How *Nature* refused to re-examine the 1989 CalTech experiment

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Abstract

In 1989 *Nature* published a paper by Lewis [1] showing no excess heat in a cold fusion experiment. Several researchers including Noninski, Miles and Fleischmann discovered errors in this paper. Noninski wrote a critique of the paper describing one of these errors, and submitted it for publication. David Lindley, an editor at *Nature*, rejected the critique. This paper examines some of the errors in the paper, and Lindley's reasons for refusing to re-examine the experiment.

Background

In May 1989, Lewis *et al.* at Caltech published a paper in *Nature* describing a failed attempt to replicate the cold fusion effect. [1] This, along with the negative reports from Harwell [2] and MIT [3] are often cited as proof that cold fusion does not exist. All three were subsequently shown to be ambiguous, and marginally positive:

Harwell. Researchers at Harwell invited outside experts who re-examined the data and found some evidence of anomalous excess heat. [4]

MIT. A copy of the raw data trace from a pen recorder showed some signs of unexpected heat. The pen recorder data was converted to one-hour round points in the published version. These points were moved down to the zero line, and some were moved to the left and right, suggesting that the change was made manually by a person rather than a computer. This led some people to suspect the data was tampered with. [5, 6] See Ref. 5, pp. 21 - 24. Storms thinks the apparent heat in the original data is not significant. It is only instrument noise. [7] However, it should not have been erased in the published version.

CalTech. The results from Caltech may have been positive. Lewis thought they were negative but in the opinion of several other researchers, he misinterpreted his own data. That is the subject of this analysis.

The *New Energy Times* has a well-organized list of papers, excerpts, and full-text papers describing Harwell, MIT and Caltech. [8]

There were several problems with the Lewis study. It was not continued long enough to allow high loading. The size and shape of the cell was not recommended because it did not

promote stirring. This meant they had to use a magnetic stirrer, which added noise to the system. The worst problem is that Lewis made a simple mistake in analysis. He thought the heating coefficient (the calibration constant) was changing as the experiment proceeded. He thought that at the beginning of the test, 1 watt of electrochemical power caused the temperature to rise 14.0°C, and later that same power caused the temperature to rise 15.9°C, 14% higher. While it is conceivable that happened, that would mean the instruments were malfunctioning or the cell was configured wrong, so the experiment should have been done over. It is more likely that the instruments were working correctly, and the higher temperature was caused by 14% anomalous excess heat added to the electrochemical power.

Instruments sometimes do "drift," gradually changing as an experiment proceeds. These were professional grade instruments so they probably did not do this. A change in the calibration constant with the same glassware other components in the same configuration is unheard of, and probably physically impossible. Furthermore, this only happened with heavy water in the cell; in a control test with ordinary water the heating coefficient did not change. So, the most likely explanation is that the temperature increase was real, and it was caused by anomalous heat. The experiment worked. Miles described this in Ref. [5], p. 20:

> The most disturbing aspect of the Caltech report concerns the heating coefficient in a Pd/D_2O+0.1 M LiOD experiment that was allowed to increase with time from 14.0 KW^{-1} (0.0714 WK^{-1}) at 14.7 hours to 15.9 (0.0629 WK^{-1}) KW^{-1} at 115.0 hours. This could just as well have been interpreted as a 13.6% excess power effect. Experiments at Caltech for Pd/H_2O + 0.1 M LiOH gave lower heating coefficients (h = 12.5 ± 0.7 KW^{-1}) that did not show this large increase with time. Using a constant h value of 14.0, KW^{-1} yields excess power that increases with the electrolysis time up to 76 mW [9]. The excess power density of 1.0 W/cm^3 Pd for an analysis of the Lewis study [19] is in excellent agreement with reported excess power in other F-P experiments using similar current densities [10, 11].

Lewis may have observed the same level of excess heat that Fleischmann and others did with similar materials and electrochemical conditions. But instead of concluding that he was seeing 14% excess heat, Lewis concluded that the instrument had changed 14%. He did not specify a reason why it might have changed, and he did not perform recalibration tests that would confirm the change and pinpoint the source of the error in the instruments.

