ER-16
WPolansky/ses
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ER-10
LIanniello
4/16/91

ER-61
DMayhew
4/17/91

APR 19 1991

Mr. Walter L. Wagner
Development Director
STAFUS
1126 12th Avenue, Suite 105
Honolulu, Hawaii 96816

Dear Mr. Wagner:

Your March 19, 1991, letter to President Bush that expressed concern regarding the lack of Federal funding for cold fusion research has been referred to me for reply.

Since our previous communication with you nearly a year ago, there has been some change in the status of cold fusion. It appears that the number of cold fusion research efforts has declined and that results indicative of cold fusion continue to be viewed with skepticism. Also, the failure of others to reproduce reported results remains a crucial issue.

The Department of Energy continues to monitor claims attributed to cold fusion. We were aware that the University of Texas-Naval Weapons Center collaboration were reporting helium production from the electrolysis of heavy water in palladium. Hopefully, this information will be published soon in a scientific journal and will foster a comprehensive peer review of the overall experiment. All major scientific discoveries to date have been subjected to and have survived the peer review process. It is intense and time consuming, but it works!

The Department of Energy continues to be receptive, at a modest scale and through its regular funding process, to high quality research proposals on cold fusion aimed at understanding the pertinent physical phenomena. The Division of Advanced Energy Projects is the appropriate office for the submission of such proposals.

Thank you for writing to the President. Your views have been noted and given serious consideration.

Sincerely,

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

bcc: ES/1, ER622/FSTL (3)

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ES#91-006904 ER#91-01025 Due Date: 4/17/91

Department of Energy
Washington, DC 20585

Mr. Walter L. Wagner
Development Director
STAFUS
1126 12th Avenue, Suite 105
Honolulu, Hawaii 96816

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

From: KELLY, SALLY
WAGNER, WALTER L.

To: AD

Doc't date: 04/08/91 Doc't no: ER 9101025
Recvd date: 04/08/91 E.S. no: 91-006904
Due date: 04/17/91 Rte sym: ER-16
Comp date: Other id: KELLY
Doc't type: MEM State code:

Subject: CONCERNS ABOUT COLD FUSION

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KELLEY,SALLY DC O
WAGNER,WALTER L HI C
STAFUS

SUBJ: NUCLEAR
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CONCERNS ABOUT "COLD FUSION"

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REFERRAL

APRIL 8, 1991

TO: DEPARTMENT OF ENERGY

ACTION REQUESTED:
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DESCRIPTION OF INCOMING:

ID: 226563

MEDIA: LETTER, DATED MARCH 19, 1991

TO: PRESIDENT BUSH

FROM: MR. WALTER L. WAGNER
STAFUS
1126 12TH AVENUE
SUITE 105
HONOLULU HI 96816

SUBJECT: MORE CONCERNS ABOUT "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

008807

ID # 226563

OF

WHITE HOUSE
CORRESPONDENCE TRACKING WORKSHEET

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☐ H - INTERNAL
☒ I - INCOMING
Date Correspondence Received (YY/MM/DD) 91/3/25

Name of Correspondent: ☐Mr ☐Mrs ☐Miss ☐Ms. Walter L. Wagner

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

Subject: More concerns about "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
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	Referral Note:				
DOE	F	91/04/08	LJ		91/04/1
	Referral Note:				
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A - Appropriate Action
C - Comment/Recommendation
D - Draft Response
F - Furnish Fact Sheet to be used as Enclosure
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STAFUS

Walter L. Wagner

The Static Fusion Development Company A Subsidiary of Solaria Institutes

1126 12th Ave. Suite 105 Honolulu, Hawaii 96816 (808) 735-5815

USE: 3440 S. 7120 W. Suite 2
Salt Lake City, UT
84120
(801) 250-8136

March 19, 1991

President George Bush
The White House
Washington, D.C.

Dear President Bush:

Eleven months ago I wrote to you concerning "cold fusion", a copy of which correspondence is enclosed for your review.

Contrary to my expectations, the D.O.E. has so severely politicized itself against this area of research, that no monies are being made available; contrarily they continue to fund "hot fusion" (as they have for the past three decades) to the tune of multi-million dollar expenditures, nothwithstanding the expectation that "hot fusion" cannot be made to work in the next 20-30 years. (see enclosed newspaper clipping re D.O.E.)

Last week, the Naval Weapons Station in China Lake, California, announced the most important results to date in "cold fusion" research. Specifically, they are the first research team to find the nuclear "ash" of a fusion fire with unmistakable clarity. They have, in essence, fully confirmed the results of Dr. Pons and Dr. Fleischmann, first announced two years ago.

I am enclosing for your reference the pre-print of their results, as well as the National Cold Fusion Institute summary of those results, which also concludes that "cold fusion" is fully confirmed.

In that "cold fusion" will prove to be the most revolutionary technology in society since the discovery of coal, it is imperative that the United States take the forefront in this research. Again, I ask for your support in fostering this research.

While I realize you have many other concerns on your mind, I can assure you that development of "cold fusion" will insure that the U.S. will never again be a hostage to mid-east oil.

Yours very truly,

Walter L. Wagner

Walter L. Wagner
Development Director

March 19, 1991

To the person who first reviews this correspondence:

Thank you very much for having properly reviewed the previous correspondence sent to President Bush last year, and insuring that he had the opportunity to review it personally.

The same statements made in my previous correspondence, and cover letter, remain true.

I respectfully request that you insure that President Bush again personally has the opportunity to review the enclosed material, as it ultimately is a matter of great national concern.

So that you might know that President Bush and I are thinking along similar lines in other matters of great national concern, I am enclosing for your review a copy of correspondence written to a friend serving in Saudi Arabia, several weeks before the war was initiated.

Incidentally, though not mentioned in my letter to President Bush, you might wish to make him aware that a major international conference will be held late this June in Italy on cold fusion research. The U.S. would do well to have official participation, in addition to participation by the D.O.E. (if they're attending).

Thank you for insuring that this material is forwarded to President Bush for his personal review.

Sincerely,

Walter L. Wagner

Walter L. Wagner

January 1, 1991

LtCol Robert W. Scott, USAF
549-78-7695
AFSOC Deployed / A-3-4
APO NY 09855

Dear Bob:

I just received a very nice Christmas letter from Cindy, and she enclosed your form letter to fill me in on your activities.

I trust you were able to have a semblance of a Christmas in the Moslem world, and I hope the New Year will begin a new beginning for this world bringing lasting peace and prosperity.

It must be very difficult to be away from your loved ones on such an extended basis. We've not experienced this on such a massive scale since the Vietnam era days. (Though our submariners and other Navy personnel still are on 6-month sojourns occassionally.)

Fortunately, the vast majority of the U.S. (and the world!) are in support of you and your endeavours; though I do find a certain element of narrow-minded persons operating in the U.S. trying to end U.S. involvement in world affairs. These are the people who want to enjoy the creature comforts of the U.S., and turn their backs to the world outside the U.S. until it would be too late to do anything about it. Fortunately, they are a small minority still, though they do obtain a following by playing on the fears of the short-term consequences of military action in Saudi Arabia/Persian Gulf. Theirs is an approach of appeasement - that which Neville Chamberlain tried when he proclaimed peace in his time.

It is a difficult situation to analyze the military capabilities of Iraq, and I trust that President Bush has better sources of information than do I. Allow me, however, to interject my own analysis (which you might wish to compare with that held by your superior officers).

The prospect for chemical warfare I believe is very real, and any first strike by the allied forces should be directed at eliminating missile attacks laden with chemicals. Fortunately, I believe we have adequate precautions (e.g., gas masks should work in diluted poison-gas environments), and the winds will blow the gases away without the prospect of long-term effects.

Likewise, missile attacks laden with 'super-bombs' (e.g. propane bombs) could be nasty, and every effort should be made to

prevent these from being launched, or if launched, from arriving at their targets (does the army use those multiple cannons which spray 'bullets' at an incoming missile, like the navy ships are equipped with?)

As to the prospect that the Iraqis have developed a nuclear bomb, I do not believe so for the following reasons.

While the Iraqis have had more than an adequate period of time in which to construct a bomb since the day that Israel destroyed their nuclear reactor which was under construction on the outskirts of Baghdad (nine years ago!?), the evidence given by Hussein, while superficially appearing to lead us to believe he has developed such capability, when fully analyzed appears to be pure bluff. (Again, President Bush should have far better information on this issue than do I at present.)

Recall that the U.S. began a nuclear bomb production program in January, 1942 (shortly after Pearl Harbor), and by December, 1942 the world's first controlled nuclear chain reaction was developed by Enrico Fermi, using a nuclear reactor having wooden beams as its physical support, with a graphite moderator, and unenriched Uranium lumps 'piled' in layers mixed in the graphite, housed in a subterranean 'Squash Court' which was appropriated from the University of Chicago P.E. Department, for secrecy purposes. Thus, it only took less than a year from inception of concept, to fruition, to build a nuclear reactor (which was quickly disassembled thereafter, as it was designed simply to demonstrate proof of concept).

Thereafter, a breeder-reactor was buiilt, and two an one-half years later, sufficient Plutonium had been bred from the U-238 that a nuclear bomb could be built (and was subsequently dropped on Nagasaki). Concurrently, the U.S. also was operating a U-235 enrichment plant using centrifugal separation of isotopes, and it also produced sufficient U-235 to build two bombs by the same time as the Plutonium bomb was completed. One of those was exploded in the U.S (as a test), the other dropped on Hiroshima, three days before the Plutonium bomb was dropped.

