



PROCEEDINGS of  
The 9<sup>th</sup> International Conference on Cold Fusion

# Condensed Matter Nuclear Science

Edited by Xing Z. Li

May 19-24, 2002

International Convention Center at Tsinghua University, Beijing, CHINA

清华大学出版社



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北 京

图书在版编目 (CIP) 数据

第 9 届冷聚变国际会议论文集/李兴中编. —北京: 清华大学出版社, 2003

ISBN 7-302-06489-X

I. 第… II. 李… III. 受控聚变-国际学术会议-文集 IV. TL6-53

中国版本图书馆 CIP 数据核字 (2003) 第 023361 号

出 版 者: 清华大学出版社 (北京清华大学学研大厦, 邮编 100084)

<http://www.com.edu.cn>

印 刷 者: 清华大学印刷厂

发 行 者: 新华书店总店北京发行所

开 本: 850×1168 1/16 印张: 31 插页: 3

版 次: 2003 年 5 月第 1 版 2003 年 5 月第 1 次印刷

书 号: ISBN 7-302-06489-X/O · 292

印 数: 001~500

定 价: 100.00 元

### Conference Plan (May 19--24, 2002)

|                    |   |
|--------------------|---|
| MAY 19 (SUNDAY)    | RECEPTION AT INTERNATIONAL CONVENTION CENTER OF<br>TSINGHUA UNIVERSITY                |
| May 20 (Monday)    | Compelling evidence as a <b>nuclear process</b> in nature                             |
| May 21 (Tuesday)   | Multiple-Layer Thin-film effect: Progress in <b>reproducibility</b>                   |
| May 22 (Wednesday) | Tour to Great Wall,<br>and a visit to Great Wall <b>Ti-Gold Technology Co. Ltd.</b>   |
| May 23 (Thursday)  | Theoretical Model   |
| May 24 (Friday)    | Summary and Review  |
| May 25----         | 6 Routes for Tour in China (XiAn, Tibet, GuiLin, HongKong, ShangHai,<br>Three Gorges) |

The following contributors are gratefully acknowledged

***Tsinghua University Press***

***Tsinghua Tongfang Optical Dick Co. Ltd.***

***Great Wall Ti-Gold Technology Co. Ltd.***

***MicroRoad Corporation***

***Nanometer International Company***

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Fundamental Research Division, Ministry of Science and Technology

Physics Division II, Natural Science Foundation of China

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*The 9<sup>th</sup> International Conference On Cold Fusion*

May 19-24, 2002, Beijing China





Vice President of Tsinghua University, YanKang Zheng, delivered an opening address  
( from left: QingQuan Gou, YanKang Zheng , XingZhong Li, NengKuan Chen)



Infrared Thermal Imaging System for Demonstration



3 groups of visitors took pictures with excitement



T. J. Dolan , IAEA officer (1995-2001) for fusion research, addressed in the summary session



M. Fleischmann walked out the main building of the Great Wall Ti-Gold Inc. after seeing the demonstration device

## Condensed Matter Nuclear Science

"Condensed Matter Nuclear Science" was suggested for the title of a new journal during an extended meeting held by ICCF-9 International Advisory Committee on May 21, 2002. It symbolized the new starting point of this research field after 13 year world-wide continuous study.

3 coherences were announced in Lerici, ITALY as the theme of ICCF-9, i.e. the coherence between CONDENSED MATTER PHYSICS and NUCLEAR PHYSICS; the coherence between "hot fusion" and "cold fusion"; and the coherence between application and fundamental research. This conference is organized according to the theme.

The first is the *coherence between CONDENSED MATTER PHYSICS and NUCLEAR PHYSICS*. Indeed Professor Martin Fleischmann has traced back to 1960's for the historical motivation of this research<sup>[1]</sup>. Dr. Iwamura of Mitsubishi presented most convincing experimental results to show such a coherence: when a deuterium flux is induced by the multiple-layer thin-film structure on the surface of palladium, nuclear transmutation would happen reproducibly<sup>[2]</sup>. Professor Takahashi of Osaka University showed also that the 3-body nuclear reaction might be greatly enhanced if the highly deuterided titanium target is cooled during the beam-target experiments<sup>[3]</sup>. Both of these experiments clearly showed that the nuclear interactions might be affected by the condensed matter physics.

