Department of Energy
Savannah River Operations Office
P.O. Box A
Aiken, South Carolina 29802

JUN 21 2016

CERTIFIED MAIL
RETURN RECEIPT REQUESTED

Mr. Steven Krivit
New Energy Times
369-B Third Street, Suite 556
San Rafael, CA 94901

Dear Mr. Krivit:

SUBJECT: Freedom of Information Act Request, Headquarters HQ-2016-00929-F/Savannah River Operations Office SRO-2016-00952-F

This letter constitutes our final response to your May 8, 2016 FOIA request to the Department of Energy Headquarters (DOE-HQ), Washington, DC. You requested information on the electronic search results for each email containing the word MITSUBISHI or IWAMURA, or the term MHI within the email records for Kirk Shanahan, Savannah River Nuclear Solutions. DOE-HQ transferred your FOIA request to this office for processing.

The documents identified on the enclosed Index List are responsive to your request. However, the FOIA statute, 5 U.S.C. 552(a)(2) states "[e]ach agency shall make available to the public information as follows (2) Each agency, in accordance with published rules, shall make available for public inspection and copy … (D); unless the materials are promptly published and copies offered for sale." Documents 025 and 029 are available for purchase from the Royal Society of Chemistry at the website identified on the Index List.

Please refer to the organization identified on the Index List for Documents 006, 012, 020, and 036 to download from their respective website, since they contain "all rights reserved" information on them. The Naval Research Laboratory originated Document 018. Therefore, we are transferring this document that office for review and direct response to you. If you have any questions about this document, please contact the FOIA Coordinator at the address provided below.

For purposes of assessment of any fees, you have been categorized under the DOE regulation that implements the FOIA at Title 10, Code of Federal Regulations (CFR), Section 1004.9(b)(3), as a "news media" requester. Requestors in this category are charged fees for duplication only and are provided 100 pages at no cost. There is no charge for the enclosed documents.

As Chief Counsel, DOE-SR, I am the authorizing official for the documents responsive to your request. If you have any questions, please contact Ms. Pauline Conner at (803) 952-8134.

Sincerely,

Lucy M Knowles

Lucy M. Knowles
Authorizing Official

Enclosures
Index List and Response Documents

Cc w/copy of Document 018 and Response Letter
FOIA Coordinator
Naval Research Laboratory, Code 1030
4555 Overlook Ave., SW
Washington, DC 20375-5320

SRO-2016-00952-F
Responsive Document 001

{In Archive} Iwamura in ICCF8?

Kirk Shanahan WBclarke 11/13/2001 02:55 PM

Hi Brian,

Can you look in that ICCF proceedings you have for any papers by Iwamura? Rothwell is touting their quality.

FAX would be nice if you find one. 803-208-8684.

Thanks!
Kirk

SRO-2016-00952-F
Responsive Document 002

"WBclarke"
<wbclarke@mcmaster.ca>
11/13/2001 03:33 PM

To <kirk.shanahan@srs.gov>
cc
bcc
Subject Re: Iwamura in ICCF8?

Hi Kirk,

The bold Frothwell touted Iwamura et al's paper in ICCF-8 to me some months ago. I read it then and it is spectacular if you are naive and believe everything in the paper. There are all kinds of missing details however so I put the paper in my "looney bin". I will send you a copy of this paper later today. See what you think.
On another front --- Victory. Uckan has caved in and will accept my paper without sending it back to Mcboob for a second review.

Cheers
Brian

-----Original Message-----
From: kirk.shanahan@srs.gov <kirk.shanahan@srs.gov>
To: WBclarke <wbclarke@mcmaster.ca>
Date: Tuesday, November 13, 2001 2:55 PM
Subject: Iwamura in ICCF8?

Hi Brian,

Can you look in that ICCF proceedings you have for any papers by Iwamura? Rothwell is touting their quality.

FAX would be nice if you find one. 803-208-8684.

Thanks!
Kirk

SRO-2016-00952-F
Responsive Document 003

{In Archive} Re: Iwamura in ICCF8?

Kirk Shanahan WBclarke 11/13/2001 03:52 PM

Hi Brian,

Great news on the paper! Do you plan to announce it in spf?

Kirk

SRO-2016-00952-F
Responsive Document 004

{In Archive} Re: Comments on a Mizuno paper
Kirk Shanahan   Dieter Britz   11/14/2001 06:44 AM

Hi Dieter,

The reason I have weird posting addresses these days is that my company shut down their newsgroup server on Nov. 30. So, now I am posting via Web newsgroup servers. The one that does that with the addresses is Randori (www.randori.com), which I like because it shows the dates posts were made. Actually, it's my fault, as I am able to replace the gobbledegook with my real address, but it's a manual operation I sometimes forget to do.

As you've remarked before, the transmutation stuff is garbage. I just want to make a point about Rothwell to those who still treat him as an authoritative source. I'm about done now, but I did ask Brian Clarke to send me the Iwamura paper from the ICCF8 Proceedings, so maybe one more salvo.

Brian got his anti-McK-Case paper accepted finally. I'm going to forward his response to my congratulatory note. I hope you get as good a laugh from it as I did.

Kirk

Dieter Britz <db@chem.au.dk>

Dieter Britz
<db@chem.au.dk>

To: kirk.shanahan@srs.gov
cc:
Subject: Re: Comments on a Mizuno paper

11/14/01 03:23 AM

Hi Kirk

what is that weirdo address you occasionally post from these past few days? I tried to email you to it but got a bounce.

I have these Ohmori (& Mizuno) papers in Current Topics Electrochem., and am about to read them. I also got the impression that they got those impurities off bottle labels - hard to imagine them doing actual assays for all those. And the thought of a nichrome heater immersed in that suprapure electrolyte... I'm going to ask Ohmori about this, I now have his email address.

Regards
Dieter

-- Dieter Britz   http://www.chem.au.dk/~db

SRO-2016-00952-F
Responsive Document 005

Rich Murray <rmforall@earthlink.net>
12/24/2001 03:43 AM

To kirk.shanahan@srs.gov
cc
bcc
Subject Murray: Iwamura critique 7.22.98

Dec 24 2001 Hello Kirk Shanahan, I scanned through your debates with Ed Storms about his research, and Jed Rothwell about the recent Iwamura work, listed by http://groups.google.com .

I appreciate your clarity, thoroughness, experience, patience, and humor in raising all the really obvious questions about the zillions of things that go wrong in "cold fusion" research. Here is an effort I made three years ago on a paper by the Iwamura gang in Fusion Technology.

Between you and me, Miley had asked me to peer review their original draft, which is basically what is repeated here by me.
Of course, I firmly rejected their draft.
I was really surprised to find that Miley printed their original draft completely unchanged! So, I immediately posted my original critique, with very few changes, if I may risk trusting my memory.
I still have my original critique, for Miley's eyes only, as well as his letter that asked me to review their draft-- I suspect he thought it was so good, that I would become convinced that CF was real...

William B. Clarke just sent me his two papers in Sept. Fusion Technology about no He-4 and plenty of He-3 in McKubre's SRI runs with Arata-Zhang cathodes,
along with comments and his counter-comments with Talbot A. Chubb and with B. F. Bush and J.J. Lagowski, veterans in the platoon with Melvin H. Miles. Are you on his mailing list? I'll be happy to send zeroxes of it all to you, in case you want to risk the danger of more addiction to apparently fruitless CF critiquing. However, I've found the same incredible, and far more harmful, scientific pathology in aspartame toxicity research, the extremely common sweetener in almost all diet sodas, which I've been exposing now for three years. It happens to be 10% methanol...

wbclarke@mcmail.cis.mcmaster.ca

Subject: Murray: Iwamura critique 7.22.98
Date: Wed, 22 Jul 1998 20:27:24 -0500
From: Rich Murray <rmforall@earthlink.net>
Organization: Room For All

July 22, 1998
Rich Murray Room For All
1943 Otowi Drive Santa Fe, NM 87505
505-986-9103 rmforall@earthlink.net

Yesterday, Los Alamos National Lab Library received the July "Fusion Technology," with "Detection of anamolous elements, x-ray, and excess heat in a D2-Pd system and its interpretation by the electron-induced nuclear reaction model," Y. Iwamura [iwamura@atrc.mhi.co.jp], T. Itoh, N. Gotoh, I. Toyoda, "Fusion Technology, 33, July, 1998, p. 476-492, Received Sept. 8, 1997, Advanced Technology Research Center, Mitsubishi Heavy Industries, Ltd, 1-8-1, Sachiura, Kanazawa-ku, Yokohama 236, Japan.

On May 8, Eliot Kennel [ekennel@compuserve.com], an experienced researcher who had spent two years working closely with CF researchers in Japan, posted a long and detailed critical summary of ICCF-7: "I was disappointed by a presentation by Ohmori, in which he claimed that some anomalous effect occurred during high current electrolysis, at which point the electrode becomes hot and generates a plasma. A fantastic neutron flux (106 n/sec) was claimed, but then Ohmori admitted that this might be due to electromagnetic noise from the plasma. Since he is not dead from radiation poisoning, the latter explanation is likely. It seems to me that this is probably nothing more than the burnout heat flux (at a certain point, the heat transfer coefficient decreases, which causes the surface to heat up, which causes the heat transfer coefficient to further decrease, and so on. This causes flash boiling, similar to what Ohmori observed). The low quality of this paper frankly shocked me, and may cause me to re-evaluate the isotope shift papers by the Hokkaido University group. My confidence in their research has been thoroughly shaken. Similarly, the work of the Iwamura group at Mitsubishi Heavy Industries (MHI) was disappointing, as they reported non-reproducible results which have the definite appearance of electronic noise. Several papers from China also fit into this category."

Iwamura et al apply 20-40 W from a round 1.2 cm Pt anode to a square Pd cathode, 25X25X1 mm, at 1-3A in a 1 M LiOD/D2O electrolyte for week-long runs. The bottom side of the Pd cathode mounts to a vacuum chamber with an O-ring gasket.
The cell seems to be about 6 cm diameter, if their drawing is to scale, with the vacuum chamber 4 cm high, and the cell 5.5 cm high. A Pt recombiner was tested to be >99 % efficient. Five turns of a cooling tube, perhaps stainless steel, plated with 10 micron Au, conducts "pure water" for mass flow calorimetry. At the start of each run, Ar gas is put above the electrolyte at 1 atm. The vacuum pumping speed of the turbo molecular pump is 50 L/s-- is that constant? The electrolytic cell is Teflon, with all its internal parts coated with sprayed Teflon. The composition of the vacuum cell is not given.
Two NaI scintillation counters monitor the cell from outside, while in the vacuum a third one is mounted in the base, pointing about 2 cm from the Pd cathode. A He-3 neutron detector is outside the base of the cell. Data are logged every 20 s, and the energy spectrum of X rays every 6 hours. Pressures in the cell and in the vacuum are monitored, and used to estimate the loading of the Pd, which reaches .8 in a day [8.64X10E4 s], but no independent measures of loading are given.

Fig. 3 shows two graphs of electrolytic vs vacuum pressures, for two almost identical 3.3 day runs. Page 479: "However, it is easy to see that the absorpion and desorpion of deuterium are entirely different, which suggests that the absorpion and desorption behavior of deuterium is greatly influenced by unspecified factors,i.e. , metallurgical conditions such as impurity and defects in Pd."

I think I know what the "unspecified factors" are-- leaks. EV29 shows a leak that lets gas into the vacuum, producing a steady state pressure, regardless of increasing electrolytic pressure. The trace becomes a thick line, indicating a rapidly fluctuating leak. EV34 shows an initial leak that somehow got plugged, allowing the vacuum to be restored.

We've run into O-ring seals recently, with the ill-fated Cincinnati Group. A little thermal expansion, some reuse of the apparatus, and, voila!, data stew! Pd is well known to expand and crack with high loading.

Probably, they have only one possible case of an element anomaly: Ti on the electrolytic surface of palladium sample EV27. Toward the end of my

three-hour session, I realized, with a distinct shock, that the cooling tube, probably Cu or stainless steel, plated with a delicate 10 micron Au film, wound five times around the perimeter of the electrolyte, was perhaps 80 cm long, with surface area about 40--100 cm2. I suppose the cell was used again and again, and with an accumulation of scratchs, electrochemical corrosion between the gold and the metal would release all kinds of ions during the days of operation. They found a layer of stuff, full of Ti, with a thickness from .2 to 3 microns, a 15-fold range, in a disk of deposition 1.2 cm wide, which had, "...estimated increased Ti mass is about 21 micrograms." It could just as easily be five-fold less. Why not do a chemical extraction and assey to determine the exact mass of Ti?

Page 482: "Of course, we did not add any Ti to the electrolyte or the Pd

and Pt electrodes." But, what if an overzealous underling did? These things happen.

The calorimetry is inadequate, with no insulation mentioned or depicted, and the 25X25X1 mm Pd cathode freely radiating any excess heat into the vacuum chamber, with a large heat sink, a cylinder of Pb (mass?) with 2.5 cm thick walls. Table III lists the largest Excess Heat as: max 3.2 W, about 7.5 to 15 % of the "20W to 40W" input power range--
but this seems to be just a temporary fluctuation. Fig. 8 has a histogram of excess heat distributions, showing values ranging from +3.5

to -1.5 W, for sample EV39, giving a mean of +1.14 W, a spurious 3-digit accuracy. The statistical significance of this value is not given.

The method for calculating D/Pd loading very much needs to be checked by independent measurements. Probably, the loading would vary greatly across the plate, which could be a good feature, if reactions happen only at certain values. They assume, for one, that the flow is not spotty across the plate.

The X-ray data on p. 480 is their strongest suit-- but is there only one

case of radiation below the cathode plate? Days of 50 counts per second

bursts sound convincing at first, but there seems to be no replication available in their data set. Did they try and fail to replicate the X-ray result? Fig. 5 of "Simultanous detection" by the two NaI detectors might be from sparks and glow discharges from minute leakage of D2, D2O, and Ar. Only an interval of .2 from 1.55 to 1.75 X 10E5 s is shown, and the matching lines are in an interval of .03 from about 1.65 to 1.68 X 10E5 s, from a run perhaps as long as 6 X 10E5 s. This is rather select data, considering the novelty and importance of the claim. Table II shows via ICP/MS a large range for the largest impurity, Fe, in three used Pd cathode samples: 260, 210, 30 ppm. Nothing is said about this, while much is made of the 8-fold excess of Ti for sample EV 27. What is the actual amount of the cathode analyzed? Of the eight impurities from three used Pd cathodes, namely, Ca, Ti, Cr, Fe, Ni Cu, Pt, and Au, only Fe, Cu, and Au are higher in one or two of the used samples than in the two unused samples: these three elements may come from the cooling tube, which may be copper or stainless steel, plated with a frail 10 micron layer of Au.

Page 486: "Another point to consider is that Ti atoms are not always detected. Sometimes, other elements are found, such as Si, Au, Pb, Cr, Cu, Fe, and so on; and sometimes, no elements are detected even though the experimental conditions are almost the same. In addition, the quantities of the detected elements vary. As is visible to the naked eye, the shades of the black circle are different every time; sometimes the circle corresponding to the shape of the Pt anode looks brown or metallic." Stainless steel can supply Si, Cr, Cu, and Fe. Complex, variable corrosion of the cooling tube and other components can inexplicably supply various impurities over the several dozen or so runs.

I will now move through the report in sequence:

Electrolyte: mass, Ph, volume, accumulation of impurities?

Palladium plate: mass, before and after runs? Shape changes, corrosion, subtle leaks?

Recombiner: mass, trace elements?

Cooling pipe: dimensions, composition, mass before and after runs, trace elements in cooling pipe and Au film, corrosion, subtle leaks?

Coolant flow rate: values, constancy, accuracy of measurement, exact composition of fluid, how long used, mass, trace elements, any accumulation of impurities over time, exposure of fluid to heat sources and impurities outside the cell, bubbles, suds? Accumulated gunk that slows down the pump?

Thermocouples: type, accuracy, constancy, placement inside cooling tube or on outside, insulation, actual values for solution, gas, recombiner?

Teflon: mass before and after runs, condition after runs, any deposits of gunk or absorbed gases, actual permeability of sprayed Teflon on wires, shape changes, thermal expansion, subtle leaks? How often is cell reused? Scott Little in testing the CETI RIFEX cell, found that impurities from one run could contaminate successive runs.

Pressure in electrolyte and vacuum: accuracy, actual values, constancy, any evidence of subtle leaks?

A subtle leak could release D2, D2O, and Ar into the vacuum. Any mass or shape changes in the O-ring gasket? Was the gasket reused? Did its appearance change? Teflon is an excellent insulator-- any evidence for static electricity buildup in the vacuum or on the Pb cylinder, or on the outer surface of the cell, since glow, corona, or spark discharges could cause spurious signals in the NaI detectors? Any 10-100 volt potentials available from the detectors or other electronics?

NaI scintillator abd He-3 neutron detectors: sensitivity at various energies, reliability, known characteristic weaknesses, size and shape, mass, voltages, actual background in detail throughout whole history of experiments for years, calibration with known sources, diffusion and attenuation of any radiation within and from cell, actual values and history of electric noise?

Al, MgO, etc. coatings: purity, trace radioactivity? K-40 is a common, radioactive isotope. Th-232? How much did these coatings impede D2 gas flow?

D/Pd ratio: Any checks by other methods? Accuracy, reliability, precision, stability, fluctuations, impurity effects, accumulation of impurities on plate and in electrolyte, size and shape changes in plate due to high loading, subtle leaks, spotty flow through plate, bubbles on plate, outgassing bursts, temperature spikes?

D/Pd analysis, Fig. 2: One hour is 3.6 X 10E3 sec, one day is 8.64 X 10E4 sec. What happens over the several days of the run? What are the exact values for a typical stretch of time?

X-ray events, Fig. 4: Mean background (B.G.) 3.55 counts per sec, 17 counts per minute, which is 2.4 million counts in 600,000 sec. Why the lack of counts for a day during the middle of the week? How many cumulative counts are in the peaks that rise to as much as 60 counts per sec? The energy spectrum, total counts at each energy level (how wide is this energy interval?) indicates 100,000 counts at about 10 keV, which is 1 every 6 seconds, far below background, and about 1 count in 100 minutes at 50 keV, very far below background. Above 100 keV the signal merges into the background at ~1000 counts at each energy. How typical is this kind of data pattern? Page 479: "Note that a characteristic X-ray (k-alpha, beta) of Pd (~21 keV) was not observed."

How many samples were run, and how about summaries about each and every run?

Simultaneous detection, Fig. 5: Page 480: "We observed this kind of X-ray emission many times (more than 20). In these cases, nuclear reactions must occur on the electrolyte side of the Pd." Linked electronics, rf interference, sparks? The background for # 2 is about 14 cps, and for # 3 about 15 cps. Are the apparent coincidences the only ones for this run? Exactly how many other runs? Detailed coincident data for all 20+ runs?

Neutron data, Fig. 6: Is the spike the only one in that run? The two X-ray graphs show background of 14 cps, and no X-ray coincidences for a 13.9 hour period.
There seems to be no credible evidence for any neutron emission: page 480: "Figure 6 shows the correlation between neutron and X-ray emission and indicates that the neutron and X-ray emission do not correspond. However, X-rays 2 and 3 are relatively high when the neutron bursts. [sic] It is considered that certain physical conditions that cause nuclear reactions were satisfied at about the time

of the neutron bursts...Because of the weak correlation between the neutrons and X rays [sic], in addition to the low reproducibility of neutron emissions, it is certain that the neutrons and X rays [sic] are produced by different nuclear reactions." Fig. 6 shows a sharp neutron count rate peak of 0.7 cps, above a background of about 0.05 to 0.1 cps:

the peak is an interval of about .1 X 10E5 s during an interval of 13.9 hours from 2.5 to 3.0 X 10E5 s.

Excess heat: Page 481: "...therefore excess heat is a few percent of the input power." This a meaningless claim, unless the calorimetry is extremely competent. What are voltage, resistance, current, and input power, and how precise and constant are these values? Any apparent correlations are therefore meaningless. Increased current can raise the temperature of the cell and cause all sorts of artifacts. For instance,

bubble accumulation on the plate could cause apparent heat changes, and sudden release of these bubbles can cause apparent heat bursts. The plate is horizontal. How much stirring was caused by bubbling? At 3 A,

the current density for a plate of 6.25 cm2 area is about .5 A per cm2. Was the electrolyte stratified into different temperature zones at times, and then stirred? How great are the temperature differences within the electrolyte at different times?

