Early (true) DROM report

From: IN%"KMIT@BNLDAG.BITNET" 12-APR-1989 00:41:03.58
To: kemhjj@vms2.uni-c.dk
CC:
Subj: thought you might be interested...

Received: from JNET-DAEMON by vms2.uni-c.dk; Wed, 12 Apr 89 00:40 +0100
Received: From BNLDAG(KMIT) by DKARH02 with Jnet id 5946 for KEMHJJ@DKARH02;
Wed, 12 Apr 89 00:40 A
Date: Tue, 11 Apr 89 17:36 EST
From: KMIT@BNLDAG.BITNET
Subject: thought you might be interested...
To: kemhjj@vms2.uni-c.dk
Original_To: JNET%"kemhjj@dkarh02"

From: Jnet%"SAROFF@JVNC" "The Bear who Swims" 10-APR-1989 13:33:04.78
To: @POST:ALL
CC:
Subj: OOOPS, SORRY FORWARDED THE WRONG MESSAGE, HERE'S THE RIGHT ONE ON COLD F
USION

Received: From JVNC(SAROFF) by BNLDAG with Jnet id 3597
for KMIT@BNLDAG; Mon, 10 Apr 89 13:32 EST
Date: Mon, 10 Apr 89 13:13 EDT
From: The Bear who Swims <SAROFF@JVNC>
Subject: OOOPS, SORRY FORWARDED THE WRONG MESSAGE, HERE'S THE
RIGHT ONE ON COLD FUSION
To: KMIT@BNLDAG
Original_To: @POST:ALL

31 March 1989.

PHYSICS NEWS - COLD FUSION?

Dear E632 and WA84 Colleagues,

There have been many reports in the newspapers that Prof. Fleischmann of Southampton and Dr. Pons of Utah have evidence for cold fusion of deuterium by electrochemistry. This afternoon Prof. Fleischmann gave a seminar in CERN. Because of the many media reports, the auditorium was crowded and although I arrived 20 minutes early, I had to sit on the steps. As I have given several lectures on Wrong Results in Physics, I went to this and also to the press conference afterwards - especially as the news reports had been very hard to understand scientifically, but if true, this could have a major impact on the world economy.

Martin Fleischmann had a reputation as a major expert in his subject. As his talk developed, it became clear that he was a first class scientist and it seems to me that he has made a major breakthrough, though what the fundamentals processes are is not yet fully understood.

Let me try and explain what I think I learnt (I talked to him for a while afterwards, so it may not be too bad).

Basically the catalyst used, palladium Pd, is a face-centred crystal. It can absorb a certain amount of hydrogen. If an electrical potential is applied, then over a period of time it can absorb a great deal. For F & P, they reached 0.6 atoms of deuterium per atom of Palladium after three months.

They made tests with four rods each of 10 cm length and of diameters 0.1, 0.2, 0.4 and 0.8 cm. They only have good measurements for the first three as one morning when they came in they found that the fourth and largest rod had melted and the fume cupboard was starting to smoulder!

They made calorimetric measurements and found that they were getting more heat out than they had put in and this effect increased with the diameter of the rod. It seems to be a volume effect and not a surface effect. The excess heat is about 5 megajoules per cm3 which is about 100 times greater than any known chemical process.

A second measurement was by putting a NaI crystal close when they recorded gammas. The energy spectrum of the gammas was sharply peaked between 2000 and 2400 which is characteristic of the (n,gamma) reaction on hydrogen. This could be explained as the neutrons interacting in the water bath round the experiment.

Thirdly they observed tritium production and measured and found a "characteristic" spectrum (I did not understand this fully, partly as he had an incomplete scale on the graph, but see later).

Fourthly they looked for neutrons using a polythene sphere filled with BF3. The count was three times background. In 50 hours they counted 40 000 neutrons. However there is a point that is a stumbling block for particle physicists - if you take the rate of release of heat, then there should be 10 E 13 or 14 neutrons - a huge discrepancy. He does not have the equipment to measure the neutron spectrum - the neutrons have to pass through the surrounding water bath which tends to thermalise them.

A conclusion that can be drawn from Fleischmann's talk is that the heating is not due to the reactions

2D + 2D ---> 3He + n (1)

or 2D + 3T ---> 4He + n (2)

which are the ones that spring to mind.

He gave a table of the excess enthalpy in the Pd rod cathodes expressed as a percentage of breakeven values;

0.1 mm	81%
0.2	189%
0.4	839%

>From this it can be judged that it was not too surprising that the 0.8 cm rod melted!

He opened his talk with a basic discussion of electrochemistry.

D2O + e- <---> D(absorber) + OD-

D(absorber) <---> D(lattice)

D(absorber) + D2O + e- <---> D2 + OD-

With the applied field the D can go over the potential barrier by applying a Potl. Difference at the interface. The result is that inside the Pd there can be many collisions without repulsion. Effectively there is a PD of 0.8 eV which can translate into a compression of 10 E 27 atmos. i.e. it would require this enormous pressure to achieve the same PD. Thus electrochemistry is high energy chemistry! The D is in a sea of high electron density. The structural or coherent strength of the Pd is 4000 atm. Thus it is a very strange kind of Quantum Mechanics (his phrase).

I have to go to collect my daughter at the airport, but will try and continue later.

1 April 1989.
(despite the date, it is serious!)

Re-reading what I wrote yesterday. I realise that I have been trying to explain simply. The actual talk contained some more details and two tables of results that I had only time to copy down partially. There was a fuller discussion of electrochemistry.

The question now is what is happening. The observations are of a source of heat, of emision of tritium, gammas and of neutrons, but the number of neutrons are many orders of magnitude less than would be expected if the heat produced came from reactions producing neutrons. Fleischmann talks as if you have to modify quantum mechanics - this I do not believe - we have to apply it differently.

An additional piece of information that he gave at his press conference but not at his seminar, was that the particle emission was not uniform but had fluctuations which were much larger than statistical - this I think is a very important piece of information.

There are a lot of different theories being discussed. The following comments should be considered private, qualitative and not necessarily correct.

The catalyst, palladium works by accepting an incredible number of deuterium nuclei in the spaces of its face-centred cubic lattice. The distance between eac deuterium nucleus is therefore reduced. This was first demonstrated by the observation of muon-induced catalysis where in deuterium, the electron is replaced by a muon. As the muon is some 200 times heavier, the proton and neutron are pulled closer together so that the probability of fusion is greatly increased - by many orders of magnitude. Now there are two suggestions;

1. Since the deuterium nuclei are in a very dense electron field, it may be that the electrons have an effective mass much greater than normal and this increases the probabilty of the nuclei tunnelling through the barrier.

2. the applied potential difference drives more and more deuterium nuclei into the spaces between the palladium atoms so that the separation of the nuclei decreases so that the probability of fusion increases dramatically.

Personally I have a preference for the second approach, but it is always possible that both are applicable.

Instead of saying that there is a discrepancy between the number of neutrons produced and the heat produced, perhaps we should assume that all the results are correct and that the reactions ocurring are different. Maybe the dominant reaction is fusion, D + D ---> 4He, but we need something else to share the energy and momentum produced - this could be the close neighbouring structure of the lattice. Thus the dominant reaction is to produce heat! Of course other reactions will also occur which is why there is an observation of tritium and one would expect some production of 3He and 4He and neutrons and gammas. If this were true, and again this is mainly a suggestion which needs experimental confirmation, then this would have tremendous social effects as we would have a simple source of energy without the particulate matter, sulphur and other gasses from coal and oil fired power stations that are killing so many today. Also the radiation danger would be very much less than with nuclear reactors (sell your coal and oil shares if you have any!)

In answer to a question, Fleischmann said that they had tried to look at 3He and 4He production and ratio, but the experiment is difficult for them and they prefer to leave that for experts who have the equipment - for they have been using their own money for 5 years.

Looking again at my notes, I discover that John Ellis had said in the discussion that there could be little Coulomb repulsion as there could be a classical oscillation of the lattice.

Before the Seminar, things were rather disturbed with the media - lots of TV crews and flashes popping off. The Chairman, Carlo, asked them all to leave explaining this was a scientific meeting and he did not want questions on any other subject, but afterwards there would be a press conference. After some time the media left. At the end of Fleischmann's talk, the TV crews re-entered and had to be requested to leave again before the question period.

On the way to the press conference, Fleischmann was told that there had been a report on the radio that a group (at Columbia?) had confirmed his result. He said he had not heard this and during the Press Conference he continued to emphasise, in a very proper manner, that before leaping to conclusions, there should be further confirming evidence.

Fleischmann had described his other press conference in Utah as awful, but this one went well with Carlo a good Chairman - who was also asked questions. Fleischmann explained that the work was intentional and not an accident. He said that after verification, it might take 10 to 20 years to develop an economically viable system. Carlo was asked his opinion and said that "Dr. Fleischmann has planted a seed - will the seed grow up? I think yes" Fleischmann said that he believed in Karl Popper's philosophy - you cannot prove something right, you can only prove it wrong. "We have spent 5 years trying to prove ourselves wrong, now other people should try".

In explaining why they did it, "it was not to do an ego trip (though all scientists are on an ego trip to some extent), but to try and find a plentiful source of energy. We have a social conscience"

Question - "There was a sceptical atmosphere in the room, did you feel like a chemistry bull in an arena of physics toreadors?"

Answer - "Are people correct to be sceptical?, yes, it is correct to be sceptical. But it was not a bad atmosphere. Our experiment fits partly into accepted ideas but not entirely, therefore either experiment is wrong or we have extended the conceptions of possible fusion mechanisms".

Carlo was asked if he found the meeting strange - "No, I am at home in my own lab".

Question - "Do you think it is correct?". Answer(MF) - "I think it is correct, but others should show it is correct". (Note, this was typical of some of the questions where the journalist asked "for a good quote").

Carlo was asked if CERN should work on fusion. He replied " There are different science cultures. In an orchestra everyone tries to play his own instrument, and does not have other instruments. But we have quantum mechanics in common. We should do what we do best. But there is also cross-fertilisation between chemistry and nuclear physics" He also joked that this was the first time that a chemist had discovered a neutron!

Question - "Any military applications?"

Answer(MF) - "There will always be some military application of anything, but we do not know of any such thing"

Question - " You said you did not have enough money, have you been offered money since your press conference last week"?

Answer - "Up to now have used our own money as we thought it unlikely to work, so there were some restrictions. Since then we have been approached with offers but as our capacity to spend money is limited, we have to plan carefully.

Question - "If it is fusion what will its effect be on other fusion research?"

Answer - " Glad you asked that. It would be a total disaster to cut back on other fusion research. Ours is small scale, theirs is large scale generation of electricity. It would be extremely foolish to cut back".

There was more, but I hope this gives the flavour - both Fleischmann and Carlo aquitted themselves very well and responsibly.

Friedrich Dydak had told me he had two papers confirming the F & P work and I could copy them. Later when I was returning them, Fleischmann came in for another TV interview and we talked while he was waiting for the lighting to be set up. He had not seen the papers, so I gave him copies. The main author was Stephen Jones who is at the BYU in Utah beside Dr. Pons. We looked quickly at the papers - he was particularly interested in the dates on the papers.
I explained I was interested particularly for two reasons. Firstly as I was possibly the first to observe fusion in Europe - in the early sixties I was scanning bubble chamber film of deuterium and normally when there is the decay chain,

pion ---> muon ---> electron

the muon always has the same short range (if the pion is at rest). But one day I observed an extra long range for the muon. I spent some time measuring the curvature and angles of the tracks, but could not explain it. However someone told me that the Berkeley bubble chamber group had found it and it had been explained as the muon replacing an electron and causing fusion. At this Luis insisted that this should be treated as a secret, but quickly it was calculated that it had no military or economical value. So I left it and went on to new things(incidently the Scientific American article of July 1987 by Rafelski and Jones on Cold Nuclear Fusion says that this muon -induced fusion was first suggested by Frank and Sakharov in the late 1940's).

Secondly I said I had given several serious lectures on Wrong Results in Physics and found that they exhibited certain characteristics so that they could be recognised before they had been proved wrong - after the press reports I wondered if this was a case in point, but after I had heard his conference, I was inclined to believe that his results were correct. He did not seem to appreciate this too much, not unnaturally, but we continued talking and he told me some remarkable things. I mentioned that after the press conference, Dr. Wind was looking for him as he used to work in Utrecht on electrochemistry and had been able to insert 1000 hydrogen ions per atom of palladium catalyst. Dr. Fleischmann (who had attained 0.6 ions after 3 months) said he did not believe this number of 1000. However talking with Per-Olaf Hulth this morning, he had checked this subject last night and read that 850 ions of hydrogen had been inserted - this could be used as hydrogen storage cells for cars driven by hydrogen - air mixtures. If I remember rightly, Fleischmann had replied that they had not prepared the surface of their palladium rod, and this could make a big difference. If it were possible to insert so many deuterium ions into palladium, then the rate of fusion would be greatly increased (or the charging time would be less than 3 months).

The two papers are;

1. "Observation of Cold Nuclear Fusion in Condensed Matter" by S.E. Jones

and others of Brigham Young Univ. and J. Rafelski of Univ. of Arizona.
2. "Limits on Cold Fusion in Condensed Matter; a Parametric study" by J. Rafelski and others of Arizona and S.E. Jones of BYU.

The main point of the first paper is that they claim to have observed neutrons when there was low voltage electrolytic fusion of deuterons into metallic titanium or palladium. They believe this is from the reaction;

$$d + d \longrightarrow {}^3He(0.82\ MeV) + n(2.45\ MeV) \qquad (1)$$

The distribution of counts in different channels give a broad enhancement which the authors say corresponds to neutrons of 2.45 MeV. This looks convincing - just; it would be good to repeat this.

They say they have not yet(?!advertising?) obtained results regarding the parallel reaction;

$$d + d \longrightarrow p(3.02\ MeV) + t(1.01\ MeV) \qquad (3)$$

The electrolyte contains various minmeral salts and they say that their evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode. "hydrogen bubbles were observed to form on the Pd foils only after several minutes of electrolysis, suggesting the rapid absorbtion of deuterons into the foil; oxygen bubbles formed at the anode immediately". The palladium pieces were 0.025cm thick and had the surfaces roughened or were mossy. They do not say that it took 3 months to get started by charging the deuterons into the palladium (private comment - this suggests to me that Fleischmann and Pons would have improved things if they had increased the surface to volume ratio of the catalyst and roughened its surface, but it is hard to be sure. However it does suggest that it is possible to charge the catalyst in much less than three months).

The experimental part of their paper gives an impression of haste, but there are a lot of other interesting things in their paper;
In a deuterium molecule the separation between the deuterons is 0.74 A and the d-d fusion rate is very slow about 10 E -70 per D2 molecule per sec (calculated in an interesting paper by Van Siclen, C.D. and Jones, S.E., Journal of Physics G Nucl. Phys. 12 (1986) 213 - here they state that the fusion rates for reactions (1) and (3) are nearly equal over the range 10 to 30 KeV. They also discuss whether piezonuclear fusion - i.e. by pressure - within the liquid metallic hydrogen core of Jupiter could account for the fact that the planet radiates 1.5 times as much heat as it receives from the sun. However they concluded that this process was many orders of magnitude too small to be a significant energy source - this is where the idea of Fleischmann and Pons of using electrolytic catalysis is so important). However in muon-induced catalysis the internuclear separation is reduced by about the ratio of the muon to the electron masses (200) resulting in the fusion rate increasing by an enormous factor, 80 orders of magnitude! In the second paper this variation of fusion rate as a function of the distance is quantified. This made me think of the observation by Fleischmann that they had observed large fluctuations in the signals - for the number of deuterons in a space in the lattice of Palladium is discrete and given by Poisson statistics hence the distance between the deuterons will vary appreciably - this and other factors(roughness of surface) could cause there to be local spots hot in space and time, since the fusion rate varies so violently with distance. In addition to the reactions (1) and (3), there can occur the reaction on tritium that will exist to some varable extent,

$$2D + 3T \longrightarrow 4He + n \qquad (2)$$

Although there is less tritium than deuterium, this reaction has a much higher cross section - so that this reaction (2) could also help fluctuations (but these comments on fluctations are my own, so treat them with appropriate caution).

Paper (1) also has an interesting chapter on Geophysical considerations (or the Hawaii effect). Sea water contains about one part in 7000 of deuterium. By subduction water is carried down to the earth's mantle where it might undergo fusion via the reaction;

$$p + d \longrightarrow 3He + gamma(5.4\ MeV) \qquad (4)$$

under the extreme pressure and temperature there. Calculations are done which indicate that a substantial contribution to the heat flux through the crust could come from cold fusion. This heat could also help to explain the localised heat of volcanism at subduction zones. They quote that the 3He to 4He ratio is high in rocks, liquids and gases from volcanoes. Further they then predict that tritium will be produced from d + d fusion and since tritium is relatively short-lived(12 years half-life), observation of tritium would suggest a geologically recent process. On the Mauna Loa mountain on Hawaii, tritium was monitored from 1971 to 1977 and a correlation is shown in the paper between the tritium level and volcanic activity. This is very striking for the 1972 Mauna Ulu eruption but later eruption signals were partly confused by atomic bomb tests. They estimate that in the Mauna Ulu eruption 100 curies of tritium was released per day for 30 days!

In paper (1), it is also reported that after diamonds are sliced with a laser, the concentration of 4He and 3He has been measured - it is reported that the 4He is distributed uniformly while the 3He is concentrated in spots suggesting cold fusion reactions. Similar anomalies have been reported in metal foils.

The authors also calculate that the excess heat from Jupiter could be accounted for from cold fusion in the core consisting of metallic hydrogen plus iron silicate.

The second paper calculates the cold fusion rate of d-d as a function of 1 - relative energy, 2 - separation of two hydrogen nuclei in a sphere, 3 - the effective electron mass, 4 - the effective electron charge. They do not consider the effects of the lattice of a catalyst as do Fleischmann and Pons.

It is probable that some readers will be thinking that this letter has wandered off strict physics news. They are right. It is intentional as I feel this subject will become so important to society that we must consider the broader implications as well as the scientific ones. Looking into a cloudy crystal ball, it is not impossible to foresee the situation that the experiments are so easy that schools will be doing them, that many new companies will start up, most(not all) will fail and the present big power companies will be running down their oil and coal power stations while they are building deuterium separation plants and new power plants based on cold fusion. No new nuclear power stations will be built except for military needs. There will be very little if any research on high temperature(plasma) fusion. Petrol will probably still be used for cars. Overall pollution will start to be less. Ecologists will be talking about the contamination from radioactive tritium and asking about the effect of this tritium on the ozone layer.

CONCLUSIONS

It is known(from muon cataysis) that if two nuclei of deuterium or tritium are held close together, then they can fuse releasing energy. Fleischman
n
and Pons thought of achieving this by using electrolysis to insert deuterium nuclei inside a palladium catalyst. They observed production of more heat than they put in. They also observed tritium production, gammas of an energy consistent with neutrons interacting with the surrounding water bath, and neutrons directly. They thus conclude they have observed fusion of heavy hydrogen producing energy, i.e. cold fusion. A paper by Jones et al. reports on the operation of similar electrolytic cells with observation of neutrons with an energy spectrum consistent with that expected from deuterium fusion. They also describe interesting though rather anecdotal evidence for fusion in volcanoes, Jupiter, diamonds and metal foils. The theory, while not fully developed, suggests that the deuterium nuclei inside the lattice of the catalyst, are held so closely together that the probability of fusion(the tunneling effect) is dramatically increased by many orders of magnitude. it may be expected that this will cause major changes in the energy industry and major social, economic and hence political changes.

Douglas R. O. Morrison.

From: IN%"FUSION%ZORCH%AMES.ARC.NASA.GOV@VM1.NoDak.EDU" 26-APR-1990 01:20:52.45
To: Multiple recipients of list FUSION <FUSION@NDSUVM1>
CC:
Subj: I see you have 22. Here is 21 if it helps.

> Date: 14 Apr 90 22:44
> From: MORRISON@ch.cern.decnet.vxprix
> To: D_BROADHURST@uk.ac.open.acs.vax
> Message-ID: <9004171413.AA18720@dxmint.cern.ch>
> Subject: Thanks for message. Here is latest CF News. Am preparing another.
>
> >X-Vms-To: MINT::"D_BROADHURST@vax.acs.open.ac.uk"

Dear E632 and WA84 Colleagues, 18 March 1990.

COLD FUSION NEWS No. 21 - ONE YEAR AFTER.

Review

SUMMARY OF THE YEAR and UPDATE

The First Annual Conference on Cold Fusion will be held on 28 to 31 March 1990. Here we review the past year in particular new results and information since the last CF News in November.

On 23 March 1989, Martin Fleischmann and Stanley Pons announced at a Press Conference that they had produced excess heat and fusion products in a simple table top experiment. They had used heavy water and an electrolytic cell with a palladium cathode and had obtained Fusion at room temperatures - Cold Fusion. The dream of unlimited power with little pollution! - the solution of an ecological problem. Rapid confirmation from Steve Jones's and other groups launched world-wide excitement about Cold Fusion.

However there was a major discrepancy between the amount of power claimed from deuterium fusion and the very low rate of the fusion products which should be produced by the fusion. This made many doubt and in Cold Fusion News No. 4 (9 April) it was stated that more and more of the characteristics of Wrong Results in Science were being observed - or Pathological Science - the name introduced by Irving Langmuir in 1953. The Regionalisation of Results was discovered and presented on 2 May to the American Physical Society (CF News No. 13) where it was noted that Northern Europe and the major labs and the North-East of the USA found almost no fusion while reports from the Rest of the World were overwhelmingly in favour of Cold Fusion. The world was said to be divided into "Believers" and "Sceptics". Conferences were held which were mainly for Believers with positive results - this despite protests that in Science both positive and negative results should be considered simultaneously. However the Sceptics with negative results, continued to gain in number and sophistication of their experiments, in various regions of the world - this was described in Pathological Science terms as three phases; In
Phase 1 there is the original announcement followed by rapid confirmation
Phase 2 has about equal numbers of positive and negative results
Phase 3 has an avalanche of negative results (CF News No. 4).

The world followed this evolution in 1989 with Northern, Southern and Eastern Europe now all reporting only negative results.

However in 1990 we have a new phenomenon which requires the introduction of PHASE 4 - most results are positive! What is happening is that in much of the world scientists have made their experiments and found nothing and they have read the literature and concluded that there is nothing serious in Cold Fusion, so they have stopped tests, press conferences and there are only a few publications of older experiments. On the other hand "Believers" are continuing tests and are publishing their positive results. The two statements below are correct;

A. there are now more positive results being presented (or published?) than negative ones
B. The rate of new positive results is decreasing. The rate of new negative results is decreasing much more quickly, so that the ratio of negative to positive results is rising.

It is up to the reader to chose which statement he likes.

In July the Cold Fusion Panel with co-chairs John Huizenga, a distinguished chemist, and Norman Ramsey a physicist who was a 1989 Nobel laureate, and which was set up by the DOE, gave an interim report saying that no "convincing evidence that useful sources of energy will result" had been seen and "No special programmes to establish Cold Fusion research centres are justified".

In August the National Cold Fusion Institute in Salt Lake City was established. The funding came from the State of Utah.

In August a Cold Fusion Research Institute was established in Japan. It has not been easy to get information about experiments in Japan though a few early experiments were boosted in the newspapers, though looking at the papers suggested some of them were of poor quality, e.g using a single BF3 counter. However there could be many commercial experiments that are not reported. It has been said that several hundred people may be working on Cold Fusion.

In India the large Bhabha Atomic Research Centre, BARC, reported that six experimental teams had found evidence for Cold Fusion and several hundred people were working on it.

In November the final report of the DOE Panel appeared confirming the conclusions of the interim report, There was one curious omisssion - in the interim report it was said that small experiments might be justified to study some unexplained effects reported and these experiments should be peer-reviewed, but in the final report the peer-review requirement was not made.

In January Stan Pons began a series of 32 experiments at the NCFI to determine the best conditions and materials, and he was intending to start a further series of 32 experiments.

The First Annual Conference on Cold Fusion will be held next week, 28 to 31 March at Salt Lake City. The programme is crowded starting at 08.30 and going on to 20.15.

A persual of the programme is interesting. There are no experimental talks from Europe although at one time some regions' media were filled with stories of positive results. However there is one European theoretian down to talk - Prof. G. Preparata of Milan. There are no speakers from Japan. From BARC there is the Director, Dr. P.K. Iyengar and an experimentalist. There is a theoretian from the National Taiwan University. All others are working in the USA and as far as I could judge there are 17 experimental talks, 7 theoretical and 9 where it was difficult to be sure from the title. One has the impression that all the talks will be positive though some of the people going to the conference are not "Believers". Among the theoretical speakers are Nobel laureate Julian Schwinger and Peter Hagelstein from MIT. There are also two panel discussions whose members would all be considered "believers". The Governor of Utah, The Honorable Norman S. Bangarter will attend a Reception. Desert at the Reception will be provided by Mrs. Fields Cookies.

