March 1, 2005

RESPONSE

to the DOE/2004 Review of Cold-Fusion Research.

Charles G. Beaudette[*]

During 2004, the Office of Science of the U. S. Department of Energy (DOE/OS) initiated and completed a peer-review of the field in science known as cold-fusion research (CFR). The DOE/OS selected eighteen Reviewers for their expertise in the relevant scientific specialties. Remaining largely anonymous, they studied a collection of papers about the field selected and prepared by several of the scientists who have been active in CFR for the past sixteen years. Those scientists also presented selected accomplishments to some of the Reviewers during a one day meeting. The following three questions (paraphrased) were asked of the Reviewers: (1) Is there evidence of low-energy-nuclear-reactions (LENR), (2) do such reactions really occur, and (3) should research efforts be continued?

The DOE/OS published its final Report[1] on December 1, 2004. It also provided the eighteen individual reviews to the CF scientists, who had originally requested the review in a meeting with the OS in November 2003. Those researchers released the Reviews to which I, herewith, choose to respond in the hope that I might bring to them some historical perspective. My Response is written without

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[*] Author of *EXCESS HEAT & Why Cold Fusion Research Prevailed*, Second Edition, 2002, Oak Grove Press, LLC, ISBN 0-9678548-2-2 (Hardcover), and ISBN 0-9678548-3-0 (Soft cover). This publication is referenced in the endnotes as EH-2. The author can be reached at 61 Thompson Inn Road, South Bristol, Maine 04568, USA, and at < cgbeau@gwi.net> .

knowledge of the identity of the numbered Reviewers.

The DOE/OS accomplished the best peer-review evaluation that was possible under the difficult circumstances of CFR's place in the professional community. Nevertheless, it is instructive to ask, What if the editor of an archival journal were to use a similar peer-review procedure by choosing reviewers who were not active in the field, did not know of its key experiments, and were ignorant of its literature? Would that not invite the editor's dismissal? George H. Miley knows something about peer-review.[2] A hot and cold-fusion scientist, he was for many of the past fifteen years editor of three professional journals. Once, criticized by his editorial board for sending cold-fusion research papers only to other cold-fusion scientists for review, he pointed out that hot-fusion papers are sent to hot-fusion researchers for review. This is done because that is where one finds the experts. The Office of Science did not have a choice in this matter given the pariah status of the field. The peer-review work necessarily had to be done by scientists outside the field of CFR who, unavoidably, were unfamiliar with its technical development, leading scientists, significant experiments, and principal papers.

The eighteen reviews reveal two aspects of the field that are sufficiently confusing and pervasive that they need some treatment: (1) the concern with the lack of reproducibility (repeatability) of experiments, and (2) the tendency to dismiss the excess heat measurements in favor of counting energetic particles. The first calls for a review of various methodologies to show that a protocol lacking reproducibility is regularly used in accepted experimental science.

The second concern is equally difficult because, unfortunately, some measurements of excess heat threaten the canon of nuclear science. In fifteen years and hundreds of experiments, no measurement of heat, no matter how well done, can be accepted, or even allowed, as valid by the nuclear physics community. Reviewer 7: "This single-minded conclusion has been pushed ever since, even though . . . . . 'The excess heat effect itself is consistent neither with a conventional D + D fusion reaction mechanism, nor with any other nuclear reaction mechanism that appears in textbooks or in the mainstream nuclear physics literature.'"[3] This implicit defense of the canon makes its appearance in discourse with physicists as an unstated assumption that mankind does not know how to measure heat or heat flow. By the way, from that same Reviewer, one gets an identical response to the Iwamura measurement of transmutation: "From a nuclear physics perspective, such conclusions are not to be believed."[4] Apparently, mankind also does not know how to measure transmutation. This refusal is based on a religious attitude of disbelief — one does not see a request to examine and review the experiment. Data is waved off if it violates the canon of nuclear physics. As a consequence, the pariah status of CFR within the science community lasted from May 1, 1989 to December 1, 2004 — a total of 5693 days in the wilderness. DOE/OS offered to the CFR scientists a review process ordinarily used by editors to judge individual papers for publication, which, in spite of this oddity, managed to accomplish its peculiar task adequately.

The Office of Science Report of its Review of LENR found that half the Reviewers considered the evidence for excess heat compelling. From the Report: "Evaluations by the reviewers ranged from: 1) evidence for excess power is compelling, to 2) there is no convincing evidence that excess power is produced when integrated over the life of an experiment. The reviewers were split approximately evenly on this topic."[5] The finding of this new phenomenon, item (1), if confirmed, would constitute a scientific field whose purpose would be to bring understanding as to its source, presumably nuclear. The CFR community recognizes the discharge of heat energy as the *presenting symptom* of a low-energy-nuclear-reaction (LENR). Taken all in all, the Review adds up to about as much as might be expected under the circumstances. As Peter Hagelstein declared in the journal *Science's* commentary on the Review, "In the end, the reviewers said that a study should be funded if a proposal is strong. You can't ask for much more than that."[6]

If we look a little more closely at the Review by scanning the individual Reviewer reports, there is a considerable level of confusion as to how the field of cold-fusion research began, and how its several parts now relate to one another. For example, Reviewer 10 says, "At the time of the [DOE/ERAB 1989] report, these [heat generating] fusion products were assumed to follow the usual branching ratio of the d-d reaction, so the implication was that production of neutrons or tritium would be investigated."[7] Among the CFR scientists in 1988-89, there was in fact no assumption of a text book d-d reaction for the source of the claimed excess heat. However, most critical physicists wrongly assumed a necessity for copious neutron emission per watt.