Secondly, the *coherence between "hot fusion" and "cold fusion"* should be emphasized, because both are working for a clean and inexhaustible energy source. Most of the "hot fusion" scientists do not realize that both of "hot fusion" and "cold fusion" need the resonant tunneling to enhance the fusion cross-section. They always believe that the kinetic energy of hot fusion plasma is necessary to overcome the Coulomb barrier between positively charged deuteron and triton. This has been a great mistake for 50 year fusion research<sup>[4]</sup>. Indeed the kinetic energy is effective only when there is a resonance energy level for deuteron-triton fusion. Only if they understand the importance of this resonance; then, they are able to understand why the "excess heat" in the low energy nuclear reaction are never accompanied by the strong neutron or gamma radiation. Fortunately, the open mind fusion scientist, Dr. Thomas J. Dolan, started to assist fusion community for a better understanding between "hot fusion" and "cold fusion" physics<sup>[5]</sup>.

Thirdly, the *coherence between application and fundamental research* is necessary to promote this research. This is almost the only way to break through the barrier between the main stream science and this research. If we look at the difficulties for the SCIENCE magazine to publish the "Bubble Fusion" paper, we will understand the necessity of a demonstration device which will show the application of "Condensed Matter Nuclear Science". Fortunately, the Great-Wall Ti-Gold Company made a big stride forwards in this direction.

We emphasized 3 key factors for these 3 coherences as well, i.e. the deuterium(hydrogen) flux, the multiple-layer thin-film technique, and the infrared thermal imaging technology.

**Flux** is more important than deuterium (hydrogen) loading ratio in metals. This has been recognized by more and more experimentalists. This should be important to solve the problem of reproducibility.

**Multiple-layer thin-film** is an effective technique to enhance the flux. Dr. Wolf-Dieter MÜNZ, Materials Research Institute at Sheffield Hallam University, UK, showed his great interests in solving the flake-off problem in multiple-layer thin-film. This is important to establish a self-sustaining "excess heat" device.

**Infrared thermal imaging technology** has been proved to be a good diagnostic tool, which has enough time and space resolution and has no interference to the D(H)/Pd system, in order to search for the best set of the parameters.

The Italian scientists at Frascati have shown that the power density of a D/Pd system might be as high as 600 kW/c.c.<sup>[6]</sup>. This is very close to the theoretical limit ( MW/c.c.). What we have to do is to search the best set of the parameters, and scaling-up the device. Our mission is to put interesting, potentially important science into public view.



#### **Editorial Notes:**

Since we are going to publish a selected work based on the proceedings, we decided to publish all the manuscripts received although some of these manuscripts do not meet the requirement set by ICCF-9 in advance.

We gratefully acknowledge the contribution from Tsinghua University Press. They provided not only the Abstract Book in time, but also the heavy editorial work for the proceedings. This successful ICCF-9 has justified the judicious decision made by the president of Tsinghua University Press, JiaQiang Li. Particularly, Professor ZhaoQi Zhang, the formal editor-in-chief of Tsinghua University Press, has most generously devoted herself in publishing the Abstract Book and the proceedings with fully understanding the difficulties in this research.

A personal contribution from Professor ZhongQun Tian of XianMen University is specially acknowledged here for his generous support to this important research and a successful ICCF-9.

[1] M. Fleischmann, Proceedings of ICCF9 p.III.

[2] Y. Iwamura, et al., Proceedings of ICCF9 p.141.

[3] K. Ochiai, A. Takahashi, et al., Fusion Technology, **36**, 315(1999).

[4]X. Z. Li, et al., Physical Rev. C, **61**, 024610 (2000).

[5]T. J. Dolan, Proceedings of ICCF9 p.XXIV.

[6] E. Del Giudice, et al., Proceedings of ICCF9 p.82.

*Xing Zhong Li, 2002/12/25*

# SEARCHING FOR THE CONSEQUENCES OF MANY-BODY EFFECTS IN CONDENSED PHASE SYSTEMS

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## ABSTRACT

Some of the background work which led to the decision to investigate the behaviour of  $D^+$  electrochemically compressed into Pd host lattices is outlined. The key features of such "Cold Fusion" systems are described.

