Working

MIT
Department of Physics, Room 6-205
October 14, 1991

To: Charles Vest, President

CONFIDENTIAL

From: Philip Morrison , Institute Professor (emeritus)

Assessing the Request for an Inquiry by Dr. Eugene Mallove, made August 18,1991

I. My Standing in the Matter

In the presence of clear concern for equity in treatment, I begin by stating my own qualifications and limitations as an assessor.

A theorist with considerable experimental experience--though to be sure, most of it gained long ago--I think I am qualified as a referee of the overall methods and content of a paper on cold fusion, denoted Plasma Fusion Center PFC/JA-89-34, prepared and published by an MIT team from PFC and three other MIT departments, with sixteen co-authors (Journal of Fusion Energy, 9, 133-148, 1990). It is this paper that is the focus of the substantive issues here, issues that grew in interest well after the summer of 1990.

I shall not comment on the relationships between PFC research people and the media during the heated summer of 1989, though that is also part of the request for inquiry. Others may consider them.

The topic crosses disciplines; indeed, that is one of the sources of trouble. My background in nuclear physics is strong, but I am not an electrochemist. Much of my general grasp of that specialty has been gathered during the cold fusion controversy itself within the last two years. I regard my task as that of an interested scientific reader, but not one who could propose detailed improvements in the entire experiment design. The editors who found referees for the paper would probably seek more than one referee. That understood, I shall support my opinions with inferences drawn from the paper and the criticism of it offered in Dr. Mallove's submission.

I know several of the participants in this dispute, none of them better than I know Dr. Mallove himself. His astronomical specialization and interest in science education and science journalism have brought us together over perhaps a decade, before and during his stay at MIT. Dr. R. Petrasso of the PFC, a nuclear experimenter, is the second person involved whom I know rather well. Our interaction extends over a few years, about a year longer than the cold fusion era. Of course I know the Provost and other more senior members of the team, but not with so much contact as with Mallove and Petrasso. I do not think I hold any sort of animus for any of these colleagues.

Toward the idea of cold fusion itself I was rather more tolerant and optimistic than most physicists. I still believe that there may be a germ of novelty in some electrochemical phenomenon that is caught up in this complex system; it is very unlikely, though logically possible, that new findings, if established, would turn out to have high economic importance. They would at most open some way to build a new battery, possibly a fuel cell.

II. The Substantive Issues

The papers before me are somewhat tangled. The core of the topic is the publication in the JFE. It is here, at least in part, in four distinct versions: the published paper, the MS finally submitted to the JFE and dated July 1990, and two partial drafts of that MS, both by S. Luckhardt of PFC, with signs of much comment by co-authors. The drafts are dated July 10 and July 13, 1989.

With 16 authors, no paper will have a simple history. This one shows that to be true; the published version itself is not the same as the MS submitted, but bears strong mark of editorial changes, in text and in figures. All of this is entirely to be expected.

Two distinct experiments are reported. Phase I was "hastily assembled ", within days after the initial TV show from Utah. It sought both real-time nuclear radiation and excess electrochemical power. Phase II was more ambitious, designed as a much more sensitive test of all Utah claims by rough replication, and extended over a couple of months.

All MIT results were negative: real-time radiations, atomic products of fusion within the cells, and excess electrochemical power. In addition, a telling technical criticism of the published Utah gamma-ray data is included. I could find no claim by the MIT authors that was not well supported by the data they include.

III. The Ground for Complaint

What then is the ground of complaint? It is not without a logical basis. The sensitivity of the MIT tests for nuclear radiations is improved by two or three orders of magnitude over the Utah experiments. Even stronger limits follow from the search for certain fusion product atoms. In mid-1989 the Utah claimed such products in ample amount, easy to find. For the most part by 1991 those side effects were no longer claimed, and the main evidence cited was excess power during electrolysis of heavy water. It is perceived that a new form of fusion might escape all side branches to deliver energy as lattice heat alone, making helium as the only product. Even this seems limited by this experiment to a power down from the Utah claim by a thousand or so.

