Review of the Sixth International Conference on Cold Fusion ICCF6

by Jed Rothwell

Above: Shrine in front of the New Hydrogen Energy (NHE) Laboratory in Sapporo, Hokkaido, Japan– Made brief appearance in BBC's "Too Close to the Sun" Right: ICCF6 Conference Hall at gala Apex Toya Hotel at Lake Toya *Photos: EFM*

MY OVERALL IMPRESSIONS OF ICCF6

Overall, my impression of the field of cold fusion as it represented in these big international physics conference is . . . that it is going to hell in a handbasket. It is moribund, just as Morrison and others predicted it would be by now, but not for the reasons they predicted. The number of attendees has drastically declined to 175 people, the quality of the papers is down, and little progress has been made toward technologically useful devices. While some workers report large improvements, many others report no excess heat or any other sign of a CF reaction.

In my previous reviews of ICCF3, 4 and 5 I have described grave misgivings about the direction of the field. In my review of ICCF5 I wrote:

"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 . . . [T]he 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 insist on building a vacuum tube machine in the age of transistors . . . 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 can report that the situation is still like that, only worse. It turns out that even one more year of puny results was too much for the Japanese establishment. The whole program in danger of being shut down, and cold fusion in Japan — the last bastion of large scale research — is in imminent danger of collapse. Frankly, if I were in charge at MITI, I would have shut it down the main programs last year, and handed out the remaining funds to researchers who have achieved technologically significant results. If you are looking for the people to blame for the demise of cold fusion, in the U.S. you can point the finger at the pathological skeptics and the DoE. In Japan, you can blame the project managers and scientists at places like the NHE lab and IMRA Japan. The NHE did more than 50 experiments in a row with no success, and IMRA Japan did 32 experiments without success. They have ignored the literature. They have ignored improved techniques and alternatives. They make the same mistakes year after year. I do not know what causes this behavior. Perhaps they are closed-minded, timid, conservative, or locked into a bureaucratic plan written years ago by project managers who have no experience in the field. These scientists seem to think they will be funded forever even if they produce no results, in a miniature version

Conferees "Fuel-Up" at ICCF6 Opening Reception Apex Toya Hotel at Lake Toya *Photo: EFM*

of the hot fusion program. Some even say this is how science is supposed to work, as if progress doesn't matter! Politics is partly to blame. Officially, the NHE was supposed to cooperate with the universities and with successful researchers elsewhere, but people outside the program tell me that the managers have scotched all cooperation. Whatever the causes, the outcome is tragic folly.

I wrote an open letter to the NHE directorate decrying the situation (see page #). In it, I cite 16 published papers and one private communication from Pons showing errors in the NHE experimental materials or technique. I circulated this Open Letter, and I have improved it thanks to valuable comments and suggestions from many people. I have translated it into Japanese and I have sent copies to the NHE, MITI, and to various Japanese newspapers and scientific journals. I do not expect the letter to have any effect, but I feel a social responsibility to write it. I want to go on record, sound the alert.

Frankly, I feel that we, the supporters of this field, have been pussyfooting around for too long with inept researchers, and making too many excuses for them. The cold fusion effect is said to be exceptionally difficult to replicate. I do not think so: I think many of the people who are trying to replicate are doing slipshod, unprofessional jobs. They have not made a concerted effort to learn the techniques of successful scientists like Storms. They have not done serious, careful, step-by-step replications of successful cells. At this conference, the French Atomic Energy Agency described their successful replication of Pons and Fleischmann's 1992 boil-off results. They followed the directions, used the proper materials, and they got repeated, large bursts of excess heat with a performance profile remarkably similar to the original experiment. SRI also made a concerted effort to replicate this experiment, and they reported similar success. So it can be done right.

Some people do not see the 50 failed NHE experiments as a disaster. Dr. McKubre said he can learn from the data. MIT Prof. Hagelstein told me I am much too harsh and the results have scientific value. Storms thinks the technical points in my Open Letter are sound, but the tone is too emotional and strident, making the letter counterproductive. Others, including Fleischmann, Mallove, Bockris and Mizuno agreed with me.

FLEISCHMANN RETIRED, ALIENATED

Martin Fleischmann has retired from active research and returned to England. He was originally scheduled to give a one-hour keynote lecture at the conference, but this was canceled. I asked him why. He replied he "has already said everything there is to say." He feels alienated from mainstream research in the field, which he thinks is politicized. He feels that people have not listened to his advice and ideas. I told him that people do not understand him because his lectures are so technical and because he sometimes talks in "riddles," like the Delphic Oracle. I said the message does not get through, so he should consider writing a paper in collaboration with someone who communicates in simpler, more understandable prose.

George Miley (L) and Martin Fleischmann *Photo: EFM*

Stanley Pons of IMRA Europe, SA (R) and Kikujiro Namba of Technova, Inc. *Photo: EFM*

PLENTY OF GOOD NEWS TOO

I should not grouse too much. There was good news, and even some outstanding news. Here are some of the papers that impressed me. They are listed by Abstract number, principal author and title. Abstracts are numbered O (oral) 1 to 44, TS (transmutation session) 1 - 7, and P (poster) 1 - 79.

In my opinion the two best papers were from Miley and Pons.

