

Responses to DoE Review Panel Comments

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May 21, 2008

In 2004, the Department of Energy (DoE) published a review of cold fusion. [1] It was based upon a review conducted by a panel of 18 anonymous members. The reviewer's comments are in Ref. [2].

This document lists some of the major claims made by the 18 reviewers, with responses by Jed Rothwell and Michael Melich.

Several reviewers made similar assertions, such as that cold fusion might be caused by a chemical reaction. Our rebuttals are in Appendix A.

This review is not exhaustive. Although we have listed some claims that we agree with, for the most part we have listed claims we disagree with, or at least quibble with. In other words, most of our comments are rebuttals. In cases where we agree with the authors, we have not enumerated the individual claims. We feel there is no need to do this, because many resources such as the papers and books by Mallove, [3] Beaudette [4] and Storms [5] lay out the case for cold fusion in detail, and they are more comprehensive and better organized than this collection of Reviews. Beaudette has also written a comprehensive response to the DoE review. [6]

Some reviewers wrote long critiques, speculation and suggestions regarding theory. This was sometimes prompted by one of the DoE charges to the panel, to determine whether cold fusion is a nuclear effect or not. We have not commented on these critiques, because cold fusion is an experimental discovery, not theoretical. Whether it can be explained by theory has no bearing on whether it exists or should be funded. We do not condemn or endorse these critiques; we consider them irrelevant.

Reviewer 1

Claim 1.1. "The references are taken overwhelmingly from conference proceedings (primarily the ICCF series) and other sources than cannot be regarded as peer-reviewed sources. In my view, the references are also culled to present a one-sided view of the current state of experimental results."

True. The references include ~25 journal papers, ~5 official publications from EPRI and other institutions with high standards, and ~110 conference papers, graduate theses and other non-peer reviewed sources. However, the references are not "culled" to present a one-sided view. If anything, skeptical doubts about cold fusion are over-represented. See Appendix A, Skeptics have published few papers.

Claim 1.2. “The 1993 Fleischmann and Pons article is included, which reported calorimetry experiments yielding excess heat in the several tens of W range. A large number of subsequent experiments have established upper bounds in the 0.1 W range.”

This is a normalization (comparison) error.

“Upper bounds” imply a comparison of comparables. Many experiments have produced excess power higher than 0.1 W normalized against Fleischmann and Pons (and also far more than 0.1 W in absolute power, ignoring normalization).

Comparisons of the performance of the heat producing experiments can only be sensibly made if there is some consistent normalization to either units of mass or units of volume for the heat producing system. Since it is not known if the entire volume of the palladium cathode is involved it has been the practice since 1989 to normalize all energy and power densities to the volume of the cathode. Thus, in their original paper Fleischmann and Pons reported their results in term of joules per cubic centimeter and also watts per cubic centimeter of their cathodes.

Claim 1.3. “A second paper (Mengoli et al.) is included which reports small power outputs, thus clearly contradicting the 1993 letter, but at a level still in conflict with other results.”

Mengoli reported far higher energy densities than previously, and more importantly, reported production of excess enthalpy in the absence of input power (heat after death). [7]

Different power levels do not “conflict” with one another. Large, highly loaded cathodes produce much more heat than small, lightly loaded ones. Cathodes range from ~0.01 g thin film to ~100 g bulk palladium, and loading is often uneven, so two cathodes of the same size may have different amounts of active material. Cathodes with problems such as cracks or surface contamination do not load and do not produce heat, for reasons that are now well understood, but which were not widely known in 1989.

Claim 1.4. “This field is 15 years old. It has been characterized by a large number of positive but internally inconsistent results, plus an even larger number of negative results refuting many of the claims. By in large those experiments done by experienced nuclear physics groups have been negative.”

See Appendix A. Data from newly discovered phenomena often seems inconsistent.

In 1989, in the U.S., 20 groups of experienced nuclear physics researchers unsuccessfully searched for neutrons, x-rays and other nuclear effects. By 1990 over 100 groups observed excess heat, tritium and other evidence of cold fusion, and the number now exceeds 200. The reasons the 20 nuclear physics groups failed are now well understood. Cold fusion seldom produces measurable levels of neutrons and other particles. These groups did not confirm that the cathode is producing excess heat, which is the principle signature of the reaction. It is unlikely that their samples were producing the cold fusion effect.

The fact that the field is 15 years old has no bearing on results or the validity of the claims. Experimental results never become obsolete. The reviewer appears to be making another normalization error, comparing only the number of calendar years, rather than man-years of effort. Fields such as semiconductors, plasma fusion, or high temperature superconducting have had far more man-hours and funding than cold fusion.

Furthermore, experimental results never become obsolete. No one suggests that plasma fusion does not exist or that it is “invalid” because it has still not achieved some of the project goals it was expected to reach decades ago.

Claim 1.5. “As many have said, extraordinary results require extraordinary proof. Such proof is lacking.”

This is not a principle of science. It was coined by Carl Sagan for the 1980 “Cosmos” television series. Conventional scientific standards dictate that extraordinary claims are best supported with ordinary evidence from off-the-shelf instruments and standard techniques. All mainstream cold fusion papers present this kind of evidence.

Conventional standards also dictate that all claims and arguments must be held to the same standards of rigor. This includes skeptical assertions that attempt to disprove cold fusion, which have been notably lacking in rigor.

Laplace asserted that “The weight of evidence for an extraordinary claim must be proportioned to its strangeness.” “Weight of evidence” is a measure of how much evidence you have, not how extraordinary it is. There is more evidence for cold fusion than for previously disputed effects. (For example, although there were a few hundred papers published about polywater, most were speculative, and only two labs reported success. [8])

Finally, the quality of being “extraordinary” is subjective. What seems extraordinary to one person seems ordinary to another. Many scientific phenomena that experts take for granted, such as quantum effects, seemed extraordinary when they were discovered, and still seem extraordinary to non-scientists.

Claim 1.6. “Existing results are erratic; many past results (excess tritium, charged-particle production, neutron bursts) have been demonstrated to be wrong and retracted. A partial summary of early retractions is given in Morrison's 1990 article.”

Few claims have been shown to be wrong, or retracted. Tritium has confirmed by over 100 laboratories; charged particles and neutron bursts occur rarely but they have been confirmed. Existing results are somewhat erratic, but this does not indicate they are wrong. See Appendix A, Difficulty with experiments, irreproducibility and erratic performance are not grounds to disbelieve a result

Claim 1.7. “It is impossible to prove a negative: that cold fusion does not occur at any level.”

This principle applies to theory, not experimental data. A result that is widely replicated at high signal to noise ratios and published in peer-reviewed papers is thereby proved to exist, and it becomes a permanent part of the literature. No widely replicated result has ever been shown wrong or withdrawn from the canon, although some are reinterpreted in light of new theory. How widely a result must be replicated before it is accepted depends upon the field and the difficulty of the experiment. The top quark experiment has never been independently replicated because it can only be done at Fermilab. Cold fusion has been replicated at hundreds of major laboratories. No other experiment in history has required more than a dozen quality replications at major institutions before being widely accepted.

Claim 1.8. “However, repeated retractions; erratic and inconsistent claims of the levels of cold fusion; positive results clearly in contradiction with other, negative ones; and clear evidence of careless or even fraudulent work (such as the MIT analysis of the Pons-Fleishmann gamma ray spectrum) have eroded all of this field's credibility.”

We dispute several of these assertions:

As noted above, the apparent “inconsistency” is a function of our present ignorance; nature is never inconsistent.

As noted, there have been few retractions.

Erratic performance is expected. As noted above, see Appendix A, Difficulty with experiments, irreproducibility and erratic performance are not grounds to disbelieve a result

Negative or failed experiments do not detract from positive ones; both can contribute to our understanding. Experiments that produce the cold fusion effect do allow the definition of necessary conditions. Experiments that test hypotheses help specify what does or does not work. The routine conduct of “matrix experiments” as a means to explore the parametric values that control experimental outcomes is a staple of research programs. Once it is established that a physical process exists, the optimization of experimental conditions to explore the pertinent parameters can proceed, producing both expected and unexpected results.

There is no evidence that the Pons-Fleischmann gamma spectrum was anything other than an honest mistake. Fleischmann admitted the error and retracted the results immediately. Furthermore, many other researchers who are more qualified to measure gamma rays have successfully done so.

Mistakes and retractions are a normal and necessary part of the scientific process. They do not “erode” the credibility of a field as a whole, or the credibility other claims that are not retracted. Even though Fleischmann’s gamma spectrum turned out to be incorrect, that does not call into question the gamma spectrum published by Mitsubishi years later.

Finally, even if the Pons-Fleishmann gamma ray spectrum was fraudulent, and not merely an honest mistake, this does not erode “all of the field’s credibility.” This reviewer is saying we should ignore all of the work conducted by thousands of scientists over 19 years because two researchers were wrong. A reviewer who expresses such

irrational, extreme bias is not fit to serve on a scientific review panel. This review should be stricken from the record.

Claim 1.9. “The experimental evidence for ‘cold fusion’ is unconvincing. Much of the work (including several of the papers included in the packet) is of poor quality, with inadequate descriptions of apparatus, a lack of error analysis, and data presented without uncertainties.”

Gerischer and other leading experts in electrochemistry who have looked at the evidence found it convincing. Skeptics have not published papers published showing errors in the major work, such as the experiments by McKubre or Miles. This reviewer has not pointed out technical problems with specific experiments, but has listed only generalities which apply to some of the papers but not others.

There are cold fusion papers of poor quality with inadequate descriptions, but this can be said for any field of science, and especially new and poorly understood fields. Most research is mediocre. A mediocre paper by one author does not detract from a brilliant paper by another. A mistake made by one researcher does not cast doubt on the work of another. Furthermore, the fact that Fleischmann made a mistake measuring gamma rays does call into question his ability to do electrochemistry or calorimetry. By the same token, the fact that 20 groups of leading experts in nuclear physics did cold fusion experiments incorrectly in 1989 does not reflect badly on their ability to do research in their own field of expertise. Both disciplines require years of training and experience. You cannot expect a nuclear expert to do electrochemistry right the first time, or vice versa. Cold fusion requires a multidisciplinary approach.

Claim 1.10. “The evidence does not demonstrate that a new phenomenon is occurring. I do not see a scientific case for continuing these studies under federal sponsorship.”

This panel member chose no evidence to criticize in detail. It is necessary to show some effort to deal with evidence before making any useful, supported, or falsifiable conclusion.

Reviewer 2

Claim 2.1. “Nuclear physicists have measured the rates of the $d(d, \square)^4\text{He}$ reaction, as well as those of the $d(d, n)^3\text{He}$ and the $d(d, p)^3\text{H}$ reactions. It is known that, when extrapolated to near zero energies, the rates of the (d, n) and the (d, p) reactions are about seven orders of magnitude larger than that of the $(d, \square)^2$ reaction. Therefore it follows that if the ^4He is being produced by the $d(d, \square)^4\text{He}$ reaction, there would be seven orders of magnitude more neutrons and protons compared to the number of ^4He nuclei produced.”

