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HIGHLIGHTS OF THE FIFTH INTERNATIONAL CONFERENCE ON COLD FUSION

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ABSTRACT: Highlights of the Fifth International Conference on Cold Fusion (ICCF5) are reviewed. A live demonstration system from Clean Energy Technologies Inc. showed 300% to 1000% excess energy.

Wide-ranging positive results in both excess heat and nuclear products were reported from E-Quest, U. Milan, Osaka National U., Mitsubishi Heavy Industries, NTT, the Japanese National Laboratory for High Energy Physics (KEK), Los Alamos, BARC, Amoco Production Company, Shell Oil, Harwell and others. An electrical engineer from Bechtel Corporation gave a superb talk on the economic and technical aspects of the commercial development of cold fusion energy.

This is a brief report of my impressions of the Fifth International Conference on Cold Fusion (ICCF5), April 9-13, 1995, Monte-Carlo, Monaco. These are things which I thought were significant. Other people would describe the elephant differently. Any conference or trade show is a mixed bag: a few great papers, a lot of ordinary stuff, some disappointments, and many boring papers that are over my head. As I said in my review of ICCF4, I have no background in nuclear physics, and I do not make comments about subjects beyond my level of expertise. I can grasp the hands-on details of experimental setups but I have no knowledge or interest in theory.

First, a word about media: the Book Of Abstracts is about 150 pages. It is well organized for once. Abstracts are numbered in a coherent scheme. Gene Mallove now has a scanner and OCR program, and he plans to scan and upload some of the outstanding abstracts. Akira Kawasaki brought a professional broadcast quality video camera with a remote microphone and he recorded every lecture. Gene brought a smaller video camera, and they will be teaming up to offer for sale videos of the major lectures. Contact:

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Some of the talks were so good they call for long individual analyses. I have barely touched upon them here: Storms [1], Cravens [2], Klein [3], and Srinivasan [4 and 5], and the upcoming Piantelli patent, which was discussed informally during the conference.

The first lecture was the Critical Overview by Storms [1]. It was one of the best. Storms is essential reading for anyone who wants to understand this field. He distributed preprints of his upcoming Fusion Technology paper "A Critical Review of the 'Cold Fusion' Effect" which I highly recommend. I am going to have to sit down with the video of this lecture and spell out all the important points.

Patterson's company, Clean Energy Technology (CETI), got together with Dennis Cravens and brought to the conference a demonstration cell in a flow calorimeter. It worked spectacularly well. Cravens [2] discussed it on the first day. The device output 3 to 5 times input energy, ignoring energy lost to electrolysis gases, and as much as 10 times input if you include various factors like electrolysis gases and the heat lost from the cell container. I will describe it in detail in a later communication. Briefly, input was usually held at about 0.4 watts I*V, although on the last day it was raised to 0.8 watts for a while. The flow rate was 10 ml per minute. When the machine was first rolled into position and turned on in the morning, there was no excess for 10 or 20 minutes, and the temperature Delta T fluctuated around 0.2 deg C, indicating about 0.14 watts output. The rest was lost to known heat leaks from the cell container and to the effluent gasses from electrolysis, which were measured in a gas flowmeter. As the reaction turned on, the Delta T gradually rose to about 2 deg C, and sometimes rose as high as 4 deg C, indicating 20 to 40 calories per minute, or 1.4 to 2.8 watts.

Patterson's device is described in U.S. Patents 5,036,031 and 4,943,355. It is a thin film light water system. It incorporates plastic beads coated with Cu, Ni, Pd, and another layer of Ni. CETI has agreed in principle to work with some of my contacts at major institutions, especially Japanese National Laboratories and Universities. They asked me to make arrangements, which I am hustling to do. CETI is very cooperative and open. I have been following their work for about a year. Few other scientists seem to be as willing to share information, and no other scientists think as quickly or solve problems with such dispatch. A few months ago they had problems with high temperatures and pressures destroying the beads and melting the plastic cell containers. They designed new beads with an extra outer layer of nickel, and they found new, temperature resistant cell materials. They are oriented toward fixing engineering problems and building practical, commercially useful systems. That is what the field most needs.

