Department of Energy
Office of Scientific and Technical Information
Post Office Box 62
Oak Ridge, Tennessee 37831

August 10, 2016

Re: OSTI-2016-01064-F

Dear Mr. Ravnitzky:
This is in final response to the request for information you sent to the Department of Energy (DOE), Office of Scientific and Technical Information (OSTI) under the Freedom of Information Act (FOIA), 5 U.S.C. 552 on June 22, 2016.

You requested a "copy of records, electronic, or otherwise, of each letter TO and FROM universities, companies, and organizations, from the OSTI 'cold fusion' documents collection." On July 11, 2016, you were emailed an interim response letter informing you of the need for OSTI to obtain release authorization from the Department of Energy. OSTI received notification to release the letters to you in their entirety on August 8, 2016. As a result, OSTI is releasing 72 cold fusion letters in this mailing on a CD-ROM because of the volume and file size of the PDFs.

In addition, there are approximately 13 letters that are currently being reviewed by the DOE's General Counsel Office (GC) for release or redaction. Upon receipt of guidance from GC, OSTI will release in whole or in part.

This decision, as well as the adequacy of the search, may be appealed within **90** calendar days from your receipt of this letter pursuant to 10 C.F.R. § 1004.8. Appeals should be addressed to Director, Office of Hearings and Appeals, HG-1, L'Enfant Plaza, U.S. Department of Energy, 1000 Independence Avenue, S.W., Washington, D.C. 20585-1615. The written appeal, including the envelope, must clearly indicate that a FOIA appeal is being made. You may also submit your appeal to OHA.filings@hq.doe.gov, including the phrase "Freedom of Information Appeal" in the subject line. The appeal must contain all of the elements required by 10 C.F.R. § 1004.8, including a copy of the determination letter. Thereafter, judicial review will be available to you in the Federal District Court either: 1) in the district where you reside; 2) where you have your principal place of business; 3) where DOE's records are situated; or 4) in the District of Columbia.

You may contact OSTI's FOIA Public Liaison, Charlene Luther, Office of Preservation and Technology at 865.576.1138 or by mail at the Department of Energy, Office of Scientific and Technical Information, 1 Science.gov Way, Oak Ridge, TN 37830 for any further assistance and to discuss any aspect of your request. Additionally, you may contact the Office of Government Information Services (OGIS) at the National Archives and Records Administration to inquire about the FOIA mediation services they offer.

The contact information for OGIS is as follows: Office of Government Information Services, National Archives and Records Administration, 8601 Adelphi Road-OGIS, College Park, Maryland 20740-6001, e-mail at ogis@nara.gov; telephone at 202-741-5770; toll free at 1-877-684-6448; or facsimile at 202-741-5769.

If you have any questions about the processing of the request or about this letter, please contact Madelyn M. Wilson at

Sincerely,

Madelyn M. Wilson
FOIA Officer
DOE OSTI
1 Science.gov Way
Oak Ridge, TN 37830

THIS SHOULD MAKE YOUR DAY

OPM.

File - Cold Fus

Cold Fusion Research Advocates
2050 Peachtree Industrial Court, Suite 113-A
Chamblee, Georgia 30341
Phone: 770-451-9890 Fax: 770-458-2404
E-mail: JedRothwell@Delphi.com CompuServe 72240,1256

December 13, 1995

CETI Demonstrates 1,300 Watt Cold Fusion Reactor

Produces 1000 to 4000 Times Input

Last week at the Power-Gen '95 Americas power industry trade show in Anaheim (December 5-7, 1995), a 1-kilowatt cold fusion reactor was demonstrated by Clean Energy Technologies, Inc. (CETI) of Dallas Texas. The cathode is composed of thousands of 1 mm diameter co-polymer beads with a flash coat of copper and multiple layers of electrolytically deposited thin film nickel and palladium. CETI holds three U.S. patents on the beads, with additional patents pending. During the demonstration, between 0.1 and 1.5 watts of electricity was input, and the cell output 450 to 1,300 watts of heat. In April 1995, at the Fifth International Conference on Cold Fusion (ICCF5) CETI demonstrated a cell with input of 0.14 watts and a peak excess of 2.5 watts, a ratio of 1:18. In October 1995, at the 16th biannual Symposium on Fusion Engineering (SOFE '95) the University of Illinois showed a CETI cell with 0.06 watts input and 5 watts peak output, a ratio of 1:83. Ratios at Power-Gen ranged from 1:1000 to 1:4000.

The ICCF5 and Power-Gen calorimeters were designed and constructed by Dennis Cravens. The SOFE '95 calorimeter was constructed by George Miley's group at the University of Illinois.

The Power-Gen cell and calorimeter are much larger than CETI's previous cold fusion demonstration devices. The cell is 10 cm long, 2.5 cm in diameter, containing roughly 40 ml of beads. Previous cells had about 1 ml of beads. The cell itself is wrapped in opaque foam plastic because the cell geometry has been improved and the improvements are not yet covered by patent applications. Other components in the calorimeter are made of clear Lucite plastic. (Photographs of the device can be seen on the World Wide Web address below.)

The flow calorimeter reservoir holds 2.5 liters and the flow rate is set between 1.0 and 1.5 liters per minute. A control cell is mounted parallel to the hot cell. The flow to both cells is regulated with precision valves. The reservoir and pump consist of a Magnum 220 aquarium pump with a micron filter attachment, with an additional Lucite cylinder built on top of the pump unit to hold a cooling coil, gas trap, and a 3.5 watt computer cooling fan. Water is circulated by a magnetic impeller pump, driven by a 50-watt motor mounted underneath. Static in-line mixers ensure mixing. (These are plastic objects about an inch long with vanes to stir the flow.) A few weeks before the conference, Cravens decided to increase the flow rate in order to keep the temperature below 50 degrees C. The new flow rates exceeded the capacity of his flowmeters. He was not able to procure a bigger flowmeter in time for the conference, so no flowmeter was installed. Flow was measured by turning stopcocks to redirect fluid from the cell outlet tube into a graduated cylinder for 15 seconds. This test was performed many times, and the flow rate was not observed to change measurably, except when it was deliberately adjusted between runs. The water hose from the pump is coiled in an air cooled box on top of the reservoir. Air is drawn through the box by the cooling fan. The pump, cooling fan and DC power supplies electrolysis all have one common AC cord, which is monitored by a Radio Shack analog AC voltmeter and a multimeter. Total power consumption by all components is 85 watts.

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The Delta T temperatures and reservoir temperatures are measured with K-Type thermocouples, with Omega Model HH22 Microprocessor Thermometers. Power is measured with Metex M 3800 series multimeters.

The first test was marred by a malfunction in the control cell. The control cell consisted of tin plated shot, arranged as an electrochemical cathode, in the same configuration as the smaller CETI thin film beads. During tests at the lab leading up to the conference, this produced no excess heat, as expected. However, during the first test at one point produced a Delta T temperature as high as 2.6 deg C. Cravens suspected that the flow was blocked and the cell short circuited. Later that evening he confirmed both suspicions. When he opened the cell he found that some of shot had corroded after weeks of electrolysis in warm water. The tin plating had peeled off. When they set up the cell in the afternoon, they made the flow rate in the control cell 300 ml per minute, the same as the live cell. Later on, the flow slowed down and the cell was shorted out by loose tin and debris and power consumption went up. In retrospect, this was a poor choice of materials for the control cell. The control cell was replaced with a joule heater for the remainder of the conference, which raised the water temperature the normal, expected amount.

Later on, in subsequent tests, I was able to observe the machine closely, and to make direct measurements of its performance with my own instruments. I tested the flow rate on the cold fusion cell side several times. As noted above, I did not see any measurable variation except when the flow was deliberately changed from 1,300 ml to 1,000 ml per minute by closing the valves. I checked the thermocouple readings in the reservoir, inlet and outlet with two thermistors and a thermometer. They agreed closely with the thermocouple readings. The reservoir temperature can be taken by removing the cooling loop section on top and inserting the thermistor probe directly into the water. Measuring inlet and outlet temperature required a little more ingenuity. I confirmed the outlet thermocouple reading by taking a 250 ml sample of water from the outlet pipe during a flow test and immediately measuring the temperature before the sample cooled significantly. I confirmed the cold fusion inlet temperature by turning off the control side joule heater and taking a 250 ml sample from the control outlet pipe.

Here is some sample data:

Test 1, December 4, two hours

INPUT POWER
Measured AC: 0.7 A * 120 V = 84 W
Electrolysis: 0.18 A * 8 V = 1.4 W

OUTPUT POWER
Flow rate 1200 ml/minute (300 ml/15 seconds)
Delta T Temperature 16 to 17 deg C
1200 ml * 16 deg C * 4.2 = 80,640 j/min = 1,344 W

Test 2, December 5, afternoon, 30 minutes.

INPUT POWER
Measured AC: 0.7 A * 140 V = 98 W
Electrolysis: 0.02 A * 3.9 V = 0.1 W

OUTPUT POWER
Flow rate 1000 ml/min (250 ml/15 seconds)
Delta T Temperature 6.7 deg C

1000 ml * 6.7 * 4.2 = 28,140 j/min = 469 W

CETI plans to follow up on this with demonstrations of prototype consumer products, including larger cells for space heating and heat engines. They are working to develop these devices as rapidly as they can. They estimate that it will take six months to one year to make suitable prototypes. CETI is now engaged in joint R&D projects with five corporate and university strategic partners, including the University of Illinois and the University of Missouri. All five have independently verified the excess heat. The University of Illinois group has fabricated beads from scratch using a sputtering technique rather than electrolytic deposition. They have observed excess heat from their own beads as well as beads provided to them by CETI.

Akira Kawasaki and I took many photographs of the calorimeter. I scanned four of them, and John Logajan uploaded them in his home page:

WWW URL = http://www.skypoint.com/members/jlogajan

- Jed Rothwell

Cold Fusion

FACSIMILE COVER SHEET

DATE: 9/21/93

TO: Bill Barttey
State Dept.
Fax: 202-736-7336
Tel: _____

FROM: Walt Polansky

Division of Advanced Energy Projects
ER-16, GTN
Washington, D.C. 20585
301-903-5995

Message: Per our telecon this morning.

This document consists of 2 **pages (including this cover).**

TRANSMIT: 301-903-6067
VERIFICATION: 301-903-5995

9/21/93

Bill,
- Copy of a letter I sent to Rothwell February, 1992. Paragraph #3 is still valid.
Walt P.

Mr. Jed Rothwell
2060 Peachtree Industrial Court
Suite 312-F
Chamblee, Georgia 30341

Dear Mr. Rothwell:

The Department of Energy has been asked to respond to your letter, with several enclosures, to President Bush dated January 10, 1992, that expressed concern about cold fusion research in the United States. Your letter and information on cold fusion have been reviewed. We have been aware of the activities you referenced including the effort to establish a $10 million per year research program on cold fusion.

We believe that the appropriate mechanism for establishing the feasibility of a new, or unexpected, scientific result is for the proponent to submit it to the collective judgement of peers. This mechanism can be activated in several ways, such as technical presentations at scientific conferences and publications of results in prestigious, peer-reviewed journals. This is a thorough, but time-consuming process. However, all major scientific advances have been subjected to, and have survived, this scrutiny.

The November 1989 report of the cold fusion panel recommended against any special Department of Energy funding for the investigation of phenomena attributed to cold fusion. However, the panel was sympathetic toward modest support for carefully focussed and cooperative experiments within the present funding system. The Department of Energy accepted the report and its recommendations. We have been monitoring the cold fusion research area since the issuance of that report and believe that its recommendations are still valid. We continue to be available to review any research proposal of interest to the Department.

Sincerely,

Walter M. Polansky, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

cc:
Ms. Sally Kelley
Director of Agency Liaison
The White House

Relay-Version: [illegible]ews - V6.0-3 14/03/90 VAX/VMS V5.5; site physc1.byu.edu
Path: physc1.byu.edu!yvax.byu.edu!hamblin.math.byu.edu!sol.ctr.columbia.edu!
zaphod.mps.ohio-state.edu!sdd.hp.com!decwrl!pacbell.com!tandem!UB.com!zorch!fusion
Newsgroups: sci.physics.fusion
Subject: Polansky not responsible?
Message-ID: <930201200943_72240.1256_EHL69-1@CompuServe.COM>
From: Jed Rothwell <ub-gate.UB.com!compuserve.com!72240.1256>
Date: Tue, 2 Feb 1993 01:05:57 GMT
Reply-To: Jed Rothwell <ub-gate.UB.com!compuserve.com!72240.1256>
Sender: scott@zorch.SF-Bay.ORG (Scott Hazen Mueller)
Organization: Sci.physics.fusion/Mail Gateway
Lines: 45

To: >INTERNET:fusion@zorch.sf-bay.org

Via deep back channels, someone told me that in his impression, Dr. Polansky does not enjoy implementing the DoE's CF policy. My contact says that he feels Polansky is under pressure, and he been forced to do things against his better judgement. That may be true; a bureaucrat has to follow orders, after all. My contact said the policy is formed by Adm. Watkins (Bush appointee) and Dr. Happer (who will probably stay).

I have no personal relationship with any of these people. Obviously, the letters they send me contain no detailed explanation. I send them translations of official MITI statements, scientific papers, newspaper clippings, a list of corporations in the NHEP, invitations to conferences and meetings, and so on. They send me the sort of empty, bureaucratic claptrap I quoted earlier. I can only judge the man by his actions and words. It may well be that he is only following orders -- that's how the Government works, after all. If he disagrees with the policy, I am confident that someday, during a Congressional Investigation, he will have a chance to explain his actions, reveal his CYA file, and to point the finger at someone else.

Because I don't have access to his CYA file, I want to repeat that it is my *impression* that he is a hatchet man: I said "as far as I can see...," and I meant exactly that. I cannot follow his every move. If he is just some poor bureaucrat doing his job, I'm sorry for him.

I have seldom named specific names, because there is always the danger that I might finger some low level flunky. Top bureaucrats often find ways to escape responsibility. There is a good example of the danger of "naming names" in a widely read Japanese magazine. This magazine quoted a specific name of a person at MIT, and said the fellow was the culprit who dummied up the infamous 1989 CF fraud. They fingered the wrong guy!

In this case, however, I posted Polansky's name specifically to warn off my Gentle Readers, because a couple of them asked me about submitting a proposal to him. That can be a negative career move. Until we see a hint of sanity in Washington, I advise everyone in the CF business to keep a low profile. If you must submit a proposal, don't use the phrases "cold fusion," "fusion," "nuclear," or "excess heat," don't mention the Japanese program, and keep everything low key. He is definitely the guy to go to, if you send it to anyone else, your proposal will wander around the bureaucracy for a couple of months until they figure out you mean "cold fusion" then they will send it to him, and he will deep-six it.

- Jed

Relay-Version: V[illegible]s - V6.0-3 14/03/90 VAX/VMS V5.5; site physc1.byu.edu
Path: physc1.byu.edu!yvax.byu.edu!hamblin.math.byu.edu!sol.ctr.columbia.edu!zaphod.mps.ohio-state.edu!sdd.hp.com!decwrl!pacbell.com!tandem!UB.com!zorch!fusion
Newsgroups: sci.physics.fusion
Subject: Not a tea party
Message-ID: <930201185411_72240.1256_EHL54-1@CompuServe.COM>
From: Jed Rothwell <ub-gate.UB.com!compuserve.com!72240.1256>
Date: Tue, 02 Feb 1993 01:05:49 GMT
Reply-To: Jed Rothwell <ub-gate.UB.com!compuserve.com!72240.1256>
Sender: scott@zorch.SF-Bay.ORG (Scott Hazen Mueller)
Organization: Sci.physics.fusion/Mail Gateway
Lines: 47

To: >INTERNET:fusion@zorch.sf-bay.org

Steven Jones remarks:

Jed Rothwell's repeated bad-mouthing of the U.S. Department of Energy and Dr. Walter Polansky (by name) is ill-founded and in poor taste. I wonder if Mr. Rothwell can document his accusations which seem slanderous..."

I am not a fool. I do not say things like that in public if I do not have witnesses, dates, places and times. Besides, his actions are well documented. As Gene Mallove said to Polansky the other day, "a bureaucrat can run, but he can't hide."

"Your vicious sniping at the man behind his back is in very poor taste. Shame on you."

Don't be ridiculous Steve, I have faxed him far worse. I have also written to his boss, and his boss's boss. I never attack anyone behind his back. Why should I? I am out for blood. I intend to get him, along with Huizenga, Park, Parker, Maddox, Piel, Taubes and many others. I don't hide that fact, I advertise it, in order to bait them into doing foolish things. They have obliged me on several occasions. This is dead serious power politics, these people are playing for keeps, and so am I.

They screwed up in 1989, and misjudged CF. They made a dreadful mistake which might cause irreparable harm to the nation and to science. Later, rather than admit they made a mistake, in their hubris they lashed out and hurt people, and corrupted the scientific process. They will not get away with it forever. Irresponsible, foolish people in authority who make dreadful mistakes must be replaced. They must be forced to take responsibility for their statements and actions. A free society demands no less.

I am sincerely delighted to hear that the DoE supported CF at BYU up until December 31, 1991. As far as I know, you people never succeeded in getting any heat, which is a real shame. To be honest, I would not recommend the DoE give you any more money, because after all, lots of other people have gotten heat -- and heat is what matters. Heat first, neutrons later. As far as I am concerned, if you don't have heat at BYU, you have failed, and I would not want to see any more government money going to failures. You have not learned how to enhance the reaction, and others have, so they should get the funding. Good intentions and should not be funded, only positive, outstanding results. The government should not pay people to build airplanes that cannot get off the ground; computers that do not get the right answer; telephones that don't connect; or CF experiments that do not generate heat.

- Jed

Path: physc[illegible]edu!jonesse
From: jonesse@physc1.byu.edu
Newsgroups: sci.physics.fusion
Subject: Re: Rothwell
Message-ID: <1993Feb19.151902.429@physc1.byu.edu>
Date: 19 Feb 93 15:19:01 -0700
Distribution: world
Organization: Brigham Young University
Lines: 40

Speaking for myself, I would hate to see Jed Rothwell leave the net entirely. I strongly object to his vituperative attacks on Walter Polansky, Dieter Britz, Frank Close and others. But while potentially libelous attacks should certainly be curtailed, I find that Jed represents the "xs-heat" community and shows us what these guys are thinking. Who else does this for this net so thoroughly?

I think his gems are amazing, but revealing and often amusing:

"Jones, Juizenga, Morrison, Close -- believe theory overrules facts."

"One conclusive experiment can and MUST overrule the entire existing database, no matter how certain or long established it may be. ...Okay, a million, million previous experiments showed that E=Mc2. So what? Every single one of them was wrong. Period. It does not work in metal lattices under electrolysis, and Einstein was flat out wrong." (in his response to my challenge regarding missing nuclear ash products, 21 Dec. 1992)

**"You" are the one are the one who wants to throw away old data, not me! "You" are the one who wants to overthrow Einstein." (shortly after above, to me)

"You must stop looking for neutrons, because they are not there. ... Nobody at MITI or the NHEP gives a damn about neutrons. They do not care whether CF is nuclear energy, zero point energy, or green cheese. They know it yields megajoules of heat from each tiny bit of fuel, and they know the fuel is water, and that is ALL they care about. PERIOD." (26 Jan 93)

These are fairly harmless. And I find other 'true believers' have such notions in their heads. From a sociological and historical point of view, I think we should welcome such input so that we can learn what these guys are thinking and doing. Scientifically, we can make our own discernments.

Peace,
Steven Jones

Relay-Version: [illegible]ws - V6.0-3 14/03/90 VAX/VMS V5.5; site physc1.byu.edu
Path: physc1.byu.edu!yvax.byu.edu!hamblin.math.byu.edu!sol.ctr.columbia.edu!spool.mu.edu!agate!ames!pacbell.com!tandem!zorch!fusion
Newsgroups: sci.physics.fusion
Subject: Discretionary Spending
Message-ID: <930126163947_72240.1256_EHL56-1@CompuServe.COM>
From: Jed Rothwell <ub-gate.UB.com!compuserve.com!72240.1256>
Date: Wed, 27 Jan 1993 04:31:22 GMT
Reply-To: Jed Rothwell <ub-gate.UB.com!compuserve.com!72240.1256>
Sender: scott@zorch.SF-Bay.ORG (Scott Hazen Mueller)
Organization: Sci.physics.fusion/Mail Gateway
Lines: 72

To: >INTERNET:fusion@zorch.sf-bay.org

Rusty Perrin and J. A. Carr have questioned whether the DoE has any legal authority to conduct CF experiments in the first place, without authorization from Congress. This is a reasonable question. I have discussed it with Members of Congress and with officials in the DoE, and as I understand it, the rules are as follows:

1. The DoE has various discretionary funds, both at headquarters and at the individual research institutions, like LANL. These represent only a small fraction of the total DoE budget (I do not know what percent). In 1989, many CF experiments were performed using these funds. (Far too many, in my opinion.)

2. Congress does not micromanage on this level. The DoE could not perform CF experiments costing $100 million, but they certainly could perform scattered work amounting to a few hundred thousand, or even a few million, as long as each experiment did not exceed the authorized discretionary limit of the worker.

3. Dr. Walter Polansky, Director of the Division of Advanced Energy Projects, has told me and many other people that his Department does have the authority to fund CF work. He said that the 1989 DoE panel, headed by Huizenga, "was sympathetic towards modest support for carefully focussed and cooperative experiments within the present funding system. The Department of Energy accepted the report and its recommendations. We have been monitoring the cold fusion research area since the issuance of that report and believe its recommendations are still valid. We continue to be available to review any research proposal of interest to the Department."

That's what the man says, but actually, as far as I can tell, Polansky is a hatchet man who goes around shutting down CF experiments, overrjding other people's spending authority, and cutting everything including your telephone if you try anything related to CF. So, if you are thinking of submitting a proposal, I suggest you steer clear of the man.

Under normal circumstances, if a scientist at LANL, Fermilab or some other DoE lab had some level of discretionary spending authority, and he or she wished to, he could conduct experiments relating to energy. As long as the experiments stayed within the allowed budget, there would be no problem. Certainly, if he could show that his Japanese colleagues were spending 10's of millions on the subject, nobody would object to a few simple replications of the Japanese work costing $10 or $20 thousand. Nothing about CF is normal, so these customs do not apply.

"Discretionary" is a loose term, subject to definition and revision. There is no such thing as perfectly 100% discretionary money, in any institution. No DoE honcho would get away with funding a study of orgasmic energy sources. The closest thing I can think of to a real Discretionary Fund is the 5 million Yen Japanese professors get just for showing up at work: when I attended a National University, I took a nice day trip or two, and used a lot of "discretionary" video and computer equipment for purposes not directly related to the Department's work. Also, we had several pet chickens and a large turtle, whose sustenance came out that funding I believe.

In most institutions, however, discretionary expenditures that get too far away from the subject would be reviewed and censured. Projects which are too wild and flakey, like ESP spoon bending energy in the DoE, or faith-healing at NIH would get anyone into trouble, which seems reasonable to me.
Unfortunately, anything remotely related to CF falls in this category: too flakey and weird to be allowed. Research institution heads and other people with discretionary authorization have been explicitly ordered *not* to spend anymoney or allow any experiments in CF. The ostensible reason is that it is too far beyond the pale, too "pathological." This may, in fact, be one of the reasons it is banned, but I believe the main reason it that the people who are fighting against it know that if it succeeds, their reputations will go down in flames; and also because the hot fusion program, and many other energy programs, will be rapidly terminated. People engaged in a turf war find it easy to convince themselves that their opponents are flakes.

- Jed

ROUTING AND TRANSMITTAL SLIP		Date: 03/31/92
TO:	INITIALS	DATE
1. Louis C. Ianniello, ER-10		
2.		
3.		
4.		
5.		

	Action		File		Note and Return
	Approval		For Clearance		Per Conversation
	As Requested		For Correction		Prepare Reply
	Circulate	X	For Your Information		See Me
	Comment		Investigate		Signature
	Coordination		Justify		

REMARKS

Lou- I received the following package of information on cold fusion late yesterday. You may be most interested in the letter (unsigned) from Jed Rothwell to Frank Murray.

There is no need to return this package.

COPY

FROM: Walt Walter M. Polansky, ER-16	Room No.-Bldg G-347
	Phone No. 3-5995

Cold Fusion Research Advocates
2060 Peachtree Industrial Court, Suite 312-F
Chamblee, Georgia 30341

Phone: 404-451-9890 Fax: 404-458-2404

March 26, 1992

Mr. Walter M. Polansky, Director
Division of Advanced Energy Projects
Department of Energy
Washington, DC 20585

Dear Mr. Polansky,

Here is some recent information from Japan regarding cold fusion, including an article from the Japanese edition of Scientific American.

Sincerely,

Jed Rothwell

Jed Rothwell

Cold Fusion Research Advocates
2060 Peachtree Industrial Court, Suite 312-F
Chamblee, Georgia 30341

Phone: 404-451-9890 Fax: 404-458-2404

March 9, 1992

The following article appeared in the Japanese edition of *Scientific American*. The title of this magazine sometimes causes confusion; the cover of says "Scientific American" in English and "Nikkei Science" in Japanese. This magazine includes every article from the U.S. edition, translated into Japanese, plus some material unique to Japan. The March, 1992 edition carried this two page description of Takahashi's work on pages 54 and 55, in the "Information" section, which covers current events and fast breaking news.

Information

Cold fusion experiment yields stable heat reaction

"It's a real reaction." "No, it is all experimental error" - the debate about cold fusion goes on. Now, Prof. Akito Takahashi, of the Osaka University Engineering Department has successfully produced a stable cold fusion heat reaction that continued for over a month. Using the now familiar method of electrolysizing heavy water with a hydrogen-absorbing palladium cathode, he reports measuring peak heat outputs several dozen times larger than the electrical input.

Prof. Takahashi's electrolysis device consists of a cathode made of a highly pure palladium plate 2.5 cm square by 1 mm thick; the anode is 0.5 mm platinum wire wrapped in a coil one centimeter from the face of the cathode. Both electrodes are submerged in heavy water.

The current flowing between the electrodes is cycled every six hours. During the first six hours, 0.25 A (low current) is input; during the next six hour segment 4.2 A (high current) is input, and then the cycle is repeated. The experiment began last year on December 15; the heat reaction began about a week into the experiment. The reaction continues as of this writing, at the beginning of February. A peculiar phenomenon has also been noted; the heat output fluctuates in a periodic fashion with each cycle.

The strength of the heat was 50 - 70 W during the low current input, and 200 - 250 W during high current input. During the low period, the output heat energy was several dozen times greater than the electric energy used in electrolysis; during high input, it was 2 to 3 times greater. Subtracting input energy used in electrolysis from the output heat energy

left a positive balance averaging about 100 W. Total energy output for the first month was said to exceed 200 megajoules.

The power density was extremely large; depending on how you measure it, power density per cubic centimeter of the palladium cathode was as much as ten times greater than the power density of a fission reactor fuel rod.

A 4 Body Reaction?

Prof. Takahashi proposes a new theory to explain the experimental results, which indicate that a nuclear reaction is occurring in the palladium lattice. According to his theory, 3 and 4 body fusion reactions occur inside the lattice, even though, under the normal density of deuterons packed into the lattice such reactions would be exceedingly rare.

A palladium lattice is a 6 sided face-centered cubic structure. A palladium atom sits in the center of each face, and at each vertex. With this structure, the area between each vertex and the area in the very center of the lattice is called the O site. As deuterons are forced into the lattice by electrolysis they occupy the O sites first. Each cube also contains 8 T sites; when all the O sites are full, the deuterons begin to occupy the T sites. When the deuterons occupy the T sites, they create deeper potential wells than with the O sites.

At this stage, when the deuterons undergo vibrational excitation from the electric current the deuterons in the O site, at a certain probability level, the deuterons begin falling into the T sites. With this lattice structure, there are four O sites around each T site, so if the deuterons in the O sites fall smoothly into the T sites, a maximum of 5 deuterons can concentrate in each T site. Takahashi postulates that in the instant this happens, the deuterons undergo fusion.

A great deal of supporting evidence

According to the proposed theory, the O sites fill up with deuterons when the loading ratio of deuterons to palladium atoms exceeds 0.85. At this point, the deuterons begin entering the T sites, and fusion begins. It has been noted that tritium, which is thought to be a fusion product, starts to be generated after the deuterons pass this level of saturation. This was seen again in the present experiment.

Also, from experiments at SRI in the U.S., and elsewhere, it has been observed that excess heat appears in many cases when the level of saturation goes over 0.9. Takahashi's theory explains this, conjecturing that at the higher loading ratio, more of the 3 and 4 body reactions begin, which output more heat than the tritium producing reactions.

Asst. Prof. Tadahiko Mizuno, of Hokkaido University, Nuclear Engineering Dept., has verified that under electrolysis, the palladium lattice has been loaded with deuterons at a ratio as high as 1.4. He has that verified that even after all O sites in the lattice are filled,

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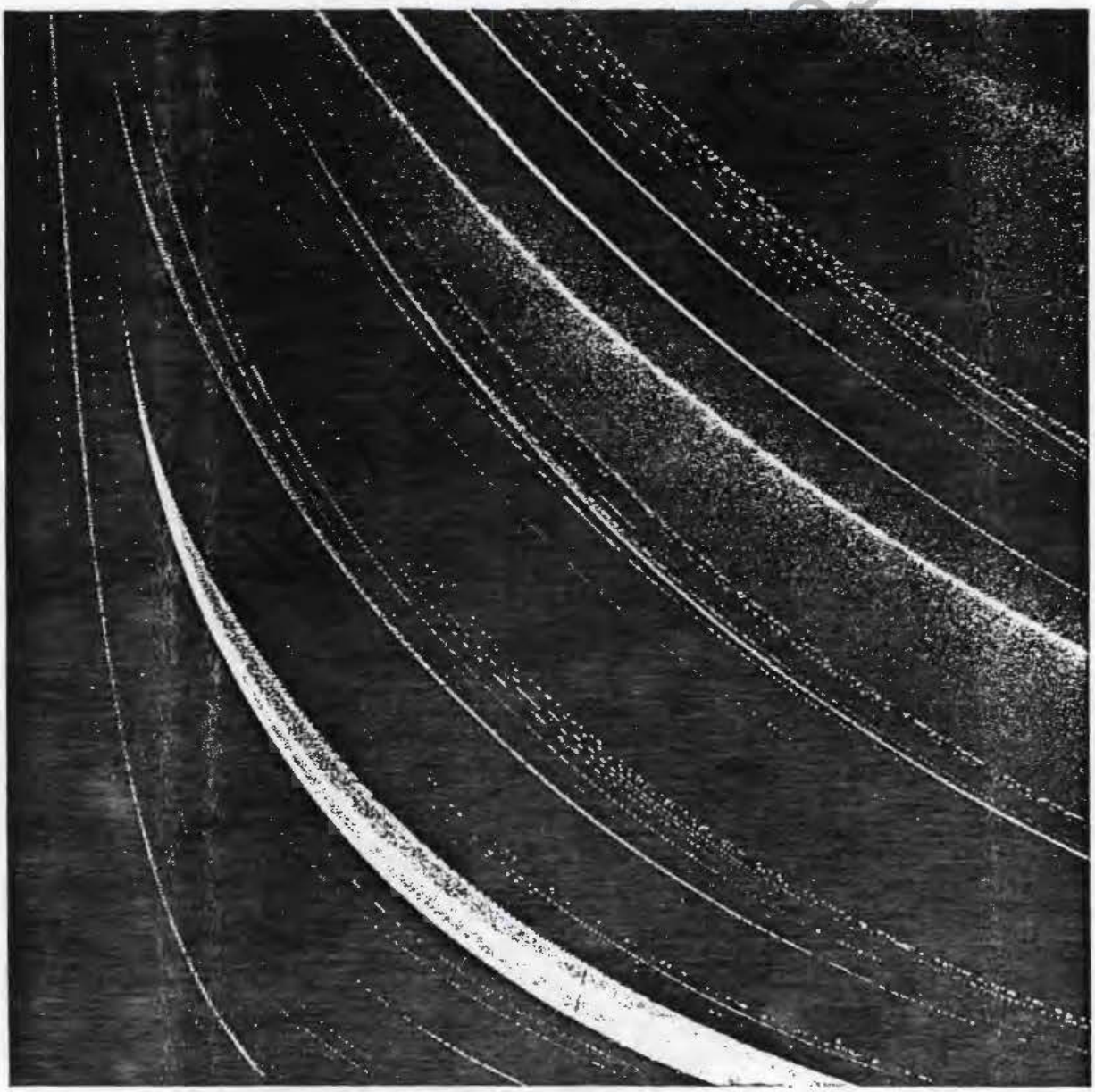
特別企画

カオス 世紀末の新理論

生命現象の解明から工学応用まで

CO_2 が増加しても，光合成量は増えない



カオスニューロンモデルの分岐図

日経サイエンス社

定価1300円
(本体1262円)

室温核融合実験で安定した発熱反応

「確かに起きている」「いや，実験誤差だ」と議論を呼んでいる室温核融合で1カ月以上安定して発熱反応を起こすことに，大阪大学工学部の高橋亮人（たかはし・あきと）教授が成功した。水素吸蔵合金として知られるパラジウムを陰極にして重水を電気分解するというお馴染みの実験系を用い，電気分解に消費した電力の最大数十倍もの発熱を観測したという。

高橋教授の電気分解装置は，厚さ1mm，縦横2.5cmの高純度金属パラジウム板を陰極にし，その表面から1cmほど離した位置に太さ0.5mmの白金線をコイル状に巻いて陽極とした。この電極全体を重水に浸し電気分解した。

電極間に流す電流の大きさは6時間おきに変える。最初の6時間は0.25A（低電流時）に，次の6時間は4.2A（高電流時）にするサイクルをくり返した。その結果，昨年12月15日の実験開始後約1週間で発熱反応が始まった。2月に入った現在もその反応は続いており，発熱量がサイクルごとに周期的に変動する奇妙な現象もみられるという。

発熱の強さは，低電流時が50～70W，高電流時が200～250Wだった。発熱量は電気分解に使った電力を上回り，低電流時は消費電力の数十倍，高電流時は同2～3倍の熱が発生した。熱出力から使用電力を差し引くと平均約100Wのプラスとなる。1月末までに発生した熱エネルギーの合計は2億Jに達したという。

発生した熱量は非常に大きく，見方によっては原子炉を上回る。原子炉の燃料棒1cm³が発生する熱出力と今回のパラジウム電極1cm³の熱出力を比較すると，今回の方が約10倍大きくなる。

4体反応が起きている？

室温核融合がパラジウム結晶中で起こるという自らの実験結果を説明するため，高橋教授は新しい仮説を提出している。それによると，パラジウム結晶内では，通常の密集度では確率的にとても起こり得ない反応，すなわち3個ないし4個の重水素が反応する3体，4体反応が起きるとしている。

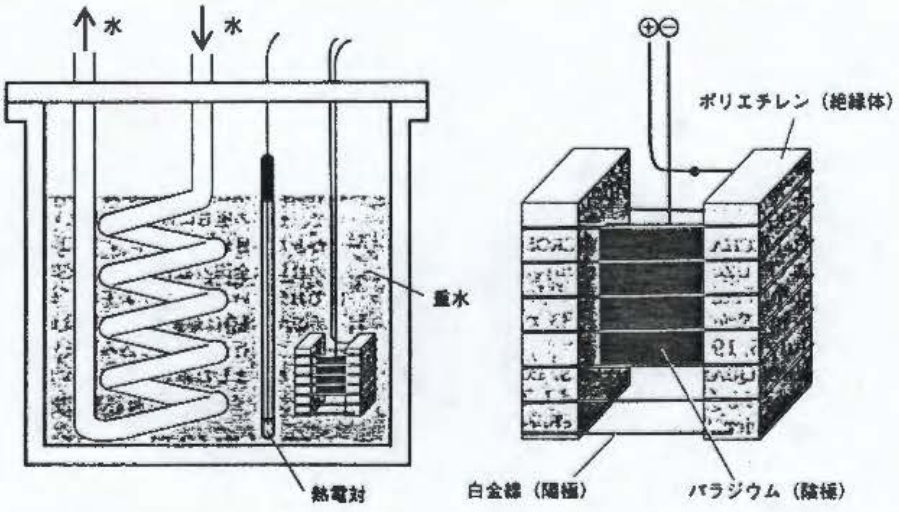
パラジウム結晶は，正6面体の各面中央と各頂点にパラジウム原子が1個ずつ位置する面心立方晶の構造をとる。この結晶は正6面体の頂点同士を結ぶ稜（りょう）の中央と正6面体の中心にO位置と呼ばれる場所がある。重水の電気分解で発生した重水素はまずここに入る。O位置が全部埋まると，重水素は次に正6面体の内部に8ヵ所あるT位置に入り始める。重水素がT位置に納まると，ここにはO位置よりも深いポテンシャル井戸が生まれる。

この段階で電流などに励起されると，O位置の重水素はある確率でT位置の井戸に落ち込む。結晶構造上，T位置の周囲に4つのO位置があるため，O位置の重水素がT位置の井戸にきれいに落ち込めば，最大5つの重水素が同じ井戸に密集することになる。この瞬間に重水素同士が核融合を起こすというのが高橋教授の仮説だ。

多くの傍証

この仮説に従えば，O位置がすべて重水素で埋まってパラジウムに対する重水素比率が0.85になった以降に重水素がT位置にも入り，核融合が起きるはずだ。今回の実験でも，この比率を超えたところで初めて核融合反応によると思われるトリチウムが出た。

また，米国スタンフォード研究所（SRI）の実験などから，重水素比率が0.9以上になったところで発熱し始めるケースが多いことがわかってきた。

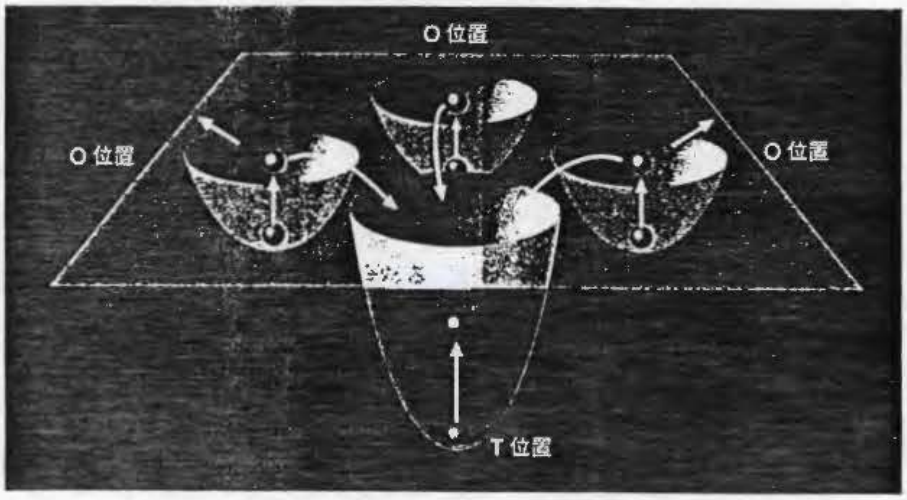


電極の工夫が，持続的室温核融合につながった

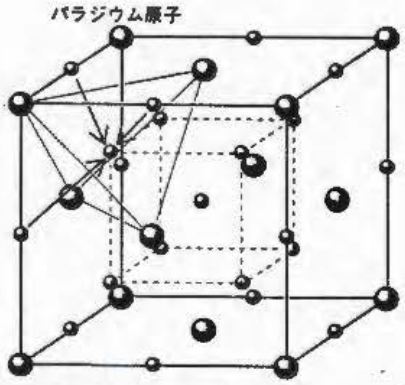
これもT位置がある程度埋まり発熱の多い3体，4体反応が増えてくるためという高橋仮説で説明できる。

北海道大学原子工学科の水野忠彦（みずの・ただひこ）助手は電気分解中のパラジウム結晶の重水素比率が1.4にまで達することを確認しており，O位置が埋まった後も重水素が結晶に入り込んでいることは確実だと話す。また，高圧状態の結晶を見る中性子回折像で重水素がT位置に納まっていることが観察されているとして，高橋仮説を支持する。

今回の実験では，核融合の証拠として，トリチウム以外にも中性子が見つかった。中性子の検出には最も精度が高い液体シンチレーターを使用した。また，クロスチェックのためヘリウム3検出器も併用した。この測定で発熱が強くなると中性子の数がむしろ減る傾向にあることが初めてわかった。高橋教授は「中性子を出さずに大エネルギーを発生する4体反応の率が増えるためではないだろうか」と推測している。



O位置にあるポテンシャルの井戸にまず重水素が埋まり，励起されるとT位置に落ち込んで3体，4体反応の確率が高まる。



●O位置の重水素　●T位置の重水素

パラジウムの結晶（面心立方晶）に重水素が組み込まれると，核融合反応が起きる可能性が出てくる。

始まった追試

これまで多くの研究者が同じパラジウム電極系を使いながら安定した発熱反応を実現できなかったことについて，高橋教授は「電極の形が適当でなかったのではないか」と話す。高橋仮説によると重水素の充填率が一定以上にならないと核融合は起きないが，この条件を実現するにはパラジウム電極に均一に重水素が入り込まなければならない。これまでの多くの実験系では，ここが不均一だったため重水素がパラジウム表面から逃げてしまったと高橋教授は見ている。

いずれにしろ，高橋教授の実験系が本当に室温核融合を安定的に実現するかどうかの確認は，他の研究者の追試を待つしかない。そしてすでに，実験結果を発表した1月27日の「電磁場における非線形現象応用国際会議」（名古屋市）を受けて，北海道大学，東京工業大学などの研究グループが追試を開始している。

高橋教授によると，米国でも複数のチームが追試に乗り出しているという。なかには，「日本政府がこの技術に重点投資をして実質的に囲い込もうとしているのではないか」などという気の早い心配をする研究者もいるそうだ。

（高木靭生・日本経済新聞大阪経済部編集委員）

日本国際賞受賞者講演会 4月に東京と大阪で開催

1992年度日本国際賞を4月27日に受賞するG. エルトゥル独マックス・プランク財団フリッツ・ハーバー研究所長とE. J. C. ポルジ英アニマル・バイオテクノロジー・ケンブリッジ科学部長が，4月25日の東京と28日の大阪の2回，受賞記念講演をする。

エルトゥル教授は1960年代から金属表面上の化学反応の研究に取り組み，反応分子と金属原子の動的な振る舞いをとらえる先駆的な業績を残した。ポルジ教授は1950年代の初めに牛の精液を凍結保存する方法を開発した。

参加希望者は国際科学技術財団（電話03-3508-7691）まで。入場は無料。

（編集部）

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March 3, 1992

From: Yomiuri Shimbun
03/02/92
[no byline]
Translation by Jed Rothwell

Low Temperature Fusion

Will It Boom Again?

[Photo caption] Prof. Akito Takahashi

[Figure captions] Cold Fusion Experiment Uses Electrolysis
Direct current
Cooling water
2.5 cm square palladium plate (cathode)
Heavy water
Insulator
Thermometer
Platinum (anode)

The report of massive heat from cold fusion experiment by Dr. Akito Takahashi, Osaka University (Dept. of Nuclear Engineering) is causing major repercussions. He reported that an electrolysis experiment output excess heat for over two and a half months. If this data is correct, the "dream of cold fusion energy," which had deflated, will once again boom. What is amazing is that this large amount of heat can still not be explained. The question is: what is going on here?

Thousands of times more heat than a chemical reaction

Osaka U. Lack of neutrons, the mystery deepens

The data was presented at an international symposium held last month in Nagoya. In this experiment heavy water is electrolysized with a palladium cathode. The object is to

pack the deuterons into the palladium, creating a condition in which fusion is more likely to occur.

According to the professor, the heat began shortly after the experiment commenced, about the middle of December, last year.

The total amount of heat averaged out over the volume of palladium came to roughly 200 watts per cubic centimeter. This is said to be about twice as much as the input energy from electrolysis.

Massive heat output has been reported from the U.S. by two teams: the originators of cold fusion at Utah U., and the team at Stanford Research, which recently suffered an accidental explosion; however, this is the first time that massive heat has been reported in Japan. Many comments were heard from the audience like, "it is too much for me to believe all at once" and "if true, this is amazing."

Even after the Nagoya meeting, researchers say that heat output continues as before. They say at present, "we could not turn off the experiment even we if wanted to." [It is stuck "on."]

What is weird about this experiment is that neutrons, which would be powerful proof that a nuclear reaction is occurring, are only seen in very small numbers. With ordinary heavy hydrogen fusion, neutrons etcetera are generated in equivalent proportion to the heat. But in this case, number of neutrons detected is only about one ten trillionth of the number theory would lead one to expect. What could be happening?

One school of thought is that the excess heat may not be caused by fusion, but by a chemical reaction. But this theory cannot explain the data from this experiment, since the total amount of heat is several thousands of times greater than any conceivable chemical reaction could generate.

Given this, Prof. Takahashi proposes that a new, previously unknown type of fusion reaction is occurring.

The professor looked at various possible kinds of fusion that do not create neutrons or tritium. As a result, he realized that his experimental results indicate that the main form of fusion that is occurring is "four body fusion," wherein four heavy hydrogen atoms fuse to form two helium atoms.

Most theorists reject his bold conjecture, skeptically claiming, "it violates common sense."

Up until now, whenever results have been completely at odds with theory, the reliability of the measurements have been called into question. In fact, in this case as well,

some people have objected to the method of measuring heat in this experiment, saying it is too rough [broad, sketchy]. Another problem is that a control experiment with ordinary (light) water was not done.

However, some of the experts voiced this opinion: "the method of measuring heat [calorimetry] is simple, but by the same token, you can also say it is very difficult to make a mistake when you do it this way. Furthermore, it is very difficult to argue with a results like this, with twice as much energy output as input."

In any case, other teams will need to replicate the experiment in order to establish the true nature of the heat. Recent research has shown that it is necessary to drive the ratio of deuterons to palladium atoms to a level of 0.9 or above in order make conditions favorable for the heat reaction to occur. This fact has major implications in cold fusion research, which has heretofore been conducted by groping in the dark [as it were]. It appears that this experiment succeeded because steps were taken to increase the permeability of the palladium, allowing more deuterons to be loaded into the palladium.

The influence of Prof. Takahashi's experiment already appears to be far reaching, as several groups here and overseas have rushed to inquire about the details, and dozens of groups are reportedly already trying to replicate.

It may be that the mystery of the excess heat will be solved sooner than anyone expects.

Yomiuri Shimbun March 2, 1992

読売新聞 （第三種郵便物認可）

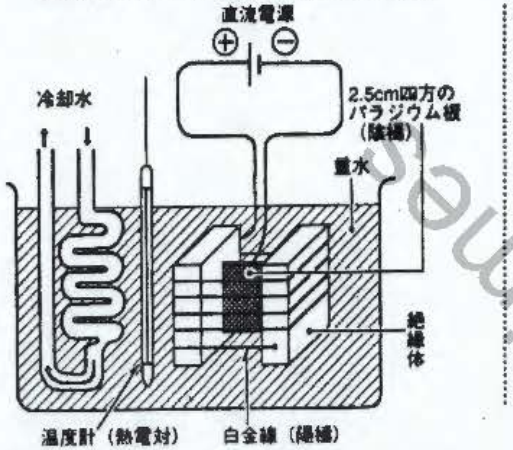
低温核融合

ブーム再び？

化学反応の数千倍発熱

大阪大 中性子出ず、ナゾ深まる

低温核融合の実験で、大阪大工学部の高橋亮人教授（原子力工学）が報告した大量発熱のデータが波紋を広げている。重水の電気分解で二か月半もの間、過剰な熱が出続けているというのだ。もしデータが正しいとすれば、一度はしぼんだ「低温核融合がエネルギー源になる夢」が再浮上する。しかし、不思議なのは、これほどの発熱は従来のどんな理論でも説明がつかないこと。一体何が起きているのか。




高橋亮人教授

このデータは、名古屋で先月開かれた国際シンポジウムで発表された。実験では、パラジウムを陰極にして重水を電気分解する。パラジウム中に重水素を詰め込み、核融合が起こりやすい状態をつくる狙いだ。

教授によると、発熱は実験の開始直後の昨年十二月中旬から始まった。

発熱量は平均でパラジウム一立方㌢当たり約二百㍗。電気分解などに要した入力エネルギーの約二倍に当たるという。

大量発熱は、低温核融合の火つけ役の米ユタ大と爆発事故が最近あった米スタンフォード研究所の二チームが報告しているが、日本の研究者では初めて。会場からは「にわかには信じられない」「本当ならすごい」という声が相次いだ。

発熱は名古屋での発表後も依然として続いており、今は「実験をやめるにやめられない状況」という。

ただ奇妙なのは、核反応が起きた有力な証拠になる中性子がごく少量しか検出できなかったこと。重水素同士の普通の核融合では、中性子などが発生、同時にそれに相当した熱が出る。しかし今回は、発熱量から理論的に予測される数の約十兆分の一の中性子しか出ていない。では何が起きたのか。

第一の可能性は、発熱の原因が核融合ではなく、化学反応だという考え方。だが、この説では今回のデータは説明できない。発熱量が、化学反応で考えられる最大の発熱レベルの数千倍にも達するからだ。

そこで高橋教授が提案するのが、未知の核融合反応が起きている可能性。

教授は中性子やトリチウムができない核融合の可能性を探った。その結果、重水素四個が反応してヘリウム二個ができる「四体核融合」が主に起きたとすると、実験がうまく説明できることが分かったという。

ただ、この大胆な仮説に対し、ほとんどの理論家は「常識的にはとても考えられない」と否定的だ。

従来のどんな理論でも説明できないとなると、熱測定のデータの信頼性に対する疑問も出てくる。実際、今回の熱測定の方法は大ざっぱだという批判があり、普通の水（軽水）を使った比較実験をしていないなどの問題もある。

しかし、専門家の中には「測定法は確かに単純だが、逆にミスの出にくい方法とも言える。まして入力の二倍もの発熱量となると反論は難しい」という声もある。

いずれにせよ、発熱の正体を明らかにするには、他のチームによる再現実験が必要になるが、最近の研究で、パラジウム中の重水素の割合を〇・九以上にすると熱が出やすいらしいことが分かってきた。手探り状態が続いてきた低温核融合の実験では大きな意味を持つ手がかりだ。今回の実験が成功したのも、重水素の吸蔵量を増やすため電極の形状などを工夫した点が効いたらしい。

実際、高橋教授の実

scienceランド

対する反響は大きく、国内でに再現実験を始めた子ームもあるという。

外の研究者や企業から数十件の問い合わせが殺到、す

るのは意外に早いかもしれない。

発熱のミステリーが解け

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March 18, 1992

Mr. Frank Murray
Energy Subcommittee
U.S. House of Representatives
Washington, DC 20515

Tel: 202-225-8056
Fax: 202-225-2021

Dear Mr. Murray,

Attached is an article from this morning's Wall Street Journal about EPRI's cold fusion research program.

Here is some news from Japan:

Last night I had a long talk with the science journalist Fujio Nakano. In March 1992, Nakano published another article about cold fusion in Japan's largest magazine, Bungeishunju, in which he stated publicly that the Japanese research thinktank "Technova" is supporting Pons and Fleischmann in France. I asked him about that, and he said that the president of Technova, Yoshihiro Kyotani, decided to go completely public and reveal the research. Kyotani used to be a top researcher at Japan Railroads (the national railroad system).

Technova is a somewhat shadowy R&D organization. Like EPRI, it controls enormous capital resources, nobody knows how much. I believe it is connected with Toyota, as well as Japan Railroads. It does research, information gathering, management consulting; it forms new industrial policies, management systems, and promotes technology transfer between Japan and other countries. The board of directors includes several famous people: Japan's top architect, a famous former foreign minister, and several distingushed European and American scientists. For more information, I suggest you contact Technova directly:

Tel: 03-3508-2280
Fax: 03-3508-7578

Nakano has the impression that if you fax them on letterhead and ask for information on their cold fusion research and company background, they will give it to you. He says they have decided to go completely public.

Nakano said that since the January 27th meeting, "things have been hopping." The power companies and big corporations are rushing to jump on the cold fusion bandwagon, fearing that Technova will get a lead on them. Dr. Mizuno, of Hokkaido University, wrote to me on March 3, saying that circumstances within Japan improved immediately after the ISEM meeting; interest picked up a great deal. He said, "10 big companies are starting a fund for cold fusion researchers in Japan." Before January, over 100 researchers were working on cold fusion in the National University laboratories, but they did not attract much attention. Now, they are in the limelight and in demand; corporations and science writers are rushing to ask their advice and publish their technical papers.

I asked Nakano, "how about the skeptics in Japan, is there still resistance at Tokyo University and elsewhere?" He said, "there are no more skeptics. Everyone now believes it is real. The only question is, has Takahashi developed a simple, reliable way to create a sustained, high output reaction. We will find out in a couple of weeks, as the replications roll in."

After I talked to Nakano, I called the editor and one of the staff writers of the Nikkei Superconductor technical newsletter. I asked the same question: what are the skeptics saying? Both the editor and the writer responded at once: "the skeptical resistance has collapsed. There are no skeptics. We have not heard a peep out of Arima since last summer when he said he is too old to shave his head, and he said he guessed he is not a real physicist anymore." [Arima is the President of Tokyo University. In April 1989 he said, "if cold fusion is as easy to get as they say, I will quit physics, shave my head, and become a buddhist monk." Finally, last summer, he admitted he was wrong, but he does not want to make a clear public statement, because he would lose face.]

The Nikkei people just published another column from me, and they asked me for additional news. I find it ironic that I cannot get any American newspaper or magazine, except Business Week, to publish anything about cold fusion in Japan, whereas the Japanese Nikkei (equivalent to the Wall Street Journal) calls me every few weeks, and publishes all the news about U.S. research that I give them! It is the same old story, we pay no attention to what happens in Japan, whereas they watch us with an eagle eye. You would think that since Japan has taken away our entire consumer electronics market, our RAM chip market, and at least a third of our automobile market, that somebody would be interesting in hearing what they are doing in the energy business. Alas, few journalists will touch news about cold fusion.

I should add, I find it very odd that Members Of Congress are not alarmed and mystified to hear that Dr. Ikegami, a well-know nuclear physicist and the head of a major high-tech research program, has offered to come over and testify before the Committee. He

wants to warn us that Japan is about to take the lead in yet another critical technology. When was the last time a high government official in Japan offered to give you detailed information about a top priority program, and to warn you that the program might hurt the U.S. economy if you do not fund similar research here? This seems to me to be a wholly unprecedented circumstance. I would say you have an alarm bell clanging in your ears, and if you ignore it much longer, it will lead to grave political and economic consequences for you and for the nation.

I asked Nakano and others: "Who is behind Technova exactly? How much money do they have?" and "what is MITI going to do?" They gave me the usual run-around, "we can't talk about that yet, give us a couple more months." Other people have told me that the MITI cold fusion industrial development program will start in April, 1992, which is the beginning of the Japanese fiscal year.

Sincerely,

Jed Rothwell

Electric Power Research Institute to Pay $12 Million More to Study 'Cold Fusion'

By JERRY E. BISHOP
Staff Reporter of THE WALL STREET JOURNAL

The utility-supported Electric Power Research Institute said it will spend another $12 million to investigate "cold fusion" because of promising experimental results.

EPRI, which has put about $2 million into "cold fusion" experiments, continues to be the only source in the U.S. of funds for the highly controversial research. It is investing the additional funds because of a key experiment's "favorable results in producing heat," said EPRI project manager Thomas O. Passell.

