PROCEEDINGS

15th International Conference on

Condensed Matter Nuclear Science

Rome, Italy
October 5-9, 2009

Edited by Vittorio Violante and Francesca Sarto

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ICCF-15

15th International Conference

on

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Vittorio Violante RdA, Francesca Sarto

Preface

We had the great honor to organize ICCF15 in Rome, occurring on the 20th anniversary of the announcement of Professor Martin Fleischman, our Honorary Chairman, and Prof. Stanley Pons in the spring of 1989.The 20th Anniversary is certainly giving a specific meaning to the Symposium supported by the content of the given lectures.

We had the honor and the privilege that the Conference, supported by the Italian Agency for Energy New Technologies and Sustainable Development (ENEA), was under the patronage of the Italian Physical Society (SIF), of the Italian Chemical Society (SCI) and of the Italian National Research Council (CNR). We also had the honor to have as speakers Prof. Martin Flaischmann, Dr. Renzo Tomellini (Genaral Directorate for Research of the European Commission), Prof Luigi Campanella (President of the Italian Chemical Society), Dr. Enzo De Sanctis (Vice-President of the Italian Chemical Society) and Prof. Robert Duncan (Research Chancellor of the Missouri University).

During the last two decades we understood that condensed matter nuclear science is an extraordinary mixing of knowledge because of the wide interdisciplinary character of such a research field and our opinion is that this aspect has characterized this event.

The letter we received from the President of the Italian National Council of Research –CNR- Prof. Luciano Maiani perfectly describes our feeling:

He said: “The high scientific value of the event on condensed matter nuclear science represents a reason of great interest for the research world involved into the many aspects of such a discipline”.

We received also appreciations and wishes for the very interesting Conference, by the Department Committee of the Mechanical Engineering Faculty of the University of Tor Vergata in Rome.

During the Conference Prof. Martin Fleischmann was awarded with the Preparata golden medal into the wonderful scenario of Castel Sant’Angelo.

The ICCF15 participants have been received by S.S. the Pope Benedetto XVI into the Vatican.

In order to draw the conclusions about the outcome of our contribution to the Conference we all need to consider the objectives and compare them with the results.

On the occasion of the anniversary of Fleischmann and Pons announcement we were due to guarantee the best success to this Conference.

The participation of prestigious Institutions, the content and the level achieved by the lectures and the increased interest concerning our discipline have been the indicators for the level of the conference as well as of the scientific importance of the matter.

The Symposium has created the appropriate conditions for a useful exchange of ideas and for an advantageous osmosis of knowledge among disciplines concerning the condensed matter nuclear science.

The material science, the enhanced screening in nuclear processes, nanoscience, interphase phenomena and other disciplines are emerging as fundamental pillars in our research field.

Now considering the question whether the F&P effect exists or not the answer is given by the results presented at the conference and on previous occasions.

Considering the question whether the full control and understanding of the effect have been achieved the answer is given by the hard work done up to now and the harder and harder work still to be done in the future.

We personally consider that the scientific out coming of this conference be the beginning of a new scientific age for our studies

A big research work has been done during the last two decades but there is still a huge effort to be carried out in the years to come and only the involvement of the whole scientific community will make this effort possible.

Our challenge is to give priority to an easy and smooth interaction with the whole scientific community optimizing the effort mentioned above.

We have already done this in the past thanks to the programs reviewing the matter, we have continued during ICCF-15 and we will do the same in the years to come. This is our future!

First of all we wish to thank the Institutions giving their Patronage to the Conference: The Italian Physical Society, The Italian Chemical Society and the CNR.

We thank also ENEA for supporting the Event.

Thanks are due to Enenergetics Technologies since they partially sponsored the Conference and in particular the CEO Ms. Alison Godfrey.

We have also to thank Dr. G.Dattoli, and Dr. G. Hubler since they guided in a masterly manner the round table on theories.

We acknowledge the chairs of the conference sessions, for their skills and professionalism in heading the discussion.

We are very grateful to the co-chair Dr. S. Lesin, to the chair and to the co-chair of the Scientific Committee Prof. Franco Scaramuzzi and Dr. F. Frisone.

Very well deserved thanks to the president of the Kyoto Club Dr. G. Silvestrini for his lecture and to Prof. Melich and Prof. D. Nagel for their help in starting the job .

Thanks are also due to ISCMNS for supporting the Preparata Golden Medal Award.

We wish also to thank Dr. M. Polidoro, she was the Architect of the Conference, and the ENEA Staff: M. Cecchini, C. Torelli, F. Simoni, Emanuele Castagna, Stefano Lecci, Mirko Sansovini, D. Karacostas, L. Crescentini ed E.Vitale because of for their valuable effort in supporting the Event. A particular thank is for the Scientific Secretary of the Conference Francesca Sarto since her professional skill and accuracy was fundamental for the success of the Symposium.

Note about the Proceedings publication

This Book of Proceedings collects most of the papers presented at the 15th International Conference on Condensed Matter Nuclear Science (ICCF-15), held in Rome on 5-9 October 2009.

All the presenting authors have been invited to submit their paper for publication in the Proceedings and about the 90% of them accepted the invitation.

The papers have been reviewed by referees chosen from the scientific community, both inside and outside the more restricted group of scientists working in the field of “cold fusion”.

Due to skepticism of some part of the scientific community and lack of an established literature on this topic in full accredited journals, the review process was not intended to filter papers but to stimulate critical revision of his own work by each author. The aim was to get papers which could be as much as possible understandable by the open scientific community, discussing in deep the possible sources of artifacts and errors in the experimental results and clearly highlighting the working hypothesis, approximations and limits of the theoretical models.

The authors were encouraged to review their papers according to the referees' comments. In many cases this process resulted in a net improvement of the article; in a few cases the authors maintained their original version. Anyway, the only version of the papers acknowledged by the authors has been published. For such a reason, the only authors are responsible for their works' content.

Sincere thanks to all referees who voluntarily dedicated their time and expertise to improve this book.

Introductory remarks

The book should give a complete picture of what went during the Conference.

In accordance with the structure of the Conference we have organized the book following the scheme below:

Section 1. Electrochemical experiments
Section 2. Gas loading experiments
Section 3. Material science aspects
Section 4. Nuclear measurements
Section 5. Theory

The study of Condensed Matter Nuclear Science (CMNS) has been marked through 15 past conferences (ICCF1 at Utah, USA, in 1989 to ICCF15 in Rome, Italy). The advances in this discipline are based on the scientific findings that have been obtained along two decades of research activity. Nevertheless because of the lack of information very few people know that some hundreds of researches, the majority of them belonging to some of the most prestigious scientific Institution in the world, have continued this study during the past 20 years.

Background Information

In 1989 two electro-chemists, Martin Fleischmann and Stanley Pons announced that they had produced nuclear fusion reactions between deuterium nuclei in a table-top experiment, under ordinary conditions of temperature and pressure, by using electrochemistry. The experimental evidence consisted of the production of large amounts of heat, which could not be attributed to chemical reactions. The heat excess was revealed by means of calorimetric measurements during electrochemical loading of palladium cathodes with deuterium.

The reactions were termed "cold fusion", by comparison with the high temperature of thermonuclear fusion. One of the most intriguing features of the experiment was the substantial lack of the typical nuclear emissions associated with the excess of power, produced in thermonuclear fusion experiments.