The CETI demo system is fairly predictable, well controlled, and well behaved, although it did get a bit quirky in the harsh conditions of the ICCF5 hallway. During breaks, the hotel coffee pots kept tripping the circuit breakers. This sent jolts of power through the transformer, which crashed the experiment. The CF reaction started up again every time, usually in about 10 minutes. The high precision flowmeter unfortunately did not survive the beating, the batteries and power supplies in it burned up. Fortunately, the low precision flowmeter -- a 10-ml lab supply graduated glass cylinder plus stopwatch -- cannot be affected by power outages and excess voltage. The experiment was subjected to other abuses: the cart holding the experiment was wheeled up to a hotel room every night, carried on elevators, and pushed around. Cravens even lifted the cell from its container to show it to people while it was running! Yet in spite of this, the reaction started up in the morning after 10 or 20 minutes of electrolysis, although on the last day it took about a half hour, and the power was turned up higher than before. The fact that the cell survived this treatment at all demonstrates that this is one of the most robust and practical electrochemical CF systems yet developed. By the last day, the batteries in the differential electronic thermometer got weak and a minor 0.2 deg C bias appeared between them, which could be observed by switching the leads to the input and output thermistors. This was not significant, because the Delta T ranged from 2 to 4 deg C when the reaction was on, and it was less than 0.4 deg C when there was no excess heat.

I asked a number of the leading CF people what they thought of the demo and the Cravens talk. Some of them were enthusiastic. Peter Hagelstein spent a long time with Cravens going over the instrumentation and results step by step, in his ultra-careful, thoughtful fashion. But when I asked other leading CF scientists what they thought, they evaded me or expressed open hostility. The excuses and nonsense they gave me would be worthy of the most pathological "skeptic." I believe they are jealous. They cannot bring themselves to admit they have been trumped by the light water approach. The most pathological skeptic on earth himself, Morrison, said that he could not judge the experiment and he would have to have Droege look at it before reaching any conclusion. That was, at least, a lot funnier than the responses of the CF scientists who oppose light water.

Sapogin [6] described Russian ultrasound cavitation machines that are related to the Griggs device, only far more efficient. These are designed by a materials scientist, Yu S. Potapov, in Kishinev, Moldavia. The device inputs 4 kilowatts of electric power into its turbopump, and it outputs 12 kilowatts thermal. So far, Potapov has produced four models, with increasingly better performance. The earliest, least effective model gave excess heat with a C.O.P. ranging from 130 to 150%. Reportedly, Potapov has set up a corporation with four factories, and they have already sold thousands of these units. I am trying to arrange to purchase some units. Several of my contacts at well-qualified laboratories have agreed to test them.

The Potapov device may or may not tap the same source of energy as the electrochemical CF cells and the E-Quest device. I have no idea whether it does or not. One startling piece of evidence seems to indicate that it may not. Sapogin reports that the device was run for many months in a closed circuit yet it did not generate any significant level of helium, tritium or other nuclear ash. Sapogin thinks he can explain this with his unitary quantum theory which he published in Il Nuovo Cimento. [7] I am glad it is not my job to explain it. This baffling result appears to contradict results from E-Quest, the Naval Weapons Center and others who have found helium commensurate with a nuclear reaction. Perhaps there are two different, unrelated processes at work. From the standpoint of business and technology, it does not matter if there are two processes or two hundred.

Griggs [7] gave a surprisingly well received talk about his ultrasound device. He described instrumentation, results, and his efforts to have the machine verified by scientists. He said that more than 40 scientists have visited him over the last few years and not one of them has found a mistake. Strictly speaking, that is not true. I found a glaring error and so did a good scientist I know who visited Griggs a year ago, but those were errors with newly installed test equipment that were soon corrected. Nobody has found any error in the overall conclusions. During the past few years, as all of these scientists visited, Griggs listened to their suggestions and improved the experiment in many ways. His biggest improvement was to add a dynamometer -- a "Lebow" brand Eaton torque sensor model 1805-5K. This gives him a second, independent method of measuring input. He has found that the mechanical power from the motor closely matches the manufacturer's performance specifications. Recently, after he modified the rotor, Griggs began experiencing problems with cavitation damage and with massive plating out of copper from an unidentified source. He has been working with experts from Georgia Tech and NASA to resolve these difficulties. They plan to use transducers, high speed cameras and other high tech tools to learn more about what is happening inside the machine.