Fig. 8 shows a frequency histogram of excess heat. Why a dip at 1.5 W? The comparison with the shape of the histogram for a different sample, with a five-fold greater frequency, is without meaning. Using these meaningless correlations, the authors say, page 481: "Up to now we observed excess heat genberation several times; however, we could not see any clear relations between excess heat generation and X-ray emission...Judging from these results, we might consider that excess heat and x-rays are generated by different nuclear reactions."

The reader by now may be familiar with this pattern of extracting correlations about "nuclear reactions" from random data sets.

Page 482: "Excess heat of about 1 W lasted for 1 day in the case of EV27, although x-ray and neutron were not detected." This is 2.5 % of 40 W input power, an absolutely meaningless result, given the poor quality of the calorimetry.

Page 487, "EV8 is the sample that emitted continuous long-term X rays. [sic]. The elements Ca, Cr, Fe, Pt, Ti, and O are detected [by EDX and WDX, Fig. 17] on the black circle on the surface of the electrolyte side. As these results indicate, a correlation between these elements detected on the Pd and nuclear products or excess heat is not clear at present."

Table 3, Summary of Multi-Layer Cathode Experiments: Why is so little data given? The excess heats given, are maximums, as large as 3.2 W, only a meaningless small fraction of input power. What is the integrated excess heat? What do the simultanous x-ray graphs actually look like? How common are "Simultaneous detection", claimed in five of the six runs?

Of the 11 references, 5 are to Iwamura reports at International Cold Fusion Conferences, and 3 to reports by Mizuno, Ohmori, and Miley, which are unable to withstand scrutiny. Rich Murray
****

RTM: aspartame toxicity: recent research 12.24.1 rmforall

Rich Murray, MA   Room For All   rmforall@earthlink.net
1943 Otowi Road, Santa Fe NM USA 87505   505-986-9103

http://groups.yahoo.com/group/aspartameNM/messages for 763 posts
http://groups.yahoo.com/group/aspartameNM/message/657 45K post
http://groups.yahoo.com/group/aspartameNM/message/763 30K post

http://www.dorway.com/tldaddic.html 5-page review
"Aspartame (NutraSweet) Addiction"
H.J. Roberts in "Townsend Letter", Jan 2000 HJRobertsMD@aol.com
http://www.sunsentpress.com/   sunsentpress@aol.com
Sunshine Sentinel Press P.O.Box 17799 West Palm Beach, FL 33416
800-814-9800 561-588-7628 561-547-8008 fax

http://groups.yahoo.com/group/aspartameNM/message/669
1038-page medical text "Aspartame Disease: An Ignored Epidemic"
published May 30 2001 $ 85.00 postpaid   data from 1200 cases
available at http://www.amazon.com
over 600 references from standard medical research
http://www.aspartameispoison.com/contents.html   34 chapters

http://groups.yahoo.com/group/aspartameNM/message/752
Headache 2001 Oct;41(9):899-901
Migraine MLT-Down: An Unusual Presentation of Migraine
in Patients With Aspartame-Triggered Headaches.
[Merck 10-mg Maxalt-MLT, for migraine, has 4 mg aspartame,
while 12 oz diet soda has 200 mg.]
Newman LC, Lipton RB.   RLipton@IMRInc.com
Headache Institute, St. Lukes-Roosevelt Hospital Center, New York
NY Department of Neurology
Albert Einstein College of Medicine, Bronx, NY
Innovative Medical Research

http://groups.yahoo.com/group/aspartameNM/message/652
Ann Pharmacother 2001 Jun;35(6):702-6
Relief of fibromyalgia symptoms following
discontinuation of dietary excitotoxins.
terpening@fpmg.health.ufl.edu cterpeni@ufl.edu
Smith JD, Terpening CM, Schmidt SO, Gums JG.
Malcolm Randall Veterans Affairs Medical Center, Gainesville, FL, USA.
gums@fpmg.health.ufl.edu   siggy@hands.ufl.edu

http://groups.yahoo.com/group/aspartameNM/message/346
WebMD: Barclay: Barth:
survey shows aspartame hurts memory in students 11.9.00
http://www.psy.tcu.edu/psy/barth.htm
Timothy M. Barth Department of Psychology   t.barth@tcu.edu
Texas Christian University TCU Box 298920 Fort Worth, TX 76129
Chairman, Physiological Psychology   817-921-7410

http://groups.yahoo.com/group/aspartameNM/message/760
Magnes Res 2001 Sep;14(3):189-94
The effect of oral aspartame administration on the
balance of magnesium in the rat.
Kovatsi L, Tsouggas M.
Laboratory of Forensic Medicine & Toxicology, Faculty of Medicine
Aristotle University of Thessaloniki, Greece kovatsi@med.auth.gr

http://groups.yahoo.com/group/aspartameNM/message/689

Measurement of molecular interaction of aspartame and
its metabolites with DNA. Clin Biochem. 1998 Jul;31(5):405-7.
Karikas GA, Schulpis KH, Reclos GJ, Kokotos G.
Dept. of Chemistry, University of Athens, Greece
http://www.chem.uoa.gr gkokotos@atlas.uoa.gr

http://ww.presidiotex.com/barcelona/index.html
Life Sci June 26 1998; 63(5): 337-49
Formaldehyde derived from dietary aspartame binds to tissue
components in vivo. ["Trok-ho"]
Trocho C, Pardo R, Rafecas I, Virgili J, Remesar X,
Fernandez-Lopez JA, Alemany M, Departament de Bioquimica i
Biologia Molecular, Facultat de Biologia, Universitat de Barcelona,
Spain. http://www.presidiotex.com/barcelona/index.html
Maria Alemany, PhD alemany@porthos.bio.ub.es

Two teams find hot aspartame releases DKP, a potent carcinogen:
Food Addit Contam 2000 Oct; 17(10): 821-7
Simultaneous formation and detection of the reaction product of
solid-state aspartame sweetener by FT-IR/DSC microscopic system.
Lin SY, Cheng YD
Biopharmaceutics Laboratory,
Department of Medical Research & Education
Veterans General Hospital-Taipei, Shih-Pai, Taiwan,
Republic of China. sylin@vghtpe.gov.tw
and
J Pharm Sci 1998 Apr; 87(4): 508-13
Hydration and dehydration behavior of aspartame hemihydrate.
Leung SS, Padden BE, Munson EJ, Grant DJ
Department of Pharmaceutics, College of Pharmacy,
University of Minnesota, Minneapolis 55455-0343, USA.
Sophie S. Leung, PhD
Dolores J. Grant, PhD grant1@niehs.nih.gov

http://www.medscape.com/MedGenMed/braintumors
Lennart Hardell, M.D., PhD, in 1999 reported in Sweden that both
cell phone use and heavy aspartame use correlate with increased
brain cancers lennart.hardell@orebroll.se +46 19 602 15 46

http://www.dorway.com/blayenn.html dodd@netdoor.com
Russell L. Blaylock, M.D. russell@misnet.com 601-982-1175
"Excitotoxins, Neurodegeneration and Neurodevelopment"
The Medical Sentinel Journal Fall, 1999 , (95 references)

http://www.dorway.com/barua.html
Journal Of The Diabetic Association Of India
1995 Vol. 35, No. 4. Emerging Facts About Aspartame
Dr. J. Barua (ophthalmic surgeon), Dr. Arun Bal (surgeon)
(79 references) barua@giasbm01.vsnl.net.in
"...the total amount of methanol absorbed will be approximately
10% of aspartame ingested. An EPA assessment of methanol states
that methanol "is considered a cumulative poison due to the low rate
of excretion once it is absorbed." The absorbed methanol is then
slowly converted to formaldehyde..."
"Reaction of formaldehyde with DNA has been observed,
by spectrophotometry and electron microscopy, to result in
irreversible denaturation."
"DKP has been implicated in the occurence of brain tumors."

http://groups.yahoo.com/group/aspartameNM/message/628
Rich Murray: Professional House Doctors: Singer: EPA: CPSC:

formaldehyde toxicity 6.10.1 rmforall

http://groups.yahoo.com/group/aspartameNM/message/645
Rich Murray: 18 recent formaldehyde toxicity [Comet assay] abstracts 6.25.1 rmforall

http://www.dorway.com/wmonte.txt
Dr. Woodrow C. Monte, "Aspartame: Methanol, and the Public Health," Journal of Applied Nutrition, Volume 36, No. 1, pages 42-54, 1984. (62 references) Professsor of Food Science
Director of the Food Science and Nutrition Laboratory
Arizona State University, Tempe, Arizona 85287
6411 South River Drive #61 Tempe, Arizona 85283-3337
602-965-6938 woody.monte@asu.edu
The methanol from 2 L of diet soda, 5.6 12-oz cans, 20 mg/can, is 112 mg, 10% of the aspartame. The EPA limit for water is 7.8 mg daily for methanol (wood alcohol), a deadly cumulative poison. Many users drink 1-2 L daily. The reported symptoms are entirely consistent with chronic methanol toxicity. (Fresh orange juice has 34 mg/L, but, like all juices, has 16 times more ethanol, which strongly protects against methanol.)

http://www.truthinlabeling.org/ Truth in Labeling Campaign [MSG]
Adrienne Samuels, PhD P.O. Box 2532 Darien, Illinois 60561
858-481-9333 adandjack@aol.com "The Toxicity/Safety of Processed Free Glutamic Acid (MSG): A Study in Suppression of Information" Accountability in Research (1999) Vol 6, pp. 259-310

http://www.dorway.com David O. Rietz 12,000 print pages
Mission-Possible-USA Betty Martini 770-242-2599
Bettyml9@mindspring.com
http://www.dorway.com/asprlink.html many links
http://www.dorway.com/nslawsuit.txt Jeff Martin, Attorney
http://www.dorway.com/upipart1.txt
UPI reporter Gregory Gordon: 96K 3-part expose Oct 1987
http://www.dorway.com/doctors.txt
What many informed doctors are saying/have said about aspartame

http://www.HolisticMed.com/aspartame 603-225-2100
Aspartame Toxicity Information Center Mark D. Gold
mgold@tiac.net 12 East Side Drive #2-18 Concord, NH 03301
http://www.holisticmed.com/aspartame/abuse/methanol.html
"Scientific Abuse in Aspartame Research"

http://www.readthelabel.org.uk/ arthur@mcbryan.co.uk
outstanding site by Arthur McBryan

http://www.aspartame.ca/ John T. Linnell admin@aspartame.ca
http://www.aspartame.ca/page_a10.html
Canadian Class Action Law Suit

The great health advantages of a no-fat vegetarian diet are well described by Dr. John A. McDougall at http:www.drmcdougall.com , which has copious scientific references and Net links, and at http://www.vegsource.com
****

Serious symptom syndrome summary:
Aspartame (NutraSweet, Equal, Canderel, Benevia) is reported by scientific studies and case histories to be toxic: * headaches
* many body and joint pains (or burning, tingling, tremors, twitching,

spasms, cramps, or numbness) * fever, fatigue
* "mind fog", "feel unreal", poor memory, confusion, anxiety, irritability, depression, mania, insomnia, dizziness, slurred speech, ringing in ears, sexual problems, poor vision, hearing, or taste
* red face, itching, rashes, burning eyes or throat, dry mouth or eyes, mouth sores * hair loss
* obesity, bloating, edema, anorexia, poor or excessive hunger or thirst * breathing problems
* nausea, diarrhea or constipation * coldness * sweating
* racing heart, high blood pressure, erratic blood sugar levels
* seizures * birth defects * brain cancers * addiction
* aggrivates diabetes, autism, ADHD, and interstitial cystitis (bladder pain)
****

http://members.tripod.com/~mission_possible/scotland_branch.html
http://www.aspartame.ca/ Canada
http://www.geocities.com/HotSprings/4578/ Canada
http://www.cybernaute.com/earthconcert2000/AspartaMalcache.htm
http://www.reseauproteus.net/therapies/nutritio/aspartame.htm
http://ww2.grn.es/avalls/aspa1.htm Spain
http://www.geocities.com/HotSprings/Falls/8669/ Brazil
http://www.phd.com.br/aspartame.htm
http://hem.passagen.se/mission.possible.sweden/
http://home.online.no/~dusan/foods/aspartame.html Norway
http://www.ostara.org/aspartam/#anfang Germany
http://www.gunneweg.nl/ Holland, in Dutch
http://www.laleva.org/ Italy
http://www.laleva.org/alimenti/dorwayaspartame.html
http://users.westnet.gr/~cgian/aspartame.htm Greece
****

http://www.vegsource.com excellent diet info
http://www.mad-cow.org/ BSE/nvCJD mad cow disease
http://www.notmilk.com dairy toxicity
http://www.litopia.com/jplant/ Jane Plant on breast cancer
http://groups.yahoo.com/group/aspartameNM/message/538 Plant
http://www.dorway.com aspartame toxicity
http://www.truthinlabeling.org/ MSG toxicity
http://www.asomat.com/links/links-content.htm dental amalgam mercury
http://groups.yahoo.com/group/aspartameNM/message/629 Boyd E. Haley
http://www.soyonlineservice.co.nz soy toxicity
http://www.thyroid-info.com Mary J. Shomon
http://www.npwa.freeserve.co.uk/ fluoride toxicity
http://www.electric-words.com/junk/junkindex.html junk science
http://www.pbs.org/tradesecrets/transcript.html Moyers on chemicals
http://www.comeclean.org reform chemical industry
****

SRO-2016-00952-F
Responsive Document 006

{In Archive} Re: Murray: Iwamura critique 7.22.98
Kirk Shanahan Rich Murray 12/26/2001 02:38 PM

Hi Rich,

Good to see you're still out there. I hadn't seen you around in the CF arena for awhile, so I wasn't keeping you up to date.

If you recall, a long time ago you asked for someone to take a look at the calorimetry of the CFers. Well, in a roundabout way I have now done that. But it all started because Ed Storms posted his raw data on the Internet and suggested people look at it. In my industrial career I have done this kind of thing many times, looking at large chemical process datasets (and an 'experiment' is just a small chemical process). So, I and, independently, Scott Little looked at the data, and immediately found a noise problem that we alerted Ed to. He corrected it and posted a second set of data, which we also both looked at. With confidence that there weren't the noise problems we saw earlier present anymore, I dove into the second data set. I noticed that Ed reported a 1.6% time drift in calibration constants, which triggered a button with me. Being trained in industrial statistical quality control, I understand the impact of variation, and of mis-measuring it. I then decided to treat the 'active' electrode as if it were in power balance, and that's when I discovered that it didn't have resultant properties that were unusual. Instead, it looked like it had a minor difference from the 'inactive' electrode. But the impact of that minor difference was major in that it leads the CFers to believe they are observing CF excess power signals! The problem I located can evidence anytime a calibration curve is used, which includes practically _every known analytical chemistry method_, not just mass flow calorimetry.

In other words, restricting myself to the CF arena, today I am safe in saying that there are NO studies available in the literature that prove the problem I outline isn't there. Thus to be conservative, we have to assume it is present, and that negates ALL the claims to excess power as detected by a calibrated method. I think that is a pretty important claim in the CF arena, and all the principals who know about it are trying like mad to ignore me. They have concocted a lot of reasons why my analysis must be wrong, but they never prove it is, so their reasons are just wishful thinking, or in many cases, outrightly wrong.

Further, Ed Storms wrote another CF review and posted it on his Web page recently. It reminded me about McKubre's ICCF3 paper on CF calorimetry, and guess what? It shows clear signs of the error I uncovered. That was back in the '91-'92 time frame! I am really serious when I say I doubt any calorimetry claim for 'excess power'.

The caliber of the CF researchers will be shown in the next few months. It is every scientist's nightmare that their work would later be found to be in error. However, it happens all the time. Steve Jones and his neutron (or was it gammas?) counting are a prime example. Now, all the CF calorimetrists are going to have to face the music, and realize they haven't proven their cases. Maybe they have stored data they can trot out to prove they are right, but I would bet in most cases they don't. We'll all just have to wait and see if we get any retractions or clarifications, or if they follow Storms' lead and try to ignore the problem, hoping it will go away. That action is what clearly demonstrates their pathological behavior.

With regards to the Iwamura paper and SIMS 'evidence' of transmutations: I was basically just responding to Jed Rothwell's statement that Ohmori and Iwamura were the 'best' evidence he had seen for transmutation. Like you, I distrust SIMS for quantitative analysis, and I also was attempting to show that simple 'complex ion' considerations could explain the 'anomalous' results that supposedly prove transmutation. The whole concept is bogus, and it's another example of scientists using a fancy piece of equipment as a 'black box', and getting burned by that.

The He analysis stuff by Brian Clarke is quite interesting. You may recall I was not impressed with the Arata and Zhang work when it was first discussed. One thing I think I briefly commented on was that they appeared to have violated a basic analytical chemistry precept of not extrapolating calibration curves. They seemed to have run standards that were orders of magnitude more concentrated in He than their subsequent samples, and just extended the line to lower concentrations to compute their He sample's content. That's a big problem, and Brian has given a specific reason why in this case. He suggests they imbedded He in their MS's walls, and then released it by D+ ion bombardment in subsequent analyses. A very reasonable scenario. Again, 'black boxing' with a fancy piece of equipment.

I am not up to date on the aspartame conflict, but I am comfortable with the idea that they may not have done adequate studies. That's often a problem. In fact, I think that we chemists need to be more aware as a group of these kind of problems. It all boils down generically to not knowing the true variation in your product/technique/data. We get a little data, and then jump to a conclusion.

I don't know if you are into the evolution/creation debate, but there's a very well written little book (~100 pages) by Philip Johnson called "Defeating Darwinism by Opening Minds" . P. Johnson is a Berkeley law professor who looks at the evolution arguments as a lawyer trying a case, and he finds it wanting. What impressed me so much was the clear parallels between the thought processes used by the evolutionists and the cold fusioneers (not a topic of the book). It seems that sloppy thinking is endemic to all human activities.

Please don't circulate this note. We will just have to wait and see what happens.

Happy Holidays,

Kirk Shanahan

P.S. I have attached the manuscript of my soon-to-be-published Thermochimica Acta paper.

calerror.pdf

SRO-2016-00952-F
Responsive Document 007

Kirk Shanahan/WSRC/Srs
07/15/2002 01:49 PM

To mikec@snip.net
cc
bcc
Subject Iwamura paper

Carrell

Dear Mike,

I'm glad to see you are considering the alternatives with regards to the Iwamura paper. I thought I'd respond to some of the questions you and Jed have raised. I would have sent this to Jed, but I gave up hope of teaching him anything long ago. Forward this to him if you like, I don't care one way or the other.

I have added another long post to spf today in response to Jed and Mike Staker's posts. You probably should see that too.

Taken somewhat in order:

"For the contamination argument to prevail, an intellectually honest critic must show a plausible physical mechanism showing Iwamurs's Fig. 4a."
MikeC

Figure 4a is a typical plot for surface segregation studies, or CVD, or any variety of normal chemical processes that deposit new species on the surface. There is no explanation required. The normal interpretation is increasing contamination. See my post of today for a few more details.

"Pr is a rare earth element, a metal, solid at room temperature. The critic must explain how the Pr got onto the target in the D2 gas, which was the only substance admitted to the chamber during the test run. If the critic does not address this issue, he is not doing his own homework."
MikeC

If it's Pr, and not Cu as noted below, or PrO2 or Pr2O3, or ...

As you have surmised in your discussions, I surmise that the 'Pr' is present in the solid or liquid starting materials. The D2 as source doesn't make sense, and that is why I say there does seem to be an isotope effect, which is interesting. However, I need to be sure all samples reported on were treated exactly the same.

"Why does the Cs or Sr mass number increase by 8? Everyone at ICCF-9 wondered about this. I know nothing about nuclear physics, but I wonder whether that could that be two alpha particles from some other reaction. But why don't the targets accept only one?"
JedR

An 8 mass unit entity would be Li-H (7Li is 92.5% of the normal distribution). Think 'SrLiH' as a molecular ion. The H comes from normal background H2 in the vacuum system. The Li would be a contaminant in Sr. At least that's one speculation.

If you look at Figs. 9b
and c, and set the peak at 92 as a rough estimator of Mo peak intensity, you can see that the spectra shown could be a superposition of a low Mo concentration and something else at mass 96. (That's possibly the SrLiH.) Now before you go ballistic on me, I have to admit that these data are the hardest to explain that Iwamura has produced to date. But does that mean anything? No, it just means I can't come up with an explanation, no big deal. Is it 'proof' of transmutation? No. (Also remember you can get up to 30% variation in isotope ratios by SIMS as well.)

Also remember that using MoS2 as a thread lubricant is common practice in UHV technology. Getting some on the sample is bad form, but possible.

"What role does the Ca play? The Cs or Sr do not transmute unless Ca is present, "

Or, the contaminant is not present unless the Ca is. Might be a big hint right there...