The role of the media has been important.

The Wall Street Journal had an article on 9th March which was favourable to Cold Fusion. It was entitled "Doubts Recede over Cold Fusion but an Explanation Remains Elusive". The article concentrates on work at Los Alamos by Edmund Storms and Carol Talcott who have reported large amounts of tritium. At first they had trouble reproducing their effect but claim that 7 of their last 9 cells gave tritium. They recorded up to 80 times higher tritium levels than in the new heavy water. The Indian experimenters at BARC are quoted as having obtained as much as 20000 times more tritium than originally present. David Worledge of the Electrical Power Research Institute, EPRI, (who are the only source of research funds since the DOE officially stopped funding) says that 22 different cells have given tritium. More experiments have been reporting excess heat; mentioned are Charles Scott of Oak Ridge National Lab, Prof Huggins of Stanford University and Prof Bockris of Texas A&M. Prof Bockris is quoted as saying "There's no doubt of the existance of an effect", unquestionably a nuclear reaction of some sort. The big problem is that "we still can't reproduce it at will".

Dr. Storms sent me a copy of his paper and a compilation(author not given) of positive results which lists 12 groups having observed tritium production. There is, however, not a list of experiments which failed to find tritium and the upper limits they gave.

It is clear the Wall Street Journal technical section is not the same as the Science section of the New York Times. If some subject is known to be controversial, the NYT takes care to consult experts with other views; for example members of the DOE Panel who would have warned readers of their study. They might also have pointed out that the levels of tritium reported are many orders of magnitude less than that expected from the excess heat claimed.

The first book on Cold Fusion has appeared - it seems hastily written and had poor reviews in Nature and The San Francisco Chronicle. Two other books should appear shortly, also written by experienced writers - one is by Frank Close who is a theoretical physicist and the other by Gary Taubes - the two books can be expected to be written in contrasting styles and to be more complete than the first. British ITV has had a programme about Cold Fusion with Frank commenting. On 26 March the BBC will show a programme on Cold Fusion in the respected "Horizon" science series. For the 23 March anniverary many newspapers will have articles.

A major feature of the past year is that it has allowed many people to have a glimpse of modern Science and scientists in action - the circumstances were extreme but maybe that is a good way to test a structure. It is unusual to announce important new results by press conference and then to give too few details (though it could be argued that the possible importance of the effect justified it). However it was seen that means of communication are now extremely rapid - by television, newspapers, telex, telefax and electronic mail. The latter is now the preferred means of communication among scientists, particularly physicists who are involved in international experiments and who have extensive networks already set up. Experiments were performed quickly world-wide and the results exchanged. Meetings were held at which all could present their results freely (it is an aberration that astonished many that at a few meetings only allowed positive results - this is not normal Science). A consensus soon emerged that room temperature fusion could not provide power. Everyone was disappointed for if it were true it would have been important for the world. While by far the greatest number of experiments did not observe any fusion products, some did and this has encouraged some scientists to continue. The fact that all agree to, that the positive findings are erratic and irreproducible, encourages these scientists but is discouraging to most. Even more discouraging to most scientists is that while those claiming power say they observe watts, those claiming fusion products observe them at a rate corresponding to nanoWatts or picoWatts or even less.

The names "Believers" and "Sceptics" was applied by believers. It might be more accurate to say that among those who have worked on or closely followed Cold Fusion, there are three classes - two small ones, "Believers" and "Sceptics" and one large one, "Non-believers"

The Regionalisation of Results is a fact though very disagreeable. It could be considered as a reminder that Scientists are People first and Scientists second.

OTHER INFORMATION

There are many other items of news - here are a few.

1. Argentine Ingenuity.

On Friday I received two papers from Dr. Granada of the National Atomic Energy Commission and two other institutes in Argentina. Both papers have been accepted by the J. of Nuclear Science and Technology. The first long paper describes how the application of a pulsed current through a cell gives a correlated neutron production in a repeatable manner. As the counting rate is very low, about 0.1 neutrons/sec, they do not claim fusion nor give a number of standard deviations. To reduce their background to make the neutron signal stand out, the normal technique is to go underground. However since there are no Gran Sasso or Frejus or Mont Blanc tunnel laboratories in Argentine, they had to find another solution - so they went underwater in a submarine! (a conventionally powered one they state). This reduced the background by a factor of 70 and they state they observe a three standard deviation effect. However these numbers suggest that they were not observing any effect in their first experiment and their graphs seem to bare this out. W again!

2. Joint Sceptics - Believers Experiment

At the Santa Fe meeting in May, Moshe Gai challenged Steve Jones to do a joint experiment with him by placing one of his cells that he said gave neutrons, inside Moshe's detector. Steve, as a good sport, accepted. The experiment was performed in August and went happily. IN early November a brief note was given to the DOE panel saying that no neutron bursts had been found, apart from some associated with cosmic rays. Thus it seemed an ideal solution had been found, Believers and Sceptics work jointly and establish the truth. This would be new as in my Pathological

Science studies, I have not come across a case where this happened fully. However there was soon major disagreements as Steve calculated that the experiment of 10 days was too short to measure the neutron bursts that Jones and Menlove had reported finding at Los Alamos. Since then there have been many rather heated exchanges, so it seems that history repeats itself and Believers and Sceptics cannot do joint experiments, desirable though this would be. Maybe this is another characteristic of Pathological Science that I should add to the present 18.

Have just heard that Nature has refused the Jones-Menlove paper.

3. Explanation of an Excess Heat Measurement

It had been suggested By Dick Garwin at Santa Fe that if the incoming current was measured by a DC device, then if there happened to be an oscillation, the AC current coming in would not be recorded. This would upset the heat balance and be recorded as an excess heat. A. Bruggeman et al. of the Nuclear Research Centre at Mol in Belgium at first found excess heat after two months. However the effect was observed in both D2O and H2O cells; also no neutrons were observed but a previously non-observed defect occurred in the gamma measurement circuit giving an ordered peak pattern in channels corresponding normally with energies from 4 to 8 MeV. The tritium yield increased by 65% which is a normal enrichment. It was clear that the "excess heat" was not due to nuclear reactions. It was shown that this "excess heat" could be reproduced by adding an AC current. Also it was shown that the circuit used earlier could oscillate. They are to be congratulated on their honest and full description of their work - alas too rare.

When I told Martin Fleischmann of this, he said that they check for this and it was not the explanation of the effects they observe.

4. Excess Heat from Minnesota

Prof. Oriani reported last year that he had observed large bursts of excess heat. The effects were erratic but could last as long as 10 hours. He was welcomed in Salt Lake City and given considerable media attention. When I phoned him in January he told me that after the accidental fire, he rebuilt his apparatus but had not been able to repeat his experiment. His name is not on the list of speakers at the First Annual Cold Fusion conference. Incidently he is the first person I have met who was at the actual seminar in 1953 at General Electric where Irving Langmuir gave his talk on Pathological Science - he said it was a great talk and it stuck in his memory.

5. Edward Teller Invents a New Particle.

At the NSF/EPRI meeting in Washington where only positive results were presented, Edward Teller suggested that it might be possible to explain some of the major contradictions by postulating a new particle with appropriate properties. He called it the "Meshugtron". He explained to me that he gave it that name as "Meshuga" means crazy in Hebrew. He does not believe the results suggest cold fusion (for he is an expert on the subject and knows one cannot simply ignore all the other experiments that have been performed, some of which he had himself proposed). However he enjoyed trying to invent a new particle for which he gave an appropriate name.

6. Fusion from Fracture of Crystals?

It has been shown that fusion should occur at vanishingly low rates in static conditions when deuterium is loaded into metals such as Deuterium. However it has been suggested that if a crystal fractures under stress(e.g. from the loading) then the deuteron ions might be accelerated by the transient high fields across the cracks to reach an energy high enough to cause fusion(would this be "hot" fusion?). Calculations at the Santa Fe meeting suggested that the numbers were not right for such an occurence However Menlove et al. claim to observed ions with TiD(0.8) and Klyuev et al claim (Sov. Tech. Phys. Lett. 12(1986) 551) to have detected neutrons from the fracture of single LiD crystals. Dr P. B. Price of Berkeley, Nature 343 (1990)542, reported that he had tried to repeat the experiment with LiD crystals and found no effect at 90% confidence. He then shows that in TiD2 and PdD2 this effect would be most unlikely.

7. Visit to BYU and the National Cold Fusion Institute

At BYU Steve Jones showed me his lab. They are doing some interesting work but it seemed on a surprisingly small scale - one would have expected that they would have been much better funded. One experiment is to look for neutrons (they are fortunate in having a really experienced neutron expert) from a titanium sample where the deuterium is loaded under pressure. Was surprised to find that their loading is very light with D/Ti only about 0.3. This is different from the philosophy elsewhere when one tries for the highest possible loading of deuterium.

The National Cold Fusion Institute has developed quickly and lots of good quality equipment is being installed. The people seemed reasonably free and open A first set of 32 cells had just been installed for a carefully planned series of tests to try and establish conditions and materials which would give reproducible effects. It was planned to start a second set of a further 32 cells for further tests. Unfortunately Stan Pons was occupied with a funding agency so that I could not see these series of tests. Incidently an advantage of Salt Lake City in winter is the proximity of Alta which is one of great centres of powder skiing with 12 1/2 metres of snow per year.

8. Solar Neutrinos and Cold Fusion

Particle Physics is in the strange situation just now of having a theory, called the Standard Model, SM, which works in the sense that almost every time one does an experiment it is in agreement with the SM. Yet one knows that the model must be wrong and expects that by going to higher energies, e.g. the SSC or LHC, new physics will be found. One of the few places where there is a disagreement is with neutrinos from the Sun. An experiment over the last 20 years by Davis et al. has given an average rate of 2.33 +/- 0.25 SNU which is much lower than the theoretical values of Bahcall of 7.9 +/- 0.8 SNU. A second major discrepancy is suggested by the variation of the neutrino flux with time which it has been suggested is inversely proportional to the number of sunspots. Several fascinating theoretical explanations for these two effects have been proposed. Recently the large Japanese neutrino detector, Kamiokande, (which had a major success in detecting neutrinos from Supernova 1987A) observed 4.2 +/- 0.7 SNU which agreed closely with the values obtained by Davis's much smaller experiment. As the sunspots are close to a maximum now, people are awaiting new results from Kamiokande. At a recent meeting Davis called out "Now is the time". However some members of the Kamiokande experiment want to close it down for a long period to install a Cold Fusion cell in its centre!

For what it is worth, in a recent lecture on Pathological Science, it was suggested that both results are probably Pathological. The belief of Bahcall that he can determine the flux of neutrinos from the centre of the sun to only 11% seems to show an excessive belief in his assumptions - and it is interesting to note that Turck-Chieze et al. calculate with almost the same input values, a value of 5.8 +/_ 1.3 SNU which is consistent with the experimental value of Kamiokande.

It is to be hoped that Kamiokande will continue to study this important question where it can make a unique contribution at the present time.

FINAL COMMENT

There has been much more happening since my November CF news but have been too busy with my normal work, however this is a not-unrepresentative sample. It will be very interesting to see if at the First Annual Cold Fusion confernce, new evidence will be presented, e.g. from the 64 cell experiment at NCFI, which will change peoples judgements.

Douglas R. O. Morrison.

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rom drom@vxcern.cern.ch Fri Apr 28 11:09:07 1995
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From: drom@vxcern.cern.ch
X-Vms-To: MINT::britz@kemi.aau.dk
Subject: Part 1 of review of 5th Intl. Conf. on cold fusion.
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COLD FUSION UPDATE No. 10

Part 1 of Review of the 5th International Cold Fusion Conference.

INTRODUCTION - SUMMARY

The Fifth International Cold Fusion Conference, ICCF5, was held in a luxury hotel in Monte Carlo, near Fleischmann and Pon's laboratory, from the 9th to 13th April. It was a remarkable meeting both from the scientific and sociological standpoint - this was the opinion of Martin Fleischmann.

There were relatively few new scientific papers claiming positive effects, and some stating that earlier effects could not be confirmed, e.g. Bressani. Some quite unusual claims were made, though many of the most obviously outlandish claims made at the previous conference, ICCF4, held on Maui in December 1993, had been avoided. This may have been achieved by controlling invitations as it appeared that unsuitable people were not sent invitations unlike previous conferences where those attending the previous meetings were invited - thus Steve Jones, Tom Droege, myself, etc. did not receive invitations this time. There was a discussion about censorship and recording. Some 200 people appeared on the Participants list, which seemed to be rather enhanced. It was not too clear who was running or paying for the conference - it was described only as a non-profit making organisation. No conference chairman was listed. The list of Sponsors is discussed below.

In 1989 there were many funding sources for cold fusion, but they have mainly dried up though there are still some which are, however special. MITI through its New Hydrogen Energy Agency, NHE, has spent over $7 million in two years but only two positive results of rather low statistical significance, were mentioned. The organisation, IMRA which depends on the Toyota car company, which is generously funding Fleischmann and Pon's new lab near Nice, did not report any major excess heat claim. The Electrical Power Research Institute, EPRI, was said by Tom Passell, to have spent over $10 million - it was implied but not clearly said, that they had stopped major funding of cold fusion. Other sources of funding appear to be small. Representatives from French agencies were present - to report they emphasised - but did not appear very impressed. Some American government employees who had been friendly to cold fusion, were also present. While there is some substantial funding, it may be noticed that if cold fusion had some hope, then very many more organisations would be expected to fund it, but they are not.

For the first time, the presence of media representatives was not announced.

For two years there have been circulating rumours of sensational results from Kevin Wolf at Texas A&M, finally Tom Passell presented a graph as a non-publication - the results are controversial.

Initially it was said by F&P and others that they knew they were observing fusion because the excess heat was found with deuterium but not with hydrogen, but now there are so many people claiming excess heat using normal light water, that this major contradiction is seldom mentioned now.

What was NOT said was most interesting. The NHE have been building exact replicas of F&P's original 1989 cells, but these tests and the results were not described. Also F&P have been working in the South of France for some 4 years now in the new well-equipped IMRA laboratory, but few hints of the work or results were given, nor was there a general invitation to visit this laboratory. Similarly people were astonished that Focardi et al. who held a econd press conference in Bologna last month to claim some steady 30 Watts for four months, did not turn up nor submit a paper (see section B4).

The theory sessions were hilarious. There are many theories of cold fusion which are inconsistent with one another and this caused some heated discussions - these involved four of the 5 most powerful voices at the conference - Drs. Chubb, Preparata, Li, and Vigier (in decrescendo). Then some well-meaning non-theorists tried to make tables of how the various theories tackled the major problems and what predictions each theory made - this caused difficulties for some, with Preparata and friends leaving before they could be asked to state what predictions their theory could made. Finally the organisers realised what was happening and abandoned the attempt. Prof. Preparata strongly attacked the theories of Prof. Li of Beijing. Later it was announced that next year's conference would not be held in Beijing as expected, but in Japan - for reasons of infrastructure, it was explained.

It would have been good if someone had presented a compilation of the experimental results, all the experimental results, not just the positive ones. One would expect a table giving for each experiment the results for deuterium, for hydrogen, for deuterium-hydrogen mixtures which Julian Schwinger emphasised should be better - and for each give the result for excess heat, and for production of p, n, t, 3He, 4He, gammas, and X-rays; in each case numbers and errors. This compilation could be used to answer some questions such as (1) does cold fusion occur in both hydrogen and deuterium in the same experiment? and what are the ratios of the products? (2) is it a surface or volume effect? (3) is there a threshold in loading? (4) is there a waiting time for effects to start?, etc. Instead the opening talk by Dr. Storms was a series of anecdotes of favourable results with no serious compilation. A graph of excess heat as a function of surface area was followed by a graph as a function of volume. The neutron flux was stated without evidence, to be 10 to 100 per second excluding bursts, but ignored many others results as such the 1989 claim of F&P of 40,000 per second and also the best experiment which was done by Kamiokande in Japan and which gave an upper limit of 10 E-4 neutrons per second.

To enhance the positive results, people who claimed to be producing excess energy by some other method, were invited and it was suggested that their claims were based on cold fusion. Thus there were talks from people who manufacture a new type of pump, discussions of sonoluminesence, and even a talk about the problems of keeping fish in tanks in Hong Kong - an interesting talk, perhaps one of the best of the conference.

These notes are meant to give the highlights and an overall impression of the meeting without boring the reader. So it is not a complete detailed account of every talk and poster - my apologies to those not mentioned.

Overall, a stranger arriving at the conference could well be impressed by the luxury, by the large well-organised and attractively printed book of abstracts (though it may have been noticed that the old trick was used of only printing on one side of the page to make it look bigger), and the reasonable lunches with unlimited wine (refused by most people from Utah). Yet there was a strange air of discouragement which was even stated by some speakers, for example saying we must find some younger people as they were conspicuously absent. There were many results, but each seemed to claim something different from the others, but few appeared to worry about the contradictions - it was considered another miracle of cold fusion which proved that the subject was new and exciting and funds were therefore needed to study it further. The lack of any single clear reliable result that all respected, plus the lack of any agreed theory, meant that it was not clear what path a True Believer should pursue. However there are still some sources of funds, especially from Japan, so there is a good chance that the next meeting will be held in Japan despite everything.

Should there be any corrections, comments or additions, would be pleased to be informed.

SUBJECTS

A7. Regionalization - Cold Fusion in France
A8. How Long will Cold Fusion Last?

B. SOME EXPERIMENTAL RESULTS ON COLD FUSION
B1. New Hydrogen Energy
B2. IMRA France
B3. Kevin Wolf
B4. Focardi et al. Bologna, Siena
B5. Bressani et al., Turin
B6. Celani et al.
B7. SRI, McKubre et al.
B8. Miles et al.
B9. KEK
B10. Patterson cell
B11. Matsumoto
B12. Notoya
B13. Russia, Sopogin et al.

C. EXPERIMENTAL RESULTS - UNCLEAR RELATION TO COLD FUSION
C1. Griggs. Hydrosonic Pump
C2. Sonoluminescence
C3. E-Quest
C4. Radioactive decay lifetime changed?
C5. Biology - Fish in Hong Kong

D THEORY

E. CONCLUSIONS

A1. SPONSORS OF CONFERENCE

The following were listed as sponsors giving financial support;

ENECO, Inc. (USA)
Aisin AW Co. Ltd. (Japan)
Aisin Seiko Co. Ltd. (Japan)
NTT (Japan)
Technova, Inc. (Japan)
AGA S.A. (France)
Cegelec S.A. (France)
Novolec S.A. (France)
Riber S.A. (France)
Setaram S.A. (France).

In addition five generous private donations were acknowledged.

It is interesting to note who are NOT sponsoring ICCF5 - these are EPRI and the Office of Naval Research, Arlington, Virginia, who were the only two sponsors named of the previous conference in Maui, ICCF4.

It was not clear if all the sponsors understood what they were sponsoring. In the hall outside the lecture hall were a few displays. One was a French company which made calorimeters. One of the representatives explained that their calorimeters were very accurate so they expected many cold fusion people would want to use them to confirm their excess heat. He then asked why I was laughing and I explained that the True Believers remaining after six years, did not really want strong independent checks of their results. Good scientists are always trying to prove themselves wrong; poor scientists who make an unexpected claim, vary their experiment only marginally, they say it is not their responsibility, they stick by their results, it is up to the questioner to prove them wrong. Later when the representative was dismantling their display, he said that they had not had any serious enquiries for calorimeters.

On the first day of ICCF3 in Nagoya, October 1992, two researchers of NTT, then the company with the highest share capitalisation in the world, announced they had achieved cold fusion reproducibly - and the shares value of NTT rose 8 billion dollars in one day, but then slipped back to normal within a few days. NTT offered to sell the apparatus but have not heard of any being sold except one to the Science ministry. As the researchers concerned then transferred, it is perhaps surprising that NTT are still supporting cold fusion.

ENECO is a company set up by Fred Jaeger which has bought up most of the cold fusion patents from F&P and almost everyone. In a talk, Fred explained that they aimed to cover all possibilities so that investors were sure to win. They have some investors but privately was told they have little capital and

.ave used much of it to send many people to this elegant and expensive conference in a five-star hotel - however no financial numbers were given so this must remain unconfirmed. Would be happy to correct this comment if sent the numbers.

A2. FUNDING OF COLD FUSION - MAJOR ORGANISATIONS

The largest amount of funding at present comes from an organisation set up by the Japanese government ministry MITI. Dr. N. Asami said that in 1993, they founded the New Hydrogen Energy agency, NHE (avoiding calling it cold fusion - just in case) with a budget of about $30 million over four years. He said that $2.5 million was spent in 1993 and $5.4 million in 1994. (Dr. Bush asked if he could apply for funds - no clear answer).

The Institute of Applied Energy set up two NHE labs, in Tokyo and in Sapporo. They work with the National Labs and with leading industrial companies. Nine universities and 11 groups are collaborating.

EPRI has been one of the main supporters of cold fusion which could well have died without its generous funding. In a talk at ICCF5, Tom Passell said that EPRI had given $10.6 million to SRI(formerly the Stanford Research Institute)and other institutes. It was impossible to get a straight answer to the question as to whether EPRI was to continue funding cold fusion, but it seems that it will not give any serious funding in the future. Thus it was said that SRI funding would now come from Japan. Looking up EPRI in the World Wide Web (invented in CERN), could find no mention of cold fusion in its list of accomplishments nor in its budget for 1995.

Tom made an interesting remark about the question of whether we reward researchers appropriately. At EPRI if you produce a solid result, you are fired; if you produce an ambiguous result, you're hired for 10 or 20 years. This might explain what happened to Steve Jones - when he produced irregular results supporting cold fusion, he was funded by EPRI; but when he showed that his results were artifacts and that many other people's results were mistaken, his funding stopped even though he was continuing research and producing results.

A3. FUNDING OF COLD FUSION - MAJOR ORGANISATIONS - BECHTEL

Information about industrial funding was scarce since most companies do not wish to talk about such matters, especially if there is some probability of loss.

From the MITI talks it is clear that some large Japanese companies are carrying out research together with MITI - it was not clear whether they do it cheerfully or reluctantly.

In Europe was told several times that the Italian automobile company, Fiat, is giving finance to the Bologna/Siena group of Focardi et al. (see section B4). Also another large Italian company is giving funding to cold fusion, plus a German automobile company - but these funds may be quite small. Shell Oil in France helped Jacques Dufour initially and is named as a co-sponsor of his present abstract (see section A7).

There had been rumours at ICCF4 that AMOCO had found excess heat, and at ICCF5, Dr. M. Eisner described how in 1989, when measuring the gradient of gravitational fields to find gas and oil, they used the gravitometer for a cold fusion experiment and found about 30% excess heat. However there were problems with reproducibility. No one at AMOCO would make a statement, but it seems that the company have not continued work on cold fusion since then.

Since IMRA depends on Technova which depends on Toyota, no funding arrangements were given though clearly millions have been spent in setting up a well-equipped lab in the Sophia Antipolis Science Park near Nice, and another lab in Hokkaido. No clear description of results on the use of F&P cells was given, though there was considerable technical discussions of how to treat palladium and to load deuterium into it. Of their two most important employees, Pons was ill with 'flu and was rarely glimpsed. Martin Fleischmann spoke four times - once briefly to open the conference; secondly a paper about positive feedback explaining that heating the system to boiling and talked of "Life after Death" which is the emission of excess heat from a cell for three hours after it has been boiled dry - it was rather obscurant mathematics ("the experimental protocols become part of the parameter space of the system"); thirdly was a talk entitled "The Experimenter's Regress", "a concept drawn from the field of sociology" which was Martin at his most charming. Agreed with him when he said that "When the dust has settled, the sociology of science will be the most important". He re-examined some of the raw Harwell data of David

Williams et al. and concluded that they had observed bursts of excess heat just as F&P had observed them. (From other sources learnt that considerable work has been done to analyse the Harwell data and a draft of a paper has been written). Fourthly he gave the concluding remarks. Overall for the last few years there has been a remarkable absence of evidence supporting or making reproducible the original work that F&P claimed to have done in the five and a half years before 23 March 1989.

A3.1 Bechtel Corporation

On the last morning Mr. Bruce Klein of the Bechtel Power Corporation was invited to speak. This is a major corporation based in California which is said to be so powerful that it can get elected a governor of California and a US President - who appointed some Bechtel people to his cabinet.

