16 days of polarisation.
ICARUS-2 cell; cell current = 0.20312 A.
$10^9 (\overline{\overline{k_2^1}})_{11}$ / ms^{-1}
0.622
0.621
0.620
0.619
0.618
0.617
0.616
0.615
0.614
(x)
region of application of calibration pulse
20,000
40,000
60,000
80,000
100,000
120,000
140,000
160,000
elapsed time/s

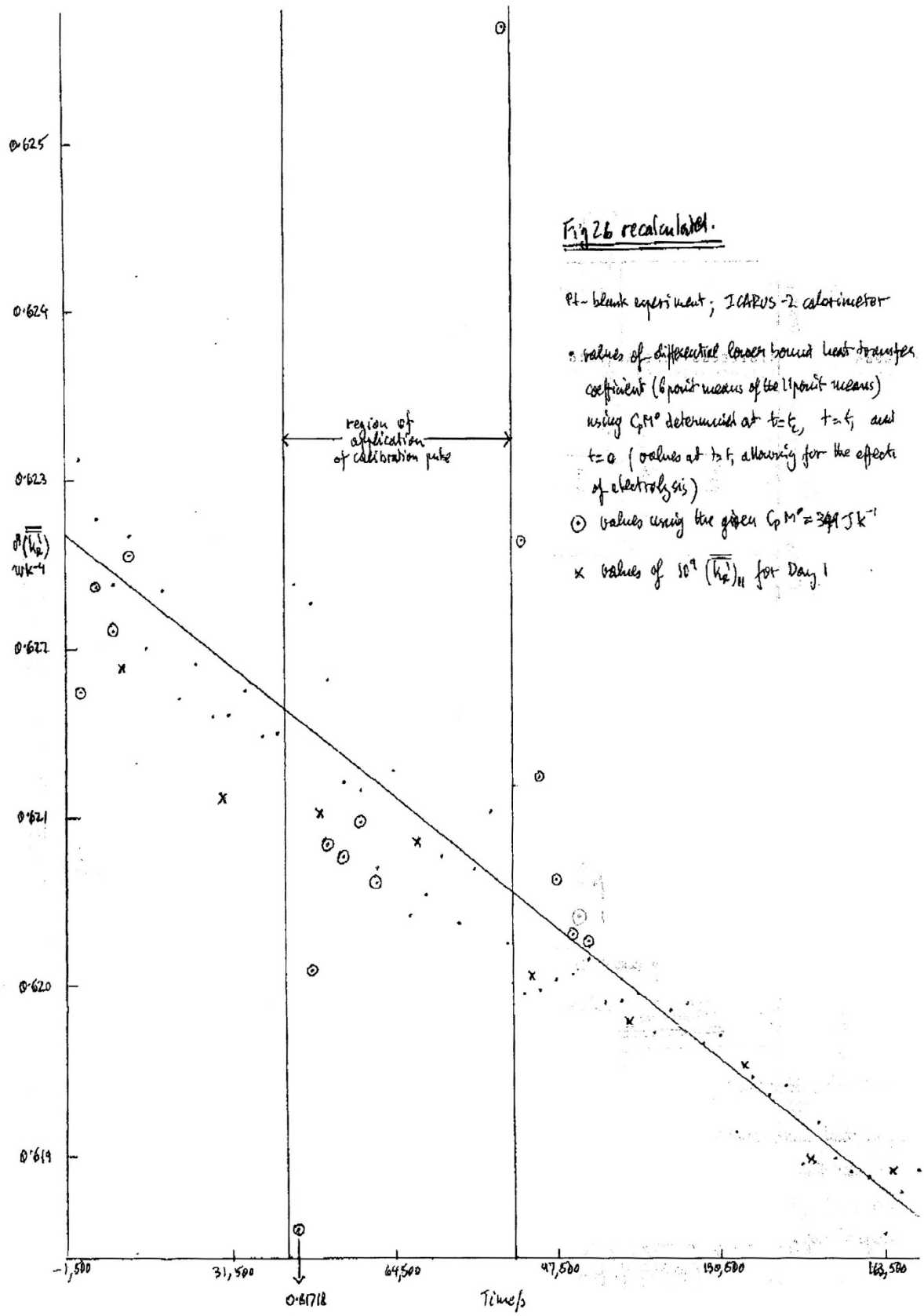


Fig 26 recalculated.
Pt - blank experiment; ICARUS-2 calorimeter
• values of differential lower bound heat transfer coefficient (6 point means of the 11 point means) using C_pM^o determined at $t=t_2$, $t=t_1$ and $t=0$ (values at $t>t_1$ allowing for the effects of electrolysis)
⊙ values using the given $C_pM^o = 349\ J\,K^{-1}$
× values of $10^9\,(\overline{h_t'})_{11}$ for Day 1
region of application of calibration pulse
0·625
0·624
0·623
0·622
0·621
0·620
0·619
$10^9\,(\overline{h_t'})$ $W\,K^{-1}$
-1,500
31,500
64,500
97,500
130,500
163,500
0·81718
Time/s

Pt blank experiment; ICARUS-2 calorimeter; values of $10^4\overline{(k_R^{-1})}_{11}$ are corrected for the observed values of $C_p t^{-1}$ whereas the given value was 344 J K^{-1}

	Time/s	Day 1 $10^4\overline{(k_R^{-1})}_{11}$	Day 1 $10^4\overline{(k_R^{-1})}_{11}$	Day 2 $10^4\overline{(k_R^{-1})}_{11}$ corrected	Day 2 $10^4\overline{(k_R^{-1})}_{11}$	Day 3 $10^4\overline{(k_R^{-1})}_{11}$ corrected	Day 3 $10^4\overline{(k_R^{-1})}_{11}$	Day 4 $10^4\overline{(k_R^{-1})}_{11}$ corrected	Day 4 $10^4\overline{(k_R^{-1})}_{11}$	Day 5 $10^4\overline{(k_R^{-1})}_{11}$ corrected	Day 5 $10^4\overline{(k_R^{-1})}_{11}$	Day 6 $10^4\overline{(k_R^{-1})}_{11}$ corrected	Day 6 $10^4\overline{(k_R^{-1})}_{11}$	Day 7 $10^4\overline{(k_R^{-1})}_{11}$ corrected	Day 7 $10^4\overline{(k_R^{-1})}_{11}$	Day 8 $10^4\overline{(k_R^{-1})}_{11}$ corrected	Day 8 $10^4\overline{(k_R^{-1})}_{11}$	$10^4\overline{(k_R^{-1})}_{11}$ corrected	$10^4\overline{(k_R^{-1})}_{11}$
	1800	0·62142		0·62339	0·62176	0·62321	0·62202	0·62234	0·62187	0·62366	0·62232	0·62358	0·62196	0·62303	0·62214	0·62258	0·62197	0·62313	0·62175
	5100	0·62204		0·62262	0·62188	0·62236	0·62180	0·62272	0·62215	0·62359	0·62299	0·62341	0·62284	0·62234	0·62192	0·62241	0·62171	0·62278	0·62237
	8400	0·62147	0·62189	0·62175	0·62145	0·62227	0·62203	0·62245	0·62214	0·62272	0·62244	0·62231	0·62196	0·62256	0·62198	0·62259	0·62227	0·62238	0·62210
	11700	0·62217		0·62216	0·62203	0·62260	0·62250	0·62261	0·62250	0·62309	0·62298	0·62293	0·62282	0·62255	0·62247	0·62277	0·62264	0·62267	0·62256
	15000	0·62208		0·62236		0·62201		0·62283		0·62164		0·62111		0·62159		0·62269		0·62200	
	18300	0·62215		0·62256		0·62170		0·62243		0·62257		0·62285		0·62213		0·62213		0·62234	
	21600	0·62219		0·62129		0·62165		0·62162		0·62193		0·62239		0·62171		0·62139		0·62171	
	24900	0·62115		0·62180		0·62182		0·62297		0·62147		0·62224		0·62180		0·62126		0·62191	
	28200	0·62108	0·62111	0·62154		0·62163		0·62177		0·62171		0·62299		0·62099		0·62157		0·62160	
	31500	0·62099		0·62235		0·62173		0·62137		0·62153		0·62188		0·62124		0·62129		0·62161	
	34800	0·62112		0·62267		0·62164		0·62179		0·62145		0·62226		0·62140		0·62116		0·62175	
	38100	0·62112		0·62193		0·62114		0·62152		0·62153		0·62191		0·62090		0·62141		0·62148	
48200	41400	0·62090		0·62023		0·61999		0·61942		0·62072		0·62389		0·61917		0·62010		0·62050	
	44700	0·62064		0·62284	0·61754	0·62159	0·61660	0·62331	0·61890	0·62213	0·61675	0·62315	0·61717	0·62162	0·61706	0·62297	0·61622	0·62238	0·61718
	48000	0·62108	0·62102	0·62286	0·61989	0·62230	0·62025	0·62165	0·61973	0·62271	0·62271	0·62308	0·62062	0·62160	0·61970	0·62248	0·61995	0·62227	0·62009
	51300	0·62136		0·62122	0·62022	0·62239	0·62143	0·62254	0·62174	0·62182	0·62033	0·62240	0·62135	0·62039	0·62085	0·62141	0·62083	0·62181	0·62085
	54600	0·62096		0·62152	0·62111	0·62092	0·62046	0·62145	0·62107	0·62077	0·62033	0·62146	0·62096	0·62158	0·62121	0·62072	0·62022	0·62120	0·62077
	57900	0·62118		0·61932	0·61908	0·62183	0·62168	0·62152	0·62142	0·62153	0·62136	0·62181	0·62160	0·62128	0·62112	0·62077	0·62055	0·62116	0·62097
heater pulse	61200	0·62075		0·61999	0·61991	0·62081	0·62072	0·62108	0·62098	0·61995	0·61983	0·62184	0·62177	0·62059	0·62050	0·62065	0·62054	0·62070	0·62061
	64500	0·62057		0·62019		0·62119		0·62225		0·62189		0·62108		0·62119		0·62102		0·62127	
	67800	0·62088	0·62085	0·61864		0·62169		0·62132		0·62053		0·62091		0·62058		0·62006		0·62042	
	71100	0·62105		0·61894		0·62028		0·62136		0·62033		0·62134		0·62118		0·62029		0·62054	
	74400	0·62091		0·61943		0·62187		0·62162		0·62111		0·62147		0·61954		0·62038		0·62077	
	77700	0·62085		0·61837		0·62037		0·62008		0·62107		0·62110		0·62100		0·62062		0·62057	
	81000	0·62088		0·62077		0·62154		0·62091		0·62083		0·62096		0·61940		0·62039		0·62069	
36400	84300	0·61933		0·62192		0·62030		0·62146		0·62090		0·62084		0·62162		0·62019		0·62103	
	87600	0·61974	0·62006	0·61964	0·62591	0·62157	0·62684	0·61941	0·62532	0·62108	0·62508	0·62192	0·62532	0·61937	0·62493	0·61879	0·62650	0·62025	0·62569
	90900	0·61986		0·61977	0·62285	0·62064	0·62338	0·61902	0·62184	0·62010	0·62205	0·62054	0·62223	0·62035	0·62354	0·61924	0·62308	0·61995	0·62264
	94200	0·62070		0·62047	0·62143	0·62011	0·62142	0·61964	0·62160	0·62087	0·62180	0·62069	0·62150	0·61962	0·62085	0·61941	0·62016	0·61997	0·62124
	97500	0·62004		0·61810	0·61874	0·62063	0·62124	0·62016	0·62016	0·62067	0·62114	0·62026	0·62069	0·62029	0·62090	0·62009	0·62097	0·62003	0·62063
	100800	0·61986		0·61928	0·62010	0·62002	0·62088	0·62056	0·62056	0·61948	0·62018	0·62102	0·62084	0·61972	0·61999	0·61924	0·61964	0·62007	0·62030
	104100	0·62091		0·62061	0·62000	0·62019	0·62032	0·61985	0·61985	0·62013	0·62022	0·62038	0·62030	0·61967	0·61982	0·62026	0·62047	0·62016	0·62028
	107400	0·61918		0·61985		0·62041		0·62015		0·62001		0·61987		0·61928		0·61972		0·61990	
	110700	0·61938	0·61979	0·61992		0·62091		0·61962		0·62009		0·62008		0·61935		0·61951		0·61990	
	114000	0·61985		0·61939		0·62066		0·61995		0·61984		0·62027		0·61981		0·61975		0·61945	
	117300	0·61957		0·62024		0·61961		0·62031		0·61976		0·61907		0·61969		0·61945		0·61973	
	120600	0·61987		0·61889		0·61972		0·62030		0·62042		0·62041		0·61994		0·61921		0·61985	
	123900	0·62026		0·62003		0·62027		0·61982		0·61964		0·62075		0·61963		0·61910		0·61984	
	127200	0·61985	0·61953	0·61918		0·61954		0·61942		0·62044		0·61938		0·61959		0·61960		0·61966	
	130500	0·61881		0·61878		0·62032		0·62082		0·62090		0·61954		0·61940		0·61903		0·61970	
	133800	0·61981		0·61930		0·62004		0·61969		0·61983		0·61903		0·61864		0·61927		0·61923	
	137100	0·61956		0·61851		0·62013		0·62100		0·61929		0·61958		0·61874		0·61900		0·61986	
	140400	0·61977		0·61926		0·61982		0·61937		0·61928		0·61955		0·61933		0·61881		0·61935	
	143700	0·61947		0·61847		0·61995		0·62064		0·61947		0·61974		0·61882		0·61874		0·61941	
	147000	0·61821	0·61898	0·61713		0·61843		0·61973		0·61975		0·62001		0·61905		0·61897		0·61895	
	150300	0·61946		0·61846		0·61953		0·62052		0·61927		0·61895		0·61893		0·61868		0·61914	
	153600	0·61830		0·61836		0·61835		0·61940		0·62019		0·61911		0·61844		0·61900		0·61898	
	156900	0·61819		0·61757		0·61897		0·62017		0·61975		0·61869		0·61885		0·61890		0·61890	
	160200	0·61897		0·61774		0·61942		0·61948		0·61921		0·61970		0·61853		0·61783		0·61887	
	163500	0·61813	0·61841	0·61751		0·61915		0·61889		0·61944		0·61871		0·61805		0·61806		0·61854	
	166800	0·61921		0·61828		0·61928		0·61952		0·61972		0·61803		0·61857		0·61801		0·61878	
	170100	0·61934		0·61884		0·61949		0·61924		0·61895		0·61914		0·61886		0·61853		0·61891	

2002-07-08

Bury Lodge heading

Dr. M. H. Miles,
Naval Air Warfare Center,
Weapons Division,
China Lake,
California,
U.S.A.

8th July 2002.

Dear Mel,

Many thanks for the text of your IMF 9 paper. Excellent, splendid and I have no comments except that we must do more!

Your earlier FAX of 3/7/2002 got stuck in the memory of my machine which has now been "unstuck" again. I don't know how long it will stay in the "unstuck" state so perhaps it would be best if you could reply c/o Wood and Co, . . . as well as to my own number. The earlier FAX contained the copies of the earlier e-mails about Peter Zimmerman's activities - is that all? Have you written to him or the Navy or the State Department? If so, could I please have copies?

In any event, I would like to have his State Department address and also the name and address of the Secretary of State ditto of the new responsible individuals at the Navy.

I have it in mind to write to the Secretary of State to ask him to ensure that Zimmerman should stop interfering with the publication processes (copies to the Navy). I would also like to commend him to get independent opinions from e.g. Carlo Rubbia, the Head of E.N.F.L, Nobel Laureate etc. etc. Because of this, letters would be better than e-mails.

I know that you must be very busy but I need your help urgently. First of all referring to my previous FAX, I need a good reference for "hot spots". Would "S. Szpak and P.A. Mosier-Boss, Il Nuevo Cimento 112A (1999) 577" do or is there a better reference? Secondly, when I was in Beijing I picked up a copy of the instruction to authors but this has got lost. I have also lost the booklet of Abstracts which contained a reduced scale version of these instructions. Have you by any chance either of these bits of paper and could you FAX them to me? It seems to me that your own paper does not conform to the instructions?

I shall be going to Italy on Friday or Saturday to discuss the next phase of their work, as I have the fluidized bed set up (it is obviously hopeless to try to take this by air). I shall be there for 1 week.

All power to your [??]

Martin

P.S. Our Chinese Colleagues seem to be willing to accept more or less anything I will care to write - also any length. So I will wax ecstatic to celebrate the absence of the Zimmermann effect,

2002-08-30

Bury Lodge heading

30 August 2002

Dr. Melvin H. Miles,
Department of Chemistry,
Bates College,
Lewiston, ME 04240-6028
U. S. A:

Dear Mel,

Welcome to Maine! Having said this, I am sorry you are having such a sweltering arrival which will certainly be balanced up by the winter. Some years ago Sheila and I had a short holiday by Moosehead Lake and it was 36°C (97°F!). Fortunately we had an air-conditioned cat. Sheila said : "why are we spending our days driving these back roads7" I said : "just stick your head out of the window and you will see why".

Incidentally, Moosehead Lake was delightful and we followed this up by a few days on Campobello Island. This was the place where the senior politicians used to spend their summer vacations to get away from the heat in New York and Washington and I can thoroughly recommend it.

I am now sending you the next installation of the calculations needed for the putative paper "Our penultimate on isoperibolic calorimetry". This instalment deals with the comprehensive analysis of a series of 8 measurement cycles for the "blank system" Pt / D_2O. This series was part of a set of ~ 250 measurement cycles carried out in 1995 to validate the ICARUS - 2 systems but the raw data for these 8 cycles are all that I have here in Tisbury. The rest have been "lost". As you will recall, I would have preferred <u>not</u> to use our own data to assess the performance of the ICARUS - 2 system but the folks at the N.H.E. never carried out such a series of calibrations; your own data for the Pd-B-Ce system did not satisfy the conditions required for carrying out the validation (see the earlier correspondence). I will need to write to you again about the complex issues of why I would have preferred not to use our own data.

Fig. 1 gives the results of $\left(\overline{\overline{k_R{}'}}\right)_{11}$, $\left(\overline{k_R{}'}\right)_{21}$ and $\left(\overline{k_R{}'}\right)_{31}$ for the first measurement cycle while Table 1 gives the data required for producing Fig. 1 (this figure is not totally accurate so any publishable version will have to be produced from the tabulation in Table 1). You have seen this type of plot before but it is good to have a further version - the results are in complete accord with the predictions given by the "instrument function" of the calorimeter as will be apparent from the final write-up.

Table 2 gives a summary of results for $(k_R')°_{251}$, $(k_R')°_{261}$, $(k_R')°_{262}$, $(k_R')°_{271}$ and the associated values of C_pM with the regression coefficients required to evaluate the water

equivalents. It also lists the values of $\left(\overline{\overline{k_R{}'}}\right)_{21}$ determined in a time region free from the effects of calibrations. The values in columns 4, 5, 6, 7 and 8 were those specified for the application of the ICARUS – 2 (and also the ICARUS – 1) methodology. You will see the close agreement between the "true" and "lower bound" values which is the basis of our statement that the accuracy of the instrumentation is very nearly equal to the precision. The difference in the values in columns 4 and 5 and those in column 8 is to be expected.

You will see from columns 2 and 3 that it is not necessary to apply the calibrations in order to attain acceptable values in the lower bound heat transfer coefficient, $(k_R')°_{251}$, and of the associated value of C_pM. However as I have already explained previously, we were unable to develop this methodology to give the true heat transfer coefficient, $(k_R')°_{252}$, hence the long saga of the calibrations.

You will also see that columns 11-18 of Table 2 are incomplete and I will send you a completed version in due course. The single set of values shown are, however, sufficient to illustrate the inadequacy of using the values based on a forward integration i.e. those in columns 11-14. As you will recall, I believe the N.H.E. used this methodology to produce their calibrations. If they had used the later sections of the calibrations (as they were told to do), they would have got much closer to the mark - see the values in columns 15-18. However, why bother to do this when one can use the much better methodology illustrated in columns 4-7?

Let me now come to the "failure" of the methodology. You will see that the logical interpretation of Fig. 1 is that the progressive lowering of the level of the electrolyte causes the minor decrease of the heat transfer coefficients with time (incidentally, one could of course make a strong case for regarding the lower bound heat transfer coefficients determined for "blank systems" as estimates of the true heat transfer coefficients; we need to cover this point in further correspondence). This suggests that one should be able to convert the heat transfer coefficients determined in successive cycles to an unique value by using the associated water equivalents. However, as is illustrated by Table 2, the accuracy of the water equivalents (the slopes of the regression lines) is insufficient to allow one to do this. This will explain to you the advent of the "Musical Boxes" which were evidently never put into use. Again, we need to correspond further about this aspect.

This brings me to the paper by Kirk Shanahan. Has this been published, will it be published? Incidentally, he did send me a copy of this paper earlier this year but I threw it away in a fit of irritation. However I will write to him if you think that this would be at all useful but I would suggest that we should reach an agreed version of the text of such a letter by correspondence. Better still, perhaps we should send him a joint letter? If we were to do this, I would need to have copies of all the correspondence which you and Dr. Imam may have had with him. I take it from the material which you have sent to me that his paper is really a response to the Navy Report NRL/MR/6320-01-8526? (Incidentally, I still do not have a copy of any Report which Stan Szpak may have produced nor of the papers submitted to Electrochimica Acta nor of any ensuing correspondence. Could you therefore please also send me any such material so that I can update my files).

I think it would be a serious mistake to regard Shanahan's paper as an attempt to further the understanding of calorimetry: it is really in the nature of a "spoiler". He takes a week paper, sets up a scenario of errors in the calibration (which may or may not be true) and then extends his negative comments to the whole field by innuendo. Incidentally, flow calorimetry is a very poor methodology which should be reserved for large-scale systems showing high levels of excess enthalpy generation. I find it next to impossible to make any connection between Shanahan's paper and the work we have done and I would suggest that we write him a preliminary letter asking him to spell this out. Perhaps we should also immediately address the question of oxygen reduction. Any comment on this would be greatly strengthened by referring to the work of Balashova. Have you the time to carry out a survey - my access to libraries is virtually zero. It is also possible that Spiro may have investigated the oxygen reduction system.

The reason that I regard Shanahan's paper as a "spoiler" is because it falls pretty well into the scenario of activities which have been used before notably by the Tobacco and Sugar industries. Britain's first Professor of Nutrition Science was so effectively rubbished that he never got another research grant! The same methodology is now being used by the Mobile Phone Industries. Perhaps, then, we could also use a preliminary letter to try to establish what resources the D.O.E. may have used to further the study of "Cold Fusion" and what resources may have been devoted to produce the kind of comments contained in Shanahan's paper. Is this information available under the freedom of information act? Do you think Gene Mallove or Jed Rothwell may have surveyed this subject?

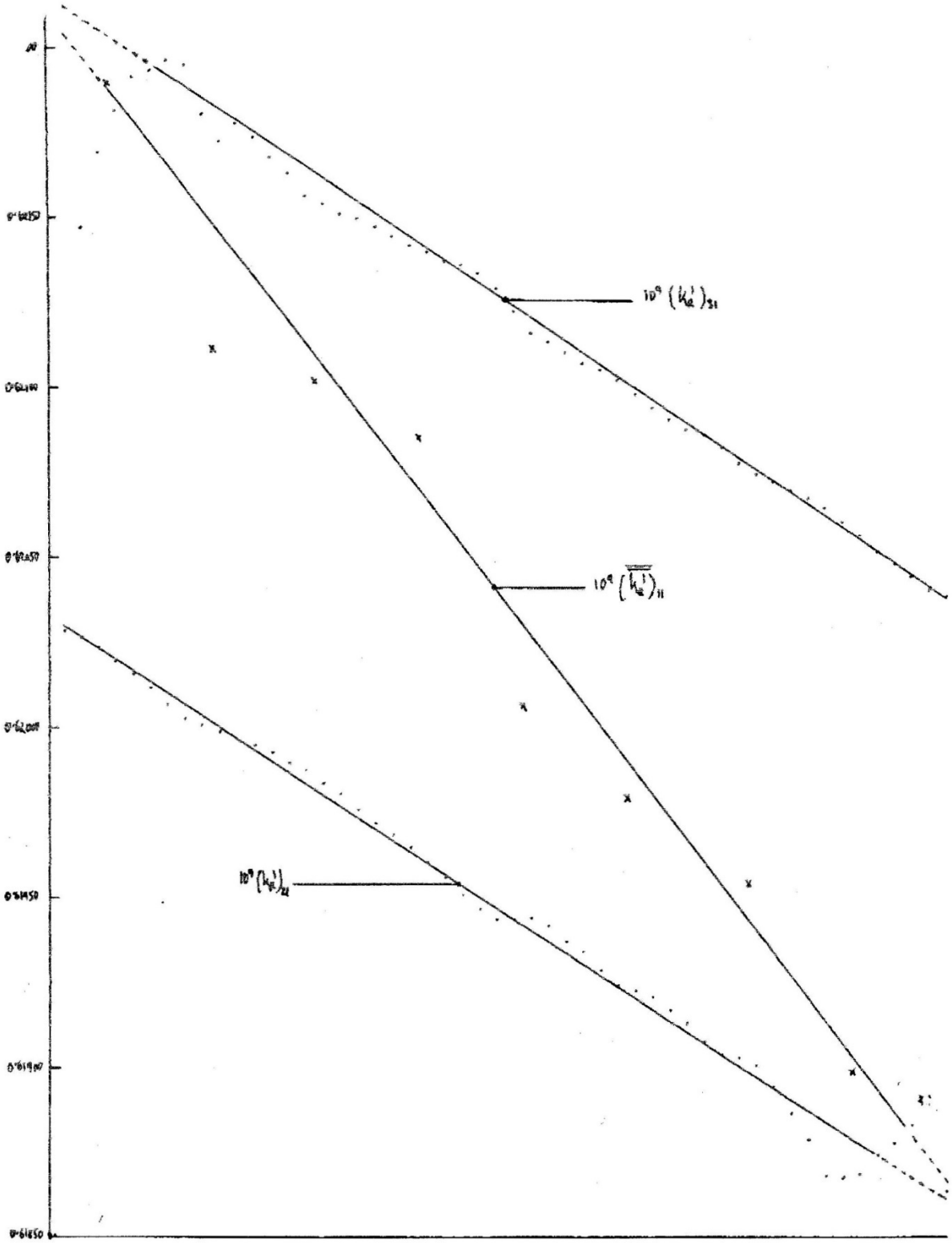
More anon and Best Wishes for your venture at Bates College,

Regards also to Linda. Do either of you like gardening? May I recommend the books of May Sarton [127] to you for getting the flavor of Maine? It will make China Lake seemed rather idyllic!

Martin

[127] JR Eleanore Marie Sarton (1912 – 1995).

Fig 1.



5

Table 1

Pt blank experiment: Day 1. The heat transfer coefficients $(\overline{h_a'})_{11}$, $(\overline{\overline{h_a'}})_{11}$, $(h_a')_{31}$, $(\overline{h_a'})_{31}$, $(h_a')_{21}$ and $(\overline{h_a'})_{21}$

Time/s	$(\overline{h_a'})_{11}$	Time/s	$(\overline{\overline{h_a'}})_{11}$	Time/s	$(h_a')_{31}$	$(\overline{h_a'})_{31}$	Time/s	$(h_a')_{21}$	$(\overline{h_a'})_{21}$
2100	0·62142			2100	0·622289	0·622859	1800	0·62027	0·620285
5400	0·62204			5400	0·62159	0·621474	5100	0·62127	0·620267
8700	0·62147	10200	0·62189	8700	0·62170	0·621689	8400	0·62023	0·620234
12000	0·62117			12000	0·62168	0·621812	11700	0·62019	0·620199
15300	0·62202			15300	0·622204	0·621944	15000	0·62019	0·620151
18600	0·62215			18600	0·62191	0·621931	18300	0·62011	0·620112
21900	0·62219			21900	0·62197	0·621957	21600	0·62007	0·620065
25200	0·62215			25200	0·62197	0·621945	24900	0·62002	0·620024
28500	0·62208	30000	0·62171	28500	0·62176	0·621802	28200	0·62001	0·620007
31800	0·62099			31800	0·62176	0·621722	31500	0·61997	0·619987
35100	0·62012			35100	0·62185	0·621774	34800	0·61995	0·619963
38400	0·62112			38400	0·62172	0·621736	38100	0·61995	0·619947
41700	0·62090			41700	0·62166	0·621672	41400	0·61993	0·619922
45000	0·62064			45000	0·62162	0·621625	44700	0·61988	0·619904
48300	0·62195	49200	0·62102	48300	0·62152	0·621558	48000	0·61987	0·619871
51600	0·62136			51600	0·62155	0·621536	51300	0·61983	0·619835
54900	0·62096			54900	0·62150	0·621505	54600	0·61981	0·619802
58200	0·62118			58200	0·62149	0·621497	57900	0·61975	0·619757
61500	0·62075			61500	0·62144	0·621473	61200	0·61972	0·619718
64800	0·62057			64800	0·62145	0·621439	64500	0·61967	0·619683
68100	0·62088	69100	0·62085	68100	0·62142	0·621415	67800	0·61964	0·619645
71400	0·62115			71400	0·62140	0·621393	71100	0·61960	0·619600
74700	0·62091			74700	0·62137	0·621370	74400	0·61956	0·619557
78000	0·62085			78000	0·62137	0·621361	77700	0·61949	0·619504
81300	0·62088			81300	0·62132	0·621331	81000	0·61947	0·619461
84600	0·61933			84600	0·62128	0·621285	84000	0·61942	0·619451
87900	0·61974	89400	0·62006	87900	0·62121	0·621211	87600	0·61944	0·619435
91200	0·61966			91200	0·62117	0·621154	90900	0·61946	0·619438
94500	0·62070			94500	0·62113	0·621126	94200	0·61941	0·619413
97800	0·62094			97800	0·62112	0·621093	97500	0·61937	0·619369
101100	0·61986			101100	0·62107	0·621066	100800	0·61933	0·619338
104400	0·62091			104400	0·62106	0·621046	103800	0·61928	0·619284
107700	0·61948	109200	0·61974	107700	0·62101	0·621016	107400	0·61922	0·619237
111000	0·61938			111000	0·62096	0·620972	110700	0·61924	0·619222
114300	0·61926			114300	0·62095	0·620934	114000	0·61918	0·619202
117600	0·61957			117600	0·62090	0·620900	117300	0·61918	0·619166
120900	0·61987			120900	0·62086	0·620871	120600	0·61912	0·619128
124200	0·62026			124200	0·62085	0·620850	123900	0·61911	0·619073
127500	0·61905	129000	0·61953	127500	0·62082	0·620813	127200	0·61901	0·619035
130800	0·61881			130800	0·62076	0·620768	130500	0·61905	0·619029
134100	0·61961			134100	0·62079	0·620756	133800	0·61894	0·618992
137400	0·61956			137400	0·61870	0·620711	137100	0·61893	0·618941
140700	0·61977			140700	0·62069	0·620667	140400	0·61882	0·618886
144000	0·61949			144000	0·62066	0·620663	143700	0·61877	0·618786
147300	0·61821	148800	0·61898	147300	0·62064	0·620635	147000	0·61871	0·618777
150600	0·61846			150600	0·62060	0·620595	150300	0·61871	0·618675
153900	0·61830			153900	0·62056	0·620552	153600	0·61867	0·618685
157200	0·61819			157200	0·62058	0·620507	156900	0·61874	0·618741
160500	0·61897			160500	0·62048	0·620471	160200	0·61882	0·618778
163800	0·61813	165300	0·61891 (Spinch)	163800	0·62043	0·620433	163500	0·61916	0·618830
167100	0·61921			167100	0·62041	0·620399	166800	0·61902	0·618968
170400	0·61934			170400	0·62038	0·620375	170100	0·61932	0·619032

Table 2

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
Days	$10^9(k_B^1)^0_{251}$ /W K^{-4} near t=0	GM /J K^{-1} r	$10^9(k_B^1)^0_{261}$ /W K^{-4} near t=t$_2$	GM /J K^{-1} r	$10^9(k_B^1)^0_{262}$ /W K^{-4} near t=t$_2$	GM /J K^{-1} r	$10^9(\overline{k_B^1})_{24}$ /W K^{-4} at 129.005	$10^9(k_B^1)^0_{271}$ /W K^{-4} near t=T	GM /J K^{-1} r	$10^9(k_B^1)^0_{361}$ /W K^{-4} evaluation near t=t$_1$	GM /J K^{-1} r	$10^9(k_B^1)^0_{362}$ /W K^{-4} evaluation near t=t$_1$	GM /J K^{-1} r	$10^9(k_B^1)^0_{361}$ /W K^{-4} evaluation near t=t$_2$	GM /J K^{-1} r	$10^9(k_B^1)^0_{361}$ /W K^{-4} evaluation near t=t$_2$	GM /J K^{-1} r
1 and 2	0·62150	-349·8 -0·99961					0·614035										
3 and 4	0·62179	-334·6 -0·99955	0·61953	-349·6 -0·99975	0·61960	-347·4 -0·99937	0·618326	0·61984	-322·5 -0·99993								
5 and 6	0·62177	-348·2 -0·99961	0·62105	-349·2 -0·99996	0·62124	-339·1 -0·99993	0·619428	0·61976	-327·7 -0·99999								
7 and 8	0·62205	-349·6 -0·99987	0·62123	-349·7 -0·99993	0·62111	-349·2 -0·99992	0·619760	0·61936	-326·8 -0·99998								
9 and 10	0·62192	-356·9 -0·99958	0·62087	-349·9 -0·99993	0·62085	-349·7 -0·99994	0·619539	0·61977	-330·0 -0·99999								
11 and 12	0·62207	-355·6 -0·99980	0·62135	-348·8 -0·99990	0·62133	-339·7 -0·99991	0·619157	0·61951	-329·7 -0·99998								
13 and 14	0·62172	-362·3 -0·99950	0·62071	-352·5 -0·99997	0·62061	-336·6 -0·99997	0·618700	0·61913	-325·0 -0·99997								
15 and 16	0·62172	-349·3 -0·99964	0·62065	-349·0 -0·99990	0·62069	-338·9 -0·99994	0·619502	0·61885	-321·8 -0·99997	0·61047	-4086 -0·10565	-0·33424	-5807 -0·18163	0·62094	-329·9 -0·99998	0·61872	-332·6 -0·99964

2002-09-08

Bury Lodge heading

8th September 2002.

Dr. Melvin Miles,
Department of Chemistry,
Bales College,
Lewiston, M.E. 04240-6028
FAX 0011-207-786-8336.

Dear Mel,

Greetings once again and you will see that I am returning to my former self! Your classes must now have started so I dare say that you will be very busy. However, let me ask you whether you received my FAX of 30/08/02? Perhaps you could confirm this as our ongoing debate would be fairly meaningless if this FAX has gone astray. As you will see, I am once again compelled to use the FAX at Wood & Co. . . . So the possibilities for letters to go astray have multiplied!

I am now sending you a revised Table 2 (for the proposed new paper on calorimetry). You will see that I have completed the calculations of $(k_R')°_{361}$ and $(k_R')°_{362}$ and the associated values of C_pM (the data are given in Columns 11-18). It was pointed out in the handbook accompanying the ICARUS-1 System that the evaluations as in Columns 11-14 were especially unreliable although $(k_R')°_{361}$ got somewhere near the mark. We will have to illustrate why this should be the case (in the paper) and it will be necessary to state categorically that N.H.E. used this particular procedure in attempts to calibrate the cells. The procedure leading to the results in Columns 15-18 was not illustrated in the handbook as these procedures are in any event subject to ambiguities. Instead, the procedure leading to Column 19 was recommended - to give a rough value of the true heat transfer coefficient. This is why I have added this extra column to Table 2. However, the core of the methodology was that leading to the data in Columns 4-7. Apart from anything else, the procedure was mathematically sound and gave the true heat coefficient at the mid-point of the measurement cycles (which is the most useful value).

Note the close correspondence between the values in Columns 4 and 6 which shows pretty conclusively that the gas evolution at the electrodes must degas the solution in the boundary layers (otherwise the true heat transfer coefficients would exceed the lower bound values by 0.00170 or 0.00057 WK^{-4} depending on whether both the hydrogen and oxygen are oxidized and reduced at the anode and cathode - or whether this side reaction is confined to oxygen reduction at the cathode - a more likely possibility since hydrogen is not oxidized at oxide coated platinum electrodes). Such shifts should be measured but are evidently not present as shown by the data in Columns 4 and 6 of table 2.

As you will see, I have now recovered the position we had reached in 1992/93 which was the basis of the design of the ICARUS 1 System. It is all a monumental waste of time!

This brings me again to Kirk Shanahan's paper. As I said in my FAX of 30/8/02 it would be a mistake to regard this as an attempt to further the understanding of calorimetry. You certainly cannot tell from the paper alone as to what his real intent may have been although the list of acknowledgments is highly suggestive. The paper is all bog-standard and I believe that this type of analysis can be found in all the standard textbooks - it may even be in the brilliant book "Numerical Recipes". So let me wish unto you some further action. Do you have any colleagues at Bates College who are interested in statistical analysis of data and/or who may have some useful textbooks? If so, could you try to check-up what the errors of the statistical estimates might be? Failing this, could you ask for Mike Melich's advice? You could tell him that I believe that Kirk Shanahan has simply applied a routine analysis to Ed Storms' data.