The experimental results thus were in contrast with hot fusion data and were not supported by accepted theories. Many scientists concluded that there were no nuclear reactions and that the reported experiments were in error. Cold fusion was considered as an example of wrong science. This produced a partition between the traditional scientific world and the community which continued its research in the field.

In the 20 years elapsed since then, increasing evidence was found of the reality of the phenomenon, and an extended search for nuclear products connected with cold fusion was performed. Reproducibility was improved, and recently the first examples of cross-check experiments were implemented. Fourteen International Conferences have been held in those almost 20 years, and the present is the 15th of the series.

In 2002, also in order to take into account the variety of phenomena investigated, a new name was introduced, namely "Condensed Matter Nuclear Science" (CMNS). "Condensed matter" is a term employed by the American Physical Society for the last few decades to embrace the characteristics and mechanisms of both solids and liquids. CMNS was meant to focus on the science of nuclear effects in systems involving solids and liquids. It is an appropriate description for the current and continuing science of the field.

The International Society for Condensed Matter Nuclear Science was founded in 2003. It remains the primary scientific society of the field.

At present, the name that many people are using to identify the field is the "Fleischmann-Pons Effect" (FPE). That effect is the production of heat and other products in a deuterium-in-metal system under unusual circumstances of very high densities of deuterium. The amount of heat produced per reaction can be up to several hundreds of times the energy released per known chemical reaction. The power densities (measured in watts per cubic centimeter of the metal) occasionally exceed those from fission nuclear power systems.

Preliminary measurements of 4He at levels that is consistent with the measured energy gain, as if the effect could be ascribed to a deuterium+deuterium fusion, giving helium plus heat as products in the palladium lattice, have been carried out in some Laboratories in the world.

Even though it is difficult to make forecasts on practical applications of these phenomena, there is no doubt that the observed effects are indicative of a process related with the field of clean energy.

Many Institutes and Companies in the world are involved in this study either on experimental activities or on theoretical studies. However the phenomenon is not well understood yet.

ICCF History

The ICCF conferences, which began in 1990, have been held with a three continent rotation: America, Europe and Asia. It is the primary venue for the international community of involved and interested scientists to show and discuss results concerning the Fleischmann&Pons effect. The papers are then published in the proceedings of the conference. The numbers, years and locations of the ICCF are:

No.	Year	LocationParticipants		Countries		Papers		
1	1990	Salt Lake City, Utah USA		296		35		
2	1991	Lake Como, Italy			57			
3	1992	Nagoya, Japan	324	18	102			
4	1993	Lahaina, Maui, Hawaii, USA				12	65	
5	1995	Monte Carlo, Monaco		207	15	76		
6	1996	Lake Toya, Hokaido, Japan			175	17	110	
7	1998	Vancouver, British Columbia, Canada				218	21	76
8	2000	Lerici, La Spezia, Italy		145	18	68		
9	2002	Beijing, China	113	17	87			
10	2003	Cambridge, Massachusetts, USA			135		93	
11	2004	Marseilles, France			20	74		
12	2005	Yokohama, Japan			63			
13	2007	Sochi, Russia	75		93			
14	2008	Washington DC, USA		180	15	97		
15	2009	Rome, Italy	150	14	70			

ICCF15 is the second conference in 10 years as number of participants and is confirming a positive trend.

In addition to the ICCFs, there have been many other conferences on the Fleischmann&Pons effect in Russia, Japan, Italy, USA, including dedicated sessions at various scientific society symposia, such as those of the American Physical Society (APS), American Chemical Society (ACS) and American Nuclear Society (ANS).

Topics

The evidence that identical experimental observations have been done by different Institutions by using the palladium cathodes produced by ENEA and belonging to the same lots of material increased, without any doubt, the level of attention for such a discipline. The palladium giving a significant probability to observe the effect has several features that have been identified; however the possibility to reproduce a material having those characteristics remains still an open problem . The consequence is the absence of a full reproducibility and of the control of the signals amplitude.

Materials have a crucial role in the field of low energy nuclear reactions since their characteristics, on the basis of statistical data, seem to be a necessary conditions to observe the phenomena.

Materials characterization before, during and after the experiments is essential to increase the reproducibility. Such a study points to know the composition of the materials, their structure and their characteristics at nano-scale, in order to select possible mechanisms, occurring during the effect or triggering the effect, that are consistent with the material features.

ICCF15 gave a significant role to material science because of the reasons mentioned above.

A section of the Conference has been dedicated to both dynamic and static gas loading experiments. In general the energy gain obtained by using such a technique are lower than those obtained by using the electrochemical approach. The results obtained with hydrides (deuterides) nano-particles deserves a very deep study of the interaction of the hydrogen isotopes with particles at nano-scale since literature data show a significant difference between the values of the thermodynamic functions of hydrides at nano-scale compared with those at macro-scale.

The (D-D) fusion reaction cross section at low energy represent an important aspect of the discipline. Results from Tohoku and Berlin Universities show an increasing of the(D-D) fusion reaction cross section, at the energy in the order of some KeV when the reaction takes place into some materials like palladium, palladium oxides and others. As the energy decreases the cross section increases up to some orders of magnitude compared with the values, at the same energy, into plasmas or in vacuum. Such a behavior may be explained in terms of an enhanced screening into the condensed matter that was unknown until a few years ago, even if the reaction is giving the typical products (neutrons, 3He, protons and tritium). This is indicative of the condensed matter effect on low energy fusion reactions.

In the field of condensed matter nuclear science the nuclear measurements are investigating mainly:

D+D reaction products as expected in plasmas or in vacuum.

Possible products due to interaction of hydrogen isotopes with metal atoms (transmutations).

Emission of particles or radiations associated with the above mentioned reactions.

A proper space has been given into the Conference to talks on measurements techniques in order to give a proper frame for a critical review of the results in this field on the basis of the limits and potentialities of the used techniques.

4He measurements have a remarkable importance in the research on F&P effect since the energy gain observed in several experiments makes a chemical process inconsistent with the resulting energy per particle that is even above 100 eV/particle. Therefore the most likely expected ash is helium four produced by a D+D reaction without emission of radiation. This is a not simple measurement since 4He is in the atmosphere at 5.25 ppm and since this element (m=4.0026 a.m.u.) have to be revealed into a gas mixture containing D2 molecules (m=4.0282 a.m.u.). A high resolution and high sensitivity mass spectrometer is required for such a measurement. In addition we have to consider that the experiment must be conceived in order to avoid any helium leakage from the ambient into the cell and vice versa.

The study of transmutations is a controversial field within the frame of the condensed matter nuclear and an accepted answer whether the phenomenon exists or not doesn't exist yet. The evidence of elements after the experiment that were considered to be absent at the beginning is not enough since reorganizing the contaminant concentration profiles and/or contaminant from the ambient could be the reason for such an evidence. The most appropriate marker for transmutation processes produced by interaction, at low energy, of hydrogen atoms with metal atoms into the lattice, is the isotopic ratio measurement. As matter of fact if the "new elements" were be due to a nuclear reaction their isotopic ratio would have to be related to the one of the reacting species and then would have to differ from the natural one.

Such study may be performed with high resolution and high sensitivity mass spectrometers and cross check between different laboratories to validate the data to be appropriate.

A round table on theories was organized to give, during the conference, a further occasion to have an open discussion on the status of the theoretical work in the field . Several participants highlighted the importance of having reliable and shared experimental results free of possible experimental artifacts.

