Summary Report on ICCF8: The Eighth International Conference on Cold Fusion

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The Eighth International Conference on Cold Fusion (ICCF8) convened May 21-26, 2000, in Lerici, Italy at the antique and beautiful Villa Marigola Conference Center. The prior conference, ICCF7, was held in Vancouver, B.C., Canada, in April 1998. Following the traditional North America, Europe, and Asia rotation, ICCF9 will tentatively be held in Beijing, China, May 2001, with some anticipated official scientific sponsorship, according to ICCF9 organizer Prof. X.Z. Li

ICCF8 was unusual in having significant sponsorship support from multiple official Italian scientific organizations. The sponsors were: ENEA (Italian Agency for New Technologies, Energy, and the Environment); CNR (National Research Council); INFN (National Institute for Nuclear Physics); and SIF (the Italian Physical Society). The ICCF8 conference proceedings will be published by the Italian Physical Society in a peer-reviewed version in January 2001.

Overall Impressions

There were no great surprises at this conference. A few researchers reported steady, reliable progress. A few were still floundering around in the dark, making mistakes they should have learned to avoid years ago.

This conference may have exposed two long-established ideas as myths: 1) High loading is essential to all forms of cold fusion (see Storms and Warner presentations below); 2) Cold fusion experiments are inherently difficult to perform (see Warner). High loading may not be needed after all, at least not with Ni, Ti, and Pt. It may be necessary with Pd, but that is probably the "worst choice" of metals, as Storms put it.

The best news from ICCF8 is that spectacular progress has been made in correlating helium-4 and helium-3 with excess heat, especially at SRI International (McKubre, 29) and Osaka University (Isobe, 14). [Editor's Note: The number after the investigator's name indicates the number of the abstract on the official ICCF8 list.]

The disappointing news was turnout. As expected, only about 145 people showed up, the lowest number yet for an ICCF, although this was the first time a conference had been held in late May, when the end of academic year events may have interfered. Jed Rothwell's comment: "The number of participants is asymptotically dropping to zero because almost no young people participate. Most of the researchers are retired professors, 65 to 75 years old. An actuarial table will show that unless young people join the researchers, the final conference will take place in about ten years, after which most major participants

will be incapacitated or dead! Most top researchers have already retired or died. You would think that scientists understand statistics and mortality rates, but they act as if they have all the time in the world."

Co-compiler of this report, Eugene Mallove, is more sanguine on this matter of attendance: Certainly, most ICCFs have drawn on the order of 200 participants, with a high of about 350 at ICCF3 (Nagoya, Japan, October 1992). There were many who had indicated an intent to come to ICCF8, but for whatever reason did not make it-among these were those who are known to be very active in the field, such as Dr. Claytor of Los Alamos National Laboratory. The ICCF7 Proceedings were dedicated to the memory of M. Okamoto, and ICCF8 will be dedicated to the memory of G. Preparata, who died in April 2000 (see obituary, p. 8, IE No. 32).

A list of ICCF8 authors is posted at: http://www.frascati.enea.it/ICCF8/programme.htm. Most of the listed authors showed up. The conference organizers said they would make continued use of the web page to publish things, such as the list of participants. Several people at the conference called upon authors to make better use of the Internet. Much lip service has been paid to the advantages of the Internet in previous ICCF conferences, but little has been done so far. Ed Wall of our New Energy Research Laboratory (NERL) suggests that abstracts and author addresses should have been made available to participants before the conference, which would allow participants to study the abstracts in advance, prepare questions, and correspond with the speaker, "making it a much richer experience for all." On the other hand, many presenters and attendees may prefer the element of surprise or anticipation and would not wish for advance notice.

The conference organizers videotaped the presentations (as did we, informally for our research purposes, as did also our colleague Akira Kawasaki). The ICCF8 organizers will be selling their video tapes at a "very reasonable" cost, they said. They displayed the video input on two large projection-screen televisions at the front of the conference room, which helped those who could not see the viewgraphs. Their cameras and microphones were well-placed and professionally handled, so the video tapes should be of good quality.

We present a selection of conference highlights:

F. Scaramuzzi, M. Fleischmann: Lectures in memory of Giuliano Preparata, centering on his contributions to theory of coherent nuclear reactions. Scaramuzzi briefly described the Italian government initiative in cold fusion, and Fleischmann said the program may include work in other controversial areas such as the effect of magnetism on health, which, as he put it, "will make us even more enemies." From Jed Rothwell's perspective, this may have been a counterproductive thing to mention, given the already perilous state of cold fusion research. Mallove's view: If theory designed to address anomalies of coherent

atomic-nuclear energy release in water-based systems has other implications, such as in medicine, cold fusion scientists should not feel constrained to address these simply out of fear of giving cold fusion opponents more targets. (It is noteworthy that as of mid-June 2000, perennial critic Robert Park of the American Physical Society had not yet mentioned ICCF8 in his "What's New" column--either a sign of emerging cold fusion ascendancy, or terminal boredom with the topic by Park.)