The reason I believe this is all bog-standard material is because we went beyond this type of analysis in 1989/90. As you may recall, we used non-linear regression analysis and, actually, eventually derived the likely errors of all the derived quantities. The reason why we did not persist with this type of analysis is because the effects were all negligibly small (as might be expected in view of the very high precision and accuracy of the experiments). Furthermore, we could not make this type of analysis user-friendly with the computing power then available to us. Of course, if you introduce arbitrary shifts into the experiments (the errors referred to by Kirk Shanahan) then all is pretty well lost - you cannot devise calibration procedures which will detect such arbitrary shifts. The reasons are that the vectors are parallel; even if they are nearly parallel, the fitting will have to be carried out in the rather broad valleys in the parameter space. I had some major difficulties in 1989 with the definitions of the parameter spaces to try to ensure something approaching orthogonality of the vectors.

Incidentally, you may note that the fitting procedures we used in 1989 always included a calibration - we simply did not use a predetermined heat transfer coefficient as seems to be alleged by Kirk Shanahan. We have continued to use this approach since then although one cannot always stick to this procedure.

I think that the supporting documents you have sent me make Kirk Shanahan's true intent more clear. The real conclusion he should have drawn from his reanalysis of Ed Storms' data is that flow calorimetry is an unreliable methodology as we ourselves discovered (see the draft of my letter to Kirk Shanahan). What he has done is simply to drop a "tank-trap" into the investigation, a trap which one simply cannot circumvent. One needs to ask: why should there be such large shifts in the calibration constants? What would be the mechanism(s) of such shifts? We should bear in mind that the heat transfer coefficients are close to the value one can calculate from the Stefan-Boltzmann coefficient and the radiant surface area. Furthermore, one can show that the errors due to lumping the conductive pathways into the pseudo-radiative heat transfer coefficient are negligibly small.

It seems to me therefore, that Kirk Shanahan's paper is just the first step in a procedure which is likely to develop into a useless slanging match designed to justify the inaction of the

agencies such as the D.O.E. One must therefore ask: should one respond to such a provocation or should one simply ignore the paper? (while at the same time assembling the information needed to justify this attitude). I am sending you the enclosed draft of a letter to Kirk Shanahan as a first step in this overall procedure. Could you please consider the text, decide which parts should be sent, which parts should be deleted, which additional points should be made in such a first letter and, indeed, send me your general comments.

Regards,

Martin

TABLE 2

			ICARUS METHODOLOGY															METHOD
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
Days	$10^9(h_a^1)^0_{251}$ /WK^{-1} near t=0	Cp M /JK^{-1} r	$10^9(h_a^1)^0_{261}$ /WK^{-1} near t=t₁	Cp M /JK^{-1} r	$10^9(h_a^1)^0_{262}$ /WK^{-1} near t=t₂	Cp M /JK^{-1} r	$10^9(\overline{h_a^1})_{21}$ /WK^{-1} at 109,105	$10^9(h_a^1)^0_{271}$ /WK^{-1} near t=T	Cp M /JK^{-1} r	$10^9(h_a^1)^0_{361}$ /WK^{-1} evaluation near t=t₁	Cp M /JK^{-1} r	$10^9(h_a^1)^0_{362}$ /WK^{-1} evaluation near t=t₁	Cp M /JK^{-1} r	$10^9(h_a^1)^0_{361}$ /WK^{-1} evaluation near t=t₂	Cp M /JK^{-1} r	$10^9(h_a^1)^0_{362}$ /WK^{-1} evaluation near t=t₂	Cp M /JK^{-1} r	$10^9(h_a^1)_{22}$ /WK^{-1} at t=t₂
Land 2	0·62150	-349·8 -0·99961					0·61904											
3 and 4	0·62179	-331·6 -0·99955	0·61853	-349·6 -0·99955	0·61960	-347·4 -0·99977	0·61833	0·61884	-322·5 -0·99997	0·63367	-2324 -0·98286	0·81320	-2814 -0·99827	0·62032	-3246 -0·99841	0·62381	-323·4 -0·99914	0·61886
5 and 6	0·62177	-343·2 -0·99981	0·62115	-340·2 -0·99991	0·62124	-339·1 -0·99993	0·61943	0·61976	-327·7 -0·99999	0·62311	-310·2 -0·99282	0·70098	-310·9 -0·99970	0·62094	-341·1 -0·99927	0·62017	-340·6 -0·99991	0·62028
7 and 8	0·62185	-349·6 -0·99937	0·62133	-346·7 -0·99993	0·62111	-340·2 -0·99992	0·61979	0·61916	-326·8 -0·99998	0·64828	-2144 -0·28512	1·15002	-1314·5 -0·96900	0·62026	-347·9 -0·99782	0·61934	-3975 -0·99819	0·62063
9 and 10	0·62192	-356·9 -0·99838	0·62077	-341·1 -0·99993	0·62086	-341·1 -0·99994	0·61958	0·61977	-334·0 -0·99999	0·62242	-341·2 -0·98504	0·53265	-363·3 -0·99973	0·62099	-333·7 -0·99951	0·62131	-339·4 -0·99952	0·62102
11 and 12	0·62207	-355·6 -0·99920	0·62135	-339·8 -0·99990	0·62133	-339·7 -0·99981	0·61916	0·61951	-329·7 -0·99958	0·63371	-273·5 -0·96670	0·99099	-262·5 -0·99231	0·62044	-335·7 -0·99921	0·61925	-335·0 -0·99933	0·62094
13 and 14	0·62172	-362·3 -0·99950	0·62071	-337·5 -0·99999	0·62101	-336·6 -0·99994	0·61874	0·61915	-325·0 -0·99997	0·64916	-2052 -0·96039	0·99681	-251·0 -0·98956	0·62041	-328·9 -0·99918	0·62007	-320·0 -0·99802	0·61904
15 and 16	0·62172	-398·3 -0·99964	0·62065	-339·0 -0·99990	0·62064	-338·9 -0·99994	0·61850	0·61885	-326·8 -0·99907	0·61947	-403·6 -0·90585	-0·33924	-530·7 -0·98163	0·62044	-329·1 -0·99938	0·61872	-332·6 -0·99964	0·61734

Bury Lodge heading

First Draft

Dr. Kirk L. Shanahan,
Westinghouse Savannah River Company,
Savannah River Technology centre,
Aiken, SC 29808,
U.S.A.

DRAFT

Dear Dr. Shanahan,

You may recall that earlier this year you sent me a copy of your paper "A Systematic Error in Mass Flow Calorimetry Demonstrated". (at least, I believe it was you who sent me this paper but it may well have been somebody else). The argument you developed seemed to me to be fairly standard but I simply could not make any connection between your paper and the work which we have carried out, so I put your paper aside. I regret to say that I have now lost this copy. However, recently, Dr. Melvin Miles (now at the Department of Chemistry. Bates College, Lewiston, ME 04240-6028) sent me a further copy of this paper together with copies of some ensuing correspondence and Mel asked whether I might wish to write to you? It is this ensuing correspondence which illustrates that you have developed some rather strange views and, indeed that you have misinterpreted the literature. I have therefore decided to try to open up a correspondence with you but as a first step I need to ask you whether my documentation is complete and whether you would wish to send me any additional material? It would be best if you could send me any such additional material by Air Mail as my Fax is presently non-operational. If you wish to use the Fax, then you could use the instrument at Wood & Co, who are in our village and are used to receiving Fax messages addressed to me. Their number is . . .

The copies of the correspondence I have here in Tisbury consists of

A letter from you two Dr. Imam dated 20/06/02

A letter from Mel Miles to Dr. Imam dated 12/07/02 (it appears from the correspondence that Dr. Imam sent this letter on to you)

A further letter from you to Dr. Imam dated 12/08/02

is this documentation adequate/complete?

To start the ball rolling, let me make some initial comments. You could have concluded from your paper that mass flow calorimetry is inherently inaccurate which is a conclusion which we reached on three separate occasions in our early work. What we found was that we could always

calibrate the systems but that the way these calibrations changed with the system parameters was not logical (as we said at the time; this defies the laws of Physics). In effect, we could not define a useful "instrument function" which, I believe, is always a necessary first step in any new investigation. We came to believe that the flow calorimeters we were using were governed by "dispersive plug flow" and moreover, hovered uneasily in the transitional region between "laminar flow" and "fully developed turbulence". Random shifts in their behavior were therefore to be expected.

This behaviour was a factor in persuading us to develop isoperibolic calorimetry. Isoperibolic calorimeters are "well stirred tanks" (in the parlance of Chemical Engineering). In fact, they are extremely well-stirred tanks. The radial and axial mixing times for the calorimeters in use at that time (as determined by tracer experiments) were 3 and 20 s respectively whereas the thermal relaxation time was of order 3,000 s. There simply was no mechanism for generating thermal inhomogeneities as was confirmed with a system of 8 thermistors racked through the cell. They were admittedly temperature rises adjacent to the electrode surfaces i.e. within the Prandtl boundary layers but the extent of these layers was negligibly small compared to the volume of the electrolyte in the cells.

As I have said: you could have concluded that mass flow calorimetry is unreliable (we decided that it should be reserved for large-scale experiments). You have not done this but instead have tried to extend your arguments to other forms of calorimetry. It is here that you appear to have misinterpreted the literature. In our early work we always included a calibration in any evaluation of the heat generated in the calorimeter i.e. we did not rely on a global calibration as you appear to believe. However, there are pitfalls in this procedure. If there are variable sources of excess enthalpy in the calorimeter, then it becomes impossible to calibrate the system: one simply concludes that the system has large errors. In fact, it is necessary to conduct extensive series of "blank experiments" to validate the systems (which is a matter which occupied us extensively at several stages of the investigation). We decided eventually that the polarization of platinum cathodes in dilute lithium deuteroxide in D_2O gave a suitable blank system.

We have continued to use the repeated calibration of the system in our work since these early days. You appear to believe that there can be marked changes in the calibration of the calorimeters from experiment to experiment. How and why should such changes take place? You might like to bear in mind that the pseudo-radiative heat transfer coefficients which one can deduce from the calibrations are close to those which one can calculate from the Stefan-Boltzmann coefficient and the radiative surface area; the effect of conductance paths (e.g. those due to lead wires) is negligibly small. (This is true for the calorimeters which we used subsequent to our initial investigation; the early versions had appreciable conductance paths due to inadequate evacuation of the Dewar Cells).

You have evidently realised that we used at non-linear regression analysis to derive the data given in our first comprehensive paper. As part of these analyses we derived the errors for all the estimated parameters (this is, in fact, a standard procedure - is this the matter you refer to in your paper?). These errors were negligibly small as might be expected bearing in mind the very high

precision and accuracy of this type of calorimetry (incidentally, I do not understand the point you have made about S/N: could you please explain this more fully?).

Our colleagues in Japan wanted to develop an independent study using inter alia calorimetric systems supplied by us. We therefore developed the ICARUS-Systems (Isoperibolic Calorimetric Research and Utilities Systems) which used a simplified method of data analysis relying on linear regression (in any event, we described the non-linear regression method as being in the nature of "gilding the lily"). It is this linear regression methodology which is the mainstay of your argument. However, the only use we make of this method is in the evaluation of the water equivalents of the cells. If we avoid regions in time where there are rapid changes in the temperature, then the effects of any uncertainties in the water equivalents on the thermal balance is our negligibly small.

It is, of course, possible to attribute changes in the thermal balances to changes in the calibration such that the vectors representing these changes are parallel in the parameter space. It is then impossible to separate these changes by any method of calibration: one has to rely on global calibrations. It is therefore essential for you to spell out why such global calibrations might change with time or from experiment to experiment.

I would also like you to spell out the mechanism(s) by which oxygen might reach the cathodes and hydrogen might reach the anodes so as to generate combustion at these services. Extensive research in electrochemistry, catalysis and chemical engineering has established that this can only be by diffusion (at least this is the current belief). The solubility of most gases in water or diluted electrolyte solutions is $\sim 10^{-3}$M but the solubility of oxygen will be reduced to 1/3 of this value and hydrogen to 2/3 in view of the partial pressure of the gases in the head spaces. The mass transfer coefficient of these gases will be $\sim 10^{-2}$cms^{-1} for such gas evolving electrodes. In turn this shows that the true heat transfer coefficients will exceed the lower bound values by 0.00170 or 0.00057 x 10^{-9}WK^{-4} for the type of electrodes which we have been investigating depending on whether both hydrogen and oxygen are oxidized and reduced or whether such a side reaction is confined to oxygen reduction (it has been established that hydrogen oxidation does not take place at oxide coated platinum electrodes). However, the difference between the lower bound and the true values of the heat transfer coefficients are much less than this which has been explained by us as being due to degassing of the solution in the spaces adjacent to the electrodes.

You also appear to have some difficulties in accepting the results of measurements in the rates of gas evolution. This is an extremely simple experiment which can be carried out in a number of ways. It is only necessary to correct the volumes observed by the barometric pressure and to take account of the vapour pressures of D_2O in the cells. Our own measurements showed that the volumes of gases evolved were within 1% of those calculated from the Faradaic current and the schedule of editions of D_2O also agreed with ~100% Faradaic efficiency. Perhaps then you could set out for me why you disagree with such measurements and, more importantly, why you believe that the gases can reach the electrodes by processes which are faster than diffusion?

It may well be that we wish to develop this correspondence?

Yours sincerely,

2002-09-09

Bury Lodge heading

9 September 2002

Dr. Melvin Miles,
Department of Chemistry,
Bates College,
Lewiston, ME 04240-6028,
U.S.A.

Dear Mel,

On checking through the correspondence, I find that I have missed a most important point in the draft of the letter to Kirk Shanahan. (Perhaps the single most important point!). I have therefore now revised this letter which I am sending you again - please dispose of the previous copy.

I have also written to Mike McKubre to ask for his advice (mainly because of his interest in mass flow-calorimetry). I find that I have lost Mike's Fax number. Do you have it and could you send it to me a.s.a.p.? Perhaps you could 'phone me?

Regards,

Martin

Bury Lodge heading

9 September 2002

Dr. Melvin Miles,
Department of Chemistry,
Bates College,
Lewiston, ME 04240-6028,
U.S.A.

Dear Mel,

Herewith a further letter in response to your Air-Mail letter from Lewiston on 3 September 2002. You will have gathered that I did, indeed, receive the full 9 pages you sent to Wood & Co.

I see that you have now rather changed your mind about the wisdom of writing to Kirk Shanahan (your hand written footnote on the letter in the letter package). I must say that I have considerable reservations about entering into a correspondence with him but I will nevertheless be very interested to have your comments on my draft letter. Instead, if his paper gets published (as no doubt it will do) we could use it as a platform for carrying out a hatchet job at ICCF10 - I have much more material for such a hatchet job!

I have also written to Mike McKubre to ask him for his comments. It may well be that you will wish to 'phone him to have a discussion?

Thanks for your comment about retirement next year. A revised project on Cold Fusion is due to go ahead in Italy later this month. I will be going there and I will see what the situation might be as regards to overseas scientists, timing, funding etc. I am really beginning to feel rather ancient and want to restrict my inputs! Just recently I wrote an historical account to try to explain why I have made so little progress - it seems to me that this was not very well received. In due course, I will send you an abbreviated version.

Regards,

Martin

P.S. Don't forget that I want to have a copy of the co-deposition papers sent to Electrochemica Acta, and of the ensuing correspondence.

Dr. Kirk L. Shanahan,
Westinghouse Savannah River Company,

Savannah River Technology centre,
Aiken, SC 29808,
U.S.A.

DRAFT to Dr. Mel Miles

Dear Dr. Shanahan,

You may recall that earlier this year you sent me a copy of your paper "A Systematic Error in Mass Flow Calorimetry Demonstrated". (at least, I believe it was you who sent me this paper but it may well have been somebody else). The argument you developed seemed to me to be fairly standard but I simply could not make any connection between your paper and the work which we have carried out. Certainly, as far as isoperibolic calorimetry is concerned, you appear to have missed a most important point. Statistics alone does not tell you whether you have adopted a sensible data processing strategy - it simply gives you an answer based on whatever the data and assumptions you plug into the relevant procedure. Thus in isoperibolic calorimetry one can choose to use the differential or the integral heat transfer coefficients and, as far as the latter are concerned, one can use forward or backward integration and apply the methodology to different parts of the measurement cycle. We had spent a considerable amount of time and effort to demonstrate that one should use the interval heat transfer coefficients based on backward integration applied to the time region of the calibration pulse to achieve precise and accurate evaluations.

As I have said, you appear to of missed this particular point so I put your paper aside (and I regret to say that I have now lost this copy). I could see no point in entering into a discussion with you as I felt sure that this would degenerate into some kind of slanging match. However, recently, Dr. Melvin Miles (now at the Department of Chemistry, Bates College, Lewiston, ME 04240-6028) sent me a further copy of this paper together with copies of some ensuing correspondence and Mel asked whether I might wish to write to you? It is this ensuing correspondence which illustrates that you have developed some rather strange views and, indeed that you have misinterpreted the literature. I have therefore decided to try to open up a correspondence with you but as a first step I need to ask you whether my documentation is complete and whether you would wish to send me any additional material? It would be best if you could send me any such additional material by Air Mail as my Fax is presently non-operational. If you wish to use the Fax, then you could use the instrument at Wood & Co, who are in our village and are used to receiving Fax messages addressed to me. Their number is . . .

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A further letter from you to Dr. Imam dated 12/08/02

is this documentation adequate/complete?

To start the ball rolling . . .

[JR The rest of this letter is identical to the previous draft]

2002-09-23

Bury Lodge heading

Your FAX has just arrived. I am drafting a letter to Elton Cairns.

23/09/02

Dr. Melvin Miles,
Department of Chemistry,
Bates College,
Lewiston, ME 04240-6028,
U.S.A.

Dear Mel,

I am now sending you a further revision of Table 2 which contains the values of $(k_R')_1$ and $(k_R')_2$ in columns 2 and 3. In consequence, all the other columns have been moved up by two notches. The Table is now so extensive that I have had to produce it in two parts. However, it seems to me that the final version (for the putative paper "Our Penultimate Paper on Isoperibolic Calorimetry" - that is if we decide to use this Table!) will be quite legible on the A4 format. Incidentally, the plots of the raw data used to derive $(k_R')_1$ and $(k_R')2$ were produced in A2 format for no particular reason except that it was not necessary to rescale the raw data. (However, we must note that this is a non-standard procedure).

Could you please destroy the earlier versions of Table 2 - that is unless you want to keep them as evidence of my style of working!

You will have noticed that $(k_R')_1$ and $(k_R')_2$ were missing from these earlier versions. The derived data illustrate once again that $(k_R')_1$ and $(k_R')_2$ are the least precise and accurate values of the heat transfer coefficients which we can derive. Of course, we can raise the precision and accuracy by taking the means of such derived data and I have shown these means. It is very satisfactory that these means are within the limits which one would predict from the means of $(k_R')°_{261}$ and $(k_R')°_{262}$ shown in columns 6 and 8 (i.e. within 0.01% as set by the temperature measurements: quantisation ± 0.001°C.

Presumably the paper "Our Penultimate------Calorimetry" should contain one plot of the raw data also showing how $(k_R')_1$ and $(k_R')_2$ were derived. These plots illustrate very clearly that one needs to use 48 hour long measurement cycles not the 24 hour cycles which were substituted by N.H.E. Do we also need to produce plots illustrating the derivation of the values in columns 4 - 15? If so, how many - or could we rely on the evidence in TR 1862? The one missing element is the calculation of the differential rates of excess enthalpy generation which can be conveniently summarised on the plots of the upper and lower tail distributions as in Fig. 24 of my paper to ICCF 9. (I am sending you a copy of this paper under separate cover). Such plots were an essential intermediate in moving from the differential to the integral heat transfer coefficients but their use has never been discussed (although they were illustrated in the Poster I gave at ICCF 7 devoted mainly to the sins of N.H.E).

I am also sending you a copy of a Table 3 showing the calculation of the thermal balances based on the backward integration of the data sets. You will see that the values of the heat transfer coefficients used in this calculation i.e. $(k_R')_{262}$ are lower by 10^9 x 0.00090 WK^{-4} than the values shown in column 8 of Table 1. The reason is straightforward: the extrapolation used in deriving $(k_R')°_{262}$ produces the differential heat transfer coefficient at $t = t_2$. As Fig. 1 shows, this value is larger by 10^9 x 0.00090 WK^{-4} than the values of 10^9 $(k_R')_{22}$ which we need to use in the calculation of the thermal balances (cf. Fig. 1).

You will see that the "excess enthalpies" or the "equivalent excess rates" shown in columns 6 and 7 of Table 3 are negative!! Presumably such negative values are due to the inadequacies of the calculation (e.g. the use of a 300s sampling interval, the use of the trapezium rule to represent the integrals, the use of the single central value of $(k_R')22$ in the calculation etc. etc.). However, as far as Kirk Shanahan is concerned (and all his predecessors - and, no doubt followers there is absolutely no evidence for the "combustion" of the deuterium and hydrogen in the cell. However, please see the P.P.S.

One additional point: these measurements used on ICARUS - 2 set up and an ICARUS - 2 cell having an extended area of silvering in the Dewar. The expected heat transfer coefficients for this cell lie in the range 10^9 x 0.610 < (k_R') < 10^9 x 0.630 WK^{-4} based on the area of silvering and the Stefan - Boltzmann coefficient. (I believe the values puzzled you?)

I received a package from Stan Szpak containing the TR 1862 (congratulations to Stan and Pam for their sterling effort), the paper submitted to Electrochimica Acta, the referees' comments and a very interesting letter from Stan. The position with regard to Electrochimica Acta doesn't surprise me in the least. I believe that I told you about my earlier experience with that Journal? I was persuaded to take part in a meeting on the understanding that the papers presented would be published in the Journal (I was in France at the time). In the event I heard next to nothing about the progress of the publication and when I did hear, the dates on the letters didn't match the dates on the Faxes received. Some letters were undated. In the end I was told that my paper was too late to be considered for the publication! Can you credit such chicanery? No matter, the paper was subsequently published in French in the Journal de Chimie Physique (there is a lot more to this story which I can tell you about when we next meet).

The refereeing of the paper is truly disgraceful (perhaps the worst I have ever seen) and I do intend to write to Elton Cairns about this. I will first send you and Stan a draft because both of you may wish to make further comments? As a first step, I want to consider the text of the paper in the light of the referee's comments because it is evident that the paper is more extensive than that with the same title which you sent me some time ago (was that a Conference Paper?). It seems clear to me that I will have to ask for your and Stan's help in obtaining copies of some papers. As I have explained to you on other occasions, it is now very difficult for me to use the libraries.

There are three problems which are currently "making the rounds". The first is any follow-up on Kirk Shanahan's paper; the second is the further analysis of the Pd rods which were part of

the early search for ^{4}He generation (the paper by Morrey et al); the third concerns the attitude of the D.O.E.

As far as the first topic is concerned, Mike McKubre has given me some very interesting information. This includes the fact that Shanahan has some responsibility for examining the loading of D and T into metals. Did I ever tell you that Stan Pons and I tried to get information about this work but found that it was classified. We could not help wondering whether this was because the parts of the work might reveal the generation of T. This was one factor in our wish to spend 1989-91 in a National Laboratory.

As regards the second problem, John Lupton (now at the Pacific Marine Environmental Laboratory in Oregon) wrote to me fairly recently to ask whether he could send the Pd samples used in the Morrey study to Brian Clark at McMaster University who would conduct further analyses. I wrote a fairly unenthusiastic reply but could not reach John Lupton. I then heard from Mike McKubre that Brian Clarke had died fairly suddenly. I shall shortly write again to John Lupton to ask him to return the samples to me together with all relevant documentation. These is a great deal hidden in these samples including evidence for photofission processes. The participants in the Morrey study did not know about this. I tried to tell John Huizenga about it in 1990 (at the meeting in Salt Lake City) but he just used this as an opportunity to trash the whole field.

As regards the third topic, I will quote from Stan's letter of 13. 09. 02: "On the positive side, we note renewed interest, at least in the D.O.E. Last Friday we had visitors from the D.O.E. who wanted to learn more about the C.F. They acknowledged that perhaps the conclusion reached by the D.O.E. panel in Sept. 89 was incorrect. However, to overcome it would be difficult before retirement of people that made the initial statements simply because they will not change their position".

I have a number of observations on this summary of Stan's contacts with members of the D.O.E. In the first place, I don't believe what they told Stan although I think that what they told him is related to their attitude. I always ask myself: how are they going to get out of the silly position they adopted? My conclusion was that they would wait until Stan Pons and I (and as many of the other early participants reporting positive results as possible) had had joined the "Faculty Meeting in the Sky". They would then reactivate the project and say that the early work was all quite wrong and just a lucky guess. (This is why the TR 1862 is such a nuisance for them: does this explain the actions of Zimmerman? Are these reactions just bluster designed to distract attention from the content of the Report?)

Secondly I cannot credit their attitude. Do they really want to make research subservient to some Political Agenda?

Thirdly, they are hardly free agents. If the view that C.F. was correct as described gains credence outside the U.S. then they will have to tag along and that would lead to all manner of awkward questions.

Fourthly, we have to take note of Stan's contact with the D.O.E. and I have written to tell him a little more about the background which led to the start of this project.

One matter is quite clear: you, Stan, Mike McKubre and I have complementary information about different aspects of the various problems. It would therefore be useful and sensible if we could maintain a four-way open discussion between the four of us. At the same time, some of the information and conclusions are rather "sensitive". I have therefore suggested to Stan (and not, as yet, to Mike) that we should use you as a Post Box. I have in mind that if you think that some parts of the various letters should not be circulated, then you should first seek advice from A.N.Others.

I wonder what you will think about all this,

Regards,

Martin

P.S. Were you not allowed to nominate some referees for the paper? If so, then could you please let me have their names?

P.P.S. I was rather unhappy about the outcome of my attempts to make the thermal balances as in columns 6 and 7 of Table 3. The reason was that the calculations of the lower bound and true heat transfer coefficients imply that there is a small positive excess enthalpy generation yet columns 6 and 7 show negative terms. Furthermore, we should be able to make such balances correct to within 10^{-5} – 10^{-4} parts of the thermal input, say to within 1.45 - 14.5 Joules. The reason is that the process of integration is somewhat analogous to "bit toggling" (which increases the precision of estimates made with any system above the level indicated by the Quantisation of the signal). I have not discussed this hitherto because I thought that any such discussion would simply increase the level of incomprehension trotted out by our critics.

You should regard my attempts to explain away the negative values of the excess enthalpies as so much "whistling in the wind - an illustration that one can explain away any effect if one tries hard enough!

The real explanation came to me when lying in the bath (my favourite occupation). I had used the values of the true heat transfer coefficient at $t = t_2$ whereas I should have used the values near $t = 0$. Using the result true $\simeq$ lower bound heat transfer coefficient and the results shown in Fig. 1 we derive the values of the true heat transfer coefficients in Column 8 of Table 3, hence the net total of the excess enthalpy generation in Column 10 or the equivalent rates in Column 11.

The magnitudes of the excess rates shown in Column 11 are at the level which one would expect for the reduction of oxygen at the cathode. My earlier musings on the degassing of the solution by the evolving deuterium in the boundary layer were incorrect (and based on incomplete evaluations of the data). We will have to grasp the nettle of explaining the construction of such thermal balances (I.C.C.F. 10 ?) and with this try to lay the ghost of the "combustion" of the gases. Should I modify my proposed letter to Kirk Shanahan? Note that the

rates in Column 11 are ~0.5% of the cell current and such rates would never have been detected by measurements of the volumes of the gases evolved. Could you please let me have a listing of Steve Jones pronouncements on this topic and your replies (copies of papers would be better).

TABLE 2 Summary of some important values of the heat transfer coefficients.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
	← ICARUS METHODOLOGY →				← ICARUS METHODOLOGY →										
Days	$10^9(k_e^{i})_1$ /WK^{-4} heat $t=t_2$	$10^9(k_e^{i})_2$ /WK^{-4} heat $t=t_2$	$10^9(k_e^{i})^0_{251}$ /WK^{-4} heat $t=0$	GM /JK^{-1} r	$10^9(k_e^{i})^0_{261}$ /WK^{-4} heat $t=t_2$	GM /JK^{-1} r	$10^9(k_e^{i})^0_{262}$ /WK^{-4} heat $t=t_2$	GM /JK^{-1} r	$10^9(k_e^{i})_{12}$ /WK^{-4} at 129,000	$10^9(k_e^{i})^0_{22}$ /WK^{-4} heat $t=T$	GM /JK^{-1} r	$10^9(k_e^{i})^0_{361}$ /WK^{-4} evaluation heat $t=t_1$	GM /JK^{-1} r	$10^9(k_e^{i})^0_{362}$ /WK^{-4} evaluation heat $t=t_1$	GM /JK^{-1} r
1 and 2			0·62150	-349·8 -0·99961					0·619035						
3 and 4	0·61913	0·61706	0·62179	-349·6 -0·99955	0·61953	-349·6 -0·99975	0·61960	-349·4 -0·99977	0·618326	0·61385	-322·5 -0·99993	0·63367	-282·1 -0·98786	0·81320	-281·4 -0·99827
5 and 6	0·62056	0·62016	0·62177	-349·2 -0·99961	0·62115	-340·2 -0·99991	0·62124	-339·1 -0·99993	0·619428	0·61976	-327·7 -0·99999	0·62719	-310·2 -0·99862	0·70098	-310·9 -0·99970
7 and 8	0·62043	0·62202	0·62205	-349·6 -0·99987	0·62123	-340·7 -0·99993	0·62111	-340·2 -0·99992	0·619740	0·61916	-326·8 -0·99998	0·64828	-211·4 -0·88512	1·15802	-181·1 -0·96900
9 and 10	0·62049	0·62446	0·62192	-356·9 -0·99838	0·62087	-341·1 -0·99993	0·62085	-341·1 -0·99994	0·619579	0·61977	-330·0 -0·99999	0·62242	-341·2 -0·98584	0·53265	-363·3 -0·99973
11 and 12	0·62075	0·62139	0·62207	-356·6 -0·99980	0·62135	-339·8 -0·99990	0·62133	-339·7 -0·99991	0·619157	0·61951	-329·7 -0·99998	0·63371	-273·5 -0·96670	0·94799	-242·5 -0·99291
13 and 14	0·61972	0·61850	0·62172	-362·3 -0·99950	0·62071	-337·5 -0·99994	0·62101	-336·1 -0·99994	0·618740	0·61913	-325·0 -0·99997	0·64916	-209·2 -0·96089	0·94681	-211·0 -0·98956
15 and 16	0·61985	0·62051	0·62172	-348·3 -0·99964	0·62065	-339·0 -0·99997	0·62064	-338·9 -0·99994	0·618582	0·61885	-324·8 -0·99997	0·61047	-408·6 -0·90565	-0·33424	-580·7 -0·98163
	mean = 0·62013	mean = 0·62059			mean = 0·62078		mean = 0·62083								

TABLE 2 continued

← ICARUS METHODOLOGY →

17	18	19	20	21
$10^9(k_R')^0_{361}$ /WK^{-4} evaluation near $t=t_2$	GM /DK^{-1} r	$10^9(k_R')^0_{362}$ /WK^{-4} evaluation near $t=t_2$	GM /DK^{-1} r	$10^9(k_R')$ /WK^{-4} near $t=t_2$
0·62032	-321·6	0·62331	-323·4	0·61886
	-0·99891		-0·99914	
0·62094	-391·1	0·62017	-348·6	0·62028
	-0·99927		-0·99941	
0·62086	-347·9	0·61934	-347·5	0·62063
	-0·99782		-0·99819	
0·62099	-338·7	0·62131	-334·4	0·62102
	-0·99951		-0·99952	
0·62094	-375·7	0·61925	-375·0	0·62704
	-0·99921		-0·99933	
0·62091	-328·9	0·62007	-320·0	0·61704
	-0·99926		-0·99808	
0·62044	-329·9	0·61872	-332·6	0·61734
	-0·99978		-0·99964	

8

Table 3

Thermal balances using the integral heat transfer coefficient based on backward integration of the data sets

negative

1	2	3	4	5	6	7	8	9	10	11
Days	$\int$ input $d\tau$ /J	$10^{-9}\int f_1(\theta)\,d\tau$ /K^4s	$10^9(k_R')_{2\sigma2}$ /WK^{-4} near t=τ2	thermal output /J	output $-\int$ input $d\tau$ /J	equivalent excess rate /W	$10^9(k_R')_{2\sigma1}$ /WK^{-4} near t=0	thermal output /J	output $-\int$ input $d\tau$ /J	equivalent excess rate /W
					J	W				
3 and 4	140 501·137	226 640·235	0·61870	140 222·313	−228·824	−0·00161	0·61950	140 403·626	181·313	+0·00109
5 and 6	141 848·957	228 529·444	0·62034	141 765·933	−83·004	−0·00050	0·62114	141 948·776	182·824	+0·00110
7 and 8	141 774·426	228 484·208	0·62021	141 708·227	−66·198	−0·00040	0·62101	141 891·015	182·788	+0·00110
9 and 10	143 166·581	230 672·081	0·61995	143 005·156	−161·424	−0·00097	0·62075	143 189·694	184·538	+0·00111
11 and 12	143 956·736	231 844·144	0·62043	143 843·093	−113·642	−0·00069	0·62123	144 028·568	185·476	+0·00112
13 and 14	145 003·768	233 772·396	0·62011	144 964·569	−39·199	−0·00024	0·62041	145 151·587	187·018	+0·00113
15 and 16	144 858·040	233 584·779	0·61979	144 761·831	−96·209	−0·00058	0·62054	144 948·649	+186·808	+0·00113

P.P.S

Please see the P.P.S in my letter.

2002-09-30

Bury Lodge heading

September 30, 2002

Dr. Melvin Miles,
Department of Chemistry,
Bates College,
Lewiston, ME 04240-6028,
U.S.A.

Dear Mel,

As I told you sometime ago, Sheila and I are going for a few days holiday in Devon. Ahead of that time. I am sending you a set of sketch figures. 2A-9, to explain the various columns in the revised Table 2 which I sent you on 23/09/02. I emphasise that these are only sketches which are required to produce the computer based diagrams (the figures are not very accurately plotted and they are also only based on a selection of data points - there should be 33 for each figure). You will see that the plots are for days 9 and 10 of the data sets.

In due course, I will send you the computer printouts and a discussion of the figures. You will also see that some of the figures do not relate directly to Table 2: they are really illustrations of what one should not do. I will probably send you some further figures to illustrate this point - principally based on the analyses of the differential heat transfer coefficients.

I have also made some progress with the reconstruction of the upper and lower tail distributions of the excess enthalpy (which were an essential step during 1990-93 in modifying the methods of data analyses). However, all this is not yet in a state in which I can send you summaries of this aspect of the analyses.

I trust that all goes well with you at Bates College!

Regards,

Martin

P.S. I have decided to send you an interim assessment of the upper and lower tail distribution of the rates of excess enthalpy generation as given by the use of the true differential heat transfer coefficients, Fig. 10. I have been trying to reconstruct the tests which I carried out during 1991-93 and which were an essential step in the development of the use of the integral heat transfer coefficients as used in the ICARUS systems. The reason why the upper and lower tail distributions are so convenient in the assessment of the performance of the calorimeters is that the differential rates of excess enthalpy production lead directly to these distributions; moreover, the theoretical description is just the complementary error function, erfc [128] (± argument).

[128] MM erfc = error function

I have been carrying out two calculations, the first, using the central value only of the mean of $(k_R')_2$ as given in column 3 of the revised Table 2. The second takes into account the variation of this coefficient with time, Fig 1. This is as far as the examination of the differential heat transfer coefficients can take us! It is all a big labour and has never been described in the literature - I thought it was sufficient two point out the end result namely, that one should use the integral coefficients to achieve the maximum accuracy allowed by these experiments.

The reason why Fig 10 is only an interim assessment is because this is based on only a part of the experimental data (Days 3-6) using only a single value of the heat transfer coefficient, the mean in column 3 of Table 2. The outcome falls short of the performance which we achieved in 1991-93 - which is hardly surprising! For one thing, the standard deviation of the rates is too low ($\sigma = 0.00498$W). Use of a more sensible value would decrease the difference between experimental and theoretical values.

I will send you an update in due course.