The mixing of knowledge and the interdisciplinary nature of the matter makes this task very complex but exceedingly challenging.

To enhance the exchange of ideas some review talks were given to experts in some discipliner crossing CMNS. The reviews were on:

Material science
Material characterizations
Optics and photonics
Nuclear measurements
Mass spectrometry

The main out coming was the evidence that F&P effect is a real effect with energy gains that cannot be explained in terms of chemical processes.

Even if a reasonable transportable reproducibility has been achieved and material characteristics, that are necessary conditions to observe the effect, have been identified the start up of the effect and the amplitude of the signals are not under control yet. A complete theoretical frame defining the effect, its trigger and able to increase the reproducibility and the amplitude of the signals is not available yet.

The increasing of the knowledge and the consequent improvement of the control of the effect is the target that is getting close.

Vittorio Violante RdA, Chairman ICCF–15

ENEA – Frascati Research Center, Rome (Italy)

Foto Group, October 5, 2009

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Castel Sant'Angelo

Castel Sant'Angelo

Welcome of the Italian Physical Society (SIF)

I am pleased to extend a greeting of the Council of the Italian Physical Society to all participants to the to ICCF15-2009 symposium.

The Italian Physical Society (in the following I will use the acronym SIF, for Società Italiana di Fisica) is a non-profit, scientific association devoted to promoting and favouring the progress of physics in Italy, to increasing its understanding and applications, and to supporting physicists.

SIF represents the Italian scientific community in the research, educational and professional fields, both private and public, relevant to all areas of physics and its applications. It has an extensive membership - mainly national - and is a leading communicator of physics to all audiences, from specialists through government to the general public.

My presence here today and the SIF patronage to the meeting confirm the loyalty of SIF to its original mission and its determination to work for advancing science, while keeping the research within the border of the experimental method.

In fact, from its foundation, which dates back to 1897, SIF has been tightly bound to the Galilei's method. This strong ideal tight is shown, among other things, by the title of its own journal of physics, Il Nuovo Cimento, that recalls the ancient Accademia del Cimento, the association founded in 1657 by Prince Leopoldo de Medici and the disciples of Galileo Galilei. SIF also adopted the association logo of the Academia del Cimento, shown in Fig. 1, in which are reproduced a burner, three crucibles full of melted metals, and a flying scroll with the motto "Provando e Riprovando" (trying and trying again), which refers to the experimental method. The motto can also be interpreted as "proving and reproving", which is also very appropriate.1 Giovanni Polvani, President of the SIF from 1947 to 1961, described in a very effective and elegant way the meaning of the logo:

"Cimento in its pregnant meaning is at the same time the trial, the test, the effort, the risk, the peril, the experiment, the comparison, the thirst for knowledge, the extent to which the metal refines in the crucible. The crucible then is the mind, and the two words (provando e riprovando) of the enterprise, mirroring each other, show the route to attain, by trying and trying again, the "beauteous truth". It is the essence of the Galilei's method."

The story of the cold fusion research is particularly hard and the link to the experimental method very appropriate.

The link to the experimental method is very appropriate for the cold fusion research, the story of which has been particularly hard.

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1) It is worth mentioning that "provando e riprovando" is found, with this latter meaning, in the first tercet of the third "canto" of the Paradise in the Divine Comedy poem by Dante Alighieri:

"That Sun, which erst with love my bosom warmed,
Of beauteous truth had unto me discovered,
By proving and reproving, the sweet aspect."

translation by Henry Wadsworth Longfellow of the original:

"Quel sol che pria d'amor mi scaldò 'l petto,
di bella verità m'avea scoverto,
provando e riprovando, il dolce aspetto."

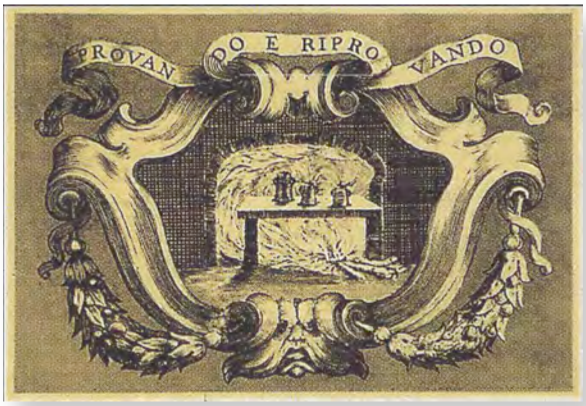

Fig. 1 – The logo of the Italian Physical Society

Here the beauteous truth, which Dante refers to, concerns the lunar spots and the influence of stars.

Cold fusion, first announced on March 1989, raised hopes of a cheap and abundant source of energy.

Enthusiasm turned soon to skepticism after replication failures of the original experiment. In November 1989, the majority of a review panel organized by DOE found that the evidence for the discovery of a new nuclear process was not persuasive.

In 2004, a second DOE review reached conclusions similar to the first, but with a smaller majority. Moreover,

"the reviewers identified two areas (properties of deuterated metal and search for fusion events in thin deuterated foils) where additional research could address specific issues, and invited funding agencies to entertain individual, well-designed proposals".

The interest in the field has been growing in recent years, as shown by the presence of a session on cold fusion at the APS meeting in 2007. A number of basic research areas interesting in itself and helpful in resolving some of the controversies have been identified. A number of researchers keep researching and publishing in the field. In brief, today could fusion is back on the menu.

Since its first edition in 1990, the International Conferences on Condensed Matter Nuclear Sciences have

provided a good forum for researchers to share their results and promote the understanding, development and application of the discipline. I am sure that also this edition of the Conference will be a success and hope it will pave the way for further sound developments.

I also wish everyone a pleasant stay in Rome.

Enzo De Sanctis

Italian Physical Society (SIF) - Bologna

In Memory of Andrei

Andrei Lipson was a long-time, highly respected member of the Condensed Matter Nuclear Science community. He was a member of the International advisory committee for the ICCF meeting series, representing Russia. Aside from his home base at the Institute of Physical Chemistry, Russian Academy of Sciences and the Joint Institute in Dubna, he "had suitcase and would travel" to do research. Stays elsewhere included over four years with me at the University of Illinois, time at the New Hydrogen Energy Laboratory in Sapporo and then at Tohoku University with Professor Kasagi, time in Israel at Energetics Technologies, at the US Naval Research Laboratory and the Naval Postgraduate School in CA with Michael Melich. He had just accepted a visiting position at the University of Missouri - Columbia when his unforeseen death occurred while riding a subway in Moscow. This seemed impossible to me—Andrei was in good heath when I saw him months earlier at ICCF-15 in Rome, and again 2 weeks before his death when he stopped at my lab to spend several days talking about joint experiments.

Several months later I gave a presentation for him that we had been working on for the ACS meeting in San Francisco. It was my honor to represent him in this manner. Many Russian colleagues and ICCF colleagues can recount Andrei's many contributions to the field. However, here I will simply present my personal view based on the years of close collaboration we had. Andrei was a great colleague and had a significant influence in my LENR research. It seems like only yesterday that I first met him at an ICCF meeting in Europe. He had just returned from a stay in Japan where he collaborated with Professor Kasagi on low-energy nuclear cross sections using ion beam-target experiments. I approached Andrei afterwards and asked if he would come to Illinois to join my work, if I could raise enough money. He consented. Later, in e-mails, I asked if he "believed" in cold fusion. He replied that he thought so, but wanted to keep an open mind because "experiments would determine the truth". I told him that I shared that view.

