

# User talk:Kirk shanahan

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## Welcome to Wikipedia!!!

Hello **Kirk shanahan**! Welcome to Wikipedia! Thank you for your contributions. If you decide that you need help, check out Wikipedia:Where to ask a question, ask me on my talk page, or place  {{helpme}} on your talk page and someone will show up shortly to answer your questions. Please remember to sign your name on talk pages using four

**tildes (~~~~); this will automatically produce your name and the date. Below are some recommended guidelines to facilitate your involvement. Happy Editing! Kukini 15:36, 24 April 2006 (UTC)**

### Getting Started

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## Cold fusion question

I wanted to run past you what I think is the situation. You and Edmund Storms have been engaged in a to-and-fro controversy. And Storms has had the most recent word in the controversy, but that does not mean that he will have the last word. Have I understood correctly? If I have, can I suggest you look up the verifiability policy. Wikipedia is looking for verifiability, not truth. That may be why findings you can see are shot through with holes are still usable in the encyclopedia. Quoting Ignore All Rules didn't help you, but there may still be more to say about how fringe and mainstream science are represented in this and other articles. Has Storms' book been reviewed by many scientists yet? Itsmejudith (talk) 16:48, 3 October 2008 (UTC)

Ok, what is the situation... I was asked to add a counterpoint view to the CF article. I tried to do that, but I am being shot down by two guys. The first is Pcarbon, who is a CF 'insider'. He is mentioned favorably several times in the Vortex mailing list, and has published a satirical letter in New Energy Times, a proCF magazine put out by Steve Krivit, who has also contributed to the CF article. The second is Kevin Baas. I know nothing about him except what has been posted here. He has some problem with OR and RS rules, but he never gets specific, except to say that Pcarbon has done so, but I disagree with Pcarbon's assessment, so Kevin's continued support needs to be explained.

Ed Storms colleted data and published it with a CF interpretation in the Proceedings of ICCF8 in 2000. I

submitted an article reanalyzing that data in a nonCF fashion in 2000 to a journal, but was shot down in peer review by two CFers who pulled the same kind of tricks we see with Pcarbon (one being specifically Storms). The editor was new, and just went with the straight vote, so I sent the paper to another journal and got it published immediately (which was in 2002), because the Editor was one of the 3 reviewers at the other journal (the one who was favorable to publication), and had seen all the silliness and didn't want that going on in his journal. 4 years later, Storms published a comment paper on my reanalysis, and I replied in standard back-to-back publication fashion (that was 2006). (In 2005 I had a separate publication, responding to a derogatory comment in a different paper that was not Storms-authored, but was Fleishmann-authored.) I have recently obtained the 2007 Storms book and read it and noted that he does not consider my final 2006 publication at all, even though he was commenting on it during review. This has also been noted in one review by Prof. Dieter Britz, published in issue 27 of New Energy Times. As far as I know, that is the only unbiased book review of Storms' book, but I may simply be ignorant. I know several CFers have positively commented on it. To be fair, it lists a wealth of references, but omits my final publication and those of Clarke that bring the calorimetry and He measurements into direct question, and is highly biased towards the 'reality' of cold fusion. However, by not mentioning my last publication (as well as Clarke's), the book is clearly heavily-biased in a very negative way, i.e. no one in their right mind would blindly accept the conclusions inside. But it certainly is verifiable... (BTW - while it is a very questionable book, I am not lobbying for its removal, just the ability to point out major flaws to the Wiki reader.)

As far as having the last word, that isn't a scientific concept. There is never a last word. There is reaching a level of understanding that the debate ceases and the applications begin. However, to reach that level requires conforming to the scientific method and getting the replicatable results that allows one to move on. That hasn't happened in CF yet.

With regards to 'verifiability, not truth', I think you actually mean 'verifiability, not Truth'. We are not going to get to the Truth in the pages of Wikipedia. But do you want lies instead? I would hope not. So you would actually be after some level of truth. The CF article, before my advent, did not have a serious listing of the criticisms of the field, i.e. the reasons the majority of scientists think that CF is bad science. I tried to add that. The discussion in Talk: Cold Fusion has centered on 3 paragraphs so far (imagine the mess if we got tho the rest of the 30 paragraphs or so I wrote.) The edits were block deletion of my added material with specific justification based on OR and RS policies. The truth of the criticisms of the field that I present are being suppressed by what I think are marginal and biased applications of those Wiki policies. For example, strictly as an example of the level of effort required (and stated thusly), I cited a self-published paper by an author who has been an active researcher in the CF field for many years. That was disallowed by Pcarbon, but I see no problem with illustrating for the interested Wiki reader what it takes to address the contamination issue. It demonstrates clearly the difference between a serious search and report vs token work, which is the only point I was trying to make. The bottom line of all this is that no valid criticisms are being allowed into the article. That seems to me to violate NPOV. I could write more on this but I won't at this time.

I cited IAR, partly as a joke, but partly because of the biased way that the OR and RS and verifiability policies were being used. This case presents an interesting dilemma to the Wiki owners. Your policies specifically mention that they were formulated to prevent crank physics from being added into articles (presumably mainline articles). But in this case, the article is being written by the 'cranks' or fringe scientists, while the mainline researcher (me) is being barred from editing successfully. I think Wiki may need to reconsider its policies in this case. I would suggest a rule for controversial topics where the proponents of one side cannot edit the other side's comments. That might not need to apply to a basic historical fact section where things are easily verifiable. The sections written by proponents could be edited by neutral parties for policy violations and the like, and there would be no restrictions on talk page discussions, etc. I mean if I were as outlandish as Pcarbon, I could have deleted lots of the other sections

of the article, and just kept going until my opponent gave up or I was banned.

The bottom line is bias. Will Wiki accept a highly biased article in order to conform to the letter of its rules, or will Wiki go for the spirit, which is to prevent abuse and argument, yet provide valuable insight on a variety of topics to its users? Kirk shanahan (talk) 18:30, 3 October 2008 (UTC)

Kirk, are you considering publishing a review of the 2007 Storms book? I would think you'd have a lot of journals more than willing to publish a paper on it. - Dan Dank55 (send/receive) 18:46, 3 October 2008 (UTC)

No, not a book review. That has already been done by Dieter Britz, issue 27 of New Energy Times has 'published' it. I'm thinking about a review article on the status of 'cold fusion', given that its proponents refuse to fairly and accurately deal with the outstanding criticisms, but that is a lot work that I'm not sure I have the time for. I've already wasted a lot on the Wiki article. Kirk shanahan (talk) 11:56, 6 October 2008 (UTC)

Also, "I replied in standard back-to-back publication fashion (that was 2006)"...do you have a journal name and date for that? That would help a lot. - Dan Dank55 (send/receive) 00:22, 4 October 2008 (UTC)

Despite its reported bias, our article does have this information: Shanahan, Kirk (2006), "Reply to "Comment on papers by K. Shanahan that propose to explain anomalous heat generated by cold fusion", E. Storms, Thermochim. Acta, 2006", Thermochimica Acta 441(2): 210-214 .

Which was added by me, but is now not adequately described. Kirk shanahan (talk) 11:56, 6 October 2008 (UTC)

Your work was already cited in the version before your edits ([http://en.wikipedia.org/w/index.php?title=Cold\\_fusion&oldid=237878109](http://en.wikipedia.org/w/index.php?title=Cold_fusion&oldid=237878109)) , and further presented in details in the Calorimetry in cold fusion experiments subarticle. In my view, this is the proper way to do it in view of the limited notability of your work. With your proposal, the article devoted more space to your work than to the 2004 DOE report, which is much more notable and representative of mainstream science. See WP:DUE guideline. Pcarbonn (talk) 14:39, 6 October 2008 (UTC)

For the record: My 1st post was July 10 to the Talk page, and I point out there that only one reference to my work is cited, with my name misspelled. On July 22, 2008, I added 2 more refs to my work and corrected my name. While the text discussing these refs has changed considerably since then, I added the refs. I haven't checked the history of the Calorimetry in cold fusion experiments subarticle, but my recollection is that I added the discussion of my work there too. Someone else can verify that. Kirk shanahan (talk) 15:23, 6 October 2008 (UTC)

Yes, and I have thanked you ([http://en.wikipedia.org/w/index.php?title=Talk%3ACold\\_fusion&diff=241846551&oldid=241842222](http://en.wikipedia.org/w/index.php?title=Talk%3ACold_fusion&diff=241846551&oldid=241842222)) for your undisputed contributions to the article. So, we agree that your work was properly cited before your disputed

edits on Sept 17, and that it still is. So, I repeat: despite its reported bias, our article does have this information. Pcarbonn (talk) 15:37, 6 October 2008 (UTC)

Thanks very much for the detailed reply. there's stuff I need to mull over. Just for now, to let you know, I only have a basic scientific literacy (O Level physics and biology) but I am a social scientist reasonably well acquainted with epistemology and philosophy of science; I also use applied stats in my work. On WP I respond to the reliable sources noticeboard and I argued vigorously for Storms' book to be considered as a reliable source simply because it is from a mainstream academic publisher. I tend to take quite a simplistic line on sourcing because I'm keen on consistency across the encyclopedia, but there are other more openly "pro-science" editors who might argue differently. A note at the fringe theories noticeboard might be useful at this stage. Itsmejudith (talk) 22:51, 3 October 2008 (UTC)

For the record, I am all for including whatever reliable references would be helpful to the article. However, you have to understand that by 'reliable' I mean 'containing useful information on the subject'. That would include many things Pcarbon doesn't want to include. For example, all the ICCF conference proceedings are 'reliable' in my definition, but that is not to say they are unbiased and empty of errors. Storms' book is a clear example of that. He fails to quote the outstanding criticisms offered by myself and Clarke, while picking and choosing to give an appearance that he has. In science, there is no totally reliable source. One always has to keep both eyes open. For Wiki, the readers should get an accurate picture of the claims and counter-claims, including those obvious to people in the field. Kirk shanahan (talk)

BTW judith, I wanted to make sure you understand that the Storms book issue really hasn't been discussed yet. The enormous discussion that occurred on the Talk page was focused on the nuclear transmutation subsection of what I wrote. There are actually 3 major and a couple of minor subsections (threads) in the Criticisms section, all of which were gutted. We just started with the transmutation one. The issue with the thread on calorimetry is unclear to me. It was sourced adequately, and just tried to lay out the criticism and the 'state of the literature' (which is that Storms' had the most recent word, but that he ignored my most recent word when he did, which completely changes the conclusions one would reach). Perhaps it could have been written more clearly, perhaps with less detail, but again, that's no reason to gut the section. Kirk shanahan (talk) 12:47, 6 October 2008 (UTC)

Dr Shanahan said : "But do you want lies instead? I would hope not." Indeed we don't want lies. If there is any lie in the article, please tell us precisely where, so that we can fix it. Pcarbonn (talk) 11:10, 4 October 2008 (UTC)

For those following this, it is difficult to point 'precisely' to lies by omission, but in science omitting crucial facts is not kosher. Kirk shanahan (talk) 11:56, 6 October 2008 (UTC)

Thanks for your replies. WP has its own definition of WP:RS reliable sources. It has to cut across all subject areas, but there are also subject-specific guidelines. It may well be that this article throws up some special issues that should be addressed on the talk page of that policy. If you think so, you could raise them, or I could do so for you. Your point about "omitting crucial facts" is an interesting one. WP tends to see article development as an incremental one. For example I start an article on a writer. Someone else adds the date of birth and death.

Someone else adds a list of books, and so on. That may not work so well with articles on controversial topics. Having said that, under WP:WEIGHT articles have to give due weight to notable points of view on a topic. You might want to read and reflect on these policies and their implications for the encyclopedia. If you don't have time to do so, you may want to have editors as advocates for your arguments, and I think a message on the fringe theories noticeboard would help. Or alert the Physics Wikiproject. Itsmejudith (talk) 15:03, 8 October 2008 (UTC)

Is anyone suggesting that Storms book would not be reliable ? Here is what Dr. Britz said in his book review published in Journal of Scientific Exploration (cited by New Energy Times (<http://www.newenergytimes.com/news/2008/NET27.htm#pubs>) ): "[Dr. Britz] remains agnostic with respect to cold fusion because even a thorough worker like Storms has not succeeded in demonstrating the effect at will. [...] The term "enthusiast" applies to Storms. [...] So, the book is not neutral on the subject. Nevertheless, these weaknesses are comparatively minor and do not detract from the major message of the book, the rather solid experimental evidence of some exotic process taking place, from a careful and self-critical researcher. [...] The Storms book certainly is recommended reading, for both skeptics and proponents. " Pcarbonn (talk) 15:43, 8 October 2008 (UTC)

I don't think anyone's suggesting that at this point. I have argued consistently that it is reliable on the basis that it emanates from a mainstream academic publisher. Everyone must remember though that I say that as someone interested in consistency of sourcing policy across the encyclopedia. If someone wants to argue that Storms book is an exception then I will consider the argument carefully. The *Journal of Scientific Exploration* on the other hand is *not* reliable (in WP terms), and I don't think *New Energy Times* is either, so there is no point in citing this review in support of the book.

May I add Journal of New Energy to the list. It was a 'journal' published by Hal Fox, a well-known CF enthusiast over about a 5 year period. It claims to be peer-reviewed, but if you examine the TOCs, there are no mainstream articles at all. It's all fringe science (possibly even the p-word type). With no mainstreamers participating, it is a group 'self-published' journal. JNE is cited in the article, so I'm asking if there is a procedure to disqualify it. (see <http://www.padrak.com/ine/PRODUCTS.html> to examine the TOCs.) Kirk shanahan (talk) 18:17, 8 October 2008 (UTC)

What we seem to have here is a case where some recent findings are not reflected in the most recent source that we use. This is something that WP warns may happen. We avoid being influenced by recentism. Perhaps that was because of the publisher's deadline for the book, or perhaps it was lack of rigour on Storms' part. As editors we have no way of knowing, although I guess that Kirk has his own view on the point. I think in this case it would be appropriate simply to say that Storms did not mention Shanahan's 2006 paper. Or we could explain Shanahan 2006 after we discuss Storms 2007. Itsmejudith (talk) 16:59, 8 October 2008 (UTC)

Storms and I were discussing the issues beginning in early 2000. After a couple of years, we mutually gave up as each saw the other as intractable. You can check [sci.physics.fusion](http://sci.physics.fusion) for some postings of Storms' comments that I made with his permission during that timeframe. Then in 2005, up

comes the Storms' comment paper. Unfortunately, it contained EXACTLY the same points we had argued to a standstill over in the 2002 timeframe. So, I wrote up all my comments from then and published them as a rebuttal to the comment. In 2006, when Storms had to be writing/editing/proofing his book, he was fully aware of all the publications and issues. He simply chose to disregard the 'tough' ones in his writing. It is inconceivable that he didn't know. He also was certainly appraised of what I was writing during the review process, as that is standard journal policy, but I can not swear that is true, as that would have been through the journal editor's actions, not mine.

Also remember he 'neglected' to mention the adverse Clarke publication I reference. That was published in 2003 as part of a series of studies Clarke did with SRI (McKubre). Storms was certainly aware of that, as he does cite one of the 4 publications arising from that collaboration. Note that this IS 'lack of rigour' on Storms' part, but most importantly, it severely bias the conclusions not to have considered these articles.

Note again that I am more lenient in my sourcing than Wiki seems to be. For example, I have no real problem with using Proceedings, yet Pcarbon is adamantly against it. Likewise for the book. It has some serious flaws, but is useful. Kirk shanahan (talk) 18:17, 8 October 2008 (UTC)

I'm curious why you say that 'Journal of Scientific Exploration' is not reliable for a book review per WP terms. Could you elaborate ? Pcarbonn (talk) 17:13, 8 October 2008 (UTC)

## Writing about oneself on wikipedia

Dr. Shanahan, you may want to check the guideline on autobiography. While not strictly applicable to the case here, it encourages caution when writing about oneself. For example, it says : "It is difficult to write neutrally and objectively about oneself. You should generally let others do the writing." Pcarbonn (talk) 13:51, 6 October 2008 (UTC)

I am still at a loss as to what you are talking about. Could you please explain your implicitly derogatory comment? Kirk shanahan (talk) 18:22, 8 October 2008 (UTC)

I'm saying that you should be particularly careful when you contribute to articles that talk about your own work, according to Wikipedia guidelines. This includes respecting the opinion of other editors, and assuming their good faith when they comment on the notability of your work. Pcarbonn (talk) 20:40, 8 October 2008 (UTC)

Good thing I'm always careful, didn't denigrate anyone's opinions or actions (just complained about them when I thought they were unfair and biased), and that it "not strictly applicable to the case here". Kirk shanahan (talk) 16:03, 9 October 2008 (UTC)

Please allow me to disagree with what you just said. However, I take your statement as a sign of good will for the future. Pcarbonn (talk) 20:03, 9 October 2008 (UTC)

Dr. Shanahan, thank you for your last contributions. They improve the article. Pcarbonn (talk) 07:31, 7

October 2008 (UTC)

Dear P, your contributions to the cold fusion article have been invaluable to pushing back the frontiers of science, they will stimulate generations of scientists-to-be to develop the field of cold fusion engineering, becoming the next generations of cold fusioners. Your stellar example will demonstrate to all the children and adults alike the True Path of science, and we all will benefit from it. (Have you caught my drift yet...)

Someone had to write the article I guess. Pcarbon is to be recognized for that. Unfortunately, it was completely biased to the supposed reality of cold fusion. He only included answered criticisms, leading the reader to suppose no others existed. When I tried to correct that, he suppressed it by distorting every Wiki policy he could, and by utilizing the anarchical feature of Wiki that allows anyone to delete anything. So, all in all, my position on Pcarbon is neutral-to-negative with regards to his contributions to the article. He wrote some useful parts, but he suppressed others. Bye. Kirk shanahan (talk)

## Request for arbitration

I have made a motion that you be added to an existing arbitration case as a named party. See Wikipedia:Requests for arbitration/Cold fusion/Workshop#Request to add Kirk shanahan as a party. Please comment there. Jehochman <sup>Talk</sup> 20:51, 15 November 2008 (UTC)

## Weighing validity of opposition

copied from Talk:Cold\_fusion#Weighing\_validity\_of\_opposition --Enric Naval (talk) 20:35, 10 December 2008 (UTC)

As the person originally posing this question, I ask that it take place on Talk:Cold fusion. 69.228.201.246 (talk) 21:59, 10 December 2008 (UTC)

Dr. Shanahan, I want to repeat a question to you which you may have missed above, based on your reply to subsequent comments. You said that your method of theoretical opposition to cold fusion is potentially applicable to forms of which do not involve electrolysis. You said your "calibration constant shift" method includes to "reverse engineer the constants required to force Storms' data to produce 0 excess power." I asked if that means starting with the assumption that there is no excess power, and then designing a general theoretical argument in support of that assumption. You said yes, but "it also includes evaluating that argument and reanalysis for credibility." Again, how do you select among a set of arguments in support of a selected hypothesis for credibility? 69.228.201.246 (talk) 19:08, 10 December 2008 (UTC)

Shanahan's assertions about experiments are not in evidence. See: Storms, E., Comment on papers by K. Shanahan that propose to explain anomalous heat generated by cold fusion. *Thermochim. Acta*, 2006. 441: p. 207-209.

Don't forget to look at the paper immediately following that one. Kirk shanahan (**talk**) 20:09, 10 December 2008 (UTC)

I assume you mean: Shanahan, K., A Possible Calorimetric Error in Heavy Water Electrolysis on Platinum. *Thermochim. Acta*, 2002. 387(2): p. 95-101.

<http://lenr-canr.org/acrobat/ShanahanKaposiblec.pdf>

See also: K.L. Shanahan, Comments on Thermal behavior of polarized Pd/D electrodes prepared by co-deposition, *Thermochim. Acta* 428 (2005) 207. We do not have this one, regrettably.

Ah, ha. It is here:

<http://sti.srs.gov/fulltext/ms2004528/ms2004528.pdf>

See also: Shanahan, Kirk (2006), "Reply to 'Comment on papers by K. Shanahan that propose to explain anomalous heat generated by cold fusion', E. Storms, *Thermochim. Acta* (2005)" (PDF), *Thermochimica Acta* 441 (2): 210-214

<http://sti.srs.gov/fulltext/2005/ms2005556.pdf>

Will add the latter to the database.

- Jed Rothwell —Preceding unsigned comment added by 68.219.54.221 (talk) 20:16, 10 December 2008 (UTC)

<http://lenr-canr.org/acrobat/StormsEcommentonp.pdf>

- Jed Rothwell, Librarian, LENR-CANR.org —Preceding unsigned comment added by 68.219.54.221 (talk) 19:50, 10 December 2008 (UTC)

Firstly, I'm not sure all of this discussion is appreciated by the others using this talk page, who have already complained about this kind of discussion being OR, but I will try to keep it short so as to minimize the impact. Secondly, I'm not sure I am quite following your question, but I will try to answer it as best I can.

I determine the credibility of a 'reversed engineered' (RE) hypothesis based upon its conformance to any available experimental data and to general expectations based upon general quality control knowledge. In the case of my reanalysis, actual reproducibility numbers were available from Storms' own report. He reported both the calibrations constants obtained by eletrolytic calibration and Joule heating calibration, AND, he reported different calibration constants obtained from different electrolytic calibrations done in different time frames. These individual points are then assumed to roughly represent a 1 sigma span for comparison to the RE constants (they were on the order of 1.5%). My results were that changes of 1-3% were needed to zero out the apparent excess heat. That compares directly and favorably to that reported by Storms. Furthermore, based on my experience in chemical laboratory statistical process control, I know that biases of 1% and RSD's of 1% are obtainable with effort, so again the RE results compare favorably to general expectation. Thus the CCS mechanism is equally accurate to the CF mechanism, but the CCS mechanism does not require new revolutionary physics, which makes it the preferred explanation for a mainline scientist. If I apply this RE method to another technique, I would have to make these same kind of considerations to try to decide if the RE approach provided reasonable results there. Kirk shanahan (talk) 20:09, 10 December 2008 (UTC)

Papers now added. A clerical error on my part caused them not to show up previously. Let me know if there are others. And please give me permission to upload the draft versions. Pretty please!

Contact directly at [JedRothwell@gmail.com](mailto:JedRothwell@gmail.com) (new ISP).

- Jed Rothwell —Preceding unsigned comment added by 68.219.54.221 (talk) 21:30, 10 December 2008 (UTC)

It is not my prerogative to give anyone permission. When I write papers, the copyright permissions are issued by my employer, not me, per my employment agreement. My employer executes a non-standard copyright agreement with journals also. You should check with OSTI about the right to distribute the papers you get from there, and with Thermochemica Acta about the others. I am out of the loop. I will say that the one manuscript I supplied to you of my first paper was submitted to you before it was entered into the OSTI database, and it was submitted to prove one point, that I had the paper written and 'in the works' in October of 2000. Kirk shanahan (talk) 13:35, 11 December 2008 (UTC)

## Weighing validity of opposition - lost comments

I noted in some diffs that some comments seem to have been lost. I have copied them here to be able to respond:

"The work that you have published in the peer-reviewed literature does not match Wikipedia's definition of forbidden original research, because it was published in a secondary source. Improvements to the article are the purpose of discussion here, and helping everyone to understand your sources should lead to improvements in the article.

I am trying to understand your "calibration constant shift" technique, which you say is potentially applicable to non-electrolytic cold fusion. We have established that you start by assuming a hypothesis of no excess heat, and then you design a general theoretical argument in support of that hypothesis. You then evaluate your argument's credibility based on conformance to available data and your expectations about quality control. If you were to later learn that there was data which was not consistent with your argument, would that invalidate it? 69.228.201.246 (talk) 21:57, 10 December 2008 (UTC)"

My argument so to speak is nothing but a mathematical demonstration of the analytical chemistry 'Golden Rule' (of which there are many) that "You can't calibrate an unstable system". As such, what I have done is show that a CCS (which occurs because the system has changed) can explain Storms' (2002 paper) and Szpak, Fleischmann, et al's (2005 paper) results. That is a done deal, and won't change. However, it is always possible that new data will shed more light on the problem and point elsewhere than a CCS, even to the point of proving CF. But, at the same time, since it has been shown twice in cases where the calibration data was available that a CCS has the potential to explain the results, every excess heat claim must show their calibration results in order to eliminate a possible CCS. To put it another way, I have delineated a systematic error that seems to explain a lot, and any claim to excess heat must prove that a CCS is not active, otherwise the CCS remains a preferred explanation (since it is a conservative one). When you go to different experimental protocols like the D2 flow through membranes, the error bar on their results is unknown at this time. But as long as they are calibrating, a CCS could be present, so again, they have to supply the data to be able to refute its presence if they want their results accepted. Kirk shanahan (talk) 13:25, 11 December 2008 (UTC)

When applied to electrolysis, does your calibration constant shift theory imply a reduction in the volume of evolved output gases? If so, how much? 69.228.201.246 (talk) 17:02, 11 December 2008 (UTC)

Again, potentially. To explain, my 'theory' has 3 levels. The first is the simple CCS, i.e. by using slightly different calibration constants, the apparent excess heat signals can be eliminated. The 2nd level is a proposal of one of possibly many ways to get a CCS. That specifically is to move heat from a zone of the calorimeter where it is detected less efficiently to a zone of higher heat detection efficiency. The zone of lower efficiency is most likely where heat losses are greater, which is most likely where heat loss paths exist, such as penetrations of power leads and thermocouples, etc.

That normally means the gas head space. In open cells, the gas just leaves this area and exits the cell. In closed cells, the recombination catalyst is found there. The first and second levels are nothing but algebra, nothing complicated there, and no one has challenged any of that. But I also proposed a third level, which was a physical/chemical mechanism that I felt explained how a CCS could be obtained AND also explained many other observations on the functioning of P&F type cells. That mechanism is what was attacked by Szpak, Fleischmann, Storms, etc., in the literature. I feel I answered all criticisms and showed how the critics were making many errors, but you read the papers and decide for yourself.

So, IF my proposed physical/chemical mechanism is correct, one would see a reduction in the volume of evolved gases from an open cell. There would be no noticeable change in the closed cell, with the possible exception of gas space pressure.

The only time gas space pressure in a closed F&P cell has been measured to my knowledge was by McKubre as reported in his 1998 EPRI report. Unfortunately, the signal is not explained, i.e. calibrated, and the numbers didn't make sense to me. I asked McKubre for help on his cell calibrations twice in 1999, and he declined to help (which may be reasonable given the fact that the data presented in the report was from 1993-4). In my 2005 paper I point out that the Szpak et al paper I am commenting on attempts to measure this by recombining the gases external to the cell/calorimeter, but they end up with 7% MORE water than they should based on their power consumption. The cause of this is proposed to be entrainment of water droplets in the gas stream, which would be another systematic error in these type of measurements.

So the key point is here that if it was measured correctly, evolved gas volume would potentially show that excess recombination was occurring. If you look here <http://home.netcom.com/~storms2/fig1.html> you can see a plot that Ed Storms has posted showing that the parasitic electrochemical recombination reaction drops off as current density increases. But there are two zones of anomalous data in this plot - the fourth point reading left to right and the several points in the .03-.07A region. These points lie above the line that goes through the rest of the data as indicated by the Will model (note that the Will model is not linear and would curve up to fit the Jones data if plotted to that region). Both regions lie about 20% or so above where the Will model would say they should lie, and I contend this is the excess non-electrochemical recombination needed for my 3rd level mechanism. But there are no excess heat data available for these runs and both sets of authors told me they saw no excess heat. So, at this point my physical/chemical mechanism remains untested, but it certainly could be. However, if it can't be proven, that does NOT negate the CCS, just forces us to come up with a new mechanism to get it. Kirk shanahan (talk) 19:05, 11 December 2008 (UTC)

I must repeat my question about the quantity of the evolved gases. Based on your understanding of quality control in calorimetry, what is the chance that those who have said your explanation is in error because of the quantity of evolved gases observed are themselves in error? 69.228.201.246 (talk) 05:47, 12 December 2008 (UTC)

The 2004 publication by Szpak, Mossier-Boss, Miles and Fleischmann contained an attempt to measure the quantity of evolved gases. Given that all these folks are 'experts' in the field, I believe we should assume that their results are the best of the typical. They were using an open cell configuration but they subsequently converted the outflowing gases to water and measured the amount formed. They observed that "the total consumption of D<sub>2</sub>O was 7.7 cm<sup>3</sup> instead of 7.2 cm<sup>3</sup>, assuming 100% Faradaic efficiency, which is within experimental error." In my 2005 comment on their paper, I noted that this is a 7% error (while the CCS is a 1-2% error), and that it is in a positive direction. In other words they 'created' 7% excess water. How could that

happen? The simplest explanation is a process called entrainment (Wiki's page on this is a little lame), which is where one phase of material is carried along in another phase. In this case small water droplets in the gas flow. I believe it is reasonable to assume this process is active in all other cells. So, this means that for an accurate measure of the actual gas flow rate, it must be 'demisted' before being measured. I know of no experimental setup that did that. So I think it is safe to assume an error bar on such measurements of 1 sigma=7%, 3 sigma=21%. That's not very good. In the few other cases where people try to measure this, as I recall, the errors were consistent with this estimate or worse.

In other words, I am saying that a) very few actual measurements of this type exist, and b) they are very error laden, to the extent that they really don't offer any reliable evidence of any kind. That might be a bit strong, but that's where I am at today. Perhaps you can find a study that disproves me. So in relation to your question, based on the above, I would say the chances are near 100% that those who claim they have disproved my CCS mechanism by measuring evolved gases are in error. 192.33.240.30 (talk) 13:56, 12 December 2008 (UTC)

The 2004 publication by Szpak, Mossier-Boss, Miles and Fleischmann (<http://lenr-canr.org/acrobat/SzpakSthermalbeh.pdf>) claims agreement with calculated volumes within "1.0%", not 7% (page 102.) 69.228.201.246 (talk) 21:16, 12 December 2008 (UTC)

The sentence quoted above comes from page 105, section 4.1, 2nd paragraph of that report. 0.5 cc (7.7-7.2) is 6.94% of 7.2. Seems we have found a logical inconsistency in their paper. Kirk shanahan (talk) 14:09, 13 December 2008 (UTC)

The 1% figure refers to a reference. I looked it up and could not find the data to support the 1% figure. ~Paul V. Keller 23:39, 18 December 2008 (UTC)

## arbitrary section break

Let's step back a moment. What is the rough difference in the expected volume of evolved gases with and without recombination in a Navy SPAWAR co-deposition cell, holding the observed excess heat constant? Isn't it on the order of several dozen if not multiple orders of magnitude? 69.228.213.202 (talk) 05:45, 20 December 2008 (UTC)

Years ago I may have calculated this, but I don't recall right now. The answer to your question lies above. They expected 7.2 cc of water (that's 0% recomb.) and got 7.7, that's a 7% excess in the wrong direction to prove excess recombination is occurring (100% recomb. would be 0 cc water). Based on the excess heat signal, I recall I estimated about 20% or so recombination had to be occurring, which made the total 'error' in their water measurement more like 25%. If you want gas volumes, convert the liquid water formed to gas at STP. Kirk shanahan (talk) 19:45, 22 December 2008 (UTC)

Melvin Miles, who made the measurements, told me that "the initial volume was 91 +/- 1 cc, the final volume was 83.3 cc, thus 7.7 +/- 1 cc was consumed versus 7.2 cc calculated by Faraday's Law. A quick approximate calculation indicates that the volume of D<sub>2</sub>O consumed would have to be smaller by about 5 cc if the excess heat in this experiment were due to recombination. This was NOT observed. The amount of D<sub>2</sub>O consumed by electrolysis is within experimental error (+/- 1 cc) of Faraday's Law. A slightly higher D<sub>2</sub>O consumption is expected experimentally because the D<sub>2</sub>

and O<sub>2</sub> gases bubbling out of D<sub>2</sub>O are always saturated with D<sub>2</sub>O vapor. This is an evaporation effect.... Fleischmann's calorimetric equations have always included this evaporative effect."

GetLinkPrimitiveParams (talk) 16:57, 19 January 2009 (UTC)

It always amuses me how single-minded people can be. IF WE ASSUME NO COLD FUSION, then the excess water in the outflow would be  $\sim 5 + (7.7 - 7.2) = \sim 5.5$ cc. Thus the measured +0.5cc grossly understates the amount of water loss by entrainment, it should be more like 5.5cc. So, we now have two explanations for the observed 7.7cc water loss; cold fusion OR under-the-surface recombination produced excessive entrainment (which is now disconnected from a direct calculation by the complexity of the process of microdroplet formation). Also we note that IF THERE IS NO COLD FUSION, the experimental error on water loss is  $\sim 5$ -6 cc out of 7-8cc, or minimally about 62.5%. Clearly not adequate to base scientific conclusions upon.

So now, you choose. Is there an unexplained revolutionary new phenomenon occurring that will necessitate rewriting physics and chemistry textbooks, or is there an interesting but completely explainable phenomenon happening? It has to be a choice, not a determination, because the two proposed explanations both fit the tidbits of data available. The sensationalist will choose the first choice, the boring, conservative scientist the second. Kirk shanahan (talk) 17:28, 20 January 2009 (UTC)

What do you mean by "entrainment"? GetLinkPrimitiveParams (talk) 20:56, 20 January 2009 (UTC)

Two-phase flow, i.e. here, liquid microdroplets carried along in a flowing gas stream. Check any chemical engineering text. Kirk shanahan (talk) 21:41, 20 January 2009 (UTC)

What is the evidence for that, and how is it any different from typical evaporation? GetLinkPrimitiveParams (talk) 06:27, 21 January 2009 (UTC)

See above paragraph beginning "It always amuses me...". Also, read any chemical engineering text. Or, think about it the next time you flush a toilet. You might also want to look at the thread on Vortex found here

<http://www.mail-archive.com/vortex-l@eskimo.com/msg29656.html>

especially the one here:

<http://www.mail-archive.com/vortex-l@eskimo.com/msg29675.html>

{BTW, Jed's comments about evaporation being computable are amusing, as when I used that argument to cast doubt on a psuedoresult produced by Mizuno, Jed wouldn't accept that. It devolved into a discussion of a rat pool party on spf.} Kirk shanahan (talk) 14:20, 21 January 2009 (UTC)

If I am understanding you, you are suggesting that 20% of the evolved gases are recombining. That means that there would have to be 5 cc more water left over. But there isn't; there is less water than predicted. So you explain that by droplet production "entrainment" -- is that right? So your theory is that recombination, which doesn't occur during other forms electrolysis, occurs with D-Pd electrolysis, produces 5 cc more water, and 4.5 cc more water droplet entrainment in addition to the expected amount

of evaporation. And the empiricists haven't embraced this explanation? Do you really think they are incapable of measuring both vapor and droplets? GetLinkPrimitiveParams (talk) 12:22, 22 January 2009 (UTC)

First off, there is less water than predicted LEFT IN THE CELL. This means the LOSS was GREATER THAN PREDICTED. 100% Faradaic Efficiency, i.e. no recombination, predicts 7.2cc loss. So 20% of that would be 1.44cc, not 5 as you suggested.

What I am saying is that, based on input power measurements and known physics/chemistry, a loss of 7.2 cc fluid was expected and 7.7 cc was found. A 0.5cc difference is non-trivial, so saying 'that is within experimental error' simply points out that the person saying it doesn't want to refine his error bars down any further at that time. I do, and as 0.5cc is non-trivial, it should be easily done, especially since there are well-known physical chemical processes (entrainment) that could account for an excess water loss. I also pointed out a report of observed evidence that directly supports the entrainment proposal. Since Miles does not acknowledge entrainment as a viable process, it is clear he has evaluated its impact as 0. This conflicts with the observation that such a process occurs in CF cells, and with the base knowledge of the field of entrainment. If the effect is not quantified, no comment can be made by Miles on its impact. I also say that observed fluid loss is made up of at least 3 components: electrolysis, evaporation, and entrainment. So, I am saying, 'YES, Miles made a mistake'. The actual water loss, being composed of 3 components, is a 'confounded' measurement. The components must be measured separately, not in a lump sum fashion as Miles did. Depending on the degree of entrainment, the major fraction of water loss could be due to it. We don't actually know because no reliable measurement was made. So, I don't know if they are incapable of measuring both vapor and droplets, because they haven't tried!

AND, in my publication on this I point out that the maximum available recombination heat, uncorrected for CCS problems, is about 0.5W, while SMMF measure 0.27W maximally. Thus the 7.2cc water retention in the cell would be  $\sim 1/2$  that. So my memory based 20% figure was low. It was probably more like 50%, or 3.6cc excess retained water in the cell. Is that an outlandish amount to be taken out by entrainment?? I have no idea, it has to be measured, and it isn't. The point is: WE KNOW ENTRAINMENT HAPPENS. If we know it happens and it can impact the numbers we are discussing, IT MUST BE MEASURED before any intelligent discussion can occur, and certainly before any conclusions are drawn.

I would also like to point out a mental error I have made in this discussion, which does not impact the conclusions, but may explain why Miles thinks I don't know what I am talking about as reported in the Vortex message. I had confounded two Szpak et al publications. The 2004 one with Miles and Flesichmann (and Mossier-Boss) is where the numbers come from. In fact they do not indicate how they determined them therein. However, in a lenr-canr version of a 1998 Fusion Technology publication, and open cell with an attached (but outside the calorimeter boundary) recombiner is shown. I assumed they were collecting water thusly instead of measuring water loss in the cell, but the 2004 paper I commented on did not make that clear. Lack of experimental details in publications is an endemic problem in the CF area. Kirk shanahan (talk) 17:41, 22 January 2009 (UTC)

Melvin Miles in email reports that he has measured evaporative and droplet loss and it is simply not as you predict. If recombination were occurring, then there would be more water not less. Instead there is less water, so you have to assume that the entrainment is huge in comparison. I would hope you will contact him and Pam Boss to discuss this, and perhaps tour SPAWAR's codeposition research facilities to observe the experiments in progress as any U.S. citizen may. GetLinkPrimitiveParams (talk) 18:13, 22 January 2009 (UTC)

Sorry for the slow response. I was 'involved' with V.

OK, a) email don't count b) especially when I don't have a copy of it, but c) I think you are referring to a Miles message that was at least partially posted to Vortex, to whit:

"vortex-1 [Vo]:Melvin Miles comments on recombination Jed Rothwell Sat, 03 Jan 2009 14:29:06 -0800

Here is a message from Melvin Miles about recombination.

Subject: Shanahan's misunderstandings of recombination . . . If Shanahan's statements about my NHE co-deposition measurements are typical of his lack of understanding of cold fusion experiments, then I would not give credence to anything he says. For example, I did not convert the outflowing gases to water as Shanahan states.

For my three NHE co-deposition experiments, I simply measured the initial and final volumes of electrolyte to obtain the volume of D2O consumed. For the cell in question (Cell A-2), the initial volume was 91 +/- 1 cc, the final volume was 83.3 cc, thus 7.7 +/- 1 cc was consumed versus 7.2 cc calculated by Faraday's Law. A quick approximate calculation indicates that the volume of D2O consumed would have to be smaller by about 5 cc if the excess heat in this experiment were due to recombination. This was NOT observed. The amount of D2O consumed by electrolysis is within experimental error (+/- 1 cc) of Faraday's Law.

A slightly higher D2O consumption is expected experimentally because the D2 and O 2 gases bubbling out of D2O are always saturated with D2O vapor. This is an evaporation effect and not Shanahan's entrainment. Fleischmann's calorimetric equations have always included this evaporative effect.

Full details of all three co-deposition studies are in my NHE Report, pp. 22-25 (see LENR-CANR.org). There was no correlation of the excess heat to recombination in any of these three cells.

[This refers to <http://www.'the Wiki banned site'.org/acrobat/MilesMnedofinalr.pdf> to which I should add page numbers. - JR]

I hope this helps to discredit Shanahan's false statements about my experiments as well as his statements about cold fusion in general.