## 1. BACKGROUND TO THE RESEARCH ON COLD FUSION

It appears to me that most scientists have the impression that my colleague Stanley Pons and I decided one day in late 1983 to go into the laboratory and to carry out the experiment best described by the statement

"Gee-whiz, let's go in the lab and charge some Pd cathodes with  $D^+$  and see what happens".

It is, of course, perfectly true that this is what happened. However, the conclusion that this was an isolated example is incorrect as has been realised by a relatively small number of research workers (among whom I would number pre-eminently the late Giuliano Preparata and his colleague Emilio Del Giudice). In fact, the decision to investigate the Pd/D system was preceded by a long period during which I asked the question: "is it possible to develop electrochemical experiments which demonstrate the need to interpret the behaviour of condensed matter in terms of the Q.E.D. paradigm?"

Questions which we posed prior to the development of work on "Cold Fusion";  
the special position of the work initiated in 1983

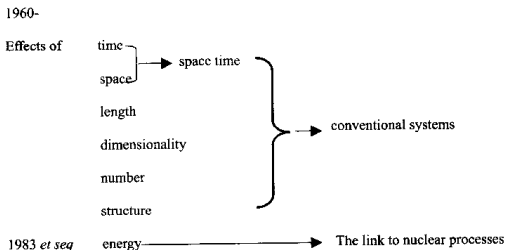


Fig. 1 Background to the work carried out in the period 1960 - 83

The work on "Cold Fusion" is now reaching the stage where we have a clear demonstration of the presence of the  $D^+$  in the lattice in an extended system with an unique wave-function<sup>[1,2,3]</sup> and it is therefore opportune to return to the earlier questions.

Of course, we should realise that there are other systems subject to Q.E.D. Coherence<sup>[4]</sup> and many more examples could be proposed.

In the limited time available in this lecture, I will restrict consideration to three topics illustrating the theme of space-time, Fig. 1. The starting point for this section of our work was the consideration of the kinetics of fast reactions in solution (in the late 1950's and early 1960's). In the development of relaxation methods it had been shown by Manfred Eigen and his co-workers<sup>[5]</sup> that the fastest reactions could be interpreted satisfactorily in terms of strong randomisation of the structure (on the time scale of the experiments) coupled to the diffusion controlled recombination of the species<sup>[6]</sup>, see Fig. 2. Consideration of

the time scales of the temperature-jump experiments suggests that these experiments were governed by a space-time of the order of  $10^{-10}$  cms.

Such fast reactions in solution had been investigated earlier by a variety of electrochemical methods (e.g. see <sup>[7,8,9]</sup>). The general principle is illustrated in Fig. 3: the concentration of the species HA is reduced to zero at the electrode surface which permits the probing of the protolytic reactions adjacent to the electrode surface. In this case consideration of the length scales suggests that these reactions are governed by a space-time lying in the range  $10^{-16}$  to  $10^{-13}$  cms. A surprising feature of many of these experiments was the conclusion that the second order recombination rates could exceed those governed by the diffusion controlled constant,  $k_D$ . The advent of the results from the relaxation methods persuaded most (all?) scientists that the electrochemical methods gave erroneous results notwithstanding the fact that this methodology was very simple. The possibility that both sets results might, in fact, be correct does not appear to have been considered. Looking at this problem from the point of view of a Physical Chemist, the conclusion that the recombination was limited by the fastest *observable* rate given by any particular experimental technique appeared beset with difficulties.

In this vein we also investigated at that time protolytic reactions close to equilibrium using hydrogen ions generated at the surface of a Pd/H electrode<sup>[9]</sup>, Fig. 4. This phase discontinuity and the high diffusion coefficient of hydrogen in palladium allows one to reach recombination rates some 6 orders of magnitude higher than is the case for relaxation methods. The protolytic reactions are now so fast that the "reaction layer" has a thickness given by the dimensions of the anions (here the OH<sup>-</sup> ions: the model originally proposed by Brdicka and Wiesner<sup>[10]</sup>). It was perhaps not very surprising that the rate of recombination was limited by the hydrogen bond frequency, Fig. 4; the space-time of these experiments was therefore of order  $2 \times 10^{-22}$  cms.