As you can see, it does not take very long to build a nuclear bomb, if you can have full governmental support, secrecy, and personnel having the knowledge of how to do so (it's really not very difficult - even my ninth grade students are able to understand the rudiments necessary). Thus, its entirely feasible for Hussein to have constructed several small-scale breeder reactors (which could also breed U-233 from Thorium - 232, instead of Pu-239 from U-238), housed in underground

bunkers to prevent their detection from orbiting satellites equipped with gamma ray detectors. I have read reports that Iraq has had a large-scale Uranium mining industry. However, I do not believe that Hussein has had the foresight or knowledge of how to go about building a bomb, and I do not believe he is anywhere near close to having done so. This is why.

Recent reports in the German press show that Germany (formerly West Germany) has been providing Iraq with centrifugal separation equipment (ostensibly to make enrighed U-235 for an energy-production reactor), right up until the time of the invasion of Kuwait. However, those same reports indicate that this program was still in its infancy, and it would be many years before the equipment already delivered would be able to produce enough U-235 to make one bomb.

It would appear that Iraq was putting all its apples in one basket in choosing the centrifugal enrichment method (presumably due to their large natural deposits of Uranium, reportedly), after having been psychologically shocked by the destruction of their reactor (which could easily have been used as a breeder reactor, as can any reactor designed for large-scale energy production).

Further, as you will recall, earlier this year Hussein proclaimed that he had obtained critical parts for bomb production, even though most of them had been intercepted in their transit to Iraq. Thus, he was in essence bragging that he had what he needed to make a bomb, and that his efforts had not been tharted.

However, a person (or country) which already has a bomb would not want to advertise that fact, if it had been a well-kept secret all along. Rather, he would advertise that fact if he wanted someone to believe he had that capability, when he in fact was bluffing.

Those parts in question were for the timing of chemical explosions in the implosion-method of nuclear bomb detonation (when the bomb is tightly squished together, reaching criticality). Since the nuclear detonation takes about 1/1000 of a second (from the time of the first fission, until all the atoms have fissioned by a chain reaction), the chemical explosives surrounding an implosion-device should all go off within 1/10,000 second, and to be safe, within 1/100,000 second. Otherwise, you'll simply blow up your Uranium or Plutonium into tiny dust particles.

However, the implosion method is simply a more refined method of detonation (the first Plutonium bomb dropped on Nagasaki was reportedly an implosion-method bomb), and not necessary at all in order to make a bomb.

Rather, a person who simply held two lumps of sub-critical U-235, U-233, or Pu-239 in his hands, and brought them together to form a critical mass, would produce a 'fizzile' bomb (it would explode before most of the material had fissioned).

That is the least refined method. A more refined method would use a cannon to propel one lump of fissile material towards another one at the end of the cannon barrel at a speed such that the diameter of the lump (say, 10 cm.) is covered in less than 1/1000 of a second; that is, at a speed of 100 meters/second, or 360 kilometers/hour (200 miles/hour), which is easily obtained by most cannons. This is the method used for the Hiroshima and New-Mexico bombs. (with much greater muzzle velocity)

Thus, when Hussein was bragging about how he had obtained a 'nuclear trigger', he was showing his ignorance of nuclear bombs, and actually indicating that he did not, at that time, yet have a nuclear bomb.

Finally, I believe that there most likely has been foot-dragging by those persons in Iraq who might have the knowledge of how to go about making a nuclear bomb. If they have that knowledge, then they also know that any first-use by Iraq of nuclear bombs would lead to the entire annihilation of their country by a nuclear retaliation. They probably also know that Mr. Hussein would not be reluctant to use nuclear weapons first, if he had them, and thus they are probably not inclined to provide him with them.

In summation, it appears to me that Iraq is presently without nuclear weapons, and that they are no further along than any country just starting out (except that they already have the necessary starting material, Uranium, in large abundance). The above descriptions of nuclear properties are, of necessity, truncated in their presentation, and are not meant to be wholly descriptive; though they are generally correct.

I trust that the above has not bored you, and perhaps even given you some reassurance. The biggest threat, I believe, is preventing incoming missiles from reaching our troops, laden with either conventional explosives or chemical/biological warheads.

To that end, a first strike at night should be designed for taking out as many missile launchers as possible, with destruction of military communications second in priority, and support facilities last.

Personally, I do not believe that we should commit ground forces to engage in close-contact with Iraqi troops, where they would be in the range of small weapons fire and tank cannons. Rather, Iraq should first be deprived of its country's military installations and fixtures, supply lines disrupted, and communications lost. After a few weeks of that, I believe the Iraqi troops in Kuwait would begin to feel trapped and starved, and would be willing to surrender without much close-proximity fighting.

I trust that President Bush and his military commanders likewise feel that we should not endanger our troops by a rash rush to 'invade' Kuwait with a liberating army. Iraq has plenty of 'muscle' in terms of convention weaponry.

Since the first New Moon after January 15 will occur around February 10 (a Sunday), I believe that optimal conditions for a first strike at night would be a few days prior, say February 6, giving the allied forces a full week of near moon-less nights in which to optimize infra-red vision systems.

Delaying a first stike until three weeks after the January 15 deadline also gives the psychological advantage of allowing Mr. Hussein to let his guard down, in believing that the allies believe his bluffs.

On another note:

I've recently moved to Salt Lake City and the snow country! After 2½ years of excellent surfing, I'm ready for continuous downhill racing! I'm working with the Department of Education here, and also am close to our Church headquarters and the Missionary Training Center (where we train tens of thousands of young missionaries in foreign language skills, with several thousand new arrivals each week!).

When you get back from your most recent sojourn, you'll deserve a break! You'll have to bring Cindy and Suzanne to visit, and maybe even get in some excellent spring skiing! (If you don't know how, I'll teach you and her - it's not that difficult)

I've just bought a 4-bedroom, 1½ bath house on a large corner lot, situated 2½ minutes from the freeway and 10 minutes from

downtown Salt Lake City. Can you believe they only want $500.00 as a down payment, and a purchase price of only $42,000.00? The house is 1800 square feet - huge! But that is a typical price for a newer house around here. Unbelieveable considering I was paying $600.00/mos. for a tiny, one-bedroom apartment in Honolulu, and now my house payment is about $450.00/mos. You might even fall in love with the area, and decide you want to live where they have Seasons again!

Take good care of yourself - Cindy and Suzanne love you and miss you, and need you back to take care of them.

I'm mailing Cindy a copy of this letter too. Hopefully, this year (1991) you two will be able to spend Christmas together!

With warmest regards,

Walt Wagner

STAFUS

Walter L. Wagner

The Static Fusion
Development Company
A Subsidiary of
Solaria Institutes

1126 12th Ave. Suite 105
Honolulu, Hawaii 96816
(808) 735-5815

April 14, 1990

President George Bush
The White House
Washington, D.C.

Dear President Bush:

I am writing to you concerning recent developments in nuclear science which will revolutionize energy production world-wide.

At the recent Conference in Salt Lake City, scientists from around the world announced results which have confirmed the claims of cold-fusion in a bottle first advanced last year by Dr. Stanley Pons and Dr. Martin Fleischmann.

Specifically, Dr. Lyengar of the Bhabha Atomic Research Center in Bombay, India announced that his team of scientists have detected tritium in great abundance in their cold-fusion cells - a sure sign of fusion; the Los Alamos National Laboratory has likewise detected tritium, as well as neutron-emission; Stanford University has measured a strong energy output; and hosts of other scientists have detected confirming results. I am enclosing for your review my most recent letter to the Department of Energy, including a copy of the Program of the cold-fusion Conference.

It is imperative that the United States take the lead in developing cold-fusion technology. We cannot afford to allow our current lead to slip away to other countries.

These developments in nuclear science can only be compared in magnitude of importance with the discovery that the Uranium atom can be fissioned, which announcement in 1939 led to a race to develop a fission-bomb.

As we did in 1941, we should devote our resources to a crash program to develop this new field. A small fraction (say, 1%) of the funds expended thus far in hot-fusion research would enable cold-fusion research to achieve our long-sought goal - - unlimited energy production from sea water!

I thank you in advance for your support.

Most sincerely yours,

Walter L. Wagner

Walter L. Wagner
Development Director

To the person who opens this letter:

Please insure that President Bush personally has the opportunity to review the enclosed letter.

As a nuclear physicist and former Federal employee, I am familiar with the filtering system necessitated by the great bulk of correspondence received by the President.

President Bush needs to be aware that cold-fusion phenomena are a reality, and that the strong negative-publicity campaign by the news-media during the past year concerning cold-fusion has been just that - - a negative publicity campaign.

The reality of cold-fusion is now beyond any scientific doubt. Too many reputable scientists, from major institutions, have confirmed the early claims last year advanced by Dr. Pons and Dr. Fleischmann.

However, the general public perception, based in large part by negativistic attacks from persons who fear for their future funding in hot-fusion, appears to have become one of doubt about the reality of these phenomena. This will soon give way, as more advances in cold-fusion technology are developed.

Our nation's future is too important to allow other countries to take the lead in cold-fusion technology. It is imperative the President Bush know that cold-fusion is a confirmed scientific fact - - a fact which cannot be changed by the strong sway of public opinion.

Thank you for insuring that this material is forwarded to President Bush for his personal review.

Sincerely,

Walter L. Wagner

Walter L. Wagner

National Cold Fusion Institute

March 15, 1991

TO THE BOARD OF TRUSTEES
AND STATE COUNCIL

Re: **Significant Nuclear Evidence Found for Cold Fusion**

Ladies and Gentlemen:

A group of researchers at the **Naval Weapons Center** in China Lake, California, under the direction of Dr. Melvin Miles, reports to have found helium^4 in electrolytic cold fusion cells that produced excess heat. A publication reporting this work will appear in the Journal of Electroanalytical Chemistry and Interfacial Electrochemistry in April 1991. In a telephone call to Dr. Miles on March 14, I learned that helium^4 has been detected in the off-gas of two heavy water cells in seven out of nine gas samples taken. Furthermore, the stunning finding is that the amount of helium detected approximately corresponds to the amount of excess heat measured. Thus, one of the gas samples that didn't show helium was taken when the cell didn't generate heat.

