

U.S. Department of Energy
Office of Energy Research

PROPOSAL COVER SHEET

8365

DATE: 08/31/88

To: ADVANCED ENERGY PROJECTS
(ATTN: RYSZARD GAJEWSKI)

THE ATTACHED PROPOSAL IS FOR YOUR REVIEW & APPROPRIATE ACTION.

PROPOSER: UTAH, UNIVERSITY OF
SALT LAKE CITY, UT

TYPE OF
PROPOSAL: NEW

P.I.(s): PONS, STANLEY

DATE RECEIVED: 08/29/88

NO. OF COPIES: 07

TITLE: THE BEHAVIOR OF
ELECTROCHEMICALLY
COMPRESSED HYDROGEN AND
DEUTERIUM

AWARD NO.: N/A

UNSOLICITED
PROPOSAL NO.: N/A

THIS PROPOSAL HAS BEEN GIVEN A PRELIMINARY ADMINISTRATIVE REVIEW
BY THE ACQUISITION & ASSISTANCE MANAGEMENT DIVISION, ER-64.

- THIS PROPOSAL WAS RECEIVED
DIRECTLY FROM THE PROPOSER
- THIS PROPOSAL WAS ALSO SUBMITTED TO:
- OTHER COMMENTS:

DISCLAIMER

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26 August 1988

DHL COURIER

U S DEPARTMENT OF ENERGY
ACQUISITION & ASSISTANCE
MANAGEMENT DIVISION
OFFICE OF ENERGY RESEARCH ER64
19901 GERMANTOWN ROAD
GERMANTOWN MD 20874

SUBJECT: New Proposal
University of Utah PID No. 8808032

Dear Sir/Madam:

We have enclosed an original and six (6) copies of the proposal for the project entitled "THE BEHAVIOR OF ELECTROCHEMICALLY COMPRESSED HYDROGEN AND DEUTERIUM" under the direction of Dr. B. Stanley Pons, Department of Chemistry. These documents have been signed by an authorized official of the University of Utah.

This proposal is in the amount of \$632,281 for the performance period 1 October 1988 to 30 September 1991. Of this amount, \$208,375 is for the first year.

We appreciate your consideration of this proposal and look forward to hearing from you when your review is completed.

Very truly yours,

Richard H. Timpson
Director
Research Administration

kb

Enclosures: 7

cy: Dr. B. Stanley Pons - Chemistry
Dr. Hugo Rossi, Dean - College of Science

Office of Research Administration

309 Park Building
Salt Lake City, Utah 84112
(801) 581-6903

FEDERAL ASSISTANCE		2. APPLICANT'S APPLICATION IDENTIFIER		a. NUMBER		3. STATE APPLICATION IDENTIFIER		a. NUMBER	
1. TYPE OF SUBMISSION (Mark appropriate box) <input type="checkbox"/> NOTICE OF INTENT (OPTIONAL) <input type="checkbox"/> PREAPPLICATION <input checked="" type="checkbox"/> APPLICATION		b. DATE Year month day 19 8 23 88		NOTE: TO BE ASSIGNED BY STATE		b. DATE ASSIGNED Year month day 19		Leave Blank	
4. LEGAL APPLICANT/RECIPIENT a. Applicant Name University of Utah b. Organization Unit Stanley Pons c. Street/P.O. Box Department of Chemistry d. City Salt Lake City e. County Salt Lake f. State Utah g. ZIP Code 84112 h. Contact Person (Name & Telephone No.) Stanley Pons (801) 581-4760						5. EMPLOYER IDENTIFICATION NUMBER (EIN) 187600525A1			
7. TITLE OF APPLICANT'S PROJECT (Use section IV of this form to provide a summary description of the project.) The Behavior of Electrochemically Compressed Hydrogen and Deuterium						6. PROGRAM (From CFDA)		a. NUMBER 18110419	
								b. TITLE Basic Energy Sciences	
9. AREA OF PROJECT IMPACT (Names of cities, counties, states, etc.)						10. ESTIMATED NUMBER OF PERSONS BENEFITING		11. TYPE OF ASSISTANCE A—Basic Grant B—Supplemental Grant C—Loan D—Insurance E—Other Enter appropriate letter(s) <input type="checkbox"/>	
12. PROPOSED FUNDING		13. CONGRESSIONAL DISTRICTS OF:		14. TYPE OF APPLICATION A—New B—Renewal C—Revision D—Continuation E—Augmentation Enter appropriate letter <input type="checkbox"/>		17. TYPE OF CHANGE (For 14c or 14e) A—Increase Dollars B—Decrease Dollars C—Increase Duration D—Decrease Duration E—Cancellation Enter appropriate letter(s) <input type="checkbox"/>		20. EXISTING FEDERAL GRANT IDENTIFICATION NUMBER	
a. FEDERAL \$208375.00 b. APPLICANT 40000.00 c. STATE .00 d. LOCAL .00 e. OTHER .00 f. Total \$248375.00		a. APPLICANT UTAH 2nd b. PROJECT UTAH 2nd 15. PROJECT START DATE Year month day 19 88 10 1 16. PROJECT DURATION 36 Months 18. DATE DUE TO FEDERAL AGENCY Year month day 19		17. TYPE OF CHANGE (For 14c or 14e) A—Increase Dollars B—Decrease Dollars C—Increase Duration D—Decrease Duration E—Cancellation Enter appropriate letter(s) <input type="checkbox"/>					
19. FEDERAL AGENCY TO RECEIVE REQUEST DOE Office of Energy Research						a. ORGANIZATIONAL UNIT (IF APPROPRIATE) Office of Energy Research b. ADMINISTRATIVE CONTACT (IF KNOWN) Ryszard Gajewski		21. REMARKS ADDED <input type="checkbox"/> Yes <input checked="" type="checkbox"/> No	
c. ADDRESS Acquisition and Assistance Management Division Office Energy Research ER64 DOE Washington dc 20545									
22. THE APPLICANT CERTIFIES THAT		To the best of my knowledge and belief, data in this preapplication/application are true and correct, the document has been duly authorized by the governing body of the applicant and the applicant will comply with the attached assurances if the assistance is approved.		a. YES, THIS NOTICE OF INTENT/PREAPPLICATION/APPLICATION WAS MADE AVAILABLE TO THE STATE EXECUTIVE ORDER 12372 PROCESS FOR REVIEW ON: DATE _____ b. NO, PROGRAM IS NOT COVERED BY E.O. 12372 <input type="checkbox"/> OR PROGRAM HAS NOT BEEN SELECTED BY STATE FOR REVIEW <input type="checkbox"/>					
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ER F 4620.1
(7-85)

U.S. Department of Energy
Grant Application Budget Period Summary
(See Reverse for Definitions and Instructions)

OMB Approval
No. 1910-1400

Please Print or Type

Organization: University of Utah		Period Covering: From: 10-1-88 To: 9-30-89		FOR DOE USE ONLY Proposal No.: Award No.:	
Principal Investigator (P.I.)/Project Director (P.D.): Stanley Pons		DOE Funded Person-Mos.		Funds Requested By Applicant	
A. SENIOR PERSONNEL PI/PI Co PIs, Faculty and Other Senior Associates (List each separately with title, A.6 show number in brackets. Attach separate sheet, if required.)		Cal.	Acad.	Sumr.	\$
1. Martin Fleischmann, Co PI, Professor,		4			16000
2. No Employee Benefits /from England					
3.					
4.					
5.					
6. (1) TOTAL SENIOR PERSONNEL					
B. OTHER PERSONNEL (SHOW NUMBERS IN BRACKETS)					
1. (1) POST DOCTORAL ASSOCIATES		12			20000
2. () OTHER PROFESSIONALS (TECHNICIAN, PROGRAMMER, ETC.)					
3. (2) GRADUATE STUDENTS		12			20000
4. () UNDERGRADUATE STUDENTS					
5. () SECRETARIAL-CLERICAL					
6. () OTHER					
TOTAL SALARIES AND WAGES (A + B)					56,000
C. FRINGE BENEFITS (IF CHARGED AS DIRECT COSTS) Post Doc 14% Grad Stud. 8% only					4350
TOTAL SALARIES, WAGES AND FRINGE BENEFITS (A + B + C)					60,350
D. EQUIPMENT (LIST ITEM AND DOLLAR AMOUNT FOR EACH ITEM) Calorimeter for temp msmt					23500
3 Potentiostat-galvanostats-power for cells					17940
1 waveform generator for potential programs					5790
Temp transducers, PC for control and recording					11000
TOTAL EQUIPMENT					58230
E. TRAVEL 1. DOMESTIC (INCL. CANADA AND U.S. POSSESSIONS)					
2. FOREIGN Fleischmann to Utah to participate in work					2500
F. OTHER DIRECT COSTS					
1. MATERIALS AND SUPPLIES Rods, heavy water, other metals,					48000
2. PUBLICATION COSTS/PAGE CHARGES					500
3. CONSULTANT SERVICES					18000
4. COMPUTER (ADPE) SERVICES					
5. CONTRACTS AND SUBGRANTS					
6. OTHER					
TOTAL OTHER DIRECT COSTS					66500
G. TOTAL DIRECT COSTS (A THROUGH F)					187580
H. INDIRECT COSTS (SPECIFY RATE AND BASE) 47% Direct Costs, except equipment					60795
TOTAL INDIRECT COSTS					248375
I. TOTAL DIRECT AND INDIRECT COSTS (G & H)					40000
J. APPLICANT'S COST SHARING (IF ANY)					208375
K. TOTAL AMOUNT OF THIS REQUEST (ITEM I LESS ITEM J)					
PI/PI D TYPED NAME & SIGNATURE Stanley Pons		DATE 8-23-88			
INST. REP. TYPED NAME & SIGNATURE JAMES J. BROPHY Vice President for Research		DATE AUG 26 1988			

For further information, other than
technical contact Richard H. Timpson
Research Administration 801-581-6903

U.S. Department of Energy
GRANT APPLICATION
PROJECT PERIOD SUMMARY

(Must be completed for all new and renewal applications.)

Please Print or Type

Categories	01 Budget Period	02 Budget Period	03 Budget Period	04 Budget Period	05 Budget Period
A. Senior Personnel Totals	16000	18000	20000		
B. Other Personnel Totals	58000	60000	62000		
C. Fringe Benefit Totals	4350	4500	4650		
Total of A, B & C	78350	82500	86650		
D. Equipment	58230	10000	10000		
E. Travel 1. Domestic					
2. Foreign	2500	3000	3500		
F. Other Direct Costs	48500	52000	58000		
G. Total Direct Costs	187580	147500	158150		
H. Total Indirect Costs	60795	64625	69631		
I. Total Direct & Indirect Costs	248375	212125	227781		
J. Applicant's Cost-Sharing (If any)	40000	8000	8000		
K. Total Amount of Request (Item I. Less Item J.)	(1)* 208375 ✓	(2) 204125 ✓	(3) 219781 ✓	(4)	(5)

*This should equal Item K on Budget Period Summary (ER/F/4620.1)

For further information, other than
technical contact Richard H. Timpson
Research Administration 801-581-6903

ESTIMATE

TOTAL COST OF PROJECT

\$ 632,281 ✓
(add K(1) thru (5))

THE BEHAVIOR OF ELECTROCHEMICALLY COMPRESSED HYDROGEN AND DEUTERIUM

Stanley Pons
Professor and Chairman
Department of Chemistry
University of Utah
Salt Lake City, UT 84112 USA

Martin Fleischmann, FRS
Professor
Department of Chemistry
The University
Southampton, Hants. SO9 5NH
ENGLAND

Abstract

The raising of the chemical potential of low atomic number nuclei (such as deuterium) to values $> 0.1\text{eV}$ results in significant probability of fusion of the nuclei. Most attempts to produce methods for controlled terrestrial fusion reactions are based on the raising of the chemical potential of the nuclei by thermal means. This is certainly not a necessary requirement, however, as has been demonstrated by experiments involving room temperature muon-catalyzed fusion reactions, a possibility that has been known for over 40 years. We have preliminary evidence that the required chemical potentials for controlled fusion reactions of small nuclei may be attained by simple electrochemical means. In a metallic host lattice, for instance, there is apparently sufficient shielding of the electrostatic repulsion of deuterium plasma by the electrons in the metal to allow very short approach distances of deuterons. Under these conditions, the cross section for fusion is significant. The chemical potential of deuterium in a palladium lattice, for instance, has been raised to values $> 0.8\text{eV}$. To attain this energy by compression, one would have to generate static pressures of 10^{27} atmospheres. Under these conditions, we have observed continuous excess thermal energy generation of $>25\%$ above that expected to arise from the most likely electrochemical reaction occurring in the device, and concomitant increase in background radiation levels. We intend to further demonstrate and extend the scope of these experiments, and we will investigate possible applications to a new type of simply controlled fusion reactor.

THE BEHAVIOR OF ELECTROCHEMICALLY COMPRESSED HYDROGEN AND DEUTERIUM

Introduction and Description of the Experiments

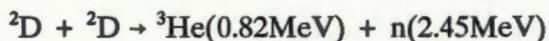
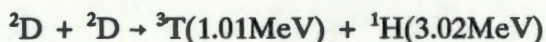
We describe here preliminary results of an experiment designed to investigate the result of compression of deuterium at very high pressures using a simple device.

The starting point of our investigation was a reconsideration of the possibility of forming metallic hydrogen or deuterium, which should occur at about 10^6 atmospheres and which should have applications as a room temperature superconductor, a powerful fuel, in nuclear devices, and as an explosive. Here we will confine attention to the deuterium system alone.

Whereas it would be difficult to achieve the conditions for producing metallic deuterium, alloys of deuterium are readily formed; a well known example is the Pd-D system. The correct description of such systems is still uncertain, but one knows that: a) the deuterium is mobile in the metal lattice (it has a high diffusion coefficient) and exists as D^+ (migration of the species in an electric field, etc.); b) the electrons go into the Pd band structure; c) the D^+ are essentially unbound; d) it is possible to raise the chemical potential of D in the lattice by perhaps 0.8eV by applying cathodic potentials to a Pd electrode. While this may appear to be a modest figure, it is in fact of astronomical magnitude: if one attempted to achieve the same result by compressing D_2 , one would require 10^{27} atmospheres! It would be straightforward to develop even higher chemical potentials (see (3) of section Discussion and Proposed Plans).

It is because of the extremely high chemical potentials of D which can be developed within the host Pd lattice by suitable electrochemical polarization, that we have considered the possibility of fusion of D nuclei. It is in fact difficult to predict the collisional behavior of $D^+ + D^+$ under these particular conditions. Clearly the repulsive potential of the nuclei will be screened to some extent by the electrons in the metal, but D_2 is not formed, i.e. the s-character is weak. One can

devise plausible arguments in favor or against any fusion reaction such as



In our view, calculations (such as nuclear force : quantum : molecular dynamic simulations) would be difficult and ambiguous (indeed perhaps impossible at this stage). In these circumstances it is best to resort to experiment. We note that in the well known magic expression (Particle Density x Temperature x Volume x Lifetime) usually associated with conditions for nuclear fusion, our particle densities are astronomically high, our temperatures are low, our volumes low, and our lifetimes infinitely long compared to the conditions in tauri, tocomacs, H-bombs, or in laser fusion. We compare, for instance, the figures for our cell and an "ideal" self-sustaining thermonuclear fusion reactor: such a device might contain a deuterium (reaction cross section about 10^{-24}cm^2) particle density of 10^{14} cm^{-3} at a temperature of 10^8K , a volume of 10^6cm^3 , and a confinement time of 1s. The pressure of this plasma is therefore about 30 atmospheres. The device described herein has the same temperature as the muon-catalyzed fusion reactors, about 0.025eV, but the particle density is on the order of 10^{23} with a volume of about 10^1cm^3 and a confinement time of $>10^6\text{s}$. The resulting calculated pressure is on the order of the measured rise in chemical potential, approximately 10^{27} atmospheres.

The basic experiment was based on the cathodic reduction of D_2O from the liquid phase at near room temperature using low current densities ($< 1.6\text{mA cm}^{-2}$). The D formed was initially absorbed into the palladium metal lattice eventually forming $\beta\text{-PdH}$. When saturation conditions had been reached at any particular current density, D_2 was evolved at the Pd cathode at a steady rate. The electrochemical cell consisted of an evacuated Dewar mounted in a constant temperature water bath and the heat release under steady state conditions was determined from the thermal

balances in the Dewar. The temperature inside and outside the Dewar were monitored with Beckmann thermometers. The secondary electrode inside the Dewar was a cylinder of platinized platinum foil about 10cm high and 5cm in diameter. The working electrode was an 8 x 8 x 0.2cm sheet of palladium metal that had been folded to fit inside the platinum cylinder. The solution was 0.1M LiOD in 99.99% D₂O. A gas sparger was placed inside the Dewar. For some experiments, D₂ was replaced in the Dewar by sparging in from an external generator.

The experiment was controlled by a galvanostat (constant current generator). The temperature of the water bath, temperature inside the Dewar, the potential between the two electrodes, the cell current, and rate of D₂ and O₂ evolution in the auxiliary generator were monitored as a function of time. The system was calibrated for the Heavy Water Equivalent, HWE, (g D₂O) and Newton's law of cooling constant, k (joules s⁻¹) by adding m grams of D₂O at elevated temperature T_2 to the LiOD/D₂O solution already in the Dewar at temperature T_1 and measuring the temperature transient; the Dewar here contained all of the components that it did during the constant current experiments. We denote the HWE of the Dewar and contents by M (g D₂O).

Treatment of the Data

We denote the initial temperature of the $(m + M)$ g D₂O in the Dewar by T_3 . A thermal balance then gives

$$(m + M)(T_3 - T_1) = m(T_2 - T_1) \quad [1]$$

so that

$$M = m(T_2 - T_3) / (T_3 - T_1) \quad [2]$$

We take the heat capacitance of D_2O to be $C \text{ J g}^{-1}(\text{°})^{-1}$. Then the rate of cooling of the Dewar is given by

$$(m + M)C \, d(T - T_1)/dt = -k(T - T_1) \quad [3]$$

Integration of [3] subject to the initial condition

$$T - T_1 = T_3 - T_1 \text{ at } t = 0 \quad [4]$$

gives

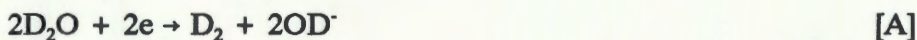
$$(T - T_1) = (T_3 - T_1) \exp (-kt/(m + M)C) \quad [5]$$

We thus plot $\ln(T - T_1)$ against t and determine k from the slope since we know the values $(m + M)$ and C .

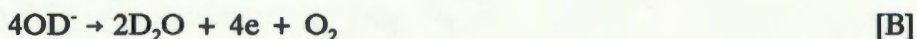
From our data, we derive the HWE of the system to be 292.7 g D_2O , and the parameter $Mk/(m + M) = MC / k/(m + M)C = 0.3113 \text{ J g}^{-1}(\text{°})^{-1}$. [6]

Thermal Balances in the Dewar

One objective during some experiments was to balance the evolution of D_2 on the Pd electrode by D_2 ionization at the platinum electrode. In that case the joule heat due to the electrolysis current would simply be the cell current x cell voltage. Note that if the electrode reactions were the evolution and reduction of oxygen from and to water, then the same relationship would hold for the joule heating. Equally, a mixture of D_2 evolution/ionization and O_2 evolution/reduction would give such a joule heating term. However, any other set of electrode reactions would reduce joule heating. In particular, if the reactions were



and



(or any other comparable pair of reactions representing the electrolysis of D_2O) then we must subtract from the cell voltage a term giving the cell voltage at which the reaction is "thermoneutral" (neither exo- or endothermic). This voltage is given by

$$-2EF = \Delta H \quad [7]$$

With the value $\Delta H = -294.6 \text{ kJ mol}^{-1}$ we obtain $E_{\text{thermoneutral}} = 1.54\text{V}$. For the case of reactions [A] and [B] the joule heating is given by (cell current \times (cell voltage - $E_{\text{thermoneutral}}$)). The two cases give an upper and lower limit to the rate of joule heating, respectively, that can occur in the cell.

We estimate the maximum possible iR drop in the cell from the change in the observed cell voltage when we step the cell current between two values. We have determined that after correction for iR loss, the reactions [A] and [B] are the reactions present; i.e. the joule heating is always given by the lower bound calculated above.

Preliminary Results

First we find that the Newton's cooling loss is always less than the lower bound of the electrolytic joule heating when the system is driven at low current densities (0.4mA cm^{-2}) during the early parts of the experiment (approximately the first 300 hours; the Pd cathode was subjected to

a prolonged period of presaturation with D before the start of measurements). We believe that the cooling of the system is due to the continuation of endothermic dissolution of D into the Pd lattice. Toward the end of this period and at somewhat higher current densities (0.8 mA cm^{-2}), the Newton's cooling loss almost balances the lower bound of the electrolytic heating. On raising the current density further to 1.2 mA cm^{-2} at about 600 hours there is a $>9\%$ excess heating effect while from 600 to 1000 hours at the final current density of 1.6 mA cm^{-2} , we observe a transition to $>25\%$ excess heating. This is accompanied by an increase in the background radiation count in the lab of $>50\%$. The long term experiments were all terminated at about this time.

Discussion and Proposed Plans

We believe that the results we have obtained so far are a strong indication of a progressive increase in the fusion of D nuclei in the Pd-lattice with increasing chemical potential (= compression). While there are alternative explanations of the excess heating effects, their possibility does not seem to be very likely. We must, of course, investigate all of these effects in detail and carry out the experiments under more carefully controlled conditions and for a much wider range of the experimental parameters. Although it is possible to suggest drastic changes to our experiments, (see (3) - (6) below), we want to stay with the Pd-D system at this stage and our proposal would cover the initial aspects of (1) - (5) below.

(1) In order to achieve better control and to extend the experimental conditions, we want to purchase Pd rods about 10 cm long and in various diameters up to about 2 cm. These will have to be specially cast (the electrode we have been using to date is made of the heaviest gauge material available commercially). We have designed and need to construct new smaller Dewars to enhance $(T - T_1)$. In the first place, we will reduce the size of the Pt electrode so as to decrease any effects of the D_2 ionization / O_2 reduction reactions. The smaller surface area of the electrodes

will decrease the electrolytic joule heating (as compared to any fusion) and will allow us to reach more negative potentials, i.e. higher D compressions. Comparisons of results for different diameters will show whether the excess thermal energy is proportional to the volume of the rods. It should be possible to devise conditions where the excess thermal energy is much larger compared to the lower bound electrolytic joule heating term than in our experiments to date; we should be able to make corrections for the effects of D_2 ionization / O_2 reduction and it may prove possible to make the excess thermal energy large compared to the upper bound electrolytic joule heating term.

Nevertheless, we should devise Dewar cells in which we can restrict the anode reaction to D_2 ionization. We would use fuel cell anodes fed with electrolytic D_2 . All Dewars will have to contain reference electrodes (α - β -Pd-D) so that we can monitor the potentials of the electrodes. They should also contain resistance heaters so that we can determine the thermal balances more accurately.

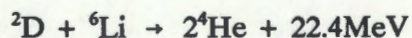
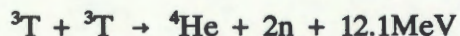
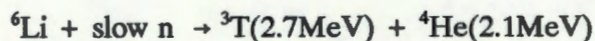
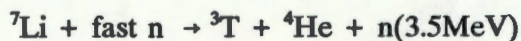
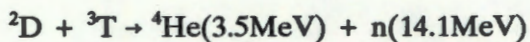
The experiments will take longer than our previous experiments in view of the greater thickness of the rods compared to the sheet electrodes. For instance, it will take approximately 12 months to charge a 2cm diameter rod to saturation with deuterium.

(2) Experiments will be carried out at higher temperatures T_1 in the external bath. This should lead to an increase of the excess thermal energy.

(3) We will attempt to increase the chemical potential of the dissolved D further by selective poisoning of the electrode surface. This will reduce the D_2 evolution at the surface and therefore make the experiment more clear cut. We cannot use the cells described in (1) above or with the same electrodes as the whole system will probably be ruined by the poisoning treatments.

(4) We will search for other metals and other materials as substrates. β -NiD is an obvious target. Experiments on surface poisoning may be better deferred until we start work with cheaper materials.

(5) We will investigate the properties of other alloy hosts. Pd-⁷Li is a clear choice. Such work will be combined with a search for more favorable solution media such as D₂O / T₂O mixtures. In particular, one questions whether it is possible to induce



although screening of the electrostatic repulsion by the lithium nucleus may be poor due to the presence of its 1s electronic structure. On the other hand, note that the capture cross sections of the dissolved D, T, and Li species may be quite different to those which apply in thermonuclear reactors (compare to mu-meson catalyzed reactions).

(6) Work under (5) raises some further questions. A Pd dispersion in, for instance, a LiD/T matrix rapidly heated would be effectively subjected to very negative potentials and the Li/D/T would be driven into the lattice in approximately 1 - 100 μ s.

Statement of Practical Application

One of the most intriguing applications is, that since the generation of excess thermal energy appears to be continuous, the system could be simply redesigned to accommodate heat exchangers

for external energy generation. Additionally, high thermonuclear energy levels may be obtained if devices constructed from dispersions of metal lattices such as those described in (6) above are utilized.

Statement of Work1) Project Objective

The principal investigators intend to confirm preliminary observations that it may be possible to attain conditions for nuclear fusion of light nuclei in host metal lattices at low temperatures ($< 1\text{eV}$) by electrochemical means. If successful, we intend to extend the scope of the experiments to determine the optimum conditions for construction of simply controlled fusion reactors.

