A Critique of the Student's Guide To Cold Fusion

By Kirk Shanahan (kirk.shanahan@srs.gov)

Plus rebuttals by Edmund Storms and Michael Staker

February 11, 2003

Dr. Edmund Storms has just released a new paper on cold fusion (CF, aka LENR, or CANR) that contains a section (in Chapter 8) that purports to address the issues I raise with cold fusion calorimetry in my paper and spf comments. I would like to address those comments dealing with my "calibration constant shift" (CCS) proposal to illustrate why they are incorrect.

In the subsection "Changes in calibration constants" Dr. Storms correctly writes:

"All calorimeters must be calibrated. The resulting calibration constant may not always remain constant. Each time such a measurement is made, slightly different values are always obtained. If the claimed anomalous energy is within the range defined by many calibrations, its reality can be questioned."

However, he goes on to say:

"Shanahan [249] argues that all claims for anomalous heat are caused by an unexpected change in the calibration constant because of some undefined process within the cell, but by not a nuclear reaction."

To be completely correct, I argue that insufficient evidence is presented in CF reports to assess whether a CCS is present or not. The difference is one of attitude. My assertion is not an explicit rejection of anomalous heat claims, as Dr. Storms would have the reader believe. Instead I require, as should any competent scientist, that convincing evidence of a radical claim be presented. The extant claims to prove a nuclear reaction present via nuclear ash are either clearly erroneous, interpretable to support other processes, or unreproduced., and I challenge the excess heat claims.

Dr. Storms continues:

"An answer to this challenge rests on three facts:

(1) many reported values of anomalous heat are well outside of this range;
(2) anomalous heat is frequently associated with universal patterns of behavior, as noted in Chapter 2; and
(3) anomalous heat sometimes is associated with helium production or transmutation products, which are clear indicators of a nuclear reaction."

Based on my studies of the CF literature, there are no papers where "Fact 1" can be proven. Instead, it is rare to find calibration data supplied or even mentioned. I challenge Dr. Storms to cite examples (hopefully readily obtainable ones, obscure references are as bad as none).

In general, the 'excess heat' observations tend to follow the typical pathological science prediction that they get smaller as the equipment gets better. In this case, 'better' means a higher percent of the cell heat captured and used to produce the signal. Dr. Storms' calorimeter for example captured about 98% of the heat deposited in the cell, and the 'effect' he observed is completely explained within that framework.

Similarly, Dr. McKubre reported some excess heat results in 1992, that were used to obtain more funding from EPRI to build a better calorimeter (actually several). All excess heat results from those calorimeters either were non-existent or much less that the 1992 results, as would be expected with a pathological science effect. Further, while no calibration data was supplied, my analysis of the later results showed that it was another 2% effect, agreeing with Dr. Storms' results.

The patterns referred to in Fact 2 are more the imaginings of Dr. Storms than anything else. Secondarily, I also propose a real effect as the root cause of the CCS, so it wouldn't be surprising to find correlated factors. The point is that Dr. Storms does not distinguish experimental error and data misinterpretation from possible real correlations, so his basis set is corrupted and cannot be trusted.

Much recent comment has been made here and in the literature as to the ability of CF researchers to objectively and accurately analyze their samples. The associated nuclear ash reported by Dr. Storms in Fact 3 is usually explained easily by bad analytical chemistry. There do remain a few isolated reports after filtering out the bad stuff, but they are not reproduced and as such are still nothing but suggestions, not proof. An objective scientist knows that one can never prove a negative, so we can never say "CF doesn't exist". Thus we must allow researchers to make suggestions, but we must also insist on reproducibility to move us from the conventional wisdom position.

Clearly Dr. Storms 'answer' lacks substance.

He continues:

"Furthermore, a process that can produce such a change in all calorimeters has not been demonstrated, only suggested. "

This is correct. A demonstration of how the CCS effect would work has been published, but there is no definitive way to know what the real chemical/physical processes underlying the effect are without research. On the other hand, the same can be said about nuclear explanations. The advantage of the non-nuclear explanation is that it conforms to our current understanding of chemistry and physics, while the "CF" explanation does not. As a default then, the 'mundane' explanation should be the preferred guide to understanding and future work.