We do continued experiments on thin film electrode concepts, we successfully applied some analysis techniques that Andrei used in Russia, such as nuclear particle detection using CR-39 film. He also contributed, in collaboration with others at the University of Illinois, developed a pioneering method for creating near metallic density hydrogen (or deuterium) states in dislocation loops in palladium. The resulting Physics Review article about superconducting properties of this state has received a number of citations.

There is not enough space to tell all I want to about Andrei's accomplishments and our close relationship. I grew to deeply respect him as a person and as a scientist. The interested reader can find out more about him from the article by his daughter, Maria, in Infinite Energy magazine following his death. Also, I was asked to speak about some of his recent research at the March 2010 American Chemical Society meeting in San Francisco. Andrei prepared a presentation on the effect of electron beam bombardment on loaded hydrides for that meeting which can be found in the proceedings based on my presentation and my comments about Andrei's many contributions to cold fusion research.

As all people in the field know, Andrei was extremely prolific in his range of research work, and he had numerous publications. As a result of his collaboration with me, we had coauthored along with others in my lab and his lab back home over 50 publications ranging from articles in ICCF meeting proceedings to articles in various journals. In addition, Andrei had a number of other publications that I was not involved in.

A characteristic of all this work is that the problems were attacked from a very fundamental, basic science point of view. Another characteristic of Andrei that I deeply admired and which made me so comfortable working with him was his high integrity and honesty in everything he did. He never allowed himself to become so emotionally involved that he would fail to bring out all the facts about anything he was studying. Andrei was very strong willed and hard to convince if he had already formed an opinion, but I could always have complete faith in any results that Andrei reported. And if your point of view had merit, Andrei would keep an open mind and help you get deeper insight into the physics issues. We will all sorely miss him.

George H. Miley

Professor, University of Illinois - USA

Foreword

Cold Fusion (LENR) One Perspective on the State of the Science”

M.C.H. McKubre
SRI International, Menlo Park, California.

Abstract. With recent publicity outside the CMNS field it has become increasingly important to clarify in non-specialist terms what is known and what is understood in the general field of so called Low Energy or lattice Enhanced Nuclear Reactions (LENR). It is also crucial and timely to expose and elaborate what objections or reservations exist with regard to these new understandings. In essence we are concerned with the answers to the following three questions: What do we think we know? Why do we think we know it? Why do doubts still exist in the broader scientific community?
In this Foreword to the Proceedings of ICCF15 I lean heavily on the experimental work performed at SRI by and with its close collaborators (ENEA Frascati, Energetics and MIT) with a view to define experiment-based non-traditional understandings of new physical effects in metal deuterides.

1. Introduction

I was tasked to review the state of the science: at least 1000 man-years worth of work in 30 minutes, and here on a few pages. Of course it is impossible, so what was and is presented here is a very brief and personal view of the state of the science, through time and space constraints necessarily avoiding consideration of many large and important research subtopics.

It is important to understand what we have come together to study. On March 23rd 1989 Fleischmann, Pons and Hawkins [1] reported results of:

i. an anomalous heat effect resulting from the
ii. extensive, electrochemical insertion of deuterium into palladium cathodes
iii. occurring over an extended period of time.

The underlined phrases are important and often forgotten. The effect reported was a heat affect. Calorimetry is the means of studying heat effects. Please note the underlined words: extensive, electrochemical insertion for a prolonged period of time, of deuterium into palladium. The experiment is electrochemistry, with which very few in the physics community were familiar. And the process occurred with an initiation time many times longer than the time constant of diffusional insertion of deuterium in palladium.

This heat effect occurred at a level consistent with nuclear but not chemical energy or known lattice storage effects, but occurred (*mostly*) without penetrating radiation (α, β, γ, n°) or lattice activation. A remarkable feature of the effect is that a prodigious amount of energy is produced. This energy is not only much greater than can be attributed to chemical reactions, there is no physical evidence for such reactions. We have seen this heat affect occurring at hundreds or thousands of times the energy of any chemical reaction. These are the characteristics of the Fleischmann Pons Effect (FPE) and from our present vantage point we can begin to answer some questions:

1. What do we think we know?
2. Why do we think we know it?
3. Why do doubts still exist in the broader scientific community?
4. How do we propose to make progress?

2. What do we think we know?

The existence of an excess power effect is an experimental question, independent of theoretical issues or preconceptions. A great many experiments in which positive excess power results have been presented can be found in the International Cold Fusion Conference series over the past 18 years. Of these the experiments based on those of Fleischmann and Pons are perhaps the most studied and discussed, which makes the FPE of interest to us in our present discussion.

In the studies done at SRI over the years, an effort was made to understand specifically what conditions are required for excess power to be observed in the Fleischmann-Pons experiment [1] (keeping in mind that different requirements apply to other kinds of excess power experiments). A number of such requirements were noted: (i) a cathode had to achieve a maximum loading of about D/Pd = 0.9 or higher; (ii) high loading needed to be sustained for 2-4 weeks; (iii) a current density above threshold was required; and (iv) relatively high loading needed to be present for a heat burst to occur. In addition, it was found that changes in the operating parameters could initiate a heat burst, which may be related to a more general correlation between excess heat and a net deuterium flux either in or out of the metal.

The research activity into the FPE at SRI has now accumulating more than 60 man-years of research. We first focused attention on the critical importance of deuterium loading, the role of chemical poisons and additives in controlling the electrochemical interface, in order to achieve and maintain high D/Pd loading. We studied the correlation of excess power production with loading and reported simultaneously with IMRA-Japan [2,3] the threshold onset of the FPE. We designed and built a novel, high-accuracy, fully automated mass flow calorimeter, and set out to perform replication studies of the Fleischmann and Pons heat effect, first to confirm the existence the effect and second to better define the physical conditions under which it can be observed.

As an interim conclusion of these activities we were able to define the parameter space in which one might expect to encounter the Fleischmann-Pons excess heat effect, evaluated as an empirical expression:

$$\mathbf{P_{xs} = M\,(x\text{-}x^\circ)^2\,(i\text{-}i^\circ)\,|i_D|} \qquad [1]$$

where x = D/Pd, x° is the threshold value typically ~0.875, the current density threshold i° typically falls in the range $75 < i^\circ < 450$mA cm^{-2}, the deuterium interfacial flux i_D = 2-20 mA cm^{-2}. It is important also to recognize a time threshold t° of at least 10 times the deuterium diffusional time constant.

3. Why do we think we know it?

Evaluation of the terms of equation [1] has been the subject of a number of reports and analyses, authored particularly by SRI, ENEA and Energetics but including data from a wide range of experimenters. This analysis will be discussed in more detail in reference [4] of this Proceedings volume. It can no longer be asserted rationally that there no heat effect in any of the very large number of experiments reported here and elsewhere [5], or that the effect is the result of (unknown) energy storage or (unseen) chemistry. Also, at this point, any claim that the Fleischmann-Pons Effect is “irreproducible” is not only unsound, it is unscientific. Where and when we are capable of reproducing all parameters critical to the effect, we reproduce the effect.