This is a misunderstanding. No one is claiming or has ever claimed that cold fusion works the same way as plasma fusion, or that it can be explained with conventional plasma fusion physics. The claim is that it produces heat far beyond the limits of chemistry without chemical ash, and it produces tritium, and helium in the same ratio to the heat as plasma fusion does. Therefore the *most likely hypothesis* is that it is a nuclear

reaction. See Appendix A, The nuclear hypothesis best fits the facts A hypothesis is not a theory, or an answer; it is guide to future research. Schwinger explained why we should not demand that cold fusion resemble plasma fusion:

“The next item of the hot fusioner’s creed are responses to suggested cold fusion mechanisms: (5) Very soon after 23 March, 1989, it was proposed that excess heat is produced by the formation of ground state ^4He in the DD fusion process. Response: Where is the accompanying γ -ray of roughly twenty million electron volts? . . .

The defense is simply stated: The circumstances of cold fusion are not those of hot fusion.”

Claim 2.2. “It is hard to imagine how 23.8 MeV of excitation energy, nearly 9 orders of magnitude more than in the case of the Moessbauer effect, could be coupled to and transferred to the phonons of the lattice!”

See Appendix A. Theoretical objections to experimentally proven facts are a violation of the scientific method and A reviewer’s inability to imagine or understand a result is not a valid reason to reject it.

Claim 2.3. “The excess heat reported remains unexplained. However, in my opinion, there is no evidence for this being a nuclear physics phenomenon.”

As noted above, see Appendix A, The nuclear hypothesis best fits the facts

Reviewer 3

Claim 3.1. “This reviewer has some criticisms about the content of the paper. First, the results presented as evidence for the existence of the various conclusions about the Pd/D system are mostly from the SRI laboratory of one of the authors. While other results are referenced and in some cases mentioned in the text, the case for the existence of LENR would have been strengthened by demonstrating reproducibility using the results of other investigators and laboratories.”

Hagelstein et al. acknowledged this. They concentrated on their own work because they felt most competent to discuss it in detail. They hoped that panel members would study other researchers, but they felt it would be best for the members to read original sources by those researchers.

Claim 3.2. “The authors apparently elected not to discuss the reported cases where explosions have occurred with these systems.”

These explosions are important, but they are rare, and cannot be reproduced on demand. They are not well documented, because they were unexpected, unplanned for, and they destroyed the instrumentation in most cases.

Claim 3.3. It would be much easier to accept LENR as the phenomenon responsible were it not for the variable results introduced by these other metal-deuterium systems.

For example, were excess heat and ^4He the only observed products, accepting LENR, with the mechanism of $\text{D} + \text{D}$ to give heat and helium-4, would make sense. But the fact that ^3He , T, and protons are reported by some investigators makes the acceptance of LENR much less comfortable. The suggestion that the experimental conditions affect the mechanism is the authors' explanation, but this would suggest that LENR in metal deuterides is an effect which occurs routinely in such systems. If so, an explanation as to why these effects were not seen in the myriad of studies of metal-deuterium systems before would be required."

These objections make no sense. This first is like saying that it would be much easier to believe in transistors if all samples of silicon produced the transistor effect, and they always produced it exactly the same way under all operating conditions. The "suggestion that the experimental conditions affect the mechanism" does not imply that the effect "occurs routinely in such systems." One can cite any number of counter-examples, to but to take a related phenomenon: plasma fusion is affected by experimental conditions; it does not occur in all samples of plasma; and it does not occur routinely, but only under special conditions which are difficult to achieve.

Claim 3.4. "This reviewer's conclusion is that the Pd/D system is far from being understood and that some challenging and potentially new phenomena are being observed in high loading experiments with the system."

We expect that all cold fusion researchers agree.

Reviewer 4

Claim 4.1. (Statement referencing a paper by Fleischmann and Pons) "The conclusion I would draw is that greater attention to a more rigorously carried out experiment is needed, not just more complex analysis of the more or less standard cell in order to adequately establish these unusual measurements and conclusions."

The method used by Fleischmann and Pons is highly rigorous and accurate. It has been reviewed and replicated by Miles and other experts. It is not unusual; as Fleischmann pointed out, it is a variation on the method developed by J. P. Joule in the 1840s, and it has been used extensively. Many other methods of calorimetry, such as flow and Seebeck, have also been used in these experiments and they have confirmed these results. The conclusion, that excess heat is produced, is not unusual: it is inescapable. Any normal calorimeter producing these results will compel this conclusion. (In other words, there is nothing exotic or unusual about the calorimetry in Fleischmann's experiment, although other cold fusion experiments have employed non-standard instruments and techniques.) The implications of the conclusion are unusual.

Claim 4.2. (Statement referencing Mengoli et al.) "The conclusion I draw from these experiments is that considerable improvement in experimental technique and instrumentation has been developed. Greater attention to the publication of details strengthen the conclusion that excess enthalpy has been developed in the cells of D/Pd."

Correct. This can also be said of Fleischmann's later experiments with improved, more expensive equipment (which were even better than the already high standard of the early work), and of experiments by McKubre, Kunimatsu, Storms and other well-funded, mainstream researchers. In our opinion, the need for improved equipment justifies funding this field. Some of the reviewers have the opposite view: the experiments should not be funded because top-notch, expensive equipment has not been used heretofore.

Claim 4.3. "The suggestion of a nuclear process is certainly unsubstantiated from the present experiments; no ^4He was looked for or detected nor were other nuclear reactions considered . . ."

Correct. Mengoli has focused on confirming and improving excess heat detection.

Claim 4.4. "I conclude from this article that co-deposition techniques are an alternative to solid Pd cathodes but the experiments with the Fleischmann-Pons type apparatus continue to lack convincing instrumentation and interpretation noted nearly a decade ago."

We agree that co-deposition is a promising technique. The statement regarding the "lack convincing instrumentation and interpretation noted nearly a decade ago" refers to the review by Morrison, which in our opinion has no merit. See Fleischmann's response to Morrison.

Claim 4.5. (Regarding Jones.) "My conclusion from this article is that as an initial experiment substantial care was exercised both in the specimen preparations and loading and with the nuclear detector qualification, background evaluation and control, and data analysis. Independent measurements would be highly desirable coupled with analysis of the foil material to assess the initial quality, the final damage and the isotopic content before and after the experiments."

[JR not competent to comment]

Claim 4.6. (Regarding theory papers by Hagelstein and Kozima) "My conclusions are that this work, published at the same meeting as the SJ article is somewhat self serving since the TiDx alpha particle measurements were known at this time and neither paper references the other. However, the approach to suggest that nuclear physics may undergo modifications in the solid state is certainly interesting. Although the set of conjectures are far from a coherent theory they do present insight into the experimental results. . ."

See Appendix A, Cold fusion is an experimental finding, so you cannot disprove it by showing errors in theories that attempt to explain it.

Claim 4.7. “If this temperature variation is important then experiments to go beyond the boiling temperature of D₂O would seem a logical step as mentioned (for different reasons) but apparently not accomplished by M. Fleischmann.”

This is a good suggestion.

Claim 4.7. “Some repetition of the direct nuclear process measurements would seem to be in order, combined with isotopic assessment of the cathode material before and after an experiment that produced excess energy. There are enough failures of the experiments to produce excess energy that efforts to understand the differences should be made by better characterization and documentation of the experimental components. My gut feeling is that unless a substantial effort is made in a laboratory equipped and funded to carry out the metallurgy, chemical and material analysis, nuclear instrumentation and experimental design and analysis, etc. there will only be more conference reports without much advancement in the understanding of the phenomena.”

We agree with these conclusions. However, a substantial effort will require substantial funding, which unfortunately has not been available because of academic opposition to the research.

Reviewer 5

Claim 5.1. “With respect to the section on Excess Heat I was disappointed that the review described some more sophisticated versions of the original Fleischman-Pons experiment but basically it seems to be ‘more of the same’ of this type of research.”

We disagree. “More of the same” better describes the plasma fusion program. Even though researchers are working on shoestring, they have made substantial progress since the original experiment. See Appendix A, Researchers have made great efforts to find systematic errors and conventional explanations

Claim 5.2. “At the end of the review they state that the ‘scientific questions posed by these experiments are, in the opinion of the authors, both worthy and capable of resolution by a dedicated program of research’. There are no specific plans offered for this ‘program of research’ that might elucidate the validity of CF but the implication is that if money were given for CF research we would get ‘more of the same’. The absence of specific research plans is a serious drawback to their proposal.”

Drawing up a detailed set of specific proposed experiments is, in itself, an expensive and time consuming process. It is unreasonable for this reviewer to expect Hagelstein and McKubre to do this without funding.

See Appendix A, Underfunded research cannot be expected to produce elaborate and expensive results

Claim 5.3. “It seems to me that the authors should have had a section which carefully addressed the many cogent arguments offered in the literature tending to discredit the

existence of CF. For example, K. Shanahan (Thermochim. Acta, 387 (12002) 95) [9] has argued . . .”

In our opinion, Shanahan’s arguments have no merit. There have not been “many cogent arguments . . . to discredit the existence of CF” in the literature. There have been only three main ones and few minor ones: chemical storage, recombination, and artifactual low-level heat. Minor arguments include positional effects, the Seebeck effect with electrode leads and a few others that have been shown to be many orders of magnitude too small to explain the effect. See Appendix A, Chemical storage hypothesis, Artifactual low-level heat hypothesis, Recombination hypothesis and Skeptics have published few papers.

Claim 5.4. “In *New Physical Effects in Metal Deuterides* by Hagelstein, *et al.* [10] it is pointed out on page 3 that ‘in no case was a calorimetric imbalance observed (19 examples) where an electrode failed to achieve a bulk average D/Pd loading of 0.90. However, all electrodes achieving a loading of 0.95 or greater (15 examples) exhibited an heat excess more than 3 times the measurement uncertainty’. If this is the case, then I fail to understand why gaseous loading of Pd with D using up to 3.1 GPa of D₂ to achieve D/Pd ratios greater than 0.95, does not lead to CF. Baranowski and coworkers (J. Less-Common Mets., 158 (1990) 347), who have had extensive experience with high pressure loading of metals with H₂ and D₂, failed to observe any evidence of excess heat in a system which is inherently simpler than the electrochemical ones.

[It is not clear whether Baranowski et al. have looked for excess heat in these systems.]

There have been reports of heat with gas loading, but if there is no heat in a given system, then evidently the high pressure system process that produces different results, for reasons that are not clear. Schwinger’s defense “the circumstances of cold fusion are not those of hot fusion” can be restated “the circumstances of electrolysis are not those of high pressure loading. Perhaps this is because high pressure loading is “inherently simpler.”