Fig 2A Estimation of $10^9 (k_p^1)^0_{251}$ (Columns 4 and 5)

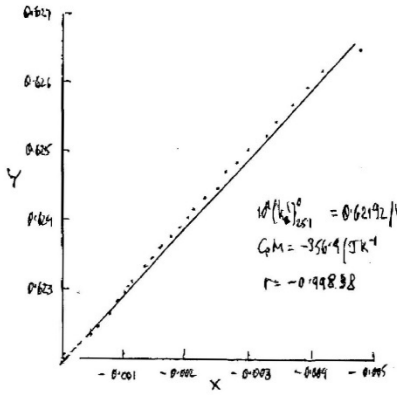


Fig 2B Estimation of $10^9 (k_p^1)^0_{251}$ Whole data set

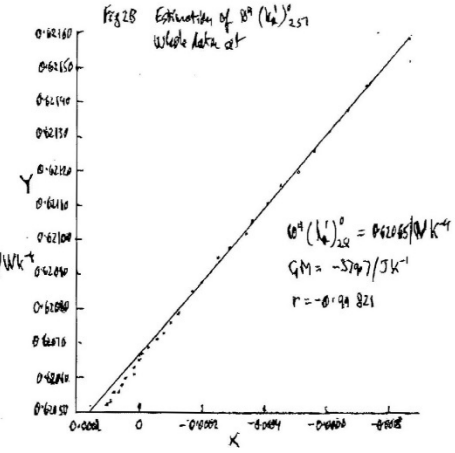


Fig 4A Estimation of $10^9 (k_p^1)_{252}$ (Columns 8 and 9)

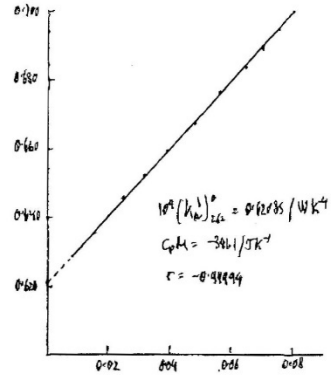


Fig 3A Estimation of $10^9 (k_p^1)^0_{261}$ (Columns 6 and 7)

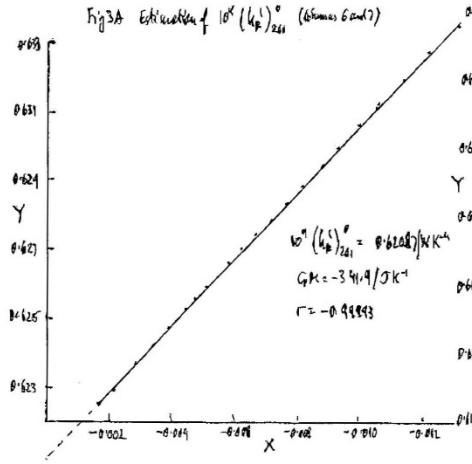


Fig 3B Estimation of $10^9 (k_p^1)_{261}$ Whole data set

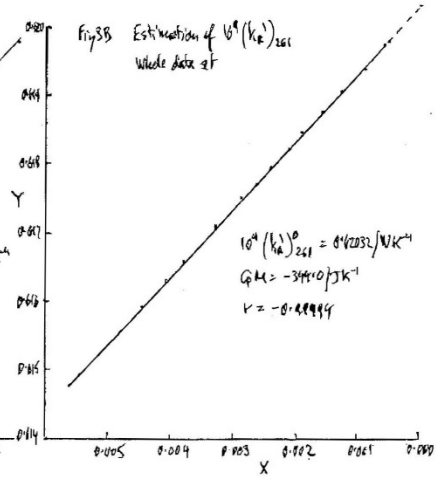


Fig 5 Estimation of $10^9 (k_p^1)_{221}$ (Column 11)

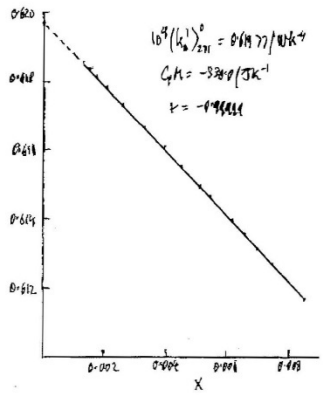


Fig 6 Estimation of $10^9 (k_p^1)^0_{251}$ (Columns 13 and 14)

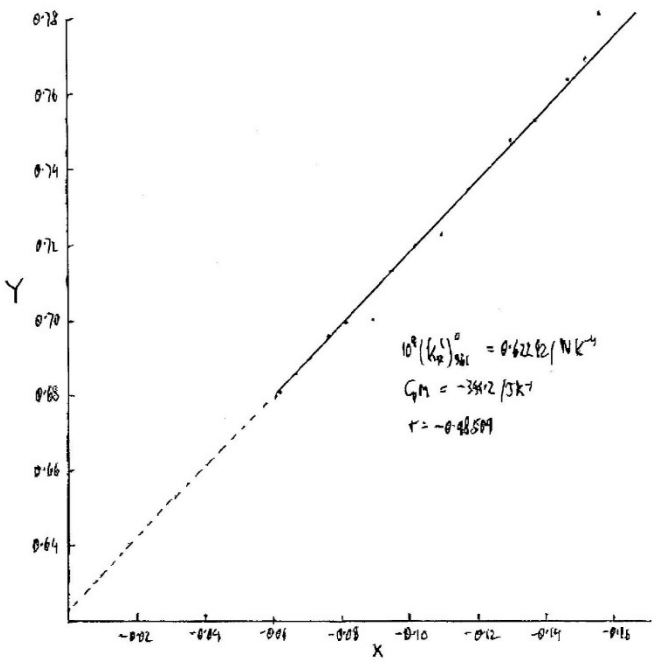


Fig 7 Estimation of $10^9 (k_p^1)_{352}$ (Columns 15 and 16)

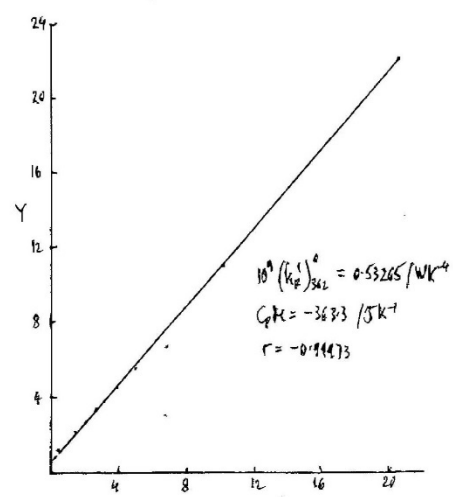


Fig 8 Estimation of $10^4 (k_2^1)^*_{361}$ (columns 17 and 18)

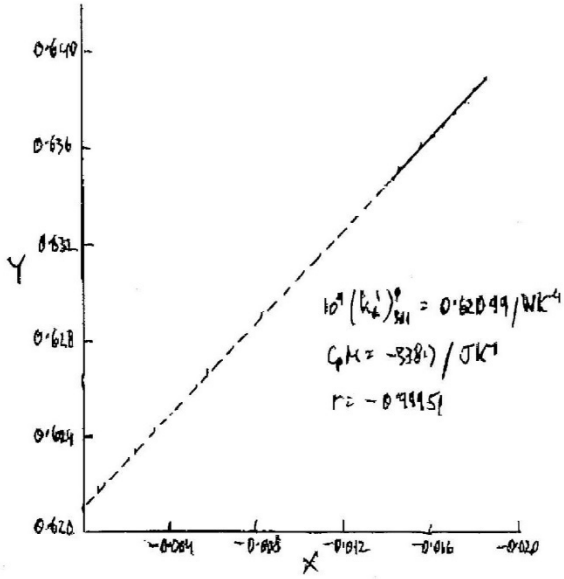


Fig 9 Estimation of $10^4 (k_2^1)^*_{362}$ (columns 19 and 20)

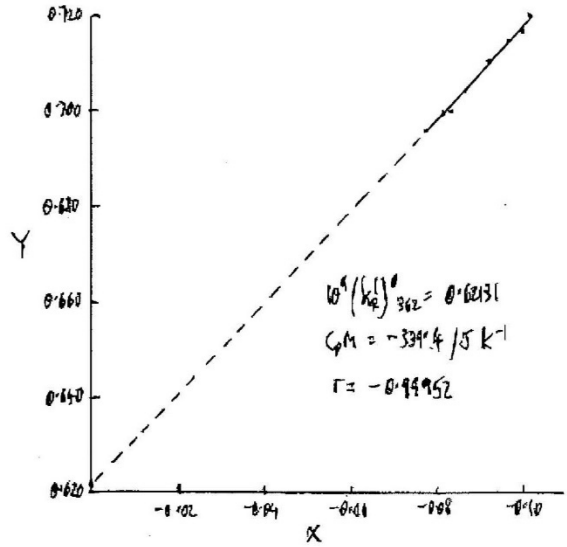
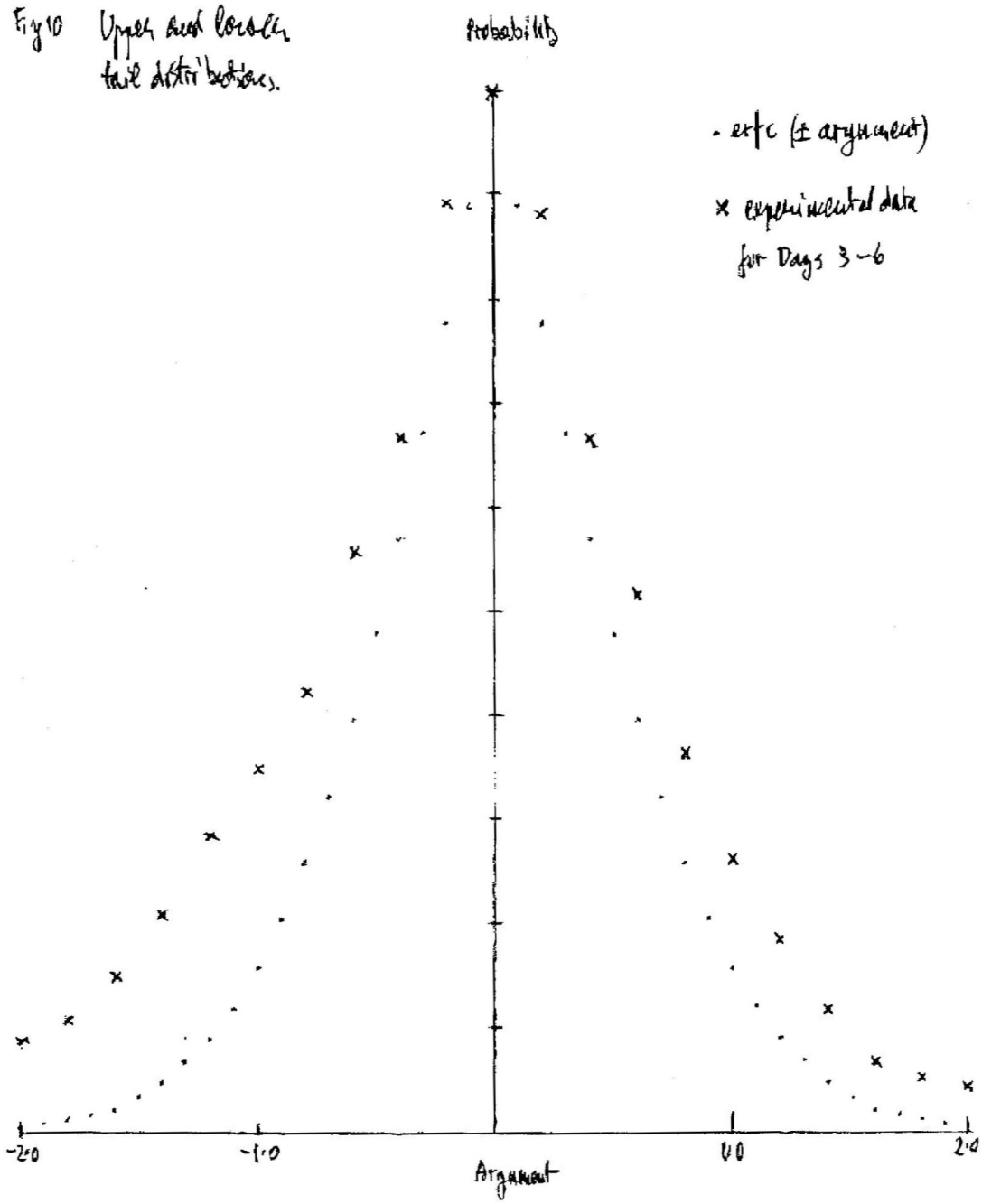


Fig 10 Upper and lower tail distributions.



2002-12-02

Bury Lodge heading

17 October 2002 – letter missing

Dr. M. H. Miles,
Bates College,
Dana Hall,
5 Andrews Road,
Lewiston,
Maine 04240-6092,
U.S.A.

2nd December 2002

Your letter package has just arrived. I see that it has a second copy.

Dear Mel,

Many thanks for your Faxes of 30/11/02 and that containing your suggestions for my letter to Elton Cairns. I am sorry to say that I have had one of my periodic disasters: I now cannot find your letter containing your suggestions for my letter to Elton!! Could I therefore ask you to send me a repeat of this fax but before you do so could you consider the following two points:

First of all, in view of what you have said about Charles Beaudette's interest (and I am very happy for Charles to keep a copy of the letter for his files), I have somewhat revised the last part of my letter (where I go back to the main text to Elton) and have added the necessary two references. I am attaching copies of these pages.

Secondly, could you please think again about the underlying politics of my letter to Elton. As you correctly surmised, I want to manoeuver him into accepting a paper on the "blanks" but I also wanted to give him a "let-out-clause" about the putative part II. in effect I want him to say: "would you please send us a definitive text of this paper". Hence all the gyrations about the two versions etc. etc. I realise that there is only a very small chance indeed that this might happen and I am rather embarrassed by the text of my letter.

But could you nevertheless consider these two points?

It is now ready.

(I haven't sent it to Stan Szpak!) But nothing ventured, nothing gained. Anyway could you please consider this part very carefully.

I decided to rewrite my rewrite of the various letters I sent to you into a form which is reasonably close to a possible part I. This text should be ready on Thursday or Friday but I think that this will be too late to get a copy (by mail) to you before you leave for California (I am green with envy). Could you therefore please send me addresses and Fax numbers at which I can reach you in California? Should I send the draft paper to Stan Szpak and ask him to contact you? I think that you will find that this draft paper answers some of your queries in your Fax of 30/11/02 and I think that we should use the draft as a basis for our future correspondence.

I will answer some of the other queries in your Fax of 30/11/02 when I send you the draft paper; also outcome of my visit to Italy etc. etc.

Do I detect that your teaching load has eased off a little? Could you please send me Charles Beaudette's address and other co-ordinates?

Regards to you both!

Martin

P.S. could you please send a copy of my extended letter to Elton Cairns to Charles Beaudette? He may also like to have sight of Part I (and of Part II when this is written!)

. . . Elton: back to my covering letter, it would be helpful if you could tell me how you react to my comment # 13)

The importance of this investigation lies partly in the fact that it demonstrates that the Szpak - Mosier-Boss of co-deposition of Pd and D is superior to the charging of massive Pd cathodes. At least this is true of low temperatures: the behaviour of these systems at higher temperatures and higher current densities remains to be investigated.

In the days when I could still carry out an experimental programme, I investigated whether it would be possible to produce rod electrodes in the fully formed β Pd-D phase. This would be somewhat equivalent to the Szpak - Mosier-Boss procedure. The outcome was that it would be relatively simple to produce such rod electrodes.

Of course, we now have the Preparata - Del Giudice procedure [8] which I am sure will be crucially important to the development of the subject and, especially, to developing an understanding of "Cold Fusion". However, there is also the tantalising possibility of the development of strange structures by charging electrodes in the presence of poisons and, even, of investigating nano-structured materials.

I am extremely disappointed in the way this field has developed, (or rather the lack of development of the field). If Stan Pons and I had been left in peace in Salt Lake City (with a modicum of funding) we would certainly have achieved a demonstration device by 1992-93. It was just necessary to be single-minded with the research programme and to be free to ignore the advice of friends (few) and enemies (many).

On re-reading the text of this letter, I find that I should add some comments about the paper by Fred Wagner et al [30] (notwithstanding the length of this letter!) as this paper [30] has been singled out for special praise by the second referee. To be quite frank, I could not make out what the paper [30] was about. The methodology described was virtually identical to that we had used except that we made a thermal balance at the time t_1 rather than t_2 (7) (see the attached figure). We did this in an attempt to avoid interpolations of the raw data, The values of the key parameters obtained were used as "starting values" for the non-linear regression fitting of the data. The fact that we used this procedure has not been commented on - the procedure was evidently misunderstood [31] but I note that we explained all this once again [20]. Judging by the comments made by the referees, this explanation once again fell on "deaf ears".

As we explained subsequently [17], [18), we could not make the non-linear regression procedure "user friendly" with the computing power then available to us; we also found that there was no advantage to making the thermal balance at t_1 rather than t_2. We therefore switched to making the balance at this point (actually post-October 1989 for various additional reasons which I can explain to you; we wanted to make this change in June 1989 but, in view of the negative reception of our Preliminary Paper, we felt that we first had to carry out two further series of measurement cycles using the original methodology). Our methodology post-October 1989 was therefore identical to that described by Wagner et al [30].

I note that if the authors had written to me (or ‘phoned me), I would have explained these matters to them. I would also have explained to them why we had chosen isoperibolic calorimetry, why it was important to use a thermal impedance with no memory and why one should try to use an “ideal reactor” (also, numerous other factors). Instead, they produced their paper [30] roughly contemporaneously with our first major publication [7] (slightly after our paper but the work described in [30] must have been done contemporaneously). Of course, if the authors had got in touch with me, the raison d’être of their publication would have disappeared (is that why they did not get in touch with me?). However, if some of the deficiencies of [30] had been straightened out, we would then have had a much more worthwhile publication.

You will see the outcome of the present situation : first of all Wagner et al [30] degraded the performance of their isoperibolic calorimeter (by using a thermal impedance with the memory); secondly, they degraded the performance further by using a flow calorimeter as a heat sink (there is a multitude of reasons for the poor performance of flow calorimeters). What they in fact produce was a rather inadequate paper which mimicked the work we had done. The unsatisfactory performance achieved is now quoted as being “state of the art” and is used as a justification for blocking the publication of much better investigations!

I don’t want to say that this has all been deliberate although it is strangely reminiscent of similar episodes in the tobacco and sugar industries (and is now being vigorously pursued by the “mobile ‘phone interests”). What really happens is that all the bruhaha is used as a foundation for justifying inaction so that all decisions become politicised. Of course, what should have happened is that the paper [30] should have been withdrawn (in common with many of the other papers) so that it could not be used as a means to block further progress. However, we do not have any mechanism for allowing the withdrawal of papers: we resort (or should I say, we should resort) to the practice of simply forgetting such papers.

Regards,

Martin

P.S. This last part of the letter will explain to you why it is such a long moan.

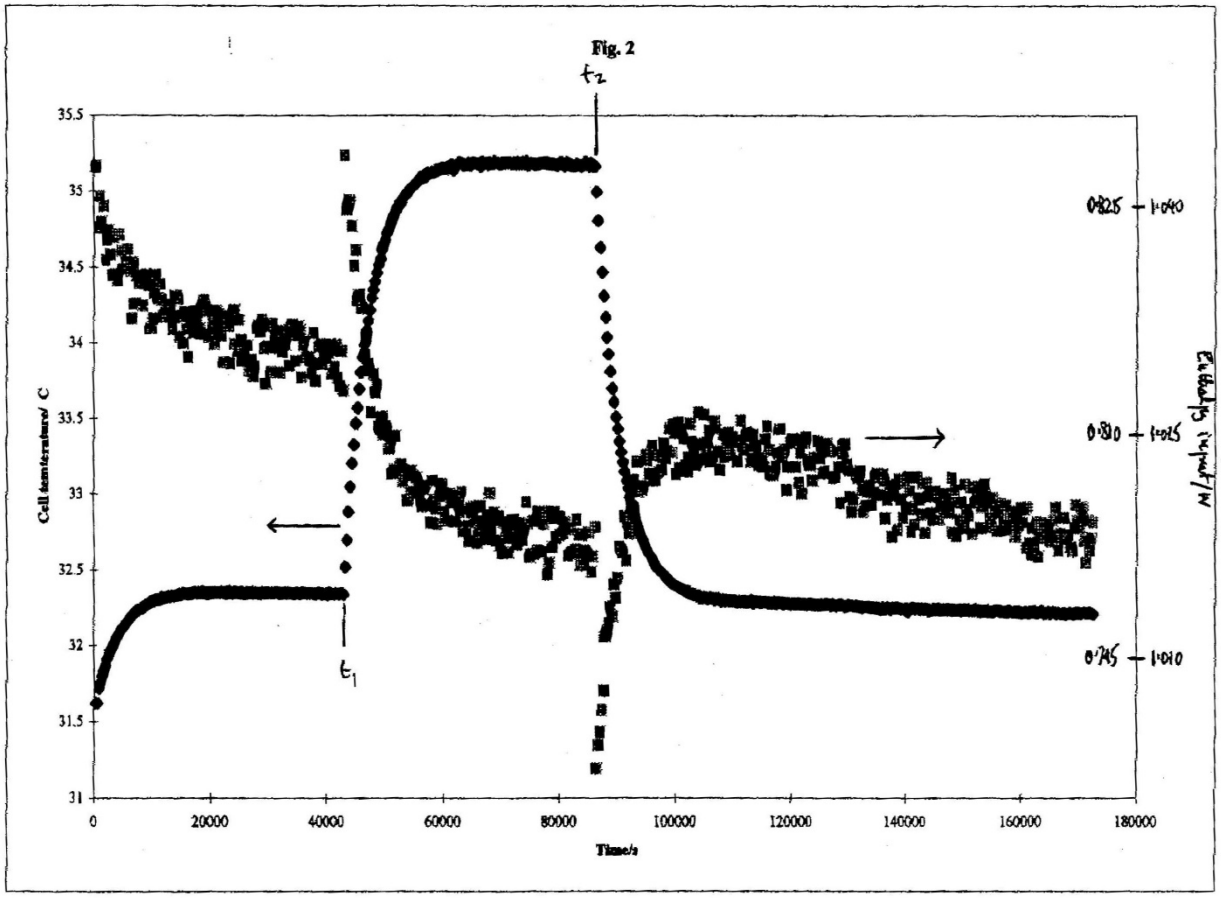


Fig. 2
t_2
t_1
Cell temperature/ C
Time/s
35.5
35
34.5
34
33.5
33
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0·825
1·040
0·810
1·025
0·795
1·010

2002-12-20

[JR This was handwritten. The text is below.]

20 December, 2002

Bury Lodge, Duck Street, Tisbury, Salisbury, Wilts SP3 6LJ

Phone (+44) (0) 1747 870384 Fax (+44) (0) 1747 870845

From Professor Martin Fleischmann, F.R.S.

Reviewed 7/30/2015 M.H.M

20/12/02

Dr. Melvin Miles,
at Ridgecrest,
California, U.S.A.

FAX 001-760-371-7454

December 20, 2002

Dear Mel,

Welcome to your old haunts!

I was going to send you the next draft of Part I sometime after Christmas to await your return to Lewiston but, as you say that you are going to work your way through the earlier letters while you are at your daughters house, I thought that I might as well spoil your vacation by sending you this long FAX. I think that it is really better if you consider Part I rather than the earlier letters as Part I is somewhat more coherent – but don't be too conscientious! Part I

I have started to work on Part II and will send you the first considerations before I leave for Austria (skiing) on the 4th January – back here on 18th. I now believe that we can do quite well with your FP-2-97120401-M7c1 and FP-2-90403-M7c3, better than I had anticipated. However, with all the shuffling of the papers, I now find that I have lost page 1 of FP-2-90403-M7c3 and pages 12-18 of FP-2-97120401-M7c1. Both of these are rather important, so when you get back to Lewiston, could you please see whether you can locate these pages and send them to me?

M7c1 Pd-Ce-B

P. 1

pp. 12-18

M7c3 Pd-Ce (MF)

in the Pd-B-Ce data sets.

I think that Parts I and II might do rather well for publication (Electrochimica Acta? I will leave you to decide whether you want to send Part I to Stan Pons). We may need to rewrite the Introduction and some other bits + pieces. Addition of the Tabular material might make them suitable for parts of Navy Reports?

You will see that I have cobbled together the mathematics for the Appendix

I hate mine!

to Peart I. Has Linda got all this in her P.C.? I hate mine!

Have a good Christmas and the thought of In and Out Burgers and Mammoth makes me green with envy.

Regards,

Martin

nanoparticles

P.S. Have you heard about Arakie's work with nanoparticles? I think that I now understand how all this hangs together within the context of R.E.D. Also, some mind-boggling work of Tesla's.

2003-02-25

Bury Lodge heading

Dr. Melvin H. Miles,
Department of chemistry,
Bates College,
Dana Hall,
5 Andrews' Road,
Lewiston,
Maine 04240-6092,
U.S.A.

25th February 2003

Dear Mel,

As is always the case, I am a very tardy correspondent and I now owe you replies to your Faxes of 20/1/03 and 2/2/03 as well as to your letter of 16/2/03 (received yesterday); furthermore a letter from me to you is long overdue. One consequence of my tardiness is that my eventual reply is always rather disorganised!

I think it will be best if I reply to your letter of 16/2/03 under separate cover; it really requires me to go over some old ground which goes to the heart of the motivation for the research. In this Fax I will therefore start with my long overdue letter which, you will see, is related to the text of the paper "Part 1 of Our Penultimate Paper.....Calorimetry" as well as to your letters of 20/1/03 and 2/2/03. I will also deal with further action required with regard to my letters to Kirk Shanahan and to Elton Cairns. This means that I will deal with the remaining points in your Faxes of 20/1/03 and 2/2/03 at the end of this letter.

Since sending you "Part 1", I have been much preoccupied with the data analysis of FP2-97120401-M7cl (the Pd-B-Ce electrode) which has turned out to be monumentally unsuccessful. You may recall that I wrote to you last year to express my reservations about carrying out such an analysis but I then decided that these reservations were possibly too pessimistic. I therefore embarked on a fairly complete analysis of this experiment only to find that my initial reservations were, in fact, correct. I have therefore decided that we should, for the present, only use a plot of $10^9 \left(\overline{\overline{k_R{}'}}\right)_{11}$ versus time for the "Part II" where the double average == refers to an average of the values denoted by ¯ but carried out over the whole experiment sequence (although the data sets are somewhat incomplete). This plot shows yet again that you were given the wrong leads for wiring up the experiment and that the evaluations had to use the values of C_pM determined for each measurement cycle. Incidentally, here is an important question for you; did you wire up the experiment yourself or was this done for you? [129] It may well be that you

[129] MM Mostly did myself, using wires provided. (Comment dated 2016)

cannot now recall such details but you will see that the answer to this question has a special importance!

The restrictions on the interpretation of experiment FP2-97120401-M7cl also forces us to ask: "how should we interpret experiment FP2-97120402-M7c2?" We have to ask this question in particular with regard to the writing of "Part II" in the sequence "Our Penultimate Paper.....Calorimetry" which I am aiming at present at ICCF 10 (see also further below) I believe that we should aim at the production of a very simple text devoted to the topic "why were we so sure that there was excess enthalpy generation in Pd based cathodes polarised in D_2O based electrolytes?" Bearing in mind the restrictions on the interpretation of the nearest "blank" experiment which have been available to us (i.e. of $\left(\overline{\overline{k_R}'}\right)_{11}$ for FP2-97120401-M7cl), I believe that we should similarly restrict our interpretation for the Pd-B electrode and anything which we may wish to say about the Pt-blank and other experiments (see more about all this below). I have actually re-evaluated the complete data set for this electrode (i.e. Days 1-68) using the recommended ICARUS-2 procedure (identical to the ICARUS-1 procedure) which we can use as a basis for this paper. The major conclusion which emerges from the comparison of the Pd-B with B-Ce experiment (coupled to the Pt-blank experiment) is that the D_2O which you used to top-up the cells was contaminated by H_2O. [130] Have you any comments about this? Was the H_2O content ever determined by NMR? (or some other suitable method). I am rather sensitive to this possibility because of some earlier experiences with the group at N.H.E. and also with the experiments which Giuliano Preparata and Emilio Del Guidice initiated in Italy. I will tell you about these events when we next meet. For the present suffice it to say that I asked Giuliano to sample the cell contents. One cell was so heavily contaminated by H_2O that we couldn't determine the H-content; another cell was heavily contaminated but we could determine the H-content; the third cell (three experiments conducted in parallel) was more or less all right and that was the only cell which showed excess enthalpy generation.

Grrr! As far as your work is concerned, we see that inevitably the rate of excess enthalpy generation remained restricted or, at least, we cannot tell what you would have observed if you had been able to use D_2O with adequately low concentrations of HDO. Sheila has always asked me: "how is it possible to mess-up such experiments?" I have replied; "it's dead easy". As my ex-colleague, Derek Pletcher, has always said: "for an experiment to succeed, it is at the least necessary for all concerned to believe that a positive outcome is possible, at least in principle". I am sure that you must have come across good illustrations of this dictum in your research career?

It seems clear that "Part II" must also refer to the blank experiments which are the subject of "Part I". As I recall we carried out 4 sequences of measurements each lasting 16 days i.e. 62 measurement cycles (the first two were carried out without calibration pulses), I have carried out a complete search of all the data sets I have here in Tisbury and I regret to say that I have only been able to find the first sequence of measurements (which I used for "Part I"). The remaining sets (and three further sets of 64 calibrations using three cells in each set i.e. 576 calibrations in

[130] JR This has been a problem with many cold fusion experiments. D_2O is hygroscopic, meaning it attracts ordinary water (H_2O) from the air. When cold fusion cells or bottles of replacement heavy water are exposed to air, they soon become contaminated with ordinary water.

total) were never sent back to me here from France. Why not? This lack of completion of the calibrations was a major reason why I was reluctant to use this experiment as a "blank"- can't you just see the first set of negative comments? Clearly, the wording of "Part II" will be crucially important.

As you will see, I want to restrict "Part II" to the information which one can derive from the $(k_R')_{11}$ - spreadsheets so that as far as the Pt - blank experiment as well as FP2-97120402-M7c2 and, also, to some extent FP-97120401-Mcl are concerned we will take a step backwards to the early stages of the evaluations. It will therefore be crucially important to ensure that anything which we will say in "Part II" is consistent with our earlier publications. As you have talked about these experiments at a number of meetings it would therefore be helpful if you could list the publications which refer to these topics - better still, if you could send me a set of the relevant papers (but excluding the publication in the ICCF 8 Conference Proceedings and the two Navy Reports). We also have to consider whether we should include relevant aspects about the codeposition experiment in "Part II" so could you please also send me any papers and relevant drafts for this experiment?

I have all the diagrams and Tables which I need for "Part II" (except that I haven't yet prepared anything for the codeposition experiment). However, as I write this letter, I have come to realise that we will also have to cover the determinations of $(k_R')_1$ and $(k_R')_2$ as we will be dealing with the initial assessments of the experiments. I will therefore have to make a further trawl through the data sets: the comparison of $(k_R')_1$ and $(k_R')_2$ with $(k_R')_{11}$ may well reveal some further important details which we have missed hitherto.

It seems clear that "Part I" is too extensive to be abbreviated in any sensible way for ICCF 10 - what do you think ? It would therefore be better if this paper could be submitted to say Electrochimica Acta (with some possible rewording?) so that we could simply refer to such a publication in "Part II". If we are to do this, I should now send the updated draft of my letter to Elton Cairns to him (as you will see I have not yet done so!) Here again I will need your help. On going through my old files, I have found that I have lost the references attached to this letter. Have you by any chance got this list in the version I sent to you and, if so, could you please send me a copy? This will save me a lot of time!

On going through this old correspondence, I have also again run across the letter to Kirk Shanahan which, as you will gather, I have also not sent off. I eventually decided that I do not want to enter into any correspondence with him partly prompted by Mike McKubre and partly by Stan Szpak. Mike, Stan and Pam variously decided to terminate their correspondence with him and the material which Stan sent to me convinced me yet again that Kirk Shanahan's contribution was in the nature of a "spoiler" (I believe that this is the way I originally described his efforts to you). I believe that he must have been asked by the D.O.E. to find a reason for discrediting the work on C.F. He is undoubtedly a clever fellow and he has come up with a simple device: he attributes all positive measurements to changes in the heat transfer coefficient which are supposed to take place by some unspecified mechanism. As far as our own work is concerned, such changes are virtually impossible as the thermal impedance is due to a heat

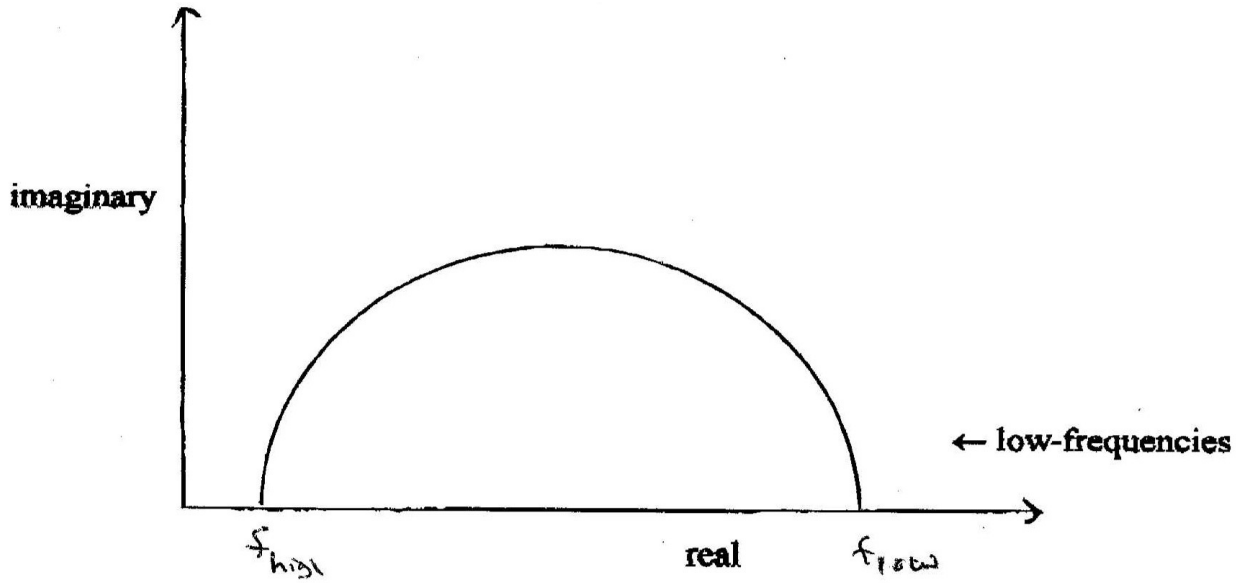
transfer across the vacuum I.e. the rate of heat transfer is just about the minimum one can achieve.

The clever part of this device is that the generation of excess heat and the heat transfer across the vacuum gap are parallel in the parameter space so that one cannot find a simple means of separating the two phenomena; One has to rely on a global calibration but Shanahan asserts that such global calibrations are not justified. Of course, what he has really done is to abandon the rational basis of science (this abandonment is hidden under a great deal of verbiage) and he then asserts that having come up with this impossible scenario, the onus is on C.F. researchers to prove that changes in the calibration do not take place.

Oh no! The onus is on Kirk Shanahan to prove that such outlandish changes in the calibration do take place. The original interpretation of the generation of excess enthalpy remains valid until such time as Kirk Shanahan (or A.N. Others) are able to prove that there are such peculiar changes in the heat transfer coefficient demanded by Shanahan's model.

I believe that Shanahan's paper will give the C.F. brigade a great deal of trouble: it will simply be cited by the D.O.E. and its henchmen as evidence that all the C.F. observations are ambiguous. I believe therefore that we have to hold on to the central point: there are no changes in the true heat transfer coefficient until such time as Shanahan (or A.N. Others) can show that such changes do take place. Incidentally, Shanahan completely ignores the fact that we have always systematically recalibrated all the measurement cycles.

As a matter of fact, I could see Shanahan's argument coming "a mile off". This is why I initiated a programme in 1992-93 designed to answer the specific question: "are there changes in the heat transfer coefficient ?" and, furthermore, "what are the characteristics of positive and negative feedback?" This programme relied on driving the calibration heater with pseudo-random binary noise and constructing the cross-correlation function of this "noise" with the "noise" in the cell temperature (and also with the cell voltage). This was therefore an attempt to carry out calorimetry in the frequency domain. It is easy to show that the real and imaginary components of the Fourier Transform of the cross-correlation function should give a diagram such as



for a "normal" calorimetric cell; the heat transfer coefficient is related to the radius of the semi-circle.

This project failed for four reasons: (i) the cells we used were not well suited for such an investigation; (ii) the cross-correlation (a commercial instrument) did not allow us to reach sufficiently low frequencies; (iii) the cross-correlator was not calibrated at low frequencies and it would have cost us ~$7,500 to have it calibrated (money which we could not allocate to this project); (iv) the accuracy which we could achieve was insufficient. However, I am sure that such a project would open entirely new chapters in calorimetry and electrochemistry and, furthermore, that it is entirely feasible (the thermal characteristics of buildings are investigated in similar ways at frequencies down to 10^{-6} Hz). You will see that the effects of positive feedback would be seen as deviations from the semi-circular plot. The investigation of positive feedback was actually a major factor in persuading us to start this particular topic. In 1992 I had carried out a major investigation on the design of filters to extract information about positive and negative feedback. This was pretty successful but I reached the point when it became clear that we had to allocate a suitable research worker to such a project alone. We did not have the resources to do this so I shelved the project in, favour of one on calorimetry in the frequency domain. However, as regards this particular project, the matter which was of most concern to us was (iv) above. There did not seem to be any point in embarking on a topic which would only give results of doubtful accuracy.

As you see, I decided against entering into a correspondence with Kirk Shanahan but this has not been a very firm decision. I am therefore writing to ask you whether you can come up with any arguments which would persuade me to go ahead with such a correspondence? It would be helpful also if you could indicate where I should change the wording of my letter and whether (and where) I should include any additional material.

This brings me to your Faxes of 20/1/03 and 2/2/03. First of all with regard to the Fax of 20/1/03. My next trip to Italy has been put off again and I am getting even more pessimistic

about the possibility that we will see any new funding there, and furthermore, that if there is such new funding, I would ever be able to use this to execute a coherent research programme. This is, after all, the usual outcome: if people secure funding, then they want to use it for their own ideas! The developments at Frascati are relevant here. I told the folks that the only research which was now justified was that aimed at producing a working prototype. Instead, they pursued a very clever piece of science. I also told them that if they came to new conclusions, then they would never be able to publish these and thus it turned out to be (so far). I wanted to develop a scaled up version of their experiment to produce a working prototype (work which I would have done personally). If this had failed, it would have had no impact on their own work. As you will see, this proved to be impossible. The work in Italy now seems to be beset with all manner of political difficulties which I will discuss with you when we next meet. Much of this is relevant to the letter which I will send you under separate cover.