4. Why do doubts still exist in the broader scientific community?

It might be appropriate to think of this question in terms that apply to parenting. First there was a difficult birth in conditions that while not initially hostile rapidly became so. Second there has been a great deal of poor communication on both sides: an inability to broadcast real scientific progress uncoupled from emotion or ambition; an almost complete lack of willingness on the part of those outside the CMNS community to delve into the work and understand what has been done, and what has changed, in 21 years. Finally, although not critical and somewhat circumvented by imagination, there has been an insufficiency of funding for such a materially complex (and I would argue potentially important) problem. The child, abused at birth and abandoned by most, that Minoru Toyoda helped rescue, now misunderstood and fiscally restrained, has just turned 21. I will not discuss the problem of fiscal constraint, in part because if we solve the issues of hostile rejection and poor communication that will not remain a problem.

Great significance was attached to early negative excess heat results reported by a small number of groups at prestigious institutions. In light of the discussion above, it is useful to see whether these experiments, as well as other early experiments, were operated in a relevant regime. Perhaps the most cited early negative result was that of Lewis *et al* [6] from CalTech in which they reported that *"D/Pd stoichiometries of 0.77, 0.79, and 0.80 obtained from these measurements were taken to be representative of the D/Pd stoichiometry for the charged cathodes used in this work.*" Also widely cited is the early negative result of Albagli *et al* [7] from MIT who discuss "*average loading ratios were found to be 0.75±0.05 and 0.78±0.05 for the D and H loaded cathodes, respectively.*" The CalTech and MIT negatives are noted in Figure 1 in a histogram illustrating a number of early SRI experiments producing positive excess power results as a function of loading.

Even lower loading results were estimated by Fleming *et al* [8] from Bell Labs in a negative report. In this paper the authors state "*the degree of deuterium incorporation was comparable to that for the open cells for the same time duration. The amount incorporated in longer electrolysis experiments was typically PdD_x (0.45 < x < 0.75).*"

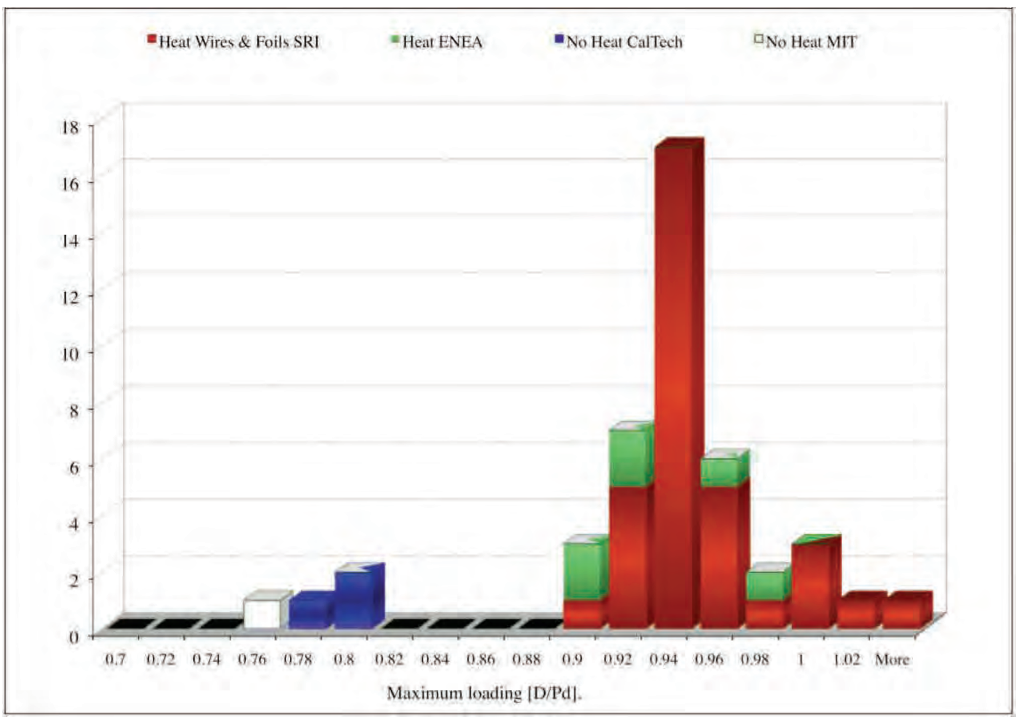

Fig. 1. - Histogram illustrating the number of early experiments at SRI and ENEA showing measurable excess power as a function of maximum cathode loading. Also illustrated are points for the MIT negative experimental result, with a stated loading of 0.75±0.05 (Ref. [7]); and for the CalTech negative experimental result, with loading measurements quoted at 0.77, 0.78, and 0.80 (Ref. [6]).

From what we know today, and Figure 1 clearly illuminates, none of the cells in any of these cited studies would be expected to evidence any excess heat. Not only for the reasons of a loading deficiency (as stated explicitly): the durations of the experiments were wholly insufficient for a (typical) 300 hour initiation time; the current density stimuli were in the large part too small; the deuterium flux was not measured. None of the criteria of Equation [1] were shown to be met, at least two demonstrably were not. In hindsight it is evident that the authors were victims of "unknown unknowns", and perhaps "indecent haste" -- but this is understandable in the circumstances of 1989. What is important is that these experiments be recognized for what they are, not what they are not. They are important members of the experimental database that teaches us under what conditions one encounters the FPE. They are not any part of a proof of nonexistence; absence of evidence is not evidence of absence.

So what are the salient criticisms today? The following attempt is made to anticipate some of the questions and at least point in the directions of resolution. Basic questions:

- "*The experiments/results are not reproducible*":
  - Some experimental teams see no results
  - Different results are seen in different laboratories
  - Inconsistent results in the same laboratory with similar samples

- "*The results are inaccurate*":
  - Mis-measurement of input power
  - Mis-measurement of output power
  - Excess power is not outside the measurement uncertainty
- "*The heat is real but is due to unknown or unaccounted chemical effects or lattice energy storage*":
  - Over-accounting for electrolysis products
  - Chemistry in the electrolyte volume outside the cathode
  - Energy storage and release (small percentage integral excess energy)
  - Hydrinos or other exotic, "high-energy" chemistry
- "*Missing nuclear products*":
  - Quantitative energetic products not seen
  - Difficulty of measuring ^4He in the presence of D_2 and ambient

"*The experiments/results are not reproducible*"

First the existence of an apparent irreproducibility is widely recognized and acknowledged, and several papers have been written on this topic [9-11]. What is sometimes forgotten is that the most reproducible effect by its very nature is systematic error. Irreproducibility of results far from being a proof of non-existence argues more the contrary, and simply indicates that not all conditions critical to the effect are being adequately controlled.

Early flippant and intentionally unserious, as well as other claimed serious attempts were made to correlate the appearance of positive FPE results with the record (or existence) of university football teams and with national character. Serious criticisms do exist, however, and it is well recognized that different experiments, even intentionally identical and performed simultaneously in the same laboratory, give different FPE results. These experiments also give different results of much more mundane measurements. In the early days of studying the FPE at SRI experiments were designed to probe the parameters of reproducibility. Sets of 12 cells were prepared, intentionally identically, and operated simultaneously to monitor the time evolution of electrochemical and physico-chemical parameters believed to be pertinent to the FPE.