But the Utah investigators are electrochemists, and skilled in calorimetry. In that domain--if not in gamma-ray or neutron detection --they worked closer to the state of the art. The MIT team claimed an intrinsic sensitivity of their own calorimetry no better than 10 mW, from the noise visible in all their power measurements. They claimed overall only a conservative 40 milliwatt sensitivity, about 3% of total power, allowing for cumulative systematic changes as the open cells ran for a week or more, losing water and gas.

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(Uncontrolled catalytic recombination of the oxygen and hydrogen produced gas is a source of possible excess power in the right range.)

The MIT estimate of the excess power expected using the scaling methods of Utah applied to the MIT electrodes and current was 80 milliwatts. The point is clear: the expected nuclear products are excluded by the MIT paper at a level down from the Utah claims by several orders of magnitude. But MIT could limit the excess calorimetric power only by less than one order of magnitude, a factor somewhere between two and five. That entails much closer scrutiny of the much less precise results of calorimetry.

The hope of the optimists--Mallove is not the only one--is that the assumptions, plausible as they are, that fusion has to proceed at least in part along known channels, is somehow wrong. Heat is the most general (and desired) product. (Helium appears to me almost equally robust; if it is not made, the process is hardly fusion. It is possible that helium is lost from the cells.)

IV. The Two Drafts Differ

The focus of Mallove's criticism is on a difference between the two partial MIT draft manuscripts. In the earlier one (Attachment 5) the excess power is corrected by a simple linear fit for long-term drift and plotted, with many points, both for heavy water and the light water control cell. I measured on the MS graphs the mean excess power over time for both cells, using for each the area of the region between the locus of zero power and the noisy data.

The mean power excess: heavy water cell +15 milliwatts
light water cell + 4 milliwatts
Neither result is significant, for the claimed power error is 40 mW, and even a less stringent definition would put the error at 20 or 30 mW.

The 13 July draft fits a less simple, least square correction to eliminate the drift, and ends up with a value I did not measure, but one visibly close to zero for both cells. The claimed error is not reduced; such small variations in the mean are simply not significant, whether they turn out zero or not.

The first hint of a small positive excess power in the heavy water cell is a source of encouragement for those who expect power from fusion, and its disappearance between drafts is the burden of Mallove's concern. But it might well have been that the new correction would have shown the other sign. After all, the light water cell also indicated an excess power at first, if smaller still. These are matters of chance at this level of power.

Objectively, one has to say that the published paper does not mislead; the change in power between drafts is rather smaller than the clearly stated and plausible overall error, at most a little outside of random error alone. The disappointment of a hopeful reader is understandable, but hard to defend in reason. The open possibility might support an effort to do experiments with more precise calorimetry; they have gone on apace since, though not at MIT or at PFC.

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V. My Recommendations

I close with an explicit answer to your questions about options for action in your letter of 9 October 91.

1) I do not believe an inquiry needs to be conducted.

2)I do not believe any formal investigation is needed.

Both of these recommendations apply only to the scientific paper, and not to the media interactions.

3) On the other hand, I recommend that PFC should spend a person-day or two of work to compute the mean excess power for all four cases, the light and heavy water cells under the two protocols of drift correction. They should also describe the two approaches to correction in more detail than the by-name-only account in the letter by Dr, Parker to Dr, Mallove of 8 August 1991. (Notice that in Figure 6 of the MS submitted from MIT, but not in the published version, one fit to the declining heater power curve is drawn.) The work need not even invoke much ado about old records; it can probably be done from the curves already published or at hand and the algorithms used.

Making these few numbers publicly available, first of all to Dr. Mallove--they are probably not important enough to publish-- would for me fairly and helpfully clear the record./Anyone is then free to make what use he can of data that are clearly below the level of significance, if possibly suggestive to the hopeful.

Sincerely,

Philip Morrison

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Log Sheet: VEST letter + packet

P. Morrison

1991

to from

13 Oct 1200. opened packet: letter PM/CMV: 10/9; letter MPR/EM: 9/1;
letter MPR/EM 8/18; no attachments, a list only;
report Albagli et al, PFC/JA-89-34, original
(I now see that this is Attachment #1 of EM,
and find also the others through #17.)