O-019 G. Miley, *"Experimental Observations of Massive Transmutations Occurring in Multilayer Thin-Film Micropheres After Electrolysis"*

This was similar to Miley's lecture and paper given at the Second International Low Energy Nuclear Reactions Conference (ILENR2), discussed here, and published in *IE* #9. Miley showed some additional data strengthening the observations of three zones of transmutation. He explained that zones are characteristic of fission reactions. He showed data for beads with glass cores. These produced little heat and only a small number of apparent transmutations, with large sections of the spectrum flat compared to the plastic core beads. In conversation, he explained that the make up of the beads was different in each of the twenty runs. Some came from CETI while others were fabricated at the University of Illinois. Some had multiple layers of nickel and palladium, some were nickel only, and thickness was varied. He said he sees no point in doing the same experiment over and over. He wants to explore a variety of materials and thin film configurations.

Several companion papers about the CETI device were scheduled for the same hour that Miley talked, but only one was delivered. Some of the authors, Cravens, Nix, and Claytor, did not show up at the conference. This was good and bad. Good, because it gave Miley more time. The conference organizers had scheduled a one hour for the CETI session, giving Miley only 15 minutes. It was bad because the other papers apparently include vital information about the CETI cell, judging by the footnotes in the Miley paper and the titles of the missing papers: "Electrical Control of New Hydrogen Energy," "Design Considerations for Multilayer Thin-Film Patterson-Type Microspheres," "Producing Excess Enthalpy . . . with Near 100% Reliability" and so on. CETI did not explain these last minute cancellations and other sudden changes in their plans. They missed a good opportunity to get their message out and excite interest in their technology, but they made up for it a month later, at the American Nuclear Society meeting in Washington.

One other paper about the CETI cell was given, by **McKubre:**

O-020 *"Electrochemistry and Calorimetry*

M. Srinivasan (L) of Bhabha Atomic Research Center, India and Daniel Gozzi of University of Rome. Gozzi's group confirmed heat-related helium-4 production and x-radiation from cold fusion cells.
Photo: EFM

in a Packed-Bed Flow-Through Electro-chemical Cell."

This was a sophisticated analysis of the electrochemistry of a Patterson-style cell. McKubre showed that the most active and highly loaded beads are probably those at the top of the bead pack near the anode. He concluded that the best way to scale up a Patterson cell would be to increase the diameter, making a broad, flat bead pack with just a few layers of beads. Patterson himself has reached the opposite conclusion. Before McKubre's talk, I asked Patterson how he plans to scale up his cells. Patterson said he will make them long and thin, adding many layers of beads. McKubre and Patterson should get together and hash this out.

O-014 S. Pons, *"The ICARUS 9 Calorimeter: Summary of Three Years Designing, Testing and Operation of this Device at the IMRA Europe Science Center"*

The paper describes a cell that was held at boiling for long periods, producing 200% excess at hundreds of watts. Cathodes are either 2.5 cm long (0.075 cc, 0.9 g) or 10 cm (0.3 cc, 3.6 g). In a 3-month run this cell produced 294 megajoules from one cathode. The calorimeter is rather complex. It is static design, requiring three calibration curves from three thermocouples to measure low, medium, and boiling level power levels. Pons showed extensive data from a huge number of different types calibration runs. Several people said they thought this calorimeter is a little too complicated, and inappropriate for this power and temperature domain. It was suggested that a Seebeck thermoelectric envelope design would be better.

O-001 P. L. Cignini, D. Gozzi, et al., "*X-Ray, Heat Excess and ^{4}He in the Electrochemical Confinement of Deuter-ium in Palladium"*

Excess heat, helium and x-rays were observed. Helium was measured by drawing off samples of gas, filtering out everything but helium, and putting the gas into a QMS (quadrupole mass spectroscopy) chamber integrated into the experimental apparatus. In a four-cell experiment lasting 950 hours, more than 1000 samples were taken from each cell. At first glance, the helium did not appear to be correlated with heat, but a sophisticated statistical analysis, taking into account the sampling periods and percent of total gas in one sample, shows that did actually appear together. The curves for helium change completely after the complex statistical massaging. X-rays were measured with x-ray film positioned 5 cm from the cell. X-ray film placed next to a Pt blank cell showed no fogging. Film next to Pd cells that produced heat were fogged, with a shadow where the anode blocked the cathode. A sophisticated microscopic analysis of the film was performed, based on the physics of film and comparisons to film of the same stock exposed to known levels of x-rays. It was concluded that the "energy of radiation" (the power of the x-rays) was 89 ± 1 keV and the total energy intercepted by the film was 12.0 ± 0.4 kJ.

This experiment contributes nothing to reproducibility, which is the main problem of cold fusion. Excess power levels were low in absolute and percent terms, so it has no immediate technological significance. The sigma level for the heat was high. The x-ray energy levels and helium may contribute to a theoretical understanding, and they prove that the heat cannot be of chemical origin. The instrumentation is superb, so it is hard to see how anyone could quarrel with the results. However, D. Morrison did quarrel, insisting that an x-ray detector would have been better than x-ray film. He said that Steve Jones offered to lend one to any CF scientist and he cannot understand why nobody has taken Jones up on the offer. Gozzi replied that he did not think a detector would work in this configuration, and that the physics of x-ray film are well understood.

Gozzi's cathode is a bundle of palladium wires, rather than a single block of palladium. This increases surface area.