The U.S. Department of Energy dropped all such support in late 1989 and the State of Utah, which spent $5 million on such research at the University of Utah, ended its support last year.

The key experiment, at SRI International in Palo Alto, Calif., was involved in an explosion this past Jan. 2 that killed one scientist and injured two others. Mr. Passell declined to discuss the explosion pending the report of an investigation. He said, however, that EPRI's decision to continue funding the SRI experiment was made in December before the explosion, but wasn't announced at the time.

SRI International is a nonprofit independent contract research and consulting think tank that was spun off from Stanford University several years ago. EPRI, which is supported almost wholly by the electric utility industry, spends about $400 million a year on electricity-related research and development.

The SRI experiment, under the direction of Michael C.H. McKubre, is an attempt to replicate the highly controversial claims made at the University of Utah in early 1989 by American chemist B. Stanley Pons and his British colleague, Martin Fleischmann. They claimed that a simple table-top electrolysis experiment, in which a palladium rod encircled by a platinum wire was immersed in "heavy" water, produced more energy as heat than was put into it electrically. They asserted that the heat was coming from energy released by the fusion of "heavy" hydrogen atoms inside the palladium rod.

For three years, nuclear physicists have charged that the two chemists were grossly mistaken, that the lack of nuclear radiation was clear evidence that fusion didn't occur and that their measurements of "excess" heat were in error.

In the SRI experiment, the amount of excess heat produced "wasn't a lot but it was definite," Mr. Passell said. He said the experiment has produced one to two watts of heat, which is as much as 10% more power coming out of the experiment than is being put into it electrically.

Dr. McKubre and his colleagues have repeated the experiment "perhaps 10 times with varying degrees of success," Mr. Passell said. "What the excess heat is due to we don't know, but we're holding out the possibility it is a nuclear reaction of some sort although not the usual kind" that scientists see when heavy hydrogen atoms fuse at sun-like temperatures, he said.

An EPRI spokeswoman noted that the institute carefully avoids the term "cold fusion," but instead uses the phrase "potential nuclear phenomena in deuterated metals." "Deuterated metals" refers to the fact that the experiments involve saturating metals, particularly palladium, with nuclei of deuterium, or "heavy hydrogen," atoms.

The $12 million pledged by EPRI would be dispensed over three years or as long as the research pointed toward some kind of new energy source. "But if we discover at any time that [the source of the heat] isn't nuclear in nature then we can stop" funding the research, Mr. Passell explained.

A final report on the SRI accident in January isn't expected for several more weeks and officials at both EPRI and SRI decline to speculate on what caused it. But reports circulating among "cold fusion" researchers suggest the cause wasn't nuclear but rather a build-up of the oxygen and hydrogen gases that went undetected when a pressure gauge failed.

File- Cold Fusion

Cold Fusion Research Advocates
2060 Peachtree Industrial Court, Suite 312-F
Chamblee, Georgia 30341

Phone: 404-451-9890 Fax: 404-458-2404

February 12, 1992

Mr. Walter M. Polansky, Director
Division of Advanced Energy Projects
Department of Energy
Washington, DC 20585

Regarding: Your letter of February 10; copy attached

Dear Mr. Polansky,

In your letter, you state that, "the cold fusion panel was sympathetic towards modest support for... experiments." I am in communication with every top cold fusion scientist in the world, and to the best of my knowledge the Department of Energy does not sponsor *any* research in cold fusion, at any laboratory. If I am incorrect, please supply me with names and telephone numbers of researchers working in this area.

You also say, "we believe that the appropriate mechanism for establishing the feasibility of... a scientific result is for the proponent to submit it to the collective judgement of peers." That is exactly what has happened *in Japan*. That is why the Japanese government spent at least $15 million on cold fusion last year; that is why Toyota has given Pons & Fleischmann carte blanche development resources; and that is why the top researchers in Japan expect that MITI will begin full scale industrial development of cold fusion *this year*. If you have any doubt about this, call me and I will immediately put you in touch with leaders of the Japanese development programs. These leaders have volunteered to testify before Congress, where they will spell out the scientific facts they believe proves the existence of cold fusion beyond any reasonable doubt.

I enclose various documents that prove what I have written here. Please contact me if you require any additional information.

Sincerely

Jed Rothwell

Jed Rothwell

cc:
Ms. Sally Kelly, Director of Agency Liaison, The White House
Congressman George E. Brown, Chairman of the Space, Science & Technology Committee
Congressman Dick Swett, member of the Space, Science & Technology Committee
Dr. Hideo Ikegami, Director of Cold Fusion program, National Institute for (hot) Fusion Science, Japan

Department of Energy
Washington, DC 20585

February 10, 1992

Mr. Jed Rothwell
2060 Peachtree Industrial Court
Suite 312-F
Chamblee, Georgia 30341

Dear Mr. Rothwell:

The Department of Energy has been asked to respond to your letter, with several enclosures, to President Bush dated January 10, 1992, that expressed concern about cold fusion research in the United States. Your letter and information on cold fusion have been reviewed. We have been aware of the activities you referenced including the effort to establish a $10 million per year research program on cold fusion.

We believe that the appropriate mechanism for establishing the feasibility of a new, or unexpected, scientific result is for the proponent to submit it to the collective judgement of peers. This mechanism can be activated in several ways, such as technical presentations at scientific conferences and publications of results in prestigious, peer-reviewed journals. This is a thorough, but time-consuming process. However, all major scientific advances have been subjected to, and have survived, this scrutiny.

The November 1989 report of the cold fusion panel recommended against any special Department of Energy funding for the investigation of phenomena attributed to cold fusion. However, the panel was sympathetic toward modest support for carefully focussed and cooperative experiments within the present funding system. The Department of Energy accepted the report and its recommendations. We have been monitoring the cold fusion research area since the issuance of that report and believe that its recommendations are still valid. We continue to be available to review any research proposal of interest to the Department.

Sincerely,

Walter M. Polansky

Walter M. Polansky, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

cc:
Ms. Sally Kelley
Director of Agency Liaison
The White House

A Petition

Submitted to the Science, Space and Technology Committee,
The House of Representatives, Washington, D.C.

We, the undersigned scientists, engineers, and interested citizens, respectfully request that the House Committee on Science, Space, and Technology, hold intensive hearings on the topic of the new physical phenomenon that is referred to generally as *cold fusion*. The purpose of these hearings, which in depth and breadth should go far beyond the hearing of the Committee that occurred in April 1989, should be to assess the enormous body of scientific evidence that has accumulated and *continues* to accumulate since that time, both in U.S. and foreign laboratories, public and private. These hearings should occur as soon as possible, because the lack of funding in the United States is seriously blocking research.

We are convinced that a new physical phenomenon of potentially enormous scientific and technological significance has been discovered and verified -- even though its precise physical mechanism is not fully understood at present, a typical circumstance in science. We believe that it will be imperative for the Congress to recommend immediate, significant funding for cold fusion research and development. This research has been substantially hampered in the United States by the negative and presently untenable conclusions of the ERAB "Cold Fusion Research" report that was performed for the Department of Energy in 1989. We recommend that the Committee examine and formally reject the conclusions of that report. We also advocate that the Committee recommend adequate funding for cold fusion research in the United States at an initial annual rate of not less than $10 million.

Signed

Name: _____
(Please sign, then print name)

Affiliation: _____
(For identification purposes only, does not indicate support of the petition by the organization)

Date: _____

Please return to: Cold Fusion Research Advocates
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Chamblee, Georgia 30341
Phone: 404-451-9890 Fax: 404-458-2404

Cold Fusion Research Advocates
2060 Peachtree Industrial Court, Suite 312-F
Chamblee, Georgia 30341

Phone: 404-451-9890 Fax: 404-458-2404

January 25, 1992

The Cold Fusion Petition has been signed by over 300 people, including:

Nobel Laureate in Physics Dr. Julian Schwinger
Dr. Arthur C. Clarke, noted science author, and Chancellor, Internat. Space Univ.
M.I.T. senior scientists Drs. Kolm, Labitt, Rafuse and Dr. Covert, who served on the Challenger Investigation.
Dr. Hawkins of Smithsonian and Boston U. (retired)
Noted hot fusion scientist Dr. Frederick Mayer
Dr. Fritz Will
Dr. Gerald S. Hawkins, D.Sc., Smithsonian (Ret.) Prof. Astronomy, Boston U. (Ret)
Scientists from Los Alamos, Oak Ridge, Naval Weapons Center at China Lake, Naval Research Laboratory, Naval Ocean Systems Center, Texas A&M, U. Hawaii, Tufts, Univ. of Utah, Rockwell, Dow, Seimens, U.S. Army, Smiths Aerospace, Motorola, and many other university and corporate research laboratories.
Dr. Ikegami, head of Japan's largest program at the National Institute
Dr. Srinivasan, head of India's major program
Dr. Iyengar, Chairman, Atomic Energy Commission, Government of India
Dr. Subbiah Arunachalam, Editor, Indian Journal of Technology, New Delhi, India
Dr. Xing Zhong Li, Dir. Fusion Power Prog, Tsinghua U., Bejing, China
Dr. V.A. Tsarev, Lebedev Physical Institute, Academy of Sci., Russia
Many of the top workers from Italy

Petition signed by 336 people as of February 10, 1992

240 Scientists and Engineers -

Dr. Radoslav Adzic	Institute of Electrochemistry, ICTM, Yugoslavia
Mr. D. P. Agarwal	Vice President, Leach and Garner Technology
Ms. Joyce L. Alms	Nova Resources Group, Inc.
Mr. Richard L. Amruso	Project Engineer, Smiths Aerospace, Inc.
Dr. George Andermann	Dept. of Chemistry, University of Hawaii
Dr. John J. Antal	Physicist, U.S. Dept. of the Army (Retired)
Mr. George Anthony	Physicist
Dr. Spallone Antonio	INFN - LNF, Franscati, Italy
Dr. John Appleby	Texas A&M University
Dr. Subbiah Arunachalam	Editor, Indian Journal of Technology, New Delhi, India
Ms. Edith E. Attenhofer	Technical Librarian, Smiths Aerospace, Inc.
Mr. Srinath Balachandran	Engineer, Energy Investment
Dr. Marcello Baldo	INFN - Catania, Italy
Mr. Thomas D. Barnard	Dow Corning Corp - Research Engineer
Dr. Robert W. Bass	Registered Patent Agent 29, 130
Mr. David R. Bean	Metropolitan State College of Denver
Dr. C. A. Bennett	University of North Carolina - Ashville
Mr. Christopher Bentley	Computer Consultant
Mr. William J. Bernt	Director of Research, Magnetic Power, Inc.
Dr. Alan Berrick	Magnetic Power, Inc.
Mr. Robert A. Birmingham	R&D Technician, Smiths Aerospace, Inc.
Dr. John O'M. Bockris	Texas A&M University
Dr. Tullio Bressani	Dep. De Fisica Sperimenatale, U. Torino, Italy
Mr. Glen Brickey	Manager of Software/CAE Design, Smiths Aerospace, Inc.
Mr. Seva Brodsky	Electrical Engineer, Graduate Student, Northeastern U.
Mr. Allen O. Brosius	Fusion Information Center
Mr. Herbert A. Bruzel	Mechanical Designer, Smiths Aerospace, Inc.
Dr. Romulus V. Bucur	Institute of Chemistry, University of Uppsala, Sweden
Mr. Michael A. Burke	Consulting Software Engineer
Mr. Forrest C. Burns	Retired Nuclear Chemist
Dr. Ben Bush	Naval Weapons Center, China Lake, CA
Dr. Robert T. Bush	Professor of Physics, California State Polytechnic Univ
Mr. Kant W. Campbell	Senior Programmer
Mr. Dean W. Carver	Fusion Information Center
Dr. Milton A. Chace, P.E.	Chace & Associates Engineering
Dr. Chun-Ching Chien	Texas A&M University
Dr. Scott R. Chubb	Private Citizen; Naval Research Laboratory
Dr. Talbot A. Chubb	Research Systems Inc, Fellow of American Physics Soc.
Dr. P. L. Cignini	CNR-CTCAT, Dept Chemistry, Univ. "La Spaienza," Italy
Dr. Arthur C Clarke, CBE	Noted Science Author, Chancellor, Internat. Space Univ.

Ms. Clair F. Coleman	Fusion/Energy Advisory Council
Mr. Neil J. Condosta	Project Engineer Group Leader, Smiths Aerospace, Inc.
Mr. William Connors	Principal Materials Engineer - Aerospace
Mr. Frank M. Costanza	Engineer, Smiths Aerospace, Inc.
Dr. Eugene E. Covert	M.I.T., Prof. of Aeronautics and Astronautics
Mr. Roger Craddock	Project Engineer, Smiths Aerospace, Inc.
Dr. Dennis Cravens	Physicist
Mr. Henry P. Dart III, Atty	Attorney, geologist
Dr. Celani De Francesco	Instituto Nazionale Fisca Nuleare, Italy
Dr. David W. DeBerry	Radian Corp., Austin, TX
Mr. Jesse J. Dean	Config. Management Specialist, Smiths Aerospace, Inc.
Mr. John D. Deith	QMI
Mr. Emilo Del Giudice	Instituto Nationale Fisca Nucleare, Italy
Mr. Michael Di Marco, P.E.	1984, 88 & 90, Congressional Dem. Prim. Candidate
Mr. Louis Dick	CERN (Geneva) and Milano University
Mr. Lee Dilley	Comdisco Systems, Inc.
Mr. Lawrence Dodson	Design Engineer, Smiths Aerospace, Inc.
Dr. Xavier Domenech	Universitat Autonoma Barcelona, Spain
Mr. A. M. Dougill	Group Leader, Smiths Aerospace, Inc.
Mr. Douglas Drake	KMS Fusion (contractor)
Mr. Jerome Drexler, D.Sc. Hon.	Inventor, industrialist
Mr. Lee John Droege	President, Snowflake Mtg, Ltd.
Mr. Thomas F. Droege	Fermilab
Dr. Robert D. Eagleton	California State Polytechnic University
Mr. James H Elkins	Consulting Engineer
Mr. Paul D. Epstein	Epstein And Fass Associates
Dr. Samuel Faile	Cold Fusion Products
Dr. Avard F. Fairbanks	Electrical Eng. & Physicist, Fusion Information Center
Dr. John J. Farrell	Franklin and Marshall College
Mr. D. J. Fazioli	Mgr, Mechanical Engrg, Smiths Aerospace, Inc.
Dr. Al Fermelia	Nova Resources Group, Inc.
Mr. Mark Ferris	Electric Tech., Metrology Lab, Smiths Aerospace, Inc.
Mr. Harvey Fiala	Rockwell International
Mr. Mark Fletcher	Engineer, Smiths Aerospace, Inc.
Mr. Fred B. Folsom	Mechanical Engineer, Smiths Aerospace, Inc.
Dr. Harold L. Fox	Fusion Information Center
Mr. Alfred J. Gartman, Jr.	Engineer, Mat'l Analyst, Smiths Aerospace, Inc.
Mr. Russ George	Research, Inc.
Mr. Jose Giner	Giner, Inc.
Ms. Judith Gluck	Chemist, Inst. of Isotopic & Molecular Tech, Romania
Dr. Peter Gluck	Institute of Isotopic & Molecular Technolgy, Romania
Mr. Mark Goldes	Magnetic Power, Inc.
Dr. Alfred D. Goldsmith	Physicist
Mr. Daniele Gozzi	Dpt. of Chemistry, Univ. "La Sapienza", Rome, Italy

Dr. Barry Green	Physicist
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Mr. Tom Green	Manager Technical Publications, Smiths Aerospace
Mr. Alfred A. Gugelmann	Lic phil nat (Physics), Thalwil, Switzerland
Dr. Lin Guang Hai	Texas A&M University
Ms. Kathy Haigh	Engineering Admin. Asst., Smiths Aerospace, Inc.
Dr. Maurice B. Hall	National Bureau of Standards, Retired
Mr. Roger P. Ham	Science columnist, Executive Intelligence Review
Dr. Peter H. Handel	University of Missouri, St. Louis
Mr. Grant B. Harris	Workframe, Inc.
Mr. Frank Hasiwar	Mathematician, Rio Hondo, CA
Dr. Gerald S. Hawkins, D.Sc.	Smithsonian (Ret.) Prof. Astronomy, Boston U. (Ret)
Col. Meredith E. Hendricks	U.S. Army, Retired
Dr. Dalibor Hodko	Texas A&M University
Mr. Stuart Hollander	Computer Consultant
Dr. Heinrich Hora	CERN, Switzerland
Mr. William M. Horton	Logistics Section Leader, Smiths Aerospace, Inc.
Dr. Hideo Ikegami	National Institute for Fusion Science, Nagoya, Japan
Mr. Paul William Isidisin	Civil Engineer, Bayonne New Jersey
Dr. P. K. Iyengar	Chairman, Atomic Energy Commission, Gov. of India
Mr. Neil R. Jacob	Senior Design Engineer, Smiths Aerospace, Inc.
Dr. King-Tsai Jeng	Texas A&M University
Dr. Shang-Xian Jin	Graduate School, Chinese Academy of Science
Mr. Dennis A. Jones	Software Engineer, Smiths Aerospace, Inc.
Dr. Walter Juda	Electrochemist, Chairman of Board, Prototech Co.
Mr. Charles Kaminski	Concerned citizen
Mr. Enloe W. Kay	Sr. Reliability Engineer, Smiths Aerospace, Inc.
Mr. Kevin Kelleher	Research Engineer, Smiths Aerospace, Inc.
Dr. John Kenny	Prof. of Physics, Bradley University, Peoria, IL
Dr. Norbert J. Kertamus	Southern California Edison
Dr. Boris M. Khudenko, P.E.	Khudenko Engineering, Inc.
Mr. James A. Kidd, Jr., P.E.	Private citizen; U.S. Army Materials Technology Lab.
Dr. Scott Kimbrough	University of Utah
Mr. James B. Klay, Jr.	Design Assurance Group Leader, Smiths Aerospace, Inc.
Dr. Henry H. Kolm	M.I.T. Senior Scientist (emeritus), Lecturer
Mr. Roland Kolter	Physicist
Mr. Melvin Labitt	M.I.T.
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Mr. Al Leedahl	Engineering Design Concepts
Dr. Jiaquan Li	Asst. Research Prof., Southwestern Inst Physics, China
Dr. Xing Zhong Li	Dir. Fusion Power Prog, Tsinghua U., Bejing, China
Dr. Bor Yann Liaw	University of Hawaii at Manoa
Dr. Bruce Liebert	Associate Professor, University of Hawaii
Dr. Charles W. Lindenmeier	Siemens Nuclear Power Corp.

Dr. Boguslan Lipinski	Tufts University, Boston, MA
Mr. Angel L. Lozano	Northrop
Dr. Eugene F. Mallove	M.I.T., Engineer and author
Mr. Alfred S. Marotta	Physicist, U.S. Army Materials Technology Lab. (Ret.)
Mr. John Marshall III	Surface Solutions, Inc.
Mr. Juan Martinez	Design Engineer, Smiths Aerospace, Inc.
Mr. Alex Matassa	Chemist, H.B. Fuller Co.
Dr. Gregory L. Matloff	Long Island University / City University of New York
Dr. T. Matsumoto	Hokkaido University, Japan
Dr. Frederick Mayer	Mayer Applied Research, Inc.
Mr. Neil McCanney	Principle Development Engineer, Smiths Aerospace, Inc.
Mr. Harold D. McKee	Software Engineer, Smiths Aerospace, Inc.
Dr. J. Rand McNally, Jr.	Fusion Energy Consultant
Dr. Ravi Kumar Meruva	Texas A&M University
Mr. E. A. Mettenet	Consultant, Utah Fusion/Energy Advisory Council
Dr. Melvin Miles	Naval Weapons Center
Mr. Laddy M. Miller	Group Leader, Smiths Aerospace, Inc.
Ms. Ljiljana Minevski	Texas A&M University
Mr. Zoran Minevski	Texas A&M University
Dr. Tadahiko Mizuno	Faculty of Engineering, Hokkaido University, Japan
Dr. Richard S. Moog	Franklin and Marshall College
Dr. John W. Moore	New Business Development Manager, Dupont R&D
Dr. Oliver J. Murphy	Lynntech, Inc.
Dr. T. S. Murthy	Dir. Isotope Group (Retired) & Consultant, BARC, India
Mr. Dean S. Musgrave	Director, Ultrafibre, Inc.
Dr. He Muzhi	Texas A&M University
Dr. Thomas Najarian	Physician
Mr. Fujio Nakano	Journalist author, Japan
Mr. Ronald Newman	BBN Software Products
Dr. Ken-ichiro Ota	Yokohama National University, Japan
Dr. Eric H. Ottewitte	Idaho National Engineering Laboratory
Mr. David A. Paddock	Engineer
Dr. Evelina Palibroda	Instutute of Isotopic & Molecular Technology, Romania
Dr. Stephen J. Pike	Nova Resources Group, Inc.
Mr. Deane H. Platt	Systems Analyst
Mr. William J. Pollard, P.E.	Pollard Engineering Company
Mr. Richard D. Powell	Lab Technician, Smiths Aerospace, Inc.
Dr. Giuliano Preparata	Universita' Di Milano, Italy
Dr. Lee Pritchard	Oakland University
Mr. Louis G. Puls, Jr.	Museion Research Corp.
Mr. Lawrence G. Quintana	Los Alamos National Laboratory
Dr. Robert P. Rafuse	M.I.T. Lincoln Laboratory
Dr. M. Ragheb	University of Illinois at Urbana - Champaign
Mr. Evan Ragland	Engineer

Dr. Raj K. Rajamani	University of Utah
Mr. Chip Ransford	Nova Resources Group, Inc.
Mr. Michael J. Ravnitzky	Concerned citizen, Alliant Techsystems, Inc.
Dr. John R. Reitz	Independent consultant
Dr. Abdu Reyimjan	Texas A&M University
Mr. Clayton R. Robert	Systems Analyst, Quantum Mechanics
Mr. Stephan A. Roen	Patent Attorney
Mr. Richard G. Rohde	Staff Programmer, State of California
Mr. Charles Ross	Kansas City Power & Light Co.
Mr. Andrew E. Rothovius	Columnist, Peterborough (NH) Transcript
Mr. Jed Rothwell	Cold Fusion Research Advocates
Mr. Donald E. Rykken	Perfection Dental Laboratory, Inc.
Dr. Carlos Sanchez	Universidad Autonoma, Madrid, Spain
Dr. Gary Sandquist	University of Utah
Mr. William J. Sauber	President, Aquari Corp.
Dr. Franco Scaramuzzi	ENEA, Frascati, Italy
Mr. Karl Scheucher	Retiree from TRW - R&D Engineer
Mr. Karl F. Scheucher	Modtech Corp.
Dr. Glen Schoessow	University of Florida
Dr. Julian Schwinger	Nobel Laureate in Physics, UCLA
Dr. Charles D. Scott	Chemical Engineer
Mr. Cliff J. Scribner	Motorola, Inc. Mechanical Engineer
Ms. Carole Seffrin	Secretary, Smiths Aerospace, Inc.
Dr. Nobuo Shimazu	Physicist, NTT LSI Laboratories, Japan
Dr. V. K. Shrikhande	Neutron Physics Division, BARC, India
Dr. Massoud Simnad	University of California in San Diego
Dr. M. Srinivasan	Head, Neutron Physics Division, BARC, Bombay, India
Dr. Supramaniam Srinivasan	Center for Electrochem. & Hydrogen Resrch, Texas A&M
Mr. George A. Stage	Orion Research
Dr. Michael R. Staker, P.E.	Private citizen; U.S. Army Materials Technology Lab.
Dr. Frank Stenger	Dept. of Computer Science, University of Utah
Mr. Mark L. Stevens	M.I.T. Lincoln Laboratory
Dr. Edmund K. Storms	Los Alamos National Laboratory
Mr. Roger Paul Stout, P.E.	Motorola Inc. Advanced Packaging Design
Mr. Veni Sumaria	Sumaria Systems, Inc.
Dr. Mitchell R. Swartz	Jet Technology
Dr. A. Takahashi	Osaka University, Dept. of Nuclear Engineering
Dr. Carol Talcott-Storms	Los Alamos National Laboratory
Dr. Donald C. Taylor	President, SRM Corporation, Denver, CO
Dr. Charles Tennakoon	Texas A&M University
Mr. Doug Tennant	Program Manager, Smiths Aerospace, Inc.
Mr. Daniel H. Thompson	Motorola, Inc.
Mr. E. C. Tibbals	Vaughn Precision Products, Inc.
Mr. Christopher P. Tinsley	Computer Engineer

Mr. Kent Tolley	Rockwell International
Dr. Massimo Tomellini	Dept. Chemistry, Univ. di Roma, Italy
Dr. Marcello Truzzi	Dept. of Sociology; Eastern Michigan University
Dr. V.A. Tsarev	Lebedev Physical Institute, Academy of Sc., Russia
Dr. Han S. Uhm	Naval Surface Warfare Center
Dr. G. R. Valenzuela	Naval Research Laboratory, Washington, DC
Dr. Stanley H. Vegors, Jr.	Physics Department, Idaho State University
Dr. Gopala Venkateswaran	Applied Chemistry Div., BARC, Bombay, India
Mr. James D. Vera	Engineer
Mr. J. L. Waisman	Consultant, Southern California Edison
Ms. Laura R. Walker	Engineer, Smiths Aerospace, Inc.
Dr. Fritz G. Will	University of Utah
Mr. Steven E. Windner	R&D, Smiths Aerospace, Inc.
Mr. Joe K. Winner	Manager of Fuel Syst. Design, Smiths Aerospace, Inc.
Dr. Michael Wixom	KMS Fusion 1984 - 1991
Mr. Larry Wolf	R&D Lab, Smiths Aerospace, Inc.
Mr. She-Sheng Xue	Instituto Nationale Fisca Nucleare, Italy
Mr. Don Yansen	Crossroads Technology
Mr. Frank Yashar	Digital Instruments - Santa Barbara, CA
Dr. Jerry Zenger	Univ. of Utah, Engineering Experiment Station
Dr. Sheng-Bai Zhu	Texas Tech University
Ms. Charlotte Zimmerman	Los Alamos National Laboratory
Dr. Dorel Zugravescu	Director Inst. of Geodynamics; Memb. Romanian Acdmy
Dr. Paul G.H. Zugravescu	General Mgr, Chiminform Data, S.A., Bucharest Romania

96 Concerned Citizens -

Ms. Riehara L. Albert	Configuration Management Clerk, Smiths Aerospace, Inc.
Mr. A. Amokrane	Executive Manager
Ms. Marie Claire Amokrane	Concerned citizen
Mr. John G. Bailey	Schiller Institute
Mr. Robert W. Barton	Robert W. Barton & Associates
Mr. Mohamed N. Baterdouk	Concerned citizen and college student at WPC, NJ
Mr. Ken Berian	Concerned citizen
Ms. Arlene C. Berry	Marketing Manager
Mr. Terry Burnsed	City Stage Ensemble
Mr. Ray Busch	Concerned citizen
Ms. Michele R. Capps	Smiths Aerospace, Inc.
Mr. Charles Carrol	Teacher, Belpre High School
Ms. Eloise E. Carrol	Corning Glass Works
Mr. Robert L. Carroll	Carroll Research
Ms. Lillian Chodorow	Champaign for Internat. Diplomacy; Gray Panthers
Ms. Shirley Palmer Collier	Concerned citizen
Mr. Wm. H Conboy	Mktg V.P. (Ret.), Shelter Insurance Co.

Mr. Joseph P. Conrad	President, Cactus Communications
Ms. Isabel S. Courtney	Southern Connecticut State University ISIS Program
Ms. Sheila M. Cronin	Southern Connecticut State University ISIS Program
Mr. Stephen R. Daisy	Retired teacher (Latin & English) State of Mass.
Mr. Terence E. Dalton	Musician
Mr. Den'alh Damron	Student, CSU
Ms. Eileen Danahy	Denver Museum of Natural History
Mr. Hal Davis	Concerned citizen; Mktg Mgr, ISI
Mr. John Devlin	Prudential Financial Services
Mr. Gordon Erickson	Concerned citizen, Tampa FL
Mr. Charles A. Evans	Manager, University of Utah Research Park
Ms. Judith B. Evered	Women's International League for Peace and Freedom
Mr. Paul D. Fairchild	Private citizen
Ms. Eliza McCormick Feld	Mass. Bar Association
Ms. Bonnie K. Fiala	Science Teacher
Mr. James Fiala	Registered nurse
Mr. John E. Fiala	Continental Airlines
Ms. Kathy Fiala	College Student
Mr. Peter E. Fiala	College student
Ms. Harriet Fine	Concerned citizen
Mr. Ronald Flores	Business Consultant - Private Citizen
Mr. Vernon Fogg	Concerned citizen
Ms. Betty Fortson	Concerned citizen
Mr. Donald L. Fredley	Eden Barn Industries
Mr. Brent Frye	Concerned citizen
Dr. Thomas J. Gilmore	Retired Chairman of Philosophy, Gonzaga University
Mr. Jeremiah Hallaren	Concerned citizen
Ms. Majorie Mazel Hecht	Managing Editor, 21st Century Science & Technology
Ms. Lyn Hendred	Concerned citizen
Ms. Evelyn Hermanstyne	Concerned citizen
Mr. Dan Hiester	Theatre producer - director
Ms. Dorothy B. Holland-Kaupp	Women's International League for Peace and Freedom
Mr. Dennis J. Ingalls	International Caucus of Labor Committees
Mr. William F. Jackson	Art Director, Smiths Aerospace, Inc.
Ms. Vannessa Johnson	Concerned citizen, Public Service Co. of Colorado
Ms. Debra King	Concerned citizen
Mr. Edwin A. Klinkhammer	Southern Connecticut State University ISIS Program
Ms. Mary Krah	Smiths Aerospace, Inc.
Ms. Doris J. Langlois	Concerned citizen
Mr. David A. Lewis	Concerned citizen
Ms. Ruth Lippmann	Concerned citizen
Ms. Elizabeth L. Lozano	Concerned citizen
Mr. Ernest G. Martin, Jr.	Attorney at Law, Gulfport MS
Ms. Lynn W. McCanney	School Teacher

Ms. Pamela H. Meares	Teacher
Ms. Katie Minor	Teacher
Mr. David D. Moon	Concerned citizen; former H.S. science teacher
Mr. Cris Moore	Concerned citizen; student, Univ. of Maryland
Ms. Dianne P. Moore	Concerned citizen
Mr. John J. Moore	Lindly & Co., Inc.
Ms. Lisabeth Moore	Concerned citizen; student, Univ. of Delaware
Mr. Guy Murchie	Author and former airline navigator
Ms. Marie J. Murchie	Retired social researcher, Brandeis University
Mr. David M. Nadeau	President, Super Steak Sub Shoppe, Inc.
Ms. Silvana Neal	Musician
Mr. Nicklos J. Plehn II	Student, University of Michigan
Ms. Paulette Pope	Vice President, TDC, Inc.
Mr. Anu Pugalia	Student
Ms. Miriam Rakieten	Concerned citizen
Mr. Emil B. Rechsteiner	Concerned citizen
Mr. Eddie Russell	President, Country Eastern Music Assoc.
Ms. Ruth L. Sanchez	Teacher
Mr. Daryl Sattui	C.E.O., V. Sattui Winery
Mr. Reinhard Scheil	Psychiatric social worker
Mr. Leo W. Seal, Jr.	Concerned citizen
Mr. Jerry Shapiro	Concerned citizen
Mr. Jon Shapiro	Concerned citizen
Ms. Mollie Shapiro	Concerned citizen
Mr. Joe Simmons	Architect
Mr. W. Evan Sloane	M.I.T. Management Alumnus (Johns Hopkins Undergrad)
Mr. James Philip Sperry	Concerned citizen, Student, Tampa, FL
Mr. Frank N. Stempfer	Eden Barn Industries
Mr. Thomas E. Stolper	Author
Mr. David A. Sutherland	Concerned citizen
Mr. H.L. Thornton	Concerned citizen
Ms. M. Jeanne Thornton	Concerned citizen
Ms. Donna Unck	University of Utah Research Park
Ms. Theo S. Vaughey	Museum Trustee
Ms. Carol White	21st Century Science & Technology Magazine

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January 29, 1992

Recent News From Japan

On Jan. 27, the top cold fusion researchers gathered in Nagoya for the ISEM IEEE meeting, to hear the latest exciting news. Dr. Ikegami of the National Institute for Fusion Science chaired this meeting. Featured speakers include Dr. Srinivasan of BARC, India, and Dr. Takahashi of Osaka National University, Department Of Nuclear Engineering.

The press covered the meeting enthusiastically and in depth, particularly the Nikkei, Japan's equivalent to the Wall Street Journal.

I have talked to several people in the last few weeks. They were all busily preparing their presentations. NTT (Japanese equivalent to Bell Labs) continues to get good heat results with gas loading. Dr. Mizuno at Hokkaido and Dr. Takahashi at Osaka have had spectacular results. Takahashi's device is a 1 mm thick X 35 mm X 35 mm palladium plate. On Jan. 17th he told me he was is inputting 30 watts of electricity and outputting 90 to 100 watts of heat. The reaction had continued for 18 days, outputting more than 100 megajoules of excess energy. By Jan. 27th, the day of the meeting, his average excess had climbed to 150 watts, and the device had output more than 200 megajoules of heat. This is *thousands* of times more heat than any chemical reaction could possibly produce.

Until this year, the generous support for cold fusion research offered by the Japanese government has come from Ministry of Education, through the National University Laboratories. Every major National University now has a group researching cold fusion. Support for cold fusion work at Japan's eminent hot fusion laboratory in Nagoya (Ikegami's group at NIFS) comes from both the Government and from a consortium of power companies. Ikegami has also been in charge of allocating special funds from NIFS to researchers at other National Universities.

The total amount spent per anum in the National Universities is impossible to compute accurately, because most of the money come from discretionary funds under control of University Departments. Estimates are that within the National Universities, Japan spent around $11 million on salary and overhead, and about $4 million on equipment during 1991, but that amount is expected to increase considerably in 1992. This is a stark contrast to the U.S., where no Federal or State money was allocated for cold fusion research in 1991, and none is available now.

This year, many top researchers are confident that MITI will begin playing a role as well, as cold fusion begins to leave the experimental laboratory environment and enter full scale industrial development.

- Jed Rothwell

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February 6, 1992

From: Nikkei Shimbun
01/28/92
[no byline]
Translation by Jed Rothwell

Excess Heat, 150 Watts Average

Stable Heat Generating Reaction is Achieved

Neutron Emissions Detected

Osaka University Cold Fusion Experiment

On Jan. 27, during an international symposium in Nagoya, Professor Akito Takahashi, of the Department of Nuclear Engineering, Osaka University revealed that he has achieved stable heat generation in a room-temperature (cold) fusion experiment. During the experiment which began in December, and ran for about one month, he got an average of 150 watts excess heat. Professor Takahashi says he considers it very likely that this excess heat is being caused by cold fusion. But other experts have not abandoned their cautious skepticism. These other experts give Takahashi high marks for the experiment which shows, for the first time, the connection between neutron emissions and excess heat, and offers a theoretical examination of the mechanism which might explain why fusion occurs.

The experimental results were revealed in the International Meeting Hall during the "International Symposium on Nonlinear Phenomena in Electromagnetic Fields."

The electrolysis device used in the experiment had a one millimeter thick, pure palladium plate as cathode. The anode was platinum wire wrapped in a coil around the cathode. The entire apparatus was submerged in heavy water and electrolyzed. The amount of electricity flowing between the anode and the cathode varied in six hours cycles from a low input of 0.25 amperes to a high input of 4.2 amperes.

As a result, around December 20th of last year the experiment began to output excess heat. During the low, 0.25 ampere runs, total heat was between 50 and 70 watts;

during the high 4.2 ampere runs, total heat was between 200 and 250 watts. During the low run, 1.25 watts of electricity used in electrolysis; during the high run 90 to 100 watts were used. Thus, the excess heat was several tens of times larger than the input during the low run.

The average excess including both low and high runs was 2 to 3 times the input. Subtracting the input electricity leaves a total average of 150 watts excess heat output. The total amount of heat put out during the entire experiment amounted to 200 megajoules.

The total heat output per cubic centimeter of palladium in this experiment was more than 10 times higher than the heat output per cubic centimeter of a fuel rod in a nuclear reactor.

The group has also detected neutrons, which are the most positive proof of a fusion reaction, by using the most accurate type of instrument, a liquid scintillator. In this study, for the first time, the researchers clearly determined that the number of neutrons declines as the strength of the reaction increases. The researchers also proved the replicablity of the experiment by changing out the cathode midway through the experiment, and restarting the experiment without difficulty.

A Surprising Amount of Heat Is Output

A discussion by Hideo Ikegami of the National Institute for Fusion Science:

This is the first instance in which the connection between the number of neutrons and the heat has been so clearly shown. Dr. Takahashi's theory to explain these results is audacious but interesting. I am very surprised to see that the heat output is several tens of times larger than the electrical input. I cannot imagine how this could be a chemical reaction. But we still have to determine the mechanism, before we can declare once and for all that this is a fusion reaction.

Mr. Jed Rothwell

From. TAKI

安定した発熱反応実現

余剰熱、平均150ワット

阪大が室温核融合実験

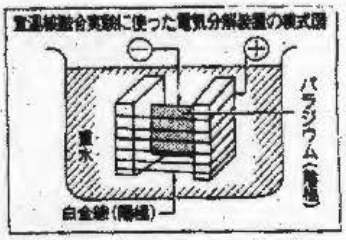
中性子発生を確認

大阪大学工学部の高橋亮人教授は二十七日、名古屋市で開かれた国際会議で室温核融合実験により安定した発熱反応を実現したと発表した。昨年末から約一カ月にわたって継続的に平均百五十ワットの余剰熱を得ているという。高橋教授はこの発熱が核融合によるものである可能性が高いとみている。ただ、他の専門家はまだ慎重な見方を崩していない。今回の実験で中性子の発生数と発熱の関係を初めて明らかにしたことや、室温核融合が起こる可能性を理論的に詳細に検討した点などに関しては他の専門家も高く評価している。

室温核融合実験に使った装置と高橋教授

実験結果は名古屋国際会議場で開かれた「電磁場における非線形現象応用国際会議」で発表された。

実験に使った電気分解装置では、厚さ一ミリの高純度金属パラジウム板を陰極にした。陽極は、細い白金線を板の表面から一ミリほど離してコイル状に巻いたものを用いた。この電極全体を重水に浸し電気分解した。電解開始に伴う電流は、六時間おきに〇



重温核融合実験に使った電気分解装置の模式図

三体・四体反応を提案

解説

高橋教授は、室温核融合が起きるメカニズムとして重水素が三つないし四つ同時に反応する三体反応、四体反応を提案している。

複雑している場所で二人の人間がぶつかる確率は低いが、三人ないし四人が同時にぶつかる確率は低い。これと同じように、「三体反応より多いというのは常識的には考えにくい」（池上英雄・核融合科学研究所教授）のが普通だ。

高橋教授の提案では、まず重水の電気分解で発生した重水素がパラジウム電極の中に吸収されパラジウムの結晶中に組み込まれる。こうすると、三体、四体反応の確率が高まり核融合が起きるというものだ。

パラジウムの結晶は面心立方晶と呼ばれる構造をしている。立方体の各頂点と各面の中央にパラジウム原子が位置しているものだ。この構造では、頂点と頂点を結ぶ線分の中央に0位置と呼ばれる場所があり、そこに重水素が入り込む。また、0位置よりエネルギー的にみて入りにくいが、このほかにも立方体内部に中位置と呼ばれる重水素の入れる場所があるという。

通常、エネルギーの障壁が高いため重水素はなかなか中位置には入りにくいが、重水素の充てん率が高まり電極の電流によって重水素が励起（励起）されると0位置にいた重水素が中位置に入り込み始めるという。一つの中位置の周りには複数の0位置があるため、三つ、ないし四つの重水素が同時に中位置に落ち込み、核融合が起きる確率が高くなる。これが高橋教授の描く核融合のシナリオだ。

海外などで行われた実験によると、パラジウム内に入る重水素の数はパラジウム原子の0・八五倍を超えると発熱などの現象がおこりやすくなるということがわかってきている。ところが0位置に全部重水素が充てんされた場合にパラジウム原子と重水素の比がこの数字に一致するという。高橋教授はこうしたことからこのシナリオにもとづいて核融合が起きている可能性は十分あると考えている。

実験で発熱が大きくなると中性子の発生がかえって少なくなることがわかっているが、高橋教授によるとこれもシナリオで説明できるとしている。発熱は三体反応より四体反応の方が多いが、四体反応はほとんど中性子が出ない反応という。四体反応の頻度が増えると発熱が多くなるかわりに中性子はむしろ少なくなるというわけだ。

この結果、実験を開発した昨年十二月二十日ごろから発熱反応が起き始めた。〇・二五気圧の低電流のときには五十一七十ワットの熱が、また四・二気圧の高い電流のときには二百一二百五十ワットの熱が出た。電気分解に使用した電力は、低電流時が一・二五ワット、高電流時が九十一百十ワットなので、低電流時には使用電力の数十倍の発熱があったことになるという。

高電流時を含めた平均でも二倍から三倍の熱が発生したことになる。発生した熱から使用電力を差し引いても平均約百五十ワットのプラスとなり、その分が余剰熱として出ている。実験開始後、発生した熱エネルギーの量は合計一百ワットに達する。

原子炉の燃料棒一立方㍍当たりの熱出力と今回の実験に用いたパラジウム電極一立方㍍当たりの熱出力を比較すると、今回の方が約十倍大きいという。

核融合が起きている証拠としては、発生した中性子は、最も精度が高い液体シンチレーターで測定、中性子の発生も確認している。この測定で発熱が大きくなると中性子数が少なくなることなど両者の関係も初めて明らかにした。また、実験途中で電極を交換した後も同様の発熱反応が続いているため実験の再現性もあるとしている。

熱出力の量に驚き

池上英雄・核融合科学研究所教授の話　中性子と発熱の関係をこれほどきれいに出したのは初めてだ。理論については大胆なアイデアだが大変おもしろい。発熱については、使った電力の数十倍もの熱出力があったというのには大変びっくりしている。化学反応によるものとは考えられない。しかし、核融合によるものと言い切るには、まだ議論しなくてはならない。

for ISEM
山口栄一

New method for inducing anomalous nuclear effects in deuterated palladium system

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Abstract

We report recent progress of our new technique for inducing anomalous effects in Pd:D (Pd:H) systems. This "*in vacuo*" method enables us to detect the charged particles emission and to perform the mass spectroscopy of released gases simultaneously. With 100 % reproducibility in obtaining the excess heat evolution, explosive gas release and rapid plastic deformation, we have found that these phenomena are induced by D (H) transport due to the temperature and strain gradients. We have also observed gigantic charged particles emission from Pd:D, where the maximum energy was approximately 3 MeV. The occurence of the charged particles burst was strongly correlated to the excess heat evolution and gas release.

1. INTRODUCTION

Two years ago, we presented a new technique for inducing anomalous nuclear effects in solid systems [1,2]. The key factor of this method is to form hetero-structures by placing thin film oxides on a surface of deuteron-loaded palladium (Pd:D) plate. The oxides provide the surface barriers for ionic transport of deuterons in vacuum.

By using this technique, we observed gigantic neutron bursts of $(1\text{–}2)\times10^6$ n/s, explosive gas release, uniform biaxial plastic deformation of all three samples, and excess heat evolution, at the same time, from Mn-O/Pd:D/Au systems set in a vacuum chamber. We also observed the latter three phenomena by applying this method to Pd:H systems having the same heterostructures. This gave the first evidence for the fact that the excess heat production is not caused by D–D reactions.

The advantage of this "*in vacuo*" method is to enable us, *in situ*, to detect the charged particles emission, to perform the mass spectroscopy of released gases, and to investigate the atomistic nature by various electronic spectroscopies. The purpose of the present work is to report recent results of the simultaneous detection of charged particle spectroscopy and the mass analysis of released gases as a function of time, as well as excess enthalpy analysis.

2. EXPERIMENT

In the present work, samples with surface-barriers were prepared by the following procedure; first, thin film Mn-O (thickness=200Å) was deposited on one surface of $3.0\times3.0\text{cm}^2$ Pd plates (thickness=1.0 mm or 2.0 mm) with an electron beam evaporator. Next, the samples were placed in a vacuum chamber, and annealed at 300—400°C for

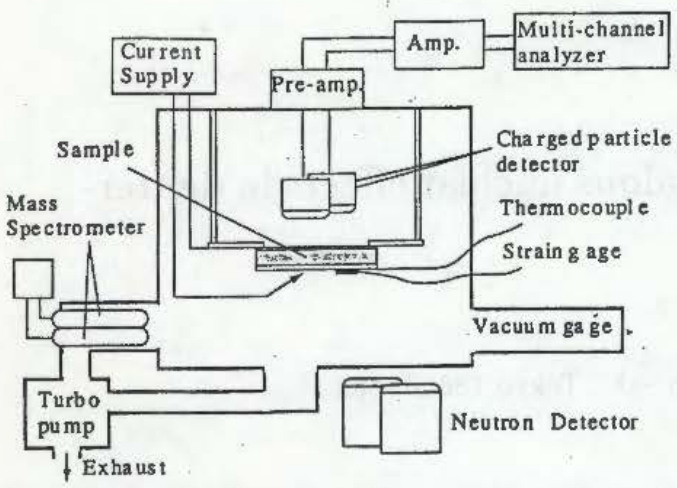

Fig. 1: Schematic diagram of the experimental set-up.

more than 18 hours under vacuum. After loading D_2 gas up to ~ 400 Torr in the chamber, the samples were cooled down by 3°C/min to room temperature. They were then left continuously in the D_2 atmosphere for more than 40 hours. Finally, just after evacuating the chamber, Au film approximately 2000Å thick was deposited *in situ* on the opposite Pd surface at $\simeq 10^{-5}$ Torr.

Measurement setup is schematically shown in Fig. 1. It contains two independent systems of charged particle detectors, mass spectrometers (Spectramass: Selectorr) and ^{3}He neutron detectors (Aloca Ltd: TPS-451S). Each charged particle detection system consists of a Si-SSD (depletion layer thickness=300μm), a pre-amplifier (EG&G ORTEC: 142), an amplifier (EG&G ORTEC: 575A) and a multi-channel analyzer for the pulse height analysis. Progammed electric current was introduced by a current generator (HP6032A), where a tungsten needle was used as an electrode for the Au-coated surface. Several thermocouple gauges were used to measure the temperatures, and a strain gauge was put on Au-coated surface to measure the deformation of the sample.

3. EXCESS HEAT

Typical data for the strain at Au surface, the voltage, the temperatures and pressures are shown as a function of time, t, in Fig. 2(a), where the constant current of DC 5A + AC 2A (50 Hz) was injected at $t \geq 0$, and the data was taken at every 2 seconds. As shown in this figure, the increase in temperatures and the subsequent increase in pressures were observed at about 50 min after the start

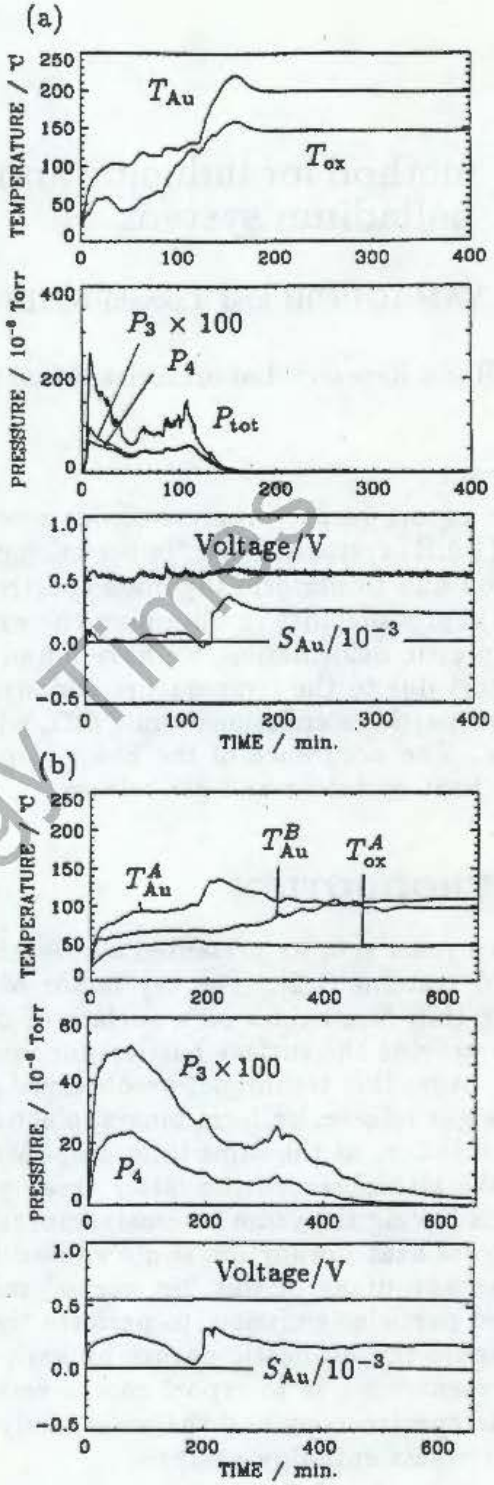

Fig. 2: Time evolution of temperatures at Au (oxide) surface T_{Au} (T_{ox}), total pressure P_{tot}, partial pressures of mass 3 (mass 4) P_3 (P_4) strain S_{Au} and voltage,; (a) constant current mode, (b) constant voltage mode.

of the electric current injection. A sudden biaxial plastic deformation of the sample occurred within 2 seconds while the suface temperature was rapidly increased. In this experiment, neither charged particles emission nor neutron emission were detected.

Figure 2(a) also shows that the circuit resistance was slightly increased around the peak in temperatures, due to the accumulation of D atoms at surface. This may result in overestimating the amount of excess heat production. To avoid such overestimation, we also performed the similar experiment by using a constant voltage mode. The results are shown in Fig. 2(b), where the constant voltage of 0.6 V was applied seriesly to two samples of Mn-O/Pd:D/Au (see the inset of Fig. 2(b)). It is clearly shown in this figure that the same increase in temperature occured for each sample. Since this constant voltage mode allows us to underestimate the excess heat, we can conclude that this temperature increase is really due to the excess heat generation. The generated excess heat was 0.5–1.0 W and it lasted more than 50 min at every run.

The result shown in Fig. 2(b) also reveals that the hydrogen transport is not caused by electron migration but by the temperature gradient. To confirm it, we performed the following experiment. We removed the current injection apparatus in the setup shown in Fig. 1. Instead, we just heated Au surface of the samples up to 130°C. The temperature of oxide surface was kept at 80–90°C. After 170 min heating, the sudden biaxial plastic deformation occured, and, at the same time, the temperature of the samples became increased. This result strongly suggests that the temperature gradient gives rise to a main effect on D/H transport.

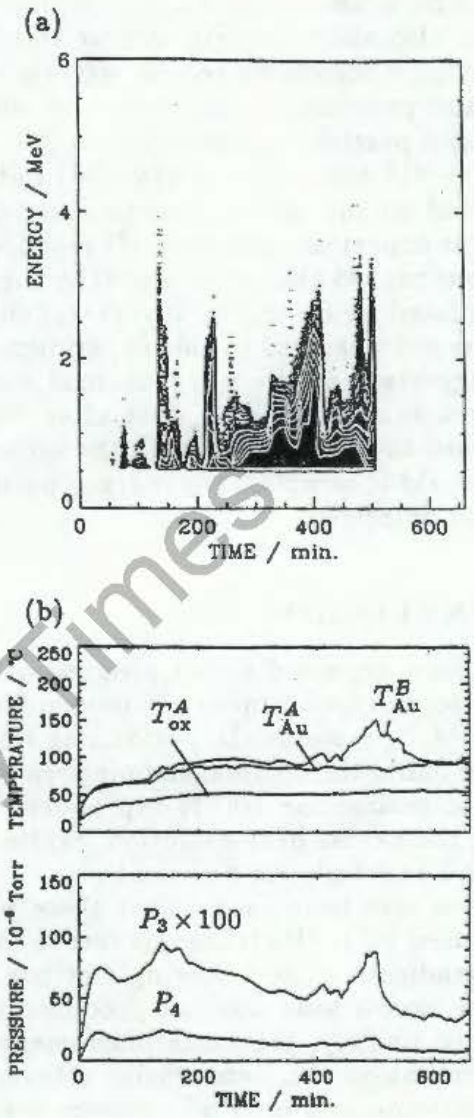

Fig. 3: Observed time evolution of (a) energy spectrum for charged particles and (b) temperatures and pressures.

4. CHARGED PARTICLES EMISSION

With low reproducibility, we detected charged particles emission. One remarkable example is shown in Fig. 3. The time evolution of the observed energy spectrum and that of temperatures, pressures and strains are shown in Fig. 3(a) and 3(b). In this experiment, we set two samples with thicknesses 2.0 mm and 1.0 mm, and started to inject a electric current of DC 5A+AC 2A (50 Hz) at $t = 0$. As shown in this figure, the first apparent burst of charged particles emission occured when $t = 131 - 142$ min. Figures 4(a)–(e) shows the time resolved energy spectra from $t = 131$ min to 182 min. As shown in these figures, the maximum energy of emitted particles is almost equal to 3 MeV. Here, note that the energy was calibrated before and just after the experiment by using ^{241}Am (5.442 MeV; 5.484 MeV) and ^{244}Cm (5.763 MeV; 5.805 MeV). Therefore, it is most likely that nuclear reaction $d + d \longrightarrow p(3.02\ \mathrm{MeV}) + t(1.01\ \mathrm{MeV})$ occured in this time region. However, further investigations are necessary to identify the produced

charged particles by using $\Delta E - E$ detector.

It is also shown in Fig. 3 that this first burst was strongly correlated to the increase in temperature and pressure. Furthermore, all other bursts of charged particles emission at $t = 213 - 233$ min, $t = 395 - 416$ min, and $t = 476 - 507$ min were also correlated to the excess heat production. Therefore, it is expected that the D–D reaction in palladium was caused and/or triggered by the internally accumulated strain in Pd. However, neutron emission was not observed in this experiment. It therefore suggests that the $p + t$ channel was strongly enhanced in this system. Just after this run, we performed the experiment with the same condition by using Pd:H samples. No charged particles emission was detected.

5. CONCLUSION

We have reproted recent progress of a new "in vacuo" technique for inducing anomalous effects in Pd:D (Pd:H) systems. In performing this method, we have found the optimal sample preparation condition to realize the 100 % reproducibility in obtaining the excess heat evolution, explosive gas release and rapid plastic deformation.

It has also been found that these phenomena are induced by D (H) transport due to the temperature gradients. The following fact has suggested that the excess heat was not produced by nuclear reactions; namely, the same phenomena of excess heat generation *etc.* was easily occured both on Pd:H systems and on Pd:D system even without detecting charged particles emission.

Nevertheless, we have observed gigantic charged particles emission. The energy spectra has shown that the maximum energy was approximately 3 MeV. The occurence of the burst was strongly correlated to the excess heat evolution and explosive gas release. Therefore, it is expected that the accumulated strain induces both D–D nuclear reaction and excess heat production in Pd:D systems.

