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THE PRESENT STATUS OF RESEARCH IN COLD FUSION

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EDITORIAL NOTE:

Martin Fleischmann has recently been asked by the Royal Society of Chemistry to give an account of the II Annual Conference on Cold Fusion for the Newsletter of the Electrochemistry Group of the Society. The editors thought it would be a very useful addition to the Conference Proceedings. We thank Martin Fleischmann and the Royal Society of Chemistry for having agreed to publish this text in these Proceedings.

In the development of any new area of research (and especially in one likely to arouse controversy!) it is desirable to achieve first of all a qualitative demonstration of the phenomena invoked in the explanation of the observations. It is the qualitative demonstrations which are unambiguous: the quantitative analyses of the experimental results can be the subject of debate but, if these quantitative analyses stand in opposition to the qualitative demonstration, then these methods of analysis must be judged to be incorrect[1].

Research in the area of Cold Fusion affords an excellent illustration of this principle. Contrary to popular belief it is relatively easy to show qualitatively that Pd cathodes polarized in LiOD solutions in D_2O generate excess enthalpy over and above that of the enthalpy input to the electrochemical cell. All that is required is that a sufficient number of electrodes of sufficiently well-controlled properties be polarized for a sufficiently long time in D_2O having a sufficiently low content of H_2O and using calorimeters of sufficient sensitivity (signal:noise) in a sufficiently well-controlled environment[2]. It will then be found that a proportion of the experiments will show temperature-time and cell potential-time plots of the form illustrated in Fig. 1. We also make the following observations about this particular type of experiment:

(i) the current efficiency for the electrolysis of D_2O is virtually 100%: there is no additional chemical source of enthalpy in the system;

(ii) heat transfer from the cell to a surrounding thermostat is controlled by radiation and the heat transfer coefficient for the particular cell is virtually independent of time;

(iii) our experiments in H_2O do not show these effects.

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1 This principle, which should be self-evident, is usually overlooked in the unseemly haste to develop research. It may come to be known as Pons' and Fleischmann's first principle (designed to irritate the scientific public in general and nuclear physicists in particular). We have some even more irritating principles but will save these for a later discourse.

2 The explanation of the term "sufficient" in each of these contexts is beyond the scope of this article; these points can be taken up by correspondence.