As a further example of this confusion, Reviewer 15: "In fact it seems that all of the workers in this field accept the P-F results as true and yet the review work shows that no effect is observed until the loading of the foils [D/Pd] is greater than 0.95. They also told us that it is extremely difficult to get the foil loading up to 0.95. P-F did no special work to load their foils and in fact based on the SRI work it would be hard to believe that their loading was above 0.9, too low to have any effects."[8] F&P obtained excess heat by driving their experiment hard for ten weeks.

In the spring of 1989, others ran five-week experiments based upon diffusion-time calculations and obtained null results. It was later that experimental inquiry led to understanding and measuring the loading requirement.[9] Reviewer 18 confuses the two CF reactions involved, "However, the proposed rate of fusion reactions would have to be so large that lethal numbers of neutrons would have to be emitted from the $D+D \rightarrow He3+n$ reaction. The second set of experiments claimed to observe neutrons stemming from this reaction, but at a rate that would indicate a much smaller energy release."[10] We note here the opinion offered by Reviewer 8, "The two most difficult things any scientist can be asked to do are trace analysis/mass balance and calorimetry."[11] It takes many months of study to develop expertise in calorimetry.

A careful reconsideration of what was announced and claimed on March 23, 1989 (89/3) at the University of Utah by Martin Fleischmann and Stanley Pons (F&P) will serve to give us a reference benchmark.[12] By assessing the Press Conference transcript, the University's press release, and the *Journal of Electroanalytical Chemistry* (JEAC) Preliminary Note[13] of 10 April, a benchmark can be set in place as follows.[14]

First claim, 89/3. A sustained deuterium-deuterium fusion reaction producing neutron radiation is claimed by F&P for their electrochemical cell operation. Fleischmann also stated that the measured neutron level indicated a fusion reaction rate that was a factor of a billion times too low (10E-9) to account for the simultaneous claim of generated heat energy.[15]

By pretending that they were experimental physicists during a few weeks prior to the announcement, the two chemists made mistakes in their attempt to measure neutron radiation. They erred badly collecting data, and their errors were quickly discovered and emphasized.

During 1988-89, S. E. Jones, a physicist at Brigham Young University, reported the detection of neutrons emitted from an electrolytic solution as a sign of room-temperature, d-d fusion reactions. He has continued these investigations, extending them to deuterated foils, and the Reviewers were provided with one of his most recent publications. Reviewer 6 commented on the low reaction rate: "That is hardly sufficient to provide a significant source of energy."[16] Reviewer 14 noted of this work, "A second class of experiments seeks to find evidence of low energy nuclear reactions, though not necessarily at the rate required to produce significant excess energy."[17] In the same way, the field of CFR views this evidence of low-level, d-d fusion reactions as a scientific curiosity, one quite separate from the excess heat producing phenomenon.

Second claim, 89/3. The F&P experiment claims to demonstrate a source of heat energy heretofore unknown to science. The event is similar to Pierre Curie's claim in 1903 of the heat energy of radium, which can melt its own weight in ice each hour without suffering apparent change. Each claim defied all the prior experience of science.

A calorimeter especially suited to the requirements of the experiment is built into the F&P cell design.[18] The Dewar flask has a silvered neck to hide changes in the electrolyte level, and it requires a hard vacuum to perform properly. By immersing the flask in a bath of water held at a precise lower temperature, 95% of the cathode's heat passes to the bath by radiation. This thermally wideband calorimeter enables a fast rise in temperature —a parameter change known to promote excess heat generation —and, because the heat also departs the cathode fast (by radiation), the temperature can fall rapidly. Additionally, its isoperibolic operation permits a large range of power capacity. Its absolute accuracy is shown to be ± 2 mW, about an order of magnitude improvement over the typical laboratory calorimeter. Considering all, the F&P cell *cum* calorimeter constitute a most desirable instrument for the purpose.

In their Preliminary Note of 10 April 1989, F&P claimed that with cell currents of 8, 64, 572 mA, they measured the generation of 0.036, 0.493, 3.02 Watts of excess heat power respectively.[19] In their seminal paper of July 1990, the experimental run shows a temperature burst phenomenon at day 65 that lasts for 48 hours.[20] The measured energy in the burst is 2 MJ or about ½ kWh, an amount equal to the energy stored in a 60 ampere-hour automobile battery. F&P asserted that given this amount of energy generated over a two day period, a source other than nuclear was "inconceivable."