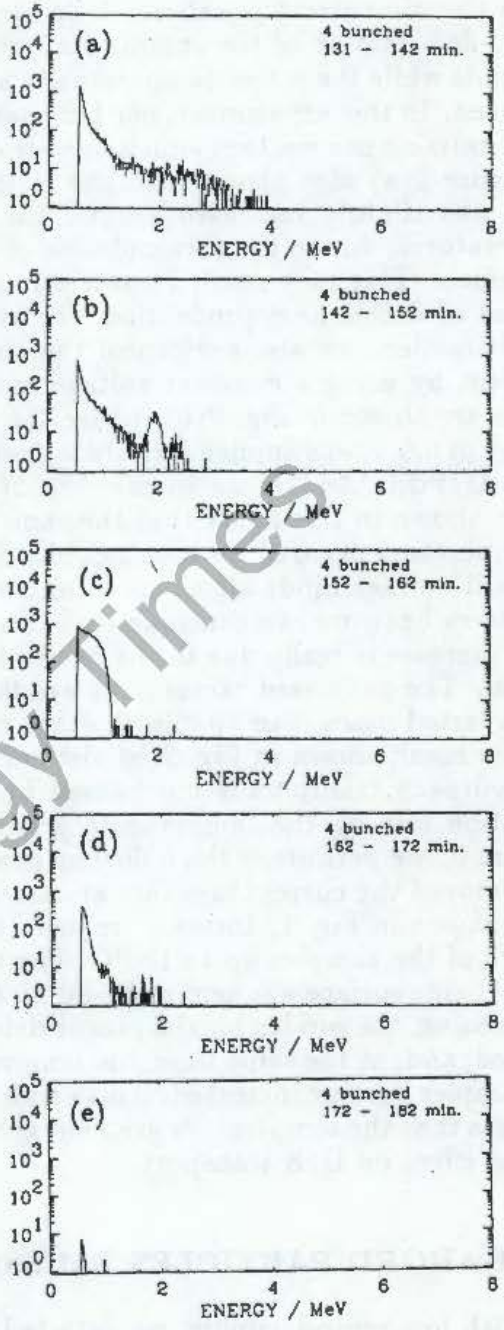

Fig. 4: Energy spectra for (a) t =131–142 min, (b) 142–152 min, (c) 152–162 min, (d) 162-172 min and (e) 172–182 min in the experiment shown in Fig. 3.

REFERENCES

1 E. Yamaguchi and T. Nishioka, Jpn. J. Appl. Phys. **29**, L666 (1990).
2 E. Yamaguchi and T. Nishioka, Proceedings of International Progress Review "Anomalous Nuclear Effects in Solid/Deuterium Systems", Provo, 1990, p. 354.

October 30, 1991

COLD FUSION

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College Station, TX 77843-3255

A memorandum has been written on the present state of cold fusion research as of October, 1991, by Professor Heinz Gerischer.

The significance of this memorandum arises from Professor Gerischer's status. He is widely recognized to be the leading physical electrochemist in Europe and would vie for the title on a still wider basis. Apart from his long term involvement in electrochemistry he is well known as a physical chemist of the highest standing and was, until 1988, the Director of the Max Planck Institute for Physical Chemistry in Berlin.

A still further significance may be attached to Professor Gerischer's memorandum because he has been, since March 1989, a dedicated opponent of cold fusion, taking the position (more pronounced, even, in Germany, France and England, perhaps, than in the United States) that the work of Fleischmann and Pons and the announcement of March, 1989, was due to poor measurements.

Indeed, I can witness the fact that until the Como Conference of July 1991, Professor Gerischer maintained a negative opinion of the field which he confided to me at the beginning of the Conference.

My translation of his memorandum follows.

MEMORANDUM ON THE PRESENT STATE OF KNOWLEDGE ON COLD FUSION

H. GERISCHER, FRITZ HARBER INSTITUTE DER MAX PLANCK,
Gesellschaft, Faraday Weg 4-6 D-1000, Berlin 33

Concerning the origin of this memorandum: Between the 30th of June and the 4th of July, there took place in Como, Italy, the Second Annual Conference about Cold Fusion. This had been organized by Italian groups, principally from the Universities of Turin and Milan. The joint chairmen of the conference were Professor T. Bressani (Turin) and Professor G. Preparata (Milan).

At the end of May I was invited to attend this conference as a skeptical observer and found that I was down on the program for the last day of the conference as giving a lecture on the pros and cons of the field. There were about 60 lecturers at the meeting in groups from the U.S.A., Italy, Japan, China, Soviet Union, and various smaller countries which all gave their results. Two comprehensive reviews had been already sent to me in manuscript form, - one of them has already appeared (1) and the other will shortly appear. On the basis of these two reviews I was able to prepare myself somewhat for the conference.

THE INDICATIONS OF OCCURRENCE OF NUCLEAR PROCESSES IN METAL-DEUTERIUM ALLOYS

In spite of my earlier conclusion, - and that of the majority of scientists, - that the phenomena reported by Fleischmann and Pons in 1989 (3) depended either on measurement errors or were of chemical origin, there is now undoubtedly overwhelming indications that nuclear processes take place in the metal alloys. The early publications were so full of errors in measurement technique and in the interpretation that the euphoria to which the discovery gave rise was rapidly replaced by disappointment when it turned out that the

laboratories with the best equipment could not reproduce the results. Only very few groups found similar effects, but even these groups could not find reproducibility in their own laboratory. Furthermore the supposition that a fusion of two deuterium atoms could take place in a solid body such as PdD_x or TiD_x was contradictory to all theories of nuclear reactions. In particular the observed (or maintained) heat effects bore no relationship to the tiny amounts of nuclear products which had been found by a few authors. The main search here went towards neutrons and tritium because according to fusion of two D^+ nuclei in hot plasmas, the following two reactions would occur with about the same probability.

$$D^+ + D^+ \rightarrow n\ (2.45\ MeV) + {}^3He\ (0.82\ MeV)$$

$$D^+ + D^+ \rightarrow H^+\ (3.02\ MeV) + {}^3H\ (1.01\ MeV)$$

Now, the sensitivity of instruments for detecting neutrons is particularly good and therefore the majority of the measurements concentrated on experiments which would give rise to such detection. This was particularly true in experiments which were carried out at low temperatures from palladium which had been previously loaded with deuterium. In the electrochemical loading of palladium by cathodic evolution of deuterium either from an alkaline or acid electrolyte in D_2O (predominantly with the electrolyte LiOD) from time to time a tritium enrichment would be found which was considerably higher than could arise from differences expected for separation factors during the discharge of D_2O from an electrolyte containing DTO. The occasional observation of neutrons with intensities which were decisively above the background, and with tritium amounts in the electrolyte which were certainly an indication for the occurrence for nuclear processes is very significant, but the amounts were so small that the heat observed could not be explained by their occurrence. Correspondingly, it has been found that the neutron production is sporadic and occurs in bursts. The

same occurs for the tritium enrichment and also for the heat production.

The occurrence of such happenings could not in any way be foreseen on the basis of previous knowledge. Correspondingly, the authors of the papers admitted that the reported positive results were only occasionally obtained, and that many experiments ran through without any observable nuclear consequences at all, - facts which lent further doubt as to the reality of the effects.

As far as the interpretation of the nuclear reactions goes it has been suggested (4,5) that a reaction occurs which plays no part at all in the hot plasmas namely:

$$D + D \rightarrow {}^{4}He\ (0.076\ MeV) + \gamma\ (23.77\ MeV) \qquad \text{(Equation 3)}$$

The gamma radiation which should occur according to this reaction is, however, not observed. However, very recently there have been reports about the observation of helium in electrolytically evolved deuterium from a $LiOD/D_2O$ solution in cells with palladium electrodes. In these experiments heat production was also calorimetrically observed. The amount of ^{4}He was indeed in the right order of magnitude according to equation three although the agreement was not quantitative. On the other hand, corresponding water blank experiments using $LiOH/H_2O$ showed neither any heat excesses nor any helium in the evolved hydrogen gas.

AN EVALUATION OF THE RESULTS OBSERVED SO FAR

Although there are many discrepancies in the reports which are at hand, and although there are many open questions, there now lie before us several indications that fusion reactions do occur between deuterides in metals. This gives rise to a new situation. It is entirely an open question whether such processes could be used as the source of energy but this, of course, can only be decided if the processes which have been revealed in the work discussed here are

researched and given a theoretical basis. In any case I consider it absolutely necessary that these phenomena are systematically researched and the conditions for their reproducibility cleared up. That a nuclear reaction can be stimulated by interaction with a solid lattice and made to take another path from that which it would take in the plasma, is an entirely unexpected discovery with possibly wide ranging consequences. It demands confirmation and further experimental evaluation. In the following a number of experimental and theoretical questions are raised which are at the present time entirely open.

EXPERIMENTAL PROBLEMS

The overwhelming problem is the lack of reproducibility in the results. The anomalous phenomena in palladium and titanium can only be seen after very long times of loading, usually after several weeks. In the loading there is a phase change in which the alpha phase is changed to the beta phase and this involves a change in lattice constants which gives rise to a volume increase of about 15%. The microcrystalline structure of the solid seems to play an important role here. However, the structure of the solid is not yet characterized. The lattice structure seems to be an important part of the conditions necessary for the setting up of the anomalous effects.

In the electrochemical experiments the characteristics of the surface in the charging of the metal with deuterium plays a great role. These effects in the electrochemical experiments are greater than those in loading from the gas phase. The reason for this is probably the increased activity of deuterium atoms which occur in the first step of loading according to the reaction:

$$D_2O + e \rightarrow D_{ad} + OH^- \qquad \text{(Equation 4)}$$

The slower the following reactions (5 and 6) are, - these lead to molecular deuterium, - the greater is the activity of the absorbed D atoms on the surface:

$D_{ad} + D_{ad} > - D_2$ (Equation 5)

$D_{ad} + D_2O + e > - D_2 + OD^-$ (Equation 6)

The activity of adsorbed atoms determines the loading of the metal lattice with D atoms which go from the surface to the interior of the lattice. In this way one can build up a super-saturation of the lattice with D-atoms and this super saturation increases with the surface activity of the adsorbed D. If one tried to reach super saturation in the metal by increasing the pressure in the gas phase, enormous pressures would be necessary.

In the literature is reported that extremely long times of electrolysis are necessary before phenomena are observed and these can give rise to the deposition of impurities on the surface of the electrode. This gives rise in turn to a change of the overpotential in the current-potential curve (7) and also in the analysis of the surface composition which will certainly take place on long term electrolysis (8). Such absorbance can give rise to an inhibition of the recombination velocity of the atoms to deuterium of the surface atoms to the molecular product in reactions 5 and 6. In this way the activity of adsorbed hydrogen can be increased. On the other hand, if the inhibition affects reaction 4 then the reverse effect occurs. In fact, it is possible to think of an acceleration of reactions 5 and 6 by the impurities. Thus, it has been shown for palladium cathodes that these contain platinum which obviously arises by the partial dissolution of the anode. It is known that if oxygen is evolved at high rates upon an anode there is certainly the danger of anodic dissolution. Correspondingly it is known that platinum deposition on palladium increases the recombination and therefore decreases the activity of adsorbed D.

Apart from the observation of neutrons, proof of the presence of the nuclear products, ^3H, ^3He, and ^4He are difficult to observe because many of the experiments are carried out in open systems the evolved gases seldom being

collected. The consumed D_2O has to be replaced. In this way one builds up impurities in the electrolyte in the cell and the products in the gas phase are lost. Only very few experiments are carried out in closed systems in which the D_2 is converted to D_2O on a catalyst and re-introduced into the electrolyte (9,10) or alternatively, the D_2 which is evolved is oxidized back to D_2O (or D^+) (11).

The latter process corresponds to the anodic process in a hydrogen oxygen fuel cell. In such experiments the reaction products build up in the electrolyte or in the gas volumes which are collected. This has so far only been used for tritium analysis.

The calorimetric measurements have been greatly improved. In the open cells there are still difficulties concerning the loss of heat along with the evolved gases. Closed cells do not have this difficulty but they are much larger in volume and are less sensitive to the measurement of heat. In general there exists in all electrochemical cells the problem that the energy input of the cell is large (large current densities) and must be carefully measured in order to set up a clear and sure energy balance. The uptake of energy varies with the cell resistance and the overpotentials of the electrode. The excess energy in most of the experiments which have been described so far is considerably less than the energy which is being used for the electrolysis. A typical excess heat measurement amounts to 10-20% of the energy put into the electrolysis over several hours. A few measurements have given 100% and more.[1] A few measurements do give 100% of heat and even more but are perhaps somewhat doubtful. On the other hand, there were plenty of indications at the conference

[1]Note added by JOMB: At a recent meeting at the Southern California Edison Company, Robert Bush of Pamona University in California revealed measurements on very thin films which when calculated to a per cc measurement of heat give about 1 kw per cc.

that some authors were withholding their results because of patent considerations.[2]

The experimental problems in the measurement of gas loading is much less well known to me and I do not wish to comment upon it. It is noteworthy that neutron production which exceeds the background intensity can only be obtained by warming up these samples which have been charged at very low temperatures.

THEORETICAL PROBLEMS

The most decisive problem is the question: How can a solid body influence the course of nuclear processes?[3] Thus, the point is, - how can the screening of the repulsive forces between the two atomic nuclei be achieved so that successful tunnelling can occur? The second question is, how can the channels be altered so that they go in the direction of reaction 3 and thus allow a ratio of 10^6-10^9 for the predominance of tritium over neutrons (compare the expected 1:1 ratio of nuclear chemistry in hot plasmas).(2)

A further riddle is the absence of hard gamma radiation which should occur corresponding to reaction 3. How can this gigantic energy of this reaction be diffused over the solid body without emitting radiation (although weak x-rays have been seen on a photographic plate)(6,12).

There are various attempts at theoretical interpretation (1), and some of these are really quite far out. It has even been suggested that an unknown elementary particle is a catalyst in these processes (13). Without sufficient

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[2]Note added by JOMB: In particular Mike McKubre and his group at SRI have been reporting 250-450% heat for some months. The rumor is that this is an understatement. EPRI has decided to continue to build a practical plant for the production of energy from cold fusion which should be completed by 1997.

[3]Note added by JOMB: On the other hand, the well accepted Mossbauer effect surely is already an example of the effect of a lattice upon a nuclear process.

material to overview the theoretical contributions I will not make any further comments on these theories.

WHAT SHALL BE DONE?

The works which have been so far reported are works which concern individual questions. Attempts are being made to re-produce experiments and to modify the technique. A systematic characterization of the electrochemical and experimental conditions has not yet been carried out. As the largest effects are being observed under these electrochemical conditions it is necessary to concentrate upon the electrochemical experiments.

Fleischmann and Pons reported in the meeting in Como concerning their experiments with palladium alloys (probably with silver) and maintain that they had been able reproducibly to observe large excess heats. No details of the experimental arrangements were given because of their wish to maintain patent conditions.[4]

There is also evidence that the composition of the electrolyte plays a considerable part in the occurrence of the anomalous effects. It is absolutely essential to research the connection between overpotential, hydrogen content, and surface characteristics of palladium or palladium alloys in a systematic way. This could be probably carried out with light hydrogen. Beginning with the hypothesis that a very high loading of D in metals is necessary for fusion because in this respect to loading, H and D hardly differ from each other. One could then come back later to the experiments with D_2O and with the knowledge of what has given rise to the high loading with hydrogen. In this respect it is necessary to have a method which allows a rapid measurement of the amount of

[4]The experiments of Fleischmann and Pons are being carried out in Nice with support of a Japanese group, connected to Toyota.

hydrogen in the palladium. Here resistance methods are probably the best. Parallel to the information obtained in these measurements it would be necessary to build calorimeters which have requisite controls and work in an enclosed manner so that heat production and nuclear products can also be observed with a minimum amount of ambiguity. The decisive proof of the rising of excess heat by nuclear reactions will, of course, be the occurrence of the corresponding amount of ^4He. If this can be confirmed, further steps for the optimization of the energy yield can be carried out.

These goals require the cooperation of scientists from various fields. One needs electrochemists, metal scientists, particularly those with knowledge of metal hydrides, engineers for the building of cells and the computerization of data, persons with experience in calorimetry, mass spectroscopy, and one needs nuclear physicists for the radiation measurements. A team of this kind should be in position to clear up the basic situation fairly quickly. Of course, the first thing is to confirm the facts. The fact that, in the Republic of Germany this work has been inhibited is no more justified. It could later on be regarded as a very unfortunate gap in German research when compared with the present activity in other countries and particularly in Japan.

REFERENCES

1. M. Srinivasan, in: "Current Science," April 1991.
2. E. Storms, appears in "Fusion Technology," 1991.
3. M. Fleischmann, S. Pons and M. Hawkins, J. Electroanal. Chem. 261 (1989), 301; 263 (1989), 187.
4. C. Walling and J. Simons, J. Phys. Chem. 93 (1989), 4693.
5. M. Fleischmann, personal communication, 1990.
6. B. F. Bush, J. Lagowski, M. H. Miles and G. S. Ostrom, J. Electroanal. Chem. 304 (1991), 171.
7. Z. B. M. Ulmann, J. Lin, J. Augustynski, F. Eli and L. Schlapbach, J. Electroanal. Chem. 286 (1990), 257.
8. Observations of Various Authors at the Meeting in Como.
9. N. J. C. Packham, K. L. Wolf, J. C. Wass, R. C. Kainthla and J. O'M. Bockris, J. Electroanal. Chem. 270 (1989), 451.
10. E. Storms and C. Talcott, Fusion Technology 17 (1990), 680.
11. F. Will, Lecture at the Meeting in Como.
12. S. Szpak, Lecture at the Meeting in Como, J. Electroanal. Chem. 302 (1991), 255.
13. F. Mayer and J. R. Reitz, Fusion Technology 18 (1991), 352.

Arthur C Clarke, CBE

"LESLIE'S HOUSE", 25 BARNES PLACE, COLOMBO 7, SRI LANKA.

Telephone: 941-699757, 694255, Celltel: 444055 Fax: 941-698730 Cable: "UNDERSEA COLOMBO"

Dr Eugene F Mallove
171 Woodhill-Hooksett Road
Bow, New Hampshire 03304

9 Jan 1992

Dear Gene,

Thanks for your letter of 31 December. By an odd coincidence see this fax I've just sent to my agent.

You may not know, but I've been interested in 'cold-fusion' for a long time, partly because of my connection with Luie Alvarez: I plugged it in Odyssey II.

I've not had time to read all the documentation which you sent, but of course, will be doing so - meanwhile here's the petition duly signed (I think this is the first time I've ever signed one!)

Incidentally, I wonder if Jim Fletcher's death has had any effect on the controversy.

My guess is:

1. **There's a 90% probability that something odd is going on**

2. **There's a 10% probability it will be of no practical importance - it seems too good to hope for!**

   Of course, I'll be delighted to be proved wrong on (2).

Meanwhile, all good wishes,

acc

Dr **Arthur C Clarke, CBE**

encls Petition
Fax to DHA
Ego 91 & letter to Maddox

Arthur C Clarke, CBE

"LESLIE'S HOUSE", 25 BARNES PLACE, COLOMBO 7, SRI LANKA.

Telephone: 941-699757, 694255, Celltel: 444055 Fax: 941-698730 Cable: "UNDERSEA COLOMBO"

Mr John Maddox
Editor - Nature
4 Little Essex Street
London WC2R 3LF

9 Jan 1992

Dear John,

Dr Eugene Mallove has just sent me a whole pile of literature on the 'cold-fusion' furor. It now seems very probable that something anomalous is happening though I doubt if it will be of any practical value. However, the implications are so enormous that even a very long shot should be investigated.

I hope Nature will give the matter full coverage - but don't send my friend Randi to make tests - he and I are currently plotting the exposure of a famous pious fraud....

All good wishes,

arthur clarke

Dr Arthur C Clarke, CBE

Chancellor: International Space University

cc Dr Eugene Mallove

DEPARTMENT OF THE NAVY
NAVAL WEAPONS CENTER
CHINA LAKE, CALIFORNIA 93555-6001

IN REPLY REFER TO
5 December 1991

An Open Letter to Fellow Scientists

Nature is a prestigious international journal that has played a leading role in many important scientific developments since it was first published in 1869. However, their treatment of the recent cold fusion controversy has been somewhat less than fair. Science is always concerned with the search for truth and the exposure of errors. Although the scientific method will eventually lead to the resolution of the cold fusion controversy, it is now clear that this anomalous effect cannot be readily dismissed as experimental error. The publication of apparent errors and incorrect conclusions, however, such as in the two papers by N. Lewis et al., certainly hinders the quest for truth about this phenomena. A more careful and unbiased review and evaluation is needed for submitted papers on both sides of the cold fusion controversy. I hope that *Nature* will honor its long tradition in seeking scientific truths and correcting errors by publishing my enclosed letter.

Melvin H. Miles

DR. MELVIN H. MILES

DEPARTMENT OF THE NAVY
NAVAL WEAPONS CENTER
CHINA LAKE, CALIFORNIA 93555-6001

2

IN REPLY REFER TO:
3910
3853/508
5 Dec 91

John Maddox, Editor
Nature
4 Little Essex Street
London, WC2R 3LF
Great Britain

Dear Sir:

Two publications by N. Lewis et al.[1,2] were a major factor in turning the scientific and public opinion against cold fusion. Ignoring any debate about cold fusion, there are apparently major flaws in the calorimetric experiments reported by N. Lewis et al. that have been brought to my attention by Dr. V. Noninski. For fairness and accuracy, these errors by Lewis and co-workers need to be presented to the scientific community.

The fundamental error in the calorimetric experiments of N. Lewis et al.[1,2] was the variation of both the electrolysis and resistor power while maintaining a constant total power. This error can readily be seen by the following simple algebraic notation:

$$P_T = P_{EI} + P_X \qquad \text{(when } P_R = 0\text{)}$$

$$P_T' = P_{EI}' + P_R' + P_X' \qquad \text{(when } P_R \neq 0\text{)}$$

where P_T is the total power, P_{EI} is the electrolysis power, P_R is the resistor power, and P_X is the excess power (if any). Thus

$$\Delta P_T = P_T - P_T' = P_{EI} - P_{EI}' - P_R' + P_X - P_X'$$

If the cell temperature is kept constant, $\Delta P_T = 0$, hence

$$\boxed{P_X - P_X' = P_{EI}' + P_R' - P_{EI}}$$

The experimental observation by N. Lewis et al. that $P_{EI}' + P_R' - P_{EI} \cong 0$ can only prove that $P_X - P_X' = \Delta P_X \cong 0$, i.e., the excess power (if any) did not change significantly with the change in P_{EI} (current density). It definitely does not follow that P_X or P_X' is zero. Contrary to the claims of the authors[1,2], a study of this nature is completely incapable of proving that no anomalous power was produced.

The increasing heating coefficients (h.c.) reported by N. Lewis et al.[1] from 14.0 to 15.9°C/W (Table 3) suggest an excess enthalpy effect of over 13% in D_2O/LiOD. The heating coefficient can be expressed as

$$\text{h.c.} = \frac{\Delta T_J + \Delta T_X}{P_T}$$

where ΔT_J is the Joule heat contribution to the cell temperature change produced by $P_{EI} + P_R$ and ΔT_X is the cell temperature change due to any excess power (P_X). In H_2O or early stages of D_2O experiments, $P_X = 0$, thus $\Delta T_X = 0$ and the true heating coefficient is obtained. If excess power is present, $\Delta T_X > 0$, hence a larger heating coefficient is obtained. This error is due to the neglect of the unknown amount of P_X and using only $P_{EI} + P_R$ to determine the total power. Recalibrations with the load-resistor method used by Lewis et al.[1,2] would not be valid when an unknown amount of anomalous power is present. If heating coefficients in H_2O/LiOH are used (Table 3)[1] where no anomalous power is expected, then an even larger excess enthalpy is suggested for the D_2O/LiOD studies. The largest amount of excess power from Table 3[1] is 1.1 W/cm^3 at 140 mA/cm^2. This is in excellent agreement with results reported by M. Fleischmann et al.[3].

A closer examination of Table 2 of *Science*[2] and Table 3 of *Nature*[1] shows that the sum of $P_{EI} + P_R$ required to maintain a constant cell temperature in D_2O/LiOD is always greater for the experiment at the lower current density. Although this effect is small, it is consistent with the presence of an anomalous power that increases with current density as reported by M. Fleischmann et al.[3].

Possible calorimetric errors due to D_2-O_2 recombination or to the rate and/or form of gas evolution proposed by N. Lewis et al.[1,2] are simply not valid. Many recent studies[4] have shown that D_2-O_2 recombination is insignificant for flooded Pt and Pd electrodes as used in the Lewis experiments. Recent studies in our laboratory show essentially the same cell temperature versus applied heating power relationship for electrolysis power as found for resistor power where no gas evolution occurs. Gas evolution, therefore, does not significantly affect the heat transfer coefficients in electrochemical calorimetric cells as suggested by N. Lewis[1,2].

Finally, the 1 p.p.m. detection limit for helium measurements in the effluent gases reported by N. Lewis et al.[1] is far too insensitive to measure the ^4He yield from the $^2H + {}^2H \rightarrow {}^4He + 23.8$ MeV fusion reaction. Assuming an excess power of 1 W/cm^3 (Pd volume was 0.31 cm^3) would yield only 0.043 p.p.m. of ^4He in the effluent gas for the N. Lewis study at 64 mA/cm^2. The detection of ^4He in the effluent gases from cells producing excess power has been reported for cold fusion studies using more sensitive methods.[5].

Sincerely,

Melvin H. Miles

DR. MELVIN H. MILES
Chemistry Division, Research Department
Naval Weapons Center
China Lake, CA 93555, USA

1. N. S. Lewis, et al., *Nature*, **340**, 525 (1989)
2. G. M. Miskelly, et al., *Science*, **246**, 793 (1989)
3. M. Fleischmann, et al., *J. Electroanal. Chem.*, **287**, 293 (1990)
4. E. Storms, *Fusion Technol.* **20**, 433 (1991)
5. B. F. Bush, et al., *J. Electroanal. Chem.*, **304**, 271 (1991)

DEPARTMENT OF THE NAVY
NAVAL WEAPONS CENTER
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IN REPLY REFER TO:
3910
3853/505
3 Dec 91

Dr. Vesco Noninski
16 Hickory Hill Lane
Framingham, MA 01701

Dear Dr. Noninski,

I have carefully studied the two papers by N. Lewis, et al. (*Nature*, Vol. 340, pp. 525-530, 1989 and *Science*, Vol. 246, pp. 793-796, 1989) along with your unpublished paper and agree completely with your conclusions. Flaws in the method used by N. Lewis make his experiments fundamentally incapable of proving whether excess power was or was not produced. Furthermore, the experimental data presented by N. Lewis yields strong indications that excess power was produced contrary to the conclusions of the authors.

The arguments you present regarding the experiments by N. Lewis would be more readily understood if expressed in simple algebraic notation as follows:

$$P_T = P_{EI} + P_X \qquad \text{(when } P_R = 0\text{)}$$

$$P_T' = P_{EI}' + P_R' + P_X' \qquad \text{(when } P_R \neq 0\text{)}$$

where P_T is the total power, P_{EI} is the electrolysis power, P_R is the resistor power, and P_X is the excess power (if any). Thus

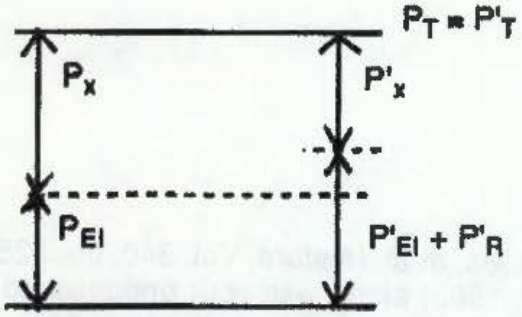
$$\Delta P_T = P_T - P_T' = P_{EI} - P_{EI}' - P_R' + P_X - P_X'$$

If $\Delta P_T = 0$ then the cell temperature would remain constant and

$$\boxed{P_X - P_X' = P_{EI}' + P_R' - P_{EI}}$$

The experimental observation of $P_{EI}' + P_R' - P_{EI} \cong 0$ can only prove that $P_X - P_X' \cong 0$, i.e., the excess power (if any) did not change significantly with the change in P_{EI} (current density). This study absolutely does not prove that no excess power was present. It is a fundamental mistake in cold fusion experiments to vary both the electrolysis power and resistor power while maintaining a constant total power.

The following diagram shows this very clearly:



Thus $P_X - P'_X = P'_{EI} + P'_R - P_{EI}$

Furthermore, the heating coefficient (h.c.) can be expressed as

$$\text{h.c.} = \frac{\Delta T_J + \Delta T_X}{P_T}$$

where ΔT_J is the Joule heat temperature change due to $P_{EI} + P_R$ and ΔT_X is the temperature change due to any excess power (P_X). In H_2O or early stages of D_2O experiments, $P_X = 0$, hence $\Delta T_X = 0$ and the true heating coefficient is obtained. When excess power is present, $\Delta T_X > 0$, hence a larger h.c. is obtained. This error results from the neglect of P_X and the use of only the electrolysis and resistor power in determining P_T in the calibration. The increasing h.c. in Table 3 (N. Lewis) from 14.0 to 15.9 suggests an excess heat effect of over 13% in D_2O.

Finally, the 1 p.p.m. limit for ^4He detection in the effluent gases reported by Lewis would not be sensitive enough to measure the ^4He yield from $^2H + {}^2H \rightarrow {}^4He +$ 23.8 MeV even when an excess power of 1 W/cm^3 is present. Therefore, the ^4He measurements by N. Lewis also prove nothing.

These two papers by N. Lewis et al. were a major factor in turning the scientific and public opinion against cold fusion, hence these major flaws need to be exposed.

Sincerely,

Melvin H. Miles

DR. MELVIN H. MILES

nature

1137 National Press Building
Washington, D.C. 20045
Telephone (202) 737-2355

In reply please quote:
N0857 DL/LB

May 3, 1991

Dear Dr. Noninski,

I enclose at last some brief comments on your letter from Dr Nathan Lewis, to whom I sent it for advice. He once again disputes your arguments (for essentially the same reasons as our independent reviewer did some time ago), and I am sorry to say that we cannot change our earlier decision not to publish your letter in Nature.

The objections to your argument are that a heat excess independent of applied current is contrived, and ceratinly not what proponents of cold fusion have suggested, and in addition that such a form of heat excess is contradicted by some, if not all, of Lewis' experiments as well as by many other negative results from efforts to duplicate cold fusion. It is no good proposing an unorthodox explanation for Lewis' results if that explanation is directly ruled out by, for example, the results from Harwell - unless you intend to produce a different version of cold fusion for every experiment that has been performed.

The only way that I know to convince the scientific world that cold fusion is real would be to show how to reproduce the effect reliably and reproducibly, overcoming all doubts about measurements errors and contaminations. Even if your contrived attempt to explain Lewis' results were right, it would persuade no one that cold fusion should be taken seriously.

I am sorry that we must persist in our negative opinion of your work, but it seems clear by now that you are not pursuing a useful path. I can see no likelihood that Nature would wish to publish your work as long as you persist in such a narrow enterprise against one part of Lewis' work.

Sincerely

Dr David Lindley
Associate Editor

Over

Published by Macmillan Magazines Ltd., 4 Little Essex Street, London WC2R 3LF, England

While it is true that our open system measurements were not sensitive to any current-density independent excess heat (as clearly stated in our orignial *Nature* manuscript), Pons and Fleischmann clearly stated in their work that they only observed current-density dependent excess heat. Our original experiments would have readily detected this heat; we clearly did not see such excesses. N&N have used the incorrect rod diameter in comparison of our results woth the original Pons and Fleischmann work; they should consistently compare our 0.2 cm diameter rods to the original 0.2 cm measurements, in which case it is clear that the current density dependent excess heat claimed by Pons and Fleischmann is far in excess of our experimental errors. N&N have now also proceeded to invent a new hypothetical type of heat that they claim could have been missed in our experiments, and which was not reported by Pons and Fleischmann in their experiments. However, the closed system measurements from our group and from other groups rule out all possible sources of excess heat, including both current density dependent and current density independent types, clearly illustrating that no types of excess power have been observed in our measurements of Pd/D_2O electrolysis. Thus, their hypothetical points are really in contradiction to the experimental data in the literature at present. If you need any further information on this topic, please feel free to contact me.

FEB 1 0 1992

ER-16
WPolansky/ses
2/5/92

ER-10
LIambiello
2/5/92

ER-61
DMayhew
2/6/92

Mr. Jed Rothwell
2060 Peachtree Industrial Court
Suite 312-F
Chamblee, Georgia 30341

Dear Mr. Rothwell:

The Department of Energy has been asked to respond to your letter, with several enclosures, to President Bush dated January 10, 1992, that expressed concern about cold fusion research in the United States. Your letter and information on cold fusion have been reviewed. We have been aware of the activities you referenced including the effort to establish a $10 million per year research program on cold fusion.

We believe that the appropriate mechanism for establishing the feasibility of a new, or unexpected, scientific result is for the proponent to submit it to the collective judgement of peers. This mechanism can be activated in several ways, such as technical presentations at scientific conferences and publications of results in prestigious, peer-reviewed journals. This is a thorough, but time-consuming process. However, all major scientific advances have been subjected to, and have survived, this scrutiny.

The November 1989 report of the cold fusion panel recommended against any special Department of Energy funding for the investigation of phenomena attributed to cold fusion. However, the panel was sympathetic toward modest support for carefully focussed and cooperative experiments within the present funding system. The Department of Energy accepted the report and its recommendations. We have been monitoring the cold fusion research area since the issuance of that report and believe that its recommendations are still valid. We continue to be available to review any research proposal of interest to the Department.

Sincerely,

Walter M. Polansky, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

cc:
Ms. Sally Kelley
Director of Agency Liaison
The White House

bcc: ES/1, ER-61, ER-622/FSTL

ER-16:WMPolansky:SueEllen:3-5995:2-5-92:C:\ESCONTRL\Rothwell.92:wp5.1

ES#92-001401 ER#92-000503 Due Date: 2/7/92

Department of Energy
Washington, DC 20585

February 10, 1992

Mr. Jed Rothwell
2060 Peachtree Industrial Court
Suite 312-F
Chamblee, Georgia 30341

Dear Mr. Rothwell:

The Department of Energy has been asked to respond to your letter, with several enclosures, to President Bush dated January 10, 1992, that expressed concern about cold fusion research in the United States. Your letter and information on cold fusion have been reviewed. We have been aware of the activities you referenced including the effort to establish a $10 million per year research program on cold fusion.

We believe that the appropriate mechanism for establishing the feasibility of a new, or unexpected, scientific result is for the proponent to submit it to the collective judgement of peers. This mechanism can be activated in several ways, such as technical presentations at scientific conferences and publications of results in prestigious, peer-reviewed journals. This is a thorough, but time-consuming process. However, all major scientific advances have been subjected to, and have survived, this scrutiny.

The November 1989 report of the cold fusion panel recommended against any special Department of Energy funding for the investigation of phenomena attributed to cold fusion. However, the panel was sympathetic toward modest support for carefully focussed and cooperative experiments within the present funding system. The Department of Energy accepted the report and its recommendations. We have been monitoring the cold fusion research area since the issuance of that report and believe that its recommendations are still valid. We continue to be available to review any research proposal of interest to the Department.

Sincerely,

Walter M. Polansky

Walter M. Polansky, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

cc:
Ms. Sally Kelley
Director of Agency Liaison
The White House

ES-F-1325.12
(11-90)

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DATE CORR: 01/30/92 DATE RECD: 01/30/92 DATE CNTRL: 01/30/92 DATE DUE: 02/12/9
LETTER: X MEMO: _ TWX: _ OTHER: _____ TO: SECY: _ DEP SEC: _ UN SEC: _ OTHER: AD

FROM: KELLEY, SALLY DC O
ROTHWELL, JED GA C

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SUBJ: RESEARCH & TECHNOLOGY
GENERAL
WANTS MORE RESEARCH ON COLD FUSION

FOR USE BY ACTION OFFICE ONLY

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1				
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REFERRAL

JANUARY 30, 1992

TO: DEPARTMENT OF ENERGY

ACTION REQUESTED:
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DESCRIPTION OF INCOMING:

ID: 303072

MEDIA: LETTER, DATED JANUARY 10, 1992

TO: PRESIDENT BUSH

FROM: MR. JED ROTHWELL
2060 PEACHTREE INDUSTRIAL COURT
SUITE 312-F
CHAMBLEE GA 30341

SUBJECT: WANTS MORE RESEARCH ON COLD FUSION

PROMPT ACTION IS ESSENTIAL -- IF REQUIRED ACTION HAS NOT BEEN TAKEN WITHIN 9 WORKING DAYS OF RECEIPT, PLEASE TELEPHONE THE UNDERSIGNED AT 456-7486.

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SALLY KELLEY
DIRECTOR OF AGENCY LIAISON
PRESIDENTIAL CORRESPONDENCE

ID # 303072

AF

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O - OUTGOING
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X I - INCOMING
Date Correspondence Received (YY/MM/DD) 92 / 1 / 15

Name of Correspondent: ☐ Mr ☐ Mrs ☐ Miss ☐ Ms. Jed Rothwell

☐ **MI Mail Report** **User Codes: (A)** _____ **(B)** _____ **(C)** _____

Subject: Wants more research on cold fusion.

ROUTE TO: | **ACTION** | **DISPOSITION**

Office/Agency (Staff Name)	Action Code	Tracking Date YY/MM/DD	Type of Response	Code	Completion Date YY/MM/DD
CoWals	ORIGINATOR	92/10/16			92/10/
	Referral Note:				
✓ DOE	R	92/10/30			92/10/
	Referral Note:				
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ACTION CODES:
A - Appropriate Action
C - Comment/Recommendation
D - Draft Response
F - Furnish Fact Sheet to be used as Enclosure
I - Info Copy Only/No Action Necessary
R - Direct Reply w/Copy
S - For Signature
X - Interim Reply

DISPOSITION CODES:
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B - Non-Special Referral
C - Completed
S - Suspended

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Type of Response = Initials of Signer
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5/81

Cold Fusion Research Advocates
2060 Peachtree Industrial Court, Suite 312-F
Chamblee, Georgia 30341

Phone: 404-451-9890 Fax: 404-458-2404

January 10, 1992 #303072

The Honorable George Bush
The White House
1600 Pennsylvania Ave. NW
Washington, D.C. 20501

Dear Mr. President,

I would like to tell you an extraordinary story. Please suspend your judgement for a moment; please read these pages with an open mind, because, as we saw last year in Russia, amazing things can happen, and there is more in heaven and earth than dreamt of in our philosophy.

Do you remember the anomalous effect called "cold fusion" which was revealed in 1989 by Drs. Pons and Fleischmann? Cold fusion has now been verified by more than 600 scientists in over 100 world class laboratories, including: Los Alamos, Oak Ridge, Naval Weapons Center at China Lake, Naval Research Laboratory, Naval Ocean Systems Center, Texas A&M, BARC India, Hokkaido University and National Institute for (hot) Fusion Science in Nagoya.

Japan is particularly active in this field. Every major Japanese university has formed a cold fusion research group, and these groups are represented at major physics meetings like Japan Nuclear Energy Conference and the ISEM-Nagoya IEEE meeting this month (see attached). One of the largest Japanese corporations has hired Drs. Pons and Fleischmann, and given them carte blanche resources. Pons and Fleischmann have made extraordinary progress. They, and the other leading experts in this field, predict that working reactors may be ready in two to five years.

I speak Japanese fluently, I work closely with Japanese and U.S. researchers. I can supply you with any amount of published scientific material to prove that research is underway, and that many scientists believe cold fusion is real.

Now, here is the unbelievable part: our government, which spends $337 million to research just one form of hot fusion, refuses to spend a penny on cold fusion. Most of our scientific magazines refuse to publish papers about cold fusion, in fact, they treat it as some kind of 'national joke.'

In October 1991 Dr. Eugene Mallove and I began circulating the enclosed Petition calling on Congress to restore token funding. Over 300 people have signed, including:

Nobel Laureate in Physics Dr. Julian Schwinger
M.I.T. senior scientists Drs. Kolm, Labitt, Rafuse and Covert
Dr. Hawkins of Smithsonian and Boston U. (retired)
Noted hot fusion scientist Dr. Frederick Mayer
Dr. Fritz Will
Representatives from all the U.S. National Laboratories listed above
Dr. Ikegami, head of Japan's largest program at the National Institute
Dr. Srinivasan, head of India's major program
Dr. Iyengar, Chairman, Atomic Energy Commission, Government of India
Many of the top workers from Italy

If cold fusion is, as the skeptics assert, 'pathological science,' with absolutely no merit; if it is completely unworthy of attention, then why have all these scientists signed this petition? Is it possible, even remotely possible, that they have been duped? What would it take to bamboozle these people?

I am not arguing from authority; I am not claiming that these famous scientists must be right, so cold fusion must be real. However, these scientists believe that it is real, they have gone on record, and many of them have volunteered to testify before a Congressional Committee about their research. So, the skeptics must take them seriously; they must reconsider, and look at the scientific evidence again. But, they refuse to do that. Dr. Feshbach of M.I.T said in May 1991: "I have had 50 years of experience in nuclear physics and I know what's possible and what's not. . . I don't want to see any more evidence! I think it's a bunch of junk and I don't want to have anything further to do with it." As Japanese scientists and industry press ahead to understand and develop cold fusion, here in the U.S. our newspapers and scientific establishment accuse cold fusion scientists of fraud, incompetence, of deliberately altering data, of foolishness and "mendacity." They call cold fusion a "ridiculous mistake," and "sad comedy." That, and that alone is their response. They will not look at the data, they refuse to discuss the matter.

Does this sound like an improbable story? Do you think I am exaggerating? Please look at the evidence I enclose. I have plenty more, please contact me if you have any questions, or contact any of the scientists who signed the petition. These scientists desperately need your help and support.

We are watching helplessly as this technology approaches commercialization, month by month, in Japan. Think what may happen if they perfect this technology while we sleep. All cold fusion researchers are alarmed about this, including the Japanese themselves. Some of the top Japanese scientists have signed the Petition, and they have volunteered to come to the U.S. and testify before the Committee about their research. These Japanese scientists support us because: they value the creativity of U.S. science, they would like to see joint research; they are appalled by way their American colleagues are being treated; and finally, they know that if Japan introduces commercial products in few years, while the U.S. does no research, there will be an explosion of outrage in the U.S., and possibly a breakdown in relations between our countries.

Sincerely,

Jed Rothwell

Jed Rothwell

Enclosures:

Petition, list of signatories, letter from Congressman Swett

Review of Experimental Observations About The Cold Fusion Effect, Dr. E. Storms, Los Alamos National Laboratory

Memorandum on the Present State of Knowledge on Cold Fusion, Dr. H. Gerischer, Fritz Haber Institute der Max Plank

Cold Fusion 1992: Basic Facts

Japanese meeting schedules

Comments of Dr. Park, one of the leading skeptics, from the Washington Post

A Petition

Submitted to the Science, Space and Technology Committee,
The House of Representatives, Washington, D.C.

We, the undersigned scientists, engineers, and interested citizens, respectfully request that the House Committee on Science, Space, and Technology, hold intensive hearings on the topic of the new physical phenomenon that is referred to generally as *cold fusion*. The purpose of these hearings, which in depth and breadth should go far beyond the hearing of the Committee that occurred in April 1989, should be to assess the enormous body of scientific evidence that has accumulated and *continues* to accumulate since that time, both in U.S. and foreign laboratories, public and private. These hearings should occur as soon as possible, because the lack of funding in the United States is seriously blocking research.

We are convinced that a new physical phenomenon of potentially enormous scientific and technological significance has been discovered and verified -- even though its precise physical mechanism is not fully understood at present, a typical circumstance in science. We believe that it will be imperative for the Congress to recommend immediate, significant funding for cold fusion research and development. This research has been substantially hampered in the United States by the negative and presently untenable conclusions of the ERAB "Cold Fusion Research" report that was performed for the Department of Energy in 1989. We recommend that the Committee examine and formally reject the conclusions of that report. We also advocate that the Committee recommend adequate funding for cold fusion research in the United States at an initial annual rate of not less than $10 million.

Petition signed by 306 people as of January 10, 1992

223 Scientists and Engineers -

Dr. Radoslav Adzic	Institute of Electrochemistry, ICTM, Yugoslavia
Mr. D. P. Agarwal	Vice President, Leach and Garner Technology
Ms. Joyce L. Alms	Nova Resources Group, Inc.
Mr. Richard L. Amruso	Project Engineer, Smiths Aerospace, Inc.
Dr. George Andermann	Dept. of Chemistry, University of Hawaii
Dr. John J. Antal	Physicist, U.S. Dept. of the Army (Retired)
Mr. George Anthony	Physicist
Dr. Spallone Antonio	INFN - LNF, Franscati, Italy
Dr. John Appleby	Texas A&M University
Dr. Subbiah Arunachalam	Editor, Indian Journal of Technology, New Delhi, India
Ms. Edith E. Attenhofer	Technical Librarian, Smiths Aerospace, Inc.
Mr. Srinath Balachandran	Engineer, Energy Investment
Dr. Marcello Baldo	INFN - Catania, Italy
Mr. Thomas D. Barnard	Dow Corning Corp - Research Engineer
Dr. Robert W. Bass	Registered Patent Agent 29, 130
Mr. David R. Bean	Metropolitan State College of Denver
Dr. C. A. Bennett	University of North Carolina - Ashville
Mr. Christopher Bentley	Computer Consultant
Mr. William J. Bernt	Director of Research, Magnetic Power, Inc.
Dr. Alan Berrick	Magnetic Power, Inc.
Mr. Robert A. Birmingham	R&D Technician, Smiths Aerospace, Inc.
Dr. John O'M. Bockris	Texas A&M University
Dr. Tullio Bressani	Dep. De Fisica Sperimenatale, U. Torino, Italy
Mr. Glen Brickey	Manager of Software/CAE Design, Smiths Aerospace, Inc.
Mr. Seva Brodsky	Electrical Engineer, Graduate Student, Northeastern U.
Mr. Allen O. Brosius	Fusion Information Center
Mr. Herbert A. Bruzel	Mechanical Designer, Smiths Aerospace, Inc.
Dr. Romulus V. Bucur	Institute of Chemistry, University of Uppsala, Sweden
Mr. Michael A. Burke	Consulting Software Engineer
Mr. Forrest C. Burns	Retired Nuclear Chemist
Dr. Ben Bush	Naval Weapons Center, China Lake, CA
Dr. Robert T. Bush	Professor of Physics, California State Polytechnic Univ
Mr. Kant W. Campbell	Senior Programmer
Mr. Dean W. Carver	Fusion Information Center
Dr. Milton A. Chace, P.E.	Chace & Associates Engineering
Dr. Chun-Ching Chien	Texas A&M University
Dr. Scott R. Chubb	Private Citizen; Naval Research Laboratory
Dr. Talbot A. Chubb	Research Systems Inc, Fellow of American Physics Soc.
Dr. P. L. Cignini	CNR-CTCAT, Dept Chemistry, Univ. "La Spaienza," Italy
Mr. Neil J. Condosta	Project Engineer Group Leader, Smiths Aerospace, Inc.