O-005 E. Botta, *"Further Measurements on ^{4}He Production from PdD_2 Systems in Gas Phase"*

Another solid job from the Italian university and INFN (Italian government Nuclear Physics Laboratories). They showed more evidence for helium production in one run. They use an interesting technique to achieve high loading. Deuterium gas is loaded into palladium plate under pressure, and "a constant electric field of a few hundred millivolts per cm" is applied from one edge of the plate to the other, which moves the deuterons across it by electromigration. The ends of the plate are sealed with gold, and I think this causes the deuterium to "pile up" against the ends. This electromigration technique was used by several other Italian workers to enhance the CF effect, and it is the basis of the proton conductors used by Mizuno, Biberian (ICCF-5), Oriani, and Karabut (P-001). With a proton conductor, you charge both sides of the plate (rather than the ends). If you leave a direct current charge running continuously, one side of the plate would become highly loaded with deuterons but the other side would not, so you use a square wave current that alternates every minute or so, charging first one side, then the other. Mizuno and others speculate that this also causes the

Francesco Celani of INFN (National Institute of Physics), Frascati, Italy—Electromigration work with thin wires boosts power dramatically.
Photo: EFM

deuterons to "slosh around" inside the conductor, occasionally piling up in local spots at a loading ratio much higher than the average for the entire conductor, which causes a powerful CF reaction in that isolated spot.

O-015 F. Celani, *"High Power*

G. Lonchampt (French Atomic Energy Commission) (L) and Jean-Paul Biberian—Reported exact replication of the Pons & Fleischmann boiling cell work. *Photo: EFM*

Microsecond Pulsed Electrolysis Using Long and Thin Pd Wires in Very Diluted LiOD-D2O Solution: Observations of Anomalous Excess Heat"

This was another interesting electromigration experiment, which was first described at ICCF5. Considerable progress has been made since then, with better results and improved flow calorimetry.

O-016 S. Storms, *"Some Thoughts on the Nature of Nuclear-Active Regions in Palladium"*

This covered much of the same ground as the paper published in *IE* # 8, p. 50. An interesting aspect of this paper is the Open Circuit Voltage (OCV) method of measuring loading, which Storms recommends. Many electrochemists recommend this, including Bockris in his recent letter to the NHE. It involves the use of a platinum wire (a "reference electrode") set inside a small glass straw (a "luggin capillary"). The end of the capillary fits between the anode wires. It can be held close to the cathode, making a conduction path between the reference electrode and one spot on the cathode. To measure loading, electrolysis power to the cell is temporarily turned off, and the voltage difference between the cathode and the reference electrode is measured. Because the cathode and the reference electrode are different metals, they have different electrochemical potential. When he begins the experiment, Storms measures 0.00 to 0.02 volts. As the palladium fills with hydrogen, the electrochemical potential changes, the voltage goes up. Strictly speaking, the OCV measures chemical activity on the surface, not loading per se. After a highly loaded cathode reaches saturation, the OCV sometimes continues to rise to much higher levels than normal, reaching 1.3 volts, even though the cathode is probably not absorbing any more deuterium. This is a good indication that the cathode has entered the special state of matter (or "the special state of grace") in which the CF effect can occur. Another indication of this special state is seen when a cathode degasses over an hour or so. The OCV falls, then rises again briefly after about 30 minutes, then resumes its decline. Outgassing is steady, it does not reverse and rise briefly, so the OCV must be decoupled from the loading ratio. When it reaches these abnormally high levels, the OCV is no longer a reliable measure of loading, but instead it indicates some other poorly understood state of the cathode. This poorly understood state just happens to be exactly what you are want: metal-ready-for-CF.

O-055 G. Lonchampt, *"Reproduction of Fleischmann and Pons Experiment"*

This paper was presented by Biberian because Lonchampt does not speak English well. It describes a marvelous series of experiments performed by French Atomic Energy Commission (CEREM), in association with the ENSEEG (Ecole Nationale Superieure d'Electrochimie et d'Electrometallurgie de Grenoble). Biberian also worked on the project, although he is not listed as an author. Lonchampt is a CEREM commissioner, and an engineer not a scientist, (thank goodness). These experiments are exact replications of the 1993 boil-off experiments reported by Pons and Fleischmann in *Physics Letters A* 176. This is exactly what cold fusion cries out for: careful, step by step replications done by people who follow directions. Biberian said that he and the other scientists in the project wanted to incorporate various "creative improvements" but Lonchampt insisted on doing a precise replication with assistance from Pons and Fleischmann. That is why it worked, as Biberian cheerfully admits. It takes an engineer to do these things right. Everything about this work is superb, even the Abstract. Let me quote it extensively:

"Experiments have been performed in calorimeters identical to the ones used by Fleischmann and Pons . . . [T]hese experiments can be analyzed in three temperature domains:

- At low temperatures, below 70 deg C, excess enthalpy is the difference between the heat radiated to the water bath, and the enthalpy input due to electrolysis.
- At intermediate temperatures, between 70 deg C and 99 deg C, excess enthalpy is the difference between the heat radiated towards the water bath plus the enthalpy contents of the gas stream, plus the variation of enthalpy of the contents of the calorimeter . . .
- In the boiling regime (without condensation), excess enthalpy is calculated from the difference between the total amount of water contained in the calorimeter evaporated and the theoretical quantity of water that should be evaporated by the energy introduced in the calorimeter . . .

Six calibration runs with platinum cathodes and 17 runs with different palladium type cathodes have been performed.