More to the point, “I fail to understand” is not a valid scientific reason to doubt an experimental result. See Appendix A, A reviewer’s inability to imagine or understand a result is not a valid reason to reject it.

Claim 5.5. “It should be kept in mind that the bulk phase Pd doesn’t care where the D comes from once it is within it!”

This is incorrect. Different methods of loading deuterium into palladium have profoundly different consequences and effects. For example, many methods cause swelling and cracking, which prevent the cold fusion reaction from occurring.

Claim 5.6. In “*New Physical Effects in Metal Deuterides*” by Hagelstein, et al there are 130 references and only 2 of them are not directly from favorable CF literature. This illustrates the rather narrow focus of these researchers.”

We believe this illustrates the fact that skeptics have not published many papers. See Appendix A, Skeptics have published few papers.

Reviewer 6

Much of this review is devoted to refuting existing cold fusion theory. See Appendix A, and Theoretical objections to experimentally proven facts are a violation of the scientific method

Claim 6.1. “Clearly the data described in the position paper is not consistent and systematic.”

See Appendix A. Data from newly discovered phenomena often seems inconsistent.

Claim 6.2. “Furthermore the scientists quoted do not spend enough effort searching for conventional causes of the phenomena claimed or for systematic errors in the measurements.”

This is incorrect. See Appendix A, Researchers have made great efforts to find systematic errors and conventional explanations

Claim 6.3. “Little has changed in Cold Fusion from the publication of John R. Huizenga's book “Cold Fusion: The Scientific Fiasco of the Century”, U. of Rochester Press, Rochester, New York (1992).”

Huizenga's book was out of date and incomplete when it was first published. It did not mention most of the important peer-reviewed research that was already published by 1992, and the revised edition does not include any new information.

Claim 6.4. “Cold fusion is inconsistent with a huge body of knowledge about nuclear processes developed over the past 70 years. Three miracles are required for “Cold Fusion” as described to occur. These are: The Fusion Rate miracle . . .”

See Appendix A. Theoretical objections to experimentally proven facts are a violation of the scientific method, A result need not be explained theoretically before it can be believed and Cold fusion is an experimental finding, so you cannot disprove it by showing errors in theories that attempt to explain it.

Claim 6.5. “Excess power is measured rather than heat.”

The meaning of this is little unclear. The reviewer probably meant that power rather than net energy is reported. Assuming that is what this means, the statement is incorrect: researchers report absolute power, power normalized to the mass of palladium, and energy. Absolute power is a good indication of the likely signal to noise ratio; high power is more easily measured.

Claim 6.6. “This quantity is prone to anomalies due to several likely causes. The storage of hydrogen gas in the Pd electrode being release and combined with oxygen from the anode could cause excess heating [recombination].”

Skeptics have not cited “several” likely causes. They have cited three: an error in calorimetry, chemical storage and recombination. All three have been ruled out. See Appendix A. Chemical storage hypothesis and Recombination hypothesis.

Claim 6.7. “The power level is determined assuming a steady state heat flow. Changes in heat conductivity due to the dynamics of gas flow can result in temperature excursions.”

Storms discussed these problems in detail, as did Fleischmann, Miles and others.

Claim 6.8. “The amount of excess heat claimed is small compared to the energy put into the system. For example take the experiment of Fleischmann and Pons, Phys. Lett. A, 118-129(1993). A palladium electrode was operated at 4 Volts with a current of approximately 0.4 Amperes for six days. The total energy input was therefore 830 kJ and the integrated excess heat reported was 26 kJ, 3 percent of the input.”

The overall energy input into the system is irrelevant. See Appendix A. Chemical storage hypothesis and Artifactual low-level heat hypothesis. The excess heat power level is sometimes three times higher than power, and in some cases there is output power only with no input power.

Claim 6.9. “The estimate of power generated by the palladium electrode is based on an equation that describes the balance heat flow in by electrolysis and heat flow out by either radiative transfer or advection of gas. . . .”

This critique only applies to one type of calorimeter, used by Fleischmann and Pons. The effect has been confirmed with many other calorimeter types, so it cannot be an artifact of this calorimeter design.

Claim 6.10. (Regarding the SRI Case experiment) “The authors attempted to discredit this explanation [helium contamination from the atmosphere] by asserting that the container was helium leak tight. Presumably this was based on the ability to hold hydrogen. I don't see how the apparatus could be guaranteed leak-tight without a helium leak check.”

The authors disproved this explanation by pointing out that the helium concentration in the cell exceeded atmosphere. Also they did perform a helium leak test.

Claim 6.11. “The Arata and Zhang experiment was another variant on the Case experiment. In this experiment a hollow palladium cathode is loaded with deuterium by

electrolysis. Their experiment claims to have found excess power, tritium, and ^3He . The ^3He is undoubtedly from the decay of the small amount of tritium present in heavy water.”

This explanation fails by 3 orders of magnitude.

Claim 6.12. “The tritium observed was 2 to 5 x10E15 atoms more than 4 orders of magnitude smaller than should be expected from reaction 1(b) in table 1.”

It is common knowledge that the level of tritium produced by cold fusion is orders of magnitude lower than the level produced by plasma fusion. Expectations have no role in evaluating experimental results. See Appendix A. Theoretical objections to experimentally proven facts are a violation of the scientific method and A reviewer’s inability to imagine or understand a result is not a valid reason to reject it.

Claim 6.13. “Note the article by McKubre et al. misstates Arata and Zhang as having observed ^4He production.”

This is incorrect. Arata and Zhang did report helium-4.

Claim 6.14. “Exposing or disproving experimental artifacts is far more difficult than generating them.”

See Appendix A, Undiscovered error hypothesis

Claim 6.15. “Better experiments could be done, however.”

Better experiments cannot be done because funding is not available. See Appendix A, Underfunded research cannot be expected to produce elaborate and expensive results

Funding is not available because scientists such as this reviewer oppose cold fusion for irrational, unscientific reasons.

Reviewer 7

Claim 7.1. “I find the large number of different experimental methods that have been applied to the cold fusion problem to be very impressive. However, one aspect of cold fusion studies has not changed, namely that the field crosses the boundaries of several rather different scientific areas, such as chemistry, electrochemistry, thermodynamics, solid state physics, hydrogen storage in solids, and nuclear physics. It is difficult to find scientists who are knowledgeable in all of these areas – either cold-fusion practitioners or peer reviewers. As I note in a few examples in section II below, I have the impression that in some instances, cold-fusion experimenters are not as expert as they should be in the methods that they have chosen to use.”

We agree. We also feel that these problems can only be ameliorated by funding the research properly.

Claim 7.2. “I find it a bit disheartening that, despite the efforts of the dedicated researchers in this field during these past fifteen years, they have been unable (a) to completely solve the nagging problem of the nonreproducibility of the experimental results, or (b) to elucidate and/or nail down all the important parameters involved in the proposed cold-fusion phenomena (plural nuclear mechanisms have been proposed) or (c) even to convince the broader scientific community that cold fusion is real.”

Reproducibility and the other problems listed by this reviewer can only be fixed with proper funding. See Appendix A, Underfunded research cannot be expected to produce elaborate and expensive results. It is impossible to do this research adequately on a shoestring. Members of the “broader scientific community” are not convinced because they have not read the literature.

Claim 7.3. “From the earliest report of Pons and Fleischmann, the statement was made that the observed excess heat could not be explained by known chemical effects (or by extension, of known solid-state effects). Therefore it was concluded that by default, any excess energy release had to be the result of unknown nuclear processes . . . So far as I can tell from my reading, there have been few attempts to search for evidence of unknown non-nuclear processes, either chemical or physical, to explain the results of cold fusion experiments.”

This is not a “default” explanation. An unknown nuclear reaction fits the facts better than an unknown chemical reaction. See: Appendix A. The nuclear hypothesis best fits the facts and Chemical storage hypothesis. If this reviewer can suggest a candidate chemical reaction that produces thousands of electron volts per atom with no measurable levels of chemical ash, he or she should do so. The reviewer did attempt to explain transmutation effects, in the next claim.

Claim 7.4. “The paper by Iwamura et al. presented at ICCF10 (Ref. 47 in DOE31 [11]) does an exhaustive job of using a variety of modern analytical chemistry methods to identify elements produced on the surface of coated Pd cold-fusion foils. There are two very unusual aspects of this work: (i) The energy source is gas pressure, permeation of D₂ gas through the foils into vacuum. (ii) The claim is made that if Cs is coated on the metal surface, it is converted into Pr; if Sr is coated on the metal surface, it is converted into Mo. The analytical results, from a variety of techniques, such as mass spectroscopy and electron spectroscopy, are very nice. It seems difficult at first glance to dispute the results. However, the Japanese workers conclude, not that the elements in question are constituents from the interior of the Pd that migrated to the surface, but that they are the products of sequential nuclear reactions, in which changes of atomic number and atomic mass of 4 and 8 are preferred.

From a nuclear physics perspective, such conclusions are not to be believed.”

See Appendix A. Theoretical objections to experimentally proven facts are a violation of the scientific method. Iwamura's results have not yet been widely replicated at a high signal to noise ratio, but if they are they will then become indisputable facts. We will be forced to say that from the perspective of these conclusions, nuclear physics are not to be believed, and must be revised.

This reviewer was one the only skeptic in this group who offered a falsifiable hypothesis to explain the results without resorting to the nuclear hypothesis: “. . . that the elements in question are constituents from the interior of the Pd that migrated to the surface.” Storms and Rothwell pointed out:

“The anomalous element could not migrate from the interior of the palladium because:

1. Deuterium atoms, flowing from the surface to the interior, would cause diffusion of the anomalous element away from the surface, not toward the surface.
2. Mass spectroscopy done at various depths shows that the anomalous element was not present in the palladium.
3. The element that was originally on the surface disappears at the same rate as the anomalous element appears.
4. The isotopes of the anomalous element are unnatural, and the isotope shifts are exactly what are expected should the missing element transmute into the new element

Since the initial element disappears, if migration is the cause of the change, we have to postulate that the element applied to the surface migrates toward the interior, while the anomalous element migrates in the opposite direction toward the surface. Such explanations are mere handwaving, and violate as many expected behaviors as does cold fusion but in a different field of science. . . . Iwamura's data certainly justifies further study. The proposed theories, regardless of their source (including the reviewer's own hypothesis), are irrelevant.”

Claim 7.5. “I note here two examples that I came across of results from cold-fusion experiments that may be indicative of processes that are not nuclear in origin. (1) Lipson et al. in a paper presented at ICCF10, noted the emission of intense low-energy x rays from Ti at ~1.4 keV when they established a deuterium-gas glow discharge with a Ti cathode at low voltage, 0.8-2.5 kV. They were able to image the x-ray emission using a camera obscura. Although these authors claim to have observed 3-MeV protons in solid-state plastic track detectors (an established nuclear detection technique, especially for heavy ions), I feel compelled to note that such low x-ray energies are really not characteristic of nuclear phenomena but of transitions in the extra-nuclear electron shells.”