Mr. Klein looked like a perfect Californian business man and he gave a delightful talk explaining how cold fusion should be marketed and developed. He started by saying that he assumed that cold fusion was established. At the same time he mentioned the problems of cold fusion, lack of reproducibility and reliability, no working model, and the fact that the US patent office will not accept patents - this he did in a manner that had True Believers laughing. Overall a very pleasant and skilful performance which appeared to delight and encourage his audience. However he missed out two things. Firstly I commented that normally in considering a new technological project, the ROI or Return on Investment, is considered. For example, if cold fusion were successful, this might mean a profit of a billion dollars and if the probability of cold fusion working were one percent, then an investment of a million dollars would give a good Return on Investment. On the other hand the reason that more than 99% of scientists do not believe in cold fusion, is because of the barrier penetration problem, the deuterium ions are forced further apart in palladium than they are normally in deuterium gas, so that the probability of fusion is at least 10 E-50 lower than desired. So if the profit from an investment in cold fusion was even a trillion dollars, $10 E12, than an investment of one cent would give a factor of 10 E14 which is very much less than 10 E-50. Thus an investment of one cent would give a very bad Return on Investment. After the disagreements had died down, added that secondly, Mr. Klein had asked for a working model - but there was a working model described with photograph, in 1989, which was claimed to provide a family with hot water the year round and "simply put, in its current state it could provide boiling water for a cup of tea". This time people seemed to remember the photo of Prof. Pons and his water heater, and there were no protests.

In preparing this note, came across a brochure for CETI, the company that is promoting the Patterson power cell being displayed in operation at ICCF5. There it was written "'Based on seeing this device at work, I have confidence in promoting this technology within Bechtel' - Mr Bruce C. Klein, PE, Bechtel". Must admit this changed my opinion of the beautiful and skilful presentation of Mr. Klein. Do not know if Bechtel is providing substantial funding apart presumably from Mr. Klein's expenses.

A4. MEDIA INTEREST

F&P's first press conference on 23rd March 1989 attracted great media attention. At their first annual conference a year later, there were again many reporters from around the world. The second and third in Italy and Japan, attracted a good number of reporters but almost entirely from the host country. For the Fourth conference in Maui in December 1993, a media expert was hired specially to deal with the expected host of reporters, but it turned out that he was under-employed as there were only two - Jerry Bishop of the Wall Street Journal and a student from the local student newspaper. This time at the reception desk, was told there was no media specialist and Tom Passell said during his talk that there was no one, not even Jerry Bishop who had been so helpful in the past. Even in the local paper, the Nice Matin, did not find any mention. Does this mean that people were so pessimistic that they did not even try to raise media interest as they had previously?

A5. COLD FUSION MAGAZINES

Some profit from cold fusion is apparently being made by publishers of magazines.

Wayne Green who has a turbulent history, started publishing "Cold Fusion" which was a glossy magazine with Mallove and friends running it, but after a sho
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time there was a break-up with Wayne Green complaining that Mallove et al. were

taking too much of the income - they say otherwise. Now it continues but not so glossy. At ICCF5, Wayne Green gave out (free) copies of issue No. 9 - price $10.00. It says that the magazine has "an all-volunteer staff" - does this mean they are not paid? There are some articles of very non-standard theories; the most interesting article is Tom Droege's report on his visit to Griggs (see section C1). There also some experimental results including Dr. Matsumoto's (see section B11).

The Letters pages are most interesting, especially one reading "I was delighted to find someone interested in cold fusion. I have been interested in hydrogen since my neighbor tried to burn me up with heavy water. My wife's brother had hired the neighbor to break up our marriage even if it killed us. This was the last of Feb. 1974. Four days later the arsonist burnt down the LDS Stake House using hydrogen. As you may know, heavy water is a marvellous arson tool, the building is burned before the fire alarm can go off. Several years ago, I did the so-called 'cold fusion' experiment. I believe it was in '84 that the UFO came & indicated they would no longer help me with my experiment. They explained that I had not conjugated with a certain high school girl. I went to Las Vegas with the Ancient Astronauts Society and they indicated that I was back in favor and that I should write the truth about hydrogen. .... Please let me help you to get started making tritium heavy water. My brother-in-law stole $10,000 from me so I am broke".

Gene Mallove gave out copies of his glossy new magazine called "Infinite Energy" - the name caused some discussion and comment. The price is $5.95 per issue. The front page is a striking colour photograph of Roger Stringham of E-Quest (see section C3). It has quite a few adverts which should bring in the money though some full-page adverts are for the magazine, for Gene's 1991 book and another is to buy tapes made by Gene et al. of the "MIT Cold Fusion Day - January 21, 1995".

This first issue contains articles of variable quality. One is the talk by Nobel Laureate Julian Schwinger read in his absence last year at ICCF4, where he recounts the saga of his calculations and beliefs in cold fusion. It is entitled "Cold Fusion Theory; a Brief History of Mine". Initially he thought it was due not to d-d reactions, but to p-d reactions from the H2O contaminant in the D2O; later he thought sonoluminescence could be the explanation. He describes his problems of getting his papers published - the first was rejected impolitely by Physical Review Letters, so he resigned from the APS, but managed to publish it in a German journal. Finally he wrote 8 papers supporting cold fusion. His article begins "As Polonius might have said:' Neither a true-believer nor a disbeliever be'". There is also a report by Bruce Klein of the Bechtel Power Corp. on the Patterson cold fusion cell (see section B10).

The Letters were also varied. In one is intriguingly written "I've being working on a set of biographical vignettes entitled 'Scientists who were Shafted'".

The date and place of the next conference, ICCF6, is given as Spring/or Summer 1996 in Beijing, China.

Both of the magazines have letters from Arthur C. Clarke who is regularly supplied with literature by cold fusion advocates. Slightly discouraging, as after Arthur invited me to give a couple of lectures in Sri Lanka last year, I tried to give him a somewhat different view of cold fusion but he feels the weight of positive evidence should not be neglected though I tried to explain that a judgement should be based on the totality of evidence, both negative and positive, and not a subset. However he did write that "I am a little embarrassed to recall that exactly two years ago, I addressed a distinguished gathering of American, Russian and other naval staff officers (including the OIC, US Pacific fleet) on the subject of 'cold fusion' - and hinted that the breakthrough would be 'real soon now'".

A6. A SCIENTIFIC MEETING? CENSORSHIP?

Normally scientific meetings are open and people can speak and note as they wish. If a True Believer was asked if ICCF5 fulfilled these requirements, he would answer that it did. However there were worries. Previously the meeting was widely announced and everyone who went to a previous meeting received a personal invitation to the next. This time only True Believers and some selected neutral people appear to have been invited. People who might be regarded as Sceptics did not receive an invitation - thus Steve Jones was not invited although he was one of the original founders of cold fusion but who has now retracted his previous positive results and publishes experiments and criticisms of positive claims. Also Tom Droege, myself and others received no invitation. Was told it was advertised in Fusion Technology, the journal that has a strange refereeing process for cold fusion papers, though it is regarded as serious for hot fusion papers.

Also on arriving the ICCF5 Information Sheet contained a section on Recording Policy which says that for copyright reasons, "The filming, video-recording, tape recording, photographing, or reproduction in any other form of the proceedings, lectures, posters, or statements of the lecturers, or any other contents of the 5th International Conference on Cold Fusion is neither sanctioned nor permitted by the Association or the governing committees of this Conference." This sounds pretty tough but it is followed by saying that it can be done "with the express consent of the participant". "We suggest that the participant in question obtain appropriate assurance, in writing or otherwise, from the person undertaking such recording of the reasons for and the future use of any such recorded material."

Have never seen such rules in a scientific conference before. So did not use my video or tape recorder, but then found that Gene Mallove was recording extensively everything and Carol White of the magazine 21st Century (has had interviews with Lyndon Larouche from his prison cell e.g. about cold fusion) and Mike Melich were also recording freely. So on the final morning brought my tape recorder and when Martin Fleischmann rose to begin the final talk, asked him if he minded if I recorded him, adding that Mallove et al. seemed to be recording freely without having asked permission. Mallove said he had asked permission, but I replied that I had asked several speakers that morning and none had been asked for their agreement - Mallove responded that he intended to ask them afterwards. Martin said that he did not mind being recorded, but would insist that I let him see what I had written - it was only normal politeness. So wonder if Mallove, White and Melich will follow Martin's insistence on the special cold fusion conference practice of consulting people before publishing?

It may be noted that Gene Mallove is selling tapes of the MIT Cold Fusion Day - wonder if he intends to market his tapes of ICCF5?

After the meeting ended, one owner of a company wished me good-bye and said he would see me again in Sapporo in October 1996. Said I would be happy to see him then if I were invited: so suggested we go over and ask Martin if I would be invited this time. Martin answered that of course I would be invited.

On the other hand this conference resembled a scientific meeting in that, in general, there was no personal animosity, though the theory sessions were a little hot at times. Similarly, though I was not on the original invitation list, was well received by almost all, Martin Fleischmann being especially warm.

A7. REGIONALISATION OF RESULTS - COLD FUSION IN FRANCE

In May 1989 when I was trying to organise all the cold fusion results that I had collected, decided to group them by country for convenience. But was greatly astonished to find that some countries and regions had mainly positive results whereas other had mainly null results. This Regionalization of Results persisted with time except that some regions that at first had mainly positive results had switched and now had mainly null results. This is quite contrary to belief in the Universality of Science.

Later Dr. Scaramuzzi who started the great excitement over cold fusion in Italy, stated that Cold Fusion stops at the Alps. And indeed for long there were no positive results to the North - not in Germany, Britain, Switzerland, Austria, Belgium, the Netherlands, etc.

However in the summer of 1993, L'Express reported that supported by the theoretician, Prof. Vigier, Dr. Jacques Dufour had begun experiments at the Shell laboratories near Rouen and then moved to the labs of CNAM, the Conservatoire National des Arts et Metiers. Dr. Dufour was quoted as saying "I obtain out an energy double What I put in." However at ICCF4, J. Dufour, J. Foos and J.P. Millot of CNAM, Paris, did not report any positive results but proposed a theory involving three-body collisions and virtual polyneutron states such as (proton plus an electron) which would explain the otherwise impossible results of low amounts of tritium, neutrons and 3He also variable amounts of 4He, and transmuted nuclei. They said they were going to test this unusual theory by sparking in hydrogen isotopes.

At Monte Carlo Drs. Dufour and Foos were delighted to tell me that they had greatly improved and extended their apparatus and while they did have anomalous effects, they had made a major effort to measure nuclear products and concluded that they were definitely not observing cold fusion. This is how the scientific method should work - try and prove yourself wrong. In detail they wrote that they had observed excess power up to 7 Watts, both on H2 and D2, representing 25 to 30% of the incident energy. They had found very small amounts of 4He and 3He and of neutrons (twice background), no tritium , but copious emission of low energy (50 to 200 keV) gammas under certain conditions. They conclude that "part of the excess energy we have measured is not of nuclear origin and probably comes from the formation of tightly bound hydrogen atoms", e.g. as proposed by Vigier. Warned them that Tom Droege had pointed out that it is very difficult to know exactly what is happening with discharges. They were anxious to find an explanation of their anomalies, so suggested that Tom be invited to visit them as he has a remarkable wide range of expertise - they appeared to welcome this suggestion to help solve their mystery.

So it seemed that cold fusion did stop at the Alps, but then next day Dr. J-P Biberian of Marseille University, presented a paper claiming large amounts of excess heat using a perovskite, AlLaO3. The deuterated sample was heated to 400 - 600 C and a current applied. He wrote that excess heats of up to ten times the input energy were found and that neutrons and photons were detected. He expected that if the current were increased, then the excess heat would increase, but found it was not the case. In his talk he claimed that a deuterium density as great as that of liquid deuterium can be obtained, but seem to remember that the separation of deuterium nuclei in liquid deuterium is only the same as that of D2 gas, that is about 0.7 Angstroms, and this is far too large a separation to give cold fusion - it is off by a factor of about 10 E-40 from Steve Koonin and Mike Nauenberg's calculation(1).

When Dr. Biberian was asked if he had done a control with hydrogen instead of deuterium, he said he had not done so - amazing, six years after F&P's press conference.

So has cold fusion crossed the Alps, or has it slid round the edge via Monte Carlo to Marseille? We await the result of Dr. Biberian's urgent controls, for example
with light hydrogen. Will he save the purity of France?

A8. HOW LONG WILL COLD FUSION LAST?

Am often asked how long I think cold fusion will last - but what is new is that some True Believers asked me this question. A short answer could be slightly longer than the supply of money. But actually it is more complicated than that and we need historical examples. N-rays lasted only a few years as Wood exposed Blondlot's mistake in a devastating way. Allison rays and Mitogenic rays lasted for more than ten years because there was never any devastating disproof and because the originators did not disprove themselves or retract. Polywater(2) lasted for many years as it was supported strongly by a distinguished Russian scientist and a distinguished American scientist, but finally first one then the other realised they were wrong and both retracted. More recently the existence of a 17 keV neutrino(3) was claimed, disproved but the claimant then attacked the null results (as True Believers in cold fusion attack the null results from Harwell, Cal Tech, and from MIT) and this caused uncertainty until there was overwhelming evidence against the 17 keV neutrino. Then 3 of the 4 main believers, decided they must be wrong and went back to re-examine their own experiments, and did further experiments which proved themselves wrong. Their retraction then stopped the matter for almost everyone. Thus the time a wrong result persists, depends on whether the original proponents retract or not. Cold fusion started with Fleischmann and Pons closely followed by a much weaker effect claimed by Steve Jones. Now Steve has retracted. If Fleischmann and Pons also retract their excess heat results (they may already have retracted their original 1989 claims to have observed 4He, neutron and tritium - not too clear), then cold fusion would end quickly. But if they persist, then it could drag on several years.

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Address for correspondence;
Email; drom@vxcern.cern.ch CERN

morrison@vxprix.cern.ch
Fax; 41 22 767 90 75

CH-1211 Geneva 23
Switzerland

Date: *Thu, 13 Jul 2000 02:17:33 GMT*
From: Douglas.Morrison@cern.ch
Subject: Status of Cold Fusion and Report on ICCF–8.

11 July 2000 UPDATE 13 DM-00-03.

STATUS OF COLD FUSION
and
REPORT ON EIGHTH INTERNATIONAL COLD FUSION CONFERENCE

Douglas R. O. Morrison

SUBJECTS

ABSTRACT

True Believers in cold fusion still continue and held their eighth meeting in Italy in May 2000. They are slightly fewer and two of the original three discoverers no longer attend. Experiments continue and give remarkable results with abundant transmutations, biological considerations, and even the conditions for alchemy are presented - but criticisms and doubts are not expressed. Many complaints are made of shortage of funds, and the only remaining government sponsor appears to be ENEA. An exception to the shortage of funds is the remarkable BlackLight Power company started by a medical researcher, R. Mills, which has collected $22 million and plans an IPO on a stock market to raise more money. Mills has doubtful results and a strange new theory of a hydrogen atom with fractional electron orbits. Here it is estimated that a lower limit of the money given in large grants, for cold fusion is $100,000,000 - the total amount will be much higher.

This paper tries to explain why some enthusiasts continue despite the overwhelming evidence against cold fusion. The talks of the summary speakers are given almost in their own words. The conditions for doing good experiments are described and the importance of trying to prove yourself wrong is emphasised. The unusual theories proposed to

explain their results are presented. It is discussed if cold fusion will continue.

1. INTRODUCTION

Yes, cold fusion is not quite dead as almost all scientists assume. There is still a small group of True Believers who meet, discuss, and compare. Their eighth International Conference on Cold Fusion, ICCF-8, was held at Lerici, a beautiful small seaside resort close to La Spezia on the Gulf of Poets (Byron, Shelley etc.) from the 21 to 26 May 2000.

The meeting was well-organised by Antonella De Ninno and Francesco Scaramuzzi of the Italian Agency for New Technologies, Energy and the Environment, ENEA, laboratory at Frascati with the assistance of the nearby marine biology lab of ENEA. The main sponsor was ENEA. The other sponsors were the Italian Physical Society, the Italian National Council for Research, and the INFN.

A major personality missing was Giuliano Preparata who died at Easter after a long illness. He was a strong and forceful character who was not afraid of controversy - it was said that if he entered a blind alley, when he left it, the alley was twice as long and twice as wide. It was not said at the meeting, but I have been told that he was an excellent teacher at Milan and formed many good students. Carlo Rubbia, the Director of ENEA, kindly put him in charge of a strengthened laboratory in Frascati and he worked there intensively until the end, despite his illness. The ICCF-8 meeting was dedicated to him.

The yearly meetings have drifted into biannual ones. The attendance was down to 145 from the previous 200 to over 300, and several important figures did not attend. Stan Pons was again absent - since the collapse of his Japanese-financed laboratory in the South of France, he appears to have retreated to a farm nearby. Another of the original three "discoverers of cold fusion", Steve Jones has also stopped attending. Three of the International Advisory Committee members did not appear - T. Bressani, C. Sanchez-Lopez and F. Jaeger, as well as many other prominent personalities of previous years - they are listed under section 8 - Brief Topics. As usual, I was the only sceptic present and was generally well received though when four of us were talking one day, they started a discussion as to whether I was dangerous or not. Two said I was not, while Mallove, a spin doctor, who is more concerned about public relations than science, said I was.

In this status report, a description of the ICCF-8 meeting will also be given. The concluding talks plus discussion on the final morning will be repeated almost in full. These are of particular interest as I am often asked how these participants can continue to believe in cold fusion despite all the scientific evidence against - are they sincere in their activities? Everyone can make up their own minds reading these Concluding Talks - my opinion is that they are sincere to themselves and are True Believers in the non-ironical sense. But, with perhaps one exception, they are not critical of the extraordinary and contradictory results and theories presented at the conference.

2. CONCLUDING TALKS BY COLD FUSION SUPPORTERS

Here an attempt has been made to reproduce the actual talks without any commentary, only with some changes for readability. The first person is used and is the speaker (not this writer).

F. Scaramuzzi announced that there would be 5 prepared talks and then free discussion.

He said there were 26 papers presented and 50 posters, giving a total of 76 contributions. The number of people registered was 145. The four biggest nationalities were 41 Italians, 35 Americans, 22 Japanese and 12 Russians.

His own personal comments on the conference were;

FRANCESCO SCARAMUZZI said;

The conference had been rich in results, some of which were;

Evidence of strong correlations of excess heat and 4He by McKubre, Arata and Takahashi who under certain conditions found both. This showed the nuclear origin of cold fusion.

Evidence of transmutations was growing steadily and indicated the nuclear nature of the effect.

For palladium, there had been studies of the charging modes and mechanisms, e.g. the work of De Ninno et al., Celani et al., in Frascati.

There has been an important and definite trend in the cold fusion experimental results. A feature was the low dimensionality of the samples corresponding to the need for thin samples or powders. The cathodes were all small in size but that would have to change in the future, but at present, with bulk samples there were problems in charging with deuterium gas.

In the field of theory, and there were many of them, some were quantum effects. I am strongly pushed to think in terms of coherence. Since we cannot see individual reactions, it must be coherence. The theory of Profs. Preparata and Del Giudice seems consistent with the experiment of De Ninno et al. Cold fusion is just the beginning of coherence applications.

Where do we stand? The problem is frustrating; there is the scepticism of conventional scientists most of whom think that cold fusion is not Science and does not exist. We need much more confirmation of results, with papers refereed and published, but major journals do not accept cold fusion papers. Arata has published five papers. Fusion Technology with George Miley as editor, does help. There is an absence now of groups in Europe except in Italy and also there is one group in Paris, but none in Germany, none in Holland, none in Britain. One way of interpreting this, is that our success has made us too optimistic and this has promoted a negative reaction.

My personal conviction (not shared) is that cold fusion is mostly Science. Progress needs lots of work and it would be much faster if we had more funds. I have no doubt that it is nuclear energy. It would be foolish to make predictions of working machines before some 10 years, but then progress will be very fast.

In Japan there have been two very important events;

a) The IMRA/Toyota organisation worked for 6 to 8 years on CF

b) the government through MITI and NHE, worked for 4 years.

Both were lost two years ago. There were many reasons, apart from errors - they were set up for short term practical applications, and also the death of Mr. Toyoda, the Director of Toyota. But we have 22 papers from Japan mainly from professors at universities who worked with IMRA and MITI.

We meet only every two years now - we should stay in touch more frequently.

There is a simple message - the production of heat is real and is nuclear.

GEORGE MILEY said;

Now the great direction is reproducibility, then we will work on the basic science and finally on applications when we have government support and industrial company proposals. At the start we were too optimistic when we talked of "Electricity too cheap to meter".

Now we need basic science to go forward. Normally governments support basic science then the information is freely available for everyone. The propagation of information is basic to scientific exchange.

Theories - there have been improvements with the main classes of theories being (a) new particles, (b) various neutron groupings, (c) coherence, (d) photo-dissociation.

The challenge is that we need a benchmark experiment, then we can measure new phenomenon, new experiments, loading of gas, flux needed for reactions to occur. A question for theorists - what is the loading and flux required? - (loading is the amount of hydrogen gas that is loaded into the electrode, e.g. by electrolysis).

Experiments; reproducibility is needed for good science. Where are we? I do not believe that we are quite there yet. For calorimetry there has been a large effort with increasing accuracy but we need sufficient heat that accuracy is not important.

For particle detectors, old techniques such as CR-39 etching plates and photographic films, are returning. It could be argued that there must be better ways as techniques advance, to measure loadings. Scanning electron microscopes are now used.

At the first cold fusion meeting and at workshops, it was said that we needed experiments that measured both heat and particles emitted, that is, integral experiments that measured many quantities simultaneously. This is still a key challenge as most experiments still tend to measure one quantity at a time.

Experiments need diagnostics - more and better, specifically designed for the experiment. Improved charged particle detectors which are so small that they can go inside the cell.

Reproductivity - did not hear one paper that is fully reproducible. We are still trying to reproduce the Pons and Fleischmann work. The field contains d-d reactions, protons, neutrons, tritium, LENR, applications.

There seem to be regimes where phenomenon do occur - the problem is how to go from one regime to another. Heavy and light water each have a regime.

For practical applications, we need not just Watts but gain. Inertial Confined Fusion needs a gain of a 100; here we need a gain of 5 times. But at present our results give gains of 10, 20, 30%. If the power input is 1 Watt and the output 1.2 Watts, then we need to re-circulate or it will not sell.

Am fascinated by the papers on biological transmutations - we see so many phenomenon which do not involve lattices.

A. ROUSSETSKI said;

In Russia there are some 20 groups working on cold fusion. There is some small amount of funds from state support, universities, the Academy of Sciences, Russian Physical Society, Russian Chemical Society and Russian Nuclear Society. Financial support comes from commercial firms - they enabled over half of us to travel here. We wish to thank the organisers for support and accommodation.

In October 2000, there will be a conference in Russia, near Sochi on the Black Sea, to discuss cold fusion transmutations - this year will be the eighth. There are meetings in Russian universities every month.

JEAN-PAUL BIBERIAN said;

In 1989, there were two questions; is cold fusion reproducible? and are there any cold fusion applications? If there are applications, then we can believe. If Christ comes back flying in the air, then people would believe. I think we solved point one.

When you have artists in the family, some wait for a prince to arrive and recognise you - it is the Cinderella syndrome. Instead you have to go out and be recognised.

One has to go and talk about cold fusion to the American Physical Society, the American Chemical Society, and the American Nuclear Society.

Enemies of cold fusion are not active when they retire, but friends of cold fusion are active after they retire, so eventually with time, cold fusion will win in the end!

There is a new generation coming who are more open-minded and who are against nuclear power.

It is 11 years since Pons and Fleischmann announced cold fusion - a solar cycle - it is time to start again.

George Miley will retire in a few months from the editorship of Fusion Technology - this may give a problem in getting published. Our other publishing help is J-P Vigier who is an editor of Physics Letters A, but have heard little of him recently. These are the two sources of entry to publishing cold fusion, but they may be lost soon. We should start our own journal with referees. An electronic journal which would be cheaper, faster and better - too good to be true. A journal on the internet - I will be busy on this - let's make it the next gateway for cold fusion. Then when one types "cold fusion" one will find lots of companies selling software with internet security.

We need to write papers on cold fusion to Nature, Science, Physical Review Letters - and if they are rejected we need to ask why - the editor needs to have a good reason .

In Italy there is an official programme funded by the government - small but it could be a start.

A. TAKAHASHI said;

To summarise the key facts;

a) Almost confirmed;
   4He in correlation with excess heat - McKubre, Isobe, De Ninno, ..
b) Nearly confirmed;
   Transmutations - Warner, Iwamura, Mizuno, Miley, De Ninno, ...
   Photofusion - Takahashi
c) In progress;
   Cold fusion theory - Hagelstein, Chubb, Del Giudice, Violante, Hora, .
d) Heat
   Large excess heat and products of glow discharge - Mizuno,
   New Hydrogen Energy, NHE, was wrong - Miles, Fleischmann. (I have no responsibility to answer - perhaps someone in Tokyo)
e) Neutrons and charged particles - Kasagi, Lipson, Karabut, Isobe, Wang ,
f) Loading with deuterons in thin wire - Coehn-Ahansohov effect now called the "Preparata effect" in memory of him.