Mel"

where Miles does not mention measuring entrainment, and in fact seems to think the idea silly. Which is why I posted a follow-up message to that post, with special reference to the sentence:

"Horace rightfully mentioned electrolyte droplet entrainment, I have witnessed the reality of this phenomenon myself (visible traces of salt remained after letting dry a spoon exposed to the outgoing vapor). "

where Michel Jullian remarks on his direct observation of entrainment. So, since entrainment is a real process, and since it seems to have been observed, it should be considered, but it never is, therefore the water loss measurements of Miles are non-conclusive.

With regards to my visiting them, why would I spend the money and the time to go visit a lab of people who denigrate my work in print and in email without ever discussing anything about it with me first? They have already set the pattern, I'll just follow it.  
Kirk shanahan (talk) 12:57, 27 January 2009 (UTC)

I am certain that the tit-for-tat strategy is incompatible with effective scientific communication. GetLinkPrimitiveParams (talk) 04:05, 1 February 2009 (UTC)

## Jed's rebuttal

Hi Dr. Shanahan. If you don't mind me dumping all this on your talk page, I would like to ask you what is your response to Jed's rebuttal of CCS, quote:

The CCS effect cannot generate palpable heat, or boil water with no input power, or cause a thermoelectric chip to generate enough electricity to turn a small motor. Therefore, it cannot explain cold fusion heat. Perhaps if it were true, it might show that some low level cold fusion heat is an artifact, but I do not know any experts in calorimetry who agree that it is true.

He then goes on:

Shanahan's hypothesis resembles the claim by Jones that all cold fusion heat can be explained as recombination. This cannot be true for several reasons, mainly: many cells use gas instead of electrolysis; many cells include recombiners, so recombination always occurs and is accounted for; most open cells without recombiners include gas flow meters and other methods of accounting for the gas; many cells have produced excess heat far above the limits of recombination (hundreds of times above it). Jones, if correct, could explain only a tiny fraction of all cold fusion results and yet he claims that he can explain them all. He has repeated this countless times and never acknowledged that his explanation cannot possibly apply to most experiments. This is intellectual dishonesty. Shanahan is also intellectually dishonest, or confused, when he refuses to describe how his proposed artifact could cause a thermoelectric generator to turn a motor, or a cell to remain so hot a person cannot touch it for days on end, with no input power. He hypothesis is not an "alternative" unless it can explain these events.

How would you reply to the first quote, and what are your comments on the second quote? Thank you for your time. Dr.K. (logos) 19:25, 11 December 2008 (UTC)

In response to the first quote Jed is completely correct. The premise of the CCS is that there really is no cold fusion heat. The problem is that his claims for 'other' evidence of cold fusion heat are anecdotal and not subject to scientific scrutiny, In other words, he's asserting something that is not true as if it was, and people buy it. In all cases where I have tried to track down the technical basis of such assertions, I cannot confirm the claim.

With regards to the second quote, I have no need to defend Steve Jones, so I will skip to the part where Jed insults me. You may note that what he says is a repeat of the prior quote, so just reread my response to it.. (Jed seems to believe in "If you repeat it enough times it becomes true...")

Seriously though, Jed's attack on Steve Jones demonstrates several typical tactics CFers use. First, they mix significantly different experiments without any proof the same base effect is at work, i.e. F&P type electrolysis vs. gas (I assume he means the D2-through-membrane experiments, but that's not perfectly clear either). When you get an anomaly in one experimental setup, you need compelling evidence to explain it. Once you have that you can potentially see the same underlying phenomenon in another setup, but the generic criticism of CF is that they have no such proof in any experiment. So their lumping all these anomalies together as 'proof' of CF is wrong. Second, Jed deliberately blurs the line between the electrochemical recombination vs. non-electrochemical. Jones (and Hansen) were looking at a parasitic electrochemical reaction, while I talk about simple H2+O2 'burning' occurring in an unexpected place. But the key is that the CFers showed the electrochemical form drops off in importance as current is increased,

and since they typically operate at higher currents, the problem is not important. The CFers hope to take this success and transfer it to the CCS problem (“Shanahan’s hypothesis resembles...”), but they can’t legitimately do that as it is completely different.

Thirdly, as I noted above, the claims being made about what has been shown are grossly inflated. There actually are very few studies where good measurements on gas flow out of the cell were accomplished, and those usually have error bars on them larger than the CCS, so they offer no proof the CCS is not active. If you want to seriously consider Jed’s claims, make sure you pin him down to something you can examine, and don’t buy the exaggerated assertions he continuously makes. Kirk shanahan (talk) 12:46, 12 December 2008 (UTC)

Thank you very much Dr. Shanahan for taking the time to explain this. Your explanation was very informative. I am very interested in your CCS theory as it currently is the main alternative to cold fusion claims. I think that your contributions to the CF article are very important and needed and your CCS theory goes to the heart of the matter, since it focuses on and directly accounts for the phenomenon of excess heat production. If you don't mind I may have a few questions from time to time which I guarantee, from now on, to be substantially shorter than this. I hope I'll see you around and it was a pleasure meeting you. Take care. Tasos (Dr.K. (logos) 17:57, 12 December 2008 (UTC))

No problem. I will hang around for awhile, but eventually I will probably stop following these pages. I'm still a little concerned that what hapened the last time will happen again, namely that all my contributions (which were bundled into a subpage before) will end up disappearing again, but I can't do a daily watch to prevent that. So, we'll see. My email is public info if you go to the sci.physics.fusion Usenet newsgroup, so if I don't respond here you can try that. Kirk shanahan (talk) 18:24, 12 December 2008 (UTC)

Your contributions should remain in the article because they add much needed scientific balance. I will be watching as well. Also thank you very much for the information. It will be a pleasure talking to you again. Tasos (Dr.K. (logos) 19:32, 12 December 2008 (UTC))

## Jed and Cold Fusion

You might find WP:SHUN a useful way of dealing with this guy. Verbal chat 16:54, 12 December 2008 (UTC)

Always good advice for Jed, except that a) it was Dr. K and "69.228.201.246" who asked for a comment here, and b) as I noted previously, I wanted to alert Wiki editors to who Jed is and his tactics. That latter objective has been accomplished. Kirk shanahan (talk) 17:22, 12 December 2008 (UTC)

You might also find it opposed to transparency and the free exchange of data and their interpretation necessary to make any headway in this debate. 69.228.201.246 (talk) 21:20, 12 December 2008 (UTC)

## As simple as the Faraday Efficiency?

Forgive me for getting lazy and asking you to help me cut to the chase. Are these bursts periods of perceived excess energy entirely accountable by postulating that for one reason or another the electrolytic efficiency drops during the periods of anomaly? Would the gas evolution rate settle the point and is there any data? Are the faithful reduced to challenging your mechanism for efficiency loss and pointing to their sketchy nuclear data?

~Paul V. Keller 19:58, 18 December 2008 (UTC)

First off, the Faradaic Efficiency (FE) concern is only applicable to electrolysis cells. In Storms' 2007 book, Table 1, he has 8 pages of excess heat claims from 4 different experiment types. Only two are electrolysis, open and closed cell F&P type cells. FE would not apply to the other two types (which are plasma and flow-through-membranes I think). FE is also not an issue in closed F&P cells, because theoretically recombination is 100%. So we can only be talking about open F&P cells if we are talking FE. In that case, measuring accurately evolved gases would potentially identify the increased recombination in the cell that I claim would cause a CCS BECAUSE it introduces a new, improperly calibrated heat source in the cell. So the CCS is not related to FE, but FE in an open cell can help prove a CCS has occurred, if the mass and heat balances work out. In the other types of apparatus however, a CCS still could occur, leading to apparent excess heat. In a closed F&P cell, I postulated that the place where the recombination location partially changes, causing the CCS, but that is speculation that needs to be proved. That a CCS can explain Storms' 2000 results and Szpak and Fleishmann's 2005 results is unambiguous however. If the calibrations of the other types of apparatus are time variant to some level, a CCS effect can be present. This means the CFers MUST supply calibration stability information to prove the effect is not explainable by a CCS.

Second, the 'faithful' have no way to challenge the CCS. It is simple arithmetic. Their challenges to date have been to the proposed mechanism, which, while I think it is right, is not a necessary component of the CCS. Neither is the idea of a shifted heat distribution exclusively necessary. A shifted heat distribution is one way a CCS could occur, but may not be the only way, especially when you start talking completely different apparatus. The 'true believers' as they are known think they have proven the speculative mechanism wrong (I disagree) but they also think that kills the CCS. It doesn't, but they won't admit that (even though I am sure Ed Storms understands, based on numerous emails). As far as I know, no nuclear data comments on excess heat. Kirk shanahan (talk) 21:26, 18 December 2008 (UTC)

Sometimes it looks to me like you are playing a game of wack-a-mole. I read above that you agree with the concept of dealing with one type of experiment at a time. Right now, I am content just looking at the open cell electrolysis system.

I am not used to your CCF terminology. The way I look at the system, putting aside possible accumulation/generation terms, your main variables seem to be the electrolytic efficiency (and thus flow rate out, wrapping this into recombination), the overall heat transfer coefficient, and the temperature measurement within the system.

I gather from what you have written above that temperature gradients within the vessel are large, and that the temperature of the outgas can be quite different from the electrode. Is that verifiably true? Is it really that hard to keep the gradients within the system small? Are the gradients big only if you have localized recombination? Seems these would be easy to check using multiple thermocouples.

With respect to the heat transfer coefficient, I wonder about a couple of things. What is the rate limiting step, does the coefficient vary much, and have you shown that multiple steady states are possible?

As far as the electrolytic efficiency, I think you already answered. I find this the easiest explanation if there is truly an anomaly and am surprised those claiming reproducible sporadic effect have not pinned it down. I would think you could make something based on the concept of a positive displacement pump, hook it up to a counter, and easily check the hypothesis by comparing the flow data to the temperature data. At the very least it would seem a good way to run a more convincing experiment. I would think you could buy a suitable pump and control it with pressure feedback.

~Paul V. Keller 00:23, 19 December 2008 (UTC)

On "Whack-a-Mole" - Yes, especially when dealing with the CFers. We (skeptics) 'whack' a particular claim, and another pops up, 'whack', another pops up, 'whack', ... After a while, you notice it's the same moles though that keep popping back up, i.e. the CFers never seem to learn. For example the CCS. Anyone can understand it. If you must translate a measurement via a calibration equation, using the wrong equation gives the wrong results. Why would they do that? Because their system has slight instabilities in it and has shifted between the time they calibrated and the time they measured. Pretty simple, yet they keep on saying it can't be. Of course it can. All I have done is show them that their 'noise' level in their experiments is 1-5% in the best cases, which is typical of a good-to-great chemical method.

On open cell electrolysis and gas evolution: In principle, if you accurately measure the off gas, however you would do that, it should reveal if the FE is <100%. If it is, that might cause a CCS. The CFers refuse to pin it down because they feel they already have. I believe this is incorrect, and that every result they have to date is subject to a conventional explanation (extend this to closed cells too). They disagree, and (key point) don't see the need to do anything about my disagreement, or in fact that of any reviewer they happen to run across who doesn't like what they write. They have lots of friends who tell them they are absolutely correct (all CFers of course). Do you see how the polarization of the field has killed the science? They don't need to respond because they are right, but they can't convince the majority of that.

On temp grads. - Just big enough to give a false signal. That is dependent on apparatus design.

Heat transfer coefficient? I have only talked about the heat capture efficiency of the calorimeter. If it is high, that minimizes the CCS impact, but as long as there are different heat loss pathways, it will be difficult to completely avoid some residual CCS effect, if the heat flow changes. I think I might see what you're getting at and what your problem is. To understand the CCS, you have to step up to a two-zone model of the system at a minimum. What you write above seems to treat the 'system' in the usual, homogeneous way. But that is not how the system is. There are multiple real heat loss pathways, and depending on how the heat generation points are distributed in the cell/calorimeter, differing amounts of heat can be lost, necessitating different calibration constants for each unique distribution. That's what I propose causes the CCS, a change in these heat loss (or capture, from the point of view of the calorimeter) pathways.

I hope this helps. I am going to be limiting my participation from now on. The article is being rewritten, and I don't have time to participate in that. I may come back later and edit if time allows. Merry Christmas. Kirk shanahan (talk) 19:39, 22 December 2008 (UTC)

## The Major Fallacies of Cold Fusion

{Please place comments to this section in the following section. Just refer to what paragraph you want to discuss if it is relevant. I'd like to keep this article intact and not all chopped up by commentary. Thanks for your help on this.}

Cold Fusion (CF) advocates (CFers, short for 'cold fusioners', a contraction of 'cold fusion engineers' proposed by CFers themselves) want the scientific mainstream and general population to believe that a new, solid-state, nuclear reaction was uncovered by the work of Fleischmann, Pons, and Hawkins. They have presented a large variety of 'evidence' that this must be the case. Their side of the story is well presented in the recent (2007) book by Dr. Edmund Storms. However, the contrary case is not so well presented there, in fact is

could be said to be misrepresented there by selective failures on the part of Dr. Storms. I wish to attempt to delineate the problems here for the Wikipedia public.

In the Storms book are several tables of collected experimental evidence for several primary claims (primary in the sense that their numbers and prominence in CFers views make them highly notable). First is the table of experiments that have detected apparent excess heat (Note the phraseology. They have detected signals, but the signals are not proven as true excess heat.) That table takes up approximately 8 pages in the book, a very impressive number. The problem is that none of those experiments provides the information needed to evaluate the veracity of the 'excess' claim over the conventional explanation offered by the Calibration Constant Shift (CCS), with the one exception of Dr. Storms' own work on Pt electrodes published at the ICCF9 in 2000, which in fact is what led to the presentation of the CCS explanation. That fact led this author to reanalyze the data supplied by Dr. Storms to show that it was within experimental error, and thus cannot be confirmed as true excess heat. Again, no other study has presented the information to be able to make that determination, so, no other study can confirm true excess heat either.

To confirm excess heat is true, it must be shown that the excess heat signal is significantly above (usually this means 3 times larger than) the experimental noise level. The consistent error the CFers make is to assume the baseline calorimeter noise is the experimental error. In fact it is a small part of the total error, and the dominant term could well be due to the CCS. (Other things might be going on as well.) Typical baseline noise is perhaps 75 mW vs. signals of 780 mW in Dr. Storms' case and even higher in others. Unfortunately as shown in my 2002 publication, Dr. Storms' CCS is a "1%" error (i.e. 1 sigma ~ 1% of input power). In Dr. Storms' case, the input power was up to ~20 W and the best excess signal was ~780 mW for a percentage of ~4%. In chemistry a "1%" technique is a very, very good technique, and it is not expected that Dr. Storms (or anyone else) could do much better. His calorimeter was ~98-99% efficient as well, which is also top-notch design. Unfortunately, this means his 780 mW signal is 'in the noise' and is not conclusive proof of excess heat.

What Dr. Storms did to allow this analysis was he published a) the input power (nicely correlated to apparent excess) and b) limited information on repeatability of calibration constants. He pointed out that a resistive heater calibration was 1.7% off from an electrolytic calibration. Again, an excellent level of reproducibility, but not good enough to show his signal was above noise. Unfortunately no other researchers have published such information, particularly on calibration stability with time, i.e. 'b' above. Without both some measure of 'a' and 'b', the veracity of an excess heat signal cannot be determined. Again, this is nothing more than the standard scientific demand that the signal be significantly above the noise. The only requirement is to recognize the noise in these experiments is not just the baseline fluctuation.

The CCS is simple to understand. It simply says that between the time the experiment is calibrated, and the time that experimental data is collected, an instability in the experiment has occurred and thus the prior calibration is no longer valid. In other words expressed mathematically, at time  $t_0$ , the experimenter determines the calibration equation is  $P_{out} = 5 * X + 3$ . Then at time  $t_1$  a change occurs, such that at time  $t_2$ , when the experimenter measures the unknown conditions, the true calibration equation (unknown to the researcher) is  $P_{out} = 4 * X + 3$ . Clearly, if you multiply  $x$  by 5 when the correct (at that time) value is 4, you will get the wrong  $P_{out}$ . This is why measuring the calibration equation at several times to statistically assess the stability of those constants is a necessity.

In an even more detailed sense, such a measurement must contain every opportunity for the equation to vary, otherwise, the determination of the level of variation is incomplete. This is another potential problem with proving excess heat is real. But this almost brings up a 'Catch-22' situation. If you get the variable signal that CFers call 'excess heat', but I would call a CCS, how can you tell it apart. The answer is that you must know what is going on. In my publications I showed mathematically that a shift in heat distribution in the cell could be one way a CCS could occur, and I proposed a chemical mechanism to get such a shift. However, this mechanism

has not been accepted by the CFers. That is fine, but the consequence of that is that they must propose a mechanism for the excess (which they do) AND show control over it (which they have NOT done). Admittedly, this is where it gets very difficult. But essentially, if the excess heat signal is true excess heat, the CFers need to develop detailed control over it. Once they do, they should be able to optimize it, increasing its strength to a point where it is well above the 'CCS' noise level. Conversely, if my proposed mechanism is correct, it should be optimizable as well, although the way to do that will undoubtedly be very different as the difference between optimizing a conventional chemical reaction and a nuclear reaction are worlds apart. We all await such experimentation.

The second largest table in Dr. Storms' book was on claims to have detected the mass 4 isotope of He, a putative fusion product. Unfortunately, the CFers and everyone else realize that  $4\text{He}$  is found in the air, and that if air leaks into the experiment, one can find  $4\text{He}$ . Complicating this is the fact that liquid He (which is primarily  $4\text{He}$ ) is used in many scientific labs, and thus potentially can be found at concentrations higher than in background air due to its use in nearby labs (connected via ventilation flow paths). That is particularly troublesome. This was recognized in the 1989 DOE report, and again in the 2004 report. What makes it crucial is the work of W. B. Clarke on samples supplied by SRI, where he showed conclusively that air had leaked into all 4 samples provided. This was in experiments published in 2002 and 2003. So, 13-14 years after the fact, we are still plagued with air inleakage problems. Therefore, no scientist will accept  $4\text{He}$  claims if they are not backed up by proof from the same experiments that air inleakage was not a problem, and that  $4\text{He}$  can be measured accurately. Air inleakage was detected by Clarke by discovering nitrogen in the samples, as well as neon in levels commensurate with the He if it came from air. No reports of  $4\text{He}$  adequately address this issue, thus none are conclusive proof of a nuclear origin if the He. To repeat, to adequately address this issue, the analysis method must be proven reliable numerically and with respect to air exclusion (and such proof must be supplied, not inferred, assumed, or asserted).

The third largest table in Storms' book was on heavy metal transmutation results. At this point, the table was not very long, but several labs/experimenters were noted. Just as with the He, yes, they were detecting new elements on their electrodes surfaces (or membranes, or whatever). But whether these new elements came from a nuclear process is not well proven. In fact the normal conclusion would have been otherwise in any other field. 'New' elements have been detected on Pd electrode surfaces since the beginning of the cold fusion story. In the beginning, they were rightly attributed to contaminants. In fact on prominent one was Pt, which is almost all cases is what the 'other' electrode is made of. In other words, the Pt electrode was somehow being dissolved and deposited on the Pd electrode. And this is normal. What is not normal is claiming it or other new elements were formed by a nuclear reaction in or on the Pd electrode.

To prove that these new elements arise from such a reaction is extremely difficult. For example, one of the earlier claims for transmutation came from the Patterson Power Cell, and its 'commercial cousin', the RIFEX kit. Scott Little performed an interesting study on one such kit, and got essentially the same results as Miley and Patterson (the purveyors of the RIFEX kit), i.e., he did find 'new' elements, but he went one step further (actually two one steps). First he computed whether the new elements detected could have been detected in his starting materials. He found that the large majority could not have been detected, i.e. the RIFEX kit was probably just concentrating a sub-trace level contaminant on the bead surfaces. Additionally Little took those elements that were found above the detection limit and traced several of them down to components that the cell itself had been constructed of. In other words, the RIFEX cell was extracting the elements from itself and depositing them on the beads to be found later as 'new' elements (just like the Pt). Yes, they were 'new', but their origin was not a solid-state nuclear reaction. CFers must prove that this is not true in their results, and none have done so to date.

Secondarily, CFers have not used their analytical techniques adequately. They have consistently misidentified these 'new' elements by assuming their analysis instruments function better than they actually do, or by

choosing one identity from what was in fact a list of possible identities. For example, Iwamura has been noted as claiming to have produced Pr and Mo from a complex membrane structure that deuterium was diffused through. The problem is that these identifications were done with a single peak from an XPS spectrophotometer. Unfortunately, XPS is a ‘fingerprint’ technique, i.e. like in regular fingerprint analysis, more than one match point must be found to be able to identify the element, but Iwamura only used one peak (match point). Multiple match points are required because usually there are several elements that can produce a given peak. Later, Mizuno’s group found that Iwamura’s Mo peak was actually S (Mo and S have overlapping peaks where Iwamura chose to pick his one peak). While Mizuno did not challenge the Pr identification, in fact someone else did (unnamed participant at one of the ICCFs), and they suggested the Pr was actually Cu, a metal commonly found in vacuum systems, as well as trace contaminants from the membrane fabrication materials. Again, it was a one-peak identification, and thus is not definitive.

Another misuse of analytical instrumentation/results comes from experiments that claim to have show altered natural isotopic distributions. This is only done with the SIMS technique. Going back to the Iwamura case above, the “Mo” claimed to have been produced supposedly had an altered isotopic distribution. Mo has several isotopes, but its major one is mass 96. In Iwamura’s results, the mass 96 peak was greatly enhanced over normal distributions, thus supposedly proving the nuclear nature of the reaction (which tend to favor particular isotopes). However, sulfur is 100% mass 32, and  $3 \times 32 = 96$ , i.e. the mass 96 came from an S3 species, so given Mizuno’s result, it is easy to see why Iwamura was misled. This kind of analysis must be done on all claimed isotopic anomalies in order to substantiate that they are real. As well, typically ignored are the di- and triatomic metal hydride species. Being 1 and 2 mass units larger than the bare metal, they would convince the unwary an isotope distribution shift has occurred, when in fact all one is looking at is hydride chemistry. To recap then, transmutation results need to properly interpreted and identified, and then need to be shown NOT to have arise from concentrating and collect contaminants. So far, only Scott Little has done a thorough enough job on that.

Other tables could have been placed in Storms’ book for other families of claims, but Storms only dealt with those bodies of evidence that he thought were adequately large to be persuasive. He was correct in that, it does require an adequate body of evidence to make a scientific claim. Unfortunately he inaccurately presented the CCS story, left out the Clarke He story, and hand-waved (i.e. did not reference) the anti-contamination argument. Proving a new revolutionary argument takes addressing all the issues, not ignoring them. Until such time as the CFers either a) address the issues above (and related ones) or b) conclusively demonstrate a cold-fusion-powered water heater, the rest of us are correct in assuming the CF case is unproven, and the Wiki article needs to reflect that reality.

## Comments on "The Major ..."

Originally posted after the fifth paragraph of the named section:

Yes, that is indeed simple. However, what is not so simple is WHY and HOW the calibration should shift in the first place, for every different kind of calorimeter that has been used, *and especially by just enough, almost always* (in your opinion), to render the measurements invalid. It is my understanding that things like that don't just happen by themselves, energy must be applied to make them happen. Where did the energy come from to do that? In other words, you are presuming your answer, in order to obtain your answer, which thereby hides the energy that causes the "problem" that you claim means no significant energy was produced! Not very scientific. V (talk) 00:22, 21 January 2009 (UTC)

"However, what is not so simple is WHY and HOW the calibration should shift in the first place, for every different kind of calorimeter that has been used, *and especially by just*

*enough, almost always* (in your opinion), to render the measurements invalid."

I have published one why and how, and have discussed it extensively in the CF Talk page. Check the archives and read the papers.

ONE "why and how"??? Why would that affect every different kind of calorimeter out there? And I repeat, decalibrations don't happen by themselves without cause. I fully recognize that possible causes of decalibration can include material degradation over time, gravity-induced sag, and other slow factors, but it is not reasonable to think that any of these will act quickly between a fresh calibration and the conduction of the experiment that uses the freshly calibrated device. Any such device that can't hold its calibration for that long is worthless and should be replaced. The labs where CF experiments are done may be underfunded, but I think the researchers would notice whether or not their measurement tools are constantly needing recalibrations. ESPECIALLY since they've known they need good measurements, ever since 1989.

"ONE "why and how"??? Why would that affect every different kind of calorimeter out there?" - To be accurate, my one why and how was proposed for a P&F type cell, and would not be applicable to other systems. However, the CCS problem is applicable to any and all calibrated methods, which constitute 99.999+% of quantitative scientific measurements. This is nothing more than a graphic illustration of the analytical chemistry (actually General Science) Golden Rule "You can't calibrate an unstable system." Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

SO? In what way is a simple electrolysis cell being fed a steady current an unstable system? Do remember that if the palladium electrode is "fresh", empty of hydrogen content, it will take a while before it loads up deuterium to the 80+% mark (as many deuterium as palladium atoms) that is claimed to be the level where excess heat starts to appear. Why wouldn't that load-up time be plenty to get a good STABLE measurement of the heat associated with the electrolysis (simple electric-resistance heating, basically), before the Main Event? (Also, I recognize that a simplistic electrolysis rig would be not-stable in the sense that as water is electrolyzed, the water level would drop and some of the electrodes might no longer be in the water, thereby leading to a change in the electrolysis rate and associated energy transformations. But alternate rigs are quite easy to imagine/use, for example one in which an electrode-holder floats on the surface of the electrolyte, and they would not have that problem.)

"And I repeat, decalibrations don't happen by themselves without cause." - Right, which is why for the P&F cells I was studying, I was able to propose a rational mechanism for the cause. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

And as I've specified both above and below this remark, it takes energy in one form or another to cause stuff like decalibration. So, if the measurement device is halfway-decent quality such that one has a right to expect it to stay calibrated under reasonable conditions, decalibration could only occur if something unreasonable is happening. You are trying to put the cart before the horse, in saying that the decalibration happens before the event that caused it, thereby allowing you to say that the event did not happen. Bad logic is automatically bad science.

"Any such device that can't hold its calibration for that long is worthless and should be replaced." - you're finally getting my point ;-}. Actually, every calibrated method has an associated error. It is a sign of pseudoscience to try to work inside the error band, so it is critical to accurately know that band. The CFers think the error band is defined by baseline noise. I simply showed that is not true. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

This is why I talked about using different scales for different jobs. A meter designed for kilowatts should

not be relied on to measure microwatts, and vice-versa. But a calorimeter accurate to a hundredth of a joule should be just fine for measuring whole joules. You appear to be claiming that EVERY CF researcher is in-essence using (the equivalent of) a calorimeter designed for kilojoules to measure tenths of a joule (I'm aware that many CF claims of excess heat involve smaller amounts rather than larger amounts, partly because of the difficulty of loading enough deuterium into the palladium). I would be skeptical of the results, too, if that claim was true. And yet every CF researcher since the 1989 DOE panel has known they need ACCURATE heat measurements, to convince the detractors. I find it difficult to believe that many such people, acknowledged as having expertise in the electrochemistry field before becoming outcasts by researching CF, would not know they need APPROPRIATE-scale calorimeters to obtain the desired-by-skeptics accuracy. Every single one? You must be joking! (rhetorical statement)

"The labs where CF experiments are done may be underfunded, but I think the researchers would notice whether or not their measurement tools are constantly needing recalibrations." - Hard to tell, they never publish such information.

What I meant is that that should be a signal to the researchers, that the measurement tool needs to be replaced.

But I know Ed Storms thinks a 1.7% variation is A-OK, even after I showed him it could cause the signals he observed. Draw your own conclusion from that "ESPECIALLY since they've known they need good measurements, ever since 1989."

I don't have quite enough data about that setup yet (and "could" does not automatically equal "did"). How did the number of joules being measured compare to the sensitivity of the calorimeter? Is it NORMAL for that particular model to be inaccurate by 1.7%? If so, why wouldn't Storms know about it, and therefore know he needed something better?

To be fair, the CCS is subtle and not often considered. That's because most people work well above the level of problem it causes. Not so the CFers. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

I'm quite sure the CF researchers WANT their experiments to produce lots of extra joules, as occasionally apparently does happen, such that there could be no doubt. And in their zeal, the ones who see indications of just a few extra joules are willing to think that that suffices for publication. It is partly because of the occasional big event that I'm willing to think that many of those small events are real. I remember reading about how gingerly Enrico Fermi pulled that last moderator rod from that first fission reactor under the Chicago sports stadium. They REALLY did not want an out-of-control reaction! In CF we don't seem to need to worry about THAT much activity, but there does seem to be a steep rise between just-barely-something-happening and (possible exaggeration) electrolyte-starts-to-boil. The difficulty is all about reaching that point-of-steep-rise. But to the extent it has ever happened even once, then that LOGICALLY means some number of the just-barely-something-happening measurements must be real.

"I fully recognize that possible causes of decalibration can include material degradation over time, gravity-induced sag, and other slow factors, but it is not reasonable to think that any of these will act quickly between a fresh calibration and the conduction of the experiment that uses the freshly calibrated device." - What I think is happening in the P&F cell is that material is slowly deposited on the electrode surface and it alters the surface energy such that H2 bubbles adhere better. Then O2 bubbles collide and merge, and the clean metal surface under the bubble catalyzes  $\text{H}_2 + \text{O}_2 \rightarrow \text{H}_2\text{O}$ .

That doesn't make any sense at all, in any experiment that isolates the two electrodes such that the gases form can be separately collected. Sure, I know that with palladium there the hydrogen should go into the metal instead of forming bubbles (it depends on NOT maximizing the amount of electric current flowing), and an experimenter might therefore expect to collect only oxygen gas --but still the electrodes are

decently separated. Aren't they?

Another separate point is that the amount of gas collected is related to the amount of electrical energy put into the electrolysis cell. If oxygen is behaving as you describe, then it is not being collected with the rest. That discrepancy, especially if enough to cause excess heat when combining with hydrogen, should be measurable. In other words: Electrical energy yields some heat directly plus hydrogen and oxygen. Hydrogen mostly permeates into the palladium electrode and the oxygen bubbles out and can be collected. In the initial stable phase, as deuterium-hydrogen permeation begins, the rate at which oxygen gas is collected would be a straight-line fraction of the electrical energy supplied. If O<sub>2</sub> starts to go to the other electrode instead of bubbling out, then the gas collection rate must change. The difference in rate-of-collection of oxygen, if some is assumed to combine with hydrogen and release heat, must match the excess heat measured, for your scenario to be true. Since oxygen collection is a measurable thing, what is the relevant data?

This requires enough bubbles to be impacted to get a noticeable shift in heat production, which requires enough contaminants or structural changes at the surface to do that. That could take very long times in very clean systems, or, in the case of the co-deposition experiments, it could happen quickly due to the special conditions of that select system. You need to remember that *\_most\_* CF experiments runs hundreds of hours. The Szpak codep process cuts that way down - why? Because it makes dendritic Pd with lots of contaminants on it (from the plating chemicals). Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

Yes, that sounds reasonable, on the face of it. However...if the system is very clean, where are the contaminants coming from, that you need to exist? Yes, I know that most CF experiments run for a long time, in order to load enough deuterium into the palladium. One question I've had, is, "Why don't the CF researchers pressurize their electrolysis cells?" Then they could use a higher current and electrolyze heavy water at a faster rate; the pressure would encourage deuterium produced at the increased rate to enter the palladium instead of make bubbles, and the critical loading point would be achieved much sooner --possibly much more easily, also, if there is some equilibrium point where at normal pressure deuterium would rather make bubbles than permeate (pressure would move the equilibrium point to a higher deuterium density in the palladium).

Regarding the Szpak process, there would still have to be an unusual closeness between the electrodes, for oxygen to so easily find its way to the hydrogen-producing electrode. And why must this electrolyte solution be so much more contaminated than an ordinary heavy-water electrolyte solution? Sure, I know it needs to have a Pd compound dissolved in it, such that Pd can be electroplated out and form an electroplating, and I know that the voltage needed to do that must be very close to the voltage for electrolyzing heavy water, but I don't know why a lot of other stuff must exist in the solution, and why it must have a similar electronegativity to hydrogen and palladium, such that the voltage used to do electrolysis causes more depositing than of just palladium and hydrogen.

Further you clearly misunderstand the process here when you imply I am tuning my conclusions just so I can reject the CF hypothesis. By defining a potential error in one case that is capable of fully explaining the observations in that one case, I establish a new error bar on that experiment, one that is significantly larger than was previously realized.

"defining a potential error" is not the same thing as proving the error was actually present. Certainly I agree that the possibility needs to be investigated. I do not agree with arbitrarily assuming the error was there, however.

But when the 'potential' error carries full explanatory power, it is expected that its presence be disproven. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

I won't disagree with the basic logic, but I can disagree with the assumption that "the potential error carries full explanatory power". So far as I can see, it does not, and therefore the logical conclusion is faulty, that its presence must be disproven. Because I have no reason to think all those experimenters out there are using identical equipment, with insignificant electrode separation, and just the right sorts of contaminants present in their electrolyte solutions.

Then I extend that new interpretation to other cases, and find it MAY be functional in nearly all or all excess heat measurements.

This requires the further assumption all the other measurements are using equipment that is subject to that exact same type of error. With laboratories around the world using equipment from different suppliers, and very likely different equipment at that (DON'T tell me there is only one possible type of calorimeter!), this assumption is not reasonable. Another related issue involves the sensitivity of these devices. We wouldn't have a unit of energy called the "erg" if it couldn't be measured. Yet CF researchers typically talk about measuring "joules" of heat, which is a unit ten million times bigger than the erg. Now while I'm aware you don't use a postage scale to weigh an empty forklift, and I'm aware that putting that vehical on a scale built to handle fully-loaded 18-wheelers will likely not yield high accuracy, there certainly exist scales of intermediate ability. So, a scale accurate to a hundredth of a joule would have to be WILDLY out of calibration to indicate detection of whole joules that didn't actually exist. Why would ALL the researchers who KNOW they need high accuracy (to convince the detractors) be using measuring equipment that could get so far out of adjustment???

"This requires the further assumption all the other measurements are using equipment that is subject to that exact same type of error." - The CCS is so generic, it must be checked for in all cases, i.e. calibration stability must be assessed.

No, it is only generic with respect to low-accuracy calorimetry equipment. That is, for equipment accurate to one part in a thousand, if it registers a 1.7% increase in heat, then that is outside the normal error zone for that equipment. If this equipment can become so out-of-calibration as to invalidate the measurement, then the cause is still going to require energy that must be explained. And then there is the oxygen-gas measuring thing...is NOBODY doing that???

In a different configuration, if a CCS can explain the signal, one has to evaluate the reasonableness of the CCS explanation, i.e. its magnitude. I did this for the Storms' cell in my original paper to prove the CCS was reasonable, and it would need to be done again in each case. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

OK, to show the CCS is reasonable in each case is what you as a critic must do, before you can expect the experimenters in those cases to prove its not there.

"With laboratories around the world using equipment from different suppliers, and very likely different equipment at that (DON'T tell me there is only one possible type of calorimeter!), this assumption is not reasonable." - I repeat - when the 'potential' error carries full explanatory power, it is expected that its presence be disproven. Your guesses as to what errors are or are not present are not proof (they are guesses). We need some proof. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

And I shall continue to disagree with your ASSUMPTION that your potential error is actually a potential error in all those cases.

I don't get your point on the forklift-stuff. The researchers use what they considered to be adequate and sufficient tools to do the job. I simply showed that this was not so. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

"Why would ALL the researchers who KNOW they need high accuracy (to convince the detractors) be using measuring equipment that could get so far out of adjustment???" - Because they've convinced themselves it is adequate and sufficient, as do any scientists. But that decision was based on the idea that baseline noise was the predominant error. It isn't, ergo, their conclusion was incorrectly derived, and is incorrect. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

This leaves you with showing that there actually is present something more than baseline noise.

This means that without additional data, the prior experiments are now to be considered inconclusive because the reported signals are now potentially 'in the noise'.

Only experiments using very similar equipment can have that problem. Thus there is more than one type of "additional data" to gather.

I won't argue with that. See my prior comments. I eagerly await all this new data. But you realize you've made my point, it has to be `_new_ data`. Possibly acquired before and unpublished then, but 'new' as per the literature as it stands today. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

You misinterpreted me. For YOU, and the CCS hypothesis, you need more data about the old experiments. You need to know what equipment they actually used, before you can say your hypothesis applies to their old data. That said, I agree that we need more new data, too. You might notice I've made a number of suggestions along that line.

As a critic I need to do no more, but I actually went further with my postulated mechanism. Whether that turns out to be right or not has no bearing on what the 'discovery' of the CCS' role in Storms' work does, and whether that is extensible to others' work.

You did indeed go farther; you assumed your mechanism was real, and you did at least indicate that lots of CF research should be discounted because of it. Even though HERE you are carefully qualifying your claims in a way that clearly means to me, "Hardly any large percentage of CF work must-be/has-been affected by the CCS mechanism".

No, I showed the CCS could explain Storms' result. I pointed out his calorimeter was top notch, and that lesser ones would have bigger problems. And I pointed out that no relevant data had been published to date (still true today) that would allow one to evaluate whether a CCS was active in all the other cases. This leads you back to the choice between boring science and sensationalism. (Out of time for today maybe more tomorrow or later tonite, but don't hold your breath. You are making a good case for being a CF fanatic yourself. Rothwell taught me not to bother too much with you guys.) Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

OK, tell me why the definition of "top notch" means it is only accurate to about one part in a hundred (such that if it shows a 1.7% difference from the norm, it can be argued-about)?

"It is my understanding that things like that don't just happen by themselves, energy must be applied to make them happen."

No, energy does not need to be applied to make them happen. Read the papers.

Energy is ALWAYS involved, one way or another, in causing ANY change-of-state larger than the scale-of-events affected by the Uncertainty Principle. Period. Whether

energy is involved slowly or quickly, whether conversions between potential, kinetic, radiant or other forms are involved, any other description for a change-of-state would be a violation of the energy-conservation law. For example, a postage scale can become un-calibrated if an internal spring becomes permanently deformed --but it takes energy to do that to that spring.

to be clear, there are scads of energy around. They run an amp or two of current through the cells, which heats things up nicely. They use 'caustic' eletrolytes, which means there is chemical energy available from the hoydroxide dissolution reactions. There is the exothermic heat of absorption of hydrogen in palladium (when they use Pd electrodes). there is the usual heat of solution for dissolution of things not in the electrolyte but in contact with it. Yes, energy is involved, but you don't need to apply any extra to get the effects we see. What i was com[pletely unclear on is why you thought you needed to emphasize that energy is ALWAYS involved. What was your point?? 18:39, 22 January 2009 (UTC)

" In other words, you are presuming your answer, in order to obtain your answer, which thereby hides the energy that causes the "problem" that you claim means no significant energy was produced! "

Sorry, you're clueless here. I tested the hypothesis that a CCS could account for the excess heat signal, and found it workable. Now, more testing must be accomplished in order to verify one or the other explanation.

I am far from clueless, although I readily admit to not knowing everything. The most important thing about Science is that it makes logical sense. (Even Quantum Mechanics can make logical sense, but it depends on the approach.) And very frequently in Science, multiple variables are involved. Experimenters try to make sure they know what the relevant variables are, in their experiments. Certainly the CCS mechanism would count as an additional variable for CF researchers to include. If it was real and not just a postulate. And if it was relevant across the board. TWO important things to prove, not just assume. Have you ever seen the CCS mechanism in NON-CF experiments? Has anyone? In the last hundred years of calorimetry, why would the CCS mechanism have waited to appear only in CF experiments?