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Mr. Frank M. Costanza	Engineer, Smiths Aerospace, Inc.
Dr. Eugene E. Covert	M.I.T., Prof. of Aeronautics and Astronautics
Mr. Roger Craddock	Project Engineer, Smiths Aerospace, Inc.
Dr. Dennis Cravens	Physicist
Mr. Henry P. Dart III, Atty	Attorney, geologist
Dr. Celani De Francesco	Instituto Nazionale Fisca Nuleare, Italy
Mr. Jesse J. Dean	Config. Management Specialist, Smiths Aerospace, Inc.
Mr. John D. Deith	QMI
Mr. Emilo Del Giudice	Instituto Nationale Fisca Nucleare, Italy
Mr. Louis Dick	CERN (Geneva) and Milano University
Mr. Lee Dilley	Comdisco Systems, Inc.
Mr. Lawrence Dodson	Design Engineer, Smiths Aerospace, Inc.
Dr. Xavier Domenech	Universitat Autonoma Barcelona, Spain
Mr. A. M. Dougill	Group Leader, Smiths Aerospace, Inc.
Mr. Douglas Drake	KMS Fusion (contractor)
Mr. Jerome Drexler, D.Sc. Hon.	Inventor, industrialist
Mr. Lee John Droege	President, Snowflake Mtg, Ltd.
Mr. Thomas F. Droege	Fermilab
Dr. Robert D. Eagleton	California State Polytechnic University
Mr. James H Elkins	Consulting Engineer
Mr. Paul D. Epstein	Epstein And Fass Associates
Dr. Samuel Faile	Cold Fusion Products
Dr. Avard F. Fairbanks	Electrical Eng. & Physicist, Fusion Information Center
Dr. John J. Farrell	Franklin and Marshall College
Mr. D. J. Fazioli	Mgr, Mechanical Engrg, Smiths Aerospace, Inc.
Dr. Al Fermelia	Nova Resources Group, Inc.
Mr. Mark Ferris	Electric Tech., Metrology Lab, Smiths Aerospace, Inc.
Mr. Harvey Fiala	Rockwell International
Mr. Mark Fletcher	Engineer, Smiths Aerospace, Inc.
Mr. Fred B. Folsom	Mechanical Engineer, Smiths Aerospace, Inc.
Dr. Harold L. Fox	Fusion Information Center
Mr. Alfred J. Gartman, Jr.	Engineer, Mat'l Analyst, Smiths Aerospace, Inc.
Mr. Russ George	Research, Inc.
Mr. Jose Giner	Giner, Inc.
Ms. Judith Gluck	Chemist, Inst. of Isotopic & Molecular Tech, Romania
Dr. Peter Gluck	Institute of Isotopic & Molecular Technolgy, Romania
Mr. Mark Goldes	Magnetic Power, Inc.
Dr. Alfred D. Goldsmith	Physicist
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Mr. Roger P. Ham	Science columnist, Executive Intelligence Review
Dr. Peter H. Handel	University of Missouri, St. Louis
Mr. Grant B. Harris	Workframe, Inc.
Mr. Frank Hasiwar	Mathematician, Rio Hondo, CA
Dr. Gerald S. Hawkins, D.Sc.	Smithsonian (Ret.) Prof. Astronomy, Boston U. (Ret)
Col. Meredith E. Hendricks	U.S. Army, Retired
Dr. Dalibor Hodko	Texas A&M University
Dr. Heinrich Hora	CERN, Switzerland
Mr. William M. Horton	Logistics Section Leader, Smiths Aerospace, Inc.
Dr. Hideo Ikegami	National Institute for Fusion Science, Nagoya, Japan
Mr. Paul William Isidisin	Civil Engineer, Bayonne New Jersey
Dr. P. K. Iyengar	Chairman, Atomic Energy Commission, Gov. of India
Mr. Neil R. Jacob	Senior Design Engineer, Smiths Aerospace, Inc.
Dr. King-Tsai Jeng	Texas A&M University
Dr. Shang-Xian Jin	Graduate School, Chinese Academy of Science
Mr. Dennis A. Jones	Software Engineer, Smiths Aerospace, Inc.
Dr. Walter Juda	Electrochemist, Chairman of Board, Prototech Co.
Mr. Charles Kaminski	Concerned citizen
Mr. Enloe W. Kay	Sr. Reliability Engineer, Smiths Aerospace, Inc.
Mr. Kevin Kelleher	Research Engineer, Smiths Aerospace, Inc.
Dr. John Kenny	Prof. of Physics, Bradley University, Peoria, IL
Dr. Norbert J. Kertamus	Southern California Edison
Dr. Boris M. Khudenko, P.E.	Khudenko Engineering, Inc.
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Dr. Scott Kimbrough	University of Utah
Mr. James B. Klay, Jr.	Design Assurance Group Leader, Smiths Aerospace, Inc.
Dr. Henry H. Kolm	M.I.T. Senior Scientist (emeritus), Lecturer
Mr. Melvin Labitt	M.I.T.
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Mr. Al Leedahl	Engineering Design Concepts
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Dr. Xing Zhong Li	Dir. Fusion Power Prog, Tsinghua U., Bejing, China
Dr. Bor Yann Liaw	University of Hawaii at Manoa
Dr. Bruce Liebert	Associate Professor, University of Hawaii
Dr. Charles W. Lindenmeier	Siemens Nuclear Power Corp.
Mr. Angel L. Lozano	Northrop
Dr. Eugene F. Mallove	M.I.T., Engineer and author
Mr. Alfred S. Marotta	Physicist, U.S. Army Materials Technology Lab. (Ret.)
Mr. John Marshall III	Surface Solutions, Inc.
Mr. Juan Martinez	Design Engineer, Smiths Aerospace, Inc.
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Mr. Harold D. McKee	Software Engineer, Smiths Aerospace, Inc.
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Dr. Ravi Kumar Meruva	Texas A&M University
Mr. E. A. Mettenet	Consultant, Utah Fusion/Energy Advisory Council
Dr. Melvin Miles	Naval Weapons Center
Mr. Laddy M. Miller	Group Leader, Smiths Aerospace, Inc.
Ms. Ljiljana Minevski	Texas A&M University
Mr. Zoran Minevski	Texas A&M University
Dr. Tadahiko Mizuno	Faculty of Engineering, Hokkaido University, Japan
Dr. Richard S. Moog	Franklin and Marshall College
Dr. John W. Moore	New Business Development Manager, Dupont R&D
Dr. Oliver J. Murphy	Lynntech, Inc.
Dr. T. S. Murthy	Dir. Isotope Group (Retired) & Consultant, BARC, India
Mr. Dean S. Musgrave	Director, Ultrafibre, Inc.
Dr. He Muzhi	Texas A&M University
Dr. Thomas Najarian	Physician
Mr. Fujio Nakano	Journalist author, Japan
Mr. Ronald Newman	BBN Software Products
Dr. Ken-ichiro Ota	Yokohama National University, Japan
Dr. Eric H. Ottewitte	Idaho National Engineering Laboratory
Mr. David A. Paddock	Engineer
Dr. Evelina Palibroda	Instutute of Isotopic & Molecular Technology, Romania
Dr. Stephen J. Pike	Nova Resources Group, Inc.
Mr. Deane H. Platt	Systems Analyst
Mr. Richard D. Powell	Lab Technician, Smiths Aerospace, Inc.
Dr. Giuliano Preparata	Universita' Di Milano, Italy
Dr. Lee Pritchard	Oakland University
Mr. Louis G. Puls, Jr.	Museion Research Corp.
Mr. Lawrence G. Quintana	Los Alamos National Laboratory
Dr. Robert P. Rafuse	M.I.T. Lincoln Laboratory
Dr. M. Ragheb	University of Illinois at Urbana - Champaign
Mr. Evan Ragland	Engineer
Dr. Raj K. Rajamani	University of Utah
Mr. Chip Ransford	Nova Resources Group, Inc.
Dr. John R. Reitz	Independent consultant
Dr. Abdu Reyimjan	Texas A&M University
Mr. Clayton R. Robert	Systems Analyst, Quantum Mechanics
Mr. Stephan A. Roen	Patent Attorney
Mr. Richard G. Rohde	Staff Programmer, State of California
Mr. Charles Ross	Kansas City Power & Light Co.
Mr. Andrew E. Rothovius	Columnist, Peterborough (NH) Transcript

Mr. Jed Rothwell	Cold Fusion Research Advocates
Dr. Carlos Sanchez	Universidad Autonoma, Madrid, Spain
Dr. Gary Sandquist	University of Utah
Dr. Franco Scaramuzzi	ENEA, Frascati, Italy
Mr. Karl Scheucher	Retiree from TRW - R&D Engineer
Mr. Karl F. Scheucher	Modtech Corp.
Dr. Glen Schoessow	University of Florida
Dr. Julian Schwinger *	Nobel Laureate in Physics, UCLA
Dr. Charles D. Scott	Chemical Engineer
Mr. Cliff J. Scribner	Motorola, Inc. Mechanical Engineer
Ms. Carole Seffrin	Secretary, Smiths Aerospace, Inc.
Dr. Nobuo Shimazu	Physicist, NTT LSI Laboratories, Japan
Dr. V. K. Shrikhande	Neutron Physics Division, BARC, India
Dr. Massoud Simnad	University of California in San Diego
Dr. M. Srinivasan	Head, Neutron Physics Division, BARC, Bombay, India
Dr. Supramaniam Srinivasan	Center for Electrochem. & Hydrogen Resrch, Texas A&M
Mr. George A. Stage	Orion Research
Dr. Michael R. Staker, P.E.	Private citizen; U.S. Army Materials Technology Lab.
Dr. Frank Stenger	Dept. of Computer Science, University of Utah
Mr. Mark L. Stevens	M.I.T. Lincoln Laboratory
Dr. Edmund K. Storms	Los Alamos National Laboratory
Mr. Roger Paul Stout, P.E.	Motorola Inc. Advanced Packaging Design
Mr. Veni Sumaria	Sumaria Systems, Inc.
Dr. Mitchell R. Swartz *	Jet Technology
Dr. Carol Talcott-Storms	Los Alamos National Laboratory
Dr. Donald C. Taylor	President, SRM Corporation, Denver, CO
Dr. Charles Tennakoon	Texas A&M University
Mr. Doug Tennant	Program Manager, Smiths Aerospace, Inc.
Mr. Daniel H. Thompson	Motorola, Inc.
Mr. E. C. Tibbals	Vaughn Precision Products, Inc.
Mr. Christopher P. Tinsley	Computer Engineer
Mr. Kent Tolley	Rockwell International
Dr. Massimo Tomellini	Dept. Chemistry, Univ. di Roma, Italy
Dr. Marcello Truzzi	Dept. of Sociology; Eastern Michigan University
Dr. G. R. Valenzuela	Naval Research Laboratory, Washington, DC
Dr. Stanley H. Vegors, Jr.	Physics Department, Idaho State University
Dr. Gopala Venkateswaran	Applied Chemistry Div., BARC, Bombay, India
Mr. James D. Vera	Engineer
Mr. J. L. Waisman	Consultant, Southern California Edison
Ms. Laura R. Walker	Engineer, Smiths Aerospace, Inc.
Dr. Fritz G. Will	University of Utah
Mr. Steven E. Windner	R&D, Smiths Aerospace, Inc.
Mr. Joe K. Winner	Manager of Fuel Syst. Design, Smiths Aerospace, Inc.
Dr. Michael Wixom	KMS Fusion 1984 - 1991

Mr. Larry Wolf	R&D Lab, Smiths Aerospace, Inc.
Mr. She-Sheng Xue	Instituto Nationale Fisca Nucleare, Italy
Mr. Don Yansen	Crossroads Technology
Mr. Frank Yashar	Digital Instruments - Santa Barbara, CA
Dr. Jerry Zenger	Univ. of Utah, Engineering Experiment Station
Dr. Sheng-Bai Zhu	Texas Tech University
Ms. Charlotte Zimmerman	Los Alamos National Laboratory

83 Concerned Citizens -

Ms. Riehara L. Albert	Configuration Management Clerk, Smiths Aerospace, Inc.
Mr. A. Amokrane	Executive Manager
Ms. Marie Claire Amokrane	Concerned Citizen
Mr. John G. Bailey	Schiller Institute
Mr. Robert W. Barton	Robert W. Barton & Associates
Mr. Mohamed N. Baterdouk	Concerned citizen and college student at WPC, NJ
Mr. Ken Berian	Concerned citizen
Ms. Arlene C. Berry	Marketing Manager
Mr. Terry Burnsed	City Stage Ensemble
Mr. Ray Busch	Concerned citizen
Ms. Michele R. Capps	Smiths Aerospace, Inc.
Mr. Charles Carrol	Teacher, Belpre High School
Ms. Eloise E. Carrol	Corning Glass Works
Mr. Robert L. Carroll	Carroll Research
Ms. Shirley Palmer Collier	Concerned citizen
Mr. Wm. H Conboy	Mktg V.P. (Ret.), Shelter Insurance Co.
Mr. Joseph P. Conrad	President, Cactus Communications
Ms. Isabel S. Courtney	Southern Connecticut State University ISIS Program
Ms. Sheila M. Cronin	Southern Connecticut State University ISIS Program
Mr. Stephen R. Daisy	Retired teacher (Latin & English) State of Mass.
Mr. Terence E. Dalton	Musician
Mr. Den'alh Damron	Student, CSU
Ms. Eileen Danahy	Denver Museum of Natural History
Mr. Hal Davis	Concerned Citizen, Mktg Mgr, ISI
Mr. John Devlin	Prudential Financial Services
Mr. Gordon Erickson	Concerned Citizen, Tampa, FL
Mr. Charles A. Evans	Manager, University of Utah Research Park
Mr. Paul D. Fairchild	Private citizen
Ms. Eliza McCormick Feld	Mass. Bar Association
Ms. Bonnie K. Fiala	Science Teacher
Mr. James Fiala	Registered nurse
Mr. John E. Fiala	Continental Airlines
Ms. Kathy Fiala	College Student
Mr. Peter E. Fiala	College student

Mr. Ronald Flores	Business Consultant - Private Citizen
Mr. Vernon Fogg	Concerned citizen
Ms. Betty Fortson	Concerned citizen
Mr. Donald L. Fredley	Eden Barn Industries
Mr. Brent Frye	Concerned citizen
Dr. Thomas J. Gilmore	Retired Chairman of Philosophy, Gonzaga University
Mr. Jeremiah Hallaren	Concerned citizen
Ms. Majorie Mazel Hecht	Managing Editor, 21st Century Science & Technology
Ms. Evelyn Hermanstyne	Concerned citizen
Mr. Dan Hiester	Theatre producer - director
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Mr. William F. Jackson	Art Director, Smiths Aerospace, Inc.
Ms. Vannessa Johnson	Concerned citizen, Public Service Co. of Colorado
Ms. Debra King	Concerned citizen
Mr. Edwin A. Klinkhammer	Southern Connecticut State University ISIS Program
Ms. Mary Krah	Smiths Aerospace, Inc.
Ms. Doris J. Langlois	Concerned citizen
Mr. David A. Lewis	Concerned citizen
Ms. Ruth Lippmann	Concerned citizen
Ms. Elizabeth L. Lozano	Concerned citizen
Mr. Ernest G. Martin, Jr.	Attorney at Law, Gulfport MS
Ms. Lynn W. McCanney	School Teacher
Ms. Pamela H. Meares	Teacher
Ms. Katie Minor	Teacher
Mr. David D. Moon	Concerned Citizen; former H.S. science teacher
Mr. Cris Moore	Concerned Citizen; student, Univ. of Maryland
Ms. Dianne P. Moore	Concerned Citizen
Mr. John J. Moore	Lindly & Co., Inc.
Ms. Lisabeth Moore	Concerned Citizen; student, Univ. of Delaware
Mr. David M. Nadeau	President, Super Steak Sub Shoppe, Inc.
Mr. Nicklos J. Plehn II	Student, University of Michigan
Ms. Paulette Pope	Vice President, TDC, Inc.
Mr. Anu Pugalia	Student
Ms. Miriam Rakieten	Concerned citizen
Mr. Emil B. Rechsteiner	Concerned citizen
Ms. Ruth L. Sanchez	Teacher
Mr. Daryl Sattui	C.E.O., V. Sattui Winery
Mr. Leo W. Seal, Jr.	Concerned Citizen
Mr. Jerry Shapiro	Concerned citizen
Mr. Jon Shapiro	Concerned Citizen
Ms. Mollie Shapiro	Concerned Citizen
Mr. Joe Simmons	Architect
Mr. W. Evan Sloane	M.I.T. Management Alumnus (Johns Hopkins Undergrad)
Mr. James Philip Sperry	Concerned Citizen, Student, Tampa, FL

Mr. Frank N. Stempfer	Eden Barn Industries
Mr. Thomas E. Stolper	Author
Ms. Donna Unck	University of Utah Research Park
Ms. Theo S. Vaughey	Museum Trustee
Ms. Carol White	21st Century Science & Technology Magazine

* Did not wish to specify a minimum dollar amount as stated in the last line of petition.

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COMPETITIVENESS

SELECT COMMITTEE ON AGING

December 21, 1991

Jed Rothwell
Cold Fusion Res. Advocates
2060 Peachtree Industrial Ct.
Suite 312-F
Chamblee, Georgia 30341

Dear Jed,

Thank you for your letter and the petition advocating that Congress hold further hearings regarding cold fusion.

I have contacted the Science, Space, and Technology Committee, which is currently investigating recent developments in cold fusion research. Hearings on cold fusion may be held in late winter or early spring.

Please don't hesitate to contact me again if you have any further questions.

With warmest regards,

Dick

Dick Swett
Member of Congress

REVIEW OF EXPERIMENTAL OBSERVATIONS ABOUT THE COLD FUSION EFFECT

COLD FUSION

KEYWORDS: *cold fusion, review, experimental*

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The experimental literature describing the cold fusion phenomenon is reviewed. The number and variety of careful experimental measurements of heat, tritium, neutron, and helium production strongly support the occurrence of nuclear reactions in a metal lattice near room temperature as proposed by Pons and Fleischmann and independently by Jones.

I. INTRODUCTION

Since the first proposals that nuclear fusion reactions might occur in a metal deuteride,[1,2] a considerable accumulation of supporting and nonsupporting information, both quantitative as well as anecdotal, has become available in refereed journals, reports, and conference proceedings. Earlier reviews were prepared by Bockris et al.,[3] Hurtak,[4] Rabinowitz,[5] Tsarev,[5] Schultze et al.,[5] and Srinivasan.[6] Books have been published by Peat,[7] Mallove,[8] and Close.[9] Dehn[10] and Huggins[11] provided a discussion of the electrochemistry of cold fusion cells and hydrogen/metal interaction, respectively. A summary of theoretical approaches was given by Miley et al.[12] and by Preparata.[12] A selective annotated bibliography was prepared by Braun.[13] Tsarev and Worledge[14] prepared overviews of the two conferences held in Utah, one in March 1990 and the other in October 1990.

Because the field is growing so rapidly, much work is still unavailable except in reports, preprints, and word of mouth.[15,a] Such information is not used in this review to a significant extent because it cannot be examined by those who may wish to make their own analysis of this controversial subject. This review includes only those published papers I have been able to personally review. My intent is to reduce duplication of effort in the field by bringing together most of the major published studies, to demonstrate that important patterns have been observed that cannot be explained adequately by chance or error, and to help focus future work in more productive directions while giving optimism for success. I, for one, am amazed to find so many excellent papers, especially in view of the very negative reporting by the press.[9,16-21] I apologize to those authors who do not find their work cited herein. Such omissions are due only to limitations on my part.

At the present time, understanding of the field is divided between those who do not believe there is suf-

[a]A great deal of up-to-date information, insight, and opinion can be obtained from *Fusion Facts*, published by the Fusion Information Center, P.O. Box 58639, Salt Lake City, Utah, 84158.

Copies of the conference proceedings cited herein can be obtained as follows:

Proc. 1st Annual Conf. Cold Fusion
National Cold Fusion Institute
390 Wakara Way
Salt Lake City, Utah 84108 ($55.00)

Anomalous Nuclear Effects in Deuterium/Solid Systems
Prof. S. E. Jones
Brigham Young University
Physics Department
Provo, Utah 84602 ($68.00 to S. & J. Scientific Company)

Cold Fusion Symp., 8th World Hydrogen Energy Conf.
University of Hawaii
Hawaii Natural Energy Institute
2450 Dole Street, Holmes Hall 246
Honolulu, Hawaii 96822 ($15.00).

ficient information to require a re-evaluation of conventional beliefs about atomic fusion and those who believe that the data, although incomplete, strongly suggest the possibility of unusual nuclear reactions in a metal lattice. An approach that would do justice to both viewpoints is difficult to contain in one review. Therefore, I adopt the more probable viewpoint by assuming that a real phenomenon has been discovered and try to show patterns of behavior that would give some understanding of its characteristics. In a few cases, I attempt to show how the proposed errors have been reduced or eliminated and how various conventional explanations do not apply. Hopefully, those who do not believe that any one study is sufficiently complete to change their minds might be impressed by the consistent patterns of behavior among various studies and the considerable background information that is now available. This review is not intended as a substitute for reading the original papers.

I suggest that the lack of reproducibility or easy explanation can no longer be used to dismiss positive results as misinterpretation of normal behavior, however tempting this might be. Nevertheless, this lack of reproducibility does justify some frustration, and it does slow progress. In any other field, this frustration would stimulate a redoubling of efforts to discover the implications of the negative observations. One would think this would be especially true in a field having such importance to nuclear physics and a potential for clean, unlimited energy. This positive approach is being taken in Japan, India, and the USSR in contrast to most other countries.[b]

A growing literature giving explanations for cold fusion is available but is not discussed in this review. Although some theories offer important insight, it is still too early to make a proper assessment. It is worth noting that conventional theories have been extended in an attempt to explain at least low-level neutron production.[12,24-101] Only a few of these attempts conclude that cold fusion is possible, but many more do not reach this conclusion. General failure of the conventional approach has inspired many new models.[95,102-122] The issue is no longer the limitations perceived by some[9,16-18,123-135] to exist in early work nor the perceived conflict with conventional theories of fusion. The problem now is to properly explain a growing body of increasingly sophisticated data being generated by competent scientists in many countries.

Many "negative" studies have been reported for heat,[126,130,136-151] tritium,[139,140,149,152-157] protons (Refs. 153, 156, and 158-167), ^{3}He (Refs. 153, 168, and 169), electromagnetic radiation (Refs. 140, 142, 145, 148, 151, 153, 164, and 170-183), and neutrons (Refs. 130, 139-142, 145, 147-149, 151-156, 164, 170-173, and 178-211). This negative work can give valuable information about conditions that prohibit the cold fusion effect and an understanding that can help bound the conditions in which the effect operates. It does not demonstrate that the effect is not real. In many cases, no nuclear products were observed because conditions were present that are now known to be unsuitable for cold fusion to occur. In other cases, it is not clear why the conditions failed to produce positive results. Many people who report positive observations also have many occasions when negative results are found. Clearly, this incredible phenomenon is not going to be easy to understand. However, it is unfortunate that so many excellent detection techniques and talented scientists have been assembled in one place during several of the better experiments only to have nothing to measure.

An equally large number of positive observations for heat,[1,155,212-234] tritium,[1,3,219,223,232,233,235-252] and neutron production[2,217,219,224,229,235,236,239,243,246-282] have been published. Recently, two unconventional approaches have given excess heat: a fused salt cell[222] and a cell using ion discharge.[224] In addition, a new particle (the iton) is claimed to have been detected.[283] Of course, these positive results are not all equally credible. Less well studied during confirmed fusion is the emission of electromagnetic radiation (from visible to gamma rays).[217,233,255,256,264,272] Emission of energetic nucleons from the deuteride surface[168,283-287] and ^{4}He formation[115,169,220,228] have been examined. Excess heat, tritium, and neutrons have not only been produced but have also been detected using a variety of techniques. This demonstrates that the positive observations are not caused by a particular method of production or detection.

Success depends on giving the deuterium atoms some additional energy, required to achieve high deuterium-to-metal ratios and to overcome the fusion barrier by an as yet unknown process. A variety of methods have been used to impart this energy. Examples are listed in the order of increased energy.

1. Direct Gas Reaction: In these experiments (Refs. 147, 168, 170, 172, 180, 188, 193, 200, 207, 210, 235, 244, 249, 252, 259, 264, 266, 269, 272, 274, 276, and 280), titanium or palladium is placed in D_2 gas at pressures from <1 bar to megabars with the usual pressure between 40 and 60 bar. Energy is proposed to be imparted to the deuterium atoms by phase changes or microcracking initiated by temperature or pressure cycling. Neutron emission has been detected, especially when titanium is used. No reaction is seen when all of the titanium is reacted with deuterium, after multiple temperature cycles, or if normal hydrogen is used

[b]Two percent of the hot fusion budget is devoted to cold fusion in Japan according to J. Bockris.[3] According to P. K. Iyengar,[22] chairman of the Indian Atomic Energy Commission, broadly based work is continuing in India. According to D. Worledge,[23] 15 million rubles will be allocated to 20 laboratories over the next 4 years for cold fusion research in the USSR.

instead of deuterium. Tritium has been detected in the gas and metal after neutrons were produced on a few occasions.

2. Conventional Wet Electrolytic Approach: Typically, a palladium or titanium cathode and a nickel or platinum anode are placed in an electrolyte consisting of a mixture of D_2O containing LiOD, NaOD, or Li_2SO_4 alone or combined with other salts. The container is designed to prevent atmospheric contamination but with a vent to allow escape of generated gas. This arrangement is called an "open" cell and is drawn to show the simplest configuration in Fig. 1. When a catalyst is placed in the assembly to recombine the generated gases, the cell can be completely sealed, and a "closed" cell is created. Direct current of various magnitudes is passed between the electrodes so that D_2 is formed at the cathode and O_2 is produced at the anode. Because of the applied voltage, very high effective pressures can be generated within the metal,[c] and the ions are given a modest energy. This very chemically active deuterium reacts with the palladium or titanium cathode to form a hydride having a high but variable stoichiometry that depends on a complex set of circumstances. The stoichiometry that is achieved in localized regions appears to play a role in making the effect occur. Many variations on this basic design have been reported. Excess heat, neutrons, protons, and tritium have been detected using this method, although not all at the same time. A sudden change in charging current seems to trigger the cold fusion effect. However, initiation also has been observed without this abrupt change.

3. Electric Current in Gas-Containing Cells: In these experiments,[224,235,240,288] a voltage sufficient to produce gas discharge is applied to electrodes in low-pressure deuterium gas. This process gives the deuterium ions more energy than they can achieve in an electrolytic cell. Both palladium and titanium have been used as the cathode. Unexpectedly large amounts of neutrons, tritium, and excess heat have been detected but not all in the same experiment. One study has achieved tritium and neutron production by passing pulsed current through alternating layers of palladium and silicon disks[240] in high-pressure D_2 gas. This is a combination of gas discharge and direct gas reaction.

4. Ion Bombardment and Implantation: In these experiments,[165–167,178,187,199,236,266,267,284,289,290] ions and molecules are accelerated to various energies and impacted on various target materials. This technique not only implants deuterium into the metal lattice to give a very high deuterium-to-metal ratio, but it can also produce ion energies that approach those used in hot fusion. Neutrons, tritons, and protons have been detected that, in some cases, were in greater quantity than expected. Implantation of hydrogen in metals has been reviewed by Myers et al.[291]

Typically, all of the conditions listed above produce bursts of nuclear products sometimes superimposed on a relatively steady lower production rate. This behavior implies a nonequilibrium state (Refs. 33, 256, 267, and 292–294) and/or two independent processes. Apparently, suitable conditions can be imposed by changing the environment or are produced as a natural consequence of electrolytic action. Note that electrolysis may appear steady in a macro sense, but the conditions are nonequilibrium when viewed on a micro level. Bubble formation produces chaotic regions in which the electrolytic reaction is turned off and on, thereby rapidly changing the chemical environment of the surface. Erratic formation of bubbles, microcracks, and dislocations make the interior of the metal very complex. These factors, by themselves, would make the phenomenon very difficult to reproduce. Indeed, most normal physical and chemical processes involving palladium are frequently difficult to reproduce.

Apparently, the creation of some types of special condition are essential if the nuclear effect is to occur at all. Reproduction of these special conditions is the primary problem in making a general study of the nuclear processes possible. Although many workers who have continued to study the cold fusion effect have been rewarded by increased reproducibility within their own laboratories, a universal recipe does not seem to

[c]The magnitude of the pressure can be calculated using the Nernst equation. However, the very nonideal nature of the solution must be taken into account to obtain the correct pressure.

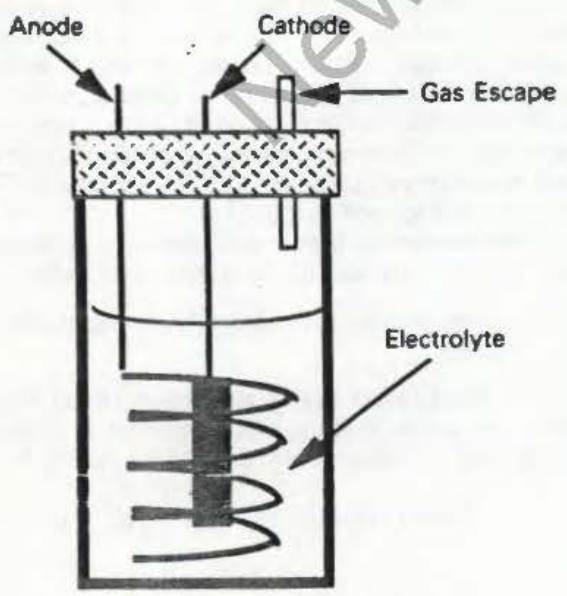

Fig. 1. A simple electrolytic cell.

exist. Just too many unknown and, therefore, uncontrollable variables are present. However, there are quite a number of conditions that are now known not to produce positive results. Some of these insights are examined.

An important special condition that must exist for any of the nuclear reactions to be initiated is the presence of high, local concentrations of deuterium, as previously suggested.[1,295] This view is widely accepted. Such high concentrations have been proposed to be associated with regions of stress,[264,296] to be enhanced by surface or bulk impurities, and to be associated with phase changes.[118] On the other hand, the deuterium concentration can be reduced by excessive microcracking or by normal hydrogen in the metal.[297] Generally, these high concentrations are expected to occur at or near the surface, especially during electrolytic charging or ion bombardment. These considerations as well as many observations suggest that tritium production occurs mainly at the surface for both titanium and palladium. Heat and neutron production are not so easily located in this respect. Nevertheless, many workers still propose that the heat reaction occurs within the entire volume of the electrode. Consequently, they report heat production as energy per cubic centimetre or energy per mole. This gives the impression of much greater energy release than has actually occurred from the small cathode volume used in the cell. This approach also underestimates energy density within the few active regions.

From the first, the deuterium-deuterium (D-D) fusion reaction was thought to be the most likely source of nuclear products, although other possibilities have been proposed. This reaction has two branches that have been measured at high energies, yielding an almost equal probability. These branches are

$$D + D = {}^3He\ (0.82\ MeV) + n\ (2.45\ MeV)$$

and

$$D + D = H\ (3.02\ MeV) + t\ (1.01\ MeV)\ .$$

A third branch,

$$D + D = {}^4He + \gamma\ (23.5\ MeV)\ ,$$

is possible, but it has a very low probability in plasma reactions. Considerable confusion and disbelief has been created by the lack of neutrons, consistent with detected tritium and heat. Data now clearly show the following:

1. No experiment has detected sufficient neutrons or tritium to account for the reported heat.[298] Although neither tritium nor neutrons are usually detected during heat production, there is evidence that ^{4}He is produced.[169,222,228]

2. While there is a clear relationship between neutron and tritium production, occasionally neutrons are produced without any tritium being found.

In order for these and other apparently conflicting observations to be explained, even to a limited extent, an important hypothesis is proposed. At least three nuclear reactions are proposed to occur within a metal deuteride lattice. One reaction produces the major source of heat, the second produces mainly tritium with a few neutrons giving a neutron-to-^{3}H ($n/^3$H) ratio near 10^{-9}, and the third appears to produce mainly neutrons. Which of these operates at any time depends on the special conditions that exist at that time. Of course, more than one of these reactions could occur at the same time but at different locations in the metal. Additional reactions have been suggested, but the evidence is less compelling. Detailed experimental justification for this suggestion and a model that combines the various mechanisms and nuclear reactions is developed in this review. The experimental studies for heat, tritium, neutron, gamma-ray and ^{4}He production are discussed.

II. EXCESS HEAT PRODUCTION

Four major logic levels are associated with understanding the source of excess heat. The first level asks the question

1. Is the excess heat caused by errors in the measurement?

This possibility is discussed in Sec. II.A.

Should the answer be "no," the question at the next level is

2. What is the reaction that produces excess heat?

As the amount of observed excess energy increases beyond a certain level, the probability for a nuclear origin increases as well. At some level, there is no possibility of evoking a chemical source without major conflict with basic chemical experience. This experience shows that there is an upper limit to the energy that can be obtained from a chemical bond. Thus, there is an upper limit to the energy that can be obtained from the limited quantity of chemicals that exist in a cell. This assertion is discussed in Sec. II.B.

If the amount of heat is sufficiently large and nuclear products are found, the question becomes

3. What nuclear reaction produces the excess energy?

Conventional fusion theory predicts a trivial fusion rate at room temperature. Consequently, if a significant fusion rate does occur, it must be caused by an

unexpected phenomenon involving special conditions that exist in a periodic array of metal atoms. Several theories have addressed this aspect but are not discussed in this review.

The question at the next logic level is

4. What is the new phenomenon, how can it be initiated in the lattice, and what nuclear reactions are influenced by the phenomenon?

At this level of acceptance, we are dealing with an approach normally applied to a mature science. As yet, the cold fusion field has not reached this level in some people's minds.

Efforts to attribute the heat to a chemical source have taken three forms

1. The calorimeter has a positive bias because energy accounting has not been properly made.
2. Energy is accumulated in some chemical form during the initial charging and released later.
3. An unknown reaction releases the energy. The latter source is proposed to be either mechanical or chemical.

II.A. Errors in Calorimetric Measurements

One needs to appreciate that the technique of calorimetry is very highly developed, and has been used with increasing accuracy for >100 yr. While an individual may make a mistake or an apparatus may fail in some respect, these problems are not common within the field of expertise. Nevertheless, all measurements contain some error. Error analysis depends on the type and design of the calorimeter used. However, in all cases, the errors can be divided into two groups: (a) random errors that relate to the precision of the various individual physical measurements needed to arrive at the excess heat and (b) systematic errors that occur because some energy has been ignored in the calculation due to design defects or unknown chemical reactions. Both types of errors are normally revealed by studying cells containing H_2O as the electrolyte, using platinum as the cathode instead of palladium, or inactive cells containing both D_2O and palladium. If such cells show no excess heat above that which can be accounted for by considering all energy entering and leaving the cell, the calorimeter is generally considered to be accurate to the degree that input power equals measured power.

The most important systematic errors involve energy that leaves the cell. When gases escape, there is an uncertainty in the chemical energy carried away by the resulting hydrogen and oxygen because of possible partial recombination within the cell. However, all of the workers reporting excess heat are aware of this problem, and, when tested, recombination is found to be negligible at currents >30 mA/cm^2 (Ref. 299) and as long as the electrodes are kept below the fluid surface.[139,143,144] Dissolved hydrogen and oxygen can be recombined on the anode, but the rate is trivial.[300] Furthermore, many workers now place a recombiner within the cell so that no gas need leave, and all of the chemical energy remains in the cell. An additional factor is the transport of D_2O vapor out of the cell with the gases.[139,212] Heat associated with this loss is small when the cell temperature and current are low but become significant when both are large. This quantity needs to be included in the heat calculations when the highest accuracy is desired. Some LiOD + $D_2O(l)$ mixture is also carried away as an aerosol within the gas stream. This effect increases as the current is increased but is small under most conditions. Of course, none of these processes is important when a closed cell is used.

Energy deposited within a cell is in the form of an electric current and is calculated by multiplying the current passing through the cell by the voltage measured between the electrodes. This energy can be determined with very high precision provided both current and voltage are measured as a function of time. Once the cell is assembled and turned on, energy flow is monitored, and any subsequent energy entering the cell will be detected within limits determined by known errors. Although some workers have charged the palladium at low current without monitoring the energy during this time, most excess heat has been found after a complete accounting was made from the time the cell was first turned on.

Besides the electric current, chemical reactions and physical strain may add energy to the cell.

II.B. Chemical Sources of Excess Heat

Typical cells contain a very limited number of elements and amount of material. Therefore, the total possible excess mechanical or chemical energy contained in these materials or produced by interaction also must be equally limited. Only energy above the lowest possible energy states of the *initial* constituents of the cell is available to be added to energy that is generated within the cell. The only additional substance that is normally added is D_2O to replace that which is lost by electrolysis from an open, unsealed cell. Deuterium oxide is generally acknowledged as being in its lowest chemical state under these conditions. Furthermore, all of the construction materials are chemically inert to each other when allowed to remain in contact over a long time. Therefore, for a chemical reaction to occur, some form of energy must be supplied from the outside. In this case, this energy is supplied by the electric current.

Only three current-induced chemical reactions are known to have the potential to affect the energy balance to a significant extent. These are the formation of

D_2 and O_2 gases from the heavy water (see Sec. II.E.3), formation of PdD_x at the cathode with the release of O_2 gas, and formation of Pd-Li alloy at the cathode by reaction with lithium ions. Peroxide formation has been proposed but not detected.

The formation of β-PdD by a reaction between palladium metal and deuterium gas[34,301,302] and chemisorption on internal cracks[303] gives off heat. This source of heat is discussed in detail by Godshall et al.[141] An observed increase in cathode temperature during the initial reaction to form PdD and a decrease during removal of deuterium is consistent with expectations.[219,229] An initial cooling of the cell has also been reported[149,216,219] that is not caused by the Peltier effect.[148] However, as the palladium cathode becomes saturated with deuterium, this relatively small energy perturbation decreases, and the temperature of the cell returns to a baseline value.

Elemental palladium and lithium form an alloy with heat release. However, the reaction in a cell is between palladium and Li^+ ions in solution. This reaction requires energy that, again, is supplied by the flowing current. Consequently, a slight temperature reduction would result to the extent that such an alloy formed. This temperature reduction is not observed because the rate of formation is limited by the diffusion rate of lithium in palladium, a very small number.

In summary, any chemical reaction that requires additional Gibbs energy supplied by the flowing current to occur will appear as a loss of energy from the cell. Should such a normally unstable compound be formed but go unnoticed, a heat effect would occur when the cell current was subsequently decreased or stopped, thereby allowing the compound to decompose. Only one such heat effect has been reported,[213] but at a level much too small to suggest this effect could be the cause of significant excess heat. Thus, critiques that explain the excess heat as being due to the formation of hypothetical compounds are not consistent with observation.[128,304] An analysis of possible chemical reactions was developed soon after the first announcement of the cold fusion effect and should be consulted for more detail.[3,305]

The palladium can contain some strain energy that would be released during hydriding.[306] While the amount of this energy is difficult to quantify, it is expected to be small,[307] and it would be released and measured only during the initial hydriding process. Excess heat lasting weeks has been detected even when fully annealed, strain-free palladium was used. Should strain energy be introduced and subsequently released during hydriding, no excess heat would be seen because the energy would be part of the measured heat balance within the calorimeter. Consequently, the proposed behavior of strain energy, either initial or induced, is not consistent with observation.

Another approach can be taken to put the chemical-mechanical source of heat in perspective. The amount of excess energy reported can be compared to various chemical and physical processes. As much as 50 MJ/cm^3 of palladium[d] has been reported to be released during the life of a cell.[212] This amount of energy is sufficient to melt 641 cm^3 or 7690 g or 72 mol of palladium at 1825 K (Ref. 308), and its production would require 1700 g of hydrogen to be burned. If released during a short time, this energy would raise a typical cell (<1 kg of glass and heavy water) to >12 000 K. Of course, only a few cells have achieved this much excess energy production, and, fortunately, it occurred over many weeks. Nevertheless, these examples of common chemical and physical processes show how difficult it is to attribute this amount of excess energy to a nonnuclear process. The only other alternative is to argue that a large positive and variable bias exists in all calorimetry measurements giving positive results. This suggestion is not supported by any other behavior pattern in the measurements.

II.C. Examples of Experimental Results for Excess Heat Production

Table I compares the various reported heat measurements, both positive and negative, and lists the accuracy for the calorimeter when a value is reported. Each entry generally represents several examples of positive and/or negative results. The presence of excess heat was usually not claimed until the accuracy limit was exceeded. On average, the calorimeters that produced negative results have lower accuracy than those that gave evidence of excess heat. Nevertheless, they should have been sufficiently sensitive to see heat if it had been produced. The absence of heat is proposed to be caused by unfavorable conditions that existed in the palladium cathode. This aspect is discussed in Sec. II.E.2.

Although all of these results have potential importance, a few studies need to be examined in detail because they are unique. In general, the heat production rate reported in these studies is so large that dismissing the results as being caused by calorimeter errors is not reasonable. Furthermore, the heat was made under a variety of conditions so that if it is caused by a chemical effect, a variety of very energetic chemical reactions must be assumed. Such an assumption is very difficult to justify.

[d]This reported value assumes that energy production is proportional to the volume of the electrode. This assumption may not be correct. It is not clear from the paper just how much total energy was produced in this experiment. If the smallest reported electrode volume of 0.01 cm^3 is assumed, the total energy would be ~0.5 MJ. When this quantity is used, subsequent calculated values need to be reduced by a factor of 100. In spite of this uncertainty, chemical sources of energy are still very unlikely.

TABLE I

Summary of Calorimetry Studies

Reference	Cathode[a]	Treatment[b]	Type[c]	Open or Closed[d]	Blank[e]	Accuracy[f]
No Excess Heat Reported						
Albagli et al.[142]	1-mm wire	725°C, vacuum	Isothermal	Open	Yes	≈5%
Armstrong et al.[150]	Sheet	?	Flow	Open	Yes	<4%
Bosch et al.[151]		Melted	Heat loss	Open	No	5%
Chemla et al.[146]	1-mm wire	?	Heat loss	Open	Yes	3 to 5%
Fleming et al.[175]	0.5- to 3-mm, melt cast, hot pressed	Anodized, vacuum	Seebeck	Open, closed	Yes	2%
Godshall et al.[141]	Cast	900°C, vacuum	Vapor	Open	?	2%
Hayden et al.[136]	Investment bar	600°C, vacuum	Flow	Closed	Yes	0.3%
Iyengar et al.[235]	Tube	Sanded	Adiabatic	Open	Yes	?
Jow et al.[138]	1 and 0.5 mm	1140°C, vacuum	Seebeck	Open	Yes	2%
Keesing et al.[148]	1-mm wire	≈1400°C	Substitution	Open	No	1%
Kreysa et al.[130]	Sheet, 1-cm rod	?	Heat loss	Open	No	5%
Lewis et al.[140]	0.25- to 3.9-mm wire	Sanded, melted	Isothermal	Open	Yes	<6%
Longhurst et al.[145]	51-μm foil, 1-mm wire	?	Heat loss	Open	Yes	5%
McCracken et al.[147]	Sheet, rod, tube, titanium sponge	600°C, vacuum	Flow	Closed	Yes	1%
Miskelly et al.[126]	2.2- and 3.9-mm wire	?	Heat loss	Open	Yes	5%
Redey et al.[143]	6.3-mm wire	650°C air and 600°C vacuum	Heat loss	Open	No	0.3 W
Ritley et al.[157]	6-mm wire	Variety	Heat loss	Open, closed	Yes	0.4 W
Stilwell et al.[144]	1.4-mm wire	245°C	Heat loss	Open	Yes	5 to 10%
Wiesmann[139]	Rod, sheet	300°C, vacuum	Heat loss	Open	Yes	≈0.5 W
Williams et al.[149]	1- to 6-mm wire, bar, cast, ribbon	Variety	Heat loss, isothermal	Open, closed	Yes	5 to 10%
Excess Heat Reported						
Appleby et al.[214]	0.5- and 1-mm wire	None	Seebeck	Open	Yes	<1%
Arata and Zhang[229]	2-cm rod	?	Cathode temperature	Open	Yes	Not applicable
Droege and Droege[218]	2-mm wire	?	Isothermal	Closed	Yes	≈0.5%
Eagleton and Bush[309]	Investment bar	Aqua regia, 200°C, air	Heat loss	Closed	Yes	0.3 W
Gozzi et al.[219]	Powder	Sintered	Cathode temperature	Open	No	Not applicable
Guruswamy and Wadsworth[232]	Palladium, titanium, zirconium	600°C, argon	Heat loss	Open	?	?
Huang et al.[234]	Coin	Arc-melted	Flow	Open	No	0.05
Hutchinson et al.[215]	6.3 mm, 1.3 cm	900°C, vacuum and 200°C vacuum	Heat loss	Open	No	?
Kainthla et al.[220]	1- and 3-mm wire	Electro	Heat loss	Open, closed	No	3%
Karabut et al.[224]	Disk	?	Heat loss	Gas	Yes	?
Lewis and Sköld[230]	3-mm bar	900°C	Flow	Open	Yes	0.2 W
Liaw et al.[222]	Wire	Torch melted	Heat loss, fused salt	Open	Yes	?
McKubre et al.[213]	Rod	Melted, 800°C, vacuum	Flow	Closed	Yes	0.2 W
Miles et al.[231]	6.3-mm wire	?	Heat loss	Open	Yes	5%
Noninski and Noninski[225]	0.5-mm wire	?	Adiabatic	Open	No	0.1%
Oriani et al.[226]	1-mm wire	None	Seebeck	Open	Yes	0.3%

See footnotes at the end of the table.

(Continued)

TABLE I (Continued)

Reference	Cathode[a]	Treatment[b]	Type[c]	Open or Closed[d]	Blank[e]	Accuracy[f]
Excess Heat Reported (Continued)						
Pons and Fleischmann[1,212]	1, 2, 4, and 8 mm	None	Heat loss	Open	Yes	<1%
Santhanam et al.[227]	Titanium and palladium	?	Substitution	Open	No	?
Schreiber et al.[216]	Old crucible	Melted, hammered	Heat loss	Open, closed	Yes	<1%
Scott et al.[217]	5.6 and 2.8 mm	900°C, vacuum	Flow	Open, closed	Yes	1 to 3%
Szpak et al.[233]	Electrodeposited palladium	None	Heat loss	Open	Yes	?
Yang et al.[223]	6, 3.5, and 2.2 mm	600°C, argon	Heat loss	Open	?	?
Zahm et al.[155]	4-mm wire	?	Heat loss	Open	No	?

[a]All cathodes are palladium except when noted. A few examples of size are given even though cathodes of various length, mass, and area were frequently used.

[b]Lack of space limits the details that can be listed in this category. In many cases, the cathode was heated in vacuum at the indicated temperature. Other treatments might have proceeded this anneal. In some cases, other treatments might have been used instead of an anneal.

[c]A variety of calorimeter types have been pressed into service, each having its own sensitivity and absolute error. They are as follows:

1. Adiabatic: Energy is measured by noting the rate of temperature change. This generally occurs so rapidly that little energy is lost to the surroundings.

2. Heat loss: Energy production causes the temperature of the cell to increase until energy loss equals energy gain. The achieved temperature is used to determine the energy production rate. Several variations on this design have been used as well as various methods of calibration. A detailed analysis of this calorimeter type is given by Pons and Fleischmann.[212]

3. Isothermal: A source of known energy is applied to the cell, and this energy is adjusted so as to keep a constant temperature as other sources of energy change. A resistor is usually used to add this known energy. However, Droege and Droege[218] used a thermopile.

4. Vapor: The cell is kept in a Freon bath that is boiling at a known rate. Changes in the boil-off rate are used to determine the energy added by the cell.

5. Seebeck: Heat is allowed to exit the cell through a thermopile, and the generated voltage is used to determine heat flow.

6. Flow: Heat is carried away by flowing water. Energy change is determined by measuring the temperature difference between water entering and leaving the calorimeter. This quantity is used in combination with the flow rate and the heat capacity of water.

7. Substitution: A quantity of known energy is used to bring the cell to the same temperature that was achieved during electrolysis. The difference between the net energy supplied to the cell during electrolysis and the known energy is considered to be excess.

8. Cathode temperature: On a few occasions, excess heat was determined by measuring the temperature increase of the cathode. While being sensitive to heat production, this method cannot give a quantitative value.

[d]Calorimeters without internal recombination of the gases are considered to be open even though they may be isolated from the atmosphere. Closed cells recombined the D_2 and O_2, thereby allowing no gas to escape. One study, designated "gas," used ion discharge in D_2 gas.

[e]Some calorimeters were run using a platinum cathode or H_2O in the electrolyte. This gives a baseline for zero excess heat production and a measure of the accuracy. A "Yes" is noted if such blanks were studied and "No" if not. Very often, however, cells that were expected to produce excess heat did not. During this time, a blank baseline could be obtained in spite of not running a formal blank.

[f]Frequently, but not always, a value for the accuracy is noted in the paper. Generally, the sensitivity was less than this value. No excess heat was claimed by the authors unless the accuracy limit was exceeded by an arbitrary amount.

1. Williams et al.[149] (United Kingdom Atomic Energy Authority, Harwell, England): This is a very broad study using palladium and titanium from a variety of sources and several calorimeter designs. However, no evidence of excess heat, neutron, gamma-ray, ^3He, ^4He, or tritium production was reported. The calorimeters were sufficiently sensitive and the current range was sufficiently broad ($\leq$600 mA/cm^2) that the

excess heat should have been detected had fusion occurred. This negative result has produced considerable skepticism about the cold fusion effect.

2. McKubre et al.[213] (SRI International): A high-pressure cell (60 atm D_2) was electrolyzed under conditions that prevented oxygen formation. The palladium was vacuum melted, machined, etched with aqua regia, vacuum annealed at 800°C, and cooled in D_2. Excess power between 1.5 and 2 W with bursts was reported. In addition, autoradiography found radioactivity[e] in the cathode even though no evidence of tritium was found in the electrolyte or in D_2 removed from the electrode. No ^{3}He or ^{4}He was detected. However, detection sensitivity may not have been sufficient to see the amount of helium associated with the small total energy production. Surface analysis found no change in the normal isotopic ratio and no elements that might be associated with a fusion reaction. Loading near a deuterium-to-palladium ratio (D/Pd) of 1, based on resistivity change, was claimed, but not all electrodes so loaded produced heat. Deuterium loss produced heat in one case. A deuterium gradient is suggested to be important.

3. Appleby et al.[214] (Texas A&M University): A very sensitive microcalorimeter was used to study small samples. Normal LiOD as well as material enriched in either ^{6}Li (98.67%) or ^{7}Li (99.8%) was used in the electrolyte without producing a significant difference in excess heat production rate. However, when the LiOD was replaced by NaOD, energy production decreased to zero over ~10 h. Subsequent replacement of the NaOD by LiOD caused excess heat to return. When the current was interrupted, excess heat production did not stop immediately but decreased rapidly at first, then more slowly over several hours.

4. Santhanam et al.[227] (Tata Institute, India): Excess heat is claimed using 1 *M* NaCl as the electrolyte and palladium or titanium as cathodes. Palladium gave the larger production rate. This rather incompletely described work is the only claim for excess heat using sodium in the electrolyte.

5. Bush and Eagleton[221] (California State Polytechnic University): An investment bar of palladium (Englehard Industries) was used. By carefully studying excess energy production as a function of charging current in several cells, a fine structure was found in the relationship. This structure is explained by the authors using a resonance theory. Kainthla et al.[220] (Texas A&M University) measured heat production over the same current range with similar detail and reported no such structure.

6. Liaw et al.[222] (University of Hawaii): This is the first study using a fused salt of KCl + LiCl in which LiD is dissolved. Excess heat was obtained between 400 and 460°C using palladium or titanium as the active electrode (anode). Only palladium that had been flame melted, which gave a porous surface, produced heat. Palladium gave a power production rate equal to 1512% excess at 692 mA/cm^2 and 460°C, a net gain of 25.4 W from 0.498 g of palladium. Excess ^{4}He was detected in the electrode but not enough to account for the heat. Titanium also produced excess heat but at a much lower rate. While lithium is present in the solution, it reacts with the aluminum cathode rather than with the anode. Apparently lithium does not need to be present with the deuterium for excess energy to be produced.

7. Karabut et al.[224] ("Lutch," USSR): Heat and neutrons were produced by using electric discharge in D_2 gas with a palladium cathode. Although this work is not reported in detail, 120 to 150% excess heat over the electrical input is claimed. The effect did not occur when the palladium was oxidized and when its temperature exceeded 500 K. The maximum neutron flux was $\sim 10^7$ n/s, which, it is claimed, corresponds roughly to the heat production rate if a branching ratio ($n/^3$H) of 10^{-8} to 10^{-9} is assumed.

8. Bush et al.[228] (University of Texas and the Naval Weapons Center): Helium-4 was found in the effluent gas (100× detection limit) while excess heat (0.14 to 0.52 W) was being produced in Pd/LiOD-D_2O cells. The amount of helium was consistent with the amount of heat. No ^{4}He was found when heat was not produced. This work confirms that ^{4}He is the nuclear product of heat production, and it shows that the reaction occurs on the surface. In addition, dental X-ray film located on the outer surface of the cells showed the presence of radiation. Normal water containing cells showed neither ^{4}He nor radiation, although some unexplained excess heat was reported. No ^{3}He was detected.

9. Droege[218] (Environmental Optics Corporation): When D_2O was replaced by H_2O, the heat continued for ~35 h, then abruptly dropped to zero. Scott et al.[217] [Oak Ridge National Laboratory (ORNL)] found that after the electrolyte was replaced by one containing normal water, excess heat, neutron, and gamma-ray production took >100 h to disappear. Huang et al.[234,311] (University of Hawaii) also found that 20 to 40 h were needed to eliminate excess heat after adding normal water. Belzner et al.[216] (Stanford University) reported loss of heat when the electrolyte was exposed to the atmosphere. In this case, the effect

[e]Blanks showed no such effect. Miles and Miles[310] have calculated some possible radioactive species that might be present.

could have been caused by CO_2 as well as by water pickup.

10. Arata and Zhang[229] (Kinki University and Osaka University, Japan): The temperature of a large palladium cathode (2 × 5 cm) was measured while deuterium was caused to repeatedly enter and leave the metal by switching the cell current off and on. Production of heat during charging and cooling during discharging were clearly observed. In addition, large bursts of heat were seen occasionally as the palladium temperature rose and fell (70 to 110°C) while the current was constant. The magnitude of this energy is far higher than can be explained by the deuterium absorption reaction, in contrast to the conclusion reached by the authors.[f]

11. Lewis and Sköld[230] (Uppsala University, Sweden): A bar (3 × 3 × 55 mm) of palladium was electrolyzed, and it produced excess heat while neutron and tritium measurements were being made. In one cell, excess heat showed bursts to 155%, and there were occasional neutron bursts associated with turning the cell off and on as well as just before excess power production ceased. Onset of power production seem to be associated with D_2O additions. No excess tritium was found in the electrolyte. Another cell gave bursts of excess heat to 39%, but neutron bursts were seen only after heat production had largely ceased.

II.D. Relationship Between Heat Production and Cell Current

When the steady component of heat production as watts per square centimetre is compared to the total cell current as milliamperes per square centimetre, a broad relationship is found,[g] as shown in Fig. 2. As first suggested by Pons and Fleischmann,[212] there is a marked increase in excess power as the current density is increased at low current density. When measurements based on a variety of cathode volumes and areas are compared, there is a suggestion that an upper limit to heat production exists at each current and that this limit approaches a constant value as current is increased. When plotted on a linear scale in contrast to the log scale shown in Fig. 2, the upper limit for an aqueous cell is described by the equation,

$$W\ (\mathrm{W/cm^2}) = 3.6I\ (\mathrm{A/cm^2})$$

to at least 0.7 A/cm^2. The solid line in the figure shows values obtained from this equation. There is an indication in the data that the process may saturate at higher currents and the large scatter indicates that current density is not the only important variable.[h] Typically, large bursts lasting several minutes superimpose on the steady heat production. Fleischmann and Pons state that tritium is produced during these episodes, but the details are still lacking and confirmation by other studies is mixed. Some bursts appear to produce tritium and some do not. At much higher temperatures, in a fused salt cell, Liaw et al.[222] have produced a much larger steady power with a similar sensitivity to current density and a similar tendency for burst formation.

It is hard to support an argument that all of these measurements, based on many different calorimeter designs, suffer from errors that can conspire to produce this relationship. Nevertheless, there are a number of factors that need to be considered when interpreting this behavior. The following partial list also gives an example of the difficulty in sorting out variables so that reproducibility can be achieved. As the current is increased,

1. Bubble coverage will increase, thereby shielding a greater average fraction of the surface from the current and producing greater local fluctuation in deuterium content as bubbles are made and released.

2. The voltage across the surface layer will increase.

3. The temperature of the cathode will increase.

4. The concentration of deuterium in the surface region of the cathode will increase.

5. The deposition rate of impurities will increase.[312]

The order of importance that should be assigned to these variables is not clear at the present time. However, a comparison between the aqueous cells at 25 to 50°C and the fused salt cell at 400 to 460°C strongly suggests that temperature is a major variable.

[f]The authors[229] conclude that the measured temperature increase of the cathode was due to a reaction with deuterium. This cannot be correct for the following reasons: The palladium was found to increase from 70 to 112°C over 1.5 min. If no energy were lost to the electrolyte, this temperature change would require 460 cal, based on the sample weight and the heat capacity of palladium. At the maximum cell current of 500 mA, 4.7×10^{-4} mol of deuterium would have been available to react with the palladium during the 1.5-min temperature increase. This would have generated ~4 cal if all had reacted. Thus, the energy generated is at least 100 times greater than the energy available from the hydriding reaction.

[g]Unfortunately, not all the positive results could be compared because certain details are lacking in the papers.

[h]The large scatter at low current densities may be partly due to temperature errors caused by poor mixing because of the small amount of evolving gas.[142] This is not a problem at high current, where bubble generation produces good mixing.

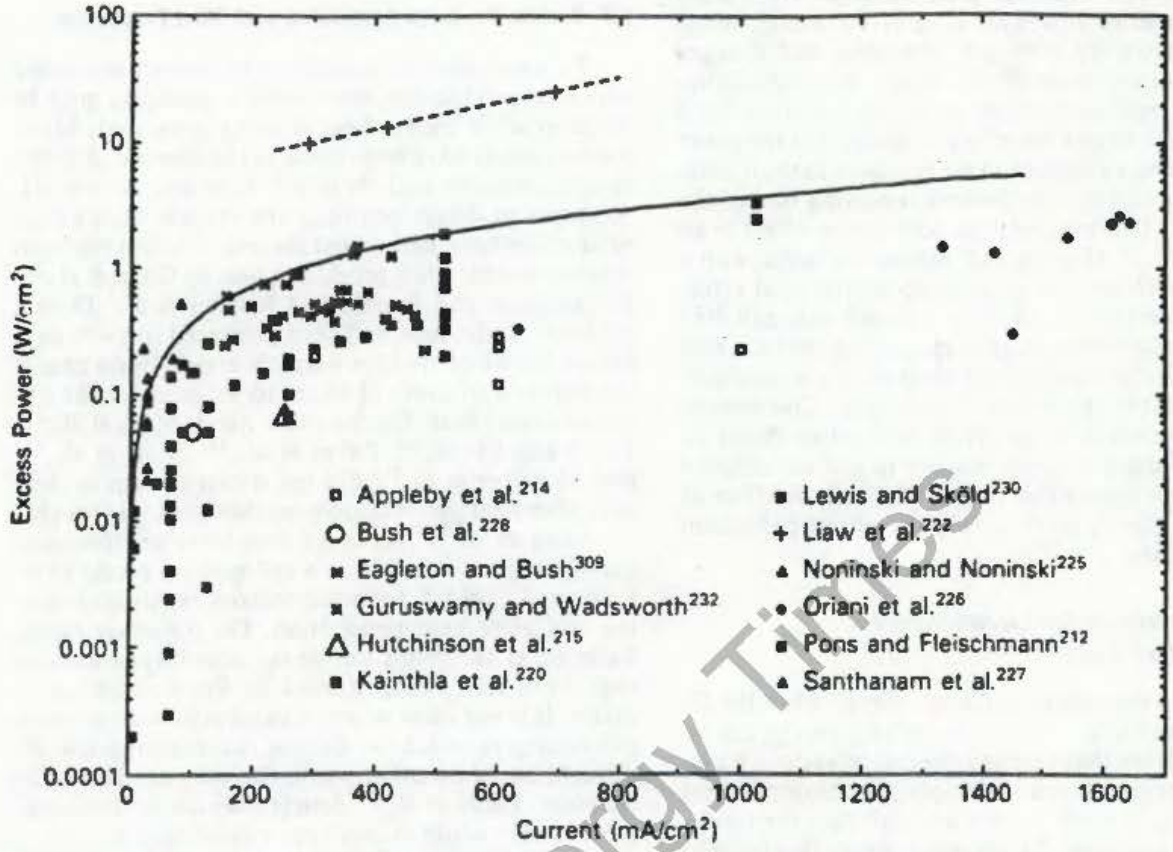

Fig. 2. Effect of total current density on excess power per square centimetre. All values are from D_2O-containing cells except those for Liaw et al.[222]

II.E. Factors Affecting Heat Production

Although there are clearly many important variables that have not yet been revealed, a few can be suggested based on both negative and positive observations.

II.E.1. Electrolyte

Electrolytes containing LiOD, LiOH, LiOD + Li_2SO_4, Li_2SO_4, LiOD + NaCN, or NaCl all have given excess heat on occasion. However, when LiOD was used at very high concentration in glass containers, excess heat was not observed, perhaps because glass components deposited on the cathode surface.[143] Nevertheless, glass or stainless steel cells have been used successfully at the usual 0.1 *M* concentration.

The effect of enriched lithium has been studied to determine whether lithium-deuterium fusion was occurring. Stilwell et al.[144] found no excess heat when normal lithium was used. On the other hand, Appleby et al.[214] produced excess heat, and the amount was insensitive to whether ^6Li, ^7Li, or normal lithium was used. When the lithium-containing electrolyte was replaced by one containing sodium, the heat effect stopped.[214] Nevertheless, heat has been produced in a fused salt cell, in which lithium does not enter the palladium, and heat has been reported[227] using an electrolyte containing NaCl. In addition, tritium has been produced on many occasions using a sodium-containing electrolyte.[235] Apparently, lithium is not essential to the production of tritium and perhaps not to the production of heat as well.

Any impurity in the electrolyte such as normal water, impurities released by chemical reactions with cell components or added with the lithium, CO_2 picked up from the air,[218] or impurities[i] present in the D_2O may affect the reaction or introduce errors in the heat measurement. The presence of copper or solder in contact with the electrolyte results in impurity deposition on the cathode.[238] Impurities in the platinum anode are slowly leached out and deposited along with the platinum. All of these factors affect reproducibility.

[i]Solids at 16 ppm were detected in a bottle of D_2O supplied by Aldrich Chemical Company and 9 ppm in a bottle supplied by MSD Isotopes.[313]

II.E.2. Palladium Cathode

Purification by vacuum annealing is a common practice. Excess heat has been obtained from both vacuum-annealed as well as "as-received" metal. While removing dissolved hydrogen, annealing also changes the physical structure of the metal. Reproducibility cannot be expected until the structure is standardized to that which allows the effect to occur. On the other hand, heating palladium in air removes carbon from the near-surface region, thereby removing one of the components that may help the cold fusion effect in an aqueous cell.[236] Heating and melting the metal with a propane torch seemed to have no detrimental effect when the metal was used in a fused salt cell.[222,j] Cleaning palladium with acids containing chlorine may not be advisable because this element is a strong poison that inhibits reaction with hydrogen. Commercial palladium contains a variety of impurities. Some of these (rhodium and silver) migrate to and concentrate at the surface during electrolysis.[314] Until the effect of these impurities is known, only the purest palladium should be used.