1400 labelled in pencil (& initialled) the two figures
of Attachment 12. I've also done some re-stapling.

Read, measured some graphs.

14 Oct Prepared & printed my report.

15 Oct Delivered to Office of Pres.

P Morrison

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MITCHELL R. SWARTZ, M.D., Sc.D.

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CONFIDENTIAL

January 28, 1992

President Charles M. Vest
Room 3-208
77 Massachusetts Avenue
Massachusetts Institute of Technology
Cambridge, MA 02139

Dear President Vest:

I attempted to reach you twice recently to confidentially discuss what appears to be a potentially severe problem for MIT regarding a published paper. Instead of responding objectively to these calls to your office, I was simply informed that because this involves "cold fusion" then your door remains effectively closed; essentially terminating a timely opportunity to minimize the impact of this matter.

This contact with your office is not made frivolously. In my two decades at MIT I have earned four MIT degrees (VI) including a Doctorate of Science. I also earned a MD degree from Harvard and have served as a physician for 15 years. In all my time at MIT no matter has ever arisen before of enough urgency to require a contact with the Office of the President.

From the initial announcement by Fleischmann and Pons of what is now called "cold fusion" I have actively followed this field, not only because of its obvious practical importance if the phenomenon turns out to be real, but because my extensive training at MIT has given me a good background with which I might evaluate the new developments.

Unfortunately, in the field of cold fusion, politics and possibly economics has blended with science. As has been widely reported by the media, there have been many cross-allegations made between members of the PFC/and MIT communities and Drs. Fleischmann and Pons, and later some of their supporters, concerning discrepancies in more than one seminal paper concerning this phenomenon. This result has been hostility with neither science nor engineering.

Last month I began preparation of a manuscript entitled, "Patterns of Failure in Cold Fusion Experiments". As a result analyses of several papers, including one published by the Plasma Fusion Center (PFC) has been undertaken. An attempt was made to get a clearer picture of the data and issues in this dispute by the use of computer analysis to quantitate differences in some of the original data for that paper and the final published data.

I believe the essence of the problem is as follows. The current antipathy to cold fusion in the scientific community was created largely by the PFC paper's putative report showing failure-to-reproduce as opposed to its later claimed too-insensitive-to-confirm the cold fusion phenomenon. Despite the paper's potentially inconclusive experimental results the PFC paper has become the single most widely quoted work used by the critics of cold fusion to show that the phenomenon is not real. Even the U.S. Patent office quotes the PFC paper as sufficient reason to turn down any and all innovations in this field. This prejudice may extend to your office wherein negligible effort is apparent to protect MIT from the consequences of another issue which might not fade away.

The importance is this: any late disclaimer denying significance of a potential heat generation curve, in the background of possible movement of "data", has the net effect of changing the crucial impact of the paper - from one which supports no conclusion about cold fusion to one which appears to show that cold fusion simply does not exist.

Therefore, this alumnus hereby formally recommends, contrary to your January 6, 1992 letter to Dr. Mallove, that you reconsider Prof. Morrison's reasonable suggestion and perform a short in-house inquiry as to the "two protocols of drift correction" which were algorithms used to derive the different curves.

Many engineers and scientists, including Prof. Morrison, appear to have not yet made up their minds about the phenomenon. Thus, there is at least a possibility that cold fusion phenomena which are produced by a variety of means could be a practical source of energy. Therefore, a more effective, but perhaps not politically realistic, solution to the problem might be for the MIT to repeat the original study with tighter control of data, analysis, and incorporating the larger body of information now available in this area (both MIT and cold fusion) to maximize the likelihood of positive results.

Your comments remain greatly appreciated.

Thank you.

Sincerely,

MITCHELL R. SWARTZ, M.D., Sc.D.

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CONFIDENTIAL

February 20, 1992

President Charles M. Vest
Room 3-208
77 Massachusetts Avenue
Massachusetts Institute of Technology
Cambridge, MA 02139

Dear President Vest:

Thank you both for hosting the conference at MIT last week, and for sharing your comments with all of us. I enjoyed speaking with you very briefly at lunch.