At that time my colleague, Alan Bewick, also constructed a temperature-jump instrument out of the residues of the Harwell Zeta Project.\* This instrument allowed measurements at space-times of the order  $3 \times 10^{-12}$  cms. Coupled reactions in solution were investigated, and we found the expected double relaxation due to these coupled reactions. However, deconvolution of the data showed that the individual rate constants were phase related. We concluded that there had to be a memory propagator in the system which in some unaccountable way was associated with a structure of size  $\sim 10^{-6}$  cm (so as to account for the change in behaviour from that shown by relaxation experiments with a restricted frequency response. Fig. 5 summarises the key observations in this series of experiments.

#### Relaxation measurements

$$x = 10^{-5} \text{ cm} \quad t = 10^{-5} \text{ s} \quad xt = 10^{-10} \text{ cms} \quad k_2 = k_D$$

#### Enhanced relaxation measurements

$$x = 10^{-6} \text{ cm} \quad t = 3 \times 10^{-6} \text{ s} \quad xt = 3 \times 10^{-12} \text{ cms} \quad k_2 \neq k_D$$

#### Electrochemical measurements

$$x = 10^{-7} \text{ cm} \quad t = 2.5 \times 10^{-8} \text{ s} \quad xt = 2.5 \times 10^{-15} \text{ cms} \quad k_2 \neq k_D$$

#### Special Electrochemical measurements

$$x = 2 \times 10^{-8} \text{ cm} \quad t = 10^{-11} \text{ s} \quad xt = 2 \times 10^{-22} \text{ cms} \quad k_2 \gg k_D$$

There is a memory propagator associated with a structure of order

$$x = 10^{-6} \text{ cm}$$

Fig. 5 The key observations for the kinetics of the reactions of ions in solution

\*Footnote: This was the earliest attempt to initiate "Hot Fusion" in a plasma. The conclusion that this had been achieved was later found to be incorrect.

Furthermore, we became concerned about the nature of the model used to interpret the behaviour of electrolyte solutions. Fig. 6 illustrates this Debye-Huckel model based on the electrostatic interactions of the ions. However, the ions are not at rest and, if we accept the model of Brownian motion of the ions, Fig. 7, we can see that this model is not consistent with microscopic reversibility because the ions will radiate energy both during acceleration and de-acceleration (the Maxwell equations). Such models therefore violate the Second Law of Thermodynamics and are inadmissible.

I presented these results to a colleague whose principal interest lay in Q.E.D. His comment was immediate: "you must model these systems in terms of Q.E.D." As I recall my response was; "just so, but how?" In the intervening years I could never find a reason for the development of a structure of order  $\sim 10^{-6}$  cm so I put the whole matter aside until 1995. In that year Giuliano Preparata and his colleagues showed that the application of Q.E.D. to water led to the conclusion that the system would be divided into coherent and incoherent regions<sup>[11]</sup>. Ions will be confined in the incoherent regions where they will themselves form a coherent system. The application of such a model to electrolyte solutions, Fig. 8, has been discussed<sup>[12]</sup>.

The development of this model therefore gives a basis for the interpretation of the reactions of ions in solution but this will certainly prove to be an intractable problem. On the other hand, the interpretation of other systems may be more straightforward. At I.C.C.F. 8 I drew attention<sup>[13]</sup> to the interesting work of Zhadin et al<sup>[14]</sup>: the application of a weak static magnetic field and an even weaker alternating weak magnetic field leads to a low frequency resonance of the electrolytic conductivity, Fig. 9. My Italian colleagues do not wish to talk about this topic at the present Conference. However, I believe that the interpretation of the topic<sup>[15]</sup> is instructive, Fig. 10: the application of the alternating field can lead to the extraction of ions from the boundary between the coherent and incoherent regions and, thereby, a transient increase in the electrolytic conductivity. This example is instructive for two principal reasons: in the first place the effects of Q.E.D. on the structure of the system take place with low or zero changes of entropy so that they are difficult to observe; secondly, the application of Q.E.D. normally leads to unexpected and unusual consequences (here the development of a "battery" induced by magnetic fields) so that the consequences are consigned to the field of "Pathological Science".