"Certainly the CCS mechanism would count as an additional variable for CF researchers to include." - Thank you for noting my PRIMARY POINT. The CCS potential error MUST BE INCLUDED in analysis of calorimetric data. The CFers DON'T DO THAT. The don't even acknowledge it is real (which it is any everyone who can do arithmetic realizes!). So congrats, you are two or three steps ahead of every CFer in understanding their calorimetry. Kirk shanahan (talk) 18:39, 22 January 2009 (UTC)

"If it was real and not just a postulate. And if it was relevant across the board. TWO important things to prove, not just assume." - have I ever said anything else?!?!? My whole point towards the field is that they need to stop claiming a nuclear process is proven. There are still potentially valid questions that they refuse to even acknowledge, much less address. The next to the last paragraph of my first publication ends with: "Additional data must be acquired and further analysis performed before the claim of Storms that platinum shows evidence for cold fusion can be taken seriously." The last paragraph says: "It [this paper] establishes the requirement that the statistical variation in calibration constants must be explicitly determined and considered,". Kirk shanahan (talk) 18:39, 22

January 2009 (UTC)

"Not very scientific"

Perfectly scientific. The fact you don't understand this really disqualifies you as an editor of scientific topics. You should recuse yourself.

Au contraire. Because Science is logical. And various things written above indicate you are failing at it, not I. V (talk) 19:01, 21 January 2009 (UTC)

It's comments like these that lead me to stop communicating. The above is nothing but an ad hominem attack. No explanation, no discussion, just "Kirk is a nasty boy". I've learned to give up when the discussion reaches this level. The use of ad hom. attacks indicates the user has run out of real arguments. Kirk shanahan (talk) 18:39, 22 January 2009 (UTC)

V, it's not as simple as putting all the crap in a special vessel and reading its temperature from an LCD on the outside as the final word on the enthalpy change. You have to do a number of calculations which rely on a number of *assumptions*, which may not be true. See, for example, the comments in this ([http://www.sciencedirect.com/science?\\_ob=ArticleURL&\\_udi=B6TGF-4KBT1J4-6&\\_user=10&\\_rdoc=1&\\_fmt=&\\_orig=search&\\_sort=d&view=c&\\_acct=C000050221&\\_version=1&\\_urlVersion=0&\\_userid=10&md5=64ac9816d27d1d665cc43a9d774c45d3](http://www.sciencedirect.com/science?_ob=ArticleURL&_udi=B6TGF-4KBT1J4-6&_user=10&_rdoc=1&_fmt=&_orig=search&_sort=d&view=c&_acct=C000050221&_version=1&_urlVersion=0&_userid=10&md5=64ac9816d27d1d665cc43a9d774c45d3)) totally independent paper from the literature from 2006:

*It has been shown that the error in all aforementioned quantities reflects on the baseline and it can have a very serious impact on the accuracy of the measurement.*

The fact is that the large majority of cold fusion experiments were done using dodgy calorimetry. And the other fact is that you only need a fraction of cold fusion experiments making calorimetry mistakes, and ordinary publishing bias does the rest. Calorimetry mistakes happen all the time, but Jed would have us believe that cold fusion researchers are uber duper careful and have eliminated all potential mistakes in most experiments! Because the heat is "real" to him, all 4000 or so of the papers that show heat must be real too, regardless of how shoddy the calorimetry, not just the bare handful of careful experiments. Do you see the problem? I really hope that you do (<http://www.springerlink.com/content/g15h6221514rm386/>) . Phil153(talk) 23:11, 21 January 2009 (UTC)

"The fact is that the large majority of cold fusion experiments were done using dodgy calorimetry." --Well, I don't know that that statement is actually a fact. If it is, then I would have to agree that a lot of the experiments must be ignored. This still leaves a few, however. Even you have previously admitted to that. To whatever extent you wish to consider me to be a CF fanatic, it is due to those few. Plus the recent indications of nuclear activity as recorded in pits in CR-39 plastic. AND because I think I know how CF can happen. I paraphrase your own words: "the hypothesis carries full explanatory power". It explains heat without significant radiation in bulk metal. It explains the requirement for high deuterium loading. It explains the presence of significant particle production when the metal is very thin. Remember, a lot of detractors think CF can't be true

because they don't know any way it MIGHT be true. Should that change...we still need data, of course. Would you care to speculate about the physical properties of "pure metallic deuterium"? The hypothesis suggests making some, to see if it will explode, since it would not be DILUTED by palladium or any other atoms...== Comments on "The Major ..." ==

Originally posted after the fifth paragraph of the named section:

Yes, that is indeed simple. However, what is not so simple is WHY and HOW the calibration should shift in the first place, for every different kind of calorimeter that has been used, *and especially by just enough, almost always* (in your opinion), to render the measurements invalid. It is my understanding that things like that don't just happen by themselves, energy must be applied to make them happen. Where did the energy come from to do that? In other words, you are presuming your answer, in order to obtain your answer, which thereby hides the energy that causes the "problem" that you claim means no significant energy was produced! Not very scientific. V (talk) 00:22, 21 January 2009 (UTC)

"However, what is not so simple is WHY and HOW the calibration should shift in the first place, for every different kind of calorimeter that has been used, *and especially by just enough, almost always* (in your opinion), to render the measurements invalid."

I have published one why and how, and have discussed it extensively in the CF Talk page. Check the archives and read the papers.

ONE "why and how"??? Why would that affect every different kind of calorimeter out there? And I repeat, decalibrations don't happen by themselves without cause. I fully recognize that possible causes of decalibration can include material degradation over time, gravity-induced sag, and other slow factors, but it is not reasonable to think that any of these will act quickly between a fresh calibration and the conduction of the experiment that uses the freshly calibrated device. Any such device that can't hold its calibration for that long is worthless and should be replaced. The labs where CF experiments are done may be underfunded, but I think the researchers would notice whether or not their measurement tools are constantly needing recalibrations. ESPECIALLY since they've known they need good measurements, ever since 1989.

"ONE "why and how"??? Why would that affect every different kind of calorimeter out there?" - To be accurate, my one why and how was proposed for a P&F type cell, and would not be applicable to other systems. However, the CCS problem is applicable to any and all calibrated methods, which constitute 99.999+% of quantitative scientific measurements. This is nothing more than a graphic illustration of the analytical chemistry (actually General Science) Golden Rule "You can't calibrate an unstable system." Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

SO? In what way is a simple electrolysis cell being fed a steady current an unstable system? Do remember that if the palladium electrode is "fresh", empty of hydrogen content, it will take a while before it loads up deuterium to the 80+% mark (as many deuterium as palladium atoms) that is claimed to be the level where excess heat starts to appear. Why wouldn't that load-up time be plenty to get a good STABLE measurement of the heat associated with the electrolysis (simple electric-resistance heating, basically), before the Main Event? (Also, I recognize that a simplistic electrolysis rig would be not-stable in the sense that as water is electrolyzed, the water level would drop and some of the electrodes might no longer be in the water, thereby leading to a change in the electrolysis rate and associated energy transformations. But

alternate rigs are quite easy to imagine/use, for example one in which an electrode-holder floats on the surface of the electrolyte, and they would not have that problem.)

"And I repeat, decalibrations don't happen by themselves without cause." - Right, which is why for the P&F cells I was studying, I was able to propose a rational mechanism for the cause. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

And as I've specified both above and below this remark, it takes energy in one form or another to cause stuff like decalibration. So, if the measurement device is halfway-decent quality such that one has a right to expect it to stay calibrated under reasonable conditions, decalibration could only occur if something unreasonable is happening. You are trying to put the cart before the horse, in saying that the decalibration happens before the event that caused it, thereby allowing you to say that the event did not happen. Bad logic is automatically bad science.

"Any such device that can't hold its calibration for that long is worthless and should be replaced." - you're finally getting my point ;-}. Actually, every calibrated method has an associated error. It is a sign of pseudoscience to try to work inside the error band, so it is critical to accurately know that band. The CFers think the error band is defined by baseline noise. I simply showed that is not true. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

This is why I talked about using different scales for different jobs. A meter designed for kilowatts should not be relied on to measure microwatts, and vice-versa. But a calorimeter accurate to a hundredth of a joule should be just fine for measuring whole joules. You appear to be claiming that EVERY CF researcher is in-essence using (the equivalent of) a calorimeter designed for kilojoules to measure tenths of a joule (I'm aware that many CF claims of excess heat involve smaller amounts rather than larger amounts, partly because of the difficulty of loading enough deuterium into the palladium). I would be skeptical of the results, too, if that claim was true. And yet every CF researcher since the 1989 DOE panel has known they need ACCURATE heat measurements, to convince the detractors. I find it difficult to believe that many such people, acknowledged as having expertise in the electrochemistry field before becoming outcasts by researching CF, would not know they need APPROPRIATE-scale calorimeters to obtain the desired-by-skeptics accuracy. Every single one? You must be joking! (rhetorical statement)

"The labs where CF experiments are done may be underfunded, but I think the researchers would notice whether or not their measurement tools are constantly needing recalibrations." - Hard to tell, they never publish such information.

What I meant is that that should be a signal to the researchers, that the measurement tool needs to be replaced.

But I know Ed Storms thinks a 1.7% variation is A-OK, even after I showed him it could cause the signals he observed. Draw your own conclusion from that "ESPECIALLY since they've known they need good measurements, ever since 1989."

I don't have quite enough data about that setup yet (and "could" does not automatically equal "did"). How did the number of joules being measured compare to the sensitivity of the calorimeter? Is it NORMAL for that particular model to be inaccurate by 1.7%? If so, why wouldn't Storms know about it, and therefore know he needed something better?

To be fair, the CCS is subtle and not often considered. That's because most people work well above the level of problem it causes. Not so the CFers. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

I'm quite sure the CF researchers WANT their experiments to produce lots of extra joules, as occasionally apparently does happen, such that there could be no doubt. And in their zeal, the ones who see indications

of just a few extra joules are willing to think that that suffices for publication. It is partly because of the occasional big event that I'm willing to think that many of those small events are real. I remember reading about how gingerly Enrico Fermi pulled that last moderator rod from that first fission reactor under the Chicago sports stadium. They REALLY did not want an out-of-control reaction! In CF we don't seem to need to worry about THAT much activity, but there does seem to be a steep rise between just-barely-something-happening and (possible exaggeration) electrolyte-starts-to-boil. The difficulty is all about reaching that point-of-steep-rise. But to the extent it has ever happened even once, then that LOGICALLY means some number of the just-barely-something-happening measurements must be real.

"I fully recognize that possible causes of decalibration can include material degradation over time, gravity-induced sag, and other slow factors, but it is not reasonable to think that any of these will act quickly between a fresh calibration and the conduction of the experiment that uses the freshly calibrated device." - What I think is happening in the P&F cell is that material is slowly deposited on the electrode surface and it alters the surface energy such that H2 bubbles adhere better. Then O2 bubbles collide and merge, and the clean metal surface under the bubble catalyzes  $\text{H}_2 + \text{O}_2 \rightarrow \text{H}_2\text{O}$ .

That doesn't make any sense at all, in any experiment that isolates the two electrodes such that the gases form can be separately collected. Sure, I know that with palladium there the hydrogen should go into the metal instead of forming bubbles (it depends on NOT maximizing the amount of electric current flowing), and an experimenter might therefore expect to collect only oxygen gas --but still the electrodes are decently separated. Aren't they?

Another separate point is that the amount of gas collected is related to the amount of electrical energy put into the electrolysis cell. If oxygen is behaving as you describe, then it is not being collected with the rest. That discrepancy, especially if enough to cause excess heat when combining with hydrogen, should be measurable. In other words: Electrical energy yields some heat directly plus hydrogen and oxygen. Hydrogen mostly permeates into the palladium electrode and the oxygen bubbles out and can be collected. In the initial stable phase, as deuterium-hydrogen permeation begins, the rate at which oxygen gas is collected would be a straight-line fraction of the electrical energy supplied. If O2 starts to go to the other electrode instead of bubbling out, then the gas collection rate must change. The difference in rate-of-collection of oxygen, if some is assumed to combine with hydrogen and release heat, must match the excess heat measured, for your scenario to be true. Since oxygen collection is a measurable thing, what is the relevant data?

This requires enough bubbles to be impacted to get a noticeable shift in heat production, which requires enough contaminants or structural changes at the surface to do that. That could take very long times in very clean systems, or, in the case of the co-deposition experiments, it could happen quickly due to the special conditions of that select system. You need to remember that most CF experiments runs hundreds of hours. The Szpak codep process cuts that way down - why? Because it makes dendritic Pd with lots of contaminants on it (from the plating chemicals). Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

Yes, that sounds reasonable, on the face of it. However...if the system is very clean, where are the contaminants coming from, that you need to exist? Yes, I know that most CF experiments run for a long time, in order to load enough deuterium into the palladium. One question I've had, is, "Why don't the CF researchers pressurize their electrolysis cells?" Then they could use a higher current and electrolyze heavy water at a faster rate; the pressure would encourage deuterium produced at the increased rate to enter the palladium instead of make bubbles, and the critical loading point would be achieved much sooner --possibly much more easily, also, if there is some equilibrium point where at normal pressure deuterium would rather make bubbles than permeate (pressure would move the equilibrium point to a higher deuterium density in the palladium).

Regarding the Szpak process, there would still have to be an unusual closeness between the electrodes, for oxygen to so easily find its way to the hydrogen-producing electrode. And why must this electrolyte solution be so much more contaminated than an ordinary heavy-water electrolyte solution? Sure, I know it needs to have a Pd compound dissolved in it, such that Pd can be electroplated out and form an electroplating, and I know that the voltage needed to do that must be very close to the voltage for electrolyzing heavy water, but I don't know why a lot of other stuff must exist in the solution, and why it must have a similar electronegativity to hydrogen and palladium, such that the voltage used to do electrolysis causes more depositing than of just palladium and hydrogen.

Further you clearly misunderstand the process here when you imply I am tuning my conclusions just so I can reject the CF hypothesis. By defining a potential error in one case that is capable of fully explaining the observations in that one case, I establish a new error bar on that experiment, one that is significantly larger than was previously realized.

"defining a potential error" is not the same thing as proving the error was actually present. Certainly I agree that the possibility needs to be investigated. I do not agree with arbitrarily assuming the error was there, however.

But when the 'potential' error carries full explanatory power, it is expected that its presence be disproven. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

I won't disagree with the basic logic, but I can disagree with the assumption that "the potential error carries full explanatory power". So far as I can see, it does not, and therefore the logical conclusion is faulty, that its presence must be disproven. Because I have no reason to think all those experimenters out there are using identical equipment, with insignificant electrode separation, and just the right sorts of contaminants present in their electrolyte solutions.

Then I extend that new interpretation to other cases, and find it MAY be functional in nearly all or all excess heat measurements.

This requires the further assumption all the other measurements are using equipment that is subject to that exact same type of error. With laboratories around the world using equipment from different suppliers, and very likely different equipment at that (DON'T tell me there is only one possible type of calorimeter!), this assumption is not reasonable. Another related issue involves the sensitivity of these devices. We wouldn't have a unit of energy called the "erg" if it couldn't be measured. Yet CF researchers typically talk about measuring "joules" of heat, which is a unit ten million times bigger than the erg. Now while I'm aware you don't use a postage scale to weigh an empty forklift, and I'm aware that putting that vehical on a scale built to handle fully-loaded 18-wheelers will likely not yield high accuracy, there certainly exist scales of intermediate ability. So, a scale accurate to a hundredth of a joule would have to be WILDLY out of calibration to indicate detection of whole joules that didn't actually exist. Why would ALL the researchers who KNOW they need high accuracy (to convince the detractors) be using measuring equipment that could get so far out of adjustment???

"This requires the further assumption all the other measurements are using equipment that is subject to that exact same type of error." - The CCS is so generic, it must be checked for in all cases, i.e. calibration stability must be assessed.

No, it is only generic with respect to low-accuracy calorimetry equipment. That is, for equipment accurate to one part in a thousand, if it registers a 1.7% increase in heat, then that is outside the normal error zone

for that equipment. If this equipment can become so out-of-calibration as to invalidate the measurement, then the cause is still going to require energy that must be explained. And then there is the oxygen-gas measuring thing...is NOBODY doing that???

In a different configuration, if a CCS can explain the signal, one has to evaluate the reasonableness of the CCS explanation, i.e. its magnitude. I did this for the Storms' cell in my original paper to prove the CCS was reasonable, and it would need to be done again in each case. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

OK, to show the CCS is reasonable in each case is what you as a critic must do, before you can expect the experimenters in those cases to prove its not there.

"With laboratories around the world using equipment from different suppliers, and very likely different equipment at that (DON'T tell me there is only one possible type of calorimeter!), this assumption is not reasonable." - I repeat - when the 'potential' error carries full explanatory power, it is expected that its presence be disproven. Your guesses as to what errors are or are not present are not proof (they are guesses). We need some proof. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

And I shall continue to disagree with your ASSUMPTION that your potential error is actually a potential error in all those cases.

I don't get your point on the forklift-stuff. The researchers use what they considered to be adequate and sufficient tools to do the job. I simply showed that this was not so. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

"Why would ALL the researchers who KNOW they need high accuracy (to convince the detractors) be using measuring equipment that could get so far out of adjustment???" - Because they've convinced themselves is adequate and sufficient, as do any scientists. But that decision was based on the idea that baseline noise was the predominant error. It isn't, ergo, their conclusion was incorrerctly derived, and is incorrect. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

This leaves you with showing that there actually is present something more than baseline noise.

This means that without additional data, the prior experiments are now to be considered inconclusive because the reported signals are now potentially 'in the noise'.

Only experiments using very similar equipment can have that problem. Thus there is more than one type of "additional data" to gather.

I won't argue with that. See my prior comments. I eagerly await all this new data. But you realize you've made my point, it has to be new data. Possibly acquired before and unpublished then, but 'new' as per the literature as it stands today. Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

You misinterpreted me. For YOU, and the CCS hypothesis, you need more data about the old experiments. You need to know what equipment they actually used, before you can say your hypothesis applies to their old data. That said, I agree that we need more new data, too. You might notice I've made a number of suggestions along that line.

As a critic I need to do no more, but I actually went further with my postulated mechanism. Whether that turns out to be right or not has no bearing on what the 'discovery' of the CCS' role in Storms' work does, and whether that is extensible to others' work.

You did indeed go farther; you assumed your mechanism was real, and you did at least indicate that lots of CF research should be discounted because of it. Even though HERE you are carefully qualifying your claims in a way that clearly means to me, "Hardly any large percentage of CF work must-be/has-been affected by the CCS mechanism".

No, I showed the CCS could explain Storms' result. I pointed out his calorimeter was top notch, and that lesser ones would have bigger problems. And I pointed out that no relevant data had been published to date (still true today) that would allow one to evaluate whether a CCS was active in all the other cases. This leads you back to the choice between boring science and sensationalism. (Out of time for today maybe more tomorrow or later tonight, but don't hold your breath. You are making a good case for being a CF fanatic yourself. Rothwell taught me not to bother too much with you guys.) Kirk shanahan (talk) 21:54, 21 January 2009 (UTC)

OK, tell me why the definition of "top notch" means it is only accurate to about one part in a hundred (such that if it shows a 1.7% difference from the norm, it can be argued-about)?

"It is my understanding that things like that don't just happen by themselves, energy must be applied to make them happen."

No, energy does not need to be applied to make them happen. Read the papers.

Energy is ALWAYS involved, one way or another, in causing ANY change-of-state larger than the scale-of-events affected by the Uncertainty Principle. Period. Whether energy is involved slowly or quickly, whether conversions between potential, kinetic, radiant or other forms are involved, any other description for a change-of-state would be a violation of the energy-conservation law. For example, a postage scale can become un-calibrated if an internal spring becomes permanently deformed --but it takes energy to do that to that spring.

to be clear, there are scads of energy around. They run an amp or two of current through the cells, which heats things up nicely. They use 'caustic' electrolytes, which means there is chemical energy available from the hydroxide dissolution reactions. There is the exothermic heat of absorption of hydrogen in palladium (when they use Pd electrodes). there is the usual heat of solution for dissolution of things not in the electrolyte but in contact with it. Yes, energy is involved, but you don't need to apply any extra to get the effects we see. What I was completely unclear on is why you thought you needed to emphasize that energy is ALWAYS involved. What was your point?? 18:39, 22 January 2009 (UTC)

The point is, any calorimeter worth its salt would be accommodating of the amounts of heat it is designed to measure. Only something out-of-the-ordinary should upset its calibration. It is the "out of the ordinary" which would involve extra energy, such as I was talking about. And yet you are claiming that the ordinary heat it is designed to measure can always throw it out of calibration. That just plain is-not-logical, for any properly designed and built piece of equipment. Especially since calibration errors should 50/50 go both ways. If someone is willing to say "heat is mysteriously appearing" when the change is +1.7%, why wouldn't that person say, "heat is mysteriously disappearing" when the change is -1.7%??? YOU might say, "In CF research, nobody would want to make such a claim", but \*I\* would say, "The researcher who sees that should realize that the +1.7% measurement could be erroneous, too." Therefore I'm willing to submit to you the possibility that those researchers have NOT seen energy-loss measurements, thereby indicating the CCS mechanism is only hypothetical and not at all real.

" In other words, you are presuming your answer, in order to obtain your answer, which thereby hides the energy that causes the "problem" that you claim means no significant energy was produced! "

Sorry, you're clueless here. I tested the hypothesis that a CCS could account for the excess heat signal, and found it workable. Now, more testing must be accomplished in order to verify one or the other explanation.

I am far from clueless, although I readily admit to not knowing everything. The most important thing about Science is that it makes logical sense. (Even Quantum Mechanics can make logical sense, but it depends on the approach.) And very frequently in Science, multiple variables are involved. Experimenters try to make sure they know what the relevant variables are, in their experiments. Certainly the CCS mechanism would count as an additional variable for CF researchers to include. If it was real and not just a postulate. And if it was relevant across the board. TWO important things to prove, not just assume. Have you ever seen the CCS mechanism in NON-CF experiments? Has anyone? In the last hundred years of calorimetry, why would the CCS mechanism have waited to appear only in CF experiments?

"Certainly the CCS mechanism would count as an additional variable for CF researchers to include." - Thank you for noting my PRIMARY POINT. The CCS potential error **MUST BE INCLUDED** in analysis of calorimetric data. The CFers **DON'T DO THAT**. The don't even acknowledge it is real (which it is any everyone who can do arithmetic realizes!). So congrats, you are two or three steps ahead of every Cfer in understanding their calorimetry. Kirk shanahan (talk) 18:39, 22 January 2009 (UTC)

Jumping to conclusions, are you? Must I always precede a statement with an if, instead of qualify it afterward?

"If it was real and not just a postulate. And if it was relevant across the board. TWO important things to prove, not just assume." - have I ever said anything else?!?!?

To the best of my knowledge, you have indeed elsewhere indicated that your CCS work practically/automatically means a very large percentage of CF experiments must be dismissed without examination, to see whether or not CCS is indeed a factor. HERE, of course, you are being more careful.

My whole point towards the field is that they need to stop claiming a nuclear process is proven. There are still potentially valid questions that they refuse to even acknowledge, much less address. The next to the last paragraph of my first publication ends with: "Additional data must be acquired and further analysis performed before the claim of Storms that platinum shows evidence for cold fusion can be taken seriously." The last paragraph says: "It [this paper] establishes the requirement that the statistical variation in calibration constants must be explicitly determined and considered,". Kirk shanahan (talk) 18:39, 22 January 2009 (UTC)

"Not very scientific"

Perfectly scientific. The fact you don't understand this really disqualifies you as an editor of scientific topics. You should recuse yourself.

Au contraire. Because Science is logical. And various things written above indicate you are failing at it, not I. V (talk) 19:01, 21 January 2009 (UTC)

It's comments like these that lead me to stop communicating. The above is

nothing but an ad hominem attack. No explanation, no discussion, just "Kirk is a nasty boy". I've learned to give up when the discussion reaches this level. The use of ad hom. attacks indicates the user has run out of real arguments. Kirk shanahan (talk) 18:39, 22 January 2009 (UTC)

And your own comment that preceded mine was not an attack, based on a faulty assumption about my level of ignorance? If you can't take it, why are you dishing it out?

And your own comment that preceded mine was not an attack, based on a faulty assumption about my level of 'scientific-ness'? If you can't take it, why are you dishing it out? Kirk shanahan (talk) 12:39, 23 January 2009 (UTC)

V, it's not as simple as putting all the crap in a special vessel and reading its temperature from an LCD on the outside as the final word on the enthalpy change. You have to do a number of calculations which rely on a number of *assumptions*, which may not be true. See, for example, the comments in this ([http://www.sciencedirect.com/science?\\_ob=ArticleURL&\\_udi=B6TGF-4KBT1J4-6&\\_user=10&\\_rdoc=1&\\_fmt=&\\_orig=search&\\_sort=d&view=c&\\_acct=C000050221&\\_version=1&\\_urlVersion=0&\\_userid=10&md5=64ac9816d27d1d665cc43a9d774c45d3](http://www.sciencedirect.com/science?_ob=ArticleURL&_udi=B6TGF-4KBT1J4-6&_user=10&_rdoc=1&_fmt=&_orig=search&_sort=d&view=c&_acct=C000050221&_version=1&_urlVersion=0&_userid=10&md5=64ac9816d27d1d665cc43a9d774c45d3)) totally independent paper from the literature from 2006:

I'm perfectly aware that even an ordinary thermometer can get out of calibration, simply due to expansion or contraction of the material upon which Degrees Celsius have been inscribed. On the other hand, for such a decalibration by that mechanism to become noticeable, the thermometer would probably break first, due to being far outside its designed temperature range. And yes, I know that measuring joules is not the same thing as measuring temperature. But the principle just described is what I was talking about, in saying that appropriate equipment can be expected to stay within its calibration range for the duration of an experiment. IF IT HAPPENS that calorimetry is a bit more sensitive to decalibration than simple thermometry, then the experimenters should know about it!!! So, for an experiment in which the researcher knows that it takes hundreds of hours to load palladium with deuterium, and when the researcher KNOWS about calorimetry sensitivity, then it would be logical to do a recalibration at some point reasonably short of the minimum expected time (e.g., at 250 hours when 300 hours are expected to pass). Do no researchers do anything like that at all? If not, then the assumption about calorimetry sensitivity --the foundation of your CCS work!-- needs to be revisited, as it is possibly an erroneous assumption. Simple logic.

*It has been shown that the error in all aforementioned quantities reflects on the baseline and it can have a very serious impact on the accuracy of the measurement.*

The fact is that the large majority of cold fusion experiments were done using dodgy calorimetry. And the other fact is that you only need a fraction of cold fusion experiments making calorimetry mistakes, and ordinary publishing bias does the rest. Calorimetry mistakes happen all the time, but Jed would have us believe that cold fusion researchers are uber duper careful and have eliminated all potential mistakes in most experiments! Because the heat is "real" to him, all 4000 or so of the papers that show heat must be real too, regardless of how shoddy the calorimetry, not just the bare handful of careful experiments. Do you see the problem? I really hope that you do (<http://www.springerlink.com/content/g15h6221514rm386/>) . Phil153(talk) 23:11, 21 January 2009 (UTC)

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## V's ad hominem attacks

As you probably observed above, V has devolved to ad hominem attacks.

As you probably observed above, Kirk devolved to personal-attack mode first. Per this quote (from a movie, not necessarily from a real famous person) at <http://www.quotes.net/mquote/96963> one can conclude that Kirk has lost this little debate. I explained how his argument was illogical, and then referred to that in saying "various things written above indicate" a failure on his part to be logical. Is it not perfectly logical that if one presents an illogical argument, then that one is exhibiting illogic? How is it an attack to describe a truth? Perhaps it can be concluded that to assume a statement-of-truth is an attack is to exhibit even MORE illogic....

"I explained how his argument was illogical" - No, you didn't. Your explanation/commentary was filled with errors I have been trying to correct. For example, see below. Kirk shanahan (talk)

To show "I can do it to" I added a few of my own, but what was really amusing was the last one I added today. I only had to alter one word to keep the chain going! Seriously though, V fails to understand a core concept of science, namely that you must quantify as best you can your errors in any scientific study, and then NOT draw conclusions from signals 'in the noise'. That is one of Langmuir's signs of pathological science. This is why the CCS issue is so crucial to understanding what is and isn't reliable about cold fusion claims.

It is only crucial if it actually is real. So far you have a hypothesis that you ASSUME is real. Whether or not CCS was actually present in Storms' experiment would involve checking the calibration of that calorimeter right after the experiment. If it wasn't done then TECHNICALLY your explanation for the results of the experiment is no better than Storms' explanation. And there is a more-fundamental issue involved, which has hardly been discussed at all. "Extraordinary claims require extraordinary evidence." This is a mantra often spouted by anti-CF people, and *it is reasonable only so long as the claims truly qualify as extraordinary*. In the case of CF, the rationale for insisting the claims are extraordinary was entirely based on the lack of a good explanation for how it could possibly be true. But that has changed, per <http://www.infinite-energy.com/iemagazine/issue81/index.html> So long as the claims were truly extraordinary, one could rationally promote all sorts of hypotheses for why an experiment must be faulty. When "extraordinary" is no longer an aspect of the situation, though,

then data such as Storms' does not need to be automatically assumed to be erroneous in accordance with a hypothesis such as CCS. Certainly it might actually be erroneous. But it is no longer reasonable to automatically ASSUME it is erroneous. V (talk) 15:52, 23 January 2009 (UTC)

"It is only crucial if it actually is real. So far you have a hypothesis that you ASSUME is real." - You really haven't got it yet after all these words and time? Once more, primarily for the interested observer, as V is emotionally committed to his interpretation, and I have given up hope he will understand it.

The CCS is mathematically true.

Oh, what fun! NOW you say it is mathematically true, when before you called it a postulate, or a hypothesis. INCONSISTENCY IS A SIGN OF BAD LOGIC. I hardly need more than this proof to support the factual statement that you indeed do exhibit illogic, sir.

Ummm..dude, go back up to "The Major Fallacies..." and read down a few paragraphs to where I talk about  $P=5x+3$  vs.  $P=4x+3$ . The next sentence starts with 'clearly', which in my book means 'patently obvious to anyone and therefore universally true'. You see why I say you're not understanding what I write? Kirk shanahan (talk) 20:09, 23 January 2009 (UTC)

It was shown to be fully explanatory for the Storms Pt CF claim in the 2002 publication. In other words, the CCS is real.

More obviously-bad logic. The fact that data supports a hypothesis does not make the hypothesis real. By that logic, I can say I have here a lucky charm that keeps the Earth from blowing up. Riggghhhhtttt....

OK, we have a different definition of 'real' I guess. Clearly, the nuclear hypothesis is not real either. What I mean by real is reasonable and rational. Since the CCS is mathematically true it is reasonable. Since the magnitudes determined for it so far are right in line with observations, it is reasonable. And, by that definition, the nuclear hypothesis is not real. Kirk shanahan (talk) 20:09, 23 January 2009 (UTC)

The experimental apparatus it was shown to be true in was one of the better ones of its type.

The calorimeter may indeed have been one of the better ones of its type. But the statement "[CCS] was shown to be true in [that apparatus]" is not proved at all, unless, as I indicated elsewhere, a calibration test was done right after the experiment, and an out-of-calibration situation was discovered that matched the CCS explanation for the experimental results.

Umm, as I said, it essentially was. Kirk shanahan (talk) 20:09, 23 January 2009 (UTC)

Therefore, it is reasonable to worry that it will be active and functional in those other similar apparati IN ORDER TO NOT BE MISLEAD INTO FALSE CONCLUSIONS.

GIGO ("garbage in; garbage out") Even perfect logic requires a valid assumption to reach a valid conclusion. Since you have in fact not actually proved that CCS was a factor in Storm's experiment, it is premature to reach such a conclusion. You haven't even so far responded to points I raised about separation of the electrodes (how close?) and oxygen-gas collection/measuring (did they), which would offer additional ways for you to support the CCS hypothesis.

Most people who see a successful application of an alternative interpretive method assume there is reasonableness in the alternative method, unless it can be shown otherwise. That is a common problem in science, some dude comes up with an equally valid but different explanation for your results, and it's back to the lab to try to figure out how to redo the experiments so they will distinguish between the options. Kirk shanahan (talk) 20:09, 23 January 2009 (UTC)

How close? Very. In most designs, possibly excluding some of the Szpak work, the wrap a Pt anode around the Pd cathode to supposedly minimize electric field gradients which allow H to escape from the Pd. I think the standoff distances are on the order of a few mm. O<sub>2</sub> collection/measuring? They have never measured O<sub>2</sub> content of the offgas to my knowledge. To my knowledge, only in the one case I mentioned regarding the external recombiner that Szpak et al used do they reconvert the H<sub>2</sub>/O<sub>2</sub> to water and try to measure it, but they (Szpak) don't even do that routinely, vis-a-vis the Miles comments. Mostly, they just measure how much water they have to add to the cell to keep a level constant, and as I noted before that is a confounded measurement. The Miles technique is the usual one. Kirk shanahan (talk) 20:09, 23 January 2009 (UTC)

Likewise, the apparatus was not that different from open cell calorimetry setups. Since similar results are claimed for those, it is also very reasonable to ask if the CCS is active there too.

And it is equally reasonable to ask why it should be assumed present, when its actual existence is yet to be proved.

One good reason is that the only other alternative available is considered unreasonable. But note that one is not choosing which is correct, one is testing both to determine if one can be eliminated. Kirk shanahan (talk) 20:09, 23 January 2009 (UTC)

In other experimental setups, similar results are claimed. Could the CCS be active there? Possibly, but we'll never know if the possibility isn't investigated. Now, I did investigate that superficially in the rebuttal to the SMMF publication that denigrated the CCS idea without explaining why, and I found the CCS to be reasonable at the level I could attain with only cursory data available. Other than that, there is no other evaluation of the CCS reported, which is interesting, since it came out 7 years ago.

That is indeed interesting. Perhaps an obvious explanation for why the CCS idea can be denigrated has already been presented on this page, and you didn't notice. Here, I'll quote it:

The point is, any calorimeter worth its salt would be accommodating of the amounts of heat it is designed to measure. Only something out-of-the-ordinary should upset its calibration. It is the "out of the ordinary" which would involve extra energy, such as I was talking about. And yet you are claiming that the ordinary heat it is designed to measure can always throw it out of calibration. That just plain is-not-logical, for any properly designed and built piece of equipment. Especially since calibration errors should 50/50 go both ways. If someone is willing to say "heat is mysteriously appearing" when the change is +1.7%, why wouldn't that person say, "heat is mysteriously disappearing" when the change is -1.7%??? YOU might say, "In CF research, nobody would want to make such a claim", but \*I\* would say, "The researcher who sees that should realize that the +1.7% measurement could be erroneous, too." Therefore I'm willing to submit to you the possibility that those researchers have NOT seen energy-loss measurements, thereby indicating the CCS mechanism is only hypothetical and not at all real.

In my 2002 proposal, I explained how a shift in the heat distribution in a cell can produce a CCS. That is also mathematically true (and has never been challenged). So, if the CCS is true, and there is a known true way to get it... And by the way, don't we all agree that "something out of the ordinary": is going on in these experiments? And it doesn't REQUIRE extra energy, just a CCS (which occurs within the framework of the available energy).

Re; "properly designed and built" - the point is that there is a new error type possible in these configurations. Kirk shanahan (talk) 20:09, 23 January 2009 (UTC)

You do bring up one point I have not touched on here before, but is the subject of my Figure 1 in my 2006 publication. The distribution of CCS changes is one-directional, so I would not expect a CCS, if produced by the chemical/physical processes I described in my pubs., to produce a negative excess heat signal. It is not random, it is guided by chemistry. This comes about because the norm in calibration is to use a 'dead' electrode, i.e. one that shows no apparent excess heat. Kirk shanahan (talk) 20:09, 23 January 2009 (UTC)

To put it bluntly, once an error is shown to be present, it must be assumed it arises from a real physical/chemical process, and that those processes or variants of them could be active elsewhere. Therefore, every researcher in 'the field', being loosely defined as anything that the error might impact, needs to evaluate the how big that error is in his/her apparatus.

AGREED, " *once* an error is shown to be present". Which you haven't actually done yet.

You've completely missed the point V. I HAVE shown it is present in 1 case. I have REASONABLY extrapolated it to similar cases. As a critic, that's all I need to do. The rest is up to the actual CFers. End of story. Kirk shanahan (talk) 20:09, 23 January 2009 (UTC)

To look at it a different way, the CFers are claiming that the signals observed, which we all attribute to a systematic error, are in fact new and revolutionary nuclear processes. First, before doing that, they need to show it really isn't the CCS everybody thinks it is. As I said in the Conclusions of my 2005 comment, "...the conclusion that a nuclear process has been proven is premature." Kirk shanahan (talk) 17:02, 23 January 2009 (UTC)

Only when there is no good explanation for CF signals is it perfectly logical to attribute CF signals to a systematic error. But that logic breaks once such an explanation exists. Do you recall why Newton had to distinctly specify the First Law of Motion, when it is implicit in the Second Law? It is because he was going up against Aristotle's very-long-standing claim that it was natural for moving objects to lose speed unless effort was continually applied. Newton's First Law both overthrows Aristotle's rule and explains the data in a new way, that moving objects typically encounter external resistance to their motion. Similarly, the hypothesis that all CF signals must be erroneous obviously demands additional hypothetical explanations for how they are erroneous. Yet that initial hypothesis can be overthrown by a good alternate explanation for the data (that is, not the mere claim that fusion is happening, but an explanation for HOW it could happen).

No, hypotheses are not overthrown by proposing alternatives. Each hypothesis should be independently tested. However, in the real world there are some economic constraints on this, so sometimes the 'believability' of the hypothesis comes into play. prior to my demonstrating the CCS effect in Storms' work, there was no acceptable

conventional hypothesis for the apparent excess heat signals, so a new and unexpected energy source was the only potulate around. Today, there is a contender. Kirk shanahan (talk) 20:09, 23 January 2009 (UTC)

"Whether or not CCS was actually present in Storms' experiment would involve checking the calibration of that calorimeter right after the experiment." - No, because right AFTER the experiment, the chemistry causing the CCS is gone. In fact that's what Storms' did, he did check it, and he got variation, variation that was of the same exact size that ended up explaining his apparent excess heat signals via a CCS. Kirk shanahan (talk) 17:02, 23 January 2009 (UTC)

Please be more clear. You seem to be saying two contradictory things. If Storms measured a calibration variation that directly explains the 1.7%, then it would be irrational to report the result, and irrational for a peer-reviewed publication to accept that report. If he measured a smaller calibration variation, then that means at least some of the 1.7% must have been real. Are you actually expecting someone to believe that an instrument that becomes even-more decalibrated during an experiment can magically recalibrate itself by itself afterward, to hide the fact that it had been uncalibrated?????

V! Hello! Are you there! The CCS is caused by chemistry, chemistry that derives from running the experiment. If the experiment is over, the chemistry is gone, and so is the CCS! When the CCS-causing chemistry goes away, the cell returns to normal, i.e. the calibration as done with a dead electrode. Simple isn't it. To be exact, citing from my paper, which replicated Storms', the calibration constant for the cell derived via electrolysis (i.e. with a dead electrode) was 2.19% greater than that theoretical at 20C; the value obtained after the experiments by electrolysis was 1.71% above theoretical; and the value obtained from a Joule heater was 3.46% above. The spread of the 10 shifts needed to zero out the excess heat signal was -2.5 to +2.33% of theoretical. But all of the CCS constants were below Storms' initial electrolysis constant, which is what he used to interpret data. Now, with these variations Storms' published, although it was in an ICCF conference Proceedings. I won't comment on the rationality of this. Kirk shanahan (talk) 20:09, 23 January 2009 (UTC)

"your explanation for the results of the experiment is no better than Storms' explanation" - Which of course, is my point. Turned around, it means my explanation is as good as Storms'. Kirk shanahan (talk) 17:02, 23 January 2009 (UTC)

I will not disagree with that, except for the matter that you apparently have not actually proved CCS is real, while now we have a good hypothesis to explain why Storms' and others' data need not be arbitrarily declared unreal.