Stringham and George, of E-Quest [9] talked about their spectacular results in greater detail than they have been willing to share previously, but still not in enough detail to satisfy me. They are getting massive helium, isotope shifts, heat and so on. Last summer they ran experiments at Los Alamos. At ICCF4, J. Huizenga insisted that he would only accept helium analysis results from Rockwell International, which is widely viewed as the best laboratory on earth for this type of work. So, E-Quest shipped samples of gas from the Los Alamos experiment in stainless steel collection bottles to Rockwell's facility in Canoga Park, CA, where they were analyzed by B. Oliver. The Rockwell tests revealed definitive proof that the excess heat comes from a nuclear reaction. Experiments that did not generate excess heat showed 0.4 ppm helium. Experiments that did generate excess heat yielded helium far above that background level, at levels as high as 552 ppm, 100 times atmospheric concentration. Rockwell also looked at the ratio of 3He to 4He as well as 22Ne to 4He in the samples and found the isotopic ratios prove the helium could not possibly have come from contamination from normal terrestrial helium. During the talk, George mentioned that an extensive SIMS analysis performed by him at Lawrence Berkeley Labs showed dramatic isotope shifts in some of the high Z trace metals, especially titanium. I find the E-Quest work tantalizing but frustrating. I do not have details, patents, or any way of getting a gadget to test myself. That would not matter if this was yet another "me too" milliwatt level, intermittent reaction, but it is one of the most important experiments in the field, so it deserves wider replication and exposure.

Other leading experiments are also being kept too secret for my taste, especially Arata, and Pons and Fleischmann. Detailed technical information about cold fusion devices must be shared if the field is ever to be commercialized, and the best mechanism for sharing it is the patent. E-Quest and many other CF workers in the U.S. have applied for patents, but they have all been blocked, except Patterson's. The patent system is a splendid institution; it allows us to share information, preserve property rights, and to foster progress. The slow progress in cold fusion demonstrates how vital the patent system is, and how much effort is wasted when the government fails to do its job. The Japanese and the Italian governments have granted many patents for cold fusion, the continued intransigence of the U.S. government may hurt U.S. competitiveness in the future.

Piantelli did not attend the conference, but his friend Bill Collis was there, and he gave us an informal update on the work. (Collis is British; he lives in Italy and speaks fluent Italian. Piantelli said that he does not understand English well enough to make it worth his while to attend the conference, but he sent his regards). Piantelli has been granted a patent which will come out in July, they hope. He is publishing a new paper in Il Nuovo Cimento, and he was chairman of a recent important CF conference in Italy. Up until now he has kept secret many key aspects of the experiment, but now that he has been granted a patent he discusses all details. Collis described three aspects of the experiment that have been kept confidential:

1. The nickel should be prepared with special surface treatments that will be described in detail. I do not know the details of this particular preparation, but I expect it is similar to the ones described by Storms, McKubre and many others. They involve cleaning the surface, removing impurities, checking for and in some cases eliminating cracks and other deformities by sanding, etching or scaling the surface, or simply by rejecting damaged metal. This is an important aspect of all CF experiments.

2. The metal sample is placed in a magnetic field of several kilogauss. This greatly enhances the absorption of hydrogen in nickel at high temperatures. A permanent magnet will do the job, but Piantelli finds it more convenient to use an electromagnet. When the magnet is turned off, the sample degasses and after a while the reaction stops. This is excellent news; it means the reaction can be controlled.

3. To trigger the reaction, Piantelli discharges a capacitor into the heating coil, giving it a brief jolt of energy. This sort of technique is widely used with other types of CF. A shock of thermal or magnetic energy will often trigger a reaction that is sufficiently loaded and ready to begin reacting in other respects. Any form of disequilibrium might help, even a vibration, like a gentle tap on the cell. These "triggers" have also been known to interrupt a reaction.

I will describe additional details about the Piantelli experiment in a later communication after I read the Il Nuovo Cimento article.

Arata [10] described his double structured cathode palladium black experiments in considerably more detail than his two most recent papers. He reported "the chemical reaction energy of 0.1 mole Pd-black used is only 4 kJ, but more than 200 MJ of excess energy was continuously produced for over 3,000 hours at an average rate of 50-100 kJ/hr [14 to 28 watts]" Arata's English is poor and the lecture was difficult to follow, but I learned a lot from the poster. Kawasaki and I have done a rough translation of his May 31, 1994 paper into English. I should finish it up and incorporate more of the information from the poster. Arata also described a model that he believes explains the reaction, and accounts for the good performance of palladium black in pure deuterium gas.

A number of Japanese corporations showed up with mainstream CF results that I would have described as "spectacular" a few years ago, including large heat bursts, boil-offs, and the like. None of them holds a candle to people like Patterson or Patopov. Iwamura [11], from Mitsubishi Heavy Industries, reported X-rays, neutron emissions and possible transmutations, and concluded, "Although we cannot identify where these Pd atoms came from (contamination or generation), we can say that anomalous nuclear reactions must occur in the electrochemical cells at room temperature." Itoh [12], also from Mitsubishi, reported on vacuum chamber gas release experiments somewhat similar to the NTT thin film work reported at ICCF3 and elsewhere. Shikano [13] of NTT reported continuing progress with those experiments.