Before considering the third example of the effects of space-time on physical processes, let us return to the position in the early 1960's. Apart from the single example of discussions of the outcome of our measurements of the kinetics of fast reactions in solution, the reactions to our proposal that such systems were subject to the twin effects of memory propagators coupled to the development of a structural element were uniformly negative (frequently, quite violently so). I therefore decided that any investigation of the possible effects of Q.E.D. had to be carried out within an hidden agenda: the systems had to be investigated within the accepted frameworks of Classical and Quantum Mechanics while the possible influence of many-body effects and of Q.E.D. would have to emerge from the interpretation of the results:

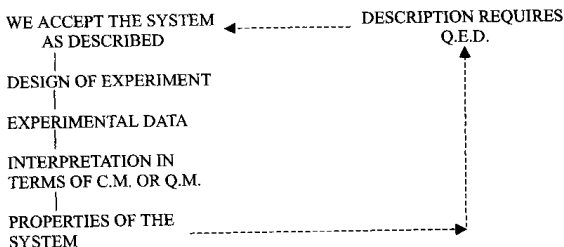


Fig.11 The hidden agenda

This process also had the advantage of avoiding the presupposition of the reality of the importance of the effects we were seeking to illustrate: after all, it might well have turned out that the effects were unimportant or not measurable! These "hidden agendas" had an important influence on the development of the work on "Cold Fusion".

Our preoccupation with ionic processes at small space-times led to the examination of transport phenomena and chemical reactions in short pores of narrow diameter. After a number of false starts we focused eventually on transmembrane ion conduction across pores in lipid bilayers, Fig. 12. (modelling in

terms of classical mechanics indicates space-times of the order  $5 \times 10^{-16}$  cms). It is of interest that the very existence of such membranes (perhaps the most directly observable result for this system) is not consistent with the Second Law of Thermodynamics! A possible explanation is in terms of Q.E.D.<sup>[16]</sup>

It has been known for some time<sup>[17,18,19]</sup>, that the insertion of pore forming molecules such as the polypeptide alamethicin, leads to steps in the transmembrane ion current such as those seen in Fig. 13 (taken from our own data<sup>[19]</sup>). Systems of this kind are ideal for investigation by electrochemical methods because we can determine the kinetics of transition between adjacent states (due to the insertion / removal of successive molecules of the pore former) and the energetics of each state (from the probability of the occupation of that state)<sup>[19]</sup>. Such measurements lead to the model shown in Fig. 14.

The measurements of the energetics led to the surprising result shown in Fig. 15. We can only explain such a result if the edge energy of the pore is negative whereas the bulk energy is positive so that we reach an upper reflecting state. In contrast, the kinetics of conventional 2-dimensional nucleation are governed by a positive edge and negative bulk energy, so that we reach an upper absorbing state, Fig. 15. We should note that the signs of these free energy terms could have been predicted from the very fact that the membranes are stable! However, we need to ask : how can it be that the bulk free energy is positive with regard to the macroscopic electrolyte? This explanation requires us to invoke the Q.E.D. of water<sup>[11]</sup>, see Fig. 8: the "pore" is filled by an incoherent domain of the electrolyte solution which, of course, has a positive free energy with respect to the macroscopic electrolyte.

The measurement of the kinetics of transition between adjacent states leads to a further surprising result. It has been found that these transitions conform strictly to a birth-and-death process i.e. the transitions only take place between adjacent states<sup>[19]</sup>. At the same time, elementary considerations based on the size of the pores, show that these pores are empty (free of ions) for an appreciable fraction of time so that the current-time transients should follow the pattern shown in the lower half of Fig. 16. This is a simple demonstration of the fact that the "noise" levels in the experimentally observed transmembrane ion currents are too small. We conclude that the pores and their adjacent liquid volumes are parts of a single quantum system, Fig. 17 (quantum systems are only subject to quantum noise). The operation of the "hidden agenda" will be apparent.

Fig. 17 also shows the ways in which we can judge the necessity of carrying out a paradigm shift to Q.E.D. The conventional view is that this has to be judged in terms of the achievement of a mathematically complete theory. However, it appears to me that this can lead to rather sterile debates and we have therefore relied on a more empirical approach: the need to avoid violation of the Second Law of Thermodynamics in the modelling and the need to develop internally consistent interpretations. As far as this second aspect is concerned, we find all too frequently that different aspects of behaviour of a given system can be interpreted quite adequately in terms of Quantum or Classical Mechanics but that these interpretations are then not internally self-consistent (the Pd/H and Pd/D systems abound with such examples). It seems to me, therefore, that we should explore the consequences of using Q.E.D. as the paradigm of necessity at least at this stage of development of research in the Natural Sciences.