It is more than two weeks since you and Professor Parker received a copy of the draft manuscript entitled "Patterns of Failure in Cold Fusion Experiments". Would you please forward any comments which you might have regarding this matter in general, and the draft in particular. Such comments, and corrections if any, would be appreciated.

Thank you. Best wishes.

Sincerely,

CONFIDENTIAL

CHARLES M. VEST, PRESIDENT

ROOM 3-208
77 MASSACHUSETTS AVENUE
CAMBRIDGE, MASSACHUSETTS 02139-4307
617-253-0148

March 10, 1992

Professor Philip Morrison
Room 6-205

Dear Philip:

I write to ask your assistance again in reviewing a specific question related to data reduction in the manuscript "Measurement and Analysis of Neutron and Gamma-Ray Emission Rates, Other Fusion Products, and Power in Electrochemical Cells Having Pd Cathodes" by Albagli et al. The background for the question raised is contained in the attached manuscript and letter, submitted to me by Dr. Mitchell Swartz, an alumnus of MIT. While most of the issues raised in the manuscript of Dr. Swartz are a proper subject for open scientific debate and evaluation by peer review in the scientific community, there is one issue that should be resolved here at MIT.

As you point out in the report submitted to me "Assessing the request for an Inquiry by Dr. Eugene Mallove, made August 18, 1991," "Objectively, one has to say that the published paper [Albagli et al.] does not mislead; the change in power between drafts is smaller than the clearly stated and plausible overall error, at most a little outside of random error alone." Thus, while one can continue to debate the basis for conclusions drawn in this paper, this should be done in open scientific debate, to which the published paper is a contribution.

However, the paper did present a curve of W(t) in its Figs. 4b and 5b, and did describe in the text how the data was reduced to obtain these curves. It is the prerogative of the authors to reduce their data according to reasonable scientific methods while describing the details of the method they used. The justification for their approach is a subject for open debate and evaluation by peer review within the scientific community. The manuscript by Dr. Swartz challenges whether the data reduction method used is as described, namely "correcting the sloping baseline with a linear function."

I ask that you review the attached materials and other material to be provided to resolve this issue of data reduction. The question I wish you to examine is:

P. Morrison
March 10, 1992
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> Is the data reduction method that was used to produce the final curves which appear in Figs. 4b and 5b of the manuscript by Albagli et al. satisfactorily described in the text?

I do not believe that your report to me need state the actual algorithm used but rather whether the method used is satisfactorily described in the paper.

Please inform me if, upon completing your review of this matter, you believe that any further action on the part of the Institute is needed.

I very much appreciate your willingness to carry out this task.

Sincerely yours,

Chuck

Charles M. Vest

CONFIDENTIAL

department of physics · mit · cambridge ma 02139 · USA

March 20, 1992

To: Charles Vest, President

From : Philip Morrison, Institute Professor (emeritus) Philip Morrison

Response to Your Letter of 10 March 1992

I. Question and Answer

Your letter put to me a specific, rather narrow question, whose background is a recent painstaking study of a particular research paper (and two draft manuscripts) from MIT. The critical study, largely directed at a few specific graphs and their captions, was carried out by Dr. Mitchell Swartz, Weston, MA; its final date is 27 Jan 1992. The research paper itself, by Albagli et al, with 16 co-authors, came from the MIT Plasma Fusion Center, and was published in the Journal of Fusion Energy, vol.9, no, 2, p. 133, 1990.

You wrote me: " The question I wish you to examine is: Is the data reduction method that was used...[to produce certain curves in the published paper referenced above] satisfactorily described?"

My reply is this: though the procedure was described in only a few lines, a technically-prepared reader who uses the entire paper can work out the missing details to a good degree.

That reader would certainly be reassured by having for comparison the data for the heater power of the light water comparison cell. Those data were not in the published paper, though they were made available by Dr. Luckhardt in a letter of August 13,1991, sent by Director Parker of the Plasma Fusion Center to Dr, Mallove (Mallove Attachment #12).