Isagawa [14], with the Japanese National Laboratory for High Energy Physics (KEK), got a number of spectacular results, including three boiling events and an "enormous" heat burst. "Under constant current conditions, the cell voltage and the cell temperature were increased gradually and all of a sudden sharply increased to boiling. . . . It was just during the calm period about 6 hours after the first boiling that the enormous heat release was observed. The temperature of the cell of about 100 ml in volume increase by 7.5 K (from 83.4 deg C to 90.9 deg C) in 13 minutes. The cell voltage showed a dip correspondingly. The excess heat can be estimated to be 6.8 W, about 110% with respect to the input electrical power. . . . Boiling occurred 3 times, the last episode continuing for about 16 hours, in the former period violent but in the prolonged later period rather gentle; the cell was driven almost to dryness." KEK has superb instrumentation so nobody can deny the results are real and beyond chemistry. This is not be-all, end-all performance compared to the Big Guns in the field like Patterson, but it is good for public relations. A few years ago a number of leading "skeptics" including Morrison touted the KEK laboratory. For example, in his Cold Fusion Update # 7 (Dec. 1992) Morrison wrote:

"On the other hand the most complete experiment in Japan according to the book of Abstracts, has been carried out over three years by Isagawa et al. at the National Laboratory for High Energy Physics, KEK - it was not chosen for presentation and was not mentioned - their evidence on excess heat, neutrons and tritium was against Cold Fusion although they found many artifacts which at first had appeared as real effects. . . . In Japan the two most careful experiments have both given strong evidence that Cold Fusion will not give excess heat. They are the KEK experiment which was rather complete, and the Kamiokande experiment."

Now that KEK has found definitive evidence of heat beyond chemistry, I expect Morrison will declare that they know nothing about physics and their experiment is flawed for mysterious reasons he cannot describe. He showed no sign of believing any of the data. He repeated verbatim his statements from previous conferences.

Claytor's abstract [15] reports continued progress at Los Alamos. "Over the past year we have been able to demonstrate that a plasma loading method produces an exciting and unexpected amount of tritium. In contrast to electrochemical [methods], this method yields a reproducible tritium generation rate . . . We will show tritium generation rates for deuterium-palladium foreground runs that are up to 25 times larger than hydrogen-palladium control experiments using materials from the same batch. The reproducibility of the technique and the large signal to noise ratio over background has allowed us to vary parameters that have been difficult to investigate with previous methods." Unfortunately, Claytor and his colleagues Tuggle and Jackson were not able to attend the conference. I hope they can submit a paper to the Proceedings anyway, even though that might not be strictly in accordance with physics conference traditions.

On the last day, Klein [2], of Bechtel Corporation, gave a superb talk on the economics and ABCs of developing cold fusion into a practical form of energy. This is required reading for anyone interested in that subject. Since this is my main concern, I am happy to report that his talk held no surprises for me, but he did a superb job in summarizing the key issues in business and technology. He pointed out, for example, that solar photovoltaic cells use zero cost energy, but they still cannot compete with conventional sources because the fuel cost is not the only economic factor. He said that a cold fusion power reactor might be economical at the large scale, like today's coal or fission; or it might be economical at the substation level; or it might even be economical in home generator units operating at 10 or 20 kilowatts, in which case people will gradually unplug from the power distribution network. This is bold speculation, and Klein is to be commended for talking about it, because his corporation's main line of work is in large scale energy installations like oil refineries and power stations, so he is speculating about something that may put him out of business. He did not describe a logical conclusion to this train of thought. There is no reason to suppose that miniaturization will stop at the level of a 20-kilowatt home reactor. Washing machines, coffee pots, children's toys, earphones and pacemakers may someday have built-in CF power.

Klein discussed many technical requirements for a practical source of energy, such as the need for a method of throttling the energy. It would be nice if we could turn on the flow of energy, turn it up, down, and then quench it. I would like to add a comment: while this is generally true, there are conventional sources of energy that cannot be quenched, and that can only be throttled to a certain extent. The best example is a ton of burning coal: once you light it, you have to let it burn. That is why coal is relatively inflexible and limited to large scale applications where continuous energy is needed and instantaneous, fine control over the reaction rate is not required.