The ICCF meetings will continue in the 21st century - the second phase, as Fleischmann says.

Noted that the attendance at ICCF meetings is decreasing with time. To attract more people, science is not enough, we need industry people. Also young people who do not reject "crazy" ideas.

ED STORMS said;

We need reproducibility and we need a theory - both at the same time. Initially Palladium was chosen as a base for heat experiments, but palladium is one of the most irreproducible metals known to man - it was a poor choice. Others such as Pt, Au, Ni, or Ti are better and also absorb large amounts of hydrogen.

There is a problem that many theories of cold fusion prefer other materials. Suggest that any theory based on palladium alone must be wrong from the experimental results. Any theory of cold fusion must work for all materials.

There should be a Web site which contains all information on the field, including accounts from this conference. People are asked to contribute.

We need to exchange samples of materials, especially if they work. This despite priority claims - does it matter if we make only $50,000,000 instead of $1,000,000,000? Have found that if people discover how a material works, they stop talking and do not tell you.

The Web site would be called;

www.alteng.org

R.A. MONTI said;

This meeting considers only one aspect of a large field called Low Energy Nuclear Reactions, LENR, which began in France in the 1960's. Even the name of cold fusion is not new. It is a large field that you are starting to discover. It started in 1938 when a biologist, Kervoran, discovered fusion. The term cold fusion is not appropriate. There are important biological and geological effects - this is a wide field and "cold fusion" is inappropriate.

F. SCARAMUZZI said

Fusion is the historical name used by Fleischmann and Pons. As thermonuclear fusion was used for hot fusion, cold fusion is the best name.

EUGENE MALLOVE said;

I very much agree with Jean-Paul Biberian, we have a Cinderella complex.
Agree that there were more attendees at ICCF-7.
We must have demonstration units. The single missing unit is a

reproducible cell. It should be widely available, even if it is from CETI. As a media person, we must have 10, 100, 1000 of them working. We do not want the Cinderella part.

While we should perfect basic science, we need illustrations which would give the whiff of money.

After the brunch, I will show the video of my book "Fire from Ice".

The famous author, Charles Beaudette's book is now available and can be purchased now - I do not want to take copies back to the States.

ROBERTO ANDREANI said;

I am responsible for a large lab of ENEA for thermonuclear fusion, hot fusion, and have the responsibility of getting ITER built. I will soon go to Garching near Munich for this.

In 1989 in Italy, I was working on fusion but we had to look at possibilities. Only heard of science problems - when one talked of cold fusion, it was not popular.

For the most common fusion objectives, one needs the production of neutrons. Best reaction is proton-Boron which gives off no neutrons.

Please accept my opinion. We work in extreme scientific ways. The Cinderella complex can be overcome by good Science.

It was said "almost confirmed" - I will tell my colleagues. The measurements must be absolutely right. It must be possible to do in one lab and repeat in another lab, a perfectly comparable experiment. I have been impressed by the strong will of the people here.

In hot fusion we have to fight strong opponents for reasons that I find debatable.

I wish you good success .

You have a strong opposition, therefore you must do experiments that are beyond doubt.

FRANCESCO SCARAMUZZI said;

Agree that we must be more positive. Accept the point that we must exchange work and make checks. The reason that it is not done, is not that we are unwilling, but lack of money.

JOHN FISHER said;

About theories - there are many with lots of arguing. It is not necessary that a theory is correct - after all, Columbus thought he was going to India. Pons and Fleischmann had a theory, and had the energy and courage to test it - their theory was useful. Without them, how long would it be before anyone else tried - a decade? a century?

I think most theories are wrong, but all are useful.

It's tough being a theorist, referees are down on you, but we press on.

TALBOT CHUBB said

There are already examples of transfers - of 4He by SRI and now of 3He.

JEAN-PAUL BIBERIAN said;

Biological transmutation is important. I have done experiments on it with sprouting seeds. There is also the production of iron and other metals. In 1799, it was discovered at the Vaudin (?) street in Paris by a very famous scientist

We have booked a First International Workshop on Biological Transmutation in Geneva. We have no funding and a non-perfect organisation. We will open a web site.

The 21st century will be very exciting!

LI XING-ZHONG said

Ten years ago, I was a visiting scientist for fusion in Austin and had a telephone call from Prof. Scaramuzzi asking me to be a member of the International Advisory Committee for a cold fusion meeting - realised that I would be a hot potato.

Now ICCF-9 will have as its main themes;

- In theory - coherence
- In solids - reproducibility - most important
- In research and development - NHE? heater? based on "heat after death"
- Cold and hot fusion should merge.

What we want is green fusion energy without nuclear energy.

Note that the above five Concluding Talks plus comments, were given by Believers in cold fusion.

3. FUNDING

3.1 GENERAL - ESTIMATE OF MINIMUM TOTAL FUNDING

True Believers in cold fusion frequently say that they would have succeeded in demonstrating it's existence if only they had been given adequate funding. Often they are convinced that there exists a plot by oil companies, the scientific establishment and others to destroy cold fusion to safeguard their own interests. We will recall some of the facts and budget estimates to see whether the total funding was adequate or not.

The first major supplier of funds was the State of Utah who gave $5,000,000 to the National Cold Fusion Institute, NCFI, which was set up in Salt Lake City with Stan Pons working full-time and Martin Fleischmann as adviser. Despite these advantages, Pons left unannounced and it was hinted that he was in the South of France. Then NCFI collapsed.

The UK research establishment, Harwell, were informed by Fleischmann before the Utah press conference on 23 March 1989, and made a major effort with a multidisciplinary team headed by an electrochemist who was a friend of Fleischmann. They could not repeat the P&F results using what they believed to be identical cells, and when they used the best technology, they found no excess heat nor emitted articles. These experiments cost a half million pounds and used four million pounds worth of equipment.

The Electrical Power Research Institute, EPRI, which has a budget of some $600 million per year, and which represents the industry, has spent some $10,000,000 on cold fusion of which the largest part, over $6,000,000, went to McKubre's group at the SRI. They stopped funding a few years ago.

Mr. Toyoda, the President of the Toyota car company was enthusiastic as he considered that oil would not last for ever, and the company should search for substitute energy sources. Some $40,000,000 was spent over eight years. Two parallel laboratories were set up by the Toyota research company, IMRA, one in Japan and the other in the South of France in the Sophia Antipolis Science Park near Nice. The French lab had the advantage of Pons full-time and Fleischmann as consultant. Security was exceedingly tight and hardly anyone visited the lab despite the Fifth ICCF conference being held nearby in Monte Carlo. At the ICCF-6 meeting held in Hokkaido, the French lab reported small heat excesses but the lab in Japan reported no excess heat. When asked the reason for the inconsistent results, no answer was given.

The ICCF-3 meeting was held in Nagoya in 1993 and on the first day there was an announcement by Nippon Telegraph and Telephone company, NTT, that they had solved the problem of reproducible cold fusion (the shares of the company rose by $8 billion that day but quickly returned to the long term trend). The Japanese Ministry of Trade and Industry, MITI, announced the setting up of a national laboratory in Hokkaido where government workers plus workers from some 20 major Japanese companies and from universities, would do research. The laboratory was very well-funded and equipped. The organisation was called New Hydrogen Energy, NHE, thus avoiding the words "cold fusion". It was said that $30,000,000 would be invested but after finding no evidence for cold fusion, (presented at the ICCF-6 meeting in Hokkaido), the NHE was terminated after four years and the loss was declared to be $20,000,000. However this loss was probably only the government loss - it is not known how much the companies invested.

The French government Commission de l'Energie Atomique, CEA, supported a relatively small effort at Grenoble but at Lerici it was learnt that this has been terminated and no results were presented at Lerici.

The US Naval Weapons Research Lab. at China Lake has supported cold fusion strongly and has done many experiments but is said to have stopped activities in 1995. The amount spent on cold fusion is unknown.

The Italian National Alternative Energy, ENEA, has supported the Frascati laboratory of Prof. Scaramuzzi since 1989. Its new Director, Carlo Rubbia provided a new laboratory at Frascati for Prof. Preparata.

Many industrial companies have invested heavily in cold fusion but never disclose how much was spent. Still more companies have invested smaller sums to keep a watching brief.

The countries involved are not widespread but are concentrated in just five nations - USA, Japan, Italy, China and France

It is impossible to give a reasonable estimate of the total amount of money that has been devoted to cold fusion, but a lower limit would be a hundred million dollars.

At present the only official support appears to be from ENEA.

On the other hand, the company, ENECO, which was set up to collect all the patents from the original cold fusioneers - Fleischmann, Pons, et al. - and to raise funds for further cold fusion research and development - appears not to be active. At the previous meeting in Vancouver in 1998, F. Jaeger of ENECO,was skilfully busy with potential investors and provided funding for many activities, but he was not at Lerici this time. There seemed to be fewer investors now.

At ICCF-6 meeting in 1996 in Hokkaido, a special session was scheduled then cancelled, for the Clean Energy Technology company, CETI, with Dr. Patterson and C. Redding as promoters and with Miley and Claytor offering supporting results. But at ICCF-8, the promoters were in attendance, but were not presenting any results - they said they were waiting.

One company that has been successful in raising money is Dr. Mills and his BlackLight Power Company - see below.

3.2. BLACKLIGHT POWER - A FINANCIAL SUCCESS STORY?

3.2.1 GENERAL - HISTORY, HYDRINOS

Back in early 90's, Dr. Randell L. Mills was associated with cold fusion groups and at a press conference at Lancaster, Pa, he announced [1] that he had performed a thousand experiments obtaining heat out which was 40 times that of heat in. He used nickel in an aqueous solution of KCO3, and he believed the heat was chemical not nuclear. Patents had been applied for. He was a medical researcher with a medical degree from Harvard. He then developed a new quantum theory which he published in a book entitled "The Grand Unified Theory of Classical Quantum Mechanics", 1048 pages, $80. The essence of this theory is that while the hydrogen atom has its known energy levels with $n = 1, 2, 3, \ldots$ etc., Dr. Mills believed that the apparent ground state was not the true ground state but there were another series with $n = 1/2, 1/3, 1/4, \ldots$ etc. so that the electrons could drop to these lower orbits thus releasing energy. And this energy released would be "in excess of the energy required to start the process".

Mills then started a company called BlackLightSM TM Power, Inc. This company "has raised over $22 million in equity capital since its inception. Its current assets of plant and equipment are worth in excess of this amount, and the company also has over $9 million in cash." This would seem to indicate clear financial success. "BlackLight has purchased a new corporate headquarters and chemical R&D facility near Princeton, New Jersey. This 53,000 square foot building, located on 11 acres for expansion should allow the company to grow." Currently, the Company has 23 full-time employees, the majority of whom are scientists, including 8 Ph.D.'s. The company is looking to employ 75 scientists and technicians as well as 25 management and support staff within the next 1 - 2 years."

Further quotations are given below from its web pages;

http://www.blacklightpower.com

Also the company "believes it has developed a new hydrogen chemical process that generates power, plasma, and a vast class of new compositions of matter." "The lower-energy atomic hydrogen product of the BlackLight Power Process reacts with an electron to form a hybrid ion which further reacts with elements other than hydrogen to form novel compounds, hydrino hydride compounds (HHCs), which are proprietary to the Company." "if the Company's data proves correct, the novel compositions of matter and associated technologies have far-reaching applications in many industries including chemical, electronics, computer, military, energy, and aerospace in the form of products such as batteries, propellants, solid fuels, munitions, surface coatings, structural materials, and chemical processes."

"The Company has developed hydrogen gas energy cells that operate at temperatures in excess of 1200 degrees Fahrenheit, produce energy in excess of 1,000 times that of known chemical reactions of hydrogen, and achieve power densities similar to those of many electrical power plants (approximately 100 mW/cm3)."

Please note that the words "cold fusion" do not appear despite Mills' impressive 1991 claims of forty times more heat out than in.

3.2.2. PATENTS, POSSIBLE IPO

"The United States Patent and Trademark Office issued Patent #6,024,935 on February 15, 2000 with 499 claims which broadly covers the Company's advanced gas energy cell. The Company has paid the issuance fee for six additional US patents."

"The Company recently executed a two year agreement with Morgan Stanley to serve as the Company's investment banking firm. The Company's goal is to be public within the next two years." There has been discussion among critics of cold fusion, as to what would happen if the Company were to attempt an Initial Public Offering, IPO, as then SEC rules would apply.

3.2.3. VERIFICATIONS

"The Company's plasma, power, and chemical technologies have been confirmed by 26 types of tests at over 25 independent laboratories as summarised in Table 1 'Summary of Independent Tests'." This looks pretty impressive, however on the Sci.Physics. fusion pages people like Dieter Britz have been trying to verify these claimed confirmations, and have found it difficult as frequently only the name of the institution is given and not the name of the person or the year.

On the Web pages, a prominent heading is Astrophysics - this states; "The detection of atomic hydrogen in fractional quantum energy levels below the traditional "ground" state - hydrinos - is reported by the assignment of certain lines obtained by the far-infrared absolute spectrometer (FIRAS) on the Cosmic Background Explorer." So I asked a friend who is a senior member of the Cosmic Background Explorer, COBE, experiment about this. He wrote;

"Their claim about
COBE FIRAS is off-base. There are lines in the FIRAS spectrum from all along the Galactic plane. When I looked at them, they could be all explained as CO, C, etc. known emission lines. Note that the energy levels in the range 1 - 90 cm^-1 for the frequency (0.01 - 1 cm wavelength) is quite low and correspond mostly to rotational levels. BLP explains this with spin-nuclear hyperfine levels for the hydrino atom. Most of these lines have to be there from interstellar molecules and represent the major cooling for interstellar clouds." In other words, there is no need for hydrinos.

3.2.4. LEGAL LETTERS

Bob Park who has just published a book "Voodoo Science", has written in his Whatsnew articles that many prominent physicists, including Nobel laureates, who had criticised Mills' hydrino theory and claims, had received lawyers' letters asking them to "stop engaging in further defamatory and disparaging activities concerning BlackLight and Dr. Mills."

This is very reminiscent of the letter from Pons' lawyer, C. Gary Triggs to Mike Salamon, after Mike had done an experiment in Pons' lab and published in Nature that there was no evidence for gamma or neutron emission. The letter said "Please be advised that any damages suffered by my clients proximately caused by any act or omission on the part of yourself or any other coauthor of the subject paper will not be tolerated. I have been instructed by my clients to take such legal action as is deemed appropriate to protect their interests in this matter." Frank Close was also honoured by an unpleasant letter from Triggs.

These affairs caused Nature to show [2] a cartoon with the caption "A single hydrino can produce enough energy to keep an expensive law firm running for a year".

3.2.5 FUNDAMENTAL WORRIES

Many objections have been made against Mills' hypothesis. In particular for it to be true, then an incredible number of experiments have to be wrong - in particular, scattering experiments with electrons, for example, should have shown the new spectral lines caused by the fractional orbits.

4. EXPERIMENTAL RESULTS AND TECHNIQUES

4.1. REGIONALISATION OF RESULTS

At the APS meeting in May 1989, I reported that the positive and null results lay in separate parts of the globe. This regionalisation has become more marked with time. Scaramuzzi said in 1992 at Nagoya that "Cold fusion stops at the Alps." Since then it slid round them and the CEA lab in Grenoble reported excess heat but this has now stopped. Also Jacques Dufour in CNAM in Paris claimed excess heat but when I visited him, I was very worried that his apparatus was at different temperatures in different rooms and had poor insulation, but he has now changed it in some way. In Spain there was one episode of neutrons probably due to a faulty BF3 counter - they are most unreliable. So now Western European cold fusion effects are only in Italy and in Paris. They are also claimed by a few groups in the USA and more groups in Russia, Japan and China, but not apparently in other countries.

4.2. CALORIMETRY

The Japanese government made a major effort to study cold fusion and set up the New Hydrogen Energy, NHE, organisation with a budget of $30 million over 4 years. The lab combined government and industry. A series of careful experiments were carried out to repeat the Fleischmann and Pons experiments with them as advisors, plus new experiments. They found no excess heat or any other anomalous effects. The NHE lab was closed down and it was said $20 million had been spent

Melvin Miles of the Naval Warfare Research labs at China Lake went there and tried to repeat some of the experiments while Fleischmann studied the calculations used.

After Miles spoke, Ed Storms stated that from his experiments, it was unsafe to use a heater pulse to calibrate the cells. Fleischmann said "Ed, you are wrong" and that he would explain next day. But next day, his explanation was all about mathematical calculations and did not answer Ed.

Miles said that the NHE people had stated that the errors were (+/-200) mW while he claimed that the errors were only (+/- 20) mW. This was already said at ICCF-6 in Hokkaido when the NHE people noted that the fluctuations claimed as excess heat by Fleischmann were all within their errors. Further they stated that the distribution of fluctuations gave a perfect Gaussian distribution with three standard deviation limits of +/- 2.3% with no indication of excess heat occurring spasmodically

Morrison said that at Provo in 1990 and at ICCF-3 in 1992, he had tried to define the conditions for obtaining results that would convince sceptics. The two major requirements were;

1. Do good experiments
2. Try to prove yourself wrong.

An example of how not to do it, is the excess heat claims [3] of Focardi et al. Basically they heat a nickel wire to 500 C, measure the temperature with vacuum and with hydrogen gas around the wire and deduce excess heat which they say comes from the hydrogen entering the nickel and fusing. They were soon sponsored by industry. They have very few temperature measurements. The basic question is "is it fusion or a failure to understand heat transfer?" Hydrogen is the second best heat conductor. I suggested several years ago that a simple way to resolve the question, would be to use helium instead of hydrogen since helium does not fuse but does conduct heat almost as well as hydrogen. They have not done this test despite letters, Faxes, phone calls and direct discussion. This experiment has been repeated carefully by another Bologna group [3a] who could not reproduce the claimed results of Focardi et al.

To do good calorimetry, one should use a null measurement like the Wheatstone bridge - that is, the apparatus should be completely isolated from the outside by surrounding it with a bath of water kept at a fixed temperature by an electrical heater. If the cell gives out excess heat, this will tend to raise the temperature of the bath and to compensate, the electrical heating is reduced - this measures the excess heat. And there is no interchange with the surroundings. Of all the experiments described here, except one, the apparatus could be affected by the room temperature. For example, McKubre's poster describes the importance of the mathematical model used to separate off room temperature fluctuations.

It might be thought that room temperature fluctuations could not be important, but usually the palladium piece used is extremely small, e.g. 0.04 cm3 for F&P. Naturally Fleischmann objected while Storms declared that he had not said it was impossible to use a heater pulse to calibrate, but one had to be very, very careful.

The essential point is that experimentalists should do experiments instead of trying to obfuscate with mathematical models using non-linear regression analysis with Kalman filtering (at ICCF-3 in Nagoya, when I asked all people who have found excess heat, if they also had used a non-linear regression analysis, no one put up their hand).

The second point is that scientists, when they have a result, generally do not rush into print or press conference, but first try to find any mistake and check that if their result is correct, what would be the consequences, and to do checks on these consequences. For example, if it works for a very small volume, 25. 10^-6 cm3 for George Miley, then they should worry that a small effect could change the result and so they should repeat with a bigger piece - one gram, 10 grams, etc.

One worry about P&F's 1989 claims, was that the hydrogen and oxygen produced at the electrodes were recombining inside the cell which would give apparent excess heat in the cell - this was a worry as the electrodes were so close together in the tiny cell. They claimed that their calculations had shown that there was no recombination - but they did not do any simple experiments to demonstrate this. But after Steve Jones realised that there was no cold fusion, his colleague in Provo, Lee Hansen, did experiments. Firstly, he moved the electrodes apart and the excess heat decreased to zero. Secondly, with the electrodes in the close P&F position, he obtained excess heat but then as he blew in nitrogen gas between the electrodes, the excess heat ceased. Now why have P&F not done these simple experiments instead of doing calculations based on doubtful assumptions? And others who claim excess heat - have they seriously tried to prove themselves wrong?

It should be noted that groups which have made null measurements using a calorimeter with external water bath kept at a constant temperature, have found no excess heat and no particle emission.

4.3 NEUTRONS

On the 23 March 1989, the observation of neutrons formed the best experimental evidence that a nuclear reaction was taking place and justified the name "cold fusion". Jones claimed only neutrons. Pons and Fleischmann, P&F, showed a very impressive peak of gammas from the absorption of fast neutrons by protons - unfortunately the peak was at 2.5 MeV which agreed with their calculations. However at Harwell on the 28 March, it was pointed out to Fleischmann that the neutrons have to slow down first and therefore the peak should be at the well-known value of 2.2 MeV. Within two days this peak at 2.5 MeV had moved to 2.2 MeV. Later the neutron claim was said to have been a "mistake". However in 1991, P&F claimed that they were observing 5 to 50 neutrons per second. But now this is forgotten.

Many claims have been made for neutron emissions. Thus in the Gran Sasso tunnel, Italian-American groups have claimed signals but these appear to be from the radon background. The best experiment was in Kamiokande where Steve Jones and Howard Menlove inserted many cells in this huge 3,000 ton detector used for neutrinos - they claimed success but finally it is agreed that no significant neutrons were observed.

Tullio Bressani and his group claimed [4,5] to have observed a neutron peak at 2.45 MeV which they wrote was significant at the five standard deviation level but when he gave the review talk on this subject at ICCF-6, he omitted to mention his own result. This may be because earlier I had long discussions with him pointing out that there was no peak at 2.45 MeV but a very broad enhancement and the increase from 3 to 7 MeV was even more significant, but was only background.

At ICCF-8, Lipson et al. made a similar claim to have observed a peak at 2.45 MeV, but when we discussed, his graph was the same shape as the Bressani graph, i.e. a broad excess from 2 to 7 MeV. In both cases the graph was obtained by subtracting one distribution from another and both have much higher statistics with a very high peak near zero - always an error-prone procedure.

Many experiments have searched for neutrons and found none while a few have found very low numbers.

The overall conclusion is that the balance of evidence shows there is no emission of neutrons from any experiment of the cold fusion type.

4.4. CHARGED PARTICLES AND GAMMAS - GOOD SCIENCE - TRY TO PROVE YOURSELF WRONG

At ICCF-8, there were very few results on the emission of nuclear ash. Claims of neutron emission are discussed above. Helium and other claims are presented in the first section in the words of the concluding speakers. It was noted that copious X-ray emission is expected of 21 keV X-rays which are characteristic of palladium - but none have ever been reported.

There are some reports of observations of particle emission from groups employing glow or spark discharges. This is natural as it is not cold fusion but lukewarm fusion as the fluctuations in the discharges can give sufficient energy to the deuterons to cause fusion as even a few keV can cause fusion as discussed in section 4.6. Note, it is not the average which should be taken but the highest energy since the cross section rises extremely steeply with energy. Incidently, someone was heard to say that one does not need a complicated system as he found that an ordinary car spark plug does perfectly well.

At the cold fusion meeting in Provo in 1990, people told me they were happy when I said that to convince others, it was essential to do Good Science.

In Morrison's 1993 paper[6] "Review of Progress in Cold Fusion", there is a major chapter "Do Good Experiments" where there is a detailed discussion for calorimetry, particle detection, etc. It was emphasised that if one does find a positive result, it is essential to design further experiments to try and prove oneself wrong - this is what normal scientists always do. For example, if an excess heat is found with a tiny 0.04 grams piece of palladium, one would have expected Fleischmann and Pons to repeat their experiments with 0.4g, then 4g, and then 40 grams to check that the excess heat scales with mass, for one could suspect that there was a small error which looks like an enormous number of watts per gram, but with 4 grams of palladium, this small error would give negligible excess heat - but this is not done. Some claim that they must use thin films so their mass is 10^-3 grams, but they do not enlarge all their apparatus to attain even one gram.

There is an uncomfortable feeling that people do not want to check or to prove themselves wrong.

In 1990, McKubre of SRI, agreed strongly with me and said that SRI would now do experiments measuring many species of particles and gammas as well as excess heat. In an account of ICCF-3 in 1992, it was written "Mike McKubre said that the 3C's of cold fusion were Collaboration, Co-operation and Correlation. After three and a half years, there was no excuse for working on a single variable. All of experiments should be addressed and a correlation matrix established, The Harwell work which gave a null result, had correlations, we can similarly get information." Harwell did 127 varieties of experiment, and searched for excess heat, neutrons, gammas and tritons, but did not find any significant signal of them. But in the year 2000, SRI only reports on excess heat and helium - but it is well-known that 4He is very accident-prone ever since 1924 when Paneth and Peters found that they had wrongly claimed helium production from hydrogen, so that every 4He result is criticised. Why did SRI who received generous funding, not measure also some other products where the signature is unequivocal?