Re: the 'extraordinary claims' discussion. In most scientific research, there is a building upon what has been discovered before. The results one gets correlate to those others got, perhaps as interpreted through a theory. That correlation is a datum, which separately adds to the confidence of one's conclusion. In the CF case a large block of prior knowledge is claimed to be not relevant. Thus, the number of correlations to prior data is greatly reduced, and that lost confidence must be replaced with more supporting data. That's why "extraordinary...". The theories currently proposed to explain CF are numerous, not accepted, sometimes contradictory, and based on cherry picked data, thus they don't add a lot of confidence, if any. Kirk shanahan (talk) 17:02, 23 January 2009 (UTC)

No objection to your remarks about those other hypotheses. The particular hypothesis to which I am referring has only one real problem so far: not many know of it yet. There is some

discussion about it on my talk page here. Would you care to add something to it (point out an error in fact or logic)?

"Storms' does not need to be automatically assumed to be erroneous in accordance with a hypothesis such as CCS" - I never assumed such. I used his data straight, no alterations. What I assumed need testing was his assumptions, and I was proven right. Kirk shanahan (talk) 17:02, 23 January 2009 (UTC)

FALSE. "the signals observed, which we all attribute to a systematic error," is what you wrote above, thereby proving that you are seeking ways to show that an error must be present, since *that is your primary assumption*.

"But it is no longer reasonable to automatically ASSUME it is erroneous" - It is, however, required that real errors found in other work that rationally might be present in yours be evaluated. Kirk shanahan (talk) 17:02, 23 January 2009 (UTC)

AGREED. As long as we are talking about real and not hypothetical errors. V (talk) 19:00, 23 January 2009 (UTC)

For the interested reader who's made it this far. By now I hope you can appreciate the impact of my discovery of a CCS in Ed Storms' work. Because of uncertainties in its extent, it has to be checked for before conclusions can be drawn. This has cast a nasty big old black cloud over all cold fusion calorimetry, because I have redefined the error bar to be much larger than was thought. That's tough to handle if you are a CF researcher, but life ain't easy. Kirk shanahan (talk)

It's hard to appreciate theoretical predictions which the people making the empirical measurements say simply are not explaining the measurements. You seem certain that you will always be able to produce an alternative explanation. What do you say about the researchers who have tried to verify your predictions and have been unable to do so? GetLinkPrimitiveParams (talk) 22:21, 23 January 2009 (UTC)

Another late response, sorry.

Umm...and who might that be?? To my knowledge, NO ONE has tried to do so. Kirk shanahan (talk) 18:43, 27 January 2009 (UTC)

P.S. There isn't much that needs to be done. Check their calibration data for the extent of variation, see if the apparent excess heat signal is reasonably explained by that (being generous on 'reasonable' since we are working with anomalies.) Kirk shanahan (talk) 18:43, 27 January 2009 (UTC)

Prior to my publication of 2002 which delineated the CCS in action, one could reasonably assume the cold fusion researchers had adequately characterized their calorimetric error. They were in fact following the norm in taking the baseline noise as the 'error'. However, my paper showed that this was not correct, in fact the baseline noise was significantly smaller than that induced by the CCS. Therefore, if they were 'good' scientists the CFers would stop and reconsider, and then move forward with the new error limits in mind. Instead they continue as before, trying hard to ignore Shanahan, and when they can't, they run him down. You decided if that is 'good' science.

Also, someone, I don't recall who right now, has criticized me for using the term 'CFers'. I use it because it is an abbreviation of the term 'cold fusioner', which was coined, I believe, by Eugene Mallove, former Scientific Press Officer at MIT who resigned because he thought the negative MIT results on CF had been deliberately

fudged, and the founder and first editor of Infinite Energy magazine, and author of some proCF books. They picked the term, I just use it, so stop criticizing me for that. Kirk shanahan (talk) 13:18, 23 January 2009 (UTC)

P.S. CFer is also a contraction of 'cold fusion researcher', so even if someone decides Mallove didn't coin it and 'cold fusioner' is derogatory, I'll still use CFer. Kirk shanahan (talk) 17:17, 23 January 2009 (UTC)

## A Modest Condensation

Due to various repetitions above, I'll try to condense certain points here. First, I wrote:

"In other words, you are presuming your answer, in order to obtain your answer, which thereby hides the energy that causes the 'problem' that you claim means no significant energy was produced! Not very scientific."

Later, you wrote:

"In my 2002 proposal, I explained how a shift in the heat distribution in a cell can produce a CCS. That is also mathematically true (and has never been challenged). So, if the CCS is true, and there is a known true way to get it... And by the way, don't we all agree that "something out of the ordinary": is going on in these experiments? And it doesn't REQUIRE extra energy, just a CCS (which occurs within the framework of the available energy)."

And now I get to write (perhaps you should read the second paragraph below first):

***You say you need a real heat-distribution-shift to be present*** in order for the CCS to become a factor. I'm aware of the difference between the meaning of "a shift in the distribution of existing heat", and "extra heat". I'm also quite aware that by definition a heat-distribution shift, throughout an electrolysis cell, means that some parts become cooler while other parts become warmer, if one is to say that no excess heat has appeared in the cell. Yet you have not been consistent with that definition; you have explained only where some EXTRA heat might come from (a hydrogen-oxygen chemical reaction). Also, you have not explained why this must be associated with a cooling in some other part of the cell, or how such cooling could happen, so that there is merely a heat-distribution "shift". AND THEN, once you have a hot spot to cause the CCS effect, you try to use CCS to say that no significant heat appeared, pretty much as I wrote in the first paragraph above. ***Of course*** from such contradictions I conclude your logic is faulty. I don't have to care at all, not one whit, about what SORT of extra heat might have been present to cause your CCS effect. ***All I need know is that SOME extra heat appeared in fact -- and you agreed with it! -- to conclude that your method cannot possibly work to explain that quantity of excess heat out of existence.***

Nevertheless, I'm willing to think that with some rephrasing of statements on your part, your goal was not so much to explain all excess heat out of existence, but instead to show how the magnitude of excess heat could be explainable in terms of chemical and not nuclear reactions. The whole idea that CF can happen ultimately depends on there being THAT much excess heat, after all. In that small percentage of experiments where it is claimed something as extraordinary as (possible exaggeration) electrolyte-solution-boiling happened, I doubt even your CCS idea can be stretched far enough to explain that. Certainly one would not need a calorimeter in such an experiment, to realize that a huge amount of heat was coming from somewhere. I note that those are also the experiments where it is claimed that the electrolysis can be switched off, and heat continues to appear for quite a while (an hour?) afterward. It should be obvious that if hydrogen had been combining with oxygen to create heat in accordance with the CCS idea, then that would STOP

almost as fast as turning off the electricity to the cell. Obviously, we need more of that sort of experiment!

Next, regarding your mathematics, I'll start by half-jokingly noting that it is of course obvious that  $2+1=2$ , since two hydrogen molecules plus one oxygen molecule yields two water molecules (heh, also  $2+2=2$ , when the result is hydrogen peroxide). Obviously, *math can require referents to make sense*. In a more serious vein, there's a web page I'd like for you to at least skim through, because in one sense it is highly relevant. This page (equations and all) was created (probably a copyvio) from a magazine article that was published in 1962, and its math has never been seriously challenged (a couple of minor typos were pointed out in later issues). Well, this article indicates that we should be able to make a device that looks like a "reactionless drive" --if the math was perfect and unchallenged, why don't we have any such devices more than 45 years later???

<http://www.rexresearch.com/dean/davis4.htm> One answer, highly relevant here, is that mathematics, like logic, can be perfectly correct, yet lead to a wrong conclusion when an initial assumption or concept or data-item is faulty (GIGO). I do not yet know what initial information and concepts you used for your CCS idea.

I do know of another way to test part of what you wrote in our previous discussion. Simply take the electrodes used in the experiment, before and after it, to an X-ray spectrometer. Look for an addition of the contaminants that your CCS idea requires to be present, particularly contaminants able to catalyze the the hydrogen-oxygen reaction. V (talk) 20:20, 25 January 2009 (UTC)

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Writing now for the readers who are following this debate: I have to ask myself what benefit I am getting from doing all this writing, and the answer is 'none'. I originally got involved naively thinking I would edit the 'anti' section and be done, but this has dragged on forever. I am going to respond to V one last time and that is it.

So far V, you don't seem to have grasped what I am communicating. For example, you wrote:

“Yet you have not been consistent with that definition; you have explained only where some EXTRA heat might come from (a hydrogen-oxygen chemical reaction).”

What I have explained it that in P&F type cells, open or closed, the possibility of a CCS must be considered. The CCS does not arise due to extra heat, at least in a closed cell. That is the part-gets-hotter-part-gets-cooler idea you mentioned (except that I get the impression that you think an actua cooling method must be injectd into the cell to accomplish this, which is not true). In an open cell however, the heat 'shift' that occurs is that the 'heat', i.e. energy, that is normally carried away outside of the cell through the gas vent now appears in the cell itself, inside the calorimter boundary. This heat is then mathematically double-counted (with possible scaling errors from a CCS) by the measuring of this previously non-existent heat in the cell and the continued accounting for it that occurs in the calibration equations. In fact, I suppose that it is not even really a 'CCS', but it certainly isn't proper calorimetry. The 'CCS' in the open and closed cells in my proposition are related by the fact that I propose both are caused by under-the-surface, at-the-electrode chemical recombination. It is also possible in cells to get catalytic deposits formed in the gas space that would promote the chemical recombination. (This is exactly what a recombination catalyst does in a closed cell.) Whether that would shift the distribution enough in closed cells is an open question, but it would definitely impact open cells. The point is that this is all just a redistribution of energy put into the system by the electrolysis power supply. The 'cold fusion' explanation however, postualtes a new energy source, as you keep referring to. Again, my explanations are all done without any extra energy source. But the fact is that I have explained this ad nauseum. Somehow however, you keep missing it. This was the last time I try to get it across. So to conclude my response to your first paragraph, I propose that NO EXTRA HEAT appeared in the cell outside of that supplied by the electrolysis power supply.

On to paragraph 2. “Nevertheless, I'm willing to think that with some rephrasing of statements on your part, your goal was not so much to explain all excess heat out of existence, but instead to show how the magnitude of excess heat could be explainable in terms of chemical and not nuclear reactions.”

As noted above, this is not my position. At this time, because no CFer has considered my proposal seriously, the origin of all apparent excess heat signals is suspect. In the case of the Storms data set I reanalyzed, it would seem to be a false signal generated by a CCS. My goal was to get CF researchers to recognize a previously unrecognized error source in their data analysis methodology that has the potential of explaining all apparent excess heat results to date. I tried to achieve this goal by 1) explaining the error, 2) demonstrating it in action in the Storms research, 3) proposing rational and reasonable mechanisms for it to occur, and 4) publishing it in the open literature.

What I wrote above was due to a possible misunderstanding about how you were using the term "excess heat". To get at that, first there must be an agreement about "ordinary heat", which I at least had thought was that which results from electrical resistance in the electrolysis cell; this production of heat should be constant as long as the cell runs at a constant voltage/ampereage, and the electrolyte in the cell is maintained at a fixed level. Do note that while electrolysis itself is the conversion of electrical energy into chemical energy (gas molecules separated from water), it does not involve HEAT (all associated heat is already included in the electrical resistance thing). So, "excess heat" can be anything beyond that "base" level, with one caveat; it is known that when hydrogen permeates palladium, this is a modestly exothermic event, which should add to the measurable total heat. However, the rate at which this heat is produced should drop off as the palladium becomes more and more loaded-up with hydrogen. That takes us almost back to the original "base" level, beyond which anything at all might be called "excess heat". OR, "excess heat" can be something not explainable by chemical reactions such as hydrogen-plus-oxygen-yields-water. Obviously if that or any other exothermic reaction occurred, energy can be added to the system above the base level represented by the system's electrical resistance. The added amount can either be explained by chemistry (it would not be "excess heat"), or it cannot be explained by chemistry (it would be "excess heat"). I'm pleased that you have now clarified your meaning. However, your "final rebuttal" here is essentially you simultaneously saying that no excess heat above the base level occurs, and that nevertheless the water-producing reaction occurs. ***You cannot have both without violating the Energy Conservation Law of Physics.*** And therefore you have lost this debate, period. Nobody ever again needs to take your illogical/nonscientific CCS idea seriously. —Preceding unsigned comment added by Objectivist (talk • contribs) 19:12, 26 January 2009 (UTC)

Saying it is so doesn't make it so. Kirk shanahan (talk) 21:16, 26 January 2009 (UTC)

Say anything you want, now. The record is clear, that you claim hydrogen can chemically combine with oxygen inside a system (underwater at surface of electrodes, via catalysis) --one of the most exothermic reactions known!-- without contributing detectable heat energy to the system. Unless that reaction's rate is miniscule (and thus irrelevant to any OTHER purpose), you are outright/totally wrong about the physics of the situation.V (talk) 14:48, 27 January 2009 (UTC)

Proof positive that V hasn't understood a thing I've written. Kirk shanahan (talk) 16:33, 27 January 2009 (UTC)

Proof positive that Kirk hasn't written anything actually understandable, in terms of real Science. V (talk) 17:04, 27 January 2009 (UTC)

With regards to the Wiki article, my goal was to present the available conventional explanations for any data proposed to support the nuclear postulate for the Fleischmann-Pons-Hawkins Effect.

“I doubt even your CCS idea can be stretched far enough to explain that.” Your doubt is insufficient to negate the need to check for it. You seem to be laboring under the assumption that some methods must be ‘bullet-proof’ in some conditions. In my professional career I have always been impressed by men’s ability to screw up an analytical method. So, I don’t assume, I check.

“It should be obvious that if hydrogen had been combining with oxygen to create heat in accordance with the CCS idea, then that would STOP almost as fast as turning off the electricity to the cell.” No, it wouldn’t. The electrodes are loaded with H when the power is cut. It comes out VERY SLOWLY, i.e., over many hours. (This in fact is a prime requirement for the ‘good’ electrodes.) As well, the calorimeter is now in a COMPLETELY different state. There is no way a cal. eqn. determined under active electrolysis could hold. Clearly, a ‘CCS’ situation if there ever was one. Also, I believe the ‘heat-after-death’ experiments are all done in open cells, which will allow atmospheric O2 to diffuse back into the cell, and THAT process can be hampered by the cell design. Many hours of ‘excess heat’ is not surprising at all under these conditions. To recap, all such experiments are done in an uncalibrated system. Any attempts to interpret the data in that condition are suspect.

From para. 3, “I do not yet know what initial information and concepts you used for your CCS idea. “ That’s your fault, not mine. For the Wiki article, you really don’t need to know that anyway, as the papers are published, and any such issue if serious should have either been brought out in peer review or in the attempted rebuttals.

Re. para. 4: That’s been done many, many times, including the fact that they have added nearly every element known to man to the electrolytes. They find all kinds of stuff afterwards. Originally, they (the CFers) made no claims about it, and the rest of us knew it was contamination concentration/deposition. These days they claim it is ‘heavy metal transmutation’. If you had read my 3rd paper, you would realize that the one new point I brought out in that was the fact that whatever the active state was that caused the CCS, it seemed to disappear when certain conditions were met (namely nearly 0 or 0 voltage or current). I had a Figure that illustrated how there were 3 sequences of 3 runs in the data separated by the points where that condition had been reached. This makes it very difficult to do what you suggest, as the actual active state seems to voltage dependent, and disappears when it reaches 0. Nasty problem. That requires in situ studies of the surface under actual run conditions. Not easy to do at all. But, one could still define the conditions necessary to get the active state without actually defining it. There’s lots of chemistry studies that have done just that in other areas. But, you do have to realize a) that it’s a surface effect and b) it’s not nuclear to want to do those studies.

That’s it, no more for me thanks. Kirk shanahan (talk) 17:00, 26 January 2009 (UTC)

## Constant calibration shift

Hi. I've started to read a little of the discussion about CCS at Talk:Cold fusion. I don't know why one editor says your theory is impossible to explain in an easily understandable manner: I haven't read any details about it yet (though I'm curious) but think that the name, "constant calibration shift", is self-explanatory. Also, I think the burden of proof is on the CF-ers to show that this is not happening.

I think you mentioned a Wikipedia page which had been deleted, which had information that would help explain things. If you can find the name of the page (or give some clues; there should be links to it from previous versions of the Cold fusion page) we can request to have it userfied (undeleted and place in a user's subspace) as was done for User:Abd/Calorimetry in cold fusion experiments. It might be helpful to userfy all the deleted cold fusion subpages. The purpose of userfication is to improve the article or to get material to put into other articles, i.e. generally to work on improving the encyclopedia; userspace is not part of the encyclopedia. ☺ Coppertwig (talk) 12:10, 17 March 2009 (UTC)

The name is 'calibration constant shift', not 'constant calibration shift'. The calibration constant is the experimentally determined number in a calibration equation. The shift is what is observed by the CF researcher Ed Storms in 'initial' and 'final' calibrations, and in 'electrolytic' and 'joule heater' calibrations. I reanalyzed his data to show that his observed shift could explain his apparent excess energy peaks. The concept of the shift impacting the signal is extensible to any calibrated method in any field. Quickly, at  $t=0$  you determine  $K = 4$ , so you need to multiply your observable  $O$  by 4 to get the correct answer. Then at time  $t_1$ ,  $K$  shifts to 5, but you don't know this because you haven't recalibrated. You run your experiment, multiply your  $O$  by 4, and get the wrong answer, because you should have multiplied it by 5. That's it, hard isn't it. Note that in the real world, recalibration is routine. People do that because they need to. How do they know they need to? They get inconsistent answers if they don't. Apparent excess energy peaks are those inconsistencies, traceable back to a calibration constant shift.

However, the problem with the Wiki article and the CF field is not just in the calorimetry. I will be overly bold and say in ALL cases, a conventional explanation for the reported result (which is them used to claim cold fusion has occurred) is available and has not been excluded. In Science when you have more than one available explanation, it is illegitimate to claim one is the only correct one. You need data to show that. No such data is available from the CFers. Ergo, no one should be claiming 'CF' is nuclear. They should be saying that more research is needed, research specifically designed to eliminate one or the other explanation. The Wiki article needs to have this spelled out for the reader, but people don't seem to understand what having the field decalred 'pariah' did to 'RS'. Kirk shanahan (talk) 16:13, 17 March 2009 (UTC)

Sorry I got the words in the wrong order, but your explanation matches what I thought (except that I was imagining adding a calibration constant to something, not multiplying). Now I'm curious as to exactly what is being calibrated and how, etc. I don't find any page named "Calibration constant shift" in the deletion log. Do you remember the name of the page you mean when you said "I also note in passing that the secondary page PCarbonn set up to isolate the impact of my work has now diaspeared as I predicted" at Talk:Cold fusion, or approximately when that page might have existed? Thanks. ☺ Coppertwig (talk) 17:43, 18 March 2009 (UTC)

It is the one that Abd tried to resurrect. Initially the parts on the Shkedi work were in the main article. Then I added an explanation of my work, and PCarbonn decided it suddenly was 'too big' and he clipped it all out and put it into the subpage. I protested as I feel it is unlikely that the average reader would read it, so the average reader would read the Cold Fusion page and get the impression that there was no strong conventional alternative to interpreting apparent excess heat data as a nuclear reaction. The reason P didn't mind the Shkedi work being in was that the CFers had done some studies to delineate it well, and showed it most likely was not important (which I agreed with in print in my 2002 paper). So what P had written into the article was more 'already answered' objections, instead of outstanding unresolved ones, giving the reader the impression that the CFers were slogging through their critics comments and resolving them ("as we all knew they would") as opposed to ignoring alternative explanations as they are actually doing. Kirk shanahan (talk) 19:57, 18 March 2009 (UTC)

P.S. I predicted the deletion because when I tried to edit the article similarly in 2006, the same thing happened. A subpage was set up (cold fusion controversy) that disappeared as soon as I stopped watching the page. Kirk shanahan (talk) 20:00, 18 March 2009 (UTC)

What is being calibrated is the calorimeter. There are multiple types, isoperibolic (original F&P, Arata 2008 demo, usually open cells where the D2 and O2 are allowed to escape to the atmosphere); closed cells where the gas is reconverted to water by an internal catalyst that usually use an integrating calorimetry approach like mass flow or Seebeck. Most use a simple

linear type calibration equation, with the exception of the original F&P design that used a more complicated form taking radiative losses into account. The linear form is  $P_{out} = m * "X" + b$ , where "X" depends on the type. The b is an additive constant like you were thinking, but that just causes baseline shifts (observed in real data), while the m factor causes the spurious signals that people call excess heat. You have to calibrate calorimeters because when you put a known  $P_{in}$  through it, and use the equations without the constants (i.e. absolute measurement) you don't get the full  $P_{out}$  value due to losses. The calibration equation 'adjusts' the real world data for this problem (this is standard procedure, absolute measurements are notoriously difficult, 99.99+% of measurements are done with calibration equations). Kirk shanahan (talk) 20:14, 18 March 2009 (UTC)

## Begin to narrow discussion

Let's start with CCS and how it affects excess heat findings. First of all, it's obvious that if the "calibration" of a calorimeter shifts, this will affect inferred absolute power levels. I.e. as I understand the matter, a known power is applied by some means, under conditions where the power is presumably totally converted to heat, and then the measured heat "power" is compared, and then a "calibration constant" is calculated so that measured power can be referred back to absolute generated power.

For various reasons, the conditions under which heat is generated during the experiment can shift in terms of how it affects the output power measurements. For example, suppose heat is generated in one portion of a cell, and isn't rapidly circulated, and sensors are located close to the heat source, or far from it. If the location of heat generation changes, the required "calibration constant" will change. There may be other changes which can affect the true calibration constant, and the degree to which they may apply will vary with the type of calorimeter and the nature of the heat source.

Am I correct so far? Please correct, if I'm not. --Abd (talk) 18:05, 22 April 2009 (UTC)

Your terminology is a bit sloppy, which could end up leading you down the wrong road, but we won't know that until you finish responding in this one instance to my challenge. Kirk shanahan (talk) 11:39, 27 April 2009 (UTC)

How about fixing the terminology first? I.e., I consent and request that you edit my statement above to make terminology more precise and accurate, rather than exploring wrong roads and wasting time. If not, well, I will then continue anyway. (If I don't agree with your edit, we should discuss that before going on. A house built on sand isn't going to serve well as a meeting place. Thanks. --Abd (talk) 15:03, 4 May 2009 (UTC)

That's not the challenge and I won't be drawn into your infinite loops of pointless discussions. You don't know what you're talking about and can't prove you do, but you expect me to fix what you write and then claim to have 'helped' and 'participated'. Nope. It's 'put up or shut up' time. Kirk shanahan (talk) 11:45, 11 May 2009 (UTC)

You've got it backwards, Kirk. My intention was to meet your challenge, directly, but I'd need your cooperation. You made the comment that my terminology was "a bit sloppy." So, before going ahead, I asked for specifics. Since you aren't willing, and seem to be more interested in establishing my ignorance, and you are, indeed, an expert, I'd lose that contest. There are kites you can fly. I have utterly no obligation toward you, nor to work toward inclusion of your work in the encyclopedia, which I have done to a degree and would do more, assuming I understood it sufficiently. Let me know, on my Talk page or by email, if you change your mind. I was taking you seriously. Not any more. --Abd (talk) 18:47, 11 May 2009 (UTC)

No Abd, you weren't taking me seriously. You were treating me like some neophyte crackpot instead of a PhD physical chemist who works with metal hydrides and has published papers in that area as well as in the area of systematic errors in cold fusion calorimetry. This while displaying a significant lack of knowledge of the field and my work. And, you write inordinately long and off-topic rants that people have difficulty following and that are rarely anything more than exuberant effervescence on how real cold fusion must be. I have stated before that you should not be editing the CF page, and I repeat that. I do so because you show no balance in your suggestions, and no desire to understand the underlying problems of the field (as was the problem with Pcarbon and V). I gave you the opportunity to correct me on that in the normal way, i.e. by proving you could state the case against apparent excess heat completely, including why my work impacts all such claims, and you have failed to pass the test. Yes, this is a test of your abilities here Abd. So far you have about 5 out of 100 points. Did your professors in school (I am assuming you went to college) have as much difficulty when you took their courses? Did they correct your essay answers after every 5th line that you wrote? No? Then I won't either. Prove you know something here or go away. (P.S. I find your comment on the CF Talk page about dropping out due to disruptive editors totally hilarious. You realize that is a spot-on description of you, Pcarbon, and V, (and others) don't you? The only way to present a balanced article on CF is to let both sides have their say. Neither side likes what the other has to say and Wiki policies allow edit warring to keep the side out that has the fewest proponents. Hardly a way to present a balanced article.) Kirk shanahan (talk) 19:21, 11 May 2009 (UTC)

## Abd shows his fanaticism

Transplanted from Cold Fusion Talk page:

" "Careless Clumsy Scientists"? " - Your words, not mine, but I'd tend to agree.

"I'm more interested in those neutrons." - What neutrons? There aren't any neutrons.

"I'm also more interested in how CCS can explain the very strong correlation between excess heat measurements and helium, specifically what Storms claims as  $25 \pm 5$  MeV/He-4." - What excess heat? There isn't any excess heat. And no heat means no correlation to He, which just comes from leaks anyway.

"How, in an extensive series of P-F cells, set up and measured in the same way, helium was only found in the cells that also generated excess heat." - There isn't any excess heat. There may be a CCS, whose speculative mechanism might explain why leaks develop, but I haven't thought that through.

"No matter how many of these findings are developed, critics like Shanahan find ever-more-preposterous ways to explain them away." - ad hominem - 'preposterous' is a value judgement, esp. from Abd, who can't understand my comments, as proven in the section above.

"Shanahan, however, is practically alone at this point, the only scientist who is making specific criticisms, at least we have to give him that -- at least with regard to calorimetry." - Yup, I'm the only one stubborn enough to try to present what the mainline scientist sees when he/she looks at CF.

"Nevertheless, the stretch gets greater and greater. For example, the long-maintained 4 degree C. temperature rise after the formation of palladium deuteride in the gas-loading experiments of Arata." - A) Data wasn't shown

for a long enough period to determine that, B) no proof that it wasn't thermocouple malfunction (needed to show T's returned to the same value).

"I believe that McKubre has confirmed that." - You believe a lot of things. Doesn't prove them.

" This is not an electrolysis experiment, there is no supplied energy, and only the natural heat of formation of palladium deuteride is involved: hydrogen shows the expected generation of heat as the gas is admitted to the cell, which then settles down, within hours, to ambient temperature." - As shown where?

"Deuterium shows the same kind of initial release, but then settles down to a steady generation of heat for thousands of hours, showing no sign of lessening." - thermocouple malfunction. Also, do they ever come back together?

"Absolutely, skeptics should give this every shot, but ... at some point skeptics need to do some experimental work themselves." - Not if the problems are obvious and clear.

"N-rays were debunked through careful experiment that showed the origin of the "effect." " - No, they were debunked by Wood palming the crystal that supposedly diffracted the n-rays, and having Blondlot excitedly point out the diffracted spots to him. (JIC, no crystal = no diffraction = no spots)

"Likewise polywater." - There was a good bit of polywater research that led to an alternate explanation, but the book I read suggested that the support of the idea was silenced by politicians of the Soviet Union, i.e. the original claimants would have persisted forever, just like the CFers.

"That was never done with Fleischmann's excess heat, and, indeed, the excess heat has been verified in 153 peer-reviewed papers, I'll provide a link to a list of them." - No need, I've read them. That's how I proposed a conventional explanation for them.

"Many of these reports are not just selected experiments, only showing "success": they show a series of experiments, reporting "failures" as well as "successes." " - And I agree that it takes a 'special active state' forming. What's your point?

"Where Fleischmann screwed up was in reporting neutrons. What we now know, quite conclusively, is that neutrons are rare, not normally produced in these experiments at levels sufficient to be considered anything more than a by-product (unless somehow they are efficiently "used")." - as in non-existent.

"What is produced is plenty of alpha radiation, starting with a Chinese group in 1990, and, again, how does Shanahan explain that?" - air leaks. (JIC alpha rad = He ions, neutralized later to atoms)

"Here is the press release:..." - goody.

"Krivit received a lot of attention for his presentation, his comments were widely reported in the media." again, goody.

"Journalists can be considered experts, though in a different way than scientists, as such." - in other words, journalists aren't science experts. So why is he giving a talk at what is, at least in other sections, a science meeting? Points out the problem with CFers, they try to use anything and everything (except good science) as support.

"Shanahan's view of the field is highly biased." - yes, towards good science, and all my comments are scientific in nature and subject to test and refutation.

"It's notable because it has been published, at least parts of it have." - Seems to be an out-of-place sentence. To what are you referring?

"Some of the criticisms he makes are odd." - ad hominem again

"The general field isn't "cold fusion," it is more commonly called "low-energy nuclear reactions," " - the CFers are trying to rename the field to avoid the associations that have been set up for the term 'cold fusion'. This was a tack adopted when Patterson had success patenting his gizmo in '95 by not mentioning it worked because of 'cold fusion'. My use of 'CF' is in alignment with mainline understanding. I don't support "science by bamboozeling".

"and Vyosotski's work with biological effects, which involve two different kinds of nuclear transformations, have long been of interest to LENR researchers as shown by the notice that Storms takes of it." - prove it is cold fusion and then you can include it in those discussions Until such time, it is just another anomalous result the CFers are throwing in the basket to try to bamboozle people with apparent numbers.

"(The forms are transmutation, as has been described above to some extent, and acceleration of radioactive decay, which is roughly possible in theory, i.e., chemistry is known to be able to affect decay rates under some circumstances.)" - supposition.

"I'm not surprised to see a session focused on reviews, " - A) they weren't in one section, they are scattered about. B) In a normal ACS session this doesn't occur. People present new research instead.

"because the goal of CF researchers there would be, not to present new research, unless it is truly striking as with the SPAWAR neutrons, a subject Shanahan notably avoids above," - no I didn't, I have clearly said there are no neutrons, starting in 2002.

"but rather overviews to "spread the word" to other chemists. The new research would be more likely to be presented at ICCF conferences." - only because the CFers are afraid of substantive critical comment from the more general ACS audience (if there was any).

"No suggestion is made that the papers presented at this conference are "reliable source," in themselves, that is, they are not peer-reviewed, nor are they, by virtue of presentation, "published." Rather what was the subject of my comment was the notability of the conference, due to media coverage and due to the obvious increase in attention paid by the ACS." - As I said on the CF Talk page, the only value the ACS session has to the article is another tick mark to the statement that the CF fanatics remain.

"Conference papers are primary source, occasionally they are useable, more often not, unless they are cited in a reliable source, in which case the full reference would include the citing source as well as a primary reference. --Abd (talk) 17:50, 18 May 2009 (UTC)" - Most of the editors who have opposed my attempts to edit the CF article have insisted that conference papers are not RS, since I tried to use some in specific instances as examples of what to do and what not to do. It amuses me that you say this, because it is not how it has worked in the CF article. There, it is: No conference papers!!! KirkShanahan (talk) 19:53, 18 May 2009 (UTC)

Kirk, you are treading on thin ice. That section header above might be seen as uncivil. Don't worry, I'm not going to complain, but someone else might.

So complain. A) I don't care. B) This is my Talk page, if I can't express my opinion here, then I may as well leave anyway. KirkShanahan (talk) 12:00, 19 May 2009 (UTC)

There are limits to incivility here, Kirk, and, yes, editors are warned or blocked for incivility on their own talk pages. I just said I wasn't going to complain, so why did you say, "So complain"? Is that really what you want? It could be arranged. But I would rather not. --Abd

(talk) 22:17, 19 May 2009 (UTC)

The following Abd response was properly indented for clarity by myself. KirkShanahan (talk) 11:34, 19 May 2009 (UTC)

*I have clearly said there are no neutrons, starting in 2002.* For our purposes, Kirk, you are not an authority, on your own.

Fanatic alert! - Let's revisit what was said, in full - "'...a subject Shanahan notably avoids above," - no I didn't, I have clearly said there are no neutrons, starting in 2002." - Abd, I am authority on what I've said and wrote. Your quote is out of context and therefore misleading. Keep it correct, and thereby keep it civil please, as this is an extended ad hominem. KirkShanahan (talk) 12:00, 19 May 2009 (UTC)

the following was split off from the preceding Abd comment in order that I might respond separately. KirkShanahan (talk) 12:00, 19 May 2009 (UTC)

This is the apparent fact at this point, subject to review. There are neutrons at a very low level, way below what was expected for ordinary fusion, way below what Fleischmann reported. This was the 2009 Mosier-Boss report, published in *Naturwissenschaften*. We would not use this, at this point, as an authority that neutrons are present, but only that they have been reported in a peer-reviewed article. There is no contrary work to my knowledge. There is plenty of work refuting higher levels of neutrons, which is why nobody in the cold fusion field is claiming that neutrons are emitted by the primary reactions taking place. There were long reports of very low levels of neutrons, and the Mosier-Boss report may be seen as confirming this, using a technique not so vulnerable to the problems of electronic detectors at low levels.

No, the apparent facts are that they have pits, and that they claim they come from neutrons, but there are two conventional explanations available that require no miracles to obtain, and that it is likely based on the CR39 in the gas phase studies, that both are active at the same time, so the mainline scientist will need studies to determine which of the three alternatives are present. Until such studies are supplied, the explanation requiring miracles need not be accepted. I.e. there aren't any neutrons. KirkShanahan (talk) 12:00, 19 May 2009 (UTC)

Hand-waving, Kirk. You gave no specific objection, but only a vague "three alternatives." And you are showing your ignorance of this research. I recommend reading the paper. Triple tracks in CR-39 as the result of Pd–D Co-deposition: evidence of energetic neutrons, Pamela A. Mosier-Boss & Stanislaw Szpak & Frank E. Gordon & Lawrence P. G. Forsley, *Naturwissenschaften* (2009), 96:135–142. You can find a copy hosted on [newenergytimes.com](http://newenergytimes.com), I'm not going to link to it because that particular link probably violates Wikipedia policy on external links. But it's there. No, it's not gas phase. There are controls. The alpha evidence, from prior papers, is long-term and has been confirmed by many groups. The neutron evidence is new, in fact. Because there is so much alpha radiation, if there has been enough activity to accumulate a significant number of neutrons, the plastic is heavily pitted from the alpha. I'm not going to describe the controls, read the paper yourself. But it seems nobody ever bothered before to look for rare triple-tracks among the copious pitting. Mosier-Boss et al look at the *back* of the CR-39, the side away from the electrode, and they look at the areas of the plastic on the front where major pitting is absent. Neutrons, of course, would penetrate to the back, whereas alpha particles would not. --Abd (talk) 23:00, 19 May 2009 (UTC)

I commented on this at 12:45 19 May 2009 (UTC) here on this page ( note your response was at 2300 19 May). I also commented on 22 Apr 2009 at 0302 UTC, see Archive 29. Gave directions there to go to spf. Let me know if you need me to teach you how to use Usenet.

'Nobody bothered' because nobody runs it the way the SPAWAR group does. Most get a couple of hundred pits on a plate. They get 10's od thousands. Kirk shanahan (talk) 19:42, 20 May 2009 (UTC)

What you are consistently failing to appreciate above is correlation and the use of controls. You can claim that some helium measurements are flawed by leakage, you can claim that excess heat is only an artifact -- or could be -- you can come up with this or that theory to explain away the experimental results, individually, but you haven't addressed, at all, the issue of correlation. Storms reports Miles in this way: 12 studies produced no extra energy and produced no extra helium. Of 21 studies producing extra energy, 18 produced extra helium with an amount consistent with the amount of excess energy. The exceptions were one sample having a possible error in heat measurement and two studies using a Pd-Ce alloy. This is a very strong correlation. How would your theory explain the correlation, or do you think it is sheer coincidence, or do you suspect fraud? Or take a look at McKubre's chart showing the relationship between energy and helium producing from finely divided palladium on carbon heated in D2 gas; there is a linear relationship showing about 31-32 MeV/He4 and, of course, any helium not captured and measured would increase the inferred energy/He4.

Another error packed paragraph, topped off with more ad hominem. Starting there - "consistently failing to appreciate " - No Abd, what you are failing to appreciate is my background in statistics. In fact ALL claims of correlation are either ludicrous on the face of it (like the one where there was a cluster of 8-10 points centered on a point, and one point outside that produced the 'correlation'), or are based on sample sizes that are too small to allow anything except the conclusion that a correlation was observed in the obtained data set, but may not be extrapolatable to larger sets based on typical small sample statistics. I.e. there aren't any 'global' correlations. Of course, cold fusion fanatics are constitutionally incapable of limiting their conclusions thusly, instead always seeing 'conclusive' proof in all their work, no exceptions.

"You can claim that some helium measurements are flawed by leakage"- Not 'claim', by normal scientific standards this was proved by the Clarke work of 2002, which was nothing but a confirmation of the concerns the 1989 DOE review panel had, which were based on the history of trace level He measurements. Pretty solid science I'd say. To respond correctly to this, the CFers must establish their He analyses are adequate for the task AND PUBLISH THAT SO WE CAN SEE IT TOO. None have done so to date, so, we don't have to conclude anything based on their flawed reports. KirkShanahan (talk) 12:00, 19 May 2009 (UTC)

"you can claim that excess heat is only an artifact " - No, again, I SHOWED it is in the ONE CASE where that was possible. It requires calibration data to be supplied for someone else to assess a CCS presence, and Storms is the ONLY CF author to present such data to date. The CCS is PROVEN to be a viable explanation in that case, AND has been shown to be a logical possibility in ALL other excess heat claims. Normally, that means the issue is open and undecided (which is an anathema to CFers) and thus the mainline scientist again needs to embrace no miracles to explain the data. KirkShanahan (talk) 12:45, 19 May 2009 (UTC)

"but you haven't addressed, at all, the issue of correlation." - As I said - no heat , no correlation. What about that don't you understand? B) Have you stopped to consider what it means if I am right? If my CCS is the cause of the apparent excess heat in those experiments where a 'correlation' to He is claimed, and there really is no excess heat, then what does the 'correlation' mean? (Answer: Just a chance result from a single data set.) You need to study the whole picture much more carefully than you have Abd. KirkShanahan (talk) 12:45, 19 May 2009 (UTC)

For any given piece of CR-39 showing the triple tracks characteristic of neutrons, you could claim that a cosmic ray did it, creating a shower of neutrons. However, somehow the cosmic rays must know which

cells have active electrodes and which do not, and which cells have deuterium and not hydrogen. The hydrogen/deuterium shift I could believe, except that deuterium alone doesn't create the neutron tracks above background, it is just the co-deposition electrolysis creating that thin layer of palladium deuteride. (Background is about 1 triple-track per CR-39 chip over the life of these experiments, I think it is weeks; the detected level is about 10 triple-tracks.)

If you had read the references cited by Mossier-Boss, et al, as I did, you would know that the triplet incidence rate is about 2 orders of magnitude below what was published in those references. This means as a certainty the vast majority of the pits do not come from neutrons. The majority of the pits are so overlapped that MB, et al, had to go searching in areas of the plates where severe pit overlap was low enough to see triplets, as distinguished from all other overlaps. This is pure n-ray bunk. They searched for rare triple overlaps in lightly exposed regions of heavily exposed plates, and expect us to believe these triplets come from neutrons, instead of from simple overlap of pits caused by whatever is causing them, **JUST LIKE ON THE REST OF THE PLATE**. As a scientist, that line of logic is disgusting, and it simply illustrates the level of fanaticism the CFers will go to to provide 'evidence' of their claims. The mainline assumption is that the triplets are due to random chance. You can even see triplets in Kowalski's scratch. KirkShanahan (talk) 12:45, 19 May 2009 (UTC)

Your idea about no pits in the 'controls' is misguided. I proposed two physical mechanisms for pit formation back in 2002. Both would be active in 'active' Cf cells. One would be active in normal electrolysis cells. None would be active where the plate is just immersed in electrolyte. KirkShanahan (talk) 12:45, 19 May 2009 (UTC)

You have very little understanding of how WP:RS works; you have mistaken general rules for specifics. For example, we have used a conference paper as a source at Martin Fleischmann. It depends!

