The Collapse of the NHE Project

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In 1994, Japan's Ministry of International Trade and Industry (MITI) instituted a research project in cold fusion, which they called "New Hydrogen Energy," or NHE.[1] An R&D laboratory was established in Sapporo, Hokkaido, where experiments were conducted for four years. Estimates of the total cost vary from $23 to $30 million.[2] When the program ended in August 1997, officials held a press conference and told the Japanese newspapers, Reuters, and *The New York Times* that none of the project goals had been met and no excess heat had been detected in any of the experiments. That was not true. Visiting scientists at the NHE, including Melvin Miles (China Lake Naval Air Warfare Center), did detect and report excess heat. Miles is one of the world's leading cold fusion researchers, and he has published extensive positive results previously.[3]

In this issue, we present an inside look at the NHE program, written by Melvin Miles (p. 18). This is a careful, balanced, detailed, and fair description, free of malice.

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In his recent lecture at the American Chemical Society conference, Miles described his NHE results. I had heard nothing about them from my Japanese correspondents. I became suspicious, and I asked Akito Takahashi (Osaka University) to fax me the section of the NHE Final Report which mentioned Miles' work. (See Excerpts from the NHE Final Report, p. 29.) He and I translated it and sent it to Miles, who was shocked to learn that it did not mention his positive results. He felt that it is important that both his research paper and his personal impression of NHE be published, to counter this negation of his work.

The NHE managers disagreed with Miles. At ICCF-7 they mentioned his excess heat, but seemed unconvinced that it was real. Miles and I expected this would be their final conclusion. He submitted a detailed, formal paper to the NHE managers in which he claimed that eight out of ten cells produced significant excess heat.[4] (There were also two control runs and one equipment failure.) The authors of the Final Report had a professional responsibility to describe Miles' own conclusions about his research—however briefly. They should have said they were skeptical about his conclusions, and they should have explained why. If a lab technician had disagreed with the NHE managers, omission might have been justified, but Miles was the scientist in charge of this set of experiments, and he was convinced the results are positive. It is highly improper to ignore such critical differences of opinion. This report is the formal summary of a $30 million government research project, not an advertising campaign or a political platform.

In Japan, consensus is valued and it is considered unseemly to run roughshod over the minority. Japanese reports often reach inconclusive, mealy-mouthed conclusions to accommodate all points of view. This Final Report goes to the opposite extreme, squelching all debate.

When a government project ends in abject failure, it is not uncommon for the final report to put a brave face on defeat and to highlight any little progress that was made. This report does just the opposite: it covers up success! It makes a bad situation look hopeless. The authors are not listed, but they would undoubtedly include Kazuaki Matsui and Naoto Asami (project and lab directors, respectively). I have discovered that they both support the Final Report conclusions. I find it unbelievable that they would willingly display themselves in such a poor light.

The final report is terse, lacking the level of detail appropriate to a formal document describing a multimillion dollar project. Whether these NHE managers honestly disagreed with Miles or not, we shall probably never know. They refuse to communicate. When the project began, Fleischmann was allowed to visit. He submitted three formal reports and repeatedly requested access to data, to no avail. Although officially listed as a formal adviser, he was frozen out by the NHE management—an appallingly cavalier manner in which to treat a distinguished Fellow of the Royal Society.

There is no way of knowing why the managers behaved the way they did. Did they actually disagree with Miles' conclusions? Were they covering up their own inability to replicate his results? Perhaps they were simply unable to understand his technique and analysis. One thing is certain: they consistently and deliberately ignored his results. Miles says that when the cell he brought from China Lake began to produce excess heat, Asami politely refused to look at it. For weeks, the experiment sat there producing excess heat, yet Asami and the other managers did not bother to look at the instruments! It is astonishing that a person trained in a scientific discipline and charged with the responsibility of managing a multimillion dollar research program would act in this outlandish fashion. Perhaps Asami was so depressed by the imminent demise of the program, and four wasted years of his life, that he suffered paralysis of will. Or perhaps, as Miles suspects, Asami refused to look at the experiment because he felt he understood the calorimetry perfectly well, and Miles was obviously wrong, and it was not worth the bother. Whatever the reason, Asami's actions prove that he and the other managers never gave this experiment a fair chance. They never seriously analyzed it. They could have learned all about the experiment while it was underway, and Miles was available to explain it. I doubt very much they sat down after he left, read his thirty-two-page formal report, and earnestly debated the pros and cons of the calorimetry.

The U.S. DOE ERAB cold fusion panel members also refused to visit some laboratories or look at some experiments. The panel also shortchanged Melvin Miles. When the panel members visited him, he gave them his initial, negative results. Later, when he began to observe excess heat, he sent them a revised report. The ERAB report described his initial negative results only, ignoring the positive ones! This is appalling, but somehow it does not seem as bad as Asami's refusal to walk a few paces into a room to look at an experiment. The DOE panel was a farce. It was intended to reach a negative conclusion quickly, at minimum cost. Asami had a larger mandate. He was given millions of dollars and extensive facilities, and he began, at least, with a positive attitude.