Back in 1990, Julian Schwinger pointed out[7] that p-d fusion is much more likely than d-d fusion. It was suggested that the ratio of H2O to D2O be varied from (100%/0%), to (90%/10%), to (50%/50%), to (10%/90%) and finally pure D20. But Schwinger's suggestion has never been tried by True Believers. However Ettore Fiorini of Milan, whom some consider one of the most complete and careful experimentalists in Italy, studied both d-d and p-d fusion during electrolysis with a palladium electrode. Also mechanical straining was added to search for fracto-fusion. No excess heat was found. Also gammas, neutrons, helium, and tritium were searched for, but none were found - this in a lab with a very low radioactive background. Now if Ettore can do such an extensive series of experiments with limited resources, why has SRI not been able to do similar experiments considering that they have been well funded having received over $6 million from EPRI?

In Dick Feynman's famous Commencement Address at Caltech in 1974, called the "Cargo Cult Science" lecture, he says "we really ought to look into theories that don't work, and science that isn't science". He finishes with advice to the new students;

"So I have just one wish for you - the good luck to be somewhere where you are free to maintain the kind of integrity I have described, and where you do not feel forced by a need to maintain your position in the organisation, or of financial support, or so on, to lose your integrity. May you have that freedom."

4.5. TRANSMUTATIONS - HOW MANY MIRACLES?

In the first few years of cold fusion, no one predicted that transmutations (e.g. alchemy) would be claimed. Yet at ICCF-3 in Nagoya in 1993, five groups claimed that transmutations had been observed, including mercury into gold!

Still when one considers how many major miracles were already required for cold fusion, why not one more? Indeed it is well established that the first miracle is the most difficult to believe, but once that hurdle is overcome, it is easy to believe other miracles. In 1989, the list of miracles, or major violations of laws of Nature that had been confirmed by thousands of experiments, was;

1. The rate of cold fusion claimed by Fleischmann and Pons, and Jones was some 10^40 times larger than expected. It is hard to explain simply how large a number is 10^40 - suggestions please, for any practical analogies? (the best suggestion so far, is from Frank Close. The radius of the proton is 10^-15 metres. The radius of the Universe now {if10^10 years of expansion at the speed of light} is 10^26 metres. The ratio of the Universe to the radius of the proton is then 10^41.
2. The relative absence of nuclear "ashes". If the reaction was nuclear as claimed, then neutrons, protons, tritons, and 3He should be produced in huge quantities; plus about 10^-6 times less 4He and gammas of 24 MeV should be observed.
3. The ratios of these ashes is well-determined even in muon-catalysed fusion which is cold, but the ratios claimed by True believers, varied widely but not as expected.
4. When Fleischmann was invited to CERN on 31 March 1989 by Carlo Rubbia, after his talk, the first question was from Carlo, who asked if they

had repeated the experiment on D-D fusion in D2O, but in normal light water, H2O, as this would be a control and nothing should be observed in water. This was one of the rare times that Fleischmann looked uncertain and he replied it was the next experiment. There were a variety of replies before Pons and Fleischmann agreed that H2O gave no cold fusion. However in recent years at ICCF meetings, many groups, such as Miley's, say they find fusion with H2O and no one comments let alone complains in public though a few of the better scientists say in private, that they are unhappy.

There are a number of other miracles such as biological effects, creating black holes, solving the solar neutrino problem, etc. but as they are not general, they will not be counted.

So the fifth miracle could be the observation of transmutations from one element to another. Most of the claims involve a very small piece of metal which is treated, for example, by electrolysis, and it or other material elsewhere, is examined by a very sensitive apparatus and traces of other elements are detected. Here it must be emphasised that the quantities are very small - so small that some wonder if they were not trace elements existing somewhere else in the apparatus which the electrolyte had transferred to a new site.

It will be recalled that in the whole history of Polywater, the sample sizes were always less than one cm3 - here they are much less. For example, Miley uses five small layers of 1000 A thickness. It would be good to see even one cm3 of transmuted product. Miley also claims excess heat but is severely criticised also for these claims as errors are not considered and instrumentation is inadequate. It may be significant that Miley's work is based on the Patterson power cell, CETI, which could not present any results at ICCF-8 despite selling 40 kits at $3,750 each some years ago. Incidently, Miley uses normal water, H2O and not D2O.

4.6 LOW ENERGY D-D RATES

At Lerici, J. Kasagi of Tohuku University presented results on the measurements of cross sections for deuterium ions hitting deuterium-loaded metal targets with some partly surprising results.

Previously F.E. Cecil and G.M. Hale had shown results[8] at ICCF-2 at Como in 1991 where the cross section fell precipitously as the energy decreased towards 2 keV as would be expected from the strong potential barrier effect. Nothing anomalous was observed. The target was CD2 sheets.At ICCF-6 in 1996, Kasagi found that the cross section fell very steeply with decreasing incident energy, but was slightly higher than predicted. He interpreted this difference in terms of a screening effect, Us, and values of 19 +/- 12 eV and 60 +/- 10 eV were calculated for Ti and Yb metals resp. A CERN expert was surprised and considered these values very high.

At ICCF-8, Kasagi again found slightly higher cross sections than predicted and gave Us values of 600 eV for PdO, 310 eV for Pd-black (palladium deposited on carbon balls) and Fe, and 75 eV for Au and TI. These values are very high and merit checks. It may be noted that at the lowest energy of 2.5 keV, the counting rate was one per week. The rates of the products of the reactions found in 1996 and 2000, were normal, that is, the production rate of 24 MeV gammas was about a million times lower than the emission of protons, 3He and tritons - this is in contradiction with True Believers claims that cold fusion proceeds almost entirely by production of 4He plus electromagnetic energy of 24 MeV (a gamma) which somehow converts into very low energy X-rays or phonons which cannot be detected, not even as 21 keV X-rays characteristic of palladium.

In 1996, Kasagi noted the emission of high energy alpha particles which he interpreted as a secondary interaction of 3He with another deuterium giving an alpha plus proton with a Q of 18.35 MeV. It is not clear if secondary interactions were considered in the 2000 data.

4.7 ALCHEMY

R. A. Monti showed a poster entitled "Nuclear Transmutation Process of Uranium". He claimed that a series of positive results were obtained from 1993 to 1995 and then independently, at the ENEA labs from 1997 to 1998 and more tests have now been made.

Essentially a mixture of compounds are heated in a furnace to 1150 degrees C. The compounds were listed and include 330 g carbon, 900 g KNO3, 80 g sulphur which are the basic ingredients of gunpowder.

He wrote that when Bockris tried a similar procedure he failed due to "lack of knowledge of elementary alchemy" as he was completely "out of season". "The right season is 25 March to 15 June." Monti tested this by showing that in tests done 4 May and 25 May, he reduced the amount of uranium originally present from 4.39 to 3.07 grams and from 4.56 to 2.5 grams resp. but when he tried on January 8, the uranium was reduced from 5.34 to only 5.08 grams.

It may be recalled that Bockris together with his unusual student, Champion, had some success in transforming mercury to gold, but never talked of a seasonal effect (these adventures terminated when Champion was sentenced to prison for another affair in Arizona).

It would appear that the historical alchemy, transforming mercury to gold, is now replaced by the more contemporary elimination of uranium which has greater investor appeal - Monti said that it was not difficult to find investors. He claimed that Eucan Technologies Gmbh had signed an agreement with ENEA starting in October 1996.

4.8 BIOLOGY AND COLD FUSION

In 1993 L. Kervran was awarded the Ignoble Prize for Physics[9] for his book "Biological Transmutations" in which he argues that a cold fusion process produces the calcium in eggshells.

V.I. Vysotskii et al. of Kiev (abs. 008) reported using time of flight mass spectroscopy to study nuclear transformations in microbiological studies using Bacillus subtilis. The expected reaction was 23Na + 31P = 54Fe in a growing culture in sugar-salt nutrient medium deficient in Fe but containing 23Na and 31P isotopes. The mass spectrum showed that the rare isotope 54Fe was enhanced as expected.

F. Celani et al. of Frascati (abs. 096) reported impurities in the heavy water. Some were bacteria which DNA sequencing techniques showed were of the Rarlstorica family and were exceptionally hard to destroy. These bacteria metabolised the mercury which was used as a thin film of the surface of the palladium to avoid de-loading the hydrogen.

Biberian of Marseilles, said that the First International Workshop on Biological Transmutation is being held in Geneva.

5. MATERIAL SCIENCE

It is many years since Fleischmann declared that cold fusion was easy, just high school level chemistry. As many groups who found a positive result then found that they could not repeat it, they reasoned that since cold fusion must be true, then there had to be some subtle special way of preparing or choosing the electrodes. Hence many groups quickly started a programme of studying the material of electrodes and of ways of loading them with deuterium or hydrogen. At ICCF meetings, a large fraction of papers is now devoted to these material science questions.

A consistent feature of these experimental papers, is that the authors do not read previous publications. There is an enormous literature, even journals, on hydrogen isotopes in palladium and other metals.

Once a Japanese expert, Prof. Y. Fukai, was asked to speak[10] to ICCF-3 in Nagoya. The great problem of cold fusion is that the two deuterium nuclei are too far apart to fuse - because of the large potential barrier. In D2 gas, they are 0.74 Angstroms apart and to obtain the modest fusion rate of 10^-20 fusions per second, a separation of 0.14 A is required. But in palladium crystals, they are even further apart, 2.84 A for the orthohedral placings and 1.74 A for tetrahedral placings! It was suggested that coherent oscillations could reduce this distance but Fukai said their maximum amplitude corresponded to 1 eV which was too small. He also showed that the suggested use of a screened Coulomb potential was erroneous. His talk did not please everyone - one senior theoretician said that "something was missing from the talk - could you tell me why metals exist? You could not answer: And if you would answer, I would shoot it down. People find heat. You think we are idiots but people find things"

Del Giudice presented alternative ideas and after his talk, Ed Storms commented that he had claimed that there were three phases of hydrogen in palladium, alpha, beta, and gamma. But in previous work only alpha and beta phases are described and in a rather different way - there are no references to a gamma phase. Could you please quote any other experimental evidence in favour of the existence of a gamma phase? As always, there was a reply, a flow of words, but could not detect any answer from Del Giudice.

There have been claims that 4He is found in cold fusion by Arata et al. and Case et al. Here activated carbon is employed as a carrier, It is said that helium is not absorbed by carbon but Rich Murray did a literature search and found that Maggs et al. Nature, 18 June 1960 p. 956-958 and P. Malbrout et al. Chem. Abs. 126 148921, both found substantial absorption of helium.

6. THEORIES

Cold fusion has an incredible number of theories all which claim to explain it, but generally, the theories are mutually exclusive. For ICCF-4, I made a list of the 23 theories that were proposed then.

Further at ICCF-3, Rabinowich, Kim, chechin, and Tsarev reviewed [11] all theories and found serious faults in all of them.

It will be recalled that at a previous ICCF meeting, the experimentalists thought it would be profitable to have a comparison of theories and of their predictions, e.g. would excess heat be provided by proton-proton fusion as well as by deuteron-deuteron fusion? However after a compilation was started, some theoreticians refused to give even simple predictions and some walked out of the room - the attempt was abandoned.

This ICCF conference was similar to previous meetings - there were many predictions, most were mutually exclusive, and clear predictions and statements were missing.

Del Giudice presented the theory of Preparata, Bressani and Del Giudice which requires coherence in the palladium metal lattice. He did not respond to the suggestion of Storms at the end, that a theory to be successful, should also explain how excess heat is found in non-metals. Previously I had asked if cold fusion was predicted to occur in ice since it also has a lattice structure, but it seems that this calculation may not have been done.

Drs Talbot and Scott Chubb in their agreeably harmonious double act, presented their approach based on ion band states.

A list of the theories presented at ICCF-8, is given in Appendix 2.

7. PREDICTIONS OF COMMERCIAL APPLICATIONS

As pointed out by several in the Concluding Session, for cold fusion to be accepted, it is necessary to have some commercial application that can be readily purchased and which works reliably. Hence it is worthwhile recalling the previous predictions.

1. July 1989 - zero time. Pons not merely predicted an application but stated that it existed then. This was a water boiler "giving off 15 to 20 times the amount of energy that is put into the cell. Simply put, in its current state it could provide boiling water for a cup of tea". "It wouldn't take care of the family's electrical needs, but it certainly could provide them with hot water year round' said Pons who said he has always believed that the practical application could happen this fast."
2. 1992 - less than one year. Pons working with IMRA (Europe) said he had obtained 1000 kW per cm3 of electrode using a new type of palladium alloy. He expected a practical application before the end of the year.
3. 1992 - one year. Fleischmann said a 10 to 20 KiloWatt power plant should be operational in one year
4. Nov. 1993 - six years. Pons expected that by the year 2000, there

should be a household power plant operational.
5. May 2000 - no predictions for 10 years - Chairman of ICCF-8, F. Scaramuzzi.

It may be concluded that the time to a commercial application is receding into the distant future as time passes.

8. BRIEF TOPICS

8.1. US PATENT OFFICE

Gene Mallove explained to me that in the US Patent Office, there are experts assigned to each subject. When a patent application arrives with the words "cold fusion" it is sent to the expert. It is generally acknowledged that the USPO has decided, after due study, that cold fusion comes into the same category as infinite energy machines or perpetual motion machines, and are immediately rejected. The result is the people filing patents cunningly avoid using the words "cold fusion".

There is great prestige if having your application granted a US patent and it helps in fund raising even though the granting of a patent does not necessarily mean that the proposed machine will work as claimed.

The US Patent Office has apparently decided that some patents that have been granted, should not have been approved and they are now trying to withdraw their approval. Naturally the applicants object and now some of them are considering filing law suits.

In Mallove's glossy magazine, "Infinite Energy", it is stated that Thomas Valone had been fired from the US Patent office. He has a curious history. After he joined the USPO, he invited cold fusion believers to apply to join the Office, writing that the conditions were good (canteen, swimming pool, pension, etc.) and said that they could help to approve patent applications for new energy devices. Then early in 1999, he organised a Conference On Future Energy, COFE, under the auspices of the US State department and had invitations sent to foreign embassies. When this was blocked, he shifted and had the same conference organised by the Commerce Department. However people at the department were told that at the American Physical Society's Centennial meeting in Atlanta, March 1999, some thousand people had been roaring with laughter at cold fusion in talks given by James Randi, Bob Park and Peter Zimmerman - the Commerce Dept. sponsorship was withdrawn. However the COFE meeting was still held - it was an unusual meeting with some serious talks about wind energy etc., but also talks on anti-gravity, Zero Point Energy, cold fusion, etc..

8.2. INFINITE ENERGY

There are a few publications devoted to cold fusion and to various forms of desirable energy sources which have a doubtful justification, such as Zero Point Energy, ZPE. The most glossy of them is undoubtedly the magazine Infinite Energy whose editor is Mallove. He was chief science writer at MIT until he split with them and accused some MIT staff of unethical conduct over cold fusion. He is a spin doctor who is very skilled at public relations and exploits fully the slightest occasion such as any favourable statement by a well-known personality who has often not seriously studied cold fusion.

8.3. QED AND QCD

At ICCF-8, a talk was given about quantum electrodynamics, QED, where it was said that there was the mystery of why quarks were not observed. This was a fine talk for the late 1960's but experiment and theory have moved on since. The colour quantum number is now accepted and supported by many experiments. The corresponding theory is called Quantum ChromoDynamics, QCD.

It would be entirely appropriate if at the next meeting, ICCF-9, Asymptotic Freedom and Quantum Chromodynamics were to be explained to us by Dr. Mallove.

8.4. LIBEL CASE - FLEISCHMANN, PONS AND OTHERS VERSUS LA REPUBBLICA

After La Repubblica wrote that cold fusion was scientific fraud, they were sued for 8,000,000,000 lira (about $5,000,000 then) by

Drs. Fleischmann, Pons, Bressani, Del Giudice and Preparata. I was asked by La Repubblica to provide scientific evidence for the case. The five Believers lost the case and also had to pay La Repubblica's costs - the judges said that Fleischmann and Pons had lost touch with reality. In 1996 they announced in Nature [12] that they would appeal but nothing further has been heard of this - perhaps because of the reply in Nature[13].

8.5. TOP TWENTY TECHNOLOGICAL FOUL-UPS OF THE TWENTIETH CENTURY.

The Ig Nobel Board of Governors, commissioned by the Wired News and the Annals of Improbable Research, have made a list of the top twenty foul-ups involving technology in the last century - they said it was a difficult choice from the several hundred thousand candidates. Number one was Blondlot with his N-rays. Number 16 is Chernobyl while Cold Fusion was 18th.

8.6 PAPERS, NEW BOOK

At ICCF-8, it was surprisingly hard to obtain papers giving the results or theories. There was a small table, later two small tables. Initially the first one seemed to contain only material from Mallove - copies of his glossy magazine with the unusual and unphysical title "Infinite Energy", plus copies of a book that he was selling called "How cold fusion prevailed" - again the title is unusual as few appear to believe that cold fusion has prevailed except the author.

The book was written by Charles Beaudette who says he first went to a cold fusion meeting in 1995 "for a lark" he wrote. However he was impressed - as an engineer. The book is unremittingly biased in favour of cold fusion - it even makes Mallove's book seem almost neutral. Embarrassing incidents like the moving of the gamma peak by Fleischmann and Pons from 2.5 to 2.2 MeV, secondly, widening it and thirdly, increasing the number of counts by a factor of ten, are ignored - they were under stress, he explained. When criticisms are made of results or of disagreements with many previous experiments, they are not discussed, rather the writer of the criticism is attacked but the justification of the attack is not made - "attack the messenger, not the message".

If any copies of papers which were suitable for refereeing by a journal, were left on the table for distribution, they must have vanished before I saw them. In contrast I left out a copy of the review of world energy that I had been working on since 1991 and updated several times, and invited people to contact me if they wished a copy. A few papers were obtained by asking the people who made presentations.

8.7. PERSONALITIES NOT AT ICCF-8

Former regular attendees or major personalities at ICCF meetings who did not come to Lerici for a variety of reasons unknown, apart from Stan Pons, Steve Jones, Tullio Bressani, Carlos Sanchez-Lopez and Fred Jaeger, include; A.J. Appleby, N. Asami, R. Bass, H.E. Bergson, J. O'M Bockris, B. F. Bush, R. T. Bush, F. E. Cecil, T. N. Claytor, S. Crouch-Baker, J. Drexler, Tom Droege, R. D. Eagleton, J. Foos, L. Forseley, Y. Fukai, D. Gozzi, Wayne Green, W. N. Hansen, Nate Hoffman, R. Huggins, J. R. Huizenga, H. Ikegami, B.Y. Liaw, B. E. Liebert, Scott Little, Bruce Klein, G. Kreysa, K. Kunimatsu, K. Matsui, T. Matsumoto, H. O. Menlove, K. Nagaoka, T. Nakata, R. Notoya, M. Okamoto, T. Omura, F. Oriani, M. Rabinowich, M. Schreiber, A. Spallone, D.T. Thompson, V.A. Tsarev, J-P Vigier, Fritz Will, D. Worledge, E. Yamaguchi, etc.

9. FUTURE MEETINGS

It was announced that Prof. Li would host the next meeting, ICCF-9, in Beijing in two years time. The month was not announced but could be again in the spring time.

There will be a meeting in October 2000, in Russia near Sochi on the Black Sea in a holiday area. This is the eighth of the series where all are welcome. Details and list of 7 sponsoring organisations are given in Appendix 3.

The series of meetings in Asti in Italy, will continue. These are private meetings, by invitation only.

10. WILL COLD FUSION CONTINUE?

When the First Cold fusion conference was held in March 1990, there was a joke that the first would also be the last, because it seemed so evident that cold fusion had been disproved and shown to be ridiculous. But this prediction was wrong, for although over 99.9% of scientists think that cold fusion is disproved and ridiculous, nonetheless there is a hard core of True Believers and hopeful investors who have just had their eighth conference eleven years after the 1989 publications of Fleischmann and Pons, and of Jones. Why? Will they continue?

Initially they had a dream which we all have - would it not be wonderful to have a limitless energy source which did not pollute? Yes, but while most consider that such a practical energy source does not exist, these True Believers think that cold fusion has been proved but that there is a conspiracy by entrenched interests to suppress cold fusion, e.g. by refusing it patents and funds.

They are not discouraged by a lack of success of reproducing lab experiments and of making a practical application, despite predictions and even claims (e.g. Pons working boiler[14] in 1989 - we are still waiting for our cup of tea).

Would anything discourage them? Doubtful. There are always willing investors who hope that this is a secret process missed by the mainstream, which would make them very rich and famous. It is the great lottery syndrome - if the prize is large enough, the buyers do not care what the odds are.

There is a saying by Planck that a wrong theory only dies out when the promoters are gone. Well, we do not wish harm to anyone, but of the three original promoters, Jones has re-evaluated his work,found flaws and turned completely against cold fusion, even doing experiments to show where Fleischmann and Pons had gone wrong; Pons for a second time, has vanished and does not attend ICCF meetings anymore; Fleischmann does attend ICCF meetings but has not provided any new work for some years. These defections appear not to influence True Believers. But if Fleischmann were also to drop out?

As cold fusion continues its slow decline, there is a change of direction. Instead of feeling strong enough to stand alone, the media enthusiasts, Mallove, Fox, et al., are linking up with a loose grouping of True Believers in other unusual energy sources such as anti-gravity, zero point energy, ZPE, which to work, would require yet more violations of the Laws of Physics. A recent example was the Conference On Future Energy, COFE. As usual with doubtful presentations, there was a mixture of serious speakers (e.g. from DOE, wind power) whose reliability can be checked, and doubtful ones whose reliability is hard to check. Again there is a worthwhile dream - clean, non-polluting, cheap energy - and under the cover of this dream, some do not mind proposing impractical solutions which have been disproved many times.

11. CONCLUSIONS

I have often looked at experiments which gave results that appeared to violate the laws of Nature which had been established by previous work. Later these experiments turned out to be false, but I have often found it very difficult to see just where the error was. But the fact that I had not detected the flaw, did not mean that the experiment was correct and that the laws of Nature had been violated.

Rather I feel the same as being at a circus watching a magician. Normally he and I know that the laws of nature are being obeyed but there is a trick which is hard to spot. At trick one, I may spot the trick and am happy that there is no problem with the laws of Nature - similarly with trick number two. But suppose at trick three, I do not see how the magic is performed. The magician may say "I won, I tricked you" and it is left unsaid that the laws of nature have not been violated. But suppose the magician says "You did not see anything wrong with my demonstration, therefore it is true. See, I have supernatural powers. The old laws of Nature have been replaced by new laws". And if I protest, I am told that I have a closed mind, am an establishment figure, and do not face up to the happening performed in front of me. But almost all magicians admit that it is all trickery and the laws of Nature are not threatened.

So if someone comes along and says, "Look - excess heat - do you see anything wrong?", then I feel as if I am at the circus, and although I do not immediately see anything wrong, I am reluctant to give up well-established laws of Nature unless the proof is very strong. Here reports on cold fusion happenings are described, especially in the summary talks by True Believers in cold fusion in their words, and then some clues as to possible explanations are offered. How many Elvis sightings constitute a proof?

APPENDIX 1 - PROBLEM FOR EDWARD TELLER

Back in 1992, Edward Teller attended a private meeting on cold fusion in Washington. He delighted the media-aware people, e.g. Mallove, by proposing a new particle which would explain the contradictions of the then cold fusion results - how to have lots of excess heat without commensurate production of protons, neutrons, 3He, tritium and gammas. When I phoned him, he explained that the clue was in the name of this hypothetical particle which in his native Hungarian means "Crazy'. Little was heard of this afterwards.

At the 2000 meeting in Lerici, a friend of Teller attended. If he were to list the properties required of another new hypothetical particle that could explain all the various results of cold fusion experimenters, then the list of requirements would look something like this;

1. Gives heat of cold fusion at a rate 10^40 times more that expected from potential barrier considerations
2. Gives excess heat in cold fusion in both light hydrogen and in deuterium
3. This excess heat should give some 4He and possibly some tritium but no protons, no neutrons (except in certain labs), no 3He (except in certain labs) and no gamma rays of 24 MeV.
4. When the fusion takes place in palladium, X-rays of 21 keV, characteristic of palladium, should not be observed.
5. Transmutations should occur on electrolysis, mainly into stable ground states, but not into radioactive isotopes
6. These positive fusion and transmutation processes should only occur with very small quantities of material, typically 40 milligrams, but not in bulk material.
7. Transmutations and excess heat should also be observed when there is no metallic crystalline structure (i.e. no coherence effects)
8. The cold fusion should occur at both low loading, e.g. by gas, as well as high loading of hydrogen into the electrode. But at very high loadings, obtained using a diamond anvil, no excess heat is produced.
9. Biological transmutations should also occur
10. Alchemy should occur but most strongly in the time window between 25 March and 15 June.

