C: cat koonin516.1
From arpa!ruthep.rutgers.edu!ABRAHAMS Tue May 16 10:52:59 1989
Date: Tue, 16 May 89 10:18 EDT
From: Elihu Abrahams <ABRAHAMS@ruthep.rutgers.edu>
Subject: koonin report

X-Vms-To: IN%"mmb@clockwise [illegible] m"
Status: R

From: IN%"koonin@SBITP.BITNET" 12-MAY-1989 22:33:26.68
To: abrahams@ruthep.BITNET
CC:
Subj:

Received: from JNET-DAEMON by ruthep.rutgers.edu; Fri, 12 May 89 22:31 EDT
Received: From SBITP(MAILER) by RUTHEP with Jnet id 2519 for ABRAHAMS@RUTHEP;
Fri, 12 May 89 22:31 EDT
Date: Fri, 12 May 89 16:03:59 PDT
From: koonin@SBITP.BITNET
To: abrahams@ruthep.BITET
Message-Id: <8905121603591.1d1a@sbitp.ucsb.edu>
X-ST-Vmsmail-To: ST%"abrahams@ruthep.bitnet"

May 9, 1989
11:30 PDT

There follows an eyewitness report on the speical session on Electrochemically Induced Cold Fusion sponsored by The Electrochemical Society last night in Los Angeles. There were about 2000 people in attendance at the Bonaventure Hotel. No cameras or recording devices were allowed in the hall, so what follows is a somewhat subjective and incomplete report based on my notes. I will also restrict my report to what I consider new information that was presented.

PROGRAM:

The scientific program of the meeting was:

--"Electrochemically-Induced Nuclear Fusion of Deuterium" (S. Pons and M. Fleischmann) 40 min.

--"Observation of Cold Nuclear Fusion in Condensed Matter" (S. E. Jones et al.) 25 min.

--"Thermal measurements of D-Pd and H-Pd Systems" (R. A. Huggins et al., Stanford) 15 min.

--"Observations of Heat Generation, Increased Tritium Concentration, and Enhanced Neutron Count in the Electrolysis of Deuterium Oxicde on Palladium Cathodes" (U. Landau et al., Case Western Reserve) 10 min.

--"Mass Spectrometric Detection of Hydrogenic Species during Electrolysis of D2O at a Palladium Cathode" (E. Struve et al., University of Washington) 10 min.

--"Electrochemically-induced Fusion of Deuterium: The Search for Neutrons and Fusion Products" (J. Jorne et al., University of Rochester) 10 min.

--"Evidence for Excess Heat Generation During Electrolysis of D2O (Pd Cathode/Pt Anode) in LiOD - A microcalorimetric Investigation" (S. Srinivasan et al., Texas A&M) 10 min.

--"The Fleischmann-Pons Effect: Facts and Theory at an Early Stage of Investigation" (J. Bockris et al., Texas A&M) 10 min.

--"Calorimetry, Neutron Flux, Gamma Flux, and Tritium Yield from Electrochemically Charged Palladium in D2O" (N. Lewis et al., Caltech) 10 min.

These talks were followed by about an hour of questions from the audience to all of the speakers. The (by now obligatory) press conference followed.

and FLEISCHMANN:

The most surprising part of the Pons-Fleischmann presentation was how little things had changed. It was basically a rehash of the same material we've been seeing for the last month. All of the Tables and Figures from the published paper (J. Electroanal. Chem.) were shown, with no apologies. The 10**27 atmospheres was still there, as was the completely fictitious "Column C" with the arbitrary 0.5 V.

Pons mumbled alot and his voice dropped significantly as he was discussing the gamma line. Under intense questioning in the session and at the later press conference, they admitted that "The gamma peak is wrong". However, they kept emphasizing that they place greatest reliability on the calorimetry data and that they are setting up a GeLi spectrometer to do better than with NaI.

Nothing definitive was said about their 3H measurements or about the 4He data many of us had expected them to announce. Several groups (MIT and Sandia) offered to make rapid (3 day) analyses of small pieces of the Utah cathodes for fusion products. But P/F responded that they were in the process of long charging experiments and that they had already made "other arrangements" for such an analysis. Who was going to do it or how long it would take was not revealed.

Two new tidbits were thrown to the crowd. The first was a brief video of one of their cells bubbling away. They dumped some dye (phenolpthaline, if I've spelled it right) in the top, and you could see it diffuse through the cell in something less that 20 seconds. As this is much longer than the thermal time constant, 1600 sec, they conclude that their cell was well-mixed, without temperature gradients. Of course, the real proof, namely a measurement of the temperature as a function of the vertical distance, was not presented, although they said they had the numbers. The second new item was the report of calorimetric data showing a ''burst'' of excess heat production of about 1 Watt lasting for about 2 X 10**5 sec. and producing 4.2 MJ. This is roughly 50 times the Joule heating going to the cell.