It wasn't me who did that!!!! It was people like Pcarbon, who wouldn't let me refer to Scott Little's excellent work on tracking down leached contamination in a Patterson Power Cell configuration CF experiment, or Mizuno's work reported in ICCF13 I believe, that showed Iwamura's 'Mo' transmutation product was actually S, and thereby critique BOTH the use of XPS and SIMS in 'proving' transmutation. I read the policy in detail, and wanted to use the clause that says that obvious things need not be sourced, but PCarbonn wouldn't let me, and others supported him. Get your story straight Abd! KirkShanahan (talk) 12:45, 19 May 2009 (UTC)

Uh, those conference papers at Martin Fleischmann are either a) use in a reference to show that the paper exists and was presented at that conference in that date, or b) being an entry in a list of published papers. They are not being used as actual sources for factual stuff, and I'm sure that people would complain if someone did use them like that. --Enric Naval (talk) 00:48, 20 May 2009 (UTC)

The usability of a conference paper may depend on a number of factors. They are true primary sources, often unreviewed. However, I think that some conferences have reviewed publications of the presentations, those might be a notch up, but I haven't investigated that. I generally assume a conference paper is similar to WP:SELPUB. Self-published documents can be used where the author is notable and the opinion of the author is relevant. The paper in question at Martin Fleischmann is one of Fleischmann's accounts of what he was looking for in doing his experiments. That is "factual stuff," but of a kind where the testimony of the person can be used. Further, if a conference paper is cited in a reliable secondary source, it may be cited through that source. Kirk, you should understand that we use "reliable" in a technical sense. There are "reliable sources" that it would be foolish to actually rely upon, and sources we

can't use, if push comes to shove, that are actually solid. Some keys to understanding this:

- The real standard for everything on Wikipedia is editorial consensus. Theoretically, if we all agree to use some text that could not be verified, the Foundation could intervene on the basis that WP:V is not negotiable; however, the Foundation would be unlikely to spit in the face of all the editors! In reality, that kind of conflict simply will not arise. But details of how to apply the policies and guidelines? Totally up to editorial consensus. You can easily get blocked for attempting to "enforce" policies against editorial consensus, happens all the time.
- Policies and guidelines generally document community practice, they do not generally control it. Rule Number One for Wikipedia is Ignore all rules. Which is actually a very sophisticated policy following the common law Public policy principle.
- You will not be the first expert to be frustrated by Wikipedia requirements regarding reliable source. Used to drive me nuts (I'm expert in certain other fields). However, it's necessary for the anyone-can-edit model we use. My hope would be to more fully integrate expert opinion into the project, but that isn't going to happen easily. It's hard enough to keep the community from blocking people like you, you so readily stick your foot in your mouth. Don't take this personally, Kirk, true experts often do this.

The "I'm right and you're wrong" style of argumentation doesn't work very well here. It hardly ever convinces anyone. Yeah, I know, all too well, it's hard to avoid when you are dealing with people who don't understand the first thing about the topic but who are very ready to take a strong stand, but... what we are required to do as editors is seek consensus. As an expert, with a conflict of interest, you take a bit of a different role, properly, in my view, that of an adviser. Now, tell me, how do you think a professional adviser will conduct himself in helping his clients? Will he berate, ridicule, or insult them? Surely he will stand his ground with regard to his expertise, but I'm talking about something else, about human interactions. --Abd (talk) 02:04, 20 May 2009 (UTC)

I'll be back, if you permit. --Abd (talk) 21:04, 18 May 2009 (UTC)

Do I have a choice? How could I stop you?? KirkShanahan (talk) 12:45, 19 May 2009 (UTC)

Very simple: you request that I not post to your Talk page. If I do, after this, outside certain narrow circumstances, you can claim violation of WP:HARASS. Besides, it would be rude. I'm here with a presumption of continued mutual consent. --Abd (talk) 22:13, 19 May 2009 (UTC)

*There isn't any excess heat. And no heat means no correlation to He, which just comes from leaks anyway.* No, Kirk, you've missed something crucial. You *claim* there is no excess heat. However, what you cannot reasonably deny is that there are experimenters who are *finding* excess heat. You claim that this is experimental error. Fine. Assume it is. Likewise assume that there is experimental error in measuring the helium. The experimental errors only take place in certain experiments, i.e., the famous difficulty in reproducing the effect. In order to maintain the hypothesis that the excess heat and the helium are errors, you must find a way to explain why they are correlated. Perhaps there is something unknown that the experimenters do that causes both errors, and that causes the helium error to increase with the excess heat error. I could imagine that if excess energy is a result of a measurement error that accumulates with time and that leaked helium accumulates with time, but ... excess heat generation in CF cells tends to be pretty erratic, not a steady level of excess (in P-F cells), and that the calculated energy is pretty much right on for the expected energy of  $d + d \rightarrow He-4$  is still a pretty strong coincidence.

However, in MucKubre's work (see Storms, p. 88), the helium level exceeded the concentration in ambient air after 15 days; the experiment continued for 45 days. It's a tad hard to explain helium levels above ambient by "leaks." And, were the helium due to leaks, the amount of helium would level off as the level in the cell approached ambient levels, instead of continuing to increase with time and with the measured excess heat. --Abd (talk) 01:44, 19 May 2009 (UTC)

ONE MORE TIME PLEASE!! I have PROVEN and PUBLISHED the fact that a CCS can completely explain apparent excess heat in the ONE PUBLISHED CASE where such analysis is possible (there are NO others or which that could be done). LOGICALLY, this problem is SYSTEMATIC, which means it can be in ANY cold fusion experiment. That means that is perfectly acceptable mainline science to say there is NO EXCESS HEAT. That is shorthand for: "The case for a nuclear origin of excess heat signals is weak at best and the current preferred explanation is the one proffered by Shanahan in ... until such time as stronger evidence for nuclear origins is presented." This explanation is separate and distinct from anything having to do with He. Therefore, in ALL experiments where a correlation is claimed, FIRST, ACTUAL excess heat must be proven. NO ONE HAS DONE THAT. Thus the appropriate, mainline position to take is that there is NO VALID HEAT-He CORRELATION, because there is NO PROOF OF REAL EXCESS HEAT. KirkShanahan (talk) 12:45, 19 May 2009 (UTC)

Also, ONE MORE TIME PLEASE!! He contamination is ALWAYS an issue. It was in the "late nineteen twenties", and it was in 2003, when Clarke found McKubre couldn't keep air out of his apparatus, and it is today. Without PROOF that air inleakage is unlikely, the mainline science position is that the conventional explanation of leaks is preferred. No CFer has documented this for their experiments where He was claimed to have been produced. KirkShanahan (talk) 12:45, 19 May 2009 (UTC)

Kirk, it seems you are coming unglued. Your arguments do not improve with capital letters. You are failing to confront the issue of correlation. Correlation can show causation or connection even in the presence of massive noise. It is not necessary to prove that excess heat is artifact-free, nor that leakage doesn't take place, in order to show that there is a correlation between *\*apparent\** excess heat and *\*apparent\** helium. I've stated, here, an example where the helium found was above background, way above background. You can't explain helium above ambient by leakage. Further, in the series of experiments described, there are 33 experiments, and 12 of them showed no "apparent" excess heat, and 18 did show excess heat. That is, whatever calorimetry they did, they calculated, from the calorimetry record, flawed or otherwise, a certain amount of heat beyond that explainable by energy input or other known factors. You can suppose, I said, that the origin of this measurement is CCS. In other words, no assumed proof of excess heat. So why the capital letters repea, and which measures helium. The results come back: No helium found in the 12 cells with no inferred excess heat. Helium found in 18 out of 21 cells with inferred excess heat. Levels of helium are correlated quantitatively with the level of excess heat inferred.

There is an obvious conclusion: something is causing *\*helium\** to be found. Okay, suppose it's leaks. Suppose that in these experiments the level of helium measured is below ambient. Fine. Why is helium only being found when there is also a finding of excess heat? At this point, one could theoretically suspect fraud. I.e., if there was excess heat, they opened the cell and allowed in enough air to cause a quantitative correlation with the measured excess heat. And down this road madness lies, because, then, we must suspect all research. Rather, we ordinarily trust that experimental reports, while work could be sloppy or incompletely reported or conclusions could be mistaken, are nevertheless honest, and we practically crucify scientists who violate this trust.

Essentially, the cells with no excess heat are a kind of control for the heat-helium connection.

Are you truly unable to understand this argument? Because it's been repeated several times now, and you haven't responded except by shouting.

In any case, it's simple to calculate the odds of a result like this being due to chance association; this is the figure reported elsewhere, from Storms, as 1 in 750,000. (I haven't verified the math and don't know if this took into account the three non-helium cells; as I assume you would know, there are reasons why those three cells might be different, but I can't be completely sure it's not hand-waving.) Further, when helium levels well above ambient are reported, the leakage explanation isn't sufficient. It's not correct that CF researchers haven't addressed the problem of leakage.... I must admit, Kirk, I'm losing faith in your ability to cogently criticize this research. But I won't give up yet. --Abd (talk) 22:38, 19 May 2009 (UTC)

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Responding to Abd's comment above in a section by section fashion:

Kirk, it seems you are coming unglued. Your arguments do not improve with capital letters.

Of course, on the Internet, capital letters indicate 'shouting'. I shout because you don't seem to hear. You never give any indication that you have read, understood, and incorporated my comments. When I ask for such an indication, you ask me to do it for you (see above section re: 'the challenge'). Shouting is all I have left, but I see that it also made no difference. At this point, one normally just walks off, because communication is not possible. I had the same problem with Ed Storms, but at least he fully repeated back my points. But when I asked him, "how then can you still not incorporate my thoughts into your work?", he just replied, "I don't believe it." That's when we both realized our discussion was ended. At least he reached that point. You haven't.. I am not 'coming unglued', I am trying to get through to an obstinate person who won't listen. I'm not the only one, see Enric's comment below (next section).

You are failing to confront the issue of correlation. Correlation can show causation or connection even in the presence of massive noise.

No, correlation never proves causation. Demonstrating control over an effect proves causation. Correlations can exist by pure chance.

It is not necessary to prove that excess heat is artifact-free, nor that leakage doesn't take place, in order to show that there is a correlation between \*apparent\* excess heat and \*apparent\* helium.

Wow! Have I succeeded? Have you grasped a bit of the problem? Probably not. So, let's go through this. The sentence above has Abd actually using the word 'apparent'. I checked the CF Talk page. Other than my specific uses and generic uses of that word, the only times it is used in reference to excess heat signals is by Kevin Baas on May 19, and by Abd on May 20 (today!). The word is only used once in the CF article, and not to describe 'excess heat'. The heat-helium correlation is a standard 'piece of evidence' offered by CFers to 'prove' CF is nuclear. Thus, when the word 'apparent' is not specified, the common usage of the 'Heat-He correlation' implies the heat is real, caused by a nuclear process, which also generates He. I contend the 'heat' has not been proven real, thus the causality relationship implied is not real either. That is just basic logic. So, what we have is an example of how Abd's sloppy terminology confuses the issue. Does he really mean 'apparent' or not?

I've stated, here, an example where the helium found was above background, way above background.

Way above? Even considering your sloppiness, that is incorrect to my knowledge. Please cite your reference. Understand, that natural He concentration in air is about 5ppm. That is a trace level measurement. 'Way above', given the difficulty of doing trace analysis, is probably say about 500 ppm, i.e. a factor of 100. Anything less is still suspicious in trace level analysis. Of course, standard statistics applies.

You can't explain helium above ambient by leakage.

Actually you can. It would be a mismeasurement problem. In fact Brian Clarke told me one time the 11 ppm He numbers that Russ George and McKubre reported were actually about half that when he measured the samples (i.e. air levels). He didn't know what the error was, but he was the expert, not George/McKubre, so I trust him first.

Further, in the series of experiments described, there are 33 experiments, and 12 of them showed no "apparent" excess heat, and 18 did show excess heat. That is, whatever calorimetry they did, they calculated, from the calorimetry record, flawed or otherwise, a certain amount of heat beyond that explainable by energy input or other known factors. You can suppose, I said, that the origin of this measurement is CCS. In other words, no assumed proof of excess heat.

OK so far.

So why the capital letters repea, and which measures helium.

What is this? --- Picking back up...

The results come back: No helium found in the 12 cells with no inferred excess heat. Helium found in 18 out of 21 cells with inferred excess heat. Levels of helium are correlated quantitatively with the level of excess heat inferred. There is an obvious conclusion: something is causing \*helium\* to be found. Okay, suppose it's leaks. Suppose that in these experiments the level of helium measured is below ambient. Fine. Why is helium only being found when there is also a finding of excess heat?

Interesting that you are finally getting specific here. Are you discussing the Miles work at China Lake of the early-mid-90's? I ask because of one point I always make: science requires replication. And this replication is not just one study of 33 runs, it is multiple studies by multiple experimenters supporting the same detailed conclusion, i.e. with 'high' reproducibility. I've looked at Miles before. Bottom line: never replicated, therefore not 'proof' of anything. Can I explain it. No, but I don't need to, 'replication required' is the gold standard.

Secondarily, if you are actually finally admitting there may actually NOT be any excess heat, then we can proceed down that path. I have stated repeatedly that I believe there is an 'effect', which I called the Fleischmann-Pons-Hawkins effect (FPHE) (named after its discoverers), that produces \*apparent\* excess heat signals, and I described what I thought it was in my first publication on this subject. I have also stated that I have not thought through what in that mechanism/process might induce more He inleakage. I don't have the time to do so, why don't you try?

Thirdly, you have to have all the details to critique the experiment. What was the time placement of the 'controls', were they properly mixed in? Did the 'controls' use the same reactants? (CFers love to mix H and D, claiming they are equivalent chemically. This is grossly incorrect.) So to answer your question, I would need to have all these kinds of answers. Please note that I have looked at the Miles work a long time ago, and I concluded it was interesting and suggestive, but as I said, it was never adequately replicated. Storms however, takes anything as proof and glosses over the problems, to the extent of ignoring rebuttals of his position.

At this point, one could theoretically suspect fraud. I.e., if there was excess heat, they opened the cell and allowed in enough air to cause a quantitative correlation with the measured excess heat. And down this road madness lies, because, then, we must suspect all research. Rather, we ordinarily trust that experimental reports, while work could be sloppy or incompletely reported or conclusions could be mistaken, are nevertheless honest, and we practically crucify scientists who violate this trust.

A) I have never, ever used the 'fraud' excuse, and B) all my criticisms are technical in nature, see above for an example.

Essentially, the cells with no excess heat are a kind of control for the heat-helium connection.

'A kind of' is correct. But are they a 'good' kind or a 'bad' kind.?

Are you truly unable to understand this argument? Because it's been repeated several times now, and you haven't responded except by shouting

Are you truly unable to understand the problems with this argument? I thought them through years ago. Do I need to shout some more?

In any case, it's simple to calculate the odds of a result like this being due to chance association; this is the figure reported elsewhere, from Storms, as 1 in 750,000. (I haven't verified the math and don't know if this took into account the three non-helium cells; as I assume you would know, there are reasons why those three cells might be different, but I can't be completely sure it's not hand-waving.)

It's also simple to understand the relevance of a correlation coefficient – squaring it gives the fraction of variability explained by the implied math model (linear correlation => linear regression line). As I noted in the next section, at about 10% variability, scientists begin to question the level of control. That translates to an  $r$  of  $\sim 0.95$ . Does the 'correlation' you are discussing show an  $r >$ , say, 0.9? If it doesn't, that means there is enough variability in the data set to suggest a significant uncontrolled factor or factors, and that leads to the 'small data set' concern. That concern is what drives the reproduction requirement, and as I said, I don't believe it has been reproduced. By the way, this is the preferred logic for deciding if a correlation is significant in chemical process control and improvement, which of course is what we are talking about, since CF is not nuclear(!).

Further, when helium levels well above ambient are reported, the leakage explanation isn't sufficient.

Agreed, if you can believe the reported He numbers. Such believe is promoted by supplying analytical method protocols and run results from controls. No such data has been supplied by CFers. So I, being a conservative mainliner, assume they messed up somewhere, not they they found a revolutionary new nuclear process.

It's not correct that CF researchers haven't addressed the problem of leakage.... I must admit, Kirk, I'm losing faith in your ability to cogently criticize this research. But I won't give up yet. --Abd (talk) 22:38, 19 May 2009 (UTC)

The 2003 Clarke paper shows that one of the top scientific labs doing CF research couldn't keep air out of their apparatus. This is after the 1989 DOE report made that clearly a priority point. And remember, the 2004 DOE report concluded similarly that there was no compelling He evidence (otherwise, their conclusions would have been different from the 1989 report). I must admit, Abd, that I lost faith long ago in your ability to cogently criticize this research. But I won't give up yet. (maybe) Kirk shanahan (talk) 16:31, 20 May 2009 (UTC)

(:I see a lot of new comments, I will get to them as I can, I have other work to do. Kirk shanahan (talk) 16:31, 20 May 2009 (UTC))

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## V's latest ad hominem

V has continued to attack me professionally on the Talk: Cold Fusion page. His latest example is immediately below. This has reached the level of offensiveness. Let me address the issues below.

Skip this to get to the example specified above. Kirk Shanahan is somehow claiming that to attack his ideas and logic is the same thing as to attack him personally ("ad hominem", in the section title, refers to a personal attack), even though he uses the words "attack me professionally" above. That is bad logic, always has been, always will be. People are provably much more than than the things they express. It is quite possible for anyone to express nonsense. That does not mean anyone is inherently nonsensical. V (talk) 14:07, 20 May 2009 (UTC)

Then there is Kirk Shanahan wanting us to believe that a small amount of chemical heat from hydrogen-oxygen recombination can throw off a calorimeter designed to register lots more than a "small amount" of heat, and throw its calibration off to the extent that if it measures a lot of heat while an electrolysis cell boils its electrolyte away and melts one of its electrodes, we have to pretend it didn't actually happen; that it was all an illusion associated with a temporarily uncalibrated calorimeter. I agree that that speculation should be in the article, for entertainment purposes if nothing else! I disagree on giving it any more weight than claims that hydrinos can explain cold fusion. V (talk) 15:24, 19 May 2009 (UTC)

I have published a proposed explanation of the apparent excess heat observed in typical F&P electrolysis cells called the 'Calibration Constant Shift' (CCS). The idea is predicated on two concepts: 1) there is an effect active, which I called the FPHE, and 2) there is no change in the heat content of the cell at any time (due to new heat sources for example). This FPHE causes a redistribution of heat in a cell, such that more heat is collected in the cell, i.e. cell losses (which are what causes calibration to be required) are reduced, which is why an apparent excess heat signal develops. This proposition has never been scientifically challenged, even though a (separate) physical/chemical mechanism I proposed was (erroneously). Thus it is fair to say that the CCS is feasible, and may be due to heat redistribution in nay case where a calibration equation is used to measure heat output.

Now, on to heat-after-death (HAD). In HAD, the electrolyte is removed from the cell, leaving just gas space. Seriously, do you expect the heat loss profile to remain unchanged in that case? You no longer have the liquid, which provides good thermal contact for heat transfer from the electrode to the cell walls and out into the calorimeter. Now you are just left with gas phase heat transfer and flow through the miscellaneous cell parts. A completely different situation! So, will the calibration remain the same? No? Then why do we believe numbers from Cold Fusion researchers (CFers) that are obtained without recalibrating? There is really only one reason, fanaticism.

With regards to V's claims of 'small amount of chemical heat', this is a gross misrepresentation and really just illustrates V's unwillingness to learn. I have assured him many times I know what is going on, but because of his fanaticism, he insists I don't. The amount of heat involved is not 'small', as in 'irrelevant', it is just exactly the amount needed to produce the effect. I have estimated that in the Szpak, Mossier-Boss, Miles, and Fleishman 2005 publication, this amount was about 20% of the available recombination energy. That isn't the majority, but it isn't small. In fact, in high-loss calorimeters with full recombination, a quite large signal might be expected.

By the way, the change in calibration constants needed to induce the effect in the Storms' work I reanalyzed was only 2-3%. I also noted during examination of McKubre's 1998 report that a 1% change could change the excess heat peak by 50%. Thus, the CCS is not a major problem, it is a minor noise issue. The apparent excess heat peaks are just the experiment's noise.

As well, it is only 'temporary' in the sense that it does seem easy to destroy the 'special active state' that leads to the redistribution of heat. In the HAD case, it's only 'temporary' if you immediately refill the cell with electrolyte, which the CFers don't.

With regards to 'melting', perhaps I missed it, but I don't recall HAD experiments 'melting' the Pd. I definitely do recall claims of melted Pd arising from explosions, but I have already posted that that appearance is most likely due to shock wave induced flow, not melting due to temperature. Pd is ductile, and it flows relatively easily as opposed to glass, which would shatter in an explosion. And Russ George did claim to have 'melted' the Pd he used in ultrasonication experiments, but I suspect it was more the cavitation jet effect he noted.

All of this V calls 'speculation'. That is incorrect. The CCS is published and unchallenged. The fact that a CCS would be expected in a HAD condition is ridiculously easy to see. The 'temporariness' of the problem explains many features of the CFers inability to reproduce experimental results in detail. Explosion-induced flow might be called speculative, but it's not much of one. About the only one who seems to be wildly speculating is V with regards to my expertise. Please stop it V. What amazes and frustrates me is that the Wikipedia community allows this kind of 'armchair expert' nonsense to go on (and on and on ...). KirkShanahan (talk) 16:47, 19 May 2009 (UTC)

Kirk, you are bandying about "ad hominem," but using the term in a way that doesn't seem correct. Above, in responding to me, you wrote: *"Some of the criticisms he makes are odd."* - *ad hominem* again. That's not an ad hominem criticism, it speaks directly of the criticisms themselves, not to the person making them. V's criticism above you can argue is wrong, but it doesn't claim that your comments are wrong because you made them, but because, he thinks, your arguments are preposterous. Again, not an Ad hominem argument. Maybe you should read the article!

I have a suggestion. If you cannot handle criticism of your claims here, but take it personally, as if it impugns you professionally, even when it is only doing what Wikipedia editors must do with expert opinion (challenge it and criticize it and compare sources), helping us here is probably not going to work for you or for us. --Abd (talk) 23:11, 19 May 2009 (UTC)

In 'The Art of War' by Sun Tzu, there is an anecdote about what happened when he introduced the concept of formation fighting to the Chinese emperor. This was a new concept and Sun Tzu claimed that he could teach it to anyone, which the emperor found hard to believe. So, he challenged Sun Tzu to teach his concubines how to formation fight. Sun Tzu agreed but only if he could have absolute authority over them. The emperor granted him this, so Sun began. The concubines thought this was great fun. Laughing and giggling, they 'royally' messed up the first lesson. Sun Tzu blamed himself, and went through the process again. The concubines messed it up again, greatly entertaining themselves and the emperor. Sun Tzu blamed himself again and tried again, reinstructing them. Again they failed, with great glee. However, this time Sun Tzu said basically that the third time was their fault, not his. He called the emperor's favorite concubine up to the front and beheaded her. Then he had them do it again. Perfection was obtained.

In this case, I only have 'the pen' to address maliciousness on the part of my 'students'. Both V and yourself have failed to grasp what is really very, very simple. Both of you have repeatedly blamed me for this, saying my propositions are 'odd' or 'illogical' or whatever. In fact what is odd and illogical is your inability to understand. It has gone on so long, I must assume (as did Sun Tzu) that it

is deliberate. Therefore I will point this out.

It's not that I can't take your criticisms (in fact they are nothing new, other cold fusion fanatics have repeatedly said the same things), it's that I can't take the pure maliciousness of your unwillingness to learn. I can't see why you think you are qualified to edit the CF article when you are so clearly and obviously biased. I guess you believe that if you read a couple of sports section articles on the Denver Broncos, you could then successfully coach them to a Super Bowl championship too.

And by the way, deliberately failing to understand and blaming the 'teacher' is an *ad hominen* attack on the 'teacher', as shown by Sun Tzu's story.

There's an old saying: "If you can't stand the heat, get out of the kitchen". Both you and V have proven that you "can't", so why don't you both just go elsewhere and bother someone else, so we can get on with writing a balanced and fair article on cold fusion? Kirk shanahan (talk) 11:35, 20 May 2009 (UTC)

No problem, Kirk. You've got it backwards, though. Sun Tzu beheaded the emperor's favorite? Not much of a favorite, I'd say, or Sun Tzu would have lost his own head. And I'm not your student, and you have no authority over me whatever, much less absolute authority. I'm a Wikipedia editor trying to extract advice from someone who, from a record of publication, I'd expect would be an expert, but, it appears, is too attached to himself. I've had physicians like that. I fired them and found better ones. Bye. --Abd (talk) 12:25, 20 May 2009 (UTC)

Abd, see here (<http://www.afpc.asso.fr/wengu/wg/wengu.php?l=Sunzi>), Sun Tzu beheaded not one, but *two* of the favourite concubines of the emperor, and that was after being told by the emperor not to behead them. You can doubt that the anecdote itself is true, but you can't doubt that Kirk got the gist of the anecdote right. (Mind you, Kirk was wrong in that it wasn't *the* favourite concubine, but two of the favourites ones, but that's still not getting it "backwards") You know what, the whole point of this is that you missed the whole point of the anecdote: that people in Talk:Cold\_fusion are disobeying in purpose and that they won't obey until heads are rolled. And you missed it all because of a single detail that you got wrong anyways. You keep doing this all the time. That's *annoying*.

If this was an isolated incident then I wouldn't complain, but you keep assuming that people has gotten the sources wrong, basing yourself just in your own personal opinion of what the sources should have done or said, without bothering to actually check the sources. Have you got any idea of how annoying that is to other persons? (and going into into condescendency, depending on how it's worded). --Enric Naval (talk) 12:53, 20 May 2009 (UTC)

Two? really? I guess I'll have to reread that section. That's for the support Enric, Abd and V are immensely annoying, and I'm sure they love that.

Abd, yes, the favorite(s). That's why he extracted agreement fro the emperor on absolute authority. He knew what was coming. If the emperor had intervened, he would have gone back on his word, which traditionally is far more important than one or two concubines, probably even a wife or two.

The fact that you state you are not my 'student' clearly points out the problem. I am an acknowledged expert in the field, and you won't learn from me. Does this

point out any problems to anyone? Kirk shanahan (talk) 13:36, 20 May 2009 (UTC)

## Requested 'Idiot''s Guide" to the Calibration Constant Shift

V says Abd requested this...

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$2 * 3 + 1 = 7$  TRUE!  $3 * 3 + 1 = 10$  FALSE! SEE!

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That's it, at least for idiots. For the rest of us, a few more words are in order, but you know, I've already said them many times before. Maybe the Idiot's Guide approach will work.

Anyone who thinks this equation is false:  $(3*3)+1=10$

--that person is wrong, whether idiot or genius. Notwithstanding what is written below, the preceding, by itself, does NOT adequately describe CCS in a nutshell, because there is no background explanation preceding it. (And now, on to some of that background....) V (talk) 22:12, 20 May 2009 (UTC)

In any case, in the above, the TRUE equation is the calibration equation actually in effect at the time of an apparent excess heat event. The equation is of the form  $y = mx + b$ , where  $m$  and  $b$  are the constants,  $x$  is the measured value, and  $y$  is the computed value. However, unbeknownst to the researchers, this is a changed equation from what they measured hours before, i.e.  $m=2$ ,  $b=1$  vs.  $m=3$ ,  $b=1$ . Why this happened we may never know. Clearly, both equations are algebraically correct. The problem lies in the assumption that the second equation determined at time  $t_0$  is correct at time  $t_1$ .

That's it! Hard isn't it. Kirk shanahan (talk) 18:00, 20 May 2009 (UTC)

Thank you. Now all you have to do is explain how/why it *must* have happened. If you cannot, then you have no basis for insisting that it had actually happened. The burden of proof is on the accuser, always. You are accusing that the measuring tools become miscalibrated during the action being measured, when in no other experiment using those tools, there has been no reason to reach such a conclusion. So, the appropriate quotable response is, I believe: "Put up or shut up." (I'm aware you've done some of that putting-up elsewhere, but we want the explanation-for-idiots version here.) V (talk) 22:12, 20 May 2009 (UTC)

Let me take a moment to correct a gross misconception that you have. It is not my job to explain 'cold fusion'.

Interjection: I never had that particular misconception. The only job I'm interested in seeing you accomplish involves describing CCS details in a way that is more reasonable than I've previously seen. V (talk) 21:06, 21 May 2009 (UTC)

'Cold fusion' is not my primary area of research or even interest. There is no 'must' around when discussing what I have done or might do. That's the way science works. You publish a paper (or hopefully discuss an idea with colleagues before you publish) and everybody tries to find what is wrong with it. It's kinda like a game. The one who finds something wrong presents that idea. Then people get to try to shoot it down too. After all the shooting has subsided to a tolerable din, everybody starts synthesizing what has been brought up. In that process, only the ideas left standing after all the shooting are normally considered. In my case I was in the second tier, the critics, not the originators. I proposed a problem with the originator's ideas, and they tried valiantly to shoot it down. But, they couldn't.

Interjection: So YOU say. It seems to me that all they need to do is present alternate evidence of excess heat, such as images of boiling electrolyte and melted palladium, for the calorimeter measurements to become well-nigh irrefutable (that is, there can be more than one way to show that excess heat had existed). Now, I'm aware that experiments in which the phenomenon being sought reaches such extremes --those experiments are still quite rare; more work needs to be done to identify the parameters that lead to those results. Just as, originally, lots of work needed to be done in order to fairly reliably produce even moderate amounts of excess heat. Which likely takes us right back to why "they couldn't", at the time you posted your criticism (no alternate evidence available). V (talk) 21:06, 21 May 2009 (UTC)

So, it's now up to all to synthesize my ideas into the general mix. Of course, this is what has NOT happened with the CFers, and that is one additional reason why they can clearly be labeled 'psuedoscientists', they were great right up to the last step. But to do real science you have to do it all.

Interjection: Yes and no; the thing you proposed needs to be considered as a factor after it is proved that such a factor actually exists. So far as I've seen, though, nobody has yet demonstrated an actual case of genuine CCS in action. Until that happens, just exactly why should anyone pay lots of attention to it??? V (talk) 21:06, 21 May 2009 (UTC)

As a critic, I need do no more than what I have done to date. The originators however, if they intend to continue in the field, need to incorporate my ideas into their experiments to try to resolve if my proposal can/cannot be proven true vs. theirs, i.e. we have two viable alternate explanations and more work needs to be done to distinguish them. Otherwise, they just replicate the mistakes of their predecessors. Most importantly, they need to realize that their claims to have proven a nuclear origin of the effect are incorrect. The jury is still out.

I assume you are aware that the jury doesn't need only evidence of excess heat to conclude that a conventional explanation is inadequate; neutron tracks are potentially pretty convincing evidence, too. Anyway, partly repeating what I wrote above, while YOU may claim that CCS is viable, I don't see any reason to agree with that assessment until the pheomenon is reliably documented to have occurred in experiments that specifically look for it. V (talk) 21:06, 21 May 2009 (UTC)

Now, on to your specific comments. "then you have no basis for insisting that it had actually happened" - strawman - What I have said many times is that I have a conventional explanation for \*all\* the effects claimed by cold fusion researchers to prove a nuclear origin of the effect.

Interjection: And I disagree because of (so far as I know at this writing) the lack of evidence that CCS has ever happened anywhere. V (talk) 21:06, 21 May 2009 (UTC)

I have also stated many times that, as a default, I prefer the conventional explanation. But if you have been listening and actually read my papers, you would have noted that my basic point is that there is no way today to make a conclusion as to the origin of the FPHE.

Interjection: That would depend on the amount and quality of the evidence, some of which, especially if verified, cannot possibly have a chemical origin (neutron tracks). If you want to insist that the only evidence that needs to be considered are mere claims of detecting excess heat, all of which supposedly can be explained by a hypothesis (CCS) that as yet has no other supporting evidence, well, I phrased the preceding that way just to point out the precariousness of that position. V (talk) 21:06, 21 May 2009 (UTC)

This cuts both ways. Thus I do NOT insist it "actually happened". That is your strawman interpretation of what I write, formulated by picking and choosing comments from me and the CFers designed to support \*your\* strawman not mine. Let me be clear, once more, the jury is out, no verdict has been given. You may sputter and

wheeze at this all you like, but when it comes to explaining where I stand on this issue, \*I\* am the expert, not you. If I say you've got it wrong, you've got it wrong.

It appears you are jumping to conclusions moreso than me. What **\*\*I\*\*** say is that "Here are some claims about excess heat that YOU say can be explained by CCS happening" --and all I want you do do is more-precisely explain how CCS could indeed actually have been there. It is quite logical to me that if there is no rationale for why it should happen (especially after experiments designed to find it don't), then there is no reason to think it actually happened --which means the excess heat needs a different explanation. Now I'm fully aware that that statement can in turn be applied to CF as an explanation --a rationale for "how" is needed to exactly the same extent-- yet I am not the originator of that logic, the originators were those who dismissed the reports of excess heat two decades ago. I'm simply repeating/applying their logic to your CCS notion. Meanwhile, there now ARE several explanations for how CF could happen, putting that aspect of the argument into a position superior the position of CCS. Sure, none of those explanations yet has additional supporting evidence, but with CCS in CF experiments not even having at least an explanation...see? V (talk) 21:06, 21 May 2009 (UTC)

“The burden of proof is on the accuser, always” - This isn't a court of law.

Interjection: Technically, true, but pragmatically, when such terms are frequently used as "the jury is still out" and "making one's case", when both sides manage to view the same evidence and reach different conclusions, well... the difference between a court of law and certain parts of the Scientific Process is just about irrelevant. V (talk) 21:06, 21 May 2009 (UTC)

It is reasonable and rational to expect that I clearly present and defend my proposal, but the ‘proof’ comes from those willing to do the experimental work required. For a variety of reasons, that is not me. I have conformed to standard scientific practice by presenting and defending (successfully) a counter-explanation for the FPHE. I have done all that is reasonably and rationally required of me. Science is a community thing, it is not done by just one guy, and this ‘sharing of the load’ I describe is SOP.

I wasn't asking or expecting you to do CCS experimentaton, I was asking about a more thorough explanation. Over on the CF talk page I wrote this: "non-RS is the only place to find proposed explanations for HOW (detailed "how", not just "Duh, fusion did it") " ---and here all you are saying, in effect, is, "Duh, CCS did it". V (talk) 21:06, 21 May 2009 (UTC)

“You are accusing that the measuring tools become miscalibrated during the action being measured, when in no other experiment using those tools, there has been no reason to reach such a conclusion.” - Your grammar seems a bit off here. I assume you mean that ‘in no other experiment using those tools has there been reason to reach such a conclusion’. The double negative in what you wrote cancels out and it really says ‘in those experiements there has been reason to reach such a conclusion’. While correct, that seems inconsistent with you usual position.

Thank you, I admit I made that particular grammatical mistake. V (talk) 21:06, 21 May 2009 (UTC)

Assuming my version is what you meant, I need to explain then why what you actually wrote is actually correct. The excess heat measurement is a classic ‘small difference between large numbers’ problem. Most uses of calorimetry are to measure the two large numbers, and quit there. In other words, they calibrate the calorimeter to measure up to 20W, and then measure experimental values in that range, and make their claims based on those measurements, not on the differences between measurements.

Nicely stated. Thanks. I shall keep in mind that that is "generic background explanation". V (talk) 21:06, 21 May 2009 (UTC)

The calorimeters used by CFers are fine for measuring the 20 W (or whatever) input power up to a certain error limit, with the standard caveat that different types will give different error bars. Based on Storms' reporting of the calibration constant variation (and having suspected such from McKubre's work), I explicitly detailed how a 1-5% variation in calibration constants (which is essentially what Storms reported (using standard statistical quality control procedures)) could lead to excess heat signals. In other words, I showed that the CFers are 'working in the noise' when claiming excess heat peaks. This does not negate in any way the utility or validity of calorimetry, it simply defines clearly what the error bars are on such work, and guess what, it's a 'normal' error bar of 1-5%, very typical of chemical measurements. I also explained, via my two-zone model of the cell (or calorimeter) how one might get a CCS. I don't claim it is the only way, but I do believe that heat redistribution can cause a CCS. So it is correct to say "when in no other experiment using those tools, there has been no reason to reach such a conclusion", meaning in those experiments there has been reason to reach such a conclusion (that the excess heat peaks are 'in the noise').

Ummm...actually, the conclusion I had in mind was, "CCS happened in lots of other experiments, even predating CF experiments by decades", and what you have just written does not seem to support that. My intention was to attach to that conclusion (non-grammatically erroneous!) the notion that it hadn't been seen anywhere --else CCS as a hypothesis for explaining various measurements would have first appeared many decades ago. Also, I need to specify that my interpretation of what I had previously read, of what you wrote, included the notion that you were using CCS to extend the typical error range *rather farther* than you appear to be describing here and now. It seems obvious enough, though, that you are indeed trying to use CCS to extend the error range somewhat, just so that the excess heat claims of CF proponents can continue to be ignored by the mainstream (heh, you wrote that you were one of the obstacles that a CF paper needed to pass by, to get into the mainstream!). As previously stated, my objection to that involves both the need to prove that CCS does indeed happen in other experiments, plus a need to explain the process by which it can be expected to happen in a heavy water experiment but not a light water experiment. V (talk) 21:06, 21 May 2009 (UTC)

I further proposed a mechanism to get heat distribution change. Likewise, I do not claim this is the only way that might happen. But I do contend that my mechanism has high predictive power and can explain a variety of results without further modification. Normally this is a sign of a good theory. Since I know of no cases as you imply, I would pose this question for you: In what other application of those methods do the researchers make claims based on signals in the noise? Kirk shanahan (talk) 12:16, 21 May 2009 (UTC)

My answer must of course refer to the magnitude of the noise when CCS is not part of the system. If the CF'ers are finding heat outside *that range*, yet it is inside the noise range after CCS is *PRESUMED* to occur, then it becomes extremely important to prove that CCS does indeed occur. V (talk) 21:06, 21 May 2009 (UTC)

I'd still like to see the idiot's guide to your explanation for how CCS might happen. V (talk) 21:06, 21 May 2009 (UTC)

Then read my papers. Kirk shanahan (talk) 15:27, 26 May 2009 (UTC)

## (Convenience break)

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You know V, I first thought we were making some progress, but I see really that we aren't. Examples:

"Anyone who thinks this equation is false:  $(3*3)+1=10$  --that person is wrong, whether idiot or genius." - Of course you missed the entire point, but it's not important.

Due to other obligations, it will take some time to properly reply to the stuff you wrote in this area. But at the moment I can reply to the above, as follows: YOU have missed the point. An "idiot's guide" is NOT any such thing if the explanatory cart is placed before the explanatory horse. So, you missed that point when you wrote your equations before you wrote your accompanying explanation, and you missed it again, even after I tried to less-blatantly specify the flaw in that explanation. And if you think that "your papers" are explanatory enough, equivalent to an idiot's guide, when full of specialists' jargon, then you need to seriously think again! V (talk) 14:41, 28 May 2009 (UTC)

“The only job I'm interested in seeing you accomplish involves describing CCS details in a way that is more reasonable than I've previously seen.” – Can't do that. As I noted with my Sun Tzu reference, the fault is yours, not mine. I have explained it so an idiot can understand it. Go read the paper. The manuscript version, which has all the technical details, is in Rothwell's Website.

More illogic (this time of the inconsistency variety) from Kirk. V (talk) 08:03, 30 May 2009 (UTC)

Good grief! Now you're criticising Sun Tzu! ROFL doubled!!!! Kirk shanahan (talk) 15:22, 1 June 2009 (UTC)

When criticism is deserved, why not? I notice you aren't defending the idea that eliminating a member of a group improves the maximum possible strength of the group....