2) The work to be performed consists of the following tasks:

- 2.1 Casting of palladium host lattice rod electrodes in a variety of diameters. Construction of the appropriate secondary electrodes, dewar cells, and associated calorimetric equipment. Construction of measuring equipment, and purchasing of constant temperature baths, appropriate temperature and radiation measuring devices.
- 2.2 Calorimetric, weight gain, and radiation measurements of the palladium generators as a function of rod volume. Chemical analysis of Dewar contents.
- 2.3 Calorimetric, weight gain, and radiation measurements of the palladium generators as a function of bath temperature.

- 2.4 Extension of the experiments to include other metal lattices known to sorb deuterium. Studies of the increase in reactant particle density (increase of chemical potential) in the metal lattice by poisoning of competitive surface electrochemical processes.
- 2.5 Extension of the experiments to include potentially more powerful, interesting, or useful reactants such various alloys of deuterium, tritium, lithium, and boron.
- 2.6 Extension of the experiments to include dispersions of electrodes in various reactants. These experiments would require special facilities, and should be discussed at a later point.
- 2.7 Preliminary engineering and economic analyses according to the results of paragraphs 2.1-2.6.

3) Deliveries

The principal investigators shall provide the data of experiments performed according to paragraphs 2.1-2.6 along with analyses and conclusions based on this data.

4) Performance Schedule

- 4.1 Complete construction of the equipment and electrodes within 120 days after start of work.

- 4.2 Complete thermodynamic measurements as a function of electrode volume and temperature within 24 months after start of work (large electrodes will take about one year to charge).
- 4.3 Complete measurements as a function of electrode material and nature of reactants within 34 months after start of work.
- 4.4 Decision to determine the feasibility of proceeding to experiments utilizing dispersions of electrodes to be made in third year.
- 4.5 Complete data correlation, economic analysis, and final report 36 months after start of work.

Facilities

The experiments will be conducted at the University of Utah. The Department of Chemistry at the University of Utah has excellent equipment and plant facilities. We have recently occupied a new building which has increased our space by 50%. The research described herein will be performed in this new facility. The department owns all of the normal instrumentation found in first rate Departments, including mass spectrometers, x-ray diffractometers, FTIR equipment, Raman spectrometers, ESR and ENDOR facilities, ORD and polarimeter equipment, several superconducting NMR spectrometers, and numerous smaller items for chemical analysis. The equipment services are staffed by a team of 8 Ph.D. chemists. We have excellent shop facilities including machine, electronics, and glassblowing which are staffed by 10 technicians.

In addition to the Departmental facilities, our research group has a vacuum FTIR spectrometer (purchased under the DOD University instrumentation program), reflectance MSRS UV-VIS reflectance spectrometer, EMIRS reflectance spectrometer, several high performance PC computers dedicated to various experiments, and several electrochemical systems set up on various experiments. These are composed typically of potentiostats, waveform generators, transient recorders, integrators, cells, oscilloscopes, and related circuitry and interfaces. We also own the usual items found in most well-equipped analytical laboratories. The major equipment will be available for use in the proposed research. Most of this equipment was purchased under DOD grants and contracts.

Other Support

We certify that we originally sent portions of this proposal to the Office of Naval research for inspection and advice. After correspondence with ONR, it was felt that the most appropriate source of support for this type of project was the Advanced Energy Projects Division of the Department of Energy. We have therefore resubmitted this proposal to that Division. We do not anticipate that we will request other research support if funded by DOE.

August 23, 1988

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Publications

Pre 1980

(7,7)

1. Harry B. Mark, Jr., and B. Stanley Pons, "An in situ Spectrophotometric Method for Observing the Infrared Spectra of Species at the Electrode Surface during Electrolysis", *Analytical Chemistry*, **38**, 119 (1966).
2. B. Stanley Pons, James S. Mattson, Leon O. Winstrom, and Harry B. Mark, Jr., "Application of Deposited Thin Metal Films as Optically Transparent Electrodes for Internal Reflection Spectrometric Observation of Electrode Solution Interfaces", *Analytical Chemistry* **39**, 685 (1967)
3. Alan Bewick, Gary J. Edwards, John M. Mellor, and B. Stanley Pons, "Anodic Oxidation of Methylbenzenes at Potentials in the First Voltammetric Wave", *Journal of the Chemical Society, Perkin II Trans.* 1952 (1977).
4. Alan Bewick, John M. Mellor, and B. Stanley Pons, "Distinction Between Heterogeneous and Homogeneous Electron Transfer Steps by Spectroelectrochemical Methods", *Electrochimica Acta*, **23**, 77 (1978).
5. Alan Bewick, John M. Mellor, and B. Stanley Pons, "Use of Ion-Exchange Resins to Trap Electrochemical Intermediates", *Journal of the Chemical Society, Chemical Communications* 738 (1978).
6. John M. Mellor, B. Stanley Pons, and John H. A. Stibbard, "Synthesis of Substituted Barbaralanes by Electrochemical Reduction of Bridged 1,5 Benzodiazepins", *Journal of the Chemical Society, Chemical Communications*, 761, (1979).
7. John M. Mellor, B. Stanley Pons, and John H. A. Stibbard, "Electrochemical Synthesis of Substituted Barbaralanes by Reduction of Bicyclo(3.3.1)-nonadienediones in the Second Wave", *Journal of the Chemical Society, Chemical Communications*, 759 (1979).

Publications

1980

(4,11)

8. Alan Bewick, Keiji Kunimatsu, and B. Stanley Pons, "Infrared Spectroscopy of the Electrode-Electrolyte Interface" *Electrochimica Acta*, 25, 465 (1980).
9. Alan Bewick, John M. Mellor, and B. Stanley Pons, "Distinction Between ECE and Disproportionation Mechanisms in the Oxidation of Methylbenzenes using Spectroelectrochemical Methods", *Electrochimica Acta*, 25, 931 (1980).
10. Parbury P. Schmidt and B. Stanley Pons, "An Analysis of the Vibrational Spectra of Symmetrically solvated Alkali Metal Cations", *Journal of the Chemical Society, Faraday II Transactions*, 76, 979 (1980).
11. B. Stanley Pons and Parbury P. Schmidt, "Global Spline Collocation Techniques in the Simulation of Electrode Processes", *Electrochimica Acta*, 25, 987 (1980).

Publications

1981

(7,18)

12. L. G. Chatten, Marilyn Boyce, R. E. Moskalyk, B. Stanley Pons, and D.K. Madan, "Analysis of Allopurinol Tablets by Differential Pulse Polarography", *The Analyst*, 106, 365 (1981).
13. B. Stanley Pons, David J. Santure, R. Craig Taylor, and Ralph W. Rudolph, "Electrochemical Generation of Naked Metal Anionic Clusters $\text{Sn}_x\text{Pb}_{9-x}$ ($x = 0$ to 9)", *Electrochimica Acta*, 25, 365 (1981).
14. Stanley Pons, "Approximation of Chronopotentiometric Responses by Orthogonal Collocation", *Electrochimica Acta*, 26, 601 (1981).
15. Stanley Pons, "Simulation of Electrode Responses by Orthogonal Collocation. Spherical Electrode Geometry", *Canadian Journal Of Chemistry*, 59, 1538 (1981).
16. Timothy Davidson, Stanley Pons, Alan Bewick, and Parbury P. Schmidt, "Vibrational Spectroscopy of the Electrode Solution Interface. Use of Fourier Transform Infrared Spectroscopy", *Journal of Electroanalytical Chemistry*, 125, 237 (1981).
17. John M. Mellor, Stanley Pons, and John H. A. Stibbard, "Electrochemical Synthesis of Substituted Barbaralanes by the Electrochemical Reduction of Bicyclo(3.3.1)nonadienediones in the Second Wave", *Journal of the Chemical Society, Perkin I Transactions*, 3092 (1981).
18. John M. Mellor, Stanley Pons, and John H. A. Stibbard, "Synthesis of Substituted Barbaralanes by Electrochemical Reduction of Bridged 1,5-Benzodiazepins", *Journal of the Chemical Society, Perkin I Transactions*, 3097 (1981).

Publications

1982

(11,29)

19. Parbury P. Schmidt and Stanley Pons, "Electrochemical Generation of Solvated Metal Atoms", *Electrochimica Acta*, 27, 867 (1982).
20. Stanley Pons and S. B. Khoo, "Reductions in Aprotic Media 1. Cathodic Reduction Limits in Acetonitrile", *Electrochimica Acta*, 27, 1161 (1982).
21. Bernd Speiser and Stanley Pons, "Simulation of Edge Effects. 1. Two Dimensional Collocation and Theory for Chronoamperometry", *Canadian Journal of Chemistry*, 60, 1352 (1982).
22. Stanley Pons and S. B. Khoo, "Reductions in Aprotic Media. 2. Influence of Cathodic Hydrogen Reduction on the Course of Hydrocarbon Reduction", *Journal of the American Chemical Society*, 104, 3845 (1982).
23. L. G. Chatten, Stanley Pons, and Patricia McLeod, "The Electrochemical Analysis of Trimethoprim", *The Analyst*, 107, 1026 (1982).
24. Bernd Speiser and Stanley Pons, "Simulation of Edge Effects. 2. Theory for Cyclic Voltammetry", *Canadian Journal Of Chemistry*, 60, 2463 (1982).
25. Bernd Speiser, Stanley Pons, and A. Rieker, "Use of Orthogonal Collocation for the Simulation of Quasi-reversible Electrode Processes", *Electrochimica Acta*, 27, 1171 (1982).
26. Stanley Pons, Jerome McAleer, Bernd Speiser, and Parbury P. Schmidt, "Simulation of the Dropping Mercury Electrode by Orthogonal Collocation", *Electrochimica Acta*, 27, 1171 (1982).
27. Stanley Pons, Bernd Speiser, and Jerome McAleer, "Orthogonal Collocation Simulation of the Rotating Disk Electrode", *Electrochimica Acta*, 27, 1177 (1982).
28. Parbury P. Schmidt and Stanley Pons, "Electrochemical Generation of Solvated Metal Atoms. II. Analysis of the Stabilities of Solvated Cations and Atoms", *Electrochimica Acta*, 27, 875 (1982).
29. Timothy Davidson, Stanley Pons, and Alan Bewick, "Vibrational Spectroscopy of the Electrode-Solution Interface. 3. The Use of FTIR for Observing Double Layer Reorganization", *Journal of Electroanalytical Chemistry*, 140, 211 (1982).

Publications

1983

(8,37)

30. Bernd Speiser and Stanley Pons, "Simulation of Edge Effects. 3. Application to Chronoamperometric Experiments", *Canadian Journal Of Chemistry*, 61, 156 (1983).
31. Stanley Pons, Alan Bewick, and Timothy Davidson, "Vibrational Spectroscopy of the Electrode Solution Interface. 2. Use of FTIR for Recording Spectra of Radical Ion Intermediates", *Journal of the American Chemical Society*, 105, 1802 (1983).
32. Bernd Speiser, Stanley Pons, and A. Rieker, "Electrochemistry of Anilines. Part I. Oxidation of 2,6-Di-tert-butylanilines to Radical Cations; General Considerations, Electroanalytical Experiments, and Spectroscopy", *Journal of Electroanalytical Chemistry*, 147, 205 (1983).
33. Stanley Pons, "The Use of FTIR Spectroscopy for in situ Recording of Species in the Electrode-Electrolyte Solution Interface", *Journal of Electroanalytical Chemistry*, 150, 495 (1983).
34. A. Scott Hinman, Stanley Pons, and Jerome F. McAleer, "Spectroscopic Determination of the A.C. Voltammetric Response", *Journal of Electroanalytical Chemistry*, 154, 45 (1983).
35. L. Amankwa, L. G. Chatten, and Stanley Pons, "Electrochemical Studies on Minoxidil and its Determination in Tablets by Differential Pulse Polarography", *The Analyst*, 108, 1221 (1983).
36. Bernd Speiser, A. Rieker, and Stanley Pons, "Electrochemistry of Anilines. Part II. Oxidation to Dications, Electrochemical and UV-VIS Spectroelectrochemical Investigation", *Journal of Electroanalytical Chemistry*, 159, 63 (1983).
129. L. G. Chatten, S. Pons, and L. Amankwa, "Voltammetric Studies of Zomepirac Sodium and its Determination in Tablets by Differential-pulse Polarography", *The Analyst*, 108, 997 (1983).

Publications

1984

(9,46)

37. Stanley Pons, Timothy Davidson, and Alan Bewick, "Vibrational Spectroscopy of the Electrode Solution Interface. 4. FTIR Experimental Considerations", *Journal of Electroanalytical Chemistry*, 160, 63 (1984).
38. Stanley Pons, Monika Datta, Jerome F. McAleer, and A. Scott Hinman, "Infrared Spectroelectrochemistry of the $\text{Fe}(\text{CN})_4^{4-}/\text{Fe}(\text{CN})_3^{3-}$ Redox System", *Journal of Electroanalytical Chemistry*, 160, 369 (1984).
39. Alan Bewick, Keiji Kunitatsu, Stanley Pons, and Joel Russell, "Infrared Spectroelectrochemistry by EMIRS: Experimental Considerations", *Journal of Electroanalytical Chemistry*, 160, 47 (1984).
40. Stanley Pons, "Polynomial Approximation Techniques for Differential Equations in Electrochemical Problems", in A. Bard, ed., "Electroanalytical Chemistry", vol. 13. Marcel Dekker, New York, 1984.
41. Stanley Pons and A. Scott Hinman, "Anodic Oxidation of Furans", *Electrochimica Acta*, 29, 1251 (1984). ONR26
42. Adel Amer, Hans Zimmer, K. J. Mulligan, Harry B. Mark, Jr., Stanley Pons, and Jerome F. McAleer, "Polymerization of 3-Methyl-2,5-Dibromothiophene Utilizing n-Butyl Lithium and Copper II Chloride", *Journal of Polymer Science, Polymer Letters Edition*, 22, 77 (1984).
43. Stanley Pons, S. B. Khoo, Alan Bewick, Jerry J. Smith, A. Scott Hinman, G. Zachmann, and Monika Datta, "FTIR Spectroelectrochemistry of Adsorbed TCNE Anion Radicals", *Journal of Physical Chemistry*, 88, 3575 (1984). ONR28
44. John Cassidy, Stanley Pons, and B. Speiser, "Simulation of Edge Effects. Part 4. Application to Cyclic Voltammetry", *Canadian Journal Of Chemistry*, 62, 716 (1984).
45. R. Y. Yan, J. D. Miller, John K. Foley, and Stanley Pons, "Electrochemical Features of the Ferric Sulfate Leaching of CuFeS_2/C Aggregates", *Proceedings of the Electrochemical Society*, 1984, 391. ONR36

Publications

1985

(21,67)

46. A. Scott Hinman, John Cassidy, and Stanley Pons, "Voltammetry and Coulometry with Immersed Thin Layer Electrodes. Part 1. Model for Effects of Solution Resistivity in Linear Sweep Voltammetry", *Electrochimica Acta*, 30, 89 (1985). ONR30
47. Alan Bewick and Stanley Pons, "Infrared Spectroscopy of the Electrode- Electrolyte Solution Interface", in R. J. H. Clark and R. E. Hester, eds., "Advances in Infrared and Raman Spectroscopy", 12, Heyden and Sons, Ltd., London, 1985, p.1. ONR41
48. A. Scott Hinman, John Cassidy, and Stanley Pons, "Voltammetry and Coulometry with Immersed Thin Layer Electrodes. Part 2. Practical Considerations and Experimental Results", *Electrochimica Acta*, 30, 95 (1985). ONR31
49. Stanley Pons and Alan Bewick, "Subtractively Normalized Interfacial Fourier Transform Spectroscopy of Difluorobenzene Adsorbed at Polycrystalline Platinum Electrodes", *Langmuir*, 1, 141 (1985). ONR33
50. Carol Korzeniewski, Randall B. Shirts, and Stanley Pons, "Field Induced Infrared Absorption in Metal Surface Spectroscopy: The Electrochemical Stark Effect", *J. Phys. Chem.*, 89, 2297 (1985). ONR42
51. Stanley Pons, S. B. Khoo, Jiri Janata, Stephen W. Feldberg, and John K. Foley, "Homogeneous Reaction Involving Components of Different Redox Couples. Part I. Theoretical Consideration and Digital Simulation, *Electrochimica Acta*, 30, 569 (1985). ONR37
52. S. B. Khoo, Stanley Pons, Jiri Janata, Stephen W. Feldberg, and John K. Foley, "Homogeneous Reaction Involving Components of Different Redox Couples. Part II. Polarographic Studies of the Interactions of Alkali Metal Cations with Anthracene and Fluoranthene in N,N-Dimethylformamide and Acetonitrile, *Electrochimica Acta*, 30, 575 (1985).
53. Saibal Bandyopadhyay, F.E. Massoth, Stanley Pons, and Edward M. Eyring, "FTIR/PA Study of Quinoline Adsorbed on Calcined and Sulfided Mo/ η -Al₂O₃ ", *J. Phys. Chem.*, 89, 2560 (1985). ONR35
54. Stanley Pons and John K. Foley, "In-Situ Infrared Spectroelectrochemistry", *Analytical Chemistry*, 57, 945A (1985). ONR44
55. Kevin Ashley and Stanley Pons, "Modulated Surface Vibrational Spectroscopy at the Electrode/Solution Interface", *Trends in Analytical Chemistry*, 4, 142 (1985). ONR43
56. John R. Pradko, Stanley Pons, Saibal Bandyopadhyay, Jerome F. McAleer, and A. Scott Hinman, "A Spectroelectrochemical Investigation of the Oxidation of Bilirubin IX-a in Dimethylformamide", *Journal Bioelectrochemistry and Bioenergetics*, 13, 267 (1985). ONR32

Publications

1985 (Continued)

57. John Cassidy, S. B. Khoo, Stanley Pons, and Martin Fleischmann, "Electrochemistry at Very High Potentials: The Use of Ultramicroelectrodes in the Anodic Oxidation of Short Chain Alkanes", *J. Phys. Chem.*, 89, 3933 (1985). ONR29
58. Andrzej Czerwinski, Hans Zimmer, Adel Amer, Chien van Pham, Stanley Pons, and Harry B. Mark, Jr., "The Effect of 'Water and Transition Metal Ion Doping' on the Conductivity of Poly(3-Substituted Thiophene-2,5-diyls)", *Journal of the Chemical Society, Chemical Communications*, 1158 (1985). ONR45
59. Dennis S. Corrigan, John K. Foley, P. Gao, Stanley Pons, Michael J. Weaver, "Comparisons Between Surface-Enhanced Raman and Surface Infrared Spectroscopies for Strongly Perturbed Adsorbates: Thiocyanate at Gold Electrodes", *Langmuir*, 1, 616 (1985).
60. Stanley Pons and Alan Bewick, "Modulated Surface Vibrational Spectroscopies at Electrode-Solution Interfaces", *Naval Research Reviews*, 37, 33 (1985). ONR34
61. Martin Fleischmann, Jamal Ghoroghchian, and Stanley Pons, "Electrochemical Behavior of Dispersions of Spherical Ultramicroelectrodes. Part 1. Theoretical Considerations", *J. Phys. Chem.*, 89, 5530 (1985). ONR52
62. Martin Fleischmann, Saibal Bandyopadhyay, and Stanley Pons, "The Behavior of Microring Electrodes", *J. Phys. Chem.*, 89, 5537 (1985). ONR50
63. John K. Foley, Stanley Pons, and Jerry J. Smith, "FTIR Spectroelectrochemical Studies of Anodic Processes in Thiocyanate Solutions", *Langmuir*, 1, 697 (1985). ONR46
64. Carol Korzeniewski, and Stanley Pons, "In-Situ Infrared Spectroelectrochemistry", *Journal of Vacuum Science and Technology B*, 3, 1421 (1985). ONR47
65. William I. Higuchi, Thomas Masada, Uwe Rohr, J. Fox, C. Behl, and Stanley Pons, "Examination of Iontophoretic Transport of Ionic Drugs Across Skin", *J. Pharm. Sci.*, 74, 3 (1985).
66. John Cassidy and Stanley Pons, "Simulation of Edge Effects in Electroanalytical Experiment by Orthogonal Collocation. Part V. Chronoamperometry at Ultramicroelectrode Ensembles", *Canadian J. Chem.*, 63, 3577 (1985). ONR48

Publications

1986

(21,88)

67. Carol Korzeniewski and Stanley Pons, "Surface Infrared Spectroelectrochemistry. The Interaction of the Electric Field in the Electrical Double Layer with Pyrene Adsorbed on a Platinum Electrode: Effects on the Infrared Surface Difference Spectrum.", *Langmuir*, **2**, 468 (1986). ONR58
68. Alan M. Bond, Martin Fleischmann, S. B. Khoo, Stanley Pons, and Jim Robinson, "The Construction and Behavior of Ultramicroelectrodes: Investigations of Novel Electrochemical Systems", *Ind. J. Tech.*, **24**, 492 (1986). ONR38
69. Stanley Pons, Joel Russell, John K. Foley, and Mark Severson, "Vibrational Spectroelectrochemistry", *Modern Aspects of Electrochemistry*, **17**, J. Bockris, B. Conway, and J. White, eds.; Plenum Press, New York, 1986, p.223.
70. John W. Bixler, Alan M. Bond, D. A. Lay, Martin Fleischmann, and Stanley Pons, "Instrumental Configurations for the Determination of Sub-micromolar Concentrations of Electroactive Species with Carbon, Gold, and Platinum Microdisk Electrodes in Static and Flow Through Cells, *Anal. Chim. Acta*, **187**, 67 (1986). ONR67
71. John Cassidy, Jamal Ghoroghchian, Fereshteh Sarfarazi, Jerry J. Smith, and Stanley Pons, "Simulation of Edge Effects in Electroanalytical Experiments by Orthogonal Collation. Part VI. Cyclic Voltammetry and Ultramicroelectrode Ensembles", *Electrochimica Acta*, **31**, 629 (1986). ONR55
72. John Foley and Stanley Pons, "In-Situ Infrared Spectroelectrochemistry", *Instrumentation in Analytical Chemistry, 1982-1986*, American Chemical Society, Washington, 1986. ONR74
73. John Cassidy, Jiri Janata, and Stanley Pons, "Hydrogen Response of Palladium Coated Suspended Gate Field Effect Transistor", *Anal. Chem.*, **58**, 1757 (1986). ONR69
74. John Daschbach, Dale Heisler, and Stanley Pons, "Time Resolved Infrared Spectroelectrochemistry", *Applied Spectros.*, **40**, 489 (1986). ONR53
75. Jianguo Li, Jerry J. Smith, and Stanley Pons, "Far Infrared Spectroscopy of the Electrode--Solution Interface. The Vibrational Interaction of Adsorbed Metal Atoms with Surface States of a Gold Electrode", *Langmuir*, **2**, 297 (1986). ONR54
76. John K. Foley, Carol Korzeniewski, John Daschbach, and Stanley Pons, "Surface Infrared Spectroelectrochemistry, A. J. Bard, ed., "Electroanalytical Chemistry", **14**, 309 (1986). ONR66
77. Jianguo Li, John Daschbach, Jerry J. Smith, Michael D. Morse, and Stanley Pons, "The Infrared Spectra of Metal Atom Vibrations. SNIFTIRS Studies in the Far Infrared Region Using Time Resolved FTIR Techniques". *J. Electroanal. Chem.*, **209**, 387 (1986). ONR59
78. S. B. Khoo, John K. Foley, and Stanley Pons, "Electrolyte Effects on the Cyclic Voltammetry of TCNE and TCNQ", *J. Electroanal. Chem.*, **215**, 273 (1986). ONR64

Publications

1986(Continued)

79. Carol Korzeniewski, John Foley, and Stanley Pons, "In-situ Infrared Spectroelectrochemistry", *Ind. J. Technology*, 24, 347 (1986). ONR73
80. Martin Fleischmann, Jamal Ghoroghchian, Debra R. Rolison, and Stanley Pons, "The Electrochemical Behavior of Dispersions of Spherical Ultramicroelectrodes", *J. Phys. Chem.*, 90, 6392 (1986). ONR61
81. Timothy Dibble, S. Bandyopadhyay, Jamal Ghoroghchian, Jerry J. Smith, Fereshteh Sarfarazi, Martin Fleischmann, and Stanley Pons, "Electrochemistry at Very High Potentials. Oxidation of the Rare Gases and Other Gases in Non-Aqueous Solvents at Ultramicroelectrodes", *J. Phys. Chem.*, 90, 5275 (1986). ONR62
82. Andrea Russell, Kari Repka, Timothy Dibble, Jamal Ghoroghchian, Jerry J. Smith, Martin Fleischmann, Charles H. Pitt, and Stanley Pons, "Determination of Electrochemical Heterogeneous Electron Transfer Rates from Steady State Measurements at Ultramicroelectrodes", *Analytical Chemistry*, 58, 2961 (1986). ONR60
83. Kevin Ashley, John K. Foley, Qiu Mei, Jamal Ghoroghchian, Fereshteh Sarfarazi, John Cassidy, Brian Holton, Peter J. Stang, and Stanley Pons, "Electrochemical Oxidation and Reduction of a Substituted Alkylidene Cyclopropanaphthalene", *Journal of Organic Chemistry*, 59, 2089 (1986). ONR56
84. Jamal Ghoroghchian, Fereshteh Sarfarazi, Timothy Dibble, John Cassidy, Jerry J. Smith, Andrea Russell, Martin Fleischmann, and Stanley Pons, "Electrochemistry in the Gas Phase. Use of Ultramicroelectrodes for the Analysis of Electroactive Species in Gas Mixtures", *Analytical Chemistry*, 58, 2278 (1986). ONR57
85. Kevin Ashley and Stanley Pons, "Recent Advances in UV-Visible Spectroelectrochemistry", *Trends in Analytical Chemistry*, 5, 263 (1986). ONR77
86. Carol Korzeniewski, Stanley Pons, Parbury P. Schmidt, and Mark Severson, "A Theoretical Analysis of the Vibrational Spectrum of Carbon Monoxide on Platinum Metal Electrodes", *J. Chem. Phys.*, 85, 4153 (1986). ONR71
87. Kevin Ashley, Jerry J. Smith, S. Bandyopadhyay, Jerome F. McAleer, S. Pons, Edward M. Eyring, Harry B. Mark, Jr., Gordon Dunmore, and J. Ghoroghchian, "The Electrochemistry and Properties of Polyphenylene Formed from the Anodic Oxidation of Biphenyl in Aprotic Solvents", *J. Molec. Elect.*, 2, 183 (1986). ONR 65

