EPRI
Electric Power
Research Institute

January 11, 1994

TO: Kurt Yeager
            John Taylor
            Robin Jones

FROM: Tom Passell

SUBJECT: Report of Richard Garwin re/SRI Deuterated Metals Project

Enclosed is a draft of Dr. Richard Garwin's report from his full day review of SRI Project 3170-23 conducted by Dr. Nathan Lewis and himself on October 19, 1993. As you may know, Dr. Lee Hammarstrom of SAF/SS in the Pentagon requested a review of the SRI work on Deuterated Metals by the JASONS, a group of consultants chaired by Garwin. Intially, Dr's Michael McKubre and Steven Crouch-Baker briefed a subcommitee of the JAONS in a 3-hour session in La Jolla, CA on July 14, 1993. The laboratory visit and full day review was a follow-up to the shorter session at the invitation of McKubre. Also enclosed are McKubre's comments on the draft. Overall, having been present all day at the visit of October 19, I believe Garwin's draft report marks a major shift in thinking by critics of the SRI work and is a credit to the investigators at SRI under McKubre's leadership.

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December 23, 1993
(Via FAX to 9 (415) 859 - 4286)

Dr Michael C.H. McKubre
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Dear Mike,

Sorry to have taken so long with our brief report designated nominally for Lee Hammarstrom, but here it is.

Actually, I have talked with Lee a couple of times in the interim, so this would come as no surprise to him. In fact, he is not anxiously awaiting the report, in view of our oral communications.

Please give me a call today if possible, or sometime soon to let me know if there are any errors in our presentation. I would also like to hear from you of progress or lack thereof since our visit two months ago.

Very best regards for the holiday season.

Sincerely yours,

Richard L Garwin

Encl:
12/23/93 Draft LTR RLG to L.M. Hammarstrom.   (122393.LMH)

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December 23, 1993
(Via FAX to 9 (703) 267-4123)

Dr Lee M. Hammarstrom
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Dear Lee,

This is a brief informal report from Nate Lewis and Dick Garwin on the basis of our visit to SRI International Tuesday, 10/19/1993. We regret that you were unable to attend, but we hope that this will provide you with the information you might have obtained had you been there.

In addition to the two of us and Mike McKubre, McKubre's colleagues (Stewart Smedley, Fran Tanzella, Steve Crouch-Baker) participated fully, as did Robin Jones and Tom Passell from EPRI and David Golden of SRI. We met from about 0900 to 1730, with a full and frank exchange of views, knowledge, opinion, and analysis.

Description.

The SRI project was begun by Tom Passell (to create a hydrogen sensor and to study catalysis) before there was any suggestion of cold fusion or excess heat. Understandably, he has maintained a strong interest in the program. Of course, all of us would be fascinated and would feel great admiration if it were possible reliably to produce excess heat. The same would be true of a new way of producing nuclear particles under such circumstances, even modest numbers of neutrons, x rays, or gamma-rays.

Neither Nate Lewis nor I has any reluctance to entertain and recognize a purely experimental discovery. We don't need a theory to make us believe our eyes. But we do need a significant, reproducible effect, and that is what McKubre and his colleagues are attempting to produce.

We had a good deal of discussion of the new type of cells L1- L4 which have been running since April; we held one in our hands and are now quite familiar with its construction. We also had extensive discussions of data from one of these cells, which according to a summary chart has provided about 3% excess heat. This is not a derived kind of excess heat, related to the minimum electrochemical energy required to electrolyze water to produce dihydrogen(g) and

Draft for McKubre (12/23/1993)

dioxygen(g), but an honestly phrased fractional excess over the total power delivered to the electrochemical cell itself.

The electrochemical cells sit in a thermostated water bath. Typically, two cells are run at the same time, with constant current provided by a regulated power supply. An isothermal-flow calorimeter is used to determine the power dissipated in the cell. Crudely put, a constant-displacement pump provides a flow of the calorimetric fluid (water in the most recent experiments) so at a rate of about 1 ml/s, but the actual water flow is measured much more accurately than the constancy of the pump by having the water delivered to a beaker that rests on the platen of an electronic scale, so that over almost all measuring intervals the mass of the water is measured accurately. An automatic "siphon" occasionally empties the beaker, so that the measurement is inaccurate over a small fraction of the intervals.

From the mass flow and the known specific heat of water, the power transferred to the water could be calculated if one knew the temperature rise. This is determined by measuring the inlet temperature with two platinum resistance thermometric devices (RTD) and measuring the outlet temperature within the calorimeter housing by two additional RTD. To minimize heat transfer to the surrounding thermostated bath, the calorimeter is immersed in a single-ended metal Dewar flask. Water from the thermostatic bath enters and descends along the inner wall of the flask, entering the calorimeter at the base and having the temperature measured by two RTD at that point. After passing outside the coiled compensating heater which encloses the electrochemical cell, the water begins its exit from the cell through a small venturi aperture, in order to mix the streamlines, and has its temperature measured by two additional RTD at the exit.