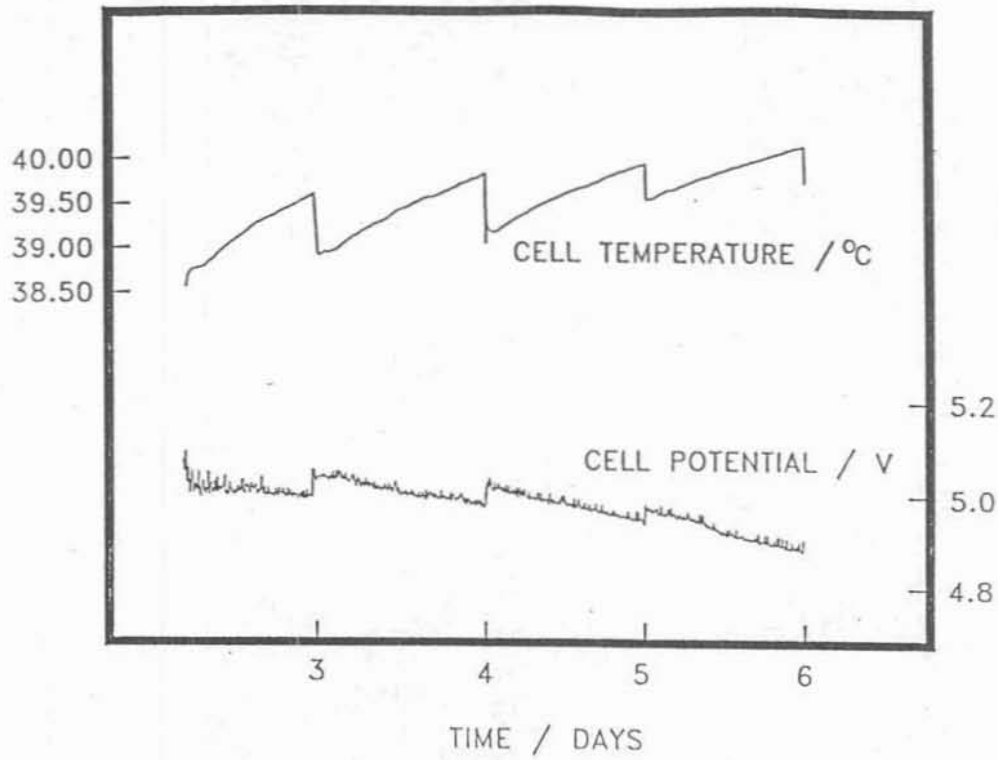

FIG. 1. Cell temperature (upper) and cell potential (lower) vs. time since cell was started for the electrolysis of D_2O in 0.6M Li_2SO_4 solution at pH 10 at a palladium rod cathode (0.4 x 1.25 cm). The cell current was 400mA, the water bath temperature was 30.00°C, and the room temperature was 21°C. The rate of excess enthalpy generation at the end of each day was 0.045W (day 3), 0.066W (day 4), 0.086W (day 5), and 0.115W (day 6). The accumulation of excess enthalpy for this period was on the order of 26KJ.

How then are we to explain an increasing thermal output of the cell coupled to a decreasing thermal input? The first law of thermodynamics requires that there be a source of enthalpy in the system and the strength of this source increases with time during the period illustrated. Such observations were valid in 1989 (they were valid before then!), they were valid in 1990 and they are valid now.

The next step naturally is to seek a quantitative interpretation of such data. The last two years have seen the development of something akin to a cottage industry whose objective appears to be to explain away the reality of the phenomena by a combination of using incorrect (or inappropriate) methods of data analysis and invalid methods of overestimating the errors of the calorimetry (10% is the target figure). There is in truth scope for the former because the experiment is complex; the latter will come as a surprise to chemists who have hitherto relied on calorimetric methods as the main plank of thermodynamics. In judging the validity of such methods of calculation and of such estimates it is important to bear in mind the qualitative information conveyed by the experiments: any quantitative evaluation which removes or obscures the qualitative information must be judged to be invalid. Equally, failure to observe comparable qualitative effects when using other calorimetric methods must be judged to be due to inadequacies of the experimental methods[3] or, possibly, erroneous interpretation of the results.

Our own interpretation of the data in Fig. 1 gives the values of the rates of excess enthalpy generation and of the total excess enthalpy also shown on the Figure. Over the period shown the specific excess enthalpy amounts to 172 kJ cm^{-3} corresponding to 1.52 MJ (g mole $Pd)^{-1}$. It is our view that enthalpies of this magnitude can only be attributed to the operation of nuclear processes. The most rudimentary measurements of the generation of tritium and of the neutron flux (or rather the lack of it!) show that the nuclear reaction paths

$$^{2}D + {}^{2}D \rightarrow {}^{3}He(0.81\ MeV) + n(2.45\ MeV) \qquad (i)$$

$$^{2}D + {}^{2}D \rightarrow {}^{3}T\ (1.01\ MeV) + {}^{1}H(3.02\ MeV) \qquad (ii)$$

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[3] These include: failure to control the H_2O content of the electrolyte, inadequate experiment times and other factors such as cracking of the electrodes (repeated use of the cathodes?) or lack of symmetry of the disposition of the anodes around the cathodes leading to low D/Pd ratios; excessive sophistication of the instrumentation (which obscures the significance of the results) and incorrect choices of the experimental protocols.

which are dominant in high energy fusion (and which have roughly equal cross-sections under those conditions) contribute to only a very small extent to the observed phenomena.

We reach the conclusions:

(i) the lattice has an important influence on the nuclear processes;

(ii) the observed processes are substantially aneutronic;

(iii) the generation of excess enthalpy is the main signature of these new nuclear processes.

These conclusions were valid in 1989, they were valid in 1990 and they are valid now. As chemists we are naturally interested in the main signature of the processes-side reactions can give important information but, in the end, one always has to investigate the major reaction route. Research in Chemistry teaches one that an understanding of the major processes normally leads also to an understanding of the side reactions[4]. This dictum has not been followed in most of the research carried out during the last two years.