This was not a clear-cut result. If there was excess heat, it was marginal. There are reasons to think it was an instrument error. McKubre pointed out that at both CalTech and MIT the cathodes were only loaded to around 80%, which is not high enough to produce heat. (See p. xvii in Ref [12])

Noninski & Noninski (father and son) found another problem in Lewis' calorimetry. Lewis tested the heavy water cell in two different states. In the first state, the cell had only electrolysis power going into it. In the second state, later on, electrolysis power was reduced and an electric heater was turned on to compensate. The total power going into the cell was the same in both

states. The cell reached the same temperature in both conditions, so the total output heat was the same. Lewis assumed that if electrolysis power is lower, and current density is lower, anomalous cold fusion power – if present – must also be lower. There were reports that anomalous power is proportional to current density. Lewis assumed that the cell in second state would have to be cooler if anomalous power was present, even though total power input into both was balanced. However, this is not true. While it is true that generally speaking, anomalous power is proportional to current density, there are other control parameters, and this one does not always dominate. Anomalous power from cold fusion occurs at different ratios to input electrolysis. This was already known in 1989, and subsequent studies confirmed it. Anomalous power, once it begins, does not always change when you reduce electrolysis power. The change may be delayed. Indeed, in some cases you can turn off electrolysis completely and anomalous power continues by itself for a while, at a stable level, in what is called "heat after death." [13] Electrolytic conditions give rise to the effect, but the effect then continues independently of those conditions. By testing with a compensation heater Lewis proved only that anomalous power – if present – was a small fraction of input power; that it was not strictly proportional to the input power; and that it was fairly stable. Miles described this in Ref. [14] equations 5 – 8.

Noninski submitted a letter to the editor to *Nature* describing the problem. The editors at *Nature* sent the submission out for peer-review. It was rejected in the first round. At Noninski's request it was sent out again, this time to Lewis himself. Lewis rejected the critique of his own paper. David Lindley, an editor at Nature, sent a final rejection to Noninski, with the comments by Lewis attached. In 1993, Noninski published a similar letter in *Fusion Technology*. [15]

Lewis described the situation:

> We also sometimes observed abrupt or gradual changes in the rate of heat loss from these cells, presumably resulting from a change in the rate and/or form of gas evolution. [16] These changes often resulted in a sustained temperature rise of the cell (which might be interpreted in terms of the onset of excess enthalpy production), but recalibration with the load resistor method during this period showed no evidence for any anomalous power production, even after the reported activation period for the Pd rods [17, 18] had been exceeded.