When Miles invited Asami to look at the cell, Asami told him he should discuss the matter with Masao Sumi instead. Sumi's excuse for not looking made no sense: "[MITI] wanted large effects and would probably not be influenced by reports of small excess power although these would still be of scientific interest." It has been common knowledge from the start that cold fusion usually produces small effects. If MITI is not interested in small power gains, they should not have entered the field. Researchers must learn how to generate small effects first, and then find a way to amplify and scale them up. The NHE could not skip that step and graduate directly to 1,000 watt reactors! Every effect and useful phenomenon discovered in modern history has begun on a small scale. Airplanes began as kites and six-inch square metal airfoils in the Wright brothers' wind tunnel; the first fission reactor at the University of Chicago produced a half-watt of power (although it did weigh 864,000 pounds); transistors began with a small, handmade, impractical point-contact device that intermittently amplified a signal by 30%.

Some of the Problems at the NHE

When the NHE program finally collapsed, it came as a surprise to no one. We predicted this in 1996, because the NHE ignored recommendations made by experts, including recommendations in the leading Japanese journal of physics.[7,8] The problems at the NHE turned out to be much worse than we realized. Late in the program, when the NHE began feeling desperate to see positive results, they invited Miles for six months, and Ed Storms for a brief visit. Michael McKubre of Stanford Research Institute International (SRI) also spent some time there. After years of trying to do the job with engineers alone, someone decided that research scientists with Ph.D.s in materials science and electrochemistry might help. These outsiders agree on several criticisms, especially with regard to materials and the isoperibolic calorimetry which seems senselessly crude and inaccurate. It was based upon *a single calibration point*, taken *three or more days* into the run. It would be hard to come up with a less reliable method. Fleischmann knew about this methodology, because he analyzed the early NHE data published at the ICCF conferences, and he figured out what they were doing.

The project was burdened by a top-heavy, unresponsive management structure in charge of priorities, budgets, and research topics. It had a three-layer bureaucracy, steering committees, and an advisory board to manage a laboratory with a dozen people in it! Industry, academic, and government agencies contributed matching funds, vetted decisions, and developed a detailed plan before the project began. They did not consult with the leading cold fusion scientists, or with any leading electrochemists as far as the cold fusion scientists know. (Electrochemistry is a small world.) You cannot do groundbreaking research according to a schedule cast in concrete by a committee four years in advance. Research calls for flexibility and creativity. Asami understands this, which is why he let Miles and other staff members do what they felt was best. The problem was, the laboratory was staffed by engineers who had no experience in basic research. They did not *know* what was best, or where to start, because they were not Ph.D. electrochemists with experience in basic laboratory research.

"Type A" Palladium

For many years Martin Fleischmann has recommended a particular type of palladium made by Johnson-Matthey. He handed out several samples of this material to experienced researchers, and, as far as he knows, in nearly every test the samples produced excess heat. Fleischmann calls this material "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. It was designed to have great structural integrity under high loading. It lasts for years, withstanding cracking and deformation that would quickly destroy other alloys and allow other gasses to seep through the filters. This robustness happens to be the quality we most 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 will not load above ~60% to ~70%. Below 85 to 90%, bulk palladium never produces excess heat. A sample of palladium chosen at random from most suppliers will *never* reach this level of loading. You could perform thousands of tests for cold fusion with ordinary palladium and never see measurable excess heat. That is essentially what the NHE did: they performed the wrong experiment hundreds of times in succession, using materials which cannot work. This is like trying to make a twenty-seven-story building out of doughnuts.

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 listened to Martin Fleischmann's advice. Unfortunately, people seldom listen to advice or follow directions. Fleischmann sometimes compounds the problem by speaking in a cryptic, convoluted style and by using complex mathematical equations that few other people can understand. He sometimes takes a long time to respond to inquiries; he answered one of my questions two years after I asked. However, in this case he has made himself quite clear on many occasions. For example, he wrote:

> . . .We note that whereas "blank experiments" are always entirely normal it is frequently impossible to find *any* measurement cycle for the PdD_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. 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. [Fleischmann, M. *Proc. ICCF-7*, 121.]

Fleischmann recently gave me some additional information. 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. I asked him 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 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. They did not have any of the Type A left. Perhaps they used some other Johnson-Matthey material instead. They have refused to tell Fleischmann the batch number or say when or where they acquired the material, but as far as he knows, there was no Type A material available at that time.