Some Comments on Research since 1989: the Second Annual Conference on Cold Fusion. Como, Italy. 29th June - 4th July 1991

The holding of the Second Annual Conference on Cold Fusion prompts a reassessment of the work carried out since March 1989. The papers presented at the conference will be published by the Italian Physical Society later this year. Summaries of earlier work can be found in the following reviews:

J. O'M. Bockris, Guang H. Lin and N.J.C. Packam (Texas A&M University) "A Review of the Investigations of the Fleischmann-Pons Phenomena", Fusion Tech., 18 (1990) 11.

J. O'M. Bockris and D. Hodko (Texas A&M University) "Is there Evidence for Cold Fusion?", Chemistry and Industry 21 (1990) 688.

[4] This principle is so well understood by Chemists that it doesn't qualify for Pons' and Fleischmann's second principle of research in Chemistry; it could, however, qualify as the second principle of research in Nuclear Physics. As the saying goes: Chemists are interested in making Chemicals but Physicists are not interested in making Physicals.

M. Srinivasan (Bhabha Atomic Research Centre) "Nuclear Fusion in a Lattice: an Update on the International Status of Cold Fusion Research", Current Science, April 1991.

E. Storms (Los Alamos National Laboratory) "Review of Experimental Observations about the Cold Fusion Effect" to appear in Fusion Tech (1991).

There are other excellent reviews but these are not so readily accessible.

In what follows, comments about work done prior to the Como meeting will be indented in the text. The results will be described under several headings.

Tritium Generation

New results on tritium generation in closed electrochemical systems were presented notably by the collaborative groups between CNR, Padua, and the Department of Physics, University of Padua (G. Mengoli, C. Manduchi et al.) and by the group at the National Cold Fusion Institute, Salt Lake City (F. Will et al.). Conditions have been achieved by these and other groups which lead to an increase in the T-content of the closed systems by factors of 25-40 over that of the initial inventory.

Details of an interesting new variant of the electrochemical method were reported by the collaborative group between the Naval Ocean Systems Laboratory, San Diego and the D.O.E., Washington (S. Szpak). In these experiments palladium and D are codeposited at high negative potentials which appears to lead to an immediate generation of tritium and the detection of autoradiographic images in the immediate vicinity of the cathodes (presumably due to the emission of soft x-rays); it also leads to an immediate generation of excess enthalpy.

Further results on the generation of tritium (and neutrons) in solid state cells consisting of alternating layers of Pd powder or foil and of Si powder or wafers were reported by the group at Los Alamos National Laboratory (T.N. Claytor). Increases in the tritium levels up to 150 times of the initial inventory have been achieved under pulsed current conditions.

References to earlier work will be found in the reviews. These earlier investigations (notably those at the Bhabha Atomic Research

Centre) include spectacular examples of the autoradiography of Ti loaded with D and subjected to temperature cycles, the detailed characterisation of the tritium β-ray emission spectra and of x-ray emission from the Ti samples (presumably due to Coulomb excitation induced by the β-decay). There are also reports by the groups in India and at Texas A&M University of the generation of very high levels of tritium in and at palladium cathodes.

The generation of high energy tritons is referred to below under the heading "Miscellaneous Observations".

Neutrons

A survey of neutron detection experiments was presented by H. Menlove (Los Alamos National Laboratory) and this was also a major component of the surveys of gas loading experiments by F. Scaramuzzi (Frascati), of research in Japan by H. Ikegami (National Institute for Fusion Science, Nagoya), in the Soviet Union by V. Tsarev (Lebedev Institute, Moscow) and in China by Xing Z. Li (Tsinghua University, Beijing). A major new result is the detection of neutrons from experiments on Pd cathodes conducted in the Kamiokande mine by H. Menlove and S. Jones (Brigham Young University). These measurements rely on the detection of highly energetic γ-rays from the (n,γ) reaction on ^{35}Cl (present as 20% NaCl in the surrounding water bath). It appears that the results obtained are at a higher level of statistical significance than that of the neutrinos detected in the Kamiokande mine from the 1987 supernova.