At low temperature, 8 experiments have produced an excess energy rate between 1 and 5%. In the intermediate regime the water vapor carried away by the gases of the electrolysis are large, and cannot be evaluated precisely . . .

At boiling, three positive experiments have been successful, giving excess enthalpies rates of 80% to 150% . . .

In conclusion, we confirm the results published by Fleischmann and Pons more particularly in the boiling regime."

The Abstract says "at boiling, three positive experiments" but a table shown during the lecture listed five. Here are the last two columns, showing excess heat a percent of input before boiling and during boiling:

Before	During	
16 %	153%	
-0.2 %	0 %	With a platinum null electrode
3 %	18 %	
7 %	36 %	
20 %	97 %	
7 %	29 %	

Other runs with palladium generated no excess heat. The conclusion presented during the lecture was: "The Fleischmann-Pons calorimeter is very accurate and well adapted to this work, however several precautions must be taken. The Dewar must be of excellent quality. Calibration of the thermistors must be very precise. Care must be taken to minimize the thermal losses by heat conduction. All electrical feed-throughs must be sealed off . . ." Finally, below 70 deg C there is sometimes a small level of artifactual excess heat "due to heat conduction not included in our equations." Pons and Fleischmann do take these factors into account in their equations. In any case, the excess below 70 deg C is marginal, just as Fleischmann has been saying all these years.

This project is continuing. They plan to build larger cells to avoid spilling, and cells

Martin Fleischmann (L) discusses Triode-Configuration Cold Fusion Cells with developer Evan Ragland
Photo: EFM

with condensers like IMRA Europe's ICARUS 9, for experiments with continuous boiling.

P-004 S. Crouch-Baker, *"Mass Flow Calorimetric Studies Under Non-Steady State Conditions"*

SRI also did a quality replication of the Pons-Fleischmann boil-off experiment. They used high precision equipment and extensive modeling to examine the power levels just before boiling ensues, and during boiling. The goal was to answer an interesting chicken-and-egg question. Does the heat cause the CF reaction, or does an increasing CF reaction cause the heat? It could be a combination of the two, what Fleischmann calls "'positive feedback' between the temperature and the rate of excess enthalpy generation." SRI concluded that the cell begins to heat up because of normal electrochemical processes. The cathode surface changes, resistance rises, more power is consumed, and the temperature rises. You can accomplish the same thing with a platinum cathode. You can even drive a platinum cathode to boiling with this mechanism, but you get no excess heat. With fully loaded palladium however, the temperature rise triggers a burst of excess heat. I conclude that you should be able to trigger a CF reaction in fully loaded palladium by raising the temperature by some other means, like raising the temperature of the incoming electrolyte in a flow calorimeter, or perhaps even by heating the cathode with a laser. It would be interesting to find out if this works.

P-016 E. Ragland, *"Triode Cell Experiments for Controlled Fleischmann /Pons Effect"*

Ragland has been working on the triode configuration for cold fusion since 1989. We published a brief note about this work in *Infinite Energy* #3, p. 42. A triode is a cathode with two anodes: in the configuration he is now using, it is a 1 cm square palladium plate cathode, and two separately powered platinum wire anodes, one on each side of the plate. High power is shifted back in forth between the anodes for periods lasting from 1 to 16 seconds, usually 8 seconds. The low-power side anode is not turned off completely, because it would start degassing. This shifting back and forth apparently causes high deuteron mobility. This is the sloshing deuteron model described by Mizuno and Biberian (see the discussion of Botta, above). The method might also be compared to the high-low A. Takahashi loading.

Ragland had a cell running all summer. He says input was between 1 and 2 watts, the flow rate was 25 ml/min, and the Delta -T was generally 4 to 5 deg C, indicating total output of 8 or 9 watts. Over the past year he has tested four palladium plates from Johnson Matthey. All four successfully generated heat at about the same level. Ragland says that after the cell begins working, he can progressively cut the purity of the heavy water until it is down to about 20%, and the heat continues. That is the first time I have ever heard that claim! It's extraordinary if true. It is one of several aspects of the report that make me nervous. I must see independent testing of this device before I can accept it.

Fleischmann was impressed by this work. I listened to his conversations with Ragland. He said, "this [triode configuration] is something I have wanted to try for years." He thinks a cylindrical cathode might work well. A wire going down the center of the cylinder would constitute one anode, and a wire wrapped in a spiral around the outside would constitute the second one. Biberian was also impressed with this design. He said he hopes to try it soon. **(See Accompanying Article, p.22)**

O-031 T. Claytor, *"Tritium Production From Palladium and Palladium Alloys"*

This was presented by Ed Storms, because at the last minute the DoE told Claytor he was not authorized to go to ICCF-6. It was similar to the paper Claytor gave at ILENR2, so I will not discuss it here. Claytor's presentations are lucid, but I found Storms even easier to follow. (Biberian, who presented Lonchampt's paper, remarked that perhaps we should arrange to have all papers given by someone other than the author!) One point brought out by Storms was the multiplicity of techniques used to make sure the tritium really is tritium. Storms said that this project has been peer reviewed at Los Alamos for several years running, and it has passed this review, which is much tougher than any journal peer-review, he said. Nevertheless, he said that "because of pathological skepticism," all funding for the project will probably end this year. Since the U.S. government is planning to spend $3 billion on a new reactor to generate tritium for nuclear weapons, the decision to terminate this program is a threat to national security and a fantastic waste of money.