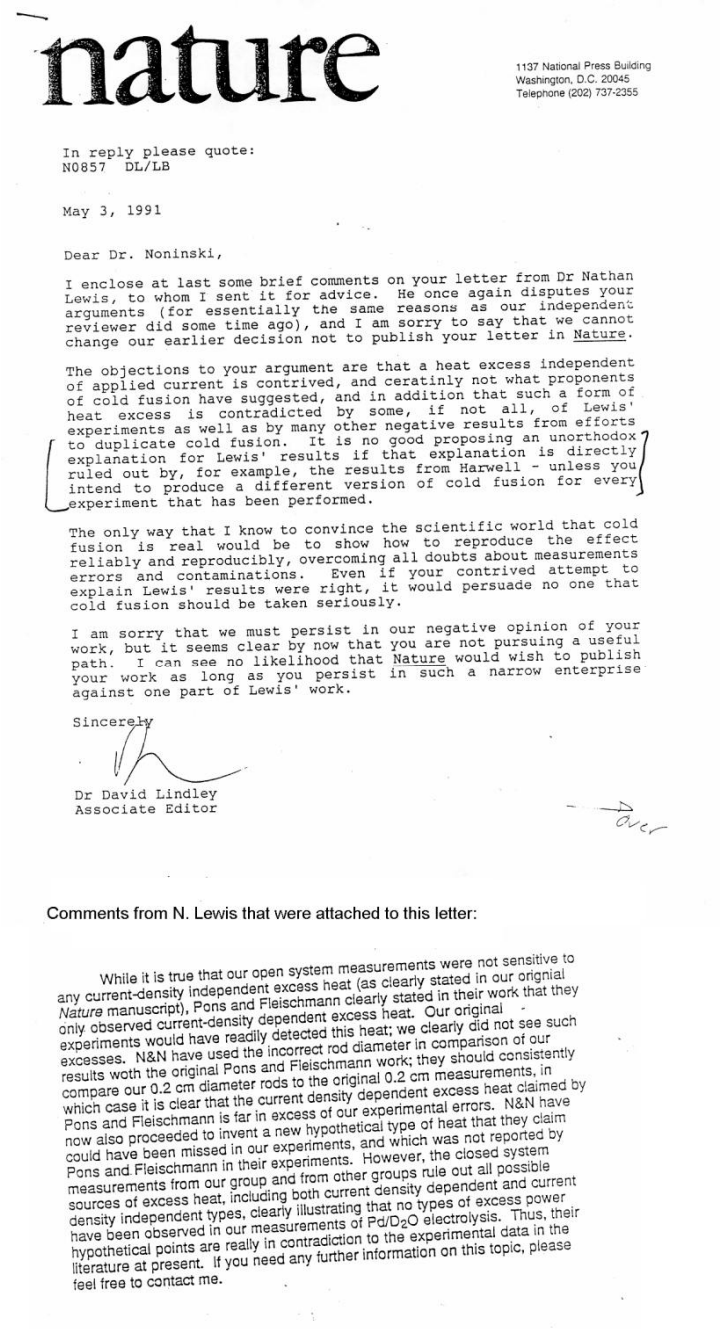
Noninski was saying that "recalibration with a load resistor" would not reveal any change in temperature if the anomalous power remains steady during the period. It probably would remain steady, even if electrolysis power was reduced somewhat.

I would add that if Lewis thought the "abrupt changes in the rate of heat loss" were caused by a change in the rate of gas evolution, he should have measured gas evolution with a gas flowmeter. He should not have presumed this explained the change. An "abrupt change in the rate of heat loss" might just as easily be an abrupt increase in anomalous heat. You cannot tell the difference between these two phenomena unless you measure how much gas is leaving the cell. The rate of gas evolution does not generally change in a cell where the components are not

moved and power levels are unchanged. This paragraph gives the impression that Lewis saw anomalous heat then went out of his way to avoid looking for the cause of it.

Lindley Letter

Figure 1 shows a facsimile of the letter. The text is transcribed below.



nature

1137 National Press Building
Washington, D.C. 20045
Telephone (202) 737-2355

In reply please quote:
N0857 DL/LB

May 3, 1991

Dear Dr. Noninski,

I enclose at last some brief comments on your letter from Dr Nathan Lewis, to whom I sent it for advice. He once again disputes your arguments (for essentially the same reasons as our independent reviewer did some time ago), and I am sorry to say that we cannot change our earlier decision not to publish your letter in Nature.

The objections to your argument are that a heat excess independent of applied current is contrived, and ceratinly not what proponents of cold fusion have suggested, and in addition that such a form of heat excess is contradicted by some, if not all, of Lewis' experiments as well as by many other negative results from efforts to duplicate cold fusion. It is no good proposing an unorthodox explanation for Lewis' results if that explanation is directly ruled out by, for example, the results from Harwell - unless you intend to produce a different version of cold fusion for every experiment that has been performed.

The only way that I know to convince the scientific world that cold fusion is real would be to show how to reproduce the effect reliably and reproducibly, overcoming all doubts about measurements errors and contaminations. Even if your contrived attempt to explain Lewis' results were right, it would persuade no one that cold fusion should be taken seriously.

I am sorry that we must persist in our negative opinion of your work, but it seems clear by now that you are not pursuing a useful path. I can see no likelihood that Nature would wish to publish your work as long as you persist in such a narrow enterprise against one part of Lewis' work.