Measuring the total energy of the experiment from beginning to end, as suggested by a number of the Reviewers, would make the excess energy analysis less, rather than more, meaningful. Both approaches require a thorough search for artifacts that might provide energy from chemical or mechanical storage, or its inference from data reduction procedures. But including the whole experiment raises a confounding consideration. The question of the efficiency of the experiment as an excess power generator —as a source of supply —now must be included in the evaluation. The electrolytic cell might be, and in some experiments appears to be, an especially inefficient supply reactor, especially when compared with newer experiments. After all, some electrolytic cell experiments generate excess heat after the excitation current is turned off —the implication being that the electrolytic action only provides a start-up function.[21] To answer the scientific question: Is there a new phenomenon of excess heat, we should turn to the power-flow, time-slice analysis as the most rewarding analytical approach to data reduction with the electrolytic cell.

Reviewer 7 observes that, "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."[22] Searching and coming up with null results is not the sort of activity that leads to published papers. 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.

The claim that excess heat has been observed is, and was from the beginning, based upon superior calorimetry and excellent data reduction. Considering themselves experts at both calorimetry and electrochemistry, and having worked with the experiment for four years at the time of the announcement, their seminal paper (90/7) confirms that F&P were certain of their energy measurements. It is my observation that their subsequent publications over the next six years support their initial claim.

Excess heat measurements are the starting point of the cold-fusion episode. Note the confusion that results when Reviewer 7 starts elsewhere: "The main problem with this direct-heat scenario is symptomatic in many ways of the entire history of cold fusion. One begins by proposing a very unusual new mechanism, namely d + d fusion at room temperature, that some chemists and solid-state scientists can accept but most nuclear specialists cannot."[23] This is a wrong reading of early events. No one began by "proposing a very unusual new mechanism." One begins at the beginning with the discovery by measurement of a heat energy generation phenomenon previously unknown to science. From that beginning, analysis of the experiment proceeds to the hypothesis that the cell operation contains a source of energy from an unknown nuclear reaction. Science, after several decades, found the source of Pierre Curie's heat, and now it must find the source of F&P's heat.

<u>Third claim, 89/3.</u> No dangerous radiation accompanies the generation of excess heat. F&P apparently were not harmed in four years of experimentation. When testing for dangerous levels of radiation or radioactivity, they found none.

Some Reviewers seemed to work with an unstated assumption that a patched-up, text-book d-d fusion reaction might provide the energy source, rather than an utterly new reaction. Reviewer 12, for example, wonders: "The most puzzling part for nuclear theory is the lack of neutrons commensurate with the heat production and the complete reversal of the ratio for the reaction channels. This is still the crucial and seemingly insurmountable physics problem that needs to be resolved."[24]

A larger view is taken by Reviewer 6, who points out that to begin a review of "Cold Fusion" it is useful to remind oneself of the quote by Dr. Gordon Baym from his article in Phys. Rev. Lett 63,191(1989). "We are searching for new experimental phenomena in an area in which theory must be supported by consistent, systematic data. Any search for 'anomalous phenomena' is, in its early stages an experimentally, not theoretically driven field. It is necessary to stay as close as possible to conventional physics for as long as one can hold out, and only when driven up the wall should theorists invoke new physics."[25] Here, in this third claim, F&P met that "wall." They knew their heat measurements were valid, and that the amount of energy was so great that only a nuclear source could deliver it. They had been living with this "wall" since February of 1985. In March 1989 —their working secret now revealed locally —they were obliged to anticipate the community's reaction by announcing their claim to the world, even though their seminal paper explaining their work was still sixteen months in the future.

There was much misunderstanding in the DOE/ERAB 1989 report on CFR. Its composition never allowed logical space for steady-state or burst heat evidence of an unknown, high-level reaction that needed to be studied further. It did allow that the 89/3 claim number one, low-level d-d fusion at an unusually high rate, should be studied further. Reviewer 10 offers: "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."[26] Exactly. Excess heat and the helium created in its production need to be explained. A new kind of reaction waits to be discovered.

Fourth claim, 89/3. Hypothesis: the heat energy source is an unknown nuclear reaction. I offer two quotes from F&P. The first is of March 22, 1989 (their manuscript date): "It is inconceivable that this could be due to anything but nuclear processes."[27] The second is of July 1990 when they repeated the hypothesis that, ". . . the bulk of the energy release is due to an hitherto unknown nuclear process or processes (presumably again due to deuterons)."[28] Those statements still stand firm in the CFR literature. Given the size of the heat measurements, their conclusion is the only reasonable hypothesis. So, the heat energy emerges from a nuclear reaction presently unknown to science. No detail is offered concerning the type of reaction it might be or what products (nuclear ash) it necessarily must produce.

For example, in the mid-nineties there was concern that the palladium was being consumed by the unknown reaction, and relief was offered by an experimenter in Minsk, Belarus, who found that niobium could be substituted for palladium, and noted that large ore reserves for niobium exist.[29] See also the Iwamura experiment where deuterons seem to form alpha particles that then enter large nuclei. Wilson, et al., in their July 1992 critique, found no calorimeter artifact to obliterate F&P's two MJ energy burst. (That article is worth study, along with the F&P rebuttal.[30]) We can see here that the field of CFR science does not allow a chemical, mechanical, foundry, or data reduction (statistical) explanation to satisfy the excess heat energy measurement.