Publications

1987

(20,108)

88. D. R. Rolison, R. J. Nowak, S. Pons, M. Fleischmann, and Jamal Ghoroghchian, "Alumina and Zeolyte Supported Ultramicroelectrodes", "Molecular Electronic Devices", F.L. Carter and H. Wohltjer, Eds., Elsevier, Amsterdam, 1987. ONR81
89. Martin Fleischmann and Stanley Pons, "The Behavior of Microelectrodes", *Anal. Chem.*, 59, 1391A (1987). ONR82
90. A. Scott Hinman, Brad J. Pavelich, Stanley Pons and Anne E. Kondo, "Oxidative Voltammetry of Some Tetraphenylporphyrins in the Presence of Nucleophiles Leading to Isoporphyrins", *J. Electroanal. Chem.*, 234, 145 (1987). ONR83
91. Martin Fleischmann, Stanley Pons, D.R. Rolison, and P.P. Schmidt, "Ultramicroelectrodes", Datatech Science Publishers, Morganton, NC, July 30, 1987. ONR84
92. Stanley Pons, "Gas Phase Electrochemistry", in "Report form the Utah Conference on Ultramicroelectrodes", Datatech Science Publishers, Morganton, NC, 1987. ONR85
93. John Daschbach, Daniel Blackwood, Joseph W. Pons, and Stanley Pons, "Electrochemistry of Ferrocene in Acetonitrile. Evidence for Irreversible Kinetic Behavior Due to Passive Film Formation". *J. Electroanal. Chem.*, 237, 269 (1987). ONR86
94. Kevin Ashley, Fereshteh Sarfarazi, Simon J. Buckland, John K. Foley, Qiu Mei, Brian Halton, Peter J. Stang, and Stanley Pons, "An Electrochemical and Spectroelectrochemical Study of Substituted Alkylidenecyclopropabenzene. 1-(Diphenylmethylene)cyclopropabenzene in the First Anodic and Cathodic Voltammetric Waves in Acetonitrile", *Can. J. Chem.*, 63, 2062 (1987). ONR131
95. Mira Josowicz, Jiri Janata, Kevin Ashley, and Stanley Pons, "Electrochemical and Ultraviolet - Visible Spectroelectrochemical Investigation of Selectivity of Potentiometric Gas Sensors Based on Polypyrrole", *Anal. Chem.* 59, 253 (1987). ONR87
96. Stanley Pons, "In-situ Vibrational Spectroelectrochemistry", in A. Bradshaw and H. Conrad, eds., "Vibrations at Surfaces 1987", Elsevier, Amsterdam, p. 713 (1987). ONR88
97. Martin Fleischmann and Stanley Pons, "The Behavior of Microring and Microdisk Electrodes", *J. Electroanal. Chem.*, 222, 107 (1987). ONR89
98. Carol Korzeniewski and Stanley Pons, "Infrared Spectroscopy of the Electrode Surface", in E. Yeung, ed., *Progress in Analytical Spectroscopy*, vol. 10, Pergamon Press, Oxford, 1987, p. 1. ONR78
99. Andrzej Czerwinski, David D. Cunningham, Adel Amer, John R. Schrader, Chiem Van Pham, Hans Zimmer, Harry B. Mark, Jr., and Stanley Pons, "The Electrochemical Behavior in Aqueous Media of Conducting Polymers. II. The Insoluble Fraction Obtained on the Cu(II) Catalyzed Polymerization of (2,5-Dibromo-3-group IV substituted) Thiophenes", *J. Electrochem. Soc.*, 134, 1158(1987). ONR80

Publications

1987 (Continued)

100. Jianguo Li, Jim Liang, Carol Korzeniewski, and Stanley Pons, "Electrochemical and Raman Spectroelectrochemical Investigation of Underpotentially Deposited Silver on a Gold Substrate", *Langmuir*, **3**, 470 (1987). ONR79
101. Jianguo Li and Stanley Pons, "An Electrochemical and Spectroelectrochemical Analysis of the Oxide / p-InP Semiconductor Surface Using FTIR and Raman Scattering Spectroscopies", *J. Electroanal. Chem.*, **233**, 1 (1987). ONR90
102. Carol Korzeniewski, Mark Severson, Parbury P. Schmidt, Stanley Pons, and Martin Fleischmann, "A Theoretical Analysis of the Vibrational Spectra of Ferricyanide and Ferrocyanide", *J. Phys. Chem.*, **91**, 5568, (1987). ONR 91
103. S. B. Khoo, John K. Foley, Carol Korzeniewski, and Stanley Pons, and Curt Marcott, "An Infrared Spectroelectrochemical Investigation of the Ion Pairing Reactions of the Anions and Dianions of TCNE and TCNQ", *J. Electroanal. Chem.*, **233**, 223 (1987). ONR92
104. Carol Korzeniewski, William McKenna, and Stanley Pons, "An *in situ* Infrared Study of the Oxidation of SO₂ on Platinum Electrodes", *J. Electroanal. Chem.*, **235**, 361 (1987). ONR93
105. Parbury P. Schmidt, Mark Severson, Carol Korzeniewski, and Stanley Pons, "The Analysis of Vibrational Spectra of Species Adsorbed at the Electrode Solution Interface. A Theoretical Model for the Effects of Potential on the Observed Spectra", *J. Electroanal. Chem.*, **225**, 1 (1987). ONR94
106. Stanley Pons, "In-situ Vibrational Spectroelectrochemistry", *J. Electron. Spectros. Rel. Phenomen.*, **45**, 303 (1987). ONR95
107. Stanley Pons, "Investigation of Electrode-Solution Interfaces by In-Situ Methods", in "Proceedings of the Robert A. Welch Foundation, XXX. Advances in Electrochemistry", Robert A. Welch Foundation, Houston, 1987, p.144. ONR96

Publications

1988

(36,144)

108. John K. Foley, Carol Korzeniewski, and Stanley Pons, "Anodic and Cathodic Reactions in Acetonitrile / Tetrabutylammonium Tetrafluoroborate: An Electrochemical and Infrared Spectroelectrochemical Study", *Can. J. Chem.*, 66, 201 (1988). ONR97
109. Mark W. Severson, Stanley Pons, Parbury P. Schmidt, Jianguo Li and Jerry J. Smith, "Infrared Spectra of Lithium Deposited on a Gold Electrode: Interpretation using a Cluster Model", *Phys. Rev. B*, 37 (1988) 810. ONR98
110. J. Desilvestro, Stanley Pons, E. Vrachnou and M. Grätzel, "Electrochemical and FTIR Spectroscopic Characterization of Ferrocyanide-Modified TiO₂ Electrodes Designed for Efficient Photosensitization", *J. Electroanal. Chem.*, 246 (1988) 411. ONR99
111. Tom Koenig, Tim Curtess, Rolf Winter, Stanley Pons, Kevin Ashley, Qiu Mei, Peter Stang, Brian Halton, Simon J. Buckland, and Debra R. Rolison, "He(I) Photoelectron Spectrum of Methylcyclopropene Derivatives", *J. Org. Chem.*, in press. ONR100
112. Daniel J. Blackwood and Stanley Pons, "An in situ Surface Fourier Transform Infrared Study of the Adsorption of Isoquinoline at a Stationary Mercury Electrode", *J. Electroanal. Chem.*, in press. ONR101
113. Daniel J. Blackwood and S. Pons, "An in situ Surface Fourier Transform Infrared Study of Complexes of Lithium, Magnesium, Potassium, and Lanthanum Ferrocyanide at Platinum and Gold Electrodes", *J. Electroanal. Chem.*, 244 (1988) 301. ONR102
114. Rossella Brina, Stanley Pons, and Martin Fleischmann, "Ultramicroelectrode Sensors and Detectors. Considerations of the Stability, Sensitivity, Reproducibility, and Mechanism of Ion Transport in Gas Phase Chromatography and in High Performance Liquid Phase Chromatography", *J. Electroanal. Chem.*, 244, 81, (1988). ONR103
115. Joseph W. Pons, John Daschbach, Martin Fleischmann, and Stanley Pons, "The Behavior of the Mercury Ultramicroelectrode", *J. Electroanal. Chem.*, 239, 427 (1988). ONR104
116. Kevin Ashley, Stanley Pons, and Martin Fleischmann, "Mathematical Modelling of Transport through Conducting Polymer Films. I. Theoretical Considerations.", *J. Electroanal. Chem.*, referees comments answered 5-15. ONR105
117. Kevin Ashley, Stanley Pons, and Martin Fleischmann, "Mathematical Modelling of Transport through Conducting Polymer Films. II. The Poly(paraphenylene) System", *J. Electroanal. Chem.*, ONR106
118. K. Ashley, D.B. Parry, J.M. Harris, and Stanley Pons, "A Conducting Polymer Electrochemically Generated Via Anodic Oxidation of Toluene", *J. Chem. Soc. Chem. Comm.*, in press 6-1. ONR107
119. W. McKenna, C. Korzeniewski, D. Blackwood, and S. Pons, "An Infrared Study of Thiocyanate at the Mercury Electrode Interface", *Electrochimica Acta* 33 (1988) 1019. ONR108

Publications

1988 (Continued)

120. Daniel Blackwood, Carol Korzeniewski, William McKenna, Jianguo Li and Stanley Pons, "Infrared Spectroelectrochemistry of Surface Species: An in situ Surface Fourier Transform Infrared Study of Adsorption of Isoquinoline at a Mercury Electrode", in Molecular Phenomena at Electrode Surfaces, ACS Symposium Series, American Chemical Society, Washington, in press. ONR109
121. Martin Fleischmann and Stanley Pons, "The Behavior of Microdisk and Microring Electrodes. Mass Transport to the Disk in the Unsteady State: Chronopotentiometry", J. Electroanal. Chem., in press. ONR110.
122. Martin Fleischmann and Stanley Pons, "The Behavior of Microdisk and Microring Electrodes. Mass Transport to the Disk in the Unsteady State: Chronoamperometry", J. Electroanal. Chem., in press ONR111.
123. Martin Fleischmann and Stanley Pons, "The Behavior of Microdisk and Microring Electrodes. Mass Transport to the Disk in the Unsteady State: A.C. Electrochemistry.", J. Electroanal. Chem., in press ONR112.
124. Martin Fleischmann and Stanley Pons, "The Behavior of Microdisk and Microring Electrodes. Mass Transport to the Disk in the Unsteady State: Coupled Chemical Reactions", J. Electroanal. Chem., in press ONR113.
125. Kevin Ashley and Stanley Pons, "IN SITU Infrared Interfacial Spectroelectrochemistry", Chem. Rev., 88 (1988) 673. ONR114
126. D.R. Rolison, E. A. Hayes, R. J. Nowak, S. Pons, and M. Fleischmann, "Electrode-Modified Zeolites - Electrode Microstructures Contained In and On a Heterogeneous Catalyst", J. Phys. Chem., in press ONR115
127. R. Brina and S. Pons, "The Use of Narrow Gap Line Microelectrodes as Sensitive and Species Selective Gas Chromatographic Detectors", J. Electroanal. Chem., submitted 6-28 ONR116
128. J. Daschbach, S. F. Simpson, J. M. Harris, M. Fleischmann, and S. Pons, "The Behavior of Disk Electrodes. Optical Imaging of the Concentration Distribution Over a Disk Electrode Under Galvanostatic Conditions", J. Electroanal. Chem., with MF 6-28.ONR117
129. See paper omitted in 1983. ONR118
130. M. R. Anderson, D. Blackwood, and S. Pons, "The Effect of Solvent Type on the Infrared Spectrum of Carbon Monoxide Adsorbed at Platinum Electrodes", J. Electroanal. Chem., in press ONR119.
131. M. R. Anderson, D. Blackwood, and S. Pons, "The Behavior of the Infrared Spectrum of Carbon Monoxide Adsorbed at Platinum Electrodes from Non-Aqueous Solvents", J. Electroanal. Chem., ONR120.

Publications

1988 (Continued)

132. A. Russell, C. Osterberg, D. Blackwood, M. R. Anderson, T. Richmond, and S. Pons, "The Activation of C-F Bonds by Addition at W(0): An Electrochemical Study." To JEC 6-1. ONR122
133. L. J. Li, M. Hawkins, J. W. Pons, M. Fleischmann, S. Pons, and L. M. Abrantes, "The Behavior of Microdisk and Microring Electrodes. The Chronoamperometric Response at Microdisk and Microring Electrodes", J. Electroanal. Chem., submitted. ONR123
134. L. M. Abrantes, M. Fleischmann, L. J. Li, M. Hawkins, J. W. Pons, J. Daschbach, and S. Pons, "The Behavior of Microdisk and Microring Electrodes. Chronopotentiometry and Linear Sweep Amperometry at a Microdisk Electrode", J. Electroanal. Chem., submitted. ONR124
135. K. Ashley, D. Parry, J. Harris, Stanley Pons, D. N. Bennion, Rodney Lafollette, Jeffrey Jones, and Edward King, "Properties Electrochemically Generated Poly(paraphenylene)", *Electrochim. Acta*, accepted 6-23. ONR125
136. V. Srinivasan, S.M. Sims, W.I. Higuchi, C.R. Behl, and Stanley Pons, "Iontophoretic Transport of Drugs: A Constant Voltage Approach", in *Modern Controlled Release Systems*, CRC Publishing, Boca Raton, submitted mid 87. ONR126
137. M. Kowalski, N.H. Werstiuk, J. W. Pons, P.J. Stang, and Stanley Pons, "An Electrochemical and Photoelectronic Spectral Study of Compounds with High Ionization Potentials: The Anodic Oxidation of Vinyl Triflates in Aprotic Solvents", *J. Am. Chem Soc.*, ONR127.
138. T. Masada, W.I. Higuchi, U. Rohr, J. Fox, C. Behl, and S. Pons, "Examination of Iontophoretic Transport of Ionic Drugs Across Skin I. Baseline Studies with the Four Electrode System" *J. Pharm. Science*, in press. ONR128
139. M. Fleischmann, J. Daschbach, and S. Pons, "The Behavior of Microdisk and Microring Electrodes. Application of Neumann's Integral Theorem to the Prediction of the Steady State Response of Microdisks", *J. Electroanal. Chem.*, submitted 8/1. ONR129
140. J. Ghoroghchian, S. Pons, and M. Fleischmann, "Electrocatalytic Hydrogenation of Acetylenic Hydrocarbons", *Electrochimica Acta*, submitted.
141. Richard D. Newmark, Martin Fleischmann, and Stanley Pons, "The Observation of Species on Platinum Colloids Using ^{195}Pt NMR", *Phys. Rev. Lett.*, submitted.
142. M. Fleischmann, N. Garrard, J. Daschbach, and S. Pons, "Electrochemical Kinetics Using Single Electron Counting", *Journal of Electroanalytical Chemistry*, submitted. ONR129
143. M. Fleischmann, J. Daschbach, and S. Pons, "The Behavior of Microdisk and Microring Electrodes. Prediction of the Chronoamperometric Response of Microdisks and of the Steady State for c.e. and e.c. Catalytic Reactions by Application of Neumann's Integral Theorem", *J. Electroanal. Chem.*, submitted 8/1. ONR130

144. L. M. Peter, D. Blackwood, and S. Pons, "In Situ Characterization of the Illuminated Silicon/
Electrolyte Interface by Fourier Transform Infrared Spectroscopy", Phys. Rev. Lett.,
submitted. ONR132

Curriculum Vitae

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Education: High School, Worthing, Sussex, England.
1939-1945.

Imperial College, London 1945-1949.
ARCS (Associate of the Royal College of Science) 1947
1st Class Special Honours Degree awarded in 1948.

Imperial College, London, 1947-1950
Degree of Ph.D. of London University, 1951.

Academic Career: 1950-1967

Successively Research Fellow and Imperial Chemical Industries Research Fellow at Kings College, University of Durham, Newcastle-upon-Tyne (now the University of Newcastle-upon-Tyne) then Lecturer and Reader in Physical Chemistry, University of Newcastle-upon-Tyne.

1967-1983

Professor of Electrochemistry at University of Southampton. (Chair originally endowed by the Electricity Council).

1977-1982

Science Research Council Senior Fellow.

1983-present

Honorary Research Professor. Also now hold part-time positions at Harwell and University of Utah.

1986. Elected Fellow of the Royal Society.

Other positions: Ex-Secretary and President of ISE. Ex-member of SERC Chemistry Committee. Consultant to several British, European and U.S. Industrial and/or Government Laboratories.

Medals: The Royal Society of Chemistry. Medal for Electrochemistry and Thermodynamics (1979).

Olin-Palladium Medal of The Electrochemical Society (1985).

- * 1. M. FLEISCHMANN and H.R. THIRSK. An investigation of electrochemical kinetics at constant overvoltage. The behaviour of the lead dioxide electrode.
Trans. Faraday Soc., 51 (1955) 71.
2. M. FLEISCHMANN, J. SOWERBY and H.R. THIRSK. The electrochemical polarization of silver single crystals in hydrochloric acid.
Trans. Faraday Soc., 53 (1957) 91.
3. M. FLEISCHMANN and MISS M. LILER. The anodic oxidation of solutions of plumbous salts.
Trans. Faraday Soc., 54 (1958) 1370.
4. M. FLEISCHMANN, S.J. BONE and W.F.K. WYNNE-JONES. The exchange of β -lead dioxide with plumbous ions in solution.
Trans. Faraday Soc., 55 (1959) 1783.
5. M. FLEISCHMANN and H.R. THIRSK. The rate and nature of phase changes at electrode surfaces.
Proceedings of the First International Symposium on Batteries, Bournemouth, (1958).
6. A. BEWICK, A. BEWICK, M. FLEISCHMANN and MISS M. LILER. Some factors in potentiostat design.
Electrochim. Acta, 1 (1959) 83.
7. M. FLEISCHMANN and H.R. THIRSK. The potentiostatic study of the growth of deposits on electrodes.
Electrochim. Acta, 1 (1959) 146.
8. M. FLEISCHMANN and I. SANGHI. Some potentiostatic studies on zinc.
Electrochim. Acta, 1 (1959) 161.
9. M. FLEISCHMANN and K.B. OLDHAM.
Electrochemistry, Annual Reports Chemical Society, 55 (1959) 67.
10. M. FLEISCHMANN and H.R. THIRSK. Anodic electrocrystallisation.
Electrochim. Acta, 2 (1960) 22.
11. M. FLEISCHMANN and H.R. THIRSK. Some problems in electrocrystallisation.
J. Electrochem. Soc. Japan, 28 (1960) 175.
12. A. BEWICK and M. FLEISCHMANN,
Fast reactions in solution, Annual Reports Chemical Society, 57 (1961) 90.
13. I. DUGDALE, M. FLEISCHMANN and W.K.F. WYNNE-JONES. The anodic oxidation of silver sulphate to silver oxide at constant potential.
Electrochim. Acta, 5 (1961) 229.
- * 14. A. BEWICK, M. FLEISCHMANN and H.R. THIRSK. Kinetics of the electrocrystallisation of thin films of calomel.
Trans. Faraday Soc., 58 (1962) 2200.

15. M. FLEISCHMANN, MISS I.M. TORDESILLAS and H.R. THIRSK. Kinetics of electrodeposition of γ -manganese dioxide.
Trans. Faraday Soc., 58 (1962) 1865.
16. M. FLEISCHMANN, K.S. RAJOGOPALAN and H.R. THIRSK. The growth of thin films of cadmium hydroxide on cadmium amalgam.
Trans. Faraday Soc., 59 (1963) 741.
- *17. M. FLEISCHMANN and H.R. THIRSK. Electrocrystallisation.
Advances in Electrochem. and Electrochem. Eng. Interscience, New York, 3 (1963) 123.
18. A. BEWICK and M. FLEISCHMANN. The design and performance of potentiostats.
Electrochim. Acta, 8 (1963) 89.
19. M. FLEISCHMANN and H.R. THIRSK. The growth of thin passivating layers on metals.
J. Electrochem. Soc., 110 (1963) 688.
20. M. FLEISCHMANN, I.N. PETROV and W.F.K. WYNNE-JONES. The investigation of the kinetics of the electrode reactions of organic compounds by potentiostatic methods.
Proceedings of the First Australian Conference on Electrochemistry, Hobart, Pergamon Press, Oxford, 500.
21. M. FLEISCHMANN and H.R. THIRSK. Electrochemical kinetics of formation of monolayers of solid phases.
Electrochim. Acta, 9 (1964) 757.
22. M. FLEISCHMANN, J. PATTISON and H.R. THIRSK. Electrocrystallisation of thin films of thallos chloride on thallium amalgam.
Trans. Faraday Soc., 61 (1965) 1256.
23. G.W.D. BRIGGS, M. FLEISCHMANN and H.R. THIRSK. The behaviour of electrodes of the second kind.
Proceedings of the Fourth International Symposium on Batteries, Brighton, Pergamon Press, Oxford, (1965) 167.
24. R.D. ARMSTRONG, M. FLEISCHMANN and H.R. THIRSK. Anodic behaviour of mercury in chloride ion solutions.
Trans. Faraday Soc., 61 (1965) 2238.
25. M. FLEISCHMANN, J.A. HARRISON and H.R. THIRSK. Electrocrystallisation of thin films of nickel.
Trans. Faraday Soc., 61 (1965) 2742.
26. A. BEWICK, M. FLEISCHMANN, J.N. HIDDLESTON AND LORD WYNNE-JONES. Examination of proton transfer reactions by temperature jump and electrochemical methods.
Discuss. Faraday Soc., 39 (1966) 149.

27. M. FLEISCHMANN, J.R. MANSFIELD and LORD WYNNE-JONES. The anodic oxidation of aqueous solutions of acetate ions at smooth platinum electrodes, Part I: The non-steady state of oxidation of platinum.
J. Electroanal. Chem., 10 (1965) 511.
- * 28. M. FLEISCHMANN, J.R. MANSFIELD and LORD WYNNE-JONES. The anodic oxidation of aqueous solutions of acetate ions at smooth platinum electrodes. Part II: The non-steady state of the Kolbe synthesis of ethane.
J. Electroanal. Chem., 10, (1965) 522.
29. R.D. ARMSTRONG, M. FLEISCHMANN and H.R. THIRSK. The anodic behaviour of mercury in hydroxide ion solutions.
J. Electroanal. Chem., 11 (1966) 208.
30. M. FLEISCHMANN and J.A. HARRISON. The calculation of potentiostatic current-time curves for the electrocrystallisation of metals.
J. Electroanal. Chem., 12 (1966) 183.
31. M. FLEISCHMANN and J.A. HARRISON. The relative roles of adatoms and of ions in solution near step lines during electrocrystallisation reactions.
Electrochim. Acta, 11 (1966) 749.
32. A. BEWICK and M. FLEISCHMANN. The design of potentiostats for use at very short times.
Electrochim. Acta, 11 (1966) 1397.
33. A. BEWICK, M. FLEISCHMANN and J.N. HIDDLESTON. The determination of the kinetics of fast reactions in solution by electrochemical methods. *Proceedings of the Third International Congress on Polarography*, Southampton, Macmillan, (1966) 57.
34. R.D. ARMSTRONG and M. FLEISCHMANN. The formation of thin layers of solid phases on liquid electrodes.
J. Polarographic Soc., 11 (1965) 31.
35. R.D. ARMSTRONG, M. FLEISCHMANN and J. KORYTA. Anodic polarographic waves involving insoluble mercury salt formation.
Coll. Czechoslov. Chem. Comm., 30 (1965) 4324.
36. G.W.D. BRIGGS and M. FLEISCHMANN. The anodic deposition of NiOOH from nickel acetate at constant potential.
Trans. Faraday Soc., 62 (1966) 3217.
37. M. FLEISCHMANN and J.N. HIDDLESTON. A palladium-hydrogen probe electrode for use as a microreference electrode.
J. Sci. Instr., 1 (1968) 667.
- * 38. M. FLEISCHMANN, J. KORYTA and H.R. THIRSK. Kinetics of electro-deposition and catalytic activity of thin films of ruthenium.
Trans. Faraday Soc., 63 (1967) 1261.

