DEBATE BETWEEN DOUGLAS MORRISON and
STANLEY PONS & MARTIN FLEISCHMANN

These two documents that first appeared in the Internet's sci.physics.fusion forum in 1993. The first was written by Douglas Morrison (CERN), the second by Martin Fleischmann (Univ. Southhampton) and Stanley Pons (IMRA Europe). Morrison wrote a critique of the article:

M. Fleischmann, S. Pons, "Calorimetry of the Pd-D2O system: from simplicity via complications to simplicity," Physics Letters A, 176 (1993) 118-129

Pons and Fleischmann respond to his critique.

This debate refers mainly to paper "Calorimetry of the PD ED2O System: from Simplicity via Complications to Simplicity," published in *Physics Letters A*. A simlar paper was later published in the ICCF-3 conference proceedings and handed out by Fleischmann, which is reproduced on this web site LENR-CANR.org.

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COMMENTS ON CLAIMS OF EXCESS ENTHALPY BY FLEISCHMANN AND PONS USING SIMPLE CELLS MADE TO BOIL

Douglas R.O. Morrison.

M. Fleischmann and S. Pons [1] have published in Physics Letters A a communication entitled "Calorimetry of the Pd-D2O system: from simplicity via complications to simplicity". There they claim evidence for the production of excess enthalpy of greater than one kW per cc of Palladium in a Pd-D2O system. They comment that this is comparable with the rates obtained in a fast breeder reactor. They note that the reproducibility is high. In this letter serious doubts are expressed about this claim and the methods used to derive it.

Essentially they perform electrolysis in a transparent test tube which is open so that the gases and vapour can escape freely. The cathode is a small rod of palladium of 0.2 cm diameter and 1.25 cm length giving a total volume of 0.039 cm3. There are three stages.

For the first stage a moderate current is used for electrolysis. It is noted that at short times the heat transfer coefficient decreases - this they ascribe to the heat of absorbtion of hydrogen ions in the lattice.

In the second stage the current densities are raised to increase the temperature above 50 C - this with D2O. Finally, in stage three, the cells are driven to boiling point. A complicated (non-linear regression) analysis [2] is employed and it is calculated that there is excess enthalpy generated in the lattice, the amount calculated increasing steeply with time (and temperature).

In the third stage the behaviour near and during boiling is observed using a video camera. From this video, the time for the cell to go from about half-empty to dry, is taken - more precisely the amount of liquid boiled off is estimated over the final 10 minutes before the test tube was declared dry. A new simple calculation is made in which the enthalpy input is calculated as

(cell voltage - 1.54 V).(cell current)

and the enthalpy output is taken as composed of two terms, the energy radiated and the heat resulting from the vapourization of the D2O remaining in the cell 600 seconds before it is dry (this latter term is dominant). It is this simple calculation that gives the highest values claimed, namely "the excess rate of energy production is about four times that of the enthalpy input" and

that the excess specific rate is 3.7 kW per cc of Palladium.

There are several major problems with this calculation.

First is that the "cigarette lighter effect" has been forgotten. In the last century it was difficult to make reliable matches to light cigarettes. A reliable smokeless lighter was invented which consisted of a rod of palladium into which hydrogen had been introduced under pressure. This caused the lattice of the palladium to expand and thus stored energy. To light a cigarette, the top of the rod was uncovered; some hydrogen escaped releasing some of the stress and thus releasing energy which resulted in a small rise in temperature of the end of the rod. Palladium is a catalyst of hydrogen and oxygen which burn to give water plus energy. The palladium now slightly heated, catalyzes the escaping hydrogen and the oxygen of the air and the resulting heat of combustion which is mainly deposited on the surface of the rod, raises its temperature. This temperature rise releases more hydrogen which is catalyzed by the still more efficient hot palladium, and so on until the tip of the rod is so hot that the cigarette can be lit. The reliability of this system is high.

In the simple calculation used for stage three, a significant effect is omitted, of the heat produced by the catalytized recombination of the hydrogen with the oxygen. The oxygen is released from the anode by electrolysis, and towards the end when the cell is about dry, from the air. There is no mention in Fleischmann and Pons's paper of any attempt to measure the amount of oxygen, deuterium and water in the gases and vapours leaving the test tube.