A scientist might think that the issues Klein discussed are premature, but they are not. If everyone in this field would move these issues to the top of his agenda, progress would be swift, and the field would be inundated with funding. Many scientists think that Klein and I put the cart before the horse. They believe a theory must be developed before the reaction can be controlled, and that discussions of engineering problems and commercial development schemes are premature. Events have proved these scientists wrong. Patterson has already shown a proof-of-principle demonstration device. Ultrasound excess heat devices are already being sold in large numbers at a profit. Cold fusion (or some form of energy similar to it) *has already* been successfully commercialized, so this discussion is not premature, it is starting two years late. The history of modern technology includes many examples of commercial products that were developed and sold before a comprehensive theory explained them, including such things as Marconi's long distance radio, airplanes, antibiotics, high temperature superconductors and aspirin. The latter two are not fully understood even today.

Many of the papers were disappointing, because many workers are stuck in the rut of trying to replicate the 1989 simple palladium - heavy water electrolysis method. This requires high loading and other conditions which are nearly impossible to achieve. Why anyone would still be trying to use this method so many years after better methods have been invented is a mystery to me. Over the years many excellent alternatives to pure palladium have emerged: thin film [3], palladium black [10], light water [3, 16, 17], ultrasound [7, 8, 9], proton conductors [18]. Other methods, like sparking [19] and glow discharge [20, 26], have not been as widely replicated, but they show promise.

Yet the majority of scientists in the field ignore these promising approaches and continue using only palladium. Instead of selecting the easiest and most successful methods, they insist on using the oldest, least effective, and most frustrating technology, as if they were computer scientists who insisted on building a vacuum tube machine in the age of transistors. Many papers describing the heartbreaking difficulties these people face: the high loading, surface treatments, problems with cleanliness, and the many heroic techniques they are forced to employ. Kunimatsu [21] and others continue to search for ways to improve loading in palladium with electrolysis, instead of using other methods in which loading does not matter. Scientists who use pure palladium must wait 30 days or longer for a tiny, marginal, excess heat reaction to flicker on -- a reaction which often abruptly dies. Contrast this with the E-Quest device, which turns on in a fraction of a second and produces a 300% excess, or the Patterson CETI cells, which turn on in 20 minutes and produce up to 1000% excess. The Pd D2O reactions seldom produce enough nuclear products to be detected with any real certainty. Okamoto [22] reported that the NEDO Icarus program, for example, saw only two excess heat reactions during the entire year, peaking at 16% excess. Six years of low level results have failed to convince mainstream scientists that CF is real. Six more years will not convince anyone either. I was disappointed to hear about these puny results, but on the bright side, I asked Okamoto about the Icarus results, and he said their calorimetry is excellent, so they have no doubt that the 16% excess is real. (Okamoto is at the Tokyo Inst. of Technology, not in the Icarus project.)

As I expected, Pons and Fleischmann [23] did not reveal any details about their recent work. They have not revealed much since 1992, even though they have achieved some spectacular successes since then, including long boiling events. Pons was suffering from a cold during the conference and did not attend most sessions, so Fleischmann gave two lectures. In my opinion, the major important point he made is that heat promotes the CF reaction. This is very important and it has been overlooked by many people in the field even though Fleischmann, Ikegami [24] and other mainstream leaders have pointed it out many times over the years.

In 1994, Srinivasan was forced to retract many of his excess heat claims for the nickel - light water cells. He told me that he found that many were due to recombination, but not all of them, and he does not retract the tritium findings. Apparently, the nickel cells, like palladium ones, can produce tritium with little or no heat. That was disappointing, but on the other hand Srinivasan reported a number of other extraordinary experiments from various labs at BARC that range from weird to extremely promising. He acts as a representative from India, because not many Indian scientists are able to attend these international conferences. He gave two lectures to cover the work of many other groups. [4, 5] His dynamic and fascinating talks move at such a rapid pace and touch on so many amazing topics, they leave your head spinning. I will have to watch the video and summarize what he said in a later communication.