[JR not competent to comment]

Claim 7.6. “(2) Arata and Zhang (Ref. 65 in DOE31 [12]) showed electron micrographs of Pd-black, highly deuterated and non-deuterated, and commented that it was ‘startling that no crystal damage seems to exist’ even though ‘the highly deuterated

sample was heated to a very high temperature and generated huge excess energy.’ However, it was noted that the micrographs showed different particle shapes for the deuterated and non-deuterated samples. This paper raises the following question: Have systematic structural studies been done of the structures of metal electrodes before and after the proposed coldfusion reaction had been completed, with light hydrogen and with deuterium, to look for bulk damage of the metal, and to determine any changes in the locations of the metal atoms (e.g., by x-ray diffraction) and of the deuterium atoms (e.g., by neutron scattering) in the lattice?”

[This is an interesting question, but it does not seem “indicative of processes that are not nuclear in origin” since nuclear processes can cause bulk damage, sintering and so on. JR does not know answer.]

Claim 7.7. “Playing the Devil’s Advocate, I might also ask the proponents of this research the following questions: (1) ‘If a cold fusion program were to be funded, what would you propose to do that is new, that you have not already done during the past fifteen years?’”

Cold fusion researchers have outlined many experiments that should be funded, such as replications of the Iwamura experiment that this reviewer discussed. Also, as we noted in response to Claim 5.2, drawing up a detailed set of specific proposed experiments is, in itself, an expensive and time consuming process. As noted already, it is unreasonable for this reviewer to expect researchers to do this without funding.

Claim 7.8. “(2) What do you hope to learn that is significant, scientifically (and for society)?” This latter question is especially important, I think, because we cannot ignore the fact that from its beginning, cold fusion has not simply been advertised as a possible new scientific phenomenon that requires elucidation, but as a potential, limitless supply of energy.”

Cold fusion has not been “advertised” as a possible new phenomenon and a potential limitless supply of energy. Every salient fact about it proves that it *is* a new phenomenon and it *is* a potential limitless supply of energy. If it can be controlled, there is no question it can become a supply of energy. By 1992 it already reached temperatures and power density equivalent to a fission reactor core, and cold fusion reactions have already produce hundreds of times more energy than the largest plasma fusion reaction in history, at PPPL. (The PPPL produced 0.6 MJ at roughly 10 MW, whereas some cold fusion reactions have produced hundreds of megajoules, albeit at around 100 W.)

Claim 7.9. “One must guard against the hype (and the political pressures) that have accompanied the cold fusion debate.”

This is not hype. If anything, the revolutionary potential for cold fusion has been understated, and the difficulties of developing it have been overstated. Based on previous research and development into similar solid-state surface effects and catalytic effects, such as solid state radar systems, it is reasonable to estimate that cold fusion can be

developed at a modest cost, perhaps \$300 to \$600 million, even if the underlying physics of the reaction cannot be fully explicated. The potential benefit to society far exceeds these costs.

Claim 7.10. “Even if cold fusion were to turn out to be real, it must be realized that a phenomenon that so far has been observed sporadically and often in a non-reproducible, i.e., uncontrolled, manner, will not easily be transferred into the marketplace.”

It is much too early in the development of this technology to conclude that it can or cannot be controlled and easily transferred to the marketplace. Cold fusion research is far closer to practical application than plasma fusion, even though cold fusion research has been done a shoestring against enormous political opposition, and plasma fusion has been lavishly funded for 60 years. Cold fusion is also much less sporadic and more controllable than semiconductor effects were in 1948, which was roughly 20 years after research in semiconductors began, and just before breakthroughs at Bell Labs led to the development of the transistor.

Reviewer 8

We agree with all claims made by this reviewer, especially this one:

Claim 8.1. “Most scientists simply aren't good enough to do extremely demanding experiments in every aspect of the research -- and highly deuterided palladium seems unwilling to cut us a break at any stage.”

Additional funding is essential to bringing more skilled workers to this field.

[SHOULD WE ENUMERATE OTHER CLAIMS?]

Reviewer 9

We agree with all claims made by this reviewer.

Reviewer 10

We agree with many of the claims made by this reviewer.

Claim 10.1. “In the years since the report numerous other calorimetric experiments have been undertaken, with either the original PF Pd/D₂O materials or with others, in a number of different calorimeter arrangements. Many of these experiments, like those at SRI, have involved closed cells, with a great deal of care taken in the design and calibration of the calorimeters. These closed systems, in which the electrolysis products recombine in the cell, especially when used with a flow calorimeter, are easier to understand and less prone to systematic error. The SRI experiments have also been undertaken to assess the effect of variables (e.g. level of Pd loading by deuterium, current density) on the observation of excess heat effects.”

This is correct, and it is a key point. It is unfortunate that other reviewers such as #1 and #5 failed to recognize that the experiments have been changed and improved since 1989.

Claim 10.2. “Most of the reported excess heat affects are reported as excess power (in W or W/cm³) at a given time. However instantaneous excess power is not the real issue, even in closed cells, since it does not account for possible accumulation of materials that later recombine.”

This is correct. Instantaneous excess power is not proof that cold fusion is occurring. It is quoted to show that the signal to noise ratio is high and the result are significant. The overall energy output during a continuous heat event proves that the effect is beyond the limits of chemistry. (As explained in Appendix A, Chemical storage hypothesis.)

Claim 10.3. “For example, certainly at high current density, Li metal can plate out on the cathode and later react with oxygen. Even if such reactions don’t occur, it is unlikely that the electrochemical cell is at a true steady state during the whole time of operation, so the excess energy (J), which involves integration of the power with time from the start of the electrolysis, is more meaningful, but not often given. It is also misleading to report results in terms of volume or moles of the Pd cathode, e.g. W/cm³ or MJ/cm³ or W/mol or MJ/atom Pd, since there is no evidence that the effect scales with electrode volume or weight.”

Overall energy input from the start of electrolysis is meaningless. You need only measure excess energy production during a single uninterrupted event to prove that the heat is beyond the limits of chemistry. See Appendix A, Chemical storage hypothesis.

The energy is normalized to the mass of the cathode to prove that the effect exceeds the limits of chemistry.

Claim 10.4. “An important point made from the calorimetric experiments is that the excess energies observed cannot be accounted for by chemical processes, e.g. by build up of some chemical reactant during the charging phase (where small endothermal changes might not be detected but are significant over time) followed by the triggering of a discharge process where a heat burst is observed. If this is true, then nuclear processes must be invoked.”

This is true, and it is why nuclear processes must be invoked.

Claim 10.5. “The levels of excess energy obtained in the SRI experiments were modest, ranging from 0.01 to 1.12 MJ (representing 0.2 to 3.9% of the input energy). Some of the results, e.g., cell P1a that produced 0.07 MJ (2.1% of the input energy), might be accommodated by a chemical reaction. For example, if one assumes that Li metal plates on the cathode and later reacts with oxygen from the anode, the reaction enthalpy would be roughly 300 kJ/mol Li, so this amount of energy would require about 1.6 g of Li.”

A 0.07 MJ reaction is modest, but many others recorded during continuous production are far beyond the limits of chemistry.

Claim 10.6. “However for levels of excess energy in the 1 to 10 MJ region (or more), as reported in a number of studies, it is difficult to find a chemical explanation.”

It is impossible to find a chemical explanation. No such explanation has ever been proposed by a skeptic, and one would violate more laws of physics than a nuclear explanation. See Appending A, Chemical storage hypothesis

Claim 10.7. “Either one must explain these results by flaws in the measurement or seek a nonchemical (i.e. nuclear) explanation.”

Flaws in the measurement technique are ruled out because many different measurement techniques have been used successfully; and most of the techniques have been in common use since the mid-19th century. See Appendix A, Undiscovered error hypothesis.

Claim 10.8. “A nonchemical explanation is the only logical choice left.”

A nuclear effect is the only known candidate, but it is conceivable that some source of energy unknown to science has been discovered.

Claim 10.9. “The reports of heat evolution after the current has been turned off clearly indicate that some storage process is occurring, but do not necessarily negate a chemical storage process.”

The reports of heat evolution after the current is turned off (so called heat after death) do not indicate there is storage, for the same reasons heat during electrolysis cannot be chemical: because some of these events produce heat far beyond the limits of chemistry, and no chemical ash is ever discovered after these events. See Appendix A, Chemical storage hypothesis.

Claim 10.10. “[Heat after death] and are indeed difficult to understand in terms of a nuclear process if high loading and pressure is needed).

High pressure and loading are needed to form the nuclear active material with bulk palladium. Once this material is formed, it stays at the necessary level of loading for extended periods of time in some cases, and produces the cold fusion effect the whole time.

Claim 10.11. “The rather low temperatures attained in most cases also suggest that the electrochemical approach has doubtful applicability for practical energy (heat) generation.

[This is incorrect. High temperature electrochemical processes are common.]

Claim 10.12. “In a general summary of the calorimetric results, the observation of sudden and prolonged temperature excursions (bursts of excess heat), has been made a sufficient number of times that, even if not totally reproducible, still have not been

explained in terms of conventional chemistry or electrochemistry (a conclusion also made in the 1989 ERAB report). However the systems are sufficiently complicated, the measurement sufficiently difficult, and the effects sufficiently small, that it is difficult to conclude from these effects alone that nuclear processes are involved.”

See Appendix A, The nuclear hypothesis best fits the facts

These are not necessarily temperature excursions. They are excess power events. In some cases, with some calorimeter types, the temperature of the cell is held constant during these events by reducing auxiliary joule heating power.

The systems are not complicated or unusual from the electrochemical point of view, and similar measurements at these power levels have been made with confidence for 150 years. The effects are not small, except in comparison to the overall energy input for the entire experiment, and this overall input is meaningless, as explained in Appendix A, Chemical storage hypothesis

Claim 10.13. “Even with all of the careful work that has been done on electrochemical cells and calorimetry, the system is still not under experimental control, in the sense that one knows exactly the materials needed and the operating conditions to get the same results, even semiquantitatively, every time.”

This is true, and it is one of the reasons substantial additional funding is needed for the research.

Claim 10.14. “Another problem with the proposal of ^4He as the major product, as is recognized in the review, is that the proposed D-D branching ratio must be assumed to be very different from that in previous studies of deuterium fusion and the absence of gamma rays, which would accompany this route, must be explained.

It does not have to be explained; it only needs to be proved experimentally. See Appendix A, A result need not be explained theoretically before it can be believed.

Claim 10.15. “For example calorimetry with anodes that can oxidize D₂ (fuel cell anodes), if they can be made to operate in closed cells at the needed current densities, especially with a cell resistance minimized by close spacing of anode and cathode, would be interesting.

This approach is interesting. It has been used successfully by Kunimatsu.