In the Fleischmann and Pons paper, it is noted as a further demonstration result, that "following the boiling to dryness and the open-circuiting of the cells, the cells nevertheless remain at 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 C." This dramatic effect cannot be explained by Fleischmann and Pons as being due to electrolysis since there is no liquid and no electrolysis. However it is exactly what would be expected with the "cigarette lighter effect" where the hot palladium rod continues to catalyze the interaction of the hydrogen which is slowly escaping from the rod, with oxygen from the air.

It might be expected that this effect would occur also with normal water, H2O, being used instead of heavy water, D2O, but no description is given in the paper of any results of tests of the stage three boiling using normal water, H2O.

An interesting confirmation of this using electrochemistry was reported by Kreysa, Marx and Plieth [3]. They write "We have to report here that as we removed the deuterium-loaded palladium sheet from the cell and laid it on the table it did burn a scald into the table. One can still argue that this was due to deuterium fusion. Therefore we loaded the palladium sheet cathodically with hydrogen using an electrolyte containing only normal water (no enriched heavy water) and laid it on to a piece of wood where it also burnt a scald." They say it releases 147.3 kJ per mole D. "The principle of flameless catalytic combustion of hydrogen" - the official name of the 'cigarette lighter effect' - "is used in catalytic hydrogen burners (D. Behrens (ed) Waserstoffetechnologie - Perspektiven fur Forschung und Entwicklung, Dechema, Frankfurt/M 1986)." To be more quantitative they laid a hydrogen-loaded sheet of palladium on to glass rods

and "measured, after an incubation time of 15 s, a temperature rise of the palladium from 20 to 418 degres within 74 seconds." The 15 second delay is the time during which the gradual escape of hydrogen releases a small amount of energy from the lattice, thus heating the palladium sufficiently for it to become an efficient catalyst. They estimate a heat flow of 35.9 W and a heat flow density of 179.6 W/cm3".

It may be noted that Fleischmann and Pons used an exceedingly small piece of palladium, 0.04 cm3, which works well as a catalyst, but which means that after catalyzing a larger volume of heavy water, the power calculated is apparently larger than with Kreysa et al. because the volume of palladium is so small. Should Drs. Fleischmann and Pons wish to test their previous conclusions [1], it would be interesting if they were to describe experiments where they repeated their published experiment but with a substantially larger amount of palladium and a relatively small volume of D2O.

Secondly, there is the assumption that ALL the liquid present in the tube 600 seconds before dryness, was boiled off. That is none of it was carried out as a liquid, from the test tube. Now the video shows that there is highly turbulent motion. And as Kreysa et al. [3] showed, 74 seconds after the palladium becomes dry, temperatures of a few hundred degrees can be reached. Thus it is reasonable to expect that with such a chaotic system, some fraction of the liquid is blown out of the test tube as liquid and therefore should not be counted. The existence of such a fraction is omitted from the simple Fleischmann and Pons calculation. And no attempt to measure this fraction is described.

Thirdly, the input enthalpy is taken as the current multiplied by the (cell voltage - 1.54V). It is not explained how these quantities are measured. This is crucial as when the cell is boiling vigorously, the impedance must be fluctuating strongly. Thus the current will have both an AC and a DC component. If only the DC component were measured, then the input enthalpy would be underestimated. A detailed description of the current and voltage measuring systems showing their fast response characters is needed, but is not presented.

Since these three important aspects of the experiment have been omitted, it is not possible to say whether or not excess enthalpy has been observed in the last 600 seconds to dryness (stage three).

There are two important problems with stage two.

Firstly, a complicated non-linear regression analysis is employed to allow a claim of excess enthalpy to be made. This method of Fleischmann and Pons [2] has been carefully studied by Wilson et al. [4] who state that "they significantly over-estimate the excess heat.......an additional significant overestimate of excess energy occurs when the calibration is made above 60 C". Now stage two is mainly above 50 C and rising to 100 C. Further Wilson et al. write "Because of the paucity of experimental details in their publications, it has been difficult to determine quantitatively, the effect of calibration errors." A reply by Pons and Fleischmann [5] did not address the main questions posed by Wilson et al.