39. M. FLEISCHMANN, S.K. RANGARAJAN and H.R. THIRSK. Effects of diffusion through solutions and along surface on electro-crystallization processes, Part I.
Trans. Faraday Soc., 63 (1967) 1240.
40. M. FLEISCHMANN, S.K. RANGARAJAN and H.R. THIRSK. Effects of diffusion through solution and along surface on electro-crystallization processes, Part 2.
Trans. Faraday Soc., 63 (1967) 1251.
41. M. FLEISCHMANN, S.K. RANGARAJAN and H.R. THIRSK. Effect of diffusion through the solution and along surface on electro-crystallization processes, Part 3.
Trans. Faraday Soc., 63 (1967) 1256.
42. G. ATHERTON, M. FLEISCHMANN and F. GOODRIDGE. Kinetic study of the Hofer-Moest reaction.
Trans. Faraday Soc., 63 (1967) 1468.
43. M. FLEISCHMANN, J.R. MANSFIELD, H.R. THIRSK, H.G.E. WILSON and LORD WYNNE-JONES. The investigation of the kinetics of electrode reactions by the application of repetitive square pulses of potential.
Electrochim. Acta, 12 (1967) 967.
44. R.D. ARMSTRONG and M. FLEISCHMANN. The anodic formation of mercurous oxalate.
Z. Physik. Chemie, 52 (1966) 131.
45. R.D. ARMSTRONG, M. FLEISCHMANN and J.W. OLDFIELD. The anodic formation of phosphates of mercury.
J. Electroanal. Chem., 14 (1967) 235.
46. M. FLEISCHMANN and D. PLETCHER. The electrochemical oxidation of aliphatic hydrocarbons in acetonitrile.
Tetrahedron Letters, (1968) 6255.
47. M. FLEISCHMANN and J.W. OLDFIELD. Generation-recombination noise in weak electrolytes.
J. Electroanal. Chem., 27 (1970) 207.
48. M. FLEISCHMANN, D. PLETCHER and G.M. RACE. The electrochemical oxidation of mercury(II)-olefin complexes.
J. Chem. Soc. B, (1970) 1746.
49. G.W.D. BRIGGS, M. FLEISCHMANN, D.J. LAX and H.R. THIRSK. Texture, growth and orientation of anodically formed silver oxides.
Trans. Faraday Soc., 64 (1968) 3120.
50. M. FLEISCHMANN, D.J. LAX and H.R. THIRSK. Kinetic studies of the formation of AgO.
Trans. Faraday Soc., 64 (1968) 3128.

51. M. FLEISCHMANN, R.D. ARMSTRONG and J.W. OLDFIELD. Properties of anodically formed mercury barbiturates. *Trans. Faraday Soc.*, 65 (1969) 3053.
52. M. FLEISCHMANN, J.R. BACKHURST, J.M. COULSON, F. GOODRIDGE and R.E. PLIMLEY. A preliminary investigation of fluidised bed electrodes. *J. Electrochem. Soc.*, 116 (1969) 1600.
53. M. FLEISCHMANN, D.J. LAX and H.R. THIRSK. Electrochemical studies of the $\text{Ag}_2\text{O}/\text{AgO}$ phase change in alkaline solutions. *Trans. Faraday Soc.* 64 (1968) 3137.
54. M. FLEISCHMANN and D. PLETCHER. Electrosynthesis of organic compounds. *Platinum Metal Review*, 13 (1969) 46.
55. M. FLEISCHMANN and D. PLETCHER. Organic Electrochemistry. *Royal Institute of Chemistry Reviews*, 2 (1969) 87.
56. M. FLEISCHMANN, J.R. BACKHURST, F. GOODRIDGE. Fluidised Zinc/Oxygen Electrode System. *Nature*, 221 (1969) 55.
57. M. FLEISCHMANN and F. GOODRIDGE. Anodic oxidation under pulse conditions. *Discuss. Faraday Soc.*, 45 (1968) 254.
58. M. FLEISCHMANN, D. PLETCHER and G.M. RACE. The electrochemical oxidation of the mercury II-propene complex. *J. Electroanal. Chem.* 23 (1969) 369.
59. M. FLEISCHMANN, G. FAITA and D. PLETCHER. The electrochemical oxidation of cyclohexene-chloride ion mixtures in acetonitrile. *J. Electroanal. Chem.*, 25 (1970) 455.
60. M. FLEISCHMANN and D. PLETCHER. The electrochemistry of polynuclear hydrocarbons in the molten salt system $\text{AlCl}_3\text{-NaCl-KCl}$. *J. Electroanal. Chem.*, 25 (1970) 449.
61. M. FLEISCHMANN and J.W. OLDFIELD. Fluidised Bed Electrodes. I. Polarisation predicted by simplified models. *J. Electroanal. Chem.*, 29 (1971) 211.
62. M. FLEISCHMANN and J.W. OLDFIELD. Fluidised Bed Electrodes. II. Effective resistivity of the discontinuous metal phase. *J. Electroanal. Chem.*, 29 (1971) 231.
63. M. FLEISCHMANN, J.W. OLDFIELD and D.F. PORTER. Fluidised Bed Electrodes. III. The cathodic reduction of oxygen on silver in a fluidised bed. *J. Electroanal. Chem.*, 29 (1971) 241.
64. M. FLEISCHMANN, D. PLETCHER and A. RAFINSKI. The kinetics of the silver(I)/silver(II) couple in acid solution. *J. Applied Electrochem.*, 1 (1971) 1.

65. M. FLEISCHMANN, D. PLETCHER and C.J. VANCE. The reduction of alkyl halides at a lead cathode in dimethylformamide. *J. Electroanal. Chem.*, 29 (1971) 325.
66. M. FLEISCHMANN and G.W.D. BRIGGS. Oxidation and reduction of nickel hydroxide at constant potential. *Trans. Faraday Soc.*, 67 (1971) 2397.
67. J. BERTRAM, M. FLEISCHMANN and D. PLETCHER. The anodic oxidation of alkanes in fluorosulphonic acid; a novel synthesis of $\alpha\beta$ -unsaturated ketones. *Tetrahedron Letters*, (1971) 349.
68. M. FLEISCHMANN and G. SUNDHOLM. The influence of crystal growth on the polarographic behaviour of the cis-dichlorodiammine platinum(II) complex. *J. Electroanal. Chem.*, 30 (1971) App. 4.
- *69. M. FLEISCHMANN, K. KORINEK and D. PLETCHER. The oxidation of organic compounds on a nickel anode in alkaline solution. *J. Electroanal. Chem.*, 31 (1971) 39.
70. M. FLEISCHMANN, D. PLETCHER and G. SUNDHOLM. The anodic oxidation of organomercury compounds. *J. Electroanal. Chem.*, 31 (1971) 51.
71. M. FLEISCHMANN and D. PLETCHER. The electrode reactions of organic molecules. Chapter in *Reactions of Molecules on Electrodes*, John Wiley and Sons, (1971) 347.
72. M. FLEISCHMANN, D. PLETCHER and K. KORINEK. The oxidation of organic compounds at a cobalt electrode in alkaline media. *J. Electroanal. Chem.*, 33 (1972) 478.
73. M. FLEISCHMANN, J.W. OLDFIELD and L. TENNAKON. Electrodeposition of copper in a fluidised bed. *J. Applied Electrochem.*, 1 (1971) 103.
74. M. FLEISCHMANN, K. KORINEK and D. PLETCHER. The oxidation of hydrazine at a nickel anode in alkaline solution. *J. Electroanal. Chem.*, 34 (1972) 494.
75. D. CLARK, M. FLEISCHMANN and D. PLETCHER. The partial anodic oxidation of propylene in acetonitrile. *J. Electroanal. Chem.*, 36 (1972) 137.
76. M. FLEISCHMANN and D. PLETCHER. The partial anodic oxidation of aliphatic hydrocarbons. *Chemie-Ing-Techn.*, 44 (1972) 187.
77. M. FLEISCHMANN, D. PLETCHER and A. RAFINSKI. Homogeneous kinetics using electrochemical cells with a large ratio of electrode area to solution volume. Part I. *J. Electroanal. Chem.*, 38 (1972) 323.

78. M. FLEISCHMAN, D. PLETCHER and A. RAFINSKI. Homogeneous kinetics using electrochemical cells with a large ratio of electrode area to solution volume. Part 2. *J. Electroanal. Chem.*, 38 (1972) 329.
79. M. FLEISCHMANN, D. PLETCHER and C.J. VANCE. The oxidation of ethylaluminium and ethylmagnesium compounds at a lead anode in tetrahydrofuran. *J. Organometal. Chem.*, 40 (1972) 1.
80. B. DANDAPANI and M. FLEISCHMANN. Electrolytic separation factors on palladium. *J. Electroanal. Chem.*, 39 (1972) 323.
81. B. DANDAPANI and M. FLEISCHMANN. Electrolytic separation factors on platinum under non-steady-state conditions. *J. Electroanal. Chem.*, 39 (1972) 315.
82. M. FLEISCHMANN, K. KORINEK and D. PLETCHER. The kinetics and mechanism of the oxidation of amines and alcohols at oxide-covered nickel, silver copper and cobalt electrodes. *J. Chem. Soc., Perkin Trans. II*, (1972) 1396.
- * 83. M. FLEISCHMANN and M. GRENNES. Electrocristallization of ruthenium and electrocatalysis of hydrogen evolution. *J. Chem. Soc. Faraday Trans. 1*, 68 (1972) 2305.
84. M. FLEISCHMANN, G. MENGOLI and D. PLETCHER. The reduction of simple alkyl iodides at tin cathodes in dimethylformamide. *Electrochim. Acta*, 18 (1973) 231.
85. M. FLEISCHMANN, P.J. HENDRA and A.J. McQUILLAN, Raman spectra from electrode surfaces. *Chem. Comm.*, (1973) 80.
86. M. FLEISCHMANN, G. MENGOLI and D. PLETCHER. The cathodic reduction of acetonitrile. A new synthesis of tin tetramethyl. *J. Electroanal. Chem.*, 43 (1973) 308.
87. V.R. KOCH, L.L. MILLER, D.B. CLARK, M. FLEISCHMANN, T. JOSLIN and D. PLETCHER. Anodic fluorination from fluoroborate electrolytes. *J. Electroanal. Chem.*, 43 (1973) 318.
88. D.B. CLARK, M. FLEISCHMANN and D. PLETCHER. The partial anodic oxidation of aliphatic hydrocarbons in aprotic solvents. *J. Electroanal. Chem.*, 42, (1973) 133.
- * 89. J. BERTRAM, J.P. COLEMAN, M. FLEISCHMANN and D. PLETCHER. The electrochemical behaviour of alkanes in fluorosulphonic acid. *J. Chem. Soc. Perk II* (1973) 374.
90. J.P. COLEMAN, M. FLEISCHMANN and D. PLETCHER. Anodic oxidation of polyfluorinated aromatic hydrocarbons in fluorosulphonic acid - production of stable cation radicals. *Electrochim. Acta*, 18 (1973) 331.

91. D.B. CLARK, M. FLEISCHMANN and D. PLETCHER. The partial anodic oxidation of n-alkanes in acetonitrile and trifluoroacetic acid. *J. Chem. Soc. Perk II*, (1973) 1598.
92. M. FLEISCHMANN and D. PLETCHER. Chapter in *Advances in Physical Organic Chemistry*, Academic Press, 10 (1973) 155.
93. A.K. CHU, M. FLEISCHMANN and G.J. HILLS. Packed bed electrodes. I. The electrochemical extraction of copper ions from dilute aqueous solutions. *J. Applied Electrochem.*, 4 (1974) 323.
94. A.G. DOUGHTY, M. FLEISCHMANN and D. PLETCHER. Anode effects on reactions in anhydrous hydrogen fluoride at 0°C. *J. Electroanal. Chem.*, 51 (1974) 456.
95. A.P. BROWN, M. FLEISCHMANN and D. PLETCHER. A new design of thin layer cell - The application to the study of the reactions of silver (II). *J. Electroanal. Chem.*, 50 (1974) 65.
96. A.G. DOUGHTY, M. FLEISCHMANN and D. PLETCHER. A study of the fluorine evolution reaction at a platinum electrode in anhydrous hydrogen fluoride. *J. Electroanal. Chem.*, 51 (1974) 329.
97. G.J. BIGNOLD and M. FLEISCHMANN. Identification of transient phenomena during the anodic polarisation of iron in dilute sulphuric acid. *Electrochim. Acta*, 19 (1974) 363.
98. P. BINDRA, M. FLEISCHMANN, J.W. OLDFIELD and D. SINGLETON. Nucleation. *Disc. Faraday Soc.*, 56 (1973) 180.
99. M. FLEISCHMANN. Some engineering aspects of electrosynthesis. *Pure and Applied Chem.*, 5 (1973) 1.
100. M. FLEISCHMANN, T. JOSLIN and D. PLETCHER. A further study of diffusion and its role in electrode reactions. The development of a novel double potential step technique. *Electrochim. Acta*, 19 (1974) 511.
101. P. BINDRA, A.P. BROWN, M. FLEISCHMANN and D. PLETCHER. The determination of the kinetics of very fast electrode reactions by means of a quasi steady-state method. The mercurous ion/mercury system. Part I. Theory. *J. Electroanal. Chem.*, 58 (1975) 31.
102. P. BINDRA, A.P. BROWN, M. FLEISCHMANN and D. PLETCHER. The determination of the kinetics of very fast electrode reactions by means of a quasi-steady state method. The mercurous ion/mercury system. Part II. Experimental results. *J. Electroanal. Chem.*, 58 (1975) 39.

103. M. FLEISCHMANN and D. PLETCHER, Industrial Electrochemistry.
Chem. Britain, 11 (1975) 50.
104. M. FLEISCHMANN and G.H. KELSALL. An investigation of the local
behaviour of a copper fluidised bed electrode.
Chemistry and Industry, 8 (1975) 329.
105. M. FLEISCHMANN, W.B. GARA and G.J. HILLS. Electrode reactions
of organic compounds as a function of pressure.
J. Electroanal. Chem., 60 (1975) 313.
106. M. FLEISCHMANN and R.E.W. JANSSON. Characterisation of
electrochemical reactors.
Chemical Engineer, (1975) 603.
107. M. FLEISCHMANN, P.J. HENDRA, A.J. McQUILLAN, R.L. PAUL and E.S. REID.
Raman spectroscopy at electrode-electrolyte interfaces.
J. Raman Spec., 4 (1976) 269.
- *108. M. FLEISCHMANN, P.J. HENDRA and A.J. McQUILLAN. Adsorption of
pyridine at the surface of a silver electrode,
Chem. Phys. Lett., 26 (1974) 163.
109. M. FLEISCHMANN, P.J. HENDRA and A.J. McQUILLAN. Raman spectroscopic
investigation of silver electrodes.
J. Electroanal. Chem., 65 (1975) 933.
110. M. FLEISCHMANN, P.J. HENDRA, A.J. McQUILLAN and R.L. PAUL. Raman
spectroscopy at the copper electrode surface.
J. Electroanal. Chem., 66 (1975) 248.
111. M. FLEISCHMANN, P.J. HENDRA, R. COONEY and E. REID. The Raman
spectrum of adsorbed I_2 on a Pt electrode surface.
J. Raman Spec., 6 (1977) 264.
112. M. FLEISCHMANN, P.J. HENDRA, E. REID and R. COONEY. A Raman
spectroscopic study of corrosion of lead electrodes in aqueous
chloride media.
J. Electroanal. Chem., 80 (1977) 405.
113. M. FLEISCHMANN, P.J. HENDRA, R.P. COONEY and E.S. REID. Thiocyanate
adsorption at Ag electrodes: A Raman spectroscopic study.
J. Chem. Soc. Faraday I, (1977) 1691.
114. M. FLEISCHMANN, P.J. HENDRA and R.P. COONEY. The Raman spectrum of
carbon monoxide on a platinum surface.
Chem. Comm., 7 (1977) 235.
115. M. FLEISCHMANN, P.J. HENDRA and R.P. COONEY. Raman spectra from
adsorbed iodine species on a smooth Pt electrode surface.
J. Raman Spec., 6 (1977) 264.
116. M. FLEISCHMANN and R.E.W. JANSSON. The characterisation and
performance of three-dimensional electrodes.
Chimie Ingenieur Technik, 49 (1977) 283.

117. M. FLEISCHMANN, C. GABRIELLI, M.T.G. LABRAM, A.I. McMULLEN and T.H. WILMSHURST. Alamethicin induced conductances in lipid bilayers. Part I. Data analysis.
J. Membrane Biol. 55 (1980) 9.
118. A. BEWICK and M. FLEISCHMANN. Formation of surface compounds on electrodes.
Topics in Surf. Chem. (Proc. Int. Symp.), (1977) 45.
119. M. FLEISCHMANN and R.E.W. JANSSON. Dispersion in electrochemical cells with radial flow between parallel electrodes. I. A dispersive plug flow mathematical model.
J. Applied Electrochem., 9 (1979) 427.
120. M. FLEISCHMANN, J. GHOROGHCHIAN and R.E.W. JANSSON. Dispersion in electrochemical cells with radial flow between parallel electrodes. II. Experimental results for capillary gap cell and pump cell configurations.
J. Applied Electrochem., 9 (1979) 437.
121. P.J. HENDRA and M. FLEISCHMANN. Raman spectroscopy at surfaces.
Topics in Surface Chem. (Proc. Int. Symp.) (1977) 373.
122. R.E.W. JANSSON and M. FLEISCHMANN. Effect of cell design on selectivity and conversion in electroorganic processes.
AIChE Symp. No. 185, 75 (1979) 2.
123. L.G. CHATTEN, M. FLEISCHMANN and D. PLETCHER. The anodic oxidation of some tetracyclines.
J. Electroanal. Chem., 102 (1979) 407.
124. M. FLEISCHMANN and A. BEWICK. The role of electrochemistry in the development and investigation of very small structures.
Microstruct. Sci & Technol. NAS Washington.
125. M. FLEISCHMANN and R.E.W. JANSSON. Electrochemical pump cells.
Trans SAEST, 12 (1979) 277.
126. M. FLEISCHMANN and R.E.W. JANSSON. Cells for electrosynthetic reactions.
Chem. Weekbl., (1979) 539.
127. M. FLEISCHMANN and R.E.W. JANSSON. The environment in electrochemical reactors.
Physicochem. Hydrodynamics, (1979) 1005.
128. M. FLEISCHMANN and I. JUSTINIJANOVIC. The characterisation of three-dimensional electrodes using an electrochemical tracer method.
J. Applied Electrochem., 10 (1980) 143.
129. M. FLEISCHMANN and Z. IBRISAGIC. Performance of bipolar trickle reactors.
J. Applied Electrochem., 10 (1980) 169.
130. M. FLEISCHMANN and Z. IBRISAGIC. Electrochemical measurements in bipolar trickle reactors.
J. Applied Electrochem., 10 (1980) 151.

131. M. FLEISCHMANN and Z. IBRISAGIĆ. Examination of flow models for bipolar trickle reactors.
J. Applied Electrochem., 10 (1980) 157.
132. M. FLEISCHMANN, P.J. HENDRA, I.R. HILL and M.E. PEMBLE. Enhanced Raman spectra for species formed by the coadsorption of halide ions and water molecules on silver electrodes.
J. Electroanal. Chem., 117 (1981) 243.
133. M. FLEISCHMANN, J. ROBINSON and R. WASER. An electrochemical study of the adsorption of pyridine and chloride ions on smooth and roughened silver surfaces.
J. Electroanal. Chem., 117 (1981) 257.
- * 134. M. FLEISCHMANN and R.E.W. JANSSON. The application of the principles of reaction engineering to electrochemical cell design.
J. Chem. Techn. Biotechnol., 30 (1980) 351.
135. M.Y. ABYANEH and M. FLEISCHMANN. The electrocrystallisation of nickel.
Trans. Inst. of Metal Finishing, 58 (1980) 91.
- * 136. M. FLEISCHMANN, M. LABRAM, C. GABRIELLI and A. SATTAR. The measurement and interpretation of stochastic effects in electrochemistry and bioelectrochemistry.
Surf. Sci., 101 (1980) 583.
137. M. FLEISCHMANN, J. GHOROGHCHIAN and R.E.W. JANSSON. Reduction of 2-nitropropane in an undivided, flooded, bipolar cell.
J. Applied Electrochem., 11 (1981) 55.
138. M.Y. ABYANEH and M. FLEISCHMANN. The electrocrystallisation of nickel. Part I. Generalised models of electrocrystallisation.
J. Electroanal. Chem., 119 (1981) 187.
139. M.Y. ABYANEH and M. FLEISCHMANN. The electrocrystallisation of nickel. Part II. Comparison of models with the experimental data.
J. Electroanal. Chem., 119 (1981) 197.
- * 140. M. FLEISCHMANN, P.J. HENDRA and J. ROBINSON. X-ray diffraction from adsorbed iodine on graphite.
Nature, 288 (1980) 152.
141. A. BEWICK, M. FLEISCHMANN and J. ROBINSON. Structural investigations of electrode-solution interfaces.
Proc. of the Fachgruppe Angewandte Elektrochemie der GDCh. 90 (1980) 87.
142. M. FLEISCHMANN and R.E.W. JANSSON. Cells for electrochemical reaction.
Chemisch. Weekblad, 39 (1977) 539.
143. M. FLEISCHMANN and A. BEWICK. Formation of surface compounds on electrodes.
Proc. of IBM meeting on Surface Chemistry. Plenum Press (1977).

144. M. FLEISCHMANN and P.J. HENDRA. Raman spectroscopy of surfaces. *Proc. of IBM Meeting on Surface Chemistry*, Plenum Press, (1977).
145. M. FLEISCHMANN and I.R. HILL. Raman spectroscopy. Chapter in Vol. 4 *Comprehensive Treatise of electrochemistry*, Eds. J. O'M Bockris, B.E. Conway, E. Yeager and R.E. White.
146. M. FLEISCHMANN, S. DAOLIO and D. PLETCHER. A study of the deposition of molybdenum at a vitreous carbon cathode from an aqueous citrate bath. *J. Electroanal. Chem.*, 130 (1981) 269.
- * 147. M. FLEISCHMANN, I.R. HILL and M.E. PEMBLE. Surface enhanced Raman spectroscopy of $^{12}\text{CN}^-$ and $^{13}\text{CN}^-$ adsorbed at silver electrodes. *J. Electroanal. Chem.*, 136 (1981) 361.
148. M. FLEISCHMANN and D. PLETCHER. Chapter in *Chemical Industry*, Eds. Sharp and West, Ellis Horwood, (1982) 581.
149. S. EHDAIE, M. FLEISCHMANN and R.E.W. JANSSON. Application of the trickle tower to problems of pollution control. I. The scavenging of metal ions. *J. Applied Electrochem.*, 12 (1982) 59.
150. M. FLEISCHMANN and I.R. HILL. Electrochemical effects in surface enhanced Raman Scattering, Eds. Chang & Furtak, Plenum Press (1982).
151. M. FLEISCHMANN and I.R. HILL. Surface enhanced Raman scattering from silver electrodes: formation and photolysis of chemisorbed pyridine species. *J. Electroanal. Chem.*, 146 (1983) 353.
- * 152. M. FLEISCHMANN and I.R. HILL. The observation of solvated metal ions in the double layer region at silver electrodes using surface enhanced Raman scattering. *J. Electroanal. Chem.*, 146 (1983) 367.
153. M. FLEISCHMANN and R.E.W. JANSSON. The reaction engineering of electrochemical cells. I. Axial packed bed electrodes. *Electrochim. Acta*, 27 (1982) 1023.
154. M. FLEISCHMANN and R.E.W. JANSSON. The reaction engineering of electrochemical cells. II. Packed bed electrodes with orthogonal flow of current and solution. *Electrochim. Acta*, 27 (1982) 1029.
155. M.Y. ABYANEH and M. FLEISCHMANN. The role of nucleation and of overlap in electrocrystallization reactions. *Electrochim. Acta*, 27 (1982) 1513.
156. M. FLEISCHMANN, P.R. GRAVES, I.R. HILL and J. ROBINSON. Raman spectroscopy of pyridine adsorbed on roughened β palladium hydride electrodes. *Chem. Phys. Letters*. 95 (1983) 322.
157. M.Y. ABYANEH, M. BERKEM and M. FLEISCHMANN. The electrocrystallisation of nickel: The effects of additives. *Trans. of the Inst. of Metal Finishing*, 60 (1982) 114.