It has also been suggested that cold fusion has an 11-year solar cycle, but this may not be a serious suggestion, so will be excluded to lighten the requirements.

APPENDIX 2 - THEORIES AT ICCF-8

1. Runbao Lu, (abstract 010) "Electron-ion bound state and its introducing of nuclear fusion".
2. H. Hora, G. Miley and J.C. Kelly (abs. 011) "Swimming electron layers theory" - dielectric effects in the metallic plasma.
3. A. Takahashi, M. Ohta, and T. Mizuno (abs. 012) Low Energy Photofission, LEPF with multi-photons of 0.1 to 10 keV.
4. M. Ohta and A. Takahashi, (abs 013) electron-phonon plus heavy

electron gives screening.
5. S.R. Chubb and T.R. Chubb (abs. 025) Interaction between ion band states.
6. A.D. Vita (abs 028) Mechanical statistics of a second order phase transition in Pd-metal hydride.
7. M. McKubre et al. (abs 029) extended lattice coherent processes.
8. J.C. Fisher (abs 030) Polyneutrons - mobile droplets of neutron liquid give reactions, e.g. with a 100 neutron droplet and Oxygen, O;

100n + 18 O ----> 102n + 16 O

9. Y.E. Kim and A.L. Zubarev (abs. 033) Ultra low energy nuclear fusion for Bose nuclei in ion traps.
10. V. Violante et al. (abs. 034) Electro-magnetic oscillations produced by coherent oscillations of the Fermi level electrons in the metal lattice.
11. J.J. Dufour (abs. 040) "Hydrex" is a resonance, 1H1 of a proton and an electron (lifetime a few seconds, size a few fm, energy a few eV). Nucleons, ANuZ, plus several Hydrex catalyses alpha particle emission giving transmutations , e.g.

ANuZ + 2 1H1 -----> A-2NuZ + 4He2 + x MeV.

Also e.g. uranium into lead.
12. H. Kozima (abs. 044, 045, 046) Trapped Neutron Catalysed Fusion, TNCF model. Energy band of neutrons interacts coherently with lattice nuclei. e.g.

n + 46Pd -----> 13Al + 33As or -----> 26Fe + 20Ca.

13. A.A Nassikas (abs. 053) "Cold fusion as a Space-Time Energy Pumping Process" based on "Quantum Space Time-Aether".
14. Y.Z. Li (abs. 062) A selective resonant tunnelling model shows that when the Coulomb barrier is thick, can have fusion with no strong neutron or gamma emission.
15. P.L. Hagelstein (abs. 064) Fast ion emission from metal deuterides is explained in terms of a second order off-resonance fusion reaction with the lattice phonons. The strong force between deuterons is viewed as a very high order phonon non-linearity which gives an intimate coupling with phonon and fusion event. A clean separation has been found between the coherent part of the non-linear interaction from the incoherent part giving a collective phonon mode which couples to the coherent part. "The model predicts the possibility of alpha emission from Pd-D with alpha energies up to 21 MeV as reported by the NRL group."
16. Yu. N. Bazhutov and V.G. Grishin (abs. 081) Erzion model of Cold Nuclear Transmutations, CNT. The Erzion is a stable heavy particle which catalyses CNT. They claim to have detected the Erzion in cosmic rays. This will explain many problems such as dark matter in the universe, the solar neutrino problem, ball lightening etc. Numerous applications include transmutation into gold.

APPENDIX 3 - RUSSIAN CONFERENCE AND SPONSORS

The 8th Russian Conference on Cold Nuclear Transmutations, RCCNT-8, will be held at Dagomys near Sochi. from the 4 to 11 October, 2000. The subjects will include ball lightning as well as cold fusion and transmutation.
The full cost is $900 which includes hotel and meals etc.

The sponsoring organisations are;
Russian Academy of Sciences
Russian Physical Society
Nuclear Society of Russia
Mendeleev Chemical Society of Russia
Moscow Lomonosow State University
Russian Peoples' Friendship State University
State Technical University, MADI.

APPENDIX 4. HOW WILL TRUE BELIEVERS RESPOND TO THIS STATUS REPORT?

Some will say it is biassed, part of the Establishment attack on cold fusion. But since I am independent, this is not too serious.

Some, the spin doctors, e.g. Dr. Mallove, will scan the report carefully searching for any error or fault that they can detect.

Thus the Concluding Talks will get particular attention as Mallove can compare what is written with the video recording he made (and no doubt will offer for sale at a fair price, as he has done previously). Having detected a few faults, they will then declare it is ALL wrong, typically using the phrase "this report is full of errors, for example, ...", thus implying that the entire report can be safely ignored. This report probably contains hundreds of facts and pieces of information, but these will be ignored, and, in particular, there will be no discussions of the inconsistent results such as some people using hydrogen as a blank control for deuterium, while others claim cold fusion with hydrogen - or the list of miracles needed for cold fusion. These commennts will then be distributed by the spin doctors, to their supporter, sponsors, and potential infestor.

Will some lawyer send a cease and desist letter in the Triggs style? I do not know.

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Address for Correspondence;
Dr. Douglas R.O. Morrison
CERN
CH-1211 Geneva 23
Switzerland
Tel 41 22 767 35 32
Fax 41 22 767 90 75
Email; douglas.morrison@cern.ch
NOTE; this paper has no connection with CERN.

Date: *Fri, 14 Jul 2000 22:33:06 GMT*
From: Jed Rothwell <JedRothwell@infinite-energy.com>
Subject: E. Storms replies to D. Morrison

Dear Douglas,

I read your Report on Eighth International Cold Fusion Conference and would like to correct some of the factual errors you included. While I appreciate the need for different ways of interpreting data, I cannot understand how a fair and objective scientist can publish so many factual errors which appear to be made for the sole purpose of discrediting work supporting the "Cold Fusion" effect. We all know you reject the effect based on reasons you have given in the past. Nevertheless, a competent scientist should give a fair and accurate description of available information so that a reader of his evaluation can come fairly to his own conclusions. Instead, you distort the facts and give a completely false impression. Your report is particularly damaging because much of what you write is correct and completely reasonable, thereby making the distortions less obvious.

Because you made your report public, I am also making this letter to you public in an attempt to undo part of the damage.

In the ABSTRACT you make the bold statement about "the overwhelming evidence against cold fusion." The only "evidence" against cold fusion is the conflict with theory, which is not evidence, and the difficulty many people have had in reproducing the effect. No one in science believes that difficulty in reproducing an effect is evidence against the effect. While such difficulty hinders the study and causes people to avoid the effort, it is not evidence. Surely, you know this simple fact.

In your summary of my comments, you attribute to me the statement that Pt, Au, Ni, or Ti absorb a large amount of hydrogen. I did not make this statement. Indeed, I said just the opposite. These metals do not absorb large amounts of hydrogen, yet they are claimed to make excess energy. The point being that theories that focus on palladium, which does absorb hydrogen, are perhaps barking up the wrong tree. In addition, you incorrectly noted the website of www.altenergy.org where a comprehensive review of the conference papers can be found.

You make the statement that the extensive study at Harwell found no excess heat. While it is true they reported no excess heat, subsequent evaluations of their work revealed the presence of overlooked excess energy. Why did you omit this important point? In the same vein, you noted that the NHE laboratory in Japan also reported no excess energy, yet Dr. Melvin Miles reported at the very conference you attended that excess energy was actually obtained but ignored. You later dismiss Miles claims by quoting errors attributed to using the Pons-Fleischmann method of calorimetry. You completely ignored the independent work of Miles done at NHE showing excess heat which has been published in a peer-reviewed journal.

In your list of countries doing work in cold fusion, you omitted Russia, a very important contributor. In addition, considerable work was done in India in the past.

On several occasions, you note my concern about using a heater to calibrate the calorimeter and use my comments to criticize the method employed by Pons-Fleischmann. You completely ignore the comments I made when you raised this issue during the conference. I made clear that my comments were of a generic nature and did not apply to the method used by Pons and Fleischmann. They used a tall, narrow cell and applied the heater pulse while bubble stirring by electrolysis was operating. Both factors will reduce the expected temperature errors to insignificant values, as Fleischmann measured and so stated.

Considerable study by many people has shown that this criticism of the Pons-Fleischmann is completely invalid. So why do you continue to raise this issue?

You once again raise the issue of using a null method when doing calorimetry in the form of a Wheatstone bridge. Surely you are aware of the more modern methods of data collection which are as accurate and certainly more convenient then this method. The problem is not in the data acquisition method. Properly designed calorimeters are sufficiently stable and accurate to detect the claimed amounts of excess energy, as a number of us have demonstrated. Why do you not note and evaluate such claims rather than suggest a useless method?

In your repeated efforts to discredit the Pons-Fleischmann work, you raise the issue of uncertain recombination in the cells by noting the study made by Jones et al. The Jones work has been completely discredited and shown not to apply to the Pons-Fleischmann conditions. I suggest you read the literature rather than beating this dead horse once again. Also, you describe a meeting at CERN in 1989 where Fleischmann was asked about using a control cell containing H2O, noting that Fleischmann looked uncertain, which suggested evasion. In fact, Pons and Fleischmann studied many control cells using H2O with Pt and Pd cathodes and detected no excess energy. However, other people have found excess energy when the cathode was nickel rather than palladium. The nuclear reactions producing the excess energy under these conditions was shown not to be simple fusion, but an interaction between hydrogen nuclei and the alkali metal used in the electrolyte. Although, this reaction is difficult to explain, on going studies continue to show the anomalous effects. While you are correct in stating that many people, even in the cold fusion field, do not believe the claims, you should at least represent the controversy honestly.

Your description of the Kasagi experiment, which measured the D(d,p)T reaction is completely confusing. They demonstrated an enhanced cross-section for this one branch of the fusion reaction in certain metals, including Pd. Because the conditions were not even remotely similar to those required to produce the cold fusion effect, the work can only suggest the existence of a mechanism which, if enhanced, could produce the anomalous tritium. This work has no direct relationship, at this point, to heat production resulting from the He producing branch. At this point, the observations only show a conflict with accepted theory. In other words, the theory you and others use to discredit cold fusion is not so perfect after all.

In describing the work of Prof. Bockris in his attempt to do alchemy, you say that Champion was his student. This is completely false. Champion hired Prof. Bockris to duplicate certain claims being made by Champion, which Prof. Bockris was able to do on several occasions. Because the results were so controversial, the studies only resulted in considerable grief for Dr. Bockris rather than any change of attitude, as you demonstrated in your comments.

Claims by Prof. Arata in Japan and Dr. Case in the US have been duplicated at SRI in the US. In your effects to discredit the claimed helium production, you mistakenly say that activated carbon was used in both studies. This is false. The Arata studies used pure palladium while only the Case work is based on a hydrogen catalyst containing carbon and palladium. Both studies produced excess energy along with He4 in amounts consistent with a fusion reaction. While it is true that carbon can absorb He at low temperatures, as you stated, during the duplication of the Case claims at SRI, desorption of He from the carbon was looked for and not detected. Does not good experimental evidence have any effect on your opinions?

While I appreciate your continued interest in cold fusion and the opportunity to discuss our different attitudes at the various conferences, I would find your efforts much more useful if you would be more accurate in your assessment.

Sincerely,

Edmund Storms

Date: *Sun, 16 Jul 2000 23:54:35 GMT*
From: **Douglas.Morrison@cern.ch**
Subject: **Response to comments on my cold fusion status report.**

16 JULY 2000.

REPLIES TO MAIL ABOUT THE STATUS REPORT ON COLD FUSION

Douglas R.O. Morrison

INTRODUCTION

If one writes a 28 page review of a subject, it is natural that there should be some errors that should be corrected. I would like to thank those who wrote to me with the intention of being helpful. However certain other comments received seemed less helpful, and this hate mail will be ignored.

GENERAL COMMENTS

1.NOT ENOUGH EXPERIMENTAL DETAIL

Sorry, but I had assumed that the five Concluding Speakers' accounts would cover this - after all, it was their job to select and give the highlights.

2. BIAS

A response to this was posted yesterday in Sci.Physics.fusion.

Everyone has some past experiences which colours their approach. Here the Baysian ideas of bias are used and it is assumed that the reader has some notions of the deep fundamental ideas of statistics. First one asks what one expects and goes from there to derive probabilities.

PERSONAL RESPONSES

3. THOMAS VALONE

Thomas Valone has sent me a message regarding section 8.1 about the US Patent Office in the status report.

He does not dispute most of the section, but complains that when I wrote about the Conference On Future Energy, COFE, that he organised, I had said that there was a talk on anti-gravity and there was no such subject. He is correct - what I should have said, was that there was a talk where it was "concluded that space travel faster than light may be possible because experiments show that the force of gravity itself propagates orders of magnitude faster than light". Later "gravitational heat energy" is discussed as a "free energy source". I apologize for writing anti-gravity but must consider that the talk on gravity described here is completely flawed.

The overall point that I was trying to make, is that it is an excellent objective to have a conference on new energy sources, but it should not be discredited by having such doubtful papers presented.

Thus as I wrote, COFE "had some serious talks about wind energy etc.". Mr. Valone also wrote that there were many good talks. In my brief note, I clearly had no space to list his conference in detail, only to convey that the serious talks were given in bad company. A complete account of the conference can be found at; http://www.alterenergy.org/News/COFE.html

Dr. Vallone kindly offered to send me a copy of the proceedings - I am pleased to accept his offer. I will send him a review "World Energy and Climate in the Next Century" which has been copied and distributed to working groups of The Royal Society, Pugwash, and the World Federation of Scientists, and also translated into Arabic and published by OAPEC. Also am sending an extension "Energy in Europe; Comparison with Other Regions" which was presented at the Millennium Clean Energy Congress which was sponsored by an incredible number of UN, governmental, and Non-governmental organisations (Vice-President Gore was supposed to attend but this was at the time of the New Hampshire primaries).

The Alternative Energy Institute (like COFE) should make a decision; do they wish to be considered as a serious organisation or to be considered as a home for fringe activities propagating discredited ideas and which also has some serious people?

4. ED STORMS
I would like to thank Ed for the serious tone of his letter - so different from some other communications. On the other hand, was surprised by some of his phrases which seemed out of character, such as "you distort the facts and give a completely false impression", "you completely ignored", "you should at least represent the controvery honestly". Well, I will try and respond to his points paragraph by paragraph;

Para. 1 and 2 - no comment - expressions of opinion.

P3. You asked where is the "Overwhelming evidence" against cold fusion? For this see the paper "Review of Cold Fusion" which I presented at the ICCF-3 conference in Nagoya - strangely enough it seems not to have been published in the proceedings despite being an invited paper - will send a copy if desired. As Dieter Britz has shown, most cold fusion papers were published before 1993 and are therefore in my summary. There it is shown that for every subject (excess heat, neutrons, tritium, 4He, 3He, Gammas, protons) there are more null papers than positive papers. Further, and which is very damning, the quality papers almost all show null effects. The fact that cold fusion is in contradiction with a vast body of research, is expressed by saying that from this research work, theories have been developed which are in agreement with the experimental results. Thus when it is written that cold fusion is in disagreement with theory, this basically means that it is in disagrement with the overwhelming experimental evidence on which the theory is justified.

P4. Sorry for my mistake in misquoting you. I appreciate you making the point that theories should take into account metals other than palladium.

P5. Harwell - "subsequent work revealed the presence of overlooked excess energy". This is a completely misleading statement.
What I wrote was "Harwell did 127 varities of experiment, and searched for excess heat, neutrons, gammas and tritons, but did not find any significant signal in any of them". Please note the phrase "significant signal".
Remember what happened; Before the press conference of 23 March 1989, Fleischmann talked to his friend David Williams, an electrochemist, and told him of a simple experiment that would verify his Utah work. Harwell assembled a multi-disciplinary team which spent half a million pounds on this "simple experiment". They tried to repeat Fleischamnn and Pons work and could not get the same results - despite having Fleischmann's help! Also there is the problem of analysing these different results. For example, should they use Newton's Law of Cooling as Fleischmann and Pons did at that time with a T to the power one term, or should they guess that they should switch, as F&P did later, to using Stefan's Law with a T to the power four term? Strangely enough, this did not seem to worry Fleischmann and Pons!
I wrote "When they used the best technology, they found no excess heat". Now "best technology" is not the Fleischamnn and Pons technique. Hope you agree that when they used best technology (the null method), they found no excess heat? Would it be fair to ask you why did you "completely ignore", in your phrase, the best technology results of Harwell?
Now some desperate people looked at the data using not the best technology, and claimed that they had found excess heat - which David Williams et al. deny -
they say that there were minor statistical fluctuations but when all the results were combined, there was no significant signal. And what I wrote on page 16 was "did not find any significant signal".

I am sorry that you have adopted the position of certain people who search for the slightest fluctuation and claim that this particular run showed excess heat while neglecting all the other runs which show that there is no significant signal. Further, and what is worse, they neglect the very careful work done with one of the world's best calorimeters where they have three temperature controlled water baths round the object being studied - this is a super-Wheatstone bridge technique. The major point is, that it is much better to do a good experiment to show that outside (room) temperature effects are not important by eliminating them, rather than doing a poor experiment where one has to do doubtful calculations to try to prove that heat exchange with the environment is not important or is adequately corrected for..

The Harwell series of experiments were magnificent and it is pretty mean to look for a fluctuation and to try and ignore the totality of their results on neutrons, tritium. gammas and tritons, apart form excess heat with what was probably the world's best calorimeter.

P5A. Similar comment about the NHE lab experiments in Japan. But here we can make a more precise statement - which in fact is in my report but I see it needs expanding to make it clear to all.

I wrote two paragraphs about Miles's visit to NHE lab. He and Fleischmann claimed to find exceptional excess heat peaks. But they were all very small (much smaller than the Fleischmann and Pons claims incidentally). This was answered by the NHE people at ICCF-7 when they said that there were fluctuations but these fluctuations were always within a few stndard deviations and therefore did not represent significant signals of excess heat. In my report,
I quoted that Miles claimed errors of +/- 20 mW while NHE people said the errors were ten times bigger, +/- 200 mW. Now the General Electric group who did a thorough analysis of the Fleischmann and Pons work, concluded that F&P's calculated errors were far too small (the reponse of F&P did not answer the points made by the GE group of Wilson et al.).

However this question may be settled another way. It is universally agreed that the excess heat claimed is not reproducible - even by True Believers. Then for a True Believer, the result of a series of runs should be a combination of two sets of results - firstly, a Gaussian distribution of random fluctuations with a certain standard deviation, and secondly, some runs where excess heat occurs and this would have a different distribution with a significantly higher average value. So, combining these two sets of runs,
one would expect a messy distribution of excess heat values. But the actual results found as I wrote, "the distribution of fluctuations gave a perfect Gaussian distribution with three standard deviation limits of +/- 2.3% with no indication of excess heat occurring spasmodically".

I hope this is clearer to all now.

P6. I am sorry that in one place I missed out Russia as an important collaborator. However, I did mention them extensively elsewhere and indeed Appendix 3 is devoted to them.

Incidentally, I had lunch today with the Director of a major Russian Laboratory who is an excellent physicist, and he was very surprised to hear that someone in his lab was publically involved in cold fusion.

6A. Do not understand the comment about India - I was only talking about countries where experiments were being done now. I was not making a list of countries which have stopped such as Spain which could not find neutrons after I visited the group.

P7. I do not think that the balance of publications on the reliablity of the Fleischmann and Pons methods, is in favour of them. As I wrote above, the most complete and serious analysis was that done by the General Electric group and I would strongly recommend everyone to return and study their paper carefully.

P8. You say the "more modern methods of data collection which are as accurate and certainly more convenient than this " null method. Well. I am an experimentalist. If there is any doubt, then "you should try and prove yourself wrong" and use both methods. I do not admire the lazy way of saying this is "more convenient" and then do some unclear calculations to support this point of view. This is not the way of good scientists - they do the work.

P9. Answer as above. "The Jones work has been completely discredited" - could you please send me a publication where Fleischmann and Pons repeated the very simple and inexpensive Jones(actually Lee Hansen) experiments? Experimentalist do experiments.

Also could you send me any publication which "discredits" the Provo results?

P9a. On the 31 March 1989, to which I refer, Fleischmann did not say that he had done a control experiment with light water - he said that the 8 mm rod that gave no effect, was their control! This I checked by looking at the video tape of Fleischmann's talk.

P10. Sorry if I was confusing. Your conclusion is that "the theory that you and others use to discredit cold fusion is not so perfect after all". Well, I was being polite. There are two possiblities - either the hundreds of experiments that have been made previously are wrong, or the new and very difficult experiments of Dr. Kasagi is wrong. Which do you choose? You may remember my polite conclusion; "These values are very high and merit checks". Too bad that you force me to reverse my politeness.

Further, I discussed the possible effect of secondary interactions, which you seem to have missed.

P11. This is interesting. I had been told that Champion came to Bockris and asked to be his grad student but Bockris was not interested, until he was told that $200,000 would be given to his funds for research. Now you say this is "completely false". Your story is that "Champion hired Prof. Bockris". Well, that does not sound good. One would expect a Distinguished Professor like Bockris would check out anyone who wanted to hire him? and find out the source of the money and if Champion had a criminal record? The claims that you talk about - are they the conversion of mercury to gold? If so would a Distinguished Professor not have some doubts? How would you react to such an offer?

P12. My mistake if only one of Arata and Case used activated carbon. I will correct this and other mistakes.

P13. Thank you for your best wishes for more accurate work. I will try and do so. May I humbly suggest on my part, that you consider the possiblity that 99.9% of scientists are correct in their opinion of cold fusion and try to re-evluate all the experiments that you like and also those that you do not like, with the thought that maybe cold fusion does not and cannot exist? (more accurately, could only exist with a very low probability of 10^-40.

Also could you please do experiments and not make calculations (no doubt using a non-linear regression analysis with Kalman filtering) to disprove things such as recombination in the Hansen manner.

When a group of excellent scientists thought that Steve Jones was the only recuperable cold fusion experimentalist, they took him aside and asked him to segment his counters and see if he got the expected result. He did segment them and realised that all his claims of neutron bursts were false. Then he awoke and realised that cold fusion was crazy - but then he asked, how come these other guys are getting results that are obviously wrong? So with Lee Hansen, he did some trivial experiments which any self-respecting experimentalist would have done ages ago, and showed how you can get false results of excess heat.

So Ed, is there any change you can make to your experiments which is the equivalent of segmenting Jones's counters? For example, using a null method as Harwell did, or as Tom Droege did on a smaller scale?

Maybe the committee was underestimating and you are also recuperable? Please think about it and do simple experiments to try and prove yourself wrong such as blowing nitrogen gas between the electrodes every time you think that you have excess hea.

Fleischmann's original response to Morrison's lies

Date: Mon, 17 Jul 2000 16:45:30 GMT
From: Jed Rothwell <JedRothwell@infinite-energy.com>
Subject: Fleischmann's original response to Morrison's lies

D. Morrison hopes that if he posts the same tired, discredited nonsense time after time, eventually he will win the debate. All of his claims about the Harwell fiasco, the General Electric study and so on, are false. For the sake of other readers, who might be taken by his lies, here is Fleischmann's response to his original claims.

- JR

KEY: *text* means original text was underlined
text means original text was italicized
text means original text was underlind AND italicized

Greek letters in the original have been spelled out in this posting.

[[approx.]] substitutes for "tilde" notation used in the paper.

Subscripts are indicated by {x} bracket notation.
Superscripts are indicated by {{x}} double bracket notation.

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Abstract

We reply here to the critique by Douglas Morrison [1] of our paper [2] which was recently published in this Journal. Apart from his general classification of our experiments into stages 1-5, we find that the comments made [1] are either irrelevant or inaccurate or both.

In the article "Comments on Claims of Excess Enthalpy by Fleishmann and Pons using simple cells made to Boil" Douglas Morrison presents a critique [1] of the paper "Calorimetry of the Pd-D{2}0 system: from simplicity via complications to simplicity" which has recently been published in this Journal [2]. In the introduction to his critique, Douglas Morrison has divided the time-scale of the experiments we reported into 5 stages. In this reply, we will divide our comments into the same 5 parts. However, we note at the outset that Douglas Morrison has restricted his critique to those aspects of our own paper which are relevant to the generation of high levels of the specific excess enthalpy in Pd-cathodes polarized in D{2}0 solutions i.e. to stages 3-5. By omitting stages 1 and 2, Douglas Morrison has ignored one of the most important aspects of our paper and this, in turn, leads him to make several erroneous statements. We therefore start our reply by drawing attention to these omissions in Douglas Morrison's critique.