A. De Ninno et al. (93): "The Fleischmann-Pons Effect in a Novel Electrolytic Configuration." Thin wire Pd deposited on glass. This is a miniature, or even microscopic, version of the electromigration in wire experiments which have been popular in Italy for several years, work which Dr. Celani and his group has done. These experiments are based on the theories of Fleischmann and Preparata. Some Americans, including Storms, do not think that electromigration can produce as much pressure within the metal lattice as the Italians claim. These Italians may be putting too many eggs in one basket by concentrating so much on this approach. This presentation needs to be examined more carefully, but on the face of it, the calorimetry is unconvincing, because the devices are so small and power levels so low: around 50 mW total (input plus apparent excess heat). The levels of material purity claimed are also suspiciously high. The wires are so fine, they frequently de-laminate from the substrate. We do not see the point of making such tiny devices, but Peter Hagelstein of MIT pointed out they have the advantage of loading quickly and starting the cold fusion reaction quickly. Perhaps, but as far as we know, the thin film Pd devices do not turn on any faster than the Ni or Ti cathodes, which have thousands of times more mass, and which make stronger, more easily measured excess heat.

P. Hagelstein (64): "A Model for Fast Ion Emission in Metal Deuterides." A continuing and now much more robust theory paper than Hagelstein of MIT has ever delivered--seeming to approach the stage where calculations of effects from system descriptions and reaction products might be carried out. Hagelstein told us he has been working more closely with McKubre at SRI International recently, and they have successfully designed experiments which address theoretical questions. McKubre's presentation confirmed this close collaboration. Hagelstein sees significant progress both with theories (his own and the Chubbs' in particular) and he thinks there is better agreement between data and theory now than in previous ICCFs. He seemed quite optimistic.

F. Celani et al. (96): He weighed in with the longest title we may have seen: "High Hydrogen Loading of Thin Pd Wires Through Alkaline-earth Carbonate Precipitation on the Cathodic Surface: Evidence for New Phases in the Pd-H System. Unexpected Problematics Due to Bacteria Contamination in Heavy Water." In the final portion of the lecture, Celani explained that they have discovered bacteria which survive remarkably well in heavy water, and which may be a previously unknown species. This is interesting, but a distraction. Eugene Mallove, who sat near critic Douglas Morrison of CERN, observed that

this discovery by the Celani group seemed to amuse him, perhaps to provide grist for future Morrison commentary.

Y. Iwamura et al. (59): "Nuclear Products and Their Time Dependence Induced by Continuous Diffusion of Deuterium Through Multi-layer Palladium Containing Low Work Function Material." Iwamura's already formidable experiment has been improved. His group now performs continuous in situ spectroscopic analysis of apparent transmutations in a multi-layer thin film metal sample. The apparatus is installed in a clean room and the cell is sealed throughout the experiment. Deuterium gas is diffused through the diaphragm "cathode" by producing a vacuum on one side of it. (It is not strictly a cathode, or an electrode, but that is what they call it.) The sample is not removed from the apparatus during the experiment, making contamination unlikely. Analysis in situ is performed with XPS (X-ray Photoelectric Spectroscopy) and SIMS (Secondary Ion Mass Spectrometry), and after the experiment with ICP-MS (Inductively Coupled Plasma Mass Spectrometry). With the Pd-D sample, Mg, Si, and S appear in the Pd, and increase monotonically as the experiment progresses. The Si and S isotopic abundance is not natural. X-rays are observed. A Pd-H blank sample run in parallel shows no excess heat, significant element formation, or X-rays.

Iwamura works for Mitsubishi Heavy Industries, in its Advanced Technology Research Center. Several large Japanese companies are continuing to support research projects (despite the demise of the official program, see IE, No. 30, p. 26). Some corporate projects are surreptitious. They sent representatives to the conference, but they do not want to go on record and we would not want to endanger the funding by naming them. They might even be collaborating with US-based companies. The researchers are grateful for the funding, and express their appreciation to their anonymous corporate benefactors, and so do we. Occasionally the behavior of these corporations is dumbfounding. After Toyota made a show of abandoning cold fusion research a few years ago, two other Japanese automobile companies jumped in. They explained to a startled Japanese researcher, "now the field is wide open"--as if only one company can do research at a time.