Reviewer 2 avers, "The excess heat reported remains unexplained. However, there is no evidence for this being a nuclear physics phenomenon"[31] Curie's heat energy from radium was quickly and correctly hypothesized as having the same source as radioactivity. X-Z Li, physicist at Tsinghua U., Beijing, declares, ". . . the usual nuclear technology for neutron or gamma radiation is no longer applicable to detect this low-energy sub-[Coulomb-]barrier resonance. The calorimetric technology in chemistry turns out to be the better choice because the energy released in any nuclear reaction is always there."[32] The hypothesis, of course, comes from elimination of other possibilities for the source of the energy —a logical conclusion given the data. The lack of particle-type evidence is a roadblock for the nuclear physicists. The continuing refusal of some scientists to see heat as a strong indicator of nuclear activity will not stand upright much longer. We are left with the wondrous heat of the F&P experiment and its freedom from obvious and dangerous radiation. The only proper course was to search for a new reaction channel beginning at the announcement of 89/3.

Revolutionary change can come from outside a specialty, as is explained by W.I.B. Beveridge in "The Art of Scientific Investigation:"

> Thus in subjects in which knowledge is still growing... all the advantage is with the expert, but where knowledge is no longer growing and the field has been worked out, a revolutionary new approach is required and this is more likely to come from the outsider. The scepticism with which the experts nearly always greet these revolutionary ideas confirms that the available knowledge has been a handicap.[33]

The reviews demonstrate how some Reviewers judge CFR evidence by its fit, or lack of fit, to the nuclear canon. If it does not fit, it is rejected. If the evidence is rejected, its use to confirm the phenomenon of excess heat is not allowed. "Available knowledge" is thus shown as a substantial inhibition to discovery.

CFR investigations circumvented the lack of particle evidence in the F&P experiment by taking an innovative step. They turned to look broadly, outside CFR, for anomalies in nuclear experiments, and found some to pursue, such as the Kasagi[34] experiment. Digging up anomalies in nuclear experiments proved fruitful.

In summary, we can see from this overview of the four original claims that there are two nuclear reactions occurring within the cathode of the electrolytic cell, one at a high-level producing well-measured heat, and the other at a low-level producing neutrons. The former is an unknown (LENR) reaction channel; the latter is a seemingly conventional d-d fusion reaction channel, but lattice-mediated (LENR) to provide a perceptible rate at room temperature.

Concern for experimental reproducibility (repeatability) sometimes overshadows experimental results in cold-fusion research, both among its practitioners and its critics. Dr. Franco Scaramuzzi, an esteemed hot-fusion physicist who practiced cold-fusion research for fourteen years, expresses this concern, "A well known physicist was asked what he thought of CF. His answer was that it was not good science, because of the lack of reproducible experiments. I wrote to him presenting the following arguments: a) I agree that reproducibility is a "must" in experimental research; b) however, a new field, at it beginning, is often characterized by a lack of reproducibility, and it is the task of the scientists operating in the field to understand what is going on, in order to pursue reproducibility; c) this has been done in the case of CF, making meaningful, even though slow, progress (I sent him a paper of mine in which I discussed this problem). My letter did not produce any effect, in the sense that he did not change his mind, and went on demanding reproducibility, as if it were an intrinsic characteristic of research and not something that has to be pursued."[35] The Reviewer group also found the lack of reproducibility to be a major concern. Reviewer 14: "The lack of reproducibility continues to be a serious problem. None of the important phenomena can be duplicated reliably. This has made it impossible to obtain a quantitative understanding of what is taking place."[36] Reviewer 18: "Although much systematic work has been done on the materials properties that produce a successful cell, the reproducibility is still, at best, only 50%."[37]

This matter is seen as more than an inconvience: Does not a lack of reproducibility tag the field as less than properly scientific? Well, in some specialties of science, it does not. For example, in cosmology, there are no experiments whatsoever, let alone repeatable ones, (although earth-bound experiments do try to partially simulate cosmic events). Nevertheless, cosmology is considered just as scientific an undertaking as other specialties that directly utilize experiments. How, then, in an observational science, are the results to be validated? They are validated, simply enough, by experts who conduct a thorough step-by step review of the data gathering (measurement) process and find therein no error of procedure.

Furthermore, besides placing the specialties of chemistry-physics (with their wholly repeatable experiments) as one category side by side with cosmology as a second category for comparison of their discovery protocols, we can proceed to identify an intermediate position between the two, one that can be occupied by a biological category. We look at the report in *Nature*, Vol, 385, 23 February 1997, page 810, of the cloning of a mammal from an adult cell to produce the sheep "Dolly."[38] This biological experiment, as reported, was eminently not reproducible (repeatable). While the protocol was of the experimental sciences (biology), the application was without repeatability. Of 227 nuclear-transfer starts, in this case, only one live birth resulted —one success and 226 failures in an experimental field.[39] The failed ones were discarded, and the successful one was presented in *Nature* as an accomplishment of *science*. Reproducibility (repeatability) is not a requirement for scientific respectability. (Also, we note here, the experimental failures may not have diagnostic value.) This is the case with "Dolly" and with CFR, which together occupy the protocol space between cosmology and chemistry-physics.