This would be the first time that anybody would have been able to relate excess heat to the quantity of nuclear byproducts formed and would represent a stunning confirmation of cold fusion occurring in deuterated palladium. Also, importantly, two blank cells containing light water electrolyte have been run and no helium was found in the six gas samples collected.

The findings of Dr. Miles and his group could be regarded as the most dramatic cold fusion finding since the announcement of Drs. Fleischmann & Pons on March 23, 1989. It is likely to carry tremendous significance and will stimulate many groups worldwide to confirm Dr. Miles' finding.

The NCFI had already started a program of its own aimed at helium

390 Wakara Way, Salt Lake City, Utah 84108/801-581-5571, Fax: 801-581-6674

detection last year. In December 1990, a collaborative effort was discussed with Rockwell International in which they would perform helium analysis both on palladium electrodes and on the gas collected in experiments performed at NCFI. We expect to be able to send our first samples to Rockwell International for helium analysis within a month or two.

Sincerely,

Fritz G. Will

Dr. Fritz G. Will, Director
National Cold Fusion Institute

cc: Richard Auchterlonie
Greg Williams
John Morris
Milt Wadsworth
Siva Guruswamy
David Pershing
Andy Riley
Bob Seader
NCFI Staff

Preliminary note.

Helium Production During the Electrolysis of D_2O In Cold Fusion Experiments.

B. F. Bush and J. J. Lagowski
Department of Chemistry, University of Texas
Austin, TX 78712

M. H. Miles* and G. S. Ostrom
Chemistry Division, Research Department, Naval Weapons Center
China Lake, CA 93555

INTRODUCTION:

Our interest in the "cold fusion" process [1,2] was piqued by the apparent lack of systematic investigation into the composition of the gaseous products produced during the electrolysis of D_2O. A critical issue in determining whether or not the cold fusion process exists is the quality of the evidence concerning the composition of the gaseous products. The low intensity of neutrons has prompted proposals of other fusion processes such as $d + d \longrightarrow {}^4He + \gamma$ [3] and $p + d \longrightarrow {}^3He$ [4,5]. Accordingly, we report the results of experiments designed to detect helium in the effluent gases from electrolysis reactions at palladium cathodes while rigorously excluding possible helium contamination from other sources. The calorimetric

* To whom correspondence should be addressed.

electrolysis experiments reported here were performed at China Lake, and the analyses designed to establish the composition of the effluent gases were performed in Austin.

EXPERIMENTAL:

The effluent gas from calorimetric electrolytic cells designed to detect excess enthalpy [6,7] was collected with the rigorous exclusion of air, and passed through an activated charcoal cryofiltration system (Fig. 1) to remove all gases except helium [8]. The first stage of the cryofilter acts as a cryopump to sweep any helium entrained in the effluent gas into the filtration system, while the second stage of the cryofilter removes any D_2 that gets past the first stage.

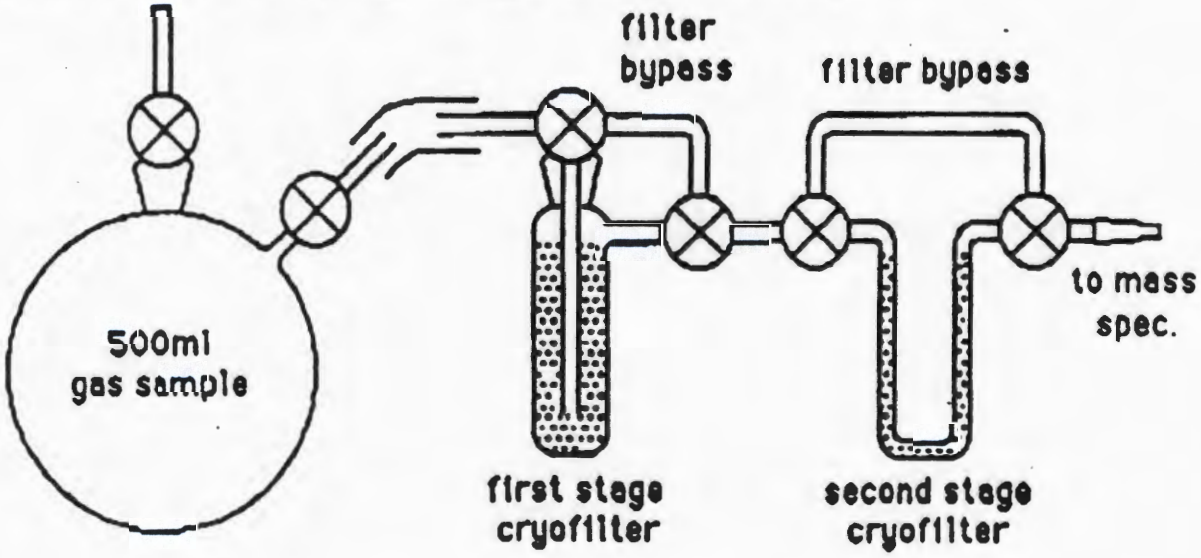

Fig. 1: The two stage activated charcoal cryofilter, designed to remove all gases except helium.

Strenuous efforts were made to avoid contamination of the effluent gas from the electrolytic cell with any external source of

2. Two identical systems were always run simultaneously using calorimetric cells as described previously [7]. Connections between the cell, flask, and oil bubbler employed thick-walled rubber vacuum tubing. All connecting lines, as well as the cell, were flushed vigorously with boil-off nitrogen, which contained no ^4He (see Table 1), for at least 10 minutes prior to attaching a gas collection flask. Furthermore, the flasks were generally connected to the cell for at least two days of D_2O electrolysis before removal. The gas evolution rate was calculated to be 6.75 $mL \cdot min^{-1}$ at 528 mA (200 mA/cm^2) and 700 torr assuming ideal gas behavior; thus the 500 mL collection flask was further flushed with more than 19 times its volume of evolving D_2 and O_2 gases per day. Actual measurements of the gas evolution rate by the displacement of water yielded 6.75 ± 0.25 $mL \cdot min^{-1}$ for cell A and 6.69 ± 0.15 $mL \cdot min^{-1}$ for cell B. All solvent additions were made only after vigorously sparging the make-up D_2O with nitrogen for about 5 minutes. The D_2O was always added through the septum and stopcock into the cell using a gas-tight syringe (Hamilton No. 1005).

Commercially available argon gas, which might be considered as an "inert" atmosphere in these experiments, contained a substantial quantity of ^4He, but nitrogen from liquid N_2 boil-off contained no detectable ^4He (Table 1). The palladium rod cathodes (Johnson Matthey, 99.96%, A = 2.64 cm^2, V = 0.35 cm^3) were wet polished with silicon carbide paper prior to use in these experiments. This surface treatment would likely remove any measurable helium contamination in the palladium [9]. Collection flasks were prepared in Austin for effluent gas collection at China Lake by applying a 10mtorr vacuum

process was repeated three times per flask. The collection flasks (500mL) processed by this method contained no detectable ^4He.

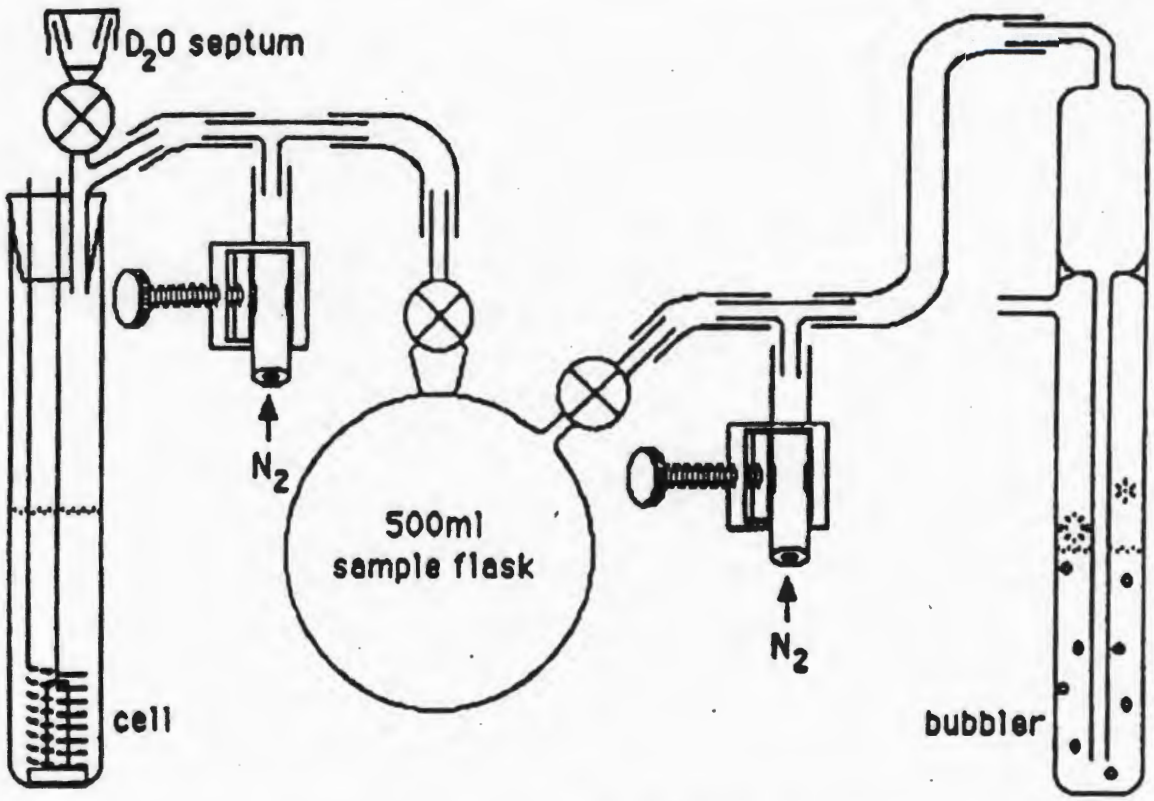

Fig. 2: Electrolytic cell with positive pressure gas discharge line used to collect samples of effluent gas.