Our *Infinite Energy* home page has a link to an earlier version of the Claytor paper. Considerable progress has been made since that version was written, by using

Melvin Miles of the Naval Air Warfare Center, Weapons Division—Excess heat highly correlated with helium-4 measurements *Photo: EFM*

alloys and by introducing a small amount of carbon dioxide into the chamber.

O-004 M. Miles, *"Heat and Helium Measurements Using Palladium and Palladium Alloys in Heavy Water"*

Miles always gives good presentations. His instruments and techniques are second to none. Unfortunately, this project has been terminated for lack of money, so many of the results reported here were old. However, some of his best results have not been reported previously because they came at the end of the project, using palladium alloys. Also, the calorimeter, which was already superb, was improved in the last year of the study, increasing the sigma level of the results. Quotes from Abstract:

"Our best experiments produced up to 30% excess heat, 0.52 watts of excess power, and 1,400 KJ of excess enthalpy..."

You might doubt these results coming from some labs, but Miles can measure 0.5 watts with as much confidence as most people measure 5 watts.

There is a remarkable correlation of excess power with the source of the palladium. The best reproducibility was obtained using Pd-B materials supplied by the Naval Research Lab. Seven out of eight experiments that used Pd-B cathodes produced excess power. A high success ratio was also obtained using Johnson-Matthey materials. Seventeen out of twenty-seven experiments that used this palladium source produced excess heat. In contrast there were several palladium sources that never produced excess power in any experiment.

That is an important observation! Materials are key to cold fusion, as everyone aught to know. Perhaps a clever technique like Ragland's triode cell can transform any Pd or Ni sample into a good one. (Ragland thinks so.) But if that is not the case, then the only way to improve reproducibility is to focus on materials: the cathode, anode, electrolyte, lead wires, glass, and everything else that goes into the cell.

The main purpose of Miles' experiments was to correlate helium in the gas stream with excess heat production. Quote: "Thirty experiments have shown a correlation between either excess power and heat production or no excess power and no excess helium . . . The only valid experiments that showed significant excess power by no excess helium involved a Pd-Ce cathode. The odds are less than one in a million that our complete set of thirty-three heat and helium results could be obtained from random experimental errors." I asked him where he thinks the helium from that Pd-Ce experiment is. He said it is probably still inside the cathode, or it leaked out gradually after the experiment.

Richard Oriani of the University of Minnesota—Confirmed excess heat in ceramic proton condutors
Photo: EFM

O-036 R. Oriani, *"A Confirmation of Anomalous Thermal Power Generation from a Proton-Conducting Oxide"*

Oriani is working with proton conductors that Mizuno fabricated by hand. Neither he nor Mizuno has seen the dramatic heat bursts that Mizuno observed years ago, but they do get significant excess heat and "heat after death." Oriani's best heat after death result was 0.8 watts lasting 17 hours. Oriani originally duplicated Mizuno's static gas calorimeter. He found that contamination from air and other gasses caused unexpected results that might be mistaken for excess heat. I do not think this could explain the tremendous heat bursts and melted solder connections Mizuno observed, but it does call into question the low grade heat produced by most of the samples. To get around this problem, Oriani built an excellent Seebeck calorimeter designed to operate at 400 deg C. (Proton conductors only work at high temperatures.) The design requires an internal heater that takes a lot of power, which unfortunately creates a lot of thermal noise, so the sigma is low even when the conductors produce a half-watt or so. However, two of the specimens "produced positive deviations from the calibration curve by more than four standard deviations . . ." Oriani concludes "verification of the claims has been achieved." This paper has been accepted for publication in *Fusion Technology.*

Oriani is sending some of his successful used cathodes back to Mizuno, who will look for evidence of transmutation.

Oriani plans to try a molten-salt CF device, like the one that produced a few bursts of heat at the University of Hawaii years ago. I wish he would stick with proton conductors instead.

O-035 T. Passell, *"Search for Nuclear Reaction Products in Heat Producing Pd"*

This is a work-in-progress, with no completely convincing results yet. Passell is looking for host metal transmutations, using prompt gamma activation analysis (PGAA). He thinks he has found an ~18% reduction in the ratio of boron-10 to palladium-105 in a cathode that generated excess heat at SRI. This just happens to fit a theory of his. It fits too well, enough to make you uneasy. Unfortunately, he has only looked for the boron and palladium ratio. He has not produced an entire spectrum of all elements and isotopes, the way Miley, Mizuno and others have done. I do not understand why not. I am dissatisfied with this work for another reason. Passell began by saying that he has a drawer full of cathodes from a variety of used cathodes and virgin material samples from many labs, not just SRI, but also IMRA Europe and the Navy. These cathodes were used in successful experiments, some of them highly successful. Yet he has only looked at one pair, and only for one transmutation. I expect he is sitting on a gold mine of information! Some of these cathodes produced thousands of megajoules per mole of metal. If Passell would hurry up and look under the surface layers, I expect he will find what Bockris and Minevsky found: areas of massive transmutation, where 70% of the palladium is transmuted into other elements, with unnatural isotopic distributions. This should be a top priority. I do not understand why this kind of research takes so long. When a dynamic worker like Mizuno or Miley decides to do something like this, he gets it done in a matter of weeks, where others seem to take months or years.