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

Tadahiko Mizuno (Hokkaido University) said the NHE staff thinking was unoriginal. Miles said "the engineers did excellent work in duplicating other people's experiments but did not come up with many new ideas." An unfair comment, perhaps. In engineering, originality is not valued as highly as knowledge, competence, and teamwork. The finest engineers are essentially cautious, thorough perfectionists. They are supposed to stick close to the building codes and the textbooks. They look up the answer when they can, rather than working things out from first principles. They take no unnecessary chances; people's lives depend upon the machines they build. Engineers are not the right people to do groundbreaking new science. By the same token, you would never assign a group of research scientists to build a practical machine! Engineering, laboratory research, and theoretical science draw upon the same knowledge base, but they require different skills. Miles feels that if cold fusion had been closer to technological fruition, this would have been an ideal group to work on it, but in 1995 it made no sense to staff a cold fusion project with engineers. MITI is supposedly a pragmatic, experienced organization, yet somebody in authority there does not understand the difference between industry and academic science. McKubre summarized the problem: "The purpose of the NHE project was to give Japan a competitive advantage in the development of cold fusion *technology*. Not to research a phenomenon to practicality."

You cannot do groundbreaking research according to a schedule cast in concrete by a committee four years in advance. Research calls for flexibility and creativity.

This desire for competitive advantages explains why the NHE was a closed club. It was an industrial consortium: the sort of thing you form to make a better computer chip or some other incremental improvement to existing technology. The information gleaned from the experiments was supposed to be made available exclusively to corporations and huge national agencies that paid millions to participate. Research in basic science cannot be done on this basis. Results must be published in journals and made freely available to everyone.

A good engineer must have initiative, and this is one quality the NHE staff lacked. They built fine instruments and they reproduced some experiments diligently, but they did not take steps on their own initiative to learn more about the field and make sure they were doing things right. They did not read the literature, and they often missed the point. Engineers tend to respect authority and reach for the textbook, but unfortunately there are no textbooks in cold fusion. There are no blueprints to follow and no authorities, except Fleischmann, whom they made a point of shutting out. There is excellent advice scattered through the literature in papers by Storms, Cravens, Fleischmann, and others. Unfortunately, the NHE engineers did not know about it. They blindly repeated bulk-palladium experiments more than 100 times, without checking the most important parameters, such as whether cathodes were expanding too much (excess volume), loading unevenly, clotting at the surface with galvanized contamination, and without measuring the open circuit voltage (OCV). They finally began doing these things in the last year of the program, after Storms visited and acquainted them with the techniques, but by that time it was too late.

As a practical matter, I do not think the NHE engineers could have read much of the English literature. Miles says that Mari Hosoda, a female laboratory technician, "spoke English reasonably well" and was able to answer most of his questions. I take that to mean the others in the laboratory did not speak English well enough to carry out ordinary, day-to-day research activities, so they would have found it tedious and unrewarding to read English papers. They would have found Fleischmann's abstruse instruction handbook particularly tough.

The engineers sometimes became so focused on accuracy and the job at hand, they lost track of the larger purpose.

When Storms visited the NHE, he discussed the excess volume problem. A cathode made of weak material will swell up too much when it loads. He demonstrated his technique for measuring the volume of a cathode before and after electrolysis. He measures the thickness of a palladium foil at six points with a handheld micrometer and extrapolates the total volume. There are more accurate ways of measuring volume. The most well-known and fundamental is Archimedes' method: you submerge the object in water and see how much fluid is displaced. But, as Storms explained, this does not work with a cathode. You must measure the volume when the cathode is in the beta phase (~70% loaded, with seven deuterons for every ten palladium atoms). In this state, the cathode acts like a balloon with a hole in it. The gas rapidly escapes, or "outgasses," and the swelling goes down, so you must measure the volume quickly before gas escapes. When you put the sample in the test tube of water, bubbles from the escaping gas cause the water level to slosh up and down, and you cannot get an accurate reading. When the cathode reaches the alpha phase (~30% loaded) it is no longer under much pressure and outgassing slows down. The swelling also goes down. The cathode is stable and quiet, but it tells you nothing about excess volume.

Months after he returned home, Storms talked with the NHE engineers again. They said they were now measuring excess volume per his recommendation, but they were not satisfied with the accuracy of the handheld micrometer. They decided to use Archimedes' method, but they were having troubles with bubbles. So they decided to test only alpha phase samples. They forgot that this defeats the purpose!

Storms later acquired samples of NHE palladium and tested excess volume himself. He found that some of it expands 20 to 30%. Anything above 2% will prevent the cold fusion reaction. Storms also described a case in which the researchers did not have the right background to interpret the data. When contamination from the electrolyte is galvanized onto the cathode surface, it can block the surface and prevent loading. This is a common problem. Storms asked the NHE researchers if they had experienced it. They assured him their cathode surfaces were clean. He asked for proof, and they brought out a six-inch thick binder filled with analytical reports, SEM (scanning electron microscope) and XPS (X-ray photoelectron spectroscopy) studies of the cathode surfaces. Storms saw that the XPS data showed a thick layer of silicon on the surface, more than enough to prevent loading. He recommended steps to reduce contamination. The engineers quickly learned to interpret the data correctly, and they took the steps he recommended, but it was too late in the program to have an impact.

On top of the technical difficulties, the language gap, and the knowledge gap, there was a morale problem in the lab. Some of the NHE researchers were unwillingly shanghaied from their regular jobs and exiled to Hokkaido, far from their families and friends for six months, or for years. They were ordered to research cold fusion, although they had no particular interest in it. Some of them openly stated that they wanted to get the job over with quickly, publish a negative result, and go home.