The explanation of this diversity of protocol is that if it is not possible to reproduce the required initial conditions of an experiment, then it is not possible to reproduce the experiment. The "Dolly" experiment uses biological cells that are not exact duplicates of one another to initiate each experiment. The CFR experiment uses metallic cathodes from the foundry that are not duplicates of one another at the atomic-lattice level. In both cases, the exactness of these central elements, or some important aspect of them, establishes the experiment's initial conditions. As mankind does not know how to duplicate these initial conditions, so it is not reasonable to expect to be able to repeat the experiments. Of course, it can still be reasonable to expect to repeat the significant experimental outcomes —to once again clone another sheep, or to once again generate excess heat.

The DOE/ERAB (1989) report recognized this methodology in its preamble, "even a single, short but valid cold fusion period would be revolutionary."[40]

When the limits of current knowledge or technology make an experiment intrinsically not replicable (repeatable), then each experimental run is a new experiment —each is a *solitary* experiment. The protocol for these experiments might require outright discard for those that fail, and (presumably modest) acclaim for those that have an interesting result. Such is the case with CFR. The EPRI/NSF conference in October of 1989, for example, had as its original plan to bring together those experimentalists who had achieved interesting results —apparent excess heat — to compare techniques with one another so as to improve the experiment's design. Those others, who had obtained no interesting result, were welcome, but were not invited, because, presumably, they had little to offer. Science properly follows this protocol when experiments are not repeatable. Progress advances by successful reproduction of the interesting result in similar, though not identical, experiments, and by the meticulous review of experts.[41]

This lack of appreciation of the appropriate protocol by the larger scientific community was exhibited on October 26, 1989, by *Nature* at the end of an editorial about CFR: "Critics, on the other hand, maintain that if you are allowed to keep positive results and throw away the rest you can never be proved wrong: it becomes, as one skeptic put it, religion, not science."[42] This misunderstanding of protocol delayed CFR from enjoying the early attention it deserved. Fortunately, some of the DOE 2004 Reviewers understand this protocol. Reviewer 8 ventures, ". . . not all experiments are created equal. It is unscientific to give all experiments equal weight."[43] Reviewer 12 offers, "We should look at the best available experiments in order to get more information on whether there is some new physics involved."[44] Reviewer 13: "In the current state of the field, finding nothing in a given experiment teaches us nothing . . . "[45]

Several Reviewers did not recognize this entirely proper methodology. Reviewer 1 asserts that, "In my view the references are also culled to present a one-sided view of the current state of experimental results." And further, "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."[46] But, where each experiment is a solitary experiment, failed experiments refute nothing, as with "Dolly." Further, from Reviewer 5: "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"[47] Here, the word, "favorable," is used to establish an illicit protocol. The CFR literature is where the important references are and ought to be. And, from Reviewer 10, "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."[48] Experimental control is not yet available in CFR as is the case with other fields of *accepted* science.

For CFR to achieve reproducibility, experiments must be found that are insensitive to the variations in cathode structure. Progress has been made in this direction by several experimenters moving in the direction of operating at an elevated temperature,[49] using thin films, maintaining a longitudinal electric field in the cathode,[50] and by depositing the palladium on a cathode substrate from the electrolyte.[51] Presumably, such progress will continue. One should also note that, when doing experimental work in a field where initial conditions cannot be replicated, neither the experimenter nor the critic have warrant for complaint —you work with what you have.

I am pleased to note that only Reviewer 13 mentioned the word pathological: "The proponents' assertion that there is reproducibility of 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.'" This is a far cry from September 1989 when a physicist lectured the faculty of the University of Utah to explain that F&P were offering only pseudoscience, the pathological science of Irving Langmuir.[52] In his lecture, that physicist overlooked four scientific conditions Langmuir set forth as definitive of a pathological claim. Let me juxtapose Langmuir's requirements with the 89/3 claims: (1) Output is constant with increasing input, but F&P's paper of April 10, 1989, claims their output excess heat increases with increasing current; (2) The effect is close to the limits of detectability, but F&P measure hundreds of mW with a calorimeter good to 2 mW; (3) There are claims of great accuracy, but F&P make no claim of great accuracy; (4) Fantastical theories are proposed, but F&P propose only a presently unknown nuclear channel. Langmuir says nothing about reproducibility. Perhaps, with this DOE 2004 Report, as published, the field of CFR is now free of pathological criticism.