II.E.3. Correction for Electrochemical Energy Loss

An open electrolytic cell loses energy when the D_2 and O_2 gases leave. The amount of this energy can be subtracted from that entering the cell in various ways. The most common is to multiply the thermoneutral potential[k] by the cell current and subtract the resulting energy term from the electrical energy flowing into the cell. Minor variations in the thermoneutral potential have been used in the various studies (1.526 to 1.540 V). The recommended value is 1.527 V, −70.46 kcal/mol, or −294.6 kJ/mol (Ref. 315). To the extent that a value higher than the recommendation is used, the reported heat will overestimate the actual excess energy, especially at higher cell currents. This quantity is accurate as long as only D_2O is being electrolyzed. In practice, some H_2O is present in the heavy water, and this is electrolyzed preferentially. Therefore, the actual neutral potential will be slightly smaller than the mentioned value, depending on the amount of normal water present. This error also will cause an overreporting of the excess heat. The normal water concentration in heavy water cannot be based solely on the amount claimed by the manufacturer because new bottles can differ from this claim, and normal water can be picked up during the experiment. Furthermore, hydrogen from normal water will enter the palladium preferentially[143,297,316] and reduce or stop excess heat production if the quantity is excessive. Closed cells do not suffer from this problem.

[j]It is not certain whether this treatment would add or remove carbon.

[k]The thermoneutral potential is the enthalpy for the formation of D_2O expressed in volts (1 faraday = 96 489 C).

II.F. Nuclear Products Associated with Heat Production

To learn whether neutrons or tritium are associated with heat production, these nuclear products must be detected while excess heat is being generated. Many measurements have been made in the absence of excess heat production and these are discussed in Sec. III. Attempts to detect neutrons and tritium during heat production have had mixed success. Tritium has been detected in cells while producing heat by Gozzi et al.,[219] Fleischmann and Pons,[1] and Bockris et al.[3] During the latter study, heat had been produced for ~10 days before bursts of tritium were observed. In no case is the detected tritium sufficient to account for the observed excess heat. On the other hand, Miles et al.,[231] Lewis and Sköld,[230] Zahm et al.,[155] Scott et al.,[217] and McKubre et al.[213] did not detect tritium in their cells after heat production even though it was sought.

Scott et al.[217] reported low-level neutron and gamma-ray emission from a cell making excess heat. Lewis and Sköld[230] reported neutron bursts both during and after heat production. On the other hand, Salamon et al.[164] did not detect neutrons or gamma rays from cells being studied by Pons and Fleischmann. It is not clear whether these cells were actually producing excess heat during the examination although, based on other work, this may not be a critical point. Zahm et al.[155] detected neither neutrons nor gamma rays while excess heat was being observed.

The only detected nuclear product that is clearly associated with heat production is ^4He. These studies gave the following general results:

1. Morrey et al.[169] (group effort): Various pieces of palladium supplied by Fleischmann and Pons, both used and unused, were analyzed for ^4He and ^3He. The work established that once helium enters the lattice, even by alpha-particle bombardment, it is retained. Surface helium can be removed by etching. Unfortunately, there was sufficient ^4He in the unused palladium that production in the used metal could not be established. No ^3He was detected.

2. Liaw et al.[222] (University of Hawaii): Electrodes used in a fused salt cell were analyzed for ^4He. One of four samples showed ^4He at 14σ above background. The other three samples were only slightly above background. The amount of ^4He detected was not sufficient to account for the observed excess heat.

3. Bush et al.[228] (University of Texas and the Naval Weapons Center): Sufficient ^4He was detected in the evolving gas to account for the observed excess heat within a factor of 10. Blank cells showed no ^4He. No ^3He was detected. Helium retained by the electrode was not determined.

III. TRITIUM, NEUTRON, AND RADIATION PRODUCTION

Attempts have been made to detect neutrons, tritium, gamma rays, protons, ^{3}He, and ^{4}He as likely nuclear products. Of these, neutron detection has been given the greatest effort although it is the more difficult measurement because of the generally low emission rate. In addition, shifts in the ^{6}Li/^{7}Li ratio and the palladium isotopic ratio have been sought.[314] Techniques such as particle track[153,168,287,317] and autoradiography[213,235] have been used to detect high-energy particles, but these techniques cannot be used for identification.

If tritium and neutrons are actually produced in a metal lattice, the existence of at least two nuclear reactions cannot be doubted. The only issues are (a) whether tritium is produced in the cell rather than entering from an outside source as contamination and (b) whether the neutrons actually are emitted from the fusion cell rather than from cosmic-ray showers, electrical noise, or other external sources. Each of these issues is addressed later. However, the detection by different laboratories of both neutrons and tritium at the same time to give an internally consistent ratio largely eliminates the possibility of chance or contamination as an explanation.

III.A. Neutron/Tritium Ratio

Most workers do not have facilities available for the routine detection of both tritium and neutrons. Consequently, only a few measurements of the relative production rates have been reported. Values for the $n/^3$H ratio and the cell conditions are listed in Table II. These data are plotted as the log (number of tritium atoms) versus log (number of neutrons) in Fig. 3. Clearly, the ratio is significantly less than unity, with a lower limit near 10^{-9}, and it has a range of values. Indeed, low-level neutron emission has been observed without the presence of tritium being detected even

TABLE II

Reported Tritium Production and $n/^3$H Ratio

Reference	$n/^3$H	Tritium (μCi)[a]	Cell Design
Wolf et al.[252,b]	10^{-7}	≈7	Palladium in LiOD
Claytor et al.[240]	2.7×10^{-9}	0.17	Palladium + silicon (SiO_2) in D_2
	$0.8(\pm 1) \times 10^{-9}$	0.32	Palladium + silicon (SiO_2) in D_2
	$2.4(\pm 4) \times 10^{-9}$	0.044	Palladium + silicon (SiO_2) in D_2
Iyengar et al.[235]			
1[c]	10^{-7}	7	Titanium (rod) in NaOD
2	10^{-8}	380	Pd-Ag (tube) in NaOD
3	10^{-9}	190	Pd-Ag (disk) in NaOD
4	8×10^{-9}	1.9	Palladium (tube) in LiOD
5	1.7×10^{-6}	0.03	Palladium (cube) in LiOD
6	1×10^{-6}	0.21	Palladium (pellet) in LiOD
7	9×10^{-4}	0.009[d]	Palladium (ring) in LiOD
8	3.2×10^{-4}	0.0009	Palladium (coil) in Li_2SO_4
9	7×10^{-7}	0.17	Palladium (button) in LiOD
Sona et al.[248]	2×10^{-6} to 10^{-7}	?	Palladium (sheet) in LiOD
Gozzi et al.[219,247]	3.4×10^{-6}	0.01	Palladium (rod) in LiOD
Packham et al.[237]		0.08 to 340	Palladium (wire) in LiOD + NaCN
Storms and Talcott[238]		0.01 to 0.06	Palladium (wire) in LiOD
Chêne and Brass[250]		0.0005	Palladium (wire) in LiOD
Sánchez et al.[256]		≈0.03	Titanium (sheet) in Li_2SO_4
Iyengar et al.[235] (p. 57)		56.3	Palladium (tube) in LiOD
Iyengar et al.[235] (p. 84)		0.007 to 0.03	Titanium in D_2 at 900°C
		0.07	Pd-Ag in D_2 at 600°C
Kaushik et al.[246]		≈30	Titanium in D_2 cycled
Chien and Huang[241]		≈40	Palladium in LiOD

[a] 1 μCi = 2.2×10^6 disintegration/min = 3.4×10^{-11} mol = 2.1×10^{13} atoms = 3.7×10^4 Bq.
[b] First report of $n/^3$H ratio but tritium value has since been repudiated.[318]
[c] Numbers for points shown in Fig. 3.
[d] This value, obtained from a table (p. 34), is inconsistent with 16.25 μCi given in the text[235] (p. 52).

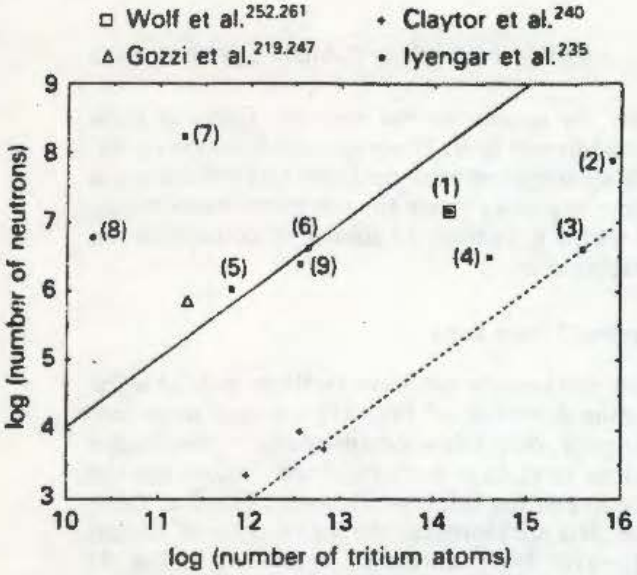

Fig. 3. Relationship between the number of tritium atoms and the number of neutrons produced in electrolytic cells. The "+" symbol describes results from a dry electrolytic cell. A dashed line shows a constant $n/^3H$ ratio of 10^{-9}, and the solid line shows a constant ratio of 10^{-6}. These lines do not imply a fit to the data points. Numbered points correspond to the numbering in Table II.

though it was sought.[217,230,259,274,278] This experience suggests two neutron-producing reactions. One reaction produces mainly neutrons and the other is associated with tritium production with a branching ratio near 10^{-9}. Conditions might conspire to produce a mixture of these reactions in most cells. Regardless of the interpretation, the $n/^3H$ ratio is clearly much less than unity, falling between 10^{-4} and 10^{-9} for a variety of cell conditions.[319] Although the $n/^3H$ ratio falls in a rather wide range, it seems likely to me at least that a much wider range would result if these two reactions were not coupled in some way.

III.B Tritium Production

Tritium has been detected after using at least six different methods of production. These include

1. wet D_2O electrolytic cells containing palladium
2. dry SiO_2 electrolytic cells containing palladium
3. thermal cycling of titanium in D_2
4. high-voltage discharge of D_2 with a titanium cathode
5. ion bombardment using D^- on palladium or titanium
6. codeposition of palladium and deuterium onto nickel.

At least six different methods for tritium detection have been used. Each has been successful in demonstrating the presence of tritium on some occasion. These include

1. liquid scintillator
2. proportional counting of beta emission
3. autoradiography
4. beta-excited K_α X ray from titanium
5. mass spectrometer[1]
6. silicon surface-barrier particle detection of ejected triton.

III.B.1. Comparison of Reported Results

Table II lists the various reports of tritium production. The amount of tritium listed is a crude estimate in some cases because insufficient detail was provided.

The rate of tritium production cannot be determined because tritium is produced in a series of bursts during the active life of a cell and the result is accumulated in the electrolyte. Most measurements are made at time intervals that are long compared to the burst interval.

Tritium production appears to occur during neutron bursts. Several of the more extensive studies of tritium production are worth examining in more detail:

1. Iyengar et al.[235] [Bhabha Atomic Research Centre (BARC), India] report a very extensive study in which tritium and neutrons were produced by electrolytic cells of various designs containing various electrolytes using titanium, palladium or Pd-Ag alloy cathodes. Tritium was also produced by gas loading titanium and palladium under various conditions. Using autoradiographs as well as other techniques, this tritium was found to occupy isolated locations on the titanium surface but is more uniformly distributed in palladium. When made by gas loading, the resulting tritium was contained mostly in the metal rather than in the deuterium gas. Tritium was detected in old Ti-D samples (10 to 20 yr), using various techniques, at levels between 9 and 4000 μCi. However, the history of these samples is uncertain.

2. Storms and Talcott[238] [Los Alamos National Laboratory (LANL)] studied a variety of surface poisons and palladium alloys in an effort to find the best conditions for tritium production. Although only a small amount of tritium was made, it was done in cells that allowed for complete inventory of tritium entering and leaving the cell. All produced tritium was found only in the electrolyte.

[1]Kay et al.[320] have pointed out the pitfalls of this technique.

3. Claytor et al.[240] (LANL) produced tritium in D_2 gas (0.001 to 0.32 μCi) by applying a pulsed voltage (1200 to 2500 V, $\geq$150 μs), through alternating layers of palladium and silicon disks surrounded by the gas under pressure. In 8 cells out of 30, excess tritium $>3\sigma$ was observed. Recent cells have shown a reproducible production rate of 0.5 nCi/h with no production in control cells. Various techniques were used to verify that the tritium was being produced within the cell.

4. Packham et al.[237] (Texas A&M University) studied a variety of cells in an attempt to produce tritium. Levels between 0.08 and 340 μCi in the electrolyte were seen in 14 out of 28 open cells. One cell that was producing heat showed two bursts of tritium in the gas. Because this tritium was recombined and returned to the cell, the amount of tritium going only into the electrolyte is not known.

5. Kaushik et al.[246] (BARC, India) cycled the temperature of titanium chips in D_2 gas and found that only 4 out of >1000 chips examined showed the presence of tritium at the $\approx$30-μCi level.

6. Adzic et al.[243] (Case Western Reserve University) studied open and closed cells from which tritium production up to 49 times the starting concentration was observed. Very low charging currents were proposed to improve the probability of success.

7. Szpak et al.[233] (Naval Ocean Systems Center) electrodeposited palladium deuteride onto copper in an open cell. Heat, tritium (×10), and X rays were detected. These effects were not seen in control studies. An image was recorded on X-ray film within the cell that showed the presence of low-energy radiation.

8. Taniguchi et al.[286] (Osaka Radiation Research Institute, Japan) did not look for tritium but did detect what appeared to be the high-energy proton that accompanies tritium production. A very thin palladium cathode (10 to 18 μm) was electrolyzed using LiOD or LiCl in the electrolyte. A silicon barrier detector on the outside of the cell measured counts up to 100 times background for which the energy was determined. The energy spectrum was consistent with, although not identified as, 3-MeV protons with a lower energy component, as if they had been produced on the surface next to the electrolyte. If, as argued later, the reaction occurred on promontories, some of the protons would have had to pass through the electrolyte to reach the detector, thereby further reducing their energy.

III.B.2. Possible Errors in Claiming Tritium Production

Tritium can be present as contamination in cell materials or in the environment; various chemical reactions in the scintillator fluid can mimic the presence of tritium; and tritium can accumulate in the electrolyte as a consequence of electrolysis. Each of these possible errors is discussed starting with the problem of contamination.

Tritium can enter a cell with the construction materials and through the cell walls from room air. Both methods of contamination have been studied. Because tritium has a very low concentration in the normal environment, significant contamination from room air or from commercial materials has a very low probability. Indeed, workers at Texas A&M University[237] made a rather thorough analysis of common construction materials associated with their cells, including a number of palladium samples. They found no evidence for tritium contamination. A study of normal water-containing cells provides a method to detect tritium contamination from all sources. Excess tritium has not been reported when normal water cells are studied.[m]

A study of a cell operating in a high-tritium environment has been made.[322] As expected, tritium pickup is nearly linear with time starting at time zero. This is in contrast to the behavior of an active cold fusion cell that typically shows an initial delay followed by burst formation of tritium.[237,238,241,256] Figure 4

[m]However, neutron production has been reported from a light water cell.[321]

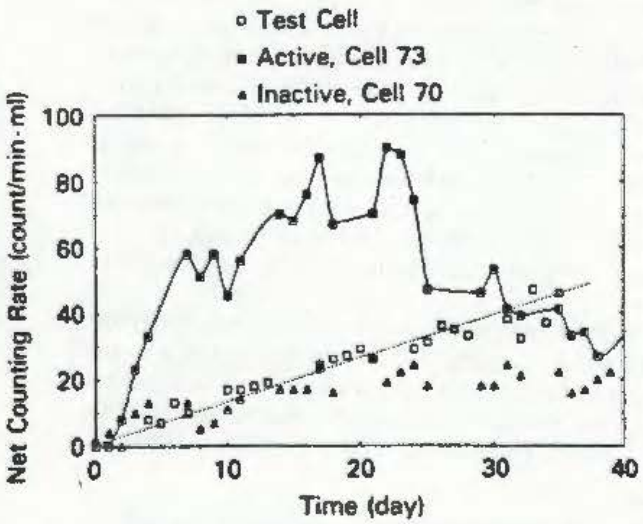

Fig. 4. Comparison between the net counting rate in an active cell, an inactive cell, and a cell stored in a high-tritium environment as a function of time from the onset of the respective studies. The net counting rate was obtained by subtracting the counting rate of the solution before the studies were started. Cells 70 and 73 were in series, and the increase in tritium content of cell 70 was due to enrichment that should affect cell 73 as well. The environment in which the test cell was placed contained $\approx$50 times more tritium than the environment of cells 70 and 73.

compares the concentration of tritium as a function of time in an active and an inactive cold fusion cell to an identical cell stored in a high-tritium environment. The patterns of tritium increase in the cells are completely different. While other cell designs are expected to have different pickup rates should environmental tritium be present, a smooth, nearly linear increase is expected for all designs as tritium in the room diffuses through the cell walls or enters when the cells are opened for sampling.

The tritium might be present in the palladium electrode, although the method of manufacture largely rules out this possibility.[323] Of the hydrogen isotopes, tritium has the least tendency to remain in palladium[324] when it is heated to temperatures used for processing, forming, or annealing. Nevertheless, Wolf[318] reports finding tritium in two pieces of unused palladium wire as well as in an annealed electrode after it was used in a normal water cold fusion cell.[n] This contrasts with a very complete study[325] using 45 samples of palladium from a variety of sources, including the source used by Wolf. This work did not find any evidence (±3 dpm/ml, 99% confidence = ±12% of background) for tritium contamination in the examined palladium but did find that certain conditions during the analytical procedure would cause an *apparent* presence of tritium. Even if tritium were present in the palladium cathode, it would quickly leave the metal[o] and appear in the evolving D_2 gas (>95%) during electrolysis.[297] This behavior is in sharp contrast to its delayed production[p] and subsequent retention in the electrolyte during most successful cold fusion experiments.

These considerations indicate that the tritium reported to have been made by cold fusion is very unlikely to have been caused by contamination of the cell or the palladium. Of course, it is impossible to "prove" that the observed excess tritium is not caused by contamination. However, when a number of laboratories make sufficient tritium by a variety of techniques using a variety of materials with a similar pattern of production, the argument for contamination as the source of tritium becomes increasingly difficult to justify.

Tritium can also accumulate within the electrolyte during electrolysis because the evolving D_2 and O_2 gases contain approximately one-half as much tritium as does the electrolyte.[q] Consequently, the tritium content of the electrolyte in an open cell will nearly double after extended electrolysis. Equations describing this tritium increase have been published (Refs. 3, 145, 149, 235, 238, 248, 326, and 327). Of course, closed cells containing a gas recombiner will not show this effect.

Chemiluminescence is caused by the presence of certain impurities in the scintillator fluid used to detect tritium. Some scintillator fluids are more sensitive to this effect than others. Because the effect generally decreases with time after the sample is mixed with the fluid, the effect can be separated from the true presence of tritium. Nevertheless, this is an area where care needs to be taken. However, this effect cannot be used to dismiss all claims for tritium production because most workers take the effect into account, and tritium has been detected using other techniques not subject to this problem. Further discussion about tritium measurement can be found in Ref. 328.

III.C. Neutron Production

Of the various aspects of the cold fusion effect, the production of neutrons has been studied the most thoroughly (see Table III). Neutrons have been produced after using at least eight different techniques. These include

1. thermal cycling of titanium, palladium, and $YBa_2Cu_3O_7$ in D_2
2. pulsed electrolysis
3. wet (D_2O) electrolytic cells containing palladium or titanium
4. dry (SiO_2 or Al_2O_3) electrolytic cells containing palladium
5. ion bombardment over a range of energies (50 eV to 60 keV)
6. sudden release of deuterium from palladium
7. chemical reactions in D_2O (Ref. 329)
8. physical breakup of deuterided $YBa_2Cu_3O_7$ (Ref. 267), LiD, and titanium with deuterium-containing substances (see Tsarev[5]).

Neutrons have been detected using at least seven techniques:

1. ^3He detector
2. BF_3 detector
3. detection of (n,γ) reactions in water
4. ^6Li glass combined with a scintillator
5. photon recoil scintillation spectrometer
6. fission track detector using film containing uranium
7. neutron activation of silver.

Each of these production and detection techniques has been successful in demonstrating the presence of neutrons on some occasion. When the energy of the

[n] No tritium was detected in the electrolyte of this cell.
[o] The loss is a first-order reaction with a half-life between 12 and 24 h, depending on the charging current and the condition of the palladium.[297]
[p] Wolf et al.[252] reported a delay of several weeks before tritium was observed in certain cells.
[q] This ratio applies to palladium cathodes in alkaline solutions.[236,326,327]

emitted neutrons is measured, values near 2.45 MeV (Refs. 2, 252, 257, 258, and 267) as well as peaks near 4 and 6 MeV (Ref. 258) are found. Neither neutron emission nor tritium was detected when Pd-D and Ti-D were bombarded with muons.[195,198,202] Most positive results are not consistent with radon decay.[135]

Errors in neutron measurement involve the detection of extra neutrons caused by cosmic-ray showers and electrical noise in the detector circuits. Most people who have studied the cold fusion effect are aware of both problems. However, when the counting rate is low, it is hard to be sure that neither of these problems is occurring[170] unless extra precautions are taken, including

1. keep the mass low around the detector to avoid cosmic-ray spallation events
2. compare the results to an identical but inert cell,
3. measure the background while the cold fusion cell is being studied
4. use a cosmic-ray-sensitive veto counter
5. make the measurements underground
6. use multiple counters
7. use anticoincidence noise rejection
8. use a sensitive detector (>10%) and determine the sensitivity using a standard source.

A number of studies have now given positive results during which all of these precautions were employed. High count rates and especially a count rate that is correlated with another measurement are easier to believe than a single, low-level experience. Such correlations have been observed.

Two regimes of neutron production are observed: (a) average rates <1 n/s over a relatively long time with burst behavior and (b) rates near 10^6 n/s for a short time. The large difference in these rates suggests either two separate nuclear reactions or an occasional high concentration of the special condition. A very high D/Pd ratio, by itself, does not seem to be sufficient to cause neutron production. Low-level neutron emission from thermal cycled titanium in pressurized D_2 has been duplicated by at least five independent, carefully done studies. Numerous studies showing how neutron emission is coupled to various parameters are discussed in detail as follows:

1. Jones et al.[2] (Brigham Young University) produced low-level neutron emission at 2.5 MeV by electrolyzing palladium in an electrolyte containing a complex mixture of metal salts. Bertin et al.[257] (University of Bologna, Italy) used titanium as the cathode instead of palladium and obtained essentially the same result.

2. De Ninno et al.[244] (Frascati Research Centre, Italy) cooled titanium chips in high-pressure D_2 to liquid nitrogen temperature. Neutron bursts were observed when the assembly was warmed to room temperature. This is the first report of this phenomenon. Menlove et al.[259] (LANL) repeated the procedure and observed that bursts of neutrons had a high probability of emission (3 to 12σ) near −30°C. No effect was found when normal hydrogen or palladium was used in place of titanium. This effect was also seen by Izumida et al.[274] (Hitachi, Japan), who found a >3σ increase in count rate between −53 and 0°C. Iyengar et al.[235] (BARC, India) also observed neutron emission (up to 200 n/s) when titanium was held in D_2 (10 atm) at 77 K and warmed during evacuation. Jorne[112] (University of Rochester) placed palladium in D_2 gas at 90 atm and −80°C. When warmed slowly, neutron and gamma-ray bursts occurred as the temperature changed between ≈500 and 600 K. No effect has been found in any of these studies when normal hydrogen was used. Jianyu et al.[282] (China Institute of Atomic Energy, People's Republic of China) thermal cycled titanium and palladium in pressurized deuterium gas using an underground laboratory. Seven out of 10 assemblies gave bursts of neutrons (3 to 75 times background) between −100°C and room temperature using titanium, but not when palladium was used.

3. Sona et al.[248] (CISE SpA, Italy) charged a palladium sheet from one side using a palladium anode in an electrolytic cell. The neutron signal started increasing when the current was started, and this steady increase lasted for 400 min and reached 4 times background. Over the next 400 min, the signal dropped and reached background where it remained for 41 h after the current had been stopped.

4. Granada et al.[263] (Centro Atomico Bariloche, Argentina) subjected an electrolytic cell with a palladium cathode to alternating current on-current off conditions (≈2-min cycle). Neutron production was highest at about the center of the current-on time and near the center of the current-off part of the cycle. Repeated cycles reduced the effect. Neutron production was ~0.3 n/s. Takahashi et al.[258] (Osaka University, Japan) also produced neutrons from palladium (0.017 n/s, 1.4 times background) using the pulsed-current technique (19-min cycle). They found the best LiOD concentration to be 0.6 *M*. Perfetti et al.[270] (Frascati Research Centre, Italy) found neutron emission from both palladium and titanium cathodes when a 1-h-on/1-h-off cycle was used. Mathews et al.[277] (Indira Gandhi Centre, India) found a current increase to cause increased neutron production in a cell containing a titanium cathode with palladium and nickel salts in a D_2O electrolyte. Arata and Zhang[229] (Kinki University and Osaka University, Japan) were able to produce bursts at average rates exceeding 10^8 n/s by cycling the current off and on. In this case, the cathode consisted of a 2-cm-diam palladium rod attached to a

TABLE III

Summary of Neutron Measurements

Reference	Metal[a]	Method[b]	Detector[c]	Efficiency[d] (%)	Background[e] (count/s)	Excess[f]
Negative Results						
Aberdam et al.[193]	Palladium, titanium	Electro	^{6}Li scintillator	2.3	2×10^{-5}	0
	Titanium	60 atm				0
	Ti_4Ni_2O	60 atm				0
Abriola et al.[181]	Palladium	Electro	Germanium, BF_3	?	?	<13
Albagli et al.[142]	Palladium	Electro	BF_3, NaI	0.02 to 0.03	0.7	0
Alber et al.[186]	Palladium	Electro	NE-213, BF_3	?	0.02	0
Aleksan et al.[192]	Palladium, titanium	Electro	^{6}Li scintillator	12.5	7×10^{-4}	<0.014
Baurichter et al.[182]	Palladium	Electro	Germanium, scintillator		?	<20
Barwick et al.[168]	Palladium, titanium	15 atm	CR-39	Not applicable	Not applicable	0
Behrisch et al.[167]	Titanium	Implantation	Surface barrier			0
Besenbacher et al.[199]	Palladium, titanium	Bombardment	Scintillator			0
Blagus et al.[201]	Palladium	Electro	^{6}Li scintillator	0.5	?	<0.2
Blencoe et al.[276]	Palladium	<38 atm	BF_3	6	0.16	0
	Titanium	<38 atm	BF_3		0.148	0.164?
Broer et al.[190]	Palladium	Electro	NaI	1.5	1	0
Brudanin et al.[210]	Palladium, titanium	Electro, gas	BF_3, germanium	0.3	10^{-3}–$^{-5}$	0
Brudanin et al.[287]	Palladium	Electro	CR39, silicon			0
Bruschi et al.[207]	Titanium	<1 atm	NE-213	1.3	?	0
Butler et al.[185]	Palladium, titanium	Electro	^{3}He	9.2	0.0028	0
	?	Gas	^{3}He	9.2 to 15		0
Campbell and Perkins[204]	TiD	Infrared heat	NE-213	?	?	0
Davies et al.[198]	Palladium, titanium	Muon	NE-213			0
Deakin et al.[176]	Palladium	Electro	X ray	0.0039	0.0003	0
Dignan et al.[178]	Pd-Ir	Implantation	NaI	0.0004		0
Ehrlich et al.[194]	Palladium	Electro	Boron proportional counter	2.6	0.016	0
Faller et al.[154]	Palladium	Electro	Germanium		40 (gamma only)	0
Gottesfeld et al.[170]	Palladium	Electro	^{3}He, NE-213	16	0.005	0
	Titanium	53 atm				0
Guilinger et al.[152]	Palladium, titanium tritide	Electro	^{3}He	1.3 to 4	0.0017	0
Hajdas et al.[172]	Palladium	Electro	NE-213, BF_3,	0.006	0.0001	0
	$LaNi_5$	12 atm	NaI, NE-102			0
Henderson et al.[179]	Palladium, titanium	Electro	NE-213, NaI	?	0.1 to 0.7	0
Hill et al.[180]	Palladium	Electro	Germanium, BF_3	0.35	0.01	0
	Titanium	50 atm		0.28	0.01	0
Ilić et al.[153]	Palladium	Electro	^{3}He, CaF_2	0.14	?	0
			CR39, germanium			
Kamm et al.[200]	Titanium	12 to 62 atm	Boron proportional counter	0.123	0.029	0

Kashy et al.[197]	Palladium, titanium	Electro	NE-213 (2.5 MeV)	1	?	0
Kocsis et al.[211]	Palladium, titanium	Electro	^{3}He	6.3	0.002	0
Kuzmann et al.[208]	Fe-Zr	Electro	BF_3, NE-213	?	?	0
Lewis et al.[140]	Palladium	Electro	^{3}He, germanium	14 to 20	0.2	0
McCracken et al.[147]	Palladium	Electro	^{3}He, BF_3, NaI	1.6	0.0083	0
	Titanium	40 atm			0.0083	0
Myers et al.[166]	Palladium, titanium, zirconium	Implantation	Particle detector			0
Porter et al.[156]	Palladium	Electro	Germanium, ^{3}He, NE-213	1.3	0.058	0
Rehm et al.[158]	Palladium	Electro	Particle	28	0.001	0
Salamon et al.[164]	Palladium	Electro				0
Schirber et al.[188]	Palladium, titanium	<2.4 kbar	^{3}He	≈9.2	0.0028	0
	$Ti_{0.8}Pd_{0.2}$	50 atm			0.0028	0
	Zirconium, vanadium	50 atm			0.0028	0
Schrieder et al.[159]	Palladium	Electro	Particle detector			0
Silvera and Moshary[171]	Palladium	105 kbar	BF_3	0.09	0.0006	0
Southon et al.[184]	Titanium tritide	Electro	NE-213 (14 MeV)	2.5	0.0006	0
Vielstich et al.[209]	Palladium	Electro	NE-213 (2.5 MeV)	43	0.0012	0
Werle et al.[189]	Titanium	50 atm	^{3}He	10	0.8	0
Ziegler et al.[162]	Palladium	Electro	Particle detector	≈100	5×10^{-5}	0
			Positive Results			
Bertin et al.[257]	Titanium	Electro	NE-213 (2.5 MeV)	4	0.055	0.3 times background
Celani et al.[255]	Palladium	Electro	^{3}He, NaI	1		4 to 16 times background
Claytor et al.[240]	Palladium	Electrodischarge	^{3}He	1.3	0.2	3 to 8σ
De Ninno et al.[244]	Titanium	50 atm	BF_3	0.005	0.00064	35 times background
Fleischmann and Pons[1]	Palladium	Electro	BF_3	0.024	?	3 times background
Govorov et al.[280]	Pd-Sm	1 atm	^{3}He	10.5	?	1.9 times background
	Pd-Ru					
Gozzi et al.[219]	Palladium	Electro	NaI(γ)	0.005	0.0008	180 times background
Granada et al.[263]	Palladium	Electro	^{3}He	17.5	0.001	6σ
Gu et al.[266]	Palladium	Bombardment	BF_3	?	0.033	10 times background
Harb et al.[275]	Palladium	Electro	^{6}Li scintillator	0.53	0.002	2 times background
Iyengar et al.[235]	Palladium, titanium	Electro, gas discharge	BF_3, ^{3}He		Variety	
Izumida et al.[274]	Titanium	20 to 50 atm	^{3}He	0.5	1.7×10^{-2}	>3σ
			BF_3	0.02	2.8×10^{-4}	>3σ
Jianyu et al.[282]	Titanium	Gas	^{3}He	20	0.005	<75 times background
Jorne[264]	Palladium	60 atm	NE-213	1	0.03	2 times background
Jones et al.[267]	Palladium	Electro	^{6}Li scintillator (2.2.5 MeV)		0.001	3.5 times background

See footnotes at the end of the table.

(Continued)

TABLE III (Continued)

Reference	Metal[a]	Method[b]	Detector[c]	Efficiency[d] (%)	Background[e] (count/s)	Excess[f]
Positive Results (Continued)						
Karabut et al.[224]	Palladium	Discharge	^{3}He	5	0.1 to 0.01	10^8 times background
Lewis et al.[230]	Palladium	Electro	^{3}He	8	0.2	2 to 10 times background
Li et al.[317]	Palladium	9 atm	CR-37, CaF_2	Not applicable	Not applicable	Tracks
Lin and Liu[273]	Palladium	Electro	^{3}He, BF_3, germanium	?		≈30 times background
Lipson et al.[278]	Titanium	D_2O (mill)	?		0.05	6 times background
Menlove et al.[259]	Titanium	1 to 40 atm	^{3}He	21 to 44	0.2 to 0.03	3 to 9σ
	Palladium, titanium, vanadium	Electro				3σ
Miljanić et al.[249]	Palladium	Electro	NE-213, LiI	0.04	0.0077	3 times background
	Titanium	25 atm	BF_3	0.01	0.00055	5 times background
Perfetti et al.[270]	Palladium, titanium	Electro	^{3}He	1	0.03	4 times background
Prelas et al.[268]	Palladium	Bombardment	BF_3, scintillator			2 to 4 times background
Rout et al.[236]	Titanium	Discharge	Silver activation		Not applicable	10^6 to 10^8
Sánchez et al.[256]	Titanium	Electro	BF_3, NaI	?	0.00056	$\leq 2 \times 10^4$
Sato et al.[265]	Palladium	Electro	^{3}He	7	0.025	3 times background
Scott et al.[217]	Palladium	Electro	NE-213, NaI	0.146	0.0009	3 times background
Seeliger et al.[271]	Palladium	Electro	NE-213, 2.5 MeV			5 times background
Sinha et al.[281]	Palladium, titanium	Electro	^{3}He, NaI	?	0.003	4 to 6 times background
Sona et al.[248]	Palladium	Electro	^{3}He, BF_3	0.02	0.0013	<3.3 times background
Shani et al.[272]	Palladium	3 kg/cm^2	NE-213			
Takahashi et al.[258]	Palladium	Electro	^{3}He, NE-213	0.17	0.019	≈70 times background
	Pd-Ag	Electro	^{3}He, NE-213		0.04	1.4 times background
Wolf et al.[252]	Titanium	67 atm	NE-213 (2.5 MeV)			0
	Palladium, titanium	Electro		5	0.013	3.8 times background
Yagi et al.[269]	Titanium	Gas	NE-213	0.13	0.003	3σ
	SiO_2	Gas	NE-213	0.13	0.003	3σ
Yamaguchi and Nishioka[254]	Palladium	Gas	BF_3			≈10^5 times background

[a]The elements, alloys, or compounds are listed.

[b]Material studied in an aqueous electrolysis cell is indicated by "Electro" and a dry electrolytic cell by "electrodischarge"; gas loading is generally designated by the pressure of the D_2 gas; and "discharge" and "bombardment" designate the use of high-voltage discharge in low-pressure D_2 and bombardment by high-energy ions, respectively. When in D_2 gas, the material was generally cycled with respect to pressure and/or temperature.

[c]Frequently, variations in the listed detectors were used. In addition to neutron detection, many workers made provisions to find gamma rays by using the listed detector. The ^{3}He and BF_3 detectors are sensitive only to near-thermal neutrons while the various scintillators such as NE-213 and ^{6}Li combined with NE-213 can be used to determine the energy of both neutrons and gamma rays within a wide energy range. Pulse-height measurement is used to distinguish between neutron and gamma emission. Detectors made from germanium or NaI are sensitive to gamma and X rays. CR39 is a track detector that is sensitive to high-energy particles. Frequently, two detectors are used to eliminate the possibility of spurious counts in one detector being interpreted incorrectly.

[d]The stated value for neutrons per count, expressed as percent, is listed. The value is only for the neutron detector. Occasionally, several configurations were studied, giving multiple values for the efficiency. The smaller the value, the more likely small emission rates would be missed.

[e]The background is given as counts per second only for neutron detection. In some cases, a variety of values were observed depending on the configuration. This value as well as the listed efficiency can only be used as a rough guide to the sensitivity of the detection system. The original paper needs to be consulted for more detail.

[f]Excess neutron detection rate is expressed as a multiple of background or sigma above background. Generally, a range of values was reported, depending on the particular experiment.

gold-plated bar of titanium.[f] On the other hand, neither Blagus et al.[201] (Ruder Boskovic Institute, Yugoslavia), using much shorter cycles (2 Hz, 2.2 mHz, and 0.56 mHz), nor Aiello et al.[205] (Universita di Catania, Italy), using loading and unloading in D_2 gas, produced neutrons.

5. Iyengar et al.[235] (BARC, India) reported a large variety of observations during which neutrons were detected from palladium, Pd-Ag, titanium, and Ni-Ti electrodes using LiOD or NaOD in the electrolyte. Steady production as well as large bursts were measured. Neutron production was correlated with gamma-ray and tritium production. Pulsing the current between 1 and 2 A during part of a cell history seemed to encourage production of nuclear products.

6. Gozzi et al.[219] (University of Rome, Italy) correlated a sudden increase in cathode (sintered palladium powder) temperature (>120°C) with a burst of neutrons (7.2×10^5). In this case, it is not known whether the heat rise caused conditions that produce neutrons or, on the other hand, a nuclear event produced neutrons and caused a temperature rise.

7. Yamaguchi and Nishioka[254] (NTT Basic Research Laboratory, Japan) coated a sheet of palladium (1 mm thick) on one side with Mn-O and on the other with gold. After being held for a day in 0.5 atm D_2 at room temperature, the sample was placed in vacuum. Within ~3 h, an explosive release of deuterium occurred that was associated with a burst of neutrons (1 to 2×10^6 n/s). Sudden heating caused alloying between the gold and palladium. Repeated cycles using the same sample produced similar neutron bursts but after a shorter time in vacuum. Neutron production and heat were not observed while using normal hydrogen. This heat cannot be caused simply by loss of the small amount of contained D_2 gas because this reaction is endothermic.

8. Bém et al.[330] (Institute of Nuclear Physics, Czechoslovakia) found that a thin layer of titanium (1.4 to 1.7 mg/cm^2 on molybdenum) that had been reacted with tritium showed low-level neutron production at 14 MeV when it was used as a cathode in an electrolytic cell. On the other hand, Guilinger et al.[152] [Sandia National Laboratories (SNL)] failed to find evidence of fusion using a thin film of titanium tritide on copper in an electrolytic cell, and Southon et al.[184] (McMaster University, Canada) detected no neutrons after a larger piece of titanium had been heated in T_2 gas and electrolyzed. However, there were other major differences between these studies besides the thickness of titanium that could account for the different results.

9. Sánchez et al.[256] (Universidad Autonoma Cantoblanco, Spain) electrolyzed titanium in a cell containing Li_2SO_4 in D_2O. Bursts of neutrons (up to 4×10^4 times background) were correlated with changes in cell current, gamma-ray detection (2 to 2.3 MeV), and tritium production. The neutron rate dropped in a linear manner after the cell current was turned off.

10. Lipson et al.[278] (Institute of Physical Chemistry, USSR) found that ball milling titanium with D_2O or deuterated polypropylene caused neutron emission (0.3 to 0.4 count/s) during the process, for a short time (8 to 10 min) afterward, and when the material was cooled in liquid nitrogen. Repeated cycles caused the effect to disappear.

11. Silvera and Moshary[171] (Harvard University) reported an important negative study done at very high pressures (105 kbar) using thin palladium (97 μm thick × 208-μm diameter) in D_2 gas. A D/Pd ratio of 1.34 ± 0.1 was claimed. This is much higher than any other reported value. However, the palladium had been pressure bonded to rhenium, an operation that would have severely disrupted the periodic array of atoms within the lattice. Detection sensitivity was not sufficient or stable enough to be able to conclude that energy was being produced although there was a slight indication of excess heat (<2.3 mW). No neutrons were observed above the 1.86 ± 0.3 count/h background, and no effort was made to detect tritium. Fractofusion[g] is not possible under these conditions, although heat and/or tritium production might have been possible but their production was inconclusive.

12. Shani et al.[272] (Ben Gurion University and Hebrew University, Israel) made studies of gas-loaded palladium and high-pressure D_2 that suggest that the emission of 2.5-MeV neutrons is enhanced by an external source of neutrons.

III.D Gamma-Ray and Other Emissions

Many workers have examined cells with gamma-ray detectors. However, only a few have detected gamma rays, and this was only while some other aspect of the cold fusion effect was occurring. Because normal water is usually present, some, if not all, of the gamma rays may result from neutron/proton interaction to give a 2.2-MeV signal. A calculated, idealized gamma spectrum produced by the 2.45-MeV neutron energy associated with D-D fusion has been published.[331]

1. Scott et al.[217] (ORNL) measured gamma rays while neutrons were being emitted and heat was being produced. Counts were seen between 2.61 and 3.14 MeV with no increase outside this range down to 2.12 MeV

[f]It is not clear which metal, the palladium or the titanium, caused the neutron burst.

[g]Fractofusion is fusion that is proposed to occur when cracks form in the metal hydride.

or above 5.2 MeV. Correlation between gamma-ray and neutron emission also has been reported by Celani et al.[255] (Frascati Research Centre, Italy) for gamma-ray energy between 100 and 500 keV, and >800 keV; Jorne[264] (University of Rochester) for gamma-ray energies >360 keV; and Sánchez et al.[256] (Universidad Autonoma Cantoblanco, Spain) for energies between 2 and 2.3 MeV. Iyengar et al.[235] (BARC, India) measured correlated neutron and gamma-ray emission (1.186 and >3 MeV) in a variety of cells. Bush et al.[228] (University of Texas and Naval Weapons Center) detected electromagnetic radiation while heat was being produced using X-ray film.

2. Matsumoto[283] (Hokkaido University, Japan) saw tracks in nuclear-sensitive film located on the outside of a cell containing a palladium cathode and an electrolyte of 3% NaCl in D_2O. The shape of the tracks was used to argue for a new particle called an "iton."

3. Jones et al.[332] (Brigham Young University) detected charged-particle emission from deuterium-loaded palladium foils that showed clear energy peaks. Because the inferred energy depends on an uncertain knowledge of the type of particle, the absolute energy is unknown.

III.E. Nuclear Products from Bombardment or Implantation

Nuclear products have been produced by bombarding palladium or titanium with deuterium. Energy has been added to the atoms by using high voltage discharge, ion acceleration, or acceleration of D_2O or D_2 clusters. These techniques add energy to the deuterium atoms before they contact atoms residing in the metal lattice, resulting in what is called "lukewarm fusion" by some workers. Although some fusion is expected to be produced by such high energies, a large quantity of products, a low $n/^3H$ ratio, and sustained emission after bombardment has ceased have been observed and are not expected. On the other hand, some experiments did not produce unexpected results.

III.E.1. High-Voltage Discharge

Karabut et al.[224] (Scientific Industrial Association, USSR) produced heat and neutrons using discharge in D_2 gas with a palladium cathode. The purity of the surface was found to be important. A similar study by Besenbacher et al.[199] (University of Aarhus, Denmark), during which the palladium was covered by 50 Å of copper, failed to produce neutrons. Palladium, after being silver-soldered in air to copper, also failed to produce results in a discharge cell [Ruzic et al.[288] (University of Illinois)]. Prelas et al.[268] (University of Missouri) formed a microwave plasma in D_2 (0.5 to 10 eV) that was caused to impact on palladium metal. Evidence of low-level neutron and gamma-ray emission (2 to 10 times background) was obtained while the plasma was operating. A gamma-ray peak centering on 8.1 MeV was observed during one experiment after a lengthy discharge.

Rout et al.[236] (BARC, India) subjected titanium to plasma discharge of D_2 in a Mather plasma focus device[333] that produced ion energies in the 10- to 100-keV range. This technique produced significant amounts of tritium (up to 392 μCi) and a very low $n/^3H$ ratio ($<10^{-5}$).

III.E.2. Ion Bombardment

Chambers et al.[285] (Naval Research Laboratory), after several previously reported negative attempts, detected the emission of particles that were consistent with the presence of high-energy tritons ($\leq$5 MeV) after bombarding titanium with 300- to 1000-eV deuterons. The magnitude of the emission and its continuation for up to 6 min after the beam had been turned off are not consistent with expectation. Behrisch et al.[167] (Max-Planck-Institut für Plasmaphysik, Federal Republic of Germany) found that bombardment of titanium by 4.5-keV D_3^+ failed to produce detected reaction products.

Cecil et al.[284] (Colorado School of Mines) bombarded thin films of palladium (1 μm) deposited onto molybdenum (3 μm) using 95-keV D^+. Evidence of emitted 3-MeV photons and 1-MeV tritons was obtained during the bombardment. After the bombardment was stopped, when the foil had been sufficiently implanted with deuterium, a current was passed through the foil. Particle emission, assumed to be protons, was detected near 3 and 5 MeV. Later studies[283] using titanium implanted with deuterium followed by several thermal cycles produced large bursts of activity that were inferred to be 10-MeV tritons or ^3He. The presence of any emission during such a treatment, but especially its energy, is unexpected. Gu et al.[266] (Mississippi State University) observed neutron production (9 times background) while bombarding palladium with 1-keV D^+. Neutron production was also observed by Durocher et al.[187] (University of Manitoba, Canada) using 60-keV D^+. However, in the latter case, the flux was claimed to be consistent with known fusion theory. Further studies reported by McKee et al.[334] (University of Manitoba, Canada) showed neutron emission from palladium and titanium targets when loaded with deuterium using 60-keV D^+ and 30-keV D_2^+. Myers et al.[166] (SNL) detected nearly equal proton and triton emission while bombarding palladium, zirconium, and titanium with 10-keV D^+ but found no emission after the beam was turned off. Dignan et al.[178] (San Francisco State University) bombarded a thin film of palladium ($\approx$2000 Å) with 1-keV neutral deuterium and D_2 at 77 K and found no evidence of emitted neutrons or 23.8-MeV gamma rays.

III.E.3. Cluster Bombardment

Beuhler et al.[289] (Brookhaven National Laboratory) bombarded TiD, ZrD, and perdeuteriopolyethylene with clusters of D_2O containing 20 to 1500 molecules accelerated to 200 to 325 keV. A maximum in the resulting photon count rate occurred when the cluster size was near 200 D_2O molecules. The fusion rate was much larger than expected from conventional theory, and larger abundances of ^3H and ^1H were seen in the spectrum compared to ^3He, thereby giving an apparent branching ratio[t] of <0.88. Such studies fall in the transition region between hot fusion and cold fusion. This work has been discussed in detail by Rabinowitz et al. in a series of papers.[335]

Fallavier et al.[290] (Institut de Physique Nucléaire de Lyon, France) used clusters of frozen deuterium ions in the size range between $(200\ D)^+$ and $(300\ D)^+$. Bombardment of TiD and polyethylene targets using an energy of 100 to 150 keV produced no evidence of fusion.

IV. DISCUSSION

During the last 2 years, all of the observations made by the original discoverers of the cold fusion effect have been confirmed by numerous observations throughout the world. In addition, many new conditions have been discovered that cause the effect to occur. In spite of this effort, many people find a major problem in accepting the cold fusion effect because there is a lack of expected nuclear signatures, the magnitude of the effect is much larger than expected based on current theory, and there is still difficulty in replication. As a result, various arguments are proposed to attribute the heat to chemical processes, the tritium is explained as contamination or experimental error, and the neutrons are assumed to be caused by cosmic rays or instrument error. These explanations had reasonable basis during the early history of the field. Now, the variety of techniques and accuracy of the work make this approach much less tenable.

When *d-d* fusion occurs, the reaction products can be tritons (^3H), protons (hydrogen), helium (^3He), and neutrons. Based on considerable experience with "hot" fusion, these reaction products should be produced in negligible quantity, or at least in nearly equal amounts, and be accompanied by X radiation. To the extent that neutrons form, gamma radiation (2.22 MeV) should also be seen from *n-p* interaction with the surrounding water bath. The absence of significant neutrons as well as the absence of any expected nuclear product sufficient to cause the observed heat has added to the skepticism. In addition, the apparent absence of 14-MeV neutrons resulting from *t-d* (^3H-^2H) fusion is a concern to some. Because of these apparent conundrums, the field is still handicapped by considerable doubt and limited support in many countries.

Neutrons are now known to be produced as bursts ($\approx 10^3$ to 10^7 n/s) as well as at a steady but lower rate (<1 n/s). When measured, the expected energy for *d-d* fusion of 2.45 MeV is found along with neutrons near 3 and 7 MeV. Neutrons in unexpectedly large quantity have been found to issue from palladium, titanium, or several alloys after being loaded with deuterium by gas reaction, electrolysis, or ion bombardment. Nonequilibrium conditions, such as produced by temperature changes, increase the probability of production although an overt creation of nonequilibrium is not always a requirement. Physical commutation (ball milling) of titanium in a deuterium-containing media and chemical reactions involving deuterium-containing compounds also can result in neutrons. In some cases, neutrons seem to result from fracturing (fractofusion) of the material lattice. Although titanium is known to easily fracture when it hydrides, this effect is less obvious in palladium. Nevertheless, fissures are produced in palladium during electrolysis,[336,337] and each time it is cycled through the α-β transition.[297]

Values for the $n/^3H$ ratio fall, at the present time, between 4×10^{-4} and 10^{-9} using electrolytic cold fusion cells. Using conventional techniques, the expected branching ratio for *d-d* fusion is near unity for impact energies at least as low as 3 keV (Refs. 338, 339, and 340) and for muon fusion at even lower energy.[341,342] Ion impact studies by Beuhler et al.[289] near 100 eV indicate a slightly less than unity branching ratio. A variety of approaches have been used to explain this apparent conflict. Kim[97] argues that the muon fusion data are not applicable and suggests either that the branching ratio is nonlinear to give smaller $n/^3H$ values at lower energy or there is some resonance enhancement for tritium production in the low-energy region. Resonance between deuterium atoms has also been proposed by Zakowicz and Rafelski.[64] Mayer and Reitz[100] argue that a variety of resonance reactions are possible between deuterium and various impurity metals, and these reactions can result in preferential tritium production. On the other hand, Chatterjee[114] suggests that the branching ratio is very sensitive at low energies to the energy available in the final nuclear products after energy is drained off into the lattice electrons. This idea is extended by Hora et al.[96] using a proposed electron surface layer as the medium for screening and energy extraction. Collins et al.[111] suggest that a tunneling process in the lattice leads to an excited state of ^4He that decays primarily by the production of tritium, a proton, and energetic electrons. Handel[84] also suggests a tunneling process that involves neutron transfer. If a source of virtual neutrons were available in the system or real neutrons were

[t]This value is an upper limit because of an uncertain baseline for the ^3He peak caused by the residual X-ray background.

supplied from the outside, Kim[108] suggests that the $n + {}^6Li = {}^4He + T$ reaction might be the source of both tritium and helium without the production of neutrons. In a more novel model, Hagelstein[104] proposes that a virtual, coherent interaction between a deuteron and a proton occurs and that this not only makes tritium but also deposits the resulting energy into the lattice, thereby solving two problems. Finally, polarization of the two deuterium nucleons by the Oppenheimer-Phillips[343,u] process is proposed by several authors[29] to distort the reaction branches toward tritium production at very low approach energy. Using a statistical model, Bush[319] calculates a value of 1.64×10^{-9} for the $n/^3H$ ratio. This brief summary does not exhaust all of the published suggestions but is included to show that a variety of hypotheses have been proposed to explain the low $n/^3H$ ratio. Of course, there is no universal agreement as yet. On the other hand, there is general agreement that the fusion rate is extremely sensitive to the energy of approaching deuterons for the energies involved in the cold fusion process.[99] In addition, the two branches for D-D fusion may have different sensitivities to energy in this low-energy region if deuteron polarization is important. Therefore, when energy is added to the deuteron, the fusion rate will increase and the $n/^3H$ ratio may change. Proposed processes that add energy are fractofusion, energy amplification at the tips of dendrites on the surface, a high-energy tail to the Maxwell velocity distribution,[43] and phase changes[42,95] that add kinetic energy to the deuterons. These processes add a relatively small amount of energy and would be important only if the fusion process were very sensitive to the deuteron energy. Much more energy can be added outside the metal environment by gas discharge or ion bombardment. Whether these processes can add sufficient energy to explain the observations depends on the chosen nuclear model. In addition, the $n/^3H$ ratio may have a spectrum of values if the deuterons have a variable energy or if several different nuclear reactions occur simultaneously. A mainly tritium reaction is proposed when the least energy is added and a more neutron-rich product when the greatest energy is added.

The expected 14-MeV neutrons appear to be absent. These neutrons would result if fusion produces high-energy tritons that fuse with environmental deuterium. Even if energy measurements are lacking, the resulting neutrons should, nevertheless, add to the general neutron flux. Consequently, the $n/^3H$ ratio should be at least 10^{-5} even if no other neutrons were being made during tritium production. The absence of this neutron flux suggests that tritons are being made with insufficient energy to initiate *d-t* fusion. This energy reduction would result if the nuclear energy were coupled to the lattice in some way. In one case,[330] 14-MeV neutrons were measured after tritium was placed in a cell in advance of the cold fusion reaction. This unique positive result demonstrates that when correct conditions are present to allow fusion to take place, *d-t* fusion can occur, and the reaction produces neutrons of the expected energy. This result does not demonstrate that 14-MeV neutrons will result following *d-d* fusion.

Other types of radiation are expected and have been sought. High-energy particles, when passing through a metal lattice, must produce X rays. However, these X rays have very low energy and, consequently, are difficult to detect unless special provisions are made. Exposure of X-ray film located near the cathode has been reported in a few cases,[228,233] but a quantitative study made during X-ray emission has yet to be reported. However, there have been several unsuccessful attempts to detect X rays.

The initial skeptical reaction was to assume that tritium resulted from contamination. A great deal of effort has been devoted to showing that contamination is not an important factor and how contamination, if it should occur, would behave in an electrolytic cell. The tritium observed in a cold fusion cell does not act like tritium that is known to be present as contamination.[297] Tritium has now been made in electrolytic cells having a variety of electrolytes and cathode metals, by gas loading titanium, and by voltage discharge in low-pressure D_2 gas. It has been detected using scintillator fluid, proportional counting, K_α X-ray emission, and autoradiography. Amounts up to 380 μCi and $>10^5$ times background have been produced on several occasions. Although it is not possible to *prove* that contamination is not present, dismissal of all claimed tritium production based on contamination can no longer be supported by fact. If this assertion is to be made believable, it needs to be backed up by evidence rather than opinion.