J.J. Dufour et al. (40): "Hydrex Catalyzed Transmutation of Uranium into Lead." Uranium hydrides were subjected to magnetic fields and electric currents. This seemed to increase the speed at which uranium converts to lead by a small but easily measured extent, and it produces commensurate excess heat. The samples were 500 to 900 mg, and they initially contain 1.5 to 2 ppm of lead, evenly deposited throughout the sample. After the treatment, the "periphery" of samples (presumably meaning the surface layers) contain 2000 ppm of lead, or 20 ppm for the entire sample. Gamma activity decreases by up to 1%. The lead is measured by dissolving the sample and measuring with ICP-MS. Dufour believes that cold fusion is caused by what he calls "HYDREX" (electromagnetic metastable proton electron resonance), and like most experimentalists with a pet theory, he always seems to find evidence for the theory. However, people with

other theories were impressed by the care with which these experiments were performed, and some of them found support for their own ideas. There have been other reports of radioactive decay being sped up or enhanced. The first and most impressive example was in a series of experiments performed in the 1960s by O. Reifenschweiler, which were later reported in Physics Letters A. Also, there are a host of groups such as CETI, Cincinnati Group, Trenergy, and Monti America, which have or continue to claim success with low-energy remediation of ingredients of nuclear waste.

J. Warner and J. Dash (6): "Effect of Cold Work on the Amount of Excess Heat Produced During the Electrolysis of Heavy Water with Titanium Cathodes." Warner is a graduate student with Prof. John Dash at Portland State University (Oregon). He gave a fine lecture. These results were described at the recent APS meeting in Minneapolis in March 2000. The group continues to make progress. This experiment is relatively foolproof and it nearly always produces excess heat with high confidence. One of the reasons it works so well is because they stick to the "KISS" formula--"Keep It Simple, Stupid!"--meaning they do not burden the experiment with complex instrumentation, or attempt to measure anything other than excess heat in situ. After the run they look for evidence of anomalies and transmutations with electron microscopy and EDX at Portland State, and with NAA at another lab, Reed College. This simplicity is an advantage and a disadvantage. Storms pointed out that this method cannot reveal much about the reaction. You cannot measure electrochemical conditions and control parameters such as OCV (Open Circuit Voltage), or look for helium or X-rays with such simple equipment. When you add in on-line detectors, the experiment becomes complicated and difficult. Still, it would be nice if other researchers would master the art of generating excess heat and doing reliable calorimetry before they launch ambitious programs to measure helium or X-rays. Another problem with this experiment is that the acidic electrolyte erodes about 40% of the cathode, destroying most evidence of transmutation or interesting surface changes. On the other hand, this bolsters his contention that some spots on the surface metal are transmuted, because it is unlikely that contamination would stick to the metal.

Warner does not think that loading is an issue with this experiment, because the effect either turns on immediately or not at all, and the material does not appear to form a titanium deuteride.

Our group at Cold Fusion Technology, Inc. (New Energy Research Laboratory, NERL) will soon attempt to replicate these titanium excess heat results with an identical Calvet or SEC (Seebeck Envelope) calorimeter (made by Thermonetics, Inc.), and Storms will also try to do this when he gets a chance. He has done a preliminary test of a sample provided by Warner, which showed no excess heat. Dash and Warner have been quite helpful. If other groups can replicate Warner et al., it will bolster the claims. Warner says he would be delighted, particularly if NERL or Storms can do it before his oral doctorate examinations. Compare

Warner's response to that of another company (our good friend CETI), which threatened to sue anyone who attempted to replicate their bead cathodes. Multiple replications would prove that cold fusion is easier than most experts have claimed over the years, and the missing element has been good teaching, enthusiasm, and the will to show other people how to do the work. Every summer for several years Dash has hired two or three high school students to perform cold fusion experiments. Every year they fail at first, for prosaic reasons such as leaky seals, contamination, or bad electrical connections. After a few failed attempts, the students learn how to produce excess heat. Dash's calorimetry has improved over the years. We very much doubt he is making a mistake, although one cannot be sure until he is replicated. Dash and Warner now use an advanced Calvet calorimeter along with an array of eight isoperibolic cells wired in series. They ignore an excess heat signal under 100 milliwatts, that is, only two or three sigma. This is a conservative estimate of the error margin with the Calvet calorimeter. Other cold fusion scientists should be as careful, summarily rejecting low-sigma results. It would give people more confidence in this field.

M. McKubre et al. (29): "The Emergence of a Coherent Explanation for Anomalies Observed in D/Pd and H/Pd Systems." There is so much to say about this, it is hard to know what to squeeze into this preliminary note. McKubre has outdone himself with the Case and Arata replications, which are far better than the originals. The helium issue is now closed. Cold fusion produces helium commensurate with heat, meaning the helium-4 production rate is comparable to a hot fusion deuterium-deuterium (D-D) reaction yielding helium-4 plus a 23.8 MeV gamma ray, only there is no gamma ray, just equivalent energy in the form of heat. The helium can only be a product of the reaction, not contamination, for several reasons: mainly because in 4 out of 16 cases it was measured at levels far above atmospheric concentration. (In other words, the helium may have leaked out of the cell, but under no circumstances could it have leaked in.)