Other people working on the project were sincerely interested and hoped for positive results. They were deeply discouraged by the negativists. Mizuno's lab is close to the NHE. The unhappy people who sincerely wanted to do cold fusion would slip out of the NHE, visit Mizuno, and complain. They told him they wished they could quit the NHE, move into Hokkaido University, and work with him instead. They said that when they saw signs of excess heat, the managers would dismiss it as experimental error. They told him, "No matter how positive the data might be, the NHE will never let us publish it."

Soon after the program got underway, Mizuno and most other Japanese cold fusion scientists were frozen out. Miles wondered why Mizuno never came to visit him. It turns out Mizuno never knew Miles was there, because no one at the NHE told him. Mizuno feels the NHE managers did not want professional electrochemists, academics, and other "free agents" coming around because they were "too critical." They poked around, made suggestions, and embarrassed people. When a project runs into trouble, people sometimes become defensive, which only makes matters worse.

Complaints About the NHE by Outsiders

After Melvin Miles sent us this detailed report, I decided to circulate the section of the NHE Final Report dealing with his work to other scientists who had participated in the project for comments. I sent copies of the translation and the Miles abstract to Fleischmann, McKubre, Storms, Takahashi, and Mizuno. I was surprised at the vehemence of their responses. They fell into two camps: angry and sympathetic.

Fleischmann, who seldom communicates unnecessarily, responded with a twelve-page, blistering fax. He told me much the same story two years ago when we met. He is still quite, understandably, upset by the way they treated him. Takahashi and Mizuno discussed the NHE report during a conference on January 20, and Mizuno sent me an angry, two-page e-mail, condemning the NHE and Japanese science in general.

Miles, McKubre, and Storms are more moderate. They sympathize, but they do not excuse the mistakes. McKubre was kind enough to answer several questions about the NHE calorimetry. Storms discussed the NHE problems with materials last year and has little to add at this time. Miles did a splendid job of presenting the sympathetic view, so I will devote the rest of this article to the more outspoken views of Fleischmann and Mizuno. I myself view the NHE with a mixture of anger, consternation, and sympathy for people who challenged the unknown and found themselves facing a far more difficult task than anyone on earth could have predicted.

Fleischmann began his association with the NHE by providing them with a 500-page instruction handbook covering every aspect of the isoperibolic calorimetry that he and Stanley Pons devised at the IMRA lab in France.[9] Some people have criticized this calorimetry because it relies upon complex equations. There are alternatives. A slightly more elaborate cell with an external envelope for the temperature probes is easier to model and easier for most researchers to deal with. First-principle flow calorimetry is easier to understand, and it yields more readily identifiable and quantifiable results, although Miles and others say it can prevent positive feedback. Calvet calorimetry is reportedly accurate and relatively easy. The difficulties experienced by the NHE researchers show that it takes a skilled person to operate the Fleischmann-Pons calorimeter. However, people who criticize the complexity miss the point. Fleischmann and Pons designed this cell for a specific purpose. They ran sixty-four cells at one time, monitoring them with a single, small computer. This allowed

Excerpts from the NHE Final Report

The NHE project was under the auspices of an MITI subsidiary agency, the New Energy Development Organization (NEDO). Here is one section of the NHE Final Report, titled *Shinsuiso energii jissyou gijyutsu kaihatsu* (New Hydrogen Energy Verification, Engineering and Development), NEDO-NHE-9701 (June, 1998), p. 120. The authors of this report are not listed. This translation is by Akito Takahashi and Jed Rothwell, with footnotes and commentary by Jed Rothwell.

Section 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°C, was upgraded to an ICARUS-2, which allows operation at higher temperatures in the 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 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 3.51 [not reproduced here.].

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 in all three cases, as in previous tests, excess heat was not replicated. Sample results are shown in Figures 3.51, and 3.52 [not reproduced here]. After maintaining a 200 mA current for a period of one week, current was increased 250 mA for approximately one month, and then the experiment terminated with a boil-off event. 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. In the analysis of the boil-off event, when we took into account evaporative losses, a peak value seems to indicate excess heat, but this was only caused by an overflow, [See Translator's Note 1.] 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 Figures 3.5.9 through 3.5.12 [not reproduced here]. [See Translator's Note 2.] 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 the cell, while the NHE team added heavy water constantly and automatically. [See Translator's Note 3.] Calculated excess heat fluctuthem to test many variations of the experiment simultaneously, and many types of material. This Edisonian approach is an excellent idea. You could not do it with a bank of sixty-four flow calorimeters or Calvet calorimeters. That would be a nightmare of complexity. The equipment alone would cost more than a million dollars. Given the poor reliability of these systems, on most days several of the machines would be broken.