In their analysis of CFR, I am sorry to note, three Reviewers referred substantively to the necessity, in CFR, for a "miracle." Reviewer 6: "Certainly the weight of the evidence present thus far is not strong enough to overcome the three miracle requirement." And continuing, "This theory [Hagelstein] was apparently developed to explain Huizenga's miracle number 3, concealed nuclear products."[53] Reviewer 17: "If the experimental results of significant energy release in electrolytic cells were correct and the energy release were due to nuclear fusion, the theory would need to explain not one, but two 'miracles.'"[54] And Reviewer 18: "As to the second miracle, all experiments . . ."[55] What can one say other than that such terminology should be avoided. Not only is it unscientific, but in this case also anti-intellectual, not to mention condescending. The usage stems from a 1992-93 book[56] that mistakenly ignored the significance of well-measured heat energy. Its author looked, instead, for a nuclear reaction that was some sort of paste-up variation of the known d-d fusion reaction. As it failed to give consideration to valid heat measurements, so it needed "miracles."

While the quest for a source is not yet answered, the CFR literature has at least two suggestions to indicate a more appropriate kind of exploration than that suggested by "miracles." (1) P. L. Hagelstein, MIT, has written that a strong, uniform, optical phonon field* might impart a considerable angular momentum to two deuterium nuclei (in a compact state, almost touching) thus preventing them from fusing,[57] and thereby opening the way for a different, and maybe slower, kind of reaction between them. (2) X.-Z. Li, Tsinghua University, has written that a combined resonance of the Coulomb barrier and the nuclear well, might produce a slow d-d fusion reaction, one that takes 10,000 seconds for them to fuse into helium four.[58] This is the kind of exploration activity that is needed, whatever the outcome. Missing from it are the physics departments of MIT, Princeton, Caltech, UC Berkeley, Austin, Harvard, Rochester, and so forth.

Heat is the presenting symptom of nuclear reactions in this field of study. Reviewer 13 tells us, "The only normalizing measurement seems to be heat generation." He continues, "Without the measurement of heat generation I don't think any experiment is going to be convincing. How do you know anything —of low energy nuclear reaction interest such as cold fusion —is going on?"[59] Reviewer 15 also emphasizes this order of procedure, "The question of excess heat is tied up with the production of nuclear products and so one first must be convinced that excess heat is produced."[60] I can recall how astonishing it was in the spring of 1989, to see the hours spent measuring neutron emission from experiments with no assurance they were generating excess heat, not to mention the number of columns in *Nature* devoted to reporting those measurements. It would seem that some Reviewers looked for evidence for nuclear reactions in order that they might better appreciate the evidence for excess heat —they put the cart before the horse.

The physicist, Y. Arata, Osaka University, tells us that it was two years before he mastered the generation of excess heat; Dr. M. Miles, an electrochemist, took six months. Dr. McKubre spent months studying up on calorimetry. CFR requires of the dedicated scientist many months of study to come to the point of appreciating the reality of the calorimetric measurement of the excess heat phenomenon. This emphasis on calorimetry will persist and grow. Future

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* The cathode of an electrolytic cell is thought to develop a strong, optical phonon field because of the electronic and chemical reactions at its surface interface.

generations of nuclear physicists may have to specialize not only in particle counting, but also in calorimetry.

We hear often that CFR scientists are motivated by the prospect of an endless supply of "clean" fuel. Indeed, that is the motivation for fifty years of hot-fusion research, where it is known that deuterium is the fuel. This interest in a clean fuel, however, is not enough for the working scientist to commit a large part of his career to either hot or cold fusion research. It was the firm knowledge of the properties of d-t, and other, fusion reactions, along with a more generalized belief that in twenty years or so a commercial reactor could be realized, that offered intellectual motivation to scientists at the beginning of hot-fusion research. The phenomenon of excess heat is the motivating knowledge in CFR.

But how could a scientist be sure that the excess heat energy really existed — that the phenomenon was real —sufficiently to commit his career to it? A few knew by producing it themselves in their laboratory, and by being absolutely certain about the quality of their calorimetry. Some had to study the work of other experimenters. (In the early days, Hagelstein preceded his theoretical talks with the statement, "I believe that excess heat exists.") In either case, about a year is required because of the multi-disciplinary nature of the new science. Those who accept the reality of excess heat find their position to be much like that of Pierre Curie in 1903 —they have no knowledge of the energy source. With the conviction that excess heat is a newly discovered natural phenomenon, there is a place for those who find inspiration by working in an entirely new field of science. As Reviewer 8 expressed it, ". . . we've got the start of a science."[61]

The Review did not emphasize excess heat, but the Report noted that about half the Reviewers found the evidence for "excess power" compelling. If (those who can be identified as) the nuclear physicists are set aside for the moment, two-thirds find the evidence for excess heat compelling. When the Review asked about low-level nuclear reactions (LENR), fully half the Reviewers replied that they recognized evidence for LENR in the papers studied. All Reviewers [except maybe one] called for continuing research support by government funding agencies.

Now, after sixteen years in intellectual isolation, there is regained a chance that the field will be adequately recognized by the scientific community.