The inconsistency is that you cannot say that someone is unable to understand something, and ALSO say that that person is deliberately misunderstanding that particular thing. Only if it is understandable/actually-understood can it be deliberately misunderstood. I have now read the Sun Tzu anecdote. You have not proved you have explained a CCS-causation mechanism in a way that can be understood by concubines, to say nothing of idiots (are you aware that one Chinese concubine managed to become Empress, ruling alone for a significant time? No idiot, she!). All Sun Tzu had to do was encourage them to follow orders (and beheading was probably excessive; "strength in formation" is always reduced when the size of the formation is reduced, obviously). You cannot assume the casual reader to have sufficient depth of background (equivalent to "tools to make the tools to make the tools...") to know the terms that specialists bandy about. In my case, some time back, I recall encountering something I perceived to be a logical flaw in your explanation of how CCS could happen. LOGICALLY, in any argument-chain, a flawed link means the rest of the chain, that depends on that link, is incapable of holding the weight it is given. There was no reason for me to read the rest of the chain until the perceived flaw was resolved. YOU failed to resolve it, instead choosing to be offended as if attacked. Get over it; I don't treat your hypothesis any differently from others that don't make complete sense --here's an example I am copying from the CF Talk page:

*I'd like to interject a remark regarding my first impressions when I first encountered "hydrino" descriptions. I was aware that in Quantum Mechanics electron orbits are "mapped" in terms of whole numbers; the circumference of the lowest orbit is basically equal to one wavelength of the electron; the circumference of the next-lowest orbit is equal to two wavelengths, and so on. Well, if hydrinos are real, the only way it could make sense in terms of QM (to me, anyway) is if the first orbit smaller than the QM-standard-lowest-orbit was such that twice its circumference equalled one electron-wavelength; in other words, the electron orbits twice while doing one "vibration". Obviously the next smaller orbit would have the electron orbiting three times while doing one vibration, and so on. The PROBLEM I have with that is an orbit is itself a type of vibration (cyclic); we would be saying that the electron can do two or more orbit-vibes at the same time it is doing one (ordinary) vibe, and there is something contradictory about that. So, I cannot much support the hydrino hypothesis, until that contradiction is resolved. But I can keep a somewhat-open mind about it, on the off-chance that the contradiction has been resolved in a way*

*about which I am simply currently ignorant.*

Likewise, I await from you, Kirk, an explanation of a CCS-causation mechanism that makes sense. No cart before the horse, either! V (talk) 14:37, 1 June 2009 (UTC)

For the rest of you, V has the way science is done completely backwards. He apparently won't accept experimental evidence without a 'theory' that justifies it (even while fighting valiently for cold fusion, which has no theory explaining it either). However, real science is data-driven. If an experimentalist can develop a reproducible data set, then that stands as what theory must explain. It doesn't work the other way, i.e. if you don't have a theory to explain good data, you can't believe the data. So, being backwards, V makes vehement demands for a theory of how a CCS can happen. First off then, I don't have to provide one to have demonstrated a CCS is fully capable of explaining excess heat signals. I did that and published it. Second, as an addition, I *\*also\** published, in the same paper, my speculative mechanism as to how such could occur. I have repeatedly tried to explain the CCS details to V, and have pointed him numerous times to my paper(s). He fails to read and understand. I actually don't know if he is incapable or unwilling to do so. But, bottom line, I can't get through to him, so I will not respond to him any more. If any of you can't understand my points, please feel free to comment, but first make sure you have read my paper(s). The original manuscript is on Jed Rothwell's site (the banned lenr-can dot org website). Go to the library, look under the 'S', and find my one entry, then download and read. If you come across the way V does, I will also not respond to you. Kirk shanahan (talk) 15:22, 1 June 2009 (UTC)

P.S. If you want to understand why the 'bogosity factor' on the hydrino theory is so large, consider how quantum numbers were derived. Kirk shanahan (talk) 15:22, 1 June 2009 (UTC)

It appears that Kirk has described the Physics community quite well; for twenty years it has failed to accept experimental evidence without a theory that justifies it. Kirk, all I've done is apply their logic to you; how do you like it? Also, you are mixing apples and oranges. With respect to CCS, YOU say it should be considered as a factor in every CF experiment that reports excess heat, but you do not say it should be considered as a factor in every other use of calorimetry equipment that measured heat in experiments for more than the last hundred years. ***It is that double-standard which requires greater explanation/conflict-resolution than just your say-so.*** What specific factors lead to CCS in CF experiments and nowhere else (including experiments designed to find it)??? Also, you have NOT to my knowledge "repeatedly tried to explain the details to V" --I only know of one attempt, at which you failed miserably due to illogic, and which, after being unable to explain why it wasn't illogical, you simply stopped trying. I see no reason to read technical papers full of understandable technical jargon (for all I know, the whole purpose of the jargon is to hide its illogic), when concepts can ALWAYS be explained in plain speech. Details tend to require math, but concepts don't --and concepts that are true ***always always*** make logical sense, Quantum Mechanics included. It is therefore extremely important, if CSS is valid in "positive" CF experiments and yet applies nowhere else, that some concept exist to uniquely distinguish CF heat measurements from all other heat measurements. Without that concept, why should anyone believe there is something different, that makes heat detected in CF experiments (and only in CF experiments) only an artifact and not "real"? Just because Kirk Shanahan says so? HAH!!! Per Occam's Razor, the simplest explanation for the heat in CF experiments is that it IS real. (Oh, but now it needs some ***other*** explanation, without which the data must be discarded, as the Physics community has done for 20 years, right?...) V (talk) 18:32, 1 June 2009 (UTC)

I don't know where you get the idea that the CCS applies only to cold fusion. It doesn't. I don't know where you get the idea that I have commented on any other application of calorimetry. I haven't. I don't know where you get the idea that I think the way the physic community rejected cold fusion is legitimate. I don't. You are reading your own biases into what I write, so that you can interpret what I say to favor your POV-pushing position, instead of just reading what I write. If you could ever read something in a normal, calm, unbiased fashion, you might be able to make some sense. Kirk shanahan (talk) 18:47, 1 June 2009 (UTC)

I quote from what I wrote above: *but you do not say it should be considered as a factor in every other use of calorimetry equipment* --where do you get the idea from that, that I said you *have commented on any other application of calorimetry* ??? It is your LACK of such commentary which is condemnable/revealing as a double-standard, since you focus its application upon CF experiments exclusively, when, if CCS is real, it should have been affecting calorimetry experiments for more than a century. So, YOU are the one *reading your own biases into what I write* --and thereby are spouting nonsense instead of sense. NOR am I in any way trying to legitimize *the way the physic community rejected cold fusion* --I was simply giving you the opportunity to be on the rejection side of such behavior AND WITH MUCH MORE REASON, since CCS needs a specific mechanism to ONLY be significant in CF experiments. That is, if it had been significant in many other experiments, discrepancies would have been in the literature such that the CCS explanation would have been developed decades ago. Yet, since chemists' heat measurements have successfully been validated by scaling-up throughout the chemical engineering industry for decades, without such significant discrepancies appearing, it is logical to conclude that CCS has not been a factor needing to be discovered. Now, you say, it is a relevant factor. OK, WHY????!! V (talk) 19:50, 1 June 2009 (UTC)

You are truly astoundingly dense V. I really believe you can't handle this, but I will do it once more so other readers can see how dense you really are. I have written this many times before here, on the CF Talk Page, and in my papers, but you can't follow it. So, for the last time: HERE IS THE CONCEPT. THE CONCEPT V. HERE IT IS. MAKE SURE YOU GET THIS.

---concept---concept---concept---concept---

IF one measures a calibration constant(s) at time t0, and THEN conducts an experiment at time t1, AND the system has shifted such that the calibration determined at t0 is incorrect, THEN IT IS AN ERROR TO APPLY THE EQUATION DETERMINED AT TIME t0 TO DATA COLLECTED AT TIME t1.

ONE MUST USE THE CORRECT CALIBRATION EQUATION TO GET A CORRECT RESULT FROM IT!

---endconcept---endconcept---endconcept---endconcept---

If Joule used a calibration equation that was inappropriate for the time he applied it, he would be wrong. If Einstein used a calibration equation that was inappropriate for the time he applied it, he would be wrong. If Nerst used a calibration equation that was inappropriate for the time he applied it, he would be wrong. If V used a calibration equation that was inappropriate for the time he applied it, he would be wrong. If Storms used a calibration equation that was inappropriate for the time he applied it, he would be wrong. If Fleischmann used a calibration equation that was inappropriate for the time he applied it, he would be wrong. If Shanahan used a

calibration equation that was inappropriate for the time he applied it, he would be wrong.

Heh, how nice of you to reveal YOUR OWN mental density, by once again avoiding the question I actually asked. I understood what you meant by the name "Calibration Constant Shift" when I saw the simple algebra and out-of-order explanations at the top of the "Requested Idiot's Guide" section. What I have been asking since then is "how can it happen?" You have gone in all sorts of directions to try to avoid answering that, including the intellectually dishonest route of claiming I should be banned. Tsk, tsk; banning a Questioner doesn't make the Questions irrelevant.

**But at last**, below this text I'm writing here-and-now, you have finally begun to say something that might be relevant. I'll insert comments as I go through it. V (talk) 05:28, 2 June 2009 (UTC)

Now, I got the REAL DATA from Ed Storms' cold fusion experiment, where he 'saw' a cold fusion excess heat signal, and I REANALYZED it to see if a slight shift in calibration constant would wipe out the signal. IT DID. The shift was 1 to 3%. THAT'S TRIVIAL IN THE REAL WORLD. That means the apparent excess heat signals COULD COME FROM A CALIBRATION SHIFT. That means Storms cannot claim it comes from cold fusion without proving it didn't come from a calibration shift. He did no such work. Therefore, he is INCORRECT in concluding cold fusion caused the signal.

OK, does "That's trivial in the real world" mean that CCS happens to that extent almost all the time in practically all other calorimetry measurements? If not, then you still have no case, until you can explain (as I've been repeatedly asking) why CCS is most likely to happen in CF experiments. V (talk) 05:28, 2 June 2009 (UTC)

I don't need to do anything else. I showed that a minor CCS could produce Storms' results. Now, does ANY OTHER CFER check for this. NO! In fact, they denigrate the idea, BUT IT IS JUST MATH!!! If they don't check to see if this is happening in their cell/calorimeter, likewise they cannot conclude it isn't and that the signals they observe come from CF. Without checking, there are two alternative explanations, and neither has been eliminated.

BAD LOGIC. You are making the unwarranted assumption that CCS MUST be happening in their experiments, without explaining why it must be happening. Again I point at the rest of the calorimetry field; if CCS happens practically everywhere, THEN OF COURSE the CFers would have a most excellent reason to look out for it. So they denigrate your hypothesis because you fail to explain, SENSIBLY, why only their experiments should suffer from it, and not all those other experiments in chemistry being measured with calorimeters. V (talk) 05:28, 2 June 2009 (UTC)

However, I continued to go on and show how such a shift might occur. Postulation only V. Not proof of anything, just a good idea. IF you recognize that the CF cell/calorimeter is ACTUALLY heterogeneous, with defined and specific heat loss pathways, then you realize you need, as an absolute bare minimum, a two zone model to describe it. Each zone will have a different heat collection efficiency. If heat moves from the low eff. zone to the high eff. zone, it will produce a need to recalibrate!

Well, that looks like an attempt to answer my question, but the last part of it is about as clear as mud. Certainly the experimental apparatus has multiple different components (heterogeneous). And it is reasonable to say it has defined and specific heat loss pathways, simply because we need to make sure the calorimeter covers all the pathways, to be able to make its measurement. However, EVERY chemistry experiment that yields or absorbs heat is in a similar "boat" to what was just described. That means they all need a minimum-two-zone descriptive model, and blah-blah-blah and recalibration, duh... --and your logic disintegrates with that point-of-failure, so again I don't really need to read more, until after you explain what is so different about CF experiments from other experiments. **DO NOTE that I can agree the CFers should check for CCS if it happens all**

*over.* But so far, I don't see you saying that (especially because I'm half-remembering some remarks on some Talk page or other about experiments specifically designed to find CCS and failing). V (talk) 05:28, 2 June 2009 (UTC)

In CF cells of the F&P type, this can happen if heat that is produced by recombination moves from either exiting the cell via the gas stream (open) or from the recombination catalyst (closed) to the electrode!

Now seems like a good time to recall that some time back you talked about hundreds of hours of accumulation of crud on the electrodes, after which somehow a clean electrode surface would be available to catalyze hydrogen-oxygen recombination. More illogic, that was, of the obviously-self-contradictory sort.... V (talk) 05:28, 2 June 2009 (UTC)

THIS IS ALL IN MY FIRST PAPER! READ IT!! Kirk shanahan (talk) 20:27, 1 June 2009 (UTC)

I'm amazed it got published. Should I suspect it had so much gobbledygook/jargon in it that the reviewers passed it because they didn't want to admit they couldn't understand how it SUPPOSEDLY made conceptual sense? V (talk) 05:28, 2 June 2009 (UTC)

Let me ask you something V. Did F&P have a viable explanation for how 'cold fusion' worked the first time they presented it? No?? Then why do you insist I do so. As I've said many times, science is data-driven. If you have good data, you aren't required to provide an explanation when you present the data. That can, and often is, done by others. You requirements that you are trying so hard to place on me are bogus. They show your bias, because as I note just above, you don't apply the same requirements to F&P.

WRONG --AND you are still wildly missing (or deliberately ignoring) the point I am trying to get at, especially since you have not responded to my Questions above, and (below) are trying to change the subject. That looks like evidence you cannot have a case, and refuse to admit it. ***If CCS is real, it must be detectable in more than one type of experiment.*** (Even neutrinos, the least detectable physical thing so far known, can be detected in more than one type of apparatus.) But in more than a century of calorimetry experiments, nothing like CCS has ever been seen. That statement actually includes CF experiments, ***because if CCS was the explanation for excess heat IN THIS TYPE OF EXPERIMENT, why doesn't it almost always happen?*** That is, if an experimenter is able to set up 10 identical-as-possible electrolysis cells, and only three shows signs of excess heat, why is CCS happening in those cells and not the other seven?? ***The factors involved in macroscopic events are far more controllable than the factors involved in nanoscopic events.*** For CCS to be real, macroscopic things must be happening. For CF to be real, nanoscopic things must be happening. Since the nanoscopic environment can be quite different in different regions of reasonably pure solid metal (examples: actual impurity distribution; the shapes of irregularities in the crystal structure, ...), it is quite logical that it can be difficult to pin down the parameters needed to reliably reproduce conditions in which cold fusion could happen. But for a macroscopic thing like CCS, the conditions needed to reliably reproduce that are of a larger and more manipulable scale (the calorimeter itself is a macroscopic device, and its calibration is a macroscopic thing, see?) -- ***yet it has been said that CCS has not been reproduced in experiments that looked for it.*** Therefore Occam's Razor tells us that the CCS hypothesis has a much lower probability of being true, than cold fusion. Finally, you still seem to be ignoring the fact that multiple explanations for CF have been proposed; ***CCS is not needed*** to explain the detected excess heat (which is perfectly fine, since, as I've just explained, CCS doesn't appear anywhere else, when it should appear). (later) I see I left out three key words in the preceding, that better

explain my position: ***NO SUPPORTING EVIDENCE*** . That is, the ONLY data "for" CCS is the data that CCS is designed to explain; there is no other data anywhere else hinting that CCS should exist. YOU should be able to specify a rationale for that (which IS something I've been asking for; the hypothesis needs to fit ALL the data, which includes lack-of-data, see?) V (talk) 20:43, 2 June 2009 (UTC)

Furthermore, you have devolved to the personal attack again, as is your custom. Again, the problem is yours, not mine. End of story. Kirk shanahan (talk) 11:21, 2 June 2009 (UTC)

I quote from Kirk Shanahan: *You are truly astoundingly dense V. I really believe you can't handle this -- AND -- you have devolved to the personal attack again, as is your custom.* I guess it is once again time to ask you, Kirk, to look up what the psychologists mean, when they use the word "projection". Have a nice day! V (talk) 13:52, 2 June 2009 (UTC)

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“It seems to me that all they need to do is present alternate evidence of excess heat, such as images of boiling electrolyte and melted palladium, for the calorimeter measurements to become well-nigh irrefutable” - Dude, you're really going backwards now! A) ‘They’ FIRST proposed the nuclear explanation, i.e. ‘true’ excess heat. That is the first alternative. I proposed a SECOND alternative. They have NEVER shot down my proposal. Therefore, there are TWO proposals out there to explain apparent excess heat. The appropriate thing for a good scientist to do is test the CCS experimentally. B) I have ALSO proposed conventional explanations for HAD and apparent melting, which by the way you are lumping together without justification. So the situation is that I have heard all their explanations and seen all their evidence, and find a conventional explanation available for all. That means the case is undecided, and will NOT be decided by ‘more of the same’. New work has to be done to test the conventional explanations, or to prove the nuclear one. What is going on so far is just to repeat the old stuff that was inconclusive. Bad science! You ABSOLUTELY must get this concept down – “When two viable explanations exist for a set of observations, it is inappropriate to conclude one or the other is true (or false).”

“neutron tracks are potentially pretty convincing evidence” – conventional explanation available, not neutrons, therefore ‘CF is nuclear’ is premature.

“I don't see any reason to agree with that assessment until the phenomenon is reliably documented to have occurred in experiments that specifically look for it” – note crucial concept above – you are doing this backwards, which is the sign of having an explanation you like and forcing the experimental data to ‘match’ it. Let me be very clear: Demonstrating a CCS is a viable explanation in ONE case, and ONE case only, is adequate grounds to consider the possibility in any similar experimental data set. The ONLY criterion here is: Was a calibration equation used?

“And I disagree because of (so far as I know at this writing) the lack of evidence that CCS has ever happened anywhere.” – Lack of evidence??? See *Thermochimica Acta* 387 (2002) 95.

“It appears you are jumping to conclusions moreso than me.” - I have jumped to no conclusions. I have, literally, spent years coming to mine.

“but with CCS in CF experiments not even having at least an explanation...see?” See *Thermochimica Acta* 387 (2002) 95. (or the manuscript on Rothwell’s site – technically the same thing.)

“Yes and no; the thing you proposed needs to be considered as a factor after it is proved that such a factor

actually exists. So far as I've seen, though, nobody has yet demonstrated an actual case of genuine CCS in action.” - Where did you learn to do science? Even in high school the ‘scientific method’ is taught as “observe-hypothesize-test-refine”. The “test” is of the hypothesis, i.e. the first thing you do after observing something interesting is propose an explanation (a ‘hypothesis’). You formulate a hypothesis, then you go test it. Normally, one tries to propose rational and reasonable hypotheses before going off and spending time and money to collect more observations. Buy you ‘prove’ such a factor exists by proposing it and designing a refined experiment to test it There is no other way to ‘prove’ it exists. So, no, you don’t have to prove something exists before you consider it. Further, ‘somebody’ has demonstrated an actual case of genuine CCS in action. That somebody is me, and I did it with Ed Storms results.

“Ummm...actually, the conclusion I had in mind was, "CCS happened in lots of other experiments, even predating CF experiments by decades", and what you have just written does not seem to support that.” – How would I know if a CCS has occurred in other, pre-CF, experiments? Cite the ones you refer to. Otherwise your argument is just B.S. The way science normally works is that serious errors don’t become apparent until some grandiose claims are made based on them. Then the scientific community responds and finds the errors. Then the experimenters are supposed to go back and prove (i.e. satisfy themselves) that such an error was present. No CFer has done that, and I have no clue about whether such a claim was ever made pre-CF. Agin, cite your cases or quit bringing up this fictional red herring. As far as I know, the CF use of calorimetry is unique, that’s why they all claimed back in 1990 that one had to be an expert in CF (i.e. worked with F&P or under their direction) for at least two years before the researchers could be considered ‘trained’.

“else CCS as a hypothesis for explaining various measurments would have first appeared many decades ago” – one of the many ‘Golden Rules of Thumb’ in analytical chemistry is that “You can’t calibrate an unstable system.” The term ‘CCS’ is a short-hand notation I introduced here in CF discussions to simplify the discussion. It describes what happens when your system is unstable and you try to calibrate it. The principle is well known and has been for generations. All I did was to point out dramatically that it has the potential to explain most, if not all, claims for excess heat. P.S. That doesn’t touch on any other specific claims aimed at supporting nuclear reactions. But those other claims are all explainable by even less dramatic conventional explanations.

“If the CF'ers are finding heat outside that range, yet it is inside the noise range after CCS is PRESUMED to occur, then it becomes extremely important to prove that CCS does indeed occur.” – Finally, something that is absolutely correct. Wonder why the CFers can’t see this?? And why they can’t see that is EXACTLY what I did in my 2002 paper...

I am devoting too much time trying to get the concubines to do the formation, while NOT having the authority of the emperor to lop off heads. Lacking that authority, the laughing and giggling can go on forever. I disengage now. But for the record I restate, there are conventional explanations for every type of claim made to support the proposed nuclear nature of ‘CF’. (The CCS is only directed at apparent excess heat so far.) Thus the jury should still be out. People who claim the decision is in are doing so not based on science but on personal belief. The CF article needs to make that clear, and does not. Kirk shanahan (talk) 15:27, 26 May 2009 (UTC)

## Please don't chop up the comments of others

Kirk, twice today on Talk:Cold fusion you interspersed a reply to me in the middle of my comment. In both cases, I framed it with smalltext notes, but that's usually considered quite rude, and, if special care isn't taken, it can make our comments appear to be by the same person, i.e., you. Since my first section had no signature. Or the reader has to read down below you to figure out who wrote what was above you. Please don't do this. Thanks. --Abd (talk) 04:28, 28 May 2009 (UTC)

Then stop writing multi-page tracts with so many errors in them it takes books to correct you. Stick to

proposed edits to the articles. Stop trying to promote CF. Kirk shanahan (talk) 13:55, 28 May 2009 (UTC)

## Civility

Comments like "Do you really not undersand English?" are not acceptable, per our policy on WP:CIVILITY. Do not repeat this behavior. Thanks. Hipocrite (talk) 13:22, 28 May 2009 (UTC)

Your comment is outrageous. After V writes: "I WILL say that your 3rd sentence is a bit "off" and after his and Abd's) multitude of insults, slams, and personal attacks, you write: "Comments like "Do you really not undersand English?" are not acceptable". Where were you when Abd and V were doing this multiple times to me! I really think Wikipedia is incapable of handling controversial issues like CF All my experience here suggests that. Kirk shanahan (talk) 13:54, 28 May 2009 (UTC)

I don't think warning V and Abd for their persistant disruptive behavior has any efficacy at this point. If you can't tune them out, try unwatching the cold fusion talk page until such time as they give up. Hipocrite (talk) 14:28, 28 May 2009 (UTC)

Hipocrite, I sure hope you have been paying attention; you will see that Kirk is far more guilty of doing personal attacks than either Abd or myself. Me, all I do is point out the irrationality present in various statements Shanahan has made, and that has absolutely NOTHING to do with a 'personal' attack. To be more specific, there IS a difference between statements such as "Kirk has spouted the following nonsense" and "Kirk is nonsensical" --ONLY THE LATTER would be a personal attack (and it "does not count" here because I'm trying to present examples here, rather than to actually make such statements -- which if you will look for them you will see I indeed have not made any such statements as that latter example --even Kirk won't be able to find one!). As to why Kirk consistently appears to confuse statements similar to the former example, that I've actually written, with the latter example, that is HIS problem, not mine or yours. (And if he thinks that deliberately mischaracterizing other people's remarks will "win points" somehow, then he needs to think again, because I'm perfectly willing to state, however many times it takes, over and over again, until he stops, that Kirk is mischaracterizing my remarks!) V (talk) 16:30, 28 May 2009 (UTC)

Wow, ROFL!! Check this out - And if V thinks that deliberately mischaracterizing other people's remarks will "win points" somehow, then he needs to think again, because I'm perfectly willing to state, however many times it takes, over and over again, until he stops, that V is mischaracterizing my remarks! All I do is point out the irrationality present in various statements V has made and he never incorporates that. He keeps claiming I am irrational and make no sense, this while others are clearly following what I say, and even understanding it! As I explained with the Sun Tzu story, constantly failng to incorporate my comments is an implied personal attack against the one who is trying to teach. That's exactly what the concubines were doing to Sun Tzu. Oh for an axe... Kirk shanahan (talk) 19:46, 28 May 2009 (UTC)

More mischaracterization (perhaps "lie" is a more accurate word) by Kirk, who has indeed made various statements that are either irrational or make no sense, while I have never stated that he is irrational. I will quote an example of me explaining in detail a bit

irrationality spouted by Kirk (preceded by the stuff that led up to my explanation):

*Then there is Kirk Shanahan wanting us to believe that a small amount of chemical heat from hydrogen-oxygen recombination can throw off a calorimeter designed to register lots more than a "small amount" of heat, and throw its calibration off to the extent that if it measures a lot of heat while an electrolysis cell boils its electrolyte away and melts one of its electrodes, we have to pretend it didn't actually happen; that it was all an illusion associated with a temporarily uncalibrated calorimeter. I agree that that speculation should be in the article, for entertainment purposes if nothing else! I disagree on giving it any more weight than claims that hydrinos can explain cold fusion. V (talk) 15:24, 19 May 2009 (UTC)*

*More ad hominem from V. I will post a reply on my Talk page immediately. KirkShanahan (talk) 15:54, 19 May 2009 (UTC)*

*(Partly because his talk page does not have a reply there yet)... More bad logic by Shanahan. My paragraph above contains a description of the CCS hypothesis, admittedly written to blatantly expose its most fundamental flaw (excess heat can possibly be detected by means other than a calorimeter). Since CCS is "pushed" and defended by Shanahan, there is nothing faulty in writing about "Kirk Shanahan wanting us to believe" it. NOR is there an inherent fault in, generically, Person A wanting Person B to believe something-or-other (sometimes that desire is even vitally important, like when Person A discovers the house they are in to be on fire). But somehow Shanahan has concluded that my statement somehow describes a flaw in Shanahan; by definition the statement "More ad hominem from V" can only be true if my statement describes a flaw in Shanahan. WELL, WHERE IS THAT DESCRIPTION???*

Kirk never answered that question. I will add here that I don't know what Sun Tzu story he is talking about, in the last part of his post above, nor do I see a logical connection between failure to do something and an action which could be called "a personal attack". V (talk) 13:13, 29 May 2009 (UTC)

As you may have now noticed, I added a list of suggestion to improve the article to the Talk page. I have been trying to get the bulk of these accepted for months. Literally since the Sept. 17, 2008 version. When I added some of the text to the article, it was all block deleted by Pcarbonn. Thereupon we embarked upon me trying to explain why what I wrote was right, and pcarbon wikilawyering me to death. (Note I attempted to seek consensus here, in what is a controversial subject.) Then Pcarbon got banned, but immediately V and Abd stepped up to the plate. Am I supposed to expect any better treatment at their hands if I edit into the article

what I think it needs to be neutral POV? I think not. So, I try to explain to V and Abd. But none of it gets through. So, that's how we got where we are today. What is a workable pathforward to an article that fairly and equitably presents *\_both\_* sides of the controversy? Long ago I suggested segmenting the article into 3 parts (history, pro, con) and forbidding advocates from editing the other side's section, but after watching Wiki at work, I doubt we could do that without massive user banning activity. (P.S. - I would gladly not edit the pro side.) Kirk shanahan (talk) 16:21, 28 May 2009 (UTC)

I suggest that you suggest one concrete small change to the article on the talk page - either an addition, removal, modification or move of some text. Hipocrite (talk) 16:28, 28 May 2009 (UTC)

Kirk, Hipocrite is very much on the other side of the cold fusion debate, but is giving you sound advice. It seems you understand practically nothing about Wikipedia guidelines and policy. If what you put in the article in September was like what you just suggested, you are right: nearly everyone would be in favor of taking it out, Wikipedia is "against you." If you look at my response to your specific suggestions, at Talk:Cold fusion, you will see that I note that some aspects of your suggestion may actually be good ideas. But I don't think you have any clear idea as to how to get there. I do. Wikipedia guidelines and policies may be difficult for people not familiar with certain theories of knowledge and process, but they are actually brilliant. Frustrating, sometimes, but necessary. (It can be quite irritating to know something for a fact, for that fact to be well-known in the field, but to be unable to put it in the article because, for some reason, it's not been published in a peer-reviewed journal or other reliable source.) If you will open your mind to the possibility of learning something here, you might actually come to recognize why the guidelines and policies are the way they are. Otherwise, you will probably rant and rave until you are blocked or banned, continually being convinced that the system is warped against you. Your choice.

Your suggestion about having each side edit their subarticle actually has in it an element of wisdom. But they wouldn't actually be articles; rather they will be detailed and well-organized consensus documents, the best that each side can develop, not in article space, and used, then, to assure that article decisions are made based on clear and organized evidence. In some kinds of RfCs and before ArbComm, on the Evidence page, nobody is allowed to edit the evidence presented by any other editor, nor to respond to it within that editor's section. --Abd (talk) 18:43, 28 May 2009 (UTC)

More ROFL!! Yes, Abd, I have severe heartburn over the way you and V and PCarbonn wikilawyer the policies. You don't see me asking about them because, over a year ago, whenever anyone quoted policy at me, I went and read them in detail. In all cases, I found that appropriate outs were provided that would allow this situation to be resolved, but PCarbon in particular would never accept that, since he too was a CF fanatic, and accepting the provided outs would have let anti-CF text into the article. So the wiklawyering began. You too are an expert at using policy to stop good editing. All one needs to do is look at all the errors and misrepresentations in your response to my recent suggestions to see that. Your misinterpretatins of policies leaves the CF article as a clear example of the blind leading the blind, because you refuse to comprehend what is SOP in

the fields used to generate CF data, even when I spell it out for you, letter by letter.

For example, your big diatribe about the 23.8 MeV number. You understand of course, the the Clarke, Bos, and Oliver paper (clearly RS) suggests that McK's He numbers are 2x what they are supposed to be. That means the computed MeV/He should be 2x also. Thats up around 60 MeV/He atom. That is clearly outside the realm of reality, and that in turn means the conclusion that those results supports CF is unfounded. You're going to let me put that in the article right??? Ha! Sure... Kirk shanahan (talk) 19:46, 28 May 2009 (UTC)

Why do you keep responding to Abd when I very clearly asked you to suggest one concrete small change to the article on the talk page - either an addition, removal, modification or move of some text? Do you think that you and Abd arguing is helping anything? If so, what exactly is it helping? Hipocrite (talk) 19:48, 28 May 2009 (UTC)

For a couple of reasons. A) It's my talk page here, and I can choose to respond or not as I see fit. B) As I indicated above, Abd isn't going to let me do anything anyway. We will just have more pages and pages of Abd (and V) garbage. He's a fanatic, and ignoring fanatics doesn't make them go away. You have to confront them and get them to quit, leave, move on, whatever. However, I am always willing to be proven wrong, so why don'y YOU, as a 'neutral' pick one of my suggested changes and we all discuss it on the CF Talk page. Kirk shanahan (talk) 20:10, 28 May 2009 (UTC)

No thanks. Propose a concrete change to the article and we can discuss it. Hipocrite (talk) 20:11, 28 May 2009 (UTC)

That's a shame. I proposed several. Several have already been the subject of 'discussion'. I was hoping someone outside the Shanahan-(V,Abd) pairing would participate. As it stands, if I edit the article to put in the anti-CF side that is needed for balance, those two will immediately delete it, just as PCarbann did. Kirk shanahan (talk) 11:17, 29 May 2009 (UTC)

I don't discuss changes that are not expressed as concrete proposals to add, remove or modify text from the article. Make a proposal to do that, and I'll happily discuss it with you. Hipocrite (talk) 11:29, 29 May 2009 (UTC)

OK, let's go back then to a concrete proposal I have made several times: Revert back to the version of Sept. 17, 2008 ([http://en.wikipedia.org/w/index.php?title=Cold\\_fusion&oldid=239093535](http://en.wikipedia.org/w/index.php?title=Cold_fusion&oldid=239093535)) and start over. That version had everything I have said above that is needed (with a couple of exceptions that will need to be acted on), a historical section (written by PCarbann), a 'pro' section (also written by PCarbann), and a just-modified 'anti' section (to which I will claim primary authorship). Abd has already said it is 'too wordy' (ROFL again), but I am not adverse to editing it, I just can't tolerate the block deletion tactic of PCarbann. Kirk shanahan (talk) 11:48, 29 May 2009 (UTC)

Too many changes. Propose one concrete addition, removal or modification of text from the article, please. Hipocrite (talk) 13:06, 29 May 2009 (UTC)

You know H, it's fine to be a stickler for rules and 'one-at-a-time' approach, but there are consequences to such. Here, the problem

will be that we have two contentious editors who fail to understand the 'RS' situation w.r.t cold fusion. They will do just as PCarbott did, and bog down the process of getting the facts in to give the article balance, because they don't want such. But, we'll try it for awhile. I don't have infinite time, and you've got nobody else who trying to present the mainline view of the field. You (generically speaking here) may want to fold that into your editing process. Kirk shanahan (talk) 13:32, 29 May 2009 (UTC)

I have demonstrated an ability to ignore Abd and V if they can't focus on article edits before, and I'll do it again. Thank you for your efforts. Hipocrite (talk) 13:38, 29 May 2009 (UTC)

As I said, ignoring them doesn't solve the problem. What I intend to do in the discussion I have hopefully started, is to condense irrelevant sections and then paraphrase any cogent points I can find. I always have difficulty doing so with their writings, but I will try. I will focus on potential points they raise that refer specifically to the item(s) under discussion with reference to improving the article. If this violates Wiki policy somehow, so be it. I can't deal with the misrepresentations and misdirections of Abd and V anymore. Kirk shanahan (talk) 14:03, 29 May 2009 (UTC)

## Continuing with V's Intransigence

Picking up from section "Convenience Break" above:

I wrote: "If you have good data, you aren't required to provide an explanation when you present the data." V replied: "WRONG" - Sorry V, but the above statement is perfectly true.

That word "WRONG" was not for the part you quoted of yourself, but for this part that you did not quote (and so your feeble attempt to distort the conversation gets you nowhere):

*Your requirements that you are trying so hard to place on me are bogus. They show your bias, because as I note just above, you don't apply the same requirements to F&P.*

I also wrote, to better explain why you are wrong, this: *(later) I see I left out three key words in the preceding, that better explain my position: **NO SUPPORTING EVIDENCE**. That is, the **ONLY** data "for" CCS is the data that CCS is designed to explain; there is no other data anywhere else hinting that CCS should exist. YOU should be able to specify a rationale for that (which IS something I've been asking for; the hypothesis needs to fit ALL the data, which includes lack-of-data, see?)* --That is, the rationale for lack of data for CCS in other places needs to be part of the hypothesis, and that "requirement" is not in the slightest "bogus"; it is another key to pointing the way toward verifying CCS as a real phenomenon.

Now compare that to your false claim that I *don't apply the same requirements to F&P*; I explained the difference between macroscopic control of experimental parameters (which we do have) and nanoscopic control (which is still-budding technology). SOME things are quite solidly established,

such that excess heat does not appear until quite a lot of hydrogen has permeated into metal. It could very well be that the nanoscopic structure of the palladium used in electrolysis experiments usually allows hydrogen to leak out almost as fast as it permeates in, so that only rarely does the critical proportion of hydrogen-to-metal get reached (and takes many many hours, when it does happen; relevant quote: "a stern chase is a long chase"). I think I've mentioned an electron-catalysis hypothesis that matches many aspects of CF experiments (why metal is required, why so much hydrogen permeation is needed, why neutron production is so rare, yet happens more often in codeposition experiments, etc.). By comparison CCS hypothesis is nowhere near as complete (yet you resist completing it). Electron-catalyzed fusion is also is not limited to electrolysis experiments, while CCS, if needing hydrogen-oxygen recombination as a source of energy to shift the calibration constant of a calorimeter, cannot explain excess heat production when deuterium gas is pressurized into metal (no oxygen around, nor any electric-resistance heating; the only heat sources are the well-understood Gas Law and the exothermics of hydrogen "dissolving" into metal--different substances, different heats-of-solution; nothing strange there). V (talk) 12:20, 3 June 2009 (UTC)

“since you have not responded to my Questions above” - But I have, many many times. You just don't like my answer.

WRONG, AGAIN, since my Questions have been and still are about a completed hypothesis, even when not specifically phrased that way, while your repeated so-called (and actually worthless) "answer" is to say you don't need to answer my Questions. V (talk) 12:20, 3 June 2009 (UTC)

V also wrote: “ *If CCS is real, it must be detectable in more than one type of experiment.* ” – Sorry V, that's wrong too. What if the mechanism is only applicable in these situations (which in fact is what I propose in my first paper)?

Then you need a complete rationale to support that hypothesis, which you have refused to provide (and to the bare extent you have provided anything along that line, fails to explain excess heat production in pressurized-gas experiments). V (talk) 12:20, 3 June 2009 (UTC)

“can be detected in more than one type of apparatus” – But it has, closed and open cells, cells irradiated with laser light, cells irradiated with ultrasonics, isoperibolic calorimeters, flow calorimeters, Seebeck calorimeters, etc., etc. What is it about understanding all this that is so hard for you? You keep attributing things to me that you claim I have or haven't done, and you are flat out wrong on them.. I tell you, but you don't hear or comprehend.

WRONG, AGAIN; because you are strictly focussing on CF experiments, while calorimetry has been used in all sorts of chemistry experiments for many decades, and in none of those places has CCS appeared to have affected the data --yet the calorimeters used should have been just-as-susceptable (perhaps more so, to the extent that "older" equals "lesser precision"). I quote from above, a place you failed to respond:

*Well, that looks like an attempt to answer my question, but the last part of it is about as clear as mud. Certainly the experimental apparatus has multiple different components (heterogeneous). And it is reasonable to say it has defined and specific heat loss pathways, simply because we need to make sure the calorimeter covers all the pathways, to be able to make its measurement. However, EVERY chemistry experiment that yields or absorbs heat is in a similar "boat" to what was just described. That means they all need a minimum-two-zone descriptive model, and blah-blah-blah and recalibration, duh... --and your logic disintegrates with that point-of-failure, so again I don't really need to read more, until after you explain what is so different about CF experiments from other experiments. DO NOTE that I can agree the CFers should check for CCS if it happens all over. But so far, I don't see you saying that (especially because I'm*

*half-remembering some remarks on some Talk page or other about experiments specifically designed to find CCS and failing).* V (talk) 12:20, 3 June 2009 (UTC)

“But in more than a century of calorimetry experiments, nothing like CCS has ever been seen.” – So what? To see neutrinos, don't you need a special apparatus that detects neutrinos?

Interjection: YES, except that "special" in this case is also NOT-special in a certain way; widely different substances have been used to detect neutrinos, from chlorine-containing compounds ( <http://www.bnl.gov/bnlweb/raydavis/research.htm> ) to pure ordinary water ( Super-Kamiokande ) to elemental gallium ( GALLEX ) to pure heavy water ( Sudbury Neutrino Observatory ). V (talk) 12:20, 3 June 2009 (UTC)

Why would the CCS be any different?

Interjection completion: OK--Where is the equivalent wide differences in substances, reacting, that cause CCS? V (talk) 12:20, 3 June 2009 (UTC)

The cold fusion electrolysis cell is quite unique. And as I have said, I haven't studied other configurations, like the Arata experiments (even tho there are obvious problems with that), so I have no idea why they might show a CCS. But I know what it takes to discredit the idea of a CCS being active, and no cold fusioner has done that. Ergo, the possibility remains.

HAH! The possibility can be ignored so long as (A) it remains undetectable anywhere else, AND (B) no rationale exists to explain why it has never affected experiments anywhere else. Your "ad hoc" hypothesis is simply too incomplete to be credible. I refer you again to the paragraph quoted a short distance above, which explains similarities between CF and other experiments, and to which you did not originally respond. V (talk) 12:20, 3 June 2009 (UTC)

“That is, if an experimenter is able to set up 10 identical-as-possible electrolysis cells, and only three shows signs of excess heat, why is CCS happening in those cells and not the other seven???” – Let's be very clear, 10 cells where 3 show ‘abnormal’ or ‘unusual’ activity are not identical.