"but the Ca is located 400 angstroms below the surface, which is pretty far on the atomic scale. "
JedR

Nope, not for segregation. In some research I am involved with, we take 90 at% Pd - 10 at% Rh alloy, oxidize it to what we call a composite, which is nanoparticulate PdRhO2 inside pure Pd, reduce it with H2 to nanoPdRh in Pd, and then anneal it back to Pd.9Rh.1. Atoms moving all over the place there. By the way, this process alters the macrostructure of the material.

We have also observed disproportionation of metal hydride alloys at lower T's (~150C I think) in the presence of H2 that when under vacuum. There, they can take ~300C without a problem.

Bruce Koel of USC looks at monolayer films of Au on Pd and finds they alloy with a single ramp to 400C.

It isn't surprising to see effects upon running D2 at 70C for days and weeks through these composite structures.

"Iwamura selected this depth to make what he
calls a "bulk" barrier. The Pd at the surface where the Cs or Sr is located does not appear to transmute. So I suppose a reaction of some sort must be occurring 400 angstroms down below. Why would that affect the Cs or Sr?!? I have asked three distinguished experts in this field what the Ca does, and they gave me three totally different answers. (Different as far as I can tell.) It makes me feel better about being a rank amateur, knowing that the experts have no clue."
JedR

Surface chemistry is that way. Black magic. The key is multiple technique analyses. Iwamura is just doing one, so not being able to figure out what is happening is par for the course. It might take years to figure this out. That's one big reason I don't do surface chemistry as my primary work. Too much inadequate data, too much expense and time to get real answers.

"Iwamura addresses this issue. He points out that
it would be thermodynamically impossible for all atoms of Pr or Mo contamination in the bulk to gather up and migrate to the upper surface. "
JedR

Based on his computations, which are _loaded_ with assumptions. See my spf post for a few more details. I don't accept I's assertions on this.

"Shanahan has not addressed any of the eight points I raised to my satisfaction."
JedR

Well...of course...I can never do that...by definition. I have addressed all of his points on spf tho...

"This is right. The pressure gradient is away from the target surface toward the vacuum chamber below. There is a diffusion flow of D atoms through the substrate toward the vacuum, which would tend to sweep anything loose in that direction. This is the principle of the diffusion vacuum pump."
MikeC

This isn't relevant here. The D flux is unlikely to be a factor other than that it opens up the Pd matrix by hydriding. It can assist in O migration based on some Russian work. There's another big hint.

"And Pr is a rare earth element which
is obtained only by careful chemical processes. "
MikeC

Assumption on your part. It is found in nature as ore, probably an oxide. It may contaminate the CaO. I've no real idea. Besides it might be Cu, which could come from the UHV system as well. Don't know, just speculation. But not knowing does not mean I can forget the contamination issue.

"How does Pr get into the starting material? And why is it not seen by XPS at the beginning of the run? Explain, please."
MikeC

Found there in nature. Not purified away adequately during CaO production? Or is it Cu?

"Well, the XPS would not be seen at the beginning because the Pr is supposedly below the surface. That begs the question: how does it emerge later on? "
JedR

Below the surface to start, more correctly, out of the sampling volume. How it emerges is by migration/segregation. This is not just diffusion, but is a thermodynamically driven chemical process. A contributor could be the PdO that probably formed during the various preparation steps. The CaO may be reacting too, depends which oxide is more stable. Note that Cs and Sr will form very stable oxides, and might disproportionate PrxOy to get there (or CuOx).

"I doubt Shanahan can explain that, or much else."
JedR

Nasty, nasty, his bias is showing...

"He has not yet
attempted to explain how the Pr could replace Cs atom by atom, keeping the total number of atoms the same the whole time to the limits of detection. That's a remarkable trick!"
JedR

a.) Cute, don't wait for the response, just declare victory! Works for me!
""Jed has no response to this...."" (Cute, but not fair or reasonable.)
See my spf post.

b.) If you check the Figures, the total number of atoms present is not constant. See Fig. 4a and 7a. This is something Jed seems to have trumped up. If you see the claim for exact atomic quantification in the paper, let me know please. I may well have missed that too.

". . . Elsewhere, Talbot Chubb told me about an expert in spectroscopy who raised a legit concern about Fig. 4 (c). She says the Binding Energy of Cu is almost exactly the same as Pr: Cu 931.1, 950.0, Pr 931.0, 951.1, and the resolution of this XPS is not good enough to distinguish them. I responded:

This is not a problem because the paper says: "The test piece is removed from the chamber and its surface is analyzed by secondary ion mass spectrometry (SIMS)." Cu and Pr are miles apart in a SIMS. There is no SIMS data in this paper for Pr, but on the other hand it does not say "sometimes analyzed" or "analyzed in the case of Sr => Mo only." It would be crazy to do it only sometimes and not mention that in the paper

She had some concerns about molecules being confused with atoms in the SIMS, but the paper points out that this SIMS has a new gizmo to separate the two (p. 4647)."
JedR

It is a problem, because it could be Cu migrating over the surface. The Cu is a lot more common in the equipment and thus more likely. The note about lack of SIMS data is telling. Did the SIMS support Pr, or Cu ? If it did, at least a one-line comment should have been included! This is something that a reviewer should have caught during peer review. As Jed notes, not having the comment allows a lot of wild assumptions, which is just to say the situation is indeterminate. I would assume such data already exists. The comment about the molecular ion discrimination technique is not warranted. We need proof, not assertions and assumptions.

"But all gradients are away from the surface, as I mentioned in the original text. As for it Pr being in the "original material", the CaO -Pd sandwich was built up by sputtering, so both CaO and Pd must have been contaminated by Pr. How does one do this? Presumably the sandwich was also fortuitously contaminated with Mo of an abnormal isotope distribution. Isn't imagination wonderful?"
MikeC

The primary gradient of concern is of 'Pr' in the sample. It starts with no Pr at the surface. Thermo always wants some present everywhere.

The sputtering would not be done simultaneously. Typically, a little pot of the material is zapped with an e-beam, although one could use an ion beam as well, and the material is kicked into the gas phase thereby, subsequently to fall out on the surface of interest (and everywhere else in the sputter system). If the 'Pr' was in the CaO, the Pd probably would not have seen it, since the sputtering steps would have been separate to produce a layered structure.

I've addressed the 'abnormal' distribution above. Just remember superposition of multiple spectra. It's an endemic problem in MS.

"I wish someone would
explain a little more of what Iwamura means on p. 4644, column 2."
JedR

Which part? Para 1 is clear. Para 2 is clear, but contains a little wishful thinking. Para 3 says there are some peak intensity anomalies, and an explanation is offered. The variation is not outside what would be induced by 'chemical effects', i.e. oxygen or carbon interactions. There is an 'extra' unexplained peak at 939ev, indicates potential problem. Peaks comes from somewhere... Notes time dependency in peak intensities. Ah ha! Pd intensities do not change, tends to suggest the 'covering up' case is not correct (see spf). C contaminant noted. Para 4 points out just Pd does not do the 'transmutation'. Final sentence would seem to be correct. Para 5 is short and sweet, and the final sentence is also correct based on the data shown.

Most of column 2 is 'typical', no big surprises. The wishful thinking involves the quatitativeness of the XPS. What does Jed not understand?

This was very piecemealish. I hope it helps. Write with more questions, but be advised I will avoid responding to opinions.

Kirk Shanahan

SRO-2016-00952-F
Responsive Document 008

"W. B. Clarke" <wbclarke@mcmaster.ca>
07/15/2002 07:25 PM

To <kirk.shanahan@srs.gov>
cc
bcc
Subject Iwamura

Hello Kirk,

I have been reading the screeching and blathering by Frothwell, and your attempts to reply to him in a rational way on the chat groups.

It appears to me from reading Iwamura et al's recent stuff (and looking over their older stuff in FS&T and ICCF-8) that they are probably seeing spurious "peaks" at certain masses via SIMS that they interpret as due to genuine isotope anomalies. In their recent Jpn J. Appl. Phys paper they "see" a whomp at mass 141 when a Cs layer is used, and a whomp at mass 96 when a Sr layer is used. These are seen only with D2 and not with H2, so the suspicion falls on some peculiar combinations of Cs, Sr, and possibly Ca with D that pop off the surface when bombarded with 40Ar ions. The enhancement at mass 96 (assumed by Iwamura et al to be 96Mo) could be due to 88SrD4 --- most of normal Sr is 88Sr. The apparent 141Pr enhancement could be due to something like 133CsD4 --- note that Cs is monoisotopic. You can also make strange combinations of Ca (almost mono isotopic mass 40) and D and 40Ar and D --- for example in my rare gas mass spectrometer, I can see a peak at mass 41 due to ArH, a transitory species that lives long enough to get from the source to the collector. I have also seen (40A)2H and other strange combinations. I believe that Iwamura's SIMS analyses uses 40A (atmospheric Ar is practically all 40Ar at the precision claimed by Iwamura) -- it is easy to imagine combinations of 40Ar and D to make a spurious enhancement at mass 96. I would bet an extremely large sum of money that the blips at masses 96 and 141 seen by Iwamura et al. are nothing more that the effect of naive TB experimenters using methods they are not really familiar with. Mark my words.

Cheers
Brian

SRO-2016-00952-F
Responsive Document 009

{In Archive} Re: Iwamura
Kirk Shanahan W. B. Clarke 07/16/2002 06:55 AM

Hi Brian,

So you've been watching the fun and games....

Jed is not a scientist, no matter what he says or how hard he thinks he tries. He is incapable of objective analysis. He does like his rep as a 'hard-hitting critic' of cold fusion, but the fact is, he is only a critic of antiCF claims. For proCF, he is a pushover. In any case, he is unteachable as I have noted, and I had a policy for a while of ignoring him. It worked well and I should get back to it.

Mike Staker on the other hand is a newcomer, and I was watching him to see where he 'fits'. He has shown in this discussion that he 'fits' right in with the cold fusioneers, sadly so. He certainly self-destructed in his last post in response to ,e didn't he!

You should have also seen my post suggesting Sr2O(2+) as the source of 96. I tried to understand this without invoking multiatom species, but I couldn't. So, I simply point out that it can be such, and ask for proof that the technique they used to suppress those kinds of species worked as advertised. Should be par for the course, but of course, per the CF crowd my request makes me a 'PS'. Your suggestions may be valid as well. The simple fact is from the data presented we can't tell, we need more info. Unlikely we'll get it though.

The only thing that is intriguing about the paper is the supposed H/D isotope effect. I'm still trying to decide if it is worth pursuing.

Kirk

SRO-2016-00952-F
Responsive Document 010

"W. B. Clarke"
<wbclarke@mcmaster.ca>
07/16/2002 03:38 PM

To <kirk.shanahan@srs.gov>
cc
bcc
Subject Re: Iwamura

Hello Kirk,

Your SrO(2+) idea may be OK but apparently Iwamura doesn't see any effect with H, only with D. Thus, the "effect" has to be related in some way to D. I admit that I haven't read his paper carefully enough yet. Apparently, Frothwell can make up his mind by sniffing the breeze -- it must be of great comfort to be so sure of things on the basis of such puny evidence. If my countryman Oscar Wilde was alive today, he would have had a field day poking fun at the TBs. He may even have written a play about them. Instead of "The importance of being earnest" how about "The true believer's true believer."

Cheers
Brian

----- Original Message -----
From: kirk.shanahan@srs.gov
To: W. B. Clarke
Sent: Tuesday, July 16, 2002 6:55 AM
Subject: Re: Iwamura

Hi Brian,

So you've been watching the fun and games....

Jed is not a scientist, no matter what he says or how hard he thinks he tries. He is incapable of objective analysis. He does like his rep as a 'hard-hitting critic' of cold fusion, but the fact is, he is only a critic of antiCF claims. For proCF, he is a pushover. In any case, he is unteachable as I have noted, and I had a policy for a while of ignoring him. It worked well and I should get back to it.

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The only thing that is intriguing about the paper is the supposed H/D isotope effect. I'm still trying to decide if it is worth pursuing.

Kirk

SRO-2016-00952-F
Responsive Document 011

{In Archive} Re: The papers
Kirk Shanahan Dieter Britz 07/18/2002 08:14 AM

Hi Dieter,

:I have now read the Iwamura paper. Not bad actually, but of course
:there must be something wrong there, I don't believe in LENT or
:whatever they call it now. I did find a few weak points I might
:comment on in spf, no doubt getting it in the neck from Frothie.
:The team has also patented all this; I am about to put that into
:the Patents file. Being on holidays, I feel free to do that sort
:of stuff, in working hours (other peoples' that is).

While the Iwamura paper looks 'good' when compared in form to other standard publications, it has several basic flaws that I have tried to point out. What was especially significant was the Rothwell post on Vortex about the unnamed skeptic who pointed out the 'Pr' peaks were most likely Cu. That tied it all back to the preparation of the films. The only thing interesting to me is the implied isotope effect on contaminant migration, but, there was only one H run done for each configuration, so who knows if it's replicable. I saw several things that as a reviewer I would have wanted corrected before publishing. The biggest is the built-in bias against contamination as the cause. They don't present the data to support that, and they shouldn't have been allowed to publish the paper with that forced conclusion, and no examination of the other issues.

Oh well, too late I guess...

:Yes, I did wonder at the amount of typing you devote to your
:arguments with the two-three blokes.

OK, I'll bite. What's 'two-three' mean?

I have been totaling up my time recently, and I find I am spending too much time for too little return. I do seem to have the penchant for teaching, even though I'm not in academics. And, in my current job assignment, I am the only chemist in a group of 15 metallographers. I used to have a chemist as a supervisor, but even that left me starved for conversation (re. chemistry). I do like chemistry, and I feel somewhat isolated where I am now. The Internet was one way of maintaining professional level contacts. Also, I realize that I have 'the addiction', I seem to have withdrawal symptoms if I don't get my 'CF fix' routinely. But intellect shall prevail! I think I've learned just about as much as I can or want from the 'field', such as it is. I am getting less and less out of it, and in turn, I seem to be 'helping' no one. The ones who need the help the most ignore me because they are emotionally committed to their suppositions. For them, it has become a matter of faith. I know about faith, as you know, but in my dealings in the CF area I have always tried to use standard science and logic. But, I need to quit. I have a couple of items to wrap up though.

For example, and this is where I need your professional advice. In my most recent post in the 'F&P HAD' thread, I talk about Szpak's T claims, and attack it from the computational side. However, it has occurred to me that I may be being to harsh, in that the concentration of the _reactive_ species (water) will not actually be changing that much, even though the electrolyte concs change by 100-200%. I'd need to look up the detailed model Szpak references, or I can ask you I bet. Was I too quick to reject the steady state condition? Or will the electrolyte concentration changes grossly affect things? There are other things I don't like about the study as well, that I didn't get into yet, so I probably could put out a couple more long posts. I guess in the end I have to ask what good it does to comment on spf. No one who matters (actual researchers, funding orgs.) is watching. (Just the historian!)

I also have begun a first draft of a paper that responds to Ed Storms' claim that my TA paper is 'only' applicable to his Pt work. Dieter, in fact I can go back through all the CF calorimetry literature and see evidences of the same problem, including F&P's work. There is the wrinkle in their work about 'bursts', but if you postulate a messed up calibration and just look at delta-T, you find the same delta-Ts there as in Storms' (and probably McKubre's) work, so I can make a case that they just don't measure recombination properly. However, that paper keeps getting bigger and bigger, and again I don't know what value it has in the end. What I was contemplating doing was just do a so-so job on it, and then putting it out as a government report, and not actually getting it published, just publically released.

Brian Clarke wants me to co-author a paper commenting on the Bush/Miles plots where they 'correlate' 4He measurements to excess heat. Turns out their 'normalization' process includes a dilution correction for rate of electrolysis offgas production. That implies a fixed rate of He production (which is indistinguishable from a leak), which is a very suspect assumption. The should have also published the raw data (which Brian got from Bush for his 3 points). If you do that almost all the numbers seem statistically indistinguishable. Of course, my part is to show the Pex values are bogus (which they are, Bush has written about his procedures, and he doesn't check during the event either). I may go ahead with this one, just to pad the resume.

In the end though, while I have gotten a couple of things out of the field, I don't think the cost/benefit ratio is really good. I may be being impatient, and I maybe should wait a while longer to see if any more benefits accrue, but I may also accrue one big detriment. In my work with Flanagan, we have found some interesting chemistry that can be tied into the 'nanotech' world, and I may try to get some funding from a DOE office for more work. But guess what, the guy who I turn the proposal in to is J. J. Smith, co-author of Szpak, and supposedly a true blue believer, if James has any credibility (which I doubt)! I hope Smith doesn't recognize me!

I am going to try to stop responding to the 'groupies' like Salsman and the like. The one guy that bugs me right now is Staker. He's made some claims that I would like to evaluate, but his behavior seems a clear flag that I should expect zip from him, so I guess I just drop that too.

In the end, the thing that keeps me coming back is that the likes of McKubre keep getting funding to do sloppy science. I really wanted to impact the funding orgs by pointing out minimum acceptable quality levels, but again, I don't know who's listening.

In any case, we need to stay in touch, even if it's not about CF, and I will definitely try to do so.

Kirk

SRO-2016-00952-F
Responsive Document 012

Kirk Shanahan/WSRC/Srs
09/08/2003 10:12 AM

To sciencejournal@wsj.com
cc
bcc
Subject WSJ 'Cold Fusion' article

Dear Ms. Begley,

In your recent article you wrote:

"I, for one, would love to hear smart physicists explain why the excess heat from the deuterium-filled palladium reflects not nuclear fusion but the release of mechanical energy - sort of like letting go of a stretched spring."

Well, I'm not a physicist, just a physical chemist, but I have partially explained it in my recent paper (attached below). The key is that the 'cold fusion' effect occurs with platinum (Pt), and Pt does NOT form hydrides. Thus any claim that it takes a certain 'bulk loading' to get apparent excess heat is simply a red herring. You can get apparent excess heat without ANY loading. What is more likely is that it takes time to build up the active surface layer on any kind of electrode surface, and this has confused the cold fusion researchers (often called 'cold fusioneers', a term introduced by Dr. E. Mallove, an author of pro-cold fusion books and newsletter).

My explanation of the apparent excess heat is that the system has changed, and thus the calibration constants of the equation used to interpret the raw data have changed, and that produces apparent excess heat. (My paper describes this and applies it to some recent data presented as 'more' proof of cold fusion.) One could say this is an 'error', but that is simplistic. In fact, it is probably a surface chemistry mediated change in steady state. (No fancy physics required...) Simply put, the apparent excess heat is an artifact of algebra.

"I'd love to see a smart critique of a 2002 paper by Japanese scientists, published in a Japanese physics journal that few American scientists see, describing (shades of medieval alchemists) the transmutation of elements through cold fusion."

You might want to take a look at the sci.physics.fusion Usenet newsgroup article I wrote in 2001 in response to a paper by Iwamura, et al on purported transmutations in layered structures. The message id is 3bf286d0.30aa.1804289383@opus.randori.com, and you can get it by putting that number into the Google search bar on the "Groups" page. I have to admit I haven't followed up on the more recent papers since they seems to be 'more of the same'. I have commented many times in sci.physics.fusion about supposed transmutation results from many authors. Generally I see bad analytical chemistry and bad experimental science as the root causes of the observations, and no one bothers to try to alter their work to account for that.

"What these claims need is critical scrutiny by skeptics. That's how science normally functions. But in cold fusion, it isn't. And that's the worst pathology of all."

Actually, I'd like to claim I have given the field, especially the claims of excess heat, quite a bit of critical scrutiny. You can view some of the results on sci.physics.fusion, but in general a lot goes on by email since the principals never want to use public discussion forums. The real pathology of the field is well illustrated by the exchanges between myself and Mr. Jed Rothwell on sci.physics.fusion in the recent thread "Where have all the crackpots gone?" Therein you will see me reminding Mr. Rothwell that my 2002 paper directly addresses apparent excess heat results, which Mr. Rothwell routinely 'conveniently' forgets. When reminded, he devolves into character assassination to 'scientifically' defend his position.

In summary, the pathological aspect of cold fusion research(ers) is that when faced with a mundane alternative explanation, and unable to mount any cogent refutation, they resort to ignoring said explanation, and continue down the road that they somehow know is 'right'. The non-pathological approach would be to modify experimental protocols to try to prove or disprove the mundane explanation. Instead, the cold fusion researchers resort to name-calling and wishful thinking.

"But the real pathology is the breakdown of the normal channels of scientific communication, with no scientists outside the tight-knit cold-fusion tribe bothering to scrutinize its claims."

As noted above, the breakdown occurs primarily on the cold fusioneers' side. I have presented a viable alternative, which they simply ignore. That is pathological.