## RESEARCH IN "COLD FUSION"

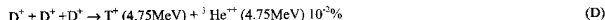
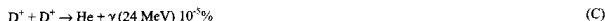
By the beginning of the 1970's (and, certainly, by the beginning of the 1980's) we had reached a stage which demonstrated the need to invoke Q.E.D. in the modelling of numerous systems (however, I note that we had certainly not reached a complete understanding of many of these systems). The time was therefore opportune to take the next step in the development of the research programme: the addition of small amounts of energy per species to an extended quantum system and the investigation whether this could, in the limit, lead to nuclear reactions, Fig. 1. We judged the likelihood of any clear demonstrations of such effects to be very small! (I recall saying to Stan Pons at the time: "there is a chance in a billion that we will see anything". Nevertheless, we also judged the consequences of any positive observations to be quite incalculable and we therefore decided to go ahead with this topic. A positive result in this field of research would have given us the necessary impetus to reinvestigate other systems such as those described in the previous section)

Although the winter of 1983 was the time at which we started systematic work on this topic, this was not the first time at which I had considered carrying out research in this field. I had found the papers of Alfred Coehn<sup>[20,21]</sup> at the end of 1947. Coehn had found that hydrogen deposited at the apex of a zig-zag configuration of Pd wire, Fig. 18, could be made to move more rapidly towards a negatively polarised end of the wire than by diffusion alone, see Fig. 19, and less rapidly towards the positively polarised end (the dotted line shows a reversal of the polarity applied to the wire). There was at that time one of the periodic phases of interest in "exploding wires" and it appeared to me that the observations of Coehn might open up a slim chance of inducing nuclear processes if his methodology were combined with such explosions

(which one would now describe under the heading of inertial confinement). However, at that time judged from the available perspectives of Quantum Mechanics, this was a very slim chance indeed.

This topic became better defined at the beginning of the 1970's by which time we had realised that the behaviour of  $D^+$  in Pd could only be explained if it were in a many-body system<sup>[22]</sup> We therefore investigated the Raman Spectroscopy of  $\beta$ -Pd H with the intention of carrying out the experiment described by Fig. 20. The results we obtained were confusing and the topic developed rapidly into Surface Enhanced Raman Spectroscopy, SERS. We decided that the topic of using laser excitation of  $D^+$  confined in the Palladium lattice was hardly consistent research in the University Sector!

The experiments of Coehn were not the only additional factor influencing our decision to start work on the topic of "Cold Fusion". The discovery of the "Hot Fusion" channels (A) and (B) by Oliphant, Harteck and Rutherford<sup>[23]</sup> had been followed rapidly by measurements of the first channel by Dee<sup>[24,25]</sup> using a Willson Cloud chamber.



A surprising feature of these measurements was that a significant proportion of the tracks were at  $180^\circ$  rather than the expected  $\sim 160^\circ$  (corresponding to the incident energy of 200kV of the deuterons). As Phillip Dee observed, the tracks at  $180^\circ$  had to be attributed to the fusion of deuterons which had lost their incident energy in the target!

It is appropriate here to "fast forward" to the present day. It has been found that the cross-sections of the fusion channels observed at high incident energy are not a good guide to the behaviour at much lower energies and that more complicated collision processes come into play at these lower energies e.g. channel (D)<sup>[26]</sup>. One can therefore speculate whether channel (C) which has a very low cross-section for fusion  $\sim$  *in vacuo*, may not have a much higher cross-section for fusion in a lattice where the production of  ${}^4\text{He}$  would not necessarily be accompanied by the generation of a  $\gamma$ -ray.

This early work in the 1930's developed rapidly into the production of self-gettering neutron tubes. It is difficult now to obtain specifications for these tubes but such information about the neutron flux as is available suggests that the excess energy produced in these tubes would be a measurable fraction of the breakeven value. Needless to say, this fraction would be much higher if there were additional outgoing channels. However, it proved to be impossible to initiate a programme on the thermal balances in these tubes as they had been withdrawn.