The NHE could only run three of these cells, not sixty-four, so they had no need for enhanced reliability and simplicity. (Miles reports that they had at least six extra glass cells in storage, but they could only run three cells at a time in the water bath.) They might have done better with a more elaborate, expensive isoperibolic cell with external temperature probes, like the one Miles designed and brought with him to Japan. This is easier to model. Fleischmann told me that if he was making one or two cells for demonstration kits, he would use a similar design, with a copper sleeve around the cell, temperature probes embedded in the copper, and a thin outside sleeve. (If the outside sleeve was too well-insulated most of the heat would escape from the lid.)

Miles said the 500-page handbook included complex full page physics equations, and it was so difficult it would have taken him six months to master it. He used his own equations developed earlier at China Lake. He did not follow the manual's instructions to the letter—for which the NHE Final Report criticizes him. However, he did study the manual, and the more he learned, the more his respect for Fleischmann grew. His own equations are based upon a similar model, but in a considerably simplified form. The NHE criticism of this technique is hard to justify when none of their adopted techniques were based upon those of Fleischmann. They were, I believe, merely engineering

ated 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 1A. 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 the condensate collection vessel. [See Translator's Note 4.]

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, [See Translator's Note 5.] and based on the comments they made to us, we conducted long-term experiments in which electrolysis continued for a half-year, [See Translator's Note 6.] 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.

Translator's Notes

1) The English word "overflow" is used here. Based on the NHE claims made during ICCF conferences and in an interview published in the *Nihon Keizai Shimbun*, I surmise this means entrainment; that is, unboiled fluid leaving the cell in droplets or froth. Fleischmann addressed this issue years ago in his lectures and letters, pointing out that "we recover ~95% of the alkali by dissolving the residues and titrating; some is undoubtedly lost by irreversible reactions with the glass walls of the Dewars." [Letters to Steve Jones] In other words, he proved that significant amounts of electrolyte do not leave the cell, because if it did, it would carry off the reagent and virtually all of the reagent is found in the cell after a boil-off test.

2) These graphs bear no resemblance to the graphs shown by Miles at the American Chemical Society or in his ASTI paper. As discussed above, Miles is convinced the cells did produce heat, but the authors of this report apparently disagree.

3) 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, January 2000.]

4) 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.

5) Fleischmann says this is not so. He gave them only three samples, which were used in early tests, published in ICCF-5.

6) Running a cold fusion experiment for six months without results is a preposterous thing to do. I very much doubt that Fleischmann and Pons recommended this course of action.

Commentary

The conclusions reached in this report are exactly the opposite of those Miles described in his report on the NHE and in his ACS and ASTI presentations. Miles thinks the NHE data evaluation method is not based upon Fleischmann and Pons:

> 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. [Miles, M.H. "Report on Calorimetric Studies at the NHE Laboratory in Sapporo, Japan," ASTI proceedings].

The NHE says their calorimetry is based upon Fleischmann and Pons' method described in the handbook. This report accuses Miles of departing from that established standard in his tests with the Fleischmann-Pons cell. (His own cell, which he brought from China Lake, works by conduction rather than radiation across a vacuum gap, so the model and equations would need to be different.) Experts who have examined the NHE methods think that their method is a departure from the Handbook and indeed from all other physics-based models, for two main reasons:

1) They employ only one calibration pulse, which is performed three days into the run. This method is unheard of, outside the NHE.
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.

Fleischmann examined the single calibration pulse published by the NHE at the ICCF conferences. [Fleischmann, M. "Cold Fusion: Past, Present, and Future," *Proc. ICCF-7*, p. 119.] He says it proves the cell was already producing excess heat when this supposedly zero-point setting was established. He bases this onto first principle, absolute methods of analysis:

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.

He published papers about this and informed the NHE, but they ignored him.

McKubre described the arbitrary zero-point setting:

> The Japanese retroanalysis 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 three 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, January 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. The NHE workers chose to keep the equations simple and fold all minor sources of noise into one large estimate of uncertainty.

approximations (see Excerpts from the NHE Final Report, p. 29), and I suspect the handbook remained permanently unopened.

In his fax to me, Fleischmann listed his complaints about the NHE and about his former employer IMRA, which worked closely with the NHE:

For several years, Fleischmann repeatedly requested detailed information about the experiments performed at the NHE. He asked for copies of raw data. The NHE ignored his requests. He used data they published at ICCF conferences instead.

He asked for data from "blank" runs (control runs with platinum or non-working palladium). They did not respond, and they have not published any blank run data at ICCF conferences, so we do not know whether control runs were ever carried out. Miles says that to his knowledge, no blank runs were performed with Fleischmann-Pons cells at the NHE. This is astounding. I have never heard of anyone doing these experiments without control runs.

The NHE claims that they used Johnson-Matthey Type A palladium in their final round of tests. Fleischmann disputes this, because he only gave them three samples of Type A, and he knows they used these in the initial stages of the project. Two of these samples produced excess heat and one failed for prosaic reasons. As far as Fleischmann knows, Johnson-Matthey stopped making this type of palladium many years ago and gave him all of the remaining stocks. [See *"Type A" Palladium*, p. 27.]