The improvement in the methods of characterising individual neutrons continues apace such as in the time-of-flight spectroscopy reported by T. Bressani (I.N.F.N., Sezione di Torino). An improvement in the statistical significance of the data has been a general feature of the results such as in the time-correlated detection of bursts of neutrons from gas loaded samples of Pd reported by T. Tazima (National Institute for Fusion Science, Nagoya).

Earlier work has shown that both gas loaded samples and Pd cathodes show two types of signal, a very low level background count which is difficult to detect at high levels of significance (and which may be Poisson distributed) and bursts in the neutron production.

The results now available show that these bursts in the neutron production cannot be attributed to spallation induced by cosmic rays.

The results presented at the meeting confirmed yet again that the branching ratio for reactions (ii) and (i) differs markedly from unity (factors as high as 10^7-10^9 have been reported). Such changes in the branching ratio from the value ~1 for "hot" fusion show that the nuclear processes in the lattice cannot in any sense be discussed by analogy to fusion in high temperature plasmas. This is also shown by the observation of high energy neutrons (a broad peak in the range 3-6 MeV in addition to the 2.45 MeV peak due to reaction (i)) which was again reported at the meeting by A. Takahashi (Osaka University - Matsushita Electric Company group).

Excess Enthalpy Generation

The rapid generation of excess enthalpy in thin films of Pd (either sputtered or electroplated onto Ag substrates) was reported by R. Bush (California State Polytechnic University); rapid generation in Pd-D codeposited at very negative potentials was also reported by S. Szpak (Naval Ocean Systems Laboratory). M. McKubre (EPRI-Stanford S.R.I.) gave results of excess enthalpy measurements on Pd cathodes using sealed flow calorimeters while S. Pons described measurements on alloy electrodes which lead to very high releases of excess enthalpy (> 1kW cm^{-3}) so much so that the main cause of heat transfer becomes the boiling of the electrolyte[5]. An independent analysis of some of our calorimetric data was presented by W. Hansen (Utah State University, Logan) using other methods than those employed by us. This analysis has given results closely similar to those which we have obtained although our estimates of the excess enthalpy appear to be conservative (which we have already anticipated because this is a feature of our methodology).

J. O'M. Bockris (Texas A&M University) reported that Pd cathodes subjected to pulse conditions give high levels of excess enthalpy but only when the D/Pd ratio approaches ~1.

Miscellaneous Observations

The sub-title unfortunately gives the wrong connotation because some of the most interesting results are included under this heading.

The correlation of ^{4}He measurements in the gas phase with the

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5 Arguments about the magnitude of the heat transfer coefficients, the characterisation of the calorimeters etc. then become meaningless. A watch, to time the boiling to dryness and one's finger will do (if one doesn't mind getting scalded).

levels of excess enthalpy generation in Pd cathodes was reported by M. Miles (Naval Weapons Research Centre, China Lake - University of Texas, Austin collaboration).

The detection of charged particles from thin Ti foils loaded with D from the gas phase, temperature cycled to -180°C and then subjected to moderately high current densities was reported by F. Cecil (Colorado School of Mines - Solar Energy Research Institute collaboration). It is possible to make very sensitive measurements on such systems using Si surface barrier detectors and such measurements are relatively unambiguous.

> There are earlier reports of such measurements from the Soviet Union and especially from the Naval Research Laboratory, Washington, D.C. (G. Chambers). In this particular study of Ti foils ion implanted with deuterons ~5 MeV particles of mass 3 were detected (probably tritons). Such particles cannot arise in simple D-D reactions and this result should be compared with the observation of energetic neutrons in the 3-6 MeV range.

The fact that excess enthalpy generation is observed on electrodes having a D/Pd ratio ~1 was brought up several times at the meeting. By contrast, tritium generation appears to be favoured by lower charging ratios and/or non-equilibrium conditions (equally true of gas loaded samples subjected to temperature cycles). Neutron generation also appears to require non-equilibrium conditions (e.g. pulsed electrolysis).