As regards working in China: I think China should be reasonably safe at least if one sticks to the public spaces. You may recall that the husband of Professor Li's secretary is an American teaching Physics in English and the Chinese appear to be keen to develop such contacts. However, as far as safety is concerned, you will have seen that the taxi drivers in Beijing are enclosed in a "cage" and the traffic is also certainly somewhat lawless. They are now building their fifth ring road and it seemed to me that the real need for this road is due to their very selfish driving habits. The real question is whether the regime can sit on the volcano which is undoubtedly building up in the society. My overall assessment is that China should be reasonably safe - at least as long as we reach the next Olympics. However, I think that you should get a more accurate assessment from your State Department.

As far as the electrochemical experiments which you are developing for your advanced laboratory are concerned; do you know the useful book "Instrumental Methods in Electrochemistry" by R. Greef, R. Peat, L.M. Peter, D. Pletcher and J. Robinson, Ellis Horwood-John Wiley, Chichester, U.K. (1985) ISBN 0-85312-875-8. It should be distributed in the U.S. by the Halstead Press, a division of John Wiley, 605 Third Avenue, N.Y. 10158. As you will see, I have the 1985 version but there may well be more recent editions. This book accompanies the short course on Electrochemistry which has been held on a yearly basis in Southampton since 1969. It has been widely copied elsewhere and Derek Pletcher has been very active in organising such courses in the U.S. in association with the Electrosynthesis Company in Buffalo, N.Y. I don't know what the status of these courses may now be as the Electrosynthesis Company has been repeatedly taken over. (it was started by Norman Weinberg).

The original version of this book followed the lecture course more closely than do the more recent versions and also contained precise details of ~15 experiments which were carried out by the participants. You may wish to contact Derek Pletcher at Southampton to see whether such details are still available - or would you like me to do so?

Amendments / corrections to Part I of the penultimate Paper: I will take all these on board when we produce the final text. The only ones which need attention at this stage are your # 11 (where I would like you to put forward your arguments for my sending my letter to Shanahan) and # 13. The authorship of Part I is somewhat dependent on our publication plans. If this is

submitted to Electrochimica Acta followed by Part II (which I will shortly send you) and Part III (basically the codeposition experiment) then it would also be sensible if the authors were, you, I, Stan, Pam and Imam. I say this because the precision and accuracy determined for "blank experiments" is at the root of all further investigations. However if Parts I, II and III become separated in the literature, then we should perhaps go ahead with just you and me. After all, it was the performance of the instrumentation which persuaded you to apply this to the other systems. I would like to leave the decision to you; if you opt for a restricted authorship together with suitable acknowledgements, then could you please send me a suitable wording for such acknowledgments?

I was interested to see that you had made some progress with the F.B.E. concept. [131] You may recall that I tried to get this under way with the engineers in S.L.C. I got them all the components for a test rig as well as an adequate quantity of spherical Pd particles made from the correct material by J.M. The engineers never did the experiment; the rig and the Pd particles disappeared.

The Italians are getting rather keen on F.B.E.s which might well be relevant to your future plans - although I have severe reservations about whether the Italians will ever get themselves organised. If we can get this project under way, then a part will have to be based on the codeposition concepts.

Your Fax of 2/2/03. My comments are mostly covered by the preceeding parts of this letter. Your situation is really rather desperate and all I can add is that I have had no funding since 1995 - when I was 67! Everything I have done since then has been on a voluntary basis and at my own expense. In my bleaker moments I conclude that the "empire" has just about choked us off so is it not foolish to attempt to carry on? However, see also the letter which I will shortly send to you.

Yes, I was thoroughly dismayed by the nature of the supposedly "negative" papers in 1989/90. We should add the paper by Kreysa, Marx and Plieth to your list. This was based on just a single experiment - can you credit that it was ever published and then cited as a "state-of-the-art" investigation? The folks at M.I.T. had evidently calibrated excess enthalpy out of their system. Parker later on tried to disown the paper but he was the lead author! The sweeping statements in the CALTECH paper were evidently based on ~ 2 weeks work but even then an unbiased assessment of their experiments would lead one to conclude that they too had observed excess enthalpy generation. The only half-way decent investigation was that at Harwell but they hadn't interpreted their data. When I did this subsequently to their publication. (Mike Melich and I persuaded them to release their data) it turned out that the experiments were poorly executed. The errors were so large that one couldn't reach any definite conclusions; the best experiment was a "blank" for which the thermal balances were at the ~ 2.3 σ level. Nevertheless, the group did observe "bursts" in excess enthalpy production and they also observed the classic signs of "positive feedback". I tried to get Ron Bullough (the then Chief Scientist) to reopen the interpretation but he would not do so. I wrote three papers on the Harwell data sets. The first delineated the differences between their and our experiments; the second was devoted mainly to

[131] MM Fluidized Beds Electrochemistry?

the bursts in excess enthalpy generation; the third was devoted to conclusions which one could draw from changes in current density which again showed excess enthalpy generation (incidentally, Mike Melich also reached the same conclusion). This is a topic which has been neglected since that time. I also had all the material ready for a fourth paper which showed that the isothermal calorimetry (which the group at Harwell were very keen on) could not possibly have given any sensible conclusion. I never wrote this paper because the first were all rejected "out-of-hand".

Can you be surprised that I believe that all of this is best explained on the basis of "Conspiracy Theories?" However, enough of this griping.

Regards,

Martin

P.S. I will cover the activities of Steven Jones in the letter which I will send under separate cover

2003-02-27

Bury Lodge heading

Dr. Melvin H. Miles,
207 College Street,
Lewiston,
ME 04240
U.S.A.

27th February 2003

Dear Mel,

All I can say is BINGO! However, as it is I who is answering your letter there is somewhat more to follow.

Of course, as far as I am concerned, my "take" on the project you have outlined is rather complicated. As you know it was the advent of the D.U. shells which finally persuaded me to embark on the C.F. project. A long time ago now I learnt a little about the Soviet programme on the effects of intense compression on the properties of materials. I actually knew very little about this programme (I didn't even know that it flew under the banner of S.B.E.R.,) [132] but, as it happens, one does not need to know very much to start to put two and two together.

It was clear that much of this project goes back to the work of Bridgman in the 1930's. (which, interestingly enough, was funded by the U.S. Military). It was immediately apparent that there was a glaring inconsistency with explanations which were being developed under the aegis of the quantum mechanical paradigm. The formation of powdered material is essentially a "cold explosion" where the high energy stored in the material is released in the high velocity of the particles formed. I thought that such a process could only be explained by using the Quantum Electrodynamic Paradigm and, if we update the terminology, we would now say that the energy is released in the high velocity of the Coherence Domains. Needless to say, we have to establish how such a process can take place but the formation of Coherence Domains does at least provide a rationale for the formation of powders under such strange conditions. Furthermore, such a rationale also provides a basis for the interpretation of other properties of metals (principally of electrons in metals). You may recall that this was a topic which Stan Pons and I had intended to investigate.

It gradually became apparent that such processes were also of interest to the Anglo - U.S. Military and, no doubt, also to other military organisations. One development was the advent of the D.U. shells. We were asked to undertake one project in connection with these. As the request originated with the late Rick Richards (the then head of PERME) we took the matter very seriously and eventually brought the research to a successful conclusion. However, as so happens in Universities, there were many personnel changes and, eventually, I could not secure the

[132] MM Structure Breaking Energetic Reactions.

confidentiality of the research. I should apologise for this rambling account but you will see eventually that all the matters do in fact intertwine.

Anyway I became convinced that the operation of these devices had to be understood on the basis of QED Paradigm and this naturally led to the question; "what else may be around the corner?" As far as the question of D.U. shells is concerned one has to realise that the concentration of energy within the Coherence Domains can lead to energies in the GeV range so one needs to anticipate that it will be possible to initiate nuclear processes. There was therefore the possibility of creating "nuclear assisted explosions" and the indications are that this has been achieved. As you will understand, it is this fact which has coloured my interpretation of the behaviour of D.U. shells. In this connection, my recent trawl through the past correspondence and data sets has produced the attached letter written to you in 2001 after the San Diego meeting. As I recall, I never sent you this letter but simply consigned it to my heap of loose ends. However, you should perhaps have sight of it and I will leave you to decide whether you should pass it on (to Jim Corey?).

It is important to point out here that I had had a programme since the 1960's with the "hidden agenda" of trying to find illustrations of the QED Paradigm within conventional science. Although I had found some promising illustrations, these were perhaps not sufficiently dramatic so I was predisposed towards starting the C.F. venture in 1983-84. Of course, the outcome was rather different to anything which might have been anticipated but, as always, one must carry out research with an open mind! By 1988-89 we had reached several critical watersheds. In the first place it seemed to us that the project should most likely be classified but we did not have the information which would allow us to reach a decision on this course of action. We eventually left this to the D.O.E. but all this produced was the mess we now find ourselves in. Secondly, it also seemed to us that there was only one justification for continuing this research in the University Sector and that was the development of sources of energy. We therefore tried to build a "ring fence" around the project, a policy which I have adhered to ever since and which will explain my attitudes since the early days. Incidentally, when the "whole thing" started to go wrong in 1988-89 Stan P. and I said we wanted to go to a National Laboratory for ~ 2 years to try to achieve a separation of the project into those parts relevant to the Civilian Sector and those of possible relevance to National Security. As you know, we were quite unsuccessful with this objective.

The third aspect was that we asked ourselves the question; "what will happen if we reduce the size of the domain in which we carry out a Cold Fusion reaction?" (if it should prove to be possible to carry out reactions in small domains) The answer was blatantly obvious; sufficiently small domains will be destroyed. At that time our eyes alighted on Na_2 Re H_9 for a number of fairly obvious reasons. One of these was that (again at that time) Na_2 Re D_9 did not exist. Of course, it may have been made in the intervening years in which case my further comments are not relevant. However, again in 1988-89 one could come up with all manner of implausible reasons why Na_2 Re H_9 does exist whereas Na_2 Re D_9 does not. Of course, the most obvious reason is that Na_2 Re D_9 is destroyed on time scales short compared to those of chemical / physical investigations. What was really required was the integration of synthetic with calorimetric methods, a whole new area of research. However, as a first step we asked one of the

pukka Inorganic Chemists in S.L.C. (a former research student of Al Cotton) to try to make Na_2 Re D_9; he did not succeed. In many ways Al Cotton would have been the "man for the job" but he developed a complete antipathy towards C.F. I also went to meet Dag Noreus in Stockholm but I cannot now remember whether this was in 1989 / 90 or 91. I found out subsequently that Stan Pons had commissioned him to try to make Na_2 Re D_9 but Stan did not tell me this. Is this not strange? It is a matter which we should discuss further when we next meet.

Of course, we now have the work of Arata;

Yoshiaki Arata, M.J.A. Hiroshi Fujita and Yue-Chang Zhang, Proceedings of the Japan Academy, 78 series B, 2002, 203.

Arata is a superb experimentalist but I am not much in sympathy with his attempts to find a theoretical basis for his experiments. These are most simply explained by the destruction of the small Pd particles by the fusion reaction(s). In this connection we should also note the work of Baldauf and Kolb;

M. Baldauf and D.M. Kolb, Electrochimica Acta, 38, 1993, 2145.

It seems to me that this particular work opens up the way for an entirely new series of investigations of excess enthalpy generation. I tried to draw the attention of my colleagues in Italy to this work but hitherto without success. Yet again, there are the investigations of the production of nano particles by electroplating from appropriate liquid crystal solutions (these have been carried out in the context of the development of sensors). Some of these particles are SERS [133] active i.e. it is possible to excite (surface) plasmon polaritons in the particles. This raises the possibility that one of my original design concepts (interaction of lasers with SERS active Pd-D; a rather sophisticated version of inertial confinement) could be got to work.

Do you recall that I went on to Washington after the meeting which you organised in California ? I wanted to see the work of Debra Rolison's group (stabilised nanoparticles). Very interesting - it seemed to me that this might well open the way towards suitable weapons applications.

There is also work in Frascati (De Nino, Fratolillo and Del Giudice) - the effect of electric potentials on C.F. in suitable one dimensional electrodes. You will recall that one can eventually see the melting of the cathodes under the electrolyte although it seems to me that this would be better described as boiling of the cathodes. Conservative estimates of the rates of excess enthalpy production to achieve this are 1-50 $MWcm^{-3}$. The rather wide range in values is due to the wide range in the parameters which one must plug into such guesstimates. Of course, the magnitude of the specific excess enthalpy actually released depends on the time required to achieve melting / boiling. In this connection, I wrote to Stan Szpak last year to point out that the light emission from such electrodes must reach the visible part of the spectrum as the electrode reaches disintegration. One should be able to capture such events with a suitable camcorder. How about it? It is a suitable candidate for a kitchen sink experiment (especially if I could first prove that one can see such events in thin wires).

[133] MM Surface Enhanced Raman Spectroscopy

Do you believe that Shanahan's antipathy towards the work on "hot spots" is to be explained in part by the observations made in Frascati ? It is quite clear that Stan and Pam's work could be developed much further to the point where the spatial and temporal distributions of the Q values could be derived. The work at Frascati points quite clearly at the way(s) in which weapons could be developed. Presumably this must be of great concern to the D.O.E.

I must apologise for this rambling account. We did think about many of the possible developments which have been seen post 1989 by A.N. Others. It now seems to me that the lack of progress in this field has been mainly due to my policy of trying to produce a "ring fence" around the excess enthalpy generation and the consequent restrictions on our research programme.

My short answer to your question of whether your design concept would work is "very likely" but there are other ways of perturbing the system which it should be investigated. I believe that there is now sufficient evidence in the literature to show that what we called "uncontrolled releases of thermal energy" can be initiated (however, they are at the least semi - controlled or, at any rate, controllable). I think that we should discuss the whole topic further when we next meet.

Regards,

Martin

P.S. Surely, there must be a "black" programme somewhere (or at several locations). If there is not, then this would amount to criminal negligence. I thought that the use of D.U. shells by the Anglo - American forces during the Desert War and in Kosovo was singularly ill advised. Such a use should have been delayed until such time as we could decide whether such (or related) weaponry could be developed into a Star Wars System. The situation now is that lightweight and low technology systems could well be developed and, of course, such systems could devalue (or already have devalued) our existing weaponry. Incidentally, Jed Rothwell told me recently that the major interlocutors for their Web site are in China!! This is surprising because the telephone lines into China are closely controlled and restricted by the Chinese Authorities so, presumably, this interlocution has the blessing of Chinese Authorities. I trust that the CIA is monitoring this telephone traffic.

Incidentally, it now appears that the next generation of D.U. shells being developed in the U.K. will be Ti tipped. So what was all the hu-ha re the ^{238}U - tipped shells about? Precise comparisons of the ^{238}U and the new Ti tipped shells should prove to be interesting. Would the Ti tipped shells lead to carbonisation of the tank crews?

Of course, much of these speculations is based on circumstantial evidence (and there is a whole lot more !). However, if these speculations are somewhere near the mark, then one must have a great deal of sympathy with the Defence and Intelligence Agencies. I recall that Stan Pons and I believed that we had an adequate answer to our initial question; "can small perturbations per atom in the lattice lead to nuclear reactions?" We would have been quite content to have seen the whole project classified and to return to the search for the effects of Q.E.D. in more

conventional systems. The opposition to the work on C.F. is especially pernicious in that it frustrates the development of the interpretation of the Natural World in terms of the Q.E.D. Paradigm which must surely be the next major step in Science. In that regard the outcome of the C.F. research is totally negative - rather than positive, as I had hoped and expected. As of now, I believe that our Lords and Masters have little understanding of Q.E.D. and just about zero understanding of the consequences of Q.E.D. (although there is some evidence that the French have tried to get to grips with that - possibly the Russians also. I will tell you about this evidence when we next meet).

P.P.S. I see that I haven't commented on Steve Jones' activities in the main text. His (and Palmer's?) original intention were quite clear; to develop a neutron spectrometer capable of measuring extremely low fluxes of neutrons. The systems of primary interest to them were deuterides subjected to intense compression in diamond anvil presses. I thought that such a project was entirely laudable; however, I can recall whispering to Stan when we first went to meet them; "we cannot possibly be associated with this project". Apart from their general attitude and lack of understanding they had carried out their measurements next to a D^+ accelerator and their attempts to shield their apparatus were quite ludicrous.

This project eventually degenerated into measurements of the total neutron flux in a deep mine somewhere near Provo. Can you believe that someone would attempt to do this in Utah of all places? Of course, the statistical significance of the data was hopeless. I can only surmise that someone must have told them not to continue with their original experiment design.

Of course, if Stan P. and I had entered into a meaningful discussion with the group at B.Y.U. we would have suggested suitable materials for investigation in diamond anvil presses. Na_2 Re D_9 comes to mind but, if this compound does not exist, then $^{238}UD_3$ would have done. Naughty?

This is potentially still a good project - the system could be developed into a form of two - dimensional spectroscopy which would really sort out some of the SBER effects.

I had another slant on this topic: Manduchi, Mengoli and Zannoni developed an absolutely superb neutron spectrometer. (Incidentally, with the resources care of yours truly). However, their work has been discontinued and Giuliano Mengoli evidently finds the whole topic to be embarrassing.

P.P.P.S. Your interpretation of the behaviour of the personnel at the San Diego meeting is spot on. Do you still have a copy of Linda's tape? If so, I would like to review this with you when we next meet. I am afraid that I just shrugged my shoulders; I said to myself; "this is their show but they will live to regret this (I hope not!)."

2003-03-20

Bury Lodge heading

March 20, 2003

Dr. Melvin H. Miles,
Department of chemistry,
Bates College,
Dana Hall,
5 Andrews Road,
Lewiston,
U.S.A.

Dear Mel,

As you will see, I am sending you this Fax from the number of Wood and Co, and I am also sending a copy of this letter to your home address. There are several important matters that need attention : the first is to ask whether you received my lengthy Fax followed by my letter of 27th February 2003 enclosing also a letter of January 2001 addressed to both you and Stan Szpak (which, I believe, I decided at that time not to send to you). One difficulty with using the Fax at Wood & Co is that I frequently have to leave the messages at their offices and I therefore become detached from their contents. You will be able to see what is coming I now cannot find the copy of my Fax! However, I do like to keep a complete record of our correspondence; therefore, if you did receive that Fax, then would you please send me a copy by mail ? This will save me a lengthy search which might well prove to be fruitless.

I believe that I can remember gist of the contents of this Fax which raised three important points. The first was an outline of my attitudes to the paper produced by Kirk Shanahan where I asked for your comments on whether or not I should send the letter which I drafted some time ago (you will see that it is very important for me to find out whether you did, in fact, receive my Fax). The second raised the question of how we should proceed with regard to my letter to Elton Cairns and the related question of the authorship of "Our Penultimate Paper on the Calorimetry of the Pt/D_2O Systems; Part 1: the Pt/D_2O Blank System". The third point related to the progress (perhaps more exactly, the lack of progress) with regard to "Part II: the Pd-B and Pd-Be-Ce Systems". As I recall I pointed out that we really needed to examine the true heat transfer coefficients, $(k_R')_2$ and that such an examination might well produce some new information.

Anyway, I have now completed this examination and the outcome is somewhat different to anything which I had anticipated. The first important point is that we can use your schedule of additions of D_2O together with the systematic variations of $(k_R')_{11}$ with time revealed by "Part I" or the mean of $(k_R')_{11}$ i.e. $\left(\overline{k_R'}\right)_{11}$ averaged over the whole data set for the Pd-Be-Ce system (this is the only aspect for this system which I will use in Part II) to predict the variation of $(k_R')_{11}$ with time for the Pd-B system. We find that the observed variation is much larger that the predicted one and we can see increases in $(k_R')_{11}$ which correlate with the schedule of additions.

How are we to interpret such behaviour? I believe that there is really only one explanation and that is the addition of D_2O "quenched" the excess enthalpy production to some extent. The most likely reason for such a behaviour is that the D_2O which you used to "top up" to the cell contained appreciable amounts of HDO. The continued electrolysis would then have led to the progressive removal of the hydrogen as HD (the H/D separation factor is very large under the conditions of the experiment).

All of this leads to a series of important questions which you may or may not be able to answer after this long time interval. Was the H-content of the D_2O even determined and, if so, what was it? It is most important in this connection that we repeatedly pointed out that this should be done and there is a great deal more which I can tell you when we next meet. (we installed an NMR instrument specifically for this purpose). Secondly, D_2O is often supplied in rather small bottles while you must have added ~ 220 ml of D_2O during the course of the experiment. Did you use such a succession of bottles or did you have the D_2O in a single container? [134] If you did use a succession of small bottles, then did you make a record of the dates on which you started to use each bottle and, if so, what was this record?

This brings me to the second important question. As I told you in my Fax, the marked variation of $(k_R')_1$ with time during the course of the experiment means that we also have to examine the comparable variation of $(k_R')_2$ with time. This variation is within the limits given by that predicted by the cell currents and schedule of additions (see above) but the correlation is rather poor: all we really achieve is a "scatter diagram". The major reasons for the scatter in $(k_R')_2$ are the inadequate relaxation of the time-series following all the perturbations (addition of D_2O, changes of cell current, application and cessation of the calibration pulses) due to the contraction of the measurement cycles; the rather large volumes of D_2O added at different times ; the excessive levels of the "noise" in some of the measurement cycles (especially the "noise" in the cell potential-time series); the influence of "positive feedback". Elimination of the most unsatisfactory calibrations leaves a set of just 22 values of $(k_R')_2$ which correlate quite well with the predicted changes in the heat transfer coefficient!

Of course, such a reduction in the data set is, to some extent, subjective - or at least it will be claimed that this is so by the many opponents of C.F. I note here that it would be possible to put this process of data reduction on a quantitative basis and, furthermore, that the relatively good correlation of $(k_R')_2$ with the predicted changes achieved indicates that we should investigate the more precise and accurate interval heat transfer coefficients. However, I am reluctant to embark on such a venture for reasons which I will now explain.

The second major point which emerges from a consideration of the values of $(k_R')_2$ is that these are systematically and substantially lower than the values of $(k_R')_1$ over nearly the whole data set. Such a behaviour would require the cell to be endothermic over nearly the whole of the experiment duration, a behaviour which has been previously observed by the group at N.H.E. (but not commented on by the group). We have never observed such a behaviour for appropriate "blank" experiments; for a recent example see "Part I". The "true" heat transfer coefficients are

[134] MM We used a succession.

only slightly smaller than the "lower bound" values which appears to be adequately explained by the reduction of electrogenerated oxygen. As you will know, it appears that the group at N.H.E. never carried out a series of "blank" experiments (or, at least, they have never admitted that they carried out such experiments). I have the plots for two measurement cycles of such a "blank" experiment which we set up for them in 1993 (they discontinued this experiment shortly after we left). It would be possible to expand these plots to A1 or A0 size and to reconstruct the original data set which we could then use to evaluate the integral heat transfer coefficients. Should we embark on such a venture?

Of course, the straightforward explanation for values of the "true" heat transfer coefficient being smaller than the "lower bound" values is that the heater calibration currents and/or the heater resistances were in error. An increase of Q from the given value of 0.2500W to the range 0.2700 - 0.2750 would remove the anomaly and such values of Q are much more in line with those which applied to the work carried out in Sophia Antipolis. This leads to an important question: have you any information which would allow us to specify Q without any ambiguity?

To summarise the previous pages; the D_2O which you used contained sufficient HDO to restrict the excess enthalpy production; the heater calibration pulses were in error. Of course, we cannot tell whether these happenings took place by accident or design but, if you should discuss your work with Jim Corey, you might wish to tell him that I favour the second interpretation. There are some further details regarding the work in Japan which I should tell you about when we next meet.

I have now started to write "Part II" which I will take with me to Italy when I next go there on 30th March (for one week). I would therefore like to have your comments on my questions by 28th March - even if these comments are incomplete.

There is one further which is pertinent to our future plans and this is any work which Wilf Hansen might wish to do. You may recall that you wrote to me about this last year (or in 2001?) and I replied that I had written to him on 4th January 2001 to list all the data which I have here in Tisbury. Wilf never replied further to my letter so I took this to mean that he is no longer interested. I think that it is really most straight forward if I send you a copy of the relevant part of My letter to Wilf. Here again, I would appreciate having your comments. However, you will have gathered that I am somewhat reluctant to embark on further extensive reanalysis.

As I have already said, I am going to Italy yet again on 30th March. However, I am somewhat pessimistic about the possibility of raising funding for a constructive research programme. I increasingly have the feeling that I get "wheeled out" whenever they have the need to raise funding which needless to say, never comes my way. This next visit should show whether I am right or wrong and I will write to you further on my return!

All the best,

Yours,

Martin

Extract from my letter to Wilf Hansen dated 4/01/01

To start the ball rolling, I should explain to you why I am so keen on the analysis of FP2-97120401-M7cl. It appears that our Japanese colleagues never carried out a "blank" experiment although this was part of our original deal with the group in Sapporo. The system Pt-D_2O was recommended to them and they were given analyses of such experiments which we had carried out in Sophia Antipolis. The question therefore is: are we to believe that they never carried out any such blank experiments? I, for one, do not believe this; if they did indeed carry out such "blanks", they would have surely soon realised that the relevant calorimetric set-ups behaved exactly as we had described in the associated Handbooks. Furthermore, if they did indeed, carry out such "blanks" then they have never admitted that they have done so and, of course, they have never given me the relevant data sets. The importance of Fp2-97120401-M7cl lies in the fact that there was little (if any) generation of excess enthalpy and certainly not on Days 3-10 during which the current density was quite low. This experiment will therefore serve in lieu of a "blank". It strikes me that when I write up this work I should also send this Report to you?

The situation with regard to the analyses of the NHE experiments is actually more complicated than would follow from this simple account. The experiments actually fall into two groups: those carried out with the ICARUS-one system (up to the winter of 1994) and those carried out with the ICARUS-two (subsequent to winter 1994). Several of the experiments in the first group showed clear indications of excess enthalpy generation according to the preliminary analyses which I carried out in 1994 (I wrote two Reports on these Analyses). However, our Japanese colleagues never sent us the relevant complete data sets!

During 1994 I became very concerned about the untidy wiring of the experiments as well as the possibility that there might be errors in the relative magnitudes of the powers delivered to the cells and calibration heaters. I therefore devised a set of switching boxes to get over these potential difficulties. The wiring in ICARUS-1 was of the 4-terminal type (or rather, it should have been) whereas ICARUS-2 relied on heavy gauge wiring between the switching boxes and the cells. This matter was taken out of my hands (there is a great deal more which I could tell you about this episode but I do not care to put pen-to-paper).

I am sure that you will be able to guess what happened. Our Japanese colleagues used the ICARUS-2 set ups but, for some of the experiments at least, used the ICARUS-one wiring to connect the cells to the switching boxes. One of the experiments in this group is the Pd-Ce-B set PF2-47120401-M7cl and this will explain to you the further reason why I want to hammer this particular nail into the coffin.

We should ask ourselves: incompetence or design? Our Japanese colleagues are rather keen to spread this aura of incompetence but I do not believe that they are incompetent at all. It is all rather reminiscent of the Japanese attack on Manchuria: the General Staff did not want this but their hand was forced by the Junior Officers. More anon.

I wonder whether you would like to analyze Days 3-10 of FP2-97120401-M7cl? We could use this to start the discussion and broaden this to all further issues.

As you know, Stan Pons and I are accused of all manner of misdeeds which is why I am so keen to analyze other people's data sets. In a way, the best collection of data is that which was the basis of the paper by: G. Lonchampt, L. Bonnetain and P. Hicter, Proceedings of the Sixth International Conference on Cold Fusion, editor M. Okamoto, October 1996, Vol I, page 113. Originally the members of this group were not allowed to meet us (I met them unofficially at CERN in Geneva!). Professor Bonnetain asked we to get them a "proper [??]" Pd rod, which I did with considerable difficulty (more about this when we meet). This rod was sufficient for just 7 experiments which are reported in this paper. All these experiments were driven to boiling.

The paper was presented at the 6 Meeting by Jean-Paul Biberian (who has now apparently replaced Bonnetain. He appears to divide his time between Grenoble and Marseille where his address is:-

Dr. Jean Paul Biberian,
Faculté des Sciences de Laminy,
80 Route Leon Lachamp,
13009 Marseille,
France.

. . .

In his presentation at the 6th Conference, Biberian made a special point that the electrodes used in the Grenoble study had been supplied by us - a matter which caused me some difficulty. This statement has disappeared from the paper. Why?

You will see that it would be very important to analyse these data sets and I have asked the folks at Grenoble (and especially Jean-Paul) repeatedly to give the data sets to me; they have also never turned up. Last time (at the 8th Conference) I said to Jean-Paul: "you mean that the D.S.G. does not allow you to give these data to me". he proceeded to mumble around this point and again promised to extract the relevant information.

I am giving you this background because it might be desirable for you to approach Biberian directly for these data sets. You could explain that you know that he presented this material at the 6th Conference, that you had previously analysed some of our data sets collected in Salt Lake City and that you had also been concerned with the interpretation of the Harwell study. However, it might be as well if you were to keep our projected collaboration "under wraps" just at present. It could well be that there would be no embargo on your receiving these data sets?

Next, there is an interesting study by the IMRA Materials Laboratory in Nagoya. This study was actually totally misconceived (which I can explain to you in due course); the outcome was that these experiments mimicked exactly our earliest experiments. I urged repeatedly that these results should be comprehensively analysed and wrote a first report on this subject. I never received any reply (not even an acknowledgement). I have fairly complete documentation for

these experiments. Clearly any further action on these experiments raises some tricky issues. Needless to say, the IMRA Group observed excess enthalpy generation!

Next, there is the data set which Stan Pons gave to G.E. in 1989, I have the relevant documentation. At that time they agreed with us that we had observed excess enthalpy generation. As I recall, the level was such that they should have paid up. Should we do any further about this?

Finally, I have various data which we collected at Salt Lake / NCFI on two Carlisle DC2000-XL Mini Data Cartridges. These data would have to be translated to floppy disks or CD ROMs. The last time I looked into the question of recycling this material we found an organisation in Texas which could do this - at a price. My enthusiasm promptly plummeted especially as I do not know whether any of the resulting material would be usable without the relevant laboratory notebooks. These should be in Stan's possession but, of course, they may now be "lost". Would you like to comment on any possible action with regard to this material.

Finally, finally, I have a CD-ROM with some Japanese data sets collected with the ICARUS-two set ups. Dr Asami gave this disc to me at the 7th conference but none of the sets correspond to those which I previously requested (I used July 1994 as a cut-off point). I think the data on this disc are pretty useless.

Finally, finally, finally (this can go on for ever) there are the interpretations of the "Harwell" data sets which certainly require a further airing. We actually wrote three papers on our own reinterpretation (and I also wrote extensively to Ron Bullough who was then the Chief Scientist at Harwell) but these papers were all rejected. The third paper is of special interest as it dealt with the measurements using 4 mm diameter electrodes. It appeared from some comments made to me by Mike Melich that either he or you had also investigated these data sets. Again, should we do anything further? I also have an embryonic paper on the Harwell isothermal calorimetry.

Finally, finally, finally, finally, I am glad that you are in touch again with Mike Melich. Yes, indeed, Mike McKubre has also observed the generation of ^{3}He. (Much more diagnostic than that of ^{4}He).

2003-03-29

Bury Lodge heading

29th March 2003.

Dr. M. H. Miles,
Bates College,
Dana Hall,
5, Andrews Road,
Lewiston,
Maine 04240-6092,
U.S.A.

Dear Mel,

As I told you in my last letter, I shall be going to Italy tomorrow. Before leaving, I completed a first draft of Part II of "Our Penultimate …… Systems" and I will ask Sheila to send this to you by air mail on Monday (to your home address). You will find that the paper is incomplete. In particular, Fig 7 has to follow and I will be taking the necessary material with me to Italy to try to complete this job there. Depending on the results, we may be able to make a comparison with Fig 12 of our 1990 publication (reference (5) in our draft paper). This would be in an additional Fig 8.

I suspect also that the detailed evaluation of the rates of excess enthalpy generation may lead to some additional conclusions but it may well turn out that we cannot make these sufficiently firmly so as to include such material in the paper.

Furthermore, it may well be that we should insert a comment on page 6 (following paragraph 2) about the "switching boxes" (the "musical boxes") to allow the control of the level of the electrolyte and to eliminate the uncertainties about the relative magnitudes of the powers delivered to the cell and calibration heaters. The difficulty here is that this would require a further 3-4 figures. What do you think? I don't want to lose the simplicity of the paper which is aimed at showing just how easy it was (and still is!) to demonstrate the presence of excess enthalpy generation in Pd-based systems. Should the discussion be extended to stress these points once again (the need to use calorimeters with adequate sensitivity, the need to calibrate the calorimeters using suitable "blank" systems, the need to explore the whole of the time region of the experiment (hence the need to use thermal impedances with no memory) the need to control the isotopic purity of the D_2O, the need to make sensible choices about the electrode materials.*) Could you please comment?

** The need to create a reasonably comprehensive programme, the need to include electrodiffusion and other methods of perturbing the system etc. etc.!)*

We will also have to rework the diagrams: the scales must be reduced so that we can include the figure legends in a suitable way; also, some of the diagrams are confusing - the psychology of the presentation is very important here! Your comments here are essential.

Could you also please comment about a further Part III dealing with the codeposition system (and further potential publications) and how we should proceed with my letter to Elton Cairns?

Regards and I hope that my visit to Italy may lead to positive results!

Yours,

Martin

P.S. People are 'phoning me in great excitement about Daviss' article, in the New Scientist. [135] I haven't seen it yet - have you any comments?

[135] JR Daviss, B., *Reasonable Doubt*, in *New Scientist*. 2003. p. 36

2003-05-12

Bury Lodge heading

Dr. Melvin H. Miles,
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12th May 2003

Dear Mel,

I was very relieved to hear that the results of your colonoscopy were satisfactory. It is a good idea to have such examinations on a regular basis especially if there are indications that you should do so in your family history. I have had seven such examinations since 1988 and I am only sorry that I did not start having these way before my operation at that time. Most of the difficulties with the bowels which we have seemed to built in with our new lifestyles are perfectly avoidable!

As you will see, I am now answering your Faxes / letters of April 10th and 25th but first of all let me tell you that I am sending you a revised version of "Our Penultimate Paper on the calorimetry of the Pt / D_2O and Pd / D_2O Systems ; Part II : the Pd-B and Pd-B-Ce Systems" to your home address. As I mentioned to you when I sent you the previous version, I still had to calculate the time dependence of the rate of the specific excess enthalpy generation (the most important information is in Figs. 4A, B and C (also Fig. 9) and Table 5) and I had thought it likely that this completion might require us to rewrite the main text. Thus it has turned out to be. I think that you will like Fig. 5 and this naturally raises the question ; "what would the other seven studies referred to in the Introduction reveal?"

Of course, what all this shows is that a fairly sensible development of a straightforward (albeit tedious) investigative methodology has been continuously frustrated. It also means that we must assume that this "frustration" has been part of a deliberate policy - call it "conspiracy" if you will. I have often been asked whether I believe that such a conspiracy has been organised by the oil companies (the seven sisters). I have replied that I doubt whether the oil companies have the resources to do this. The question then is : "who would have the resources to organise such a conspiracy ?" (that is, if we continue to believe in such conspiracy theories). I think that the answer to this question is blindingly obvious and has been so since 1989. The rôle of the oil companies is then reduced to applauding on the sidelines and lending a helping hand when this might be required. Do your own experiences with the ERAB Committee not fit into such a picture? I had no contact with the members of the Committee except for a brief conversation with Al Bard at a Scientific Meeting (which he misunderstood – or misrepresented) and my listening to a lecture by another member of the Committee which showed that they had misunderstood the concept of the thermal neutral potential (or chosen to misunderstand this?). I

tried subsequently to develop a correspondence with Al Bard but abandoned this for reasons which I can explain to you - it is a rather interesting story.

I thought that the article by Daviss read very well indeed although he did include a number of incorrect statements which have accumulated in the peripheral literature – all my attempts to correct these statements have come to nought. Thus Stan Pons and I were not in favour of the March 1989 Press Conference. Indeed, prior to this we had tried to persuade Steve Jones / D.O.E. to postpone the consideration of a publication until September 1990 (more about this below). I was only a visitor to Utah and I felt that I had to "toe the line". I was therefore supportive of the University's position. At the same time I wanted to stop the whole business but I realised that the request to cancel this charade had to come from "on high". I therefore tried to contact Lord Porter (the then President of the Royal Society) to ask him to put my views to Mrs. Thatcher with a request that she should get hold of George Bush (senior) to ask him to slam on the brakes. Do you know this part of the saga? In the event, I failed to get hold of George Porter and events took their miserable course.

The allegations with regard to our presumed attitude to the Press Conference are usually coupled to statements best summarised by: "Physicists do not hold Press Conferences unless they had previously had a paper accepted for publication". These statements are wrong on two counts. First of all, we had had a Preliminary Paper accepted for publication, indeed we told the University Authorities that we would not go ahead with the Press Conference unless such a paper had been accepted. As a matter of fact, we wrote two papers one submitted to Nature and one to the Journal of Electroanalytical Chemistry. It had been our intention to "pull" the second paper if the first had been accepted. However, it was rejected and the Press Conference Intervened. There is a great deal more to this story which I will tell you about when we next meet. One important point is that the Journal rushed ahead with the publication and we never had a chance to consider the page proofs. If we had had such an opportunity, we would have made many corrections! (The paper was written in one afternoon - we were very preoccupied with the Patents!)