RECENT PUBLICATIONS PERTINENT TO THIS PROPOSAL

158. M. FLEISCHMANN, R.L. MCCREERY and P. HENDRA. Fiber optic probe for remote Raman spectroscopy. *Analyt. Chem.* 55 (1983) 146.
159. M. FLEISCHMANN, C. GABRIELLI and M.T.G. LABRAM. Analysis of the voltage-gated alamethicin induced channel conductance in lipid bilayers as a triggered birth and death stochastic process. *Phys. Chem. of Transmembrane ion Motions*, Elsevier Publishers, Amsterdam (1983) 367.
160. M. FLEISCHMANN, C. GABRIELLI and M. LABRAM. The measurement and interpretation of fluctuations in electrochemical systems: the electrocrystallisation of silver and the voltage-gated alamethicin induced channel conductance in lipid bilayers. *J. Electroanal. Chem.* 150 (1983) 111.
161. M. FLEISCHMANN, P. GRAVES, I. HILL, A. OLIVER and J. ROBINSON. Raman spectroscopic and X-ray diffraction studies of electrode-solution interfaces. *J. Electroanal. Chem.* 150 (1983) 33.
162. M. FLEISCHMANN, C. GABRIELLI, M.T.G. LABRAM, T. MARKVART. Alamethicin induced conductances in lipid bilayers. II. Decomposition of the observed (compound) process. *J. Electroanal. Chem.* submitted.
163. M. FLEISCHMANN, C. GABRIELLI and M. LABRAM. Alamethicin induced conductances in lipid bilayers. III. Derivation of the properties of the elementary process from the (compound) observed process. *J. Electroanal. Chem.* submitted.
164. E. BUDEVSKI, V. BOSTANOV, M. FLEISCHMANN, C. GABRIELLI, M. LABRAM W. OBRETE NOV and Z. STOYNOV. The analysis of fluctuations in the rates of electrocrystallisation of silver. Part I. Experimental data. *J. Electroanal. Chem.* submitted.
165. E. BUDEVSKI, M. FLEISCHMANN, C. GABRIELLI and M. LABRAM. The analysis of fluctuations in the rates of electrocrystallisation of silver. Part II. Comparison of experimental data at low over-potential with models. *J. Electroanal. Chem.* submitted.
166. E. BUDEVSKI, M. FLEISCHMANN, C. GABRIELLI and M. LABRAM. The analysis of fluctuations in the rates of electrocrystallisation of silver. Part III. Comparison of experimental data at high overpotentials with models. *J. Electroanal. Chem.* submitted.
167. M. FLEISCHMANN and A. SARABY-REINTJES. The simultaneous deposition of nickel and hydrogen on vitreous carbon. *Electrochim. Acta.* 29, (1984), 69.
168. M. FLEISCHMANN, C.L.K. TENNAKON, A. BAMPFIELD and P.J. WILLIAMS. Electrosynthesis in systems of two immiscible liquids and a phase transfer catalyst. III. Characterisation of thin film contactor cells. *J. Applied Electrochem.* 13 (1983) 593.

169. M. FLEISCHMANN, C.L.K. TENNAKON, P. GOUGH, J.H. STEVEN and S.R. KORN. Electrosynthesis in systems of two immiscible liquids and a phase transfer catalyst. IV. Electrosynthesis using thin film contactor cells.
J. Applied Electrochem. 13 (1983) 603.
170. M. FLEISCHMANN and G.H. KELSALL. A parametric study of copper deposition in a fluidised bed electrode.
J. Appl. Electrochem. 14 (1984) 269
171. M. FLEISCHMANN, I.R. HILL and J. ROBINSON. Surface enhanced Raman scattering from silver electrodes: Potential and cation dependences of the very low frequency mode.
Chem. Phys. Lett. 97 (1983) 441.
172. M. FLEISCHMANN, I.R. HILL, G. MENGOLI and M.M. MUSIANI. A Raman spectroscopic investigation of the electropolymerization of phenol on silver electrodes.
Electrochim. Acta. 28 (1983) 1545.
173. M. FLEISCHMANN and G.H. KELSALL. A feasibility study of mercury deposition in a lead fluidised bed electrode.
J. Applied Electrochem. 14 (1984) 277
174. E. BUDEVSKI, M. FLEISCHMANN, C. GABRIELLI and M. LABRAM. Statistical Analysis of the 2-D nucleation and electro-crystallisation of silver
Electrochim. Acta, 28 (1983) 925.
175. M. FLEISCHMANN, I.R. HILL and G. SUNDHOLM.
A Raman spectroscopic study of quinoline and isoquinoline adsorbed on copper and silver electrodes.
J. Electroanal. Chem., 158 (1983) 153.
- * 176. M. FLEISCHMANN, I.R. HILL and G. SUNDHOLM.
A Raman spectroscopic study of thiourea adsorbed to silver and copper electrodes.
J. Electroanal. Chem., 157 (1983) 359.
177. M. FLEISCHMANN, I.R. HILL, G. MENGOLI and M.M. MUSIANI.
The Synergetic effect of benzylamine on the corrosion inhibition of copper by benzotriazole.
Electrochim. Acta, 28, (1983), 1325.
178. M. FLEISCHMANN, P.R. GRAVES, I.R. HILL and J. ROBINSON
Simultaneous Raman spectroscopic and differential capacitance measurements of pyridine adsorbed on roughened silver electrodes.
Chem. Phys. Lett., 98, 503, (1983).
179. M. FLEISCHMANN, G. MENGOLI, M.M. MUSIANI, B. PELLI and I.R. HILL
The effect of Triton on the electropolymerisation of phenol; an investigation of the adhesion of coatings using Surface Enhanced Raman Scattering (SERS)
Electrochim. Acta, 28 (1983) 1733.

180. R.J. GALE, J. SEFAJA and M. FLEISCHMANN.
Modulated differential reflectance spectroscopy of lead dioxide films during growth.
Anal. Chem., 53 (1981) 1457.
181. M.Y. ABYANEH, M. FLEISCHMANN and M. LABRAM
Electrocrystallisation.
Proc. of symposium on Electrocrystallisation, Meeting of the Am. Electrochem. Soc., Hollywood, Florida, (1981).
182. D.E. WILLIAMS, C. WESTCOTT and M. FLEISCHMANN.
A statistical approach to the study of localized corrosion.
5th Int. Conf. on Passivity.
Bordeaux, France, (1983).
183. M. FLEISCHMANN and D. MASHEDER,
The electrochemical preparation of carbanions.
Part 1. Cyclic voltammetry.
Electrochim. Acta, in press.
184. M. FLEISCHMANN, D. MASHEDER and I. TABAKOVIC
The electrochemical preparation of carbanions.
Part 2. Mechanistic investigations.
Electrochim. Acta, in press.
185. M. FLEISCHMANN and D. MASHEDER,
The electrochemical preparation of carbanions.
Part 3. Synthetic experiments.
Electrochim. Acta, in press.
186. G. MENGOLI, M.M. MUSIANI, M. FLEISCHMANN and D. PLETCHER
Studies of pyrrole black electrodes as possible battery positive electrodes.
J. Appl. Electrochem., 14 (1983) 285.
187. A. SARABY-REINTJES and M. FLEISCHMANN,
Kinetics of electrodeposition of nickel from Watts Baths.
Electrochim. Acta, 29, (1984) 557
188. A.M. BOND, M. FLEISCHMANN and J. ROBINSON
The use of platinum microelectrodes for electrochemical investigations in low temperature solution-solid eutectic mixtures and glasses of non-aqueous solvents.
J. Electroanal. Chem., 180 (1984) 257.
- ✓ 189. L.M. ABRANTES, M. FLEISCHMANN, I.R. HILL, L.M. PETER
M. MENGOLI and G. ZOTTI.
An investigation of Cu^{I} acetylides films on Cu electrodes:
I. Electrochemical and Raman spectroscopic analysis of the film formation. *J. Electroanal. Chem.*, 164 (1984) 177.
- 4 190. M. FLEISCHMANN, F. LASSERRE and J. ROBINSON and D. SWAN
An investigation of coupled homogeneous reactions using microelectrodes. Part 1. EC' and CE reactions. *J. Electroanal. Chem.*, 177 (1984) 97.
- ✓ 191. M. FLEISCHMANN, F. LASSERRE and J. ROBINSON
An investigation of coupled homogeneous reactions using microelectrodes. Part 2. ECE and DISPl Reaction.
J. Electroanal. Chem., 177 (1984) 815.

192. A.M. BOND, M. FLEISCHMANN AND J. ROBINSON
Voltammetric measurements using microelectrodes in highly dilute electrolyte solutions: Theoretical considerations.
J. Electroanal. Chem. 172 (1984) 11-25
193. A.M. BOND, M. FLIESCHMANN AND J. ROBINSON
The use of platinum microelectrodes for electrochemical investigations in low temperature glasses of non-aqueous solvents.
J. Electroanal. Chem. 180 (1984) 257-263
194. A.M. BOND, M. FLEISCHMANN AND J. ROBINSON
Electrochemistry in organic solvents without supporting electrolyte using platinum microelectrodes.
J. Electroanal. Chem., 168 (1984) 299-312
195. D.E. WILLIAMS, C. WESTCOTT AND M. FLEISCHMANN
Studies of the initiation of pitting corrosion on stainless steel.
J. Electroanal. Chem. 180 (1984) 549.
196. A.M. BOND, M. FLEISCHMANN, S.B. KHOO, S. PONS AND J. ROBINSON
The construction and behaviour of ultramicroelectrodes: investigations of novel electrochemical systems.
J. Am. Electrochem. Soc.
197. M. FLEISCHMANN, P.R. GRAVES AND J. ROBINSON
The Raman spectroscopy of the ferricyanide/ferrocyanide systems at gold, β -palladium hydride and platinum electrodes.
J. Electroanal. Chem. 182, (1985) 87.
198. M. FLEISCHMANN, P.R. GRAVES AND J. ROBINSON
Enhanced and normal Raman scattering from pyridine adsorbed on rough and smooth silver electrodes
J. Electroanal. Chem. 182 (1985) 73.
199. M. FLEISCHMANN, P. GRAVES, I.R. HILL, J. ROBINSON AND A. OLIVER
The identification, energetics and structure of species in the electrode-solution interphase using SERS, DSERS, SUERS and INSEX
J. Am. Electrochem. Soc.
200. D.E. Williams, C. Westcott and M. Fleischmann. A statistical approach to the study of localised corrosion. 5th Inst. Conf. on Passivity. Bordeaux, France, (1983).
201. D.E. Williams, C. Westcott and M. Fleischmann. Stochastic Models of pitting corrosion on stainless steels. Part I. Modelling of the initiation and growth of pits at constant potential. J. Electrochem. Soc., submitted.
202. D.E. Williams, C. Westcott and M. Fleischmann. Stochastic Models of pitting corrosion on stainless steels. Part II. Measurement and interpretation of data at constant potential. J. Electrochem. Soc. submitted.
203. D.E. Williams, C. Westcott and M. Fleischmann. A stochastic Approach to the study of localized corrosion.

204. M. Fleischmann, S. Bandyopadhyay and S. Pons,
The behaviour of Microring Electrodes.
J. Phys. Chem., 89, (1985) 5537-5541.
205. M. Fleischmann, J. Ghoroghchian and S. Pons.
Electrochemical behaviour of dispersions of spherical
ultramicroelectrodes. 1. Theoretical considerations.
J. Phys. Chem., 89 (1985) 5530.
206. M. Fleischmann, G. Mengoli, M.M. Musiani and C. Pagura,
An electrochemical and Raman spectroscopic investigation
of synergetic effects in the corrosion inhibition of copper.
Electrochim. Acta, 30 (1985) 1591.
207. M. Fleischmann, I.R. Hill, G. Mengoli, M.M. Musiani and
J. Akhavan.
A comparative study of the efficiency of some organic inhibitors
for the corrosion of copper in aqueous chloride media using
electrochemical and surface enhanced Raman scattering
techniques. Electrochimica Acta, 30 (1985) 879.
- 208/ M.M. Musiani, G. Mengoli, M. Fleischmann and B.B. Lowry.
An electrochemical and SERS investigation of the influence of
pH on the effectiveness of some corrosion inhibitors of copper.
Submitted to Electrochim. Acta.
209. G. Zotti, S. Cattarin, G. Mengoli, M. Fleischmann and L.M. Peter
Photoelectrochemistry of copper(I) acetylide films electrodeposited
onto copper electrodes.
J. Electroanal Chem., 200 (1986) 341-351
- 210 A.J. Bloom, M. Fleischmann and J.M. Mellor
Nitroacetamidation of styrenes
Perkin Trans. 1 1984, pp.2357-2363
- 211 J. Cassidy, S.B. Khoo, S. Pons and M. Fleischmann.
Electrochemistry at very high potentials: the use of ultramicroelectrodes
in the anodic oxidation of short chain alkanes.
J. Phys. Chem. 89, 1985, 3933.
- 212 M. Fleischmann, G. Sundholm and Z.Q. Tian
An SERS study of silver electrodeposition from thiourea and
cyanide containing solutions.
Electrochim. Acta 31 (1986) 907-916.
- 213 M. Fleischmann, A. Oliver and J. Robinson
In situ X-ray diffraction studies of electrode solution
interfaces, Electrochim. Acta 31 pp. 899-906 (1986).
- 214 A.J. Bloom, M. Fleischmann and J.M. Mellor
Nitration Procedures using electrogenerated reagents
Electrochimica Acta, 32 (1987) 785-790

1980 JUL 33 53 17 01

Revised Budget



28 February 1989

AIRBORNE

RYSZARD GAJEWSKI
U S DEPARTMENT OF ENERGY
ACQUISITION & ASSISTANCE MANAGEMENT DIVISION
OFFICE OF ENERGY RESEARCH ER64 DOE
19901 GERMANTOWN ROAD
GERMANTOWN MD 20874

SUBJECT: Revised Budget
University of Utah PID No. 8808032

Dear Mr. Gajewski:

We have enclosed an original and five (5) copies of the budget revision which was sent to you by FAX machine on 27 February 1989. This revision is for the project entitled "THE BEHAVIOR OF ELECTROCHEMICALLY COMPRESSED HYDROGEN AND DEUTERIUM" under the direction of Dr. B. Stanley Pons, Department of Chemistry. These documents have been signed by an authorized official of the University of Utah.

This revision increases the amount requested for the first 18 months to \$322,462.

We appreciate your consideration of this proposal.

Very truly yours,

Richard H. Timpson
Director
Sponsored Projects

kb

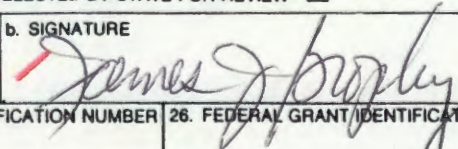
Enclosures: 6

cy: Dr. B. Stanley Pons
Dr. Hugo Rossi, Dean

Office of Sponsored Projects

309 Park Building
Salt Lake City, Utah 84112
(801) 581-6903

Fax Machine (801) 581-3007

FEDERAL ASSISTANCE		2. APPLICANT'S APPLICATION IDENTIFIER		a. NUMBER		3. STATE APPLICATION IDENTIFIER		a. NUMBER	
1. TYPE OF SUBMISSION (Mark appropriate box) <input type="checkbox"/> NOTICE OF INTENT (OPTIONAL) <input type="checkbox"/> PREAPPLICATION <input checked="" type="checkbox"/> APPLICATION		b. DATE Year month day 19 89 2 27		NOTE: TO BE ASSIGNED BY STATE		b. DATE ASSIGNED Year month day 19		Leave Blank	
4. LEGAL APPLICANT/RECIPIENT						5. EMPLOYER IDENTIFICATION NUMBER (EIN) 187600525A1			
a. Applicant Name University of Utah b. Organization Unit Stanley Pons c. Street/P.O. Box Department of Chemistry d. City Salt Lake City e. County Salt Lake f. State Utah g. ZIP Code 84112 h. Contact Person (Name & Telephone No.) Stanley Pons (801) 581-4760						6. PRO-GRAM (From CFDA) a. NUMBER 81049 MULTIPLE <input type="checkbox"/> b. TITLE Basic Energy Sciences			
7. TITLE OF APPLICANT'S PROJECT (Use section IV of this form to provide a summary description of the project.) The Behavior of Electrochemically Compressed Hydrogen and Deuterium						8. TYPE OF APPLICANT/RECIPIENT A-State B-Interstate C-Substate D-County E-City F-School District G-Special Purpose District H-Community Action Agency I-Higher Education Institution J-Indian Tribe K-Other (Specify): Enter appropriate letter I			
9. AREA OF PROJECT IMPACT (Names of cities, counties, states, etc.) Salt Lake City				10. ESTIMATED NUMBER OF PERSONS BENEFITING 5		11. TYPE OF ASSISTANCE A-Basic Grant B-Supplemental Grant C-Loan D-Insurance E-Other Enter appropriate letter(s) A			
12. PROPOSED FUNDING		13. CONGRESSIONAL DISTRICTS OF:				14. TYPE OF APPLICATION			
a. FEDERAL \$ 656,162.00 b. APPLICANT 56,000.00 c. STATE .00 d. LOCAL .00 e. OTHER .00 f. Total \$ 712,162.00		a. APPLICANT Utah 2nd b. PROJECT Utah 2nd 15. PROJECT START DATE Year month day 19 88 10 1 16. PROJECT DURATION 36 Months 18. DATE DUE TO FEDERAL AGENCY Year month day 19 89 2 27				A-New B-Renewal C-Revision D-Continuation E-Augmentation Enter appropriate letter A 17. TYPE OF CHANGE (For 14c or 14e) A-Increase Dollars B-Decrease Dollars C-Increase Duration D-Decrease Duration E-Cancellation F-Other (Specify): Enter appropriate letter(s)			
19. FEDERAL AGENCY TO RECEIVE REQUEST DOE Office of Energy Research						20. EXISTING FEDERAL GRANT IDENTIFICATION NUMBER			
a. ORGANIZATIONAL UNIT (IF APPROPRIATE) Office of Energy Research b. ADMINISTRATIVE CONTACT (IF KNOWN) Ryszard Gajewski						21. REMARKS ADDED <input type="checkbox"/> Yes <input checked="" type="checkbox"/> No			
c. ADDRESS Acquisition and Assistance Management Division Office of Energy Research ER64 DOE, Washington, DC 20545									
22. THE APPLICANT CERTIFIES THAT		To the best of my knowledge and belief, data in this preapplication/application are true and correct, the document has been duly authorized by the governing body of the applicant and the applicant will comply with the attached assurances if the assistance is approved.				a. YES, THIS NOTICE OF INTENT/PREAPPLICATION/APPLICATION WAS MADE AVAILABLE TO THE STATE EXECUTIVE ORDER 12372 PROCESS FOR REVIEW ON: DATE b. NO, PROGRAM IS NOT COVERED BY E.O. 12372 OR PROGRAM HAS NOT BEEN SELECTED BY STATE FOR REVIEW			
23. CERTIFYING REPRESENTATIVE		a. TYPED NAME AND TITLE James J. Brophy Vice President for Research				b. SIGNATURE 			
24. APPLICATION RECEIVED 19		25. FEDERAL APPLICATION IDENTIFICATION NUMBER		26. FEDERAL GRANT IDENTIFICATION					
27. ACTION TAKEN		28. FUNDING		29. ACTION DATE		30. STARTING DATE		31. CONTACT FOR ADDITIONAL INFORMATION (Name and telephone number)	
<input type="checkbox"/> a. AWARDED <input type="checkbox"/> b. REJECTED <input type="checkbox"/> c. RETURNED FOR AMENDMENT <input type="checkbox"/> d. RETURNED FOR E.O. 12372 SUBMISSION BY APPLICANT TO STATE <input type="checkbox"/> e. DEFERRED <input type="checkbox"/> f. WITHDRAWN		a. FEDERAL \$.00 b. APPLICANT .00 c. STATE .00 d. LOCAL .00 e. OTHER .00 f. TOTAL \$.00		19		19		19	
								32. ENDING DATE 19	
								33. REMARKS ADDED <input type="checkbox"/> Yes <input type="checkbox"/> No	

ER F 4620.1
(7-85)

U.S. Department of Energy
Grant Application Budget Period Summary
(See Reverse for Definitions and Instructions)

OMB Approval
No. 1910-1400

Please Print or Type

Organization:	Period Covering:	FOR DOE USE ONLY
University of Utah	From: 10/1/88	Proposal No:
Principal Investigator (P.I.)/Project Director (P.D.): Stanley Pons	To: 3/31/90	Award No.:
A. SENIOR PERSONNEL P/VPD Co PIs, Faculty and Other Senior Associates (List each separately with title, A.6 show number in brackets. Attach separate sheet, if required.)	DOE Funded Person-Mos.	Funds Requested By Applicant
	Cal. Acad. Sumr.	\$
1. Martin Fleischmann, Co PI, Professor	6	25,000
2. No Employee Benefits/From England		
3.		
4.		
5.		
6. (1) TOTAL SENIOR PERSONNEL		
B. OTHER PERSONNEL (SHOW NUMBERS IN BRACKETS)		
1. (1) POST DOCTORAL ASSOCIATES	18	30,000
2. (1) OTHER PROFESSIONALS (TECHNICIAN, PROGRAMMER, ETC.)	18	27,000
3. (2) GRADUATE STUDENTS	18	31,000
4. () UNDERGRADUATE STUDENTS		
5. () SECRETARIAL-CLERICAL		
6. () OTHER		
TOTAL SALARIES AND WAGES (A + B)		113,000
C. FRINGE BENEFITS (IF CHARGED AS DIRECT COSTS) Postdoc 14%; Tech. 30%; Grad. St. 8%		14,780
TOTAL SALARIES, WAGES AND FRINGE BENEFITS (A + B + C)		127,780
D. EQUIPMENT (LIST ITEM AND DOLLAR AMOUNT FOR EACH ITEM) Calorimeter for temp. msmt.		28,500
3 Potentiostat-galvanostat-power for cells		17,940
1 Waveform generator for potential programs		5,790
Temp transducers, PC for control and recording		11,000
TOTAL EQUIPMENT		63,230
E. TRAVEL 1. DOMESTIC (INCL. CANADA AND U.S. POSSESSIONS)		
2. FOREIGN Fleischmann to Utah to participate in work		4,000
F. OTHER DIRECT COSTS		
1. MATERIALS AND SUPPLIES Rods, heavy water, other metals		73,750
2. PUBLICATION COSTS/PAGE CHARGES		750
3. CONSULTANT SERVICES		
4. COMPUTER (ADPE) SERVICES		
5. CONTRACTS AND SUBGRANTS		
6. OTHER		
TOTAL OTHER DIRECT COSTS		74,500
G. TOTAL DIRECT COSTS (A THROUGH F)		269,510
H. INDIRECT COSTS (SPECIFY RATE AND BASE) 47% Direct costs, except equipment		
TOTAL INDIRECT COSTS		96,952
I. TOTAL DIRECT AND INDIRECT COSTS (G & H)		366,462
J. APPLICANT'S COST SHARING (IF ANY)		44,000
K. TOTAL AMOUNT OF THIS REQUEST (ITEM I LESS ITEM J)		322,462
P/VPD TYPED NAME & SIGNATURE Stanley Pons	DATE 2/27/89	
INST. REP. TYPED NAME & SIGNATURE JAMES J. BROPHY Vice President For Research	DATE FEB 27 1989	

Equipment Justification

Potentiostat-galvanostats are instruments used for accurately controlling the potential and/or current applied between the electrodes in the cell. There will be several of these operating at the same time, and for extended periods of time. We have requested three of these to control three cells simultaneously. Suitable instruments cost \$5,980 each.

The calorimeter setup requested will consist of glass evacuated dewar type cells to contain the rods, counter electrodes, and solutions; two constant temperature baths to hold the cells; accurate thermistors and voltmeters to monitor relative changes in the temperature of the cells and bath; and a scintillation counter to monitor the changes in the tritium content of the dewars. The cost of these components is \$28,500.

A waveform generator is requested to drive the potentiostats above when applying potential programs to the experiments. This device will be required for experiments dealing with the determination of the heavy water equivalent of each cell. These experiments require precise timing of applied voltage/current levels. The cost for a suitable instrument is \$5,790.

A personal computer is requested for recording of the various variables in the experiments: cell current, cell voltage, applied voltage, bath temperature, dewar temperature, and scintillation counts for blanks, controls, and dewars. The device will also be used for calculation and plotting of the cooling curves and thermal equivalents, as well as general calculations. Interfaces for the various transducers (A/D converters; suitable bus configuration), extended memory, large hard disk, and a printer output are required. The components cost \$11,000.

Travel Justification

Professor Fleischmann intends to travel from the University of Southampton, Southampton, UK, to London to Salt Lake, and return, three times during the first eighteen months of the project. Travel to London return is under \$100, and airfare (return) from Gatwick to Salt Lake either on Delta or British Air-Continental is estimated at \$1200. Professor Pons will be responsible for local expenses in Salt Lake City.

Materials and Supplies

The metal rod electrodes are to be purchased in 10 to 20 cm lengths and in diameters from 1 to 30 mm. High purity metals are required. High purity deuterium oxide is used as the solvent and fuel. We estimate that we will require 20 kG during the first year. Platinum wire will be used as the counter electrode in each cell. Each cell requires approximately 5 feet of wire. In addition, there will be Pt supports, framing, and wire necessary for the neutron counting experiment, as well as a 50 x 50 x 1 mm Pd sheet as the working electrode. The costs for primary electrode metals will be \$51,000, platinum \$7,750, and deuterium oxide \$15,000.

U.S. Department of Energy
GRANT APPLICATION
PROJECT PERIOD SUMMARY

(Must be completed for all new and renewal applications.)

Please Print or Type

OMB Approval
No. 1910-1400

Categories	01 Budget Period	02 Budget Period	03 Budget Period	04 Budget Period	05 Budget Period
A. Senior Personnel Totals	25,000	29,000			
B. Other Personnel Totals	88,000	92,000			
C. Fringe Benefit Totals	14,780	15,660			
Total of A, B & C	127,780	136,660			
D. Equipment	63,230	22,800			
E. Travel 1. Domestic					
2. Foreign	4,000	5,000			
F. Other Direct Costs	74,500	78,000			
G. Total Direct Costs	269,510	242,460			
H. Total Indirect Costs	96,952	103,240			
I. Total Direct & Indirect Costs	366,462	345,700			
J. Applicant's Cost-Sharing (if any)	44,000	12,000			
K. Total Amount of Request (Item I. Less Item J.)	(1)* 322,462	(2) 333,700	(3)	(4)	(5)

*This should equal Item K on Budget Period Summary (ER/F/4620.1)

ESTIMATE

TOTAL COST OF PROJECT

\$ 656,162

(add K(1) thru (5))

REVIEW OF PROPOSAL: "The Behavior of Electrochemically Compressed Hydrogen and Deuterium", by S. Pons and M. Fleischmann

COMMENTS ON THE PROPOSAL

1) Statements such as "the resulting calculated pressure is on the order of the measured rise in chemical potential, approximately 10^{27} atmospheres" (page 2) demand support: where are the calculations? In general, theoretical calculations are strikingly absent in the proposal.