Unfortunately your article was somewhat one-sided. Clearly you had only talked to the cold fusioneers, and 'surprisingly' they failed to mention my work. Hopefully this short note will give you some pointers on where to get the opposing viewpoint you need for balance.

Sincerely,

Kirk L. Shanahan

thermoacta.pdf

SRO-2016-00952-F
Responsive Document 013

{In Archive} Re: You Web Page
Kirk Shanahan Ludwik Kowalski 12/01/2003 02:26 PM

OK, I've forwarded a couple of old messages.

I also note that I dropped the 'r' in 'Your' in the Subject.

I have also commented extensively on Iwamura's, Miley's, Arata's, Oriani's, etc. work on the Usenet newsgroup sci.physics.fusion, which I see was basically absent in your Web pages also. You might look into spf.

Ludwik Kowalski <kowalskil@mail.montclair.edu>

Ludwik Kowalski <kowalskil@mail.montclair.edu>
To: kirk.shanahan@srs.gov
cc:
Subject: Re: You Web Page

12/01/2003 02:23 PM

I do not recall your paper. Perhaps it was
never received, or was deleted by accident.
Please send it to me again.
Ludwik Kowalski

On Monday, December 1, 2003, at 02:02 PM, kirk.shanahan@srs.gov wrote:

>
> Dr. Kowalski,
>
> In looking over your web page(s) on cold fusion, I find no reference
> to my explanation of the effect. Why is that when I have sent you the
> paper? Did I miss it?
>
> Kirk Shanahan

SRO-2016-00952-F
Responsive Document 014

Kirk Shanahan/WSRC/Srs
03/11/2005 08:09 AM

To "Vanek, Thomas" <Thomas.Vanek@science.doe.gov>
cc
bcc
Subject Fw: NEW ENERGY TIMES (tm) 10 March 2005 -- Issue #9

FYI - Some outfall from the cold fusion review. Several derogatory articles on the review included in the newsletter below.

----- Forwarded by Kirk Shanahan/WSRC/Srs on 03/11/2005 08:08 AM -----

Steven Krivit <steven@newenergytimes.co m>
03/10/2005 09:56 PM

To steven@newenergytimes.com
cc
Subject NEW ENERGY TIMES (tm) 10 March 2005 -- Issue #9

NEW ENERGY TIMES ™ 10 March 2005 -- Issue #9

Your best source for cold fusion news and information.

Table of Contents:

1. From the Editor: A Conversation About Peak Oil with Colin Campbell


Photo credit: www.peakoil.net

Geologist Colin Campbell has a simple way of explaining peak oil: "Understanding depletion is simple. Think of an Irish pub. The glass starts full and ends up empty. There are only so many more drinks to closing time. Its the same with oil."

After 40 years of working as an exploration geologist for Texaco and Amoco, Campbell is in a unique position to assess and analyze the precarious situation of a civilization built and powered by the assumption of perpetual cheap oil.

Campbell is the founder of the Association for the Study of Peak Oil. ASPO is one of the leading international groups of researchers collecting and sharing data and views on the implications of the forthcoming depletion of oil and natural gas. The Web site is http://www.peakoil.net/. The organization is directed by Kjell Aleklett.

The two questions I posed to Campbell were, "What is your objective in bringing peak oil awareness to the public?" and "Why bother to host an international conference on oil and gas depletion? (http://www.peakoil.net/iwood2005/iwood2005.html.)" I think it is a pointless way to spend one's time because, personally, I understand Peak Oil, and I accept it as fact. However, in speaking with Campbell, I realized that not everybody spends time looking at energy research all day and that some people dismiss the matter.

Everyone in the civilized world needs to understand that tough times may be imminent as oil becomes less abundant. But I'm a practical guy; I like to know what the solutions are, and I want to get working on them. Right now, solar and wind don't seem to offer practical solutions for the masses, and cold fusion research is moving at an excruciatingly slow pace. The type of nuclear power that everybody loves to hate, fission, seems to be the only tenable solution not only for global warming but also for oil depletion. Acceptable, that is, if you don't worry about the hot waste. We can bury that - umm, somewhere. Hey, I even read some brilliant ways to continue using coal without contributing to global warming: Bury the carbon dioxide underground!

An impressive number of largely unchallenged facts indicate that our favorite nonrenewable energy drink is soon to become backordered. I wanted to know, What's the point in telling people about Peak Oil when they can do nothing about it, and who would care?

Campbell reminded me that the topic is still largely unknown in most of the world. Here in the United States, I told him, it doesn't seem that the mainstream press has picked up on the story. One can go to overseas media, even in the oil-rich Middle East, and learn about Peak Oil. (Al Jazeerah article.) But one won't find much talk about it here in the United States.

Campbell talked about the historical relationship between major technological changes and their effects on society. The use of coal, for example, brought on the Industrial Revolution. The use of oil, starting in the 1850s brought on another wave of technological progress and prosperity. And all along, the financial foundation of modern societies was built on the premise and assumption of cheap energy. And this dependency on "dirt-cheap" energy is poised to be more destabilizing than the depletion of the energy itself, Campbell said.

I also spoke with Julian Darley, founder of the Post Carbon Institute (http://www.postcarbon.org ,) to ask him what he proposed as a solution to Peak Oil.

Darley demonstrates an insightful and creative approach to urban planning, and advocates accepting the inevitability of high-priced energy. He suggests that people, city planners in particular, design ways around the problem. Reduce consumption - drastically. Reduce the need to drive. Sell the homes in the suburbs and rebuild and retrofit urban environments so they can be more self-sustaining. He certainly has a point. If no cheap alternatives to gasoline appear, what else is there to do? Darley was among 10 experts interviewed in a disturbing as well as entertaining Peak Oil awareness movie, "The End of Suburbia" (http://www.endofsuburbia.com/).

Not everybody agrees with Campbell, Darley and the Peak Oil crowd. Charles H. Featherstone, a Washington, D.C.-based journalist specializing in energy, wrote an article titled "The Myth of "Peak Oil" on Jan.12 (http://www.mises.org/fullstory.aspx?control=1717&id=76).

As the title implies, Featherstone expresses grave doubts that there is anything to worry about. He provides many facts about the price of oil, as well as facts about oil suppliers and producers. I waded through the article eager to see how he was going to argue against the data presented by geologists such as Hubbert and Campbell. But 5,800 words later, not a single challenge to the facts of oil and gas depletion had happened.

And why is Featherstone so smug? He states that the real question we should be asking is, How can we best use the petroleum we have until other economically viable alternatives present themselves? He has faith that "whatever ends up replacing petroleum will come in its own good time." I can't help but wonder what Featherstone has in mind. From what I hear, most governments are hoping to sway the public to build more nuclear fission plants. Beyond that, they are putting their best bets on hot fusion. Hot fusion concerns me. People used to joke that "hot fusion is 20 years away and always will be." Now, hot fusion seems to be 45 years away. Not a good sign. Perhaps Featherstone has his hopes on cold fusion. Perhaps he is a "true believer," like me.

Campbell's words have sent money managers and financial advisers scrambling for safety. "The second half of the Age of Oil now dawns, to be marked by the decline of oil and all that depends on it," he said. "This realization undermines the foundations of the current financial system, which assumes that tomorrow's expansion provides collateral for today's debt. If expansion cannot happen while the oil production which drives it declines, that it implies that equivalent amounts of 'capital' will have to be removed from the system. Sounds like the Second Great Depression."

2. To the Editor

Even though I cognitively know the evidence for lenr-canr is probative--that there is a reproducible *phenomenon*--the thing that still makes me gag is this: if nuclear reactions are so easy to cause at temperatures, pressures, and electrical currents like those we see and use every day, then how can there be any life at all? How can there be any stable objects at all? Why hasn't everything long since just blown up?

Obviously it hasn't. All I have to do to plunge into deepest skepticism is a very simple physics experiment: Just look round. Matter is stable--very, very stable. This militates against lenr-canr with utmost force. How do we reconcile this?

Tatiana Covington
Tuscon, Arizona, USA

Thanks to Tatiana for a most insightful question. We've asked some of the cold fusion researchers to provide an answer, and we present two of the responses below.

Dear Tatiana,

This curious question is even more interesting for me because it is the same I received in 1986 (from one prominent late Russian scientist) during defence of my Ph.D. thesis in Moscow. My thesis was on experimental detection of DD-reaction (neutron emission) during fracture of deuterated crystals (LiD and heavy ice).

The answer is simple: Time. Let us assume that for some reason the cold fusion DD-reaction can "easily" occur with all deuterium in the universe.

1. The mass of universe on reasonable estimate is M ~ 10^54g. Suppose that hydrogen is 80 percent of its mass and deuterium is 10^-4 fraction of the hydrogen, the mass of deuterium roughly would be M(D) ~ 10^50 g. The mass of deuteron is m(d) ~ 3.4x10-24g, or the number of deuterons in our universe would be: N(d) = M(D)/m(d) = 3x10^73.

2. On the other hand, let us assume that the "cold fusion" in the universe is determined by D+D -> He-4 + 24 MeV reaction (with maximal energy yield). Comparison with PdDx He-4 yield (for instance, McKubre experiment) gives the yield of DD - reaction in Pd of Y(dd) ~ 10^-11/s per deuteron pair.

3. Again, for some unknown reason, we assume that the rate of DD-reaction in universe's deuterium is the same as in PdDx. Then the time during which all deuterium in universe will be "burned" in cold fusion reaction would be t = N(d)/Y(dd)~ 10^84 s. Notice that the time of universe existence (~ 14 billion years) is only ~ 4.4x10^17 s. So it means that during next 10^67 sec or 10^60 years we may have cold fusion.

This number will not be significantly affected by more reasonable suggestion the CF is occurred only in solid part of universe, say only on the our Earth (M = 6x10^27 g). In that case, the deuterium could be burned for ~ 10^40 sec, the time that is much larger than time of universe.

If we consider such a huge time of deuterium burning in cold fusion, then things during our life and during life of the Earth (~ 5 billion years) look pretty stable!

Regards,
Andrei Lipson
University of Illinois, Urbana
Russian Academy of Science

Dear Tatiana,

My reply is as follows:

(1) LENR is a kind of resonance phenomenon;
(2) It is very difficult to keep this resonance in a steady state.
(3) We are looking for the mechanism which may keep this resonance state in a self-sustaining way, but it has not been successful yet. Possibly, a deuterium flux through Pd film is a method to maintain this resonance.

(4) Fission was discovered in 1939, the first fission reactor was in operation in 1942 with the support from the whole nation in the war. LENR was announced in 1989, but we are still in the stage of confirmation of this phenomenon because the lack of financial support.

Best regards,
Xing Zhong Li
Tsinghua University, Beijing, China

3. Notable Quotables

Jed Rothwell on the debate about the reality of cold fusion:

"If several hundred researchers could all make large mistakes using 100- and 200-year-old techniques, science would never work in the first place. That is like asserting that you can select 200 carpenters at random, have each of them build a wooden house, and when they finish, every single house might collapse because of mistakes the carpenters made. That would not happen in the lifetime of the universe. Of course, newly built houses do collapse from time to time. Individual carpenters do make drastic mistakes, and so do individual electrochemists. But they are never all mistaken."

And Francesco Celani of Italy's National Institute for Nuclear Physics said, "In Italy we have a saying: 'Nobody is so blind as people that don't want to see.' Remember the Galileo Galiei-Cardinal Bellarmino 'discussions.'"

4. Department of Energy Dumps Cold Fusion (Again)

"I think a review is a waste of time," said Princeton University physicist Will Happer regarding the 2004 cold fusion review. "But if you put together a credible committee, you can try to put the issue to bed for some time."

Perhaps Happer, a member of the original 1989 cold fusion panel and former head of Department of Energy's Office of Energy Research (now the Office of Science) had greater insight than the cold fusion "believers."

As a quick refresher, here are the conclusions of the 18 peer-reviewers selected by the Department of Energy's Office of Science last year:

A) Half of the reviewers found the evidence for excess power compelling.
B) Less than one-third of the reviewers believed that the evidence for low energy nuclear reactions was conclusive.

The bottom line, as stated by the Department of Energy, was that "the conclusions reached by the reviewers today are similar to those found in the 1989 review." The interpretation is that "nothing's new, the claims of cold fusion are still not believable, nor are they worthy of a dedicated research program."

Officially, the Department of Energy claims that it did not slam the door on cold fusion research.

"We have always been receptive to research proposals," Jim Decker, principal deputy director of the Department of Energy 's Office of Science, said. "We make decisions on funding research proposals on the basis of peer review and relevance."

A New Energy Times survey performed in late February indicates that U.S. cold fusion researchers fail to sense much sincerity from the Department of Energy. Only two researchers report plans to send cold fusion proposals to the Department of Energy. Alternatively, a few researchers indicate that they will submit proposals to the Department of Defense.

Perhaps the clearest indicator of the Department of Energy's true attitude towards cold fusion is seen in the response to Dr. Melvin Miles, a professor of chemistry with the University of La Verne, in southern California.

Miles is considered one of the pioneers of cold fusion research and was the first to identify the relationship between heat production and nuclear products. At the time, Miles was working at the U.S. Navy's China Lake research facility. A few years later, in another major achievement, he collaborated with Ashraf M. Imam, a metallurgist at the Naval Research Laboratory to develop and test a special palladium-boron alloy for use in cold fusion experiments. The alloy resulted in a series of cold fusion experiments that generated excess energy in eight of nine runs and a U.S. patent. The Patent and Trademark Office doesn't recognize the validity of cold fusion, so the application required careful wording.

"We didn't use the words 'cold fusion'; we just talked about producing heat," Miles said of his 18th U.S. patent.

Miles is a published author of 200 papers, 70 of them in the cold fusion field. A physical chemist, he has been recognized for his excellence in science by a 1966 NATO Postdoctoral Research Fellowship Award, and the following awards from his 24-year tenure with the China Lake Naval Weapons Center: Sigma Xi Award for the Best Scientific Paper in 1985 and 1988, William B. McLean Award in 1987, Fellow Award in 1989.

On Jan. 24, 2005, Miles submitted to Decker a pre-proposal to study cold fusion. The cold fusion field is quite broad; it includes experiments that produce excess energy but no neutrons, and it includes other branches that produce neutrons but no excess energy. The area of study pertaining to excess energy is, by far, the most controversial, as well as relevant to civilization's future energy needs.

James Horwitz of the Energy Department's Office of Science telephoned Miles on Feb. 17, 2005, with the following bad news:

1. Proposals for the optimization of cold fusion nuclear effects cannot be considered because the 18 Department of Energy panel members concluded that such nuclear effects do not exist.
2. Electrochemical cells have been studied to death, for example, by McKubre at SRI. Proposals of further electrochemical studies likely will not be funded by Department of Energy.
3. Any proposed new experiments need an acceptable theory to justify such further studies.
4. More peer-reviewed journal publications are needed before this field can be considered for funding.

"Because of these points, Jim Horwitz concluded that he cannot justify sending my proposal out for review," Miles commented. "I am really quite shocked at what Jim Horwitz said."

New Energy Times asked Horwitz for his side of the story. Horwitz made no corrections to Miles' report. Instead, he offered a one-page explanation of the procedures and criteria for proposals and explained how Miles' proposal fell outside of such criteria.

"The proposed work as stated by Professor Miles is *'to optimize the cold fusion excess power effects by going to higher temperature,* '" Horwitz wrote. "As this proposal is aimed at optimization and commercialization of the cold fusion process, I suggested that Professor Miles either restructure the proposed research towards the fundamental science or submit the white paper/formal proposal to one of the applied technology offices within the Department of Energy."

In Miles' proposal, the "Summary of Goals" states the following:

"1) Establish the experimental conditions for the production of both reproducible and large excess enthalpy effects.
2) Determine more accurately the correlation between the excess enthalpy and helium-4 as the nuclear product.
3) Investigate possible methods such as fluidized bed reactors and the use of higher temperatures for the commercialization of the excess energy production."

The proposal comprises four pages of text and 10 pages of Miles' prior references and publications.

Oddly, the quote allegedly by Horwitz does not appear in Miles' proposal. The word "optimize" does not even appear in Miles' proposal. The sentence does occur, however, in correspondence from Miles after the rejection by Horwitz and after the telephone call.

The alleged quote of Miles by Horwitz and the justification by Horwitz to reject the proposal based on such quote are mistakes by Horwitz at best and a botched cover-up at worst.

Horwitz's candid comments to Miles reveal a rare glimpse into the U.S. government's less-than-visionary behind-the-scenes attitude toward cold fusion.

Here is why:
Item 1 from the phone conversation between Horwitz and Miles is contradicted by the Department of Energy's own final report (http://newenergytimes.com/DOE/DOE-CF-Final-120104.pdf).
Item 2 shows that the Energy Department is still clueless about the heat-generating effect of cold fusion.
Item 3 runs counter to the fundamental principle of scientific discovery.
Item 4 displays an ignorance of the numerous publications that have appeared in 55 peer-reviewed publications worldwide.
(http://newenergytimes.com/reports/publishedpapers.htm)

Miles has his suspicions about Horwitz: "I think he's afraid to fund this area because of the criticism and flak he would get. They are afraid to fund the area so they try to find reasons why not to do it."

Horwitz's concluded his letter by stating, "I want to apologize to Professor Miles for any misunderstandings that were generated by my phone call." Perhaps Decker owes Miles an apology for a misunderstanding, too. In this month's Scientific American, he was quoted as saying, "We never said we would not fund proposals in cold fusion."

It looks like they just did.

Naturally, Miles is disappointed. "I've been wondering if it's time to retire and forget about cold fusion," he said. "Based on this response, there's no need to keep working on it. I don't see how I'm going to get any funding."

Miles has had a bumpy ride, achieving both successes and failures with cold fusion. The Horwitz response perhaps tops them off. Starting in 1989, Miles, while at China Lake, was unable to see any excess heat effects for the first few months. The Department of Energy had been conducting its first cold fusion review at the time and took notice of his negative findings.

"Apparently, the Department of Energy found my China Lake work to be sufficiently accurate in 1989 to include my results along with MIT and Caltech as evidence against cold fusion in their ERAB report," Miles remembers.

Only a few weeks after the ERAB panel deadline, Miles saw his first evidence of excess heat. He wrote to each one of the panel members, but apparently nobody cared. None replied.

In the mid-1990s, the Navy's Office of Naval Research, under the direction of Bob Nowak, funded a major cold fusion research program, and Miles was included on the team. During the tail end of that program, Miles and Imam developed and tested their palladium-boron alloy.

"They closed that program just at the time when we were starting to get good results," Miles said. "They had already made the decision to phase it out, so this just came at the wrong time. Politically, once they decide not to fund something, they don't like things turning up that will contradict their decision."

The going got rough for Miles after that. A change in administration at the Office of Naval Research resulted in Miles' receiving orders to report to the stock room.

"Richard Carlin, who took over Bob Nowak's job told people outright that he wouldn't fund me because my reputation was ruined because of my work in cold fusion," Miles said. "Even though I was the only electrochemist there, he funded people all around me at China Lake. He very seldom gave me any money, and if he did, he would fund it and then take it back. They were trying to get rid of people because China Lake was running in the red. So they wrote up a memorandum where I was supposed to report to the stockroom clerk who had a high-school education and help her with an inventory of the chemicals."

A way out of the stock room appeared for Miles in 1977 when the New Energy Development Organization, the equivalent of the Department of Energy in Japan, offered him a six-month job to perform cold fusion research at the New Hydrogen Energy laboratory in Sapporo.

"It was one of the best labs I had worked in," Miles said. He brought one of his palladium-boron alloys to Japan, used it to produce the excess heat effect, and taught the Japanese researchers at the lab how to perform calorimetry.

Will Miles consider foreign research jobs now?

"I'm not going to propose outside of the country," he said. "I would like to work in this area, but I don't see much hope."

Miles turned down a job offer from Tsinghua University in China, because he doesn't want to be that far from his family. The administration at the University of La Verne has been very supportive of his interests in performing cold fusion research.

"I could have been released from all teaching duties to work on cold fusion if my proposal had been funded," he said. "I believe that this was my last opportunity to get back to cold fusion research. Because of Jim Horwitz's comments, I see no chance for any cold fusion funding and will now likely retire."

Miles and his wife, Linda Miles, enjoy visiting their cabin in Oregon.

"It's like a national forest up there with Douglas firs and ponderosas and a lot of wildlife. I'm planning on bringing up all my cold fusion papers and writing a book," he said.