The detection of neutrons from essentially aneutronic processes naturally poses difficulties and an unfortunate feature of much of the work to date has been the mismatch of the effort and information devoted to the instrumentation on one hand and the experiment design on the other[6]. At the final round table discussion we appealed that, as far

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[6] We do not decry the effort devoted to the instrumentation but note that the experiment design is usually rather crude in comparison. However, we also note that several groups (ours included) have information that the neutron generation rate lies in the range 5-50 neutrons s^{-1} $Watt^{-1}$ (if excess enthalpy generation is observed). There appears to be some coyness about reporting such facts. It might well prove to be more useful therefore to increase the scale of the experiments rather than to improve the instrumentation to find next to nothing at all.

as electrochemical experiments are concerned, research workers should at the very least report the light water content of the electrolyte used, the D/Pd charging ratio and the number of times an electrode has been used as well as the position of any particular measurement cycle in the experiment sequence.

Theory

A number of ideas about the possible mechanism of "Cold Fusion" were presented at the meeting. The subject was reviewed by G. Preparata (University of Milan). Our speculation (in the last section) about the possible explanations of the phenomena are based on a paper which will appear as part of the conference proceedings[7,8].

What about the explanations?

We have to look for answers to the following questions:

(a) Why is it so much easier for deuterons to overcome the Coulomb barrier to fusion when the deuterons are in a lattice than when they are, say, in a high temperature plasma?
(b) Why is the outcome of fusion in a lattice so different to that of fusion "in a vacuum"?
(c) How does it come about that the lattice can affect processes which take place at very short space-times, say 10^{-21}s and 10 F?

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7 Summary of the present position. Contrary to the views which were expressed in 1989 that the phenomenon of "Cold Fusion" would "go away" with improved experiments and instrumentation, the evidence has become stronger and the statistical level of significance of the various measurements has increased.

8 Readers will find that there is only a passing resemblance between this report and Douglas Morrison's Cold Fusion Update No. 5 (which must be about his 27th message about the subject). The difference is that we have reported what people actually said not what we feel they ought to have said (or wished they would have said). It is for this reason that we have separated our own thoughts about this section into the footnotes 5-8.
In the context of the reportage of this field we would add that it is not helpful to classify those workers who obtain positive results as "Believers" and those who obtain zero effects as "Skeptics" nor to give off the cuff comments about the validity of particular results. A judgement as to the latter frequently involves almost as much effort as the original investigation and science normally does not progress via such back-biting. We trust that Heinz Gerischer will publish his observations on the meeting which were an object lesson on how to avoid such nonsense.

Not surprisingly, simple models based on the collisions of two deuterons (with the possible screening by single electrons) cannot provide an explanation of (a) (the ingoing channel): the overlap amplitude

$$\eta - \exp\left\{ - \int_{r_N}^{r_o} \left[2\mu\left(V(r)-E\right)\right] dr \right\}$$

is simply too small. Here r_N is a typical nuclear distance (a few Fermis), r_o is the classical turning point, μ is the reduced mass, $V(r)$ $(=\alpha/r)$ is the Coulomb potential and E the kinetic energy of the colliding nuclei. Based on such arguments one would conclude that there can be no fusion in a lattice - it must all be a delusion, mistake, fraud or something to that effect.

It is important to realise, however, that whereas much of plasma physics has been based on ideas drawn from the solid state the transfer of concepts in the reverse direction cannot be achieved in a simple way[9]. An important key to the understanding of the system is given by the strange properties of D (and of H and T) in such lattices. We must ask: how can it be that D can exist at a ~100 molar concentration and high supersaturations without forming D_2 in the lattice? How can it be that D diffuses so rapidly through the lattice (diffusion coefficient $> 10^{-7}$ cm^2 s^{-1} greater than that of either H or T!) whereas He is practically immobile? The answer to the last question is, of course, that deuterium is present as the deuteron whereas ^{4}He does not form α-particles. These observations alone set interesting limits on the intense fields experienced by D as it moves through the lattice (say >30eV $(A)^{-1}$ and <70eV $(A)^{-1}$). The deuterons sit in deep energy wells and yet behave as though they are almost unbound (they behave as classical oscillators). This fact in itself shows that the deuterons must be part of a macroscopic quantum system so that they can experience such marked anharmonic excitations.