A sensing current of 0.5 mA is fed to each RTD for a couple of seconds every four minutes. All measurements are taken by a Keithley digital multimedia. The multimeter was time shared to measure all of the parameters electrically.

In the absence of excess heat, the energy communicated to the electrochemical cell would be determined by the product of the known magnitude of stabilized current supplied, and the voltage measured at the base of the electrochemical cell. Because that voltage fluctuates with time and with current density, the experimental configuration has been designed to reduce the dependence of the entire calorimeter upon the linearity of the individual calorimetric measurements. This has been achieved by modifying the power to the compensating heater in each interval, so that a desired total power is achieved through the sum of the power input to the electrochemical cell and that input to the compensation heater. This is done by averaging the Pd/Pt cell voltage over a brief interval during the previous 4-minute measuring period, and then commanding the heater current to provide a value of supplementary power that is sufficient to obtain the nominal 12 W (or 30 W) or whatever is the target total power for the next 4-minute portion of the experiment.

Therefore, the output temperature should be nominally constant. More particularly, the measured total power (cell plus compensating heater), as determined by the measured calorimetric fluid mass flow and temperature rise, should be constant with time, except for the 4-minute delay in compensating power. Measurements of excess power are therefore in principle straightforward, based on the value of the power required from the compensation heater in order to obtain the desired total power delivered to the calorimetric fluid.

The situation is complicated by the fact that the electrochemical cell generates hydrogen and oxygen, approximately according to the electrochemical process that requires 1.53 volts drop in near-equilibrium. Thus, the gas which is catalytically recombined within the electrochemical cell produces a very substantial amount of recombination power (typically 4-6 W) at the top of the cell, well above the compensating heater. A primary criterion of the cell design is to ensure that essentially the only place for this heat to go is into the calorimetric fluid, and the purpose of the venturi is to ensure that the RTDs are sensitive only to total power and not more sensitive to power produced at one point in the cell than at another.

Only certain cells provide excess heat, and those are stated to be ones in which the cathode surface is treated appropriately, either by minor chemical constituents in the electrolyte (Si or Al), or by ion-beam bombardment. Furthermore, it is thought that one batch of cathode Pd works, and another does not produce excess heat. We were told that the silica rods that support the anode are essential to observe excess heat - presumably because of silica dissolution in the electrolyte. So many influences are thought to be at work that it would be easy enough to excuse the lack of excess power in a particular cell.

In any case, it takes hundreds of hours of conditioning of the cell before a reduction to near-zero current and a subsequent ramp up of current appear to yield some excess heat. The magnitude of excess heat is deduced by subtracting from the measured calorimetric power the sum of the heater power and the electrochemical cell power (I x V).

The uncertainty in excess power measurement is about 50mW, but the excess power appears to be on the order of 500 mW or even 1 W peak. However, excess power is still a deduced quantity and depends upon the calibration of the calorimeter.

A desirable feature of the experiment is that the calorimeter is to a considerable extent, self calibrating. That is, if it were possible to maintain the total power within the calorimeter the same, the output temperature (or temperature rise times flow rate) should be constant, and RTD and mass measurements are near enough absolute that there is little chance for error there. An additional complexity comes from heat leakage from the calorimeter to the bath, which is determined routinely by the difference between the heater input (in case of no or insignificant power to the electrochemical cell) and the measured flow calorimeter power. A small complication enters because it is important to keep the cathode from "de-loading," and that requires the continued passage of some minimum current through the cell.

The temporary detection of excess power (particularly if it returns to zero after a period, and never goes negative) can be measured quite sensitively. However, there are a number of potential confounders (dirt effects) that could mimic such an excess power. One would be an increased apparent temperature of the output RTDs, which are nominally 100-ohm platinum resistors.

The single digital meter used to measure all of the parameters of the experiment, steps from one to another within a 4-minute measuring interval. To a considerable extent (particularly with the RTDs), the use of a single meter reduces substantially the prospect for error. However, because some of the measurements are far more susceptible than others to corruption, the use of a single meter does not totally eliminate the possibility of measurement error. For instance, with a one per cent effect on a 12 W power level, the total temperature span is about 3 K, and the one per cent about 30 mK. An RTD has a resistance proportional to temperature, so that the

detection of 20 mK out of 300 K is about 70 ppm, or about three microvolts out of the total RTD voltage of about 50 mV, with a current of about 0.5 mA. More precisely, with the stated temperature coefficient of 0.385 ohm/K, the voltage represented by 20 mK is 3.85 microvolts.

A false signal of this magnitude would be produced by a shunt resistance of some 1.5 megohms, or a current leakage into the resistor itself of some 30 nanoamperes.