Mass spectral measurements were made using a Bell & Howell 21-491 mass spectrometer. The mass spectrometer had sufficient resolution to easily separate D_2 and ^4He when the mass peaks were displayed as analogue signals on an oscilloscope. After removing the air from the cryofilter by evacuating and flushing the filters with nitrogen multiple times, the evacuated filters were connected to the mass spectrometer, and the gas collection flask was attached to the filters using a short section of thick-walled rubber tubing; then the air

in the rubber tubing was evacuated through the two filter bypasses as shown in Fig. 1. The filters were then cooled by immersion in liquid nitrogen for several minutes whereupon the stopcocks were manipulated to open the collection flask to the first stage of the cryofilter for approximately 10 seconds while the stopcock to the second stage remained closed. After allowing at least 1 minute for adsorption of the effluent gas into the activated charcoal of the first stage cryofilter, the stopcock to the second stage cryofilter was opened. Concurrently, the valve controlling the evacuation of the ion source of the mass spectrometer was closed to enhance sensitivity by preventing rapid evacuation of the sample from the source. If helium had not been observed after approximately 3 minutes, then the inlet to the ion source of the mass spectrometer was valved off, and any helium diffusing through the cryofilter was allowed to accumulate between the filter and the valve. The opening of this inlet to the mass spectrometer surged any helium present into the source, thus enhancing the concentration of the helium to be observed. When operated in this manner, there is enough effluent gas in one 500 mL flask to perform two helium determinations should the results of the first determination be ambiguous for any reason. As demonstrated by samples 6-9 in Table 1, the detection limit for helium is approximately 8×10^{11} atoms of ^4He in the gas condensed into the cryofilter.

RESULTS:

The reproducibility of our method for collecting gaseous samples and analyzing for helium is illustrated by the data presented in Table

1. No helium was detectable in routinely repeated experiments involving collection flasks filled with boil-off nitrogen (Sample 1). Collection flasks (500 mL) were then filled with boil-off nitrogen at Austin and shipped to China Lake where they were either connected to the gas collection system and flushed with boil-off nitrogen (Samples 2-5) or simply returned unopened (Samples 6,7). These flasks contained no detectable ^4He except in two cases (Table 1). We ascribe the miniscule amount of ^4He detected in sample 4 (01/09/91 B-1) and the large amount of ^4He detected in sample 6 (01/16/91 N_2) to air leaks that may have occurred during shipment by air freight due to reduced atmospheric pressure in flight. Deuterium oxygen mixtures could not be shipped by air freight due to the explosion hazard. Thus the nitrogen standard samples 2-7 in Table 1 represent worst case situations.

Table 1: Reference and detection limit samples.

sample	contents	Mass spec results[a]	conclusion
1) 01/08/91	500mL,vacuum & filled with N_2, 3X	No ^4He observed	N_2 contains no ^4He[b]
2) 01/09/91 A-1	500mL NWC[e] N_2 from gas discharge line, cell A	No ^4He observed	in spite of accumulation & surge into mass spec.
3) 01/09/91 A-2	500mL NWC[e] N_2 from gas discharge line, cell A	No ^4He observed	in spite of accumulation & surge into mass spec.
4) 01/09/91 B-1	500mL NWC[e] N_2 from gas discharge line, cell B	^4He observed at detection limit[c]	He accumulated then surged into mass spec.
5) 01/09/91 B-2	500mL NWC[e] N_2 from gas discharge line, cell B	No ^4He observed	in spite of accumulation surged into mass spec.
6) 01/16/91 N_2	500mL, vacuum & filled with N_2, 3X; round trip shipment	large amount of ^4He observed[d]	air freight shipment induces flask leakage
7) 01/17/91 N_2	500mL, vacuum & filled with N_2, 3X; round trip shipment	No ^4He observed	air freight shipment without leakage
8) $8X10^{11}$ ^4He atoms	10mtorr air in 500mL vacuum	No ^4He observed	condensable gas needed to sweep ^4He into filter
9) $8X10^{11}$ ^4He atoms	10mtorr air in 500mL N_2	^4He observed at detection limit[c]	^4He accumulated then surged into mass spec.
10) $1.6X10^{11}$ ^4He atoms	10mtorr air in 100mL N_2	No ^4He observed	100mL flasks not big enough for sampling
11) $8X10^{11}$ ^4He atoms	50mtorr air in 100mL N_2	^4He observed large peak, long dwell[d]	more ^4He observed than expected

Notes: a) mass spectrometer, always at highest sensitivity b) this result is an example of experiments that were performed routinely to test the N_2 c) detection limit is approximately 2:1 signal to background ratio, mass spectrometer at highest sensitivity d) peak with large signal to background ratio, peak dwelled a long time in mass spectrometer, mass spectrometer at highest sensitivity e) NWC = Naval Weapons Center at China Lake CA.

The ^4He detection limits (samples 8-11 in Table 1), the purity of the flush gas (samples 1-7), and our ability to exclude ^4He contamination from the air were determined concurrently with the analyses of effluent gas samples from China Lake. We believe that the analysis of effluent gas produced by the electrolytic cells are definitive. The results tabulated in Table 2 indicate that the effluent gases contained ^4He when electrolysis of D_2O produced significant excess heat and power. A second measurement was performed when the first measurement was ambiguous. The helium detection limit of our technique is approximately 0.14W in terms of excess power (P_{ex}) or about 1.08/1 in heat ratios ($\Delta H_{out}/\Delta H_{in}$), with the calorimetry being accurate to 3% (±0.03/1). The excess power of 0.14W (8% excess heat)

reported in Table 2 corresponds to approximately $2X10^{12}$ atoms of ^4He in a 500mL flask as referenced to the 10mtorr air in 500mL of N_2 (Table 1). The excess power observed is roughly proportional to the concentration of helium in the effluent gas within the limits of experimental resolution. The calorimetric results reported in Table 2 were measured shortly before the removal of the gas collection flask; however, fairly constant values were obtained throughout the day.

Table 2: D_2O-LiOD electrolysis. The presence of helium in the effluent gas compared to the generation of excess power and heat.

sample	P_{ex} watts	$\Delta H_{out}/\Delta H_{in}$	Results[a]
1) 12/14/90 A	0.52[b]	1.20/1[b]	^4He observed as large peak, long dwell; no ^3He[b]
2) 5/5/75	0.46	1.27/1	^4He observed as large peak, long dwell[d]
3) 11/25/90 B	0.36	1.15/1	^4He observed as large peak, long dwell; no ^3He
5) 4/29/65 A	0.24	1.10/1	^4He observed medium peak, some dwell; no ^3He
6) 11/27/90 A	0.22	1.09/1	^4He observed as large peak, long dwell[d]
4) 11/14/79	0.17	1.12/1	^4He observed at detection limit; no ^3He
7) 3/26/69 A	0.14	1.08/1	^4He observed at detection limit; no ^3He
8) 1/18/37	0.07	1.03/1	No ^4He or ^3He observed
9) 12/17/90 B	0.29[c]	1.11/1[c]	No ^4He or ^3He observed[c]

Notes: a) mass spectrometer, always at highest sensitivity b) current was 660 mA, all other experiments used 528 mA c) the D_2O solution level of the cell was found to be excessively low resulting in a erroneous calorimetric result d) no measurement of ^3He was made

In a preliminary experiment, dental X-ray films were positioned near the outer surfaces of two operating D_2O-LiOD electrolytic cells in an attempt to detect ionizing radiation. In both instances, the dental films were found to be significantly exposed when developed. It was not possible for hydrogen or deuterium to sensitize the film because the cells were completely sealed for effluent gas analysis. A cell containing H_2O-LiOH and producing no excess heat gave no exposure of the film in a similar experiment.

As a final experiment, the D_2O-LiOD in the electrolytic cells was replaced by H_2O-LiOH to serve as a control experiment. The H_2O-LiOH electrolysis, being conducted in an identical manner to the D_2O-LiOD electrolysis, is the best indication of our ability to exclude ^4He contamination from the air. However, fusion via the $p + d \rightarrow {}^3He$ pathway cannot be ruled out either theoretically [4,5] or experimentally [10] since our palladium electrodes likely retained some deuterium from the previous experiments. Although some unexplained excess heat effects were observed, no ^3He or ^4He was detected (Table 3). Furthermore, no exposure of dental X-ray films occurred in these H_2O-LiOH/Pd cells.

Table 3: H_2O-LiOH electrolysis.
Checking for ^4He in effluent gas.

Sample	results[a]
1) 1/9/91 A-2	No ^4He or ^3He observed
2) 1/16/91 A	No ^4He or ^3He observed
3) 1/16/91 AA	No ^4He or ^3He observed
4) 1/16/91 B	No ^4He or ^3He observed
5) 1/17/91 A	No ^4He or ^3He observed
6) 1/17/91 B	No ^4He or ^3He observed

Note: a) mass spectrometer, always at highest sensitivity; any gas passing through the cryofilter was allowed time to accumulate and then surged into the mass spectrometer.

DISCUSSION:

The use of the activated charcoal cryofilter removes the interfering D_2 and O_2 from the effluent gas allowing unambiguous observation of helium by mass spectrometry. Further, ^4He can be identified in the presence of D_2 because of its higher ionization potential; likewise ^3He can be distinguished from HD. As the accelerating voltage of the electron gun ionizer in the mass

spectrometer is lowered, helium related peaks will disappear due to a decrease in ions produced, but molecules incorporating isotopes of hydrogen will continue to be ionized.