Interjection: I specified "identical-as-possible", and I phrased it that way because it is not currently possible to ensure the nanoscopic conditions inside palladium metal are identical in a group of experimental set-ups. I repeat that different nanoscopic conditions can play a key role in CF experiments, *provided CF is real* . Meanwhile, CCS remains macroscopic. V (talk) 12:20, 3 June 2009 (UTC)

The experimentailst may have thought they should be, but guess what, they weren't. My proposed mechanism gives one an idea of what to look for to answer your question, if it so happens my proposed mechanism has some correctness to it. That is why I proposed it, to give CFers something to look for, instead of mindlessly wandering around chanting “It's Nuclear. It's Nuclear.”.

(A) You are *acting* no differently, apparently mindlessly chanting, "CCS did it..." without adequate details.

(B) WRONG --since more-complete CF hypotheses exist than the simple/basic "It's Nuclear".

“For CCS to be real, macroscopic things must be happening. For CF to be real, nanoscopic things must be happening. “ Absolutely correct for once. But, since you are trying to use it to ‘prove me wrong’, I would guess you don't know why. CCS is a macroscopic thing. The proposed energetics of the CF solution to the dilemma of why 3 of 10 are abnormal forces the CF solution to be nanoscopic. Unfortunately, I challenge the proposed energetics directly, by saying “There ain't no excess heat!” The CFers have not produced a counter to

my CCS proposal such that they could say the reverse thing towards me.

Yet you utterly fail to explain why CCS does not happen in the other seven cells, when all ten are producing lots of hydrogen and oxygen that might recombine and do "whatever" to shift calibrations, after the same length of run-time (or even longer, since run-time is not a known/fixed parameter for CF experiments). And, because you fail to explain that, that is a major reason why CCS does not make adequate sense, especially when the CF explanation can make a lot of sense. V (talk) 12:20, 3 June 2009 (UTC)

“But for a macroscopic thing like CCS, the conditions needed to reliably reproduce that are of a larger and more manipulable scale (the calorimeter itself is a macroscopic device, and its calibration is a macroscopic thing, see?) -- *yet it has been said that CCS has not been reproduced in experiments that looked for it.* Therefore Occam's Razor tells us that the CCS hypothesis has a much lower probability of being true, than cold fusion. “ – More screwy logic V. Occam’s Razor tell us the CCS is much more likely because no ‘miracles’ are required.

Interjection: There are no miracles required for electron-catalyzed fusion, either, and therefore you are wrong again, especially because ECF is a more complete hypothesis than CCS, which fails to explain why CCS doesn't happen in long-running "failed" CF experiments. I am therefore correct about Occam's Razor. V (talk) 12:20, 3 June 2009 (UTC)

You are correct that, in principle, the CCS is more manipulatable than the proposed CF mechanism. But you have to go out and try to do that to be able to do that. You follow? You have to define the parameters to be manipulated quantitatively via experimentation. Please point to where a CFer has manipulated a cold fusion cell in an attempt to prove that there is a special active surface state that fosters at-the-electrode recombination which only develops under special circumstances? Can't find one?

I recall that somebody among the CF editors mentioned that there had indeed been some experiments specifically set up to find CCS, and they failed to find it. Meanwhile, I can remind you of the pressurized-gas experiments as an alternate way to produce excess heat. That's a widely different manipulation of the conditions of the experiment, right? See, if CF is real in electrolysis experiments, then it does not really matter HOW the deuterium gets into the metal; all that matters is that enough gets in! (I'd like to see a variant electrolysis experiment that uses a pressurized cell; perhaps that hydrogen-leakage thing I mentioned above can be reduced to the point that excess heat will practically always appear.) V (talk) 12:20, 3 June 2009 (UTC)

So why do you expect there to be any ‘proof’ for the conventional explanation at this point? There isn’t. The mechanism is just ‘proposed’, not proven. What is proven mathematically is that the CCS might have the potential to explain the large majority of excess heat claims. Pretty darn good reason to go try to prove or disprove it’s there don’t you think?

NOT AS LONG as it can't explain its nonexistence everywhere else, ESPECIALLY including "failed" CF experiments. V (talk) 12:20, 3 June 2009 (UTC)

“Finally, you still seem to be ignoring the fact that multiple explanations for CF have been proposed; *CCS is not needed* to explain the detected excess heat (which is perfectly fine, since, as I've just explained, CCS doesn't appear anywhere else, when it should appear).” - The reason multiple explanations have been proposed is because none of them fit all the data,

Interjection: That may have recently changed, with the electron-catalyzed-fusion hypothesis. V (talk) 12:20, 3 June 2009 (UTC)

and none of them give any predictability.

What sort of predictability would you like? I've seen proposals associated with ECF that involve elemental lead as the metal to use (lots cheaper), and also cold/superconducting titanium (the idea there was that if CF energizes electrons, in a superconductor the result might be the straightforward generation of Direct Current power). Of course, none of them have been tried yet.... V (talk) 12:20, 3 June 2009 (UTC)

And, you are flat out wrong in stating that CCS must appear somewhere else. It might, it may, but there is no requirement that it does.

I misphrased that, not to mention it is only half the argument; CCS must be *able* to appear somewhere else OR there must be an iron-clad rationale why not. V (talk) 12:20, 3 June 2009 (UTC)

CCS is needed because none of the other theories have any predictive power nor have they produced any improved control over the effect. The proof you understand what you are doing is in demonstrating control over the effect. Never been done with 'cold fusion'. Kirk shanahan (talk) 18:06, 2 June 2009 (UTC)

WRONG AGAIN, PARTLY, simply because the percentage of experiments producing some excess heat has risen significantly over the past two decades. For CCS to be the explanation of that, it means the calorimeters have become more faulty instead of more reliable! V (talk) 12:20, 3 June 2009 (UTC)

V has clearly demonstrated (once again) that he has a completely warped view of how science is conducted. He insists vehemently that I MUST provide an theory for how a calibration constant shift can occur before anyone needs to consider it. Of course this is nonsensical. It does however, fit nicely with the way cold fusioners think. They routinely gather insufficient amounts of data and leap to grandiose conclusions and then defend them to the death with whatever tactics they can use. The fact is that once the CCS is defined mathematically, and demonstrated in one case as reasonable, and extended to other similar cases inductively, then it must be considered a possibility. There may well be cases where it is unlikely, as might be the case in the fictitious examples V keeps referring to, but that is not determined before the data is collected. Only after. And, for the record, V's recollection is wrong on whether anyone has tested for a CCS. They haven't. It's easy to do, but they don't do it.

I'd love to just quit arguing with V, as he has proven beyond a shadow of a doubt that he doesn't understand scientific research (even if he does have a PhD as Hipocrite has suggested), but V is a disruptive editor of the CF article. He attempts to force his warped views on science and cold fusion onto the rest of us, and goes on and on and on about it (just like Abd). Instead of seeking consensus and some semblance of scientific fact in a technical article, he tries to wear the rest of us down so we quit. For that reason, I feel I must continue to point out his errors and inaccuracies.

BTW V, the mechanism you want is fully described in my first paper, which is downloadable from the Internet. By not doing so and reading it, you prove your intent is just to argue and browbeat. For more information on it, you can go to my 2nd and 3rd (esp.) publications, but I know you won't. Before I conduct any more discussions with you, you must prove that you have read and understood the homework I am assigning. You must repeat back my physical/chemical mechanism for a CCS. then you will have shown you have read it. But, I'm sure you'll find some lame excuse not to do this, because to actually read and understand what I write will force you to change your viewpoint. Old saying: "The Truth hurts..." Kirk shanahan (talk) 13:22, 3 June 2009 (UTC)

Kirk Shanahan has clearly demonstrated (once again) that he has a completely warped view of how science is conducted. He insists vehemently that he must NOT provide a theory for how a calibration constant shift can fail to occur before anyone needs to consider it. Of course this is nonsensical. It does however, fit nicely with the way CF-detractors think. They routinely play with large amounts of obsolete data and leap to grandiose conclusions and then defend them to the death with whatever tactics they can use. The fact is, *once the CCS is defined mathematically, and demonstrated in one case as reasonable, and NOT extended to other nearly-identical cases where it should have occurred and obviously didn't, then it must be considered completely ignorable, due to very obvious logical inconsistency* . There may well be cases where it is likely, as might be the case in the examples Kirk keeps referring to, but that is not determined *in the absense of complete data*, involving both CCS-events and non-CCS-events. Only after. And, for the record, Kirk's claim that a CCS is easy to find is wrong. No one ever found it, in more than a hundred years of calorimetry measurements, before Kirk concocted it out of thin air.

I'd love to just quit arguing with Kirk, as he has proven beyond a shadow of a doubt that he doesn't understand scientific research, but Kirk is a disruptive editor of the CF article. He attempts to force his warped views on science and cold fusion onto the rest of us, and goes on and on and on about it (as bad as Hipocrite's Soviet-style vetoing of anything he doesn't write). Instead of seeking consensus and some semblance of scientific fact in a technical article, he tries to wear the rest of us down so we quit. For that reason, I feel I must continue to point out his errors and inaccuracies.

BTW Kirk, the mechanism I want is not at all described in any of your papers, which are downloadable from the Internet. By not writing and posting it, you prove your intent is just to argue and browbeat. Before I conduct any more discussions with you, you must prove that you have read and understood the homework I am assigning. *You must present a physical/chemical mechanism for failure of CCS to appear in experiments basically identical to those in which you claim it does appear, failed experiments which provided plenty of time and opportunity for CCS to appear*. Only then you will have shown you have a hypothesis worth reading. But, I'm sure you'll find some lame excuse not to do this, because to actually think about the problem is to understand that it is inherently logically flawed, forcing a change in your publicly stated viewpoint. Old saying: "The Truth hurts..." V (talk) 16:19, 3 June 2009 (UTC)

Clearly there is no point in continuing this. This page however will serve as a guidepost to interested readers. Some may think V is right, some nmay think I am right. Bottom line, my approach is how science is done. I stand by it. Kirk shanahan (talk) 17:40, 3 June 2009 (UTC)

And I stand by my approach; science is always about both generalities and specifics, not just one of them, and logical consistency throughout is paramount. V (talk) 18:01, 3 June 2009 (UTC)

Psst V, if we do a simple substitution of 'DCF' (Deuterium Cold Fusion) for 'CCS' in the above, let's see what we get. *You must present a physical/chemical mechanism for failure of DCF to appear in experiments basically identical to those in which you claim it does appear, failed experiments which provided plenty of time and opportunity for DCF to appear*. . Wow! I guess F&P are jerks just like me!! Insisting DCF works when they can't explain why it doesn't work in all those failed runs.

And of course, the mainline physicists who lambasted F&P (and others) for proposing a wild, hare-brained theory that doesn't

conform at all to what we know is the way D-D fusion works, they were all *\*completely\** justified and correct in their rejection of 'DCF', right? Kirk shanahan (talk) 13:35, 4 June 2009 (UTC)

Tsk, tsk, I already explained it clearly. Have you forgotten so soon? ***We are not yet able to ensure the palladium electrodes are identical at the nanoscopic scale.*** Since fusion, if it occurs inside metal, is necessarily nanoscopic (even sub-nanosopic), it follows that different events can occur in different electrodes (leading to other differences like megajoules or no joules of excess heat). Meanwhile, CCS is macroscopic --you agreed that it is!-- and at *that* scale, we can make the cells identical enough to expect consistent experimental results. And therefore CF is still a better, more "fitting" explanation, for the experimental results, than CCS. V (talk) 08:06, 7 June 2009 (UTC)

If you substitute the word 'ghostly' for 'nanoscopic', you get something which means about as much as what you wrote. Until you define what your nanoscopic process is definitively, you could just as well say 'a ghost did it'. Not a scientific explanation in either case. Kirk shanahan (talk) 02:33, 8 June 2009 (UTC)

More illogic from Kirk: I had previously written, and I now quote: "(examples: actual impurity distribution; the shapes of irregularities in the crystal structure, ...)" as examples of things at the nanoscopic scale that can differ between one piece of metal and another. To the extent that the hypothesis of fusion is supported by the evidence, that evidence is quite clear that differing batches of palladium can affect the electrolysis experiments. We do not yet know the **KEY WAY** those batches are different, that enhances or inhibits those experimental results. But it is perfectly logical that if fusion is sometimes happening inside the palladium, then *something* about its internal structure needs to be "right", to encourage it. For all I know, all it needs is some kind of "dam" (of impurities?) that causes deuterium flowing through the metal to accumulate in some locale faster than it can exit; the gas-pressurization experiments, so far as I've read about them, don't seem to have the reliability issues of the electrolysis experiments (not counting the codeposition experiments). ***MEANWHILE*** back to the subject Kirk is trying to avoid: CCS is still macroscopic, and we can make the cells identical enough at that scale to expect consistent experimental results, if CCS was the explanation for the claimed observations of excess heat. Since we don't get consistent results in the standard electrolysis experiments,

CCS does not fit *all* the data, and therefore is lacking in believability as an explanation for that data. V (talk) 05:46, 8 June 2009 (UTC)

There is a definition of insanity that says it is repeating the same thing time after time and expecting a different result. I think we've reached that point. V's post above rehashes stuff he and I have discussed many times, and V won't listen and learn. I quit here and now dealing with him, not because I am wrong or don't have an answer, but because V is unteachable on this. Kirk shanahan (talk) 15:11, 8 June 2009 (UTC)

Kirk, do you enjoy describing yourself in such detrimental terms? You've been pushing CCS for how long, expecting different results than explanations of its faults? Okay.... V (talk) 13:37, 9 June 2009 (UTC)

## Vacation

Some of you will undoubtedly be glad to know that I will be taking some vacation in the next few weeks. I doubt I'll participate here much. Please don't take my silence as agreeing to anything. I have been very clear where I stand. All the attacks and crazy logic V, Abd, and PCarbonn have thrown at me haven't changed a thing. it takes real, solid logic to do that. Kirk shanahan (talk) 13:38, 4 June 2009 (UTC)

## Guide Posts for the Interested Reader

I'm an interested reader, but am finding all the above discussions so filled with diversions that the idea of CCS is getting obscured. I am also a scientist (qualified chemist, etc), have published my own research (not in cold fusion or related areas). So, all I am interested in is understanding Kirk's work as it relates to reports of excess heat. Leaving aside wiki-disputes and all related issues, I'd like to summarise what I understand from the above discussion about CCS. Kirk, I'd be interested in comments / corrections, and in advance, I admit to not having read your paper. I'll do that if my interpretation of the above turns out to be way off, I promise, and naturally all comments here are my interpretation and nothing more, albeit sometimes with my own training / experiences reflected.

- Calorimetry is a well-established, dependable, uncontroversial method of measuring heat changes in chemical systems. There are numerous possible calorimetric set-ups, each requiring appropriate measurements be taken and errors recognised for valid and accurate results to be obtained. Calibration of such systems is essential.
- Reports of excess heat in CF systems have been happening ever since the original P-F work. These reports suggest an anomaly not explained by conventional theory. However, the results are also not (yet) reproducible in the way that would make them uncontroversial. If observations of excess heat become reliably and independently reproducible then they will certainly pose a challenge in that the origin of the effect requires explanation.
- CCS offers one potential such explanation, although the quantity of excess heat reported would be an important factor. CCS seems to me to illustrate one way in which apparent 'signal' can actually be 'noise'. Recognising that larger differences between signal and noise reduce the reasonableness of dismissing signals as noise, evidence of sufficiently large excess heat would allow CCS to be discounted as the explanation.
- Since postulated CCS effects tend to interfere at the third and fourth significant figure, they need not be considered in conventional calorimetric experiments. However, these CF experiments are examining excess heat above expectation from existing understanding, and as such are a case where CCS

interference becomes substantial.

Crude (made up) illustration. Suppose we had a measurement,  $M$ , where calibration showed that at time  $t$ ,  $M = 931.6t + 117.5$ , so that the measured heat was  $M = 931.6 * 1 + 117.5 = 1049.1 = 1049$  J (4 sig. fig.) IF the calibration equation by the point where  $t = 1$  was actually  $M = 931.3t + 117.4$ , then the measured heat would actually be  $M = 931.3 * 1 + 117.4 = 1048.7 = 1049$  J (4 sig. fig). The result is the same (to 4 sig fig), and this is the typical situation for calorimetry.

However, the CF version of the experiment has the above measurement as the 'expected' and the 'excess heat' measurement being (say) 1049.5 J. The 'excess' is then  $1049.5 - 1049.1 = 0.4$  J for the first case but  $1049.5 - 1048.7 = 0.8$  J for the second. Because the numbers are so large compared to the difference, a variation of much less than 1% in the measurement in bulk calorimetry has become a 50% variation in the difference. If the excess heat measurement were 1149.5 J (for an  $\sim 100$  J difference), then CCS would be a fairly trivial error to ignore - it might make a 1% difference in the effect, but it would not call into question whether the effect was actually an artefact.

- Without reading Kirk's paper, my science and maths background immediately suggests to me that:
  1. errors in small differences between large numbers are notorious for substantial increases in size
  2. the question of what is artefact in such cases is resolved statistically, and such statistical techniques are well established and uncontroversial
  3. variations in calibration values at the relative scale I have described would be very difficult to measure (but not impossible) and irrelevant for most regular applications of calorimetry
  4. simple entropy suggests a plausible reason for small variations in calibration values - for example, in a gas-generating electrochemical system, slight differences in the energy of released gas molecules (ie. gas molecules within different bubbles having slight differences in average kinetic energy) as against energy going to the electrode or to bulk solution are entirely plausible. Conservation of energy requires the sum of the pathways along which energy is being released to be constant, but entropy almost guarantees minor variations over time. Consequently, if the calibration is measuring only the electrode then minor fluctuations in calibration are also entirely plausible.
  5. a gradual increase in reported frequency of the observation would be expected as measurement sensitivity increases

Does this seem a reasonable summary, Kirk? Please tell me if I am grossly off, or off in parts. Others, I would appreciate any comments be directed solely at CCS and the wiki-issues be set aside in this thread. Obviously, with it being Kirk's page he can over-rule my request, but I find it helpful to get the science clear in my head before turning to the wiki issues. Thanks. EdChem (talk) 23:35, 4 June 2009 (UTC)

EdChem. your arithmetic is correct of course, and this is the basic error the CFers are making. They fail to realize that their calibration has shifted. Note that even if you are just doing straight calorimetry, not a difference, if the cal shifts enough, you can get a mismeasurement. This is why you periodically check you calibration, i.e. recalibrate. The problem is that the CFers never considered the problem prior to my bringing it up, and have refused to do so after I did.

Next, make sure you understand why the less efficient a cell is, the bigger the potential 'excess heat' signal. Then understand that shifting heat between a lossy region and a high capture efficiency region might produce an excess heat signal. Then all you need is a chemical/physical means to do that, and I also supplied a candidate process for that too. If you really want to understand, make sure you read my first paper at least. It's not that long and you already understand half of it.

But the issues on the CF Talk page are not just this. I attempted to put some mainline thinking into the article and was Wikilawyered to death over that. All the claimed CF evidence has potential conventional

explanations which the CFers don't eliminate. My whole point is that they are incorrect is exclusively asserting the 'nuclear' explanation. But V and Abd with their crazy logic have blocked every attempt I make to do so. That's the real issue here. And one I can't seem to beat. Kirk shanahan (talk) 02:29, 8 June 2009 (UTC)

Kirk, thanks for your response. I'll add that paper to my list of reading. The potential for a greater anomaly with greater efficiency ~~inefficiency~~<sup>changed to inefficiency when error noted by Kirk below.</sup> EdChem (talk) 23:08, 8 June 2009 (UTC) seems obvious to me - simple random variation ensures greater differences will be observed when measurements are less precise. One thing I have been wondering, shouldn't there also be 'insufficient heat' measurements to match the 'excess heat' cases? After all, if the calibration is sometimes causing artificially inflated observations, shouldn't there also sometimes be artificially low observations?

As for the CF pages, they are a whole other issue. I wanted here to focus specifically on the science because I find it difficult to comment about article content without understanding the science being described. In other words, for me to form a sensible view on the appropriate coverage (if any) of CCS on the CF page I believe it is necessary for me to comprehend CCS and how it may offer an explanation for anomalous heat observations. EdChem (talk) 06:17, 8 June 2009 (UTC)

Other way round Ed. As the efficiency goes up, anomaly size goes down. This conforms to one of Langmuir's criteria of pathological science. Think of it this way. As the heat losses go down there is less need to bump up the signal (calibration constants approach ideal). Then any 'new' heat source coming from moving heat from a lossy region to a higher efficiency region (in my proposal) is likewise bumped up less by the now-invalid calibration. W.r.t. the unidirectional nature of the CCS - in theory there should be 'negative-going' excess heat signals, if it was a random process. But in practice, the calibration process is always done with 'dead' electrodes, which means that usually there is no heat redistribution. Then the electrode becomes active, that brings on the redistribution to what I call 'at-the-electrode'. That makes the observations unidirectional. Your problem is typical in that you are trying to think of random changes in cal constants, but the changes are not random. You can see the pattern in the data table in my first pub, but I also made a graph of it and discussed it at some length in my 3rd pub on the subject. W.r.t. the impact of the CCS, it's straightforward. Since no one reports information that can be used to evaluate it's presence or absence, all claims of extraordinary results have to be put on hold until the issue is resolved. In principle, such information currently exists in CFers lab notebooks. But, it's been 7 years now (actually 9) and no CFer has even accepted the ideas I present in my pubs. Unlikely we will ever get the data to resolve the issue. Kirk shanahan (talk) 13:43, 8 June 2009 (UTC)

*Other way round Ed. As the efficiency goes up, anomaly size goes down. Of course - this was a typo on my part, I meant *The potential for a greater anomaly with greater inefficiency seems obvious to me.* I have edited to correct my typo. I'll need to give some thought to unidirectionality, thanks for the explanation.* EdChem (talk) 23:08, 8 June 2009 (UTC)

In the interest of efficiency, Shanahan's theory is not described fully by "CCS." His theory, if I have it right, is that there is an effect that causes unexpected recombination at some particular spot on an electrode, that this effect is the non-nuclear equivalent of Storms' NAE (nuclear active environment), and that this effect then causes calibration, based on known heating at some other location, to be off. This effect would then appear only with some

electrodes and not with others, depending on heat distribution in the cell and whether the effect arises or not. It would show an analogous or similar difficulty of reproduction as the original "cold fusion" phenomenon. My problem: sure. Maybe. However, that would not explain correlation with other effects, such as helium (multiple studies showing on the order of 25 MeV/He4), radiation (apparent alpha tracks, copious, multiple reports now, secondary sources covering it) (low-level neutrons, much secondary sources on the Mosier-Boss report), X-rays, the sheer magnitude of the P-F effect in some cases, steady heat from non-electrolytic gas loading of nanoparticle palladium, local melting of electrodes immersed in heavy water (well, maybe that one -- I suppose the recombination, we could speculate, could get pretty hot), an astonishing array of reports of elemental transformations, etc. --Abd (talk) 11:34, 8 June 2009 (UTC)

As to coverage of the Shanahan theory, we need to consider availability of sources. Primary reliable source exists, the Shanahan papers. These are, by the way, theoretical review papers, analyzing, to some extent, existing reports, they are not reports of experiment, so they are, technically, secondary source, reporting speculative and unconfirmed analysis of prior work. The publication establishes notability for his theory, barely. That is increased by the fact that cold fusion researchers have responded to his criticism, there was mention of it in the 2004 DoE review. There is, to my knowledge, no "mainstream" response to Shanahan, his work has been largely ignored, perhaps due to the unfortunate policy of some major journals to refuse to even submit any paper on cold fusion (positive or negative) for peer review. However, other journals will publish, such as *Thermochimica Acta*, where Shanahan's work has been published. My view is that his work is sufficiently notable to be reported, with balance, which would mean that the responses -- in the same journal -- would be covered as well. As matters stand, his work stands out as almost unique as to recent criticism of cold fusion work in peer reviewed journals, there are few other examples. The calorimetry has been widely accepted as legitimate, and we have plenty of reliable secondary source on that. It's only rejected, as it is, by sources and authorities which are depending on old analysis, not by a full review of the field, mostly on the basis that it *must* be artifact, because theory, it's claimed, says it's impossible. The criticisms I've read simply don't match the peer-reviewed publication record, claims that, for example, as accuracy increases the effect disappears. That's based on early replication failures where some did report excess heat, then found none as accuracy increased, probably because they weren't setting up the effect, and there has been a recent study, ICCF14, 2008, a Bayesian analysis of excess heat replications, that was able to accurately predict excess heat findings based on characteristics of each study as shown in the papers. Note, however, that this doesn't fully address Shanahan's objections, because CCS may depend on the same conditions, such as details of the palladium microstructure or loading ratio (which are connected, microcracks in the palladium prevent high loading from being achieved).

I'm skeptical that the CCS effect could apply to all forms of calorimetry that have been used to demonstrate excess heat. However, the proof of the pudding is in correlation with other phenomena that are both expected to occur with fusion (or other nuclear process that we might not call fusion): the reaction products, such as helium, tritium, and various forms of radiation, as well as transmutation products caused, perhaps, by secondary reactions. The Be-8 hypothesis, for example, expects energetic alpha particles, which can cause secondary fusion, they have more than enough energy to overcome the Coulomb barrier. Signs of this are reported, this is the explanation advanced by Mosier-Boss in the recent well-noticed report on neutrons at very low levels, but quite adequately above background, and correlated with excess heat, I believe. Helium has been reported, and leakage from ambient, the normal reason given to dismiss this, isn't adequate to explain the results, leakage would show asymptotic approach to ambient, but the time-analysis shows levels that pass ambient without slowing down. The point is that Shanahan's theory is reasonable enough to warrant, outside,

more detailed attention, but here it's a detail, not supported by ample source, just enough to warrant mention here, and possibly more mention in a detailed article such as existed in what is now User:Abd/Calorimetry in cold fusion experiments, mostly written by Shanahan. --Abd (talk) 13:36, 8 June 2009 (UTC)

[unindent]A point by point response. Sorry for the length EdChem, but Abd does that. He rolls 5000 errors into one paragraph, and it takes 5000 paragraphs to explain each one. And he doesn't seem to learn when it goes against what he 'believes'.

“In the interest of efficiency, Shanahan's theory is not described fully by "CCS." “ - If you want to lump all three parts of my proposal for a conventional explanation of apparent excess heat, feel free. Just don't do what the Cfers tried to do: Criticize the most speculative part of the 3 (incorrectly at that), then claim the whole thing was 'proven' wrong. I learned from this that I had to be more specific, so when I use 'CCS' I simply mean the demonstrated mathematical fact that calibration constant shifts can and appear to induce apparent excess heat peaks. Everything past that is somewhat speculative and not restrictive of other ways to get a CCS.

“His theory, [snipped for brevity] phenomenon. “ - So why couldn't you do this when I asked you too? Your grade is now up to 50%. You still need to understand the impact of this and some of the subtleties of it. Hint: I give some of it away in my latest response to EdChem.

“My problem: sure. Maybe. However, that would not explain correlation with other effects, such as helium (multiple studies showing on the order of 25 MeV/He4), - Already answered, but since EdChem is watching...first off – not enough replication. Second, if there is no excess heat, there is no real correlation. Third, the error bars on the data in the correlation plots need to be doubled or tripled at a minimum. That means the next time the experiment is replicated, a completely different result could easily be obtained based on statistics. Third, chemical processes are highly autocorrelated, so that needs to be taken into account in analyzing the data, in addition to the fact that linear regression analysis is based in error-free X's. The fact that the errors in X are large requires less confidence in any derived correlation coefficient.

“radiation (apparent alpha tracks, copious, multiple reports now, secondary sources covering it) (low-level neutrons, much secondary sources on the Mosier-Boss report), “ - Pits in CR39 are known to arise for other reasons than radiation. One of those sources is physical damage. Explosions produce shockwaves. Shockwaves produce physical damage. Every exploding bubble (or 'nanoscopic nuclear explosion') could in theory produce a pit. Now, go look at the physical distributions of the SPAWAR pits. A second source might be oxidative attack by pure O2 in a bubble. That would also explain the pits in CR39 plates placed above the electrolyte in earlier experiments (as I recall, by Oriani?) - Easy to come up with untested conventional explanations.

“X=rays,” - I don't believe there is any such evidence that also doesn't have a conventional explanation. Explain this more and I could comment more.

“the sheer magnitude of the P-F effect in some cases,” - such as?

“steady heat from non-electrolytic gas loading of nanoparticle palladium,” - the Arata experiment I suppose. Again, lots of conventional possibilities, but I haven't thought it through enough to settle on one, primarily because of lack of adequate replication.

“local melting of electrodes immersed in heavy water (well, maybe that one -- I suppose the recombination, we could speculate, could get pretty hot),” - Yes it can, in my earlier explanations of the Patterson Power Cell, posted to spf in c.1995-6, I cited a study by a chem. eng. who blew a hydrocarbon/oxygen mix over a Pt wire at high speed (to limit the amount of oxidation, he was trying to make oxygenated hydrocarbons, not CO2). The Pt got white hot, i.e. >900C. H2 would do the same thing. However, the 'melting' Abd is talking about is not really

melting if I understand what experiment he is referring to. I believe he refers to the ET experiment where ultrasonics are shot into the cell. This is a variant of the Russ George expts of the same nature. George claimed that the sonication induced bubbles over the Pd foil surface. These bubble catastrophically collapse making a 'cavitation jet' of material that is injected at high speed into the Pd. That chews it up, simulating 'melting'. As I've noted before Pd is ductile and flows easily under these kinds of stresses (jets, explosions).

“an astonishing array of reports of elemental transformations, etc. --Abd (talk) 11:34, 8 June 2009 (UTC) “ - contamination concentration, not 'transmutation'.

“As to coverage of the Shanahan theory, we need to consider availability of sources. Primary reliable source exists, the Shanahan papers. These are, by the way, theoretical review papers, analyzing, to some extent, existing reports, they are not reports of experiment, so they are, technically, secondary source, reporting speculative and unconfirmed analysis of prior work. “ - No, the papers are a presentation and defense of a theoretical explanation of an observed but unexplained phenomenon. As such, they are primary.

“The publication establishes notability for his theory, barely. That is increased by the fact that cold fusion researchers have responded to his criticism, there was mention of it in the 2004 DoE review.” - forgot my response to this - I can't find a reference to my work in the review. When the individual reviewer comments were 'leaked', there was one reviewer who suggested my work needed to be considered. The Cfers however, always ignore my work or claim it was successfully dealt with, which is not true by any stretch of the imagination. Kirk shanahan (talk) 15:00, 8 June 2009 (UTC)

“There is, to my knowledge, no "mainstream" response to Shanahan, his work has been largely ignored, perhaps due to the unfortunate policy of some major journals to refuse to even submit any paper on cold fusion (positive or negative) for peer review.” - And what would you expect? A mainstream that suddenly jumped up a published a bunch of attaboys for my work, this after they all moved on in c. 1995? Get real Abd, your bias is making you expect things that are completely irrational. The bringing up of journal policies is also irrelevant to this issue.

“However, other journals will publish, such as Thermochemica Acta, where Shanahan's work has been published. My view is that his work is sufficiently notable to be reported, with balance, which would mean that the responses -- in the same journal -- would be covered as well. “ - I attempted to do this and was struck down by PCarbonn. If you do it make sure that you note that a) all substantive comments were against the proposed and speculative mechanism, not the CCS, or the heat redistribution explanation, b) I rebutted all such points point-by-point, c) Storms chose to ignore that rebuttal in his book. It goes to reliability issues. It shows that Storms uses a 'baby with bath water' approach in rebutting my arguments, which is not logical, and that he is highly biased when presenting the 'facts' of cold fusion.

“As matters stand, his work stands out as almost unique as to recent criticism of cold fusion work in peer reviewed journals, there are few other examples.” - Except for the Clarke work, and the reports in self-published papers and ICCF (and such) proceedings that are clear examples of problems with the claims of the field.

“The calorimetry has been widely accepted as legitimate, and we have plenty of reliable secondary source on that.” - No you don't.

“It's only rejected, as it is, by sources and authorities which are depending on old analysis, not by a full review of the field, mostly on the basis that it must be artifact, because theory, it's claimed, says it's impossible.” - Understand this Abd – My pubs are primary peer-reviewed source that disagrees with what you wrote. Get up to speed. Your biased interpretations of the state of the field do NOT lead to a good article.

“The criticisms I've read simply don't match the peer-reviewed publication record, claims that, for example, as accuracy increases the effect disappears.” - Why do you continue to expect to find main stream antiCF publications past the '93-95 timeframe? Can't you understand what being declared 'pariah' does?

“That's based on early replication failures where some did report excess heat, then found none as accuracy increased, probably because they weren't setting up the effect, and there has been a recent study, ICCF14, 2008, a Bayesian analysis of excess heat replications, that was able to accurately predict excess heat findings based on characteristics of each study as shown in the papers. ” - Not RS, as it is a publication from a conference of fanatics, 'reviewed' by fanatics who stopped critically thinking a long time ago. Critical thinking is the basis of accurate and reliable peer review. If it gets published in an actual peer reviewed journal I might take official note.

“Note, however, that this doesn't fully address Shanahan's objections, because CCS may depend on the same conditions, such as details of the palladium microstructure or loading ratio (which are connected, microcracks in the palladium prevent high loading from being achieved).” - Two more points. You're up to 52% now. Further explain the details of the above for more points. For ex. How does the microstructure (note: not 'nanostructure') impact the conventional explanation? What part would the loading level play in this? Etc.

“I'm skeptical that the CCS effect could apply to all forms of calorimetry that have been used to demonstrate excess heat. “ - That's because you haven't thought it through.

“However, the proof of the pudding is in correlation with other phenomena that are both expected to occur with fusion (or other nuclear process that we might not call fusion): the reaction products, such as helium, tritium, and various forms of radiation, as well as transmutation products caused, perhaps, by secondary reactions.” - All of which have conventional explanations, when there is enough replication to get an idea of what is happening. For ex, even Storms didn't bring up tritium detection results, and he said it was because there weren't enough.

“The Be-8 hypothesis, for example, expects energetic alpha particles, which can cause secondary fusion, they have more than enough energy to overcome the Coulomb barrier. Signs of this are reported, this is the explanation advanced by Mosier-Boss in the recent well-noticed report on neutrons at very low levels, but quite adequately above background, and correlated with excess heat, I believe.” What you believe aside, this 'theory' is an excellent example of what happens when a scientist deliberately ignores a reasonable and rational explanation he/she doesn't like. Things just go wild.

“Helium has been reported, and leakage from ambient, the normal reason given to dismiss this, isn't adequate to explain the results, leakage would show asymptotic approach to ambient, but the time-analysis shows levels that pass ambient without slowing down.” - garbage, air inleakage is the most probable explanation.