In his instruction handbook for the NHE, Fleischmann said there are some thirty ways to analyze the calorimetric data. Twenty-nine of these variations produce reasonably accurate results in a close range of values. One is "highly error prone" and usually incorrect. The NHE later refused to tell him which method they selected, but based on his retroactive analysis of the data they published at ICCF conferences he concludes they selected the one unreliable method which he warned them against.

The NHE claims that the calorimetry is error prone and enthalpy fluctuates around the zero line. However, for the margin of uncertainty to be as large as they claim, Fleischmann says, "one would need an error in the cell temperature of 5 to 6°C." This is ridiculous; no thermocouples are that inaccurate. It is particularly ridiculous because two thermocouples were used, and they agreed to within a fraction of a degree.

After Fleischmann returned to England from France, his files and personal papers from the laboratory were shipped to his house. Someone from the NHE or IMRA France removed many of his personal documents and critical data showing excess heat. They have refused to return these documents despite many requests over the years.

At ICCF-5, the NHE showed data from two experiments. The first was subject to a fault which could not be identified (probably a bad connection). The second was corrupted by excessive noise. These facts were spelled out to the NHE by Fleischmann in two formal reports. He showed that in the second experiment, despite the noise, excess heat was measured.

I sent this list of objections to the project managers, Matsui and Asami, along with the translation of the Final Report and the abstract of the Miles paper. They did not respond to me. However, they asked Elliot Kennel (a U.S. nuclear engineer who worked at the NHE for one year) to reassure Melvin Miles of their good intentions. Kennel wrote to Miles:

> 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.
>
> Matsui-san and Asami-san have indicated to me that they don't care what Jed writes, but they do care about your opinion.

Miles refused to be mollified, responding curtly:

> 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.

Matsui and Asami told Takahashi that they will reevaluate the Final Report in February to see whether it fairly represents Miles' viewpoint. They might revise it, but that would hardly matter now. To undo the damage, they would have to formally retract their claims in letters to the scientific journals and hold a formal press conference to inform the Japanese newspapers and *The New York Times* that their previous announcement was in error. I do not think they plan anything so dramatic!

The Big Mistake Hypothesis

Was the NHE project a conspiracy to make cold fusion look bad or to turn it into a "black project" in nuclear weapons research? Was it intended to fail from the very beginning? I do not believe in conspiracy theories and I would not entertain such a lurid question normally, but Martin Fleischmann thinks the program may have been a set-up. He believes that the military and government agencies like the CIA are afraid that cold fusion may have weapons applications, and they are actively campaigning to suppress it. I doubt this, because I have little respect for the acumen of the CIA and other intelligence agencies. After all, the former CIA head (now MIT professor) John Deutch knew all about cold fusion in the early days but has been pompously ignoring it ever since. It is only barely plausible that lower echelon CIA operatives cared about cold fusion enough to suppress it, while the Director, an MIT chemist, ignored it.[10] We do not know whether cold fusion has nuclear weapons applications, but if it does I do not think the CIA would be smart enough to realize it. I respect Fleischmann. I do not think he is paranoid or blinded by his bitter experiences. He has many years of experience in industrial research. He was close to the NHE project and he conferred with the leaders before they froze him out, so he is in a much better position to judge what happened than I am. There is no doubt the program had powerful enemies, but my gut feeling is that Miles is correct; the program failed mainly because the problems are difficult, and engineers are not the right people to solve them.

Why would the enemies within the Japanese government bother to spend $30 million to kill cold fusion, when it was already dead? Long before the NHE project got underway, cold fusion was discredited in the eyes of the Japanese public. There was no funding available, and cold fusion scientists were shut out of the major scientific publications, ridiculed at conferences, and ostracized from universities and corporations. Cold fusion has never been a threat to its most prominent enemies like Akito Arima (retired president of Tokyo University) or John Huizenga (DOE ERAB panel). If anything, they have benefitted by periodically attacking it and making a name for themselves in the newspapers.

Perhaps Fleischmann underestimates the power of stupidity, and its role as one of the great driving forces of history. Barbara Tuchman gave numerous examples in her masterpiece *The March of Folly* (Alfred A. Knopf, 1984). When I think of the mistakes, the waste, and the heartbreaking lost opportunities of history, I do not find it difficult to imagine that the NHE project was bungled. MITI has made colossal mistakes, much more expensive than this. Shortly before the NHE began, MITI's Fifth-Generation

Computer Project ended without achieving any major goal. Their breeder reactor program is one of the most expensive fiascoes in the history of energy. If everything had worked according to plan, plutonium breeder reactor electricity would be five to fifteen times more expensive than electricity from conventional sources. In 1992, after spending $5 billion on the Monju reactor, a senior government official said, "It is almost inconceivable that such a good idea could have turned this bad. We spent the last twenty years building this project, and will probably spend the next twenty killing it."[11] Monju was closed and put on "standby" in 1995 after three to five tons of sodium leaked out, came in contact with water, and burst into flames.[12] It will not be reopened until 2003 at the earliest.[13] In 1999, fuel preparation for the Joyo Fast Breeder reactor resulted in Japan's worst nuclear accident, at Tokaimura, which killed one worker and severely injured another. This led to the first major public protests against nuclear energy in decades and the delay of two power reactor projects.[14] The breeder reactor program is endangering the fission power industry in Japan more effectively than a conspiracy or a propaganda campaign ever could.