All of the relevant analyses were performed with the mass spectrometer at its' highest sensitivity setting. Strenuous efforts to prevent helium infiltration due to air leaks were generally successful. Had gross air leaks occurred, helium would have been detected at concentrations several orders of magnitude above those observed.

The concentration of helium (^4He) observed in the gaseous products maintained an approximate correspondence to the amount of excess power measured in the electrochemical calorimetric cells (Table 2). This indicates that ^4He is produced at or near the surface of the palladium electrode rather than deeper in the bulk metal and that the preponderance of the helium escapes from the electrode and resides in the effluent gas. Another study of helium in electrolyzed palladium tends to support this behavior [9].

Although the exact nature of the fusion reaction or reactions producing the excess heat effect is not known, the process

$$^2D + {}^2D \longrightarrow {}^4He + \gamma + 23.5\ MeV \qquad (1)$$

can be used as a basis for an estimate of helium production. For this fusion process, 1 watt corresponds to the production of 2.66×10^{11} ^4He s^{-1}. The highest excess power observed at 528 mA (0.46W or $1.3 W/cm^3$, sample 2 in Table 2) would therefore produce 5.4×10^{14} atoms of ^4He in the time period required to fill the 500 mL collection flask with D_2 and O_2 gases (4440s). It is apparent from Table 1 that this amount of ^4He would be more that two orders of magnitude above the detection limit for the analytical method used in this study. The

large amount of ^4He observed in this experiment (Table 2) is likely within an order of magnitude of this theoretical estimate of helium production.

CONCLUSIONS:

Our cold fusion experiments show a correlation between the generation of excess heat and power and the production of ^4He, established in the absence of outside contamination. This correlation in the palladium-D_2O system provides strong evidence that nuclear processes are occurring in these electrolytic experiments. The major gaseous fusion product in D_2O-LiOD is ^4He rather than ^3He. No helium products are found in H_2O-LiOH experiments. These results add to the accumulating evidence for cold fusion that involves 12 countries and more than 70 laboratories [11].

ACKNOWLEDGEMENTS:

We would like to thank Drs. Joseph M. Nunez and John F. Martino for assistance in the dental film experiments. We also thank Dr. Richard A. Hollins for encouragement, helpful discussions and assistance in electrochemical calorimetric measurements. One of us (G.S.O.) expresses appreciation for an ONT/ASEE post doctoral fellowship. We would also like to thank the staff of the analytical services laboratory at The University of Texas for technical discussions that made it possible for us to perform these studies. Finally, we gratefully acknowledge the generous financial support of the Robert A. Welch foundation.

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JOURNAL OF ELECTROANALYTICAL CHEMISTRY AND INTERFACIAL ELECTROCHEMISTRY

International Journal devoted to all Aspects of Electrode Kinetics, Interfacial Structure, Properties of Electrolytes, Colloid and Biological Electrochemistry

Professor R. Parsons FRS
Department of Chemistry
The University
SOUTHAMPTON SO9 5NH
Great Britain

Dr. M.H. Miles
Department of the Navy
Chemical Weapons Center
Research Dept., Chemical Div.
CHINA LAKE, California 93555
U.S.A.

Re: RPS 118 JEC 1531

Southampton, 19- 2-1991

Dear Dr. Miles,

I am pleased to acknowledge receipt of your preliminary note entitled:

Helium production during the electrolysis of D2O in cold fusion experiments. (Bush,R.F., Lagowski,J.J., Miles,M.H. and Ostrom,G.S.).

I have read this and it seems to me to be suitable for publication in this form. I shall now send it straight in to the Printers.

I enclose the Transfer of Copyright Agreement. Please sign this and return it to Elsevier in the envelope enclosed.

Yours sincerely,

Roger Parsons

Prof.Roger Parsons

P.S. I got copies from Prof. Lagowski and the ring mid tips so I am sending the note in. If there is any problem with clearance, please let me know: the fax number is (703) 676960

JOURNAL OF ELECTROANALYTICAL CHEMISTRY AND INTERFACIAL ELECTROCHEMISTRY

EDITOR: Professor Roger Parsons, FRS

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JEC 1531

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Helium production during the electrolysis of D2O in cold fusion experiments.

by: Bush,B.F., Lagowski,J.J., Miles,M.H. and Ostrom,G.S.

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SIGNATURE: Melvin H. Miles

NAME (PRINTED): Dr. Melvin H. Miles

DATE: 25 February 1991

Cold-Fusion Spark Almost Dead at DOE

Tribune Washington Bureau

WASHINGTON — The Department of Energy is asking Congress for no 1992 appropriation for nuclear cold fusion, saying the issue will no longer even be on DOE's back burner unless repeatable cold-fusion experiments are forthcoming and shared with the whole scientific community.

During an interview Wednesday, James F. Decker, director of energy research for the Department of Energy, told *The Tribune* that "at this point, we see no future energy source [based on a 30- to 40-year time frame] in cold fusion."

Interviewed after serving as principal witness at a House appropriations subcommittee hearing on basic energy sciences that warrant continuing federal support, Dr. Decker said that when the announcement first broke of successful cold nuclear fusion experiments by University of Utah scientists B. Stanley Pons and Martin Fleischmann, DOE was instantly interested — but not for long.

He recalled that the department had quite a few scientists researching it. "But it probably would have been hard to keep them working on it, since there was no evidence of anything significant that might lead to an energy source. For DOE to even consider cold fusion, I think there would have to be some significant new results that were repeatable.

"One thing that's been plaguing this whole fusion business is that results have been reported, but then no one else could repeat them. In contrast, the way of science is that if something is truly there, someone ought to be able to complete the experiment if it is right. But that has not happened in cold fusion.

STAFUS

Walter L. Wagner

The Static Fusion Development Company
A Subsidiary of
Solaria Institutes

1126 12th Ave. Suite 105
Honolulu, Hawaii 96816
(808) 735-5815

May 23, 1990

Donald K. Stevens
Associate Director for Basic Energy Sciences
U.S. Department of Energy
Washington, D.C. 20585

Dear Dr. Stevens:

In that your letter to me was sent third class, I only received it yesterday. Many thanks for your kind response to the letter which I wrote to President Bush.

The enclosure (Cold Fusion Research, November 1989) you sent me was informative, but presented little new material in the way of the criticisms and skepticisms voiced thus-far. I too have been critical of many of the published reports, and the inadequacies in dealing with the complexities of static fusion (cold fusion).

While the Report you sent me, and the great bulk of the skepticisms voiced thus far, have focused on the inabilities of certain groups to obtain positive results evidencing the existence of static fusion phenomena, I have focused my attention on those groups which have obtained strong confirmational results.

In particular, as is well noted in your Report, several groups have detected levels of tritium (which is easily and frequently counted in liquid scintillation 'cocktails') well in excess of those one might expect from electrolytic enrichment of natural contaminations.

Additionally, while several groups have reported the presence of high-speed neutrons (usually in 'bursts'), one group in particular has obtained extremely large numbers of neutrons well above a natural background. Dr. Lyengar of BARC (whom I mentioned in my letter to President Bush) reports that his

group has detected an estimated 20,000,000 neutrons during a five minute period in one cell, and later about 1,000,000 neutrons over an 85 minute period in another cell. As you well know, neutrons can only be produced by some form of nuclear fusion (or fission) process. These rates of production are sufficiently high, that if the process is developed to the point of understanding of the phenomena, that useful energy production rates should be obtainable. It is for this reason that Dr. Lyengar has approximately 100 researchers of his staff actively working on static fusion. (As related to me in a private conversation we had at Salt Lake City)

One plausible scenario, which I will be investigating, is that most of the neutrons produced in static fusion are released as thermalized neutrons, and are not readily detected by a detector sitting outside the fusion-cell. Under this scenario, one would expect a certain degree of induced radioactivity, both within the 'electrode' or other condensed matter medium wherein fusion occurs, as well as outside the fusion-cell in the surrounding materials. This is just the kind of experimental evidence obtained by at least one researcher, who reported his results of an autoradiograph of an electrode, over a period of several weeks, wherein multiple, scattered pockets of radioactivity were detected. A slide of this autoradiograph was shown at the Salt Lake City conference, but is not mentioned in your Report.

As your Report indicates researchers should do, I will be examining as many facets of static fusion as possible from any one cell; specifically searching for fast neutrons, induced radioactivity from thermalized neutrons, gamma rays, brehmstrahlung radiation, tritium, 'excess heat', helium, etc. I expect that not only will my cells be replicates of systems thus-far attempted, but also include searches for D-Li fusion, in particular when applied high-voltages are periodically shocked through the condensed matter medium wherein the static fusion process will be occuring.

I expect to have a Grant Proposal prepared in the next week and submitted to Dr. Gajewski for your considered review.

Most sincerely yours,

Walter L. Wagner

Walter L. Wagner
Development Director

WLW/ddu

MAR 6 1990

Mr. Walter L. Wagner, Director
STAFUS
1126 12th Ave., Suite 105
Honolulu, HI 96816

Dear Mr. Wagner:

This is a much belated response to your January 24, 1990, letter.

Unfortunately, an air of skepticism surrounding the experimental results claimed to be indicative of cold fusion still persists, reports from Japan notwithstanding. In that regard, not much has changed since our letter exchange of last summer. Accordingly, and for the reasons stated in my June 23, 1989, letter, I still cannot recommend that you submit a full proposal.

It's been good hearing from you. Thanks for your inquiry.