Reviewer 11

We agree with most of the claims made by this reviewer, except for the following:

Claim 11.1. “There is strong evidence of nuclear reactions in palladium, and suggestions of reactions in the titanium foil experiments. The body of evidence does not rise to the level of being conclusive at this time. What is required for the evidence to be conclusive is either a testable theoretical model . . .”

See Appendix A, The nuclear hypothesis best fits the facts and A result need not be explained theoretically before it can be believed

Claim 11.2. “. . . or an engineering demonstration of self powered system that continues to produce heat without an external power supply such that the device would appear to be a perpetual motion machine if not for the nuclear reaction.”

This requirement has already been fulfilled. Cold fusion reactions without an external source of power have often been observed, in heat after death and gas loaded devices.

A scaled-up engineering demonstration is not needed. Calorimetry is sufficient. It has been well established for 150 years and in order to disprove it one would have to disprove the laws of thermodynamics. In other words, cold fusion experiments both with and without input power are already:

Nuclear, or

Perpetual motion machines, or

Powered by some completely unknown source of energy

The first hypothesis seems most likely. See Appendix A, The nuclear hypothesis best fits the facts

Reviewer 12

We agree with most of the claims made by this reviewer.

Experiments involving excess power/heat. More careful experiments have been done in recent years (e.g. SRI work). There seem to be increasing evidence for the production of excess heat, even though the reason is totally unknown. Reproducibility has been improved, but it still has not reached a satisfactory level. Yes, it is likely that an unknown process (in materials physics or in nuclear physics) is responsible. However, the link to nuclear reaction is still not strong enough at the present time.”

We agree that it is not certain the reaction is nuclear, but this is the most likely hypothesis. See Appendix A, The nuclear hypothesis best fits the facts

Claim 12.1. “Progress has been made in characterizing the Pd electrode over the past 15 years, but more needs to be done to better understand the sample properties. In other words, materials problems need to be addressed, as well as the physics and chemistry of metal deuterides.

It would be crucial to have independent verifications of the “charged-particle emission” from metal deuterides. In other words, more careful measurements are needed to sort out the proposed “screening” effect.

Good theoretical studies on the behavior and interactions of deuterium in metals are also needed.

Very few exist at the moment.”

We agree with all of these points. The studies this reviewer recommends will require substantial funding, so we fail to see why this Reviewer recommends only “a few carefully selected programs.” A “few” programs will not even address the questions this reviewer listed here. See Appendix A, Underfunded research cannot be expected to produce elaborate and expensive results

Claim 12.2. “The quality of work is so inconsistent in this field, including the work of some key players, which makes it difficult to clear the black cloud and to increase the credibility of the field. Repeated retractions and conflicting experimental results in the past certainly did not help.”

There have not been repeated retractions. The black cloud is caused by ad hominem attacks against the key players by academic rivals. The key players, such as Fleischmann and Pons, have acted beyond reproach and have been persecuted to an extent unprecedented in the history of modern science. To blame them for this travesty is to join in the persecution.

Conflicting experimental results are to be expected at this stage. See Appendix A, Data from newly discovered phenomena often seems inconsistent. We agree that some of the researchers in the field are not skilled. Additional funding is needed to attract more skilled people, and to allow the use of better equipment. It is impossible to do the kind of quality research this reviewer demands on a shoestring.

Reviewer 13

We agree with most of the claims made by this reviewer, especially this one:

Claim 13.1. “In spite of the lack of reproducibility and predictability, positive observations have been made a number of times and by several different groups under what seem to be credible experimental conditions. I conclude there must be something of nuclear origin going on. It defies both the expectation for the d,d fusion rate and its branching ratio and that is a lot of defiance!”

This is true.

Claim 13.2. “The irreproducibility of the evidence for excess heat generation and He production in different batches of Pd expected to be the same, or in different experimental runs on the same material and the non-predictability of the conditions under which or precise timing at which they will be observed is very disconcerting for a scientific claim. The proponents’ assertion that there is reproducibility if 50% (or maybe even less) of experimental attempts indicate at least some excess heat, never mind how much or when it occurs is frustrating to the objective scientist and has some of the characteristics of ‘pathological science’.”

Pathological science as defined by I. Langmuir does not include this characteristic. Prior to the discovery of cold fusion, scientists did not claim that irreproducible effects are disconcerting or pathological, but only that they are difficult to study. Many scientists have made their reputations by successfully studying irreproducible effects with great

skill, such as the Top Quark. They are not accused of doing “pathological” science, but rather praised for doing difficult science.

People tend to redefine many aspects of cold fusion as “pathological” which in other contexts they consider normal and expected, or even laudable (such as doing difficult science). The editor of the *Scientific American* once claimed that Langmuir listed “the mechanism is not fully understood at present” as a characteristic of pathological science. Needless to say, Langmuir did not include this criterion in his definition.

In any case, reproducibility has improved since 2004 for many techniques.

Finally, see Appendix A, Data from newly discovered phenomena often seems inconsistent, Difficulty with experiments, irreproducibility and erratic performance are not grounds to disbelieve a result.

Claim 13.3. “It is disappointing that McKubre has not been able to do an integral of the total power in and out from the beginning of an experiment to show that there is a net out.”

McKubre could do this easily, but the result is not meaningful. See Appendix A, Chemical storage hypothesis

Claim 13.4. “The lack of understanding of what is happening in the material that makes the results so unpredictable – even after 15 years of effort – is very unsatisfying.”

We agree this is unsatisfying. We think this problem can only be fixed with proper funding.

Claim 13.5. In response to charge 3), yes, I think it is important to get to the bottom of the science that is going on, not with some massive attack on it, but in considered support of well conceived proposals submitted to address the scientific issues.”

“A massive attack” is not defined, but if the reviewer means we should not spend \$100 million the first year, we agree. We think that \$10 or \$20 million would be appropriate.

Claim 13.6. “In the current state of the field, finding nothing in a given experiment teaches us nothing whether it is in a search for charged particles, neutrons, gamma rays, He, or T. The only normalizing measurement seems to be heat generation.”

This is true, and it is an important point that many other reviewers failed to grasp.

Reviewer 14

Claim 14.1. “Reference #9 [13] points out that the SRI group has observed a total energy release that is claimed to be greater than 2000 eV/atom. It is argued that such a release is too big to be chemical and so it must be nuclear. However, such a conclusion seems unwarranted. The well known products of deuteron fusion are not observed in the

expected manner, and no credible explanation of the enhanced fusion rates has been proposed.”

Excess heat, tritium, and helium production commensurate with conventional fusion prove that this is a fusion reaction (perhaps combined with some other nuclear or unknown reaction). The fact that the exact mechanism is not known does not cast doubt on this conclusion. See Appendix A, A result need not be explained theoretically before it can be believed

Claim 14.2. “The chemical explanation appears to fail by a factor of 1000 but explanations resting on known fusion processes fail by factors that are enormously larger.”

Cold fusion researchers do not claim that they are studying a “known fusion reaction.” They say this is an unknown reaction.

In any case, this is narrow view of what constitutes “known factors” and conventional science. The reviewer has considered only nuclear physics and neglected other physics, and chemistry. A chemical cell that produces megajoules of energy without any known fuel and without producing at detectable level of chemical ash violates many more basic laws than aneutronic fusion appears to violate.

Claim 14.3. “There is an obvious a gap in understanding the origin of the large heat releases. This puzzle deserves to be resolved. However, the existence of this puzzle is hardly convincing evidence that the energy source is nuclear.”

It is convincing evidence that the energy source is not chemical. An unknown nuclear reaction fits the facts better than an unknown chemical reaction. See Appendix A, Chemical storage hypothesis and The nuclear hypothesis best fits the facts.

Claim 14.4. “One can summarize the essential question as follows: Is the electrolytic cell a battery or is it a nuclear energy source? Answering this question would require accurate end-to-end energy measurements . . .”

End-to-end measurements are not needed, and would be meaningless. You need only measure excess energy production during a single uninterrupted event to prove that the heat is beyond the limits of chemistry. See Appendix A, Chemical storage hypothesis.

Reviewer 15

This review is a travesty. It has no place in a serious scientific discussion, and it should be stricken from the record.

Claim 15.1. “The August 2004 Popular Mechanics gives an overview of the present DOE review and the cover states that one can build an H bomb in the basement. Also, the article claims that this is a cheap way to make tritium. Clearly this article sets the tone for this field of research, one of paranoia with the added impetus that someone will get there first. It is this aspect of the field that the DOE must somehow deal with.”

This remark is deplorable, and Popular Mechanics is not a scientific journal. The article in it was not written by a cold fusion researcher and it does not represent the views of any cold fusion researcher. The DoE and the cold fusion researchers are not obligated to “deal with” lurid claims in popular press that are far removed from actual science.

Claim 15.2. “As one of the reviewers stated, one can never disprove something and this is my feeling about ‘cold fusion’.”

The notion that one “cannot disprove something” is preposterous, and negates the scientific method at many levels. Cold fusion can be conclusively disproved by the following methods: Show that several different calorimeter types used millions of times over the last 180 years do not work, and routinely give the wrong answer for the range of heat they are designed to measure. Prove that the laws of thermodynamics are inoperative. Prove that x-ray film, tritium detectors, mass spectrometers and the other instruments that have repeatedly been used to confirm cold fusion at high signal to noise ratios do not work correctly. Either prove these instruments and techniques do not work, or show why thousands of skilled experts have mysteriously failed to use them correctly over the last 20 years.

Claim 15.3. “The workers are true believers and so there is no experiment that can make them quit. Likewise there is no series of experiments that can convincingly confirm their work or they would have done them by now. Of course their answer is that they have done them, but reproducibility and predictability still eludes them.”

These remarks are contradicted by hundreds of papers published by distinguished scientists in mainstream, peer-reviewed journal papers. This reviewer is obviously biased and has made no effort to read the literature or learn anything about cold fusion. These remarks are factually wrong: reproducibility and predictability are greatly improved since 1989, and far exceed levels in other accepted areas of science. See Appendix A, Difficulty with experiments, irreproducibility and erratic performance are not grounds to disbelieve a result

This reviewer made several other outrageous and unsupported claims, which we will not bother to enumerate.

Reviewer 16

Much of this review is devoted to refuting existing cold fusion theory. See Appendix A, Cold fusion is an experimental finding, so you cannot disprove it by showing errors in theories that attempt to explain it and Theoretical objections to experimentally proven facts are a violation of the scientific method

Claim 16.1. “The experiments reported by Lipson and by McKubre/ Violante contain intriguing results, but they are very hard to interpret according to our current understanding of nuclear physics as evidence that nuclear reactions, rather than some experimental artifact, are taking place.”

The argument that “some experimental artifact is taking place” cannot be falsified or refuted.

In the case of cold fusion, experimental errors are ruled out because the effect has been widely replicated.

An artifact of the experimental equipment is ruled out because many different equipment types have been used to verify the effects, such as different calorimeter types (flow and Seebeck) and different methods of tritium detection.

See Appendix A, Undiscovered error hypothesis.