5. The Department of Energy Lies Again

Myth-busters Jed Rothwell and Ed Storms are at it again. They have analyzed the Horwitz fiasco

and provided in-depth facts and references. The Department of Energy promised to evaluate cold fusion claims fairly, twice. Both times, when given a chance to keep the promise, the Department of Energy failed. The Web address is:
http://www.lenr-canr.org/acrobat/LENRCANRthedoelies.pdf

6. Ed Storms Continues the Dialogue with the U.S. Department of Energy Reviewers

Ed Storms, of Lattice Energy, LLC, USA, has written a detailed response to the 18 reviewers who participated in the 2004 DOE Cold Fusion Review. The reviewers' comments (_ http://newenergytimes.com/DOE/2004-DOE-ReviewerComments.pdf) were helpful in pinpointing the current areas of weakness in cold fusion research. They were equally effective in displaying some of the continued misunderstandings. Storms has contributed an excellent addition to the debate on this most controversial science anomaly. The link to his paper is http://lenr-canr.org/acrobat/StormsEaresponset.pdf .

7. Open letter to the U.S. Department of Energy and its Team of 18 Scientists

Ludwik Kowalski, a physicist recently retired from Montclair State University, New Jersey, U.S., also had a few things to say to the Department of Energy-selected reviewers. Kudos to Kowalski for calling on his fellow scientists to attend to the foundation of the scientific method: experimental facts. His blog is at http://blake.montclair.edu/~kowalskil/cf/196open.html .

8. Great, Not-So-Great, and Realistic Expectations from the Department of Energy Re-Review

An insightful review and analysis by Scott R. Chubb:

http://www.infinite-energy.com/iemagazine/issue59/greatnotsogreat.html

9. Cold Fusion Explosion and Accident Report


Radial fracture pattern of the bottom of the flask after explosion. Photo by Mizuno.

On January 24, 2005, at around 4:00 p.m., an explosion rocked a cold fusion laboratory at Hokkaido University, Japan. The experimental design was the plasma electrolysis method, one of several methods used to perform cold fusion experiments. Physicist Tadahiko Mizuno, one of Japan's most experienced cold fusion scientists and a guest of his were in the laboratory at the time of the explosion.

Mizuno and the guest suffered wounds to the face, neck, arms and chest from shards of glass. A large piece of glass next to Mizuno's carotid artery was safely removed.

"I feel fortunate that neither of my eyes were seriously wounded and that neither I, nor my guest were seriously wounded," he said.

However, the explosion was so loud that it rendered both victims temporarily deaf. A week following the accident, their hearing recovered, though Mizuno said that the "singing in the ear continues strongly."

A definitive explanation is unknown, though Mizuno suspects that a mixture of hydrogen and oxygen in the headspace of the cell was ignited. Mizuno has performed these experiments hundreds of times, and this apparatus had been well-tested over the last five years.

Before the experiment, Mizuno had checked all of his equipment and had made sure that the exhaust tube was clear.

"The outlet tube leading to the mass spectrometer was definitely not blocked or impeded, so the gas in the headspace was at one atmosphere," he reported.

A high-pressure build-up of hydrogen and oxygen has been ruled out.

At the time of the explosion, a collector that would normally have aided in the collection and removal of the effluent gasses was removed, though this was not unique.

"The funnel around the cathode was taken off for the analysis of the generation gas during plasma electrolysis," Mizuno said. "I have performed such measurements 40 times in the past and confirmed the safety of this procedure many times."

Mizuno turned the experiment on when he arrived in the laboratory that afternoon. It had not been on long enough to develop the plasma, which usually takes about 20 minutes. About 5 seconds later, when he observed that electrolysis started, he increased the voltage to 20 volts and the current to 1.5 amps. About five or six seconds later, Mizuno reported seeing a bright white flash of light from the submerged portion of the cathode, where the plasma normally would develop.

The light "expanded, and at the same instant the cell exploded," Mizuno said. The safety doors to the incubator were blown open, and glass and electrolyte were blown up to 6 meters from the experiment platform.

Mizuno documented the event in his accident report (http://newenergytimes.com/news/2005mtexplosion/Report.pdf). He listed several possible causes, though he was tentative about any of the prosaic explanations.

Chemist Dieter Britz from the University of Aarhus was curious about how such a small amount (3cc) of hydrogen gas might have caused such a large explosion in the cell.

"It is also hard to imagine that there should have been enough for such a violent explosion," Britz said. "You have no doubt seen the school experiment, where a lighted taper is inserted into a tube with some hydrogen in it. You get a nice 'pop.' In an open cell, [such as this] after a short time of electrolysis, that is what I would expect. So this is very strange, and I have no guesses."

The explosion was perhaps similar to the one on Jan. 2, 1992, that killed SRI International researcher Andy Riley, though the SRI cell was closed and under high pressure. Mike McKubre, the director of the energy research center at SRI, who was wounded in the 1992 explosion, as well, cautioned that any exposed metal can cause a recombination explosion.

"I found it is impossible to impress on people just how explosive a stoichiometric mix of hydrogen and oxygen is, McKubre said. "Even a few cc's can be dangerous, even deadly. You don't need to search for an ignition source. Any metal will do."

The only other well-known cold fusion explosion was that of Martin Fleischmann and Stanley Pons in 1985, though a source who wishes to remain anonymous states that the Lawrence Livermore National Laboratory had a Fleischmann-Pons-type explosion in 1989, as well.

Mike Carrell, a previous board member for Infinite Energy magazine, postulates a two-stage reaction in the Mizuno explosion.

"First there is a spark or flash, then an expanding glow, then an explosion," Carrell said. "When the disturbance reaches the surface, the stoichiometric H2-O2 mixture may well have ignited, contributing to the explosion."

Horace Heffner, a cold fusion enthusiast, offered this analysis. "It appears that the explosion may well have been ignited in the flask, but the main energy from the explosion came from the top interior of the Yamato 1L-6 incubator. It looks like the explosive force was primarily downward, and the overpressure on the conical cap on the flask blew the flask apart in radial directions, leaving the base cracked but in place. It looks like the base of the flask may be stuck (by prior heating) to the polypropylene insulation underneath it.

"Assuming the plastic door was not blown to pieces, the overpressure was clearly enough to blow open the plastic door before the glass shards went through the open door. This indicates the overpressure hit the door before the flask pieces. The source of the blast pressure that opened the plastic door was therefore not inside the flask but rather probably coming from the top of the 1L-6 downward."

Heffner speculated that hydrogen from the reaction flask is dumped into the interior of the 1L-6 where it can accumulate in various spaces and thus be exploded by an ignition event in the flask.

The big question on everyone's minds is whether this was a chemical explosion - or a nuclear explosion. A physicist who considered the amount of energy required to convey the 800cc of electrolyte a distance of up to 6 meters, was unconvinced that this was a chemical reaction.

Jed Rothwell, who translated Mizuno's book *Nuclear Transmutation: The Reality of Cold Fusion* to English, assisted with this story and reports that Mizuno is back at work starting the experiments again, despite the trauma.

"Mizuno has guts," Rothwell said. "All cold fusion researchers have guts. They are an ornery bunch, but you have to admire them."

Photographs taken by Mizuno and others are here:
http://newenergytimes.com/news/2005mtexplosion/explosion.htm

10. Mizuno Paper Published

Despite the recent interruptions to his research, Mizuno has recently succeeded in getting the paper, "Hydrogen Evolution by Plasma Electrolysis in Aqueous Solution" published in the Japanese Journal of Applied Physics (Vol. 44, No. 1A, 2005, pp.396-401, http://jjap.ipap.jp/link?JJAP/44/396).

His co-authors include Tadashi Akimoto, Kazuhisa Azumi, Tadayoshi Ohmori, Yoshiaki Aoki also from Hokkaido University, as well as Akito Takahashi from Osaka University.

The text of the abstract follows:

Hydrogen has recently attracted attention as a possible solution to environmental and energy problems. However, hydrogen should be considered an energy storage medium rather than a natural resource. Free hydrogen does not exist on earth.

Many techniques for obtaining hydrogen have been proposed. It can be reformulated from conventional hydrocarbon fuels, or obtained directly from water by electrolysis or high-temperature pyrolysis with a heat source such as a nuclear reactor. However, the efficiencies of these methods are low. The direct heating of water to sufficiently high temperatures for sustaining pyrolysis is very difficult. Pyrolysis occurs when the temperature exceeds 4,000°C. Thus, plasma electrolysis may be a better alternative. It is not only easier to achieve than direct heating, but it also appears to produce more hydrogen than ordinary electrolysis, as predicted by Faraday's laws, which is indirect evidence that it produces very high temperatures.

We also observed large amounts of free oxygen generated at the cathode, which is further evidence of direct decomposition rather than electrolytic decomposition. To achieve the continuous generation of hydrogen with efficiencies exceeding Faraday efficiency. it is necessary to control the surface conditions of the electrode, plasma electrolysis temperature, current density and input voltage. The minimum input voltage required to induce the plasma state depends on the density and temperature of the solution. It was estimated as 120 V in this study. The lowest electrolyte temperature at which plasma forms is 75°C. We have observed as much as 80 times more hydrogen generated by plasma electrolysis than by conventional electrolysis at 300 V.

11. ChangChun University, China, Takes Up Cold Fusion


Front row, left to right: Jian Tian, John Dash, Xing Zhong Li

With the assistance of professor Xing Zhong Li of Tsinghua University, Beijing, China, and professor John Dash of Portland State University, a new cold fusion research effort has begun at ChangChun University, ChangChun City, China.

Dr. Jian Tian, dean of the school of biological sciences, directs the effort and oversees the work of eight undergraduate students working on cold fusion research. Tian has a background in material science.

Tian invited Dash to ChangChun University in October of 2004 for a week to train students in his cold fusion recipe. Dash had recently trained high-school students at the Leonardo da Vinci scientific high school in Milan, Italy, on his simple but effective cold fusion demonstration.

By the end of the week, the students had performed two successful cold fusion experiments.

"The students stayed up all night preparing their graphs, and on Friday morning I walked into a packed lecture hall with a banner welcoming me," Dash said.

The president and vice president of ChangChun University attended and were enthusiastic about the work, Dash reported. They asked him to suggest a reasonable amount of funds which would support professor Tian's group of cold fusion researchers. Xing Zhong Li later reported that Dash's suggestion had been approved: "The Vice-President in charge of research and foreign affairs promised the equivalent of US$100,000."

12. Italian Physical Society Publishes Cold Fusion Nano-Particle Paper

The Italian Physical Society published a paper by Yoshiaki Arata on the subject of nano-particles and cold fusion in December 2004. The paper is in English, and it begins on Page 6 of the PDF file at http://newenergytimes.com/Library/2004Arata-FormationOfSolidDeuterium.pdf .

Here is an interesting viewpoint from Arata, excerpted from the conclusion of his paper:

"I am amazed and impressed by this mechanism of 'nature' as much as I respect it. Simultaneously, only proper experiments enable us to comprehend its mechanism. Furthermore, we should not forget our current understanding of science is based on previous excellent experiments done by the earlier generations. As seen in recent discoveries of new materials one after the other, our knowledge is confined to comprehend only some parts of the mechanisms of nature. Hence one should not repeat such foolishness as denying 'heliocentricism' as was done in the past, which resulted from adhering too strongly to one's own knowledge or to what was common sense in those days. For myself, I always warn myself with a voice not to be too much possessed by my own current knowledge."

13. 6th International Workshop on Anomalies in Hydrogen / Deuterium Loaded Metals (Italy)

Workshop organizer William Collis announced the sixth in the series of Italian cold fusion conferences to be held on 13-16 May, 2005, in Siena, Italy. The workshop is a program of the International Society for Condensed Matter Nuclear Science (http://www.iscmns.org). Co-sponsors of the workshop are professor Francesco Piantelli of the University of Siena. Tiziano Ghidini, Chief Executive Officer of Ecodep srl, LumEnergia and the Monte dei Paschi di Siena Foundation.

The conference takes places in Certosa di Pontignano, a 14th-century monastery converted exclusively for conferences by the University of Siena. Further details on the workshop can be found at http://www.iscmns.org/siena05/siena.htm .

14. American Physical Society March Meeting

Monday-Friday, March 21-25, 2005; Los Angeles, Calif.
Session U33: Cold Fusion
Thursday, March 24, 2005
Room 511C, Los Angeles Convention Center
Abstracts are located at http://newenergytimes.com/Conf/APS2005/2005.htm .
Presentations will include work from the following (primary) authors: Szpak, Miley, Cravens, Apicella, Swartz, Hagelstein, T. Chubb, George, Miles, Stringham, Krivit, Storms, S. Chubb

15. Why Is Everybody Waiting For America When It Comes To Research?

A conversation with Martin Fleischmann, discoverer of cold fusion, by Haiko Lietz

New Energy Times is pleased to present the most current interview with Martin Fleischmann. In this sensitive and insightful conversation with journalist Haiko Lietz, Fleischmann reflects on society, science, and his personal struggles in life.

"I think that we have to acknowledge that our society has become orientated towards consumption rather than production. And a society that becomes orientated towards consumption abandons scientific investigation. There are plenty of historical precedents for this phenomenon. And in the end, what has happened in the past is that societies which abandon the pursuit of science die. Our society will not necessarily die, but it will become unimportant."

The English version is at http://newenergytimes.com/Conversations/FleischmannByLietz.htm.

The German version is at http://www.heise.de/tp/r4/artikel/19/19257/1.html .

16. "Second Chance for Cold Fusion," by Haiko Lietz

On Dec. 16, 2005, Haiko Lietz reported on German National Radio about the result of the 2004 Department of Energy cold fusion review, quoting David Nagel (The George Washington University) and James Decker (Department of Energy). Unfortunately, a biased editor changed the script so the online version (http://www.dradio.de/dlf/sendungen/forschak/331039/) contains errors. Only the original report, which is available on demand (_ http://ondemand-mp3.dradio.de/file/dradio/2004/12/16/dlf_1635.mp3), contains the author's words.

17. "Cold Fusion Acceptable For Scientists to Discuss, but Not Media," by Sam Smith

Reprinted by permission of Progressive Review (http://prorev.com/), (Jan. 23, 2005)

At the March meeting of the American Physical Society, 14 papers will be delivered in a session on cold fusion. This isn't the first time for such a session, and cold fusion also has been considered a respectable subject at the American Chemical Society.

Cold fusion advocate Ed Wall reported, "They have been presenting at APS for a number of years, as well as the American Chemical Society. They generally do not generate much of a turnout, but because the scientists doing the CF research are in good standing in such organizations, the methods employed are standard stuff and the quality of the work they do appears to be good, they were able to argue (Scott Chubb, most persuasively) that they should be allowed to present their work."

There is one place, however, where cold fusion is not permitted to be discussed or debated: the American press.

Wall said, "Once CF started getting treated as a serious science, not just by a strong-willed minority of appropriately credentialed scientists but by scientific and engineering establishments around the world (Japan), it appeared as more than bizarre that it was still considered heresy in the United States."

Cold fusion is far from the first new scientific idea to get the cold shoulder from scientists, the establishment and the media. Galileo's problems are well-known, but in a Nobel Laureate's talk last June titled "Pathological Disbelief," Brian D. Josephson, a physicist from the University of Cambridge Lecture, gave some other examples:

METEORITES: The issue is whether meteorites have an extra-terrestrial origin. The arguments in favor were visual sightings, stones found at sites of apparent landings, which were often warm. Incorrect arguments against were that objects falling from space contradicted the laws of mechanics. The alternative explanations offered were the presence of an optical illusion or that a stone was struck by lightning. The cause of capitulation occurred when a massive meteorite fell near Paris.

CONTINENTAL DRIFT: The arguments in favor (Wegener, from 1912) were the fit of the South American and African coastlines (Bacon 1620), matching fossils, rocks and coal found in the Antarctic. The argument against was simply: The claimed phenomenon is impossible. The cause of eventual capitulation occurred when other geological observations led to theory of plate tectonics.

Josephson brings in the subject of cold fusion as an example of a current scientific idea that is getting its share of pathological skepticism.

In his talk, he quoted Charles G. Beaudette as offering the following six characteristics of scientific skeptics:

1. They do not express their criticism in those venues where it will be subject to peer review.
2. They do not go into the laboratory and practice the experiment along with the practitioner.
3. They offer assertions are offered as though they were scientifically based when, in fact, they are mere guesses.
4. They employ satire, dismissal and slander.
5. When explanations are advanced, they advance reasons to reject them. These reasons often

assert offhand that the explanation violates some conservation law.
6. They reject evidence outright if it does not answer every possible question at the outset.

The problem with the media is even greater because they go to the established scientific profession rather than the ground-breakers for confirmation.

Most of what editors know about science they learned in high school. I was attracted to the cold fusion issue because of political, rather than scientific, factors. After the initial Fleischmann-Pons-Hawkins experiments had proven faulty, a number of anomalies developed. Some of the media seemed to go out of their way to beat a presumed dead horse, and a couple of anti-cold fusion books even appeared. The Department of Energy initially made it clear publicly that it wanted nothing to do with the matter (although it has backtracked a bit). The Patent Office refused to consider it.

Meanwhile, in other countries, research continued, sometimes - as in Japan - with public monies, and some hardy American scientists kept plugging away, all gathering at international conferences notable for media absence. Even Toyota put money into the research, although the Japanese have since slashed their funding.

In some eastern nations, cold fusion researchers were less of a target for hostility than their counterparts in the United States. As one investigator put it, "In the United States there is a degree of envy among cold fusion researchers for their Japanese colleagues. In Japan, the debate over cold fusion is polite and scientific. Researchers are not rashly judged or branded incompetent for suggesting cold fusion could be real. Their American counterparts would like to conduct research in a similar atmosphere, without accusations and emotionalism."

The potential import of cold fusion, should it prove valid, along with the economic interests involved - including those involved in conventional energy or getting government money for other alternatives - raised the suspicion that some of the opposition might not be scientific. The hostility seemed to go beyond skepticism and veered toward political or public relations campaigning.

The Progressive Review, in its role as an underground railroad for the new, the imaginative, and the abused, has remained hospitable to the cold fusionists without offering the slightest guarantee that they are right. They simply deserve to have been treated a lot better than they have been.

18. ArchiveFreedom.org Founded to Fight Scientific Censorship

ArchiveFreedom (http://www.archivefreedom.org/) was founded in 2004 by Paul LaViolette after his papers were censored and blocked from ArXiv.org, an online repository of physics research.

LaViolette sensed injustice and took action.

"Through a Freedom of Information Act request, I got the names of a few others who had filed suit against ArXiv.org over this issue," he said.

He also found out that the U.S. National Science Foundation filed a suit against the Cornell-based ArXiv.org, which funds the program.

ArXiv.org is an electronic preprint archive. It was founded in 1991 at Los Alamos National Laboratories and funded by the National Science Foundation. It was formed as a way for scientists to disseminate new discoveries and theoretical developments rapidly to the worldwide scientific community. The intent was an open forum for papers written by credentialed physicists, that is, those who consistently had papers approved for publication in peer-refereed journals. Over time, the criteria for approval of submitted papers to the archive became more complicated and restrictive.

LaViolette made contact with others who had been censored from the ArXiv.org site and joined forces with Tony Smith and Carlos Castro.

"Carlos has been very helpful with the ArchiveFreedom.org Web site and has also been instrumental in expanding the size of our group to over a dozen people who have been blacklisted by ArXiv.org," LaViolette said. "The group continues to grow as members of the physics community who have been discriminated against learn about us."

Other support has come from Victor Xianto and Nobel Laureate Brian Josephson, who suddenly found himself censored and blocked one day by ArXiv.org director Paul Ginsparg of Cornell University.

"The ArXiv.org administrators maintain a list of physicists whom they have blacklisted or ostracized so that any paper those individuals attempt to submit is systematically rejected regardless of its scientific content," LaViolette said.

Usually, these blocked papers have already been accepted for publication in reputable peer-refereed science journals or are under review.

New Energy Times attempted to obtain Ginsparg's point of view. His office voice-mail offered the following greeting, "Hi, this is Paul Ginsparg. If you're calling to offer money or other resources, please leave a message. Otherwise, send an e-mail." Since we didn't have any spare cash at the moment, we resorted to sending the e-mail, but days later there was no reply.

The founders of ArchiveFreedom.org hope to bring about a change in this suppression by alerting other physicists and the public to this blatant discrimination. ArchiveFreedom.org also relates the case histories of those scientists who have been censored and/or blacklisted.

"This is a rebellion, and we feel we can win," LaViolette said on behalf of the group.