Heat has been produced near 450°C at rates up to 25 W, which is >10 times the cell input energy rate. Typical cells at room temperature have excess energy production rates between 7 and 300%. In most cases, this rate is a factor of >10 above the sensitivity or uncertainty in the calorimeter measurements. Because of the care used in the design of more recent studies, dismissal of excess heat based on accumulated or absolute error is no longer tenable. The energy production rate shows a linear increases with cell current up to ~700 mA/cm^2 where it has a predicted production rate of 2.5 W/cm^2. An increase in temperature to ≈450°C results in a factor of 10 increase. Of course, the total amount of energy produced by a cell depends on how long the cell is run after excess energy production starts. Some cells stop production before the patience of the researcher has been exhausted. Other cells are

[u]Polarization of the deuteron was proposed to explain the unexpectedly high transmutation function during nuclear reactions between deuterium, and sodium, aluminum, silicon, and copper.[344]

reported to have produced a total energy >10 MJ. The energy production rate and the total energy obtained from a significant number of cells is far in excess of that produced by any conceivable chemical reaction. *Indeed, if this huge excess were produced by chemical reactions, the discovery would be as novel and important to the field of chemistry as nuclear reactions would be important to physics.* Recent detection of ^{4}He after two studies[222,228] of heat production adds support to the conclusion that excess heat is caused by a nuclear reaction.

The lack of reproducibility in the cold fusion effect is tied directly to a difficulty in creating the required conditions within the lattice or on the surface of the host metal. Regular success cannot be expected until many electrodes have been examined in detail to determine the conditions that both produce and do not produce the effect. In addition, a detailed understanding of the chemistry, metallurgy, and solid-state physics of many metal hydrides, not just palladium, is essential. Even palladium, which has been studied extensively for years,[345] still reveals new variables that affect its interaction with hydrogen.

IV.A. Do Nuclear Reactions Take Place on the Surface or in the Bulk?

A first step toward understanding the cold fusion effect is to determine where nuclear reactions occur. Is it a bulk or a surface effect? Perhaps a combination occurs, depending on the particular nuclear reaction and on the method used to add deuterium. The behavior of tritium can reveal the origin of the tritium-neutron reaction, and the behavior of helium can be used to understand the heat-producing reaction.

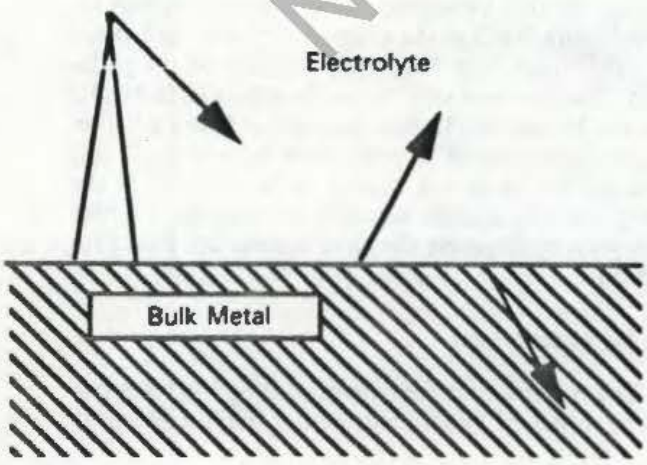

Fig. 5. Typical recoil paths of tritium from an idealized surface and dendrite.

IV.A.1. Tritium Production

Tritium that is located within bulk palladium will exit an electrolytic cell with the evolving gases.[297] Only tritium that forms on the surface can enter the electrolyte and then only if it is released by nuclear recoil energy. These conclusions are based on the behavior of tritium that was placed in a palladium cathode and then followed as it left an electrolytic cell. Because all observed tritium has been found in the electrolyte, except for one report,[3] it is reasonable to conclude that tritium is produced on the surface of the cathode. If this tritium is on the bulk surface, at least half will recoil toward the bulk material and subsequently appear in the evolving gases. The remainder will enter the electrolyte. This nearly 50/50 distribution between gas and electrolyte is not observed. Therefore, the tritium does not appear to form on the bulk surface. To be consistent with observation, we must conclude that tritium forms at the tips of dendrites[v] or promontories located above the surface, as first suggested by Lin et al.[347] supported by Rabinowitz and Worledge,[335] and developed further by Kim.[99] In this location, the solid angle toward the metal structure is sufficiently small so that most of the emitted tritons pass into the electrolyte rather than into the metal. Figure 5 shows typical recoil paths from an idealized surface and dendrite. Tritium has also been produced when Pd-D was electrodeposited,[233] a process that would be expected to produce a rich supply of dendrites.

Tritium production, as a surface reaction, is not expected to require a high average D/Pd ratio in the bulk material. General experience is consistent with this conclusion. However, a high deuterium-to-metal ratio is expected to exist in the dendrites where the fusion reaction is proposed to happen. Tritium production at this location is thought to occur because the deuterium atoms are given some additional energy by the applied cell voltage.[3,33,305,348] If tritium actually forms on the tips of dendrites, then the nuclear reaction does not take place in palladium but in a structure created by electrodeposited impurities. Unfortunately, this suggestion adds an overwhelming complexity to the problem because the surface impurity layer is very complex.[232,313,349-351] However, it does mean that a variety of metals may be useful as cathodes for tritium production, provided the correct impurities are present in the electrolyte.

To the extent that the nuclear energy is not coupled to the metal lattice during tritium production, bremsstrahlung X-radiation and 14-MeV neutrons should be observed. The apparent absence of 14-MeV neutrons suggests that tritium is produced with relatively little

[v]Electrodeposition of impurities is not the only way in which promontories can form on palladium. Stress relief during changes in hydrogen content can also cause variations in surface character.[346]

energy. If tritium forms on dendrite surfaces, methods that allow energy coupling to the metal lattice become a challenge to visualize.

When titanium or palladium are subjected to nonequilibrium conditions, such as temperature cycling after gas loading, tritium is formed even though no dendrites are present. Both metals are known to form cracks during this process. Although titanium and its alloys are more susceptible to cracking than palladium, fractofusion[235,306,342,351-354] is proposed to occur in both cases. During this process, deuterium atoms are given energy by the charge separation created when a crack is formed. Because the generated voltage gradient is high (>10 keV/cm), the fusion rate and the $n/^3H$ ratio would be more like that expected from high-temperature fusion. In addition, the initial formation of a crack may force the deuterium atoms very close together within slip planes, thereby increasing the probability for fusion[355] without additional energy being required. However, the reaction is very brief, the crack formation rate is limited, and the total number of cracks that can form is also limited. Therefore, this reaction is not expected to produce a significant amount of nuclear products. The small amount of tritium that is produced is expected to remain in the metal lattice during gas loading or be swept into the evolving gas during electrolysis. Bursts of neutrons and tritium would be expected if a large number of cracks should form at the same time. Because the resulting $n/^3H$ ratio by this process is expected to be larger than by surface production, the relatively small amount of tritium would probably be missed by the usual detection methods. Consequently, this reaction would appear to produce mainly neutrons. However, there is no reason why this reaction and surface production could not occur simultaneously in an electrolytic cell, thereby giving a variable $n/^3H$ ratio.

IV.A.2. Heat Production

Helium formation appears to be associated with heat production. A small fraction of the necessary helium has been detected within the metal,[222] but much more has been found in the evolving gas.[228] It is well known that once helium is captured* within a palladium lattice, it will not leave[356] except near the melting point. Therefore, for helium to be detected in the gas, it must have been produced sufficiently close to a surface so that recoil energy would allow it to be released from the lattice.† Nuclear recoil would direct some of the helium toward the bulk where it would remain. Consequently, some helium should be found in the bulk material, as has been the case. This heat-producing reaction is not expected to occur on the outer surface where tritium is thought to be produced because heat and tritium production are not initiated by the same conditions and seldom occur at the same time. In addition to the outer surface, there are internal environments that have contact with the evolving deuterium gas. Heat production, therefore, is proposed to occur in contact with the internal fracture system but near the outer surface where the D/Pd ratio would be sufficiently large. If this view is correct, thin films or foils of palladium should work better than bulk material because a greater fraction of the palladium would be involved in heat production and because thin films have a smaller tendency to relieve stress by crack formation. The presence of fewer cracks is proposed to result in a higher average D/Pd ratio and a higher fusion rate. In this case, most of the helium should be found within the metal film rather than in the gas.

The effect of dilution by normal water addition is more of a problem to understand. Studies have shown that heat continues for 20 to 100 h (see Sec. II.C) after normal water is added. Replacement of deuterium by protium in palladium produces some heat but hardly enough to account for the apparent excess. The deuterium replacement rate can be estimated by analogy to the tritium replacement rate. The half-life for tritium replacement by deuterium has been determined to be in the range of 12 to 24 h, depending on conditions.[297] Near-surface replacement would be completed first. Therefore, heat production should stop immediately. This apparent contradiction between helium behavior and water replacement behavior could be explained by some *p-d* fusion taking place or by assuming that isolated regions of high deuterium concentration remain in spite of normal hydrogen being present.

Heat production has been suggested to involve lithium that is located on the surface. This view was supported by a study showing that replacement of LiOD by NaOD caused a loss of excess heat with a somewhat faster rate than after addition of normal water.[214] In contrast to this experience, heat production was reported using NaCl in the electrolyte[227] and by a fused salt cell[222] that does not deposit lithium on the palladium. The fact that tritium can be produced in NaOD may not be relevant to heat production because the reaction environments appear to be different. At this time, lithium does not appear to be involved in the heat-producing nuclear reaction, although its presence does seem to improve the environment for heat production.

IV.B. Ion Bombardment

The tritium- and neutron-producing branches can also be enhanced by giving deuterium additional energy

*This includes helium that might be present as a contaminate. Therefore, the detected helium is not expected to result from helium that might have been contained in the palladium before the experiment started.

†This recoil energy is relatively small if the nuclear energy is coupled to the lattice as is apparently required for this reaction to occur at all.

using an ion beam or by gas discharge with the target being the cathode. This method should be much more neutron rich and fusion efficient than an electrolytic cell. Experience has shown that the target surface must be completely free of certain impurities if this technique is to succeed.[224] Consequently, the surface environment is important even when deuterium is given additional energy. Heat production has been reported on one occasion using the discharge technique, but helium was not sought.[224] Neutron production is said to have been sufficient to account for the observed heat if a small branching ratio is assumed. Because tritium and helium production were not reported, the true relationship between nuclear products and heat is still unknown in this case.

IV.C. Effect of Metal Environment

Because pure palladium was used successfully during the early work, it has been given particular attention. Later studies have shown that fusion can be obtained using palladium alloys as well as titanium. However, because fusion is apparently produced by more than one process, the results have been somewhat different between various metals. Titanium seems to be more likely to produce neutrons during thermal cycling than does palladium. On the other hand, palladium and especially Pd-Ag alloys seem more likely to give tritium and neutrons in an electrolytic cell. A variety of alloys, elements, and physical forms have been studied including palladium (both polycrystalline and single-crystal), Pd-Li, Pd-C, Pd-S, Pd-B, Pd-Be, Pd-Rh, and Pd-Rh-Li (Ref. 238); La-Ni (Ref. 172); Fe-Zr (Ref. 208); Ti-Pd, zirconium, and vanadium[188]; Pd-Sm and Pd-Ru (Ref. 280); Ti-Ni-O (Ref. 193); Pd-Ir (Ref. 178); Pd-Ag (Refs. 235 and 258); SiO_2 (Ref. 269); and vanadium (Ref. 259). Only SiO_2, titanium, palladium (drawn, rolled, hammered, annealed, and electrodeposited), Pd-Sm, Pd-Ru, and Pd-Ag have given evidence of a fusion reaction. Heat has been reported using titanium as well as palladium, although palladium appears to be the better choice for this aspect of the effect. The presence of large amounts of most elements that are normally present in palladium as low-level impurities, excluding silver, do not improve the reproducibility of tritium production. Therefore, at least one nuclear reaction involving these elements seems unlikely.

Rather special and difficult to reproduce conditions are an essential feature of the cold fusion effect. To the extent that nuclear reactions occur on the surface, the environment will be complex and not involve the base metal directly. Therefore, those variables that affect the surface and those impurities that reside on or are plated onto the surface need increased attention. On the other hand, heat production seems to be sensitive to the bulk properties of the metal.

Palladium has been found to show several unusual effects that might be relevant to heat production. In passing completely into the β-phase, excess volume is created.[297] This process continues without apparent limit each time the material is cycled between the α- and β-phases after being within the β-phase. Measurements of alternating current resistivity and capacitance of palladium cycled in this manner indicate that rifts or fractures are formed and these contain a significant amount of ionized[337] hydrogen under high pressure.[357,y] Detailed visual examination shows fractures of various sizes and shapes mainly associated with grain boundaries.[358,z] These fractures open and close depending on the charging or discharging conditions. During electrolytic charging, a flux of deuterium flows into the surface, through the metal and is exhausted from the electrode through that part of the fracture system connecting to the surface.[297] Thus, even under steady-state conditions, the metal hydride is not at equilibrium. Furthermore, the high-pressure, ionized deuterium gas present in the closed rifts is sensitive to current flowing through the palladium.[aa] This fracture system offers an additional environment, consistent with helium-loss experience,[bb] in which the heat-producing reaction might occur. This general behavior suggests that attention needs to be paid to the interior surfaces of palladium as well as to its outer surface.

IV.D. Proposed Nuclear Reactions

With very few exceptions, all of the effects attributed to cold fusion require the presence of deuterium. For a nuclear reaction to occur, either two deuterons must fuse together or a deuteron must fuse with some other nuclei such as a proton or a metal. A conclusion based on *d-p* fusion is made less likely because the presence of normal water eventually stops the reaction without producing an increase when the protium concentration in the palladium just begins to increase.[cc] Interaction with a metal nucleus may be possible but seems unlikely to be the main reaction because, except for palladium, other metals are present in very low and highly variable concentrations. When many of these impurities were added in higher concentration, no improvement in reproducibility was observed. In addition,

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[y] J. Dienes gives a theoretical analysis of formation conditions for internal fracture.[355]

[z] Matsumoto[356] proposes that this fracture system is partly caused by the fusion reaction, and the fusion reaction causes the size of the voids to grow. At the present time, there is no supporting evidence for this viewpoint.

[aa] It is worth noting that the addition of deuterium to the rift and dislocation system is expected to be slightly exothermic.[359]

[bb] For helium to be found in the evolving gas, it must be produced on or very near a surface that is in contact with the evolving gas.

[cc] After D_2O is replaced by H_2O, the H/D ratio in the palladium will slowly rise over several hours with the fastest change occurring in the near-surface region.

evidence for helium generation during heat production is starting to be accumulated. Consequently, *d-d* fusion remains the simplest explanation consistent with most experimental data. The observations of different combinations of heat, tritium, and neutron production are proposed to result from a different combination of the three branches for this fusion reaction. These branches are

1. $D(d,n)^3He$
2. $D(d,p)^3H$
3. $D(d, \text{energy})^4He$.

This explanation requires that each branch be sensitive to different conditions within or on the surface of a metal and that each branch couple most of the resulting nuclear energy to the lattice. While there is also some evidence for other reactions involving neutron transfer to the palladium and/or metallic impurity atoms, this does not seem to be the main source of heat or tritium. Nevertheless, these possibilities suggest that several nuclear reactions might be catalyzed by a metal lattice under special conditions.

V. CONCLUSION

It is easy to dismiss one or even a few observations of unexpected behavior that cannot be reproduced. This is done routinely by scientists during their work because most of such observations usually are caused by unknown errors. However, when many measurements, using a variety of techniques, are found to give similar results and begin to reveal patterns of behavior, the observations can no longer be ignored. This situation now exists in the cold fusion field. It is now far easier and more rational to begin the process of understanding cold fusion as a real phenomenon rather than finding ways to dismiss it. In spite of early difficulties, useful theoretical models are being constructed, and the work is gradually becoming reproducible. Unfortunately, the number of important variables is so large, the necessary diagnostic equipment is so expensive, and the general support is so minimal that a clear and convincing understanding will be very slow in coming.

At the present time, heat production equal to at least ten times the input energy has been achieved. This magnification is well above breakeven on a laboratory scale. The heat effect appears to involve mainly the near-surface rather than the entire bulk material, and it has a limited lifetime in present cells. Consequently, a possibility exists for considerable magnification of the effect should ways be found to involve a greater fraction of the metal for a longer time. This potential provides an important incentive for possible commercial application. The low availability and high cost of palladium may not be a limitation as first thought, provided the effect can be made to occur in thin films. If this phenomenon can be developed to the level of commercial usefulness, heat could be produced without the formation of significant undesirable products now associated with nuclear as well as combustion energy. Even "hot" fusion is not as benign nor as far above breakeven after years of study as is "cold" fusion. In addition, tritium might also be produced under different conditions without the large number of radioactive products that are a by-product of current methods. During this time of environmental concern, these possibilities are extremely important and should not be ignored.

A large fraction of the limited resource has been devoted to proving that cold fusion is real in contrast to understanding how it works. Except during the early euphoria, support has been minimal in many countries. As a result, many people have continued to do excellent work in spite of very little support from the scientific institutions or their peers, with a few exceptions. Although there are still many uncertainties, I suggest that the possible applications are so important and the present evidence for the reality of the effect is so strong that a more optimistic attitude and more support are warranted.

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REFERENCES

1. M. FLEISCHMANN and S. PONS, "Electrochemically Induced Nuclear Fusion of Deuterium," *J. Electroanal. Chem.*, **261**, 301 (1989); see also M. FLEISCHMANN and S. PONS, Errata, *J. Electroanal. Chem.*, **263**, 187 (1989).

2. S. E. JONES et al., "Observation of Cold Nuclear Fusion in Condensed Matter," *Nature*, **338**, 737 (1989); see also J. B. CZIRR and G. L. JENSEN, "A Neutron Coincidence Spectrometer," *Nucl. Instrum. Method Phys. Res. A*, **284**, 365 (1989).

3. J. O'M. BOCKRIS, G. H. LIN, N. J. C. PACKHAM, "A Review of the Investigations of the Fleischmann-Pons Phenomena," *Fusion Technol.*, **18**, 11 (1990); see also J. BOCKRIS and D. HODKO, "Is There Evidence for Cold Fusion?," *Chem. Ind.*, **21**, 688 (1990); see also J. BOCKRIS, "Cold Fusion II: The Story Continues," *New Sci.*, **1752**, 50 (1991).

4. J. J. HURTAK, "Industrial Applications of Cold Fusion," *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22–27, 1990, p. 215, University of Hawaii.

5. M. RABINOWITZ, "High Temperature Superconductivity and Cold Fusion," *Mod. Phys. Lett. B*, **4**, 233 (1990); see also V. A. TSAREV, "Cold Fusion," *Usp. Fiz. Nauk.*, **160**, 1 (1990) and *Sov. Phys. Usp.*, **33**, 11 (1990); see also J. W. SCHULTZE, U. KÖNIG, A. HOCHFELD, C. van CAIKER, and W. KIES, "Prospects and Problems of Electrochemically Induced Cold Fusion," *Electrochim. Acta*, **34**, 1289 (1989).

6. M. SRINIVASAN, "Nuclear Fusion in an Atomic Lattice: An Update on the International Status of Cold Fusion Research," *Curr. Sci.*, **60**, 417 (Apr. 1991).

7. F. D. PEAT, *Cold Fusion*, Contemporary Books, Chicago, Illinois (1989).

8. E. MALLOVE, *Fire from Ice: Search for Truth Behind the Cold Fusion Furor*, John Wiley & Sons, New York (1991).

9. F. CLOSE, *Too Hot to Handle*, Princeton University Press, Princeton, New Jersey (1991); see also F. CLOSE, "Cold Fusion I: The Discovery That Never Was," *New Sci.*, 1752, 46 (1991); see also B. PIPPARD, "Footnote to History," *Nature*, **350**, 29 (1991).

10. M. DEHN, "Electrochemistry Primer," *Fusion Facts*, **2**, *10*, 7 (1991); see also F. A. KROGER, "The Role of Cathodic Overvoltage in Achieving Nuclear Fusion by Electrolysis of Heavy Water," *Fusion Facts*, **2**, *10*, 13 (1991).

11. R. A. HUGGINS, "Fundamental Considerations Relating to the Electrochemical Insertion of Hydrogen and Palladium into Mixed Conductors," *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22–27, 1990, p. 181, University of Hawaii.

12. G. H. MILEY, M. H. RAGHEB, and H. HORA, "On Aspects of Nuclear Products," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28–31, 1990, p. 202, National Cold Fusion Institute (1990); see also G. PREPARATA, "Some Theories of 'Cold' Nuclear Fusion: A Review," *Fusion Technol.*, **20**, 82 (1991).

13. T. BRAUN, "A Selective, Annotated Bibliography, Part 1," *J. Radioanal. Nucl. Chem. Lett.*, **136**, 1 (1989); T. BRAUN, "A Selective, Annotated Bibliography, Part 2," *J. Radioanal. Nucl. Chem. Lett.*, **137**, 407 (1989); T. BRAUN, "A Selective, Annotated Bibliography, Part 3," *J. Radioanal. Nucl. Chem. Lett.*, **144**, 161 (1990); T. BRAUN, "A Selective, Annotated Bibliography, Part 4," *J. Radioanal. Nucl. Chem. Lett.*, **144**, 323 (1990); and T. BRAUN, "A Selective, Annotated Bibliography, Part 5," *J. Radioanal. Nucl. Chem. Lett.*, **144**, 1 (1990).

14. V. A. TSAREV and D. H. WORLEDGE, "New Results on Cold Nuclear Fusion," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990; see also D. WORLEDGE, "Technical Status of Cold Fusion Results," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28–31, 1990, p. 252, National Cold Fusion Institute (1990); see also V. A. TSAREV, *Usp. Fiz. Nauk*, **160**, *1*, 1 (1990).

15. *21st Century Sci. Tech.*, **2**, 6 (1989); see also K. A. McDONALD, "Claims of Proponents of Cold Fusion Still Spark Controversy a Year Later," *Chron. Higher Educ.*, **37**, *A4*, 8 (1990); see also R. POOL and M. CRAWFORD, "How Cold Fusion Happened—Twice!" *Science*, **244**, 423 (1989).

16. G. TAUBES, "Cold Fusion Conundrum at Texas A&M," *Science*, **248**, 1299 (1990); see also D. M. ANDERSON, J. O'M. BOCKRIS, D. H. WORLEDGE, and G. TAUBES, "Letters and Response About Cold Fusion at Texas A&M," *Science*, **249**, 463 (1990); see also P. J. SKERRETT, "Cold Fusion at Texas A&M: Problems, But No Fraud," *Science*, **250**, 1507 (1990).

17. W. J. BROAD, "Cold-Fusion Claim is Faulted on Ethics as Well as Science," *New York Times*, **CXL**, 1 (Mar. 17, 1991).

18. A. T. BOOTHROYD, "Physics," *1991 Britannica Book of the Year*, p. 278 (1991).

19. T. WATERS, "Science Duds 1989: Old Fusion," *Discover*, 43 (Jan. 1990).

20. J. MADDOX, "End of Cold Fusion in Sight," *Nature*, **340**, 15 (1990).

21. S. BEGLEY, H. HURT, A. MURR, and M. ROGERS, "The Race for Fusion and the Follies of Science," *Newsweek*, 49 (May 8, 1989).

22. P. K. IYENGAR, Indian Atomic Energy Commission, Personal Communication (1989).

23. D. WORLEDGE, quoted in *Fusion Facts*, **2**, *10*, 1 (1991).

24. G. BENEDEK and P. F. BORTIGNON, "Cold Nuclear Fusion: Viewpoints of Solid-State Physics," *Nuovo Cimento*, **11**, 1227 (1989).

25. J. SCHWINGER, "Nuclear Energy in an Atomic Lattice. 1," *Z. Phys. D*, **15**, 221 (1990); see also J. SCHWINGER, "Nuclear Energy in an Atomic Lattice," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28–31, 1990, p. 130, National Cold Fusion Institute (1990); see also J. SCHWINGER, "Cold Fusion: A Hypothesis," *Z. Naturforsch. A*, **45**, 756 (1990).

26. R. A. RICE, G. S. CHULICK, and Y. E. KIM, "The Effect of Velocity Distribution and Electron Screening on Cold Fusion," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28–31, 1990, p. 185, National Cold Fusion Institute (1990); see also R. A. RICE, G. S. CHULICK, Y. E. KIM, and J. YOON, "The Role of Velocity Distribution in Cold Deuterium-Deuterium Fusion," *Fusion Technol.*, **18**, 147 (1990).

27. Y. E. KIM, "Surface Reaction Mechanism and Lepton Screening for Cold Fusion with Electrolysis," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28–31, 1990, p. 194, National Cold Fusion Institute (1990).

28. J. R. McNALLY, Jr., "On the Possibility of a Nuclear Mass-Energy Resonance in D + D Reactions at Low Energy," *Fusion Technol.*, **16**, 237 (1989).

29. M. RAGHEB and G. H. MILEY, "Deuteron Disintegration in Condensed Media," *J. Fusion Energy*, **9**, 429 (1990); see also M. RAGHEB and G. H. MILEY, "On the Possibility of Deuteron Disintegration in Electrochemically Compressed D^+ in a Palladium Cathode," *Fusion Technol.*, **16**, 243 (1989).

30. J. SCHNEIDER, "How a Rectangular Potential in Schrödinger's Equation Could Explain Some Experimental Results on Cold Nuclear Fusion," *Fusion Technol.*, **16**, 377 (1989).

31. A. TAKAHASHI, "Opening Possibility of Deuterium-Catalyzed Cascade Fusion Channel in PdD Under D_2O Electrolysis," *J. Nucl. Sci. Technol.*, **26**, 558 (1989).

32. T. BRESSANI, E. del GIUDICE, and G. PREPARATA, "First Steps Toward an Understanding of 'Cold' Nuclear Fusion," *Nuovo Cimento*, **101A**, 845 (1989).

33. G. M. HALE, R. D. SMITH, and T. L. TALLEY, "Nuclear Reactions and Screened-Coulomb Fusion Rates," *J. Fusion Energy*, **9**, 187 (1990).

34. M. A. HARITH, V. PALLESCHI, A. SALVETTI, D. P. SINGH, and M. VASELLI, "Theoretical and Experimental Studies on the Cold Nuclear Fusion Phenomena," *Fusion Technol.*, **17**, 704 (1990).

35. M. BALDO, R. PUCCI, and P. F. BORTIGNON, "Relaxation Toward Equilibrium in Plasmon-Enhanced Fusion," *Fusion Technol.*, **18**, 347 (1990).

36. A. BURROWS, "Enhancement of Cold Fusion in Metal 'Hydrides' by Screening of Photon and Deuteron Charges," *Phys. Rev. B*, **40**, 3405 (1989).

37. C. J. BENESH and J. P. VARY, "Fusion Rates of Squeezed and Screened Hydrogenic Nuclei," *Phys. Rev. C*, **40**, R495 (1989).

38. F. PARMIGIANI and P. G. SONA, "Theoretical Considerations on the Cold Nuclear Fusion in Condensed Matter," *Nuovo Cimento D*, **11**, 913 (1989).

39. G. ROSEN, "Deuterium Nuclear Fusion at Room Temperature: A Pertinent Inequality on Barrier Penetration," *J. Chem. Phys.*, **91**, 4415 (1989).

40. V. V. KOMAROV, O. MELSHEIMER, and A. POPOVA, "Does Cold Fusion Exist and Is It Measurable?," *Z. Naturforsch. A*, **45**, 759 (1990).

41. S. N. VAIDYA and Y. S. MAYYA, "Theory of Screening-Enhanced D-D Fusion in Metals," *J. Appl. Phys. Lett.*, **28**, L2258 (1989).

42. S. E. SEGRE, S. ATZENI, S. BRIGUGLIO, and F. ROMANELLI, "A Mechanism for Neutron Emission from Deuterium Trapped in Metals," *Europhys. Lett.*, **11**, 201 (1990).

43. S.-B. ZHU, J. LEE, and G. W. ROBINSON, "Non-Maxwell Velocity Distributions in Inhomogeneous Materials," *J. Fusion Energy*, **9**, 465 (1990); see also S.-B ZHU, J. LEE, and G. W. ROBINSON, "Kinetic Energy Imbalance in Inhomogeneous Materials," *Chem. Phys. Lett.*, **161**, 249 (1989); see also S.-B ZHU, J. LEE, and G. W. ROBINSON, "Nonlinear Effects on Thermonuclear Reaction Rates," *Phys. Lett. A*, **144**, 361 (1990).

44. H. TAKAHASHI, "Dynamical Screening of Potential by Mobile Deuterium and Fusion Rate of Accelerated Deuterons in PdD_x," *J. Fusion Energy*, **9**, 441 (1990).

45. T. TAJIMA, H. IYETOMI, and S. ICHIMARU, "Influence of Attractive Interaction Between Deuterons in Pd on Nuclear Fusion," *J. Fusion Energy*, **9**, 437 (1990); see also A. B. HASSAM and A. N. DHARAMSI, "Deuterium Molecules in the Presence of Electronic Charge Concentrations: Implications for Cold Fusion," *Phys. Rev. A*, **40**, 6689 (1989).

46. M. VASELLI, M. A. HARITH, V. PALLESCHI, G. SALVETTI, and D. P. SINGH, "Screening Effect of Impurities in Metals: A Possible Explanation of the Process of Cold Nuclear Fusion," *Nuovo Cimento D*, **11**, 927 (1989).

47. A. R. LEE and T. M. KALOTAS, "On the Feasibility of Cold Fusion," *Nuovo Cimento A*, **102**, 1177 (1989).

48. K. LANGANKE, H. J. ASSENBAUM, and C. ROLFS, "Screening Corrections in Cold Deuterium Fusion Rates," *Z. Phys. A*, **333**, 317 (1989); see also H. J. ASSENBAUM, K. LANGANKE, and C. ROLFS, "Effects of Electron Screening on Low-Energy Fusion Cross Sections," *Z. Phys. A*, **327**, 461 (1987).

49. P. NORDLANDER, J. K. NØRSKOV, F. BESENBACKER, and S. M. MYERS, "Multiple Deuterium Occupancy of Vacancies in Pd and Related Metals," *Phys. Rev. B*, **40**, 1990 (1989).

50. O. E. ROSSLER, J. BECKER, M. HOFFMAN, and W. NADLER, "Fermi Gas Like Hypothesis for Fleischmann-Pons Experiment," *Z. Naturforsch. A*, **44**, 329 (1989).

51. I. SHIMAMURA, "Intramolecular Nuclear Fusion in Hydrogen-Isotope Molecules," *Prog. Theor. Phys.*, **82**, 304 (1989).

52. S. FENG, "Enhancement of Cold Fusion Rate by Electron Polarization in Palladium Deuterium Solid," *Solid State Comm.*, **72**, 205 (1989).

53. B. J. B. CROWLEY, "Nuclear Fusion in High Density Matter," *Nucl. Fusion*, **29**, 2199 (1989).

54. A. J. LEGGETT and G. BAYM, "Can Solid-State Effects Enhance the Cold-Fusion Rate," *Nature*, **340**, 45 (1989); see also A. J. LEGGETT and G. BAYM, "Exact

Upper Bound on Barrier Penetration Probabilities in Many-Body Systems: Application to 'Cold Fusion,'" *Phys. Rev. Lett.*, **63**, 191 (1989).

55. J. KONDO, "Cold Fusion in Metals," *J. Phys. Soc. Jpn.*, **58**, 1869 (1989).

56. X. W. WANG, S. G. LOUIE, and M. L. COHEN, "Hydrogen Interactions in $PdH_n (1 \leq n \leq 4)$," *Phys. Rev. B*, **40**, 5822 (1989).

57. C. J. HOROWITZ, "Cold Nuclear Fusion in Hydrogen and Normal Metals," *Phys. Rev. C*, **40**, R1555 (1989).

58. Z. SUN and D. TOMANEK, "Cold Fusion: How Close Can Deuterium Atoms Come Inside Palladium?," *Phys. Rev.*, **63**, 59 (1989).

59. V. I. GOLDANSKII and F. I. DALIDCHIK, "Mechanism of Solid-State Fusion," *Nature*, **342**, 231 (1989); see also V. I. GOLDANSKII and F. I. DALIDCHIK, "On the Possibilities of 'Cold Enhancement' of Nuclear Fusion," *Phys. Lett. B*, **234**, 465 (1990).

60. M. GRYZINSKI, "Molecular Nature of Cold Fusion in Condensed Matter," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

61. M. JANDEL, "Pressure Enhanced Fusion Rates in Lattice Channels," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

62. D. SEELIGER and A. MEISTER, "A Simple Plasma Model for the Description of *dd*-Fusion in Condensed Matter," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

63. V. I. VYSOTSKII and R. N. KUZ'MIN, "The Theory of Nonthreshold Cold Fusion in Solids," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

64. W. ZAKOWICZ and J. RAFELSKI, "Low Energy Nuclear Resonance and Cold Fusion," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

65. M. BELLINI, L. CASETTI, and M. ROSA-CLOT, "Nuclear Fusion in Excited Hydrogen Molecules," *Z. Phys. A*, **337**, 207 (1990).

66. R. S. FISHMAN and G. D. MAHAN, "Binding of Charged Particles in Lattice Defects," *Phys. Rev. B*, **40**, 11493 (1989).

67. S. Y. LO, "Enhancement of Nuclear Fusion in a Strongly Coupled Cold Plasma," *Mod. Phys. Lett. B*, **3**, 1207 (1989).

68. W. SCHOMMERS and C. POLITIS, "Cold Fusion in Condensed Matter: Is a Theoretical Description in Terms of Usual Solid State Physics Possible?" *Mod. Phys. Lett. B*, **3**, 597 (1989); see also V. C. SAHNI, "Comment on 'Cold Fusion in Condensed Matter: Is a Theoretical Description in Terms of Usual Solid State Physics Possible?,'" *Mod. Phys. Lett. B*, **4**, 497 (1990).

69. W. N. COTTINGHAM and D. A. GREENWOOD, "The Fusion Rate of a Confined Deuteron Pair," *J. Phys. G*, **15**, L157 (1989).

70. L. BRACCI, G. FIORENTINI, and G. MEZZORANI, "Nuclear Fusion in Molecular Systems," *J. Phys. G*, **16**, 83 (1990).

71. L. L. LOHR, "Electronic Structure of Palladium Clusters: Implications for Cold Fusion," *J. Phys. Chem.*, **93**, 4697 (1989).

72. S. ICHIMARU, S. OGATA, and A. NAKANO, "Rates of Nuclear Fusion in Metal Hydrides," *J. Phys. Soc. Jpn.*, **59**, 3904 (1990).

73. J. W. MINTMIRE et al., "Chemical Forces Associated with Deuterium Confinement in Palladium," *Phys. Lett. A*, **138**, 51 (1989).

74. M. Y. AZBEL, "Possibility of Cold Fusion," *Solid State Comm.*, **76**, 127 (1990).

75. F. LIU, B. K. RAO, S. N. KHANNA, and P. JENA, "Nature of Short Range Interaction Between Deuterium Atoms in Pd," *Solid State Comm.*, **72**, 891 (1989).

76. V. C. ROGERS and G. M. SANDQUIST, "Isotopic Hydrogen Fusion in Metals," *Fusion Technol.*, **16**, 254 (1989); see also V. C. ROGERS and G. M. SANDQUIST, *J. Fusion Energy*, **9**, 351 (1990), and *J. Fusion Energy*, **9**, 483 (1990).

77. C. D. VAN SICLEN and S. E. JONES, "Piezonuclear Fusion in Isotopic Hydrogen Molecules," *J. Phys. G*, **12**, 213 (1986).

78. B. I. SPINRAD, "On Cold Fusion," *Fusion Technol.*, **17**, 343 (1990).

79. Z. SLANINA, "Towards Molecular-Thermodynamic Aspects of Postulated Pd/D Low-Temperature Nuclear Fusion: A Useful Example of a Failure of Conventional Translational Partition Function," *Thermochim. Acta*, **156**, 285 (1989).

80. E. W. BECKER, "Triple Collision Reaction of Deuterons as a Possible Explanation of Cold Nuclear Fusion," *Naturwissen.*, **76**, 214 (1989).

81. P. H. PARMENTER and W. E. LAMB, Jr., "Cold Fusion in Palladium: A More Realistic Calculation," *Proc. Natl. Acad. Sci. U.S.*, **87**, 8652 (1990).

82. P. M. RICHARDS, "Molecular-Dynamics Investigation of Deuterium Separation in $PdD_{1.1}$," *Phys. Rev. B*, **40**, 7966 (1989).

83. C. T. WHITE, B. I. DUNLAP, D. W. BRENNER, R. W. MOWREY, and J. W. MINTMIRE, "Limits of Chemical Effects on Cold Fusion," *J. Fusion Energy*, **9**, 363 (1990).

84. P. H. HANDEL, "Intermittency, Irreproducibility, and the Main Physical Effects in Cold Fusion," *Fusion Technol.*, **18**, 512 (1990).

85. K. B. WHALEY, "Boson Enhancement of Finite-Temperature Coherent Dynamics for Deuterium in Metals," *Phys. Rev. B*, **41**, 3473 (1990).

86. K. SZALEWICZ and J. D. MORGAN III, "Fusion Rates for Hydrogen Isotopic Molecules of Relevance for 'Cold Fusion,'" *Phys. Rev. A*, **40**, 2824 (1989).

87. K. SOHLBERG and K. SZALEWICZ, "Fusion Rates for Deuterium in Titanium Clusters," *Phys. Lett. A*, **144**, 365 (1990).

88. E. SIMÁNEK, "Quantum Tunnelling Through a Fluctuating Barrier: Enhancement of Cold-Fusion Rate," *Physica A*, **164**, 147 (1990).

89. S. WEI and A. ZUNGER, "Instability of Diatomic Deuterium in fcc Palladium," *J. Fusion Energy*, **9**, 367 (1990).

90. F. F. MUGUET and P. M.-P. BASSEZ-MUGUET, "Ab Initio Computations of One and Two Hydrogen or Deuterium Atoms in the Palladium Tetrahedral Site," *J. Fusion Energy*, **9**, 383 (1990).

91. J. W. HALLEY and J. L. VALLES, "Estimate of Nuclear Fusion Rates Arising from a Molecular-Dynamics Model of PdD_x," *Phys. Rev. B*, **41**, 6072 (1990).

92. S. FUJITA, "On the Feasibility of Nuclear Fusion in F. C. C. Metals," *Phys. Stat. Sol. B*, **156**, K17 (1989).

93. J. D. MORGAN III and H. J. MONKHORST, "Simple Model for Accurate Calculation of Coulomb-Barrier Penetration Factors in Molecular Fusion Rates," *Phys. Rev. A*, **42**, 5175 (1990).

94. Y. E. KIM, "Surface-Reaction Theory of Cold and Warm Fusion," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

95. E. TABET, "Nuclear Reactions From Lattice Collapse in a Cold Fusion Model," *Phys. Lett. A*, **144**, 301 (1990); see also E. TABET, "Developments of a Dynamical Model for Cold Fusion in Deuterated Palladium," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

96. H. HORA, L. CICCHITELLI, G. H. MILEY, M. RAGHEB, A. SCHARMANN, and W. SCHEID, "Plasma and Surface Tension Model for Explaining the Surface Effect of Tritium Generation at Cold Fusion," *Nuovo Cimento D*, **12**, 393 (1990); see also H. HORA, G. H. MILEY, M. RAGHEB, and A. SCHARMANN, "Surface Models for Cold Fusion and the Possibility of Multilayered Cells for Energy Production," *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22-27, 1990, p. 169, University of Hawaii.

97. Y. E. KIM, "Cross Section for Cold Deuterium-Deuterium Fusion," *Fusion Technol.*, **17**, 507 (1990); see also Y. E. KIM, "The Efimov Effect and the Anomalous Branching Ratio for Deuterium Fusion in Deuterium/Solid Systems," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

98. J. P. KENNY, "Electropionics and Fusion," *Fusion Technol.*, **19**, 547 (1991).

99. Y. E. KIM, "Surface Reaction Mechanism for Deuterium-Deuterium Fusion with a Gas/Solid-State Fusion Device," *Fusion Technol.*, **19**, 558 (1991).

100. F. J. MAYER and J. R. REITZ, "Nuclear Energy Release in Metals," *Fusion Technol.*, **19**, 552 (1991).

101. Y. E. KIM, R. A. RICE, and G. S. CHULICK, "The Role of the Low-Energy Proton-Deuteron Fusion Cross Section in Physical Processes," *Fusion Technol.*, **19**, 174 (1991).

102. L. TURNER, "Peregrinations on Cold Fusion," *J. Fusion Energy*, **9**, 447 (1990); see also L. TURNER, "Thoughts Unbottled by Cold Fusion," *Phys. Today*, **42**, 140 (1989).

103. G. PREPARATA, "Theoretical Ideas on Cold Fusion," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 91, National Cold Fusion Institute (1990).

104. P. L. HAGELSTEIN, "Coherent Fusion Theory," *J. Fusion Energy*, **9**, 451 (1991); see also P. L. HAGELSTEIN, "Status of Coherent Fusion Theory," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 99, National Cold Fusion Institute (1990); see also P. L. HAGELSTEIN, "Coherent Fusion Reaction Mechanisms," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

105. S. R. CHUBB and T. A. CHUBB, "Bloch-Symmetric Fusion in PdD_x," *Fusion Technol.*, **17**, 710 (1990); see also S. R. CHUBB and T. A. CHUBB, "Quantum Mechanics of 'Cold' and 'Not-so-Cold' Fusion," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 99, National Cold Fusion Institute (1990); see also S. R. CHUBB and T. A. CHUBB, "Lattice Induced Nuclear Chemistry," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990; see also S. R. CHUBB and T. A. CHUBB, "Cold Fusion as an Interaction Between Ion Band States," *Fusion Technol.*, **20**, 93 (1991).

106. R. W. BUSSARD, "Virtual-State Internal Nuclear Fusion in Metal Lattices," *Fusion Technol.*, **16**, 231 (1989).

107. T. MATSUMOTO, "'Nattoh' Model for Cold Fusion," *Fusion Technol.*, **16**, 532 (1989); see also T. MATSUMOTO, "Progress of Nattoh Model and New Particles Emitted During Cold Fusion," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

108. Y. E. KIM, "New Cold Nuclear Fusion Theory and Experimental Tests," *J. Fusion Energy*, **9**, 423 (1990).

109. L. C. JENSEN and K. S. MORTENSEN, "Beyond Fusion, Annihilation Reactions of Confined Hydrogen," *J. Fusion Energy*, **9**, 417 (1990).

110. M. DANOS, "Coulomb-Assisted Cold Fusion," *J. Fusion Energy*, **9**, 413 (1990), and *Fusion Technol.*, **17**, 484 (1990); see also M. DANOS and V. BELYAEV, "Theoretical Studies/Theory of Neutron Transfer Fusion," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

111. G. S. COLLINS, J. S. WALKER, and J. W. NORBURY, "Deuteron Tunneling at Electron-Volt Energies," *J. Fusion Energy*, **9**, 407 (1990).

112. J. JORNE, "Electrochemically Induced Nuclear Fusion of Deuterium: The Existence of Negatively Charged Deuterium Ions," *Fusion Technol.*, **18**, 518 (1990).

113. C. HARGITAI, "Considerations on Cold Nuclear Fusion in Palladium," *J. Radioanal. Nucl. Chem. Lett.*, **137**, 17 (1989).

114. L. CHATTERJEE, "Could Spectator Electrons Legalize Cold Fusion?," *Fusion Technol.*, **18**, 683 (1990).

115. C. WALLING and J. SIMONS, "Two Innocent Chemists Look at Cold Fusion," *J. Phys. Chem.*, **93**, 4696 (1989).

116. W. ZAKOWICZ, "Possible Resonant Mechanism of Cold Fusion," *Fusion Technol.*, **19**, 170 (1991).

117. J. RAFELSKI, M. SAWICKI, M. GAJDA, and D. HARLEY, "How Cold Fusion Can Be Catalyzed," *Fusion Technol.*, **18**, 136 (1991).

118. E. TABET and A. TENENBAUM, "A Dynamical Model for Cold Fusion in Deuterated Palladium," *Fusion Technol.*, **18**, 143 (1990).

119. J. C. JACKSON and W. A. FOWLER, "Cold Fusion Results Still Unexplained," *Nature*, **339**, 345 (1989).

120. J. L. RUSSELL, Jr., "Plausibility Argument for a Suggested Mechanism for Cold Fusion," *Ann. Nucl. Energy*, **17**, 545 (1990); see also J. L. RUSSELL, Jr., "Virtual Electron Capture in Deuterium," *Ann. Nucl. Energy*, **18**, 75 (1991).

121. G. ANDERMANN, "A New Theoretical Model (Nu-Q*) for Rationalizing Various Events of 'Cold Fusion' in Deuterium Loaded Palladium Cathodes," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 295, National Cold Fusion Institute (1990); see also G. ANDERMANN, "Theoretical Model for Low Temperature Nuclear Events," *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22-27, 1990, p. 107, University of Hawaii.

122. L. FONDA and G. L. SHAW, "Fluctuations and Nonreproducibility in Cold Fusion from Free Quark Catalysis," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

123. H. OHASHI and T. MOROZUMI, "Decoding of Thermal Data in Fleischmann & Pons Paper," *J. Nucl. Sci. Technol.*, **26**, 729 (1989).

124. S. FREEDMAN and D. KRAKAUER, "Biases in Cold Fusion Data," *Nature*, **343**, 703 (1990).

125. R. D. PETRASSO, X. CHEN, K. W. WENZEL, R. R. PARKER, C. K. LI, and C. FIORE, "Problems with the γ-Ray Spectrum in the Fleischmann et al. Experiments," *Nature*, **339**, 183 (1989).

126. G. M. MISKELLY, M. J. HEBEN, A. KUMAR, R. M. PENNER, M. J. SAILOR, and N. S. LEWIS, "Analysis of the Published Calorimetric Evidence for Electrochemical Fusion of Deuterium in Palladium," *Science*, **246**, 793 (1989).

127. M. KEDDAM, "Some Comments on the Calorimetric Aspects of the Electrochemical 'Cold Fusion' by M. Fleischmann and S. Pons," *Electrochim. Acta*, **34**, 995 (1989).

128. E. S. RITTNER and A. MEULENBERG, Jr., "A Chemical Interpretation of Heat Generation in 'Cold Fusion,'" *J. Fusion Energy*, **9**, 377 (1990).

129. G. HORANYI, "Some Basic Electrochemistry and the Cold Nuclear Fusion of Deuterium," *J. Radioanal. Nucl. Chem. Lett.*, **137**, 23 (1989).

130. G. KREYSA, G. MARX, and W. PLIETH, "A Critical Analysis of Electro-Chemical Nuclear Fusion Experiments," *J. Electroanal. Chem.*, **266**, 437 (1989); see also G. KREYSA et al., "Experimental Study of Possible Cold Fusion in Titanium and Palladium Electrodes," *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22-27, 1990, University of Hawaii.

131. R. L. GARWIN, "Consensus on Cold Fusion Still Elusive," *Nature*, **338**, 616 (1989).

132. J. HUIZENGA and N. RAMSEY, Energy Research Advisory Board Report, U.S. Department of Energy (1989); see also D. LINDLEY, "Official Thumbs Down," *Nature*, **342**, 215 (1989).

133. D. R. O. MORRISON, "Review of Cold Fusion," *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22-27, 1990, p. 233, University of Hawaii; see also D. R. O. MORRISON, "The Rise and Decline of Cold Fusion," *Phys. World*, **35** (Feb. 1990).

134. D. LINDLEY, "The Embarrassment of Cold Fusion," *Nature*, **334**, 375 (1990).

135. M. CRIBIER, M. SPIRO, and J. FAVIER, "Conventional Sources of Fast Neutrons in 'Cold Fusion' Experiments," *Phys. Lett. B*, **228**, 163 (1989).

136. M. E. HAYDEN et al., "High Precision Calorimetric Search for Evidence of Cold Fusion Using In Situ Catalytic Recombination of Evolved Gases," *J. Fusion Energy*, **9**, 161 (1990); see also M. E. HAYDEN et al., "High Precision Calorimetric Search for Evidence of Cold Fusion," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989); see also U. NAGER et al., "High Precision Calorimetric Apparatus for Studying Electrolysis Reactions," *Rev. Sci. Instrum.*, **61**, 1506 (1990).

137. J. W. FLEMING, H. H. LAW, J. SAPJETA, P. K. GALLAGHER, and W. F. MAROHN, "Calorimetric Studies of Electrochemical Incorporation of Hydrogen Isotopes into Palladium," *J. Fusion Energy*, **9**, 517 (1990).

138. T. R. JOW, E. PLICHTA, C. WALKER, S. SLANE, and S. GILMAN, "Calorimetric Studies of Deuterated Pd Electrodes," *J. Electrochem. Soc.*, 137, 2473 (1990).

139. H. WIESMANN, "Examination of Cathodically Charged Palladium Electrodes for Excess Heat, Neutron Emission, or Tritium Production," *Fusion Technol.*, **17**, 350 (1990).

140. N. S. LEWIS et al., "Searches for Low-Temperature Nuclear Fusion of Deuterium in Palladium," *Nature*, **340**, 525 (1989); see also N. S. LEWIS and C. A. BARNES, "Calorimetry, Neutron Flux, Gamma Flux, and Tritium Yield from Electrochemically Charged Palladium in D_2O," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989).

141. N. A. GODSHALL, E. P. ROTH, M. J. KELLY, T. R. GUILINGER, and R. I. EWING, "Calorimetric and Thermodynamic Analysis of Palladium-Deuterium Electrochemical Cells," *J. Fusion Energy*, **9**, 229 (1990), and presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989).

142. D. ALBAGLI et al., "Measurement and Analysis of Neutron and Gamma-Ray Emission Rates, Other Fusion Products, and Power in Electrochemical Cells Having Pd Cathodes," *J. Fusion Energy*, **9**, 133 (1990); see also D. ALBAGLI et al., "Measurements of Neutron and Gamma Ray Emission Rates and Calorimetry in Electrochemical Cells Having Pd Cathodes," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989); see also K. KUMAR, I. S. HWANG, R. G. BALLINGER, C. R. DAUWALTER, and A. STECYK, "Analyses of Palladium Cathodes Used for Heavy Water Electrolysis," *Fusion Technol.*, **19**, 178 (1991).

143. L. REDEY, K. M. MYLES, D. DEES, M. KRUMPELT, and D. R. VISSERS, *J. Fusion Energy*, **9**, 249 (1990); see also L. REDEY, K. M. MYLES, D. DEES, M. KRUMPELT, and D. R. VISSERS, "Calorimetric Measurements on Electrochemical Cells with Pd-D and Pd-H Cathodes," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989).

144. D. E. STILWELL, K. H. PARK, and M. H. MILES, "Electrochemical Calorimetric Studies on the Electrolysis of Water and Heavy Water (D_2O)," *J. Fusion Energy*, **9**, 333 (1990).

145. G. R. LONGHURST, T. J. DOLAN, and G. L. HENRIKSEN, "An Investigation of Energy Balances in Palladium Cathode Electrolysis Experiments," *J. Fusion Energy*, **9**, 337 (1990).

146. M. CHEMLA, J. CHEVALET, R. BURY, and M. PERIE, "Experimental Investigation of Thermal and Radiation Effects Induced by Deuterium Discharge at the Palladium Electrode," *J. Electroanal. Chem.*, 277, 93 (1990); see also M. CHEMLA, J. CHEVALET, and R. BURY, "Effets Thermiques associés à la décharge electrochimique de l'hydrogéne et du deutérium sur palladium," *C. R. Acad. Sci. Paris, Ser. II*, 309, 987 (1989).

147. D. R. McCRACKEN et al., "In Search of Nuclear Fusion in Electrolytic Cells and in Metal/Gas Systems," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989) and *J. Fusion Energy*, **9**, 121 (1990).

148. R. G. KEESING, R. C. GREENHOW, and M. D. COHLER, "Thermal, Thermoelectric, and Cathode Poisoning Effects in Cold Fusion Experiments," *Fusion Technol.*, **19**, 375 (1991).

149. D. E. WILLIAMS et al., "Upper Bounds on 'Cold Fusion' in Electrolytic Cells," *Nature*, **342**, 375 (1989).

150. R. D. ARMSTRONG, E. A. CHARLES, I. FELLS, L. MOLYNEUX, and M. TODD, "Some Aspects of Thermal Energy Concentration During the Electrolysis of D_2O Using a Palladium Cathode," *Electrochim. Acta*, **34**, 1319 (1989).

151. H.-S. BOSCH, G. A. WURDEN, J. GERNHARDT, F. KARGER, and J. PERCHERMEIER, "Electrochemical Cold Fusion Trials at IPP Garching," *J. Fusion Energy*, **9**, 165 (1990).

152. T. R. GUILINGER et al., "Investigation of Fusion Reactions in Palladium and Titanium Tritide Using Galvanostatic, Coulometric, and Hydrogen Permeation Techniques," *J. Fusion Energy*, **9**, 299 (1990).

153. R. ILIĆ, J. RANT, T. SUTEJ, M. DOBERSEK, E. KRISTOF, J. SKVARC, and M. KOZELJ, "Investigations of the Deuterium-Deuterium Fusion Reaction in Cast, Annealed, and Cold-Rolled Palladium," *Fusion Technol.*, **18**, 505 (1990).

154. S. H. FALLER, R. W. HOLLOWAY, and S. C. LEE, "Investigation of Cold Fusion in Heavy Water," *J. Radioanal. Nucl. Chem. Lett.*, **137**, 9 (1989).

155. L. L. ZAHM, A. C. KLEIN, S. E. BINNEY, J. N. REYES, Jr., J. F. HIGGINBOTHAM, and A. H. ROBINSON, "Experimental Investigations of the Electrolysis of D_2O Using Palladium Cathodes and Platinum Anodes," *J. Electroanal. Chem.*, **281**, 313 (1990); see also A. C. KLEIN, "Anomalous Heat Output from Pd Cathodes Without Detectable Nuclear Products," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

156. J. D. PORTER et al., "Limits on Electromagnetic and Particle Emission for Palladium-D_2O Electrolytic Cells," *J. Fusion Energy*, **9**, 319 (1990); see also LBL-27521, Lawrence Berkeley Laboratory (1989).

157. K. A. RITLEY, K. G. LYNN, P. DULL, M. H. WEBER, M. CARROLL, and J. J. HURST, "A Search for Tritium Production in Electrolytically Deuterided Palladium," *Fusion Technol.*, **19**, 192 (1991); see also K. RITLEY, H. WIESMANN, P. DULL, K. LYNN, and M. WEBER, "A Search for Cold Fusion Signatures in Cathodically Charged Palladium," *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22-27, 1990, p. 61, University of Hawaii.

158. K. E. REHM, W. KUTSCHERA, and G. J. PERLOW, "Search for Protons from the $^2H(d,p)^3H$ Reaction in an Electrolytic Cell with Pd-Pt Electrodes," *Phys. Rev. C*, **41**, 45 (1990).

159. G. SCHRIEDER, H. WIPF, and A. RICHTER, "Search for Cold Nuclear Fusion in Palladium-Deuteride," *Z. Phys. B*, **76**, 141 (1989).

160. P. B. PRICE, "Search for High-Energy Ions from Fracture of LiD Crystals," *Nature*, **343**, 542 (1990).

161. U. R. SUNDQVIST, P. HAKONSSON, A. HEDIN, R. V. BUCUR, B. JOHNSSON, and R. WAPPLING, "On the Observation of Charged Particles in Cold Fusion," *Phys. Scripta*, **40**, 303 (1989).