“The point is that Shanahan's theory is reasonable enough to warrant, outside, more detailed attention, but here it's a detail, not supported by ample source, just enough to warrant mention here, and possibly more mention in a detailed article such as existed in what is now User:Abd/Calorimetry in cold fusion experiments, mostly written by Shanahan. --Abd (talk) 13:36, 8 June 2009 (UTC) “ - Your analysis does not include the CCS' impact on reported excess heat. For more points, explain how it impacts that. 74.230.182.89 (talk) 14:50, 8 June 2009 (UTC) Kirk shanahan (talk) 15:00, 8 June 2009 (UTC)

Kirk, no, I'm not looking for points. I was trying to facilitate your discussion with EdChem, so more of the foundations might come out more quickly. Try to tone down the common hostility and contempt. My comments were, I think EdChem will recognize, friendly. I suspect you don't understand the meaning of "primary" and "secondary" source. Primary sources are largely unusable, were you aware of that? By calling them secondary sources, I was *\*raising\** their usability, as being comments on the calorimetry work of others. However, as to your own theories, they are, indeed, primary sources. Some of what you

write is preposterous, in the larger context, or even from some simple facts I alleged. For example, SPAWAR CR-39 pits. Copious pits on the side facing the electrode. Fine, chemical damage from oxygen, perhaps, except the oxygen is evolved at the other electrode, and there is no massive damage on the back side. Okay, damage from miniexplosions at the surface, from some unexpected recombination or other effect. When they accidentally left the protective plastic on the CR-39, they still got pits in the pattern of the electrode, but at reduced levels. When they put the CR-39 outside the cell, with the electrode next to a thin plastic or borosilicate window, the CR-39 was still pitted, at reduced levels as would be expected from alpha losses in the plastic. The question of chemical damage is considered in the paper presented in the ACS Sourcebook (2008), "Detection of Energetic Particles and Neutrons Emitted During Co-deposition," Mosier-Boss et al. This is largely a review of their earlier work, published, as I think you know, in peer-reviewed journals. In the Sourcebook, they argue that damage from energetic charged particles creates conical pits, which can be recognized as such by how the image of the pit changes as focus is changed. General chemical damages looks not at all like radiation damage, and it's hard to understand how local heating, a miniexplosion, could create a conical pit, that's not the damage pattern that would form. They've considered dendrites and damage from high electrical field, possibly; again, the intervening window would be an obstacle, and, again, there are the triple tracks. Various controls have eliminated other explanations. Triple tracks, quite simply, couldn't be the result of chemical damage. They aren't visible in the heavily pitted areas, they are masked by all that damage, but they are found on the other side of the detector, and in areas where the alpha pitting doesn't show because the distance to possible NAE is too far, the alphas are absorbed. Additional apparent neutron tracks or other knock-ons are found by deeper etching of the plastic, so the neutron tracks aren't just surface phenomena, as would be chemical damage. What's unique about your complete CCS theory is that the conditions are hypothesized to be like those of the alleged NAE: unusual, not found with controls, because there is an unusual environment required. So you bypass the arguments like those Objectivist (V) has been making. But there are all the other phenomena to explain, then, and you are mostly standing on unverified speculations, but we'd have to look at each experimental finding in detail. The triple-track findings were important because neutrons are characteristic of fusion reactions, and there should be *\*no\** fusion reactions taking place in the cell. The triple-tracks, sorry, can't be the result of chemical damage. These results explain and confirm *\*all\** the early work on neutrons: both the negative findings -- what they confirmed was a flux below the detection levels of much early work -- and *some* of the positive ones -- which showed very low levels, sometimes in bursts. What was always a problem with that early work was that levels were so close to background. But by putting the detector right in the cell, the SPAWAR group captured more of the neutron tracks, accumulated for weeks, without increasing background. (A CR-39 detector accumulates damage gradually over time, because of cosmic rays and other environmental radiation. So they always had control detectors, I think, or areas of the detector away from the electrode also serve as a kind of control.) I think they sometimes expose a corner of the detector to a radioisotope to create calibration tracks. *Don't try to answer this all at once*. Just pick one part, and you and Ed can discuss it, if you really are interested in getting to the bottom of this. (EdChem, discussion of something here doesn't mean that I think the time is ripe for inclusion in the article, though there is secondary source review of much of this. This is, as it would be on Talk cold fusion, background.) --Abd (talk) 22:58, 8 June 2009 (UTC)

(noindent) Every post like this moves Abd closer to an indefinite block. He seems to have little or no expertise in the topics he is attempting to discuss, yet he persists. Mathsci (talk) 23:36, 8 June 2009 (UTC)

Eh? Wikipedia editors are supposed to have expertise in a topic? Mathsci, you are confidently predicting, for me, a fate which might befall you, more likely. Be careful. Now, Kirk, if my posting here is offensive to you, you should know that you have no obligation to permit this on your Talk page, you can ask me to stop and I will, period. I'm discussing here with your permission. Mathsci has no concept of the issues being discussed, as far as I've been able to see, and is purely following a wikipolitical agenda, making disruptive comments about editors instead of discussing issues, it's not about finding consensus among

editors, which sometimes takes a lot of discussion. EdChem appears to be both knowledgeable and serious about learning what your theory is about. I suggest answering him carefully, it appears he's likely to ask cogent questions. I've raised reasons to respect your theory, and reasons to consider it ultimately as moot, but I'm not the judge, the community consensus is the judge, and I trust that process. It rarely fails me when it's given enough time. --Abd (talk) 04:40, 9 June 2009 (UTC)

Making unjustified and impertinent assertions does not make anything you say correct, Abd. There are deep problems with the way in which you think you can "read" other people. You pose as having some kind of academic authority, but I can't quite see why. In my case I am an established academic. I don't find your *ad hominem* remarks offensive. That is because they are written by you. I do not find them particularly intelligent. In fact, in scientific subjects, your comments seem to verge on trolling. You seem to relish winding up other editors with your non-sequiturs. Mathsci (talk) 23:31, 9 June 2009 (UTC)

I've stated many times I have *no* academic authority, so I have no clue as to what "pose" means. Mathsci claims to be an established academic. I have no particular reason to doubt that, though surely the field would be relevant. "Pure mathematician" is the user page claim. So, exactly what qualification does this confer as to expertise with cold fusion issues? I haven't seen in *any* of his posts a sign that he understands the issues involved; if he does, he's hidden it well, the posts are almost entirely ad-hominem. I find it ironic that Mathsci denies that I can "read" other people, but in the same sentence, is claiming that he knows what I think. I'm not found to be "particularly intelligent"? Gee, is that an insult? Given test scores back when, my reputation in many fields, indeed, given my reputation here, in general, except among certain editors who'd rather I didn't exist, the claim simply exposes the claimant for what he is, and I don't have to say it. My intelligence, such as it is, confers on me no privileges here. --Abd (talk) 03:02, 10 June 2009 (UTC)

Your namespace editing record speaks for itself. The problem is with your wikilawyering about sources; you also often try to evaluate scientists or would-be scientists by wikilawyering. FYI many mathematicians are well versed in quantum field theory, just as many self-respecting string theorists use the notion of derived category. I've personally also had experience with fraud in chemistry. The use and evaluation of scientific sources does not vary from subject to subject. Mathsci (talk) 07:27, 10 June 2009 (UTC)

Thanks for the support Mathsci. Abd is quite frustrating. The other problem he has is that his criticisms are usually half-baked. I have tried to get him to think things through before running off at the keyboard. A prime example is his discussion of pits in CR-39. He misses the simple fact that they haven't done any 'controls' to test any of these supposed reasons why the two different mechanisms I proposed in 2002 'couldn't be true', they just handwave it away, if anything. Kirk shanahan (talk) 12:47, 10 June 2009 (UTC)

Honestly, Kirk, you make me wonder if you've read the SPAWAR work. Lots of controls. And you are singularly vague. The SPAWAR work has been published for many, many years, regularly, in peer-reviewed journals. If you are going to cite mechanisms that you "proposed" in 2002, you could at least state what they are, and show why the controls used by the researchers were, in your view, inadequate. But you don't even mention the controls. This is next to useless. Mathsci, you don't know what's in front of your nose, you mistake your projections for reality. If your judgment as to what is happening on Wikipedia is corrupt, why should I trust your judgment on science? What does "fraud in chemistry" have to do with this? There are no significant standing allegations of fraud with respect to the vast bulk of the research involved. Wikilawyering about sources? What wikilawyering? I'm arguing that we should apply RS,

straight on, and it doesn't get complicated unless there is conflict of sources. Enough. Kirk, you were carrying on a discussion with EdChem which was interesting. Mathsci is just here to disrupt and distract. Be careful who you align yourself with. Carry on. --Abd (talk) 18:24, 10 June 2009 (UTC)

I assume Abd's quite unreasonable hostility to me is due to the fact that I was partially responsible for precipitating his unpalatable topic ban. I hope that Abd will now be able to move on, stop making personal attacks, conduct discussions in a more rational way and hone his editing skills on uncontroversial wikipedia articles, in particular learning how to use secondary sources. I wish him all the best of luck. Mathsci (talk) 21:52, 10 June 2009 (UTC)

Wow! Mathsci, you had something to do with my alleged ban? (I'm not banned, per WP:BAN, WMC *tried* to ban me, but that's moot.) I had thought the ban was a unilateral, isolated, quirky decision by WMC, but if you were involved, that changes the complexion of the whole thing. WTF are you doing here, Mathsci? --Abd (talk) 00:11, 11 June 2009 (UTC)

I don't know what you're talking about, because you commented at length when I made the suggestion on a previous thread on WP:ANI. Please see the current discussion there. Writing "what the fuck are you doing here" seems highly uncivil and not helpful to discussions. You have been evading discussions about (a) your misuse of scientific sources and (b) your personal evaluations of science and scientists. That seems to be the crux of the matter. Mathsci (talk) 07:41, 11 June 2009 (UTC)

## Cold fusion mediation

I have been asked to mediate the content dispute regarding Cold fusion. I have set up a separate page for this mediation **here**. You have been identified as one of the involved parties. Please read through the material I have presented there. Thank you. --**Cryptic C62** • **Talk** 19:23, 5 June 2009 (UTC)

## Comments on Kitamura, et al, Phys. Rev. A 2009 paper

Following is an explanation of the comment I made on the Cold Fusion talk page regarding the paper Kitamura, et al, Phys. Rev. A, 373 (2009) 3109-3112. I banged this out rather quickly, so I reserve the right to edit it if someone notes a big problem (or even a little one).

DO NOT PUT COMMENTS IN THIS. I WILL DELETE THEM. GO TO THE NEXT SECTION (Comments on...) AND POST THERE IF YOU WANT> I MAY RESPOND IF I THINK IT'S WORTH IT.

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The paper presents 4 data figures (Figs 2 and Figs 3a, b, and c) and 1 data table. One figure shows a baseline signal from a calorimeter when no powder was present. However, all experiments are conducted with powder present, so as usual, the CFers are making the assumption that results obtained under condition A are directly translatable to Condition B. This is always a dangerous assumption, and in this case seems clearly incorrect. The other three figures show results for a 0.1 micron Pd powder, Pd black, and a mixed PdO/ZrO2 powder. All three

figures show peaks divided into two regions. The first is where ‘hydriding’ is occurring and the pressure does not rise. The second ‘phase’ (to use Kitamura, et al’s term) is where pressure has begun to rise. The first phase represents about 15% of the total X axis span of ~1800 minutes. During the second phase in all 3 figures, there are broad and weak ‘peaks’ observable in the results for D2. These are somewhat obscured in 3a and 3b because the H2 run overlaps the D2 run data trace. However, Kitamura, et al, say that the results of Fig 3a and 3b are in the noise. The Y scale is quite compressed so it is difficult to estimate the magnitude of the peaks from the paper, but the same data found in 3b and 3c is presented in another location which facilitates the analysis (the peaks in 3b are larger than those in 3a). The maximum intensity of the ‘peak’ in 3b is about 0.1 Watts. Note that the noise span shown in Fig 2 gives a visual estimate of about +/- 0.075W (which also is a typical number seen in CF reports). Now on the Figure 3c, which is the only one claimed to show ‘excess energy’. In this Figure we have a different situation in that the two curves (H2/D2) now do not overlap. This immediately makes the ‘bump’ in the D2 run more obvious. However it is also clear that something is wrong with the H2 data, as the noise characteristics are significantly different for most of the trace, and the zero is offset to negative watts (which is why the curves no longer overlap). The D2 data does not have a good baseline region, and seems to show an abrupt baseline shift at about 1400 minutes, where it shifts down to overlap with the H2 data (still running negative). There is however a region from about 200-500 minutes (full scale is ~1800 min.) that appears flat. However it is positively displaced from the nominal zero by about the same amount that the H2 data is negatively displaced (approx .060W). This visually gives the impression automatically of an additional 0.12 W in a peak just to start. The D2 peak lies about 0.1W above the presumed offset baseline, at ~0.15W absolute. By the way, Kitamura, et al admit in the text that there is a thermocouple malfunction in the H2 data of this Figure which causes the offset. So, we have 0.1W peak in 3b that is noise and an apparent 0.15W peak in 3c that is not, except that if the D2 data has an offset like the acknowledged offset in the H2 data, the peak is only 0.1W. Folks, this is ‘seeing what you want to see in the data’. The Figure 2 presentation of the baseline noise is not applicable to the experimental conditions. We have admittedly two runs where no excess heat is observed that show significantly larger baseline variation, and we have a claimed excess heat example that is not readily distinguishable from those other two null runs. Yet there is the claim of excess heat. Truly amazing that this made it through peer review.

Additionally there are another couple of problems which may not be evident to any by a hydride chemist like myself. The data table lists 14 runs with 3 types of materials, and it uses a code to explain what gas/sample/cycle combination is being reported. The last digit of the code is the cycle, i.e. the number of times the given sample has been exposed to hydrogen and then evacuated and baked. 11 of these runs are first cycles. Only one is a third cycle, leaving two second cycle reports. The problem is that it is a well known fact in hydride chemistry that any hydride material needs to be cycled at least a few times (few=>3 or more) to ‘activate’ the sample. The term ‘activate’ is somewhat amorphous when applied across the board to all hydride materials, but for Pd, it basically means the surface needs to be cleaned off before reproducible results can be obtained. So, 13 of the 14 runs are clearly in the region where reproducibility is a known issue. This means the data presented are likely meaningless anyway, since the expectation is a high degree of variability anyway. Second, Kitamura, et al list the supposed loading extent for the Pd in their samples, and these disagree positively and negatively with what would be obtainable for an activated Pd material at the pressure and temperature (T is actually not specified!) obtained. This data is available in the open literature, and Kitamura, et al even list the primary place where you would find it as reference #6. Yet they list the obtained loadings (given in H/M units, which is the ratio of how much H (or D) is in how much M) as ~.44 for the 0.1 micron Pd, .79 and .88 for the Pd black (except for 2 and 3 cycles), and around 1.0 for the PdO/ZrO2 sample. The anticipated loading level for Pd at the pressures obtained folding in some span to cover temperature variation is about .65-.75. So they claim to overload the PdO/ZrO2 and Pd black, and underload the 0.1 micron stuff. Underloading is expected if the surface of the material is junked up with contaminants, i.e. it hasn’t been activated properly. The overloading on the Pd black is only for the first cycle. The second and third cycle are listed as loading to 0.23 and 0.24 respectively. Again, it looks like the surface is still dirty. That suggest most of the supposed loading in the first cycle was due to H2 reactions with something else (like absorbed O). In the PdO/ZrO2 case, this is clearly the problem and Kitamura,

et al acknowledge this, but they assume a loading level of 1.0, which again is nothing more than wishful thinking. They will not get much more than 0.75 under their conditions of reaction on an activated sample. Most of their observed first phase heat is reaction with O. (Which is why I put 'hydriding' in quotes above when talking about the first phase results.)

I should also note that Kitamura et al are using two experimental configurations. The second consisted of a 'matched' set of two calorimeters that were run simultaneously, one with D2 and one with H2. They apparently think this is somehow useful and significant. In fact, it just means they ran two experiments at the same time instead of separately. But, what it appears to show is that all H2 runs were done in one calorimeter while all D2 were done in the second. Thus, since cross experiments were not done, I can suggest that there is a difference in the calorimeters which makes the H2 runs less noisy than the D2 runs. More experiments must be done and reported on to remove this concern. (Also remember that the thermal conductivity of D2 and h2 are quite different.)

So what we have here is a typical CF paper. Kitamura, et al, are confirmed CFers, and thus they know that to get CF in Pd you have to get  $\sim 1.0$  H/M, so they make sure they get that in their data analysis, never mind what the rest of the scientific world would say. And they know that the 'great' Arata and Zhang got CF (which is what they are trying to replicate), so they know they must see excess heat with at least some of their samples, so they present data that is ambiguous (and messed up at that) to 'prove' A&Z were correct. Real n-ray stuff here. Again, how this got published I can't see. (Well actually I have a theory. You may recall I like to say there is no RS available from the CF world, right? I suspect what happened is they submitted their paper to PRA and the PRA editor couldn't find anybody who would review the paper (remember the pariah status) except those suggested by Kitamura, et al. Of course those suggestions were fellow CFers, who stopped doing critical review years ago. So, they sent in 2 or 3 "Sure! This is great! Publish as is!" reviews and the PRA editor went with it.)  
Kirk shanahan (talk) 13:12, 4 September 2009 (UTC)

## Comments on the preceding section - re: Kitamura Phys Rev A 2009

Put your comments here if you want... Kirk shanahan (talk) 13:10, 4 September 2009 (UTC)

Warning to others: Shanahan is an expert, but a highly biased one. He's been arguing strongly on the internet against cold fusion since the early 1990s. His only published work, however, is on his theory that calibration constant shift explains the excess heat finding, which is probably his wishful thinking, there are so many independent confirmations of excess heat, using various forms of calorimetry, that his theory is unlikely to be correct. It still involves, by the way, an unexplained anomaly, just a different one from the nuclear hypothesis, one which allegedly causes unexpected local heating that throws off the calorimetry.--Abd (talk) 17:33, 4 September 2009 (UTC)

Warning, Abd (who failed to sign his post) is a potentially banned commentator at the cold fusion talk page. He is banned because he writes pages and pages of wrong stuff, and then refuses to listen to those who take the time to try to correct him. I defy Abd to show where my 'bias' has resulted in a significant error in what I have written. It's very easy to cry wolf, but when you do it when there isn't one, you should be ignored (or banned as disruptive), just like in the fable. I have been on the Internet regarding cold fusion since the mid-90's and I did not start out arguing 'strongly' against it, I was simply asking pointed questions. The answers I got were never satisfactory, and as I studied the field more and more, I came to see all the flaws and fallacies in the claims. Then I started to get a little more insistent, because I believe in doing 'good' science. Then I wrote my paper on the calorimetric error that probably leads to rejecting all reported excess heat claims (at least in P&F cells, other things may be going on in other configurations as is the case with Kitamura, et al). Since no CF researcher has published any data that directly allows one to evaluate my claim, yes, the idea

that it 'probably' explains the excess heat signals is still speculative until the CFers prove otherwise on way or the other. They (deliberately?) don't publish the data that could do so. I don't do CF experiments myself, I work/worked in related fields. Yes, there are 'many independent confirmations' of excess heat SIGNALS, but whether they are caused by real heat or not is still unproven. I readily admit something is going on, and I have proposed what I think is a good candidate for that something, no nuclear involved. Abd and other CFers can't separate the effect and data from the interpretations, they insist on 'nuclear' being in the description. I don't and they can't comprehend that. I don't have other publications on the other problems I see in the field because I don't believe they would be accepted for publication because they are not novel enough. Maybe I'm wrong on that, but I'd rather spend my time on good research and good publications. The kind of problems that permeate CF research are common problems carried to extremes by pseudoscientists and are easily noted by trained scientists who actually take the time to read and study the papers. Kirk shanahan (talk) 16:33, 4 September 2009 (UTC)

It is typical for Shanahan that he claims the peer reviewers were biased or ignorant. I think that Shanahan is important, he knows much that is of value, and his skeptical view should always be considered, but should not be swallowed whole, without careful examination and comparison with the full literature.--Abd (talk) 17:33, 4 September 2009 (UTC)

Only in recent years, when the abandonment of even considering CF claims by the mainline scientist has resulted in a total lack of understanding by those people about what CF is and the current claims. In the Kitamura case, I outline the problems I detected with little effort and point out why the paper should never have made it through the peer review process. To be clear, the other reason it might is just poor peer review, i.e. the editors only did a cursory job on the paper. I also agree, and insist, that you take the time to study my views and comments, unlike Abd who fires off a response to my comment without apparently understanding it (see below) and who loads his comment with ad hominem attacks on me and my supposed 'bias'. 'Shooting the messenger' isn't a valid scientific process. Kirk shanahan (talk) 16:33, 4 September 2009 (UTC)

What he has done above is to neglect the context. This is a confirmation of Arata; one of the problems with Arata is that absolute calorimetry isn't done, Arata does not report net energy generation, he only reports temperature of the cell, and pressure. I've reviewed the original Arata reports fairly carefully, and what they show is heat of hydride formation, roughly similar between hydrogen and deuterium loading (with some extra "bump" with deuterium? -- I'd have to look again), but the crucial finding is what happens after this. A cell with hydrogen settles to ambient within a few hours, a cell with deuterium settles to 4 degrees C. above ambient and stays there, steady for as long as has been reported, 3000 hours.--Abd (talk) 17:33, 4 September 2009 (UTC)

The Arata data is not very convincing to me. There are a lot of problems that are not dealt with adequately in the limited experiments reported. The only similarity between what Kitamura did and Arata was they both used a type of calorimetry on similar materials. The Kitamura data is a joke, it proves nothing, as I described above, and they make novice errors in dealing with palladium chemistry. FYI, the data presented only covered about 1800 MINUTES, that's 30 hours, not 3000 like you claim. So, there is no confirmation of anything arising from the Kitamura data. You operate on rumor and innuendo Abd, I don't. Kirk shanahan (talk) 16:33, 4 September 2009 (UTC)

I don't have a copy of the Kitamura paper, unfortunately, so I can't see exactly what Kitamura confirmed and what Kitamura extended by providing calorimetry. What I do know is that the striking finding from Arata is the continued, continuous heat generation with no apparent input, beyond the hydride formation phase. Shanahan might have the information on which he could judge the power level that 4 degrees C continuous represents, by studying a known release (heat of hydride formation) and thus determining the time behavior of temperature with known heat generation, my guess is that it is possible to roughly

estimate the thermal resistance of the experimental cell from this. And from that, what continuous energy release will produce a 4 degree C temperature differential, and from that, the total energy generation in 3000 hours at this differential. (The design of the cells and where the temperature is measured makes the local heat generation problem a moot issue. The only thing in those cells is some nanoparticle palladium or palladium alloy and the gas.--Abd (talk) 17:33, 4 September 2009 (UTC)

Ohh! So you haven't even read it yet! You don't even have a copy of it! But you can tell me why I can't possibly know what I am saying?? Folks, this is clear proof that Abd is a POV-pusher. He shoots from the hip (with a machine gun) at anyone who dares to contest cold fusion claims. Unfortunately Abd, you missed completely. Also, as usual, he tries to shift the focus away from what is being discussed, Kitamura et al, onto the singular report by Arata that I am not commenting on here. For the record, there are good reasons to expect some problems in the Arata set-up that would cause a thermocouple/thermistor/whatever shift and/or malfunction. Kirk shanahan (talk) 16:33, 4 September 2009 (UTC)

My sense is that 3000 hours of heat at the level reported could not be attributed to a low-level chemical reaction being maintained from 7 grams of palladium/alloy and deuterium gas. Shanahan, you should be qualified to attempt this calculation. Care to try it? One further problem: the heat appears flat even at 3000 hours, so we can infer that it would last much longer. Most chemical reactions would show a rate decrease with time. I find it odd, even considering fusion as a possible explanation, that the rate doesn't decline, because I'd expect the NAE (Nuclear active environment) spots to become more rare. Apparently from the Arata results, that doesn't happen rapidly, if at all. If the NAE is sustained, that's new and very significant.--Abd (talk) 17:33, 4 September 2009 (UTC)

Talking about Arata again, not Kitamura. A flat line at 3000 hours when everything else has been over for some time usually suggest a malfunction. Replication is in order, with attempts to make sure such malfunctions cannot occur. Kirk shanahan (talk) 16:33, 4 September 2009 (UTC)

However, most of this is moot for Wikipedia. We depend on those ignorant reviewers, and we don't second-guess them. If there is reason to doubt a report in reliable source, we attribute it, that is always a ready compromise, and we don't do OR, original research, except -- in my opinion -- for background. We don't draw conclusions, we let our readers do that.--Abd (talk) 17:33, 4 September 2009 (UTC)

As Abd has recently pontificated on on the Vortex mailing list, Wiki is not supposed to be based on 'primary' sources like Kitamura at all. Further, I also contend that recentism is active here as there hasn't been any time for response in the literature. Including it in the CF article is inappropriate, which is the comment on the CF talk page I was explaining at length here. Kirk shanahan (talk) 16:33, 4 September 2009 (UTC)

I've now signed my comments above, which Kirk chopped up, as he commonly does. He's right about primary sources, but he has missed that the Kitamura report is partly a secondary source, commenting on the primary Arata sources, plus there is quite a bit of other secondary source on Arata, his work is quite well known. I DGAF if the work is "used" at cold fusion, that's up for neutral editors to decide. I'm now, like Shanahan, covered by conflict of interest guidelines. He's welcome to talk to himself all he wants.... He's right, I haven't read the Kitamura paper, except for some other coverage of it, especially Kirk's comments above, which aren't RS on what is in Kitamura, unfortunately. --Abd (talk) 17:33, 4 September 2009 (UTC)

Kitamura et al report in their Abstract that they are trying to replicate the ZA&Z reports. They discuss these reports in the Introduction of their paper. This is by no means anything other than a report on why they did the work, to try to replicate A&Z.

As I noted, they failed to do so based on good scientific standards, yet claim to have actually done so. This in no way gives credence to the A&Z claims. The way it works is that a report (replication or not) must pass scientific scrutiny by experts in the field (not peer reviewers, every psudoscience fiasco of the last century was published at one point or another) before it can be considered 'evidence' for or against anything. Kitamura fails to pass muster and thus is not RS.

Again, Abd is switching to the A&Z reports, even though I have said I'm not talking about that. That's just bad debate tactics. Further Abd uses the 'call to authority' argument ("his work is quite well known"), which means nothing except Abd has no acceptable argument to present. He then tries to blow smoke by claiming some critical problem based on 'conflict of interest', instead of responding to the challenge I issued to define any such problem he can cite. And, it is of interest to note that, when challenged thusly, Abd cuts and runs. Pure crying wolf. Kirk shanahan (talk) 18:30, 4 September 2009 (UTC)

BTW, my listing of potential problems with the Arata 2007 demo can be found at the end of Item 2 in the Archive 26 of the cold fusion talk page. Dated Mar. 19, 2009. More stuff Abd ignores. Kirk shanahan (talk) 19:14, 4 September 2009 (UTC)

Direct link to list:

Talk:Cold\_fusion/Archive\_26#Yoshiaki\_Arata.27s\_experiment.  
--Enric Naval (talk) 14:22, 5 September 2009 (UTC)

Nah, Kirk, I'm not debating you, it's not worth it. If you want a debate, how about you post to the Vortex list, where you'll be sliced and diced before breakfast? Here, you can be the big frog in a small pond all you like, the editors who have more actual knowledge than you having all been banned. Except more will keep showing up. That's what happens in situations like this, it's endless. Visible, you are, Kirk. That's all that's needed from me. --Abd (talk) 21:25, 4 September 2009 (UTC)

In case you didn't know (which is probably true as you never check things out before pulling the trigger) I was on Vortex for awhile. I tried to conduct reasoned debates there, and was met with nothing but garbage. When I finally responded in kind I was chastised by the moderator, while those doing the same to me for many more posts were congratulated. Sorry, no dice. If you folks at Vortex seriously think you can shake the technical basis of my commentaries, then I suggest we meet on 'neutral ground', the Usenet newsgroup sci.physics.fusion, which was specifically created to discuss cold fusion issues. It has been taken over by crazies these days, but we can ignore them like usual on an unmoderated Usenet newsgroup. You guys start it up by posting something, and I will respond as my workload and desire allows. Make sure you notify me by email that you have done so, I don't routinely follow the group anymore. There is a common 'Netiquette' for Usenet, but the group is unmoderated, so anything can go. I however only respond to technical arguments there. Here there are supposed to be rules which I complain about when violated; you won't see that there. BTW, I will essentially do there what I have done here, answer posts

point-by-point. But the problem I always find with the CFers is that when I answer their objections they never accept that I have done so. It's like with Ed Storms. We had up to 100 emails exchanged on the CCS issue. In the end, he repeated back exactly what I was saying, at which point I asked him why then he couldn't accept what I said as true. He replied that he didn't know, but it just couldn't be true. So after your buddies have tried to 'slice and dice' my arguments, and I have shown their slicing and dicing to be incorrect or irrelevant, they still will not change because they are committed fanatics who have abandoned good scientific practice in favor of a permanent emotional commitment to the CF idea. In other words, I do not expect to convince *them* of anything at this point. I will show the interested reader however where their logic quits and their emotion begins. I also will not deal with Jed Rothwell or yourself. Both of you don't have the technical skills to keep up with me, even though I have tried to educate you, so, in advance, I declare I only want to deal with primary CF researchers like Storms, McKubre, Miles, Szpak, etc. I don't want to mess with groupies. You might try starting with Steve Krivit if the primaries won't play (which is the norm by the way), but I have minimal respect for him, as he seems to 'know' who I am and what I say based on about 15 minutes of phone time. In other words, he seems as biased and bigoted as any other CFer, but I will give him the benefit of the doubt for awhile, until he establishes he *modus operandi*. Kirk shanahan (talk) 14:11, 5 September 2009 (UTC)

Oh, and BTW, please always remember that my position is that there are real anomalies detected by CFers, but the problem is that they jump to the 'nuclear' explanation based on insufficient and incomplete evidence, and in science that jump to a conclusion is illegitimate. So, my thrust has always been to point out conventional alternatives that have NOT been eliminated by published research (as I said, I don't deal in rumors and innuendos, just 'the published facts'). Kirk shanahan (talk) 14:18, 5 September 2009 (UTC)

*98.210.193.221 added a Table to my Talkpage that can be found as Table 1 from Mosier-Boss et al (2008) "Reply to comment on 'The use of CR-39 in Pd/D co-deposition experiments': a response to Kowalski" (<http://www.epjap.org/index.php?option=article&access=standard&Itemid=129&url=/articles/epjap/pdf/2008/12/ap08414.pdf>) Eur. Phys. J. Appl. Phys. **44**: 291–5, p. 292. This just clutters up my page, so I deleted it. Check the reference if you want to see it* Kirk shanahan (talk) 11:32, 21 September 2009 (UTC)

Do you think the neutron tracks and charged particles are included in those real anomalies? 99.56.139.186 (talk) 06:19, 8 September 2009 (UTC)

No, I think the CFers get a pin and make all of them,

one-at-a-time. C'mon 99, of course they are real, but you don't need to (and probably shouldn't) hang the 'nuclear' label on them. I don't believe they are caused by neutrons or charged particles (except in those cases where a radioactive filler was detected in O-rings used in the cell construction). They are caused by physical damage from the shockwaves of the exploding H<sub>2</sub>+O<sub>2</sub> bubbles formed at the electrode surface (think 'depth charges'). Kirk shanahan (talk) 11:23, 8 September 2009 (UTC)

Can you cite any examples of CR-39 pits having been created that way? When was radioactive filler discovered in cell O-rings? Given that the detected charged particles were deflected by a magnetic field, any ideas for alternative hypotheses there? 98.210.193.221 (talk) 04:18, 16 September 2009 (UTC)

“Can you cite any examples of CR-39 pits having been created that way?” – See the Galileo report issued by Steve Krivit (GalileoProjectReport.pdf). Two examples are shown there, one from Kowalski and one from Mosier-Boss, on page 30. I first noted it in a report c. 2002 based on a different report (which I can't locate today), which is why I posted the comment to spf that year about the potential shockwave damage.

“When was radioactive filler discovered in cell O-rings?” – May 2007 apparently. See Scott Little’s report at <http://www.earthtech.org/experiments/PACA/report.htm>. I believe he found it, as he did with the contamination problem being the probable root cause of ‘heavy metal transmutations’. This finding casts doubt on any experiment where such a problem was not searched for, and by default that will probably include all prior CR-39 work, unless very good reasons are given to expect such a thing could not have occurred (just like my delineating the CCS causes similar problem with apparent excess heat reports). I have noticed recent CR-39 articles are checking for this. This problem can explain how Oriani, et al, get pits in CR39 suspended above the electrolyte. Pits in CR39 in the electrolyte of course can still come from shockwaves. And, in both cases, oxidative attack by O<sub>2</sub> can potentially cause pits as well, although I suspect it is less likely above the electrolyte.

“ Given that the detected charged particles were deflected by a magnetic field, any ideas for alternative hypotheses there?” - reference? I have some ideas, but I want to review the publication before responding.

With regards to the Table you stuck on my page, what is your point? When you have electrolysis with Pd present, you can get the FPHE, and you will get the ‘microexplosions’ which will pit the CR39. All examples in your Table are perfectly consistent with the conventional explanation I offer. Kirk shanahan (talk) 15:34, 16 September 2009 (UTC)

I can't find GalileoProjectReport.pdf -- do you have a full URL? -- but it seems to be from 2008. What is FPHE? Didn't the SPAWAR team reply specifically to the charges from those 2007 and 2008 attempts at reproduction by Krivit? The ref for charged particle detection is: Szpak S, Mosier-Boss PA, Gordon FE (2007) "Further evidence of nuclear reactions in the Pd–D lattice: emission of charged particles" (<http://www.springerlink.com/content/75p4572645025112/>) *Naturwissenschaften*, vol. 94 pp. 511–514. 98.210.193.221 (talk) 00:16, 17 September 2009 (UTC)

“I can't find GalileoProjectReport.pdf -- do you have a full URL? -- but it seems to be from 2008.” <http://www.newenergytimes.com/v2/projects/tgp/2007TGP/2007GalileoProjectReport.pdf> Hint: Go to Steve Krivit Web Site, New Energy Times, and type “Galileo Project Report” in the search bar.

“What is FPHE?” - Read the doc found on the LENR-CANR website at (fixing the link of course): <http://www.lenr-canr.org/acrobat/ShanahanKaposiblec.pdf>

“Didn't the SPAWAR team reply specifically to the charges from those 2007 and 2008 attempts at reproduction by Krivit?” - The SPAWAR team was an active participant in the project.

“The ref for charged particle detection is: Szpak S, Mosier-Boss PA, Gordon FE (2007) "Further evidence of nuclear reactions in the Pd–D lattice: emission of charged particles" *Naturwissenschaften*, vol. 94 pp. 511–514. “ - Sorry, no mention of ‘deflection’ by magnetic fields. Wrong referenece, try again. They do state that turning on an electric field produced pits, i.e. no field, no pits. But I don’t think this completely jives with current positions on this. This does modify my response regarding your Table. Apparently you can have codeposited Pd present and no pits. However, that just jives with other experience in the field that the effect is not 100% reproducible. However if you are interested in getting the whole picture, you should read: <http://www.earthtech.org/CR39/index.html>. Also, the original ref I was referring to from 2002 is Oriani, R. A., Fisher, J. C.; *Jpn. J. Appl. Phys.*, 41 (2002) 6180-6183, where the last line on 6180 says: “It is necessary to distinguish between etch pits that identify charged particle tracks and those that result from surface damage or internal defect in the plastic.” And later in the same paragraph on 6181; “We rejected etch pits that occurred in clusters and those that formed linear arrays fearing that they may have arisen from surface scratches.” In other words, surface damage => pits. Shockwaves can cause surface (and probably subsurface) damage.

You didn't answer the question about what the point was to the Table you cluttered up my talk page with. Kirk shanahan (talk) 11:42, 17 September 2009 (UTC)

Thanks. I have read those materials. The table illustrates the state of the SPAWAR position on the CR-39 experiments. Why do you suggest that the dependent variable reflecting pits present or not, which is completely correlated with the independent existence of an external electrostatic or magnetostatic field, does not constitute deflection? 98.210.193.221 (talk) 19:06, 18 September 2009 (UTC)

From [wordnetweb.princeton.edu/perl/webwn](http://wordnetweb.princeton.edu/perl/webwn) :

deflect – “turn from a straight course, fixed direction, or line of interest” or “turn aside and away from an initial or intended course”

You may note the embedded concept of something being there first before it is deflected. The paper that you reference does not say that, it says ‘nothing’ without the magnets, ‘something’ with. ‘Deflection’ has nothing to do with this.

Your comments are not well thought out, they are biased heavily towards cold fusion, they are not about the Kitamura paper (and are thus red herrings), and I didn’t like the way you avoided the issue of signing posts on the main CF Talk page. I don’t believe I want you posting to my Talk page anymore. Please stop. If you don’t, I will delete any comments made by you, or anyone else, who doesn’t sign their posts with a named account. Even if you sign one, it will still need to be applicable and make some level of sense before I answer it. Kirk shanahan (talk) 11:32, 21 September 2009 (UTC)

I keep having this nagging sensation that this IP could be User:Nrcprm2026.  
--Enric Naval (talk) 18:05, 21 September 2009 (UTC)

Doesn't really matter to me. All names on the Internet can be fake, and my prior experience is in the unmoderated Usenet group sci.physics.fusion.

But the point is to have a discussion, presumably aimed at improving the article(s) you're working on. Most of my time on Wiki has been spent fighting people who oppose the idea that CF isn't proven. They don't want to discuss and present, they want to suppress and advocate, and I'm done with that. Thus, I am getting more militant. Kirk shanahan (talk) 18:47, 21 September 2009 (UTC)

## Deflection of charged particles

"emission of highly energetic charged particles emitted from the Pd/D electrode when this system is placed in either an external electrostatic or magnetostatic field" -- That's from the 2007 SPAWAR charged particle detection abstract. (<http://www.springerlink.com/content/75p4572645025112/>) Why is it not a "deflection"?

Why do you call deletion of the table of SPAWAR CR-39 results "militant"? Is "militant" more accurate than the word you used in the previous sentence, "suppress"? 99.27.202.101 (talk) 14:16, 16 December 2009 (UTC)

W.r.t your first question: If charged particles are emitted, then they would be deflected by an electric or magnetic field. Please note your quote is of the published SPAWAR group position that an external mag or elec field is required to get emission, but also note that during 'The Galileo Project' Little proved this was not true and Mosier-Boss agreed and recommended a protocol change based on that, at least accoring to Little's report on his Web page. I don't believ Little makes stuff up, so I believe it.

Where was that recommendation of a protocol change made? 99.27.134.160 (talk) 22:51, 30 December 2009 (UTC)

30. S. Little, “‘Extraordinary Evidence’ Replication Effort”, <http://www.earthtech.org/CR39/index.html> Kirk shanahan (talk) (Look under the 'Magnetic Effects' section.) 13:40, 31 December 2009 (UTC)

How can that page, with its modification date of April, 2007, be a response to the charged particles paper published in June, 2007? 99.56.138.51 (talk) 20:46, 1 February 2010 (UTC)

From that page "Pam Boss reported (during the March 2007 APS meeting) (...)" <---- the page was updated with the results that were announced at the APS. The paper reporting those same results was published later, but the results were already known. --Enric Naval (talk) 17:09, 2 February 2010 (UTC)

Exactly. And one also needs to realize that both sets of authors (Little and the SPAWAR group) were originating reports based on their **joint** experience. Yet when The SPAWAR folks published, they reverted to the prior 'state-of-the-art'. Why? Probably beause the Galileo Project results conflicted with what they wanted to conclude. Kirk shanahan (talk) 12:55, 3 February 2010 (UTC)

W.r.t you second question, the deletion was not the 'being militant' I was referring to. I was referring to the fact that I had become somewhat fanatical myself in trying to get Wikipedia to balance the article with inclusion of the antiCF side of the story. In fact, that was a short-lived phase as I have determined I am unlikely to prevail over the CF fanatics in the long term. I hate to waste my time, so I have moved on to more productive avenues, thus my delayed response to you. Kirk shanahan (talk) 17:04, 28 December 2009 (UTC)

I hope some day you will be interested in accurately representing both sides. If you can do that, I would be willing to bet you could become one of the most effective editors of cold fusion the encyclopedia has yet seen. 99.27.134.160 (talk) 22:51, 30 December 2009 (UTC)

This comment definitely requires a response. It is a fallacy to believe that any Wikipedia editor must only add what appears to be neutral information to a given page. I seriously doubt that is possible to obtain anyway, since everyone has a bias. In fact, the Wikipedia idea is for multiple editors to add pieces of information that together present a neutral article. I was specifically asked to add the 'con' view to the Wikipedia CF article because the editors at that time recognized it was 'pro' biased. It was not my job nor desire to try to repeat what was already there, or to significantly alter what the prior editors had added. My 'job' was to add the 'con' side. Unfortunately, when I did so, Pcarbonn block deleted it all, for spurious reasons. I subsequently gave it my best shot to get the information returned to the page, and failed to overcome the actions of the numerous pro-cold fusion fanatics that haunt that page. Those fanatics have successfully denied the Wikipedia reader an intelligent explanation of why cold fusion claims are not persuasive. It's a sad state, but I can do nothing about it.

What is really funny though is that the proCFers say exactly the same thing in reverse! They claim the CF article is so 'con' they can't understand how an organization (Wikipedia) claiming to put out neutral article could have written it! Of course an unbiased look at the article shows it has a history section, which is not as 'pro' as they would like, and a lot of information on what is claimed by the CFers, but precious little on why what they claim is bogus. The latter is why the article is unbalanced, not because of some incorrectness in the presentation of the proCF case. All their comments do is show the intensity of their fanaticism. Kirk shanahan (talk) 13:38, 31 December 2009 (UTC)

In fact the neutrality policy WP:NPOV is one of the most fundamental policies here.  
99.56.138.51 (talk) 20:47, 1 February 2010 (UTC)

Exactly! So why is the CF article so biased??? Kirk shanahan (talk) 12:31, 3 February 2010 (UTC)

## Literature question

Have there been any peer-reviewed publications questioning cold fusion subsequent to yours? 99.27.202.101 (talk) 16:39, 16 December 2009 (UTC)

Subsequent to 2006, no, I am not aware of any technical antiCF papers. W. B. Clarke, et al published several papers in the 2002-3 time frame, which was after my 1st publication, but Brian Clarke passed away unexpectedly also during that time frame. Kowalski had a Comment regarding the SPAWAR publication, but he isn't an antiCFers, and his comment was highly focused, so it doesn't really qualify (but it might if you're generous). I have submitted a Comment on Kitamura's paper, and they apparently will be publishing a Response. Based on an email I sent today, both should be moving to peer review shortly. I also am working on another paper for submission soon I hope. The mainstream believes cold fusion was stopped in c. 1994, and does not consider any effort on the subject worthwhile. I am an anomaly in that respect. Kirk shanahan (talk) 16:52, 28 December 2009 (UTC)

Thanks. Your description places the editorial board of *Physics Letters A* and several European journals out of the mainstream. 99.27.134.160 (talk) 22:48, 30 December 2009 (UTC)

That's excellent! Keep on thinking I am God, able to alter the status of mainline journals with

the wave of my wand (or pen, or keyboard (I did. I actually waved my keyboard over the screen when I wrote the comment above.)). No "99", Phys. Lett A and Eur. Phys. J. are still 'mainline' or 'mainstream' journals. The fact that some pseudoscience slipped in does not change their status. I think I am safe in saying that every 'scientific fiasco' has been published at one point.

No, 'mainline' and 'mainstream' are terms used to describe 'the average scientist'. Today, the average scientist when asked any question about 'cold fusion', typically says; "What? I thought that issue was settled years ago." and "Wasn't that stuff determined to be garbage science." (or the equivalent). Note the period in that last 'quote'. The statement is written like a question, but the period makes it a statement, and that's exactly how it is spoken. You might think of the scientist slightly raising one eyebrow while saying it. In other words, the mainline scientist, when challenged on this issue, immediately assumes you (the questioner) are 'one of them', i.e. a pseudoscientist. How do I know this you might ask? Because that's exactly what has happened to me on many occasions. In those situations, I have to educate the person as to the validity of the idea that CFers have documented many anomalies, but that I believe they have misexplained them all because of a bias towards the nuclear solution.

In any case, the reason there are no other antiCF publications out there is just that, everyone believes the issue was settled back in '92. They have moved on. As I have explained many times previously, I didn't get into metal hydrides until 1995, and when I found my colleagues didn't have good explanations for why CF was 'junk', I went hunting. I can now say I know why CF is junk, and the prime reason is that CF researchers do not look at anything but nuclear explanations. Unfortunately, non-nuclear explanations are much more rational and reasonable. Kirk shanahan (talk) 13:47, 31 December 2009 (UTC)

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