19. Murder Investigation of Eugene Mallove

Two new press releases are posted at www.eugenemallove.org along with links to recent articles:

http://www.eugenemallove.org/family_and_friends_release.html
http://www.eugenemallove.org/police_release_0128.html

The Norwich Bulletin also ran a recent article:
http://www.norwichbulletin.com/apps/pbcs.dll/article?AID=/20050203/NEWS01/502030311/1002

20. Department of Shameless Self-Promotion: Cold Fusion Book Review

Review of *The Rebirth of Cold Fusion: Real Science, Real Hope, Real Energy*
Reviewed by Scott R. Chubb
Infinite Energy Magazine, Issue 59 (http://www.infinite-energy.com/)

The Rebirth of Cold Fusion: Real Science, Real Hope, and Real Energy, by Steven B. Krivit and Nadine Winocur, should be required reading for anyone interested in cold fusion and LENR. Not only is this book technically sound, but it is so well-written that experts, novices, and newcomers to the field all will enjoy reading it. Remarkably, the book not only covers virtually all of the most important technical details of LENR but also includes an important record of the politics and history of the field and the potential impact of the associated discoveries on world development.

The book is also remarkably timely: To their credit, because Krivit and Winocur published their book immediately after ICCF11 and just before the much-anticipated re-evaluation of cold fusion by the Department of Energy, they are providing accurate information about an evolving, new, important area of science that has been seriously misrepresented, at a time when candor is absolutely necessary. For this reason, the book itself might help to foster the Rebirth of Cold Fusionby advancing the process of disseminating accurate information about the field. Thus, the book could be remembered not only because it is well-written and accurate but also because its publication could alter the history of the associated debate.

All books, of course, reflect particular biases and trends that are in vogue at the time that they are published. An important difference between *The Rebirth of Cold Fusion* and the earlier books that have presented a positively biasedaccount of cold fusion is associated with developments in the field. In particular, as opposed to the apparent confusion in the field that prompted Gene Mallove to use the phrase Searching for the Truth Behind the Cold Fusion Furoras a subtitle to his 1991 book *Fire from Ice* , or the decision by Charles Beaudette to identify a single effect (Excess Heat) in the title of his book (in 2000) as the key phenomenon in cold fusion research, Krivit and Winocur have written their book at a later time, when the relevant science is now known to be real. As a consequence, their book documents the birth of a new field as opposed to depicting fragments of the relevant story.

An additional important difference is that Krivit and Winocur became involved with cold fusion more than a decade after the initial debate began. Thus, their book resonates with optimism and hope, and their perspective, both figuratively and in fact, reflects an idealism that has been lost by many of us who have been involved with the controversy since the beginning.

I thoroughly enjoyed this book. I give it my highest recommendation. In writing it, Krivit and Winocur have done a tremendous service not only to the cold fusion field, but also to science as a whole.

21. Cold Fusion in the News

A Brief Review of the Science and Events at the 11th International Conference on Cold Fusion (ICCF11) by Scott R. Chubb
http://www.infinite-energy.com/iemagazine/issue59/reviewoficcf11.html

21st Century Science & Technology, "Cold Fusion: The Experimental Evidence," by Ed Storms (Winter 2004-2005)
http://www.21stcenturysciencetech.com/current.html
A guide for both general readers and specialists to the thousands of experiments that establish the overwhelming evidence for cold fusion and suggestions for crucial directions in future research.

Il Sole 24 Ore, "From Japan, a breakthrough for the neutralization of nuclear ashes. A new research project might involve Italy," (Jan. 22, 2005)
http://www.ilsole24ore.com/
[Editor's note: The following newspaper article stirred up a heated debate among researchers in Italy as well as those in Japan.

Anger and tensions grew internationally as 70 scientists on an e-mail discussion listed witnessed a lively exchange between Camillo Franchini, the retired head of the Chemistry Department at an Italian military laboratory, and physicists Akito Takahashi of Osaka University and Yasuhiro Iwamura of Mitsubishi Heavy Industries. The discussion broke down when the conversation degraded from scientific debate to personal attacks.

A frustrated member of the list remarked, "How dare you fill my Inbox with 0.5M of files! Remove me from the list! You people generate more heat than light."

Fortunately, after intervention by several observers to the exchange, the personal attacks subsided, and the "flame war" was extinguished. An agreement to challenge the scientific facts was made among all parties, and a proper debate will resume at the forthcoming Siena

Workshop. (http://www.iscmns.org/siena05/siena.htm)]

(English translation by Misa Celani)

"Milan, Jan. 22, 2005 The dream of transforming or, more accurately, transmuting radioactive nuclear ashes into harmless residues might come true. This could be achieved by repeating, with radioactive substances, the successful experiment recently performed by Mitsubishi Heavy Industries (1.) The Japanese experiment showed that, in a properly assembled gas cell, natural cesium and strontium can be transmuted into different chemical elements, specifically into praseodymium (a rare earth element) and molybdenum, with a negligible energy input. This outcome is capable of opening novel prospects for the future scene of world energy availability. The radioactive isotopes of cesium and strontium are the most abundant and harmful components of the nuclear ashes: Their transmutation into nonradioactive isotopes might at last remove the main obstacle hindering the extensive and long-term exploitation of the nuclear energy.(2)

In response to the successful Japanese research, a complex Italy-Japan research project is about to be presented to the Italian government. The aim of the project is to assess the industrial feasibility of the nuclear waste remediation. The government will be asked for a contribution of 16 millions euros along a five-year period. The overall project, amounting to 25 million Euros, 13 for the first two years and 11 for the following two years (3), should involve, from Japan, the team of Mitsubishi Heavy Industries and that of Akito Takahashi, president of the International Society of Condensed Matter Nuclear Science. and for Italy the Istituto Nazionale Fisica Nucleare Frascati team directed by Francesco Celani, STMicroelectronics, Cornaredo Labs directed by Ubaldo Mastromatteo, Centro Sviluppo Materiali, Roma, and Orim S.p.A., Macerata.

Other national groups like the Pirelli laboratories, even if not directly involved, might contribute by developing mathematical models of the peculiar phenomenon. Once the project is financed, an ad hoc laboratory could be built in Italy close to Rome in a very short time."

[Editor's Notes:
(1) Performed by Yasuhiro Iwamura of Mitsubishi Heavy Industries.
(2) The intention is to use a subset of the "cold fusion" process to remediate waste from conventional nuclear fission reactors.
(3) Math error is original.
(4) Reportedly, another article appeared in the same issue of Il Sole discussing the prospects of bringing a new Italian fission plant online.

Concord Monitor, "For Slain Man's Family, No Arrests Mean No Closure," by Annmarie Timmins (Jan. 24, 2005)
http://www.concordmonitor.com/apps/pbcs.dll/article?AID=/20050124/REPOSITORY/501240347/1031
"Soon after Eugene Mallove of Pembroke was found murdered in Connecticut, outside his childhood home, the local police said they had talked to a couple of suspects and expected to have fingerprint and DNA evidence within a month. That was eight months ago, and the police said last week that they are no closer to solving the case. Some of that DNA evidence - the best

hope of tying someone to the scene - still hasn't come back from Connecticut's state lab."

The Inquirer, "Researchers Report Bubble Fusion Results Replicated: Cold fusion No Longer Confusion," by Nick Farrell (Jan. 21, 2005)
http://www.theinquirer.net/?article=20839
[Editor's Note: Ironically, this article created even more misunderstanding by collapsing the distinction between the acoustic cavitation / sonoluminescence experiments in the hot fusion field with the cold fusion field.

The work performed at RPI, Purdue, ORNL researchers recently is hot, not cold, fusion. If you believe the evidence of those doing the hot fusion version of acoustic cavitation, they make 10 ^ -6 watts in their beaker bubbles. If you believe those doing the cold fusion version of acoustic cavitation, they make 10 ^ + 2 watts in their beaker bubbles. The bubble race is on.

BBC Horizon, "An Experiment to Save the World: Has This Man Created Nuclear Fusion? Horizon Investigates" (Feb. 17, 2005)
http://www.bbc.co.uk/sn/tvradio/programmes/horizon/experiment_prog_summary.shtml
The BBC Horizon group produced an entertaining show about bubble fusion. We can't really call it a documentary. You'll see why. Here is the opening statement by narrator Dilly Barlow:

"We have assembled a team of experts to conduct a unique experiment to test out these claims. If the result is positive, then this man will be on the way to a Nobel Prize, and a dream of a shortcut to a world with unlimited cheap energy could finally be within reach. But if it fails, one of the great dreams of science will surely die."

In case you are not inspired to read the entire transcript, we'll disclose the punch line right away. Their expert, physicist Seth Putterman from UCLA, failed in his made-for-TV science experiment to replicate the work of Rusi Taleyarkhan.

Comments to the BBC Horizon discussion list say it all:

"Horizon dumbed down. You've really ruined Horizon, it's like the worst combination of the discovery channel and American factumentaries. Appalling, just about unwatchable. I really had to turn off last weeks effort...."

"I take it that the BBC's impartial approach to news and reporting doesn't extend as far as Horizon. With regards to the recent Sonofusion programme, for shame! Watchers are treated to a rather uninspiring visual medley of archive (frequently repeated) footage and a rather over-hyped narrative that gives a rather disrespectful look at those who've spent years trialing and proving experiments that are worthy of publication in some of the best respected journals in the scientific world."

"[Years ago] Horizon's original view was to expand and inform the rest of us about scientific developments, expanding our knowledge (or 'horizons' if you will). In general the impression of the content and approach of the programme, to this recent development has been derisive, cynical and divisive. The director seems more interested in proving the science is wrong than providing a balanced presentation of fact (like documentaries do)."

Another BBC documentary is rumored to be in the works about cold fusion - but not by the Horizon group. Cold fusion fans eagerly await the next suspenseful BBC "science" programme.

Scientific American, "Back to Square One: Government Review Repeats Cold Fusion Conclusions," by Charles Q. Choi (March 2005)
http://www.sciam.com/article.cfm?chanID=sa006&colID=5&articleID=00059015-99C5-1213-987F83414B7F011C
(Subscription required for full article)
After 15 years, cold fusion got a second chance at legitimacy from the U.S. Department of Energy, often seen by cold fusion advocates as their greatest enemy. This rematch, many hoped, would vindicate the field or kill it once and for all. Instead, history repeated itself, with a verdict that evidence remained inconclusive.

Conventional physics holds that nuclear fusion ignites at multimillion-degree temperatures. In March 1989 controversy erupted when electrochemists Martin Fleischmann and Stanley Pons, then at the University of Utah, claimed room-temperature experiments with palladium electrodes in heavy water generated heat far in excess of any chemical reaction. The suggestion was that the deuterons--hydrogen nuclei bearing an extra neutron each--making up the heavy water were fusing....

[Editor's note: We also recommend Charles' earlier article: http://www.spacedaily.com/news/energy-tech-04w.html]

Scienza e Conoscenza, "Più caldo o più freddo? Davvero il clima sta cambiando? - Di Giovanni Zavalloni (March 2005)
http://www.scienzaeconoscenza.it/rivista.php?idRivista=26
(Subscription only)

22. Speakers Available - Experts on the Subject of Cold Fusion

Steven B. Krivit - General audiences (Co-author of *The Rebirth of Cold Fusion*)
Charles G. Beaudette - Academic audiences (Author of *Excess Heat and Why Cold Fusion Research Prevailed, 2nd Ed.*)
David J. Nagel - Government and military audiences (Participant in the 2004 Department of Energy Cold Fusion Review)
* Please send requests to: info2@newenergytimes.com

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* Please send requests to: info2@newenergytimes.com

23. Recent Updates to the New Energy Times(tm) Web Site

The Formation of "Solid Deuterium" Solidified Inside Crystal Lattice and Intense Solid-State Nuclear Fusion ("Cold Fusion",) by Y. Arata
http://newenergytimes.com/Library/2004Arata-FormationOfSolidDeuterium.pdf
The Original Fleischmann-Pons-Hawkins Cold Fusion Paper
http://newenergytimes.com/library/1989fph/1989fph.htm
Young Scientists
http://newenergytimes.com/students/students.htm
Cold Fusion Papers Published in Peer-Review Journals (March 2005)
http://newenergytimes.com/Reports/PublishedPapers.htm
Why Is Everybody Waiting for America When It Comes to Research?"
A conversation with Martin Fleischmann, the discoverer of cold fusion
http://newenergytimes.com/Conversations/FleischmannByLietz.htm
American Physical Society March 2005 Meeting, Los Angeles, Calif.
http://newenergytimes.com/Conf/APS2005/2005.htm

24. Support New Energy Times(tm)

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If you are interested in providing a tax-deductible donation, please contact us for special arrangements.

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New Energy Times(tm) wishes to thank Cindy Goldstein for her exemplary editorial assistance.

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Shoot And Run Productions

26. Administrative

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* If you do not wish to receive future communications from New Energy Times, please click here to unsubscribe.

SRO-2016-00952-F
Responsive Document 015

{In Archive} Re: ROME ICCF-15
Kirk Shanahan Steven Krivit 07/08/2010 07:13 AM

No. Abstracts and direct phone calls.

Steven Krivit Kirk, Did you attend ICCF-15? 06/28/2010 10:27:33 PM

From: Steven Krivit <steven1@newenergytimes.com>
To: "kirk.shanahan-srs.gov" <kirk.shanahan@srs.gov>
Date: 06/28/2010 10:27 PM
Subject: ROME ICCF-15

Kirk,

Did you attend ICCF-15?
If not, how did you learn about the Kidwell presentation?

Thanks,

Steven

In what may be another excellent effort, D. Kidwell and coworkers have 'gone the extra mile' and discovered that the claimed production of Pr in deuterium flow through Pd membrane experiments may well be due to contamination. The abstracts of talks to be given at ICCF15 is posted to the Web (http://iccf15.frascati.enea.it/docs/Abstracts-11-9.pdf), and in it (Session 3, talk O_6) an abstract states that the NRL lab has conducted a study where samples that were supposed to have produced transmutation were examined at the NRL lab and at another lab that has claimed prior success (MHI). When NRL found no Pr when MHI did, a Pr contamination was found at the MHI lab where the analyses for Pr were conducted. In other words, the lab that claimed to have detected Pr produced by heavy metal transmutation was in fact contaminated with the very element they found! Surprising isn't it (not to chemists).

This note is posted to illustrate to Wiki editors that the idea of contamination as the source of 'transmutation products' is normal, everyday chemical thinking, not OR or anything like it. I will not be responding to comments on this, as my only point is what I just said. Kirk shanahan (talk) 15:20, 21 September 2009 (UTC)

SRO-2016-00952-F
Responsive Document 024

high density hydrogen isotopes"
Physics Letters A

Dear Dr.Shanahan,

Unfortunately, I reject the publication your Comments as well as Reply of Dr.Kitamura.

For your guidance, I append the reviewers'and my comments below.

Thank you for giving us the opportunity to consider your work.

Yours sincerely,

Vladimir M. Agranovich, Dr.
Editor
Physics Letters A

Comments:

Dear Dr. Shanahan,

The independent referee suggested do not
publish in Physics Letters A your Comments as well as
Reply of Dr. Kitamura. The Comments and Reply do not contain
convincing results of investigations and thus do not contain
the results which need an urgent publication in journal of letters.
The referee suggests to send this discussion to another more special
journal
(J. High Temp.Soc., Ann. Rev. Mater. Sci. or similar) where
the Comments and Reply can indeed find the readers interesting in your
results.
I agree with the suggestion of referee and have to admit that
publication in PLA
of the paper by Dr.Kitamura was also not well grounded.

Sincerely,

Vladimir Agranovich

r wu <wur@uci.edu>
01/20/2010 01:53 PM

To <kirk.shanahan@srnl.doe.gov>
cc
bcc Kirk Shanahan/SRNL/Srs
Subject Re: Your Submission

History: This message has been forwarded.

Please send the responses to Prof. Agranovich, who handled this case.

On 1/20/10 4:58 AM, "kirk.shanahan@srnl.doe.gov" <kirk.shanahan@srnl.doe.gov> wrote:

Dear Dr. Wu,

Unfortunately, I find your rejection of my comment on the grounds you stated as professionally unacceptable. I also thoroughly agree that the original publication by Kitamura, et al, was inappropriate for PLA, but the paper did appear in PLA. Therefore, the scientific discussion of the paper should also appear in PLA. It is highly unusual and definitely unfair to expect me to find a journal that will publish a Comment on a paper not published originally in their journal. In fact, I feel it is the responsibility of a journal to carry on at least one round of discussion on papers they publish in their own journal. I respectfully request that you reconsider this rejection.

Sincerely,

Kirk L. Shanahan

"R. Wu" <wur@uci.edu>
Sent by: ees.pla.13.67f89.25749aac@eesmail.elsevier.com 01/20/2010 02:20 AM

To

kirk.shanahan@srnl.doe.gov

cc
Subject

Your Submission

Ms. Ref. No.: PLA-D-09-03593
Title: Comments on "Anomalous effects in charging of Pd powders with

SRO-2016-00952-F
Responsive Document 023

{In Archive} Re: ICCF15 Pr paper
Kirk Shanahan   David A. Kidwell (Federal)   11/09/2009 09:08 AM

Hi David,

I just downloaded your Pr presentation slides from the LENR--CANR site, so you don't need to send me them separately.

In glancing through them, I noticed you discussed the mass 96 peak that Iwamura attributed to Mo. Mass 96 is also a trimer of sulfur (S is 100% mass 32). And Mizuno, either in ICCF13 or 14, announced that in his group's replication of the experiment, they detected a sulfur contaminant. I think that is most likely, but a detailed study of the MS's is needed to confirm that of course. (As a surface scientist I expect you recall that MoS_2 is used to prevent galling during bakeout in bolt threads.)

FYI, my thesis was in surface science (Ru and Os single crystals), and I can appreciate the level of effort needed to do this work. You went the extra mile.

I also agree about the Pr migrating around under D_2 flux. That was about the only intriguing thing out of the study, and I actually have been tinkering with some R&D in the area. When Iwamura first reported his results, I checked a Certificate of analysis for CaO online and it listed a possible contaminant, but today I can't remember if it was Pr or the other element (Sr?) Iwamura claimed to find.) We are currently setting up to see if a La-Ni-Al alloy will disproportionate under H_2 at what is considered 'low' temp (260C) for those alloys, and I saw some interesting effects with Pd and Cu migration on ion exchange resin beads (in relation to the Patterson Power Cell.).

Keep up the good work.

Kirk

SRO-2016-00952-F
Responsive Document 022

"I would not worry about safety of Pd loaded with hydrogen due to CF events. The trigger, if there is one, is not clear." - which is exactly why it could be a major safety problem when dealing with kilogram quantities of metal hydrides that are fully loaded. As I copied into my last email to you on this, most of my reason for getting into this was curiosity as to why the issue was unresolved. Part of that was the fact that when I asked my new hydrogen separation and storage scientist colleagues back in '95 if cold fusion was real, I got a resounding 'No', but then got a resounding silence when I asked 'How do you know that?' That intrigued me, so I investigated in my free time. But the fact remains, that if CF is real, we may have a potential problem here, and we are extraordinarily safety conscious here.

"I am not as negative as you on the requirements for reproduction. ...{snip}...' I am not any more negative than you are. I am however, extremely negative on people who claim certainty where none exists. The failure to delineate the controlling factors for CF does _not_ justify claiming an exclusive nuclear explanation for the motley collection of anomalies that CFers have. But that is exactly what they do. _That_ is the pseudoscience, jumping to an unjustified conclusion and proceeding as if it were proven. And I might add, extracting funding and other support from unsuspecting sources under false pretenses (please note, I am NOT talking about 'fraud', just bad science). What will be very interesting to see is how the field reacts to your and Mizuno's replication of the Iwamura results. You showed that Pr contamination was a distinct possibility. Mizuno did likewise for the 'Mo', which was actually S. Good scientists will fold that thinking into their experimental protocols and conduct extra experiments aimed at detecting contamination. I seriously doubt we will see the CFers doing so. They already had that chance with calibration shifts and they completely dropped the ball on that.

"Catalysts are one of the least understood areas of Chemistry. ...{snip}..." - yes, I know. My thesis work was in surface science. Many of my contemporaries and friends at that time were in the field. Alex Bell signed my thesis. Here we use ~2% Pd on zeolites in our process, one of my colleagues is studying that and making more now, also looking at Pt on z. I have studied 50% Pd on kieselguhr, which is a form of silica. I have lots of papers on the subject, and on Pd nanoclusters. As you saw from my comment I also am studying them, though only as a control for another sample of more interest.

"Keep up the good work and healthy skepticism of everything." Of course, it is a requirement of being a good scientist. And for the record, pathological skepticism is as bad as pathological belief. You need good, believable, technically defensible reasons, not just a personal choice.

Kinda burnt out on this today. More tomorrow maybe.

{In Archive} Re: ICCF15 papers
Kirk Shanahan David A. Kidwell (Federal) 10/29/2009 03:24 PM

Using the quote and reply approach again...

"I am of the opposite on EMAILs. They are permanent records and as such require lots of time to write and double check about 30-60 min/EMAIL - even for this short response. Thus, I cannot go over all my objections is writing at this time." - Sorry to hear that. I suppose I shall call then, to at least get a list of what I should consider. When's a good time?

