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MEMO

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We have
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TO: "COLD FUSION" VISITORS

FROM: JOMB

In the following, we seek to avoid misunderstandings about the work on electrochemical Cold Fusion which is going on (in three sub-groups) at Texas A&M University.

1) We are interested in the experiments reported by Fleischmann and Pons, and by Jones, which mention cold fusion obtained by electrochemical confinement.

2) We take the attitude that the presence of cold fusion in the experiments carried out by these workers is unproven.

3) Our attitude is to stress experiment. We seek to find out whether there <u>are</u> neutrons evolved from palladium electrodes under certain circumstances; whether tritium <u>is</u> produced during deuterium evolution at palladium electrodes; and whether the sometimes observed excess heat can be replicated in our laboratories.

Of course, we are interested in attempting to bring the reproducibility under better control.

4) When we have obtained reproducibility in the region of > 50%, and can instruct others how to do the experiments with the same success rate, then we shall investigate the dependence of heat evolution, neutron production and tritium evolution as a function of the variables such as overpotential, metal substrate, D/Pd ratio, dislocation density, dendritic promontories, etc.

When we have established some of these dependencies, perhaps in a year or

so, we shall then have a basis on which to decide if the New Phenomena originate in nuclear processes.

5) We are particularly unenthusiastic in the discussion of the application of present theories of fusion in plasmas to idea of fusion in electrochemical confinement because we think that the difference of conditions, particularly in respect to screening by electrons of deuterium-deuterium interaction, is an extreme one, and that it has not yet been properly investigated theoretically.

Our attitude is that we may be in an emerging area of science, and that in such situations experiment usually molds theory to fit it.

Historically, when new science is emerging, it is often reviled and denigrated until the new paradigm is accepted. It is, of course, too early to say whether this is the situation in this field.

6) At the time of writing, the phenomenon is less than three months old. Two or three years (5-6 Centers, 100 people) will be the right sort of time to think of in order to make a decision as to whether it is worth Big Money. The idea that a number of meetings are already planned, and even decisions made up on the basis of happenings at them at this time, appears to us to be unwise, partly because of the emotional outbursts by physicists which have occurred at some of them and the great negativity widely shown; but mainly because of the small degree of knowledge among us all.

Although we welcome criticism, we believe that spending a great deal of time in angry condemnation of the phenomena we are investigating is not a good way to further understanding of New Phenomena which understandablt exist. We would rather tell you in a relaxed way, about our results, and compare them with the positive results of others in various parts of the world. We believe it is agreement among scientists, particularly between those in various

countries, which eventually decides what is regarded as "truth" for a few decades in a field.

We think the new (and shaky) "facts" should be isolated from comparison with the older theories until the facts are firm and agreed upon - at least to a good degree.

8) About negative results: We think that, in attempts to verify a newly claimed phenomena, negative results have much less value than positive ones. Negative results can be obtained without skill and experience.

It has always been the anomalies which can be seen in a Science which gives rise to the new ways of thinking which cyclically invade the sciences. The constant reiteration of the old way (particularly with the great Anger and Emotion) we are seeing among our colleagues and visitors has not been the way that changes in scientific attitudes have come in the past.

Therefore, when persons tell us that they have carried out the electrolysis of deuterium evolution in palladium and see nothing new, particularly if (as is usual) they are furious about it, have spent little time on it, and have little experience as to how to do experiments of the type named, we tend to discount their contribution.

This is particularly so because the phenomena under consideration are undoubtedly elusive. Added to this is the fact that the effects - when they indeed turn on - are difficult to find in electrodes as small as 1- and 2-mm diameters (quickly chargeable), and can only easily be detected (when they display) in most calorimeters when the size of the electrode is something in the region of 4-6 mm. However, a 6-mm electrode takes 72 days to charge before the experiment can begin.

Thus, as we are now less than 72 days from the announcement, and as to start experiments it will be necessary not only to charge electrodes but to

gather equipment of various kinds both electrochemical and nuclear, -to say nothing of super-pure Pd rods, - it is remarkable that those who were not already working in electrochemistry before the announcement was made could have made experiments at all, let alone gotten results upon which the National Policy (in funding) is to be founded.

Most of the experiments in which negative results have been obtained have come from Laboratories which have little record of research in physical electrochemistry; or, when in a tiny number of cases the laboratories were electrochemical, little experience in nuclear measurements.

The most common errors to date are:

a) Insufficient charging times. The latter is obtained from the use of the formula $\Delta^2 = 2DT$. Our habit is to calculate using this formula for α and β Pd (hence, 2 T's), and the D is for the two forms of palladium, then double the time to allow for possibility of a third form which seems to exist. (We get c. 30 days for 4 mms and 72 days for 6).

b) The second most common error is the use of electrodes less than 4 mm in diameter.

There is, at first, an apparent advantage to using these because they charge up more quickly. The disadvantage is that the effects they give are often too small to see: they need a micro-calorimeter.

c) Contact with the wet atmosphere eliminates the observation of the heat. Water must be excluded from the D_2O.

d) Use of inappropriate palladium is difficult to specify. There is some evidence is that Johnson-Matthey's "puratronic" palladium works best, but we do not know why, and the evidence is conflicting. Annealing, hammering, cold work, casting, have all been suggested.

e) The final experiment in which the phenomena are sought should be carried out at more than 500 ma/cm^{-2}. After the charging time of 30 days for the 4-mm rods and 72 days for the 6-mm rods, there has to be a further time up to 500 ma/cm^{-2} which may last several days before anything abnormal is seen.

We do not give up an electrode until 7 days after the current density has been turned up to 500 ma cm^{-2}.

f) Much confusion and waste of money is carried out by examination of electrodes which have never "woken up".

When an electrode doesn't show the heat, there is little point in examining it in great detail with neutron counters ("the most sensitive in the world") or etc., or X-ray monitors. If it does not show heat, it is less likely to produce tritium, etc.

Reports are full of accounts of people who did this and spent time seeking nuclear particles and not finding them (no wonder, if the electrode did not give heat).

g) There has been too much accent on very accurate calorimetry. Our experience is that when the effect switches on, it switches on very definitely, and using the size of rods mentioned above, calorimeters which are only measured to ~100 milliwatts can easily measure the effects.

h) Keeping water out: The separation factor of water to deuterium is 9 times in favor of water, so that a 0.5% water-containing solution will evolve about 5% hydrogen. Small amounts of hydrogen seem to poison the electrode. It's probably better to keep the water below 0.1%.

i) The use of LiOH instead of LiOD: Remarkably, a number of laboratories have used LiOH - not good for obvious reasons.

j) Lack of preelectrolysis of the solution: The latter is very

necessary, for it removes the water and takes away other undesirable impurities.

k) Lack of knowledge of the Tafel parameters

Exchange current density?

Overpotential?

D/Pd ratio?

We have found that these vital basic elements are understandably little known to physicists working on fusion. It's vital to know them because the fugacity developed in the electrode depends upon the detailed relationship between the Tafel slope and the overpotential, - and then depends on the relevant mechanism of deuterium evolution, intermediate concentration, etc.

l) Neutron measurements: Arrangements for screening out cosmic ray showers are, of course, essential. Neutrons as a function of the state of the surface are informative.

m) Tritium: Tests for the elimination of chemiluminescence is essential. The plot of the tritium build up in the solution as a function of time may be informative. Conversely, we don't always find tritium when we find heat.

Finally, there is no doubt that irreproducability is the bane of these experiments. We are looking increasingly towards the concept that the phenomenon occus at the surface rather than in the interior, although of course the state of internal saturation will effect the surface concentration of intermediate deuterons.