It should also be noted that the proposed mundane explanation is supported by some evidence. Specifically, the IR photos taken by Szpak, et al, show localized hot spots on "CF" active electrodes, which is completely consistent with the proposal of combined H2 and O2 bubbles burning at the cathode. That in turn leads to a shift in the heat production location partitioning in the cell, which can produce a CCS. In addition, high surface area cathodes seem to be the most likely to produce the "CF" effect. High surface area should correlate to good bubble retention through surface tension and adhesiveness effects, which should assist in forming the mixed H2/O2 bubbles.

"At this time, if a person wants to reject anomalous heat, the proposed prosaic process must be demonstrated using as much rigor as was used in making the initial claim, not simply suggested."

This is completely corerct, if we are to ever sort out the "CF" mess. However, rationally speaking, any such work must be done by those best equipped to do so. That does not require the suggestor must be the experimentor. Science is usually a collaborative enterprise. In this case, a suggested cause is postulated. Logically, the current batch of CF researchers would be the ones to test it out. However, in a clear demonstration of how pathological science is conducted, they refuse to even accept the viability of the suggestions, based on no technical reasons, apparently only on their dislike of the implications to their pet theories.

There are many, many other things wrong or misrepresented in Dr. Storms' paper, but I would like to address only one more. In section II.2, Dr. Storms writes:

"Arata and Zhang [78] at Osaka University in Japan were the first to generate anomalous energy using finely divided palladium. This powder is contained in a palladium capsule, which is pressurized with very pure deuterium, generated by electrolysis. The claim was duplicated at SRI [79, 80] with Prof. Arata's help."

Now reference 80 is to the _second_ paper published by the late Prof. W. Brian Clarke on samples supplied from an Arata cell and it reports a detection of a possible tritium content. This does represent one of those cases whose result I trust, but which is unreproduced. What was omitted however by Dr. Storms was the companion paper (the _first_ one) that showed _no_ detection of 4He from the material, which was a direct contradiction to the claims of Prof. Arata, et al. An interesting omission that illustrates again the pathology of the field. The supportive results are selected and touted, while the direct contradictions are forgotten.

Furthermore, in a preprint, submitted and accepted for publication I believe, Dr. Clarke checked gas samples from the famous 'Case-replication' at SRI, and found that all 4 samples has significant air content, and that the SRI numbers touted by R. George were erroneous.

The basic conclusion a competent scientist draws from these papers is that the analytical capabilities of the CF researchers need tuning up.

I'd like to suggest to those who use Dr. Storms' paper to start them out in the CF field that they start at the end of the references. The last one (#249) is my paper. If you read it first and understand that the problem I discuss is generic to all CF calorimetry, you will be much better prepared to sift the literature. The error characteristics I describe will be prevalent throughout the reports, which should convince you that there is no energy basis to the claims. Heat-ash correlations will disappear, and thus will not be valuable for 'proving' CF exists. That then makes it much easier to understand that nuclear ash claims are just 'messed-up' analytical studies. That should make it easy for you to not get caught up in the tempest in a teapot that is 'cold fusion'.

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

The following critique of the Student's Guide was accepted as a Letter-To-The-Editor of the LENR website. Dr. Edmund Storms answered the comments within the text. The reader is welcome to provide additional comments.

Dr. Edmund Storms has just released a new paper on cold fusion (CF, aka LENR, or CANR) that contains a section (in Chapter 8) that purports to address the issues I raise with cold fusion calorimetry in my paper and spf comments. I would like to address those comments dealing with my "calibration constant shift" (CCS) proposal to illustrate why they are incorrect.

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It is very easy to reframe skepticism to make it appear to be based on the absence of suitable information. Frequently, such an argument is justified when only a few examples are available. In this case, Dr. Shanahan has gone to considerable effort to demonstrate that one unpublished calorimetric study reveals a problem that may have universal application to dozens of studies that have shown excess energy. In my mind, his approach he describes here is consistent with my description of his approach as provided in the Guide.