Stages 1 and 2 In the initial stage of these experiments the electrodes (0.2mm diameter x 12.5mm length Pd-cathodes) were first polarised at 0.2A, the current being raised to 0.5A in stage 2 of the experiments.

We note at the outset that Douglas Morrison has not drawn attention to the all important "blank experiments" illustrated in Figs 4 and 6 or our paper by the example of a Pt cathode polarised in the identical 0.1M LiOD electrolyte. By ignoring this part of the paper he has failed to understand that one can obtain a precise calibration of the cells (relative standard deviation 0.17%) *in a simple way* using what we have termed the "lower bound heat transfer coefficient, (k{R}'){11}", based on the assumption that there is zero excess enthalpy generation in such "blank cells". We have shown that the accuracy of this value is within 1 sigma of the precision of the true value of the heat transfer coefficient, (k{R}'){2}, obtained by *a simple* independent calibration using a resistive Joule heater. Further methods of analysis [3] (beyond the scope of the particular paper [2]) show that the precision of (k{R}'){11} is also close to the accuracy of this heat transfer coefficient (see our discussion of stage 3).

We draw attention to the fact that the time-dependence of (k{R}'){11}, (the simplest possible way of characterising the cells) when applied to measurements for Pd-cathodes polarised in D{2}0 solutions, gives direct evidence for the generation of excess enthalpy in these systems. It is quite unnecessary to use complicated methods of data analysis to demonstrate this fact in a semi-quantitative fashion.

Stage 3 Calculations Douglas Morrison starts by asserting: "Firstly, a complicated non-linear regression analysis is employed to allow a claim of excess enthalpy to be made". He has failed to observe that we ***manifestly have not used this technique in this paper*** [2], the aim of which has been to show that the simplest methods of data analysis are quite sufficient to demonstrate the excess enthalpy generation. The only point at which we made reference to the use of non-linear regression fitting (a technique which we used in our early work [4] was in the section dealing with the accuracy of the lower bound heat transfer coefficient, (k{R}'){11}, determined for "blank experiments" using Pt-cathodes polarised in D{2}O solutions. At that point we stated that the *accuracy* of the determination of the coefficient (k{R}'){2} (relative standard deviation [[approx.]]1.4% for the example illustrated [2], can be improved so as to be better than the *precision* of (k{R}'){11} by using non-linear regression fitting; we have designated the values of (k{R}') determined by non-linear regression fitting by (k{R}'){5}. The values of (k{R}'){5} obtained show that the *precision* of the lower bound heat transfer coefficient (k{R}'){11} for "blank experiments" can indeed be taken as a measure of the accuracy of (k{R}'). For the particular example illustrated the relative standard deviation was [[aprox.]] 0.17% of the mean. It follows that the calibration of the cells using such simple means can be expected to give calorimetric data having an accuracy set by this relative standard deviation in the subsequent application of these cells.

We note here that we introduced the particular method of non-linear regression fitting (of the numerical integral of the differential equation representing the model of the calorimeter to the experimental data) for three reasons: firstly, because we believe that it is the most accurate single method (experience in the field of chemical kinetics teaches us that this is the case); secondly, because it avoids introducing any personal bias in the data treatment; thirdly, because it leads to direct estimates of the standard deviations of all the derived values from the diagonal elements of the error matrix. However, our experience in the intervening years has shown us that the use of this method is a case of "overkill": it is perfectly sufficient to use simpler methods such as multi-linear regression fitting if one aims for high accuracy. This is a topic which we will discuss elsewhere [3]. For the present, we point out again that the purpose of our recent paper [2] was to illustrate that the simplest possible techniques can be used to illustrate the generation of excess enthalpy. It was for this reason that we chose the title: "Calorimetry of the Pd-D{2}0 system: from simplicity via complications to simplicity".

Douglas Morrison ignores such considerations because his purpose evidently is to introduce a critique of our work which has been published by the group at General Electric [5]. We will show below that this critique is totally irrelevant to the recent paper published in this Journal [2]. However, as Douglas Morrison has raised the question of the critique published by General Electric, we would like to point out once again that we have no dispute regarding the particular method of data analysis favoured by that group [5]: their analysis is in fact based on the heat transfer coefficient (k{R}'){2}. If there was an area of dispute, then this was due solely to the fact that Wilson et al introduced a subtraction of an energy term which had already been allowed for in our own data analysis, i.e. they made a "double subtraction error". By doing this they derived heat transfer coefficients which showed that the cells were operating endothermically, i.e. as refrigerators! Needless to say, such a situation contravenes the Second Law of Thermodynamics as the entropy changes have already been taken into account by using the thermoneutral potential of the cells.

We will leave others to judge whether our reply [6] to the critique by the group at General Electric [5] did or did not "address the main questions posed by Wilson et al." (in the words of Douglas Morrison). However, as we have noted above the critique produced by Wilson et al [5] is in any event irrelevant to the evaluations presented in our paper in this journal [2]: we have used the self-same method advocated by that group to derive the values of the excess enthalpy given in our paper. We therefore come to a most important question: "given that Douglas Morrison accepts the methods advocated by the group at General Electric and, given that we have used the same methods in the recent publication [2] should he not have accepted the validity of the derived values?"

Stage 4 Calculation Douglas Morrison first of all raises the question whether parts of the cell contents may have been expelled as droplets during the later stages of intense heating. This is readily answered by titrating the residual cell contents: based on our earlier work about 95% of the residual lithium deuteroxide is recovered; some is undoubtedly lost in the reaction of this "aggressive" species with the glass components to form residues which cannot be titrated. Furthermore, we have found that the total amounts of D{2}0 added to the cells (in some cases over periods of several months) correspond precisely to the amounts predicted to be evolved by (a) evaporation of D{2}0 at the instantaneous atmospheric pressures and (b) by electrolysis of D{2}0 to form D{2} and O{2} at the appropriate currents; this balance can be maintained even at temperatures in excess of 90 degrees C [7]

We note here that other research groups (eg [5]) have reported that some Li can be detected outside the cell using atomic absorption spectroscopy. This analytic technique is so sensitive that it will undoubtedly detect the expulsion of small quantities of electrolyte in the vapour stream. We also draw attention to the fact that D{2}0 bought from many suppliers contains surfactants. These are added to facilitate the filling of NMR sample tubes and are difficult (probably impossible) to remove by normal methods of purification. There will undoubtedly be excessive foaming (and expulsion of foam from the cells) if D{2}0 from such sources is used. We recommend the routine screening of the sources of D{2}0 and of the cell contents using NMR techniques. The primary reason for such routine screening is to check on the H{2}0 content of the electrolytes.

Secondly, Douglas Morrison raises the question of the influence of A.C. components of the current, an issue which has been referred to before and which we have previously answered [4]. It appears that Douglas Morrison does not appreciate the primary physics of power dissipation from a constant current source controlled by negative feedback. Our methodology is exactly the same as that which we have described previously [4]; it should be noted in addition that we have always taken special steps to prevent oscillations in the galvanostats. As the cell voltages are measured using fast sample-and-hold systems, the product (E{cell} - E{thermoneutral, bath})I will give the mean enthalpy input to the cells: the A.C. component is therefore determined by the ripple content of the current which is 0.04%.

In his third point on this section, Douglas Morrison appears to be re-establishing the transition from nucleate to film boiling based on his experience of the use of bubble chambers. This transition is a well-understood phenomenon in the field of heat transfer engineering. A careful reading of our paper [2] will show that we have addressed this question and that we have pointed out that the transition from nucleate to film boiling can be extended to 1-10kW cm-{{2}} in the presence of electrolytic gas evolution.

Fourthly and for good measure, Douglas Morrison once again introduces the question of the effect of a putative catalytic recombination of oxygen and deuterium (notwithstanding the fact that this has repeatedly been shown to be absent). We refer to this question in the next section; here we note that the maximum conceivable total rate of heat generation ([[approx.]] 5mW for the electrode dimensions used) will be reduced because intense D{2} evolution and D{2}0 evaporation degasses the oxygen from the solution in the vicinity of the cathode; furthermore, D{2} cannot be oxidised at the oxide coated Pt-anode. We note furthermore that the maximum localised effect will be observed when the density of the putative "hot spots" will be 1/delta{{2}} where delta is the thickness of the boundary layer. This gives us a maximum localised rate of heating of [[approx.]] 6nW. The effects of such localised hot spots will be negligible because the flow of heat in the metal (and the solution) is governed by Laplace's Equation (here Fourier's Law). The spherical symmetry of the field ensures that the temperature perturbations are eliminated (compare the elimination of the electrical contact resistance of two plates touching at a small number of points).

We believe that the onus is on Douglas Morrison to devise models which would have to be taken seriously and which are capable of being subjected to quantitative analysis. Statements of the kind which he has made belong to the category of "arm waving".

Stage 5 Effects In this section we are given a good illustration of Douglas Morrison's selective and biased reporting. His description of this stage of the experiments starts with an incomplete quotation of a single sentence in our paper. The full sentence reads:

"We also draw attention to some further important features: provided ***satisfactory electrode materials* are used, the reproducibility of the experiments is high;** following the boiling to dryness and the open-circuiting of the cells, the cells nevertheless remain at a high temperature for prolonged periods of time (fig 11); furthermore the Kel-F supports of the electrodes at the base of the cells melt so that the local temperature must exceed 300 degrees C".

Douglas Morrison translates this to: "Following boiling to dryness and the open-circuiting of the cells, the cells nevertheless remain at high temperature for prolonged periods of time; furthermore the Kel-F supports of the electrodes at the base of the cells melt so that the local temperature must exceed 300 degrees C".

Readers will observe that the most important part of the sentence, which we have underlined, is omitted; we have italicised the words "satisfactory electrode materials" because that is the nub of the problem. In common with the experience of other research groups, we have had numerous experiments in which we have observed zero excess enthalpy generation. The major cause appears to be the cracking of the electrodes, a phenomenon which we will discuss elsewhere.

With respect to his own quotation Douglas Morrison goes on to say: "No explanation is given and fig 10 is marked 'cell remains hot, excess heat unknown'". The reason why we refrained from speculation about the phenomena at this stage of the work is precisely because explanations are just that: speculations. Much further work is required before the effects referred to can be explained in a quantitative fashion. Douglas Morrison has no such inhibitions, we believe mainly because in the lengthy section *Stage 5 Effects* he wishes to disinter "the cigarette lighter effect". This phenomenon (the combustion of hydrogen stored in palladium when this is exposed to the atmosphere) was first proposed by Kreysa et al [8] to explain one of our early observations: the vapourisation of a large quantity of D{2}O ([[approx.]] 500ml) by a 1cm cube palladium cathode followed by the melting of the cathode and parts of the cell components and destruction of a section of the fume cupboard housing the experiment [9]. Douglas Morrison (in common with other critics of "Cold Fusion") is much attached to such "Chemical Explanations" of the "Cold Fusion" phenomena. As this particular explanation has been raised by Douglas Morrison, we examine it here.

In the first place we note that the explanation of Kreysa et al [8] could not possibly have applied to the experiment in question: the vapourisation of the D{2}O alone would have required [[approx.]]1.1MJ of energy whereas the combustion of all the D in the palladium would at most have produced [[approx.]] 650J (assuming that the D/Pd ratio had reached [[approx]] 1 in the cathode), a discrepancy of a factor of [[approx.]] 1700. In the second place, the timescale of the explanation is impossible: the diffusional relaxation time is [[approx.]] 29 days whereas the phenomenon took at most [[approx.]] 6 hours (we have based this diffusional relaxation time on the value of the diffusion coefficient in the alpha-phase; the processes of phase transformation coupled to diffusion are much slower in the fully formed Pd-D system with a corresponding increase of the diffusional relaxation time for the removal of D from the lattice). Thirdly, Kreysa et al [8] confused the notion of power (Watts) with that of energy (Joules) which is again an error which has been promulgated by critics seeking "Chemical Explanations" of "Cold Fusion". Thus Douglas Morrison reiterates the notion of heat flow, no doubt in order to seek an explanation of the high levels of excess enthalpy during *Stage 4* of the experiments. We observe that at a heat flow of 144.5W (corresponding to the rate of excess enthalpy generation in the experiment discussed in our paper [2] the total combustion of all the D in the cathode would be completed in [[approx.]] 4.5s, not the 600s of the duration of this stage. Needless to say, the D in the lattice could not reach the surface in that time (the diffusional relaxation time is [[approx.]] 10{{5}}s) while the rate of diffusion of oxygen through the boundary layer could lead at most to a rate of generation of excess enthalpy of [[approx.]] 5mW.

Douglas Morrison next asserts that no evidence has been presented in the paper about stages three or four using H{2}0 in place of D{2}0. As has already been pointed out above he has failed to comment on the extensive discussion in our paper of a "blank experiment". Admittedly, the evidence was restricted to stages 1 and 2 of his own classification but a reference to an *independent review of our own work* [10] will show him and interested readers that such cells stay in thermal balance to at least 90 degrees C (we note that Douglas Morrison was present at the Second Annual Conference on Cold Fusion). We find statements of the kind made by Douglas Morrison distasteful. Have scientists now abandoned the notion of verifying their facts before rushing into print?

In the last paragraph of this section Douglas Morrison finally "boxes himself into a corner": having set up an unlikely and unworkable scenario he finds that this cannot explain Stage 5 of the experiment. In the normal course of events this should have led him to: (i) enquire of us whether the particular experiment is typical of such cells; (ii) to revise his own scenario. Instead, he implies that our experiment is incorrect, a view which he apparently shares with Tom Droege [11]. However, an experimental observation is just that: an experimental observation. The fact that cells containing palladium and palladium alloy cathodes polarised in D{2}0 solutions stay at high temperatures after they have been driven to such extremes of excess enthalpy generation *does not present us* with any difficulties. It is certainly possible to choose conditions which also lead to "boiling to dryness" in "blank cells" but such cells cool down immediately after such "boiling to dryness". If there are any difficulties in our observations, then these are surely in the province of those seeking explanations in terms of "Chemical Effects" for "Cold Fusion". It is certainly true that the heat transfer coefficient for cells filled with gas (N{2}) stay close to those for cells filled with 0.1M LiOD (this is not surprising because the main thermal impedance is across the vacuum gap of the Dewar-type cells). The "dry cell" must therefore have generated [[approx.]]120kJ during the period at which it remained at high temperature (or [[approx.]] 3MJcm-{{3}} or 26MJ(mol Pd)-{{1}}). We refrained from discussing this stage of the experiments because the cells and procedures we have used are not well suited for making quantitative measurements in this region. Inevitably, therefore, interpretations are speculative. There is no doubt, however, that Stage 5 is probably the most interesting part of the experiments in that it points towards new systems which merit investigation. Suffice it to say that energies in the range observed are not within the realm of any chemical explanations.

We do, however, feel that it is justified to conclude with a further comment at this point in time. Afficionados of the field of "Hot Fusion" will realise that there is a large release of excess energy during Stage 5 at zero energy input. The system is therefore operating under conditions which are described as "Ignition" in "Hot Fusion". It appears to us therefore that these types of systems not only "merit investigation" (as we have stated in the last paragraph) but, more correctly, "merit frantic investigation".

Douglas Morrison's Section "Conclusions" and some General Comments

In his section entitled "Conclusions", Douglas Morrison shows yet again that he does not understand the nature of our experimental techniques, procedures and methods of data evaluation (or, perhaps, that he chooses to misunderstand these?). Furthermore, he fails to appreciate that some of his own recommendations regarding the experiment design would effectively preclude the observation of high levels of excess enthalpy. We illustrate these shortcomings with a number of examples:

(i) Douglas Morrison asserts that accurate calorimetry requires the use of three thermal impedances in series and that we do not follow this practice. In point of fact we do have three impedances in series: from the room housing the experiments to a heat sink (with two independent controllers to thermostat the room itself); from the thermostat tanks to the room (and, for good measure, from the thermostat tanks to further thermostatically controlled sinks); finally, from the cells to the thermostat tanks. In this way, we are able to maintain 64 experiments at reasonable cost at any one time (typically two separate five-factor experiments).

(ii) It is naturally essential to measure the heat flow at one of these thermal impedances and we follow the normal convention of doing this at the innermost surface (we could hardly do otherwise with our particular experiment design!). In our calorimeters, this thermal impedance is the vacuum gap of the Dewar vessels which ensures high stability of the heat transfer coefficients. The silvering of the top section of the Dewars (see Fig 2 of our paper [2] further ensures that the heat transfer coefficients are virtually independent of the level of electrolyte in the cells.

(iii) Douglas Morrison suggests that we should use isothermal calorimetry and that, in some magical fashion, isothermal calorimeters do not require calibration. We do not understand: how he can entertain such a notion? All calorimeters require calibration and this is normally done by using an electrical resistive heater (following the practice introduced by Joule himself). Needless to say, we use the same method. We observe that in many types of calorimeter, the nature of the correction terms are "hidden" by the method of calibration. Of course, we could follow the self-same practice but we choose to allow for some of these terms explicitly. For example, we allow for the enthalpy of evaporation of the D{2}0. We do this because we are interested in the operation of the systems under extreme conditions (including "boiling") where solvent evaporation becomes the dominant form of heat transfer (it would not be sensible to include the dominant term into a correction).

(iv) There is, however, one important aspect which is related to (iii) i.e. the need to calibrate the calorimeters. If one chooses to measure the lower bound of the heat transfer coefficient (as we have done in part of the paper published recently in this journal [2]) then there is *no need to carry out any calibrations nor to make corrections.* It is then quite sufficient to investigate the time dependence of this lower bound heat transfer coefficient in order to show that there is a generation of excess enthalpy for the Pd-D{2}0 system whereas there is no such generation for appropriate blanks (e.g. Pt-D{2}0 or Pd-H{2}0). Alternatively, one can use the maximum value of the lower bound heat transfer coefficient to give lower bound values of the rates of excess enthalpy generation. It appears to us that Douglas Morrison has failed to understand this point *as he continuously asserts that our demonstrations of excess enthalpy generation are dependent on calibrations and corrections.*

(v) Further with regard to (iii) it appears to us that Douglas Morrison believes that a "null method" (as used in isothermal calorimeters) is inherently more accurate than say the isoperibolic calorimetry which we favour. While it is certainly believed that "null" methods in the Physical Sciences can be made to be more accurate than direct measurements (e.g. when a voltage difference is detected as in bridge circuits: however, note that even here the advent of "ramp" methods makes this assumption questionable) this advantage disappears when it is necessary to transduce the primary signal. In that case the accuracy of all the methods is determined by the measurement accuracy (here of the temperature) quite irrespective of which particular technique is used.

In point of fact and with particular reference to the supposed advantages of isothermal versus isoperibolic calorimetry, we note that in the former the large thermal mass of the calorimeter appears across the input of the feedback regulator. The broadband noise performance of the system is therefore poor; attempts to improve the performance by integrating over long times drive the electronics into 1/f noise and, needless to say, the frequency response of the system is degraded. (see also (vii) below)

(vi) with regard to implementing measurements with isothermal calorimeters, Douglas Morrrison recommends the use of internal catalytic recombiners (so that the enthalpy input to the system is just E{cell}.I rather than (E{cell} - E{thermoneutral, bath}).I as in our "open" calorimeters. We find it interesting that Douglas Morrison will now countenance the introduction of intense local "hot spots" on the recombiners (what is more in the gas phase!) whereas in the earlier parts of his critique he objects to the possible creation of microscopic "hot spots" on the electrode surfaces in contact with the solution.

We consider this criticism from Douglas Morrison to be invalid and inapplicable. In the first place it is inapplicable because the term E{thermoneutral,bath}.I (which we require in our analysis) is known with high precision (it is determined by the enthalpy of formation of D{2}0 from D{2} and 1/2 0{2}). In the second place it is inapplicable because the term itself is [[approx.]] 0.77 Watt whereas we are measuring a total enthalpy output of [[approx.]]170 Watts in the last stages of the experiment.

(vii) We observe here that if we had followed the advice to use isothermal calorimetry for the main part of our work, then we would have been unable to take advantage of the "positive feedback" to drive the system into regions of high excess enthalpy generation (perhaps, stated more exactly, we would not have found that there is such positive feedback). The fact that there is such feedback was pointed out by Michael McKubre at the Third Annual Conference of Cold Fusion and strongly endorsed by one of us (M.F.). As this issue had then been raised in public, we have felt free to comment on this point in our papers (although we have previously drawn attention to this fact in private discussions). We note that Douglas Morrison was present at the Third Annual Conference on Cold Fusion.

(viii) While it is certainly true that the calorimetric methods need to be evolved, we do not believe that an emphasis on isothermal calorimetry will be useful. For example, we can identify three major requirements at the present time: a) the design of calorimeters which allow charging of the electrodes at low thermal inputs and temperatures below 50 degrees C followed by operation at high thermal outputs and temperatures above 100 degrees C b) the design of calorimeters which allow the exploration of Stage 5 of the experiments c) the design of calorimeters having a wide frequency response in order to explore the transfer functions of the systems.

We note that c) will in itself lead to calorimeters having an accuracy which could hardly be rivalled by other methods.

(ix) Douglas Morrison's critique implies that we have never used calorimetric techniques other than that described in our recent paper [2]. Needless to say, this assertion is incorrect. It is true, however, that we have never found a technique which is more satisfactory than the isoperibolic method which we have described. It is also true that this is the only method which we have found so far which can be implemented within our resources for the number of experiments which we consider to be necessary. In our approach we have chosen to achieve accuracy by using software; others may prefer to use hardware. The question as to which is the wiser choice is difficult to answer: it is a dilemma which has to be faced frequently in modern experimental science. We observe also that Douglas Morrison regards complicated instrumentation (three feedback regulators working in series) as being "simple" wheres he regards data analysis as being complicated.

Douglas Morrrison also asserts that we have never used more than one thermistor in our experimentation and he raises this issue in connection with measurements on cells driven to boiling. Needless to say, this assertion is also incorrect. However, further to this remark is it necessary for us to point out that *one does not need any temperature measurement in order to determine the rate of boiling of a liquid?*

(x) Douglas Morrison evidently has difficulties with our application of non-linear regression methods to fit the integrals of the differential equations to the experimental data. Indeed he has such an idee fixe regarding this point that he maintains that we used this method in our recent paper [2]; we did not do so (see also 'stage 3 calculations' above). However, we note that we find his attitude to the Levenberg-Marquardt algorithm hard to understand. It is one of the most powerful, easily implemented "canned software" methods for problems of this kind. A classic text for applications of this algorithm [12] has been praised by most prominent physics journals and magazines.

(xi) Douglas Morrison's account contains numerous misleading comments and descriptions. For example, he refers to our calorimeters as "small transparent test tubes". It is hard for us to understand why he chooses to make such misleading statements. In this particular case he could equally well have said "glass Dewar vessels silvered in their top portion" (which is accurate) rather than "small transparent test tubes" (which is not). Alternatively, if he did not wish to provide an accurate description, he could simply have referred readers to Fig 2 of our paper [2]. This type of misrepresentation is a non-trivial matter. We have never used calorimeters made of test-tubes since we do not believe that such devices can be made to function satisfactorily.

(xii) As a further example of Douglas Morrison's inaccurate reporting, we quote his last paragraph in full:

"It is interesting to note that the Fleischmann and Pons paper compares their claimed power production with that from nuclear reactions in a nuclear reactor and this is in line with their dramatic claims (9) that **"'SIMPLE EXPERIMENT' RESULTS IN SUSTAINED N-FUSION AT ROOM TEMPERATURE FOR THE FIRST TIME**: breakthrough process has potential to provide inexhaustible source of energy". It may be noted that the present paper does not mention "Cold Fusion" nor indeed consider a possible nuclear source for the excess heat claimed."

Douglas Morrison's reference (9) reads: Press release, University of Utah, 23 March 1989. With regard to this paragraph we note that: a) our claim that the phenomena cannot be explained by chemical or conventional physical processes is based on the energy produced in the various stages and not the power output b) the dramatic claim he refers to was made by the Press Office of the University of Utah and not by us c) we did not coin the term "Cold Fusion" and have avoided using this term except in those instances where we refer to other research workers who have described the system in this way. Indeed, if readers refer to our paper presented to the Third International Conference on Cold Fusion [13] (which contains further information about some of the experiments described in [2]), they will find that we have not used the term there. Indeed, we remain as convinced as ever that the excess energy produced cannot be explained in terms of the conventional reaction paths of "Hot Fusion" d) it has been widely stated that the editor of this journal "did not allow us to use the term Cold Fusion". This is not true: he did not forbid us from using this term as we never did use it (see also [13]).

(xiii) in his section "Conclusions", Douglas Morrison makes the following summary of his opinion of our paper:

The experiment and some of the calculations have been described as "simple". This is incorrect - the process involving chaotic motion, is complex and may appear simple by incorrectly ignoring important factors. It would have been better to describe the experiments as "poor" rather than "simple".