The helium-3 and tritium results were also definitive. The ratio of helium-3 to helium-4 in nature is about 1:800,000. With the Case replication it reaches 1:67, 12,000 times higher than in nature, and in the inner chamber of the Arata replication the ratio is 44,000 times higher than nature. McKubre thinks the helium-3 is mainly the product of tritium decay, so we may now soon add McKubre's group to the list of those generating tritium in cold fusion experiments.

Huge amounts of tritium were found in some cells. The researchers devised a machine to pierce the Arata double structured cathodes and extract a sample of the gas inside them while rigorously excluding outside contamination. Samples were tested at one of the world's most sensitive mass spectrometers, in a laboratory specializing in the detection of helium, hydrogen, and other light elements at McMaster University. One sample of unknown contents from a newly opened cell turned out to have over 1000 times more tritium than anything previously submitted to that instrument. It put the instrument out of commission for three months!

Here is a quote from McKubre's summary at the end of his talk: "These results obtained in three different metal sealed cells, by three different calorimetric methods, with both electrochemical and gas loading experiments. The helium-4 analysis have now been performed in four different institutions, helium-3 analysis in two different institutions."

Helium has been detected for many years by different researchers, but never in such large amounts, at such high concentration. It has never been observed with such large signal-to-noise ratios, or produced so consistently and reliably. The Case cell works about two-thirds of the time at SRI International, and the Arata DS-cathodes work every time. These are difficult experiments and it is extremely unlikely they would work as smoothly in laboratories with less skilled researchers. This is no claim of "easy replicability," such as Warner and Dash make.

After the lecture, McKubre was asked about evidence of transmutations or other changes to the cathode material. He said it was too early to comment on this subject. Tom Passell, retired from EPRI, told us that he has acquired some of the used cathode material and he is conducting the analysis. He has subsidized, below-cost access to high resolution mass spectroscopy.

In this research, SRI cooperated closely with Osaka University, McMaster University, Pacific Northwest Labs, ENEA, and individuals including Les Case, Russ George, and Tom Passell. This openness and wide-ranging collaboration is exactly what this field needs most. SRI, McKubre, Tanzella, and Tripodi should be congratulated for these spectacular results.

J. Kasagi et al. (71): "Low Energy Nuclear Reactions in Solids." Continuing work by the group from Tohoku University in Japan, which demonstrates that with very low-energy bombardment of metals, such as palladium, by deuterons and lithium ions, unexpected enhancements in the reaction rates of D + D or Li + D occur. The implications for cold fusion are many.

E. Storms (32): This was originally slated to be about thin layers of Pd on inert substrates, but Storms has not had much luck with those cathodes, so instead he spoke about excess heat from Pt-yes, that's right, platinum! These results are presented in detail at:

http://jedrothwell.home.mindspring.com/Pt-energy.htm
http://jedrothwell.home.mindspring.com/Active-Pt-quattro-pro.htm

Furthermore, Eugene Mallove of NERL visited the Storms laboratory in April 2000, and found a cell active at that time producing excess heat from a Pt-Pt cathode-anode combination. The data and care with which they were obtained were extremely convincing. Note well, ye skeptics: This does i mean that Pt-Pt control-experiment methodology has been overturned--read on!

Storms now has two active Pt cathodes. He had some interesting new things to say about this experiment. During the lecture, Storms pointed out that this is the first cold fusion experiment in history to be fully published on the web, including all data points (in spreadsheets).

When Storms cleaned off the Pt and removed all depositions from the surface, especially by placing the cathodes in boiling water for a while, the cathodes ceased producing excess heat. He then took steps to deliberately build up layers of deposition, by putting the cathode in concentrated electrolyte, that is, high molarity electrolytes, made by dissolving a lot of powder reagent in water.

Michael Melich told Jed Rothwell, "This is old news. Pons and Fleischmann reported excess heat from Pt in 1989." It may be old news, but no one else has followed up on it actively to our knowledge. Excess heat from Pt has been the guilty secret of cold fusion, perhaps because it seems to show that any metal will produce heat under any conditions, and there are no "blank" (null, or control) experiments. The instruments always show heat sooner or later, with any combination of heavy water, light water, Pd, Pt, Ti, Al, or even Au or W. The skeptics long ago seized on this and said it proves that all excess heat must be an instrument artifact. They may have a partial point, albeit not for the right reasons. We suspect that many reports of heat from oddball configurations with non-standard calorimeters may be, in fact, artifacts.