There was no direct repsonse to many of the criticisms that were raised last week in Baltimore and I saw lots of prevarication and evasion in the questions they had to respond to. Fleischmann said he had to see full details published in a refereed journal before he could respond (talk about the pot calling the kettle black!) None of the pieces of hard data that are required to prove their claim were shown. I can't understand why they would be holding tese back now, as their scientific priority is surely guaranteed. However, they did say at the press conference that the published paper has "lots of omissions and mistakes in it". This is, of course, hardly news.

JONES:

The new pieces of data presented were reports of

1) continuing experiments at BYU using electrolytes with Pd+, Li+, and H+/D+ ions in them. Still seeing 2.5 MeV neutrons at about 0.04/sec.

2) Ongoing experiments in collaboration with LANL using 3He proportional counters. Nothing yet announced.

3) Work in collaboration with Bologna in the Gran Sasso using liquid scintillators with pulse-shape discrimination. The neutron signal seen in a counter near the cell turns on and off with the cell current, while a counter

remote from the cell shows nothing. They see a signal of about 50 neutrons per hour lasting for about 3 hours, before it urns off. There is some evidence that the energy is 2.5 MeV, but the energy resolution is poor. Overall, it's a 7

sigma effect.

My general impression is that Jones is carefully following up previous work and it's looking somewhat more positive than before.

HUGGINS:

Reported only calorimetry involving direct comparisons between light and heavy water cells. There was no direct stirring, but he did say gyroscopic motion of the whole apparatus (water bath included) was used to keep things well mixed. He reports that the heavy-water cells run conistently hotter that those with light-water, but there are lot's of reasons why this might be so and he didn't address them very carefully. Bottom line is a claim of for 15% excess energy production in a short (35 hr) run and up to 10 MJ/mole Pd in longer runs.

Huggins stressed that an important experimental consideration was the microstructure and interstitial content of the Pd. Thought it better to use cast vs. wrought samples and to arc melt them to drive out hydrogen that might be in there. Also emphasized the Carbon contamination was a no-no, so that graphite crucibles were out. However, it was pointed out to him that commercial suppliers use graphite containers in making their Pd.

LANDAU:

Emphasized that this was a progress report of preliminary results. Ran 4 cells including light/heavy water comparision, and a Pt cathode instead of Pd. Claims excess heat of 0.144 W (6 W/cm^3 Pd) at a current density of 255 mA/cm^2 with D2O but nothing (0.025W) with H2O. No tritium observed beyond that expected from electrochemical separation. Many neutron points at the 3-4 sigma level were presented, but nothing convincing.

STRUVE:

Reported on mass-spectrometry of gases evolved at a Pd cathode in 99.95% pure D2O but no(!) electrolyte. They saw A=2,3,4,5,6 signals with a quadrupole mass spec and got excited by the latter two peaks, which could be attributed to DT and TT molecules. However, a higher-precision instrument capable of distinguishing nuclear mass showed that A55,6 were due to tri-atomic hydrogen ions. It seems to me that any experienced mass spectrometrist would have recognized this immediately, and not held a press conference. The only excuse is that they were graduate students, I guess.

JORNE:

Looked for neutrons (nice scintillation counter, carefully calibrated efficiency), gammas, and fusion products. Limit on the neutron rate was <0.5/sec. P/F would imply 4 X 10^4/sec, while Jones would imply 0.4/sec. The limit on the gamma rate is 44/sec.

In a search for fusion products, they looked for 3H in the Pd with a mass spectrometer. They say A54 (DD molecules) and A=3 (HD, perhaps, or 3H) There is

''some suggestion'' that they're seeing 3H. but they can't prove it. I don't know what's making this suggestion to them.

SRINIVASAN:

This and the following talk were clearly the most interesting in the session. He

hour lasting for about 3 hours, before it urns off. There is some evidence that the energy is 2.5 MeV, but the energy resolution is poor. Overall, it's a 7 sigma effect.

My general impression is that Jones is carefully following up previous work and it's looking somewhat more positive than before.

HUGGINS:

Reported only calorimetry involving direct comparisons between light and heavy water cells. There was no direct stirring, but he did say gyroscopic motion of the whole apparatus (water bath included) was used to keep things well mixed. He reports that the heavy-water cells run consistently hotter that those with light-water, but there are lot's of reasons why this might be so and he didn't address them very carefully. Bottom line is a claim of for 15% excess energy production in a short (35 hr) run and up to 10 MJ/mole Pd in longer runs.