There was an interesting contrast between Kennel, Hagelstein and Smullin [25] on one hand and Karabut [26]. In 1992, Karabut et al. first reported excess heat and gamma rays from a glow discharge experiment. Hagelstein has been working hard for the past few years to replicate this experiment, but he has achieved little success. Kennel et al. "call attention to various means by which false positive signals can be observed in x-ray and gamma spectroscopy." They cast doubt upon Karabut's findings: "The authors wonder if other researchers presenting data on gamma and x-ray emission from cold fusion experiments may have fallen prey to similar phenomena. However, it is most expressly not the intention of this paper to make the claim that all positive results are due to detector artifacts and faulty estimates of statistical significance. . . ." Kennel has not been able to replicate Karabut's gamma measurements, so naturally he has doubts about them. Yet at the same time, Karabut has improved the heat measurements with a single flow calorimeter, instead of three static calorimeters for each of the three main components. This puts the excess heat on much firmer ground. So perhaps the excess heat is real but the gamma rays are an artifact? I cannot judge gamma detection schemes. I have heard a great deal of sincere debate about detecting gammas, low level neutrons and other nuclear signatures, and I have seen serious questions about various techniques raised by supporters and opponents of CF alike, including, of course, Kennel and Hagelstein. On the other hand, I have never seen a single reason to doubt that flow calorimetry *always detects* multiwatt excess heat levels. Problems can arise at milliwatt levels, but never between 1 and 100 watts. The "skeptics" have offered various reasons to doubt flow calorimetry, but their ideas have no scientific merit. Although I know nothing about detecting gammas, I conclude that it must be easier to measure heat with 19th century techniques than it is to measure low level gammas with modern equipment. No serious scientist will dispute that flow calorimetry always detects multiwatt levels of heat, whereas many serious scientists like Kennel have shown how errors might creep into gamma detection.

DuFour, at Shell Research, [19] made the same improvement as Karabut, with equally good results. He combined several separate calorimeters for different component into one unified flow calorimeter, which accounts for all inputs and output. He continues to detect up to 7 watts of excess heat. It is good to see that the oil companies are seriously pursuing this form of energy.

Another oil company finally came of the woodwork. Amoco reported some old but extremely important early results. Eisner [27], of the University of Houston, described the 1989 experiments that he and Lautzenhiser and Phelps of the Amoco Production Company performed. According to Amoco's 1989 report [28], the first experiment "yielded a 30% energy gain over the life of the experiment (two months). In June 1989, the experiment was modified and a second run also yielded "about 30% excess energy until the catalyst become waterlogged." Other successful runs were performed. Their conclusion: "The calorimetry conclusively shows excess energy was produced within the electrolytic cell over the period of the experiment. This amount, 50 kilojoules, is such that any chemical reaction would have been in near molar amounts to have produced the energy. Chemical analysis shows that no such chemical reactions occurred. The tritium results show that some form of nuclear reactions occurred during the experiment." Amoco has superb closed flow calorimeters, their signal to noise ratio is exceptionally high. They are world class experts in this type of work. They got excess heat far beyond the limits of chemistry and nuclear products in these early experiments. It is a shame they did not talk about it back in 1989, but at least they have set the record straight today. It is not clear to me whether they are still working on cold fusion or not.

As a humorous aside, let me add that when we showed the Amoco results to Morrison at ICCF4 in 1994, after the closing ceremony, he turned pale as a ghost and took off like a shot. This is one of the many nightmare results that "skeptics" wish they could forget, along with KEK, Mitsubishi, NTT, Los Alamos, E-Quest, SRI, Canon, etc., etc.

Hansen [29] described more about his detailed analysis of the 1989 Harwell data, which he previously discussed at ICCF3 and ICCF4. The Harwell experiments were performed in the summer of 1989 by inexperienced junior scientists, who mistakenly concluded that there was no excess heat. Hansen has more experience with electrochemistry and calorimetry than the Harwell researchers, and he was given full access to their data. The Harwell researchers relied upon hardware which they did not fully understand, and upon a sophisticated analysis which turned out to be unnecessarily complex. Hansen's linear regression "is much simpler and faster" than Harwell's technique. By using better algorithms with the same data, he achieved an order of magnitude better precision than they did. You might call this a triumph of software over hardware. The Harwell experiments are not intrinsically important. Much better results have been achieved since 1989, and far better techniques have been discovered, so there is no technological imperative to look back at these early results. But for political reasons it is important to show that the conclusions published by Harwell were incorrect. In 1989 Harwell, Cal Tech, and MIT were held to be the "Big Three" that proved cold fusion does not exist. All three were later shown to be positive results. The best discussion of this is the 1994 Journal of Physical Chemistry paper by M. Miles. [30] No "skeptic" has ever published a scientific paper demonstrating that any major positive cold fusion excess heat results were wrong, but the big three negative results from 1989 evaporated long ago. For years, the "skeptics" have predicted that cold fusion would fade away, but their data and their conclusions faded away instead.

Fleischmann [31] talked about Harwell in his second lecture, titled, The Experimenter's Regress. As he put it, "the judgement of whether or not a given result is 'negative' or 'positive' is frequently dependent upon the methods of data analysis used. . . . We present here a comparison of a number of 'historically interesting' data sets and show that the conclusions reached have frequently not been justified." This is an important piece of scientific history, and it should be explored someday. But I think that now is not the time to look back at old data. We should concentrate on present results instead. The old data from Harwell, MIT and Cal Tech do show convincing excess heat, but it is not as convincing as the Patterson demonstration cell, so if our goal is to bring mainstream scientists into the field I think we should emphasize the strongest data from the most recent experiments, rather than looking back at old data. Fleischmann's other point was that there is a lot to be learned by looking at old data.