Dr. Storms continues:

"An answer to this challenge rests on three facts:

(1) many reported values of anomalous heat are well outside of this range;
(2) anomalous heat is frequently associated with universal patterns of behavior, as noted in Chapter 2; and
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I suggest Dr. Sananhan look at the papers listed in Table 1 in the review "Cold Fusion, an Objective Assessment" that is available in full text on www.LENR-CANR.org. All of the listed papers provide a value for the uncertainty in the respective measurement. While few people provide all information on which this error is based, normal scientific evaluation does not require such detail. The absence of detail is never used to reject a measurement in conventional science, especially when the claimed signal is very large compared to the uncertainty, as is the case for many of the measurements.

In general, the 'excess heat' observations tend to follow the typical pathological science prediction that they get smaller as the equipment gets better. In this case, 'better' means a higher percent of the cell heat captured and used to produce the signal. Dr. Storms' calorimeter for example captured about 98% of the heat deposited in the cell, and the 'effect' he observed is completely explained within that framework.

The fraction of heat captured by a calorimeter has no relationship to its accuracy. A calorimeter is only required to be stable with respect to time. Such stability is routinely demonstrated by repeated calibration over a period of time and is one factor on which the stated accuracy is based.

Similarly, Dr. McKubre reported some excess heat results in 1992, that were used to obtain more funding from EPRI to build a better calorimeter (actually several). All excess heat results from those calorimeters either were non-existent or much less that the 1992 results, as would be expected with a pathological science effect. Further, while no calibration data was supplied, my analysis of the later results showed that it was another 2% effect, agreeing with Dr. Storms' results.

This statement is not true, as a reading of the papers published by Dr. McKubre demonstrates.

The patterns referred to in Fact 2 are more the imaginings of Dr. Storms than anything else. Secondarily, I also propose a real effect as the root cause of the CCS, so it wouldn't be surprising to find correlated factors. The point is that Dr. Storms does not distinguish experimental error and data misinterpretation from possible real correlations, so his basis set is corrupted and cannot be trusted.

Dr. Shanahan assumes that the reported excess is not real and proposes a mechanism that he uses to explain the results. This mechanism is based on a change in calorimeter behavior each time the current was raised or when the cathode was cleaned. This change was just enough to produce the apparent excess. The assumed mechanism has not been demonstrated to be real. Nothing I have proposed is based on my imagination. All behavior is based on measurements, which are interpreted in conventional ways, in contrast to the proposals made by Dr. Shanahan.

Much recent comment has been made here and in the literature as to the ability of CF researchers to objectively and accurately analyze their samples. The associated nuclear ash reported by Dr. Storms in Fact 3 is usually explained easily by bad analytical chemistry. There do remain a few isolated reports after filtering out the bad stuff, but they are not reproduced and as such are still nothing but suggestions, not proof. An objective scientist knows that one can never prove a negative, so we can never say "CF doesn't exist". Thus we must allow researchers to make suggestions, but we must also insist on reproducibility to move us from the conventional wisdom position.

The number of observations for helium production and the presence of transmutation products is increasing as anyone can plainly see from the listed literature. A person no longer has to accept the distorted claims provided by skeptics such as Dr. Shanahan.

Clearly Dr. Storms 'answer' lacks substance.

He continues:

"Furthermore, a process that can produce such a change in all calorimeters has not been demonstrated, only suggested. "

This is correct. A demonstration of how the CCS effect would work has been published, but there is no definitive way to know what the real chemical/physical processes underlying the effect are without research. On the other hand, the same can be said about nuclear explanations.

The nuclear explanation has been explored by dozens of people with increasingly accurate and consistent results. This is the requirement of science in contrast to proposing trivial mechanisms based only on the imagination.

The advantage of the non-nuclear explanation is that it conforms to our current understanding of chemistry and physics, while the "CF" explanation does not. As a default then, the 'mundane' explanation should be the preferred guide to understanding and future work.

This is true. However, nature occasionally demonstrates behavior that does not fit conventional explanations. Therefore, novel explanations need to be explored without being rejected because mundane explanation can be imagined. A person needs to ask what benefit results from rejecting LENR before it has been given a fair hearing?