Sincerely,

Original signed by:
Ryszard Gajewski

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

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cold fusion

STAFUS

Walter L. Wagner

The Static Fusion Development Company A Subsidiary of Solaria Institutes

1126 12th Ave. Suite 105 Honolulu, Hawaii 96816 (808) 735-5815

January 24, 1990

Ryszard Gajewski, Director
Division of Avanced Energy Projects
Office of Basic Energy Sciences, ER-16
Department of Energy
Washington, D.C. 20545

Dear Dr. Gajewski:

Much Aloha from Hawaii! It's been six months since we last corresponded, and I am again writing to seek support for static fusion research.

To refresh your memory, I am enclosing a copy of your last letter to me, and also a copy of an anecdotal newspaper report of a most recent result from Japan which follows a scheme I proposed early last year.

Since your last letter, not only has Japan taken a lead in static fusion research at their new National Institute for Fusion Science, but Dr. Pons has founded the the National Cold Fusion Research Laboratory in Salt Lake City.

You will note in your letter to me that the phenomenological picture of D-D fusion in a metal matrix you considered as needing a solid quantitative backup. I believe these most recent results from Japan provide the needed confirmation of my theoretical predictions.

Without going into the details of the refined versions of schemes of static fusion upon which I have been working, I again would like you to consider this letter as a pre-proposal for a full-fledged proposal for a research grant in the field of static fusion research, with emphasis on D-D fusion in metal matrices, including Lithium alloy metals, as well as D-Li electrochemical cells, and other areas as a function of the results of the research.

If you believe the climate might be more conducive to such a grant proposal, your speedy reply would be appreciated. We should not drop the ball, to let Japan rule the court.

Sincerely,

Walter L. Wagner

Walter L. Wagner, Director

battlefield," liberating 3
es, killing or wounding govern
ment soldiers and freeing 6
commune and village officials.

1/11/90 Star Bulletin

Japanese team claims cold fusion duplication

NAGOYA, Japan — A team of Japanese scientists says it has confirmed the achievement of nuclear fusion at room temperature, using a similar method as that of other researchers at a Japanese state-run university who late last year claimed they had achieved fusion.

Researchers at the National Institute for Fusion Science reported Jan. 10 detecting bursts of neutrons at a level nine to 17 million times higher than that occurring naturally after sending a high charge of electricity through palladium rods that had absorbed deuterium gas.

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Department of Energy
Washington, DC 20545

June 23, 1989

Mr. Walter L. Wagner
STAFUS
1126 12th Ave., Suite 105
Honolulu, HI 96816

Dear Mr. Wagner:

Thank you for your pre-proposal of May 16, and the letter of June 6, 1989. Having reviewed both these communications I feel that I cannot in good faith encourage you to submit a full-fledged proposal.

I believe that the field of cold fusion phenomena is at a stage where some rudimentary understanding of the processes involved is needed before serious consideration is given to building generators, even on a bench-top scale. Right now, even reproducibility of the reported experiments is a problem. The phenomenological picture you invoke (D-Li fusion in electrochemical cells, D-D fusion in metal matrices) needs a solid quantitative backup. Without it, it is just one of many schemes appropriate for informal discussions, but not as a basis for developing a working generator. I am convinced that a proposal based on the information contained in your two communications would have trouble passing an expert technical review.

It has been a pleasure meeting you at Santa Fe. I certainly wish you well in your endeavors.

Sincerely,

Ryszard Gajewski

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

STAFUS

Walter L. Wagner

The Static Fusion Development Company
A Subsidiary of
Solaria Institutes

1126 12th Ave. Suite 105
Honolulu, Hawaii 96816
(808) 735-5815

May 16, 1989

Dr. R. Gajewski
United States Department of Energy
ER-16, GTN
Washington, D.C.
20545

Re: Pre-Proposal for Static Fusion Research

Dear Dr. Gajewski:

After speaking with Dr. Dwayne Barney earlier today, I was advised to write a Pre-Proposal regarding my interest in obtaining grant research monies for applied research in static fusion (cold fusion).

As a pre-proposal, I as seeking grant monies to investigate the feasibilities of implementing one or more of the static fusion inventions we have developed and submitted with U.S. Patent and Trademark Office.

These invention titles, in the order of their submittal to the patent office, are:

1. Static Fusion of Wet Lithium-Deuterium (with applied voltage)
2. Static Fusion of Dry Deuterium
3. Static Fusion of Dry Lithium-Deuterium
4. Static Fusion of Dry Lithium-Deuterium in a Metal Matrix
5. Static Fusion of Dry Lithium-Deuterium in a Metal Matrix with Applied Voltage

These five inventions all rely upon static fusion (as opposed to the kinetic fusion devices which D.O.E. has funded in the past, e.g. laser implosion, electron-beam implosion, Tokamak, etc.), wherein the fusing nuclei are brought relatively close together (on the order of less than one Angstrom) and held there for long periods of time (hours), allowing a small but appreciable number to fuse via quantum tunneling.

My calculations, based on the empirical data generated by Pons, Jones, and Scaramuzzi, show that these inventions should yield macroscopic amounts of utilizable energy beyond the energy input, within a small volume.

MAY 0 1 1990

Dr. Walter L. Wagner
Development Director
STAFUS
1126 12th Avenue, Suite 105
Honolulu, HI 96816

Dear Dr. Wagner:

Your letter to President Bush has been referred to me for response.

We at the Department of Energy are carefully monitoring reports regarding observations of phenomena associated with cold fusion. We are well aware of the experimental results referred to in your letter. We are also aware of significant skepticism in major portions of the scientific community regarding those results. That skepticism was reflected in the report of the Cold Fusion Panel of the Energy Research Advisory Board; a copy of that report is enclosed.

The skepticism notwithstanding, and in concert with the Cold Fusion Panel recommendations, the Department of Energy is presently supporting several research projects, at national laboratories and at universities, aimed at detecting and understanding any nuclear phenomena in condensed matter that may be construed as indicative of cold fusion. However, we see no justification at this time to institute a special "crash program" in cold fusion. That position, too, is in agreement with the Cold Fusion Panel conclusions.

As to your request for funding of your projects, I understand that Dr. Gajewski has provided you with information pertinent to applying for a research grant. You should feel free to contact either him or Dr. Walter Polansky if you have any further questions.

Sincerely,

Original signed by
Donald K. Stevens

Donald K. Stevens
Associate Director
for Basic Energy Sciences

Enclosure

bcc: ES/4, ER-61, ER-622/FTL

ES#90-6643 ER#90-627 Due Date: 5/1/90

ER-16:RGajewski:mfr:3-5995:4-26-90:c:\ES\Wagner:wp

ER-16	ER-~~10~~	ER-61
R.G.		TOM
Gajewski	Stevens	Mayhew
4/27/90	4/27/90	4/30/90

APR 2 3 1990

Dr. Walter L. Wagner
Development Director
STAFUS
1126 12th Avenue, Suite 105
Honolulu, HI 96816

Dear Dr. Wagner:

Thank you for your letter of April 14, 1990.

You should feel free to submit a research grant application to this division. As all our proposals, it will be subjected to a technical peer review. The eventual funding decision will depend on the outcome of the review and availability of funds.

Sincerely,

Original signed by:
Ryszard Gajewski

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

Enclosures

ER-16:RGajewski:mfr:3-5995:4-23-90:c:\Gajewski\Wagner.3:wp

ER-16

Gajewski
4/ /90

STAFUS

Walter L. Wagner

The Static Fusion Development Company
A Subsidiary of
Solaria Institutes

1126 12th Ave. Suite 105
Honolulu, Hawaii 96816
(808) 735-5815

April 14, 1990

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

Dear Dr. Gajewski:

Since receiving your last letter to me dated March 6, 1990, in which you indicated that "an air of skepticism" still persisted concerning the results which are indicative of the existence of the reality of 'cold fusion' phenomena, I have had the great pleasure of attending the First Annual Conference on Cold Fusion held in Salt Lake City on March 28-31, hosted by the National Cold Fusion Institute.

For your edification, I am enclosing a copy of the program (which was signed by several of the prominent participants), which summarizes the topic areas which were discussed at the conference.

To summarize the results of the conference, suffice it to say that numerous researchers from numerous prestigious institutions have in essence verified the claims of Dr. Pons and Dr. Fleischmann. Specifically, several groups (most notably the Los Alamos National Laboratory, and the Bhabha Atomic Research Center) have detected levels of tritium in 'cold-fusion' cells which are thousands of times above the background levls of their control cells. Additionally, other groups have detected 'bursts' of neutrons which are hundreds of times above the background level of their detectors.

As you well know, tritium and/or neutrons can only be produced in deuterated cells by some process of nuclear fusion. Thus, the clear and convincing evidence which came forth at this conference is that cold-fusion phenomena are a reality, even if the theoretical understanding of the phenomena is not yet adduced.

My conversations with Dr. Lyengar of the Bhabha Atomic Research Center revealed that they have placed approximately 100 of their research scientists in the area of cold-fusion research (though this may have increased as of this writing).

Likewise, Japan now has a dedicated, national effort towards development of cold-fusion, and indications are that the Soviet Union has likewise conducted extensive research in this area.

What I personally find most interesting is that my theoretical predictions for the existence of cold-fusion in a deuterated metal matrix appear to have been confirmed not only by Dr. Scaramuzzi of Italy, but also by the Los Alamos National Laboratory.

As such, I am encouraged that my other theoretical predictions for other modes of cold-fusion (more properly, static-fusion) are likewise sound.

In light of the $6,100,000,000.00 reportedly spent on hot-fusion (more properly, kinetic fusion) during the past 35 years, as reported in the March 30 issue of Science, without any real evidence that the process will work (the projections are that the next generation of people will have to develop the process), it seems absurd that the U.S. government is not allocating substantial funding in this new field. After all, is not energy independence the ultimate goal of your Department? Development of cold-fusion will fulfill our wildest dreams of energy abundance - - the entire body of water in our oceans becomes our fuel!