A single length of palladium wire was used from a known source and sectioned into 13 identical lengths. These wire sections (typically 3 or 5 cm in length and 1 or 3 mm in diameter) were machined to remove surface damage and inclusions, spot welded with 5 contacts (one cathode current and 4 wires for axial resistance measurement), annealed, surface etched (to remove surface contaminants) and mounted in 12 identical cells. These processes all were performed in the same batch and all by the same person. The twelve cells were filled with electrolyte from a single source and then operated electrically in series (*i.e.* with identical currents) in a 3 × 4 matrix in the same constant temperature chamber.

The variables measured were current (one measurement), cell voltage, pseudo-reference cathode potential, temperature and electrical resistance (D/Pd loading) all being monitored with the same instruments. Intermittent measurements were made of the cathode interfacial impedance. With 12 intentionally identical experiments, every one behaved differently. Not only in terms of their heat production, significant and marked differences were observed in: the current-voltage-time profile for both the cell voltage and reference potential; the ability and willingness of each electrode to absorb deuterium measured by the resistance ratio *vs*. time curve; the maximum loading achievable; the interfacial kinetic and mass transport processes reflected in the interfacial impedance. Every one of these parameters was different for each of the 12 electrodes, in every set tested!

This matrix experiment was repeated several times in an attempt to understand the origins of the irreproducibility, and therefore control it. Trace impurity differences were observed to be contributory and there were two sets: deleterious impurities (poisons) that we learned to avoid; impurities that were beneficial to high loading in controlled amounts.

We were not able to control the variability of results simply by electrochemical (and trace chemical) means. The second major factor of experiment variability is the palladium metal cathode: source and condition. Figure 2 plots as a histogram the number of cells attaining the specified loading (whether in a calorimeter or not) varying by metal source or lot #. The first material used extensively at SRI, designated as Engelhard Lot #1 (E#1 on the plot) demonstrated in an astonishing 32% of all experiments a maximum loading D/Pd >0.95, with 36% >1.0, and 14% (3 cathodes) > 1.05. An electrode capable of attaining and <u>maintaining</u> high loading, is an electrode that is capable of producing excess heat thus a total of 82% of all samples of E#1 material, if properly stimulated, would have been expected to demonstrate the FPE. Unfortunately this apparent success illuminates the problem. Other materials even from the same manufacturer were far inferior and none yet has been found to approach the loading ability of Engelhard Lot #1[1].

Fortunately there is some consistency of behavior within a consistent set of materials. Electrodes made from the same material lots produce similar excess heat in different calorimeters, in different laboratories. Recently we have been working collaboratively with the Violante team of ENEA (Frascati) and the Energetics team of Dardik, Lesin *et al* to conduct comparative studies on material of similar general form: Pd foils 80 mm long, 7 mm wide and 50 μm thick, designed and produced by ENEA. Figure 3 presents a comparison of results obtained in two different calorimeters, one at SRI and one at ENEA, following Energetics current protocols[2].

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[1] Important but equally confounding, E#1 had the highest levels of impurities of any material we have ever employed in these studies, far higher in fact that the manufacturer's specification of 99.7% purity.

[2] The unique feature of Energetics' experiments is the use of a fractal sinusoid current stimulus designated by them as a SuperWave™. Alone among all of the current modulations tested at SRI, this waveform is capable <u>simultaneously</u> of supporting high D/Pd loading <u>and</u> high interfacial deuterium flux. In the terms of equation [1], <u>both</u> are needed for excess heat production.

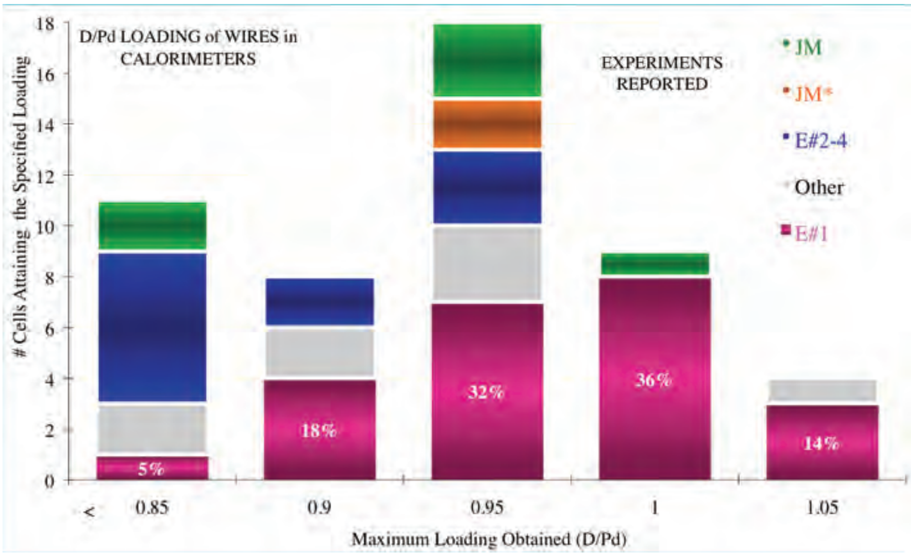

Fig. 2. - Histogram demonstrating the ability of a Pd cathode to load in 1M LiOD *versus* material source. JM = Johnson Matthey, JM* was a special lot designed to replicate pre-1989 materials, E = Engelhard.

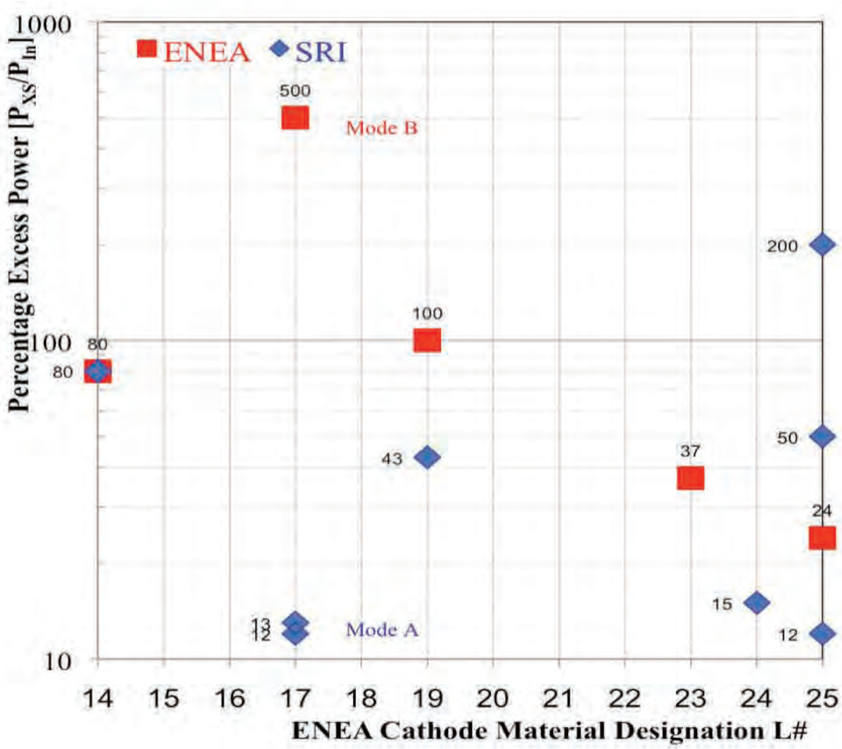

Fig. 3. - Comparison of results obtained from the same material lots performed independently at ENEA (Frascati) in a closed-cell mass flow calorimeter and SRI (Menlo Park) in an open-cell heat flow calorimeter [12,13].