It should also be noted that the proposed mundane explanation is supported by some evidence. Specifically, the IR photos taken by Szpak, et al, show localized hot spots on "CF" active electrodes, which is completely consistent with the proposal of combined H2 and O2 bubbles burning at the cathode.

This behavior is not consistent with H2+O2 recombination because significant oxygen does not exist in the region of the cathode. Oxygen is produced at the anode and the resulting bubbles rise rapidly to the surface and are vented to the gas above the electroilyte. This fact is well known to anyone who has taken the trouble to observe an electrolytic cell. What is the point of proposing a mechanism that can not occur based bon simple observation?

That in turn leads to a shift in the heat production location partitioning in the cell, which can produce a CCS. In addition, high surface area cathodes seem to be the most likely to produce the "CF" effect. High surface area should correlate to good bubble retention through surface tension and adhesiveness effects, which should assist in forming the mixed H2/O2 bubbles.

This proposed mechanism makes no sense and can be easily rejected by simple observation.

"At this time, if a person wants to reject anomalous heat, the proposed prosaic process must be demonstrated using as much rigor as was used in making the initial claim, not simply suggested."

This is completely corerct, if we are to ever sort out the "CF" mess. However, rationally speaking, any such work must be done by those best equipped to do so. That does not require the suggestor must be the experimentor. Science is usually a collaborative enterprise. In this case, a suggested cause is postulated. Logically, the current batch of CF researchers would be the ones to test it out. However, in a clear demonstration of how pathological science is conducted, they refuse to even accept the viability of the suggestions, based on no technical reasons, apparently only on their dislike of the implications to their pet theories.

This is not true. People doing CF research have considered all possible explanations, as a reading of the papers and reviews would reveal to any objective person. What benefit does such a statement have in advancing a correct understanding of the novel proposals?

There are many, many other things wrong or misrepresented in Dr. Storms' paper, but I would like to address only one more. In section II.2, Dr. Storms writes:

"Arata and Zhang [78] at Osaka University in Japan were the first to generate anomalous energy using finely divided palladium. This powder is contained in a palladium capsule, which is pressurized with very pure deuterium, generated by electrolysis. The claim was duplicated at SRI [79, 80] with Prof. Arata's help."

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The statements made by Dr Shanahan are not correct, as a reading of the cited paper will show. Dr. Clarke found some He-4 in the Pd supplied by Prof. Arata, but much less than the amount claimed by Prof Arata. However, we now know that most of the He-4 leaves the metal and is found only in the gas, which Prof. Arata measured. I suggest this distortion of the facts reveals a pathology of the skeptical approach.

A negative result proves noting about the reality of a claim, as a basic understanding of logic shows. All negative results, of which there are many, were omitted in the Guide because they reveal nothing about the subject. They only show that the effect is difficult to produce. All new effects are difficult to produce. Even heavier than air flight was difficult

to produce until the Wright Brothers solved the problem. Skeptics rejected the whole idea of flight for many years, even after successful work was published, just as they are now doing with CF. Anyone wishing to understandthe style of the skeptical mind should read "Forbidden Science" by Richard Milton.

Furthermore, in a preprint, submitted and accepted for publication I believe, Dr. Clarke checked gas samples from the famous 'Case-replication' at SRI, and found that all 4 samples has significant air content, and that the SRI numbers touted by R. George were erroneous.

The basic conclusion a competent scientist draws from these papers is that the analytical capabilities of the CF researchers need tuning up.

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The hubris that this statement represents is amazing. Dr. Shanahan would have the reader believe that his single analysis of one calorimetric measurement discredits all such measurements made by hundreds of competent and well-trained scientists at dozens of laboratories worldwide. In addition, he leaps from this flawed analysis to a rejection of all measurements involving nuclear products. What is the purpose of such an approach? What benefit results from rejecting a potential source of clean energy based on such flimsy evidence? Does such an approach strengthen science? This is for you to decide based on the provided evidence.

Edmund Storms, Ph. D.

Dr. Storms has responded to my comments with a document he intends to post on the LENR-CANR Website. I post his comments with my responses here. He indicates he will not be responding further, and I concur, there is no point in our continued debate, since he and I have been over this ground repeatedly in private emails over the last 3 years. I will respond here simply for the student. I expect to have no impact on the closed circle of cold fusion aficionados.