We urge the readers of this journal to consult the original text [2] and to read Douglas Morrison's critique [1] in the context of the present reply. They may well then come to the conclusion that our approach did after all merit the description "simple" but that the epithet "poor" should be attached to Douglas Morrision's critique.

Our own conclusions

We welcome the fact that Douglas Morrison has decided to publish his criticisms of our work in the conventional scientific literature rather than relying on the electronic mail, comments to the press and popular talks; we urge his many correspondees to follow his example. Following this traditional pattern of publication will ensure that their comments are properly recorded for future use and that the rights of scientific referees will not be abrogated. Furthermore, it is our view that a return to this traditional pattern of communication will in due course eliminate the illogical and hysterical remarks which have been so evident in the messages on the electronic bulletins and in the scientific tabloid press. If this proves to be the case, we may yet be able to return to a reasoned discussion of new research. Indeed, critics may decide that the proper course of inquiry is to address a personal letter to authors of papers in the first place to seek clarification of inadequately explained sections of publications.

Apart from the general description of stages 1-5, we find that the comments made by Douglas Morrison are either irrelevant or inaccurate or both.

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Date: *Tue, 18 Jul 2000 14:48:28 GMT*
From: **Jed Rothwell <JedRothwell@infinite-energy.com>**
Subject: **Re: Response to comments on my cold fusion status report.**

Date: *Mon, 17 Jul 2000 13:15:39 -0700*
From: **Edmund Storms <storms2@ix.netcom.com>**
Subject: **Debate**

Dear Douglas,

I would like to suggest an approach which might help clarify our different opinions about cold fusion. Your review of ICCF-8 and my comments about your review, I suggest, would make a good starting point for a debate between us about the subject. If you agree, this exchange could be published on the internet as part of Vortex-l and/or sci.physics.fusion. In addition, this will give other people a chance to raise issues we might have missed. To start the ball rolling, I have extracted the comments you made about my comments from your longer reply, to which I will reply. If you think this is a worthwhile project, I would invite you to respond in kind.

Best regards,

Ed Storms

I would like to thank Ed for the serious tone of his letter - so different from some other communications. On the other hand, was surprised by some of his phrases which seemed out of character, such as "you distort the facts and give a completely false impression", "you completely ignored", "you should at least represent the controversy honestly". Well, I will try and respond to his points paragraph by paragraph;

I will avoid such comments in the future because I realize you do not believe you are distorting facts, any more than I believe I'm doing such a thing. I will simply state the facts and allow the readers to come to their own conclusions.

Para. 1 and 2 - no comment - expressions of opinion.

P3. You asked where is the "Overwhelming evidence" against cold fusion? For this see the paper "Review of Cold Fusion" which I presented at the ICCF-3 conference in Nagoya - strangely enough it seems not to have been published in the proceedings despite being an invited paper - will send a copy if desired. As Dieter Britz has shown, most cold fusion papers were published before 1993 and are therefore in my summary. There it is shown that for every subject (excess heat, neutrons, tritium, 4He, 3He, Gammas, protons) there are more null papers than positive papers. Further, and which is very damning, the quality papers almost all show null effects. The fact that cold fusion is in contradiction with a vast body of research, is expressed by saying that from this research work, theories have been developed which are in agreement with the experimental results. Thus when it is written that cold fusion is in disagreement with theory, this basically means that it is in disagreement with the overwhelming experimental evidence on which the theory is justified.

**While it is true, many papers as well as much unpublished work show null effects, this does not provide "overwhelming evidence" as you claim. Early in the field's history, much was not understood about conditions needed to make the effect work. Also, most of the work was based on the original method proposed by P-F, a method which has been found to resist reproduction. As understanding developed, methods using finely divided palladium in ambient D2 gas, gas discharge techniques, and proton conductors have been more easily duplicated. In addition, in spite of the known difficulties inherent in the P-F method, positive results continue to be reported.

The second point you raise goes to quality of work. This issue is very subjective and is difficult to quantify in a short answer. I admit, much early work was either poorly done or showed obvious limitations, not all of which would be fatal. On the other hand, work at SRI (Stanford Research International) under the direction of Dr. McKubre employed very high quality calorimetry. This work showed anomalous energy in 19 samples, they showed the same patterns of behavior found in other equally good studies, and they revealed some of the requirements need to make the effect work. Surely, this study along with ones of the same high quality done in recent times should have some impact on the issue, and not be ignored in favor of poor work done in the distant past.

The third point involves theory. Here, the important issue is being ignored. The present theory of fusion is based on studies using high energy plasma or high energy ion bombardment. The theory applies very well to these conditions. However, cold fusion involves low energy and a solid environment of regularly spaced atoms, i.e. a lattice. To equate these two conditions is like trying to equate air and a rock. I realize that some scientists argue that the same type of reactions should result from, and the same rules should apply to both environments. However, this assertion is a matter of debate, not an absolute requirement of nature. As such, it can not be used as a basis for rejecting cold fusion unless the assertion is proven to be true. Competent theoreticians on both sides of the issue have made very good arguments for their respective views. We need to be patient and wait to see which side prevails.**

P4. Sorry for my mistake in misquoting you. I appreciate you making the point that theories should take into account metals other than palladium.

*This is an important point on which I would like to elaborate further. Because of the field's history, palladium has been given an extreme amount of attention. Early in the history, skeptics pointed out that palladium does not have the basic properties required to produce the effect. The atoms are too far apart, the electron structure is not sufficiently unique, and the claimed concentration of deuterium was too low to produce anomalous interaction. We now know that beta-PdD is not the active material. Instead, another phase having a very high deuterium content and having unknown electron and atom structures is the active material. We also know that many other metals, most of which do not absorb significant deuterium, are claimed to produce anomalous energy. Clearly, the conditions in which the anomalous effects occur are not understood and may, when they are understood, provide the mechanism demanded by skeptics. Again, we will just have to be patient.**

P5. Harwell - "subsequent work revealed the presence of overlooked excess energy". This is a completely misleading statement.

What I wrote was "Harwell did 127 varieties of experiment, and searched for excess heat, neutrons, gammas and tritons, but did not find any significant signal in any of them". Please note the phrase "significant signal".

Point taken. However, even P-F never claimed a significant signal by your definition.

Remember what happened; Before the press conference of 23 March 1989, Fleischmann talked to his friend David Williams, an electrochemist, and told him of a simple experiment that would verify his Utah work. Harwell assembled a multi-disciplinary team which spent half a million pounds on this "simple experiment". They tried to repeat Fleischmann and Pons work and could not get the same results - despite having Fleischmann's help!

**Of the many mistakes made by P-F, the worst was claiming the method was "simple" and could be easily reproduced.

As for Fleischmann's help, according to Fleischmann, Williams refused to accept the help, deciding instead to attempt a completely independent replication. If this approach had been successful, the work would have provided a more convincing proof than if Fleischmann had been involved. Unfortunately, they made some serious mistakes by ignoring Fleischmann's advice.**

Also there is the problem of analyzing these different results. For example, should they use Newton's Law of Cooling as Fleischmann and Pons did at that time with a T to the power one term, or should they guess that they should switch, as F&P did later, to using Stefan's Law with a T to the power four term? Strangely enough, this did not seem to worry Fleischmann and Pons!

If absolute calorimetry were being used, this issue would have been important. However, P-F used relative calorimeter based on a heater calibration and based on a result assumed to be null, measured during the long wait for anomalous heat. All that is required for their method to succeed is stability. This is why P-F were not worried. They would see a null signal for weeks, with periodic calibrations using the heater to make sure the calorimeter was stable. If they were lucky, the signal would rise above the null value. Again, the heater calibration was used to determine whether this increase was real or not. Use of T to the first power (Newton) or T to the 4th power (Stefan) would only influence the amount of anomalous heat claimed, not the existence thereof. Unfortunately, the description provided by P-F is very difficult to understand. As a result, what they did in the real world was not properly understood.

I wrote "When they used the best technology, they found no excess heat". Now "best technology" is not the Fleischmann and Pons technique. Hope you agree that when they used best technology (the null method), they found no excess heat? Would it be fair to ask you why did you "completely ignore", in your phrase, the best technology results of Harwell?

**Attributing failure to see anomalous energy only to the method used is not appropriate in this field because other variables are equally important. The sample is very important in producing the effect because potentially active samples are so rare. As I summarized in my review in Infinite Energy Vol. 6, Issue 31 page10, only a small fraction of samples from certain batches have been found to be active. Unless an active sample is transferred from one calorimeter to another, it is not possible to reach any conclusion about the role of the calorimeter. **

Now some desperate people looked at the data using not the best technology, and claimed that they had found excess heat - which David Williams et al. deny - they say that there were minor statistical fluctuations but when all the results were combined, there was no significant signal. And what I wrote on page 16 was "did not find any significant signal".

I am sorry that you have adopted the position of certain people who search for the slightest fluctuation and claim that this particular run showed excess heat while neglecting all the other runs which show that there is no significant signal. Further, and what is worse, they neglect the very careful work done with one of the world's best calorimeters where they have three temperature controlled water baths round the object being studied - this is a super-Wheatstone bridge technique. The major point is, that it is much better to do a good experiment to show that outside (room) temperature effects are not important by eliminating them, rather than doing a poor experiment where one has to do doubtful calculations to try to prove that heat exchange with the environment is not important or is adequately corrected for..

On the other hand, McKubre used a water bath stable to +/-0.003 deg and calorimeters stable to <0.05 watts in which he detected heat up to 2 watts on one occasion and heat significantly above the detection limit on 19 occasions, yet you ignore this work. As you have suggested, I have included in my reviews the fact that the effect is difficult to produce no matter what kind of calorimeter is used, good or bad. In contrast, I also include in my reviews the fact that many people have produced the effect and each has seen the same pattern of behavior, i.e. a relationship to applied current, a relationship to the D/Pd ratio, and a relationship to the properties of the palladium used. These patterns can not be produced by chance or error alone. Why do you not include and evaluate these observations in your reviews?

The Harwell series of experiments were magnificent and it is pretty mean to look for a fluctuation and to try and ignore the totality of their results on neutrons, tritium. gammas and tritons, apart form excess heat with what was probably the world's best calorimeter.

Everyone, believer and skeptic alike, admits that neutron emission is very rare and at a very low level, much below the detection limit of Harwell. Tritium is produced only very rarely and under conditions different from those that produce heat. Apparently, microwhiskers of metal plated on the cathode surface are required, a bit of information not known at the time of the Harwell study. Gamma emission is absent even when helium is being produced, much to the disappointment of skeptics. On the other hand , tritons and alpha emission have been detected when the work is done under conditions which permit their detection. Failure of Harwell to see these other anomalous effects is not the issue at the present time.

P5A. Similar comment about the NHE lab experiments in Japan. But here we can make a more precise statement - which in fact is in my report but I see it needs expanding to make it clear to all.

I wrote two paragraphs about Miles's visit to NHE lab. He and Fleischmann claimed to find exceptional excess heat peaks. But they were all very small (much smaller than the Fleischmann and Pons claims incidentally). This was answered by the NHE people at ICCF-7 when they said that there were fluctuations but these fluctuations were always within a few standard deviations and therefore did not represent significant signals of excess heat. In my report, I quoted that Miles claimed errors of +/- 20 mW while NHE people said the errors were ten times bigger, +/- 200 mW.

It is easy to say errors are 20 mW or 200 mW, but it is much more difficult to prove these assertions. Miles went to some trouble in his paper to justify his claim of 20 mW. The NHE people simply stated their value as a belief. Yet, you emphasize the 200 mW value. Why?

Now the General Electric group who did a thorough analysis of the Fleischmann and Pons work, concluded that F&P's calculated errors were far too small (the response of F&P did not answer the points made by the GE group of Wilson et al.).

The GE group came to the conclusion that the error claimed by P-F was too small, but it was not large enough to cause them to reject all of the P-F claims. On the other hand, their failure to reproduce the effect caused them to reject the P-F claims, not the error analysis. Hansen also evaluated the P-F work and also came to the conclusion that the errors were well below the claimed anomalous energy. (See Storms, Review of the 'Cold Fusion' Effect, J. Sci. Exploration 10 (1996) 185 for more details). Three published and many unpublished evaluations of the P-F errors have come to the conclusion that errors in calorimetry did not produce the claimed anomalous results. Perhaps you might want to examine the literature in this area in more detail.

However this question may be settled another way. It is universally agreed that the excess heat claimed is not reproducible - even by True Believers. Then for a True Believer, the result of a series of runs should be a combination of two sets of results - firstly, a Gaussian distribution of random fluctuations with a certain standard deviation, and secondly, some runs where excess heat occurs and this would have a different distribution with a significantly higher average value. So, combining these two sets of runs, one would expect a messy distribution of excess heat values. But the actual results found as I wrote, "the distribution of fluctuations gave a perfect Gaussian distribution with three standard deviation limits of +/- 2.3% with no indication of excess heat occurring spasmodically".

I hope this is clearer to all now.

** This approach is valid when a process is being influenced by random variables, and it is suggested here because skeptics believe the anomalous effects are caused by random error in the calorimetry. However, all of the work shows that the effect is not random. It depends on the nature of the palladium, i.e. it being crack-free, and on the particular batch used. As Miles published, and other people have experienced, once a piece of palladium becomes active, it stays active and can be made to produce anomalous energy at will. Miles took an active piece which made anomalous energy at China Lake in the US and showed the same effect at NHE in Japan. A dead piece was dead at both places while using the same calorimeter. **

P6. I am sorry that in one place I missed out Russia as an important collaborator. However, I did mention them extensively elsewhere and indeed Appendix 3 is devoted to them.

Incidentally, I had lunch today with the Director of a major Russian Laboratory who is an excellent physicist, and he was very surprised to hear that someone in his lab was publicly involved in cold fusion.

I hope you did not blow someone's cover.

6A. Do not understand the comment about India - I was only talking about countries where experiments were being done now. I was not making a list of countries which have stopped such as Spain which could not find neutrons after I visited the group.

Point taken.

P7. I do not think that the balance of publications on the reliability of the Fleischmann and Pons methods, is in favour of them. As I wrote above, the most complete and serious analysis was that done by the General Electric group and I would strongly recommend everyone to return and study their paper carefully.

** I agree. Also study Hansen, "Report to the Utah State Fusion/Energy Council on analysis of selected Pons Fleischmann calorimeter data", Proc of the Second Annual Conference on Cold Fusion, June 29-July 4, 1991, page 491. (Available from Infinite Energy.)**

P8. You say the "more modern methods of data collection which are as accurate and certainly more convenient than this " null method. Well. I am an experimentalist. If there is any doubt, then "you should try and prove yourself wrong" and use both methods. I do not admire the lazy way of saying this is "more convenient" and then do some unclear calculations to support this point of view. This is not the way of good scientists - they do the work.

** Most people in the field do try to prove themselves wrong. However, one does not have to use methods more appropriate to Faraday to do this. Modern data acquisition is very reliable and, in most cases, is made redundant. Each person's approach needs to be examined rather than insisting that everyone use a particular 'null method'. In fact, some of the methods have a null method built in because they compare the potentially active cell to a dead cell, the anomalous energy being the difference between the output of the two cells. **

P9. Answer as above. "The Jones work has been completely discredited" - could you please send me a publication where Fleischmann and Pons repeated the very simple and inexpensive Jones(actually Lee Hansen) experiments? Experimentalist do experiments.

Also could you send me any publication which "discredits" the Provo results?

P-F did not repeat the J-H work because it has no relationship to their work. P-F measured the amount of deuterium lost from the cell and compared this to the amount expected from applied current. No recombination was detected within +/-1%. As I show in my review in Infinite Energy 6, #31 (2000) 10, the applied current determines the amount of recombination. J-H used a very low current where recombination is high, while P-F used a high current where recombination is low. J-H made fools of themselves by ignoring this effect and by claiming that all anomalous energy can be explained by unrecognized recombination, while ignoring those claims for anomalous energy obtained from sealed cells containing a recombiner - a situation in which recombination is total.

P9a. On the 31 March 1989, to which I refer, Fleischmann did not say that he had done a control experiment with light water - he said that the 8 mm rod that gave no effect, was their control! This I checked by looking at the video tape of Fleischmann's talk.

Fleischmann said many things in the past which were wrong or incomplete. I'm sure you have done the same thing. The question is, what does this have to do with the present discussion about the reality of the claims? P-F published 11 null studies involving Pd-H2O or Pt-D2O, all of which showed no anomalous energy. Their failure to do many null studies early in their work, I suggest, has no bearing on the present situation.

P10. Sorry if I was confusing. Your conclusion is that "the theory that you and others use to discredit cold fusion is not so perfect after all". Well, I was being polite. There are two possibilities - either the hundreds of experiments that have been made previously are wrong, or the new and very difficult experiments of Dr. Kasagi is wrong. Which do you choose?

No other measurements of the fusion cross section exist at the low energy being explored by Kasagi. In addition, Kasagi is exploring this reaction in a lattice, not in a plasma in which most of the studies you note were made. The choice you suggest simply does not exist. Even ôconventionalö physicists are interested in the Kasagi work because it is very straightforward and very conventional in its approach. You might reasonably object to it having any relationship to cold fusion, but that is a different approach from the one you have chosen.

You may remember my polite conclusion; "These values are very high and merit checks". Too bad that you force me to reverse my politeness.

Further, I discussed the possible effect of secondary interactions, which you seem to have missed.

** Your comments are all reasonable challenges to the Kasagi work. Nevertheless, the results do open some new issues in trying to explain the CF claims, do you not agree?**

P11. This is interesting. I had been told that Champion came to Bockris and asked to be his grad student but Bockris was not interested, until he was told that $200,000 would be given to his funds for research. Now you say this is "completely false". Your story is that "Champion hired Prof. Bockris". Well, that does not sound good. One would expect a Distinguished Professor like Bockris would check out anyone who wanted to hire him? and find out the source of the money and if Champion had a criminal record? The claims that you talk about - are they the conversion of mercury to gold? If so would a Distinguished Professor not have some doubts? How would you react to such an offer?

The money was supplied by Mr Teelander, a rich investor, and the whole situation was checked out by the University, and approved. Universities accept grants to do research all the time, especially when amounts as high as $200,000 are involved. As for my approach, Champion asked me to do the work at LANL, which I refused because I did not think there was a snow ball's chance in Hell of getting approval. Nevertheless, the experiments were interesting and the claims, although hard to believe, are important. The question is, does a person reject an idea just because it is hard to believe or does a person go to a little trouble to check it out, especially for $200,000? Unfortunately, John Bockris, like the good scientist he is, checked it out, found positive results, and then paid a dear personal price for his efforts. But that is the nature of the present system in science these days - a system you seem to want to defend.

P12. My mistake if only one of Arata and Case used activated carbon. I will correct this and other mistakes.

P13. Thank you for your best wishes for more accurate work. I will try and do so. May I humbly suggest on my part, that you consider the possibility that 99.9% of scientists are correct in their opinion of cold fusion and try to re-evluate all the experiments that you like and also those that you do not like, with the thought that maybe cold fusion does not and cannot exist?

** Well, Douglas, I have done this over the years each time I write another review, of which four are now in print. In addition, I have seen the effect work with my own eyes even though I have tried to prove my self wrong. I have built over 9 calorimeters of various designs, I have studied the variables which produce error, and I have studied palladium to determine its important properties. All of this work is published in 21 papers, some in peer reviewed journals. At least to me, the work proves the reality of the claims. Can you say you have done as much to reach your conclusion?**

(more accurately, could only exist with a very low probability of 10^-40.

Also could you please do experiments and not make calculations (no doubt using a non-linear regression analysis with Kalman filtering) to disprove things such as recombination in the Hansen manner.

A person can do all the filtering or non-linear regression analysis a person can stand, but this will have no usefulness if the phenomenon being analyzed has no relationship to the claims being made. As I note above, Hansen's studies are completely irrelevant.

When a group of excellent scientists thought that Steve Jones was the only recuperable cold fusion experimentalist, they took him aside and asked him to segment his counters and see if he got the expected result. He did segment them and realized that all his claims of neutron bursts were false. Then he awoke and realized that cold fusion was crazy - but then he asked, how come these other guys are getting results that are obviously wrong? So with Lee Hansen, he did some trivial experiments which any self-respecting experimentalist would have done ages ago, and showed how you can get false results of excess heat.

** Yes, this is a fair description. Jones knew the effect could not be true so he set out to discover the mistakes other people were making. He showed that recombination operates in cells to which a few mA are applied. Rather than trying to show himself to be wrong by going to a higher current, he concluded that recombination was occurring in the P-F cells to which hundreds of mA were being applied, this was in spite of direct measurements by P-F showing that recombination was not taking place in their cells. To prove either Jones or P-F wrong, I studied recombination as a function of applied current. This work, published in Infinite Energy, shows that Jones is wrong and P-F are correct. Perhaps you would like to comment on this work and forget Jones.**

So Ed, is there any change you can make to your experiments which is the equivalent of segmenting Jones's counters? For example, using a null method as Harwell did, or as Tom Droege did on a smaller scale?

Maybe the committee was underestimating and you are also recuperable? Please think about it and do simple experiments to try and prove yourself wrong such as blowing nitrogen gas between the electrodes every time you think that you have excess heat.

** The question that naturally comes up when anomalous heat is observed is, what aspect of the measuring system could have failed. After all, only a few measurements are involved, i.e. temperature, the cooling water flow rate, and the applied power. All of these variables can be checked independently. In my case, I use a sealed cell containing a recombiner. Therefore, recombination is not an issue and blowing nitrogen would serve no purpose. On the other hand, when I obtained anomalous energy using Pt, I tried changing the current and showed that aspects of the behavior were completely reproducible. All of this work was published on the internet and was evaluated by many skeptics. As a result of their comments, I made additional measurements in an attempt to find the source of the energy. At this point, the excess energy is very difficult to explain by operation of conventional processes.**

Morrison's Comments Criticized

Date: ***Tue, 18 Jul 2000 14:52:00 GMT***
From: Jed Rothwell <JedRothwell@infinite-energy.com>
Subject: Re: Fleischmann's original response to Morrison's lies

Dieter Britz <db@kemi.aau.dk> wrote:

>This is interesting. Rothwell, would you please tell us the origin of
>this text?

It was originally posted here, in sci.physics.fusion, in August 1993, with the attached introduction. I believe a version of it was later publish in Phys. Letters A, in response to the Morrison paper published there.

- JR

Originally-From: mica@world.std.com (mitchell swartz)
Subject: Morrison's Comments Criticized
Date: Tue, 17 Aug 1993 13:30:44 GMT
Organization: The World Public Access UNIX, Brookline, MA

Dear Colleagues:

There has been considerable misinformation circulating about the paper by Drs. Fleischmann and Pons in Physics Letters A,176 (1993), May 3. We were particularly repelled by the various outlandish criticisms made repeatedly in this electronic forum by Douglas O. Morrison, which were transparently intended to tear down the work of other scientists without regard for the facts. Dr. Morrison's stubborn belief that cold fusion research is "pathological science" is incorrect. Continuing to push that idea does not serve him well, nor does it help the cause of understanding the extraordinary phenomena associated with hydrogen-loaded metals that have been revealed in numerous experiments these past several years. Accordingly, we have decided to post the document that follows, which was prepared by Drs. Pons and Fleischmann and which was previously circulating within the cold fusion community.

Best wishes.

Sincerely,

Dr. Eugene F. Mallove
Dr. Mitchell R. Swartz

Date: Wed, 28 Mar 2001 14:54:38 GMT
From: Rich Murray <rmforall@earthlink.net>
Subject: Douglas R. O. Morrison: Feb 25 2001

Douglas R. O. Morrison: Feb 25 2001

Date: Wed, 28 Mar 2001 13:43:22 +0200
From: Douglas Morrison <douglas.morrison@cern.ch>
Organization: CERN
To: Rich Murray <rmforall@earthlink.net>

Dear Sir, Madam,
It is with great sadness that I inform you that my father, Dr. Douglas R.O. Morrison, passed away on February 25, 2001, after a short illness.
Sincerely, Fiona Morrison-Cassidy

Should you need to contact me or our mother please send me an email to my home at: andrew.cassidy@bluewin.ch

Thank you.
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http://www.hawaii.edu/News/kulama/000211/events.html
UHM Physics colloquium: Future World Energy and Climate--
The Rise of Renewable Energies, by Douglas R.O.
Morrison, CERN, Geneva, Switzerland, 3:30 p.m.
Feb. 17, 2000 Watanabe 112. (956-2937)

http://www.natureasia.com/get.pl5/contents/contents001102.en.shtml
Now you see it, now you don't Nov 2 2000
DOUGLAS R. O. MORRISON reviews
The Undergrowth of Science: Delusion, Self-deception and
Human Frailty by Walter Gratzer
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http://www.google.com
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