The NHE program began with good intentions and high hopes. Mizuno says that when it started, many scientists came out of the woodwork and applied for grants, including some "vaunted experts" who had earlier criticized cold fusion or made vague, noncommittal statements about it. Mizuno says these people are "always ready to jump on the bandwagon," but at the same time they are "careful to leave a clear path of retreat in case things go sour." They change their opinions as quickly as a weathercock changes direction, they take no responsibility, and they say one thing in public and another in private. When EPRI announced it would fund cold fusion research, Tom Passell of EPRI saw similar behavior. Scientists who were publicly excoriating the field, privately came to him asking him for grants to study it. Mizuno says the NHE was supervised by MITI and by a panel of "so-called outside experts," which means, in practice, people who have not performed cold fusion experiments or published papers. It means the people in charge of the program were nuclear physicists and others with no qualifications and no clear idea what needed to be done. This works about as well as putting a group of electrochemists in charge of a tokamak project, or putting the Boston Red Sox baseball team in charge of the Metropolitan Opera.

Mizuno suspects that Arima had a high-level advisory role in the NHE. He is a very influential scientist, and he despises cold fusion. He would have tried to kill the program, and failing that, he would have tried to limit the budget and prevent first-rate people and resources from being assigned to it. Y. Takahashi of Tokyo University was probably a high level advisor to the NHE, and Hideo Ikegami from the National Institute of Fusion Science (NIFS), Japan's tokamak hot fusion research center, was listed as one. These two "used to be" cold fusion researchers, as Mizuno put it. Sometime or other they stopped doing research and carefully distanced themselves from it. Ikegami measured excess heat with isoperibolic cells, and he faxed copies of the data to me and to several other people, but he never published this or any other data. I asked him why he held back. He said, "We only publish world-class research. Anyone can think of a dozen reasons why this kind of the cell would produce artifactual heat." But he did not offer any reasons then, or at any other time, as far as I know. When the NHE program collapsed, Ikegami told *The New York Times*, "We couldn't achieve what was first claimed in terms of cold fusion. We can't find any reason to propose more money for the coming year or for the future."[15] It is difficult to know what to make of this statement. Who did he mean by "we"? The NHE engineers? Melvin Miles? Ikegami himself? If he meant that the NHE engineers failed, most people would agree. But if he meant that every experiment at the NHE lab failed, including Miles', and that all money for research in Japan should be cut off, then I think he is one of those irresponsible elite scientists who change direction like weathercocks and keep a clear path of retreat ready. We do not know what Ikegami meant. People have asked him repeatedly, but he does not respond. I suppose a mainstream scientist would say Ikegami was making an honest reappraisal of his previous support for cold fusion and a brave admission that he had been wrong.

Political dirty tricks and media pressure was brought against the NHE project soon after it began. After ICCF-3, Japanese National Television (NHK) broadcast a "muckraking" exposé-style television news report damning the project, and cold fusion in general. The project was portrayed as a government waste and fraud. Japanese television viewers would be inclined to believe the worst because many government projects in Japan are, in fact, boondoggles. The cameramen attended ICCF-3 but they did not use the footage of lectures or interviews. Instead they portrayed themselves as muckraking reporters who had to sneak around stealing information from hostile scientists and bureaucrats. They set up a hidden camera in a car parked across the street from a restaurant where an NHE planning session was being held to give the impression they were catching people going into a nefarious, back alley, secret meeting. In fact, the reporters had been invited to attend the meeting and set up the cameras inside, but they declined. Later, they interviewed a low-level bureaucrat assigned to the project. They blurred the image of his face and disguised his voice. He said, "MITI forces us into projects like this by telling us that if we do not participate, they will not let us into the real juicy plums later on. Nobody really wants to do it." If corporations were pressured to contribute to the project, that would explain the hostile attitudes of the researchers Mizuno met and the internal sabotage and poor planning.

We can only speculate about what happened inside the NHE. From my discussions with Miles and the atmosphere Mizuno describes with despondent researchers taking refuge in his lab, I suppose morale went to pieces. After years of failure, people lost hope and began looking for scapegoats. They picked Fleischmann and Pons. They took steps to cover up their mistakes and hide data. I think they hid positive data for the same reason MIT did in 1989. Not because they feared the data shows excess heat—by that time, they were convinced there can be no such thing. They feared that release of the data might prolong a useless controversy. They thought the data was meaningless.[16]