I have snipped out Dr. Storms' reprinting of my original comments (and CCS = calibration constant shift).

| It is very easy to reframe skepticism to make it appear to be based on
| the absence of suitable information.

It is a simple fact that 'good' decisions cannot be made without suitable information. If I can demonstrate why suitable information is not available, then I have called into question any decisions made up to that point. That is the purpose of skepticism, i.e., not being taken in by smooth arguments and assertions that in fact have insufficient support.

| Frequently, such an argument is
| justified when only a few examples are available. In this case, Dr.
| Shanahan has gone to considerable effort to demonstrate that one
| unpublished calorimetric study reveals a problem that may have universal
| application to dozens of studies that have shown excess energy.

My study of one set of results is illustrative. I have repeatedly remarked that I found no case in the literature that contradicts my proposal, and that most can be seen to show characteristics consistent with the CCS problem. In all cases, insufficient information is present to assess the CCS possibility directly.

| In my mind, his approach he describes here is consistent with my
| description of his approach as provided in the Guide.

Dr. Storms barely describes my apporach in his Guide. He gives it 10 lines total. At best, one line of that presents my argument, and refers the reader to my paper. What he does in the other 9 lines is to try to refute my challenge, and that refutation is what I challenged in this spf thread.

| I suggest Dr. Sananhan look at the papers listed in Table 1 in the review
| "Cold Fusion, an Objective Assessment" that is available in full text on
| www.LENR-CANR.org.

Dr Storms has said this to me many times, and I have always responded that I have done so. This comment here is strictly to give the reader the impression that I am an unstudied lout.

| All of the listed papers provide a value for the
| uncertainty in the respective measurement.

More specifically, for the precision of the calorimeters. This is usually very good, so the resultant changes showing excess energy peaks are actually to be trusted as indicators that something happened. What is not to be trusted however, it the accuracy of the curves. In other words, something did happen, but there is no proof it was nuclear (since it is the size of the effect that leads to the nuclear conclusion).

| While few people provide all
| information on which this error is based, normal scientific evaluation
| does not require such detail.

It does when there is a question outstanding about it. Further, remember that the issue is not precision, it is accuracy, specifically impacted via a CCS.

| The absence of detail is never used to
| reject a measurement in conventional science, especially when the claimed
| signal is very large compared to the uncertainty, as is the case for many
| of the measurements.

Wow. I write a whole paper on just how this occurs, and Dr. Storms refuses to accept it. Also note that I don't 'reject' the measurements, I reject the conclusion that only a nuclear event could produce apparent excess heat.

| The fraction of heat captured by a calorimeter has no relationship to its
| accuracy.

Yes, it does. The CCS problem can produce larger excess energy signals with a calorimeter that captures less of the heat produced in it.

| A calorimeter is only required to be stable with respect to
| time.

That should read: A calorimeter is only required to be stable in time. The CCS problem is exactly that, a shift in stability of differing amounts at different times.

| Such stability is routinely demonstrated by repeated calibration
| over a period of time and is one factor on which the stated accuracy is
| based.

You can have 1000 shuttle flights problem free. It only takes one to recognize that something was missed.

| This statement is not true, as a reading of the papers published by Dr.
| McKubre demonstrates.

This refers to my claim that the McKubre results fall within the same range of effect as Dr. Storms results.

Actually, reading the papers will not illustrate the problem, since it is not addressed, and the raw data is never presented. What I commented on is the fact that I got the 1998 Technical Report, which had a CD with his data on it, and I analyzed that data in a standard fashion, i.e. the same way that Dr. Storms analyzes his data, as best I could with no separate calibration data presented. My claim that the signals presented as the strongest CF result in the 1998 Technical Report were a 2% effect are based on my analysis of the sensitivity of the excess peak height to changes in calibration constant. You won't find that anywhere but here, since I haven't published it. It isn't publishable, except perhaps as a small part of a larger paper, since I had incomplete calibration data dealing with the impact of mass flow rate changes.