Storms' conclusions:

Up to 800 mW of power, and 330 kJ of total energy was produced by the Pt sample, in completely reproducible patterns of behavior.
There is a linear relationship between applied current and excess power.
These results suggest that other metals which produce this effect may also involve surface layers (rather than the bulk).
Many researchers . . . who are committed to understanding palladium, should have an open mind, and consider that perhaps they are barking up the wrong tree.

It isn't really the metal producing the excess heat effect; it is something being deposited on the metal, and palladium is not the best substrate, because it takes time for the palladium to load such that it can support the surface layer of "X."

Storms concluded by saying, "Indeed, people ask me what am I using to calibrate; what is dead . . .?" [In other words, if Pt produces heat, what metal never produces heat and can be used as a reliable control blank.] "Clean Pt is dead--there is no doubt about that. Obviously it can come alive. . . With some humor and irony, I suggest you use palladium [as a control]."

X.Z. Li (62): "Nuclear Physics for Nuclear Fusion." Theoretical discussion of a resonant mechanism whereby a large cross-section for fusion without strong neutron or gamma emission may be obtained. A theory from which both cold fusion and hot fusion can learn.

G.G. Miley (65): "Advances in Thin Film Electrode Experiments." At first glance this looks impressive, but we have very strong doubts about the calorimetry and think thin film on glass is a poor choice of materials. The thin film pulls off too easily (de-laminates) and it does not have as much surface area as beads or convoluted structures like coated Ni fibrex, which looks like compressed steel wool.

We at NERL helped arrange the funding for this research and frankly, we are very disappointed by the results so far, although the thin Pd-wire experiment that showed a heat burst at a high loading ratio is intriguing (but not definitive and not reproduced, as yet). Miley and his students did good work a few years ago working with CETI, but unfortunately that partnership failed, the University of Illinois was upset with CETI, and the graduate students who were working on that project graduated. The new crop of people made major errors. Apparently, Miley was so busy with his other commitments, including hot fusion projects and meetings, that he did not adequately supervise the project, so it went badly off the track, although he thinks it has gone well. Storms also has doubts about the Miley calorimetry and he is preparing to test Miley thin film cathodes when they can be delivered to him. The Storms and NERL tests of the Miley cathodes will determine whether such cathodes can be used for demonstration cells, as we had hoped.

U. Mastromatteo (42): "An Energy Amplifier Device." At first glance we find this unconvincing, but intriguing. The author, a senior researcher with an Italian solid-state electronics firm, described a thin film device manufactured like a semiconductor. (We will present the author's full paper in a subsequent issue of Infinite Energy.) The only major indication of cold fusion is apparent excess heat, and the calorimetry is based on secondary or tertiary evidence from the metal melting point and loading behavior, even though the device is small and low powered, and could easily be installed in a conventional calorimeter. This research is well-funded, so there is no reason for such inadequate instrumentation. It is possible that the apparent excess heat may be caused by stored energy in the melted substrate (a phase change which does not show up as a temperature increase).

Mastromatteo is at SGS-Thompson Microelectronics, one of Europe's largest semiconductor companies. (http://us.st.com/stonline/index.shtml). The company has vast experience fabricating similar devices, deep pockets, and they are seriously interested in developing a commercial heat source. That's wonderful, but why haven't they done adequate calorimetry yet, since the work was initially reported at ICCF7 in 1998? One possibility: Dr. Mastromatteo told Eugene

Mallove that this project does not have high priority within the company's lucrative traditional business.

Y. Arata and Y.C. Zhang (18): "Definitive Difference among [Bulk-D2O], [DS-D2O], and [DS-H2O] Cells in the Deuterization and Deuterium-reactions." This was most certainly a very important paper, and it may have been definitive, but alas we have few clues about what Arata said. His English is so poor and halting, that despite thirty years of experience listening to Japanese-accented English, Jed Rothwell could not make out what he said. His viewgraphs do not help much either. They show a tangle of graphs too small to read from a distance, and tables without headings. We will review the video tape, but will probably not understand what he has in mind until the proceedings are published in January 2001. Note, however, that McKubre at SRI has obtained remarkable confirmatory excess heat and helium results with Arata's double-structured cathode.

T. Passell and R. George (100): "Impurity Analysis of Palladium Exposed to D2 and H2." Passell performed neutron activation analysis on four samples of Arata's double cathode powders which had been exposed to many atmospheres of pressure of either deuterium or ordinary hydrogen. Four samples appeared to show evidence of enhanced zinc isotope ratio content--factors ranging from 6 to 14 of exposed samples relative to virgin samples. Tests on samples obtained from Dr. Li and colleagues at Tsinghua University showed similar results for ordinary hydrogen exposure. This is a fascinating paper about analytical work that was financed on a shoestring by Passell.