162. J. F. ZIEGLER et al., "Electrochemical Experiments in Cold Nuclear Fusion," *Phys. Rev. Lett.*, **62**, 2929 (1989).

163. J. A. KNAPP, T. R. GUILINGER, M. J. KELLY, B. L. DOYLE, D. WALSH, and S. S. TSAO, "Thin-Foil Electrochemical Cells: High-Sensitivity Fusion Tests and In-Situ Ion Beam Measurements of Deuterium Loading," *J. Fusion Energy*, **9**, 371 (1990).

164. M. H. SALAMON et al., "Limits on the Emission of Neutrons, Gamma-Rays, Electrons and Protons from Pons/Fleischmann Electrolytic Cells," *Nature*, **344**, 401 (1990).

165. P. CHAMBERS, J. M. ERIDON, and K. S. GRABOWSKI, "Upper Limit on Cold Fusion in Thin Palladium Films," *Phys. Rev.*, **41**, 5388 (1990).

166. S. M. MYERS, D. M. FOLLSTAEDT, J. E. SCHIRBER, and P. M. RICHARDS, "Search for Cold Fusion at D/Pd > 1 Using Ion Implantation," *J. Fusion Energy*, **9**, 263 (1990); see also S. M. MYERS, D. M. FOLLSTAEDT, J. E. SCHIRBER, and P. M. RICHARDS, "Search for Cold Fusion in Superstoichiometric Palladium Deuteride Using Ion Implantation," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989).

167. R. BEHRISCH, W. MOLLER, J. ROTH, and B. M. U. SCHERZER, "Search for Fusion Reactions Between Deuterium Atoms Implanted into Titanium," *Nucl. Fusion*, **29**, 1187 (1989).

168. S. W. BARWICK, P. B. PRICE, W. T. WILLIAMS, and J. D. PORTER, "Search for 0.8 MeV ^3He Nuclei Emitted from Pd and Ti Exposed to High Pressure D_2," *J. Fusion Energy*, **9**, 273 (1990); see also P. B. PRICE, S. W. BARWICK, W. T. WILLIAMS, and J. D. PORTER, "Search for Energetic-Charged Particle Emission from Deuterated Ti and Pd Foils," *Phys. Rev. Lett.*, **63**, 1926 (1989).

169. J. R. MORREY et al., "Measurements of Helium in Electrolyzed Palladium," *Fusion Technol.*, **18**, 659 (1990).

170. S. GOTTESFELD et al., "Experiments and Nuclear Measurements in Search of Cold Fusion Processes," *J. Fusion Energy*, **9**, 287 (1990); see also R. E. ANDERSON et al., "Neutron Measurements in Search of Cold Fusion," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

171. I. F. SILVERA and F. MOSHARY, "Deuterated Palladium at Temperatures from 4.3 to 400 K and Pressures to 105 kbar: Search for Cold Fusion," *Phys. Rev. B*, **42**, 9143 (1990).

172. W. HAJDAS, ST. KISTRYN, J. LANG, J. SROMICKI, B. JENNY, and P. WACHTER, "Search for Cold-Fusion Events," *Solid State Comm.*, **72**, 309 (1989).

173. M. GAI et al., "Upper Limits on Neutron and Gamma-Ray Emission from Cold Fusion," *Nature*, **340**, 29 (1989), and presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989); see also *J. Fusion Energy*, **9**, 217 (1990).

174. S. M. BENNINGTON, R. S. SOKHI, P. R. STONADGE, D. K. ROSS, M. J. BENHAM, and T. D. BEYNON, "A Search for Emission of X-Rays from Electrolytically Charged Palladium-Deuterium," *Electrochim. Acta*, **34**, 1323 (1989).

175. R. FLEMING, F. DONAHUE, S. MANCINI, G. KNOLL, and B. HEUSER, "An Attempt to Measure Characteristic X-Rays from Cold Fusion," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989).

176. M. R. DEAKIN, J. D. FOX, K. W. KEMPER, E. G. MYERS, W. N. SHELTON, and J. G. SKOFRONICK, "Search for Cold Fusion Using X-Ray Detection," *Phys. Rev.*, **40**, R1851 (1989).

177. R. D. PETRASSO, X. CHEN, K. W. WENZEL, R. R. PARKER, C. K. LI, and C. FIORE, "Reply to M. Fleischmann, S. Pons, M. Hawkins, and R. J. Hoffman, 'Measurement of γ-Rays from Cold Fusion,'" *Nature*, **339**, 667 (1989).

178. T. G. DIGNAN, M. C. BRUINGTON, R. T. JOHNSON, and R. W. BLAND, "A Search for Neutrons from Fusion in a Highly Deuterated Cooled Palladium Thin Film," *J. Fusion Energy*, **9**, 469 (1990).

179. R. A. HENDERSON, K. R. CZERWINSKI, H. L. HALL, K. T. LESKO, E. B. NORMAN, B. SUR, and D. C. HOFFMAN, "More Searches for Cold Fusion," *J. Fusion Energy*, **9**, 475 (1990).

180. J. C. HILL et al., "Search for Cold Fusion Using Pd-D_2O Cells and Ti-D Mixtures," *J. Fusion Energy*, **9**, 305 (1990).

181. D. ABRIOLA et al., "Examination of Nuclear Measurement Conditions in Cold Fusion Experiments," *J. Electroanal. Chem.*, **265**, 355 (1989).

182. A. BAURICHTER et al., "Search for Cold Fusion in Palladium," *Z. Phys. B*, **76**, 1 (1989).

183. J. P. BRIAND, J. DWEYNCK, P. CHEVALLIER, and J. L. BOBIN, "Cold Fusion: An Alternative Diagnostic," *Nucl. Instrum. Methods Phys. Res. A*, **285**, 487 (1989); see also J. P. BRIAND, G. BAN, M. FROMENT, M. KEDDAM, and F. ABEL, "Cold Fusion Rates in Titanium Foils," *Phys. Lett. A.*, **145**, 187 (1990).

184. J. R. SOUTHON, J. W. STARK, J. S. VOGEL, and J. C. WADDINGTON, "Upper Limits for Neutron Emission from Cold *d-t* Fusion," *Phys. Rev. C*, **41**, R1899 (1990).

185. M. A. BUTLER, D. S. GINLEY, J. E. SCHIRBER, and R. I. EWING, "High-Sensitivity Search for Neutrons During Electrochemical Reactions," *Fusion Technol.*, **16**, 388 (1989); see also R. I. EWING, "High-Sensitivity Neutron Detectors Used at Sandia National Laboratories to Monitor and Diagnose 'Cold Fusion' Experiments: Negative Results," *J. Fusion Energy*, **9**, 473 (1990); see also R. I. EWING, M. A. BUTLER, J. E. SCHIRBER, and D. S. GINLEY, "Negative Results and Positive Artifacts Observed in a Comprehensive Search for Neutrons from 'Cold Fusion' Using a Multidetector System Located Underground," *Fusion Technol.*, **16**, 404 (1989); see also R. I. EWING, M. A. BUTLER, J. E. SCHIRBER, and D. S. GINLEY, "A Sensitive Multi-Detector Neutron Counter Used to Monitor 'Cold Fusion' Experiments in an Underground Laboratory: Negative Results and Positive Artifacts," *Nucl. Sci.*, **37**, 1165 (1990).

186. D. ALBER et al., "Search for Neutrons from 'Cold Nuclear Fusion,'" *Z. Phys. A*, **333**, 319 (1989).

187. J. J. G. DUROCHER et al., "A Search for Evidence of Cold Fusion in the Direct Implantation of Palladium and Indium With Deuterium," *Can. J. Phys.*, **67**, 624 (1989).

188. J. E. SCHIRBER, M. A. BUTLER, D. S. GINLEY, and R. I. EWING, "Search for Cold Fusion in High-Pressure D_2-Loaded Titanium and Palladium Metal and Deuteride," *Fusion Technol.*, **16**, 397 (1989).

189. H. WERLE, G. FIEG, J. LEBKUCHER, and M. MÖSCHKE, "Trials to Induce Neutron Emission from a Titanium-Deuterium System," *Fusion Technol.*, **16**, 391 (1989).

190. R. S. RAGHAVAN, L. C. FELDMAN, M. M. BROER, J. S. KRAUS, A. C. JAMES, and D. W. MURPHY, "Search for Neutrons from Cold Fusion in PdD," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23–25, 1989, LA-11686-C, Los Alamos National Laboratory (1989); see also M. M. BROER, L. C. FELDMAN, A. C. JAMES, J. S. KRAUS, and R. S. RAGHAVAN, "Search for Neutrons from Deuterium-Deuterium Nuclear Reactions in Electrolytically Charged Palladium," *Phys. Rev. C*, **40**, R1559 (1989).

191. F. M. MUELLER et al., "Search for Fusion in Deuterated Transition Metals: Dynamical Pressures Above 1 Megabar," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23–25, 1989, LA-11686-C, Los Alamos National Laboratory (1989).

192. R. ALEKSAN et al., "Limits on Electrochemically Induced Fusion of Deuterium by Neutron Flux Measurement," *Phys. Lett. B*, **234**, 389 (1990).

193. D. ABERDAM et al., "Limits on Neutron Emission Following Deuterium Absorption into Palladium and Titanium," *Phys. Rev. Lett.*, **65**, 1196 (1990).

194. A. C. EHRLICH, D. J. GILLESPIE, and G. N. KAMM, "A Search for Neutrons in Single-Phase Palladium-Deuterium," *Fusion Technol.*, **16**, 529 (1989); see also D. J. GILLESPIE, G. N. KAMM, A. C. EHRLICH, and P. L. MART, "A Search for Anomalies in the Palladium-Deuterium System," *Fusion Technol.*, **16**, 526 (1989).

195. M. CHEN et al., "Measurements of Neutron Emission Induced by Muons Stopped in Metal Deuteride Targets," *J. Fusion Energy*, **9**, 155 (1990).

196. F. BOTTER et al., "Search for Emission of Neutrons from a Palladium-Deuterium System," *Phys. Lett. B*, **232**, 536 (1989).

197. E. KASHY et al., "Search for Neutron Emission from Deuterium-Loaded Palladium," *Phys. Rev. C*, **40**, R1 (1989).

198. J. D. DAVIES, G. J. PYLE, and G. T. A. SQUIER, "Search for 2.5 MeV Neutrons from D_2O Electrolytic Cells Stimulated by High-Intensity Muons and Pions," *Nuovo Cimento A*, **103**, 155 (1990).

199. F. BESENBACHER, B. B. NIELSEN, P. HORNSHØJ, E. LAEGSGAARD, and N. RUD, "Search for Cold Fusion in Plasma-Charged Pd-D and Ti-D," *J. Fusion Energy*, **9**, 315 (1990).

200. G. N. KAMM, A. C. EHRLICH, D. J. GILLESPIE, and W. J. POWERS, "Search for Neutrons from a Titanium-Deuterium System," *Fusion Technol.*, **16**, 401 (1989).

201. S. BLAGUS et al., "Search for Neutron Production During Heavy Water Electrolysis on Palladium Electrodes," *Z. Phys. A*, **333**, 321 (1989).

202. K. NAGAMINE et al., "Evidence Against Condensed Matter Fusion Induced by Cosmic-Ray Muons," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989).

203. B. BALKE et al., "Limits on Neutron Emission from Cold Fusion in Metal Hydrides," *Phys. Rev.*, **42**, 30 (1990).

204. R. B. CAMPBELL and L. J. PERKINS, "A Study of 'Cold Fusion' in Deuterated Titanium Subjected to High Current Densities," *Fusion Technol.*, **16**, 383 (1989).

205. S. AIELLO, E. D. FILIPPO, G. LANZANO, S. L. NIGRO, and A. PAGANO, "Nuclear Fusion Experiment in Palladium Charged by Deuterium Gas," *Fusion Technol.*, **18**, 115 (1990).

206. M. BITTNER et al., "Method for Investigation of Fusion Reactions in Condensed Matter," *Fusion Technol.*, **18**, 120 (1990).

207. L. BRUSCHI, M. SANTINI, G. TORZO, and G. NARDELLI, "Search for Neutron Emission From a Deuterium-Titanium System," *Europhys. Lett.*, **10**, 303 (1989).

208. E. KUZMANN et al., "Investigation on the Possibility of Cold Nuclear Fusion in Fe-Zr Amorphous Alloy," *J. Radioanal. Nucl. Chem. Lett.*, **137**, 243 (1989).

209. W. VIELSTICH, T. IWASITA, H. von BUTTLAR, K. FARZIN, and K. UEBELGUNN, "Search for Neutrons from Controlled Deuterium Concentrations in Palladium," *J. Electroanal. Chem.*, **303**, 211 (1991).

210. V. B. BRUDANIN et al., "Does Cold Nuclear Fusion Exist?," *Phys. Lett. A*, **146**, 347 (1990); see also V. B. BRUDANIN et al., "Once More About Cold Nuclear Fusion," *Phys. Lett. A*, **146**, 351 (1990).

211. M. KOCSIS et al., "Search for Neutrons from Cold Nuclear Fusion," *J. Radioanal. Nucl. Chem. Lett.*, **145**, 327 (1990).

212. S. PONS and M. FLEISCHMANN, "Calorimetric Measurements of the Palladium/Deuterium System: Fact and Fiction," *Fusion Technol.*, **17**, 669 (1990); see also S. PONS and M. FLEISCHMANN, "Calorimetry of Palladium-Deuterium Systems," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 1, National Cold Fusion Institute (1990); see also M. FLEISCHMANN, S. PONS, M. W. ANDERSON, L. J. LI, and M. HAWKINS, "Calorimetry of the Palladium-Heavy Water System," *J. Electroanal. Chem.*, **298**, 293 (1990).

213. M. C. H. McKUBRE et al., "Calorimetry and Electrochemistry in the D.Pd System," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 20, National Cold Fusion Institute (1990); see also J. CHAO et al., "Three-Dimensional Computer Simulation of an Isoperibolic Calorimeter for Cold Fusion Experiments," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 308, National Cold Fusion Institute (1990).

214. A. J. APPLEBY, Y. J. KIM, O. J. MURPHY, and S. SRINIVASAN, "Anomalous Calorimetric Results During Long-Term Evolution of Deuterium on Palladium from Alkaline Deuteroxide Electrolyte," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 32, National Cold Fusion Institute (1990); see also S. SRINIVASAN, Y. J. KIM, O. J. MURPHY, C. R. MARTIN, and A. J. APPLEBY, "Evidence for Excess Heat Generation Rates During Electrolysis of D_2O in LiOD Using a Palladium Cathode: A Microcalorimetric Study," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989).

215. D. P. HUTCHINSON, C. A. BENNET, R. K. RICHARDS, J. BULLOCK IV, and G. L. POWELL, "Initial Calorimetry Experiments in the Physics Division at ORNL," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, National Cold Fusion Institute (1990); see also ORNL/TM-11356, Oak Ridge National Laboratory; see also #Y/DZ0490; see also G. L. POWELL, J. S. BULLOCK IV, R. L. HALLMAN, P. J. HORTON, and D. P. HUTCHINSON, "The Preparation of Palladium for Cold Fusion Experiments," *J. Fusion Energy*, **9**, 355 (1990).

216. M. SCHREIBER, T. M. GÜR, G. LUCIER, J. A. FERRANTE, J. CHAO, and R. A. HUGGINS, "Recent Measurements of Excess Energy Production in Electrochemical Cells Containing Heavy Water and Palladium," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 44, National Cold Fusion Institute (1990); see also M. SCHREIBER, T. M. GÜR, G. LUCIER, J. A. FERRANTE, and R. A. HUGGINS, "Recent Experimental Results on the Thermal Behavior of Electrochemical Cells in the Hydrogen-Palladium and Deuterium-Palladium Systems," *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22-27, 1990, p. 71, University of Hawaii; see also T. M. GÜR, M. SCHREIBER, G. LUCIER, J. A. FERRANTE, J. CHAO, and R. A. HUGGINS, "Experimental Considerations in

Electrochemical Isoperibolic Calorimetry," *Proc. Ist Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 82, National Cold Fusion Institute (1990); see also A. BELZNER, U. BISCHLER, S. CROUCH-BAKER, T. M. GÜR, M. SCHREIBER, and R. A. HUGGINS, "Two Fast Mixed Conductor Systems: Deuterium and Hydrogen and Palladium—Thermal Measurements and Experimental Considerations," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989), and *J. Fusion Energy*, **9**, 219 (1990).

217. C. D. SCOTT, J. E. MROCHEK, T. C. SCOTT, G. E. MICHAELS, E. NEWMAN, and M. PETEK, "Measurement of Excess Heat and Apparent Coincident Increases in the Neutron and Gamma-Ray Count Rates During the Electrolysis of Heavy Water," *Fusion Technol.*, **18**, 103 (1990); see also "Initiation of Excess Power and Possible Products of Nuclear Interactions During the Electrolysis of Heavy Water," *Proc. Ist Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 164, National Cold Fusion Institute (1990); see also TM-11322, Oak Ridge National Laboratory; see also C. D. SCOTT, E. GREENBAUM, G. E. MICHAELS, J. E. MROCHEK, E. NEWMAN, M. PETEK, and T. C. SCOTT, "Preliminary Investigation of Possible Low Temperature Fusion," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989), and *J. Fusion Energy*, **9**, 115 (1990).

218. L. J. DROEGE and T. F. DROEGE, "A Zero Gradient Calorimeter for the Measurement of Anomalous Heat from the Electrolysis of Deuterated Metals," *Proc. Ist Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 229, National Cold Fusion Institute (1990); see also T. F. DROEGE, "A Discussion of an H_2O Run Which Followed Operation with D_2O," SMATN-90-2, Environmental Optics Corporation, Batavia, Illinois (Apr. 1990).

219. D. GOZZI, P. L. CIGNINI, L. PETRUCCI, M. TOMELLINI and G. de MARIA, "Evidences for Associated Heat Generation and Nuclear Products Release in Palladium Heavy-Water Electrolysis," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory (1989), and *Nuovo Cimento*, **103**, 143 (1990); see also D. GOZZI et al., "Nuclear and Thermal Effects During Electrolytic Reduction of Deuterium at Palladium Cathode," *J. Fusion Energy*, **9**, 241 (1990).

220. R. C. KAINTHLA et al., "Sporadic Observation of the Fleischmann-Pons Effect," *Electrochim. Acta*, **34**, 1315 (1989); see also O. A. VELEV and R. C. KAINTHLA, "Heat Flow Calorimeter with a Personal-Computer-Based Data Acquisition System," *Fusion Technol.*, **18**, 351 (1990).

221. R. T. BUSH, "Cold 'Fusion': The Transmission Resonance Model Fits Data on Excess Heat, Predicts Optimal 'Trigger' Points, and Suggests Nuclear Reaction Scenarios," *Fusion Technol.*, **19**, 313 (1991); see also R. T. BUSH, "The TRM (Transmission Resonance Model) for Cold 'Fusion' Fits Calorimetric Data on the Pons-Fleischmann Effect and Suggests Solutions to Nuclear 'Anomalies,'" *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22-27, 1990, University of Hawaii; see also R. D. EAGLETON and R. T. BUSH, "Design Considerations for Palladium Electrodes as Suggested by a Deuterium Cluster Model for Cold Nuclear Fusion," *J. Fusion Energy*, **9**, 359 (1990); see also R. T. BUSH and R. D. EAGLETON, "'Cold Nuclear Fusion': A Hypothetical Model to Probe an Elusive Phenomenon," *J. Fusion Energy*, **9**, 397 (1990).

222. B. Y. LIAW, P.-L. TAO, P. TURNER, and B. E. LIEBERT, "Elevation Temperature Excess Heat Production Using Molten Salt Electrochemical Techniques," *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22-27, 1990, University of Hawaii; see also B. Y. LIAW, P.-L. TAO, P. TURNER, and B. E. LIEBERT, "Elevated-Temperature Excess Heat Production in a Pd-D System," to be published in *J. Electroanal. Chem.*

223. C. S. YANG, C. Y. LIANG, T. P. PERNG, L. J. YUAN, C. M. WAN, and C. C. WAN, "Observations of Excess Heat and Tritium on Electrolysis of D_2O," *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22-27, 1990, p. 95, University of Hawaii.

224. A. B. KARABUT, Ya. R. KUCHEROV, and I. B. SAVATIMOVA, "Cold Fusion Observations at Gas-Discharge Device Cathode," presented at Specialists' Conf. Nuclear Power Engineering in Space, Obninsk, USSR, May 15-19, 1990.

225. V. C. NONINSKI and C. I. NONINSKI, "Determination of the Excess Energy Obtained During the Electrolysis of Heavy Water," *Fusion Technol.*, **19**, 364 (1991).

226. R. A. ORIANI, J. C. NELSON, S. LEE, and J. H. BROADHURST, "Calorimetric Measurements of Excess Power Output During the Cathodic Charging of Deuterium into Palladium," *Fusion Technol.*, **18**, 652 (1990).

227. K. S. V. SANTHANAM, J. RANGARAJAN, O'N. BRAGANZA, S. K. HARAM, N. M. LIMAYE, and K. C. MANDAL, "Electrochemically Initiated Cold Fusion of Deuterium," *Indian J. Technol.*, **27**, 175 (1989).

228. B. F. BUSH, J. J. LAGOWSKI, M. H. MILES, and G. S. OSTROM, "Helium Production During the Electrolysis of D_2O in Cold Fusion," *J. Electroanal. Chem.*, **304**, 271 (1991).

229. Y. ARATA and Y. ZHANG, "Achievement of an Intense Cold Fusion Reaction," *Fusion Technol.*, **18**, 95 (1990), and *Proc. Jpn. Acad. B*, **66**, 1 (1990); see also Y. ARATA and Y. ZHANG, "Cold Fusion Caused by a Weak 'On-Off' Effect," *Proc. Jpn. Acad. B*, **66**, 33 (1990).

230. D. LEWIS and K. SKÖLD, "A Phenomenological Study of the Fleischmann-Pons Effect," *J. Electroanal. Chem.*, **294**, 275 (1990).

249. S. MILJANIĆ, N. JEVTIĆ, S. PEŠIĆ, M. NINKOVIĆ, D. NIKOLIĆ, M. JOSIPOVIĆ, L. PETKOVSKA, and S. BAČIĆ, "An Attempt to Replicate Cold Fusion Claims," *Fusion Technol.*, **18**, 340 (1990).

250. J. CHÊNE and A. M. BRASS, "Tritium Production During the Cathode Discharge of Deuterium on Palladium," *J. Electroanal. Chem.*, **280**, 199 (1990).

251. N. JEVTIĆ, S. MILJANIĆ, S. PEŠIĆ, J. DOBROSAVLJEVIĆ, and J. PUPEZIN, "The Change in the Isotope Ratio and a Spectroscopic Analysis of the Gas from Gas Load Experiments," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990.

252. K. L. WOLF, N. PACKHAM, J. SHOEMAKER, F. CHENG, and D. LAWSON, "Neutron Emission and Tritium Content Associated with Deuterium-Loaded Palladium and Titanium Metals," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23–25, 1989, LA-11686-C, Los Alamos National Laboratory; see also K. L. WOLF, N. J. C. PACKHAM, D. LAWSON, J. SHOEMAKER, F. CHENG, and J. C. WASS, "Neutron Emission and the Tritium Content Associated with Deuterium-Loaded Palladium and Titanium Metals," *J. Fusion Energy*, **9**, 105 (1990).

253. N. WADA and K. NISHIZAWA, "Nuclear Fusion in Solids," *Jpn. J. Appl. Phys.*, **28**, 2017 (1989); see also Y. E. KIM, "Neutron Burst from a High-Voltage Discharge Between Pd Electrodes in D_2 Gas," *Fusion Technol.*, **18**, 680 (1990).

254. E. YAMAGUCHI and T. NISHIOKA, "Cold Nuclear Fusion Induced by Controlled Out-Diffusion of Deuterons in Palladium," *Jpn. J. Appl. Phys.*, **24**, L666 (1990), and *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990.

255. F. CELANI et al., "Further Measurements on Electrolytic Cold Fusion with D_2O and Palladium at Gran Sasso Laboratory," *Fusion Technol.*, **17**, 718 (1990); see also F. CELANI, "Measurements in the Gran Sasso Laboratory," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990; see also F. CELANI, V. di STEFANO, S. PACE, and S. BIANCO, "Preliminary Measurements on Electrolytic Cold Fusion at Underground Gran Sasso Laboratory," LNF-89/048(P), Frascati Research Centre.

256. C. SÁNCHEZ, J. SEVILLA, B. ESCARPIZO, F. J. FERNÁNDEZ, and J. CANIZARES, "Nuclear Products Detection During Electrolysis of Heavy Water with Ti and Pt Electrodes," *Solid State Comm.*, **71**, 1039 (1989); see also F. FERNÁNDEZ, J. SEVILLA, B. ESCARPIZO, and C. SÁNCHEZ, "Nuclear Effects in Electrolytically Deuterated Ti and Pd," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990; see also J. SEVILLA, F. FERNÁNDEZ, B. ESCARPIZO, and C. SÁNCHEZ, "Some Characteristics of Titanium and Palladium Samples Used in Cold Fusion Experiments," *Fusion Technol.*, **19**, 188 (1991).

257. A. BERTIN et al., "Experimental Evidence of Cold Nuclear Fusion in a Measurement Under the Gran Sasso Massif," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23–25, 1989, LA-11686-C, Los Alamos National Laboratory, and *Nuovo Cimento*, **101**, 997 (1989); see also A. BERTIN et al., "First Experimental Results at the Gran Sasso Laboratory on Cold Nuclear Fusion in Titanium Electrodes," *J. Fusion Energy*, **9**, 209 (1990).

258. A. TAKAHASHI, T. TAKEUCHI, T. IIDA, and M. WATANABE, "Emission of 2.45 MeV and Higher Energy Neutrons from D_2O-Pd Cell Under Biased-Pulse Electrolysis," *J. Nucl. Sci. Technol.*, **27**, 663 (1990); see also A. TAKAHASHI, "Neutron Spectra from D_2O-Pd Cells with Pulsed Electrolysis," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990; see also A. TAKAHASHI, T. IIDA, F. MAEKAWA, H. SUGIMOTO, and S. YOSHIDA, "Windows of Cold Nuclear Fusion and Pulsed Electrolysis Experiments," *Fusion Technol.*, **19**, 380 (1991).

259. H. O. MENLOVE et al., "Measurement of Neutron Emission from Ti Plus D_2 Gas," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23–25, 1989, LA-11686-C, Los Alamos National Laboratory, and *J. Fusion Energy*, **9**, 215 (1990); see also H. O. MENLOVE et al., "Measurement of Neutron Emission from Ti and Pd in Pressurized D_2 Gas and D_2O Electrolysis Cells," *J. Fusion Energy*, **9**, 495 (1990); see also H. MENLOVE et al., "Reproducible Neutron Emission Measurements from Ti Metal in Pressurized D_2 Gas," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990.

260. S. E. JONES, "Investigation of Anomalous Particle Emission in Solid/Deuterium Systems," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990.

261. K. WOLF, "Neutron Emission Studies," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990.

262. R. ZHU, "Experimental Study on Anomalous Neutron Production in Deuterium/Solid Systems," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990.

263. J. R. GRANADA et al. "Neutron Measurements on (Pd-D_2O) Electrolytic Cells," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990; see also J. R. GRANADA, R. E. MAYER, P. C. FLORIDO, V. H. GILLETTE, and S. E. GOMEZ, "Neutron Measurements on Electrolytic Cells (Pd-D_2O) Performed Under Very Low Background Conditions," *J. Nucl. Sci. Technol.*, **27**, 379 (1990); see also J. R. GRANADA et al., "Thermal Neutron Measurements on Electrolytic Cells with Deuterated Palladium Cathodes Subjected to a Pulsed Current," *J. Nucl. Sci. Technol.*, **27**, 30 (1990).

231. M. H. MILES, K. H. PARK, and D. E. STILWELL, "Electrochemical Calorimetric Evidence for Cold Fusion in the Palladium-Deuterium System," *J. Electroanal. Chem.*, **296**, 241 (1990).

232. S. GURUSWAMY and M. E. WADSWORTH, "Metallurgical Aspects in Cold Fusion Experiments," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 314, National Cold Fusion Institute (1990).

233. S. SZPAK, P. A. MOSIER-BOSS, and J. J. SMITH, "On the Behavior of Pd Deposited in the Presence of Evolving Deuterium," *J. Electroanal. Chem.*, **302**, 255 (1991).

234. N. HUANG, Q. H. GAO, B. Y. LIAW, and B. E. LIEBERT, "A Flow Calorimeter Used in Duplication of 'Cold Fusion,'" presented at Special Session Cold Fusion, Electrochemical Society Mtg., Hollywood, Florida, October 20, 1989.

235. P. K. IYENGAR and M. SRINIVASAN, "Overview of BARC Studies in Cold Fusion," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 62, National Cold Fusion Institute (1990); see also P. K. IYENGAR et al., "Bhabha Atomic Research Centre Studies in Cold Fusion," *Fusion Technol.*, **18**, 32 (1990), and BARC-1500, Bhabha Atomic Research Centre (1989).

236. R. K. ROUT, M. SRINIVASAN, A. SHYAM, and V. CHITRA, "Detection of High Tritium Activity on the Central Titanium Electrode of a Plasma Focus Device," *Fusion Technol.*, **19**, 391 (1991); see also M. SRINIVASAN et al., "Observations of Tritium in Gas/Plasma Loaded Titanium Samples," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

237. N. PACKHAM, K. L. WOLF, J. C. WASS, R. C. KAINTHLA, and J. BOCKRIS, "Production of Tritium from D_2O Electrolysis at a Pd Cathode," *J. Electroanal. Chem.*, **270**, 451 (1989).

238. E. STORMS and C. TALCOTT, "A Study of Electrolytic Tritium Production," *Fusion Technol.*, **17**, 680 (1990); see also E. STORMS and C. TALCOTT, "Electrolytic Tritium Production," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, National Cold Fusion Institute (1990).

239. G. SCHOESSOW, University of Florida, Private Communication (1989).

240. T. N. CLAYTOR, P. A. SEEGER, R. K. ROHWER, D. G. TUGGLE, and W. R. DOTY, "Tritium and Neutron Measurements of a Solid-State Cell," presented at Workshop Anomalous Effects in Deuterated Metals, Washington, D.C., October 16-18, 1989; see also T. N. CLAYTOR, D. G. TUGGLE, H. O. MENLOVE, P. A. SEEGER, W. R. DOTY, and R. K. ROHWER, "Tritium and Neutron Measurements from Deuterated Pd-Si," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

241. C. CHIEN and T. C. HUANG, "Tritium Production by Electrolysis of D_2O," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

242. R. ALQUASMI, K. ALBERTSEN, H. G. CHOTKA, and H. J. SCHALLER, "Neutrons and Tritium from Cold Fusion in Pd-D," presented at Special Session Cold Fusion, Electrochemical Society Mtg., Hollywood, Florida, October 20, 1989.

243. R. ADZIC, D. GERVASIO, I. BAE, B. CAHAN, and E. YEAGER, "Investigation of Phenomena Related to D_2O Electrolysis at a Palladium Cathode," presented at Special Session Cold Fusion, Electrochemical Society Mtg., Hollywood, Florida, October 20, 1989, and *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 261, National Cold Fusion Institute (1990).

244. A. De NINNO et al., "Emission of Neutrons as a Consequence of Titanium-Deuterium Interface," *Nuovo Cimento*, **101A**, 841 (1989); see also A. De NINNO, C. PONTORIERI, F. SCARAMUZZI, and P. ZEPPA, "Emission of Neutron Bursts from a Titanium-Deuterium Gas System in a High-Efficiency Low-Background Experimental Setup," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990; see also F. D'AMATO, A. De NINNO, F. SCARAMUZZI, P. ZEPPA, and C. PONTORIERI, "Search for Nuclear Phenomena by the Interaction Between Titanium and Deuterium," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 17, National Cold Fusion Institute (1990); see also A. De NINNO et al., "Neutron Emission from a Titanium-Deuterium System," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory.

245. J. O'M. BOCKRIS, G. H. LIN, R. C. KAINTHLA, N. J. PACKHAM, and O. VELEV, "Does Tritium Form at Electrodes by Nuclear Reactions," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28-31, 1990, p. 137, National Cold Fusion Institute (1990).

246. T. C. KAUSHIK et al., "Preliminary Report on Direct Measurement of Tritium in Liquid Nitrogen Treated TiD_x Chips," *Indian J. Technol.*, **28**, 667 (1990).

247. D. GOZZI et al., "Neutron and Tritium Evidences in the Electrolytic Reduction of Deuterium on Pd Electrodes," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

248. P. G. SONA et al., "Preliminary Tests on Tritium and Neutrons in Cold Nuclear Fusion Within Palladium Cathodes," *Fusion Technol.*, **17**, 713 (1990).

264. J. JORNE, "Neutron and Gamma-Ray Emission from Palladium Deuteride Under Supercritical Conditions," *Fusion Technol.*, **19**, 371 (1991).

265. T. SATO, M. OKAMOTO, P. KIM, Y. FUJII, and O. AIZAWA, "Detection of Neutrons in Electrolysis of Heavy Water," *Fusion Technol.*, **19**, 357 (1991).

266. A. G. GU, R. K. F. TENG, M. S. MILLER, and W. J. SPROUSE, "Preliminary Experimental Study on Cold Fusion Using Deuterium Gas and Deuterium Plasma in the Presence of Palladium," *Fusion Technol.*, **16**, 248 (1989); see also A. G. GU, R. K. F. TENG, M. S. MILLER, and W. J. SPROUSE, "Experimental Study on Cold Fusion Using Deuterium Gas and Deuterium Ion Beam with Palladium," *J. Fusion Energy*, **9**, 329 (1990).

267. S. E. JONES et al., "Anomalous Nuclear Reactions in Condensed Matter: Recent Results and Open Questions," *J. Fusion Energy*, **9**, 199 (1990).

268. M. PRELAS, F. BOODY, W. GALLAHER, E. LEAL-QUIROS, D. MENCIN, and S. TAYLOR, "Cold Fusion Experiments Using Maxwellian Plasmas and Sub-Atmospheric Deuterium Gas," *J. Fusion Energy*, **9**, 309 (1990).

269. M. YAGI et al. "Measurement of Neutron Emission from a Ti-D_2 System," *J. Radioanal. Nucl. Chem. Lett.*, **137**, 411 (1989); see also M. YAGI et al., "Measurement of Neutron Emission from a SiO^2-D_2 System," *J. Radioanal. Nucl. Chem. Lett.*, **137**, 421 (1989).

270. P. PERFETTI, F. CILLOCO, R. FELICI, M. CAPOZI, and A. IPPOLITI, "Neutron Emission Under Particular Nonequilibrium Conditions from Pd and Ti Electrolytically Charged with Deuterium," *Nuovo Cimento*, **137**, 421 (1989).

271. D. SEELIGER et al., "Search for DD-Fusion Neutrons During Heavy Water Electrolysis," *Electrochim. Acta*, **34**, 991 (1989).

272. G. SHANI, C. COHEN, A. GRAYEVSKY, and A. BROKMAN, "Evidence for a Background Neutron Enhanced Fusion in Deuterium Absorbed Palladium," *Solid State Comm.*, **72**, 53 (1989).

273. T. LIN and C. LIU, "Cold Fusion Experiments at Department of Nuclear Engineering, National Tsing-Hua University," *J. Fusion Energy*, **9**, 487 (1990).

274. T. IZUMIDA et al., "A Search for Neutron Emission from Cold Nuclear Fusion in a Titanium-Deuterium System," *Fusion Technol.*, **18**, 641 (1990).

275. J. N. HARB, W. G. PITT, and H. D. TOLLEY, "Statistical Analysis of Neutron Burst Size and Rate During Electrolysis of LiOD Solutions," *Fusion Technol.*, **18**, 669 (1990).

276. J. G. BLENCOE, M. T. NANEY, D. J. WESOLOWSKI, and F. G. PEREY, "Tests for 'Cold Fusion' in the PdD_2 and Ti-D_2 Systems at 350 MPA and 195-300K," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, and *J. Fusion Energy*, **9**, 149 (1990).

277. C. K. MATHEWS et al., "On the Possibility of Nuclear Fusion by the Electrolysis of Heavy Water," *Indian J. Technol.*, **27**, 229 (1989).

278. A. G. LIPSON, D. M. SAKOV, V. A. KLYUEV, B. V. DERYAGIN, and Y. P. TOPOROV, "Neutron Emission During the Mechanical Treatment of Titanium in the Presence of Deuterated Substances," *JETP Lett.*, **49**, 675 (1989); see also B. V. DERYAGIN, A. G. LIPSON, V. A. KLYUEV, D. M. SAKOV, and Y. P. TOPOROV, "Titanium Fracture Yields Neutrons?," *Nature*, **341**, 492 (1989).

279. A. F. PARA, V. SANGIUST, P. L. CAVALLOTTI, U. DUCATI, and P. F. BORTIGNON, "Neutron Monitoring and Related Measurements During Electrolysis of Heavy Water with Palladium and Titanium Cathodes," *Fusion Technol.*, **18**, 131 (1990).

280. B. V. GOVOROV et al., "Neutron Emission from Palladium Saturated with Deuterium," *Russ. J. Phys. Chem.*, **64**, 287 (1990), translated from *Zh. Fiz. Khim.*, **64**, 539 (1990).

281. B. SINHA et al., "Observation of Neutron Bursts in Electrolysis of Heavy Water," *Indian J. Technol.*, **27**, 275 (1989).

282. H. JIANYU et al., "Experimental Study on Anomalous Nuclear Effects in Deuterium/Solid Systems," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

283. T. MATSUMOTO, "Observation of New Particles Emitted During Cold Fusion," *Fusion Technol.*, **18**, 647 (1990); see also T. MATSUMOTO, "Observation of Quad-Neutrons and Gravity Decay During Cold Fusion," *Fusion Technol.*, **19**, 2125 (1991).

284. F. CECIL, D. FERG, T. FURTAK, C. MADER, J. A. McNEIL, and D. L. WILLIAMSON, "Study of Energetic Charged Particles from Thin Deuterated Palladium Foils Subject to High Current Densities," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory, and *J. Fusion Energy*, **9**, 195 (1990); see also F. E. CECIL, H. LIU, D. BEDDINGFIELD, and C. GALOVICH, "Observations of Charged Particles Bursts from Deuterium Loaded Ti Thin Foils," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

285. G. CHAMBERS, G. HUBLER, and K. GRABOWSKI, "Search for Energetic Charged Particle Reaction Products During Deuterium Charging of Metal Lattices," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*,

Provo, Utah, October 22-23, 1990; see also G. CHAMBERS, J. E. ERIDON, K. S. GRABOWSKI, B. D. SARTWELL, and D. B. CHRISEY, "Charged Particle Spectra of Palladium Thin Films During Low Energy Deuterium Ion Implantation," *J. Fusion Energy*, **9**, 281 (1990).

286. R. TANIGUCHI, T. YAMAMOTO, and S. IRIE, "Detection of Charged Particles Emitted by Electrolytically Induced Cold Nuclear Fusion," *Jpn. J. Appl. Phys.*, **28**, L2021 (1989); see also R. TANIGUCHI, T. YAMAMOTO, and S. IRIE, "High Sensitivity Measurement of Charged Particles Emitted During Pulsed Electrolysis of D_2O," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

287. V. B. BRUDANIN, V. M. BYSTRITSKY, V. G. EGOROV, S. G. STETSENKO, and I. A. YUTLANDOV, "Search for the Cold Fusion $d(d,{}^4He)$ in Electrolysis of D_2O," *Phys. Lett. A*, **151**, 543 (1990).

288. D. N. RUZIC, K. D. SCHATZ, and P. L. NGUYEN, "A Novel Apparatus to Investigate the Possibility of Plasma-Assisted Cold Fusion," *Fusion Technol.*, **16**, 251 (1989).

289. R. J. BEUHLER, G. FRIEDLANDER, and L. FRIEDMAN, "Cluster-Impact Fusion," *Phys. Rev. Lett.*, **63**, 1291 (1990); see also R. J. BEUHLER, Y. Y. CHU, G. FRIEDLANDER, L. FRIEDMAN, and W. KUNNMANN, "Deuteron-Deuteron Fusion by Impact of Heavy-Water Clusters on Deuterated Surfaces," *J. Phys. Chem.*, **94**, 7665 (1991); see also P. M. ECHENIQUE, J. R. MANSON, and R. H. RITCHIE, "Cluster-Impact Fusion," *Phys. Rev. Lett.*, **64**, 1413 (1990).

290. J. FALLAVIER, J. KEMMLER, R. KIRSCH, J. C. POIZAT, J. REMILLIEUX, and J. P. THOMAS, *Phys. Rev. Lett.*, **65**, 621 (1990).

291. S. M. MYERS, P. M. RICHARDS, W. R. WAMPLER, and F. BESENBACHER, "Ion-Beam Studies of Hydrogen-Metal Interactions," *J. Nucl. Mater.*, **165**, 9 (1989).

292. P. H. FANG, "Deuterium Fusion Through Nonequilibrium Induction." *Fusion Technol.*, **19**, 369 (1991).

293. J. RAFELSKI, "Theoretical Prospects for CCF: Conventional Cold Fusion," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

294. P. T. GREENLAND, "Issues Connected with Cold Fusion: A Room Temperature Mechanism for the Production of X-Rays," *J. Phys. B*, **23**, 1679 (1990).

295. J. GITTUS and J. BOCKRIS, "Explanations of Cold Fusion," *Nature*, **339**, 105 (1989).

296. R. KIRCHHEIM, "Interaction of Hydrogen with Dislocations in Palladium—I. Activity and Diffusivity and Their Phenomenological Interpretation," *Acta Metall.*, **29**, 835 (1981); see also R. KIRCHHEIM, "Interaction of Hydrogen with Dislocations in Palladium—II. Interpretation of Activity Results by Fermi-Dirac Distribution," *Acta Metall.*, **29**, 845 (1981).

297. E. STORMS and C. TALCOTT-STORMS, "The Effect of Hydriding on the Physical Structure of Palladium and on the Release of Contained Tritium," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990, and *Fusion Technol.*, **20**, 246 (1991).

298. L. B. REES, "Cold Fusion: What Do We Know? What Do We Think?," *J. Fusion Energy*, **10**, 111 (1991).

299. V. J. CUNNANE, R. A. SCANNELL, and D. J. SCHIFFRIN, "$H_2 + O_2$ Recombination in Non-Isothermal, Non-Adiabatic Electrochemical Calorimetry of Water Electrolysis in an Undivided Cell," *J. Electroanal. Chem.*, **269**, 163 (1989).

300. M. J. JONCICH and N. HACKERMAN, "The Reaction of Hydrogen and Oxygen on Submerged Platinum Electrode Catalysis. I. Effect of Stirring, Temperature, and Electric Polarization," *J. Phys. Chem.*, **57**, 674 (1953).

301. R. P. SANTANDREA and R. G. BEHRENS, "A Review of the Thermodynamics and Phase Relationships in the Palladium-Hydrogen, Palladium-Deuterium and Palladium-Tritium Systems," *High. Temp. Mater. Processes*, **7**, 149 (1986).

302. D. M. NACE and J. G. ASTON, "Palladium Hydride. II. Thermodynamic Study of Pd_2D from 15 to 303 K. Evidence for the Tetrahedral PdH_4 Structure in Palladium Hydride," *J. Am. Chem. Soc.*, **79**, 3627 (1957); see also D. M. NACE and J. G. ASTON, "Palladium Hydride. I. The Thermodynamic Properties of Pd_2H Between 273 and 345 K," *J. Am. Chem. Soc.*, **79**, 3619 (1957).

303. J. F. LYNCH, J. D. CLEWLEY, and T. B. FLANAGAN, "The Formation of Voids in Palladium Metal by the Introduction and Removal of Interstitial Hydrogen," *Phil. Mag.*, **28**, 1415 (1973).

304. L. PAULING, "Explanations of Cold Fusion," *Nature*, **339**, 105 (1989).

305. J. O'M. BOCKRIS, N. PACKHAM, O. VELEV, G. LIN, M. SZKLARZCYCK, and R. KAINTHLA, "Seven Chemical Explanations of the Fleischmann-Pons Effect," presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989, LA-11686-C, Los Alamos National Laboratory; see also R. R. KAINTHLA et al., "Eight Chemical Explanations of the Fleischmann-Pons Effect," *Int. J. Hydrogen Energy*, **14**, 771 (1989).

306. A. F. ABUTAHA, "Cold Fusion—The Heat Mechanism," *J. Fusion Energy*, **9**, 345 (1990); see also A. F.

ABUTAHA, "Cold Fusion – Engineering Perspectives," *J. Fusion Energy*, **9**, 391 (1990).

307. F. M. MAZZOLAI, P. G. BORDONI, and F. A. LEWIS, "Elastic Energy Dissipation Effects in the Palladium-Hydrogen System," *J. Less-Common Metals*, **74**, 137 (1980).

308. R. HULTGREN, P. D. DESAI, D. T. HAWKINS, M. GLEISER, K. F. KELLEY, and D. D. WAGMAN, *Selected Values of the Thermodynamic Properties of the Elements*, p. 388, American Society for Metals (1973).

309. R. D. EAGLETON and R. T. BUSH, "Calorimetric Experiments Supporting the Transmission Resonance Model for Cold Fusion," *Fusion Technol.*, **20**, 239 (1991).

310. M. H. MILES and R. E. MILES, "Theoretical Neutron Flux Levels, Dose Rates, and Metal Foil Activation in Electrochemical Cold Fusion Experiments," *J. Electroanal. Chem.*, **295**, 409 (1990).

311. N. HUANG, Q.-H. GAO, Y. LIAW, A. SEKI, and B. LIEBERT, "Effect of Light-Water Additions on Excess Heat Generation in Palladium-Deuterium System," *Proc. Cold Fusion Symp., 8th World Hydrogen Energy Conf.*, Honolulu, Hawaii, July 22–27, 1990, p. 43, University of Hawaii.

312. J. S. BULLOCK IV, G. L. POWELL, and D. P. HUTCHINSON, "Electrochemical Factors in Cold Fusion Experiments," *J. Fusion Energy*, **9**, 275 (1990).

313. E. STORMS, Los Alamos National Laboratory, Unpublished (1990).

314. D. R. ROLISON, W. E. O'GRADY, R. J. DOULE, Jr., and P. P. TRZASKOMA, "Anomalies in the Surface Analysis of Deuterated Palladium," *Proc. 1st Annual Conf. Cold Fusion*, Salt Lake City, Utah, March 28–31, 1990, p. 272, National Cold Fusion Institute (1990).

315. D. D. WAGMAN et al., "The NBS Tables of Chemical Properties, Selected Values for Inorganic and C_1 and C_2 Organic Substances in SI Units," *J. Phys. Chem. Ref. Data*, **11**, 2 (1982).

316. S. G. McKEE, F. A. LEWIS, J. P. MAGENNIS, and T. J. KELLY, "The Establishment of Equilibria in the Pd-H_2-D_2-H_2O-D_2O System," *J. Less-Common Metals*, **89**, 475 (1983).

317. X. LI et al., "The Precursor of 'Cold Fusion' Phenomenon in Deuterium/Solid Systems," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990.

318. R. POOL, "Wolf: My Tritium Was an Impurity," *Science*, **248**, 1301 (1990); see also K. WOLF, D. LAWSON, J. SHOEMAKER, B. DEAN, and D. COE, "On the Observation of Tritium from the Electrolysis of Heavy Water," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990.

319. R. T. BUSH, "An Approximate Theoretical Branching Ratio for the D-on-D TRINT Reaction," *Fusion Facts*, **2**, *3*, 31 (1990).

320. B. D. KAY, K. R. LYKKE, and R. J. BUSS, "Problems with the Mass Spectrometric Determination of Tritium from Cold Fusion," *J. Fusion Energy*, **9**, 491 (1990).

321. T. MATSUMOTO, "Cold Fusion Observed with Ordinary Water," *Fusion Technol.*, **17**, 490 (1989).

322. E. STORMS, Los Alamos National Laboratory, Unpublished (1991).

323. D. M. SHELDON, "Hoover Strong on Their Palladium Manufacturing," *Fusion Facts*, **2**, *3*, 30 (1990).

324. R. LÄSSER and K.-H. KLATT, "Solubility of Hydrogen Isotopes in Palladium," *Phys. Rev. B*, **28**, 748 (1983).

325. K. CEDZYNSKA, S. C. BARROWES, H. E. BERGESON, L. C. KNIGHT, and F. G. WILL, "Tritium Analysis in Palladium with an Open System Analytical Procedure," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990, and *Fusion Technol.*, **20**, 108 (1991).

326. D. CORRIGAN and E. W. SCHNEIDER, "Tritium Separation Effects During Heavy Water Electrolysis: Implications for Reported Observations of Cold Fusion," *J. Electroanal. Chem.*, **281**, 305 (1990).

327. B. DANDAPANI and M. FLEISCHMANN, "Electrolytic Separation Factors on Palladium," *J. Electroanal. Chem.*, **39**, 323 (1972).

328. M. DEHN, "Tritium Primer," *Fusion Facts*, **2**, *7*, 18 (1991).

329. S. Y. KARPOV, Y. V. KOVAL'CHUK, V. E. MYACHIN, and Y. V. POGORELSKII, "Possible Mechanism of Cold Nuclear Fusion," *Pis'ma, Zh. Tekh. Fiz.*, **16**, 91 (1990).

330. P. BÉM, I. PASEKA, I. PECINA, V. PRESPERIN, O. SPALEK, M. VOMÁCKA, and J. CONDRAK, "Search for Neutrons from DT-Cold Fusion," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990.

331. T. A. PARISH, R. T. PERRY, and W. B. WILSON, "Neutron Sources and Spectra from Cold Fusion," *J. Fusion Energy*, **9**, 479 (1990).

332. S. E. JONES, T. K. BARTLETT, D. B. BUEHLER, J. B. CZIRR, G. L. JENSEN, and J. C. WANG, "Preliminary Results from the BYU Charged-Particle Spectrometer," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22–23, 1990.

333. J. W. MATHER, "Dense Plasma Focus," *Method. Exp. Phys.*, **9B**, 187 (1971).

334. J. S. C. McKEE et al., "Neutron Emission from Low Energy Deuteron Injection of Deuterium-Implanted Metal Foils (Pd, Ti, and In)," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

335. M. RABINOWITZ and D. H. WORLEDGE, "An Analysis of Cold and Lukewarm Fusion," *Fusion Technol.*, **17**, 344 (1990); see also Y. E. KIM, M. RABINOWITZ, G. S. CHULICK, and R. A. RICE, "Theories of Cluster-Impact Fusion with Atomic and Molecular Cluster Beams," *Mod. Phys. Lett. B*, **5**, 427 (1991); see also Y. E. KIM, M. RABINOWITZ, Y. K. BAE, G. S. CHULICK, and R. A. RICE, "Cluster-Impact Nuclear Fusion: Shock-Wave Statistical Analysis," to be published in *Mod. Phys. Lett.*; see also M. RABINOWITZ, Y. E. KIM, R. A. RICE, and G. S. CHULICK, "Cluster-Impact Fusion: Bridge Between Hot and Cold Fusion?," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990; see also G. S. CHULICK, R. A. RICE, Y. E. KIM, and M. RABINOWITZ, "Comment on 'Cluster-Impact Fusion' by P. M. Echenique et al.," submitted to *Phys. Rev. Lett.*

336. W. KRAUSE and L. KAHLENBERG, "On Palladium-Hydrogen," *Trans. Electrochem. Soc.*, **68**, 449 (1935).

337. G. A. MOORE, "The Comportment of Palladium-Hydrogen System Toward Alternating Electric Current," *Trans. Electrochem. Soc.*, **75**, 237 (1939); see also D. P. SMITTY and G. J. DERGE, "The Occlusion and Diffusion of Hydrogen in Metals: A Metallographic Study of Palladium-Hydrogen," *Trans. Electrochem. Soc.*, **66**, 253 (1935); see also G. A. MOORE, "The Occlusion and Diffusion of Hydrogen in Metals," PhD Thesis, Princeton University (1939).

338. N. E. BROWN and N. JARMIE, "Differential Cross Sections at Low Energies for $^2H(d,p)^3H$ and $^2H(d,n)^3He$," *Phys. Rev. C*, **41**, 1391 (1989); see also G. M. HALE, R. D. SMITH, and T. L. HALLEY, "Nuclear Reactions and Screened-Coulomb Fusion Rates," *J. Fusion Energy*, **9**, 187 (1990).

339. A. KRAUSS, H. W. BECKER, H. P. TRAUTVETTER, and C. ROLFS, "Low-Energy Fusion Cross Sections of D + D and D + ^{3}He Reactions," *Nucl. Phys. A*, **465**, 150 (1987).

340. A. SCALIA, "The Nuclear Fusion for the Reactions $^2H(d,n)^3He$, $^2H(d,p)^3H$, $^3H(d,n)^4He$," *Nuovo Cimento A*, **101**, 795 (1989).

341. D. V. BALIN et al., "Experimental Investigation of the Muon Catalyzed *d-d* Fusion," *Phys. Lett. B*, **141**, 173 (1984).

342. J. S. COHEN and J. D. DAVIES, "Is Cold Fusion Hot?," *Nature*, **342**, 487 (1989); see also J. S. COHEN and J. D. DAVIES, "The Cold Fusion Family," *Nature*, **338**, 705 (1989).

343. J. R. OPPENHEIMER and M. PHILLIPS, "Note on the Transmutation Function for Deuterons," *Phys. Rev.*, **48**, 500 (1935).

344. E. O. LAWRENCE, E. McMILLAN, and R. L. THORNTON, "The Transmutation Functions for Some Cases of Deuteron-Induced Radioactivity," *Phys. Rev.*, **48**, 493 (1935).

345. F. A. LEWIS, "The Hydrides of Palladium and Palladium Alloys. A Review of Recent Researches," *Platinum Metal Rev.*, **4-5**, 132 (1960–1961); see also F. A. LEWIS, "The Palladium-Hydrogen System, Part III: Alloy Systems and Hydrogen Permeation," *Platinum Metal Rev.*, **26**, 121 (1989); see also F. W. LEWIS, "The Palladium-Hydrogen System, A Survey of Hydride Formation and the Effects of Hydrogen Contained Within the Metal Lattices," *Platinum Metal Rev.*, **26**, 20 (1982); see also F. A. LEWIS, "Molecular Hydrogen Diffusion Overpotential and Local Cells Hydrogen Transfer Processes at Electrodes of Palladium, Palladium Alloys, and Other Transition Metals, and Implications for Theories of Hydrogen Overpotential," *Sur. Tech.*, **11**, 1 (1980); see also A. W. CARSON, T. B. FLANAGAN, and F. A. LEWIS, "Absorption of Hydrogen by Palladium/Platinum Alloys," *Trans. Faraday Soc.*, **56**, 363 (1960); see also F. A. LEWIS, *The Palladium Hydrogen System*, Academic Press, New York (1967); see also L. J. GILLESPIE and W. R. DOWNS, "The Palladium-Deuterium Equilibrium," *J. Am. Chem. Soc.*, **61**, 2494 (1939); see also J. C. BARTON, F. A. LEWIS, and I. WOODWARD, "Hysteresis of the Relationships Between Electrical Resistance and Hydrogen Content of Palladium," *Trans. Faraday Soc.*, **59**, 1201 (1963).