Discussion.
Concentrating on cells L3 and L4, we note that a chemical reaction involving the Pd at perhaps 1.5 eV per atom would correspond to about 3.5 kJ of heat; this is to be compared with the 3MJ of "excess heat" observed, so such an excess could not possibly be of chemical origin. If it were to be related to the electrolysis of water at 1.5 eV per hydrogen (3 eV per mole of water) it would correspond to the electrolysis of some 10 moles or 180 g of water.

The current experiments are being done at near-atmospheric pressure and room temperature, in view of the greater difficulty and hazard of working at high pressure as was done for awhile in this program. However, work done by others has the characteristic that the longer it is pursued and the better the circumstances, the smaller the effect. This "quit while you are ahead" trait does not give confidence that theirs is a real effect; quite the contrary.

We are concerned about a number of possibilities for producing apparent excess heat where none exists. The excess seems to be proportional to the current, when conditions are "right" for it to manifest itself. If the multimeter would read low while measuring the cell voltage, this would be the sign of the effect. That it does not occur until after "conditioning" takes place may simply mean that it doesn't occur until a while into the experiment, although time alone is not enough for every cell to exhibit the excess heat.

We believe that there are a few things (probably irrelevant) not very well understood by the experimenters. One is the magnitude of the heat loss by evaporation from the warm bath, although this would seem to have no impact at all on the analysis. Furthermore, bringing all the experimental circuits to the same plastic Jones barrier strip, itself mounted on a plastic panel, is asking for trouble. If the sensing leads from each of the circuits were brought to a separate two-bar Jones strip mounted on a common grounded metal panel, this would eliminate the potential for leakage along the plastic panel or the plastic of the Jones strip.

Although the cells are operated "closed," they are not in fact totally closed from the beginning of the experiment, since the cell volume is not adequate to contain the amount of hydrogen that must be absorbed in "loading" the cathode. Under existing safety regulations, the gas lead to the cell consists of two concentric tubes, so that one can actually FLUSH the cell. Previously, with only a single tube, the following would appear to happen as the cell was electrolyzed at the beginning of the experiment:

> As current was passed through the cell in order to produce hydrogen on the cathode, the absorption of hydrogen into the cathode would yield an excess of oxygen in the

cell. The catalytic recombiner within the cell would do its best, but there would be an oxygen-rich atmosphere within the cell. At a 1:1 loading of hydrogen atoms in the Pd, the 3 cm rod of 0.3 cm diameter would contain 23 milliequivalents of hydrogen atoms, corresponding to 5.7 millimoles of oxygen molecules in the cell. If the cell free volume were 100 cc, the pressure of oxygen gas would be 1.2 atm, or some 18 psi absolute. If one wanted to load the cell so that it remained hydrogen rich, in the presence of hydrogen loading of the cathode, the equivalent of 36 psi of hydrogen would need to be added, to combine with the oxygen (or to load the cathode), and an additional 35 psi of hydrogen in order to bring the cell to 20 psi gauge.

So we are somewhat concerned that the gas leads to each cell not leak at all, and would like to make sure that there is a mass and materials balance when the cells are opened, so that no significant amount of heavy water has escaped.

While cells that do not "load" to the requisite 0.92 D:Pd level would indeed serve as controls, we believe it highly desirable to run a number of cells on light water equal to the number of experimental cells. Of course, these are not directly comparable, and the purpose of having light-water cells would not be to do anything nearly so silly as to compare the power dissipation at equal current in the two cells, as has been done by some experimenters. The purpose, rather, would be to see whether such light-water cells (which are much easier to experiment with and considerably cheaper) would ever provide appearance of "excess heat". If there is an experimental artifact, it might for some reason exhibit itself more readily in connection with light-water cells, which would be a blessing to allow it to be tracked down and vanquished more readily.

Two other concerns have to do with the performance of the catalyst and with possible high-frequency oscillations in the electrochemical current.

The paper notes that at high current densities the presence of large deuterium (or hydrogen) and oxygen bubbles "disrupted the electrolyte continuity," and one would like to be very sure that the stabilized current did not change substantially during a small fraction of the time. The validity of the experimental results is totally dependent on the cell current being maintained truly constant, so that when the average of the fluctuating cell voltage is measured, the product of the constant current by this average voltage (and the time) is the true energy input.

Concluding remarks.
This is a serious effort to obtain reliable calorimetric data on heavy water electrolyzed in a cell with a palladium cathode. It is larger in scale and has more electrochemical expertise than the work of Tom Droege of Fermilab, who obtains excellent data but no excess heat.

We have found no specific experimental artifact responsible for the finding of excess heat, but we would like to see eventually (as would the experimenters!) a larger effect and one that can be more reliably exhibited. Alternatively, a larger number of light-water cells might more readily exhibit the phenomenon if it not "real," and this would seem to be a relatively easy way to challenge the hypothesis that the peculiarity is specific to heavy water.

Sincerely yours,

Richard L. Garwin

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