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[9] A proper consideration of such condensed systems has to be based on the Quantum ElectroDynamics (Q.E.D.) of superradiating plasmas. Those interested in this topic should consult the papers of G. Preparata.As far as the present system is concerned one concludes that the deuterons form an ordered array (D/Pd =1!) in a coherence domain described by a macroscopic wave function; the deuteron plasma interacts with the coherent oscillations of the electron plasma.

These observations in turn give an explanation of the increase in the overlap amplitude, for hot fusion is based on the increase of E by increasing the temperature (say to 100 eV at 10^8K) while "cold" fusion depends on the lowering of V. The increase in η due to this is sufficient to explain the very low rates of neutron production which have now been extensively reported. Summation over all the "fusion paths" of the coherence domain gives the further increase in η demanded by the high rates of enthalpy generation.

The second important set of questions is why the outcome of fusion in a lattice should be so different to that in a plasma? The simple view is that processes at short space-times cannot be affected by the much slower lattice vibrations[10,11]. It is thought by many that the outgoing channels must therefore follow (i) and (ii). By the same token processes such as

$$^{2}D + {}^{2}D \longrightarrow {}^{4}He + \gamma \ (23.9 \text{ MeV}) \qquad \text{(iii)}$$

are not expected to contribute significantly (the cross-section of (iii) is about 10^{-6} that of (i) and (ii) under "hot" fusion conditions). However, in plasma fusion the dipole coupling for (iii) is to the vacuum whereas in the lattice this coupling is to the macroscopic wave-function of the coherent electron plasma. It would therefore be expected that most of the energy flow will be to the electron plasma which will

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[10] In Quantum ChromoDynamics (Q.C.D.) this separation of a process at short space-times from those at much longer space-times is referred to as "asymptotic freedom". However, in the presence of coherent interactions, this principle is generally inapplicable, as shown by Preparata.

[11] This view is widely held notwithstanding the fact that lattices <u>do</u> affect nuclear processes as is shown, for example, by Moessbauer spectroscopy. You may wish to ask yourself whether the short space-time scale of γ-ray emission could ever be affected by phonon interactions? Why should the lattices be so stiff on such short time scales? The answer to this conundrum lies once again in the Q.E.D. of the superradiant plasmas as has been shown by Preparata. As electrochemists are imaginative people you might then ask yourself what other consequences this might have for Chemistry, Nuclear Physics, Cosmology or anything else that comes to mind.

rapidly thermalise the system[12, 13]. On this view the formation of T and n is due to incoherent cooling of the compound nuclei the H-T configuration then being favoured because of the large electromagnetic current due to the rapid motion of the proton (in the n-^{3}He configuration it is the neutron which is rapidly moving).

Conclusions

This short account has been based on only a small part of the information available. It is probably too soon, for example, to attempt a comprehensive explanation of the formation of high energy tritons (~5MeV) and neutrons (3-6 MeV) except to say that they certainly cannot arise in simple two-body collisions. We also note that other explanations of the phenomena have been put forward: we have simply chosen the one which at this time is most free from objections and which also has the essential advantage that it leads to predictions for the outcome of novel experiments. Future surveys will have to cover the much wider range of observations already to hand and may well have to include the strange patterns of behaviour of compressed deuteron plasmas which are being reported in related fields of research such as in the application of Plasma Focus devices.

It is our view that the scientific interest of the subject has now been amply established; the scope for technological applications remains to be evaluated. However, to date, it has certainly been true that all aspects of electrochemistry, no matter how esoteric, eventually find some practical use.

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[12] The information about the system is in the macroscopic wave-functions. Neglect of this fact leads to paradoxes of the Einstein-Rosen-Podolsky type.

[13] This argument should not come as a surprise to electrochemists since there is some analogy between the deuteron-electron plasma coupling and the dipole fluctuation induced activation of outer sphere redox reactions. Indeed, it is our view that the latter processes would best be described by the relevant macroscopic wave functions of that superradiant system.