The different lots of materials reflect different sources, rolling and annealing conditions and are designated by the ENEA "L" number, L14, 15, 16, 17 *etc*. on the horizontal axis. Two experiments with a particular lot of material, L14 run at SRI and ENEA, produced completely independently of each other (without knowledge of the other's experiment) 80% excess power/input power. With another material, L17, SRI saw 13% and 12% excess power and ENEA observed 500%, but the excess heat production in the two laboratories appeared in different modes. The L19 material showed 43% at SRI and 100% at ENEA. The general point is, that the same lot type of material will give the same approximate level of excess power result in two different laboratories. There is a consistency of behavior, and that behavior varies very much with the lots; the lot numbers without points in Figure 3 produced no excess power at SRI or ENEA (although all lots were not tested at both locations).

"*The results are inaccurate*"

The issue of mis-measurement of input electrical power has been recently raised [14] both generally for dc current stimulus and specifically in reference to Energetics proprietary SuperWave™ modulated current waveforms. It is quite difficult to understand the basis for this criticism and how and why it persists or surfaces after 21 years. Measurement of current, voltage, resistance, and time are some of the most familiar to engineers and scientists. Industries depend on the accurate measurements of power and energy for waveforms far more complex than any used (so far) in FPE experiments. In general the electrochemical cells are operated under controlled current conditions so that only the voltage varies. Accurate determination of power is thus a scalar, not vector operation and only simple precautions are needed for accurate measurement.

The Nyquist sampling theorem states that one can perfectly reconstruct an analog signal by sampling at twice the highest frequency component. Of primary concern in such measurements therefore is ensuring that higher frequency components are not present unmeasured in the input signal. In general this is very easily arranged by constraining the power bandwidth of the current (and voltage) source. For Energetics' SuperWaves™ that contain deliberate high(er) frequency components the solution to this measurement issue is more complex, but still experimentally quite trivial. Current-Voltage pairs are sampled and multiplied at a 50 k Hz. rate and only then averaged to obtain the input power. Several experimental checks on this procedure have been applied by Energetics, SRI and ENEA [15,16]:

a. The current and voltage measurement rate (50 k Hz.) used is 500 times higher than that of the highest SuperWaves™ frequency applied.
b. Ten times higher measurement frequencies (500 k Hz.), have been used with no significant difference observed in the input energy.
c. A fast commercial power meter (Yokogawa WT210/230) has been used with sampling rate of 100 kilohertz. The results were in agreement within +/- 0.5%.
d. At SRI and ENEA high frequency oscilloscopes and spectrum analyzers have been used at times of excess power production to demonstrate that the energy of frequencies higher than the Nyquist limit could have no calorimetric consequence.
e. Following our standard replication protocol [6], experiments set up using Energetic' data acquisition systems at SRI, were completely replaced with an entirely new data acquisition method and system with entirely consistent results [12,13].

Another obvious factor is that calorimeters measure total, absolute energy probably better than any other instrument. Most of the time, most of the calorimeters operate on the thermal baseline with output = input. If the issue were really low sample frequency one would expect to see an error at all times as a systematic effect of the input.

Since many different kinds of calorimeter have been shown to demonstrate consistent effects it seems also very unlikely that significant systematic errors are present in the measured output power and energy. As with the evaluation of input power, the variables needed to resolve output power (mass, time, resistance, temperature difference) are some of the easiest measurements we typically make. It is very hard to sustain rationally any argument that so many people have been mis-measuring these variables consistently for 21 years, with new people entering the field learning or copying the same errors.

A final point arguing against the universal presence of systematic error measurements is the sheer magnitude of the effect. At SRI we have seen an excess power effect at 90σ, ninety times the measurement uncertainty, and have made over one hundred observations of $P_{XS} > 3\sigma$. The effect is not fleeting and persists for hours, days, weeks, in one case longer than 1 month, giving ample time to check the measurement systems. And the output power is not small compared to the power input with power ratios $P_{Out} / P_{In} > 2, 3, 5$, the highest sustained value measured being 25 [17] averaged over 17 hours!

"*The effect is due to chemistry or energy storage*"

Several factors are often suggested in an argument that FPE excess heat is real and measured correctly but that its cause must be other than nuclear because no such nuclear processes are known. Some of these are:

1. Over-accounting for electrolysis products
2. Chemical reactions involving species in the electrolyte volume
3. Energy storage (slow and unseen) and release (rapid)
4. Hydrinos or other "exotic" chemistry

Is the FPE due to chemistry or energy storage? Simply, it is not! Anybody who has the ability and willingness to undertake simple calculations on the energies of these two different kinds of effects – nuclear and chemical – will easily be able to ascertain that the FPE is not caused by chemistry or an energy storage effect. Furthermore, if it were, that effect would be interesting and potentially very useful. The inventories of chemical species are simply too few. A continuous error such as unwitnessed and unexpected recombination of D_2 and O_2 inside intentionally open calorimeter cells has an energy capacity of the same magnitude as some heat effects observed in them, but this argument fails on two grounds:

i. the FPE is measured reliably and robustly in closed cells where this effect can play no role, and is similar in form and magnitude to the effect measured in open cells,
ii. accurate account is easily (and routinely) taken for the amount of water added for electrolyte makeup due to Faradaic loss; prolonged periods of energy excess due to unmeasured recombination would result in FPE cells requiring less[3] D_2O (or overfilling).

Detailed energy balance can be complicated in FPE experiments because these occur over long periods of time with no energy excess, and may have many and varied energy inputs. Although many have been accomplished with absolute statistical certainty, the early numbers were not very satisfying and (for example) do not suggest a basis for a useful energy source. This question of energy balance was put finally to rest resoundingly by the Energetics team in experiment L64 [17] about which there has been much comment [5,10,12-14]. This experiment lasted a relatively short time, there was very little time before the excess power burst was achieved, and the energy out was markedly greater than the energy in. There wass no time for energy storage in this process.

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[3] The amount of water needed to refill an open electrochemical cell can be readily and accurately calculated using Faraday's Law that relates the moles of species consumed by electrolysis to the total charge passed.

Energetics experiment L64 using a 7 mm × 80 mm × 50 μm Pd foil from ENEA (Frascati) and SuperWaves™ current stimulation demonstrated a maximum output power >34 W twice in the first 20 hours of the experiment, with an input electrical stimulus less than 1W. The energetic response was even more startling with 40 kJ of input energy in that first 20-hour period, 1.14 MJ of energy out, 1.1 MJ of excess energy. A factor of 25 times more energy coming out as heat than was input electrically. For this first heat burst alone the energy was 4.8 KeV/Pd atom, thousands of times more than can be accounted for by known chemistry. A second burst produced boiling in the electrolyte and at least[4] 3.5 MJ more energy, a total of more than 20 KeV/Pd atom. Similar but slightly less impressive results have been obtained on several other occasions by Energetics.

Missing nuclear products

Initially applied as a "*where is the beef?*" denunciation, the question "*where is the ash?*" was posed (or supposed) to refute the existence of the FPE on the grounds that the only products[5] possible were energetic and therefore easily observed (and even hazardous). At SRI we have made efforts at varying levels to search for a very wide range of potential nuclear products and ash.