There was a lot of good news at this conference. There were many fascinating breakthroughs. I was happy to see increased attendance this year by serious corporations and investors. Many Japanese corporate scientists were there, looking and learning, and not saying much. I would not expect them to say anything, but they came from companies that have already been granted patents, so I was glad to see that their quiet involvement in the field is continuing.

Yet, for all the good "vibes," I felt an undercurrent of pessimism and a sense that something is wrong with this field. McKubre voiced consternation that the field lingers on in a kind of twilight zone in the U.S. I think I know why. The political opposition is the main problem of course, but the other problem is that the focus of the research is wrong. There is too much emphasis on theory and basic science, and not enough on technology. Many of the inventors, businessmen and entrepreneurs at the conference agreed with me about this. An inventor friend of mine, who worked at Bell Labs on the first transistors, expressed the same frustrations I feel. He thinks researchers should try one approach, try another, build on experience, and aim for practical, near term R&D goals, letting the science take care of itself later. He said the CF scientists have to get out of this rut of repeating the same old experiments with old techniques year after year, long after better techniques have been discovered. "Why are these people so obsessed with loading?!? Why don't they try a method where loading doesn't matter!" He, and I, and other people oriented toward business R&D feel that some of the scientists misunderstand history. We think they are putting the cart before the horse. The scientists say that the mechanism of CF must be discovered and the theory must be completed before CF can be scaled up. History shows that technology evolves the other way around. Scientific theory follows in the footsteps of successful innovation and serendipitous discovery. Marconi first proved he could send radio signals across the ocean, then he began building an industry with the wrong technology (high power, long wave radio) and then finally years later the scientists caught up. They found out about the ionosphere and discovered that short wave radio works much better. The Wrights invented the airplane, and 15 years later a theory to explain wing lift emerged. Bell Labs developed a transistor in 1948 based on a faulty, incomplete theory. Four years later they developed a much better theory, and years after that people began making computers with transistors. Innovation comes first, theory and refinements follow. The devices from Patterson, E-Quest, Griggs, and Potapov prove that I am right about this. An effective demonstration system *can* be built now, even before a theory emerges.

I do not mean that a theory is unimportant! Theories are vital. The radio, the airplane, the transistor, good software development techniques and most other modern technology depends upon theories, but these innovations had to come first before theory could follow. Later, the two grew together in a synergistic feedback loop. That is the natural order of things.

When I talk about the need to put aside the palladium heavy water approach and try other methods instead, scientists often misunderstand me. One wrote to me in a plaintive tone: "There are good reasons to study the Pd system: it works, there is a lot of data on it, it gets high current density, many people have had an opportunity to scrub the data, etc. Please don't advocate quitting any particular avenue yet!" My friend missed the point. I am not advocating that we "quit" that avenue. I say we need to drop it temporarily, and to concentrate instead on what works spectacularly well today. We must build 20 kilowatt light water reactors so we can convince the world that cold fusion is real. That will bring in rivers of money -- oceans of money. There will be plenty of funding to go back and finish up the palladium system. The Wrights built a pusher propeller canard airplane (with the elevator in front), but that did not spell the end of tractor propeller designs with elevators astern. Once the industry begins in earnest, scientists will be able to back and explore any number of avenues, and develop any number of theories.

I expect the Potapov device will be verified. In that case, it is the most important, most practical, and most promising excess heat device yet invented. Given that fact, if it was up to me, I would schedule three days of discussion about the ultrasound, light water and other practical devices, and devote only a half day to electrochemical heavy water - palladium CF and other marginal techniques. The focus of a conference should be on methods that work, not methods which happened to be discovered first. We do not devote semiconductor conferences to discussions of point contact devices, even though Bell Labs invented them first. The purpose of this research should be to invent practical, profitable machines to improve people's lives and reduce pollution, not to explore esoteric aspects of metal hydrides. If the academic side of CF is emphasized, the field will wither away. Few young people are involved and there is still enormous opposition from academia. I believe that the only hope is to demonstrate working devices to industrial corporations, and to get more patents. Fleischmann and I talked about this briefly, and he strongly disagrees with me. He feels that the academic approach is good for the field.