M.H. Miles (58): "Calorimetric Studies of Palladium Alloy Cathodes Using Fleischmann-Pons Dewar Type Cells." A description of the research at the NHE laboratory, similar to the version that was published in IE No. 30.

Y.E. Kim and A.L. Zubarev (33): "Ultra Low-Energy Nuclear Fusion for Bose Nuclei Confined in Ion Traps." A theory paper by this top theorist in nuclear physics from Purdue University, who has been in the cold fusion field from the outset.

V. Violante et al. (34): "Hydrogen Isotopes Interaction Dynamics in Palladium Lattice." A complex study performed in cooperation with SRI International.

T. Mizuno et al. (103): "Production of Heat During Plasma Electrolysis in Liquid." The tungsten glow discharge experiments described in many articles in Infinite Energy, beginning with Issue No. 20, and now discussed here. Mizuno showed three new color photographs of the plasma that forms over the cathode. The form and color of the plasma changes as excess heat develops. We hope to publish a copy of the photos on a web page soon.

An interesting positive replication of this experiment was reported informally by Peter Mobberley from the UK (see also in this issue, Testing Update, p. 38). He

uses a "long reach" tungsten spark plug as the cathode. The outside electrode is cut off and a large stainless steel iron bar is used as an anode. That is an excellent choice of materials, because spark plugs are standardized; they are made of rugged, well-tested alloys; and the ceramic packaging is ideal for this purpose. Mobberley reports ~70% excess heat, highly repeatable. He has crude but seemingly adequate instruments: a Variac and an analog three-phase electric meter. Since he is in the UK, the mains voltage is high, which is convenient for this experiment, which begins to work well at 150 to 200 volts when electrolyte temperature is high enough, about 80°C. Mobberley also discussed with us privately a variation of the experiment with a plasma formed in a ceramic separator placed between two electrodes, in highly concentrated electrolyte. We at NERL will attempt to replicate this soon.

An interesting connection: Storms discovered that the excess heat effect does not take place in the Pt bulk, but rather in layers of deposited material on the surface of the Pt. When he cleaned this Pt surface off by scrubbing the metal and placing it in boiling water, the effect went away for a while until another layer of deposited material grew on the metal surface. Mobberley takes this a step further, moving the reaction site away from the metal to holes in a ceramic--in fact, he employs a pepper-shaker (known as a "pepper-pot" in the UK). Rothwell's first reaction was to say that this removes metal from the picture altogether and makes the effect look like something that happens in water, but Mallove reminded him that there is a great deal of lithium in the highly concentrated electrolyte used by Mobberley.

Mobberley's technique, if successfully replicated, might overthrow some accepted cold fusion "dogma." Perhaps the Ohmori and Mizuno glow discharge at the metal surface, and the Mobberley discharge in the middle of the electrolyte, far removed from the metal surface, is a separate phenomenon that has nothing to do with that standard approach to cold fusion. It is Mobberley's opinion that it may have to do with much-discussed lithium-proton or lithium-deuteron reactions, leading to helium-4 formation. Wouldn't that be lovely!

M. Swartz (70): "Metanalysis of Patterns of Success in LENR/CF." Mitch Swartz did not attend the conference, but his analytical paper was published in our last issue, which was distributed at ICCF8.

A. Roussetskii (75): "Application of Cr-39 Plastic Track Detector for Detection of DD and DT Reaction Products in Cold Fusion Experiments." Numerous charged particle tracks are seen from deuteron bombardment at low energy of titanium deuteride. Evidence of 3He-, triton-, and proton-generated tracks.

E. Del Giudice (92): "A Simple Model of the 'Cohn-Aharonov Effect in a Peculiar Electrolytic Configuration." This should be spelled "Coehn-Ahronov," meaning the electromigration effect, of which Martin Fleischmann is so enamored. Since

Giuliano Preparata had put considerable theoretical effort into the effect, it is now to be called the "Preparata Effect."

Y. Isobe et al. (14): "Search for Coherent Deuteron Fusion by Beam and Electrolysis Experiments." A very impressive set of three experiments conducted at Osaka University. In experiment 1, with conventional bulk palladium electrolysis, low levels of excess heat and up to 1016 atoms of helium were detected. In experiment 2, a 3 KeV electron beam struck palladium and titanium deuterides targets, and charged particles and X-rays were detected. In experiment 3, highly loaded titanium deuterides was irradiated with a deuterium or proton beam. The lecture was not long enough to do justice to three experiments, but details were available during the poster sessions.