As is well-known we initiated a programme on thermal balances within the Pd-D system using isoperibolic calorimeters of the type illustrated in Fig. 21: it seemed to us to be reasonably straightforward to pose the question "is the electrolysis of  $D_2O$  using Pd cathodes in thermal balance or is there any evidence for excess enthalpy generation?" While we still believe that this is the case, such calorimetric measurements have proved to be somewhat complicated. Thus, for example, it does not appear to have been appreciated that it is not possible to calibrate any calorimetric device if the system contained in the device generates fluctuating excess enthalpy. If that is the case, we will simply conclude that the measurement system is inaccurate and/or obtain an invalid calibration. In fact, it is necessary to carry out an extensive series of "blank" experiments to obtain the "instrument function" and to validate the behaviour of the system. We have found that the electrolysis of  $D_2O$  using Pt cathodes provides a suitable "blank" but we note also that such "blank" measurements have only rarely been carried out.

Let us consider the precision and accuracy of the instrumentation shown in Fig. 21. (using the analogy of shooting at a target, Fig.22). Our objective will always be to produce an instrument which is both accurate and precise; the worst case scenario will be if this instrument is both inaccurate and imprecise. However, all too often, instruments are either accurate but imprecise or else inaccurate but precise. Initially we believed that the behaviour of the calorimeters was given by this last description. We therefore laid great stress on the determination of the differential lower bound heat transfer coefficient which describes the local behaviour of the calorimeter with the assumption that there is no excess enthalpy generation. An example for a "blank" experiment is shown in Fig. 23. We can see that the heat transfer coefficients show a small systematic decrease with time (due to the progressive lowering of the electrolyte level caused by the electrolysis) superimposed on the natural errors of the system (due to the "noise" in the thermal measurements). Use of the central value of the heat transfer coefficient to evaluate the rates of

excess enthalpy generation gives results such as those illustrated in Fig. 24 where we make a comparison to the results predicted by a normal distribution (see further below).

By contrast, in the presence of variable rates of excess enthalpy generation, the lower bound heat transfer coefficient shows much more marked variations with time than those shown in Fig. 23. We can then use the maximum value of this coefficient as a measure of the true heat transfer coefficient and evaluate the rates at other times throughout the measurement cycle. Fig. 25 illustrates this methodology for an experiment carried out at the New Hydrogen Energy Centre in Japan. We can distinguish clearly the initial excess enthalpy due to the exothermic absorption of deuterium in the lattice; this excess enthalpy decreases with time and is followed by a slow build up due to the phenomena we are studying. The results for this experiment are in close accord with those for a closely similar experiment previously published<sup>[27]</sup>.

Although the standard deviation of the point-by-point measurements of the rates of excess enthalpy generation are quite high (5.87 mW, see Fig. 24.) the predicted mean rate is very small (0.079 mW, see Fig. 24). This low mean rate therefore sets a limit on the measurement of excess enthalpy generation in experiments on the Pd/D system, say  $10^{-4}$  rate of enthalpy input. The high standard deviation is due to the need to differentiate the inherently noisy temperature-time series when calculating the point-by-point values of the lower bound heat transfer coefficients. A better procedure is therefore the evaluation of the integral lower bound heat transfer coefficients based on the backward or forward integration of the time series, Fig. 26 see<sup>[29]</sup>. It is of some interest that such data can be reduced to a single, time-independent, heat transfer coefficient having a relative standard deviation <0.01%, Fig. 27. It has also been found that the values of the true heat transfer coefficient (based on the injection of known levels of enthalpy using the resistance heater shown in Fig. 21) are very close to the lower bound values which shows that the instrumentation is both accurate and precise (cf Fig. 22).<sup>[30]</sup>

It has frequently been asserted that our wish to evaluate experimental data sets at high precision and accuracy was part of some kind of abstract exercise. Here I would wish to make three comments on such assertions. In the first place, I can see little point in reducing the precision and accuracy of such evaluations because we will always wish to have the highest achievable levels of statistical significance. Thus we can see that the rates of excess enthalpy generation shown in Fig. 25 reach 300G! Secondly, such assertions ignore one of the principal objectives of our work which was to measure excess enthalpy generation using microelectrodes (or else assemblies of such microelectrodes), see insert on Fig. 21. The use of such microelectrodes would lead to a marked improvement in the energy efficiency because the Ohmic losses in solution would be markedly reduced. However, this particular development would require calorimetric measurements at much lower power inputs than is the case when using electrodes of conventional size i.e. it requires the development of precise and accurate calorimetry. Thirdly, elementary considerations show that the combination of such microelectrodes with the concepts of electrodiffusion might well give devices in which every discharged deuterium species would undergo fusion and thereby lead to a direct determination of the Q value(s) of the reaction(s).