2) The authors tantalizingly claim an "increase in the background radiation count in the lab" (page 6) during an experiment, suggesting the occurrence of nuclear fusion. What kind of radiation was observed? How was the radiation detected? Was the radiation consistent in type and energy with p-d or d-d fusion? These points should appropriately be addressed to permit evaluation of the merits of the proposal.

3) The proposed work includes "radiation measurements" (page 10). Unfortunately, the method of making these measurements is not discussed although it is central to the investigation, since detecting neutrons and/or gamma radiation of the proper energy would be a clean signature for fusion reactions.

4) If significant radiation is anticipated in the research, safety measures must certainly be elaborated.

5) If a paucity of theoretical justification and information on radiation is a weakness in the proposal, certainly the electrochemical/calorimetric approach is amply defined and explained. The researchers appear to be well-qualified in this area.

6) "We believe that the results we have obtained so far are a strong indication of a progressive increase in the fusion of D nuclei in the Pd-lattice with increasing chemical potential (= compression). While there are alternative explanations of the excess heating effects, their possibility does not seem to be very likely." (p. 6) Please, what are the other explanations and why are they unlikely?

7) "The experiments will take longer than our previous experiments in view of the greater thickness of the rods compared to the sheet electrodes. It will take approximately 12 months to charge a 2cm diameter rod to saturation with deuterium." (p. 7) Could not the time required be drastically reduced by heating the rod in a pressurized deuterium environment?

8) Since no references are cited, one wonders if a thorough

literature has been done. In particular, publications by C. Van Siclen and S. E. Jones (J. Phys. G, 12 (1986) 213-221) and by B. A. Mamyrin and I. N. Tolstikhin (Developments in Geochemistry 3: Helium Isotopes in Nature, New York: Elsevier, 1984) could be relevant.

In conclusion, I find the proposed research to be very intriguing and consistent with the direction of the Advanced Energy Projects Division. The personnel are evidently well-qualified and competent in electrochemical techniques. However, the proposal has a number of weak areas as delineated above that should perhaps be addressed.

Statement regarding my review of the proposal: "The Behavior of Electrochemically Compressed Hydrogen and Deuterium," by S. Pons and M. Fleischmann

I have made every effort to be objective and thorough in reviewing the proposal described above. I must make it clear, however, that I have been doing research in the subject area, which I call piezonuclear fusion, since 1985. Our research group at Brigham Young University is using neutron and gamma radiation detection techniques, along with measurements of helium-3 / helium-4 ratios (which will be performed on our samples by Alfred Nier of the University of Minnesota). We load hydrogen and deuterium into metal strips using electrochemical means as well as by heating the metal in a pressurized hydrogen-deuterium environment. We began experimental research in this area in Spring 1986 as an offshoot of our cold nuclear fusion research program supported by the Advanced Energy Projects Division of the Department of Energy. The work was discussed with Dr. Gajewski in this time period and was formally reported in our 1985-1986 Annual Report to the DOE (see attachments).

The roots of our work in this area may be traced to my efforts in 1985 to enhance fusion in isotopic hydrogen molecules without the use of short-lived muons. Early work on this was published in the paper: "Piezonuclear fusion in isotopic hydrogen molecules," by Clinton Van Siclen (who performed the detailed calculations) and myself in Journal of Physics G: Nuclear Physics, 12: 213 (1986, paper received 12 June 1985). In addition to initiating the study, I coined the term "piezonuclear fusion" in analogy to the term "thermonuclear fusion", to indicate that our approach is to induce fusion by "squeezing" the hydrogen nuclei together rather than by heating them to very high temperatures. (The idea is to reduce the width of the Coulomb potential barrier and thereby to enhance barrier penetration leading to nuclear fusion.) It later occurred to Prof. Palmer and myself in discussions at BYU in March 1986 that this end might be achievable by loading hydrogen isotopes into minerals (in particular into metals), leading to the current study. We were totally unaware of any work on this concept by Dr. Pons, Dr. Fleischmann or indeed of anyone else at this time. (Prof. Johann Rafelski had suggested the possibility of slow fusion in gaseous HD molecules in December 1985, but the Van Siclen/Jones paper indicated that this would be exceedingly slow. Prof. Rafelski became very intrigued by our idea of piezonuclear fusion of hydrogen isotopes in metal lattices when we told him about it; he is trying to establish a theoretical basis for calculating rates for this effect.) In doing a literature search, we subsequently found that B. A. Mamyrin, L.V. Khabarin and V. S. Yudenich had mentioned the possibility of hydrogen fusion occurring in metal foils in their paper "Anomalous High Isotope Ratio $^3\text{He}/^4\text{He}$ in Technical-Grade Metals and Semiconductors," Dokl. Akad. Nauk. SSSR, 237:1054 (1978), but they had no proof that fusion was occurring. We have found no further publications by these scientists on this subject, except for a reference to this short paper in Mamyrin's book "Helium Isotopes in Nature,"

New York: Elsevier, 1984.

I feel that Pons' proposed work nicely complements the ongoing cold fusion research previously initiated by us with the support of the Advanced Energy Projects Division of the Department of Energy.

Steven E. Jones
9/30/88

November 18, 1988

Professor Steven E. Jones
Department of Physics and
Astronomy
Brigham Young University
Provo, Utah 84602

Dear Steve:

Your review of the Pons/Fleischmann proposal, "The Behavior of Electro-chemically Compressed Hydrogen and Deuterium," has been forwarded to the authors for a rebuttal. Their response is enclosed. In the correspondence, you are being referred to as Reviewer #1.

It will help us in deciding whether or not to support the proposal if you could provide us with your comments on the rebuttal. Do you believe, based on the totality of the arguments offered in the proposal and in the rebuttal, the proposed project should be supported?

Your response, by return mail if possible, will be greatly appreciated.

Sincerely,

Ryszard Gajewski, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

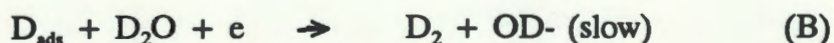
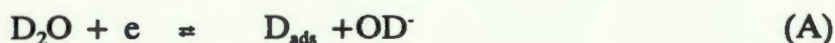
Enclosures

Reply to Reviewer #1:

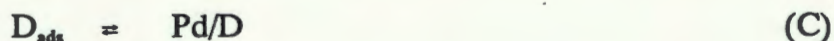
We will reply to the reviewer using the numbering of his paragraphs.

(1) The statement on page 2 of our proposal was merely intended to illustrate that IF the expression (particle density x temperature x volume x lifetime) applies to our system and if the chemical potential of the dissolved D in the lattice is converted into an equivalent pressure, then it is not unreasonable to expect significant fusion processes to take place.

The reviewer should note that the processes at the surface of the Pd electrode are



Because of the slowness of reaction step (B) the chemical potential of the adsorbed D is raised by the electrode potential difference at the interface and, as the adsorbed D is in equilibrium with D in the lattice



the chemical potential of the dissolved D is in turn raised to the value corresponding to the applied overpotential. If one wanted to raise the chemical potential by increasing the pressure of D_2 (and if step (B) could be made to go to equilibrium) then one would require a pressure given by

$$RT/2F \ln [P_{D_2}] = 0.8 \text{ Volt}$$

i.e., about 10^{27} atmospheres. Such a pressure clearly cannot be achieved on earth but it is a simple matter to raise the chemical potential of D in the lattice by applying an appropriate potential to the electrode. This is the substance of our proposal.

(2) An increase of (beta + gamma) radiation was detected in the vicinity of the experiment. The measurement was made with a Mini-Monitor 442 sandwich GM-scintillation type counter. The background count in the laboratory, and in adjacent laboratories measured with this meter had remained at 175 counts per minute prior to the last day of experimentation when the rate rose to 256 counts per minute near the Dewar. The rate at remote parts of the lab and in the adjacent labs remained normal. To our knowledge, no radioactive materials had been brought into the lab. This increase must presumably be attributed to the reactions of thermal neutrons with components of the Dewar. This is a complication which we would clearly have wanted to avoid! Please also see reply to (3).

(3) The main methods to be used will be as follows: (a) detection of any tritium generated by the reactions and correlation of the rate of generation of tritium with the excess energy production. Samples will be withdrawn and analyzed using scintillation counting equipment. (b) Detection of thermal neutrons and use of energy discriminative gamma-ray analysis. The reviewer should note that under the conditions of our experiment neutrons will be rapidly thermalized in the palladium rod (indeed the experiment was designed with this in view for safety reasons) so that it is not possible to correlate the energy of any neutrons produced with any particular nuclear reaction. Our strategy therefore will be to detect thermalized neutrons and in particular the gamma radiation generated by the reaction of these neutrons with species present in the Dewar (the electrodes, electrolyte and components of the borosilicate glass).

To be more specific, we will initially use the simplest possible means to search for thermalized neutrons. For example, we may compare results for potassium deuteroxide electrolyte with those for potassium borate using photographic plates as a detection medium. Gamma rays will be detected using sodium iodide crystals for low resolution measurements; if necessary we will use intrinsic germanium detectors.

(4) The reviewer should note that this is why we terminated our experiments. If this project is funded, then one of our first objectives will be the quantification of any radiation produced and all appropriate steps to contain and shield the experiment will be taken. The Department is well equipped with radiation-safe laboratories and various forms of radiation counting equipment. Samples will be monitored daily with scintillation counters, and the apparatus with Geiger-Müller counters. In the case of obvious generation of radiation, we plan to reassemble the experiment in laboratories containing equipment suitable for discriminating the energies of gamma rays and equipment for detection of thermalized neutrons (see also reply to (3) above). We are thoroughly familiar with the rules and regulations of our University Radiation Safety committee, and have discussed with them their requirements for radiation experiments in our laboratories. The reviewer will wish to know that we have informed the Vice President for Research at this University (a well-known physicist) of our plans.

The reviewer will wish to note that if we are correct in assigning the excess energy to a fusion process, then the source would be classified as one of low energy. We intend to keep the experiments in this category. Thus if we get a marked increase in the excess energy with change of the system parameters (overpotential), bath temperature, rod dimensions, poisoning conditions) then we will scale down the experiment appropriately (thinner and shorter rods).

See also last paragraph of our reply to question (7).

(6) The main alternative explanations for excess enthalpy generation are:

(i) generation of D_2 at voids in the lattice (see also comments by reviewer #5). However, if this explanation applies, the excess energy generated during 331 hours of polarization at the highest current density would have required formation of D_2 bubbles at a higher rate than that corresponding to the applied current, i.e., there would have been a loss of dissolved D. Such a loss is inconsistent with the observation of the generation of a constant excess enthalpy during three successive periods of 75, 155, and 101 hours.

Moreover, at least 0.5 cm^3 of bubbles at 2000 atmospheres (the tensile strength of Pd) would have been formed which would almost certainly have disintegrated our sample of Pd. The structural integrity of the sample was preserved and, indeed, it is well known that electrochemical equivalents of Pd diffusion tubes can be used indefinitely. The easiest way to discount this possibility of bubble formation is to increase the experiment times. However, we do have it in mind to search for any D_2 or, more likely, He bubbles.

(ii) Participation of the reduction of O_2 and/or ionization of D_2 i.e. a shift off the Joule heating term towards the upper bound. However, our experiments showed that the Joule heating exactly balanced the Newton's law cooling at low current densities (where the effects of any O_2 reduction on D_2 ionization should have been at a maximum) while the excess enthalpy increased with the current density. Such behavior (as well as the other points we have set out in the application) is not consistent with the participation of O_2 reduction/ D_2 ionization.

The reviewer may also like to know that in an earlier series of experiments periodic catalytic contamination of the Pd surface led to loss of dissolved D which was associated with cooling not heating presumably because of the cessation of the fusion process.

(7) We have considered doing this but unfortunately it would not reduce the experiment time. The important point is that the high chemical potential of dissolved D is established by diffusion so that one cannot "beat" the diffusional relaxation time.

We have also considered an electrochemical variant of the reviewer's suggestion, namely, the electrochemical saturation of Pd by polarization at a high temperature and subsequent cooling. As the dissolution of D in Pd is endothermic, this would produce even higher chemical potentials of the dissolved D! We do not wish to do this in our initial experimental experiments as the expulsion of excess D from the lattice on subsequent cooling would lead to spurious excess enthalpy generation (but see our comment above). The reviewer may wish to note that if we can prove that the concept works, then we intend to saturate rods at high temperature and to try to find suitable diffusion barriers. This would in effect produce Pd-D "hot rods".

The considerations set out in the above paragraph are also important to the safety of this project which has been referred to by some of the other reviewers.

As the dissolution of deuterium is endothermic, a marked rise in temperature of the rods will lower the chemical potential of the deuterium and will therefore self limit any fusion process.

(8) We have not yet read these references, but have ordered them and will do so as soon as possible. We would welcome any other useful references the reviewer may be able to supply. We have read similar documents and have not found information pertinent to this work.

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BRIGHAM YOUNG UNIVERSITY, PROVO, UTAH
Communication Systems (801) 378-7311

TO: Rysford Gajenski
ER-16
DOE

() FACSIMILE NUMBER

Country Code:

City Code:

Number: 301-353-3870

FROM: S E Jones
BYU

COMMENTS:

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<u>Linda</u>			
<u>9/10/85</u>			
<u>nn</u>			

Phone (801) 378-2749

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Further Comments on the Proposal: "The Behavior of Electrochemically Compressed Hydrogen and Deuterium"

Frankly, I was disappointed by the response to my original comments on this proposal. The contention that neutrons from fusion will be "rapidly thermalized" and that an "increase of (beta + gamma) radiation ... must presumably be attributed to the reactions of thermal neutrons with components of the Dewar" indicates, I fear, a lack of understanding of the penetrating power of 2.5 MeV neutrons, and of nuclear reactions in general. For example, energetic neutrons are much more penetrating than beta particles of comparable energy, and fusion neutrons are not difficult to detect. (There are numerous papers on this subject in papers on muon-catalyzed fusion, for instance.) And why are not gammas from proton-deuteron fusion considered? Furthermore, a background rate of 175 counts per minute in a small scintillation counter points to a dearth of shielding and a rather cavalier attitude toward detecting radiation associated with nuclear fusion. I also feel strongly that jumping from current results to experiments involving large and expensive palladium rods, requiring "about one year to charge" with deuterium, would be premature. First, smaller scale experiments of an exploratory nature are clearly needed to establish the phenomenon of fusion in metals.

However, in spite of these glaring defects, I do not recommend that all support for this project be denied. I find that the proposers have demonstrated expertise with electrochemistry and calorimetric methods. Although the proposed experiments clearly fail to demonstrate the existence of fusion processes in metals, there indeed exists some evidence that such does occur.

I think the proposers should be informed that exploratory research on fusion in metals (and other compounds) has been pursued under the auspices of the Advanced Energy Projects Division since 1985. (See our annual report dated May, 1986.) Our initial interest in the possibility of fusion in minerals stemmed from our related work on muon-catalyzed fusion in which fusion is induced as isotopic hydrogen nuclei are held closely together by a negative muon, and the correlation of this research with observations of anomalously large heat and helium-3/helium-4 ratios associated with earth's geology. We realized both could be explained by the occurrence of proton-deuteron and/or deuteron-deuteron fusion in the earth. (In particular, water is entrained in minerals in subducting zones, where excess helium-3 relative to helium-4 is common. Internal Brigham Young University reports by Profs. S.E. Jones and E.P. Palmer dated March-April 1986 discuss our early thoughts on this process. We now call the alleged process "piezonuclear fusion" in contradistinction to thermonuclear fusion, or "metal-catalyzed fusion" by analogy to muon-catalyzed fusion.) In discussing our idea with geochemists (H. Craig and A. Nier), we learned that they had seen inexplicable excess helium-3/helium-4 ratios in a number of minerals—they were considerably intrigued by our possible explanation, which they had never before heard of. Finally, we uncovered a paper by Mamyrin, Khabarin and Yudenich which formally reports the occurrence of high helium-3/helium-4 in metals and semiconductors (Sov. Phys. Dokl. 23:581 (1978)). Since then, our research has accelerated. We have looked for p-d and d-d fusion in a number of compounds, including palladium foils, under various conditions since Spring 1986. Our methods involve both neutron and gamma detectors, followed by measuring helium-3/helium-4 ratios. It would not be appropriate to discuss our results here. However, there is enough evidence to warrant further studies, in my view.

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P. 03

The subject proposal approaches the measurement with calorimetric methods, which complements our methods outlined above. I think there is room for the proposed work in addition to the ongoing effort and would encourage funding. Indeed, I recommend a joint effort, with cooperation between the presently-funded project and the complementary work now being proposed. Such a joint effort would be facilitated by the close proximity of two of the universities involved (Brigham Young and Utah).

10/04/88 17:06

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COLLEGE OF ARTS AND SCIENCES

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Reviewer #2

Dr. R. Gajewski
Department of Energy
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16
Washington, DC 20545

October 4, 1988

Dear Dr. Gajewski:

I have carefully studied the proposal submitted by Dr. S. Pons from the University of Utah entitled "The Behavior of Electrochemically Compressed Hydrogen and Deuterium". I am responding as a referee specialized in Nuclear and Particle Physics, and will not comment at the matters related to electrochemical analysis. However I wish to mention that the proposal, even though it refers to pilot experiments, never does clearly commit the author to a certain result.

The proposal addresses the issue pertinent to spontaneous fusion of hydrogen isotopes placed inside a metal lattice. The method of experimental approach selected here is to study excess heat generated by fusion energy. I support in principle the study of the general issue raised in this proposal, but have very grave doubts about the method selected, in particular I am concerned, if it is sufficiently sensitive to find a new effect not formerly observed in an incidental way by nuclear detection methods (fusion neutrons etc).

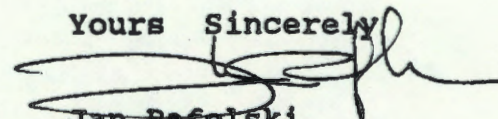
Since the energy gain from fusion is 10^7 times greater than the chemical energy gain, this method would work if fusion rates are some good fraction, say 10^{-10} of the chemical reaction rates. This implies in turn that fusion rates at the level of $10^{-16}/s$ may be detectable by this method. What is indeed badly missing in the proposal is a more accurate back of the envelope estimate how a hypothetical fusion rate relates to the excess heat and which range of fusion rates would be accessible to measurement in the proposed set up, considering the usual uncertainties of the method. Without such a discussion of this question it is in my judgement impossible to evaluate the chances of success for the proposed work, since we do not know how the expected result would show in other physical environments.

Neither does the proposal indicate what one does if the effect one is looking for, excess heat, is actually found! One can not simply claim "eureka, fusion" . There are many other sources of energy in a complex system considered for this investigation, and there is no attempt made to identify the source of heat. I do not recommend that the funding for this project be based on the present submission. I would like to reserve my final recommendation until I see an addendum or a new proposal in which two matters are put straight:

- 1: which range of fusion rates is measurable in the proposed set up;
- 2: how will the decision be made that any energy excess is of nuclear origin.

I trust that this report will be of help. Given the postal delay incurred on reception of your mail in Arizona, I am forwarding an advance copy of this report by FAX.

Yours Sincerely



Jan Rafelski
Professor of Physics

November 18, 1988

Professor Johann Rafelski
Department of Physics
University of Arizona
Tucson, AZ 85721

Dear Jan:

Your review of the Pons/Fleischmann proposal, "The Behavior of Electro-chemically Compressed Hydrogen and Deuterium," has been forwarded to the authors for a rebuttal. Their response is enclosed. In the correspondence, you are being referred to as Reviewer #2.

It will help us in deciding whether or not to support the proposal if you could provide us with your comments on the rebuttal. Do you believe, based on the totality of the arguments offered in the proposal and in the rebuttal, the proposed project should be supported?

Your response, by return mail if possible, will be greatly appreciated.

Sincerely,

Ryszard Gajewski, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

Enclosures

Reply to reviewer #2

We will reply to the reviewer's comments paragraph by paragraph.

#1 We are at a loss to know how the reviewer can make this statement. How much more specific can we be than to say that we had ca. 25% excess energy produced at the highest current density? The reviewer may wish to know that we observed this excess energy in three runs of 75, 155 and 101 hours.

#2 We believe that such effects were not observed previously because physical chemists and physicists simply do not set up experiments of several thousand hours duration to look for small calorimetric effects. A short duration experiment would also not give any detectable radiation.

#3 We would like to assure the reviewer that we have carried out many back-of-the-envelope calculations. Our own calculations showed that fusion rates of the order $3 \times 10^{-16} \text{ s}^{-1}$ would be readily detectable by the methods we have outlined. With special precautions and cell design, rates as low as $3 \times 10^{-17} \text{ s}^{-1}$ (or even $3 \times 10^{-18} \text{ s}^{-1}$) might be detectable. The fusion rate (if indeed it was that) in our experiments at the highest current densities was about $3 \times 10^{-14} \text{ s}^{-1}$. It is a straightforward matter to confirm these figures taking into account the likely Newton's law of cooling for Dewars, and the temperature differences between the inside of the Dewar and the surrounding water bath readily accessible to measurement. Further, it is our opinion that any meaningful calculations such as those proposed by the reviewer at a minimum would require a detailed quantum-mechanical molecular dynamical calculation; we have talked extensively with several of our colleagues (expert in these types of studies) regarding such a calculation. They have evidently not been made successfully in the past, and would require a major research-computing effort. We would hope to take on (or see others do so) such a project after the experimental verification has been made. We agree that it is difficult to evaluate the chance for success of this work, but we must also question the applicability of the proposed calculations in making such an evaluation easier.

#4 Our reply to the question #6 of the first reviewer and paragraph (3) of the third reviewer are relevant to this comment by the present reviewer and are attached.

1: We have replied to this under #3 above.

2: As we have pointed out in the proposal, we shall seek to correlate any excess energy released with tritium produced; we shall look for thermalized neutrons and for gamma-rays generated by any reactions of these thermalized neutrons with components of the Dewar etc.

Question (6) of Reviewer #1:

"We believe that the results we have obtained so far are a strong indication of a progressive increase in the fusion of D nuclei in the Pd-lattice with increasing chemical potential (= compression). While there are alternative explanations of the excess heating effects, their possibility does not seem to be very likely." (p. 6) Please, what are the other explanations and why are they unlikely?

Our reply:

(6) The main alternative explanations for excess enthalpy generation are:

(i) generation of D_2 at voids in the lattice (see also comments by reviewer #5). However, if this explanation applies, the excess energy generated during 331 hours of polarization at the highest current density would have required formation of D_2 bubbles at a higher rate than that corresponding to the applied current, i.e., there would have been a loss of dissolved D. Such a loss is inconsistent with the observation of the generation of a constant excess enthalpy during three successive periods of 75, 155, and 101 hours. Moreover, at least 0.5 cm^3 of bubbles at 2000 atmospheres (the tensile strength of Pd) would have been formed which would almost certainly have disintegrated our sample of Pd. The structural integrity of the sample was preserved and, indeed, it is well known that electrochemical equivalents of Pd diffusion tubes can be used indefinitely. The easiest way to discount this possibility of bubble formation is to increase the experiment times. However, we do have it in mind to search for any D_2 or, more likely, He bubbles.

(ii) Participation of the reduction of O_2 and/or ionization of D_2 i.e. a shift off the Joule heating term towards the upper bound. However, our experiments showed that the Joule heating exactly balanced the Newton's law cooling at low current densities (where the effects of any O_2 reduction on D_2 ionization should have been at a maximum) while the excess enthalpy increased with the current density. Such behavior (as well as the other points we have set out in the application) is not consistent with the participation of O_2 reduction/ D_2 ionization.

The reviewer may also like to know that in an earlier series of experiments periodic catalytic contamination of the Pd surface led to loss of dissolved D which was associated with cooling not heating presumably because of the cessation of the fusion process.

Paragraph (3) of Reviewer #3:

So far as the so-called experiment is concerned, the investigators seem to have trouble doing their energy bookkeeping and suggest that some "excesses" on the order of 10% are due to fusion. There is almost no discussion of possible heat leaks. The authors should be held to account for their statement that their experiment was "accompanied by an increase in the background radiation count in the lab of >50%. The long term experiments were all terminated at about this time." It is scientifically irresponsible to leave things this way: what radiation? Why wasn't this followed up by the University safety people?

Our reply:

#3 Again we are at a loss to know how the reviewer could make this comment. We actually pointed out that we have greater than 25% excess energy released at the highest current density. This occurred in three runs of 75, 155 and 101 hours duration. There was absolutely no possibility of heat leaks as the averaged temperature difference between the inside of the Dewar and the external water bath (which in turn was above room temperature) was 1.33(4), 1.43(6), and 1.44(2)°C respectively. Our reply to the reviewer #1 question #6 is pertinent to the interpretation of the excess energy. As this reply is lengthy, we attach an extra copy.