Claim 16.2. “Electrochemical “excess heat” cells (McKubre/Violante-type experiments). The claims of these experiments to detect almost no particle emission, and yet generate large amounts of excess heat, correlated in some cases with the production of alpha particles, are the hardest to understand in terms of known nuclear physics. In some ways, that makes them the most interesting experiments to pursue, but on the other hand, it also makes them the least likely to be accepted by the wider physics community.

Any experiment of this type must therefore be done with exceptional care and attention to detail, and documented meticulously. McKubre’s group has made considerable progress along those lines, but there is much more to do in order to establish the excess heat production, and especially the correlation to alpha-particle production, convincingly enough to be published in the mainstream physics or chemistry literature. To do this will require a careful inventory of all energy in and energy out over the lifetime of the experiment, as well as a sensitive measurement of alpha-particle production in the cell that is uncontaminated by external or initial sources of ^4He . One way to accomplish this, of course, is to scale up those effects so that they are well above any possible background.”

This is a good summary. Scaling up effects until they are well above background is difficult to do, but if it can be done it would be excellent way to deal with this problem. An inventory of all excess heat over the lifetime of the experiment is needed to establish the helium balance, and it has been made for that reason. When Miles measured the ratio of heat to helium, he took samples for much shorter periods, which made it easier to measure the energy, but it produced much smaller amounts of helium.

These are not only the most interesting experiments; they may lead to a practical source of energy, so they are the most important.

Reviewer 17

We agree with most of the claims made by this reviewer.

Claim 17.1. “The quality of the presented evidence is very uneven. Most “nuclear” measurements (particle emission) are not convincing in comparison with the state of the art in low energy nuclear physics.”

We agree. Top notch experts in particle physics have not participated in the research, and there has not been enough funding or acceptance to allow state of the art research. See Appendix A, Underfunded research cannot be expected to produce elaborate and expensive results

Claim 17.2. “Although I am not an experimentalist, it is my impression that the experimental groups who presented results, are not always applying or reporting the nuclear measurement techniques employed by them in a manner that is commensurate with the professional state of the art.”

We agree. Researchers are struggling to do experiments on a shoestring.

Claim 17.3. “The calorimetry has made significant advances, and now looks much more convincing than that of the original University of Utah experiments. The concerns described in the ERAB report have been largely addressed.”

The calorimetry has improved considerably. However, the concerns expressed by the ERAB report were not valid.

Claim 17.4. “However, unambiguous evidence for the production of excess energy during a calorimetric run was not presented at the meeting. The presenters showed evidence for long periods of surplus power output by (some of) the electrolytic cells, but it remained unclear whether such a surplus survives when the fully time integrated energy balance is considered.

In one case presented at the meeting, the net energy fed into the cell during the early part of the run and the net energy put out by the cell during the following period appeared to roughly cancel each other.”

The fully time integrated energy balance is not needed. Also, the ratio of input to output for the entire run is meaningless. You need only measure excess energy production during a single uninterrupted event to prove that the heat is beyond the limits of chemistry. See Appendix A, Chemical storage hypothesis.

In other words, when uninterrupted runs produce hundreds or thousands of electron volts per atom of palladium, it is impossible for the reactions to “cancel out” one another. This is a fact of elementary physics and it is fundamental to the claims made by the cold fusion researchers but unfortunately this reviewer and several others failed to understand it. See Appendix A, Artifactual low-level heat hypothesis

Claim 17.5. “‘Determine whether there is a scientific case for continued efforts in these studies and, if so, to identify the most promising areas to be pursued.’

My response to this question is a weak YES. It is weak, because the proponents of the “cold fusion” effects do not seem to be interested in making their observations go away

or in finding conventional explanations for them. This is never a good basis for critical experimental investigations.”

We disagree. See Appendix A, Researchers have made great efforts to find systematic errors and conventional explanations

Claim 17.6. “Finally, I make several comments on the presented theoretical speculations. First and foremost, it must be emphasized that this field is not theory driven. Conventional nuclear and atomic theory predicts that no d+d fusion reactions can occur at room temperature at a measurable rate, even in the presence of a metal catalyst.”

The reviewer is correct that the field is not theory driven. It is experimental. Therefore, the lack of theory has no bearing on whether the results are valid or not, or whether additional research funding is justified or not. See Appendix A, A result need not be explained theoretically before it can be believed. This reviewer has *avoided* making the error that several other reviewers made: Cold fusion is an experimental finding, so you cannot disprove it by showing errors in theories that attempt to explain it

Claim 17.7. “The LENR investigators should present evidence for or against excess heat being produced when the energy flow is integrated over the entire history of a run. If net energy is really produced, one deals either with nuclear reactions or a violation of the first law of thermodynamics.

Since both explanations would be revolutionary, very careful checks of possible errors in the total energy balance are necessary. It is superfluous to discuss these in detail, until evidence for net energy output is presented.”

As noted above, there is no need to measure the energy flow integrated over the entire history of a run. There is no evidence whatever that cold fusion violates the First or Second Law of Thermodynamics. On the contrary, because calorimeters are predicated upon those laws, it would be impossible to detect a violation with a cold fusion experiment. A violation would either look exactly like excess heat (which is observed), or like an inexplicable endothermic process that appears to be swallowing up heat (which has never been observed).

Reviewer 18

Claim 18.1. “Although experiments have become more sophisticated there is no new convincing or even tantalizing evidence for LENR. The discussions and conclusions presented in the Report on Cold Fusion Research prepared in 1989 by the Energy Research Advisory Board to the DOE [ERAB report] still apply.”

In our opinion, the ERAB report was invalid. Reviewer 17, who agreed with ERAB, feels that: “The calorimetry has made significant advances, and now looks much more convincing than that of the original University of Utah experiments. The concerns described in the ERAB report have been largely addressed.”

Claim 18.2. “The experimental limits on various reaction channels of the d+d fusion reaction that were done around 1989 are still valid.”

The reviewer is probably referring to neutron results, and assuming that cold fusion must produce neutrons at the same rate that plasma fusion does. Experiments have shown that this is not the case. Cold fusion does not produce a significant number of neutrons no matter how strong the reaction is. So these experiments do not set limits to the strength of the reaction. They do not prove that cold fusion is not happening. They prove it is aneutronic.

Claim 18.3. “New experiments performed since then were not done with the care that is needed to produce convincing evidence.”

We disagree. See Appendix A, Researchers have made great efforts to find systematic errors and conventional explanations. Perhaps the reviewer is demanding that new experiments show there are neutrons despite the fact that many previous experiments proved definitively that cold fusion does not produce neutrons.

Claim 18.4. “They are often in conflict with other, so-called positive, experimental results.”

This reviewer sees a conflict where other experts do not. See also Appendix A, Data from newly discovered phenomena often seems inconsistent

Claim 18.5. “The research in cold fusion is still done by a small group of relatively isolated electrochemists and physicists. In their work, they strangely neglect very basic and model independent data obtained over decades of nuclear physics research.”

Cold fusion researchers include Nobel laureates in physics and many other distinguished scientists who well aware this basic data. They assume it does not apply for some unknown reason. See Appendix A, Theoretical objections to experimentally proven facts are a violation of the scientific method

Claim 18.6. “Most nuclear physicists consider the issue of cold fusion closed with the set of measurements done around 1990.”

This reviewer has not conducted a poll of “most nuclear physicists” and cannot read their minds, so this is not in evidence.

Claim 18.7. “Although surprises can happen (such as the Mossbauer Effect) they are usually quickly understood in terms of phenomena that make sense once discovered. This is not the case in ‘cold fusion’.”

This assertion is illogical and mystifying. The reviewer seems to be arguing that surprises and difficult problems in physics that take a long time to solve should not be researched. In other words, we should only study unsurprising and easily explained

phenomena. Alternatively, the reviewer means that difficult problems can never arise – all problems are inherently easy – and data which indicates that a difficult problem has arisen should be discounted or ignored.

Claim 18.8. “The alterations in the decay ratios of the d+d fusion reaction that would be required to explain the electrochemical data in terms of LENR cannot be understood in any sensible model.”

See Appendix A, A result need not be explained theoretically before it can be believed and A reviewer’s inability to imagine or understand a result is not a valid reason to reject it

Claim 18.9. “The presentations convinced me that electrochemical flow calorimeters have been much perfected since 1989 by the work done at SRI and in Italy (Rome). They have learned that to see an excess power effect requires very large deuterium loading (>90%) to a d/Pd atomic ratio ~1, require an activating current through the foils or rods to produce excess energy and become “active” only after a substantial period of time that is not understood.”

This is correct. It contradicts claim 18.1.

Claim 18.10. “The excess power observed amounts to between 3% and 30% of activating power, with the average about 6%.”

This is incorrect. The excess power sometimes reaches 300%, and in heat after death or gas loaded cells, there is no input power (the ratio is infinite). Also the overall energy input into the system for the entire run is irrelevant. See Appendix A, Chemical storage hypothesis

Claim 18.11. “This is much less than the original Fleischman observation who reported 4 Watts out for every 1 W input.”

This is incorrect, as noted.

Claim 18.12. “Although much systematic work has been done on the materials properties that produce a successful cell, the reproducibility is still, at best, only 50%.”

For some experiments, at some laboratories, reproducibility was better than this in 2004. However, ease of reproducibility has no bearing on whether the effect is real. See Appendix A, Difficulty with experiments, irreproducibility and erratic performance are not grounds to disbelieve a result

Claim 18.13. “In the SRI work the successful cells are reported to show He4 that would be consistent with a fusion reaction of the required reaction rate if He4 is the only reaction product. Unfortunately, not every successful cell shows this helium.”

This is unfortunate, but it has to be expected in groundbreaking research, especially when it is underfunded. See Appendix A, Data from newly discovered phenomena often seems inconsistent

Claim 18.14. “Unfortunately the experimental work is often not written up with the care and detail that would pass peer review of a scientific journal. The data from the various experiments are not correlated with each other nor translated into reaction rates that can be compared. It would be difficult to motivate a group of first-class scientists to redo the few crucial experiments since it will be a thankless job.”

See Appendix A, Underfunded research cannot be expected to produce elaborate and expensive results

Claim 18.15. “However, in order to move this area of study, I recommend that small proper proposals that arise spontaneously from the field and withstand per review, could be entertained as part of the low energy nuclear physics program”

Obviously, proposals that “withstand” peer-review should be “entertained.” This can be said for any proposal in any field of science. The DoE asked the reviewer whether any particular cold fusion experiments should be funded. This does not appear to answer the question.

Claim 18.16. “There is no evidence that the branching ratios in the decay of He4 formed in the d+d reaction are altered in a solid state environment at low energies, down to a few keV.”

We disagree. We feel that cold fusion experiments have shown helium commensurate with heat at approximately the same ratio as plasma fusion. Since these experiments manifestly do not produce gamma or neutron radiation, the branching ratios must be different.