Sincerely

Dr David Lindley
Associate Editor

over

Comments from N. Lewis that were attached to this letter:

While it is true that our open system measurements were not sensitive to any current-density independent excess heat (as clearly stated in our orignial *Nature* manuscript), Pons and Fleischmann clearly stated in their work that they only observed current-density dependent excess heat. Our original experiments would have readily detected this heat; we clearly did not see such excesses. N&N have used the incorrect rod diameter in comparison of our results woth the original Pons and Fleischmann work; they should consistently compare our 0.2 cm diameter rods to the original 0.2 cm measurements, in which case it is clear that the current density dependent excess heat claimed by Pons and Fleischmann is far in excess of our experimental errors. N&N have now also proceeded to invent a new hypothetical type of heat that they claim could have been missed in our experiments, and which was not reported by Pons and Fleischmann in their experiments. However, the closed system measurements from our group and from other groups rule out all possible sources of excess heat, including both current density dependent and current density independent types, clearly illustrating that no types of excess power have been observed in our measurements of Pd/D_2O electrolysis. Thus, their hypothetical points are really in contradiction to the experimental data in the literature at present. If you need any further information on this topic, please feel free to contact me.

Figure 1. Letter from David Lindley. Comments from Nathan Lewis were Xeroxed on the back of the page. The vertical lines marking paragraph 2 were added by Melvin Miles, pointing out what he considers the worst problem. The text is transcribed below.

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Analysis of Letter

Many aspects of this letter violate elementary principles of experimental science, but let us begin with the first paragraph, which violates the standards for peer-review. Lindley says that he sent the critique to Lewis himself for "advice." In other words, he asked Lewis whether a critique of his own paper should be accepted or rejected, and Lewis decided that his own work was fine. This is not quite as bad as it looks. The paper was rejected by an "independent reviewer" in the first round. As I recall, this letter was sent after the second or third round. Noninski tried to rewrite the paper to satisfy the independent reviewer. In the later round, Lindley decided to skip the independent review and have this paper checked by Lewis directly.

The following paragraphs have many egregious technical errors.

The message is confusing; you may have to read the second paragraph through several times before you realize what Lindley is saying, and what he demands of Noninski. Here are some of the assertions Lindley has packed into these short but telling sentences:

1. Lindley demands that Noninski find a single reason, in the form of "an equation" that would simultaneously prove that all negative experiments, including Harwell and others, are actually positive.
2. In other words, Lindley asserts that all cold fusion experimental results are uniform. The experiments all produced the same result. One explanation must account for all of them. Lindley rejects the idea that some null experiments failed for one reason and some for

> another. It seems this idea never crossed his mind. He thinks that all experiments produce a single yes or no result that can only be explained by a single set of equations. The effect either exists or does not, and all experiments automatically prove the issue one way or another.

In reality, Lewis probably got positive heat but he made mistakes in his analysis, so he did not recognize it. In many other experiments, the result was actually negative for various different reasons. Lewis made a mistake in his equations and his assumptions about how cold fusion works, but many other researchers used in the proper equations and actually did get a negative result. Most got mixed results; some cathodes worked, and others did not. Noninski did not prove that other negative results were actually positive. He never set out to do that, or claimed he had done that. He did not even address these other experiments. But Lindley assumed this is what Noninski was trying to do.

We assume that the wide variety of puzzling and varying results, both positive and negative, indicate that the experiment is complicated and that it is difficult to understand what is happening. Again, it seems this scenario never occurred to Lindley.