The radiation was beta/gamma type, possibly due to the reaction of thermalized neutrons with components of the Dewar. The matter was not followed up because it would in fact have been irresponsible of us to proceed with the experiments in their present form. We need the resources asked for to carry out the experiments under properly controlled conditions. However, we fully realized the outrageous nature of our proposals which is why we spent a considerable sum (personal funds) in order to at least get some preliminary evidence that the concepts are worth pursuing.



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Dr. R. Gajewski
Department of Energy
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16
Washington, DC 20545

November 24, 1988

Dear Ryszard:

RE: Proposal of Dr. Pons "The Behavior of Electrochemically compressed Hydrogen and Deuterium"

Here: Reply to my (reviewer #2) comments:

I have considered carefully the rebuttal of Dr. Pons to my review. In my opinion the material submitted does not offer clarification of specific points I requested in my review.

As to my point 1), the rebuttal does not offer any professional background for the estimate of the range of detectable fusion rates, which are restated as given in my review. Dr. Pons does not address in a specific manner (see below) the question how such a nuclear rate can be measured by identifiable nuclear observables. Let me illustrate the gravity of the problem by noting that fusion rate of $10^{-16}/s$ implies that even in 4 months, that is in 10^7s (not 75, 155 or 101 hours) only a 10^{-9} fraction of all atoms in the Dewar would undergo a reaction and even if all reactions would produce tritium, such a small concentration would probably be below his background level of tritium in the deuterium used. On the other hand it is extremely difficult, if not impossible, to directly observe tritium as fusion product, and one has to look at the accumulated concentration in the set up envisaged by Dr. Pons.

Thus the one method proposed and only vaguely outlined how to diagnose the reactions will not work at the level needed to match the sensitivity of the calorimetric measurement. But in my opinion there are many ways this problem can be solved. Even with the fusion rate of $10^{-20}/s$ there would be about 10^4 reactions per second, plenty to observe with help of specific detectors the products of direct nuclear reactions. In my opinion nuclear detection methods are much more sensitive than the calorimetric methods, if dealt with appropriately.

In response to my point 2. Dr. Pons refers in his reply in very general terms to gamma rays, thermal neutrons and tritium as the means of understanding of the specific origin of the excess heat, if such is observed.

"gamma rays"

In which energy range, and in particular from which nuclear fusion reactions are these expected. Note that normally gamma rays are considerably less abundant than other nuclear reaction products, except for a few exceptional cases, with well known energy. Will the considerably smaller gamma rate be at all observable? And how?

"thermal neutrons"

It appears that Dr. Pons has not considered the fact that in his experimental arrangement in case nuclear reaction occur, he will not have to deal with "thermal neutrons" but with energetic reaction products which carry the considerable nuclear energy released.

"tritium"

Where does tritium come from, why should it be the product of nuclear fusion reaction that has yet to be discovered, and finally why to look for this extremely rare and elusive product of nuclear reactions (see above).

Aside from faulty and/or incomplete responses to my specific two requests, I do not see in particular a survey which would list those nuclear reactions that are possible and a proposal how to approach their identification in any specific way. There is a very incomplete list on page 8 of the proposal which surprisingly includes secondary reactions induced by neutrons. Indeed, the vague mention of tritium means presumably that Dr. Pons proposes to follow up the possibility of d-d fusion (see page 2 of proposal) as to my knowledge only in this primary fusion reaction there is an appreciable branching ratio to tritium. But ^3He produced equally abundantly in this reaction, is a much better isotope to use as tag for this reaction...Tritium is also produced in the above mentioned secondary Li-n reactions, but neutrons have to be produced in the first place in a nuclear reactions, hence it would be wiser to look for them, rather than for a secondary and rather elusive reaction product.

All this means that:

A) the nuclear part of the proposal has not been seriously addressed;

B) there is extremely limited expertise in the field of nuclear reactions.

These observations are further supported by the paragraphs from the rebuttal to the observation of the reviewer #3 pertinent to the dangers of increased background radiation.

Dr. Pons missed the opportunity to respond in an accurate and expert fashion. I conclude with near certainty that nothing will come out of the proposed diagnosis of the specific origin of the excess heat, should the latter be indeed found. However, I consider this as the most worthwhile part of the proposed research program. In my opinion mere calorimetric reconfirmation of the excess heat generation leads us nowhere. I therefore do not recommend the funding of this project.

Yours Sincerely



Jan Rafelski

Professor of Physics



Cornell University

Laboratory of Atomic
and Solid State Physics

Clark Hall
Ithaca, NY 14853-2501

Telex WUI6713054

October 12, 1988

Dr. Ryszard Gajewski
Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16
Department of Energy
Washington, DC 20545

Re: Pons/Fleischmann Proposal

Dear Dr. Gajewski,

I am sorry, but I find it very difficult to accept the preliminary findings of Pons/Fleischmann. Deuteriums in palladium are not significantly closer together than they are in solid deuterium. Thus if they are claiming fusion in Pd at the atomic length scales typical of this alloy, then they should also see similar results from pure solid deuterium. It is a rather obvious test.

The idea that the environment of palladium (as a host) is playing a role similar to the negative muon in muon catalysis of D-T is rather primitive. If the important quantity is the overlap of deuterium wave-functions, then it is not at all clear that a palladium host does any better than the molecule of deuterium.

So far as the so-called experiment is concerned, the investigators seem to have trouble in doing their energy bookkeeping and suggest that some "excesses" on the order of 10% are due to fusion. There is almost no discussion of possible heat leaks. The authors should be held to account for their statement that their experiment was "accompanied by an increase in the background radiation count in the lab of > 50%". The long term experiments were all terminated at about this time." It is scientifically irresponsible to leave things this way: what radiation? Why wasn't this followed up by the University safety people?

I don't think you should proceed with this.

Yours sincerely,

Neil W. Ashcroft
Professor of Physics

NWA:ksl

Enclosure

Reviewer #3

November 18, 1988

Professor Neil Ashcroft
LASSP
Cornell University
Clark Hall
Ithaca, NY 14853-2501

Dear Professor Ashcroft:

Your review of the Pons/Fleischmann proposal, "The Behavior of Electro-chemically Compressed Hydrogen and Deuterium," has been forwarded to the authors for a rebuttal. Their response is enclosed. In the correspondence, you are being referred to as Reviewer #3.

It will help us in deciding whether or not to support the proposal if you could provide us with your comments on the rebuttal. Do you believe, based on the totality of the arguments offered in the proposal and in the rebuttal, the proposed project should be supported?

Your response, by return mail if possible, will be greatly appreciated.

Sincerely,

Ryszard Gajewski, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

Enclosures

Reply to reviewer #3

We will reply to the reviewer according to his paragraphs.

#1 We are at a loss to know how the reviewer can make this statement. Solid D_2 (where the nuclei are held at positions determined by the s-orbitals of the electrons) is simply not comparable to $D+$ dissolved in a Pd host lattice--which is in effect a high density, high pressure, low temperature and infinitely long lived $D+$ plasma contained in a lattice where the electrons are in the d-bands of the metal. We have pointed out in the proposal that there must be collisions between $D+$ nuclei in this system and that the repulsive potential must be appreciably screened by the electrons in the host lattice but that D_2 is NOT formed in this system.

#2 We also do not know how the reviewer can make this statement. Our experiments are in no sense comparable to muon catalyzed fusion (which is dependent on reducing the D-D distance due to the mass of the muon). We only referred to muon catalysis in order to point out that the conditions in tauri, tocomacs, H-bombs or in laser fusion are not an essential prerequisite for fusion.

#3 Again we are at a loss to know how the reviewer could make this comment. We actually pointed out that we have greater than 25% excess energy released at the highest current density. This occurred in three runs of 75, 155 and 101 hours duration. There was absolutely no possibility of heat leaks as the averaged temperature difference between the inside of the Dewar and the external water bath (which in turn was above room temperature) was 1.33(4), 1.43(6), and 1.44(2) $^{\circ}C$ respectively. Our reply to the reviewer #1 question #6 is pertinent to the interpretation of the excess energy. As this reply is lengthy, we attach an extra copy.

The radiation was beta/gamma type, possibly due to the reaction of thermalized neutrons with components of the Dewar. The matter was not followed up because it would in fact have been irresponsible of us to proceed with the experiments in their present form. We need the resources asked for to carry out the experiments under properly controlled conditions. However, we fully realized the outrageous nature of our proposals which is why we spent a considerable sum (personal funds) in order to at least get some preliminary evidence that the concepts are worth pursuing.

Question (6) of Reviewer #1:

"We believe that the results we have obtained so far are a strong indication of a progressive increase in the fusion of D nuclei in the Pd-lattice with increasing chemical potential (= compression). While there are alternative explanations of the excess heating effects, their possibility does not seem to be very likely." (p. 6) Please, what are the other explanations and why are they unlikely?

Our reply:

(6) The main alternative explanations for excess enthalpy generation are:

(i) generation of D_2 at voids in the lattice (see also comments by reviewer #5). However, if this explanation applies, the excess energy generated during 331 hours of polarization at the highest current density would have required formation of D_2 bubbles at a higher rate than that corresponding to the applied current, i.e., there would have been a loss of dissolved D. Such a loss is inconsistent with the observation of the generation of a constant excess enthalpy during three successive periods of 75, 155, and 101 hours. Moreover, at least 0.5 cm^3 of bubbles at 2000 atmospheres (the tensile strength of Pd) would have been formed which would almost certainly have disintegrated our sample of Pd. The structural integrity of the sample was preserved and, indeed, it is well known that electrochemical equivalents of Pd diffusion tubes can be used indefinitely. The easiest way to discount this possibility of bubble formation is to increase the experiment times. However, we do have it in mind to search for any D_2 or, more likely, He bubbles.

(ii) Participation of the reduction of O_2 and/or ionization of D_2 i.e. a shift off the Joule heating term towards the upper bound. However, our experiments showed that the Joule heating exactly balanced the Newton's law cooling at low current densities (where the effects of any O_2 reduction on D_2 ionization should have been at a maximum) while the excess enthalpy increased with the current density. Such behavior (as well as the other points we have set out in the application) is not consistent with the participation of O_2 reduction/ D_2 ionization.

The reviewer may also like to know that in an earlier series of experiments periodic catalytic contamination of the Pd surface led to loss of dissolved D which was associated with cooling not heating presumably because of the cessation of the fusion process.



Cornell University

Laboratory of Atomic
and Solid State Physics

Clark Hall
Ithaca, NY 14853-2501

Telex WUI6713054

November 23, 1988

Dr. Ryszard Gajewski
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16
Washington, DC 20545

Re: Pons/Fleischmann Proposal

Dear Dr. Gajewski,

Thank you for your letter and the (somewhat revised) proposal by Pons and Fleischmann.

I have not changed my opinion and I will take up the rebuttals one by one.

#1. The authors have forgotten their elementary chemistry. In particular they need to be reminded of the cusp theorem. The idea that deuterium loses its electron to the d-band of palladium is very naive. It's a rigorous theorem that the gradient of the electronic charge density at the deuteron nucleus is proportional to the electron density itself (at the same position). Since this density is not very different in Pd-D from pure solid deuterium, then by a Heitler-London argument, the interactions controlling the collisions between deuterons in Pd-D will likewise not be very different from the solid deuterium case. Differences can certainly be expected at long range, but this is irrelevant from the standpoint of the present proposal. If the authors do not believe this, they might instead consider doing a little homework: screened point ion potentials appropriate to metallic environments are readily available in the literature (even for hydrogen). If they think the electrons weaken the potential in the region that matters, they should think again.

#2. The muon through its mass presents a favorable length scale for deuteron-deuterium collisions. The authors in their last proposal were implying that electron-screening would achieve the same purpose. They still hold to this view, as they say in the abstract, and the argument is specious for the reasons given above.

#3. The previous proposal had very little discussion on important experimental details. In spite of the figures given, I remain dubious. Was any attempt made to verify that the sample remained in the same bulk phase? Is electromigration a problem? Is the temperature dependence of C sufficiently small that equation (5) follows accurately from (3)?

General Remark:

It is very important to support speculative research, provided there's some physical basis to the speculation. In my mind, the authors have presented no such

Dr. Ryszard Gajewski

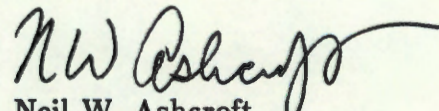
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November 23, 1988

argument. I would be willing to consider this proposal further if the authors will produce a microscopic estimate that would demonstrate in this alloy (and under conditions that are quite typical of condensed matter physics) a high likelihood of the close deuteron encounters that are necessary to fusion. I emphasize the word alloy.

Again, I do not think the proposal should be supported.

Yours sincerely,


Neil W. Ashcroft

P.S. You might seek the advice of a metal hydride physicist, for example, Prof. R. Barnes, Ames Laboratory, Iowa State University, Des Moines.

Reviewer's Report to the Department of Energy
Proposal by Prof. Stanley Pons
University of Utah
The Behavior of Electrochemically Compressed Hydrogen and Deuterium

This is a truly maverick proposal; it is also an outstanding one.

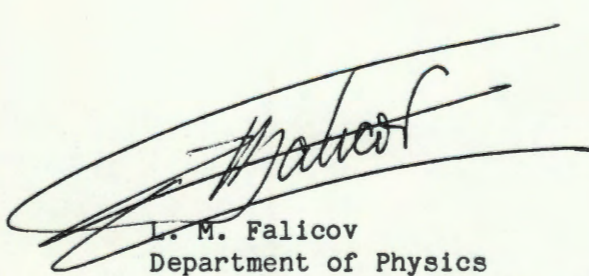
It proposes to study the feasibility of obtaining nuclear fusion in Deuterium by electrochemical compression in a Pd electrode.

There is some very interesting and high-class electrochemistry involved here. And, even though the probability of finding the ideal conditions of particle density / temperature / volume / lifetime is very small and the chances of success remote, the possible pay-off is so large that support in small scale to this project should be given.

Both principal investigators seem to have the necessary qualifications to carry out high-quality research and to be able to judge their results coolly and impartially.

It is a long-shot, with small probability of success. But it involves good science and the remote possibility of enormous pay-off.

Recommendation: support the research on a one-time-only basis. (No renewal unless positive results are CLEARLY obtained)



L. M. Falicov
Department of Physics
University of California, Berkeley, California
94720

October 8, 1988

Reviewer #4

November 18, 1988

Professor L. M. Falicov
Department of Physics
University of California
Berkeley, CA 94720

Dear Professor Falicov:

Your review of the Pons/Fleischmann proposal, "The Behavior of Electro-chemically Compressed Hydrogen and Deuterium," has been forwarded to the authors for a rebuttal. Their response is enclosed. In the correspondence, you are being referred to as Reviewer #4.

It will help us in deciding whether or not to support the proposal if you could provide us with your comments on the rebuttal. Do you believe, based on the totality of the arguments offered in the proposal and in the rebuttal, the proposed project should be supported?

Your response, by return mail if possible, will be greatly appreciated.

Sincerely,

Ryszard Gajewski, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

Enclosures

Reply to reviewer #4

We would like to assure the reviewer that we fully understand the outrageous nature of our proposal. This is why we spent considerable personal funds to try to obtain some preliminary evidence that the concepts were worth pursuing.

Thank you for your support!

We need one clear year of experimentation after the apparatus is assembled (about 6 months), basically due to the fact it will take about this long to saturate the large rods with D_2 . We therefore think that the feasibility question could certainly be answered within 18 months.



Department of Energy

Washington, DC 20545

November 18, 1988

Professor L. M. Falicov
Department of Physics
University of California
Berkeley, CA 94720

Dear Professor Falicov:

Your review of the Pons/Fleischmann proposal, "The Behavior of Electrochemically Compressed Hydrogen and Deuterium," has been forwarded to the authors for a rebuttal. Their response is enclosed. In the correspondence, you are being referred to as Reviewer #4.

It will help us in deciding whether or not to support the proposal if you could provide us with your comments on the rebuttal. Do you believe, based on the totality of the arguments offered in the proposal and in the rebuttal, the proposed project should be supported?

Your response, by return mail if possible, will be greatly appreciated.

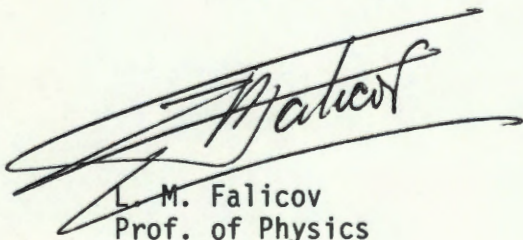
Sincerely,

Ryszard Gajewski, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

Enclosures

11-21-88

There is no controversy or discrepancy between my original report and the authors' response. I stand by my original recommendation.



L. M. Falicov
Prof. of Physics

Review of the proposal, "The Behavior of Electrochemically Compressed Hydrogen and Deuterium" by Stanley Pons.

The concept is, to this reviewer's knowledge, new, and it is most intriguing. If the project were successful, it would constitute one of the most important inventions of the 20th century. The investigators should be encouraged to pursue it.

The project appears to be an extreme limiting case of the high-payoff, high-risk type that AEP funds. The payoff approaches infinity and the probability of success unknown and could be small. The product, $0 < (\text{payoff})(\text{success probability}) < \infty$, is quite indeterminate at this point in time.

On the other hand, this reviewer has serious questions about the reported experiment with D_2O and the process itself.

1. Agreed that 0.8 eV could theoretically produce 10^{27} atmospheres equivalent for D_2 , but what if the reaction, $2(D^+ + e^-) \rightarrow D_2$ nucleates at imperfections like grain boundaries. Since the tensile strength of Pd is only 2000 atm., the material could blow apart mechanically. Pd_2D supersaturated with D probably has a lower tensile strength.
2. Agreed on the method of the thermal balance but not convinced that there are not valid alternative explanations for the excess heating effect. The investigators case would be stronger if they repeated the experiment in H_2O and found no excess heating effect.
3. The alledged increase in radiation count in the lab should be elaborated. Where measured? Is it definitive? Is it attributed to tritium from Reaction 1 at the top of page 2? A more quantitative treatment and correlation with excess heating effect would be in order.
4. Is it possible to get a runaway thermonuclear reaction? A 2 cm diameter, 10 cm long Pd rod converted to Pd_2D could produce an order-of-magnitude 0.1 kiloton explosion by Reaction 1 if detonated. The investigators are proposing to tread in an unknown region. To quote them, "In our view, calculations (such as nuclear force: quantum: molecular dynamic simulations) would be difficult and ambiguous (indeed perhaps impossible at this stage). In these circumstances it is best to resort to experiment." It would be a shame to lose Pons and Fleischmann as well as the University of Utah campus.

Reviewer #5

Thornton R. Bank

November 18, 1988

Dr. Theodore Beck
Electrochemical Technology
Corporation
1601 Dexter Avenue, North
Seattle, WA 98109

Dear Dr. Beck:

Your review of the Pons/Fleischmann proposal, "The Behavior of Electrochemically Compressed Hydrogen and Deuterium," has been forwarded to the authors for a rebuttal. Their response is enclosed. In the correspondence, you are being referred to as Reviewer #5.

It will help us in deciding whether or not to support the proposal if you could provide us with your comments on the rebuttal. Do you believe, based on the totality of the arguments offered in the proposal and in the rebuttal, the proposed project should be supported?

Your response, by return mail if possible, will be greatly appreciated.

Sincerely,

Ryszard Gajewski, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

Enclosures

Reply to reviewer #5

We will reply to the reviewer according to his numbering system.

(1) and (2) (in part). These points are covered by our reply to question #6 of Reviewer #1. As this reply is lengthy, we attach a copy to these comments.

(2) (in part) We fully intend to make the comparison with saturation using H^+ . However, the experiment is not unambiguous. One of the more intriguing possibilities is that one might be able to induce an hydrogen cycle: we have pointed out in the application that the cross-sections under the conditions we have in mind may be quite different to those in H-bombs.

The reviewer may be interested to know that we have repeatedly discussed amongst ourselves questions such as: is a part of the heat generation in the planets (especially the earth and Jupiter) due to H cycles in the Ni core? Are supernovas caused by related effects?

(3) The increase in the radiation was measured adjacent to the Dewar. It may have been due to tritium but could also have been due to the reaction of thermalized neutrons with components of the Dewar other than the D_2O . The reason we cannot be more specific at this stage is that we considered that we had to terminate the experiments. If the project is funded, then one of our first objectives is to quantify such effects (if any!) so that appropriate safety measures can be taken. We intend to correlate any heat release with tritium production and to look for thermalized neutrons and gamma-ray emission. Our replies to questions (3) and (4) of reviewer #1 are relevant to this point and are attached to this reply.

(4) Our own calculations showed that in the experiment we were conducting we might achieve a 0.042 kiloton explosion. Rescaling to the largest Pd rod we have in mind gives the figure quoted by the reviewer. This is why we approached the project with great care. There is, however, an intriguing aspect to the possible fusion of D in the Pd-lattice namely that it would be self-limiting. As the dissolution is endothermic, an increase in temperature would lower the chemical potential of the dissolved D and thereby limit the reaction. The effect would probably outweigh any acceleration of the reaction with increase of temperature. We might have the unusual situation that the heat release in any practicable device would increase with heat demand.

Our reply to #7 of reviewer #1 is also pertinent to this question and is attached.

Question (3) of Reviewer #1:

The proposed work includes "radiation measurements" (page 10). Unfortunately, the method of making these measurements is not discussed although it is central to the investigation, since detecting neutrons and/or gamma radiation of the proper energy would be a clean signature for fusion reactions.

Our reply:

(3) The main methods to be used will be as follows: (a) detection of any tritium generated by the reactions and correlation of the rate of generation of tritium with the excess energy production. Samples will be withdrawn and analyzed using scintillation counting equipment. (b) Detection of thermal neutrons and use of energy discriminative gamma-ray analysis. The reviewer should note that under the conditions of our experiment neutrons will be rapidly thermalized in the palladium rod (indeed the experiment was designed with this in view for safety reasons) so that it is not possible to correlate the energy of any neutrons produced with any particular nuclear reaction. Our strategy therefore will be to detect thermalized neutrons and in particular the gamma radiation generated by the reaction of these neutrons with species present in the Dewar (the electrodes, electrolyte and components of the borosilicate glass).

To be more specific, we will initially use the simplest possible means to search for thermalized neutrons. For example, we may compare results for potassium deuterioxide electrolyte with those for potassium borate using photographic plates as a detection medium. Gamma rays will be detected using sodium iodide crystals for low resolution measurements; if necessary we will use intrinsic germanium detectors.

Question (4) of Reviewer #1:

If significant radiation is anticipated in the research, safety measures must certainly be elaborated.

Our reply:

(4) The reviewer should note that this is why we terminated our experiments. If this project is funded, then one of our first objectives will be the quantification of any radiation produced and all appropriate steps to contain and shield the experiment will be taken. The Department is well equipped with radiation-safe laboratories and various forms of radiation counting equipment. Samples will be monitored daily with scintillation counters, and the apparatus with Geiger-Müller counters. In the case of obvious generation of radiation, we plan to reassemble the experiment in laboratories containing equipment suitable for discriminating the energies of gamma rays and equipment for detection of thermalized neutrons (see also reply to (3) above). We are thoroughly familiar with the rules and regulations of our University Radiation Safety committee, and have discussed with them their requirements for radiation experiments in our laboratories. The reviewer will wish to know that we have informed the Vice President for Research at this University (a well-known physicist) of our plans.

The reviewer will wish to note that if we are correct in assigning the excess energy to a fusion process, then the source would be classified as one of low energy. We intend to keep the experiments in this category. Thus if we get a marked increase in the excess energy with change of the system parameters (overpotential), bath temperature, rod dimensions, poisoning conditions) then we will scale down the experiment appropriately (thinner and shorter rods).

See also last paragraph of our reply to question (7).

Question (6) of Reviewer #1:

"We believe that the results we have obtained so far are a strong indication of a progressive increase in the fusion of D nuclei in the Pd-lattice with increasing chemical potential (= compression). While there are alternative explanations of the excess heating effects, their possibility does not seem to be very likely." (p. 6) Please, what are the other explanations and why are they unlikely?

Our reply:

(6) The main alternative explanations for excess enthalpy generation are:

(i) generation of D_2 at voids in the lattice (see also comments by reviewer #5). However, if this explanation applies, the excess energy generated during 331 hours of polarization at the highest current density would have required formation of D_2 bubbles at a higher rate than that corresponding to the applied current, i.e., there would have been a loss of dissolved D. Such a loss is inconsistent with the observation of the generation of a constant excess enthalpy during three successive periods of 75, 155, and 101 hours. Moreover, at least 0.5 cm^3 of bubbles at 2000 atmospheres (the tensile strength of Pd) would have been formed which would almost certainly have disintegrated our sample of Pd. The structural integrity of the sample was preserved and, indeed, it is well known that electrochemical equivalents of Pd diffusion tubes can be used indefinitely. The easiest way to discount this possibility of bubble formation is to increase the experiment times. However, we do have it in mind to search for any D_2 or, more likely, He bubbles.

(ii) Participation of the reduction of O_2 and/or ionization of D_2 i.e. a shift off the Joule heating term towards the upper bound. However, our experiments showed that the Joule heating exactly balanced the Newton's law cooling at low current densities (where the effects of any O_2 reduction on D_2 ionization should have been at a maximum) while the excess enthalpy increased with the current density. Such behavior (as well as the other points we have set out in the application) is not consistent with the participation of O_2 reduction/ D_2 ionization.

The reviewer may also like to know that in an earlier series of experiments periodic catalytic contamination of the Pd surface led to loss of dissolved D which was associated with cooling not heating presumably because of the cessation of the fusion process.