"However, one error is that "Lasser and Klatt [4] present data for all three hydrogen isotopes, although they do not go below 323K in their studies." They actually go to 30C on loading and 50C on de-loading." - OK correct, except they also don't report T absorption data. I will clarify in the Comment. The absorption data is relevant to the Kitamura expts. of course.

"However, loading ratios depend on particle size." - I wondered if Kitamura would bring this up in their anticipated 'Reply to...'. In fact I am well aware of the issue, but I don't think they understand it (or perhaps you as well). The problem is that as the particle size decreases, the surface-to-volume ratio increases. In dealing with bulk Pd powders, the surface contribution is known to be small. As you move to nanoclusters you can't say this. Now, an appreciable fraction of you H is terminally bonded to surface Pd atoms, as opposed to being located interstitially. The whole idea of 'cold fusion' and McKubre's mantra about "loading having to be >.9" is that *in the bulk material* this means that you are getting close approach of D *in the bulk.* Surface bonded H doesn't come any closer to other H than in any other chemical compound. So, the use of 'loading ratio' in clusters is something of a misnomer, because now you really have two types of H at roughly equivalent concentrations. While I have some problems with the Chinese papers you cite in your talk, you can see this effect in Figure 1 of the JPCB (2006) article. The figure is an 'inverted isotherm' in my parlance. We normally plot H/M (or N-sub-H/N-sub-M) as the X axis and P as the Y. The Pd powder curve is a pretty good standard isotherm plot (a minor issue or two). The IW plot shows a bit of spillover. The IE plot shows more, plus you are now seeing the expected loss of plateau flatness observed in other studies on nanoparticulate Pd (also somewhat apparent in my Figure in my comment). Once you get to the SG plots though, this bulk-like chemistry has disappeared. You're in a different regime completely apparently. (I am still studying this but I currently believe this also implies that the Chinese folks has misapportioned their heats just like Kitamura does.) Why H bonded in a 'compound' like any other molecule should somehow fuse seems a bigger problem to account for than why it might in highly loaded bulk Pd. (Of course, the whole '>.9' bugaboo is a red herring anyway.) Bottom line, until the known side reactions are measured and accounted for, the situation with these catalysts remains ambiguous.

"Given the exponential nature of loading, and the similarity of D and H from Lasser and Klatt data and the higher pressures reached by Kitamura, one could reasonably claim similar ratios of D/M." - Of course, they are calculating it based on presumed H uptake, and my comment on that is that the H consumption they see is confounded by other chemical reactions, which is a well-known problem in the field of Pd hydride chemistry.

SRO-2016-00952-F
Responsive Document 021

At 02:43 PM 10/14/2009, you wrote:

Dr. Kidwell,

I have followed your interactions with the cold fusion community with interest since you became involved at ICCF14, and I was pleased to see your abstract for ICCF15 that reported finding a Pr contamination in the MHI labs. I was hoping you could send me the actual papers you presented at ICCF15 (both please).

You may know that I have been an active critic of the cold fusion claims for some time now, and that I have 3 publications on calorimetric errors in print. I would send them to you but I expect you already have them, but if not, please let me know and I'll forward them to you. I also have another manuscript currently undergoing public release review which responds to the abysmal publication by Kitamura, et al, in Phys. Lett. A. As soon as I am cleared to release this I will forward a copy to you.

Kirk Shanahan

David A. Kidwell, Ph.D.
Research Scientist
Code 6177
Naval Research Laboratory
4555 Overlook Ave, SW
Washington DC 20375
202-767-3575
202-767-3321 (FAX)
David.Kidwell@nrl.navy.mil

USPS mail to NRL is delayed by 1-2 weeks.
All time critical documents should be sent
by overnight delivery/courier service **.[attachment "ICCF15 - Gas Loading-Final.ppt" deleted by Kirk Shanahan/SRNL/Srs]**

{In Archive} Re: ICCF15 papers
Kirk Shanahan David A. Kidwell (Federal) 10/27/2009 02:50 PM

David,

Thanks for responding. Is the one on the Pr contamination not available?

As requested I have attached copies of my CF-related papers. I also attached the papers I was responding to, 2 by Storms, one by Szpak, Mossier-Boss, Miles, and Fleischman, with the exception of the original publication by Storms at ICCF8. I have that, but not in electronic form.

As I mentioned, I also am responding to Kitamura, et al. That is still in the public release approval process. I will forward the manuscript ASAP.

thermoacta.pdf szpak_comment.pdf reply_to_szpak.pdf storms_comment.pdf reply_to_storms.pdf

I recently became aware that some of the 'secret' goings-on on the CMNS list have been posted to Kowalski's web page, and that in it McKubre refers to my proposal as a 'dead horse'. This is wishful thinking on his part fostered by two events. First, I obtained and analyzed the data he published in his 1998 EPRI report. It was the data set I used to develop the CCS idea. Second I had also proposed a mechanism for how the CCS could occur, which McKubre's data did not seem to support. That is neither here nor there, as his results (2 runs in 1 calorimeter) are not reproduced, and as if my proposal is eventually shown to be wrong, that does not affect the validity of the CCS concern. In fact, using the standard way of calibrating his data, I showed that a 2% change in calibration constant could flatten out his apparent excess heat peaks, which is fully consistent with the results I obtained on Storms' data.

I will give your data a look as time allows.

Kirk S.

"David A. Kidwell (Federal)" <David.Kidwell@nrl.navy.mil>

"David A. Kidwell (Federal)" <David.Kidwell@nrl.navy.mil>

10/27/2009 10:47 AM

To kirk.shanahan@srnl.doe.gov
cc
Subject Re: ICCF15 papers

Attached is the approved presentation given at ICCF15. I will send you the final paper when it is written and approved for distribution in the next few months.

Please send the three papers as I do not have access to all of them.

If you can find anything obvious about the abnormalities in this data, please write as I will take it into consideration in preparing the final manuscript.

SRO-2016-00952-F
Responsive Document 020

Kirk Shanahan/SRNL/Srs
10/14/2009 02:43 PM

To david.kidwell@nrl.navy.mil
cc
bcc
Subject ICCF15 papers

Dr. Kidwell,

I have followed your interactions with the cold fusion community with interest since you became involved at ICCF14, and I was pleased to see your abstract for ICCF15 that reported finding a Pr contamination in the MHI labs. I was hoping you could send me the actual papers you presented at ICCF15 (both please).

You may know that I have been an active critic of the cold fusion claims for some time now, and that I have 3 publications on calorimetric errors in print. I would send them to you but I expect you already have them, but if not, please let me know and I'll forward them to you. I also have another manuscript currently undergoing public release review which responds to the abysmal publication by Kitamura, et al, in Phys. Lett. A. As soon as I am cleared to release this I will forward a copy to you.

Kirk Shanahan

SRO-2016-00952-F
Responsive Document 019

"WordPress.com" <donotreply@wordpress.com>

04/22/2009 01:44 PM

To kirk.shanahan@srnl.doe.gov

cc

bcc Kirk Shanahan/SRNL/Srs

Subject [HIV/AIDS Skepticism] New Comment On: Mainstream science wrong again, for two decades

There is a new comment on the post "Mainstream science wrong again, for two decades".
http://hivskeptic.wordpress.com/2009/04/20/mainstream-science-wrong-again-for-two-decades/

Author: Henry Bauer
Comment:
Kirk:

Yes, I'll look at your articles after I get home and perhaps continue not via the blog; but I thought readers deserved to see your response to my response

See all comments on this post here:
http://hivskeptic.wordpress.com/2009/04/20/mainstream-science-wrong-again-for-two-decades/#comments

To manage your comment subscriptions, click below:
http://subscribe.wordpress.com/?email=kirk.shanahan%40srnl.doe.gov&key=f4c56f3e50a448f5df5978bc50ebd1ea

{In Archive} Anti-Cold Fusion Paper published
Kirk Shanahan   Stavila, Vitalie (-EXP)   08/09/2010 02:19 PM

Hi Vitalie,

FYI - my big anti-cold fusion paper was just published on line. There was a Response, with a list of authors covering almost all the 'names' active today. Perhaps you can access them electronically, but I can't right now.

The references are below.

Kirk

Comments on "A new look at low-energy nuclear reaction research"
Kirk L. Shanahan

J. Environ. Monit. , 2010, Advance Article
DOI: 10.1039/C001299H, Letter

A new look at low-energy nuclear reaction (LENR) research: a response to Shanahan
J. Marwan, M. C. H. McKubre, F. L. Tanzella, P. L. Hagelstein, M. H. Miles, M. R. Swartz, Edmund Storms, Y. Iwamura, P. A. Mosier-Boss and L. P. G. Forsley

J. Environ. Monit. , 2010, Advance Article
DOI: 10.1039/C0EM00267D, Letter

SRO-2016-00952-F
Responsive Document 026

Steven Krivit <steven1@newenergytimes.com>

08/16/2010 01:55 PM

To "kirk.shanahan-srs.gov" <kirk.shanahan@srs.gov>

cc Jan Marwan <info@marwan-chemie.fta-berlin.de>, "michael.mckubre-sri.com" <michael.mckubre@sri.com>, "francis.tanzella-sri.com" <francis.tanzella@sri.com>,

bcc

Subject LENR Researchers Deal Decisive Blow to Shanahan

http://blog.newenergytimes.com/2010/08/16/lenr-researchers-deal-decisive-blow-to-shanahan/

SRO-2016-00952-F
Responsive Document 027

{In Archive} Re: My new article you should read
Kirk Shanahan   Thomas Blakeslee   05/05/2011 03:40 PM

Hello Thomas,

I am somewhat at a loss as to why you think I needed to read your article (which I had already seen by the way). All you have done is become enamored of the current cold fusion arguments and repeat them. I have spent the last few years 'debunking' them, and you offer nothing new except perhaps the Rossi device. I am aware of this device as well, and I have deliberately avoided looking at it except in passing, as it very closely follows the 'Patterson Power Cell' example. The PPC was a device claimed to have about a 15-25,000% excess power output. It had just burst on the scene when I became interested in cold fusion in May 1995. I spent considerable time studying it, and came up with a conventional explanation for what was being observed (i.e., no cold fusion involved). However it was always just speculation as neither I nor anyone else tried to prove out my points. One of the big problems was the lack of information on the details of it, since Patterson was trying to market it and wanted to keep certain things secret (sound familiar?). The *patented* PPC transmorphed into the RIFEX 'kit' of which maybe 6 or 7 were sold, and then just faded from the scene. But it did take a few years to do that. I expect the Rossi device to do the same, it has all the same signs.

It would be wonderful if the Rossi device worked as advertised. We are in an energy crisis, world-wide especially, and a cheap and simple solution to the problem would be tremendous. Perhaps Rossi has actually done it. Only time will tell I suppose. However, the TANSTAAFL principle still applies.

I suppose you may not know that there are significant unanswered challenges to most (if not all) of the cold fusion claims. Conventional explanations are much more realistic and believable, except of course to die-hard cold fusion 'true believers'. Unfortunately they have resorted to 'group-think' reasoning to try to respond to the challenges, but their warped logic is clear to any who cares to look. Just for your edification, you might look up my 2010 J. of Environmental Monitoring comment on Krivit and Marwan's paper in the same journal (2009), and the back-to-back paper by 10 of the current prominent cold fusioneers (to borrow their term). Their response to my comment of course tries to prove my comments wrong, but it is fatally flawed by a deliberate attempt to misdirect the readers. The short form of the long story is that Ed Storms posted some cold fusion calorimetry data he generated on the Internet in January/February 2000. I reanalyzed that data and immediately had issues with Ed's interpretation and methods. He published his work at ICCF9 in 2000. I then wrote a comment and submitted it for publication to a journal, that unfortunately had just gotten a new editor. That editor sent my paper to 3 reviewers, 2 of which were cold fusioneers (1 of which was E. Storms, as expected), who managed to kill it by the simple expediency of having 2 of the 3 votes against publishing. After that long battle (nearly 2 years time), I reformatted the paper and got it published in Thermochimica Acta, whose editor was Lee Hansen, an anti-cold fusioneer, and as it turns out, the non-cold fusioneer who had originally reviewed the first version of my paper. That resulted in rapid publication (probably a record) in 2002. Fleischmann, Spzak, and others denigrated my paper in one of theirs in 2004 (without informing me they had done so) and I wrote a reply in 2005 showing why their data supported my thesis. Then in 2006, Storms finally tried to rebut my 2002 paper, but I replied (2006) and showed his arguments to be wrong, but that didn't stop him from claiming he had invalidated my arguments in his 2007 book (and twice again in 2010). Then in 2009, Krivit and Marwan wrote their paper, I commented (2010), and then the 'Group of 10' replied. (Storms is one of the 'Group of 10' and also repeated his claims to have dealt with my challenges in his 2010 'review' article in Naturwissenschaften.)

The way you can tell they have deliberately misunderstood the challenge is that in the 'Group of 10' response (2010) they start (after some preliminary flag-waving) by claiming my 'random hypothesis' obviously doesn't fit the facts. Then they use that to attack most of my other arguments and conclude they are wrong. Unfortunately, my first paper was about a *systematic* error in Storms' calorimetry, and I use either or both of the words 'non-random' and 'systematic' in all my other publications, including the specific one they were responding to. Now, any competent scientist knows that systematic means non-random, and it means the opposite of random. So, in positing my hypothesis as 'random' they misrepresent the primary counterargument to their claims for the express purpose of fooling the readers into believing the objections raised are wrong. And there are 10 names on the paper! And then Storms repeats this in his separate 2010 'review'! How could 10 highly trained scientists not have deliberately done this? The level of incompetence required to be present for this action to have been 'accidental' boggles the mind. If they truly are that incompetent, no one in their right mind should trust anything they do, say, or write. Nor should anyone trust someone who deliberately resorts to misrepresentation to 'win' the argument.

(The Group of 10 are: J. Marwan, M. C. H. McKubre, F. L. Tanzella, P. L. Hagelstein, M. H. Miles, M. R. Swartz, Edmund Storms, Y. Iwamura, P. A. Mosier-Boss and L. P. G. Forsley)

Unfortunately, the sensational gets more press than the mundane, and it would seem from your article that you also have fallen for their spell. Tsk! Tsk! You should really have learned by now while debunking those other things you mention that the counterpoints to wild claims have to be searched out. The tricksters trying to sell their fantasies certainly aren't going to do it for you!

Kirk Shanahan {My opinions...noone else's}

SRO-2016-00952-F
Responsive Document 028

Rich Murray
<rmforall@gmail.com>
06/29/2011 12:56 AM

To H-Ni_Fusion@yahoogroups.com, Rich Murray <rmforall@gmail.com>, Rich Murray <rmforall@comcast.net>

cc

bcc Kirk Shanahan/SRNL/Srs

Subject Rossi Energy Catalyzer: Scientific Communication and Ethics Issues, Steven B. Krivit, Senior Editor, New Energy Times: Rich Murray 2011.06.28

Rossi Energy Catalyzer: Scientific Communication and Ethics Issues, Steven B. Krivit, Senior Editor, New Energy Times: Rich Murray 2011.06.28
http://rmforall.blogspot.com/2011_06_01_archive.htm
Tuesday, June 28, 2011
[at end of each long page, click on Older Posts]
http://groups.yahoo.com/group/astrodeep/message/88
[you may have to Copy and Paste URLs into your browser]

Here is a classic example of careful, tenacious expert investigative journalism, unraveling a remarkable case of scientific delusion.

http://newenergytimes.com/v2/news/2011/37/Report2-372-EnergyCatalyzerScientificCommunicationAndEthicsIssues.shtml

Report #2 - Energy Catalyzer: Scientific Communication and Ethics Issues

By Steven B. Krivit
Senior Editor, New Energy Times

[This is the second in a series of reports based on my interviews with Andrea Rossi, creator of a device he calls the Energy Catalyzer, or E-Cat, Sergio Focardi, professor emeritus at the University of Bologna, and Giuseppe Levi, a professor in the university's Department of Physics, and based on my investigation of their claims of a low-energy nuclear reaction device that produces commercially useful levels of excess heat.

The complete list of New Energy Times reports on this topic is here.
http://newenergytimes.com/v2/sr/RossiECat/AndreaRossiAndHisEnergy-Catalyzer.shtml
]...

[more...]

---

no excess heat in June 14 Rossi demo, as no invisible dry steam at end of hose, just feeble mist, perhaps liquid water -- many unbiased critical comments on Vortex-L: Rich Murray 2011.06.25
http://rmforall.blogspot.com/2011_06_01_archive.htm

Saturday, June 25, 2011
[at end of each long page, click on Older Posts]
http://groups.yahoo.com/group/astrodeep/message/86
[you may have to Copy and Paste URLs into your browser]

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Boston University Graduate School 1967 psychology,
BS MIT 1964, history and physics,
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http://groups.yahoo.com/group/AstroDeep/messages

http://RMForAll.blogspot.com new primary archive

http://groups.yahoo.com/group/aspartameNM/messages
group with 118 members, 1,625 posts in a public archive

http://groups.yahoo.com/group/aspartame/messages
group with 1226 members, 24,342 posts in a public archive

http://groups.yahoo.com/group/rmforall/messages

SRO-2016-00952-F
Responsive Document 029

{In Archive} Re: Papers
Kirk Shanahan Dieter H. Britz 02/09/2011 08:18 AM

Hi Dieter,

The refs for the sequence are below. I hope in you summaries you will note that in the response to my comment, the list of esteemed authors all sign off on discussing my calibration constant shift as "Shanahan's random hypothesis", but that in all four of my publications to date I have referred to my hypothesis as 'systematic' and/or 'non-random'. I couldn't believe their response made it past the reviewers, because they reject my 'random hypothesis' because the effect is non-random, and then use that rejection to reject most of what else I wrote. I protested to the editor, suggesting even that the response be withdrawn, but he wouldn't do anything about it, nor would he let me write a counter-response. There is nothing in their response that can't be countered.

FYI, I also wrote a comment on Kitamura, et al's Physics Letters A paper, which was rejected by the editor because he didn't want to talk about cold fusion in his journal. I protested that, saying he already had, and he came back and said I needed to shorten it, which I did. I resubmitted it to him and haven't heard anything since. I presume it is dead in his inbox. For the record, the Kitamura stuff is junk. Kitamura even wrote a short response to my comment where he admitted that he hadn't reproduced Arata, and that the signals detected could all be classed as noise, but of course that won't get published either.

I am working on a monster article that addresses the issues that are raised in the recent reviews by K&M and Storms, but I am rapidly losing interest. In going back through the cold fusion literature, I find cases of where all the objections I noted in my papers are recognized and applied, but then apparently forgotten in favor of claiming success. I may try to put it out as a Savannah River National Lab Technical Report, but not get it actually published, just too much work for too little benefit.

I've given up on Wikipedia, too many fanatics to fight off all the time.

What is your take on Barry Kort and his claim about the Taylor expansion needing to go to the second term? Did he find a real flaw in the power measurement technology?

Kirk

A new look at low-energy nuclear reaction research
Steven B. Krivit and Jan Marwan
J. Environ. Monit., 2009, 11(10), 1731-1746
DOI: 10.1039/B915458M

2009Krivit-S-ANewLookAtLENR.pdf

Comments on "A new look at low-energy nuclear reaction research"
Kirk L. Shanahan
J. Environ. Monit., 12(9), (2010), 1756-1764
DOI: 10.1039/C001299H

Comment_JEM_12_2010_1756.pdf

A new look at low-energy nuclear reaction (LENR) research: a response to Shanahan
J. Marwan, M. C. H. McKubre, F. L. Tanzella, P. L. Hagelstein, M. H. Miles, M. R. Swartz, Edmund Storms, Y. Iwamura, P. A. Mosier-Boss and L. P. G. Forsley
J. Environ. Monit., 2010, 12(9), 1765-1770
DOI: 10.1039/C0EM00267D

JEM_article_Reply_2010_novol.pdf
(I only have a "no volume number" version of this.)

"Dieter H. Britz" Hi Kirk I have here two papers out of the J. Envir... 02/09/2011 03:56:08 AM

From: "Dieter H. Britz" <britz@chem.au.dk>
To: "Dr. K. Shanahan" <kirk.shanahan@srs.gov>
Date: 02/09/2011 03:56 AM
Subject: Papers

Hi Kirk

I have here two papers out of the J. Environ. Monit.; one by you, "Comments on 'A new look...'" and a response from Marwan et al, "A new look... reponse to Shanahan". Both papers are in online form, and I have been waiting to see them come out "properly", that is, with volume and page numbers. Have they? What is the sequence of these papers? I assume that there was an original one, to which you responded and so on. I don't want to miss any.