The second count is that Physicists are actually some of the worst offenders with regard to the holding of Press Conferences and related attention seeking devices. Thus the first measurements with Zeta (the No 1 "hot fusion" device) were announced to the House of Commons by the then Postmaster General. When the energies of the neutrons came to be measured, they were found to be different to those predicted for "hot fusion" in low density plasmas. Red faces all round but did anybody lose their jobs and why has the Physics Community forgotten this tubular episode? However, I said to myself "how interesting. If there is fusion in this system, then evidently, the process is more complicated than would be predicted from the results of Oliphant, Harteck and Lord Rutherford (1)". I will comment on this further below. It was important that the results of Dee (2), (3) (using a Wilson Cloud chamber) showing that this was in fact so. So far, I have only met one Physicist, Richard Petrasso, who has noticed the difficulties raised by Dee's measurements although Yeong Kim has evidently taken some of my comments on board.

I recently had occasion to write to John Bockris and I told him that there had actually been 13 major reasons why I had been against the holding of the Press Conference. These 13 major

reasons were divided into about 40 research topics some of which would certainly have required work by several research workers (some by experienced research workers). You may recall that I told you that I counted 10 such major reasons and 12.5 after the Conference in Beijing. However, I believe that 13 and 15.5 would be a more realistic estimate (the number naturally depends on where one places the divisions). It was my view that classification of the project would have allowed a much calmer approach to the investigation of the project than has been the case following the activities of Steve Jones / D.O.E.

You may recall that Stan Pons and I wanted to move to a National Laboratory in 1989 to allow us to start investigating some of these 13 topics - we realised that the implementation of a wide ranging research programme was premature. However, there was one topic which we could easily have started and which was central to the question: "can one induce nuclear reactions by chemical means at low temperatures?" The answer to this particular sub-set of questions (incidentally, nothing to do with our approach to the calorimetry) would have clarified the situation with regard to classification. However, our move could not be arranged although I suspect that the University did not try very hard to make such arrangements. The background to this question is relevant to anything which we (or I) might wish to say at ICCF 10 as well as to the activities of Kirk Shanahan (see more below).

I think it is best if I comment further here on one of the other 13 reasons which had convinced us that publication in 1989 was premature. This is relevant to the attitude of John Pazik which you have commented on in your letter of 10th April (and to the attitudes of the bulk of the Scientific Community) and may also be relevant to the content of your short-term class (see your letter of 25th April). This is the general framework of theory which has been used to interpret nuclear reactions especially "hot fusion". I think it would be true to say that the attitude of scientists can be summarised by the statement

$$\text{Nuclear Physics} \longleftarrow X \longrightarrow \text{Chemistry} \qquad (A)$$

i.e. there is no connection between the two subjects. I suspect that if one were to make a trawl through the literature, one would find that much of this attitude goes back to Niels Bohr and like minded bullying and pig headed people. With regard to the specific example of "hot fusion" in Deuterium plasmas it is known that the reactions

$$D^+ + D^+ \rightarrow T^+ + H^+ \qquad (B)$$

and

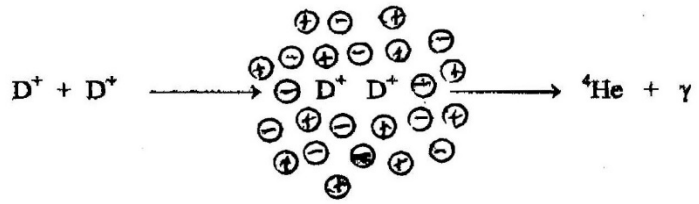
$$D^+ + D^+ \rightarrow {}^3He + n \qquad (C)$$

have roughly equal cross-sections (perfectly reasonable) while the reaction

$$D^+ + D^+ \rightarrow {}^4He + \gamma \qquad (D)$$

has a cross-section in the range of 10^{-6} - 10^{-5} of (B) and (C). Of course, this is also perfectly reasonable because (D) requires the polarisation of the vacuum (so as to allow the generation of electromagnetic radiation). It is then assumed that all fusion reactions must follow the path of

“hot fusion” in low density plasmas because of asymptotic freedom – hence Cold Fusion is impossible. It is instructive though to interpret (D) further. We can write



(E)

where the process proceeds via a virtual state in which the compound nucleus is surrounded by 12 positron-electron pairs (i.e. to make up for the mass difference in reaction (D)) - we postulate a virtual state having a life time given by the uncertainty principle. If we now consider fusion within the lattice, we can see that one (or more) of the electron states may correspond to that of an electron in the lattice which, in effect, provides a condition for resonance stabilising the compound state (the virtual state is converted to an actual state). The compound state is “cooled” so that reactions (B) and (C) cannot take place (or are markedly inhibited) whereas (D) is accelerated, (and, of course, this opens the way for other reactions such as those discovered by Takahashi). We have an immediate explanation of the results of Dee (2), (3) as well as an explanation of “Cold Fusion”.

I should emphasise here that this argument is an illustration: it is not a calculation or theory. If you want to follow up such lines of reasoning, you should consider the papers of Preparata and Del Giudice listed in the draft of my letter to Elton Cairns. Of course, arguments based on quantum mechanics are fairly reasonable as far as reactions (B) and (C) are concerned but I cannot see how (D) could sensibly be discussed in such a framework: we need Q.E.D.

We can now see how many of the arguments used by the Physics Community must have arisen. These are based on the assumption that (B) and (C) are the only significant path ways leading to the specification of the energetics of the reactions especially of the neutron. If neutrons of the “correct” energy are not observed, then “Cold Fusion” is the impossible. We can see that such arguments are truly awful: they must have been rooted in the phenomenon that text books are written using other textbooks as source material. The original literature is forgotten and we arrive at a process of progressive which, inevitably homes on to (A).

Anyway, in 1988 / 89 it seemed to me that it was necessary to do a great deal of work on the theoretical framework although the need for this has somewhat faded in view of the activities of other research workers (notably Preparata and Del Giudice).

Before leaving this topic, we should ask ourselves ; “if the methodologies used to investigate “hot fusion” are incomplete (or unsound), then how should we investigate such processes ?” The answer seems blindingly obvious to me; “by calorimetry as the first choice” - hence the long saga on which we embarked.

Next, I want to consider two related points raised in your letter of 25th April. We, always used long, narrow cells: the reasons are explained in Appendix 1 of our first full paper (4). It is relevant here that the vacuum in the cells used in the initial investigation was rather "soft" (the cells were constructed in the glassblowing workshop of the Chemistry Department in Utah). In consequence we frequently used 10 cm long cathodes where the mixing was dominated by the radial term (relaxation time ~ 3s as determined by tracer experiments). For shorter electrodes the mixing became controlled by the axial term (??[136] s). The short, fat cells were constructed for other experiments using larger electrodes (sheets up to 64 cm^2) and 2 cm diameter x 10 cm rods (the experiments with sheets were discontinued in March 1988 because of concerns about safety and the lack of funds)! Such lack of funds also prevented the start of the work on the large diameter rods. However, in any event we believed that electrodes above 0.4cm diameter would crack as was also likely because of the change in the protocol used to make these larger electrodes.

It was necessary to increase the volume of electrolyte to ~ 500cm^3 for the work with the large electrodes, hence the short, fat, cells. When Newsweek and Time visited us in S.L.C. they said that our standard cells were not photogenic and they wanted the largest cell we had available for their photographs! Incidentally, Nate Lewis was told all about this but, presumably, he had already made arrangements to have the cells depicted in the magazines copied. It soon struck me that Nate Lewis's comments were "Jason inspired". Stan Pons 'phoned me repeatedly in the U.K. (I had returned here in some disgust with the events leading up to March 1989) to say that Nate called him repeatedly (sometimes twice a day). When Stan gave his advice Nate's reply was always to the effect that: "we know how to do this much better". I told Stan that he had to answer all of Nate's questions.

When it came to the E.C.S. meeting in L.A. the details of Nate et al's experiments became public knowledge (especially the use of short, fat, cells). I said: "Nate you can't do this experiment in such cells". I believe that they then went back to Caltech and repeated the work. A realistic estimate of the time scales was that all of this was done in just about two weeks. Stan and I calculated that it would have taken us ~5.5 years to complete the work outlined in their paper even with our much larger resources. I think the conclusions were obvious ; they hadn't done most of the experiments they listed and those they had done were quite inadequate. Incidentally, the first question I ask myself when asked to referee a paper is: "Could the work have been done in the time available?" If the answer is "no" then I reject the paper outright.

Nevertheless, a realistic appraisal of their experiments is that they had seen excess enthalpy generation (which they removed by recalibration?)

As far as Steve Koonin is concerned, I told him at a meeting in Sicily in 1989: "you cannot discuss this problem in the context of quantum mechanics". He said "yes" but that is the last we ever heard of that. The problem is that people have no understanding of many-body problems - they understand quantum mechanics based on the energetics of two-body interactions so they

[136] JR Number is illegible.

simply continue to discuss such simplifications coupled to the denial that anything more complicated needs to be considered. Really, but the deficiencies of Q.M. are well-known!

Regarding your letter of 10th April, I believe that the contents of my present letter makes the position of John Pazik reasonably clear. He is just a person who follows the common herd - or else he is working to a scenario which wants to frustrate the work on Cold Fusion which he can do quite well by hiding behind the conclusions based on Q.M. I do not know him but one possible approach would be to send him the Abstract of a proposed paper for ICCF 10 (see further below) with the offer to discuss the contents with him.

Back to "Part II" of our "Penultimate Paper". Two matters which need urgent consideration are the Authorship and the schedule of additions of D_2O (one of 1MLiOD) shown in Tables 4 and 5 and also underlying Fig 3. I have based this schedule based on the contents of your letter of 24th April 2001 (the sheet headed December 8, 1997) but I cannot really read anything below Day 63. I have added a guess for Day 64 but I believe that there must have been at least one further addition between then and Day 68. Could you please therefore check these important details ?

We now have to decide what to do with Parts I and II. It strikes me that the full texts (including the Tables) might be suitable as two Chapters of a further Navy Report possibly accompanied by further Chapters on the Codeposition and Pd-B-Ce Systems as well as a general introduction? This would mean that Chapters I and II would only require minimal changes to make them suitable for inclusion in such a Report. I have it in mind to also write a greatly simplified letter to Elton Cairns asking him whether he would consider "Part I" for Electrochimica Acta. However, should we not include "Part I" in this letter to Elton Cairns ? If so, then could you send me the Instructions to Authors as well as a revised list of your corrections / amendments ?

May I also ask you to send me a copy of the Registration Forms for ICCF 10, your corrections / amendments to Part II and a collection of our submissions of papers on the Codeposition System (as you know, I am totally disorganised).

As you will see, I did not send my letter to Elton Cairns and I also continue to prevaricate about Kirk Shanahan. The reason is that I believe that he must know full well that "Cold Fusion" is possible - indeed that this is a reality. I base this on the list of his other activities. If that is so, then it would put the activities of the D.O.E. under a great black cloud. I have lived in hope that I could execute the project (one of the 13 I have alluded to) in the Padua region. We were promised a rather large sum of money most of which would have to be spent on the Q.E.D. of fluids (a very straight forward scheme of investigation). However, the 1 / 13th project might well be absorbed in the marginal costs especially as I would pay for the apparatus personally. Incidentally, you will want to know that I repeatedly told the people concerned that you are available. They know of your work and said "yes, yes, yes, how interesting". However, I have heard nothing since my visit which fills me with foreboding as regards the finances. If the project should come off, then it strikes me that you could find this to be very stimulating : the Q.E.D. of liquids would open a wide new vista including aspects of Biophysics and could form a useful

background to my earlier research interests. I also have it in mind to investigate a souped up version of the DeNinno experiment along the lines which I originally proposed to the group in Frascati.

Your Abstracts for ICCF 10 - excellent. Should we make one or more submissions on the basis of Parts I and II? I also have it in mind to submit an Abstract on BACKGROUND TO THE START UP OF THE WORK ON COLD FUSION. I will send you these Abstracts later this week. The third "BACKGROUND----" is in the nature of an hot potato. As you know, I have been very economical with the truth about this background but there are people who are now asking me point blank what might be the connection between "Cold Fusion" and the D.U. Shells. The Abstract I will send you will be rooted in the work of Bridgman and that on metal-hydrogen systems but, nevertheless, it will become quite clear what lay at the basis of our discussions to start this project. What do you think? Should you forward a copy of this Abstract to suitable contacts? (J.C. ?)

I am sure that there is a great deal more which I should have written to you about!

Regards,

Martin

References

1) M.L. Oliphant, P. Hartek and Lord Rutherford, Nature, 113 (1934) 413.
2) P.I. Dee, Nature, 113 (1934) 564.
3) P.I. Dee, Proc. Roy. Soc., 148A (1935) 623.
4) M. Fleischman, S. Pons, M.W. Anderson, L.J. Li and M. Hawkins, I. Electroanal. Chem., 287 (1990) 293.

2003-05-14

Bury Lodge heading

Dr. Melvin H. Miles,
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14th May 2003

Dear Mel,

Herewith now three Abstracts for ICCF 10. Could you please comment on all of these and especially that on BACKGROUND TO COLD FUSION : THE GENESIS OF A CONCEPT ? As you know, I have fought shy of saying anything which might lead people to make a connection between "Cold Fusion" and "D.U. Shells" but, of course, there was such a connection. I have always believed that one must be scrupulously honest in Science - dissimulation is always eventually found out. It seems to me that people are rather puzzled as to why we chose to investigate this particular topic (e.g. see Stan Szpak's and Pam Mosier-Boss's Introduction to their Report) but, as Giuliano Preparata said; "there had to be one (or several) precursor(s) to Cold Fusion" (and he got mighty close to the true story). Other people are now also making their way towards the background so is it not time to reveal this ? After all, it is now 14 years since the first announcement so the D.O.E. et al have had plenty of time to tell me of their concerns - if they had any ! I conclude that they either believe or know that there is no such connection or else that they are simply ignorant about the relevant background in Physics.

However, do you think that you should ask J.C. whether it is sensible to present a paper on this topic at ICCF 10?

Should we write a further paper on the Pd-D co-deposition system? If so, could you please send me a file of our previous drafts and some notes as to what such a paper might contain.

Regards,

Martin

P.S. You will be able to guess the nature of the 1/13th investigation which Stan Pons and I wanted to carry out in 1989. Do you believe that the D.O.E. has not investigated this topic? If not, then they are really beyond the pale; if so, then the fact that they don't present the results also places them right beyond the pale.

Below are the final versions of the three Abstracts submitted by Fleischmann. The manuscripts were attached to this e-mail. The abstracts and finished papers are as follows:

Fleischmann, M. *Background to Cold Fusion: the Genesis of a Concept*. in *Tenth International Conference on Cold Fusion*. 2003. Cambridge, MA: LENR-CANR.org

http://lenr-canr.org/acrobat/Fleischmanbackground.pdf

Fleischmann, M. and M. Miles. *The "Instrument Function" of Isoperibolic Calorimeters; Excess Enthalpy Generation due to the Parasitic Reduction of Oxygen*. in *Tenth International Conference on Cold Fusion*. 2003. Cambridge, MA: LENR-CANR.org

http://lenr-canr.org/acrobat/Fleischmantheinstrum.pdf

Miles, M., M. Imam, M. Fleischmann, *A Calorimetric Investigation of the Pd/B System*

This abstract was submitted but a paper was not published in the proceedings. The content was later published here:

Miles, M., M. Fleischmann, and M.A. Imam, *Calorimetric Analysis of a Heavy Water Electrolysis Experiment Using a Pd-B Alloy Cathode*. 2001, Naval Research Laboratory: Washington. p. 155.

http://lenr-canr.org/acrobat/MilesMcalorimetrd.pdf

BACKGROUND TO COLD FUSION : THE GENESIS OF A CONCEPT.

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Starting in the early 1960's I became increasingly concerned with the question; is it possible to devise experiments in electrochemistry which illustrate the need to invoke the Quantum Electrodynamic Paradigm for the interpretation of the results? In due course five major topics were investigated:

1) the kinetics of fast reactions in solution at time scales below 1μs;
2) the kinetics of voltage-gated transmembrane ion conduction processes;
3) Surface x-ray diffraction;
4) the kinetics of phase growth of single centres on microelectrodes;
5) mass transfer to surfaces due to wall-phase turbulence.

There was also a set of further problems which could not be investigated directly.

It became apparent that the explanations of 1), 2), 3) and 5) required the division of the solvent (water) into two domains, one of which had dimensions between 10^{-6} and 10^{-5} cm and in which the solvent was highly structured; 4) indicated that such a division might be a general phenomenon. The explanation of this phenomenon became available at a later date (1), (2).

In the early 1980's, Stanley Pons and I asked ourselves the question; if the production of structured domains applies to deuterium in host lattices (such as Pd), then would it be possible to induce nuclear processes in the deuterium by adding relatively small energies / species to these domains i.e. could one build a bridge between the low energies (~1eV) of Chemical Systems and the high energies (say 1 MeV) governing nuclear processes ? There were also two further pertinent factors. One was the observation of "cold explosions" by Bridgman in the 1930's (intense compression of lattices can lead to their fragmentation into small particles in which the high energy of the initial system is contained in the kinetic energy of the fragments; surely a process which can only be explained by Q.E.D.?); the second was our knowledge that absorption of hydrogen isotopes in metals can lead to just such a fragmentation.

We embarked on this project without any great hope that we would obtain definitive results. We investigated the Pd /D system (coupled to the use of the Pt / D system as a suitable "blank") using calorimetric methods (for reasons which we explain below). However, the outcome was radically different to our expectations; the steady-state generation of excess enthalpy without significant formation of the fusion products produced in dilute high temperature plasmas (the formation of ^{4}He could be detected but could not be related to the magnitude of the excess enthalpy generation).

The results of this investigation (3) have been extensively criticised principally because of the lack of the expected fusion products. The fact that the description of high temperature plasmas is incomplete when considering fusion in a lattice will be illustrated (as was indeed shown in (4), (5) by measurements carried out at the time of the discovery of "hot fusion" (6)). This fact prompted our use of calorimetric methods to investigate the energy balances.

1) A. Arani, I. Bono, E.Del Giudice and G. Preparata. Int. J. Mod. Phys., B9,(1995), 1813.

2) Giuliano Preparata, "Q.E.D. Coherence in Matter", World Scientific Publishing Co. Pte. Ltd., (1995) ISBN 9810222491, QC 173. 454. P74;

3) M. Fleischmann, S. Pons, M.W. Anderson, L.J. Li and M. Hawkins, J. Electroanal. Chem., 287 (1990) 293;

4) P.I. Dee, Nature, 113 (1934) 564;

5) P.I. Dee, Proc. Roy. Soc., 148A (1935) 623;

6) M.L. Oliphant, P. Hartek and Lord Rutherford, Nature, 113 (1934) 413.

THE "INSTRUMENT FUNCTION" OF ISOPERIBOLIC CALORIMETERS ; EXCESS ENTHALPY GENERATION DUE TO THE PARASITIC REDUCTION OF OXYGEN.

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Critics of the topic of "Cold Fusion" frequently assert that isoperibolic calorimeters are imprecise and inaccurate so much so that the measurements of excess enthalpy generation (e.g. (1)) could not have been made. Furthermore, any valid excess enthalpy generation has then been attributed to the parasitic reduction of electrogenerated oxygen although such assertions have not been accompanied by appropriate measurements. It will be shown that there is a connection between these two assertions.

The first step in the development of any investigative methodology must naturally be the determination of the relevant "instrument functions" determined here by the differential equation modelling the calorimeters. It will be shown that such models are characterised by the relevant heat transfer coefficients, $(k_R')_{i,\,j,\,k}$ / WK^{-4}, where i =1 denotes the determination of a local differential coefficient, i = 2, 3 denote processes of backward forward integration of the temperature - time series ; k = 5 denotes the time region adjacent to the start of a measurement cycle, k = 6, 7, 8 denote respectively time regions adjacent to the start of an heater calibration pulse, the end of this pulse and a combination of the two time regions ; k = 1, 2 denote respectively the "lower bound" heat transfer coefficient (based on the assumption that there is no excess enthalpy generation) and the "true" coefficient (based on the response to the heater calibration pulse). Omission of the symbol j denotes that we are considering a coefficient throughout the time range of a measurement cycle while omission of i denotes that we are considering "robust" estimates of the "lower bound" and "true" heat transfer coefficients at the end of the calibration period (compare e.g. (2)). The terminology (k_R') denotes that we are considering a pseudo radiative coefficient (based on the neglect of any conductive contribution to heat transfer in the Dewar - type cells).

It will be shown that the determination of the most precise and accurate coefficients (errors < 0.01%) should be based on the backward integration of the time series giving for example, $(k_R')_{261}$ and $(k_R')_{262}$. Such determinations require that the rates of any excess enthalpy generating processes are constant in time. These conditions are satisfied for "blank" experiments such as the Pt / D_2O system which we have used in our investigations. Excess enthalpy generation is restricted to that due to the reduction of electrogenerated oxygen which is constant in time for the conditions of the experiment. It is therefore straight forward to determine this rate of excess enthalpy generation which is shown to be close to the value predicted from the rates of reduction available in the literature (3); it is also close to the value determined in the original investigation (1) where the evaluation was carried out using non - linear regression (see Table 4 of (1))

An alternative strategy is to base the evaluation on the differential coefficients $(k_R')_{11}$ and $(k_R')_{12}$ coupled to the appropriate signal averaging of the derived rates of excess enthalpy generation and this methodology must be used when the precise and accurate integral coefficients cannot be evaluated. The results of the two methods of investigation will be shown to be closely similar (3)

The classification of calorimeters according to the principles of Chemical Reaction Engineering (4) will also be discussed and it will be shown that accurate evaluations should be based on the "ideal reactor", the "well stirred tank" description characterising the isoperibolic calorimeters used in the investigations.

1) M. Fleischmann, S. Pons, M.W. Anderson, L.J. Li and M. Hawkins, J. Electroanal. Chem., 287 (1990) 293.
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3) M. Fleischmann and M.H. Miles "Our Penultimate Paper on the Calorimetry of the Pt / D_2O Systems. Part l; the Pt / D_2O blank system" submitted for publication.
4) O. Levenspiel, "Chemical Reaction Engineering" (1972) John Wiley, London and Chichester, U.K.

A CALORIMETRIC INVESTIGATION OF THE Pd / B SYSTEM

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The measurements discussed in this presentation were made by one of us (M.H.L.) during his stay in the laboratories of the New Hydrogen Energy Group, Sapporo, Japan using an ICARUS-1 Calorimeter (1) coupled to an ICARUS-2 polarisation and measurement system (2). The Pd-0.5% B electrodes were prepared by one of us (M.A.I.) at the Naval Research Laboratories in Washington D.C.

It is shown that in contrast to the measurements on the Pt / D_2O "blank" system (see (3)) the "lower bound" heat transfer coefficients vary markedly within each measurement cycle and with the progression of these cycles from Day 1 to Day 68 spanning the experiment duration. This shows the presence of a markedly varying rate of excess enthalpy generation and , in consequence, the evaluation has had to be restricted to the "robust" estimates of $(k_R')_1$ and $(k_R')_2$ and the differential heat transfer coefficients $(k_R')_{11}$ and $(k_R')_{12}$ (compare (3), (4)).

It is shown that the experiment described in this paper suffers from a number of deficiencies. In the first place, the power delivered to the calibration heater was incorrectly quoted so that $(k_R')_2$ has had to be estimated from the maximum value of the "lower bound" heat transfer coefficient (compare (5)). Secondly, some of the samples of D_2O used to replenish the electrolyte (to make up for losses due to the combined effects of electrolysis and evaporation) were evidently contaminated by HDO causing marked increases of the "lower bound" heat transfer coefficient following such replenishments. Thirdly, the range of current densities used in the experiment (and the protocol) was restricted in view of the dimensions of the electrode so that this was maintained in the region for the onset of "positive feedback" (which could be detected throughout the experiment duration) (compare (6), (7)). Nevertheless, the rates of excess enthalpy generation observed in time regions free from such complicating factors agree with those observed in the initial investigation (8). This is perhaps not very surprising because an essential step in the preparation of these electrodes was the melting of the palladium in the presence of calcium boride (so as to maintain the oxygen activity at an adequate low level).

The experiment also demonstrated the phenomenon of "Heat-after-Death" (5), (9). The rate of excess enthalpy production increased markedly on Day 68 of the experiment, so much so that the cell contents "boiled to dryness" (5), (9). It can be seen that all the phenomena to which we have previously drawn attention can be demonstrated using just a single experiment provided this is carried out in an adequately comprehensive manner, over a sufficient duration, and provided the experiment is subjected to a complete evaluation.

1) The ICARUS Systems : Isoperibolic Calorimetry ; Acquisition Research and Utilities System, Version -1 (December1993), Low Power Measuring System for Three Cells, TECHNOVA INC., 13th Floor, Fukoku Seimei Building, 2-2-2 Uchisaiwai-cho, Chiyoda-ku, Tokyo 100, Japan.

2) ICARUS -2 : Isoperibolic Calorimetry ; Acquisition Research and Utilities System, Version -2.0 (December1993), Low Power Measuring System for Three Cells, TECHNOVA INC., 13th Floor, Fukoku Seimei Building, 2-2-2 Uchisaiwai-cho, Chiyoda-ku, Tokyo 100, Japan.

3) M. Fleischmann and M.H. Miles, THE "INSTRUMENT FUNCTION" OF ISOPERIBOLIC CALORIMETERS : EXCESS ENTHALPY GENERATION DUE TO THE REDUCTION OF OXYGEN, this meeting.

4) M. Fleischmann and M.H. Miles, "Our Penultimate Paper on the Calorimetry of the Pt / D_2O and Pd / D_2O Systems. Part l : the Pt / D_2O blank System" submitted for publication.

5) Martin Fleischmann and Stanley Pons, Proceedings of the Third International Conference on Cold Fusion, Editor: H. Ikegami, Universal Academy Press, Frontiers of Science Series No 4, (FSS) ISSN 0915-8502 ISBN 4-946493-12-6 (1993) p.47.

6) M. Fleischmann, Proceedings of the Fifth International Conference on Cold Fusion, (1995) 105.

7) Martin Fleischmann and Stanley Pons, Phys. Lett. A, 176 (1993) 118.

8) M. Fleischmann, S. Pons, M.V. Anderson, L.J. Li and M. Hawkins, J. Electroanal. Chen., 287 (1990) 293.

9) S. Pons and M. Fleischmann, Trans. Fusion Technol., 26 (1994) 87.

2003-05-19

Bury Lodge heading

Dr. Melvin H. Miles,
Dana Hall,
5, Andrews Road,
Lewiston,
Maine, 04240-6092,

May 19, 2003

Dear Mel,

Herewith now a corrected and completed (reference 1) of the Abstract BACKGROUND TO COLD FUSION ; THE GENESIS OF A CONCEPT proposed for ICCF-10 which I sent to you with my letter of 14/05/03. What do you think?

You must have thought it crazy of me that I repeated so much of the material in my letter of 12/05/03 which I had already written to you about in earlier letters. I think I am losing my grip! However, it does at least show that I am consistent.

Regards,

Martin

P.S. I am also now sending you the Abstract of a possible fourth contribution A CALORIMETRIC INVESTIGATION OF THE ELECTROCHEMICAL Pd-D CODEPOSITION SYSTEM. As I do not know exactly what the contents of this paper might be, I have kept the wording deliberately vague. I leave it to you to change the wording and/or sharpen up the text - if you so wish ?

Could you let Stan [137] and Pam know of the existence of this Abstract ? Of course, if they have already submitted an Abstract containing essentially similar material, then we should simply scrap the one I am sending to you.

P.P.S. The key question at the moment is whether the Abstract BACKGROUND TO COLD FUSION ; THE GENESIS OF A CONCEPT and the resultant paper should be submitted and, in the Abstract, the key point is the reference to Bridgman and the fragmentation of metals by isotopic hydrogen (incidentally reference to Schlapbach's books ?). Of course, one could say that this is all published work so that there is no difficulty in adding "grist to the mill". However, <u>it was</u> the connection to the D.U. shells which at the major factor in prompting me to embark on the "Cold Fusion" saga!

If you (and A.N. Others) think that we should go ahead with all the four Abstracts have sent to you, then could you add them to yours (making changes to the English → American adding

[137] JR This, and most other references to "Stan" in the following letters, is Stanislaw Szpak, not Stanley Pons.

the title to reference (2)) and send them by Fax, post or e-mail to Peter Hagelstein / Scott and Talbot Chubb / Jed Rothwell or whoever else may be dealing with the Abstracts / Programme,

THANKS in advance !

P.P.P.S. We you should have a 'phone conversation.

BACKGROUND TO COLD FUSION : THE GENESIS OF A CONCEPT

(This abstract repeated; same as above.)

2003-05-26

Bury Lodge heading

Dr. Melvin H. Miles,
Bates College,
5, Andrews Road,
Lewiston,
Maine, 04240-6092,
U.S.A.

May 26, 2003

Dear Mel,

Herewith now a revised and somewhat fuller version of the Abstract on A CALORIMETRIC INVESTIGATION OP THE FLECTROCHEMICAL Pd-D CODEPOSITION SYSTEM. As I told you when I sent you the previous version, I was sure that this Abstract would have to be changed. In due course, I decided to first "bite the bullet" and to write the text of OUR PENULTIMATE PAPERS ON THE ISOPERIBOLIC CALORIMETRY OF THE Pt-D_2O AND Pd-D_2O SYSTEMS. PART III: THE Pd-D CODEPOSITION SYSTEM. I am now enclosing this paper and you will see that it is based on my letter to you, Stan and Pam of 29th January 2001.

I think that this completes the urgent part of our endeavours. What is to follow ?

Have a good trip back to California but 3500 miles—Grr! Are there good Inter-states all the way? No doubt you will send me your comments in due course!

Regards to you and Linda,

Martin

P.S. I forgot that today is a Bank Holiday so Wood + Co. is shot. I will send this tomorrow and hope that you will still get this before you leave. I will send a duplicate by letter to California.

P.P.S. It is obvious that we should have had an intensive effort on the co-deposition systems. A good starting point would have been deposition onto Cu ?? of various diameters.

A CALORIMETRIC INVESTIGATION OF THE Pd / B SYSTEM Abstract repeated.

Draft of OUR PENALTIMATE PAPERS . . .

2003-06-19-paper-submission

Bury Lodge heading

Professor L.M. Peter,
Department of Chemistry,
University of Bath,
Bath,

June 19, 2003

Very blind copy!

Dear Laurie,

As you will see, I am sending you three copies of a paper OUR PENULTIMATE PAPER ON THE ISOPERIBOLIC CALORIMETRY OF THE Pt-D_2O AND Pd-D_2O SYSTEMS. PART 1: THE Pt-D_2O BLANK SYSTEM.

Authors ; M. Fleischmann and M.H. Miles.

Mel Miles and I would like you to consider this paper for publication in the Journal.

As you might consider the events surrounding the writing (and publication of the paper !) are not quite straightforward or, perhaps one should say, they are not as straightforward as they ought to be. The material covered in the paper (the instrument function of the type of isoperibolic calorimeter which we have been using and the somewhat related question of enthalpy production by the parasitic reduction of electrogenerated oxygen) is obviously related to the question of enthalpy balances in the Pd based-D_2O systems which we have been investigating. I think that it would be true to say that many members of the Scientific Community (including many Journal Editors) have taken deep exception to our interpretation of these enthalpy balances in terms of nuclear reactions - even though this matter is really quite straightforward, as I can explain to you if you should wish me to do so. The "belief system" has therefore grown up that the isoperibolic calorimetry which we have been using must be inaccurate and/or that excess enthalpy production must be due to the parasitic reduction of oxygen. At least, those are two of the pillars used to justify the rejection of papers although I don't think that some of the critics believe the validity of their own arguments. These views are often supported by citing monumentally misconceived and generally awful papers: I don't believe that any of the critics has ever attempted to measure the excess enthalpy generation due to the parasitic reduction of oxygen.

I think it is important therefore to address the twin issues of the instrument function of isoperibolic calorimeters and the enthalpy production due to the reduction of oxygen in a system which has nothing whatever to do with the Pd type-D_2O system. Furthermore, one has to use standard methods of data analysis as we have done in this paper (and which are the cornerstones of the ICARUS methodology). I say this because our first full paper on the subject: M. Fleischmann, S Pons, M.W. Anderson, L.J. Li and M Hawkins, J. Electroanal. Chem., 287 (1990) 293 did in fact contain much of the relevant information. However, the analysis was

based on nonlinear regression which is evidently a topic which is not understood by research workers; one must hope that they do at least understand linear regression!

I think it is very important to establish the basis of isoperibolic calorimetry for reasons which are not connected in any way with excess enthalpy production in the Pd-based systems. The calorimetry of electrode reactions has remained a neglected subject even though this topic could give much fundamental and useful information and, even though, the experiments are easy to implement - hence this paper.

There is, of course, rather more to this story than I have outlined in this letter. Parts II and III of the paper have been written but I am not sending these to you because we believe that Part I must be judged on its own merits; we would not want to have any objections to Parts II and III to be used as a basis for justifying the rejection to Part I.

To deal with other matters ; I have returned recently to the topic of the nucleation of new phases on microelectrodes. You will recall that Li originally found a simple behaviour for the nucleation of α-PbO_2 in two restricted ranges of overpotentials but an evidently more complex behaviour at other overpotentials. Stan Pons and I then spent a great deal of time and effort on trying to see whether this behaviour at "other" overpotentials might be due to complexities in the nucleation of the new phase i.e. we stayed strictly within the accepted view that such systems should be modelled as

Nucleation — — — — — — — — — — → phase growth. (A)

I think it would be true to say that our endeavours were uniformly unsuccessful. Have you by any chance done any more work on such systems ?

This therefore brought us to the next phase of the investigation which should have been the investigation of the model

nucleation — — — — → growth of coherence domains — — — → phase growth (B)

However, our preoccupation with the "Cold Fusion" saga just put a kybosh on all the other research projects. The importance of (B) as compared to (A) lies in the fact that one interpretation of the Quantum Electrodynamics of Condensed Phase Systems leads to the conclusion that Coherence Domains must be established in a matrix of less ordered material. I did not know about this work when I started my endeavours (to find illustrations of the need to invoke the Q.E.D. paradigm) in the early 1960's ! In truth, theoretical formulations had not started at that time but a Theoretical Physicist, Gerald Fowler, whose principal interest was in O.C.D. said "it is quite obvious, the explanation must lie in Q.E.D." I replied : "just so, but how?" and this remained a mystery until I met Giuliano Preparata and Emilio Del Giudice in 1989. (their work really dates from the mid-80's).

Anyway, in due course I found four strands which illustrated the need to invoke Coherence Domains when considering aqueous solutions (there are six such strands now and, of course, there is "Cold Fusion" which is related to this topic). Phase growth, (B), is another aspect not connected to aqueous solutions. Anyway, it is apparent that the inclusion of Coherence Domains in phase growth is one possible explanation for Li's strange results. I should add that this does

not prove their existence but it does point to an important new line of investigation i.e. the probing of the kinetics of growth of Coherence Domains versus that of Phase Growth. I should add that I have gradually become convinced that many of the strange results of the studies of nucleation are due to the fact that we do not study this process at all but rather the kinetics of growth of Coherence Domains or, at any rate, nucleation heavily corrupted by the second process. It is the advent of microelectrode substrates (coupled to instrumentation suitable for making measurements in the *f*A [138] range) which makes studies of such kinetics a reasonably straightforward matter. Of course, there are also the knotty questions of how such studies might be related to results obtained with electrodes of conventional size.

You may wonder why I hadn't raised these questions with Steve Fletcher? He is undoubtedly a very clever fellow but I believe that he is completely trapped within the framework of existing theory. He hadn't even understood that the advent of microelectrodes opened the way for the study of individual nuclei/growth centres. This led him to develop his rather strange versions of electrodes of macroscopic size. I thought that any attempts to discuss these topics with him would be quite counter productive.

I shall shortly have to visit Bath again for a discussion with Frank Walsh. Will you be around in June / would you like to have a discussion about phase growth - or anything else for that matter ?

I trust that all is well with you and the family !

Regards,

Martin

P.S. I actually wrote this letter at the beginning of the month so my visit to Bath is much delayed. I should now ask you whether you will be in the University at the beginning of July?

[138] MM *f*A = femto Amp

2003-06-19

Bury Lodge heading

Dr. M. H. Miles,
P.O. Box 1033,
Ridgecrest, CA 93556,
U.S.A.

melmiles1@juno.com ← *by Air Mail in the letter packet.*

Dear Mel,

Herewith now some comments on "positive feedback" versus "quenching". The first is that it is always difficult to get definitive information by analysing the behaviour of the system. I think that there can be no doubt now about the existence and importance of "positive feedback" to the phenomenon of excess enthalpy generation but I think that we must be on our guard as regards using this phenomenon to explain all the phenomena which we observe.

The second point which I would make is that it is best if we confine attention to the first half of the experiment for a reason which I will explain shortly. If we examine the data in Table 5 we find that the temperature rises of ~2°C due to the calibration pulses lead to a lowering of the heat transfer coefficient of about 0.01 x 10^{-9} WK^{-4}. If we now examine the original spreadsheets we see that the temperature changes due to the addition of D_2O are also of the order 2°C. If this should lead to instantaneous changes in the heat transfer coefficient, then we would expect to see increases of the heat transfer coefficient of the same order of magnitude, $0.01 \times 10^{-9} WK^{-4}$. However I doubt whether there can be such "instantaneous changes" due to the phenomenon of positive feedback (the evidence thus far is that such changes are established over rather long timescales).

This semi-quantification of the phenomenon shows that we must exclude the second half of the experiment because of the effects of "positive feedback" become confused with long term changes in the heat transfer coefficient - perhaps we can return to the second part of the data sets at a later date?