* END *

Endnotes:

1. The DOE documents "Report of the Review of Low Energy Nuclear Reactions," and "Charge Letter to Reviewers," may be located at http://www.science.doe.gov/Sub/Newsroom/News_Releases/DOE-SC/2004/low_energy/. For the Reviewers comments, "2004 U.S. Department of Energy Cold Fusion Review Reviewer Comments," see www.lenr-canr.org, or www.newenergytimes.com.
2. This Miley story can be found in EH-2 at page 142.
3. DOE, U.S. Government, Reviewer Comments (Eighteen in number), CF Review, Dec. 1, 2004, Washington, DC, Reviewer #07, page 13-17.
4. Ibid., Reviewer #07, pg 17.
5. DOE, U.S. Government Report (Final) of the Review of Low Energy Nuclear Reactions, Dec. 1, 2004, Washington, DC, page 3 (See Charge Element One).
6. Seife, Charles; "Outlook for cold-fusion Is Still Chilly," *Science*, 306, 10 Dec 2004, pg. 1873.
7. DOE, U.S. Government, Reviewer Comments (Eighteen in number), CF Review, Dec. 1, 2004, Washington, DC, Reviewer #10, page 22.
8. Ibid., Reviewer #15, page 31.
9. Reviewer 15 may be thinking of the N.S. Lewis report of the Caltech work as reported in Nature, 1989, v340, p525-530, 17 Aug 1989, Lewis, N.S.: "The D/Pd stoichiometries of 0.77, 0.79 and 0.80 obtained from these measurements were taken to be representative of the D/Pd stoichiometry for the charged cathodes used in this work." That statement reported a maximum loading of 0.80 for the work presented at the American Physical Society meeting in Baltimore on May 1, 1989, when the press was assured that there was "absolutely" no such thing as excess heat. (See also, EH-2, chapters five and six.)
10. Ibid., Reviewer #18, page 42.
11. DOE, U.S. Government, Reviewer Comments (Eighteen in number), CF Review, Dec. 1, 2004, Washington, DC, Reviewer #08, page 18.
12. There exist (1) video and transcript records of the press conference [CGB accession number 534+/10, and transcript, 625+/07], (2) press release by the University, [CGB accession number B09+/26], (3) University of Utah patent applications, and (4) Fleischmann, Pons, Hawkins, "Electrochemically Induced Nuclear Fusion of Deuterium," *J. of Electroanal Chemistry*, Apr 10, 1989; 261-2A, pgs. 301-8.
13. Fleischmann, Pons, Hawkins, "Electrochemically Induced Nuclear Fusion of Deuterium," *J. of Electroanal Chemistry*, Apr. 10, 1989, 261-2A, pgs 301-8.
    Fleischmann, Pons, Hawkins, Errata, *J. Electroanal Chemistry*, 263, 187 (1989), May 10, 1989.
14. Keep in mind that their seminal manuscript describing the experiment and calorimetry in detail was submitted in December 1989, and published in July, 1990: Fleischmann, Martin, Pons, Stanley. Anderson, Mark R., Li, Lian Jun, Hawkins, Marvin, "Calorimetry of the palladium - deuterium - heavy water system," *J. Electroanalytical Chemistry*, 287(1990) 293; Jul 25, 1990. See also, Wilson, R.H., et al., "Analysis of Experiments on the Calorimetry of LiOD-D2O Electrochemical Cells," *J. Electroanalytical Chemistry*., vol 332, 1992, p 1-31. See also, Fleischmann, Pons, "Some Comments on the Paper Analysis of Experiments on Calorimetry of LiOD/D2O Electrochemical Cells, R.H. Wilson, et al.," *J. Electroanalytical Chemistry*., vol 332, 1992, pp 33-53.
    Technical Note: Their experiment in electrolytic chemistry resides in a tall, narrow (3.3 cm. i.d.) Dewar (with hard vacuum) flask filled with an electrolyte, which is topped-off daily. A centered cathode of palladium and a peripheral anode of platinum are immersed in the liquid and connected to an external (direct current) power source. It is operated at large currents for ten weeks to watch for interesting effects. Only in July 1990 did the scientific world learn of the experiment that was announced sixteen months previously.
15. See a transcript of the Press Conference, March 23, 1989, for this item. (Video at CGB archive accession number 534+/10. Transcript at accession number 625+/07.)
16. DOE, U.S. Government, Reviewer Comments (Eighteen in number), CF Review, Dec. 1, 2004, Washington, DC, Reviewer #06, page 12.
17. Ibid., Reviewer #14, page 28, (bottom).
18. Fleischmann, Pons, Anderson, Mark R., Li, Lian Jun, Hawkins, Marvin, "Calorimetry of the palladium - deuterium - heavy water system," *J. Electroanalytical Chemistry*, 287(1990) 293, Jul 25, 1990.
19. Data from JEAC, 10 Apr 1989, Preliminary Note, Table I, pg. 304.
20. Fleischmann, Pons, Anderson, Mark R., Li, Lian Jun, Hawkins, Marvin, "Calorimetry of the palladium - deuterium - heavy water system," *J. Electroanalytical Chemistry*, 287(1990) 293; Jul 25, 1990, Fig, 9a, pg 316.

21. Mengoli, G., Bernardini, M., Manducchi, C., Zannoni, G., "Calorimetry close to the boiling temperature of the D2O/Pd electrolytic system," *J. Electroanalytical Chemistry*, 444 (1998) 155. See also, EH-2, Chapter 15.