Regards
Dieter
--
Dieter Britz http://www.chem.au.dk/~db

SRO-2016-00952-F
Responsive Document 030

{In Archive} Re: Berkeley - 1983-84?
Kirk Shanahan   Ugo Bardi   01/06/2012 09:58 AM

Oh...here is Celani's abstract for the talk he will give next week...

ISEO-WSEC Conference 2012, Geneva, 10-12 January 2012

Progress, in the Condensed Matter Nuclear Science, on excess energy production: towards practical applications?

Francesco CELANI

National Institute for Nuclear Physics, Frascati National Laboratories- Italy
Vice-President of International Society of Condensed Matter Nuclear Science

Abstract

On March 23, 1989, the international scientific environment, and not only that, was deeply surprised because of the abrupt announcement by two Scientists, one of them at world-class level (M. Fleischmann), that they had detected measurable, and unexplainable, excess energy after prolonged electrolysis of Heavy Water using Palladium (Pd) rods as cathode. Such a phenomenon, that cannot be ascribed to usual chemistry or physics reactions, was improperly given the odd name "cold fusion", remembering similarities with the "muon-catalysed fusion" predicted (1952) by A. Sacharov and measured (1956) by L. Alvarez (Nobel Laureates): both fusion were realised at room temperatures and not at the "usual" several million of °C.

The results, apart from the initial enthusiasm, were generally considered with large scepticism from most of the science community because they were completely unexpected in theory, and poorly reproducible in the experiments. As a consequence, only the Researchers and a few Institutions continued the studies that got - mostly by chance - some good results and of, enough high, scientific quality.

Among them we mention NASA and J. Bockris at A&M Texas, who started in July 1989 an investigation looking for occurring of usual Deuterium-Deuterium (D-D) fusion with emission of neutrons (i.e. strong force interaction). They did not find it but NASA detected in-explainable behaviour of Pd tube when heated at high temperatures (350°C) and Hydrogen (H_2) or Deuterium (D_2) gas were allowed to flow in and out. In short, the behaviour of energy production was as expected using H_2 gas but completely unexpected with D_2. Heat production was detected both in the incoming and out-coming phases of the gas: such effect was against any previous scientific experience! Such key results were not communicated immediately to the Scientific Community until, by chance, a report was found inside a drawer and wide-spread only in 2004. In December 2009 another similar experiment was performed, devoted to reconfirm the thermal anomalies found on 1989. The results, thanks to specific and improved instruments, were of even better quality. Again, the results were not made public until the document was found, by chance, on the web in August 2011. Recently, top level NASA Researchers are more "open" about their results produced "at home".

Apart from such episodes, over one thousand Researchers, mainly in J, I, USA, RUS, CP, IND, F, D, continued such studies, usually with low budget constrains. Among them, the methodologies developed, models introduced and results obtained, by M. Srinivasan, Preparata-Del Giudice, A. Takahashi, P. Hagelstein, E. Storm, Chubb-Chubb, M. Kubre, F. Piantelli, F. Celani, Y. Iwamura, G. Miley, T. Mizuno,

De Ninno-Violante, H. Kozima, Larsen-Widom, X.Z. Li, J. Biberian, A. Huke, were especially innovative: published most of the results found or models developed. So, in spite of adverse conditions, the progress from the science point of view was remarkable: about theory, is "growing" a model based on weak force interaction.

A big step forward happened when, thanks to Y. Arata (Osaka Univ.-J), who, since 2002, introduced proper nano-materials (Pd, at size of 5-20 nm), dispersed in an anti-sintering matrix (ZrO_2), and in contact to pressurised D_2 gas. The results of Arata were the first ones fully reproduced by other scientists (A. Takahashi, A. Kitamura, Japan) and even using materials produced by an independent Industry (Santoku K.K.). Later, the original findings were even improved with better results thanks to new materials (based on ZrO_2-Ni-Pd), always nano-sized, as prepared by B. Ahern (USA) and initially studied since 2005 by Arata.

As far as recent claims of very large excess power using "micro-nano-sized Nickel" interacting with H_2 at high pressure and temperatures are concerned, coming from groups operating in Italy and Greece, we have to underline that both groups refused, up to now, independent tests of their apparatus: we cannot give scientific credit, as to-day, to their work. BTW, on November 2011, F. Celani asked to the Italian A. Rossi, through a wide-spread science magazine (Focus), to validate one of his 10kW's device. Even the, public, "persuasion" of the Nobel Laureate Brian Josephson was enough to get such device for scientific, fully independent, tests.

Nevertheless, we believe that so many evidences have been collected by serious Scientists up to now, that the reality of Low Energy Nuclear Reactions may be soon acknowledged by the whole scientific community, opening the way towards the fully exploration of their potential for practical applications and long term sustainability of this, practically infinite, energy source.

SRO-2016-00952-F
Responsive Document 031

Kirk Shanahan/SRNL/Srs
01/22/2014 09:07 AM

To alain.coetmeur@gmail.com
cc
bcc
Subject Re: The published critics on LENR calorimetries

Alain,

I see that you are a bit confused regarding the status of at least one of the critics of cold fusion calorimetry, namely me. You seem to think that my criticisms have been successfully addressed and done away with. While that is what the cold fusion calorimetrists would have you believe, it is far from the truth. (This statement also applies to the other criticisms I made in my 2010 publication in J. Environ. Monitor.)

First, a brief history: In January of 2000, Ed Storms posted to the Internet raw data from several of his experimental runs on a Fleischmann-Pons-type electrochemical cell that used Pt for both electrodes. The data included data from calibration runs (something M. McKubre's 1998 EPRI publication did not do). Almost immediately, I and Scott Little independently pointed out that the Storms data had a significant negative feedback from the input power evident in the apparent excess heat plots. Ed agreed and determined that he had a grounding problem, which he then corrected. Subsequently, he repeated his work and also posted that data to the Internet as well. I immediately downloaded it and began my study of it. In that process I discovered a way to interpret the data without invoking nuclear reactions. I communicated this to Ed, but he disagreed with my efforts and subsequently ignored them. Ed went ahead and presented his findings at the next ICCF (either 8 or 9, I can't remember at this moment) later that year. Therefore, I wrote up my findings and submitted the paper to a journal for review and publication. That manuscript can be found in the lenr.org archive under my name, and you will see it is dated October, 2000.

The paper went to review and there were 3 reviewers who reviewed it. One was clearly Ed Storms, which is entirely understandable, as my paper was a comment on his. One other was clearly a 'cold fusioneer' (CFers), and the other wasn't. The third reviewer basically accepted my paper as written with a few minor comments/corrections. The second reviewer completely rejected my paper with a couple of paragraphs of comments, which were very non-specific. Ed completely rejected what I had written with a multipage review. Unfortunately, Ed and reviewer 2's comments made no sense in relation to what I wrote. But since the 'vote' was 2-1 against, the editor initially rejected my paper. I however appealed and submitted extensive rebuttals of both the negative sets of comments. However, as expected Ed and reviewer 2 did not accept these, wrote more inaccurate and incorrect responses to my rebuttals, and again vote 'no'. Again the editor went with the simple 2-1 vote and denied publication.

After some email communication with the editor, I decided attempting another appeal was unlikely to be effective, so I submitted the paper to another journal, Thermochimica Acta (TA). As it turned out, the editor responsible for my paper there was the 3rd reviewer from the prior review. Since he had seen all the communication regarding the negative comments by Ed and reviewer 2, he decided he did not need to subject his reviewers to more of the same, and he accepted the paper after I made some suggested minor changes. So, my first cold fusion (CF) was published in Thermochima Acta in 2002 (yes, the battles at the other journal had consumed roughly 2 years).

That paper outlined what I decided to call the 'Calibration Constant Shift' (CCS) problem. My basic criticism of Ed's calorimetric methods is that he failed to take into account variation in the calorimeter calibration constants. I determined that a relatively minor shift in those values could produce the observed apparent excess heat curves. In fact I published the table of constants that I derived for each 'run' that Ed had made, and found (a) that they all fell within a band of about +/- 3% from the nominal value, and (b) they displayed a systematic character that indicated the shifts were being determined by chemistry/physics, and not randomly.

Let me quickly fast-forward for a moment to 2010. In 2010, I had written a Comment on the 2009 paper by Marwan and Krivit in the J. Environ. Monitor., and a group of 10 prominent CF authors had written a rebuttal to that, which was published back-to-back with my Comment. However, instead of rebutting my CCS proposal (or 'hypothesis' as the 10 authors forcefully insisted it should be called), the authors constructed a strawman argument they called "Shanahan's CCSH" (H for Hypothesis) which they construed as being random in nature. (In all four of my publication in the CF field, I have used the terms 'systematic' and 'non-random' to describe my CCS proposal.) Make sure you note the primary and fundamental difference here, I wrote that the CCS was a systematic effect, which always means it is controlled by chemistry/physics and is potentially understandable, while the 10 authors _said_ I wrote about some random process. They proceeded to rebut this random process, which is of no interest or concern to me as it is not what I was proposing, and then concluded that my concerns expressed in the 2010 Comment were unfounded. Unfortunately, since they did not actually rebut _my_ proposal, they were incorrect in making that claim.

Returning now to the publication of my first paper in 2002, there was apparently no mention of my work until 2004, when Szpack, Mosier-Boss, Miles, and Fleischmann published a paper in TA. In that paper were a couple of derogatory comments about the CCS proposal. Since no one had informed me of their publication, I didn't note it until 2005, whereupon I wrote a Comment on their paper that added some explanatory words about the CCS, and illustrated how their own data could be interpreted in light of it. However, they had not included the necessary information in their paper that would have allowed an exact check of their results against the CCS proposal.

Then in 2006, Ed Storms finally wrote a response to my 2002 paper, but that response was in fact a rehash (i.e. a repeat) of email discussions he and I had had before, during, and just after my 2002 publication, in fact most of his words were repeats of the comments he made in his reviewer comments on my paper. So, I likewise wrote up my responses to his comments and both papers were published back-to-back in 2006 in TA. In my response I rebutted every point Ed made against my CCS proposal, so the end result was that there was no successful rebuttal of my work, which is also the conclusion from the 2010 exchange in J. Environ. Monitor.

(Note that when two alternative explanations of a phenomenon exist that are both valid, i.e. not clearly shown to be wrong, it is scientifically inappropriate to arbitrarily discard one in favor of the other. Both need to be considered until such time as one can be eliminated by some rational process. One can favor one over the other, but not discard one.)

But you wouldn't know that my criticisms stand from the way the CFers act. They instead act as if the simple fact they wrote their papers was enough to nullify my proposal. That just isn't true. To nullify my proposal, first one has to address _it_, and not a strawman, and second, their comments would have to be unrebutted themselves. As I mentioned, I did rebut Ed's 2006 Comment (a fact he conveniently forgot to note in his 2007 book). Unfortunately the J. Environ. Monitor. editor would not allow a response to their Reply, so the issue was left hanging. But the CFers instead have claimed great victory, with the likes of Hagelstein trashing my comments in his internal RLE report (something I can not respond to either).

In 2012 I wrote a manuscript that I distributed via the Office of Scientific and Technical Information (OSTI) system that (a) answered the J. Environ. Monitor. Comment, (b) examined the Fleischmann and Pons calorimetric method used in their original word, and found several problems with it, and (c) distributed as an Appendix a manuscript regarding problems with the Iwamura claim to have detected CF-type results when using Pd on zirconia material. That last manuscript was submitted to Physics Letters, but the editor there also gave me the run-around on publishing it. After several emails he finally informed me that he did not want to publish more 'CF' articles (obviously including even those that rebutted CF claims). The manuscript was posted to the Internet by Mark Gibbs.

So you see Alain, my criticisms (a) are new and unique, and (b) have not been fairly dealt with by the CF community. While the CCS proposal I put forth was strictly applicable to Fleishmann-Pons electrochemical cells, the generic problems of failing to accurately determine error levels on experimental conclusions and of dealing with conventional explanations of the 'CF' results fairly and not rejecting them simply because they don't contain the word 'nuclear' have not been dealt with at all by the CF community. Until they do, they will never convince the 'establishment' that they have anything, since my mundane, normal chemistry explanations of what has been observed _should_ be the accepted explanations and significant proof is needed to reject them and accept the revolutionary idea of LENR (or LANR or CF or CMNS...).

Now, in direct response to your comments....

```
>I remember of CCS theory but it does not match burst events, blank cells,
>dead cells, good calibration of some cell like McKubre isotherm cells...
>Maybe some could add some detail about that recent critic
```

On the contrary, burst events, depending on what you are specifically talking about, easily fit into the CCS proposal, at least from a mechanistic viewpoint. The CCS proposal had 3 levels to it, the first was the straightforward mathematical analysis of Storms data which showed minor variations in calibration constants could explain the observed apparent excess heat peaks. The second was an explanation of how one could get variation in calibration constants via variation in heat distributions. And the third was a chemical explanation for how one could get heat distribution changes from recombination occurring at the electrode(s) under the surface of the electrolyte. The first two parts have never been challenged. It was only the third part, the proposed chemical mechanism, that was ever challenged, and I rebutted all those challenges.

It is clear from the totality of the data that apparent excess heat producing cells are the exception rather than the rule, so it is clear that some change from normal must occur to allow the apparent signals to develop. The data also show that said state is very unstable. So if you mean signals that come and go by the term 'burst', there is no issue. The only difference that my CCS brings in is that the 'active state' stimulates at the electrode recombination instead of some nuclear reaction. If instead you mean 'heat after death', that is another issue that can either be related to the CCS in that the 'bursts' are determined in a cell with radically altered conditions, which imply radically altered calibration equations, or these 'bursts' may be due to a mathematical problem outlined in my OSTI report if the original Fleischmann-Pons calorimetric methodology is used.

'Blank cells' (I assume you mean cells that do not produce apparent excess heat signals) and dead cells fit right into the CCS proposal. "Good" calibration is immaterial to whether an apparent excess heat signal is observed or not.

>Shanahan had other critics, and I found a rebuttal on many claims, from
>electrolysis to iwamura and mizuno styles...

I mentioned the paper by Szpack, et al, the one by Storms, and the one by the 10 authors. I know of no others. If you do please let me know. Those three papers do not successfully address my criticisms.

>What I would like is detailed rebuttal, references, but als detaile
>recognition of problems ...

What I gave above was detailed history of my criticisms and the responses to them, but not the technical explanation of the criticisms themselves. Briefly, every calorimeter uses calibration constants and equations to adjust real-world data to an input=output condition. Raw output is always slightly lower than input due to thermal losses down thermal boundary penetrations like temperature sensor leads and power input leads. (There is no perfect calorimeter. There are always some losses, the only question is whether the losses are *significant*. To determine that, one needs to accurately know the error magnitudes on all experimental parameters and propagate these errors through to the final numbers, such as COP or total excess heat, etc.)

The calibration constants are experimental parameters that are dependent on the specifics of the given experiment. Thus, one must be assured the current calibration constants are applicable to the unknown's experimental run. In the Storms' experimental data, the data itself showed that the calibrations constants would vary slightly calibration run-to-calibration run and with the calibration method. My reanalysis of the Storms data simply showed that this could well be occurring during the experimental phase of the work as well, in a systematic fashion that suggested an unrecognized chemistry was the cause. This immediately causes the "new" requirement that calibration data and the variation in it be addressed before true excess heat is concluded. (In theory, this requirement was always there, but most (all?) CF researchers ignored it.) To date, no CF researcher has published such information, so it is impossible to know if any true excess heat signals have ever been detected. The only thing that can be said with certainty is that the 780 mW excess heat signals reported by Storms from Pt electrodes is likely an artifact. Further, if a CF author does not publish the calibration equation(s) used with the calibration constants, it is impossible to assess whether any detected signals are real, and if you can't do that, the publication is useless and no conclusions can be drawn from it.

It is educational to consider what would have happened if, in 1989 or 1990, these concerns were pointed out to Fleischmann and Pons. Given that their nuclear emissions data was found to be flawed, I suspect that they would never had claimed they made excess heat. And then we wouldn't have ever seen the whole wild CF story that we have today.

If you have any questions, feel free to write. I do reserve the right to not respond if I so choose.

Kirk Shanahan

SRO-2016-00952-F
Responsive Document 032

LANA Characterization Project Funding
Kirk Shanahan Bob Snyder
Dave Babineau

08/14/2015 09:23 AM

Hi Bob,

I discussed this with Dave and he asked me to send you an email about it...

The speedchart I have been using for the LANA Extreme Operating Conditions study (07OGTLCHLM - Char LANA Mat'l) is at 99%, but I just submitted 11 samples to ADS for analysis which I used that speedchart to fund, so the code needs more money. I am not going to be able to do the modeling project (07OGTLMMHI - Modeling Hydro Isotherms) this year as I need to finish up the LANA and PseudoSeebeck projects instead, so can we move the $67,500 from that code to the LANA characterization code to wrap up the effort?

Kirk

SRO-2016-00952-F
Responsive Document 033

Re: LANA Characterization Project Funding
Kirk Shanahan   Bob Snyder   08/17/2015 08:26 AM
Beth Calloway, Dave Babineau

Ditto!

Dave Babineau   From: Dave Babineau/SRNL/Srs To: Bob Snyde...   08/17/2015 06:58:48 AM

From: Dave Babineau/SRNL/Srs
To: Bob Snyder/SRNS/Srs@srs
Cc: Kirk Shanahan/SRNL/Srs@srs, Beth Calloway/SRNS/Srs@srs
Date: 08/17/2015 06:58 AM
Subject: Re: LANA Characterization Project Funding

Thanks Bob.

Sent from my iPad

On Aug 17, 2015, at 6:50 AM, Bob Snyder <Bob.Snyder@srs.gov> wrote:

> Beth
> Please make the moves Kirk has requested. Please let me know when you have completed this task
> Thanks
> Bob
>
> Sent from my iPhone
>
> On Aug 14, 2015, at 9:23 AM, Kirk Shanahan <kirk.shanahan@srnl.doe.gov> wrote:
>
>> Hi Bob,
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>>
>> Kirk

SRO-2016-00952-F
Responsive Document 034

Fw: LANA Characterization Project Funding
Kirk Shanahan Karen Graves 08/24/2015 08:32 AM

The originating memo on this email chain has the relevant speedcharts for transferring money to the LANA Characterization account...

----- Forwarded by Kirk Shanahan/SRNL/Srs on 08/24/2015 08:31 AM -----

From:	Kirk Shanahan/SRNL/Srs
To:	Bob Snyder/SRNS/Srs@Srs
Cc:	Beth Calloway/SRNS/Srs@Srs, Dave Babineau/SRNL/Srs@Srs
Date:	08/17/2015 08:26 AM
Subject:	Re: LANA Characterization Project Funding

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To:	Bob Snyder/SRNS/Srs@srs
Cc:	Kirk Shanahan/SRNL/Srs@srs, Beth Calloway/SRNS/Srs@srs
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07OGTLMMHI - Modeling Hydro Isotherms) this year as I need to finish up the LANA and PseudoSeebeck projects instead, so can we move the $67,500 from that code to the LANA characterization code to wrap up the effort?

Kirk

SRO-2016-00952-F
Responsive Document 035

Re: Fw: LANA Characterization Project Funding
Karen Graves Kirk Shanahan 08/24/2015 09:01 AM

This message has been replied to.

understand... sent email to Tritium... waiting on calloway to update and open... go ahead and charge time... it will clear once the code is open

Karen Graves
SRNL / Tritium Financial Liaison
773-41A, Rm. 129
725-6940

Kirk Shanahan The originating memo on this email chain has th... 08/24/2015 08:32:51 AM

From: Kirk Shanahan/SRNL/Srs
To: Karen Graves/SRNL/Srs@Srs
Date: 08/24/2015 08:32 AM
Subject: Fw: LANA Characterization Project Funding

The originating memo on this email chain has the relevant speedcharts for transferring money to the LANA Characterization account...

----- Forwarded by Kirk Shanahan/SRNL/Srs on 08/24/2015 08:31 AM -----

From: Kirk Shanahan/SRNL/Srs
To: Bob Snyder/SRNS/Srs@Srs
Cc: Beth Calloway/SRNS/Srs@Srs, Dave Babineau/SRNL/Srs@Srs
Date: 08/17/2015 08:26 AM
Subject: Re: LANA Characterization Project Funding

Ditto!

Dave Babineau From: Dave Babineau/SRNL/Srs To: Bob Snyde... 08/17/2015 06:58:48 AM

SRO-2016-00952-F
Responsive Document 036

Pian
Kirk Shanahan to Kirk Shanahan 03/24/2016 10:03 AM

1 attachment

kdor-8p-ovyZQCo87jLY7Gbcj4mhICJNPEuzcaS0VLo=