346. H. C. JAMIESON, G. C. WEATHERLY, and F. D. MANCHESTER, "The Phase Transformation in Palladium-Hydrogen Alloys," *J. Less-Common Metals*, **56**, 85 (1976).

347. G. H. LIN, R. C. KAINTHLA, N. J. C. PACKHAM, and J. O'M. BOCKRIS, "Electrochemical Fusion: A Mechanism Speculation," *J. Electroanal. Chem.*, **280**, 207 (1990).

348. X. JIANG and L. J. HAN, "Theory of Micropinch," *Proc. Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, October 22-23, 1990.

349. F. DALARD, M. ULMANN, J. AUGUSTYNSKI, and P. SELVAM, "Electrochemical Incorporation of Lithium into Palladium from Aprotic Electrolyte," *J. Electroanal. Chem.*, **270**, 445 (1989).

350. M. ULMANN, J. LIU, J. AUGUSTYNSKI, F. MEDI, and L. SCHLAPBACK, "Surface and Electrochemical Characterization of Pd Cathodes After Prolonged Charging in LiOD + D_2O Solutions," *J. Electroanal. Chem.*, **286**, 257 (1990).

351. J. T. DICKINSON, L. C. JENSEN, S. C. LANGFORD, R. R. RYAN, and E. GARCIA, "Fracto-Emission From Deuterated Titanium: Supporting Evidence for a Fracto-Fusion/Mechanism," *J. Mater. Res.*, **5**, 109 (1990).

352. T. TAKEDA and T. TAKIZUKA, "Fractofusion Mechanism," *J. Phys. Soc. Jpn.*, **58**, 3073 (1989).

353. F. J. MAYER, J. S. KING, and J. R. REITZ, "Nuclear Fusion from Crack-Generated Particle Acceleration," *J. Fusion Energy*, **9**, 269 (1990).

354. I. AMATO, "If Not Cold Fusion, Try Fracto-Fusion?," *Sci. News*, **137**, 87 (1990).

355. J. K. DIENES, "On Nuclear Reactions in Defects," *Fusion Technol.*, **19**, 543 (1990).

356. G. C. ABELL, L. K. MATSON, R. H. STEINMEYER, R. C. BOWMAN, Jr., and B. M. OLIVER, "Helium Release from Aged Palladium Tritide," *Phys. Rev.*, **41**, 1220 (1990).

357. H. HASEGAWA and K. NAKAJIMA, "Effect of Hydrogen on the Mechanical Properties of Pd," *J. Phys. F*, **9**, 1035 (1979).

358. T. MATSUMOTO, "Microscopic Observations of Palladium Used for Cold Fusion," *Fusion Technol.*, **19**, 567 (1991).

359. T. B. FLANAGAN, J. F. LYNCH, J. D. CLEWLEY, and B. V. TURKOVICH, "The Effect of Lattice Defects on Hydrogen Solubility in Palladium. I. Experimentally Observed Solubility Enhancements and Thermodynamics of Absorption," *J. Less-Common Metals*, **49**, 13 (1976).

Please see pages 1 and 9

October 30, 1991

COLD FUSION

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A memorandum has been written on the present state of cold fusion research as of October, 1991, by Professor Heinz Gerischer.

The significance of this memorandum arises from Professor Gerischer's status. He is widely recognized to be the leading physical electrochemist in Europe and would vie for the title on a still wider basis. Apart from his long term involvement in electrochemistry he is well known as a physical chemist of the highest standing and was, until 1988, the Director of the Max Planck Institute for Physical Chemistry in Berlin.

A still further significance may be attached to Professor Gerischer's memorandum because he has been, since March 1989, a dedicated opponent of cold fusion, taking the position (more pronounced, even, in Germany, France and England, perhaps, than in the United States) that the work of Fleischmann and Pons and the announcement of March, 1989, was due to poor measurements.

Indeed, I can witness the fact that until the Como Conference of July 1991, Professor Gerischer maintained a negative opinion of the field which he confided to me at the beginning of the Conference.

My translation of his memorandum follows.

MEMORANDUM ON THE PRESENT STATE OF KNOWLEDGE ON COLD FUSION

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Concerning the origin of this memorandum: Between the 30th of June and the 4th of July, there took place in Como, Italy, the Second Annual Conference about Cold Fusion. This had been organized by Italian groups, principally from the Universities of Turin and Milan. The joint chairmen of the conference were Professor T. Bressani (Turin) and Professor G. Preparata (Milan).

At the end of May I was invited to attend this conference as a skeptical observer and found that I was down on the program for the last day of the conference as giving a lecture on the pros and cons of the field. There were about 60 lecturers at the meeting in groups from the U.S.A., Italy, Japan, China, Soviet Union, and various smaller countries which all gave their results. Two comprehensive reviews had been already sent to me in manuscript form, - one of them has already appeared (1) and the other will shortly appear. On the basis of these two reviews I was able to prepare myself somewhat for the conference.

THE INDICATIONS OF OCCURRENCE OF NUCLEAR PROCESSES IN METAL-DEUTERIUM ALLOYS

In spite of my earlier conclusion, - and that of the majority of scientists, - that the phenomena reported by Fleischmann and Pons in 1989 (3) depended either on measurement errors or were of chemical origin, there is now undoubtedly overwhelming indications that nuclear processes take place in the metal alloys. The early publications were so full of errors in measurement technique and in the interpretation that the euphoria to which the discovery gave rise was rapidly replaced by disappointment when it turned out that the

laboratories with the best equipment could not reproduce the results. Only very few groups found similar effects, but even these groups could not find reproducibility in their own laboratory. Furthermore the supposition that a fusion of two deuterium atoms could take place in a solid body such as PdD_x or TiD_x was contradictory to all theories of nuclear reactions. In particular the observed (or maintained) heat effects bore no relationship to the tiny amounts of nuclear products which had been found by a few authors. The main search here went towards neutrons and tritium because according to fusion of two D^+ nuclei in hot plasmas, the following two reactions would occur with about the same probability.

$$D^+ + D^+ \rightarrow n\ (2.45\ MeV) + {}^3He\ (0.82\ MeV)$$

$$D^+ + D^+ \rightarrow H^+\ (3.02\ MeV) + {}^3H\ (1.01\ MeV)$$

Now, the sensitivity of instruments for detecting neutrons is particularly good and therefore the majority of the measurements concentrated on experiments which would give rise to such detection. This was particularly true in experiments which were carried out at low temperatures from palladium which had been previously loaded with deuterium. In the electrochemical loading of palladium by cathodic evolution of deuterium either from an alkaline or acid electrolyte in D_2O (predominantly with the electrolyte LiOD) from time to time a tritium enrichment would be found which was considerably higher than could arise from differences expected for separation factors during the discharge of D_2O from an electrolyte containing DTO. The occasional observation of neutrons with intensities which were decisively above the background, and with tritium amounts in the electrolyte which were certainly an indication for the occurrence for nuclear processes is very significant, but the amounts were so small that the heat observed could not be explained by their occurrence. Correspondingly, it has been found that the neutron production is sporadic and occurs in bursts. The

same occurs for the tritium enrichment and also for the heat production.

The occurrence of such happenings could not in any way be foreseen on the basis of previous knowledge. Correspondingly, the authors of the papers admitted that the reported positive results were only occasionally obtained, and that many experiments ran through without any observable nuclear consequences at all, - facts which lent further doubt as to the reality of the effects.

As far as the interpretation of the nuclear reactions goes it has been suggested (4,5) that a reaction occurs which plays no part at all in the hot plasmas namely:

$$D + D \rightarrow {}^{4}He\ (0.076\ MeV) + \gamma\ (23.77\ MeV) \qquad \text{(Equation 3)}$$

The gamma radiation which should occur according to this reaction is, however, not observed. However, very recently there have been reports about the observation of helium in electrolytically evolved deuterium from a $LiOD/D_2O$ solution in cells with palladium electrodes. In these experiments heat production was also calorimetrically observed. The amount of ^4He was indeed in the right order of magnitude according to equation three although the agreement was not quantitative. On the other hand, corresponding water blank experiments using $LiOH/H_2O$ showed neither any heat excesses nor any helium in the evolved hydrogen gas.

AN EVALUATION OF THE RESULTS OBSERVED SO FAR

Although there are many discrepancies in the reports which are at hand, and although there are many open questions, there now lie before us several indications that fusion reactions do occur between deuterides in metals. This gives rise to a new situation. It is entirely an open question whether such processes could be used as the source of energy but this, of course, can only be decided if the processes which have been revealed in the work discussed here are

researched and given a theoretical basis. In any case I consider it absolutely necessary that these phenomena are systematically researched and the conditions for their reproducibility cleared up. That a nuclear reaction can be stimulated by interaction with a solid lattice and made to take another path from that which it would take in the plasma, is an entirely unexpected discovery with possibly wide ranging consequences. It demands confirmation and further experimental evaluation. In the following a number of experimental and theoretical questions are raised which are at the present time entirely open.

EXPERIMENTAL PROBLEMS

The overwhelming problem is the lack of reproducibility in the results. The anomalous phenomena in palladium and titanium can only be seen after very long times of loading, usually after several weeks. In the loading there is a phase change in which the alpha phase is changed to the beta phase and this involves a change in lattice constants which gives rise to a volume increase of about 15%. The microcrystalline structure of the solid seems to play an important role here. However, the structure of the solid is not yet characterized. The lattice structure seems to be an important part of the conditions necessary for the setting up of the anomalous effects.

In the electrochemical experiments the characteristics of the surface in the charging of the metal with deuterium plays a great role. These effects in the electrochemical experiments are greater than those in loading from the gas phase. The reason for this is probably the increased activity of deuterium atoms which occur in the first step of loading according to the reaction:

$$D_2O + e \rightarrow D_{ad} + OH^- \qquad \text{(Equation 4)}$$

The slower the following reactions (5 and 6) are, - these lead to molecular deuterium, - the greater is the activity of the absorbed D atoms on the surface:

$D_{ad} + D_{ad} > - D_2$ (Equation 5)

$D_{ad} + D_2O + e > - D_2 + OD^-$ (Equation 6)

The activity of adsorbed atoms determines the loading of the metal lattice with D atoms which go from the surface to the interior of the lattice. In this way one can build up a super-saturation of the lattice with D-atoms and this super saturation increases with the surface activity of the adsorbed D. If one tried to reach super saturation in the metal by increasing the pressure in the gas phase, enormous pressures would be necessary.

In the literature is reported that extremely long times of electrolysis are necessary before phenomena are observed and these can give rise to the deposition of impurities on the surface of the electrode. This gives rise in turn to a change of the overpotential in the current-potential curve (7) and also in the analysis of the surface composition which will certainly take place on long term electrolysis (8). Such absorbance can give rise to an inhibition of the recombination velocity of the atoms to deuterium of the surface atoms to the molecular product in reactions 5 and 6. In this way the activity of adsorbed hydrogen can be increased. On the other hand, if the inhibition affects reaction 4 then the reverse effect occurs. In fact, it is possible to think of an acceleration of reactions 5 and 6 by the impurities. Thus, it has been shown for palladium cathodes that these contain platinum which obviously arises by the partial dissolution of the anode. It is known that if oxygen is evolved at high rates upon an anode there is certainly the danger of anodic dissolution. Correspondingly it is known that platinum deposition on palladium increases the recombination and therefore decreases the activity of adsorbed D.

Apart from the observation of neutrons, proof of the presence of the nuclear products, ^3H, ^3He, and ^4He are difficult to observe because many of the experiments are carried out in open systems the evolved gases seldom being

collected. The consumed D_2O has to be replaced. In this way one builds up impurities in the electrolyte in the cell and the products in the gas phase are lost. Only very few experiments are carried out in closed systems in which the D_2 is converted to D_2O on a catalyst and re-introduced into the electrolyte (9,10) or alternatively, the D_2 which is evolved is oxidized back to D_2O (or D^+) (11).

The latter process corresponds to the anodic process in a hydrogen oxygen fuel cell. In such experiments the reaction products build up in the electrolyte or in the gas volumes which are collected. This has so far only been used for tritium analysis.

The calorimetric measurements have been greatly improved. In the open cells there are still difficulties concerning the loss of heat along with the evolved gases. Closed cells do not have this difficulty but they are much larger in volume and are less sensitive to the measurement of heat. In general there exists in all electrochemical cells the problem that the energy input of the cell is large (large current densities) and must be carefully measured in order to set up a clear and sure energy balance. The uptake of energy varies with the cell resistance and the overpotentials of the electrode. The excess energy in most of the experiments which have been described so far is considerably less than the energy which is being used for the electrolysis. A typical excess heat measurement amounts to 10-20% of the energy put into the electrolysis over several hours. A few measurements have given 100% and more.[1] A few measurements do give 100% of heat and even more but are perhaps somewhat doubtful. On the other hand, there were plenty of indications at the conference

[1]Note added by JOMB: At a recent meeting at the Southern California Edison Company, Robert Bush of Pamona University in California revealed measurements on very thin films which when calculated to a per cc measurement of heat give about 1 kw per cc.

that some authors were withholding their results because of patent considerations.[2]

The experimental problems in the measurement of gas loading is much less well known to me and I do not wish to comment upon it. It is noteworthy that neutron production which exceeds the background intensity can only be obtained by warming up these samples which have been charged at very low temperatures.

THEORETICAL PROBLEMS

The most decisive problem is the question: How can a solid body influence the course of nuclear processes?[3] Thus, the point is, how can the screening of the repulsive forces between the two atomic nuclei be achieved so that successful tunnelling can occur? The second question is, how can the channels be altered so that they go in the direction of reaction 3 and thus allow a ratio of 10^6-10^9 for the predominance of tritium over neutrons (compare the expected 1:1 ratio of nuclear chemistry in hot plasmas).(2)

A further riddle is the absence of hard gamma radiation which should occur corresponding to reaction 3. How can this gigantic energy of this reaction be diffused over the solid body without emitting radiation (although weak x-rays have been seen on a photographic plate)(6,12).

There are various attempts at theoretical interpretation (1), and some of these are really quite far out. It has even been suggested that an unknown elementary particle is a catalyst in these processes (13). Without sufficient

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[2]Note added by JOMB: In particular Mike McKubre and his group at SRI have been reporting 250-450% heat for some months. The rumor is that this is an understatement. EPRI has decided to continue to build a practical plant for the production of energy from cold fusion which should be completed by 1997.

[3]Note added by JOMB: On the other hand, the well accepted Mossbauer effect surely is already an example of the effect of a lattice upon a nuclear process.

REFERENCES

1. M. Srinivasan, in: "Current Science," April 1991.
2. E. Storms, appears in "Fusion Technology," 1991.
3. M. Fleischmann, S. Pons and M. Hawkins, J. Electroanal. Chem. 261 (1989), 301; 263 (1989), 187.
4. C. Walling and J. Simons, J. Phys. Chem. 93 (1989), 4693.
5. M. Fleischmann, personal communication, 1990.
6. B. F. Bush, J. Lagowski, M. H. Miles and G. S. Ostrom, J. Electroanal. Chem. 304 (1991), 171.
7. Z. B. M. Ulmann, J. Lin, J. Augustynski, F. Eli and L. Schlapbach, J. Electroanal. Chem. 286 (1990), 257.
8. Observations of Various Authors at the Meeting in Como.
9. N. J. C. Packham, K. L. Wolf, J. C. Wass, R. C. Kainthla and J. O'M. Bockris, J. Electroanal. Chem. 270 (1989), 451.
10. E. Storms and C. Talcott, Fusion Technology 17 (1990), 680.
11. F. Will, Lecture at the Meeting in Como.
12. S. Szpak, Lecture at the Meeting in Como, J. Electroanal. Chem. 302 (1991), 255.
13. F. Mayer and J. R. Reitz, Fusion Technology 18 (1991), 352.

material to overview the theoretical contributions I will not make any further comments on these theories.

WHAT SHALL BE DONE?

The works which have been so far reported are works which concern individual questions. Attempts are being made to re-produce experiments and to modify the technique. A systematic characterization of the electrochemical and experimental conditions has not yet been carried out. As the largest effects are being observed under these electrochemical conditions it is necessary to concentrate upon the electrochemical experiments.

Fleischmann and Pons reported in the meeting in Como concerning their experiments with palladium alloys (probably with silver) and maintain that they had been able reproducibly to observe large excess heats. No details of the experimental arrangements were given because of their wish to maintain patent conditions.[4]

There is also evidence that the composition of the electrolyte plays a considerable part in the occurrence of the anomalous effects. It is absolutely essential to research the connection between overpotential, hydrogen content, and surface characteristics of palladium or palladium alloys in a systematic way. This could be probably carried out with light hydrogen. Beginning with the hypothesis that a very high loading of D in metals is necessary for fusion because in this respect to loading, H and D hardly differ from each other. One could then come back later to the experiments with D_2O and with the knowledge of what has given rise to the high loading with hydrogen. In this respect it is necessary to have a method which allows a rapid measurement of the amount of

[4]The experiments of Fleischmann and Pons are being carried out in Nice with support of a Japanese group, connected to Toyota.

hydrogen in the palladium. Here resistance methods are probably the best. Parallel to the information obtained in these measurements it would be necessary to build calorimeters which have requisite controls and work in an enclosed manner so that heat production and nuclear products can also be observed with a minimum amount of ambiguity. The decisive proof of the rising of excess heat by nuclear reactions will, of course, be the occurrence of the corresponding amount of ^4He. If this can be confirmed, further steps for the optimization of the energy yield can be carried out.

These goals require the cooperation of scientists from various fields. One needs electrochemists, metal scientists, particularly those with knowledge of metal hydrides, engineers for the building of cells and the computerization of data, persons with experience in calorimetry, mass spectroscopy, and one needs nuclear physicists for the radiation measurements. A team of this kind should be in position to clear up the basic situation fairly quickly. Of course, the first thing is to confirm the facts. The fact that, in the Republic of Germany this work has been inhibited is no more justified. It could later on be regarded as a very unfortunate gap in German research when compared with the present activity in other countries and particularly in Japan.

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Cold Fusion 1992: Basic Facts

Prepared by:

Dr. Eugene F. Mallove, author of *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor*, (John Wiley & Sons, May, 1991).

What happened to *cold fusion*, the "miracle or mistake," that was announced by Drs. Martin Fleischmann and Stanley Pons in March 1989? It would not be surprising if you thought that cold fusion was "dead," considering how the scientific establishment, the hot fusion community, and many in the news media have mistreated the subject. But cold fusion is far from dead. It is, indeed, alive not only in dozens of laboratories in the United States, but in numerous foreign research centers.

Tragically, what is also very much alive - particularly in the United States, in stark contrast to the Japanese experience - is a scandalous campaign by vested scientific interests and the intellectually arrogant to attack cold fusion research.

Here are the basic facts about cold fusion as they stand in 1991. If you would like to read about the evolution of this scientific controversy and the impending technological revolution, please consider reading my book, *Fire from Ice.*

Hot Fusion Versus Cold Fusion

Hot fusion is the kind of nuclear reaction that powers the Sun and the stars. At temperatures of millions of degrees, the nuclei of hydrogen atoms overcome their natural tendency to repel one another and join or fuse to form helium nuclei. This releases enormous energy according to Einstein's famous $E=mc^2$ formula. Fusion is the opposite of fission, which is the release of energy by splitting heavy uranium or plutonium nuclei.

Scientists the world over have spent more than four decades and billions of dollars to investigate the possibility of mimicking with devices here on Earth the fusion reactions of the stars. These are complex and large machines that rely on high magnetic fields or powerful lasers to compress and heat fusion fuel. The controlled hot fusion program has made enormous strides, but all agree that the earliest possible time when practical hot fusion

devices may be available is about three decades away. Hot fusion is a very tough engineering problem. If and when its operating reactors are built, they will have many problems, as numerous hot fusion experts can attest.

Cold fusion is the much disputed phenomenon that is claimed to occur when the special form of hydrogen - the plentiful hydrogen isotope, deuterium - is made to interact with metals, such as palladium, titanium, and lithium. Cold fusion, if real - and the evidence is overwhelmingly on that side, releases enormous quantities of energy, hundreds to thousands of times what ordinary chemical reactions could possibly yield. Cold fusion, in contrast to hot fusion, apparently occurs in relatively simple apparatus, albeit not yet without some difficulties. Cold fusion reactions, whatever their ultimate detailed explanation turns out to be, are not at all like conventional hot fusion reactions. If they were, cold fusion experimenters would have been killed by massive flows of radiation - neutrons and gamma rays. The continuing wonder of cold fusion is that it is apparently a very clean reaction that gives very little radiation, relatively low levels of radioactive tritium, and produces byproduct atoms that have been difficult to identify. Several excellent theories can now account for cold fusion, but it is not certain which of them are correct.

The Cold-Fusion Evidence

The most important evidence for cold fusion is the excess heat energy that comes from special electrochemical cells - much more heat coming out than electrical energy being fed in. Competent and careful researchers have now confirmed that under the proper conditions it is possible to obtain anywhere from 10% to 4,000% excess power output. Sometimes this power comes out in bursts, but it has also appeared continuously in some experiments for hundreds of hours. When this power is added up to give kilowatt-hours, the inescapable conclusion is that much more energy is being released than any possible chemical reaction could yield. In the early days of the cold fusion story, it was possible to question whether these experiments were sound. It is now virtually impossible to assail many of these experiments - except by ignoring them! This is precisely the tactic that the opponents of cold fusion have chosen.

And there is more: neutrons, tritium, energetic charged particles, and other ionizing radiations have been detected in a variety of cold fusion experiments. Moreover, many of these experiments differ significantly from one another in their approach and conditions. So there is little chance that the various laboratories are all making the same systematic errors. These nuclear effects are clearly the hallmark of nuclear processes of heretofore unknown character. By itself, this nuclear evidence points to an entirely new realm of phenomena of staggering scientific importance. The excess energy in some of these experiments is virtual proof that something very extraordinary and of enormous potential technological significance has been discovered.

Examine the attached table of results. At present there are over 100 laboratories around the world where positive cold fusion results of one kind or another have been obtained. This listing, which appeared in my book, *Fire from Ice*, is already outdated. Last spring, a conference in the Soviet Union revealed many more positive results, and at the Second Annual Conference on Cold Fusion held in Como, Italy, last July, much more positive evidence for cold fusion emerged. In the United States, "bootlegged" work, as well as some funded research at Federal research centers, has led to positive experimental results. These are the installations:

- Los Alamos National Laboratory
- Oak Ridge National Laboratory
- Naval Weapons Center at China Lake
- Naval Research Laboratory
- Naval Ocean Systems Center

Here are three of the most impressive heat results:

Dr. Michael McKubre's group at SRI (Stanford Research Institute), Inc has produced definitive proof of excess heat. In his Electric Power Research Institute-funded work, he achieved reproducible excess power with four different palladium electrodes. His group now understands the conditions necessary to produce excess heat at will. Dr. McKubre stated categorically that the excess energy produced in his group's work cannot be explained by chemistry.

Dr. McKubre's work was interrupted by a tragic, unexplained explosion on January 2, 1992. Dr. Andrew Riley, an electrochemist, died in the blast. Dr. McKubre and Dr. Stuart Smedley were also hurt.

The work of Dr. Robert T. Bush and his colleagues at California Polytechnic Institute achieved one of the highest recorded levels of power density production for cold fusion. It occurred in a thin film of palladium that was deposited on a silver electrode: almost three kilowatts per cubic centimeter came out. This is 30 times the power density of the fuel rods in a typical contemporary fission nuclear reactor! The cell produced several watts of excess power for almost two months, non-stop.

The continuing research of Drs. Fleischmann and Pons, who are now working at a Japanese-funded laboratory in France, is also impressive. At the Como conference last July, they revealed that with 10 out of 11 electrodes made of silver-palladium alloy, they were able to bring their electrochemical solution to boiling. In fact, after a gestation period to reach

boiling, they were able to boil away the entire liquid electrolyte in less than an hour in each positive case.

Balanced Scientific Evaluations and Reference Material

Several excellent scientific reviews of the cold fusion field are highly recommended. Scientists who want to learn more about the remarkable progress in this field should examine:

- Dr. Edmund Storms (Los Alamos National Laboratory), "Review of Experimental Observations About the Cold Fusion Effect, " accepted for publication in Fusion Technology, 1991.

- Dr. M. Srinivasan (Bhabha Atomic Research Centre, Bombay, India), "Nuclear Fusion in an Atomic Lattice: Update on the International Status of Cold Fusion Research," Current Science, 25 April 1991.

Several other important reference documents exist:

- Investigation of Cold Fusion Phenomena in Deuterated Metals (four volumes), by the National Cold Fusion Institute (Salt Lake City), June 1991, now available from NTIS.

- Steven E. Jones, Franco Scaramuzzi, and David Worledge (editors), Anomalous Nuclear Effects in Deuterium/Solid Systems, American Institute of Physics Conference Proceedings 228, 1991.

The technical journal published by the American Nuclear Society, Fusion Technology, formerly was exclusively devoted to hot fusion. Since September 1989, under the courageous editorship of Professor George Miley, this journal has regularly had an extensive section devoted to cold fusion. Other journals that have continued to carry cold fusion articles have been The Japanese Journal of Applied Physics and The Journal of Electroanalytical Chemistry, where the first cold fusion paper appeared. An extremely helpful newsletter on cold fusion, Fusion Facts, is published by the Fusion Information Center in Salt Lake City. Regrettably, the journal Science of the American Association for the Advancement of Science, and Nature, which is published in England, have both waged campaigns of neglect and occasionally mockery of cold fusion research. These magazines will eventually have to alter that stance, which will be profoundly embarrassing for them.

The Question of Reproducibility

Cold fusion effects have not always been easy to reproduce, but that does not make them any less real. The difficulties with reproducibility, however, are rapidly disappearing as researchers discover the conditions required to provoke the phenomena, such as sufficient deuterium loading of metal lattices, specific metallurgical requirements, and peculiar triggering mechanisms. Some experimenters now report very regular appearances of cold fusion phenomena, such as neutron bursts, tritium production, and excess power.

Critics of cold fusion research have regularly dismissed positive results simply because the effects have not always been repeatable. This is a remarkably naive view that makes one wonder where these scientists obtained their technical educations! Of course there are many natural phenomena that are highly erratic, not repeatable, and definitely not predictable, such as meteorite falls, lightning strikes, earthquakes, and the elusive "ball lightning." There are also a host of modern technical devices that will not function if subtle, sometimes poorly understood composition parameters are askew; semiconductor electronic devices are good examples of this. It is not so surprising that the exotic cold fusion phenomena are subject to similar difficulties.

Negative Results Are Not Necessarily Negative!

The worst miscarriage of science in the cold fusion story has been the absurd habit of skeptics to "argue from authority" - to suggest that because certain "good" laboratories got allegedly negative results in their attempts to replicate cold fusion in the spring and summer of 1989, that the positive results of "not so good" laboratories should be disregarded. This kind of illogic, of course, never made any sense. At the moment, "argument from authority" makes even less sense.

It is shocking but true, but in the case of three major research groups that had supposedly negative results in the spring and summer of 1989 - Caltech, the Harwell Laboratory in England, and MIT - there now appear to be significant questions about their work. Three scientists have found simple algebraic errors in the Caltech work, and have written to Nature magazine, but Nature refuses to publish these corrections. In MIT's case, serious questions about the methods used to evaluate power results have arisen. The unpublished data appear to show indications of excess heat, but the published version has been altered to remove these indications. When asked about this problem, the M.I.T. researchers expressed surprise. They have attempted to explain away this and other major problems with increasing numbers of weak and contradictory explanations.

Bringing the shortcomings of these supposedly negative results to the attention of the scientific community has been all but impossible. Powerful publications with anti-cold fusion

stances have been unwilling to air technical questions about the Caltech, MIT, and Harwell work.

Theories of Cold Fusion

When conventional (low temperature) superconductivity was discovered accidentally in 1911, there was no physical theory that could explain it, nor was there any such theory for about the next half century. The much discussed high-temperature superconductivity, which appeared in 1986-1987, still has no satisfactory theory to account for it. Yet industries and governments are bent on developing and commercializing it. The same should be true for cold fusion. However, because cold fusion seems to be an even more radical departure from conventional physics wisdom than high temperature superconductivity, and because of the past reproducibility problems of cold fusion, the latter has not been accepted as readily as high-temperature superconductivity.

But because we can't explain something perfectly at the moment does not give us the right to say that it doesn't exist - the unfortunate attitude of some physicists about cold fusion! Cold fusion does not operate like hot fusion; that has been clear from the start. It must have some other explanation. Happily, several courageous theorists, including physics Nobel laureate Julian Schwinger, have put forth remarkable theories to explain cold fusion. Each of these theories might explain this astounding new physical phenomenon.

Cold Fusion Conferences

Since the infamous, highly negative "Cold Fusion Research" report that was performed for the Energy Research Advisory Board of DOE in November 1989 (which ironically ignored the positive results of the Electric Power Research Institute/National Science Foundation conference the previous month), there have been several notable conferences on cold fusion:

- First Annual Conference on Cold Fusion, Salt Lake City, March 1990.
- Anomalous Nuclear Effects in Deuterium/Solid Systems, Provo, Utah, October, 1990.
- USSR: Conference on Cold Fusion under the auspices of the Soviet Academy of Sciences, March, 1991.
- Second Annual Conference on Cold Fusion, Como, Italy, June-July, 1991.

- Conference on cold fusion in Japan in October 1991. The next conference is scheduled for January 27, 1992, principal sponsors are Nagoya University, the JSME and the IEEE.

The Third Annual Cold Fusion Conference is also scheduled to be held in Japan in the fall of 1992, an appropriate location given the increasing interest in cold fusion by the Japanese scientific community.

What Will Happen to Hot Fusion?

If cold fusion can be adapted to make practical energy-producing devices, a prospect that seems increasingly likely, no one is going to want or need the immensely more expensive and complex hot fusion reactors. An energy source that two years ago exceeded energy break-even, that does not appear to produce harmful radiation, and that uses an abundant (essentially infinite) fuel supply will win hands down over hot fusion. That is why there has been so much negativism from the hot fusion community. If cold fusion works, the hot fusion program will die. It's as simple as that, and the hot fusion people know it. But many valuable scientific and engineering advances have already come from hot fusion development. The work in plasma physics that the hot fusion program conducts is scientifically valuable in its own right. It should be continued whatever the fate of the hot fusion reactor program.

To Good to Be True?

Probably the most difficult hurdle in trying to come to terms with cold fusion is that it seems too fantastic scientifically, and "too good to be true" economically and socially. But the same could have been and was said about many other technological revolutions as they began to happen. If it is real and is useful in practical power-producing devices, cold fusion will likely revolutionize the world in ways we can barely begin to imagine. I believe that there little doubt that cold fusion is real. We must support cold fusion research with every resource at our disposal and try to make this new form of nuclear energy viable. The stakes have never been higher. We should remember the sentiment of the famous scientist, Michael Faraday, in the last century, to whom we owe our revolutionary electrically powered civilization. He wrote, "Nothing is too wonderful to be true."

Groups Reporting Cold Fusion Evidence, *Continued*

Investigators	Institution	Heat	Tritium	Neutrons	γ-rays	^{4}He	Report Type*
Storms & Talcott	Los Alamos NL	X	X				3
Szpak	Naval Systems, San Diego	X	X		X		6
Tachikawa	JAERI, Japan			X?			4
Takagi	TIT, Japan			X			4
Takahashi	Osaka U, Japan		X?	X			1
Taniguchi	OPRRT, Japan			X			1
Tian, Z.W.	Xiamen U, China			X			2
Venkateswaran	BARC, India		X	X			
Wada	Nagoya U, Japan			X			1
Wadsworth & Gururwamy	NCR/U of Utah	X					2
Wakabeyashi	PRC, Japan		X?				4
Wan	National Ching-Hwa U, Taiwan	X	X				5
Wang, D.L.	Nuclear Energy Inst, Shichuan, China			X			2
Wang, G.G.	Nanjing U, Nanjing, China	X		X			2
Werth	Englehard Industries	X					
Wolf	Texas A&M U			X			2
Xiong, R.H.	SW Nuclear Phys Inst, Shichuan, China			X			2
Yeager & Adzic	Case Western U	X	X				2
Zelensky	Kharkov Inst, Russia		X	X	X		3
Zhou, H.Y.	Beijing Normal U, China		X	X			2

TOTAL NUMBER OF GROUPS: 92
NUMBER OF COUNTRIES: 10 (U.S., Japan, India, Italy, USSR, Canada, W. Germany, China, Bulgaria, Spain)

Dr. F.G. Will, Director of The National Cold Fusion Institute
Courtesy of The National Cold Fusion Institute
September 12, 1990
* Key: 1 = Refereed Journal Publication
2 = Conference Proceedings
3 = Nonrefereed Report
4 = Conference Presentation
5 = Newspaper Article
6 = Personal Communication
7 = Submitted to Journal

temperature probably exceeding 1064°C. The temperature of the plate's steel sample holder rose 50°C. The heat may have had nothing directly to do with the fusion reactions that evidently gave rise to the neutrons, but it was a most unusual new phenomenon in a field that seems to grow more and more curious. "Cold Nuclear Fusion Induced by Controlled Out-Diffusion of Deuterium in Palladium" was what Yamaguchi and Nishioka called their approach.

In July came *really hot* cold fusion! The cold fusioneers met in

Groups Reporting Cold Fusion Evidence

Investigators	Institution	Heat	Tritium	Neutrons	γ-rays	^{4}He	Report Type*
Adams/Criddle	U Ottawa, Canada	X	X				4
Alquasmi	U Kiel, W. Germany		X	X			4
Altkin	Perm State U, USSR		X				3
Appleby	Ctr. El. Chem. Energy, Texas A&M	X					2
Arata	Kinki U, Japan			X			1
	Belorussian State U, USSR	X					3
Bertin	U. Bologna, Italy			X			
Blanco	Oak Ridge NL			X			
Bockris	Texas A&M U	X	X				1,2
Bose	BARC, India		X	X			1
Cai	Chinese Acad Sci, China	X		X			2
Celani	Frascati Res Ctr			X	X		1
Clayton	Los Alamos NL		X	X			3
Cherepin	Metal Phys Inst, Kiev, Russia		X			X	3
Dash	Portland State U	X					4
De Marie	U Roma, Italy	X		X			4
Die, D.Z.	Nuclear Energy Inst, Shichuan & Beijing			X			2
Eng. Group	NCFI/U of Utah	X	X				6
Faler & Vegors	Idaho State U			X			5,6
Fukada	Kyushu U, Japan			X			4
Gao, G.T.	Eng Phys Inst, China			X			
Gou, Q.Q.	Science & Tech Inst, Chendu	X				X	2
Gozzi	U Roma, Italy	X	X	X			1
Huggins	Stanford U	X					2
Hutchinson	Oak Ridge NL	X					2
Ikegami	NFSI, Japan		XI	X			5,6
Ikezawa	Chubu U, Japan			XI			3
	Barc, India		X	X			1
Jones	Brigham Young U			X			1
Jordon	Case Western U			X			
Jorne	U Rochester			X			4
Krishan	Indira Gandhi Ctr, India			X			

(continued)

Groups Reporting Cold Fusion Evidence, *Continued*

Investigators	Institution	Heat	Tritium	Neutrons	γ-rays	^{4}He	Report Type*
Krishnan	BARC, India		X	X			1
Kuzmin	Moscow State U	X	X	X			3
(no name)	Karpov Inst, Russia	X					3
Landau	Case Western U			X			4
Liebert	U Hawaii	X					4
Maeda	KURRI, Japan			XI			4
Mathews	Indira Gandhi Ctr, India		X	X			
McBreen	Brookhaven NL	X	X				4
McKubre	SRI International	X	X				2
Menlove	Los Alamos NL			X			2
Miles	Naval Weapons Ctr	X				(4He)	1,2
Millikan	UC Santa Barbara	X		X			4
Mizuno	Hokkaido U, Japan			X			1
	U Mexico, Mexico		X				
Nayar	BARC, India		X	X			
Nilmura	Tohoku U			X			4
Noninski	Sofia, Bulgaria						1
Ohta	U Tokyo, Japan	X					4
Okamoto	TIT, Japan			X			4
Orlani	U Minnesota	X					4
Ozawa	Hitachi, Japan			X			4
Oyama	TAT U, Japan	XI					4
Pons & Fleischmann	NCFI/U of Utah	X	X				1
(no names)	Qinhua U, Beijing, China		X	X	X		2
Radhakrishnan	BARC, India		X	X			1
Raghavan	AT&T			X			
Raj	BARC, India					X	1
Rout	BARC, India			X			1
Saini & Raye	BARC, India	X	X				6,7
Sakamoto	Tokai U, Japan			X			4
Sanchez	U Madrid, Spain		X	X			1
Santharam	Tata Inst, India	X					
Scaramuzzi	Frascati, Italy		X	X			1
Schoessow	U Florida	X	X				6
Scott	Oak Ridge NL	X		X	X		2
Seminoz-henko	All-Union Inst, Monocrystals, Russia		X			X	3
Shyam	BARC, India			X			
Srinivasan	BARC, India		X	X			1

(continued)

ISEM-Nagoya

THE INTERNATIONAL SYMPOSIUM ON NONLINEAR PHENOMENA IN ELECTROMAGNETIC FIELDS

Nagoya Congress Center, Nagoya, Japan
January 26-29, 1992

FINAL CALL FOR PAPERS

Applied e[illegible], nuclear fusion reactor technology is now expanding its boundaries in line with the advent of such new materials as high temperature superconductors, new ceramics, magnetic fluids. Developments in computer engineering as well as the recent rapid advance in fuzzy theory and neuro-computing greatly promote the expansion by enabling analyses on electromagnetic/nonlinear phenomena.

The fourth International Symposium on Nonlinear Phenomena in Electromagnetic Fields (ISEM-Nagoya) will be held on January 26-29, 1992 in Nagoya, Japan, to stimulate the exchange of innovative ideas and to promote more advanced investigation in nonlinear phenomena in electromagnetic fields.

The ISEM-Nagoya is organized by Nagoya University, in cooperation with the Japan Society of Applied Electromagnetics, the Industrial Electronics Society of IEEE, the Japan Society of Mechanical Engineers, the Institute of Electrical Engineers of Japan, the Magnetics Society of Japan, the Japan Society of Magnetic Fluid Research, the Japan Society of Precision Engineering, the Japan Society of Applied Physics, and Nagoya Industrial Science Research Institute. This symposium also has the sponsorship from the Ministry of Education, Science and Culture in Japan, Aichi Prefectural Government, Nagoya Municipal Government, and Nagoya Chamber of Commerce and Industry.

TOPICS within the scope of the symposium will include:

1. Electromagnetic phenomena I (advanced computation, inverse problem & nondestructive testing).
2. Electromagnetic phenomena II (molten metals, magnetic fluids, magnetic materials).
3. Biological electromagnetic phenomena.
4. Mechanism and dynamics of new actuators (piezoelectric actuator, microactuator, magnetic levitations & linear drives, magnetic bearing).
5. Application and physics of high temperature superconductivity(materials of high Tc, application of high Tc).
6. Application of neural networks, fuzzy theory and chaotic motion in nonlinear analysis/control.
7. Physics of cold fusion.

Jan. 27, '92

Dr. Ikegami of the National Institute for Fusion Science will chair this meeting. Featured speakers include Dr. Srinivasan of BARC, India, and Dr. Takahashi. Dr. Ikegami asked me to remind everyone that it is not to early to submit a paper for Third Annual CF meeting he is hosting in October 1992, contact him for information.

A cold fusion seminar was included in the big Japan Nuclear Energy Conference in October 1991. This ISEM meeting is yet another big, establishment meeting in Japan where Cold Fusion is on the agenda, just like micromachines, high temperature superconductivity, or any other advanced experimental new field. The Japanese physics establishment, including the IEEE, takes cold fusion seriously, so we need to get our side on the ball too.

Cold Fusion Research Advocates
2060 Peachtree Industrial Court, Suite 312-F
Chamblee, Georgia 30341

Phone: 404-451-9890 Fax: 404-458-2404

September 16, 1991

MEETING IN JAPAN

The "Japan Nuclear Energy Conference" (Nippon Genshiryoku Gakkai) is scheduled October 15 - 18, at Kyushu National University, Engineering Department, Fukuoka City. This is an annual conference, attended by academic and commercial researchers in fission and fusion. The meeting is open to anyone who wants to attend, admission is 3,000 yen ($25) at the door. The organizers are in Tokyo: 03-3508-1261, fax 03-3581-6128.

The schedule calls for ten Cold Fusion papers to be read on the first day, in Meeting Hall C. This is a big, mainstream, annual event, in which Cold Fusion is treated like any other branch of physics. This proves beyond question that the Japanese physics establishment takes this research seriously. Mr. Nakano tells me that more and more support for Cold Fusion is coming from the Hot Fusion scientists. In the U.S. we have a bitter turf war between these two groups, whereas in Japan they are cooperating. Cold Fusion sessions like these would never be held at a U.S. Nuclear Energy Physics conference.

I have only one fax, with the schedule from Day 1, Meeting Hall C, and a map showing Halls A - D, and conference rooms H - M. Obviously, the meeting organizers expect enough listeners to fill a hall, rather than a smaller conference room. I apologize for the poor quality of this fax.

The Cold Fusion researchers are from the biggest of the top National Universities: Tokyo U., Osaka U., plus a group Hokkaido U., including Dr. Mizuno, one of Japan's leading Cold Fusion researchers. The Osaka group includes a researcher "on loan" from Matsushita Corp. There is also a group from Tokyo Institute of Technology, which is called "Japan's M.I.T." Many of these groups appeared Como, Italy.

Here is a summary of the document:

(Events C1 - C18)

MEETING HALL C (Soundproofed. Lecture Hall 103)

DAY 1 (October 15)

The left column shows morning lectures, numbered C1 - C8, which are about conventional hot fusion. Conventional sessions from 9:30 to 11:50 include:

Neutron Applications
Short pulse neutron scattering techniques
Ultra Cold Radiography Test
Plans for reactor
The JRR-2 Medical Ion Beam Reactor
Neutron Device (lead spectrometer)
and so on...

"Fusion Reactor Researchers Assoc." 32nd Annual Meeting 12:00 - 13:00

Latest results from the Superconducting Tokamak Fusion Reactor TRIAM-1M 13:00 - 13:45

First experiments with JT-60, recently rebuilt with super large electric current 13:45 - 14:30

The right column covers the Cold Fusion Sessions C9 - C18, which begin at 14:30

Fusion (Cold Fusion)

Chairman M. Okamoto, Tokyo Institute of Technology 14:30 - 15:40

C9 Pulsed control experiments with Pd - D_2O type cell

Osaka U., Speaker T. Takeuchi. Group also includes A. Takahashi, I. Iida, S. Yoshida, A. Mega and M. Watanabe who is visiting from Matsushita Corporation Research.

C10 Spectral analysis of neutrons. Osaka U. Group, Speaker is A. Mega

C11 Tritium level measurements. Osaka U. Group, Speaker is S. Yoshida.

C12 Measurement of charged particles from cold fusion reaction.

Osaka U., speaker is M. Honda.

10 minute discussion lead by chairman

Chairman M. Nakazawa, Tokyo University 15:40 - 16:30

C13 Unusual bursts of charged particles observed during cold fusion electrolysis

Osaka City U., speaker is Y. Taniguchi, with T. Yamamoto

C14 High temperature, high pressure heavy water Pd cathode electrolysis experiments

Hokkaido U., speaker is R. Taniguchi. Group includes: T. Makuragi, T. Mizuno, K. Yasuzumi.

C15 Verified D-D fusion in normal temperature condensed matter

Tokyo Inst. of Technology, speaker is M. Nakamado. Group includes: N. Takahashi, T. Kusunoki, M. Okamoto, Y. Fujii.

5 minute discussion lead by chairman

Chairman A. Takahashi, Osaka University 16:30 - 17:20

C16 Measurements of fusion byproduct particles during KeV Ion Beam irradiation of deuterated titanium

Kobe Shosen U., speaker is N. Seki. Group includes: A. Kitamura, Y. Furuyama, M. Nakajima.

C17 Wave structure seen in rate of reaction

Kokyu Research, speaker is N. Yabunouchi.

C18 Cold Fusion Super-Star observations

Hokkaido U., speaker is T. Matsumoto.

5 minute discussion lead by chairman

Reception at Reesento Hotel 18:00 - 20:00

C 会 場 （防音・103番講義室）

○は口頭発表者
発表10分，討論 5 分（計15分）

第 1 日（10月15日）

会場責任者
坂元 重樹
高橋 亮人

中性子応用（冷中性子）
座 長（武工大）相沢乙彦 9 : 30〜10 : 25

C 1 中性子散乱実験用ショートパルス熱中性子減速材
（北大）○鬼柳善明，岩佐浩克

C 2 結合中性子源装置の設計研究．（Ｉ）
前段用冷中性子源についての検計算
（京大）○坂本幸一，（京大炉）川端祐司，
宇津呂雄彦

C 3 京都大学炉（KUR）極冷中性子導管の特性測定と
極冷中性子ラジオグラフィーの試み
（京大炉）○川端祐司，米田憲司，宇津呂雄彦，
（京大）坂本幸一

（座長持ち時間 10分）

中性子応用（照射場）
座 長（立大）小林久夫 10 : 25〜11 : 00

C 4 原子炉中性子照射場の設計．（Ｉ）
医療用照射場の性能向上
（武工大）相沢乙彦

C 5 JRR-2医療照射設備の設置
（原研）○行金賢次，山田忠則，根本傳次郎，
番場正男，河原井邦雄，高橋秀武

（座長持ち時間 5分）

中性子機器（鉛スペクトロメータ）
座 長（北大）鬼柳善明 11 : 00〜11 : 50

C 6 電子線型加速器と組み合わせた鉛減速スペクトロメータ
の開発
（京大炉）○中込良広，小林捷平，山本修二，
藤田薫顕，（京大）山中章広，金沢 哲，
木村逸郎

C 7 ————．（Ⅱ）；特性実験
（京大）○山中章広，金沢 哲，木村逸郎，
（京大炉）小林捷平，中込良広，山本修二，
藤田薫顕

C 8 ————．（Ⅲ）；中性子スペクトル
（京大炉）○小林捷平，李 兆桓，藤田薫顕，
山本修二，中込良広，（京大）山中章広，
木村逸郎，金沢 哲

（座長持ち時間 5分）

「核融合炉研究連絡会」第32回会員総会 12:00〜13:00

〔招待講演 1〕座長（東工大）岡本眞實 13:00〜13:45
超伝導強磁場トカマクTRIAM-1Mの成果
（九大）伊藤智之

〔招待講演 2〕座長（阪大）西川雅弘 13:45〜14:30
JT-60大電流化改造と初期実験結果
（原研）細池 實

核融合（低温核融合）
座 長（東工大）岡本眞實 14 : 30〜15 : 40

C 9 パルス電流制御を用いたPd-D_2O系電解実験．（Ｉ）
中性子測定
（阪大）○竹内孝之，高橋亮人，飯田敏行，
吉田茂生，目賀章正，（松下中研）渡辺正則

C10 ————．（Ⅱ）；中性子スペクトル
（阪大）○目賀章正，高橋亮人，飯田敏行，
吉田茂生，竹内孝之，（松下中研）渡辺正則

C11 ————．（Ⅲ）；トリチウム測定
（阪大）○吉田茂生，高橋亮人，飯田敏行，
竹内孝之，目賀章正，（松下中研）渡辺正則

C12 常温核融合における荷電粒子測定
（阪大）○本田光生，飯田敏行，高橋亮人

（座長持ち時間 10分）

座 長（東大）中沢正治 15 : 40〜16 : 30

C13 重水電解中に見られる異常な荷電粒子バースト
（東大）○谷口良一，山本孝夫

C14 加熱，加圧下の重水中におけるPd電極のカソード
電解実験
（北大）○松本 正，小野崎康，奥仲和久

C15 常温凝縮相におけるd-d核融合反応の実験的検証
（東工大）○中田睦宵，高橋慶之，橘 文弘，
岡本眞實，藤井靖彦

（座長持ち時間 5分）

座 長（阪大）高橋亮人 16 : 30〜17 : 20

C16 keV重水素イオンビーム照射下の重水素化チタン
からの核反応生成荷電粒子の測定
（神戸商船大）○岡 伸彰，北村 晃，古山雄一，
中島 雅

C17 反応率の波動機構
（高研）薮内意雄

C18 常温核融合，Super-Starの観測
（北大）松本高明

（座長持ち時間 5分）

懇 親 会（リーセントホテル） 18:00〜20:00

第 2 日（10月16日）

会場責任者
村尾 良夫
清水 彰直

原子力安全工学（単純化BWR静的格納容器）
座 長（日立）内藤正則 9 : 00〜10 : 40

(C19〜25) シリーズ 単純化BWR 静的格納容器冷却系
の除熱性能の基礎試験

2014287
BOOK WORLD - The Fizzle in the *Fusion*.
The Washington Post, May 15, 1991, FINAL Edition
By: Robert L. Park
Section: Style, p. b04
St[illegible] Type: Review
L[illegible]Count: 68 Word Count: 752

TOO HOT TO HANDLE

The Race for Cold Fusion

By Frank Close

Princeton University Press. 376 pp. $24.95 END NOTES

At a news conference in Salt Lake City on the morning of March 23, 1989, the University of Utah announced that two chemists, Martin Fleischmann and Stanley Pons, had corralled the source of the sun's energy in a test tube of heavy water. For 5 billion years, the sun has warmed the Earth with energy derived from the fusion of hydrogen atoms to form helium. Fortunately, even in the fierce caldron of the sun, fusion is a slow process that will continue unabated for billions of years. Small wonder that humankind had thus far failed to harness fusion on Earth.

The two chemists refused to reveal the details of their experiment, setting off a wild chase involving scientists all over the world, a chase complete with dead ends, false leads and roadblocks. Frank Close, a British theoretical nuclear physicist and author of popular books on science, has produced an authoritative account of the "race for cold fusion."

[illegible]as, the scientists were chasing a shadow; the only secret was that t[illegible] was no secret. Most physicists had been skeptical from the beginning. The energy released in fusion is carried away by neutrons and gamma rays; heat is generated only when these deadly particles are stopped by surrounding materials--and they are very hard to stop. At the power levels the two chemists were claiming, their test cell should be emitting a trillion neutrons and gamma rays each second. The author describes a photograph that appeared in newspapers around the world:

"So Fleischmann and Pons appeared to have made a table-top reactor and instead of surrounding it with a protective shield of concrete blocks and lead against what would have been a lethal dose many times over, there they are holding a cell in their hands, in jackets and ties, proudly in front of what may be the most concentrated nuclear source west of Chernobyl. And they are manifestly alive."

As one of the skeptics, I explained all this to a reporter a few days after the announcement. The reporter asked the obvious question: "If everyone knows it's wrong, why are they all doing it?" Why indeed? With dozens of laboratories attempting to confirm the Utah claim, it was inevitable that a few would screw up. A loose wire, electrode contamination, calibration errors, quirky detectors, all got reported as "anomalous effects" and treated as "partial confirmations."

Clearly, such a story has elements of high comedy, but the author resists the temptation to ridicule. Although written in language an i[illegible]ested layman can follow, this is a scientist's account, and the e[illegible]ode is treated as a scientific detective story. The reader is led through the maze of contradictory reports that gradually began to sort

themselves out. In five weeks it was all over; the few remaining puzzles would be found to have mundane explanations, additional sources of error would be tracked down, scientists would return to more productive lines of research.

But the story of cold fusion was shaped less by flawed science than by common human frailties: greed, ambition, vanity. Unfortunately, the author is not much help in understanding the characters in this drama. The reader is left wondering what in the backgrounds of the two chemists could explain how they so easily abandoned a lifetime of scientific training.

Close asks in the first chapter, "Was this a delusion, an error, or a fraud?" By the end of the book, it is clear that cold fusion progressed through all three. What began as wishful interpretations of sloppy and incomplete experiments ended with altered data, suppression of contradictory evidence and deliberate obfuscations.

After a while, the objective seems to have been just to prolong the inevitable--but Fleischmann and Pons were no longer alone. Inept scientists whose reputations would be tarnished, greedy administrators who had involved their institutions, gullible politicians who had squandered the taxpayers' dollars, lazy journalists who had accepted every press release at face value--all now had an interest in making it appear that the issue had not been settled. Their easy corruption was one of the most chilling aspects of this sad comedy.

To be sure, there are true believers among the cold-fusion acolytes, just as there are sincere scientists who believe in psychokinesis, flying saucers, creationism and the Chicago Cubs. The lesson from "Too Hot to Handle" is that a PhD in science is not an inoculation against foolishness--or mendacity.

The reviewer is professor of physics at the University of Maryland and director of the Washington office of the American Physical Society.

NAMED PERSONS: FLEISCHMANN, MARTIN; PONS, STANLEY
DESCRIPTORS: Books

2011394
Two New Theories on Cold *Fusion* Swiftly Produce Heat Among Scientists.
The Washington Post, April 26, 1991, FINAL Edition
By: Curt Suplee, Washington Post Staff Writer
Section: A SECTION, p. all
Story Type: News National
Line Count: 60 Word Count: 656

Cold *fusion*, the all-but-utterly discredited notion that once promised to create cheap nuclear power in a jar of water, made a tentative bid for credibility yesterday as two groups of researchers separately announced new explanations for the controversial phenomenon.

Both announcements, from private companies that are publishing in the same scholarly, peer-reviewed journal, invoke radically unorthodox theories of atomic behavior as well as extraordinary claims. One of the announcements was publicized by the news office of the Massachusetts Institute of Technology, ordinarily a bastion of scientific orthodoxy, even though the research was done elsewhere.

Physicists Frederick Mayer and John Reitz of Ann Arbor, Mich., called a

ER-16
WPolansky/ses
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ER-10
Liaison
2/5/92

ER-61
DMayhew
2/6/92

Mr. Jed Rothwell
2060 Peachtree Industrial Court
Suite 312-F
Chamblee, Georgia 30341

Dear Mr. Rothwell:

The Department of Energy has been asked to respond to your letter, with several enclosures, to President Bush dated January 10, 1992, that expressed concern about cold fusion research in the United States. Your letter and information on cold fusion have been reviewed. We have been aware of the activities you referenced including the effort to establish a $10 million per year research program on cold fusion.

We believe that the appropriate mechanism for establishing the feasibility of a new, or unexpected, scientific result is for the proponent to submit it to the collective judgement of peers. This mechanism can be activated in several ways, such as technical presentations at scientific conferences and publications of results in prestigious, peer-reviewed journals. This is a thorough, but time-consuming process. However, all major scientific advances have been subjected to, and have survived, this scrutiny.

The November 1989 report of the cold fusion panel recommended against any special Department of Energy funding for the investigation of phenomena attributed to cold fusion. However, the panel was sympathetic toward modest support for carefully focussed and cooperative experiments within the present funding system. The Department of Energy accepted the report and its recommendations. We have been monitoring the cold fusion research area since the issuance of that report and believe that its recommendations are still valid. We continue to be available to review any research proposal of interest to the Department.

Sincerely,

Walter M. Polansky, Director
Division of Advanced Energy Projects
Office of Basic Energy Sciences, ER-16

cc:
Ms. Sally Kelley
Director of Agency Liaison
The White House

bcc: ES/1, ER-61, ER-622/FSTL

ER-16:WMPolansky:SueEllen:3-5995:2-5-92:C:\ESCONTRL\Rothwell.92:wp5.1

ES#92-001401 ER#92-000503 Due Date: 2/7/92