Of course Dr. McKubre claims he has done CF. But I likewise claim he hasn't addressed the CCS issue, and his conclusion is premature (not barred, just premature).

| Dr. Shanahan assumes that the reported excess is not real and proposes a
| mechanism that he uses to explain the results.

a.) The 'mechanism' under consideration should preliminarily be limited to a CCS. How and why the CCS occurs is a subsequent discussion that doesn't impact the excess heat peak accuracy issue. Dr. Storms routinely packages the CCS problem with my proposals as to how a CCS could occur in a CF cell. This is unnecessary and misleading, because he uses the recognized lesser degree of certainty about 'how' to reject the fact that a CCS _can_ produce apparent excess heat. The correct response is to try to insure a CCS has not occurred in the run (which is a difficult problem).

b.) The process I use is called 'sensitivity analysis'. It is a well known practice. You take some data, postulate a problem, and work out what it would do to the conclusions if present. If you end up with a reasonable explanation, normally one has to consider the postulated problem in data workup. The 'reasonableness' of my explanation arises because the observed excess heat peaks are usually within reasonable error limits. It is only the blind devotion to the nuclear explanation that causes Dr. Storms to refuse to accept my analysis.

| This mechanism is based on
| a change in calorimeter behavior each time the current was raised or
| when the cathode was cleaned.

That is the observation, i.e. each time the current was raised or when the cathode was cleaned, the calorimeter behavior 'changed'. In fact there are clear patterns in the data showing the amount of CCS is related to time. That's why I tested the idea mathematically.

| This change was just enough to produce the apparent excess.

Wouldn't that be what you would expect?!? You expect that, if the change was real, it wouldn't produce a correlated change?!?

| The assumed mechanism has not been demonstrated to be real.

Yep. I don't have a CF setup. All I can do is look at your results with my eyes.

| Nothing I have proposed is based on my imagination.

You imagine the calibration to remain constant, in the face of an analysis suggesting it has shifted. You imagine there is a nuclear event occurring. You imagine your simplistic ideas about your experiment are totally correct. You imagine no one but your compatriots is capable of understanding the issues. ...

| All behavior
| is based on measurements, which are interpreted in conventional ways, in
| contrast to the proposals made by Dr. Shanahan.

The only difference between my approach and yours is the level of complexity assumed. You assume homogeneous calorimeters. I do not. Both our approaches are 'conventional'. Mine is just one step more complex. An increase in complexity of a model of behavior is warranted when the results of the simpler approach are confusing, and when the more complex approach yields clearer understanding.

Let me be explicit. Assuming the simple calorimeter equations used (which includes the apparently complicated ones Dr. Fleischmann uses) produces a conclusion that a revolutionary nuclear process is extant. Increasing the calorimeter model complexity a bit to include some heterogeniety, produces a 'normal chemistry/physics' conclusion. I would think it is obvious that here a little more complexity in the models is required.

| The number of observations for helium production and the presence of
| transmutation products is increasing as anyone can plainly see from the
| listed literature.

Yes, they do keep multiplying don't they. The simple explanation is that the newer authors are repeating the mistakes of the prior ones. This is a typical situation, and doesn't surprise anyone. What is a problem is that the group of you who claim 'transmutation' refuse to address the criticisms.

| A person no longer has to accept the distorted claims
| provided by skeptics such as Dr. Shanahan.

'Distorted'? My comments on transmutation results usually focus on alternative explanations of mass spectral peaks (or other types) that invoke multiatom fragments while the prime authors claim monatomic fragments. Multiatom fragments are routine in mass spectra. Ignoring them is a mistake.

My comment on He detection results are based on the recent work of the late Dr. Clarke, which seriously challenge the reliability of several CF workers' methods and results.

Dr. Storms needs to specify how I have made 'distorted' claims.

| The nuclear explanation has been explored by dozens of people with
| increasingly accurate and consistent results.

So was polywater and n-rays.

| This is the requirement of
| science in contrast to proposing trivial mechanisms based only on the
| imagination.