If we now examine the variation of the "lower bound" heat transfer coefficient, $10^9 \left(\overline{\overline{k_R{}'}}\right)_{11} / WK^{-4}$, with time, Table 5 of Part II and the revised Fig. 3, we can see that the changes in the coefficient are much larger than any which could be attributed to a*n* ~~reduction~~ *increase* [139] in $10^9 \left(\overline{\overline{k_R{}'}}\right)_{11}$ due to a cancellation of the effects of positive feedback coupled to changes due to the level of electrolyte due to the addition of D_2O (see especially Days 4, 8, 12,

[139] JR This handwritten correction probably made by MF but it could be MM.

15, 25, 32 and 33). Day 14 is anomalous in this sequence. Why ? This requires further investigation. On the other hand, the changes on Days 16, 18, 20 and 22 are quite minor. I therefore believe that you started the experiment with D_2O contaminated with HDO, which you continued to use to top up the cells and changed to a bottle of D_2O having a sensible HDO content round about Day 15. The evidence about the bottle you brought into use on Day 25 is confusing : Days 25, 32 and 33 indicate that this sample of D_2O was contaminated to HDO, the evidence on Day 26 indicates that it was not but the changes on that Day may have been due to "Heat-after-Death". If we go beyond the half - way mark, then I could guess that you used a new bottle Days 35 and 36 having a sensible composition, reverted to a contaminated bottle on Day 39 and again switched to D_2O having a sensible isotopic *purity* round about Day 46. This was used up to Day 59 but (Day 60 again indicates contamination of the D_2O by HDO. I would prefer not to comment on the results from then until the end of the experiment.

The evidence from the analysis of the experiment therefore ties in rather well with the information in your letter of 24/05/03. Would you like to hazard some guesses about the later stages of the experiment?

The question of the value of the "true" heat transfer coefficient is somewhat related to these matters. If you could go back to the spreadsheets for Days 60 and 61 you will see that $10^9\left(\overline{\overline{k_R{}'}}\right)_{11}$ reaches 0.88587WK^{-4} at the start of Day 61. However, the first reliable value of $10^9(k_R{}')_{11}$ is 0.94158WK^{-4} (N.H.E. calculation). If we therefore want to ensure that there is no violation of the Second Law of Thermodynamics at any time during the experiment, then we must use a value of the "true" heat transfer coefficient close to 0.94158 x 10^{-9} WK^{-4}. However, the matter is not crucially important ; a much smaller value will still indicate excess enthalpy generation if we exclude Day 61 from the results.

Are you roasting ?

Mike Clarke [140] will try to send you this letter from his computer, I am still not hooked up !

Regards,

Martin.

[140] JR Michael Clarke lived near Fleischmann and often assisted him with the computer and various other things, as I described in the Introduction.

Bury Lodge heading

June 19, 2003

First

Mel: I actually wrote this letter at the end of last month

19/6/03

Dr. Melvin H. Miles,
P.O. Box 1033,
Ridgecrest, CA 93556,
U.S.A.

Dear Mel,

Welcome back to California and I trust that your marathon was not too unpleasant !

revised version of parts I, II and III enclosed - Also copies of letter to Lavoie Peter.

I am now sending you a copy of the Fax I sent to you on 27/05/03 and I hope that you received this. Thanks for your letter of 24/05/03. I see that Parts II and III need some corrections and I will reply in detail about the topping up of the cells (Part II) in due course. Fig. 3 of Part II is inaccurate (although I don't believe that the inaccuracies affect the conclusions I reached at the time of writing Part II) and I will reconsider the question of the contamination of the electrolyte when I correct this figure.

Many thanks also for writing to Jim Corey. I believe that the question of the background to the work on C.F. ought to be discussed although I can foresee the spate of new trouble and tribulation which will be heaped upon us.

I see that I did not comment on your ideas of starting work independently in California. This is of course an excellent notion ! In 1995 I developed some rather grandiose ideas about starting such a venture here in Tisbury but the promised support never materialised. Ed Storms has managed to keep his head above water! How about a public subscription company ? The public interest certainly seems to be intense judging from the response to the papers on the web site. If you do manage to start such a venture, then the logical extension of the Stan Szpak - Pam Mosier-Boss ideas could form a low cost cornerstone ? Moreover, such a topic would have the advantage of making the venture independent of the vagaries of the suppliers of electrodes. If you should want to continue with Calorimetric measurements, then the co-deposition of Pd and D onto smaller Cu substrates could be a good starting point. It would be straightforward to use say 1 mm diameter, 1.25 cm length Cu wires which would allow the raising of the current density to 1 – 2 A cm^{-2}. This should allow the exploration of the rate of the specific excess

energy production up to the 1 - 2 $KWcm^{-3}$ range. Codeposition onto thin films of Cu deposited on fibres (electro less deposition of Cu ?) should allow one to make a linkage to electrodiffusion and the Preparata-Del Giudice-De Nino work. Of course I am still strongly drawn to the notion of using a Cu or Ag fluidised bed (electrodes deposited onto balotini glass beads). If you should need specialised glass ware, then I could have this constructed here.

Did I ever tell you that I eventually approached Mr. Namba to see whether any of the equipment being used in Sophia Antipolis might still be available ? There was no reply! However, you might have better luck with the ex-N.H.E. brigade, Matsui, Asami, Ikegami etc.. An approach to Ikegami might be most straightforward. At the very least, their replie(s) would be interesting - even lack of replie(s).

I am now drawn to the notion of submitting Parts I, II and III to the Journal of Electroanalytical Chemistry - I think the notion of submitting these to Electrochimica Acta is a complete waste of time? To this end, I am rewriting the Introduction to Part I to remove the histrionics. I will send you updated texts of Parts I, II and II in due course. We could, of course, ask Jed Rothwell to put these papers on the web in which case it would be appropriate to reinstate the histrionics ? [141]

Regards to Linda !

Yours,

Martin

[141] JR Yes, insofar as this probably would attract more readers.

2003-06-20

Bury Lodge heading

Dr. M. H. Miles,
P.O. Box 1033,
Ridgecrest,
CA 93556,
U.S.A.

20th June 2003

Dear Mel,

I trust that you and Linda are enjoying your stay in Oregon !

As you will see, I am sending you a miscellany of material. First of all, there are the revised versions of Part 1, II and III and of these, Part 1 is the most important for the present. You will see from my very blind letter to Laurence Peter that I have decided to submit these papers (at least Part 1) to the Journal of Electroanalytical Chemistry. I believe that this is really the most important Journal for this work in view of the earlier publication record. Also, I think that pursuing Electrochimica Acta / Elton Cairns would be a complete waste of time. In view of the decision to try to achieve publication in J. Electroanal. Chem. I have substantially rewritten Part I. I believe that we should keep the deleted material as ammunition for any potential future correspondence.

It is most important, therefore, that you should send me your comments about the revised Part 1 so that I can take them into account in the submission of the final version of the text.

I have at the present only carried out a partial revision of Parts II and III but it would be very helpful if you could mark up the copies and return them to me - especially as I have not taken into account some of your previous comments. You will see that I have tried to take on board one of your most important previous comments by extending Footnote 10 of Part II- will this do?

One matter which is extremely important is that I have just heard from Stan Szpak that the paper "Thermal Behaviour of Polarised Pd/D Electrodes Prepared by Co-Deposition" has been accepted by Thermochimica Acta – I was astonished! Does this in any way queer the pitch / change the publication plans for Part III? This paper was written based on the hypothesis that this particular important study would not see the light of day unless it was part of a series of papers - what do you think? If you have any trouble with J. Electroanal. Chem. then should we switch to Thermochimica Acta for Parts I and II (also an embryonic Part IV - see below)?

Does the acceptance of the paper by Thermochimica Acta point to a change in the attitudes?

You will also find two letters addressed to you which were actually written at the beginning of the month. However, they were then overtaken by another bout of ill health (including a spell in hospital). I am waiting until the end of the month to see whether I will be sufficiently fit to go to ICCF 10. If Sheila and I stick to our original plans, then we might go to Maine after the meeting in Cambridge. Where would you recommend us to go?

Now as to Part IV, in my searches through the material I have here I found the data set which was given to G.E. in 1989 (I believe in August of that year). The importance of that data set is many fold; first of all, one can see that there had to be excess enthalpy generation (~20Wcm^{-3}) in a 0.1cm diameter x 10cm length electrode just by eyeballing the data - no need to carry out any calibration. It would be useful therefore if Part IV were written partly from the point of view that it is easy to obtain semi – quantitative information about excess enthalpy generation so can you send me the reference to your paper showing that one can get increases of temperature with decreases of enthalpy input? Thanks in advance! Secondly, the series Parts I-III is definitely light with regard to the initial studies. Thirdly, G.E. should of course have paid up and this would have allowed Stan Pons and yours truly to complete the first part of the study. Instead, we got J. Electroanal Chem. 332 (1992) 1 based on a misrepresentation of the work in J. Electroanal Chem. 287(1990) 293. The authors tried to keep the existence of their paper hidden from us. I am tempted to say "what is good for ENRON is not good for America". Of course, I don't think that we should name G.E. but it will all emerge in due course.

I have had a Fax from Jim Corey and have also had a lengthy telephone conversation with him. I think that it would be true to say that he can't see any difficulties in presenting the work in the Abstracts of the papers for ICCF 10 but I believe that he is somewhat uneasy. The real point is that people can't see the connection to the D.U. shell saga and, of course, they don't understand Q.E.D. and especially Q.E.D. applied to condensed matter. I don't either but I understand it sufficiently well to know that the Scientific Community now faces a major hurdle.

I will tell you about a "bonzo" [142] experiment when (hopefully) we meet up in Cambridge. Have a good holiday and perhaps plant a tree for me!

Regards,

Martin

[142] MM Bonzo (?) JR British slang for "crazy"

2003-07-01

From:

Professor L M Peter
Editor
Journal of Electroanalytical Chemistry
Prof. L M Peter, editor
Department of Chemistry
Bath BA2 7AY
United Kingdom

Professor M. Fleischmann
Bury Lodge
Duck Street
Tisbury
Salisbury
Wilts.
SP3 6LJ

01/07/2003

Reference Number: LP
Title: Our Penultimate Paper on the Isoperibolic Calorimetry of the Pt/D2) and Pd/D2) systems
Authors: M. Fleischmann and M. Miles

Dear Martin,

Thank you for your letter and the manuscript.

In view of the fact that it is connected, albeit indirectly perhaps, with the 'cold fusion' controversy, I have sent copies to my co-editors so that we can reach a firm decision on whether it should be considered for publication in JEC. Clearly this will need some discussion.

Best regards,

Laurence Peter

Laurence Peter

2003-07-04

Bury Lodge heading

4th July 2003.

Dr. M. H. Miles,
Somewhere in Oregon.

FAX . . .

also to . . .

> *I have been unable to contact your FAX Numbers (4th July?). I am therefore sending this one by pigeon post.*

Dear Mel,

Your FAX from Select Designs is just to hand and I will send you a reply in due course. As you will see, I am trying to use your Oregon FAX number 154-186-62402 but it does not seem possible to me to have such a number. I am therefore also sending these FAXes to Ridgecrest.

I still would like to have a further copy of your preceding FAX.

Regards,

Martin

4th ~~October~~ [July?] 2003.

Dr. M. H. Miles,
FAX address in Oregon,
via P.O. Box 1033,
Ridgecrest, CA 93556.

Dear Mindy (?),

Could you please send this FAX on to Mel's number in Oregon? I have mislaid Mel's last letter which gave the Community FAX number. Many thanks!

Dear Mel,

You will see from my message to Mindy (?) that I received your last FAX from Oregon but that I have mislaid it - one of my periodic disasters so I now do not have your local Community FAX number. Could you please send me a repeat of this FAX?

I trust that you and Linda are having a good stay in Oregon but don't overdo the forestry! Sheila and I have recently had one of our periodic bouts of trying to "downsize" which included a rather nasty house in Dorset with an immense garden. We decided that we were too old to take this on.

When I came out of hospital, I sent you a rather large letter package. Unfortunately the date (20th June) must have overlapped with your move to Oregon, so I hope that you received this package. Could you please let me know where I should send a duplicate if my previous letter has gone astray? The letter covers some matters which we need to consider rather urgently.

Sheila and I decided yesterday that I am sufficiently well to go to ICCF 10 so we shall put it to the test. I only hope that I will not have a repeat episode in Cambridge/Cape Cod/the White Mountains.

The main reason why I am writing to you today concerns Part IV of the series of papers. The background to my decision to write this Part is outlined on page 2 of my letter of 20th June 2003 and the present letter is concerned with more aspects of the background. I have now completed the evaluation of this experiment (which must have been started in June 1989) so I am contemplating writing the paper (which I would hope to complete before ICCF 10). The main point is that one can see that we must have observed excess enthalpy generation of order 10W cm^{-3} just by eye balling the data. As I told you in my letter of 20/6/03 I want to extend the comments on eyeballing the data by referring to work showing increases of temperature with decreases of enthalpy input so could you please send me the reference to your paper - better a copy if you should have it with you.

The detailed evaluation fits in with the preliminary assessment and also shows the presence of "positive feedback". It is likely that one section of the data would also demonstrate "Heat-

after Death" but, at present, I do not intend to complete this evaluation. The next important point is that we gave this data set to G.E. in response to the wish of the University to cooperate with industry (which I considered to be misguided right from the start). The key point is that G.E. promised the University a rather large [sum] of money provided we had achieved a stated performance figure. We therefore had to establish two figures: what exactly was the sum of money and what exactly was the performance figure? Two people should know the answers to these questions. One is the ex-President of the University, Chase Peterson, [143] who now lives in Park City, the other is Wilf Hansen. I wonder therefore whether you could approach Chase and Wilf to see whether they could / are prepared to answer these questions? If you could do this, you should reassure them that we do not intend to name G.E. (although this is all rather common knowledge) but that the answers to these questions will, of course, have a major influence on the way in which Part IV will be written. I just want to see whether G.E. will rise to the bait! You could also tell Chase and Wilf that we will naturally send them a copy of the paper before deciding whether and where it would be submitted.

Incidentally, there were some rather self evident deficiencies in the experiments we carried out in the Summer of 1989 which caused us to redesign the experiments. Should this be made clear in Part IV?

Incidentally, incidentally, part of the methodology used in the analysis was also used for reanalysing the Harwell data sets. A paper was written on this subject but was rejected for publication! Again, should this be made clear in Part IV?

Incidentally, incidentally, incidentally, I have had no acknowledgement from Laurie Peter. Strange? but perhaps we should say "no news is good news". I will be very interested to see how the referees will try to concoct negative comments on Part I!

Regards,

Martin

[143] JR Chace Peterson (1929 – 2014). See: Peterson, C., *The Guardian Poplar, A Memoir of Deep Roots, Journey and Rediscovery*. 2012, Salt Lake City: The University of Utah Press. http://lenr-canr.org/acrobat/PetersonCtheguardia.pdf

2003-07-28

Bury Lodge heading

July 28, 2003

Dr. M.H. Miles,

somewhere in Oregon!

I have had a further slippage in time so I am sending this to California

Dear Mel,

Many thanks for your letters, Faxes, and corrections to Parts I, II and III. Part IV "An Experiment with a Pd-Cathode in 0.1M LiOD/D_2O carried out in 1989" has now been written. There are some residual calculations but it should all be ready by the end of next week. I would like you to have this paper in good time before the meeting in Boston so I intend to send it by Air Mail to Mindy's address. I suspect there may be an error in the address I have for her so could you please send me the correct one (to Wood & Co.)? Many thanks.

I will bring the corrected versions of Parts I, II and III with me to Boston and we can copy it there. Peter Hagelstein tells me that the proposed new Journal is "ready for the off" so one option would be to publish Parts I, II, III and IV in issue 1 of that Journal. What do you think? Another option would be Thermochimia Acta. Peter tells me that attitudes to C.F. our changing and it will be interesting to see what Steve Jones has to say at the meeting - perhaps these changes in attitude explain the new position with regard to Thermochimica Acta?

I have a letter from Laurie Peter (copy attached) which I regard as being rather unhelpful. We don't need or want comments from the co-editors, we want responsible refereeing. It will be interesting to see how the co-editors will try to squirm out of the consideration of the text of Part I for publication! If they try to do so, I will insist on having signed letters from them. Should I write again to Laurie Peter ahead of ICCF 10? Note that he has not taken up the questions raised by crystal growth (a joint paper with L.J. Li) so this would be a means of returning to the correspondence. However, I am somewhat in favour of awaiting developments although time does go by.

The evident problems which we will have with J. Electroanal. Chem. makes me wonder whether it would not be more straightforward to proceed with Peter's Journal or else with Thermochimica Acts We will need to consider this carefully at ICCF 10.

Peter would like to get in touch with you so I will give him your Fax numbers - I hope this is O.K. I suspect that he will want to persuade you to be suitably uncritical of Steve Jones so that he can come away from ICCF10 "smelling of roses". I have told him that I will say nothing or else that I will be entirely constructive if the opportunity should arise. Did you ever notice that I have many other cheeks that I can turn?

Your letter from Christopher J. Richards. The problem with the poor design of the Caltech cell is typical of the way in which the subject got off to a poor start. I believe that have written to you before about this topic? If not, then could you please let me know? The difficulties with Caltech are an object lesson of how research should not be done and I would like you to have a record of this. If you should write to Christopher Richards, you might like to tell him that a replica of the cells we used can be seen in the Museum Leonardo de Vinci in Milan.

You might also like to tell him that in 1983 Stan Pons and I decided that we could add one further topic to our joint research portfolio.

The subjects we considered were

1) a further extension of non-electrochemical methods of investigating electrode / solution interfaces especially in-situ N.M.R., x-ray diffraction and microwave spectroscopy.

2) relativistic effects in chemical processes!!

3) the behaviour of electrons in metals.

4) Cold Fusion.

4) was definitely fourth on our list. However, we did not get the funding for 1) (why not?); we started on 3) but decided that we could not afford to do this project (it is actually related to 4)). Equally, we could not afford 2) so we started on 4).

Although Feynman was quite right about the difficulties of measuring mass changes in chemical reactions, he was not the first to consider this question. As a matter of fact, it would now be quite difficult to establish who was the first - it was a topic just buried in the general consciousness. However, although Feynman was right about the difficulties, his (and other people's) comments should not be taken to mean that it is impossible to measure such mass changes. Stan Pons and I could think of two ways in which this might be attempted and this was part of the proposed second research project!

Did you send me the letter from the Academic at Cornell? I put this aside intending to write to him after completing part IV but I now can't find the letter ! If it was you, then could you please send me a duplicate? As I recall he had difficulty in believing that the voltage shifts which one can observe on Pd cathodes are equivalent to achieving a D_2 pressure of 10^{27} atmospheres. He is not the only person who has had difficulties with this concept but the matter is really rather straightforward. In the 1970's we had a project on the deprotonation of weak carbon acids using "hydride ions" generated by the reduction of hydrogen in Pd-H i.e.

$Pd\text{-}H + e \rightarrow Pd + H^-$ ← whatever that may be

$H^- + RH \rightarrow R^- + H_2$

i.e. we wanted to generate carbanians under otherwise relatively mild superbasic conditions at these electrodes. There is a great deal more to this story which remained largely unpublished (for a variety of reasons some of which are related to C.F.).

As part of this project, we made measurements on diffusion membranes and found that we could shift the potential of an outgoing interface to -0.81V by polarising the ingoing interface - hence we would need 10^{27} atmospheres of H_2 to achieve the same effect chemically (even more than 10^{27} atmospheres as this was the thermodynamic limit). As a matter of fact we could even achieve ~ -2.1V under special conditions: Quite mind boggling but all this really shows is that electrochemistry is an high energy chemistry which cannot be matched by more conventional methods.

We gave up this project partly because of the advent of C.F. and partly because our real target was the deprotonation of weak carbon acids with pKa's in excess of 40, conditions which we could never achieve. It's a pity that we gave this up!

My correspondence and conversations with Jim Corey are in abeyance, I think it would be true to say that he is uneasy about some of the Abstract of "Background to Cold Fusion; the Genesis of a concept" although I believe that no one can really say that this material should not be presented - it is more a question of what else this might point to. So may I ask you to read this Abstract again very carefully taking due note of what I said at Villa Marigola [144] and tell me whether I should go ahead and / or whether some of the material should be deleted.

You will recall that in 1988 I had 10 reasons for wishing to see this project classified and that Stan Pons and I wanted to go to a National Lab in 1989 to work on one of these reasons (when I wrote to John Bockris recently I found there were 13 reasons - I must have been very conservative with my divisions in 1988). When we meet in Boston remind me to tell you about one of these reasons; it should have interested folks at China Lake.

Have a good remaining holiday at Ridgecrest. Incidentally, I can't find La Verne on my rather inadequate Atlas of the U.S.A. Where is it?

Many thanks for agreeing to deal with Chase Peterson and Wilf Hansen. I will send you a covering letter when I send you Part IV.

There is now a great deal of interest in trying to devise experiments which move the space-time with the object of the experiment. The conventional view is that such effects only become important at scales of the order of the Planck length, say 10^{-34}cm. I think this is probably correct although I have at times wondered whether space-time effects might not turn up in the boundary conditions. In the meantime, I have enough on my plate with Q.E.D.; two paradigms based on Second Quantisation.

Regards,

Martin

P.S. I see that I should have said somewhat more about the possibility of allowing for gravitational effects in nuclear processes. The conventional view (again) is that these take place

[144] MM ICCF-8, Italy, 2000

on timescales 10^{-20} to 10^{-22} s (certainly no shorter than 10^{-24} s) whereas the time scales associated with the Planck length are of order 10^{-33} s (I am quoting from memory). Space - time effects are therefore likely to be unimportant; gravitational waves therefore require extreme conditions such as the explosion of supernovae. I note though that I should revisit the late work of Jo Weber who believe that such effects might become accessible in hadronic supercolliders. (I thought not at the time). Incidentally, I tried to persuade Jo to come to Sophia Antipolis to construct a gravitational wave detector capable of seeing every supernova in the Universe. This came to nought - he was only interested in building a neutrino communication system, a project which could certainly have been brought to a successful conclusion.

P.P.S. I need to ask another big favour from you. I need to get my U.S. Social Security number which I have lost! Do you by any chance know who I should write to at the U. of U. to get this number? Alternatively, if and when you get in touch with Chase Peterson, could you ask him for the name and address of the appropriate person in the U. of U.? Many thanks in advance!

2003-09-12

12/09/03

Bury Lodge heading
Dr. Melvin H. Miles Department of Chemistry,
University of La Verne,
California, U.S.A.

Dear Mel,

My brother-in-law and I went to New Hampshire and Vermont after the meeting. As he had already booked all the hotels, we could not take up Charles Beaudette's kind offer to visit him in Maine. Our visit to the White and Green Mountains was most enjoyable except for Mt Washington where the visibility was 50 ft, the wind speed 75 mph and the temperature was 45°F. However this is par for the course. What will the Appalachians be like in 10,000 years time if the mountains continue to rise at the present rate?

On coming back to the UK I found that I had indeed heard from Laurie Peter, a copy of his letter attached. This is much along the lines which I expected: he and the sub-editors are clearly in a dead funk at the notion that they should seem to endorse anything remotely concerned with "Cold Fusion". It is all very unsatisfactory and a sad commentary on the decay of Science in our present day Society.

It seems best, therefore, if we try to go ahead with the plans to publish Parts I-IV in Thermochimica Acta and I will send you a corrected version of Part I early next week. I would suggest that we should wait to see what the response of Professor Craig may be to Part I before we send him Parts II-IV. In the meantime, if you could send a version of Fig. 3 of your paper in J. Electroanal. Chem. 482 (2000) 56 (together with a suitable Figure legend), I will incorporate this as Fig. 2 into Part IV (thanks for your Fax of 7th September 2003).

Having reached this decision, I have also decided to write once again to Laurie Peter (blind copy attached) as I would like to document the happenings in connection with this particular paper, Part I. Have you any comments and would you like to act further in this matter ?

The problem with your measurements of the reflux ratio outlined in your Fax of 07/09/03 is that the cell temperatures were really rather too low to allow definite conclusions to be drawn. I decided (a long time ago now) that the cell temperature had to be above 85°C before cooling due to evaporation became a significant thermal pathway. However there are clearly some very serious difficulties with regard to the mass balances in the cell and the ICARUS 1 and 2 cells are unsuitable for making valid estimates of these balances. We therefore developed the ICARUS-9 calorimetry to cope with this problem. It strikes me that we should investigate this whole problem once again and I will write to you further in due course.

I trust that your teaching is going well and that you are enjoying your return to

California,

Best regards,

Martin

From:

Journal of Electroanalytical Chemistry
Prof. L M Peter
Editor
Department of Chemistry
Bath BA2 7AY
United Kingdom

17/08/2003

Title: *Our penultimate paper etc.*

Authors: M. Fleischmann and M.H. Miles

Dear Martin

I sorry to have taken so long to reply to your letter submitting the manuscript dealing with the calorimetry of the Pt/D_2O system. I have been involved in consultation with my fellow editors about the paper for two reasons. Firstly, although the paper deals with a 'blank' system, it is clearly connected with the cold fusion controversy, both historically and in the general perception of any scientific audience. Secondly, the paper deals with a detailed analysis of errors for a calorimetric experiment, and the electrochemistry is in a sense relatively peripheral. As Editors, we have agonised over this paper, because all of us believe strongly in freedom to publish. I should stress that our unanimous decision is based on detailed reading of the paper, and not on the controversial history of cold fusion. We note that the style of this MS is different from usual scientific papers, in that it gives very few references and does not cite any work that has criticised the methodology or conclusions of your earlier work on calorimetry. The lack of detailed description of the ICARUS data acquisition system also makes it difficult to follow the argument (I, understand that the company does not exist any more, so that background information will not be publicly available). In conclusion, we all feel that since the paper deals with high precision calorimetry, it should be submitted to a journal devoted to this field, where it can be scrutinized by specialists. I am therefore returning the MS with this letter.

Yours sincerely

Laurie

Laurence Peter

Bury Lodge heading

11/09/03

Professor Peter Hagelstein,
Department of Electrical. Engineering and Computer Science,
77, Massachusetts Avenue,
Room 36 - 225,
M.I.T.
Cambridge 02139
U. S.A.

Dear Peter,

My brother - in - law and I had a very good visit to the White and Green Mountains following the Meeting except for Mt Washington where the visibility was 50 ft, the wind speed 75 mph and the temperature was 45°F. However, I believe that this is par for the course. I understand that the Appalachians are a rather recent creation. What will Vermont and New Hampshire be like in 10,000 years time if the mountains continue to rise at the present rate?

I am now writing to you to say "thanks for a very good meeting" - remarkable under the circumstances. I find though that I missed one of the best papers, the substitution from Israel. When this presentation started with complex wave patterns I thought: "right, I'll go and have a cup of coffee" and came back at the end of the paper. However, Mike McKubre told me that it was the best paper at the meeting and that the Israelis have done everything. Therefore, if you should have or get an early copy of this paper, then I would really like to have sight of it!

I have just heard of the death of Edward Teller - a truly remarkable man. It was a pity that he was getting to be so old; he might have been turned into a friend of our subject area though whether this would have been helpful is debatable. His career is an object lesson of triumph in the face of adversity. Remind me to tell you something about the events of 1989 when we next meet. This could well turn into a moan into what I perceive to be a faulty military doctrine being pursued by your countrymen.

There is one outstanding matter which I meant to raise with you at the meeting. What are your publication plans for the papers presented at the meeting? I am rather keen that the one on "Background to Cold Fusion: the Genesis of a Concept" should see the light of day because it reveals the true motivation for the research, a matter which I have been singularly reluctant to discuss hitherfo. I noted that I could not draw any comments from our Russian colleagues although I know that some of them have worked on closely related topics. Equally there were no comments from certain U.S. personages.

The other paper is basically a rehash of a paper "Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O Pd/D_2O Systems. Part I: the Pt/D_2O Blank System" which I brought with me to Boston (I also brought Parts II, III and IV). Although this paper makes no mention of "Cold Fusion" it has been rejected by the Editor and Sub-Editors for a reasonably prestigious

Scientific Journal. Have you words for this strange and cowardly behaviour? Of course, what these people can see quite clearly is that if our assessment of the precision and accuracy of Isoperibolic Calorimetry is correct, then our claims for the Pd/D_2O systems (and related systems) are substantiated - which is outlined in Parts II, III and IV (data for Parts II and III obtained by Mel Miles in Japan, data for Part IV taken in 1989 in Utah). The objective appears to be to stop me from publishing anything i.e. to make me drop from view.

Otto Reifenschweiler 'phoned me yesterday to say that he has written a paper on tritium generation leading to a definitive test for the existence of "Cold Fusion". You may recall that he did some interesting work in the late 1930's on the temperature dependence of the tritium decay rate, work which was endorsed by Professor Casimir of the Phillips Research Centre. As far as I can tell this work was undertaken in connection with that on self-gettering neutron tubes, devices which have rather mysteriously disappeared from view although they are still produced. It appears that Otto Reifeitschweiler's reasoning is based on thermodynamics which I find rather a strange approach. I have asked him to send me a copy of this paper.

He asked for my advice as to where he might get this paper published and I told him about the e-mail publication and your plans (or are they now actual?). I will give him your address if and when he writes to me.

Best regards,

Martin

P.S. If you should produce a hard copy version of the papers presented at the Meeting could you get an ISBN number for the publication? This is a useful device for stifling the opposition to the subject area.

2003-10-24

Bury Lodge heading

24/10/03

Dr. M.H. Miles,
Department of Chemistry,
University of La Verne,
1950 3rd Street,
La Verne, California 91750,
U.S.A.

Dear Mel,

Herewith now a copy of our joint paper on

"The instrument Function of Isoperibolic Calorimeters; Excess Enthalpy Generation due to the Parasitic Reduction of Oxygen".

which I presented at ICCF 10. Have you any comments? I have also sent this paper to Peter Hagelstein but I have told him that you haven't as yet seen it (however, I have pointed out that it a "sawn off" version of Part I of our mini series and that we have agreed on the text of that paper).

Best regards,

Martin

P.S. It strikes me that you may well wish to have sight also of the other paper I presented at ICCF10 i.e. "Background to Cold Fusion: the Genesis of a Concept" so I am now also enclosing this.

2003-10-27-letter-to-Hagelstein

Bury Lodge heading

27/10/03

Professor Peter Hagelstein,
Department of Electrical Engineering and Computer Science,
77 Massachusetts Avenue,
Room 36-225, M.I.T.
Cambridge 02139,
U.S.A.

Dear Peter,

As I told you in my last letter, I would write to you in due course about your email of 14/10/03. My reactions to the proposal that there should be a further review of "Cold Fusion" by the DoE are pretty mixed - to say the least! On the one hand I can see that if there is no injection of further funding, then the field will die. This is the process of attrition which has been frequently used very successfully to stifle research in uncomfortable areas such as those impacting on the tobacco and sugar industries and is now being used by the mobile phone interests. I am somewhat familiar with the position in the sugar industries as we carried out research on some of the synthetic steps used to produce cyclamates. I am also somewhat familiar with the mobile phone saga as will become apparent from my comments below.

The technique which is used is very simple. The "powers-that-be" sponsor some very bad research (easy to do this - one can either do the research "in house" or select "independent" researchers known to be incompetent); the result is that this research fails to find any effects. The "powers-that-be" then say that the case for further research is not proven and funding is cut off. Note that the U.K.'s first Professor of Nutrition Science never received a further research grant after he showed that eating sugar was detrimental to the health of the nation!

The reason why I am wheeling out this old chestnut is that this is the likely outcome of any renewed investigation by a DoE panel.

This letter has become separated from my statement "on the one hand" but I will now cover "on the other hand". It has been true so far that the "opposition" has not been able to stifle the small amount of research being carried out with minimal resources - we have been left alone. At the same time, progress is very slow. Furthermore, the field is now at a stage where it requires a major injection of funding. The likely outcome of following the present path is therefore that the field will eventually die - certainly we can see that it will not attract younger research workers who are so essential in any field of research.

My own view has been that we should continue with this second strategy while recognising its limitations. I would only embark on the first strategy if there were some good evidence that

the “powers-that-be” want to change their minds. A crucial factor is the situation world-wide which is rather confusing at the present time. Certainly there are signs that “powers-that-be” outside the U.S. want to change their views but I don’t know whether to take this seriously. This is an important point as far as the situation in the U.S. is concerned. The common view is that the U.S. leads research but this is only true in part. A major fraction of research in the U.S. follows that in the rest of the world - the U.S. only enters those fields if it is perceived to be in the National Interest that it should do so. There are also questions of National Security. If you read the paper “Background to Cold Fusion: The Genesis of a Concept” (which I presented at ICCF10 and which I have now sent to you) really carefully, you will see that there are two questions which should be of immediate concern to those concerned with the Military Backwash of this research (there are more questions but this will do for the present!). I must confess here that this is the major reason why I wanted to start this programme way back in 1983.

My own view about what we should do now is naturally coloured by the happenings surrounding the publication of the E.R.A.B. report. I thought that this report was written to a predetermined scenario: investigations only took place in as far as they might support this predetermined scenario. None of the members of the E.R.A.B. group ever addressed a single question to me. I met one of the members at a meeting in Stockholm which made it clear that this member at least had not understood the heat transfer determining the behaviour of isoperibolic calorimeters; I met another member of the E.R.A.B. group in Gainesville, Florida, and it was clear that the member of the group had not understood the concept of the “thermoneutral potential” (also essential for understanding the behaviour of isoperibolic calorimeters).

Mel Miles could give you an excellent example of the operation of the E.R.A.B. Committee.

I believe, therefore, that you will only get “more of the same” unless there has already been a change of mind. You might also like to consider a vignette drawn from another field of research, “Bioelectromagnetics”. I recently went to a small meeting in Italy devoted to this subject (anathema to the mobile phone interests etc.) Zhadin was there (one of the originators of the field) also Libhoff, Barnes and Blackman from the U.S. The experimental evidence for cyclotron resonance is now pretty well solid but Adair has pointed out that if such resonance is interpreted classically, then this leads to impossible conclusions about the dimensions of the orbits (with the implication that the research must be wrong!). However, what Adair should have said is; “what if this research is correct?” Well then, we are clearly using the wrong paradigm and we have shown (Del Giudice, Fleischmann, Preparata and Talpo also in “Bioelectromagnetics”) that the results are perfectly explicable if we use Q.E.D. Has Adair changed his mind? Not so far. Will he change his mind and say so? I don’t think so. Negative and incorrect views are never withdrawn. As Max Planck said; “Science progresses by funerals.”

My own view has been that the future success of research in “Cold Fusion” would probably be dependent on establishing that Q.E.D. is a paradigm underpinning research in all branches of the Natural Sciences. My favourite is nucleation and crystal growth - but we have met plenty of opposition on that score also! You may wish to know that I would be prepared to write Part V of our mini – series pointing out the principles on which such research should be designed.

However, I assume that you will go ahead with the notion of asking the DoE to hold another review (and you may already have thought about and discounted my reservations). In that case, I should answer the questions (1)-(5) which you have posed at the end of your letter. The Table 1 in the paper "The Instrument Function of Isoperibolic Calorimeters; Excess Enthalpy Generation Due To the Parasitic Reduction of Oxygen" (and associated references) gives a good indication of what I would recommend. I will deal with (4) first. If the site visits are to be confined to the U.S. than the effort at S.R.I. and at SPAWAR comes to mind in the first instance. In many respects Mel Miles had some of the most interesting and promising work at China Lake but this has been stopped for some time now. I know that Mel is aiming to start again either at the University of La Verne or on his own account but I doubt whether he would have anything to show to a visiting group. The same thing is probably true of the efforts at Texas A+M. I take it though that you will be discussing with George Miley whether he would welcome a visit and, no doubt will also be exploring this question with the Navy Research Laboratories in Washington?

In many respects though, the strongest research work at present is outside the U.S. Visits to Takahashi's, Arata's and Iwamura's research efforts would be indicated and, in Italy, I would single out the project at E.N.F.L. Frascati (Del Guidice, De Ninno and Fratolillo). Visits to these Institutions would have the advantage that they might induce a feeling of disquiet in the Review Panel. The group at Frascati should be used to site visits by now and the Review Panel would benefit from learning of the interest in this work!

My selection of institutions naturally also colours my reply to your questions (1)-(3) which I will lump together. I would expect to see the Review Panel assess the theoretical work which has been done in the first place and I would welcome in particular their comments on Giuliano Preparata's book "Q.E.D. Coherence". It might well be though that the members of the Panel would not be capable to make any comments but this in itself would be very interesting. Secondly, I would like to see some assessment of the calorimetric work since this remains the major signature of the effects. Speaking personally I would like to see some comments on the paper by M. Fleischmann, S. Pons, M.W. Anderson, L.J. Li and M. Hawkins, J. Electroanal. Chem., 287 (1990) 293 as the E.R.A.B. Committee did not review this work. Failing this Mel Miles and my paper on "The Instrument Function" ---- Reduction of Oxygen" presented at ICCF10 could be substituted while pointing out that the full paper (Part I of our mini-series) is available.

One of Mike McKubre's papers on flow calorimetry would be an excellent choice especially one of the papers which also gives details of ^{4}He measurements. The paper by Chun-Ching Chien, Dalibor Hodko, Zoran Minevsky and John O'M Bockris in J. Electroanal. Chem., 338 (1992) 189 (tritium and ^{4}He generation) could be included while that by S. Szpak and P.A. Mosier-Boss, Nuovo Cimento, 112A (1999) 577 (hotspots and codeposition) would make an excellent bridge to a site visit. (Stan and Pam are doing some very interesting new work.) I would like to see some assessment of Arata's work (e.g. Y. Arata and Y.C. Zhong in Proceedings of the 9th International Conference on Cold Fusion, Tsinghua University Press, (2002) 5, ISBN 7-302-06489-X (and references cited herein) and especially that of E. Del Giudice, A. De Ninno, A. Fratolillo, G. Preparata, F. Scaramuzzi, A. Bulfone, M. Cola and G.