There were many other good papers. I wish I could describe them all, but I would end up reproducing a large chunk of the proceedings. J. Dash, Srinivasan, S. Crouch-Baker, Y. Arata, T. Mizuno, T. Ohmori and others presented interesting work. As always, I urge all readers to get the proceedings when they are published. Read original sources. (And *Infinite Energy*, too!) As always, I have ignored theory papers and highly technical papers,

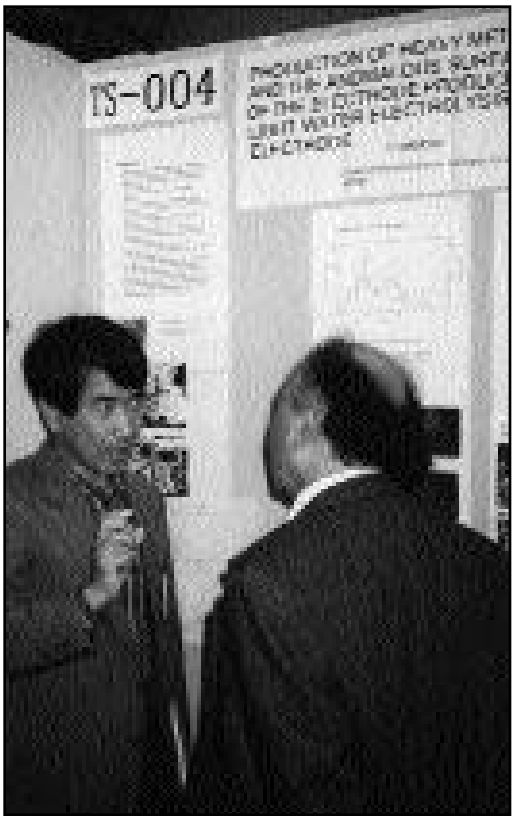
because they are over my head.

CLOSING SUMMARIES

At the end of the conference, summaries were presented by Bressani, McKubre and Ikegami. I found them excruciating. They were symptomatic of everything that is wrong with the field. Never have I felt so alienated from the cold fusion mainstream as I did listening to these talks. Don't get me wrong: these were carefully researched, quality presentations. The experimental evidence mustered by Bressani and McKubre cannot be disputed. But it is only the tip of the iceberg.

Bressani cited the increasing number and quality of experiments detecting helium correlated with heat, now at KEK, U. Osaka, Torino, China Lake and elsewhere. He concluded that the evidence of helium and x-rays proves beyond any doubt that cold fusion is a form of d-d fusion, in which all paths but the formation of helium are suppressed, and who knows what happens to the energy. I find this hypothesis just about as preposterous as Morrison must find it— for opposite reasons of course! Morrison is upset because this violates his holy textbook theories. I am upset because Bressani has dismissed a gigantic chunk of experimental evidence, from nickel and light water to the transmutations reported by Miley, Mizuno, Ohmori and Minevski, with much more to come soon, I expect. Bressani and most other scientists at the conference ignored Miley and other evidence for transmutations and anything else but d-d helium transmutations. At Japanese conferences people not only ignore Ohmori, they laugh at him. You would think that someone of Miley's status—after all, he is the editor of the American Nuclear Society's *Fusion Technology*— would command respect, especially from a group of scientists who have suffered from bigoted rejection by closed-minded mainstream scientists, but . . . alas, no.

Bressani concluded by saying how pleased he was that the conference attracted no attention from the press and "no newspapers." From my point of view the lack of publicity and the conference organizers' total disregard for public relations are two more nails in the coffin. Good press coverage leads to funding. No press coverage leads to oblivion. This is true of any branch of science, industry, fine arts, law or any other human endeavor. Imagine what the situation would be like if major newspapers were to report the fact that the French Atomic Energy Commission has fully replicated Pons and Fleischmann's high heat results! These results prove beyond any doubt that cold fusion has the potential to be technologically useful, if only it can be scaled up. If this news was broadcast by the major media, cold fusion funding would increase to billions of dollars per year and more progress would be made in the next six months than we have seen in the last seven years. Yet the NHE and the scientists at this conference hide this light under a bushel, and try to prevent the media from learning about these results!



Tadayoshi Ohmori of the Catalysis Research Center, Hokkaido University explains transmutation results to Jean-Paul Biberian
Photo: EFM

Tadahiko Mizuno, Faculty of Engineering, Hokkaido University—Transmutation results resemble those obtained by George Miley
Photo: EFM

McKubre's presentation upset me because it was so good (see accompanying transcribed text). It was a brilliant analysis of problems he has been ignoring for seven years – problems pointed out to him many times by Fleischmann, Cravens, Dash and others, including me. This summary reminded me of McKubre's ICCF-4 presentation I cite in the *Open Letter*, in which he described a heat burst caused when the flow was accidentally blocked and the cathode temperature rose higher than planned. At that time, McKubre said "we should have listened more carefully to Martin at ICCF-3 when he talked about the importance of temperature." That is what McKubre said, back in 1994. Then he and the rest of the mainstream went on ignoring Martin for two more years.