Some salient criticisms are listed below followed by comments:

1. The expected energetic radiation does not accompany heat production
2. The nuclear products claimed cannot account for the excess heat
3. The claimed quantitative product (^4He) is:
    a. Impossible to produce
    b. Difficult to measure
    c. Not found in sufficient quantity

The first question was first and most directly answered by Julian Schwinger in 1989 [18]: "*The circumstances of hot fusion are not those of cold fusion*". By this he suggested that quantum coherent superstructure of the Pd(D) lattice might be expected to change the reaction mechanism, the rate, and the product branching ratios. At present there is no consensus among those in the field as to what physical mechanism is responsible for the effect although many propositions are under active discussion and significant progress is being made [for one proposed pathway see 19,20]. Potential products are therefore equally obscure but no rational basis exists to deny the existence of the FPE on the grounds of non-observation of a hypothetical product.

Some nuclear products of FPE reactions clearly exist sub-quantitatively with the excess heat. Tritium and ^3He are produced in FPE experiments, under special circumstances, largely asynchronous with the excess energy [5]. Claims have been made for "massive transmutation" at (or above) the levels needed to account for measured excess energy [5]; these have yet to be verified.

For some time at SRI we have been performing experiments to test the hypothesis that the quantitative product of the heat producing reaction is ^4He that evolves primarily without

[4] This amount is under-estimated as the heat of vaporization of D_2O was not included in this energy total.

[5] The term "ash" in "nuclear ash" is a technically inaccurate analogy to chemical ash. In chemical combustion, the ash is left-over material that does not participate in the reaction. It the residuum of non-volatile oxidized and pre-oxidized materials. In the field of cold fusion, the term "nuclear ash" has come to mean the reaction product. This is equivalent to describing the chemical combustion products CO_2 and H_2O as "ash," which is incorrect. Thus, in cold fusion helium is sometimes referred to as "nuclear ash" but it would be more accurate to call it a potential product of nuclear reaction.

associated energetic byproducts. This hypothesis did not originate at SRI. As early as 1991 Miles and Bush [21] developed in ingenious self-sparging helium sampling system using electrolytic evolution of D_2 and O_2 to purge out atmospheric ^4He. They obtained a seemingly unassailable statistical correlation between heat and helium production computing a 1:750,000 chance that the correlation was random.

Miles and Bush also obtained a very impressive early quantification of a reaction Q value. Compared with the value predicted for an overall reaction of the sort $d + d \rightarrow {}^4He + 23.8$ MeV (*lattice*), (yielding 2.5×10^{11} ^4He s^{-1} W^{-1} of energy excess), Miles and Bush measured an average value of $1.4 \pm .7 \times 10^{11}$ ^4He s^{-1} W^{-1}, 54% of the hypothesized value. Later in a study to replicate this work at SRI Bush [22] measured an average $1.5 \pm .2 \times 10^{11}$ ^4He s^{-1} W^{-1} (58% of the "expected" value).

Numerous others have made measurements of gas phase ^4He during or immediately following FPE heat excursions [5]. In general the amount of measured helium lies between ~50 and 75% of the amount[6] predicted for a net reaction[7] $d + d \rightarrow {}^4He$. Important experimental and theoretical issues attach to the question: "is there missing ^4He?", and, if so, "why"? If the net reaction were as written, and occurred in a skin layer close to but below the Pd cathode surface, then one might crudely expect ~50% of the ^4He to leave the cathode while the rest goes deeper to be trapped. Lending some weight to this hypothesis 15 studies have found unexpected ^4He in metal cathodes after FPE energy production [5], although in no case was the amount of ^4He measured sufficient to account for the gas phase deficiency.

In considering the possible fate of ^4He it is important to recall that the surface of a heat-producing FPE cathode is not well-crystallized Pd, even if it started as such. After extensive electrolysis in LiOD (for example) the cathodic surface will have incorporated significant Li, and the electro-active metallic surfaces become covered with a many-micron layer of hydrated oxy-hydroxides incorporating adventitious (as well as deliberately added) elements from the electrolyte, and leached from cell walls and parts, and from the two electrodes. In particular this "sludge" layer will act to restrain or delay ^4He release and it is reasonable to anticipate that work must to be done to disrupt this layer to approach an accurate mass balance. Since 100% of the helium can never be recovered, this balance will underestimate the total, but as recovery techniques improve it will asymptotically approach the true mass balance.

Limited resources have limited to only 2 the number of successful heat producing experiments in helium leak-tight calorimeters for which effort was extended to scavenge ^4He held up (by whatever means) in the cell volume. Of these one performed at SRI [25] and the other at ENEA (Frascati) [15], both yielded a total mass balance of ^4He produced within approximately ±10% the 2.5×10^{11} ^4He s^{-1} W^{-1} value, supporting a claim for an overall reaction Q of ~24 MeV/^4He atom produced. This is an important result that needs further verification.

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[6] One result in the early Miles Bush work measured ^4He at greater than 2.5×10^{11} ^4He s^{-1} W^{-1} but was attributed to experimental error [23]. In the published literature only the work of DeNinno and coworkers offers evidence [24] of super-quantitative ^4He.

[7] For reasons involving local energy and angular momentum conservation it is clear that, even if this is the net process, this reaction does not occur in a single step as written without the intimate involvement of other bodies. Since thermodynamics is path independent, however, we can calculate accurately the energy of the overall exothermic process, without knowing the pathway.

5. Summary and conclusions.

On the basis of the evidence and arguments presented here, and far more extensively and compellingly elsewhere [5,10], it is apparent that the Fleischmann-Pons effect is a new effect in physics. It requires a new mechanistic description and explanation. This new effect is very likely to be associated with a significant number of other condensed matter nuclear processes that await exposition and development.

I predict that once explained, the underlying effect will not appear strange at all. It will seem, in retrospect, that it was quite clear that we should have understood it all along. It is a heat producing reaction, consistent with nuclear but not chemical effects, both temporally and quantitatively accompanied by ^4He. This new effect, the Fleischmann Pons Effect, can be accompanied by nuclear "ash", ^3H and ^3He being important. Strong evidence for other isotopes exists [5]; more may follow.

How do we make progress? We make progress through theory: quantitative predictive fundamental physics descriptions. We will continue to make progress best by using the scientific method. To do so we are going to have to engage the broader scientific community. We simply can't sit here secure behind our walls and talk in a closed group, we need to invoke enthusiasm in the broader scientific community. The organizers of this conference [ICCF15] are to be commended for recognizing this need and furthering that process.

Another way of making progress is by engaging in the process of creating a product. Here we might take advantage of the growing public and political interest in real alternative energy solutions. The FPE produces real and useful energy, process heat. In Energetics experiment L64, in a single burst, twenty five times more heat was produced than entered the cell as electric power. This heat was produced at temperatures sufficient to boil water. Such an effect has practical value. Obviously taking an experiment to the market as a product requires several steps that are non-trivial. This exercise however may be an effective means of gaining an engineering understanding of the effect even before the scientific.

Acknowledgements

I would like to acknowledge four individuals outside the SRI team who have contributed particularly to the results and thinking summarized in this Foreword. Peter Hagelstein and Jed Rothwell who will recognize vital concept development and wording; Vittorio Violante and Irving Dardik whose remarkable innovations in materials processing and fundamental understanding of the process dynamics of the FPE (and other effects) have sustained the progress that supports this field.

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