Huggins stressed that an important experimental consideration was the microstructure and interstitial content of the Pd. Thought it better to use cast vs. wrought samples and to arc melt them to drive out hydrogen that might be in there. Also emphasized the Carbon contamination was a no-no, so that graphite crucibles were out. However, it was pointed out to him that commercial suppliers use graphite containers in making their Pd.

LANDAU:

Emphasized that this was a progress report of preliminary results. Ran 4 cells including light/heavy water comparision, and a Pt cathode instead of Pd. Claims excess heat of 0.144 W (6 W/cm^3 Pd) at a current density of 255 mA/cm^2 with D2O but nothing (0.025W) with H2O. No tritium observed beyond that expected from electrochemical separation. Many neutron points at the 3-4 sigma level were presented, but nothing convincing.

STRUVE:

Reported on mass-spectrometry of gases evolved at a Pd cathode in 99.95% pure D2O but no(!) electrolyte. They saw A=2,3,4,5,6 signals with a quadrupole mass spec and got excited by the latter two peaks, which could be attributed to DT and TT molecules. However, a higher-precision instrument capable of distinguishing nuclear mass showed that A55,6 were due to tri-atomic hydrogen ions. It seems to me that any experienced mass spectrometrist would have recognized this immediately, and not held a press conference. The only excuse is that they were graduate students, I guess.

JORNE:

Looked for neutrons (nice scintillation counter, carefully calibrated efficiency), gammas, and fusion products. Limit on the neutron rate was <0.5/sec. P/F would imply 4 X 10^4/sec, while Jones would imply 0.4/sec. The limit on the gamma rate is 44/sec.

In a search for fusion products, they looked for 3H in the Pd with a mass spectrometer. They say A54 (DD molecules) and A=3 (HD, perhaps, or 3H) There is ''some suggestion'' that they're seeing 3H. but they can't prove it. I don't know what's making this suggestion to them.

SRINIVASAN:

This and the following talk were clearly the most interesting in the session. He reported on microcalorimetry. He is a professional in this business and the apparatus looked, to my untutored eye, like it was well thought through. He can

3

measure heat with confidence from 1 microWatt to 8 Watt. This sensitivity allows the use of small samples to cut charging time and they can also keep the current small. They ran Pd rods of 10 mm in length and 0.5 and 1 mm in diameter, as well as a 2 mm diameter sphere. The several electrolytes were LiOD, LiOH, and NaOD, with a volume of 5-6 ml. There were light water and Pt cathode controls.

Of 20 cells constructed, 1/3 showed excess heat production. Best was about 30 mW (10% more than the power in). They can see up to 20 W/cm^3 Pd for 10 hours at 300, 600, and 1000 mA/cm^2. However, none of their heat-producers showed any neutrons (next talk).

BOCKRIS:

Reported on neutron measurments by Kevin Wolf, a nuclear experimentalist. Two of 20 cells constructed were ''live'' and one of these reproducibly so. A scintillation counter was used. The neutrons change in a non-monotonic was with current and fall off in an R^-2 test in which the cell is moved away from the countr. The statistics were only fair, but somewhat better

than the Jones data published in Nature. Best rate was 50 n/min.

Also interesting were the 3H measurements reported. Starting from a background scintillation count rate of 60-80 dpm/ml, they see the count rate rise upon beginning electrolysis to about 10^6 dpm/ml after a few hours! They know all about the possible chemiluminescence interferences and have been careful to neutralize before counting.

LEWIS:

Not much beyond what was said in Baltimore. Some runs in which the current was changed in steps were also reported, but overall, no radiation or heat production or tritium observed. A preprint submitted to J. Electroanal Chem. by G. Kreysa, G. Marx, and W. Plieth (the first from the Dechema-Institute, Theodor-Heuss-Allee 25, 6000 Frankfurt) goes through an analysis of the P/F calorimetry data similar to Lewis' and comes to similar conclusions, I believe (I've yet to read it in detail).

OVERALL ANALYSIS:

Cold fusion is not dead yet. P/F are clearly on the ropes and are possibly on their way down. Jones is looking pretty good and there may be real physics there. I'm almost motivated to do more calculations. The A&M stuff is quite interesting. The neutrons would confirm Jones; I don't know what to make of the calorimetry. Perhaps Nature (or the ''successful'' experimenters) will reveal more of their secrets in Santa Fe two weeks from now.

Steven Koonin
KOONIN@SBITP.BITNET

4