"Proposing trivial mechanisms based only on the imagination" is always the second step in the scientific process after making observations. The idea is to hypothesize, test, and refine. The initial proposals are almost always trivial, as the full complexity of the experiments is usually not understood until relevant factors' effects are defined one-by-one. Dr. Storms' statement is an attempt to use to derogatory adjectives to influence the reader.

| This is true. However, nature occasionally demonstrates behavior that
| does not fit conventional explanations. Therefore, novel explanations
| need to be explored without being rejected because mundane explanation can
| be imagined. A person needs to ask what benefit results from rejecting

| LENR before it has been given a fair hearing?

At some point, one has to 'get real'. Otherwise one wastes all his or her time in vain imaginings. This applies to novel and mundane explanations equally. If the novel or mundane explanations seem to have merit, then they should be investigated. But when _any_ explanation is rejected because the scientist doesn't _like_ it, then we have left science behind. The thing being rejected here without a fair hearing, i.e. experimental work aimed at verification/rejection, is the CCS explanation.

Referring now to my secondary proposals about how a CCS might occur in a CF cell:

| This behavior is not consistent with H2+O2 recombination because
| significant oxygen does not exist in the region of the cathode. Oxygen is
| produced at the anode and the resulting bubbles rise rapidly to the
| surface and are vented to the gas above the electroilyte. This fact is
| well known to anyone who has taken the trouble to observe an electrolytic
| cell. What is the point of proposing a mechanism that can not occur based
| bon simple observation?

| This proposed mechanism makes no sense and can be easily rejected by
| simple observation.

What amuses me in this is the extent Dr. Storms will go to to reject my proposal. First off, there either is or isn't good mixing in the cells. If there isn't, then their calorimetry is suspect, per Dr. Storms' own publications. If the mixing _is_ good, then what prevents some O2 bubbles getting to the cathode (or H2 to the anode for that matter)? Yes, buoyancy makes the bubbles rise, but there is also strong mixing.

The relevant simple observation is that a CCS can explain 'CF'. If a CCS occurs, then there must be a 'how'. I suggest one, which is rejected out of hand without a fair hearing. But there may be others! However, those others would also have to be consistent with all extant data, and the biggest piece of data we have so far suggesting that it's bubbles is the Szpak, et al photo. Right now, recombination at the electrode surface seems the most logical mundane process to me. I always admit I could be wrong, but I want some evidence of that, not speculation from someone with a vested interest in killing the idea off.

Referring to my comment about how the rejection of the CCS explanation demonstrates pathological science:

| This is not true. People doing CF research have considered all possible
| explanations, as a reading of the papers and reviews would reveal to any
| objective person.

There are no papers but mine dealing with the CCS. How could it then have been dealt with and rejected?

| What benefit does such a statement have in advancing a
| correct understanding of the novel proposals?

The benefit in my statement was to alert the reader to the pathological aspect of the rejection of the CCS explanation. One aspect of psuedoscience is that it tends to be presented in a fashion that will 'convince' one not familiar with the area. In other words, it is typically a smooth con-job. Someone who understands this then needs to alert the interested but uninvolved reader of the problem.

Referring to my comment on Dr. Clarke's work vs. that of Drs. Arata and Zhang:

| The statements made by Dr Shanahan are not correct, as a reading of the
| cited paper will show. Dr. Clarke found some He-4 in the Pd supplied by
| Prof. Arata, but much less than the amount claimed by Prof Arata.
| However, we now know that most of the He-4 leaves the metal and is found
| only in the gas, which Prof. Arata measured.

Dr. Clarke measured and compared He released from Arata's Pd black to Arata's results on He released from the Pd black by high temperature heating, and found significantly less He than Arata claimed. So, the immediate question a competent scientist asks is: Who's results do we trust? I prefer to trust the man who has a demonstrated track record in the field (trace level He measurement), who details his work methods carefully, and who doesn't use unique and questionable analysis methods. (I had previously commented on the lack of certainty I had about the Arata techniques, so this should surprise no one.) Thus I go with Dr. Clarke.

Subsequently then I cannot just accept without question the gas phase results from Arata's lab, since one interpretation of the situation is that there is a method error in Arata's work. Couple that to the upcoming Clarke publication (I hope, I'm not sure, given his untimely death) about how badly the SRI lab messed up the He analyses, and the reader should understand why any claims to have found He need careful, detailed verification.

| I suggest this distortion of
| the facts reveals a pathology of the skeptical approach.