Question (7) of Reviewer #1:

"The experiments will take longer than our previous experiments in view of the greater thickness of the rods compared to the sheet electrodes. It will take approximately 12 months to charge a 2cm diameter rod to saturation with deuterium.." (p. 7) Could not the time required be drastically reduced by heating the rod in a pressurized deuterium environment?

Our reply:

(7) We have considered doing this but unfortunately it would not reduce the experiment time. The important point is that the high chemical potential of dissolved D is established by diffusion so that one cannot "beat" the diffusional relaxation time.

We have also considered an electrochemical variant of the reviewer's suggestion, namely, the electrochemical saturation of Pd by polarization at a high temperature and subsequent cooling. As the dissolution of D in Pd is endothermic, this would produce even higher chemical potentials of the dissolved D! We do not wish to do this in our initial experimental experiments as the expulsion of excess D from the lattice on subsequent cooling would lead to spurious excess enthalpy generation (but see our comment above). The reviewer may wish to note that if we can prove that the concept works, then we intend to saturate rods at high temperature and to try to find suitable diffusion barriers. This would in effect produce Pd-D "hot rods".

The considerations set out in the above paragraph are also important to the safety of this project which has been referred to by some of the other reviewers.

As the dissolution of deuterium is endothermic, a marked rise in temperature of the rods will lower the chemical potential of the deuterium and will therefore self limit any fusion process.



Department of Energy

Washington, DC 20545

November 18, 1988

Dr. Theodore Beck
Electrochemical Technology
Corporation
1601 Dexter Avenue, North
Seattle, WA 98109

Dear ^{Ted}~~Dr.~~ Beck:

Your review of the Pons/Fleischmann proposal, "The Behavior of Electrochemically Compressed Hydrogen and Deuterium," has been forwarded to the authors for a rebuttal. Their response is enclosed. In the correspondence, you are being referred to as Reviewer #5.

It will help us in deciding whether or not to support the proposal if you could provide us with your comments on the rebuttal. Do you believe, based on the totality of the arguments offered in the proposal and in the rebuttal, the proposed project should be supported?

Your response, by return mail if possible, will be greatly appreciated.

Sincerely,

Ryszard Gajewski, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

Enclosures

11-22-88

Dear Ryszard:

A response to the Pons/Fleischman
response is enclosed.

Ted Beck

Response to Pons/Fleischmann Response

I am not satisfied with the proposer's qualitative responses to my questions, but it appears that the contract research is required to answer the questions quantitatively. I am inclined to believe that the process is so potentially important, if it indeed works, that the project should be funded.

Some quantitative estimations of time constants for buildup of a runaway thermonuclear reaction and for the proposed self-limiting decrease in chemical potential of dissolved D and estimations of steady-state conditions would appear to be in order before serious experiments are begun. "Hand-waving" arguments were used in the proposer's response.

December 8, 1988

Professor Stanley Pons
Department of Chemistry
University of Utah
Salt Lake City, UT 84112

Dear Professor Pons:

Following yesterday morning's telephone conversation, I am enclosing copies of the reviewers' responses to your and Professor Fleischmann's rebuttals.

My reading of the situation is that the project can be allowed to proceed only if a credible capability is established to diagnose the products of the suspected nuclear reactions.

Please let me know your thoughts on this subject.

Sincerely,

Ryszard Gajewski, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

Enclosures



DEPARTMENT OF CHEMISTRY
THE UNIVERSITY
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SO9 5NH

PROFESSOR MARTIN FLEISCHMANN, F.R.S.

TEL. 0703-559122
TELEX 47661

Dr. R. Gajewski,
Director,
Division of Advanced Energy Projects,
Office of Basic Energy Sciences, ER-16,
Department of Energy,
Washington, DC 20545
U.S.A.

MF/KJW

20 December 1988

Dear Dr. Gajewski,

As you will see I am at present in Southampton and Stan Pons has sent on to me a copy of your letter of 8th December enclosing the further comments from the reviewers # 1 to 5 of our research proposal. I was relieved to see from your letter that this project may be able to go ahead provided we can establish means of credibly diagnosing products of the suspected nuclear reactions. I know that Professor Pons is actively making arrangements to this end and no doubt he is corresponding with you about this question. However, I have been so disturbed by the nature and tenor of some of the comments made by some of the reviewers that I feel compelled to write to you further in part to seek to "set the record straight" but mainly to seek to correct the impression made by these reviewers. As I hope to show you, if we were to follow their advice then this would impose an unwarranted and untenable bias to our work. I shall be sending this letter to you via Stan Pons as he may wish to comment further on some of the points I have made. It may well be also, that you will wish to send some of these comments to the reviewers in which case I may want to tone down some of the remarks!

I now realise that there has been a major lack of communication between us and some of the reviewers (notably # 2 and 3 and to a lesser extent # 1). This has been due in part to the rather outrageous character of our proposal which has made us reluctant to make definite statements: it is really still necessary to make exploratory measurements. It is also largely my fault in that I persuaded Professor Pons that we should submit a short proposal - I should explain to you that we are here restricted to six pages and discouraged from giving a great deal of background material which obscures the main objectives of the research programme. Be that as it may it is evident that these reviewers have not understood the restrictions on the diagnosis of the nuclear reactions posed by the nature of the electrochemical techniques and in consequence have failed to understand the physics of the problem. This has led them to place a quite unwarranted emphasis on energy discriminative neutron detection (and, I suspect, on the search for ^3He). We have considered most of these points (and many others) in designing the experiments outlined in our proposal and I think it is therefore desirable that I should outline some of these considerations to you in replying to the reviewers' second set of comments. However, to

/continued...

telescope my replies: may I point out to you that the dimensions of our cells lie between that of the mean free path of the neutrons and the thermalisation distance. The information content in energy discriminative neutron analysis will therefore be lost. It seems to us more sensible therefore to take the opposite approach namely to increase the dimensions of the experiment and to look for the secondary reactions induced by thermalised and, maybe, fast neutrons.

I would like to re-emphasise that the major objective of our proposal is to investigate whether we can create conditions by simple, essentially chemical, means to generate highly compressed D^+ such that there will be significant thermonuclear fusion. I have underlined significant because we would be quite content to demonstrate unequivocally the generation of excess enthalpy with appropriate supporting evidence for the generation of neutrons and γ -rays. To this end we must investigate the effects of the dimensions of the electrodes and cells, the thermal balances, the effects of electrode potential and temperature, solution composition (including isotopic composition), the effects of electrode material and surface poisoning etc. etc. All this information will be required to evaluate our results and we will be fully stretched to meet these objectives. By contrast some of the reviewers want us to concentrate on the nuclear physics aspects and seem to believe that the demonstration of $E = mc^2$ does not provide a sufficient objective on that score! To follow some of these objectives we would need to institute experiments, which require high vacuum techniques, high resolution mass spectroscopy, energy discriminative neutron and γ -ray detection etc. etc. Our proposal is not costed to achieve these objectives and, as I point out in the detailed comments nor is any conceivable experiment design (apart from energy discriminative γ -ray). It would seem more logical for us to make any supporting measurements using other peoples' instrumentation (such as that of Professor Steven Jones at Brigham Young University).

The situation with regard to the reviewers is really worse than I have outlined: in order to make their point the reviewers quote parameters which do not apply to our experiments (e.g. reviewer # 2 talks of fusion rates of 10^{-20} s^{-1} when we believe that we had $3 \times 10^{-14} \text{ s}^{-1}$ and are aiming to achieve say $3 \times 10^{-12} \text{ s}^{-1}$ in this next phase. Where is the sense in such comments?). Furthermore, they cast aspersions on our competence to make relatively straightforward measurements. Let me assure you that we do know how to make such measurements and, if we don't, whom to ask. A long time ago I supervised a small radiochemical laboratory: the instrumentation may have changed but the principles have not. More recently I have been concerned with single photon counting techniques (as has Professor Pons) and I have developed the use of position sensitive single photon counting X-ray detection methods in surface chemical problems (including the construction of the detectors), work which has been supported by the U.S. Office of Naval Research. I am well aware of the application of closely related techniques in fusion research. Furthermore I was a consultant at the United Kingdom Atomic Energy Authority (UKAEA) Winfrith Heath Laboratories in the days when the Steam Generating Heavy Water (SGHW) programme was in full swing.

/continued...

The team there has now been dispersed. At present I consult at UKAEA, Harwell, on matters not related to the substance of our proposal but, inevitably, one acquires the relevant background. I have told you all this because I take a dim view of the tenor of some of the remarks.

Although I now seem to be erring on the side of length rather than brevity, I feel it is necessary to reply in detail to the reviewers. As some of these replies to specific reviewers cut across the comments made by some of the others, I will number the comments and will indicate the origin of the reviewers' first comments by: # reviewer, lines a-b; our replies by: reply lines c-d; the reviewers' second sets of comments by: # reviewer lines e-f and my further comments by: reply lines g-h. This will allow me to cross refer where necessary.

Yours sincerely,



Martin Fleischmann

P.S. Having just completed the attached set of comments, I inevitably find that I have been driven into making rather strong statements about the interpretation of our preliminary sets of experiments. I feel that I should at the end of this letter seek to correct this impression. Professor Pons and I certainly still have many doubts about the feasibility of the project but, as some of the reviewers have said, the pay-off, if successful, would be so enormous that we do of course feel that we have to continue!

Comment 1
Reviewer # 1 lines 4-8
Reply lines 3-21

I believe that the reviewer now understands the logic behind our experiment design and, indeed, we had thought it likely that he is Professor Steven Jones. Right at the start of our project Professor Pons and I discussed its possible relevance to muon catalysed fusion research and, subsequently, we have repeatedly considered contacting Professor Jones. The reason we have not done so before now is that we could not decide whether highly compressed D^+ would offer advantages in this field - but we may be quite mistaken on this score alone. However, it now appears that Professor Jones' interests extend beyond this field and it may well be of benefit to all of us to collaborate on various aspects of these research programmes.

Reviewer # 1 lines 41-46
Reply lines 108-111
Reviews # 1 further comments
lines 23-45

I hope that you will not mind if I make a critical comment on some of the reviews: I wish that Professor Jones had revealed his identity in his first set of comments. It is bad reviewing practice (for proposals as for papers) to introduce new material in replies to a response. This remark applies equally to the second sets of comments of some of the other reviewers.

We are well aware of the formation of inert gas bubbles in metals. Indeed our knowledge of the formation of such bubbles (and, as I have pointed out in my letter, I consult for UKAEA) was part of the background knowledge which led to the Pd/D project.

Reply line 80

As we pointed out in our reply, line 80, we intend to search for He bubbles but the search for ^3He cannot be a primary line of attack for reasons which are outlined in comment 3 in response to reviewer # 2.

Comment 2
Reviewer # 1 lines 9-20
Reply lines 22-47
Reviewer # 1 further
comments lines 3-17

Quite frankly, I cannot see how Professor Jones can make this comment. He (and some of the other reviewers) seem to be unaware that D_2O has been extensively used as a moderator in much European fission research. As I have pointed out in my letter, the dimensions of our electrodes + electrolysis cell lie between the mean free path and the thermalisation distance for fast neutrons. The characterisation of neutrons of a particular energy cannot therefore be used as a primary research technique. I am sure that Professor Jones and some of the other reviewers will believe that it would be possible to modify the experiments to give a configuration in which

there will be little energy loss of the primary neutrons but if we were to do this we will probably switch off the fusion reaction. To forestall further discussion on this point let me outline a typical experiment which we have considered. In order to approach the typical thin film configuration favoured in nuclear physics, we could use the electrochemical version of the Pd-diffusion tube in which D₂O would be electrolysed on the inside of a thin tube and diffuse to the outer surface and this tube would be used as the source. At least half of any neutrons generated would then reach the detector without loss of energy. However, in any such configuration, the chemical potential of the dissolved D will drop markedly because the system is not now in equilibrium. It was established already before the second world war that the boundary condition on the ingoing interface is of the type

$$-D \left(\frac{\partial C_b}{\partial x} \right)_{x=0} = k_1 C_s - k_2 C_b$$

where D_D is the diffusion coefficient in the lattice, C_b is the concentration of D in the Pd, C_s the concentration on the surface and k is a rate constant i.e. we have the diffusion analogue of the radiation boundary condition of heat conduction (the flux is taken in the direction x positive). There is therefore a marked discontinuity at the interface and this will cause a marked fall of the chemical potential. A negative result of such an experiment therefore could not be taken to predict a negative result for the experiments outlined in our proposal. I am sure you will understand that we do not wish to make our research strategy dependent on experiments whose interpretation we can judge to be at best ambiguous but we do of course intend to make such measurements to support those which we have outlined in the proposal. Perhaps I should also add that supported films of Pd will not be usable as they will certainly detach due to formation of bubbles at the Pd/support interface.

We do of course know the reactive penetrating power of α, β, and γ-radiation of a given energy and we are well aware of proper shielding procedures (please see my covering letter - I would add that some of our experiments require shielding at least as demanding as that in nuclear physics and chemistry). The reason we referred to (β + γ) radiation is simply that the detector we used measured such combined radiation. The reviewers should bear in mind that the preliminary experiments we reported were carried out with zero funding!

Furthermore, Professor Jones will see from our proposal and our reply that: (a) there is no H in the experiments we propose for the major part of the work so that the generation of γ-rays by proton-deuteron fusion cannot be observed. As pointed out in our first reply to reviewer # 5, we intend to

Reply to reviewer #5
lines 5-8

make measurements in systems containing H alone and we will certainly include measurements on mixed H/D systems. (b) that we propose to use energy discriminative γ-ray analysis. If on grounds of safety alone, the starting point for our experiments

requires that we should increase the dimensions of the electrodes + electrochemical cells to thermalise the neutrons and maintain the cells in a large water bath to absorb the neutrons. It therefore seems more sensible to us to look for the secondary reactions of slow neutrons (and, maybe, of fast neutrons) by using energy discriminative γ -ray analysis rather than to concentrate on the characterisation of the primary neutron flux. As we have also pointed out, we shall add appropriate electrolytes to the Dewar and/or the water bath to look for such secondary reactions.

Reviewer # 1 further
comments lines 13-17

Why? As I have pointed out (and elsewhere, see comment 3) the characterisation of the energetic neutrons and of the $^3\text{He}/^4\text{He}$ ratio will be difficult and expensive and could be inconclusive. At the end of such a programme we would still have to carry out the project which we have outlined so why not do it our way? If it turns out that we are correct in our assignment of the excess enthalpy to nuclear fusion then our own programme will give most of the information required for any further work on this topic and, we submit, will give all the information required for an unequivocal assignment of the nuclear reactions involved.

Comment 3
Reviewer # 2 lines 9-21 and
lines 41-42
Reply lines 11-19
Reply to reviewer # 3 lines 14-20
Reviewer # 2 further comments
lines 7-9

I simply do not know how to reply in a sensible way to the further comments made by this reviewer nor, indeed, whether there is any point in doing so. I submit that any scientist reading his first set of comments would conclude that his question about the sensitivity of the calorimetric method we have used (and propose to use) was of primary concern to him. We have answered this but in his further comments he simply rejects this and says that we have quoted his figure given in the first set of comments. This is simply untrue - he stated that he believed that fusion rates as low as 10^{-16} s^{-1} would be detectable whereas we told him that our calculations showed that $3 \times 10^{-16} \text{ s}^{-1}$ would have been measurable in our apparatus and using particular measurement techniques. Moreover, we gave him values of actual temperature differences measured while typical values of the Heavy Water Equivalent of the Dewar + contents (292.7 g D_2O) and of the cooling rate ($0.3113 \text{ J s}^{-1} (^\circ)^{-1}$) were quoted in the proposal. Surely, it is too straightforward a matter to multiply the latter figure by the temperature difference in order to get the rate of cooling to warrant an extensive discussion of such a trivial point? Subtraction of the rate of Joule heating gives the excess rate of enthalpy production which can be equated to the enthalpy production in nuclear fusion. It is this which gives the observed rate $3 \times 10^{-14} \text{ s}^{-1}$. Temperature differences two orders of magnitude smaller than the ones we observed would certainly be measurable and if the joule heating were reduced

proportionately, we would therefore be able to measure rates as low as $3 \times 10^{-16} \text{ s}^{-1}$. Increase of the dimensions of the Pd electrode and reduction of the electrolyte volume would give a further order of magnitude without changes in the technique. Improved construction of the Dewar (better vacuum, silvering of the inside surfaces) and tracking of the Dewar temperature by that of the water bath would certainly give one further order of magnitude. However, this is all really beside the point because if we were correct in attributing the excess enthalpy to nuclear fusion, then we are certainly not dealing with such low fusion rates. Indeed our objective is to raise these rates by perhaps two orders of magnitude in the next phase of the work.

Reviewer # 2 lines 32-46

Reply to reviewer # 1 lines 68-90

Reply to reviewer #3 lines 14-20

One would conclude that the question of other sources of energy was the second matter of concern to reviewer # 2. We have answered this (as far as we can at the present stage) but the reviewer does not state whether he is satisfied by our answer.

Reviewer # 2 lines 43-44

Reply lines 31-34

Reviewer # 2 further comments
lines 9-58

I submit that anyone reading the reviewers first set of comments would conclude that he wanted a short and general statement about the type of radiation to be measured. This we have done but the reviewer now introduces a great number of new objections. As I have already told you, I consider this to be unacceptable reviewing practice but I will nevertheless attempt to answer briefly the points made.

Thermal neutrons My comments 1 and 2 to reviewer # 1 are relevant to this point and to the rest of this section. Suffice it to say that the dimensions of our apparatus lie between those of the mean free path and the thermalisation distance. I must state again that diagnostic measurements on the primary neutrons generated in any nuclear reaction will be difficult or impossible. Redesign of the apparatus to allow such measurements may lead to cessation of any fusion reaction.

γ - rays As we shall therefore be dealing with low energy neutrons, there are many n - γ reactions which could be used to detect these neutrons. As we have pointed out, we can change the electrode material and, within certain restrictions, the electrolyte in the Dewar. We can also add suitable electrolytes to the surrounding water bath (quite apart from the n - γ reaction with H_2O). Diagnostic measurements with γ -rays are not particularly difficult and we have the relevant experience to develop such measurements.

Tritium and ^3He . It seems to have escaped the reviewers attention that the main substance of our proposal is to seek to induce $\text{D} + \text{D}$ fusion! ^3He will be difficult to detect because of dominant D_2 and O_2 evolution in the cell (I have done mass

spectroscopy in the past so I am well aware of what is involved). We have pointed out to reviewer # 1 that we may look for the bubbles but such a search will interrupt the measurements and may well prove to be inconclusive. By contrast T will inevitably accumulate in the electrolyte in the Dewar but why does the reviewer insist on basing his calculation on a fusion rate of 10^{-16} s^{-1} when we have told him that an interpretation of the excess enthalpy in our preliminary experiments in terms of fusion implies a rate of $3 \times 10^{-14} \text{ s}^{-1}$? Elementary considerations show that the fraction α of T species will build up as

$$\alpha \approx \frac{S\beta}{R} \left(1 - \exp \left(-\frac{Rt}{SN} \right) \right)$$

where S is the separation factor for D in the $\text{D}_2\text{O}/\text{TDO}$ mixture, β is the fusion rate (atoms of T s^{-1}), R is total rate of electrolysis of T and D (atoms s^{-1}), N is the total number of atoms of T and D in the system and t is the time (s). In the previous experiments the characteristic time SN/R would have been > 100 days and the final concentration of T $\sim 4.4 \times 10^{-7}$ molar (certainly measurable but requiring long term experiments). The initial build would have been $\sim 4.5 \times 10^{-9}$ molar day^{-1} (admittedly difficult to measure). The redesign of the experiments should allow a reduction of SN/R certainly to 10 days (following saturation of the electrodes) an increase of the final concentration of T and in consequence of these two factors an increase in the rate of build up of T.

Reviewer # 2 further comments
lines 65-71

Professor Pons and I would have been quite content to produce an unequivocal demonstration that the excess energy is due to $\text{D} + \text{D}$ fusion and this demonstration will certainly require the resources we have asked for. If we spend an appreciable part of our time and resources on energy discriminative neutron analysis, then we shall find it difficult to delineate the scope of the problem.

Comment 4
Reviewer # 3 lines 1-5
Reply lines 2-9
Reviewer # 3 further comments
lines 5-17

Granted, but the reviewer also has to be reminded about elementary chemistry. It is well known that the electron density at nuclei is determined by the s-electron wave functions as is fully established in chemistry by inter alia the hyperfine splitting of ESR spectra (controlled by the Fermi contact potentials), spin-spin coupling in NMR spectra, chemical applications of Mössbauer spectroscopy. If the contributions by these wave functions were dominant then D_2 would have to form. However, D_2 is not formed but the reviewer refuses to answer how this can be so on the basis of his argument.

Perhaps I should explain that my personal knowledge of the behaviour of D^+ (or H^+) in $\beta\text{Pd-D}$ is in part based on work on the electrolytic separation factors of H/D between $\text{HDO}/\text{D}_2\text{O}$ and

β Pd-H/D. These can only be explained by assuming that the H or D species in the lattice behave as classical vibrators, a situation radically different to that in solid hydrogen or deuterium i.e. the species behave as though they are in a very shallow potential well. You will appreciate that our knowledge of this behaviour had a considerable bearing on the research programme we submitted!

We made the great mistake of talking out the d-bands of Pd in our reply when we were much more cautious in our proposal - and caution is indicated! The collisions of D^+ in the lattice are really a supreme example of non-adiabatic, non-Born Oppenheimer processes in which, moreover, nuclear forces must be included. Realistic calculations would be difficult (at least for us) and are unlikely to be definitive. In his General Remark the reviewer states that the demonstration of a high likelihood of deuteron encounters is a prerequisite for any funding (has he been concerned with calculations on H-bombs?). However, such a calculation would not have to demonstrate a high likelihood but a sufficiently high but, in absolute terms, low likelihood. This is obviously difficult.

Reviewer # 3 lines 5-9
Reply lines 10-14
Reviewer # 3 further comments
lines 18-21

I do not really wish to point out yet again that we do not wish to draw an analogy with muon fusion.

Reviewer # 3 lines 10-17 Reply lines 13-27
Reviewer # 3 further comments
lines 22-26

It seems to me that the reviewer now accepts in part that we observed extra enthalpy generation. Electromigration is not a problem because the electrodes are charged to equilibrium; the effects of variations of C with T will also be small as the temperature differences are themselves small. The question of the maintenance of the same bulk phase is a very important point. To date nobody has discovered a phase other than β -Pd-D either from electrochemical or X-ray diffraction measurements in these potential regions. We hope to make some supporting measurements by in-situ X-ray diffraction on the highly charged Pd-D electrodes. I have made similar measurements on β -NiH in the past.

Comment 5
Reviewer # 4

We have no disagreements evidently.

Comment 6
Reviewer # 5 further comments
lines 1-4

I am glad to see that the reviewer appreciates that it is impossible to give other than preliminary answers at this stage. It is quite impossible at the present time to complete adequate preliminary experiments before applying for

funding especially in the case of a radically new project.
lines 5-9

We will attempt to make such calculations using plausible parameters. However, to the best of our knowledge, the required thermodynamic data are not available and they will be very difficult to determine since we are dealing with systems very far from equilibrium.



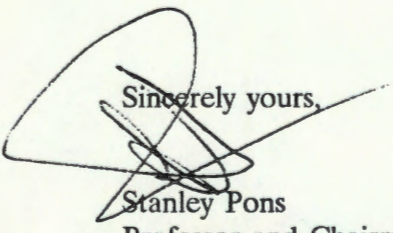
Dr. R. Gajewski
Director, DAEP
Office of Basic Energy Sciences, ER-16
Department of Energy
Washington, DC 20545

January 23, 1989

Dear Dr. Gajewski:

As per our recent conversations, and after further considerations, I am enclosing a statement of intent regarding the unambiguous detection of neutrons that may be generated in the experiments. In addition, I am enclosing the set of comments that I spoke to you about, that we wish to become part of the file. There are obviously several important points that we would like to be part of the record. If we have been overcautious in the proposal, it is simply because there are simply too many unknowns and too little data in the literature regarding the possible processes implied by the preliminary experiments; we hope to remedy this problem, and it will take a great deal of work! We look forward to working with you. Best regards.

Sincerely yours,


Stanley Pons
Professor and Chairman

SP/sdp

Department of Chemistry
Henry Eyring Building
Salt Lake City, Utah 84112

January 23, 1989

Additional comments to Pons, Fleischmann proposal.

Direct measurements of high energy neutrons formed in one of the possible reaction branches will be attempted by the use of a flat wall cell, going back to a plate type working electrode. The plate will be placed as close as possible to a flat thin glass optical window. The electrode will be surrounded by a platinum grid secondary electrode mounted on a thick platinum frame. This arrangement will give the highest exposure to any large, flat surface detector that may be used.

We have consulted with Professor Steven Jones who has described to us existing high energy neutron spectrometers that exist at Brigham Young University. These are based on lithium doped glass and various scintillators. There is ongoing research to improve the energy resolution; it is possible that these are the best spectrometers available. He has in addition offered to assist us in making the proposed measurements. Since the effective aperture of these spectrometers is at least as large as our entire cell, the efficiency of the measurements should be quite high. While there are numerous doped / activated inorganic salt and glass scintillator detectors known, the BYU group clearly has the expertise and facilities needed to make the required measurements in an unambiguous manner.