Secondly, it may be noted in fig. 8 of ref 1, that the cell voltage rises as the temperature rises

and that as 100 C is approached, the voltage rises more and more steeply. Experience by the GE group [6] was that in operating similar open cells over many hours, they also noticed a rise in cell voltage with time. They attributed this effect as being due to some of the escaping gases carrying some Lithium with them. As the level of the electrolyte is maintained by adding fresh D2O (but not any lithium salt), the concentration of lithium in the electrolyte decreases with time and the voltage rises. This was proved by atomic absorption analysis, that the cell resistance had risen (causing higher voltage due to the constant current mode operation) due to loss of lithium which was caused by sputtering of electrolyte droplets up the gas outlet tube. This may be considered confirmation that even at moderate temperatures, the outlet stream contains liquids as well as gases as discussed for stage three when the temperature was much higher and the boiling much more vigorous. It may be concluded that claims of excess enthalpy in stage two have not been established.

The overall conclusion is that many important factors have been neglected so that it has not been established that excess enthalpy was observed.

The experiment and some of the calculations have been described as "simple". This is incorrect - the process involving chaotic motion, is complex and many calibrations and corrections are needed. The calculations have been made to appear simple by incorrectly ignoring important factors. It would have been better to describe the experiments as "poor" rather than "simple". A true "simple" experiment is one where corrections and calibrations can be reduced to a minimum. This can be achieved in calorimetry by using a closed cell and by enclosing the cell in a series (eg three) baths which are each kept at constant temperature. The cell is kept at a higher temperature than the innermost bath so that if any excess enthalpy is produced, the heating of this bath can be reduced thus measuring simply the excess. Since this is a null measurement system, there is little need for complicated corrections. It is to be regretted that in the nine and a half years (the last four years well-funded) that Fleischmann and Pons say they have been working on this [7], that they have employed such a simplistic open-cell system.

It is a pleasure to acknowledge the help of many friends, in particular D. Britz, F. Close, T. Droege, R. Garwin, and S.E. Jones.

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 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

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_2O 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_2O 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_2O 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_2O solutions. At that point we stated that the accuracy of the determination of the coefficient $(k_R')_2$ (relative standard deviation ~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 ~ 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_2O 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_2O 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_2O at the instantaneous atmospheric pressures and (b) by electrolysis of D_2O 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_2O 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_2O from such sources is used. We recommend the routine screening of the sources of D_2O and of the cell contents using NMR techniques. The primary reason for such routine screening is to check on the H_2O 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 ($\sim$ 5mW for the electrode dimensions used) will be reduced because intense D_2 evolution and D_2O 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 $1/\text{delta}^2$ where delta is the thickness of the boundary layer. This gives us a maximum localised rate of heating of $\sim$ 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_2O (~ 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_2O alone would have required ~1.1MJ of energy whereas the combustion of all the D in the palladium would at most have produced ~ 650J (assuming that the D/Pd ratio had reached ~1 in the cathode), a discrepancy of a factor of ~ 1700. In the second place, the timescale of the explanation is impossible: the diffusional relaxation time is ~ 29 days whereas the phenomenon took at most ~ 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 ~ 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 ~ 10^5s) while the rate of diffusion of oxygen through the boundary layer could lead at most to a rate of generation of excess enthalpy of ~ 5mW.

Douglas Morrison next asserts that no evidence has been presented in the paper about stages three or four using H_2O in place of D_2O. 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_2O solutions stay at high temperatures after they have been driven to such extremes of excess enthalpy generation

<u>does not present us</u> 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 Li0D (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 ~120kJ during the period at which it remained at high temperature (or ~ 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".

   <u>Douglas Morrison's Section "Conclusions" and some General Comments</u>

   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_2O. 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_2O system whereas there is no such generation for appropriate blanks (e.g. Pt-D_2O or Pd-H_2O). 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_2O from D_2 and $1/2$ O_2). In the second place it is inapplicable because the term itself is ~ 0.77 Watt whereas we are measuring a total enthalpy output of ~ 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" whereas 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 <u>one does not need any temperature measurement in order to determine the rate of boiling of a liquid?</u>

 (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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