Giannetti, Proceedings of the 8th International Conference on Cold Fusion, Conference Proceedings of the Italian Physical Society, 70, (2000) 47, ISBN 88-7794-256-8. This work brings the Quantum Mechanical effects of the vector potential into consideration. I would especially like to see the Review Panel's explanation of the melting of electrodes (although boiling of the Pd is probably closer to the mark) in the absence of excess heat generation (my own estimates of the specific rate of excess enthalpy generation lie in the range of 0.5 – 50MWcm^{-3} with the upper end of the range being more likely than the lower end).

You will wish to include some work on charged particle emission although other people will be better qualified to advise you on that score.

It would also be useful to include papers on related matters. The paper by Yuji Isobe, Shigeo Uneme, Kahou Yabuta, Hiroki Mori, Takayuki Omote, Satoshi Ueda, Kentaro Ochiai, Hiroyuki Miyamaru and Akito Takahashi in Proceedings of the 8th International Conference on Cold Fusion, Conference Proceedings of the Italian Physical Society 70 (2000) 17, ISBN 88-7794-256-8 (and the papers cited therein) would be a good starting point. Inclusion of a paper having A.G. Lipson as co-author . . .

[Other pages lost]

2003-10-27

Bury Lodge heading

October 27, 2003

Dr. M.H. Miles,
Department of Chemistry,
University of La Verne,
1950 3rd Street,
La Verne,
California 91750,

Dear Mel,

Just after sending you the rather large collection of papers by Fax and the letter containing copies of the two papers I presented at ICCF 10 I received your two letter packages dated 17/10/03 and 24/09/03. Strange?

I will now revise part IV and send you this corrected text in due course. It seems to me that you, Stan, Pam and I are agreed that it would be best to publish these papers as a new TR, certainly in the short term (Frank's offer is greatly appreciated!). I will therefore send Stan an updated version of Parts I – IV as well as an electronic version as soon as we have revised Part IV. Have you Stan's email address? However we have not yet resolved how we can scan the diagrams and add them to the texts. Perhaps we will be able to do so before we send this material to Stan.

I wish you success and joy in your efforts to get your paper published in Science, Nature or the Journal of Physical Chemistry - it will certainly be an interesting test of these Journal's attitudes particularly when juxtaposed with the article in the Wall Street Journal. In this connection, have you had the interesting letter from Peter Hagelstein ? (attached). I will be writing to him this weekend and will send you a copy of my letter. You will see that I will tell him that you will be able to make some interesting comments!

Regards,

Martin

P.S. I see that you are on Peter Hagelstein's list of correspondents.

P.P.S. It might well be that the proposed new T.R. should contain a version of the paper "Background to "Cold Fusion"; the Genesis of a Concept" also a new Part V which would be devoted to the design of experiments on the applicability of the Q.E.D. paradigm (measurements at short times or small dimensions i.e. short space-times) with some results. Could you please discuss this point with Stan/Frank and let me know their views?

2003-11-21

Bury Lodge heading

21/11/03

Dr. Melvin H. Miles,
Department of Chemistry,
Bates College,
5 Andrews Road;
Lewiston,
Maine 4240-6092,
U.S.A.

Dear Mel,

Many thanks for your Fax of 10/11/03 and the letter package which enclosed also your paper on fluidized bed experiments. Splendid!

Regarding the corrections to “The “Instrument Function” ---- Reduction of Oxygen”, would it be possible for you to send a corrected text to Peter Hagelstein? I am currently at a rather low ebb and can’t take on any further work in the short term. This also affects the other publication plans: if you wish to submit either the long or short version to Thermochimica Acta, then by all means go ahead and; if you wish to submit the Pd-B paper to Fusion Technology (with Dr. Imam as co-author) then again please go ahead. I really only had one reservation: the prior publication of the co-deposition paper seemed to me to preclude the publication of the whole series in one Journal.

It is obviously rather important for Peter Hagelstein to have an absolutely correct text of our joint paper. As you will have seen, I have told him that I would like to have this assessed as one of the key papers by any new DoE Review Panel which may be set up! You will also have seen that I told Peter that you would be able to give him a good illustration of the way the ERAB Committee operated.

We were very glad to see that you have escaped the worst of the fires - it must have been quite terrifying!

Regards,

Martin

2004-03-15 a

[JR This is a fax from Miles to Fleischmann]

Melvin H. Miles, Ph.D.
4850 San Jose St., Apt. 128

DATE: March 15, 2004 TIME: 2 p.m.

TO: Professor Martin Fleischmann

FROM: Dr. Melvin H. Miles

Number of pages including cover sheet: 1

MESSAGE:

Martin,

I am concerned about your health because I have not heard from you since December. If possible, please send me a short message.

For now, I assume one or more of the following — none of which are good.

1. Health problems have limited what you can do at present.
2. Something I wrote may have offended you.
3. You have lost interest in the cold fusion battle for scientific acceptance.

If it is #2, then I apologize. If it is #3, then I understand for I suffer from this myself at times. If it is #1, then I offer you my best wishes and prayers for your recovery.

Stan Szpak tells me that the Navy report is progressing towards the publication of your series of four papers. These will all be published exactly as you wrote them.

I will be mailing you Wilford Hansen's ICCF-10 publication. I am perplexed by his conclusion of no excess enthalpy for the Pd-B system. His major error is the assumption of $k_R = 0.55\text{x}10^{-9}\text{WK}^{-4}$ versus your $k_R' = 0.855\text{x}10^{-9}\text{W}^{-4}$. This means that he allows 35.7% of the heat to be transferred by conduction. I believe he has confused the larger NHE cells that I used in Japan with your earlier smaller cells used in Utah where $k_R' = 0.65\text{x}10^{-9}\text{WK}^{-4}$.

I have studied more closely the data for Day 61 for the Pd-B experiment. I now understand your concern for a even larger k_R' value or the possible "over-filling" with D_2O. My notebook records convince me that there was no over-filling. However, a foaming problem may have developed at this large current. This would not have been directly visible to me at the time. However, I did notice foaming a few days later during the "boil-off" phase as the liquid level fell below the silvered region. Perhaps this foaming problem could affect k_R' similarly to over-filling. Please let me know what you think.

I am still waiting for Wilford Hansen to reply. I don't see how his methods can handle either Day 60 or Day 61 without producing a large negative (and impossible) excess enthalpy effect

Best wishes,

Mel Miles

[JR Handwritten equation on message by Miles, perhaps not transmitted:]

$(1 - k_R/k_R')100 = \left(1 - \frac{0.55\times10^{-9}}{0.855\times10^{-9}}\right)100 = \underline{35.7\%}$ by conduction

Bury Lodge heading

22/03/04 08:15

Dear Mel,

The news about the Navy Report is good news indeed, I am really relieved that the four papers;

Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/ D_2O systems

Part I; the Pt/ D_2O Blank System

Part II; the Pd/B and Pd-B-Ce Systems

Part III; the Pd/D Codeposition System

Part IV; An Experiment with a Pd-Cathode in 0.1M LiOD/ D_2O carried out in 1989

should see the light of day in this fashion - although I would have had a preference for publication through the more usual channels which, no doubt, would also have been your preferred mode ? However, our experience with the submission of Part 1 shows that the journals would simply have barred this route so the use of the Navy Reports would eventually have proved to be the only available option?

No, you don't need to make any apology but I do have to apologise to you. I collected your recent letters containing your various questions always saying "I really must write to Mel" but because of my preoccupation with another problem (which lasted until yesterday) I always put this off - more about this anon. However, you say that the Navy Report will be verbatim so it is really essential that I should answer your questions as this will clarify the texts/correct mistakes. I will do this during the next few days.

To answer your questions;

1) My health is reasonably O.K. - I have a further check up on Thursday.
2) I have dealt with this.
3) This is reasonably close to the mark and there is really nothing more that I can do. I will comment on this further in connection with the further review which is to be carried out (by the D.O.E.?)

Because of my feelings about Cold Fusion, I went back to the further consideration of the work on nucleation and phase growth (together with Mort Abyaneh) which had been carried out by myself and variously Li, Peter, Pons and Sousa (nucleation on microelectrodes). This work (and various earlier studies) had convinced me that these processes could only be understood within the context of Q.E.D. and/or possibly string theory. However, it is one thing to come to this realisation but it is a completely different matter to try to bring such a view to the point of publication. Hence my single-minded dedication to this task since December 2003! I am glad to say that I am beginning at last to see the light of day.

You may recall that in speaking about the background to the work on Cold Fusion at I.C.C.F. 10. I pointed out that there had been numerous preceding studies which pointed to the need to invoke Q.E.D. in the interpretation of processes in condensed matter (kinetics of fast reactions in solution, surface x-ray diffraction, trans-membrane ion currents etc.). All of these required laboratory facilities for the further development of the work but I just happened to have some of the required data to consider further the problem of nucleation. I rather think that the work which I have done recently will be my penultimate penultimate paper although I think this too will be rejected by the journals.

Do you sometimes get the feeling that Science is dying, possibly already moribund?

I should add some words about the Parts 1-1V which may prove to be relevant to the impending review.

Part I was intended to demonstrate the precision and accuracy of the methodology we adopted and to illustrate the nonsense produced by other authors (excess enthalpy due to the reduction of oxygen).

Part II This is your mainstream- I will comment on Wilf's analysis below.

Part III This is crucially important for the further development of one part of the project.

Part IV was intended to set the scene for further controversies which I think will arise during the course of the review. In 1989 Stan Pons 'phoned me here in the U.K to ask whether I had. any objections to his giving one of our data sets to G.E. and I told him "by all means". The experiment in question was that used in this paper. The important point is that one does not have to do anything very clever to show that one can reach excess enthalpy generation in the region of "breakeven" even when using extremely energy inefficient systems as we have done throughout our work. Incidentally, at that time G.E. agreed that we had seen excess enthalpy generation. However, they must have decided subsequently that such a conclusion was impolitic and this led to their paper. The important point of their paper is that they started with a statement about our analysis which is simply incorrect and inconsistent with our first full publication- we had set out fairly clearly what we had actually done! Ah well, there is much more to this story which I will have to tell you about when we next meet. Incidentally, I had no further contact with G.E. after our first meeting with them in 1989- had they talked to someone in the lab who was not concerned with the analysis, e.g. Marvin Hawkins?

Now as to the pending Re-review, I believe that I wrote to you some time ago to say that I view this with mixed feelings. It is certainly true that the main protagonists are getting older and that, if nothing is done, the field will simply die. At the same time, there are many D.O.E. personages who will see the new Review as an opportunity to apply the "Coup de Grace".

Now as to Wilf Hansen. He originally came up with his calculation scheme (to linearise the problem) when he analysed some of our early data sets. It is a good idea and I had intended to develop this further; it was certainly a valid scheme for cells which had an appreciable conductive contribution to heat transfer (which was the case for our early experiments; cells constructed in Utah). However, Wilf got this wrong in 1989/90. The cells which we used later

had heat transfer coefficients close to that calculated from the Stefan-Boltzmann coefficient so there was no point in following up Wilf's idea - one could simply lump the conductive into the radiative term by defining a "pseudo-radiative heat transfer coefficient".

Now as to the possible foaming in the cells. The Japanese were plagued by this problem due to their use of D_2O destined for N.M.R. experiments. This contained added detergent to aid the filling of sample tubes. We wrote to them at length about this and I thought that the problem had been cleared up.

I am getting back into my old ways! Regards also to Linda. How is your teaching getting on?

Best wishes

Martin

2004-03-15 b

Juno e-mail for [Melvin Miles] printed on Monday, March 15, 2004, 5:06 PM

From: "Jed Rothwell"
To: "Linda & Melvin H. Miles"
Date: Mon, 15 Mar 2004 10:41:44 -0500
Subject: RE: Martin Fleischmann

I have not heard from Martin lately, but on January 7 I got this message. I do not know who sent it, because Martin does not have e-mail. Maybe he was staying with someone? The return address was: Martinsp361j@aol.com

This sounds like he is in good spirits. He was headed out on a "geriatric skiing trip"!

I still have not heard from Peter Hagelstein this year, either.

In other news, the "New Scientist" has been nosing around. They heard about the DoE cold fusion review panel. The total number of downloads from our web site exceeded 460,000. Regrettably, it is costing me a lot of money -- $12,000 last year -- so I had to add a fund-raising screen to the site to defray expenses. [145] The skeptics will accuse me of money grubbing, but I really need the dough. See:

http://lenr-canr.org/donation.html

- Jed

- - - - - - - - - - - -

Dear Jed,

Happy New Year! (although mine is starting in a somewhat inauspicious way).

Just to let you know that I am sending you today a package containing the six papers I listed in my letter of 24/12/03. I believe that I did not tell you that I am going to Austria on 10/1/04 for 14 days geriatric skiing. However, if there are some urgent questions concerning the texts of the papers you may be able to clear these up with Mike Clarke? Incidentally, I have also sent a set of these papers to Pam Mosier-Boss via Stan Szpak.

When you receive the package, you will see that I have indicated (in red biro) the places where the Figures and Tables might appear in the text. However, I am not very fussy about such matters and, furthermore, there are a great many Figures and Tables. You may therefore prefer to gather these together and present them at the end of the texts?

[145] JR Fortunately, the cost of hosting web sites such as LENR-CANR.org has fallen drastically, to around $100 a year in 2017, plus office expenses.

You are quite right about the perambulations of the figures through various versions of the texts and it would be quite a labour to locate the originals! Will the versions we sent you not do for the Internet publication? We still do not have the software to produce electronic versions, something we will have to rectify this year.

Incidentally, I note that in my letter of 24/12/03 I said that V was a "sawn-off" version of II whereas it is in fact an abbreviated version of I. When you receive the package you will see that I have deleted Tables 4 and 5 from the text of II (lines 2 and 21 on page 7). I have included these Tables separately in the package in case you should wish to reinstate them in the text.

I still haven't heard from Peter Hagelstein,

Regards fully,

Martin

From: Michael McKubre
To: Linda & Melvin H. Miles
Date: Mon, 15 Mar 2004 15:44:17 -0800
Subject: Re: Martin Fleischmann

I talked to Martin today. He did seem a bit glum and is not going to Asti. He sounded tired and frustrated as I have heard him sound before. I don't think there are any special health concerns (at least, not new ones).

-Mike

On Sunday, March 14, 2004, at 08:11 PM, Linda & Melvin H. Miles wrote:
> Martin Fleischmann wrote to me last December stating that he was "at a
> rather low ebb" and that I should go ahead and try to publish his
> recent
> papers that included my name. I have not heard from him since and
> suspect that he is ill. Does anyone have any more recent information
> about Martin? I know that he prefers to be rather private about his
> health problems.

> Mel Miles

> Melvin & Linda Miles
> 4850 San Jose St. Apt. 128
> Montclair, CA 91763

2005-02-24

Bury Lodge heading

24/02/05 11:37

Mel and Linda Miles,
2027 Evergreen Street,
La Verne,
CA 91750,
U.S.A.

Dear Mel and Linda,

It was good to have your news at Christmas and I marvel at your energy especially when you think about all the In and Out Burgers! I was sorry to hear about the death of your mother but, as you said, she had a led a fruitful life and her death was to be expected.

I have been rather unwell at the end of last year (Anno Domini) and this lasted until the end of February (of this year). In consequence, I was unable to deal with my correspondence and, as you will see, I am only now picking up the threads again. It is not that I do not wish to correspond with you (see your letter to Stan and Pan) but simply that I haven't been able to do so!

I am now writing to you about the four draft papers

1) **Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part 1: the Pt/D_2O Blank System.**
2) **Our Penultimate papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part II: the Pd/B and Pd-B-Ce Systems**
3) **Our penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part III: the Pd-D Codeposition System.**
4) **Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part IV: an Experiment with a Pd-cathode in 0.1 M LiO/D carried out in 1989.**

together with the paper which was actually published

5) **S. Szpak, P.A. Mosier-Boss, M.H. Miles and M. Fleischmann, Thermochimica Acta, 410 (2004) 101.**

The first thing I would like to say about the texts 1) – 4) is that these are drafts (admittedly, rather advanced drafts) and, I myself have a number of changes which should be incorporated in any Volume 3 of a SPAWAR Navy Report. Thus the text of 1) is not the final text of this paper.

The text which you have was actually submitted for publication to the Journal of Electroanalytical Chemistry. In due course Laurence Peter (the Editor) wrote to me to say that the paper had not been sent out for refereeing but, instead, had been considered by him and the sub-editors (should we believe him?). It had been rejected because of the "evident connection with Cold Fusion". I therefore rewrote the text to break all connection with this subject. However, as matters progressed, I came to the conclusion that it was a waste of time to try to resubmit the paper. Laurence Peter and the sub-editors could probably see quite clearly that if what we said about the precision and accuracy the of the instrumentation was correct, then what we said about "Cold Fusion" also had to be correct. They probably did now want to be drawn into any argument about this subject area! Furthermore, the publication of 5) invalidated our original publication plans.

My difficulty with regard to the rewriting of 1) has been aggravated by my compression of my various offices into a set of boxes, one of which contains my correspondence with you; I now cannot find all this material. However, I take it that if we go ahead with the publication plans for a further SPAWAR-3 Navy Report, then I should try to locate this revised text of (1) which should be included in SPAWAR-3?

Now for some further comments on 1) – 5);

1) **<u>Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part 1: the Pt/D_2O Blank System.</u>**

The most serious deficiency of the first text of this paper is that it did not comment on the magnitudes of the observed true heat transfer coefficients viz the mean of $(k_R')_2$ = $0.62059 \times 10^{-9} WK^{-4}$ and $(k_R')_{262} = 0.62083 \times 10^{-9} WK^{-4}$ as given in Table 1 of that paper. The radiant surface area of the ICARUS-2 cell used in those experiments was 109.7 cm^2. Multiplying by the Stefan-Boltzmann coefficient $5.6703 \times 10^{-12} Wcm^{-2}K^{-4}$ we obtain $(k_R')_{262} = 0.622 \times 10^{-9} WK^{-4}$.

I want to consider here (somewhat out of sequence) page 11 of 2) i.e. the measurement cycles 4141 carried out the N.H.E. On a Pt/D_2O "Blank" system on the ICARUS-1 equipment installed at the end of 1993. As we didn't have the spreadsheets for this experiment, the evaluation remained restricted to $(k_R')_2$ giving ~$0.754 \times 10^{-9} WK^{-4}$. The radiant surface area for the ICARUS-1 calorimeters used was 133.8 cm^2 so the predicted value of the heat transfer coefficent was $0.759 \times 10^{-9} WK^{-4}$.

These agreements of the predicted and measured heat transfer coefficients (also found for extensive series of measurements carried out both in Salt Lake City and Sophia Antipolis) can be taken as a justification for the representation of heat transfer by equations such as (A.3) of the Appendix to Part 1 where the radiative heat transfer is increased slightly from the value which applies for radiation alone to allow for a small term due to the effects of conductive heat transfer.

I note here that there are some research workers who would like the contribution of the conductive term to be larger but I believe that they are simply on "a hiding to nothing".

This agreement between the measured and predicted values of the heat transfer coefficient (for appropriate "blank" experiments) also show that the marked changes in behaviour seen for

experiments with the Pd-D_2O type systems must be interpreted in terms of excess enthalpy generation in these systems (and the variation of excess enthalpy generation with the experimental conditions-see further below). Furthermore, marked increases in the true heat transfer coefficient for the cells point to malfunction of the cells due either to a "softening" of the vacuum or, else, to a faulty construction of these cells. These are points which I will discuss further below.

The importance of carrying out appropriate "blank" experiments was pointed out repeatedly to N.H.E. but they never provided us with details of any such experiments. What are we to make of this? I believe that they either conducted such experiments and concluded that our interpretations were corrrect or, else, that they simply ignored our suggestions and instructions.

I want to interject here a comment on the possible further development of the instrumentation It was apparent in 1989-1992 that it would be possible to develop the instrumentation further to allow measurements at a precision and accuracy characterised by errors of 0.0001% (possibly even by 0.00001%). The cost of carrying out a programme of work on such enhanced instrumentation was considerable and, furthermore, it required changes in the experiment designs. This venture was therefore put on the "back burner".

I have added this comment in case we should ever want to reconsider this topic. You may recall that I had a project on microthermocouples and that we wanted to develop the Szpak-Mosier-Boss work on thermal imaging into a methodology capable of resolving individual fusion steps (using scanning laser thermometry which should be able to resolve temperature changes down to 10^{-6} °C). Such measurements should give the Q-values of the individual reactions involved in the "Cold Fusion" processes. You will see that measurements with an instrument having an enhanced precision and accuracy are aimed at a similar objective. For example, with a precision and accuracy characterised by errors of 0.0001% we would reach rates of 10^3 events per second in systems showing enthalpy inputs of 1 mW. However, such measurements would clearly require a reconsideration of the whole experiment design. They would also require us to take into consideration the effects of cosmic ray background.

2) <u>Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part II: the Pd/B and Pd-B-Ce Systems</u>

this was the subject of your letter to Stan, Pam, Frank, Imam and me of 5/12/04. (for which many thanks).

If you go back to the correspondence which we had during the analysis of this experiment, it will be clear to you that I had great difficulties with the interpretation. It seems clear that we must reconsider the text in the light of what we said in Volume 1 of the SPAWAR report.

Where shall we start? Presumably with the value $(k_R')_{252}=0.85065\times10^{-9}WK^{-4}$ given in Volume 1 of that report? This is substantially higher than the value $0.754\times10^{-9}WK^{-4}$ which we can calculate for the ICARUS-1 cells (see my comments on 1) above). This ~13% increase in $(k_R')_{252}$ must have one (or several) reasons which include;

i. the construction of cells by unknown manufacturers in Japan who may have used wider diameter tubing than we did for the ICARUS-1 and ICARUS-2 calorimeters (these were constructed in the U.S.A. who used 1" diameter tubes for the inner part of the Dewar vessel). Such an increase in the diameter of the tubing could also account for the increase in the water equivalents of the cells!!!

ii. the construction of cells by unknown manufacturers in Japan who were unaware of the essential steps required to produce satisfactory Dewars (baking out, pumping down etc.). We were plagued by some problems in the same area in our early research.

iii. softening of the vacuum with increasing age of the Dewar cells

iv. unidentifiable factors.

We cannot now decide which of the explanations (i)-(iv) may be correct. As you know, I incline to the view that you were given unsatisfactory cells to "screw-up" your research – we took great care to give N.H.E. satisfactory cells!

If you check back through the correspondence, you will see that I eventually decided that some of the bottles of D_2O had been contaminated by HDO. I must admit here that I was influenced in reaching this conclusion by our early experiences with the start-up of the N.H.E. Project (1). There was clear evidence of the "quenching" of excess enthalpy generation in the key experiments due to the addition of HDO instead of D_2O (actually it looked more like the addition of H_2O to me!). You have also pointed out that the anomalous value of (k_R') on day 61 was probably due to foaming in the cell. This is another problem which we pointed out to our Japanese colleagues. Samples of D_2O sometimes contain added detergent to aid the filling of the NMR tubes! [146]

There is another explanation for the observation of apparent increases in the heat transfer coefficient in selected regions of time/temperature namely, that we are entering a regime of negative feedback. If an increase in temperature leads to a decrease in excess enthalpy generation, then this will lead to an apparent increase in the derived heat transfer coefficient.

What all this points to is the importance of establishing the true heat transfer coefficient by carrying out experiments using "blank" systems. We will obviously have to reconsider the text of (2) in the light of Volumes 1 and 2 of the SPAWAR report. I am in some difficulty here; in view of the reorganisation of my offices I now cannot find my copies of these reports. Have any of your got spare copies and could you send me a further set?

[146] MCHM The comment is more of memory aid for me. We once had a batch of D_2O that foamed badly. We traced this back to the Girdler-Sulfide process used by AECL (Atomic Energy of Canada Ltd.). We did not confirm this but I suspect that a lot of heavy water – and a lot of heavy water experiments – were heavily affected (not positively) by the presence of detergents (for whatever reason).

3) **Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part III: the Pd-D Codeposition System.**

together with

5) **S. Szpak, P. A. Mosier-Boss, M. H. Miles and M. Fleischmann, Thermochimica Acta, 410 (2004) 101.**

I like the introductory material in 5). However, in moving from 3) to 5) certain points have got lost or, at any rate, have become substantially de-emphasised. These include:

i. the fact that the true heat transfer coefficient given by the analysis due to N.H.E. (e.g. $(k_R')_{362}$=0.699861WK^{-4} given on Fig. 3 of 3)) is less than the value calculated from the Stefan-Boltzmann coefficient and the radiant surface area of the cell; we have attributed this evident anomaly to the use of $(k_R')_{362}$ by the group at N.H.E. Rather than of the recommended $(k_R')_{262}$.

ii. The fact that we observe excess enthalpy production for most of the experiment even when using such an incorrect low value of the heat transfer coefficient.

iii. The fact that Fig. 1 of 3) is missing in 5). The comparison of the rates of excess enthalpy generation observed for different methods of preparing the electrode was one of my main reasons for starting the analysis.

iv. The fact that the precision and accuracy of the instrumentation has been incorrectly represented. Thus to quote 5): "An independent evaluation of the Dewar-type cells, used by Fleischmann and Pons (and also in this research), by Hansen and Melich (18) states that these cells are "easily capable of 1.0% accuracy".

Reference (18) is (2) in the list at the end of this letter.

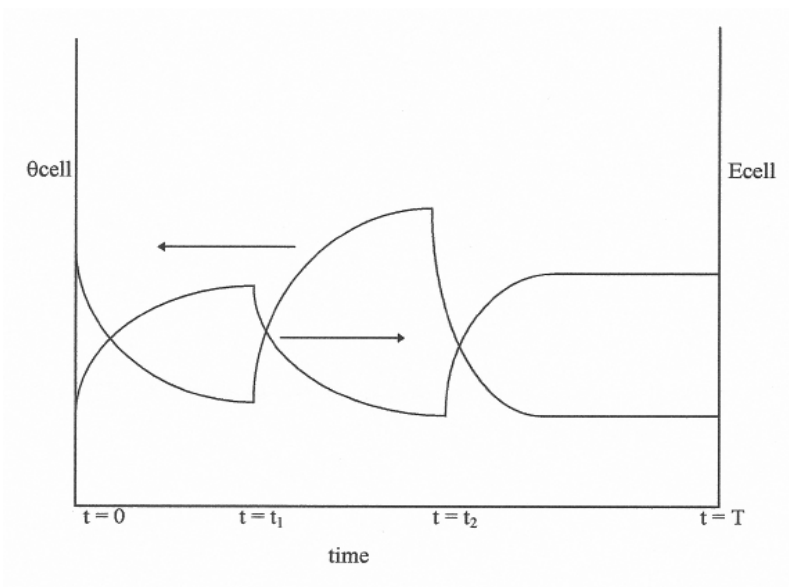
The question of whether the precision and accuracy is 99% (as implied by Hansen and Melich) or >99.99% as shown in 1) is actually not important for 3) or 5) because the rate of excess enthalpy generation is very high for the codeposition system. However, it is very important for the general development of the subject. Thus, it is only with this very high precision and accuracy that we are able to determine the rate of excess enthalpy generation due to the reduction of oxygen as shown in 1) and thereby "lay the ghost" of this particular topic. It seems to me that we will have to change 3) to make this point more clearly.

v. I also believe that Figs. 4 and 5 of 3) should be presented somewhere. After all, plots of this kind are the foundation stone of the analysis.

4) **Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part IV: an experiment with a Pd-cathode in 0.1MLiO/D_2O carried out in 1984.**

As I have explained in the previous correspondence, a major reason for wishing to publish this paper is that this was a data set given by Stan Pons to General Electric in 1989 (it may not have been the only data set given to G.E. but it was definitely given to them at that time). G.E. agreed with us that we had seen the generation of excess enthalpy although their judgement may have been based on a different portion of the data set to that illustrated in (4).

In due course though G.E. changed mind (presumably they came to the conclusion that a collaboration with the University of Utah was ill-advised) and they wrote the paper (3). I note that in this paper they referred to every aspect covered by our first full publication (4) except the Appendix 2 in which we delineated how we had actually carried out the calculation. Approximate values of the heat transfer coefficient were evaluated at $t=t_1$ of the plots containing a calibration and the "exact" values of $(k_R')_1$ and $(k_R')_2$



were then evaluated by non-linear regression (see also 5). the fact that we had used this procedure was ignored by Wilson et al [3]. This paper is a classic example of the type of criticism which has bedeviled this field of research; you set up a scenario and then heavily criticize the execution of the research on the basis of this scenario. As I have always said; if we had done what is alleged in the relevant paper, then the criticism would be justified. However the whole point is that we had not followed the scenario set up in the paper!"

I also note that one of the authors of the paper [3], Fritz Will, the Director of NCFI – what are we to make of that? The existence of the paper was kept secret from us and we only learnt about it because some unspecified person did not approve of such shenanigans. This allowed us to write the rebuttal [5].

I should point out once again that I do not want to get into a p..s f...t with G.E. but that it does seem to me that the text of 4) may need to be revised? The main point though is that we do not need to carry out a very sophisticated data analysis if we are content just with a qualitative

demonstration of excess enthalpy generation. The paper is in fact a somewhat more elaborate demonstration of [6]. You have also published work of this kind and I will leave you to make appropriate changes and additions.

yours exhaustedly,

Martin

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2005-03-02

Bury Lodge heading

02/03/05 11:28

Dr. M. H. Miles,

2027 Evergreen Street,

La Verne, CA 91750, USA

Dear Mel,

I am now sending you the paper

The Precision and Accuracy of Isoperibolic calorimetry as Applied to the Pt/D_2O System

By

M. Fleischmann and M.H. Miles

which is the rewritten version of

Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part 1 the Pt/D_2O Blank System

by

Fleischmann and M. H. Miles

removing (as far as possible) all reference to "Cold Fusion".

This rewrite was carried out following the rejection of the paper submitted in 2002 to the Journal of Electroanalytical Chemistry. However, as I have told you at several times since 2002, I realised that we could hardly meet the objection which had been raised – it was quite obvious that, in spite of extensive pruning, the paper was still part of the "Cold Fusion" saga. I therefore did not resubmit the paper.

I think though that the situation is now substantially different. One of the main criticisms raised by the reviewers of the recent D.OE. Saga is that the work carried out has not been properly documented. It seems to me that our experience with this paper is fairly typical of the difficulties which have been met by those who want to advance the investigation of the subject; a refusal to publish paper, then followed by complaints that the subject has not been documented!

As a start, should we include both texts in SPAWAR-3 together with the correspondence with the Journal of Electroanalytical Chemistry? We could illustrate this also with suitable quotations from the review's comments (presented in a neutral way). We could then go on to say

that such an attitude is doubly unfortunate because it is (a) unscientific and, more importantly, (b) prevents the development of a useful (and cheap) methodology whose application extends well beyond the area of "Cold Fusion".

What should we do?

Regards,

Yours,

Martin

2005-05-12

Bury Lodge heading

12th May 2005.

Dr. Melvin H Miles,
Department of chemistry,
University of La Verne,
La Verne, California 91750,
U.S.A.

Dear Mel,

I am afraid that I have once again fallen behind my good intentions. However, it seems that I have at last caught up with my immediate backlog so I am ready once again to take up the cudgels.

I think that the most immediate problem is to decide what to do about the four papers (see also the fifth paper) and the rewrite of the first viz:

1) Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part 1: the Pt/D_2O Blank System.

2) Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part II: the Pd/B and Pd-B-Ce Systems.

1A) Rewrite of 1) breaking all connection to "Cold Fusion".

3) Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part III: the Pd-D Codeposition System.

4) Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems. Part IV: an experiment with a Pd-cathode in 0.1MLi/D_2O carried out in 1984.

5) S. Szpak, P.A. Mosier-Boss, M.H. Miles and M. Fleischmann, Thermochimica Acta, 410 (2004) 101.

The question is: what are we to do with these papers particularly in view of the fact that they/are unlikely to ever be published? Should we just accept this situation? The second question is: when is the meeting in Japan (have you the details?) and are you intending to go? It seems to me that there is no question about the contents of 1) and 4) (although 4 needs to be strengthened in view of both your and my previous publications). A possible course of action would be to rewrite 1) so as to bring out the point that measurements on a suitable "blank" system allow us to determine the precision and accuracy of the instrumentation, to characterise heat transfer by the radiation term alone (commenting also on experiment 4141 carried out by N.H. E.) and to estimate the contribution to excess enthalpy generation by the parasitic reduction

of oxygen. This is the part of the story which "the powers that be" have sought to have suppressed so as to allow our opponents to blather endlessly about oxygen reduction (one aspect of the Catch-22 scenarios).

We could then go on to ask the question: is it necessary to implement such complicated data processing strategies? The answer is: not really, if we are content to have qualitative demonstrations of excess enthalpy generation. This is the content of 4) or rather it should be the content of a modified paper 4) (modified by your input).

One advantage of following such a publication strategy is that we can then decide at leisure what to do about 2) and 3) in the light of the previous SPAWAR reports. This is likely to take us some time. The strategy also has the advantage that it establishes the precision and accuracy for the instrumentation.

Therefore, if you agree with such a plan of campaign, could you please revise 4) as a matter of urgency and send this to me? If you want to reply to me by FAX, then could you please use 0044-747-871241 as on previous occasions or by e-mail to woodtisbury@farmersweekly.net. I will tell you when I meet you why I have again resorted to this circuitous route!

This brings me to the second part of this letter which consists of a set of comments induced by your letter of 22/03/05 and the attached enclosures. I believe that I have made these comments before in my various letters to you so I fear that this part of my letter may strike you as being somewhat repetitious!

First of all, I am sorry to hear that you are having these difficulties with the DOE. I think that your notion of moving to St. George is an excellent idea and, of course, I will be very glad to help with the design of new experiments (more below) in any way I can (although I fear that I am getting rather old and decrepit!). Southern Utah is a splendid place and I think that you and Linda would be very happy there!

Is it not quite obvious from the past history that the DOE want this project to fail and are the reasons for this not equally obvious? I note that none of the 16 reviews of the submission to the DOE mentioned QED (eg(1)) – not that the submission exactly pointed them in that direction! Can one believe that the collection of reviewers are completely ignorant about the development of modern QED (eg(1))? Their understanding of Theoretical Physics appears to stop short with the development of Quantum Mechanics without realising the shortcomings of such an approach. Furthermore, they have ignored the fact that the investigation of "Cold Fusion" was predicated on the applicability of QED (more anon). I think that I can understand the reasons for the stance being taken by the DOE but I will not commit pen-to-paper on this subject; instead, I will reserve a discussion of this topic to our next meeting. Suffice it to say that the outcome of the review is in line with my expectations which led me to oppose the whole notion of having the review in the first place. However, Mike McKubre tells me that the outcome of the review is directly opposite to anything the DOE might have wished – it has stimulated a great deal of interest.

I want to interject here some comments on the underlying basis of several of our research projects which preceded the start of work on "Cold Fusion" notably (i) the kinetics of fast

reactions in solution at short spacetimes (2 x 10^{-22} cm.s to 3 x 10^{-12} cm.s), (ii) surface X-ray diffraction, (iii) transmembrane ion conduction, (iv) kinetics of nucleation and phase growth, (v) wall-phase turbulence (see [(2), (3), (4)]). The need to invoke QED in the discussion of these topics could be delayed by following "hidden agendas" (e.g. See Fig 11 of [(2)] or Fig 2 of [(3)]). (I note here my wish not to sacrifice Ph.D. Students on the altar of my own vanity!). The beauty of such "hidden agendas" was that the need to invoke QED arose only from simple one-step discussions of the properties of the systems.

By 1982/83 we had reached the stage where it seemed possible to start the project on "Cold Fusion". As you will know, there were several factors which indicated that we might achieve a positive indication that there is a fusion channel

$$D^+ + D^+ \rightarrow {}^4He^{2+} + \text{heat}$$

operating at very low incident energies of the deuterons and based on the fact that nuclear systems also had to follow the dictates of QED. These factors included our knowledge of the early literature of the subject [(5),(6)], our knowledge of the early work on the electrodiffusion of H+ in Palladium host lattices [(7), (8)] and, especially, our knowledge of the early work of Bridgman on the induction of cold explosions by intense shear and compression of lattices, [(9)]. It has always seemed to me that the really pressing need is to explain the results of Bridgman in terms of QED.

If you check back through my various pronouncements on "Cold Fusion", you will see that I have been very reticent about revealing this part of the story. There are good reasons for my reticence which include: (i) the fact that such a revelation might lead to acute difficulties for those involved in the development of weaponry, (ii) the fact that such a revelation might simply open up another channel for the opposition to "Cold Fusion" (spurred on to a considerable extend by (i)), (iii) the fact that Soviet scientists had misinterpreted the work of Bridgman (which would aid those wishing to take the route (ii), (iv) the fact that I did not want to polarise the discussion of the topic, (v) the fact that I was not at all sure exactly how the effects discovered by Bridgman might be involved in the explanation of "Cold Fusion". [147]

As far as (v) is concerned, it is evident, however, that the development of coherent structures which are "stiff" on the times-scale of nuclear processes allows the operation of mechanisms of heat transport on a time-scale of 10^{-21} - 10^{-18} s, i.e. on a time-scale consistent with nuclear processes (the wide range in time-scales is due to the uncertainties of the parameters required for the evaluation of (v). This mechanism of heat transport is very important to counter some of the arguments which have been raised against the reality of the "Cold Fusion" phenomenon. Note that the development of QED coherence [(1)] shows that arguments based on asymptotic freedom are not valid.