22. DOE, U.S. Government, Reviewer Comments (Eighteen in number), CF Review, Dec. 1, 2004, Washington, DC, Reviewer #07, page 13.

23. Ibid., Reviewer #07, page 13.

24. Ibid., Reviewer #12, "puzzling," page 25 (bottom);

25. Ibid., Reviewer #06, page 10, 13.

26. Ibid., Reviewer #10, page 23.

27. Fleischmann, Pons, Marvin Hawkins, "Electrochemically induced nuclear fusion of deuterium," *J. Electroanalytical Chemistry*, 261 (1989) April 10, 1989, p. 305.

28. Ibid., page 308.

29. Filimonov, Veniamin A., Kobets, Vyacheslav, Skitovich, Alla V., "Self-organization processes under metals loading by hydrogen isotopes (materials science basis for cold fusion and transmutation technologies.)," Inst. for Physical Chemical Prob., Belarus State U., Minsk, Belarus,

30. Wilson, R.H., et al., "Analysis of Experiments on the Calorimetry of LiOD-D2O Electrochemical Cells," *J. Electroanalytical Chemistry*., vol 332, 1992, p 1-31. See also, Fleischmann, Pons , "Some Comments on the Paper Analysis of Experiments on Calorimetry of LiOD/D2O Electrochemical Cells, R.H. Wilson, et al.," *J. Electroanalytical Chemistry*., vol 332, 1992, pp 33-53.

31. DOE, U.S. Government, Reviewer Comments (Eighteen in number), CF Review, Dec. 1, 2004, Washington, DC, Reviewer #02, page 2.

32. Li, Xing-Zhong, "Nuclear Physics for Nuclear Fusion," *Fusion Science and Technology*, Vol. 41, No. 1, January 2002, page 67, col 2, on calorimetry.

33. Beveridge, William I. B., *The Art of Scientific Investigation*, New York, Vintage Books, Third edition, 1957, p 6.

34. Kasagi, J., H. Yuki, T. Itoh, N. Kasajima, T. Ohtsuki, A.G. Lipson, "Anomalously Enhanced D(d,p)T Reaction in Pd and PdO Observed in very Low Bombarding Energies," ICCF-7, Vancouver, Canada, April 1998, p 180.

35. Scaramuzzi, Franco, "Ten Years of Cold Fusion: An Eye-Witness Account," *Accountability in Research*, Scott R. Chubb & A.E. Shamoo, Editors, Vol. 8, Nos 1-2, (2000), Gorden & Breach Science Publishers, Philadelphia, PA., ISSN 0898-9621, p 2.

36. DOE, U.S. Government, Reviewer Comments (Eighteen in number), CF Review, Dec. 1, 2004, Washington, DC, Reviewer #14, page 28.

37. Ibid., Reviewer #18 page 43.

38. Wilmut, I., et al., Nature, Vol, 385, 23 February 1997, page 810.

39. Pennisi, Elizabeth, *Science*, 288, 9 June 2000, page 1724, Box.

40. DOE, ERAB Panel on Cold Fusion, "Final Report of the Cold Fusion Panel, Dept of Energy, Washington, DC, Nov 8, 1989, Preface (Norman Ramsey), p 2.

41. Ashworth, David, et al., *Nature*, Vol, 394, 23 July 1998, pg. 329. Signer, Esther N., et al., *Nature*, Vol, 394, 23 July 1998, pg. 329.

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43. DOE, U.S. Government, Reviewer Comments (Eighteen in number), CF Review, Dec. 1, 2004, Washington, DC, Reviewer #08, page 19.

44. Ibid., Reviewer #12, page 25.

45. Ibid., Reviewer #13, page 27.

46. Ibid., Reviewer #01, page 01.

47. Ibid., Reviewer #05, page 10, top.

48. Ibid., Reviewer #10, page 22.

49. Mengoli, G., Bernardini, M., Manducchi, C., Zannoni, G., "Calorimetry close to the boiling temperature of the D2O/Pd electrolytic system," *J. Electroanalytical Chemistry*, 444 (1998) 155.

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53. DOE, U.S. Government, Reviewer Comments (Eighteen in number), CF Review, Dec. 1, 2004, Washington, DC, Reviewer #06, page 13.

54. Ibid., page 40.

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56. Huizenga, John R., 1992, *Cold Fusion: Scientific Fiasco of the Century*, U. of Rochester Press, pp 259. See also, Huizenga, John R., 1993, *Cold Fusion: Scientific Fiasco of the Century*, 2nd Edition (pbk), Oxford University Press, NY, especially the new last chapter.

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59. DOE, U.S. Government, Reviewer Comments (Eighteen in number), CF Review, Dec. 1, 2004, Washington, DC, Reviewer #13, page 27-28.

60. Ibid., Reviewer #15, pg 33.

61. DOE, U.S. Government, Reviewer Comments (Eighteen in number), CF Review, Dec. 1, 2004, Washington, DC; Reviewer #08, page 18.