The only distortion present is in the degree the scientific community should trust the claims of nuclear ash detection.

| A negative result proves noting about the reality of a claim, as a basic
| understanding of logic shows. All negative results, of which there are
| many, were omitted in the Guide because they reveal nothing about the
| subject. They only show that the effect is difficult to produce. All new
| effects are difficult to produce. Even heavier than air flight was
| difficult to produce until the Wright Brothers solved the problem.
| Skeptics rejected the whole idea of flight for many years, even after
| successful work was published, just as they are now doing with CF. Anyone
| wishing to understandthe style of the skeptical mind should read
| "Forbidden Science" by Richard Milton.

My contentions are always on the basis of reinterpreting 'positive' results. Essentially, my position, obtained after ~8 years of study, is that claims to have shown a nuclear event is active in anything classed as CF, CANR, LENR, or whatever, are premature. That _does_ say a lot about the quality of work to date.

| The hubris that this statement represents is amazing.

"Hu-bris, exaggerated pride or self-confidence."

Hubris has nothing to do with it. In no other paper in the extant literature is the CCS problem discussed. I contend it is present to some extent in all excess heat claims to have seen CF. It behooves the intelligent reader to be prepared to look for the evidence I claim is there as he/she reads the literature. It simply saves time to do it the first time through as compared to having to go back and look for it as I did.

| Dr. Shanahan would
| have the reader believe that his single analysis of one calorimetric
| measurement discredits all such measurements made by hundreds of
| competent and well-trained scientists at dozens of laboratories worldwide.

That was the point of the publication. I took an error that seemed to me to be in all the papers, and described and demonstrated it. Unlike the CFers, I don't need to say it a thousand times to convince myself it's true. Now it's up to the reader to understand what I wrote and test the literature against it.

| In addition, he leaps from this flawed analysis to a rejection of all

| measurements involving nuclear products.

No. The rejection of nuclear product detection has nothing to do with calorimetry. They are separate issues.

| What is the purpose of such an
| approach? What benefit results from rejecting a potential source of clean
| energy based on such flimsy evidence? Does such an approach strengthen
| science? This is for you to decide based on the provided evidence.

Right. The question is whether we, through government funding, or you, through private funding, should be throwing more money at the CFers, when they refuse to participate in the normal scientific process fully. In the past, that has always been a sign of an unlikely claim, and a warning to investors to avoid the arena.

| Edmund Storms, Ph. D.

Kirk L. Shanahan, Ph. D. {My opinion ... noone else's}

"Kirk L. Shanahan" wrote:

> There are no papers but mine dealing with the CCS. How could it then have been dealt with and rejected?
> Kirk L. Shanahan, Ph. D. {My opinion ... noone else's}

Mike Staker asks:

I am sure the above poster has considered that other scientists have thought of the CCS (calibration constant shift), but rejected it as not relevent to their work for a number of reasons. Since they did not publish comments about a CCS certainly does not mean that they did not consider it. I, for one, considered it in 1991. I had extensive discussion with a number of CF workers at the time about CCS, and other sources of potential heat artifacts. We pondered these at great length in the years of 1989-1992 and beyond to this very day.

His question should not have been, "how could it have been dealt with and rejected (beacuse they did not wait for me to publish **MY** paper)?" But rather why did they reject it and what evidence do they have for rejecting it?

The CCS has a big problem: in closed systems, for long periods (longer than the experimental run by an order of magnitude), if CCS occurs there will be some data above the average calibration curve and some below it, but on the average the calibration curve will catch these events and the final calibration curve will be in the middle. A CCS event, cannot by definition, raise the heat level and violate the 1st Law of Thermo. Therefore, it becomes a matter of being sure to run proper calibration curves. The above proposer of the CCS "theory" would have mother nature control the CCS event to provide a shift only during the "run" and not during the calibration.

For example in my experiments, my calibration data ran for 11 months before the 6 week Excess Heat (nuclear heat) and has continued to run for 1.5 years since. NO CSS during the calibration. or I should say it has been averaged in .... ?

Kirk, there is something causing excess heat beyond what you have proposed....

Good hunting...