Claim 18.17. “It is very hard to see how this could happen since these decay ratios follow from very basic facts about the interaction involved and decay barriers.”

As noted above, A reviewer’s inability to imagine or understand a result is not a valid reason to reject it

Claim 18.18. “We heard of an almost desperate effort to provide a model in which the lattice coherently absorbs the 26 MeV that need to be accounted for, if the dominant channel would be emission of He4 + γ (the latter not being observed). Any analogy with the Moessbauer effect, where the lattice takes up the recoil momentum of an emitted γ -

ray is misleading and unrealistic by orders of magnitude. In addition the dominant particle emission channels would still have to be suppressed by some lattice effect. It does not appear to me worthwhile to pursue such theoretical models further.”

Perhaps these models should not be pursued, but this has no bearing on the whether cold fusion exists, and it does not answer the two questions posed by the DoE in the charges to the panel. See Appendix A, Cold fusion is an experimental finding, so you cannot disprove it by showing errors in theories that attempt to explain it

Appendix A. Common Errors

Many panel members made errors in matters of fact, analysis, logical fallacies and errors in applying scientific method. Here are some errors made by two or more reviewers.

Theoretical objections to experimentally proven facts are a violation of the scientific method

Most critics of cold fusion claim that because cold fusion appears to violate the laws of nuclear physics, it must be an experimental error. In the conclusion to his book, Huizenga dismisses all heat results *a priori* because they would require “too many miracles” to be true: “Furthermore, if the claimed excess heat exceeds that possible by other conventional processes (chemical, mechanical, etc.), one must conclude that an error has been made in measuring the excess heat.”

This is a violation of the scientific method. It is by no means certain that cold fusion experimental results actually do conflict with textbook plasma physics, but if they do, the results stand and the textbooks are wrong. Excess heat, tritium and other products of cold fusion have been widely replicated at high signal to noise ratios. This proves they are real. There is no other standard of proof in science.

If replicated results are thrown away in deference to existing theory, then no theory can be tested or falsified, and there will be no way to resolve disputes or make progress.

Furthermore, if we are to judge hypotheses based on how closely they hew to conventional physics, and the number of “miracles” they call for, that would make the chemical hypothesis even more implausible than the nuclear hypothesis. A chemical reaction that produces thousands of electron volts per atom of reactant without a trace of chemical ash violates the laws of physics more profoundly than any previously undiscovered aneutronic nuclear fusion reaction does.

Finally, it should be noted that calling a phenomenon a “miracle” does not make it a miracle. When a skeptic wrote:

“. . . the fact that I had not detected the flaw, did not mean that the experiment was correct and that the laws of Nature had been violated.”

Beaudette responded:

“But if it were necessary to violate the laws of nature to generate excess heat, then none would be generated. On the other hand, if the measurements are incorrect, then an avid pursuit of the ‘science’ must in due course explicitly and particularly reveal that incorrectness. ‘Undetected error’ may be a stopping point because of penury or fatigue, but it is not a scientific stopping point.”

A result need not be explained theoretically before it can be believed

Skeptics sometimes assert that a result must be explained by a theory before it can be believed. This is another violation of the scientific method, and this standard has never

been applied to previous breakthroughs. As Beaudette put it: “Pierre Curie announced his empirical evidence for anomalous power discharge from radium and was awarded a Nobel Prize. Similarly, the empirical evidence for an astonishingly rapid expansion of the universe is recognized as requiring explanation by the cosmologists. There was no assertion from scientists that these two examples were pathological because of the lack of causal information. The empirical data in these two examples was not hidden from view pending some additional knowledge. Science requires only that there be no procedural error in the measurements.”

A reviewer’s inability to imagine or understand a result is not a valid reason to reject it

Several reviewers rejected experimental data with comments such as “it is hard to imagine,” or “I fail to understand” the result. This is not a valid reason to reject widely replicated, high signal-to-noise experimental data. It is a violation of the scientific method. The fact that you do not understand an experimental result is no indication that the result is in error. People fail to understand countless phenomena that are, nevertheless, real beyond any doubt, such as high temperature superconductivity. Until 1939, no one understood what nuclear reaction occurs in the sun, and until 1952, no one understood the basis for biological reproduction, but no one asserted that these phenomena do not exist.

This is a variation of the error described above: a result need not be explained theoretically before it can be believed.

Cold fusion is an experimental finding, so you cannot disprove it by showing errors in theories that attempt to explain it

Cold fusion is an experimental finding, not a theory. Even if the theories that have been proposed up until now are incorrect, that has no bearing on whether cold fusion itself exists or not.

Hagelstein, Schwinger and others have published theories that attempt to explain cold fusion results. Several panel members devoted large sections of their responses to disputing these theories. This may be a valuable exercise. These theories may be wrong, or in need of peer-review and improvement. However, this exercise has no bearing on the charges assigned to the panel.

As reviewer #17 correctly put it: “First and foremost, it must be emphasized that this field is not theory driven.”

Some of the discussions of theory were prompted by the DoE charge to the panel: “to examine and evaluate the experimental and theoretical evidence for the occurrences of nuclear reactions in condensed matter at low energies (less than a few electron volts).” This is a procedural error on the DoE’s part. The DoE should have asked reviewers to examine and evaluate *experimental evidence only*. Theoretical evidence is not relevant. As noted above, theoretical evidence (or lack thereof) can never be cited as a reason to doubt replicated experimental results. Many people are convinced that the excess heat, tritium, commensurate helium and other experimental evidence proves that cold fusion is a nuclear effect, but they do not believe that any present-day theory explains the effect.

Undiscovered error hypothesis

Some skeptics claim that there might be a yet-undiscovered error in the experiments. See the comment by Beaudette about this, above, “if the measurements are incorrect, then an avid pursuit of the ‘science’ must in due course explicitly and particularly reveal that incorrectness.”

More to the point, the claim that there might be an undiscovered error is not falsifiable, and it applies to every experiment ever performed. There might be an undiscovered error in experiments confirming Newton’s or Boyle’s laws, but these experiments have been done so many times that the likelihood they are wrong is vanishingly small. Furthermore, skeptics have had 20 years to expose an experimental artifact, but they have failed to do so. A reasonable time limit to find errors must be set, or results from decades or centuries ago will remain in limbo, forever disputed, and progress will ground to a halt. The calorimeters used by cold fusion researchers were developed in the late 18th and early 19th century. A skeptic who asserts that scientists cannot measure multiple watts of heat with confidence is, in effect, rejecting most textbook chemistry and physics from the last 130 years.

As a practical matter, there is no possibility that techniques such as calorimetry, x-ray film autoradiography or mass spectroscopy are fundamentally flawed. It must be emphasized that although cold fusion results are surprising, the techniques are conventional and instruments are used within their design specifications. Cold fusion does not require heroic measurement techniques. Heat and tritium are not usually measured close to the limits of detection, although they have been in some cases, and helium and transmutations have been.

It has been argued that even though the instruments work, the researchers may be making mistakes and using the instrument incorrectly. No doubt some of them are, but most are experienced scientists at major labs. The effect has been confirmed at 180 major laboratories [Storms, Table 1]. If an experiment could be as widely replicated as this could be mistaken, the experimental method itself would not work.

Chemical storage hypothesis

The chemical storage hypothesis holds that cold fusion might be caused by a chemical reaction rather than a nuclear reaction. This is an error in elementary physics, and a failure to grasp the principal claim made by Fleischmann, Pons and the other researchers in this field. Chemical energy storage is ruled out for three reasons:

1. Some cold fusion reactions have produced hundreds of electron-volts of energy per palladium atom, which far exceeds the limits of chemical storage.
2. No endothermic storage has been observed. In some cases, when heat production events occur close together, energy storage would produce an endothermic heat deficit larger than the positive heat that follows.
3. No chemical changes or ash has been detected in cells. In some cases, the volume of chemical ash commensurate with the heat would far exceed the cell volume.

A variation of this hypothesis holds that it is necessary to measure the entire energy balance of the experiment, from beginning to end, because the heat might be caused by slow energy storage at levels too low to detect with calorimeters, which is then released quickly in bursts that can be detected. This scenario is impossible. It is like suggesting that you can slowly recharge a battery to a level of charge a thousand times higher than the battery rated for. It makes no difference how slowly the palladium is loaded with deuterium, or whether you can detect the endothermic heat deficit caused by the loading (or by the formation of other energetic species); the absolute upper limits of chemical storage remain the same.

The overall energy balance of an experiment is meaningless. The only energy balance that matters is computed during uninterrupted heat production measured at a high signal to noise ratio. The only meaningful metrics are heat energy normalized to the mass of palladium (electron volts per atom, or joules per mole), and the signal to noise ratio.

A chemical reaction and experimental error are ruled out. That leaves only nuclear or a reaction unknown to science.

Artificial low-level heat hypothesis

Some reviewers suggested that the excess heat might be an artifact that appears to produce low level heat close to the noise, which is actually zero excess. This spurious power is multiplied by long duration experiments, over weeks or months, which adds up to megajoules of spurious energy. In some experiments, low power levels close to the noise have been reported, but in other cases power exceeds 1 W, which is high enough to measure with confidence, and in some cases power has exceeded 100 W.

In a related misunderstanding, some reviewers question the common practice of reporting power, in addition to energy, and especially absolute power (power not normalized to the mass of the cathode). Researchers report this because high power is easier to measure than low power, so reporting the power level bolsters the researcher's estimate of the signal to noise ratio.

Recombination hypothesis

The recombination hypothesis, that the excess heat is actually caused by evolved deuterium and oxygen. Reviewer 5 cited Shanahan's version of this: Shanahan "pointed out that the catalytic recombination of D₂ and O₂ can also be a factor for the excess heat since the latter is evolved at the anode and the former will be evolved at the cathode . . ." [9] Recombination cannot explain the excess heat for five reasons:

1. Recombination is prevented by using tall, thin cells, and by placing the anode and cathode side by side (rather than top and bottom), or by separating the anode and cathode with a glass hood. These techniques prevent significant recombination except at power levels about a thousand times lower than most cold fusion experiments employ.
2. With open cells the evolved deuterium and oxygen gases are measured to ensure there is no recombination.

3. Closed cells must employ catalysts to ensure that all gas recombines, or the cells will explode. Recombination is automatically included in the heat balance.
4. In many cells, the heat far exceeds the total that would be produced by electrolysis joule heating and recombination combined.
5. Excess heat has been observed in gas loaded cells (with no anode and no oxygen) and in cells after electrolysis has stopped.

The Shanahan hypothesis is impossible for an additional reason: Shanahan believes that some calorimeters in which heat is measured inside the cell may have positional sensitivity. They may overestimate recombination heat because it occurs at the cathode and not the anode, in effect “moving” the heat from one location in the cell to another. This is plausible, but tests by Storms and others show that it is not true. In any case, with many calorimeter types, such as flow and Seebeck, the heat is measured outside the cell. Also, as noted in Item 5 above, many cells do not have anodes and cathodes.