Lindley does not grasp how complicated this experiment is; how many different outcomes it can produce, and how many ways it can fail. For example, in some cases it did not work because the cathodes cracked; or because researchers did not wait long enough for the cathode to load; or because the surface was contaminated. There were many other reasons it might fail, and many unknown factors. In most laboratories they tested several cathodes. Some would work, but most would fail. It was later determined that the cathode material varies a great deal and this is the main controlling factor. [19] [1] Researchers thought they were doing an exact replication, but they were not, because they did measure critical control parameters such as loading or OCV. [20] In his response, Lewis also fails to realize that he may not have replicated, and the conditions in his experiment might be quite different from those in Fleischmann's. He is an electrochemist, so he should know this. He should have known that the excess heat is not only dependent on current density, but also on loading and other control parameters. He should have realized that although current density is a control factor, it may have some latency; there must be other control factors; and some of them had not yet been discovered. The data in Fleischmann's first paper shows wide variations in the heat compared to the same current density, from 0.6 to 26.8 W for 512 mA/cm^{-2}. It generally shows increased power with current density, but it does not say the increase is instantaneous or automatic. McKubre later showed that although anomalous power does generally rise with current density, the correlation is not perfect or instantaneous (Fig. 2). [21]

[1] See especially p. 43, Table 10.

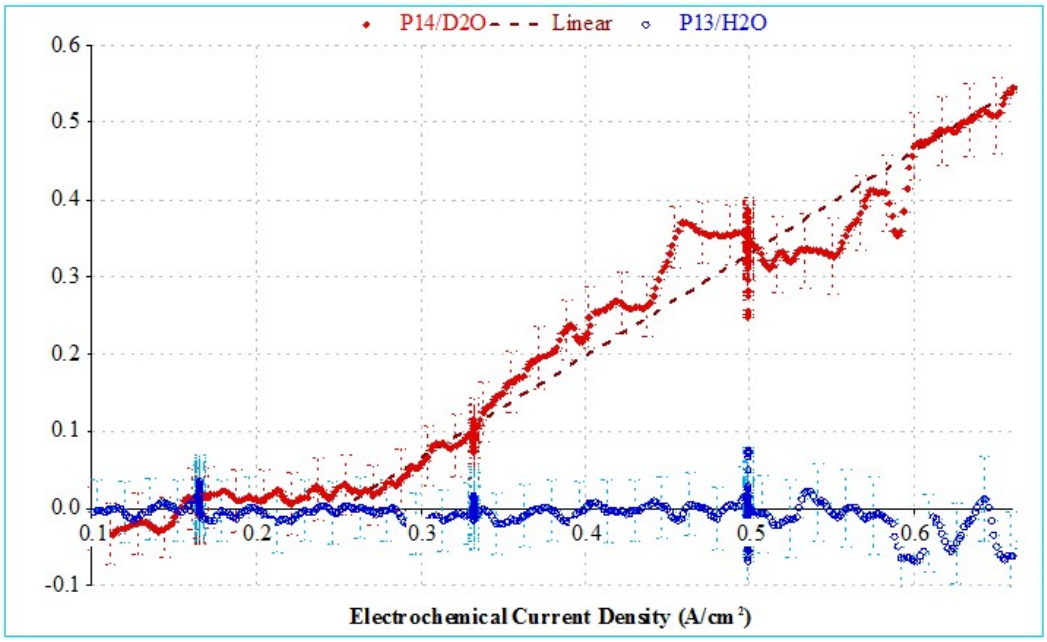

Figure 2. Simultaneous series operation of light and heavy water cells; excess power versus current density.

In the third paragraph Lindley demands that the effect be produced "reliably and reproducibly." This is the goal of any research project, but it cannot be the initial demand. Most phenomena are unreliable and irreproducible at first. The whole point of scientific research is to make them reliable.

In the last paragraph, Lindley says that Noninski's methods are "unorthodox" and that he is trying to make a special case, or invent new physics. To the contrary, Noninski is only asserting that ordinary, conventional equations should be applied. Noninski is saying that Lewis made a mistake.

It is astonishing that an editor at *Nature* could be so ignorant of how experiments are conducted, how varied and complex they are, and how people go about interpreting the results. Lindley seems to have no understanding of experimental science.

Lindley's tone in this letter strikes me as supercilious. It is mocking. He would agree it is; he favors attacking cold fusion with "unrestrained mockery, even a little unqualified vituperation." [22] This is highly inappropriate. It is a violation of academic traditions.

As of 2012, Lindley and Lewis have not changed their opinions of cold fusion. They still claim that the CalTech results definitively proved that cold fusion does not exist.

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