Footnotes

(The ICCF5 paper numbers listed here are from the Book of Abstracts.)

1. E. Storms, "A Critical Overview of Cold Fusion," ICCF5 paper # 101

2. B. Klein, "Cold Fusion Economics," ICCF5 paper # 613

3. D. Cravens, "Flow Calorimetry and the Patterson Power Cell (TM) Design," ICCF5 paper # 208

4. T. K. Sankaranarayanan et al., "Evidence For Tritium Generation in Self-Heated Nickel Wires Subjected to Hydrogen Gas," ICCF5 paper # 307

5. M. Srinivasan, "Experiments with Plasma Focus Devices: the Past, Present and Future," ICCF5 paper # 605

6. L. G. Sapogin, "On One of Energy Generation Mechanism in Unitary Quantum Theory," unnumbered ICCF5 paper

7. L. G. Sapogin, "On Unitary Quantum Mechanics," Il Nuovo Cimento, vol. 53A No. 2, p. 251 (1979)

8. J. Griggs, "Sonoluminescence, Excess Energy and the Hydrosonic Pump," ICCF5 paper # 607

9. R. George, "Cavitation Induced Micro-Fusion as Evidenced by the Production of Heat, 3He, and 4He," ICCF5 paper # 324

10. Y. Arata, "Utilization of 'Spillover-Deuterium' in Double Structure (DS) Palladium Cathodes," ICCF5 paper # 601

11. Y. Iwamura et al., "Characteristic X-Ray and Neutron Emissions from Electrochemically Deuterated Palladium," ICCF5 paper # 312

12. T. Itoh, "Observations of Nuclear Products Under Vacuum Condition from Deuterated Palladium with High Loading Ratio," ICCF5 paper # 311

13. K. Shikano, "D2 Release Process From Deuterated Palladium in a Vacuum," ICCF5 paper # 332

14. S. Isagawa, "Heat Production and Trial to Detect Nuclear Products from Palladium-Deuterium Electrolysis Cells," ICCF5 paper # 220

15. T. Claytor, "Tritium Production From a Low Voltage Deuterium Discharge on Palladium and Other Metals," ICCF5 paper # 306

16. R. Notoya, "Nuclear Products of Cold Fusion Caused by Electrolysis in Alkali Metallic Ions Solutions," ICCF5 paper # 609

17. R. Bush, "A Demonstrator For The Light Water Excess Heat Effect," ICCF5 paper # 617

18. J. P. Biberian, "Excess Heat Measurement in AlLaO3 Doped With Deuterium," ICCF5 paper # 205. See also Mizuno, Proc. ICCF4

19. J. DuFour, "Interaction Palladium/Hydrogen Isotopes Cold Fusion By Sparking In Hydrogen Isotopes," ICCF5 paper # 604

20. I. B. Savvatimova, "Nuclear Reaction Product Registration on the Cathode after Glow Discharge," ICCF5 paper # 318

21. K. Kunimatsu, "Materials/Surface Aspects of Hydrogen/Deuterium Loading into Pd Cathodes," ICCF5 paper # 501

22. M. Okamoto, "The Present Status and the Scope of the Japan Basic Research Project of New Hydrogen Energy," ICCF5 paper # 211

23. S. Pons and M. Fleischmann, "More about Boiling," ICCF5 paper # 204

24. H. Ikegemi, "The Next Steps In Cold Fusion Research," Oyou Butsuri, Vol 62, No. 7, July 1993, p. 717

25. E. Kennel et al., "Gamma and X-Ray Measurements in Electrochemically Active Systems," ICCF5 paper # 330

26. A. B. Karabut, "Excess Heat Measurements in Glow Discharge Using Flow Calorimeter," ICCF5 paper # 319

27. M. Eisner, "The Serendipitous Design and Execution of an Early Experiment which confirmed Heat in the Fleischmann-Pons Effect," ICCF5 paper # 212

28. T. Lautzenhiser, D. Phelps, "Cold Fusion: Report on a Recent Amoco Experiment," Amoco Production Company, Report T-90-E-02, 90081ART0082, 19
March 1990

29. W. Hansen, "A Statistical Approach to Electrochemical Calorimetric Analysis," ICCF5 paper # 213

30. M. H. Miles (Naval Air Weapons Center), B. F. Bush (SRI), D. E. Stillwell (CAES), "Calorimetric Principles and Problems in Measurements of Excess Power during Pd-D2O Electrolysis," J. Phys. Chem. 1994, 98, p. 1948-1952

31. M. Fleischmann, S. Pons, "The Experimenter's Regress," ICCF5 paper # 215