The nuclear hypothesis best fits the facts

We do not assert that cold fusion is unquestionably a nuclear effect and only a nuclear effect. As noted already in this Appendix, we assert that a chemical effect or experimental error is ruled out, and that the heat beyond the limits of chemistry, helium commensurate with a plasma fusion reaction, tritium and heavy metal transmutations all point to an unknown nuclear reaction. In short, the nuclear hypothesis best fits the facts, but until a detailed nuclear theory is worked out and broadly accepted, this will remain only a working hypothesis.

It is conceivable that cold fusion is caused by an unknown effect even more powerful than nuclear fusion that triggers some nuclear changes as a side effect of the main reaction, just as fission reactor heat triggers chemical changes as a side effect of fission.

Several reviewers asserted that the effect cannot be nuclear, mainly because they feel it violates the laws of nuclear physics. As Beaudette wrote in his response to Reviewer 7:

“The notion that 3.05 Watts (89/3), or 2 MJ in 48 hours (90/7), could come from some stored mechanical or chemical source within the cell does seem absurd. Certainly, critics have a duty to try to elucidate possible storage mechanisms as disturbing artifacts, but they must work with actual experimental results, as above. In the opinion of those working in the field for sixteen years, that amount of energy can only come from a nuclear source; there is no other credible hypothesis.”

Reviewers who deny that cold fusion is nuclear but stop there, without offering a credible alternative hypothesis, have not done their jobs as scientists.

Data from newly discovered phenomena often seems inconsistent

The results only appear to be internally inconsistent because the reaction is not yet understood. Once a theory has been developed, the apparent contradictions will be resolved. Apparent “inconsistency” is a function of our present ignorance; nature is never inconsistent.

There are also actual inconsistencies and mistakes, which are inevitable with any new discovery. Segre described the work of Hahn and Meitner as follows: “Their early papers are a mixture of error and truth as complicated as the mixture of fission products resulting from the bombardments. Such confusion was to remain for long time a characteristic of much of the work on uranium.” (Quoted by Mallove, p. 22)

Difficulty with experiments, irreproducibility and erratic performance are not grounds to disbelieve a result

Erratic performance is common in groundbreaking research. The reproducibility of a difficult experiment has never been held as grounds to disbelieve it. The failure rate of transistors in early 1950s was far higher than cold fusion is today, and the failure rate in cloning experiments is hundreds of times higher than cold fusion. No one ever claimed transistors do not exist because they are hard to make, or that a living animal is not a clone because hundreds of embryos died.

Difficulty has never been considered a reason to doubt a result. On the contrary, difficult experiments often impress experts. Some experiments, such as the final tokamak plasma fusion runs at the PPPL, are so difficult and expensive they can only be performed once, but no one suggests they are invalid or suspect for this reason.

Researchers have made great efforts to find systematic errors and conventional explanations

As Hagelstein wrote in his review of ICCF3: “Scientists in the field have gone to extremes in attempts to satisfy skeptics. Cells were stirred, blanks were done, extremely elaborate closed cell calorimeters have been developed (in which the effect has been demonstrated), the signal to noise ratio has been improved so that positive results can now be claimed at the 50 sigma level, the reproducibility issue has been laid to rest; but still it is not enough.”

Researchers have searched exhaustively for conventional causes and made heroic efforts to eliminate errors. Skeptical reviewers in this document, on the other hand, have never suggested a single candidate “conventional” reaction that might produce thousands of electron-volts per atom with no detectible chemical ash. They insist that there may be a chemical explanation, but they offer no specifics. We assume they have not suggested a candidate because they cannot think of one.

Underfunded research cannot be expected to produce elaborate and expensive results

Several reviewers seem to believe in what might be called the Matthew 13:12 principle of program funding: “For whosoever hath, to him shall be given, and he shall have more abundance: but whosoever hath not, from him shall be taken away even that he hath.” The reviewers apparently believe it is the researchers fault that cold fusion has not been funded adequately until now and researchers have not been able to do elaborate experiments. The researchers should not be funded in the future as punishment for not being funded in the past. The reviewers seem to believe that only research projects that

are already lavishly funded such as plasma fusion should be funded, even when these projects have failed to meet assigned goals or produce useful results.

Skeptics have published few papers

Roughly 10 skeptical peer-reviewed papers have been published, along with three skeptical books with scientific content (Huizenga, Close, Hoffman). We feel that these papers have no merit. The papers cited by reviewers by Morrison and Shanahan were effectively rebutted by Fleischmann and Storms, respectively. See: [links]

Many early papers reported reporting negative results, that is, no excess heat. Nearly all attempts to find neutrons have failed. However, a negative result does not disprove the positive results. The reasons why most of the early studies failed is now well understood.

Summary of Reviewer's Responses to DoE Charges

The DoE report [1] includes 3 charges to panel members:

“Review Criteria

Reviewers were asked to respond to the following charge in their evaluation of the written and/or oral material: (1) To examine and evaluate the experimental and theoretical evidence for the occurrences of nuclear reactions in condensed matter at low energies (less than a few electron volts). (2) To determine whether the evidence is sufficiently conclusive to demonstrate that such nuclear reactions occur. (3) To determine whether there is a scientific case for continued efforts in these studies and, if so, to identify the most promising areas to be pursued.”

The panel member's responses to the charges 2 and 3 are summarized in Table 1. The member's opinions were unclear in some cases.

Each answer block is marked:

Y We interpret the member's response as generally in agreement that nuclear reactions in condensed matter exist (for Charge 2); or that research on these reactions should be funded to some extent (for Charge 3)

N The member disagrees

U The member is undecided, or it is unclear what the member thought

- Member did not respond to charge

Most of the responses are clear cut, especially to Charge 3. In some cases, reviewer's responses were nuanced or vague. Another person evaluating these reviews might characterize a few of the ones we have marked 'Y' to be 'N' or vice versa. Because we would answer 'Yes' to both charges, we have tried to err on the conservative 'No' side here. For example, reviewer 12 characterizes the evidence for cold fusion being a nuclear effect as “intriguing, but not fully convincing.” We listed that as 'N.'

Table 1. Summary of review panel members' responses to DoE charges 2 and 3

Reviewer Number	Charge 2	Charge 3	Remarks
1	N	N	
2	N	Y	Advocates some research but not into excess heat
3	U	Y	
4	Y	Y	"A significant case" is made for Charge 2.
5	N	N	
6	N	-	
7	N	Y	"evidence is spotty" for Charge 2; would fund "carefully selected programs"
8	Y	Y	Strong support
9	Y	Y	Strong support
10	Y	Y	Possible but not conclusive for Charge 2.
11	Y	Y	Cites "strong evidence" for Charge 2.
12	N	Y	"Intriguing, but not fully convincing" for Charge 2
13	Y	Y	Despite problems "there must be something nuclear going on" for Charge 2
14	N	Y	The DoE should consider proposals of "high scientific quality" for Charge 3
15	N	N	Strong opposition; ridicules claims
16	U	Y	"A separate program" not needed but individual proposals should be evaluated for Charge 3
17	N	Y	A "weak yes" for Charge 3
18	N	U	"Small proper proposals that arise spontaneously . . . [and] withstand per review, could be entertained" for Charge 3.

For Charge 2, 'is cold fusion a nuclear effect': 10 said no, 6 said yes, and 2 answers were unclear or undecided.

For Charge 3, 'should this research be funded': 3 said 'No,' 13 said 'Yes,' 1 did not respond to the question, and 1 is unclear. Several of the people who said "Yes" specified they recommend no "separate program" for cold fusion, or that large-scale funding is not justified. But, they say, individual grant proposals should be considered.

It is interesting to note 5 members who do not think that cold fusion is a nuclear effect still believe that research should be funded, and so do the 2 members whose are undecided or unclear.

All 6 people who unequivocally believe it is a nuclear effect also believe the research should be funded.

Only 3 members said no to both questions: it is *not* a nuclear effect and it should *not* be funded. One reviewer who said it is not nuclear did not respond to Charge 3, and another (#18) responded in such vague terms we classified the answer “U” (undecided or unclear). Perhaps these two should be characterized as opposed to funding. That would make the panel overall 13 in favor of funding and 5 against.

Summary of reviewers and common errors

Table 2 lists the reviewers and the errors listed in Appendix A. In the first column it shows the reviewers' response to Charge 2, 'is cold fusion a nuclear effect?'

This Table is not exhaustive. We did not try to catch every single error, or every statement that might be construed as an error if one were to quibble. For example, many reviewers appeared to make error #1, raising theoretical objections to replicated experiments. Perhaps the most extreme examples of this notion that theory overrules experiments and that we should not even *look* at data were expressed by H. Feshbach and J. Huizenga:

“I have had 50 years of experience in nuclear physics and I know what's possible and what's not. . . . I don't want to see any more evidence! I think it's a bunch of junk and I don't want to have anything further to do with it.” - H. Feshbach [14]

“Furthermore, if the claimed excess heat exceeds that possible by other conventional processes (chemical, mechanical, etc.), one must conclude that an error has been made in measuring the excess heat.” - J. Huizenga [15]

We did not mark this error in some cases because reviewers only hint at this attitude; they do not actually come out make a Huizenga-like declaration. In other cases, reviewers do not accept that the results have been replicated. (Some even seem unaware that replications are claimed.) Reviewers who do not acknowledge that cold fusion has been widely replicated cannot be accused of committing error #1. They have made another error: “not doing your homework.” We did not list this in the table.

Errors are as follows:

1. Theoretical objections to experimentally proven facts are a violation of the scientific method
2. A result need not be explained theoretically before it can be believed
3. A reviewer's inability to imagine or understand a result is not a valid reason to reject it
4. Cold fusion is an experimental finding, so you cannot disprove it by showing errors in theories that attempt to explain it
5. Undiscovered error hypothesis
6. Chemical storage hypothesis
7. Artfactual low-level heat hypothesis
8. Recombination hypothesis
9. The nuclear hypothesis best fits the facts
10. Data from newly discovered phenomena often seems inconsistent
11. Difficulty with experiments, irreproducibility and erratic performance are not grounds to disbelieve a result
12. Researchers have made great efforts to find systematic errors and conventional explanations
13. Underfunded research cannot be expected to produce elaborate and expensive results
14. Skeptics have published few papers

Table 2. Summary of common errors made by review panel members

Reviewer Number	Charge 2	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1	N	X								X	X	X			X
2	N	X		X						X					
3	U														
4	Y				X										
5	N			X			X	X	X				X	X	X
6	N	X	X	X	X	X	X	X	X		X		X	X	
7	N	X					X			X				X	
8	Y														
9	Y														
10	Y		X			X	X			X					
11	Y		X							X					
12	N									X	X			X	
13	Y									X	X				
14	N		X				X			X					
15	N	X	X	X			X					X		X	
16	U	X			X	X									
17	N		X				X	X					X	X	
18	N	X	X	X	X		X				X	X	X	X	

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