McKubre began his summary by saying he is "pleased with the results of this conference" – a statement I find appalling. He said "I think the fire that was fueled by the fever of enthusiasm from the beginning, that has sustained many of the people in this room, has sort of resolved itself into a seriousness of purpose" He defined this purpose to "explain the phenomena, first to ourselves, then to the world." Others say *explanations* are not needed at this stage, just make the thing reproducible the way Miley and Storms are doing, and then *demonstrate* it to the world. He cited the helium and heat correlations, which surely are impressive and important. He said: "the transmutation products I think are the wild card in more ways than one. I just don't know how to assess the transmutation business. It is already spinning off mini-conferences of its own. Should that work hold up, it will certainly evolve to dominate future conferences." He cited more than 20 groups reporting excess heat. He described what he called "the mass flow calorimetry 'problem'," and he said what should have been said four years ago. The failed experiments at IMRA, NHE and SRI are all in SRI-style flow calorimeters. He cited his own model, in which excess heat is caused by loading, high current density and flux, and he said that an SRI-style flow calorimeter prevents flux. McKubre agreed with Celani's dictum: "You need to depart from the steady state." He added:

"With the mass flow calorimeter – the most sluggish and stable of animals – the hardest thing to do is to depart from the

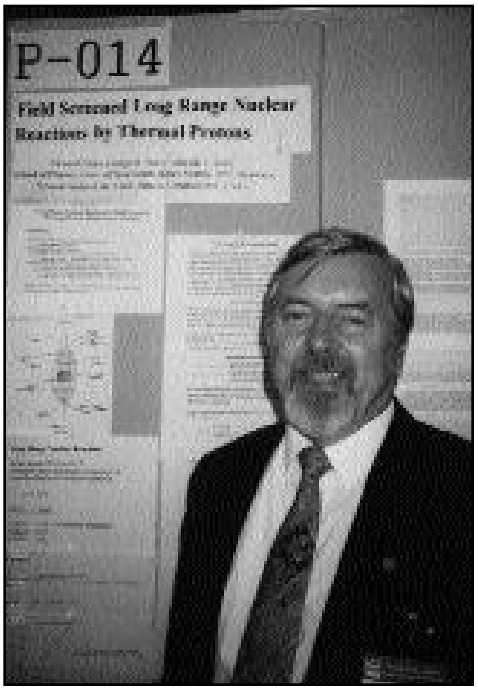
steady state . . . What we have done in part, partly in response to our critics [skeptics, not Fleischmann], is to make calorimeters in such a way as to improve the data quality. You improve the data quality by averaging things for a long time, never changing anything. . . . so the lines on a viewgraph are very clear with few irregularities you have to explain. In doing that, of course, you maintain the system as closely as possible in a steady state. We have engineered our systems to do that. That is what flow calorimeters do well. I question, now, whether that is wise."

Great! He questions that now, three years after Pons and Fleischmann made it abundantly clear that steady state, low temperature conditions are the kiss of death. He questions it now: how many years will it take him, the NHE and the others to act on these newly expressed doubts? How long will Tom Passell leave those used cathodes in his drawer? He is hiding a great mother load of knowledge – an astounding scientific breakthrough – in a drawer, for crying out loud! Is this an incurable bad habit common to scientists? How many years did Newton hide the *Principia Mathematica* in a dresser drawer? I thought that to do basic scientific research you must constantly re-examine your ideas, fiddle with the machinery, build new instruments, and try new approaches. It should not take three years for people to listen to advice from Pons and Fleischmann. They are the leading practitioners of this field. They have reported the biggest, best, most dramatic and important results. SRI replicated the boil-off results. Why didn't they move to build a calorimeter like ICARUS-9, with a condenser, designed to operate at continuous boiling? As Franklin D. Roosevelt put it: "It is common sense to take a method and try it. If it fails, admit it frankly and try another. But above all, *try something*." Also citing common sense (that rare commodity), Bockris wrote to the NHE:

"One last thing, a matter of common sense, but it doesn't seem to be realized, and that is to stop going on with experiments that don't succeed. I learned from reports I got from Tom Passell, that IMRA did 28 experiments with zero results. That is absurd. After six or seven experiments, of course the method should be changed."

McKubre cited some of the exciting new techniques reported at the conference, and said he wants to try some of them, particularly electromigration, which spurs a departure from the steady state. "The take-home message from this conference, that I will act on immediately, is to reinvestigate, or re-re-re-investigate electromigration effects. We have started on this several times, found that it is very difficult to do, and given up." He said that Preparata's experiments with high voltage electromigration require courage. I asked him if he meant they take moral or physical courage. He said he thinks there is a distinct possibility that high voltage in such an electrochemically active cell will trigger an explosion, even without a large head space.

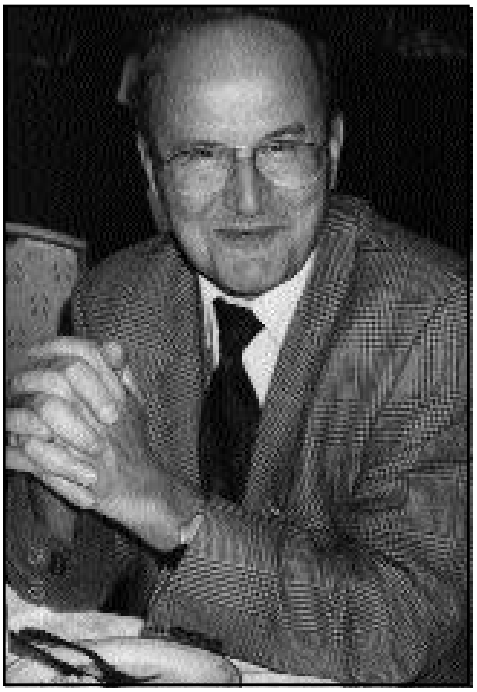
Ikegami concluded the conference by saying how pleased he is that these gatherings have "grown up to be normal scientific conferences." I say that if the field has reached maturity, it is suffering from progeria (the Hutchinson-Gilford syndrome), a horrifying, invariably fatal disease in which children starting at age three develop grey hair, high blood cholesterol, arteriosclerosis, senility and other physical changes typical of old age, resulting in death before the age of twenty.