While I can recognize the 'red-tape' of government bureaucracy, it is imperative that we cut through that tape, and direct our resources to development of this new field.

Thus, I again ask that you consider funding my projects for developing static fusion as our future energy source. I am taking the liberty of forwarding a copy of this letter to the Office of the President, as I consider energy independence to be a matter of national priority, and national security.

Most sincerely yours,

Walter L. Wagner

Walter L. Wagner
Development Director

The First Annual Conference on Cold Fusion

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Program Summary

◆

March 28-31, 1990
University Park Hotel
Salt Lake City, Utah

◆

Sponsored by the

National Cold Fusion Institute

PROGRAM

Wednesday, March 28, 1990

7:00 p.m.-9:00 p.m. **REGISTRATION and RECEPTION**

Thursday, March 29, 1990

7:00-8:30 a.m. **REGISTRATION**

8:30 **OPENING COMMENTS**
F. G. Will, Director
H. Rossi, Conference Chair
National Cold Fusion Institute (NCFI)

WELCOME
I. Cumming
Chair, Board of Trustees
NCFI

Session Chair: **H. Rossi**
University of Utah/NCFI

8:45 **Calorimetry of the Palladium-Deuterium Systems**
S. Pons
University of Utah/NCFI

9:30 **Calorimetric and Electrochemical Studies of the Deuterium-Palladium System**
M. C. H. McKubre, R. C. Rocha-Filho, S. Smedley, F. Tanzella, B. Chexal, T. Passell, and J. Santucci
Stanford Research Institute

10:00 **Palladium/Hydrogen Isotope Systems: Microcalorimetric Measurements and Surface Analyses**
O.J. Murphy, A. J. Appleby, and S. Srinivasan
Texas A& M University

10:30 Break

10:40 **Initial Calorimetry Experiments in the Physics Division at ORNL**
D.P. Hutchinson, C.A. Bennett, R.K. Richards, J. Bullock IV, and G.L. Powell
Oak Ridge National Laboratory

11:00 **Recent Measurements of Excess Energy Production in Electrochemical Cells**
M. Schreiber, T.M. Gur, G. Lucier, J.A. Ferrante, J. Chao and R.A. Huggins
Stanford University

11:20 **Quartz Crystal Microbalance Study of Palladium/Hydrogen Interactions**
G.T. Cheek
Naval Research Laboratories

11:40 **Investigation of Nuclear Processes in Deuterated Metals**
J. Santucci
EPRI

12:00 Lunch Break

Session Chair: **J. O'M. Bockris**
Texas A&M University

1:30 **Overview of BARC Studies in Cold Fusion**
P.K. Lyengar
Bhabha Atomic Research Center, Bombay, India

2:15 **Experimental Considerations in Electrochemical Isoperibolic Calorimetry**
T. Gur, M. Schreiber, G. Lucier, J. A. Ferrante, J. Chao and R. A. Huggins
Stanford University

2:35 **Some Theoretical Ideas on Cold Fusion**
G. Preparata
University of Milano, Italy

3:20 **Status Report on Coherent Fusion Theory**
P.L. Hagelstein
Massachusetts Institute of Technology

3:40 **Quantum Mechanics of "Cold" and "Not-So-Cold" Fusion**
S.R. Chubb and T.A. Chubb
Naval Research Laboratories

4:00 Dinner Break

4:15-6:00 Guided Tours of National Cold Fusion Institute (sign-up sheets at the conference registration desk)
(walk east of hotel five minutes to 390 Wakara Way)

Session Chair: J. Brophy
University of Utah

7:30 **Nuclear Energy in an Atomic Lattice**
J. Schwinger, Nobel Laureate
UCLA

8:15 Adjourn

Friday, March 30, 1990

Session Chair: **R. Huggins**
Stanford University

8:15 a.m. **Nuclear Electrochemistry Among the Hydrogen Isotopes**
J. O'M. Bockris
Texas A&M University

9:00 **A Systematic Study of Electrolytic Tritium Production**
E. Storms and C. Talcott
Los Alamos National Laboratory

9:45 **Measurement of Excess Heat and Apparent Coincident Increases in the Neutron and Gamma Ray Count Rates During the Electrolysis of Heavy Water**
C.D. Scott, J.E. Mrochek, M. Petek, T.C. Scott, G.E. Michaels, and E. Newman
Oak Ridge National Laboratory

10:30 **Panel Discussion - Thermal Phenomena**
F.G. Will, Moderator
J. O'M. Bockris
M. Fleischmann
R. Huggins
M. McKubre
S. Pons
E. Yeager

11:30 Lunch Break

Session Chair: **K. Wolf**
Texas A&M University

1:30 **Cold Nuclear Fusion in Condensed Matter: Recent Results and Open Questions**
S. Jones
Brigham Young University

2:15 **Statistical Analysis of Neutron Emission in Cold Fusion Experiment**
M. Srinivasan, A. Shyam, S. B. Degwekar, and L. V. Kulkarni
Bhabha Atomic Research Center, Bombay, India

3:00 **Low Energy D-D Fusion Experimental Cross-Sections**
G.S. Chulick, Y.E. Kim, and R.A. Rice
Purdue University
The Effect of Velocity Distribution on Cold Deuterium Fusion
R.A. Rice, G.S. Chulick, Y.E. Kim, and J.Yoon
Purdue University
Surface Reaction Mechanism for Cold Fusion with Electrolysis
Y.E. Kim
Purdue University

3:30 Break

3:40 **On Aspects of Nuclear Products**
G.H. Miley
University of Illinois

4:00 **Isotopic Mass Shifts in Cathodically-Driven Palladium via Neutron Transfer Suggested by a Transmission Resonance Model to Explicate Enhances Fusion Phenomena (Hot and Cold) Within a Deuterated Matrix**
R.T. Bush
California State Polytechnic University

4:20 **A Zero Gradient Calorimeter for the Measurement of Anomalous Heat from the Electrolysis of Deuterated Metals**
T.F. Droege and L.J. Droege
Batavia, IL and Black Hawk, CO

4:40 **Electric Field Distribution of the Palladium Crystal Lattice**
K.J. Bunch and R.W. Grow
University of Utah/NCFI

5:00 Session Adjourns

6:00-7:30 Buffet Reception
Remarks by the Governor of Utah
The Honorable Norman S. Bangerter
Hosted by Leucadia National Corporation
Dessert provided by Mrs. Fields Cookies
Home office located in Park City, Utah

Session Chair: **S. Jones**
Brigham Young University

7:30 **Neutron Emission and the Tritium Content Associated with Deuterated Metals**
K. Wolf
Texas A&M University

8:15 **Technical Status of Cold Fusion Results**
D. Worledge
EPRI

8:35 Adjourn

Saturday, March 31, 1990

Session Chair: **M. Fleischmann**
University of Utah/NCFI

8:15 a.m. **Investigation of Phenomena Related to D2O Electrolysis at a Palladium Cathode**
R. Adzic, D. Gervasio, I. Bae, B. Cahan and E. B. Yeager
Case Western Reserve University

9:00 **Anomalies in the Surface Analysis of Deuterated Palladium Metals**
D. Rolison, William E. O'Grady, R.J. Doyle, Jr., and P.P. Trzaskoma
Naval Research Laboratories

9:45 **Anomalous Effects of Deuterated Metals**
T. Schneider
EPRI

10:30 **Panel Discussion - Nuclear Phenomena**
H. Rossi, Moderator
J. O'M. Bockris
S. Jones
T. Schneider
C.D. Scott
E. Storms
K. Wolf

11:30 Lunch Break

Session Chair: **E.B. Yeager**
Case Western University

1:20 **On Empirical System ID. Possible External Electromagnetic/Electronuclear Stimulation/ Actuation and Automatic Feedback Control of Cold Fusion**
R.W. Bass
Thousand Oaks, CA

1:40 **High Sensitivity Measurements of Neutron Emission from Ti Metal in Pressurized D2 Gas**
H. Menlove
Los Alamos National Laboratory

An Investigation of Cold Fusion in Thin Titanium Films
G. Chambers, Graham Hubler, and Ken Grabowski Naval Research Laboratory

Reformulation of the Cold Fusion Problem
P. H. Handel
University of Missouri

To Be Announced
G. Andermann
University of Hawaii

2:00 **Some Observations Relating to the Structure of Hydrogen and Deuterium in Palladium**
D.R. Coupland, M.L. Doyle, J.W. Jenkins, J.H.F. Notton, R.J. Potter, and **D. Thompson**
Johnson-Matthey Technology Centre

2:20 **Three Dimensional Computer Simulation of an Isoperibolic Calorimeter for Cold Fusion Experiments**
J. Chao, W. Layman, C. Kang, T. Gur, M. Schreiber, R. Huggins, G. Lucier, and J. Ferrante
EPRI

2:40 Break

Session Chair: **F.G. Will**
NCFI

2:45 **Metallurgical Aspects in Cold Fusion Experiments**
S. Guruswamy and M. E. Wadsworth
University of Utah/NCFI

3:05 **Electrochemical Calorimetric Studies of the Cold Fusion Effect**
M.H. Miles, K. H. Park and D. E. Stilwell
Naval Weapons Center

3:25 **Thoughts on Warm Fusion vs Cold Fusion**
S.N. Yang
National Taiwan University

3:45 **Overview of Cold Fusion Phenomena**
M. Fleischmann
University of Utah/NCFI

4:30 Adjourn

STAFUS

Walter L. Wagner

The Static Fusion
Development Company
A Subsidiary of
Solaria Institutes

1126 12th Ave. Suite 105
Honolulu, Hawaii 96816
(808) 735-5815

April 14, 1990

President George Bush
The White House
Washington, D.C.