G. Mengoli et al. (7): "Anomalous Effects Induced by D2O Electrolysis at Titanium." Titanium was electrolyzed in D2O 0.6M K2CO3 at a high temperature: 95°C, in an externally heated cell. The calorimeter incorporates a Dewar cell and an external condenser. We have not looked at this closely, but water-based calorimetry at these temperatures is extremely difficult, because heat lost as water vapor is difficult to measure accurately. This research is well-funded, and we do not understand why they do not employ something like a Calvet calorimeter. The authors claim the initial heat output was ~200 mW, and it later increased to ~2 W. They also claimed there were hours of heat-after-death when electrolysis power was turned off and external heating continued. This may be difficult to believe, because it would be unprecedented for titanium electrolysis. The calorimetry looks questionable, but Mengoli also showed surprisingly strong evidence for transmutation of titanium into a radioactive scandium isotope, with what looked like unassailable evidence: gamma ray coincidence counting and determination that the half-life of the gamma decay was consistent with the radioactive isotope as identified by the energy of the gamma ray spectrum. (In many experiments calorimetry is more believable than spectroscopy, but not always.)

S. Chubb and T. Chubb (25): "Theoretical Framework for Anomalous Heat and Helium-4 in Transition Metal Systems." An enthusiastic and clear performance, as always. Chubb compared some aspects of cold fusion to HTSC, the Mössbauer effect, and other well-established phenomena; he ended by suggesting that in these effects, groups of atoms band together in what might be called a meta-particle (Jed Rothwell's term), and a change which affects them all simultaneously, which explains how the heat manages to couple to the lattice at the speed of a nuclear reaction, without tearing apart the host metal. He thinks this is true simultaneity, or as one member of the audience called it, "superluminal electrolysis."

H. Kozima (46): "TNCF Model--A Phenomenological Approach." A highly unconventional theory, which postulates the existence of "quasi-stable thermal neutrons" or free neutrons floating around inside the lattice for indefinitely long

periods of time. Several theorists suggested privately that this should have been relegated to a poster session, but Dr. Kozima has been most insistent on the validity of his theory. He has tried to connect it with other theories with which he finds kinship. Battles over cold fusion theories continue to generate much "excess heat" within the field.

M. Fleischmann et al. (106): "Case Studies of Experiments Carried out with the ICARUS Systems." A closer look at some of the apparent mistakes made in the Japanese NHE (cold fusion) program. In IE No. 30, p. 31, we listed some of the reasons why Fleischmann disputes the NHE's negative conclusions. Unfortunately, Fleischmann was pressed for time, and he reiterated these and other reasons in such detail and so rapidly that we expect that few people in the audience were able to follow the discussion. (His talk prompted the only half-way impolite, argumentative question from CERN's Douglas Morrison. At other ICCFs Morrison has repetitively challenged presenters over what he regarded as inadequate testing protocols and theories. See page 32 for more on Morrison.)

The Loading Issue

Since 1989, it has been taken for granted by most researchers that the cold fusion effect occurs in the metal lattice, and that it requires high loading, that is, nearly as many deuterons in the lattice as Pd atoms. Fleischmann has long maintained that the reaction occurs in the "bulk," (inside the metal lattice) and not on the surface. In 1989, when Preparata began developing theories, he and others set forth two propositions which have been widely accepted as givens:

1) High loading is essential; the reaction takes place in the lattice.

2) The reaction does not generally produce neutrons or tritium; it produces mainly helium-4.

The second proposition has now been proved by McKubre's results, but the first is in trouble, and its unquestioned acceptance may have delayed the development of the field for many years. There is no question it is true in a sense, at least for Pd. It has been confirmed experimentally with as much confidence as helium-4 production. The questions now raised are: Why is it true; what is true about it; and can the benefits of loading be achieved by some other method more easily?

Yet Storms ended his talk by saying, "The idea that high loading is not necessary would be a stunning reversal, a sign that much research over the last ten years has been misguided. Few of the conference attendees are interested in hearing this news." There is no doubt high loading is necessary for excess heat in Pd. Many people have observed this, and McKubre's famous curve of loading versus heat proves that loading correlates closely with heat in Pd. However, evidence has now emerged that loading is not important with other metals, and with Pd it might only be needed in order to "crowd" deuterons at the surface. In other

words, the deuterons crowded inside the bulk of the metal may contribute little or nothing to the reaction. Nothing happens until most of the bulk is filled with deuterons and they begin spilling out onto the surface, and only (or mostly) the ones at surface participate.

You might achieve the same results by eliminating most of the bulk; that is, by making the Pd a micron-scale thin film, or by using a metal like Ni, which absorbs few deuterons (or protons). As Storms put it, describing his own positive Pt results, there is "no diffusion, no loss rate; the ions stay at the surface." This is a naive, simplified model, but if anything like it turns out to be true it will be ironic that so much effort and so many millions of dollars have been spent to increase loading, if it is determined that loading plays no fundamental role in the reaction.