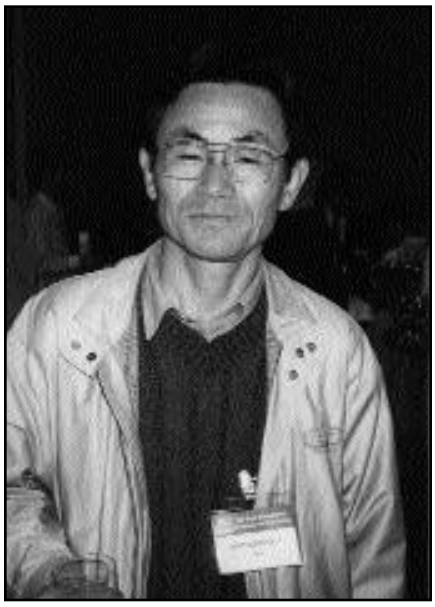
Heinrich Hora, Dept. of Theoretical Physics, University of New South Wales, Australia— Developed with George Miley the "swimming electron layer" model to explain solid-state nuclear reactions
Photo: EFM

Yeong E. Kim of Dept. of Physics, Purdue University— a theory to explain solid state nuclear reactions. *Photo: EFM*

Scott Chubb of the U.S. Naval Research Laboratory, who with uncle Talbot Chubb (NRL, ret.) has pioneered the Ion Band State theory of cold fusion reactions.
Photo: EFM



Jacques Dufour, France— Excess energy production in hydrogen gas— thousands of times beyond chemical explanation.
Photo: EFM



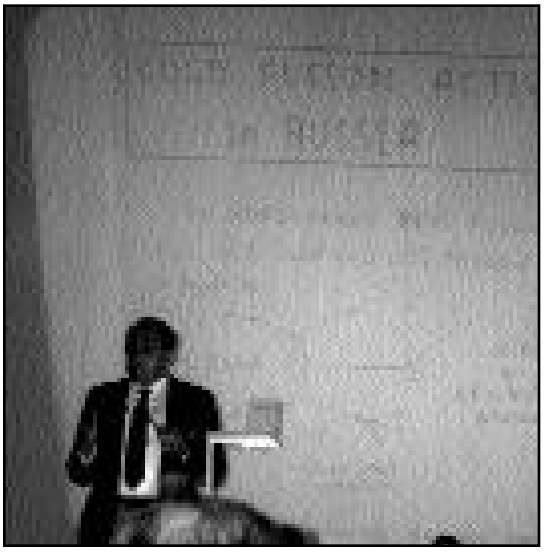
Takaaki Matsumoto, Hokkaido University—Has found numerous cold fusion anomalies. *Photo: EFM*

V.I. Vysotskii, Kiev Shevchenko University, Ukraine—Experiments suggest biological transmutations are real. *Photo: EFM*

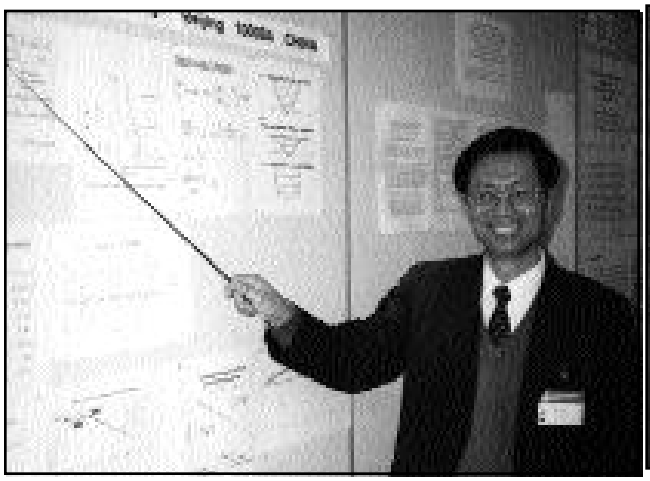
"Natasha," the Russian-to-English translator— helps Alexander Samgin of the Institute of High-Temperature Electrochemistry, Ekaterinburg *Photo: EFM*

Yoshiaki Arata, Osaka University—Massive excess energy generation in "double cathode" configuration, plus helium-4. *Photo: EFM*

Makoto Okamoto, Tokyo Institute of Technology—Correlated neutron emissions with excess heat. *Photo: EFM*



Vladimir A. Tsarev. Lebedev Physical Institute. Moscow—Reviewed cold fusion activities in the "fissioned" former USSR. *Photo: EFM*



Xing-Zhong Li, Dept. of Physics, Tsinghua University, Beijing—Excess heat found in a simple gas-loaded system. *Photo: EFM*

Lev Sapogin (L), Andrey Lipson, and Aleksei Roussetski, all of Moscow—widespread interest in cold fusion in Russia. *Photo: EFM*