Dear President Bush:

I am writing to you concerning recent developments in nuclear science which will revolutionize energy production world-wide.

At the recent Conference in Salt Lake City, scientists from around the world announced results which have confirmed the claims of cold-fusion in a bottle first advanced last year by Dr. Stanley Pons and Dr. Martin Fleischmann.

Specifically, Dr. Lyengar of the Bhabha Atomic Research Center in Bombay, India announced that his team of scientists have detected tritium in great abundance in their cold-fusion cells - a sure sign of fusion; the Los Alamos National Laboratory has likewise detected tritium, as well as neutron-emission; Stanford University has measured a strong energy output; and hosts of other scientists have detected confirming results. I am enclosing for your review my most recent letter to the Department of Energy, including a copy of the Program of the cold-fusion Conference.

It is imperative that the United States take the lead in developing cold-fusion technology. We cannot afford to allow our current lead to slip away to other countries.

These developments in nuclear science can only be compared in magnitude of importance with the discovery that the Uranium atom can be fissioned, which announcement in 1939 led to a race to develop a fission-bomb.

As we did in 1941, we should devote our resources to a crash program to develop this new field. A small fraction (say, 1%) of the funds expended thus far in hot-fusion research would enable cold-fusion research to achieve our long-sought goal - - unlimited energy production from sea water!

I thank you in advance for your support.

Most sincerely yours,

Walter L. Wagner

Walter L. Wagner
Development Director

ER-16
RGajewski
6/ /89

JUN 29 1989

Mr. Walter L. Wagner
STAFUS
1126 12th Ave., Suite 105
Honolulu, HI 96816

Dear Mr. Wagner:

Thank you for your pre-proposal of May 16, and the letter of June 6, 1989. Having reviewed both these communications I feel that I cannot in good faith encourage you to submit a full-fledged proposal.

I believe that the field of cold fusion phenomena is at a stage where some rudimentary understanding of the processes involved is needed before serious consideration is given to building generators, even on a bench-top scale. Right now, even reproducibility of the reported experiments is a problem. The phenomenological picture you invoke (D-Li fusion in electrochemical cells, D-D fusion in metal matrices) needs a solid quantitative backup. Without it, it is just one of many schemes appropriate for informal discussions, but not as a basis for developing a working generator. I am convinced that a proposal based on the information contained in your two communications would have trouble passing an expert technical review.

It has been a pleasure meeting you at Santa Fe. I certainly wish you well in your endeavors.

Sincerely,

/s/

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

ER-16:RGajewski:sad:3-5995:6-22-89:c:Gajewski:Wagner
Retyped:tla:3-3054:6/23/89

STAFUS

Walter L. Wagner

The Static Fusion Development Company
A Subsidiary of
Solaria Institutes

1126 12th Ave. Suite 105
Honolulu, Hawaii 96816
(808) 735-5815

June 6, 1989

Dr. R. Gajewski
U.S. Department of Energy
ER-16, GTN
Washington, D.C.
20545

Re: Santa Fe Workshop

Dear Dr. Gajewski:

I enjoyed having the privelege of meeting and speaking with you at our most interesting Workshop on static fusion at Santa Fe.

I trust you have had an opportunity to read my previous letter to you since your return. I have, of course, come up with many new and refined ideas on the course of research which we will be undertaking, but I will not go into that in this letter.

I did, however, want to have the chance to touch bases before too much time has elapsed, and let you know some of the news I've come across.

Perhaps the most interesting thing I learned was not at the Workshop per se, but immediately thereafter I met with two attendees in the foyer area, who came from Colorado. They related to me their own experiments along the lines of Fleischmann and Pons, and claim to have achieved boiling water on their 42 and 43rd experimental designs. As one is a biochemist, and the other an engineer, they said they were reluctant to discuss their results under such sharp review as at the Workshop, as their results were not run with a control, etc., and were more anecdotal. In any event, I was impressed that they attended, showing a strong belief in their results.

I have been in communication with Babcock and Wilcox, and have also learned that the nuclear power companies are actually highly interested in this area. Indeed, they also relayed to me another similar anecdotal report of boiling water, which I am waiting to hear more on.

Our firm is planning to work in conjunction with others, including numerous persons I met at our Workshop.

When I've learned more information, I'll let you know the details of these areas I've just touched upon.

In the menawhile, I trust you will be able to forward to me your approval to submit a formal Grant Proposal.

I look forward to being able to work with you in this exciting new field.

Sincerely,

Walter L. Wagner

Walter L. Wagner

STAFUS

Walter L. Wagner

The Static Fusion Development Company
A Subsidiary of
Solaria Institutes

1126 12th Ave. Suite 105
Honolulu, Hawaii 96816
(808) 735-5815

May 16, 1989

Dr. R. Gajewski
United States Department of Energy
ER-16, GTN
Washington, D.C.
20545

Re: Pre-Proposal for Static Fusion Research

Dear Dr. Gajewski:

After speaking with Dr. Dwayne Barney earlier today, I was advised to write a Pre-Proposal regarding my interest in obtaining grant research monies for applied research in static fusion (cold fusion).

As a pre-proposal, I as seeking grant monies to investigate the feasibilities of implementing one or more of the static fusion inventions we have developed and submitted with : U.S. Patent and Trademark Office.

These invention titles, in the order of their submittal to the patent office, are:

1. Static Fusion of Wet Lithium-Deuterium (with applied voltage)
2. Static Fusion of Dry Deuterium
3. Static Fusion of Dry Lithium-Deuterium
4. Static Fusion of Dry Lithium-Deuterium in a Metal Matrix
5. Static Fusion of Dry Lithium-Deuterium in a Metal Matrix with Applied Voltage

These five inventions all rely upon static fusion (as opposed to the kinetic fusion devices which D.O.E. has funded in the past, e.g. laser implosion, electron-beam implosion, Tokamak, etc.), wherein the fusing nuclei are brought relatively close together (on the order of less than one Angstrom) and held there for long periods of time (hours), allowing a small but appreciable number to fuse via quantum tunneling.

My calculations, based on the empirical data generated by Pons, Jones, and Scaramuzzi, show that these inventions should yield macroscopic amounts of utilizable energy beyond the energy input, within a small volume.

I believe that the discrepancy between the Jones' group and the Pons/Fleischmann group is accountable by the fact that the Jones' group was detecting purely D-D fusion, with a crude method and low neutron flux and low energy output, whereas the Pons/Fleischmann group inadvertently was detecting a D-Li fusion with a low neutron flux background from spurious D-D fusion (which is what they were wanting to fuse). The Li was present as their electrolyte. They thus had a high energy output (D-Li releases more energy than D-D, and theoretically should be a more readily occurring fusion reaction for the available D if Li is present), but low neutron flux (D-Li releases no neutrons).

The invention entitled Static Fusion of Dry Deuterium appears to have been confirmed by the research of Scaramuzzi of Italy, which reportedly utilized the same set-up as this invention.

The credential of people in our group currently are as follows (brief):

Walter L. Wagner
- currently: Department of Education/Hawaii (five years)
  Physical, Biological, and Mathematical Sciences Lecturer
- formerly: U.S. Veterans Administration (four years)
  Medical and Radiation Physics Director
- formerly: U.C. Berkeley/Physics & Space-Sciences (two years)
  Cosmic Radiation Research Associate

Richard J. Wagner
- currently: TRW, Inc. (five years)
  Satellite Engineer
- formerly: U.S. Air Force (nine years)
  Electronics Engineering Technician

Dale Hammond
- currently: B.Y.U.-Laie (twenty years)
  Science Professor

We are also working with persons at U.H.-Manoa, but our formal staffing is not yet finalized.

As a tentative grant budget I suggest the following expenditures will allow for a bench-top generator producing steam by static fusion, within the first year, and a small pilot plant within two years:

Pre-Proposal Fusion Budget

Salaries	
Research Director	$50,000.00
Research Associate	$35,000.00
Research Associate	$35,000.00
Engineer	$30,000.00
Secretary	$20,000.00
Laboratory & Office Rentals	$35,000.00
Equipment & Supplies	
Sodium-Iodide (Th-activated) Scintillator	$5,000.00
Geiger-Mueller Detector	$1,000.00
Neutron Counter	$5,000.00
Deuterated Water	$10,000.00
Deuterium Gas	$10,000.00
Liquid Scintillation Counter	$35,000.00
Voltage Supply and Electronics	$10,000.00
Tools	$10,000.00
Miscellaneous (Lithium, pressure chambers, pumps, etc.)	$30,000.00
Legal and Office Supplies	$10,000.00
TOTAL	$331,000.00

This is the total pre-proposal for the first year's expenditures. Thereafter, additional expenditures would be required for the second year, including the equipment for a pilot-plant demonstration.

The underlying theory behind static-fusion is, in essence, quite simple. In the voltage applied devices, the inner-orbital electrons are stripped from the fusing nuclei, preventing their ability to repel the atoms due to electron electrostatic repulsion. Absent those electrons, the nuclei are allowed to move closer together than normal, so that only the electrostatic repulsion of the positive nucleus is at play. By using large numbers of such atoms (on the order of 10^{24}) a probability of fusion of one pair per 100 billion years translates to several fusions per second. Thus, by simply allowing the atoms to sit in residence next to each other for a long period of time (hours), macroscopic fusion rates are achievable.

In the other devices, fusion takes place within a metal matrix, in which the electrons are incorporated into the "electron sea" of the metal, again allowing for the nuclei to move closer together than normal.

If you wish to FAX to me, please do so to this number:

FAX# (808) 524-2790; also, be sure to include my name and business number (above), as this is a shared FAX number, and I will be called when I receive messages.

I thank you in advance for your kind attention to this matter, and hope to see you at the Santa Fe Workshop.

Sincerely,

Walter L. Wagner

Walter L. Wagner
Development Director