Perhaps, to put it bluntly, some of these NHE researchers were inept. Thomas Edison founded a famous industrial research laboratory, the world's first, which you might assume was filled with the best and brightest. Actually, it was "a veritable cuckoo's nest of learned men, cranks, enthusiasts, ambitious youth, plain muckers, and quite insane people." One of the supervisors complained that, "Our present staff of juveniles are excessively stupid. All of them combined have not as much common sense as would be required to keep a ton of pig iron from floating out to sea in a calm."[17] Research laboratories attract strange people. The work is nebulous and specialized, making it difficult to tell what people are up to and who is to blame when things go wrong. How would the NHE mangers know their staff was misinterpreting the XPS studies and not seeing the silicon contamination? The managers themselves did not have the necessary skills. The staff went on making this mistake for almost four years. If this had been conducted as open, academic research, they would have published papers, shared data with Fleischmann and anyone else who asked, posted data on the Internet, and invited people like Mizuno and Storms into the lab the first year. The mistakes would have been caught early on.

I doubt the NHE engineers were dolts. They were not to blame. The problems were caused by high-level management mistakes and inflexible plans. Most big-company Japanese engineers are smart, and Asami ran the NHE the way a good research lab should be run, but even good people can find themselves in over their heads. Cold fusion is tough! The NHE picked the most difficult experiments, with bulk palladium and heavy water. Even McKubre, with all his resources and expertise, can barely replicate his own results with this method. Good people need good leadership and clear direction. They need leaders with a broad grasp of the field. The managers should have begun by making a broad survey of the field and inviting experts like Storms, Mizuno, and Fleischmann to visit and consult. They brought in only Storms, when it was too late. I criticize the engineers for not reading Storms' paper describing excess expansion, but how can they have known it existed? An engineer at Hitachi who speaks little English will not know about a paper in an obscure, out-of-print conference proceedings.

Some Anthropology

I do not know what happened inside the NHE, but I can speculate based on what takes place in other dysfunctional Japanese institutions. Bear in mind that all organizations in Japan are unique—they do not fit a rigid pattern any more than American organizations do. They are not all dysfunctional, by any means! And they are organized in many different ways: the PTA is nothing like the Mafia. But they draw on the same traditions and culture, so they have recognizable patterns, just as American or French organizations do.

Asami's management techniques, as described by Melvin Miles, are in the classic tradition of Japanese management, which stretches back hundreds of years. He ran things the way an old-fashioned Japanese boss would do it, with weak supervision, autonomy and responsibility given the individual, yet with strong, healthy group bonding. Some American cold fusion scientists have said to me: "Asami is a nice guy, he is really on our side, he is still working behind the scenes to revive the program. Don't blame him. He was just following orders." This is a misreading of Japanese culture. Japan has a reputation for being a conformist society where low-level peons knuckle under and follow orders. The catch is, those low-level peons are the ones who write the orders in the first place. Pressure to conform does not only come from above, it is peer pressure from co-workers. A great deal of power in organizations usually resides in the lower ranks.[18] The leaders are often figureheads, or weak nonentities. This is not a democratic tradition. The Imperial Japanese Army was largely run on this basis, as were eighteenth century samurai bureaucracies.

At its best, this tradition serves democratic populist ideals, empowering the factory worker, the researcher, and the young. At its worst, it leads to autocratic control by an irresponsible minority, like the small group of southern politicians who once dominated the U.S. Senate by manipulating rules and filibustering. Projects are stymied by low-level functionaries. Decision makers are invisible, like the anonymous authors of the NHE Final Report. They may be ambitious young executives. Many historians feel that up-and-coming junior army officers of the 1930s set policy and pushed the nation into war. At the other end of life, retired people with no official standing act as behind-the-scenes "advisors," pulling strings and manipulating events, while accepting no responsibility for their actions.

In a mismanaged, unhappy Japanese organization, workers may have broad autonomy and responsibility, but they strangely lack the power to change their own job description, and nobody knows who wrote the job descriptions. Tasks are frozen. Project goals cannot be changed because the clash of factions allows only weak, compromise leaders to emerge. No one is in charge, and no one has the authority to make changes. Each department is a separate fiefdom:

> The inability of Todai [Tokyo University] to take practical measures for reform from within is simply the latest symptom of the increasing paralysis of the university's decision making processes. These processes display in a more extreme form features often found in Japanese decision making, such as the preponderant weight of lower strata, the existence of figurehead entities toward the top, the osmosis of consensus from the bottom upwards, the principle of unanimity rather than majority rule, and the long strung-out *shingikai hôshiki* (deliberative consultation method). When it comes to consensus and decisions, Todai is a bowl full of jelly.[19]

If the NHE program was sabotaged deliberately, my guess is that it was sabotaged in the traditional style by a faceless, junior, back-office accountant who arbitrarily sliced the budget to cover the cost of the Kobe earthquake disaster,[20] or by a disgruntled faction of mainstream physicists acting incognito, or by a retired busybody, string-puller like Arima. Most likely, it was destroyed by institutional paralysis that reduced it to "a bowl full of jelly." The least likely scenario is that it was done-in by an organized conspiracy of men at the highest levels who planned and secretly manipulated events, like the bad guys in a James Bond movie.

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