I therefore decided that this part of the project should be delayed and that we should rely instead on the establishment of excess enthalpy generation in the first part of the work. It is evident, however, that this part of my overall strategy has been a total failure. Some of the calorimetric measurements which have been carried out range from very poor to downright

[147] MCHM Important.

dishonest and these are religiously quoted by the many critics of "Cold Fusion". There has been a virtually total disregard of "blank" experiments and some of the attributions of excess enthalpy generation to the effects of oxygen reduction are just ridiculous. As regards the discussion of the effects of QED coherence, I note that I have been asked "point-blank" now several times how "Cold Fusion" might be related to the development of weaponry specifically in the context of (i). I have therefore decided that I should comment on the importance of the work of Bridgman to the interpretation of the results of "Cold Fusion" (indeed, to the initiation of this project [(3), (4)] although you will find that some of my earlier papers indicate this importance).

A further factor which has weighed fairly heavily with me is that with the passage of time there has been a plethora of incorrect approaches and interpretations which will, in the end, frustrate further progress. Science is full of such examples: the important point is not whether an interpretation is correct but rather whether it is accepted by Society at large. (Mossbauer spectroscopy provides an interesting example).

This therefore, is more or less the end of the road for me. I recall saying to Stan Pons in 1982/83: "there is only a chance in a billion that we will see anything definitive". He replied to the effect: "Yes, but if we do, then the consequences will be incalculable." I think that this fully illustrates our attitudes towards the subject. "Cold Fusion" was in the nature of a sideline for us and we intended to continue with the other subjects which were intended to demonstrate the importance of QED (and to extend this list of topics). However, the outcome of our research was radically different to our expectations. By 1988/89 we had archived specific rates of excess enthalpy generation of the order 100 Wcm^{-3} and there was no indication that this was a limit. In the conditions which confronted our society, we had to carry on and I imagine that the situation is broadly similar for all those who have been caught up in the web of this topic? (the situation was complicated for us by the premature involvement of the administration of the University of Utah).

This therefore led to the adoption of a research strategy which has not been helpful for the development of the overall project. I note here that the adoption of the "hidden agendas" in turn led to the demonstration of the need to invoke QED from simple one-step discussions of the properties of the systems for all the examples with the exception of "Cold Fusion" (see Fig 11 of [(2)] or Fig 2 of [(3)]. However, this has not been true of "Cold Fusion" where we have to contend with a "double whammy", firstly, the need to establish that the modeling of all systems in the Natural Sciences has to follow QED and, secondly, that "Cold Fusion" is to be explained by this particular paradigm. (I must say that I now find this argument to be rather unconvincing – we need to develop a discussion!) of course, our many critics have found a simple way of dealing with these difficulties – they deny the reality of the phenomenon. These critics have also invoked a further "whammy": they are unable to find a mechanism for explaining the flow heat away from the reaction site and into the lattice. With this they seek to imply that "Cold Fusion" must be an artifact of the experiments without realising that they have simply demonstrated that <u>they</u> are unable to explain the transport of heat. This is why I have referred to "two and a half whammies" for the explanation of "Cold Fusion". Of course, the transport of heat is to be explained by the "Bridgman effect" i.e. the displacement of coherent structures on the time-scale

of the nuclear phenomena. One of the most pressing needs is the interpretation of Bridgman's experiments.

What are we to do now? The difficulty is that we are faced with the need to develop rather wide ranging investigations in the climate of opinion where funding will be difficult to get. You probably have your own agenda which, as far as I can tell, will be based on the optimisation of the electrochemical systems. I take it that materials science will play a rather large part in such studies? I say "hear, hear" but, at the same time, I would like to give some prominence to the development of electrodiffusion systems (of course, the electrochemical studies are part of these systems). I have on my desk a cell (constructed at my own expense) [148] with which I wanted to back up the De Ninno, Fratolillo, Del Quidice and Preparata work [(10)] (this is a flow cell; it is all rather rough-and-ready because it was designed as a back-up). However, it was not to be. I would not want to do such work now in this particular way – the system has to "stand alone". Instead, I would like to construct a smaller scale version to be used in isoperibolic calorimeters of the type we developed in Utah. The additional element would be based on D.C. power supplies using the methodology developed for the domestic market. I have in mind starting with the investigation of the loading of /excess enthalpy generation in such wires especially of the "Resistomet" [149] type developed (in the 1970s?) as possible replacement for the Nimonic range.

Would you like to discuss this further? The design of electrodiffusion systems has to be approached with some care. To illustrate this, I am attaching page 18 of my lecture at ICCF-11 – it is all rather worrying. Incidentally, the meeting was attended by 18 Russian scientists (!) some very senior and respected members of the "establishment" (one of these was from Belarus and one from the Ukraine). None of these scientists could be drawn to say anything about SBER although several of them had published papers in this area. However, I did get some interesting comments from non-Russian and non-U.S. Scientists outside the meeting proper.

Sorry for this turgid apologia!

Regards to Linda,

Yours,

Martin

P.S. The structures for the electrodiffusion experiments could be made by electroless deposition.

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2) Martin Fleischmann, "Searching for the Consequences of Many-Body Effects in Condensed Phase Systems" Proceedings of ICCF 9 Ed. Xing Z. Li, (2002), III

[148] MCHM I wonder what happened to this cell?

[149] MCHM What is Resistomet? JR Google finds no information about this material, so as 21st century people, we are left in the dark.

3) Fleischmann "Background to Cold Fusion: the Genesis of a Concept", Proceedings of ICCF 10 Ed. P Hagelstein, (2003)

Mel: the first three pages of this paper are attached. Have the Conference Proceedings been published by World Scientific?

4) Morteza Abyaneh, Martin Fleischmann, Emilio Del Giudice and Giuseppe Vitiello, "Concerning the Modelling of Systems in Terms of Quantum Electrodynamics (QED): The Special Case of Cold Fusion". Proceedings of ICCF 11 Ed. J.P. Biberian (2004)

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[JR Page 18 from an unidentified document]

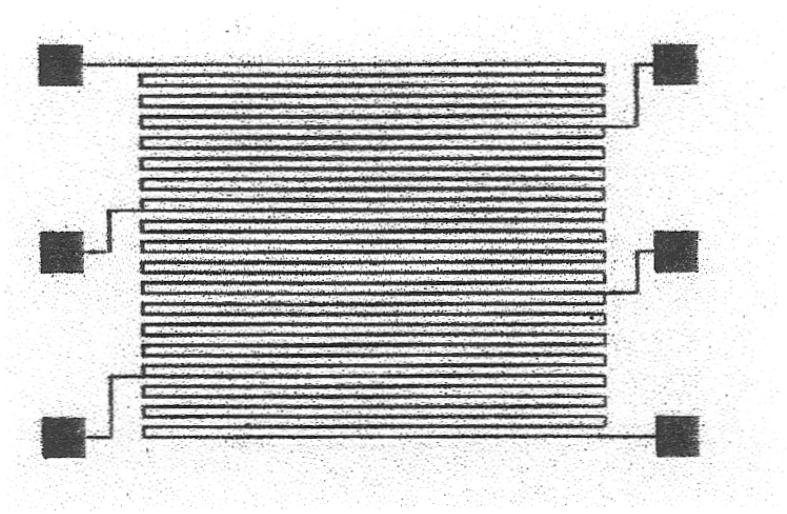

Fig. 20 The lithographically produced "bustrophedic" configuration of a fine Pd wire [36]

This work demonstrated the simultaneous generation of excess enthalpy and of ^{4}He. A surprising feature of the experiments was the eventual melting (boiling?) of the Pd observed near the most negatively polarised ends of the structure. Order of magnitude calculations of the rates of excess generation required to achieve such conditions give values lying between, 0.5 - 5.0 MW cm^{-3}. Corrections of the rates of heat transfer to allow for the effects of electrolytic gas evolution give values lying in the range 5 - 50 MW cm^{-3}. The generation of excess enthalpy at such levels clearly requires further investigation [15].

It is possible, also, that a switch from electrochemical compression of D in the lattice to the use of solid state devices (or else of devices using both effects) would reduce the irreproducibility of the phenomenon which has been observed. This is an aspect which we have not covered in this report. We believe that excess enthalpy generation takes place in the bulk of the material as mediated by the surface reactions. Such a process would be expected to be highly sensitive to the surface conditions. We note that although it is relatively easy to produce palladium, the metallurgy of this metal is very difficult. Previous work has paid insufficient attention to the effects of the metallurgical variables on the processes observed.

[Footnote in original] 15 We note that civilian applications of the methodology would require the rates of excess enthalpy generation to be restricted to ~ 10kW cm^{-3}. Present day production of Pd is sufficient to allow the conversion of a substantial part of the worlds energy needs to "Cold Fusion" systems. It is relevant that more than 50% of the worlds energy consumption takes place at temperatures below 70°.

2005-07-01

Bury Lodge heading

01/07/05 10:49

Dr. Melvin Miles,
2027 Evergreen Street,
La Verne,
CA 91750,
U.S.A.

Dear Mel,

I have been in Canada recently - no prizes for guessing where I went to and what I have been doing. I think it is really most straightforward if I send you a drawing illustrating my latest thoughts about the "improved calorimeter" design (to replace Fig. 27 on p. 84 of the SPAWAR report, Volume 1) This is the attached Fig. 1 which is intended to be more or less to scale. You will see that I have suggested the dimensions of the Dewar vessel, Fig. 1, be increased to a radius ~4.6 cm (1.8 inches) to accommodate the necessary design changes.

The first point that I want to make is that the underlying ideas is to seek to achieve a better definition of the radiative heat transfer terms than we were able to reach with the ICARUS -1 and ICARUS-2 systems. If we are able to achieve this, then many of the complications of the earlier analyses would disappear. (Note also that we could cover these points in publications by "recycling" some of the earlier material). The problem with the ICARUS-1 and ICARUS-2 designs was that the silvering in the top part of the Dewars did not completely remove the variation of the heat transfer coefficient with the level of the electrolyte in the cell. This was especially troublesome if the evacuation of the Dewars was inadequate (which seemed to be the norm for most of the work carried out in Japan). You will see that I have suggested that we should insert a tube into the inside of the calorimeter; an alternative way of enclosing a separate electrochemical cell is illustrated in Fig. 2 and this may well be preferable? The space between the glass tube and the inside wall of the Dewar vessel is filled with ballotini copper beads. Beads of 0.05 to 0.1 cm diameter may be suitable. I don't know what beads may be available in the U.S.A.? The use of ballotini beads in this region (and the region outside the Dewar) will reduce the rate of heat transfer compared to the use of a solid tube but the assembly of such a Dewar will certainly be much simpler than the system which we have been discussing. We should also immerse ballotini beads in heat transfer oil.

The region outside the Dewar is now contained in a copper tube suitably closed at the end and this tube again is filled with ballotini beads and heat transfer oil. You will see that I have suggested that the diameter of the Dewar vessel be increased to 4.6cm (say 1.8 inches) to allow the inner glass tube to be say1.8cm (say 0.7inches) and to allow the space between this inner tube and the inner tube of the Dewar to be 1 cm (say 0.4 inches). The reasoning behind choosing

these dimensions is as follows; the averaging of the thermal flux produced by the ballotini filled regions will increase with the dimensions of these regions but, at the same time, the increase of the dimensions will lead to an additional change in the temperature across these regions which we will wish to keep as low as possible. We could certainly calculate the magnitude of these effects but, for a start, a width of 1cm seems to be a possible compromise.

You will see that I have suggested that the outer copper tube should have a diameter of 6.8 cm (say 2.7 inches) and that I have also suggested that the thermistors be inserted into the ballotini filled regions. However, clearly these could also be inserted into the electrolyte space as we have done hitherto. Equally, the Joule heaters for supplying the calibration pulses could be in either of these two spaces.

It may well be that you brought some Dewar cells back with you from Japan? In that case, we could do the first experiments using an inner electrolyte space defined by a 1.5cm tube (say ~0.6inches), and a 0.5cm gap filled with copper ballotini.

I think that all we can say at this stage is "the proof of the pudding is in the eating". If the simple solution which I have suggested is satisfactory, then why should we use anything more complicated? If the solution is unsatisfactory, we can then decide whether we can achieve our objectives by changing the dimensions or whether we need to replace the ballotini filled parts by solid metal (much more difficult).

The design shown in fig. 1 prompts me to address a further question to you. You may recall that I mentioned (in one of my letters to you or possibly in one of the papers or the SPAWAR reports) that we had decided that we should add level controllers to the thermostat baths but that these were never implemented. Did the folks at NHE ever attempt to do so and/or how did you control the level of water in the thermostat tank(s)? However, note that the design, Fig. 1, should substantially eliminate any effect of the water level in the thermostat tank on the magnitude of the heat transfer coefficient. We could test this in an early experiment by measuring the precision and accuracy of repeated experiments (using the reassembled cells). If the equipment works according to our expectations, I would expect both the precision and accuracy to be characterized by errors of < 0.01%. This reassembly of the cells would probably be best carried out using the design shown in Fig. 2. Such a study could then be backed up by an investigation of the heat transfer using a relevant model. We will need the electrical conductivity of a bed of packed particles; this will almost certainly have been investigated by Chemical Engineers and the results may be in standard texts (Coulson and Richardson etc. ?)

I think that this letter will have answered most of the questions which you have posed to me but please let me know if any points need clarification. With regard to your letter of 23rd May, I will certainly be very pleased to help you with the required analyses and/or modifications of the experiments.

I see that I haven't answered your question with regards to operations near the boiling point. I think that this will have to be some sort of species of ICARUS-9 calorimetry which will need a separate development. The lack of development of this type of calorimetry is one of the most

surprising and unsatisfactory features of the "Cold Fusion" investigations. [150] However, I believe this development will be much easier once the effect of the addition of electrodiffusion to the calorimetry is established." I think, therefore, that we should establish the use of the design shown in Figs. 1 and 2, then switch to the investigation of electrodiffusion, THEN draw a deep breath and decide how we might best investigate the generation of heat near the boiling point. Incidently, the ICARUS 10-13 Calorimeters were designed and built in Sophia Antipolis in 1995 but were never put in use. These designs were based on distillations set ups used to produce conductivity water and were changed to allow the addition of steam compression to the systems (a Roots blower). They were intended to run at ~3kW output which (at the time) seemed to be the lowest level at which it was feasible to add a Roots blower to upgrade the quality of the heat. Of course, the alternative would be to develop pressurized systems but, at that time, we were anxious to avoid such complications.

It seems to me that we should discuss all the issues involved. One important point is that we have to decide whether we want to demonstrate excess enthalpy generation at the boiling point or whether we want to take the next step towards the generation of higher quality heat. The decision on this will to radically different designs.

With regard to your letter of 7th June, I see that you know that I have been in Canada. It was all very interesting although superficially quite different to the work with the pd-D type systems. I will explore with the folks in Edmonton what information I may share with you. Incidentally, I have said "superficially quite different" because, actually, both sets of experiments are to be explained by QED and they then begin to look rather similar – it all depends on where one is coming from!

Mel; I am getting rather tired of all the shenanigans with the various Journals (I have some further unpleasant experiences!). Would it be possible for me to persuade you to submit the paper to Thermochemica Acta/Journal of Physical Chemistry? A somewhat abbreviated version will appear in the ICCF-10 Conference Proceedings, Peter Hagelstein showed me the text – he has made a good job of editing this. Incidentally, I have great faith also in your care and ability to spot all the necessary corrections!

No, I have not got a repeat of my cancer problem which struck me down in 1988. The difficulties which this caused me at that time had a most serious and difficult effect on the development of the "Cold Fusion" story but that is another untold chapter! My latest health problem is diabetes. The medication which I have to take is very effective but makes me feel rather ill – also very tired. It took me ~1 week to get over my visit to Canada! Your source is probably Bob Bass who was present in Edmonton. He is a very clever fellow but, unfortunately, he seems to have a problem with his ego. It also seems to me that he is incapable of understanding the work "confidential". In this regard, I have noted that the P.C. which controls our e-mail is completely corrupted by various species of spy-ware in spite of our restriction on it's usage. We will try to tidy up the system but, meanwhile we have gone back to using the Fax

[150] MCHM Interesting comment.

and snail-mail. Incidentally, I think that I know who planted the spy-ware in our computer system.

Thank you also for the letter package of 6th June. The additional parts which require some comment (from me) are those parts which deal with your White Paper Proposal. Here I must say: "is the situation regarding the potential military applications of this methodology not abundantly clear?" As a well-informed scientist said to me "if the powers that be believed that Cold Fusion does not work then they would surely support selected parts of the research. The fact that they oppose and frustrate all further developments points to the fact that they either know that Cold Fusion works or, else, that they are not sure what might be the consequences of further work". [151]

I want to digress here to the starting point of our own research. You will recall that this was the observation of "Cold Explosion" by Bridgman in the 1930's and early 1940's. (the energy stored by shear and compression in lattices could be released in these explosions such that the fragments travelled at very high velocities while remaining at low temperatures). We were quite certain that such phenomena could only be explained by QED and that the displacement of "coherent" fragments of the lattice with respect to the "incoherent" parts opened a channel for the potential absorption and dissipation of the fusion energy of a new channel

$$D^+ + D^+ \rightarrow {}^4He^{2+} + \text{heat}$$

Nevertheless, we thought that the possibility of obtaining confirmatory evidence was very low. I have hitherto been very reluctant to talk about this aspect because I was fairly sure that this was one element which contributed to the Soviet programme on SBER and was also an element underpinning the Anglo-US efforts with regard to D.U. shells. More speculatively I wondered whether the Soviets might not be trying to develop a new nuclear technology not bound by the test-ban treaty.

I note now that the recent Russian pronouncements have broken all connections to the work of Bridgman. The presence of 18 Russian scientists at ICCF-11 (one of these was from the Ukraine and one was from Belarus) influenced the slant of the talk which I gave at the meeting. I gave some prominence to the work of Bridgman but none of the Russian scientists could be induced to make any comment about SBER although several of them had published papers in the area! However, I did get some interesting comments from non-Russian scientists outside the meeting proper.

Mel; it now seems to me that we have to soldier on regardless. We should take due account of the proposals in your White Paper and we should also bear in mind that a reasonable interpretation of the work of De Ninno, Fratillo, Del Giudice and Preparata is that they achieved specific excess energy generation in the range of 0.5-5 MWcm(-3) although 5-50MWcm(-3) is more likely. Ouch! And they weren't even trying. The publication of this work has been actively frustrated and the Administration of ENEA, Frascati, has clearly adopted an anti-Preparata stance. Thus Vittorio Violante did not refer to this work at ICCF-11 although it had

[151] MCHM Exactly.

been a mainstay of the effort at Frascati for ~2 years. I challenged him about this omission and he then gave a second lecture at the university where he again didn't refer to this work. Appalling? [152]

Best regards to Linda,

Yours,

Martin

[152] MCHM From my perspective this is a little distorted and unfair.

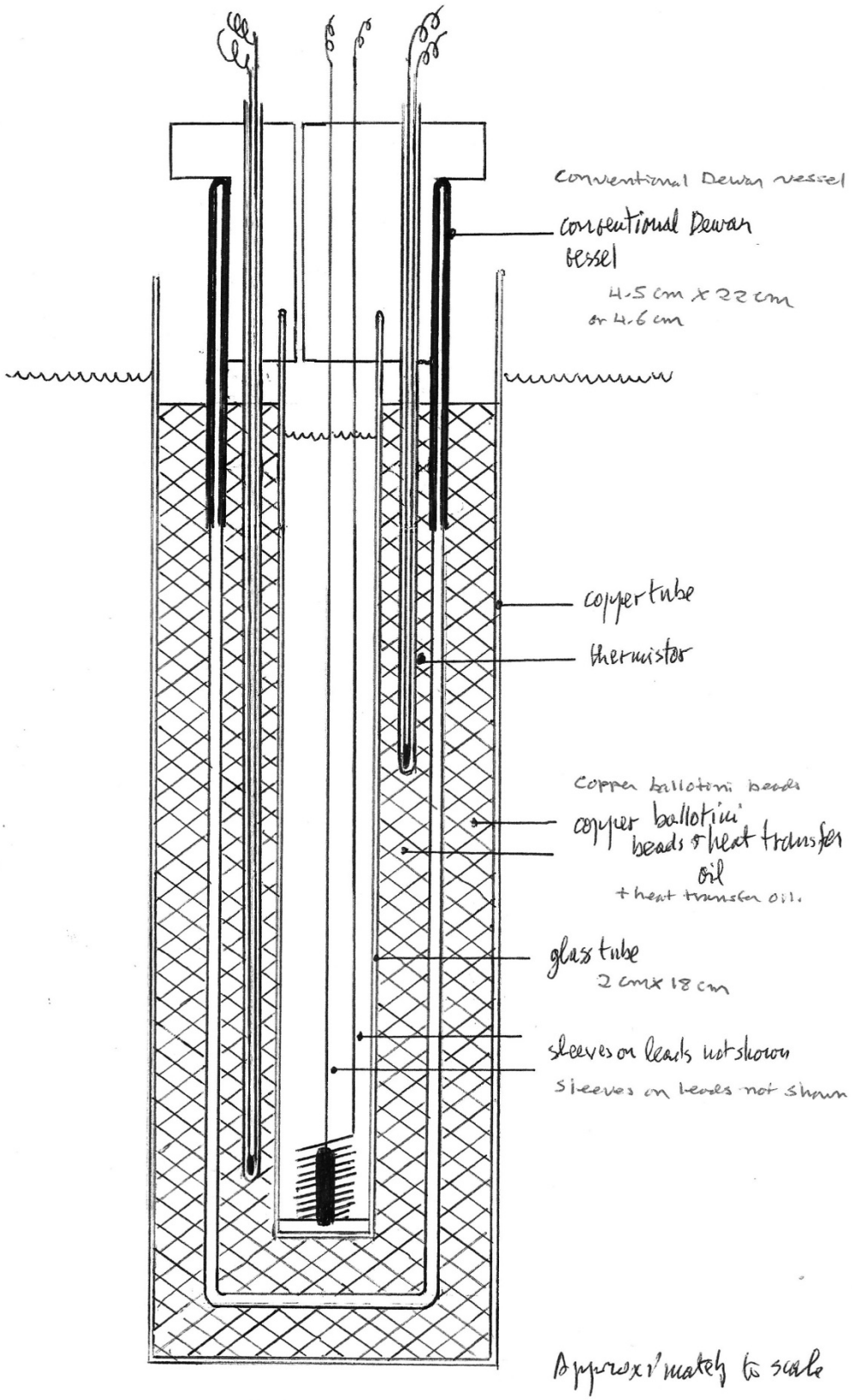
Conventional Dewar vessel
conventional Dewar vessel
4.5 cm x 22 cm
or 4.6 cm
copper tube
thermistor
Copper ballotini beads
copper ballotini beads & heat transfer oil
+ heat transfer oil.
glass tube
2 cm x 18 cm
sleeves on leads not shown
Sleeves on leads not shown
Approximately to scale

Fig 1.

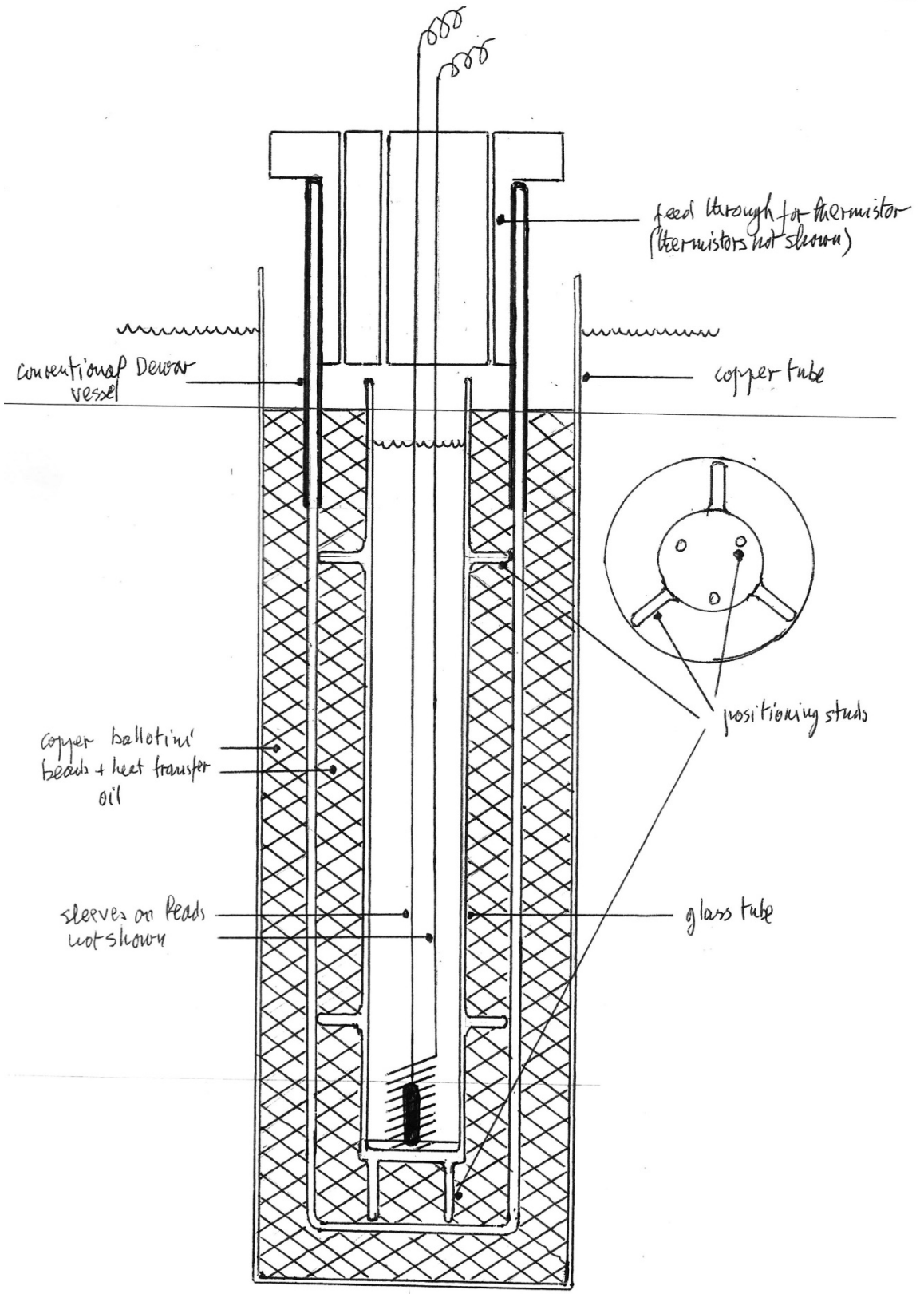
feed through for thermistor
(thermistors not shown)
conventional Dewar vessel
copper tube
positioning studs
copper ballotini beads + heat transfer oil
sleeves on leads not shown
glass tube

Fig 2.

2005-08-15

Bury Lodge heading

05/08/05 11:22

Dr. Melvin Miles,
2027 Evergreen Street,
La Verne,
CA 91750,
U.S.A.

Dear Mel,

As you will see, I am replying to your letter of 20/07/05 both to your address in Oregon and to your home address in La Verne in view of the delay in my reply! Sheila and I have been on holiday at the seaside which will explain the delay. I trust that you and Linda have had a good stay in Oregon although your struggles with your 40 acres are rather daunting!

The design of the calorimetric cell enclosed with your letter may very well work and it certainly will be easier to implement than the design contained in my letter of 01/07/05. I have just two concerns regarding your design. Firstly, whether it will give a clear definition of the heat transfer path. In the design I sent to you this will be controlled by the level of the Ballotini beads/heat transfer oil between the calorimetric tube and the inner tube of the Dewar and between the outer tube of the Dewar and the containing copper tube (at least I would hope that it will be thus defined!) What I was in fact aiming for is an improvement in the performance of the existing calorimetric cells i.e. an independence of the heat transfer coefficient from the levels of the electrolyte in the cells and in the surrounding thermostat tanks. (Leading to a simplification of the data analysis). It is not at all clear to me that your design will achieve the same objectives. However, if it does, this this will be the end of the story!

My second concern is that I was throughout concerned that the thermal impedance would not store heat thereby making it possible to analyse the transient behaviour (e.g. the effects of positive feedback). You will see that your design will not allow this to be done – at least the analysis will be much more difficult. However, this is doable and, if you opt for this design, I will write to you further about this particular point.

I think that this is one of those situations where one can only say; “the proof of the pudding will be in the eating”. If your design will fulfil all the objectives, then there will hardly be any justification for trying the designs contained in my letter of 01/07/05. I think, therefore, that you should go ahead with your design (particularly as most of the calorimetric investigations have been carried out with instruments having a pseudo conductive thermal impedance; we can make a comparison with instruments without copper tubes to define the heat transfer path). If we make measurements with designs along the lines contained in my letter of 01/07/05, we can then also

make comparisons between instruments having pseudoradiative and pseudoconductive thermal impedance: this will certainly be a worthwhile exercise.

Incidentally, I don't believe that the increased size of the Dear Cell which I suggest in in my last letter will matter too much although I believe that we should keep this increase to the minimum we can get away with.

You have not said in your letter what you plan to do next year. Have you reached any decision?

Best regards to you and Linda!

Martin

2005-09-05

Bury Lodge heading

20/09/05 12:54

Dear Mel,

Many thanks for your Fax of 30/08/05 and the letter containing further information received on 05/09/05. It is good to have your updated plans (the house prices in Southern Utah seem to be approaching the U.K. Levels).

Your letters raises three points

i. sources for copper ballotini beads

ii. possible means for constructing Dewar cells

iii. further copies of the paper "The Precision and Accuracy of Isoperibolic Calorimetry as Applied to the Pt/D_2O System"; the electronic version of this paper as related to our publication plans.

As far as i) is concerned, this goes back (at least in part) to the earlier work on fluidised bed electrodes. There was at that time considerable work on electrowinning from dilute solutions and I know of large scale work in four industrial laboratories which used copper ballotini beads (I had close contact with three of these projects. The fourth project became focused on other metal/metal ion systems). The firms concerned must presumable have secured access to supplies of ballotini beads – unless they produced these "in house". There was also an appreciable effort in University and related Laboratories. The chief of these was in the Department of Chemical Engineering at the university of Newcastle upon Tyne. Professor Frank Goodridge and Ray Plimley have now retired but Professor John Backhurst (who was also somewhat peripherally involved in that work) is still in that Department. It seems to me that an obvious way to investigate the supply of ballotini beads would be to writ to John Backhurst to ask whether supplies of ballotini beads might still be available and information about the sources of these materials. I will do this but it might be better if enquiry came from you? If so, then you should mention the likely quantities we would require and that we want to incorporate the beads as heat transfer media in calorimetry. Incidentally, John Backhurst was always a very helpful person at the time at which I knew him (40 years ago!)

I will also go to Southampton sometime next week to see whether I can lay my hands on some suitable catalogues and whether these give the relevant costs, dimensions etc.. I will write to you again about this aspect as soon as I have the relevant information. (see the P.S. to this letter)

Now as regards ii). We had a variety of Dewar cells constructed "in house" for our preliminary work but these were al unsatisfactory for a variety of reasons. We decided in the end to ask the manufacturers of Dewar vessels whether they would make the design modified

according to our needs and one of them agreed to do so. These proved to be highly satisfactory but I regret to say that I don't have record as to which manufacture this may have been. However, there cannot have been a large number and I would suggest that we start a correspondence to find out which firm it may have been. Perhaps we should refer to the earlier work – Stan Pons negotiated this deal. It might be sensible for us to check the dimensions of the available Dewars: Pyrex tubing is supplied in standard sizes and we should design the cell to fit into those dimensions otherwise the costs will escalate.

The only alternative would be for us to start a programme on the construction of the cells but I think that this would be rather lengthy and expensive.

Now as regards iii). I am sending you in the first place a copy of the paper

1) **The Instrument Function of Isoperibolic Calorimeters: Excess Enthalpy Generation due to the Parasitic Reduction of Oxygen.**

which was presented at ICCF 10. You will see that Peter Hagelstein & Co. have made a very good job of producing this text for the Conference Proceedings (I have not noted the corrections I made to these proofs). You will also see that I have referred in this paper to the text of 1)

2) **Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems, Part 1 the Pt/D_2O Blank System.**

which you asked for in your letter and which I am also enclosing. (I have decided to send the additional copies under separate cover to economise on the postage).

3) **The rewrite of 2) breaking all connections to "Cold Fusion" (a somewhat forlorn hope). The diagrams and Tables are identical to those in 2).**

4) **Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems, Part II: the Pd/B and Pd/B/Ce Systems.**

5) **Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems, Part III: the Pd/D Codeposition System.**

6) **Our Penultimate Papers on the Isoperibolic Calorimetry of the Pt/D_2O and Pd/D_2O Systems, Part IV: and Experiment with a Pd-cathode in 0.1 M LiOD/D_2O carried out in 1988.**

We need also to consider that paper by

7) **S. Szpak, P.A. Mosier-Boss, M.H. Miles and M. Fleischmann, Thermochimica Acta, 410 (2004) 101.**

As I am sure that you will have the text of this paper, I am not enclosing it with 1)-6).

You will see that there are only minor differences between 2) and 3) and I became convinced that it was hardly possible to rewrite 2) so as to break all connection with the topic of "Cold Fusion". On the other hand, there are marked difference between 2) and 3) and the text of 1). The content of 2) really consisted of three parts:

i. a description of the ICARUS procedure as it should be used to achieve the accuracy and precision claimed in the specification for the instrumentation;

ii. a description of other procedures which fail to give the accuracy and precision claimed (and some explanation of why this should have been so).

iii. some comments on further improvement in the accuracy and precision of the data analysis which could easily be achieved.

As the space Conference Papers is restricted (not that I take too much notice of the instructions!), 1) was restricted to (i) with some comments on (ii). The commentary stops basically at about Fig. 9 of 2). As you will see, this paper will be published and we have to ask ourselves whether the lack of publication of the remaining parts of (ii) as well as of (iii) need be any concern to us? Furthermore, whether we should try to get some publication of (i) in the more "normal" literature? (which would be achieved by publishing a paper along the lines of 1))

I have to confess that I have some personal reasons for wishing to publish a paper giving a reasonable full account of (ii) as well as of (i). I am convinced that the analyses of the data carried out in Japan were based on calibrations relying on forward integration of the data and that our Japanese colleagues rewrote the software (at least of ICARUS-2) and thereby introduced a number of changes in the data analysis which led to a marked decrease in the accuracy and precision of the derived data. I did a considerable amount of work on the Japanese data sets (such as I had then – none of them were anywhere as complete as the data sets you sent to me!) I never had an acknowledgement of this work nor any reply to my quite specific question aimed at establishing the basis of the Japanese interpretations. Of course, in retrospect I can't be sure that any of this material was ever sent to Japan and/or given to the N.H.E. group.

You will see that if my concerns are legitimate, it is still important to publish a paper showing that there are correct and incorrect ways of analysing the data. For example, should Figs. 11-14 of 2) not appear somewhere in the literature? I am less sure about Figs. 15-20 which take us (eventually?) into the realm of (iii). I therefore believe that you should reach some decisions about the content of the proposed paper and I will eventually rewrite Part 1. As I am uncertain about the final text, I have done nothing as yet about locating an electronic version of this paper. Incidentally, my hesitation about including (iii) in the paper is partly due to the fact that I believe that the implementation of the ICARUS-14 calorimetry would make these considerations somewhat redundant.

Somewhat similar considerations apply to 4). While this paper makes some important points, the tabular material is very extensive which will lead almost certainly to its rejection on this ground alone. Once again, therefore, could you please consider carefully what parts of this paper should be included in the final text (that is, if we submit it for publication!). It may well be that we should aim to publish this material in a SPAWAR Volume 3 report? I think that it is important here that whereas 2)-6) were reasonably logical (with an underlying aim of demonstrating how the analysis should have been done), the publication of 7) out of sequence removed the logic of following this procedure.

This brings me to 5) together with 7). My main difficulty with 7) is that the all important Fig. 1 of 5) has got lost in 7). I wonder, therefore, whether we could somehow combine 5) and 4) to produce a new text with the major aim of illustrating excess enthalpy generation along the lines of Fig. 1 of 5) which links the Co-deposition and Pd-B systems back to the original observations.

This therefore leaves 6). What should we do with this paper? Is it not just a monument to the stupidity of the scientific public?

No peace for the wicked! Regards to Linda

Yours,

Martin

P.S. When I went to Southampton last week I found the following information about copper powders which may be useful?

The best source may be Alfa Aesar. I don't have a U.S. Address but a possible international address could be:

Johnson Matthey (Deutschland) Management GMBH & Co. KG,
Post Box 110765,
D76057, Karlstuke
Germany

They supply Copper powder, 100 mesh, Catalogue number UN 3089, 500g of this powder costs £21:50

Another source could be:

Scientific and Chemical Laboratory Supplies Ltd.,
Carlton House,
Livingstone Road,
Bilston,
West Midlands,
MV14 002, England

They supply copper powder, C00 50 may be suitable, cost £8:45 per 100g.

A further source could be:

Goodfellow Metals,
Ermine Business Park,
Huntingdon, PE29 6WR,
U.K.

I have an American address:

Goodfellow Corporation,
800 Lancaster Avenue,
Berwyn, PA 19312-1780,
U.S.A.

The Goodfellow Metals Catalogues were sadly incomplete but apparently they supply spherical balls which are precision ground. The cost may be prohibitive but, of course, we would not use precision ground spheres!