In this connection we should also note the direct measurement of the temperature excursions at "hot spots" by using infrared imaging<sup>[31]</sup>, a topic which will be covered in a further contribution to this Conference<sup>[32]</sup>. It is evident that the further development of this methodology (or else the development of alternative methods for localised temperature measurements such as of laser thermometry) should allow the determination of the Q values of all such "hot spots" distributed over the surface of this film electrodes.

It also appears to me that the need to make repeated calibrations of the calorimetric systems, e.g. see Fig. 28, has not been appreciated. It is found that with increasing time the systems pass through a phase of "positive feedback" i.e. an increase of temperature as caused, for example, by the calibration pulses, leads to an increased thermal output e.g. compare Figs. 29 and 30 given by a data set collected by N.H.E. This onset of "positive feedback" leads to a delayed approach to the steady state following the application of the calibration pulse and a delayed decay to the base-line following the termination of this pulse, Fig. 30. Not surprisingly, attempts to derive the true heat transfer coefficient from calibrations subject to such effects lead to the impossible result that the true heat transfer coefficient is smaller than the lower bound value! e.g. see<sup>[33]</sup>.

Transitions through the region of positive feedback lead to the onset of oscillations, Fig. 31, which we attributed initially to a transition from an exo to an endothermic regime for the absorption of deuterium<sup>[28]</sup> but which could probably be attributed more correctly to a transition from a  $\beta$ - to a  $\gamma$ -phase.<sup>[34]</sup> We have found it to be desirable to "drive" the systems through such regions (so as to reach the boiling point of the electrolyte, Fig. 28) as prolonged operation in the region of the onset of positive feedback can destroy the phenomenon of excess enthalpy generation. It is then found that the cells can remain at elevated temperatures but at zero enthalpy input, a phenomenon which has been variously called "Heat-after-Death"<sup>[35]</sup>, "Heat-after-Life" and "After-Effect". The clearest demonstration of this phenomenon is to be found in the publications of Mengoli et al e.g. see Fig. 32<sup>[36]</sup>.

This phenomenon clearly requires further investigation as it points the way towards the construction of energy efficient systems and as it must be related to other observations of excess enthalpy generation with zero enthalpy input. Equally excess enthalpy generation at the boiling points of the electrolytes require further investigation as the operation of the systems under these conditions would provide sources of low grade heat (however, we should note that steam compression could be used to raise the quality of the heat). Measurements of this kind require the use of some form of dual calorimeter such as those illustrated in Fig. 33<sup>[37,38]</sup> where the system can be driven to the boiling point in the lower section while boiling conditions can be maintained in the upper section. It is necessary to use some form of dual calorimetry as the dynamic range of any given instrument is too small to span the whole range of thermal outputs.

Fig. 34 illustrates a measurement made in 1994 with an ICARUS-9 calorimeter where boiling conditions were maintained for ~ 50 days at ~ 100% excess enthalpy and specific excess enthalpy outputs > 1 kW cm<sup>-3</sup>. However, we should note that the energy efficiency of this system was restricted by the low concentration of electrolyte in the lower section of the calorimeter.

I believe that we need to ask: do the experiments carried out so far (as well as the experiments which could be performed as logical extensions) not indicate that useful and practicable sources of heat could be developed on reasonable time scales? However, we also need to ask: how do we now stand with regard to our understanding of this topic and does this understanding have any wider implications for the development of the Natural Sciences? Here I would recall that a major factor in the initiation of this work was the realisation that hydrogen and deuterium in the palladium lattice could be part of an extended Quantum System. Work presented at this Conference<sup>[2]</sup> (see also<sup>[11]</sup>) shows that this idea is indeed correct and that the deuterons can be described by a unique wave function. I believe that this "proof-of-concept" should give an impetus to the search for the wider implications of the Q.E.D. paradigm in the Natural Sciences. Needless to say, the integration of the "Preparata Effect"<sup>[2]</sup> into the research on "Cold Fusion" should also lead to clearer demonstrations of the phenomena and the implementation of such devices for energy generation.

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