Scaling Up, and Down

Many Italian mainstream researchers are working with ever-smaller devices, which are now on the microscopic scale.

A. De Ninno (93): She described a thin film "wire" 2 microns thick, 50 microns wide, and 80 centimeters long. They are based on the electromigration theories of Fleischmann and Preparata. The Italians may be putting too much emphasis on these theories, which may not be widely respected outside their circle. They may be betting too much research on them. They report several effects which they think are interesting anomalies, but which other researchers suspect may be prosaic instrument errors caused by the difficulty of working with such small devices. Problems such as tiny levels of impurities or a segment of the thin film that has torn off the substrate (de-laminated) might be extremely difficult to observe directly, and they might cause some of these "interesting" effects. For example, De Ninno reported an apparent cutoff of the Baranovski curve, which relates electrical resistance ratios to loading. She said the cut-off is predicted by theory, and after you take this cut-off into account, loading is actually extremely high. Other researchers say the curve may not really be cut-off, and loading is not so high. Because the electrolyte conducts almost as well as the thin film "wire," De Ninno must interrupt electrolysis power for a fraction of a second to measure resistance, and other researchers suspect that during that brief pause the wire is deloading, and losing a substantial amount of the deuterium in the wire.

Conference Summary

Friday morning, the last day of the conference, was a time to take stock of what had been accomplished at ICCF8. Conference organizer Francesco Scaramuzzi of INFN, Frascati noted that there had been twenty-six oral presentations and about fifty confirmed poster presentations. He remarked that of the 145 registered participants, the four leading countries were: Italy (41), U.S. (38), Japan (22), and Russia (12). He spoke of a "lively conference, rich with results." He said there had been "strong confirmation that we have the production of excess heat and helium and their correlation. Yes, there is excess heat and yes,

it is of nuclear origin." He considered the work on transmutation presented at the conference to be "high level" and an "indication of the nuclear character of the phenomenon." In his opinion, cold fusion could only be understood in terms of coherence--an important aspect of the behavior within condensed matter, which he said would have more general implications for physics.

Scaramuzzi acknowledged that most of the scientific community does not think that cold fusion exists and that "it is not science." He called that an "incomprehensible position" in view of the peer-reviewed articles on the subject that have already appeared. Even so, he said that he considered cold fusion to be still in the stage of a scientific problem, not quite ready for the commercial arena. Differing with this position was Dr. Biberian from France, who said in his summary, " We are sure that it is real. . .We need applications to make the world realize it. . .We need to be more pro-active." He suggested that cold fusion people should not be like the fabled Cinderella, "waiting for her prince to come." Dr. Yury N. Bazhutov of Russia, also touching on emerging commercial interest, noted that more than half of the Russian participants had received financial backing from private companies to come to ICCF8.

In his overview, Prof. Akito Takahashi of Japan agreed that "science only is not enough." He said there was a need to extend interest in cold fusion to "industry people, people from other fields, and young people." He called the 4He-excess heat correlation "almost confirmed" and low-energy nuclear transmutation "nearly confirmed."

During the open-microphone session that followed the official summaries, an Italian researcher, Dr. Roberto Andreani, who heads ENEA's laboratory for hot fusion and is trying to get support for the ITER (thermonuclear) reactor, had kind words for the cold fusion community. He joked that he does not "receive a very good reception" when he talks about cold fusion to his hot fusion colleagues. He considers that the cold fusion work at ENEA is "very well done." Andreani urged that progress be made to create "absolutely convincing experiments . . .I wish you good success in your work, but remember that you have strong opposition."

Cold fusion researcher Dr. Jean-Paul Biberian made a special announcement near the end of the proceedings: the first international conference on biological transmutation had been held in Geneva, May 1, 2000. There had been sixteen attendees from Italy, Switzerland, and France. He had been doing experiments with bacteria and seed-sprouting already, observing unexpected element changes on the order of a few percent. A non-profit organization is being formed to support continuing conferences, publications, and research.

Dr. X.Z. Li of China, who is both a cold fusion and hot fusion researcher, had the last word. He is the organizer for ICCF9 in Beijing in 2002. "What will be the theme for ICCF9?" he asked. Answering his own question, "Coherence-in solid materials, in research and development, and in cold and hot fusion!" He looked

forward to the day when cold fusion and hot fusion would "merge," since he believes that both communities are "working for the same goal: nuclear fusion without strong radiation." Well, at least some hot fusion people--those in aneutronic hot fusion--have that vision. Li suggested that cold fusion researchers should consider the possibility of developing a heater based on the "heat-after-death" phenomenon.