But I do not think I should stop abruptly. As a physicist, I want to outline the logic of the procedure, address the results, and even add a little new matter. Dr. Swartz's study seems to me to warrant a fuller explanation for your records (to augment my first response) and for possible transmission to others you may wish to inform.

II. Source Documents Used

The letter and manuscript from Dr. Swartz are the direct basis for my comments. But it was valuable as well to use the August 18,1991 letter of Dr. Eugene Mallove to you, with its many attachments, and my letter of last October (harmlessly misdated in

Dr. Swartz's study). Both of these were available also to Dr. Swartz, and cited in his Appendix.

I was also supplied through your office with a new and fuller account of the data treatment procedures, an account prepared by Dr. Stan Luckhardt of the Plasma Fusion Center, who carried out the original calculation (Luckhardt MEMO, 3/10/92). Dr. Luckhardt and I have spoken by phone as well.

I return all those documents for your files. I have destapled and restapled some of the papers, and made a few tick marks.

III. My Standing

I refer to my earlier letter for a full statement of my own "limitations and qualifications" as an assessor. I still believe that there may be a germ of electrochemical novelty in this complex system, though perhaps independent of deuterium and palladium.

IV. The Substantial Issue: A Shifting Thermal Baseline

The research paper dealt with the comparative release of various energies during the electrolysis of light and heavy water using cells with Pd cathodes. All interest here centers on only one product of the process, thermal power release, although most of the research, and three-fourths of the paper, was devoted to a search for a variety of other products, on which limits were set at much greater sensitivity than for heat.

The center of attention is one calorimetric result: a light-water cell and a matched heavy-water cell are followed over 60 to 80 hours. The calorimetry is not absolute; both cells were open for the release of gaseous products, possibly carrying a small liquid entrainment. The necessarily changing level of electrolyte meant a changing internal cell resistance, and a changing heat flow from the cell. That heat flow was monitored by a feedback system, which controlled the current to a thermal heater inside the cell, acting to keep a constant cell temperature within. The electrical inputs were monitored as well.

A noisy, fast-fluctuating heater current records what happened in the gassy, bubble-stirred, perhaps transiently bubble-blocked, system. The signal noise sets a statistical limit to the accuracy of any thermal power measurement at about plus or minus 40 milliwatts; this result is stated clearly, though not in detail quantitatively supported. The graphs do show the eye just the rough amount expected, the usual more or less two-sigma band of plausible uncertainty.

But the cell fluid level slowly changes, and with it the observed heater power. One plot is given, (Figure 6), for the total heater

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power in the heavy-water cell. Plainly the heater power declines over the run of more than a week by several times the width of its noise band. The noise is reduced both by digital filtering (the data were mainly sampled every two minutes for some 80 hours), and then by binning those data points. The sloping mean baseline observed is then adjusted to form a new horizontal axis, the mean zero line, for excess cell power, "by fitting the drift with a linear function and subtracting from the signal." The procedure cannot disclose any constant power difference between the two cells, since the initial value of the baseline is set at zero within the visible noise. But any change in power yield between the two cells over time would appear. No significant change appears during the long run to break the linear fall.

A couple of very modest but eye-catching peaks do appear at 24 hour intervals: they may imply an ambient temperature minimum around midnight.

The binned data comprise some eighty numbers, each plotted as a dot. An exact dot count, as expected, does not work. The binning--"time averaged over 1-hour blocks"--is subject to the usual inclusive or exclusive decisions, especially because the digital sampling rate was changed at one time, as stated in the caption to Figure 6. Whether there are 45 or 43 dots in one forty-hour period is not material; these statistics cannot show such a nicety.

Another point seems important to flag. The heater power measured goes to supply most of the heat loss from the constant-temperature cell. The more power supplied, the less power comes from internal cell processes. Now, the steady downward drift in the heater-power baseline for the heavy-water cell is slower than the similar drift seen for the light water cell, at only about 60% of the light-water rate. If no correction were made for that linear power decline, there would appear to be a higher "excess power" developed by the light water comparison cell, not by the one with heavy water. (I believe the Noninski paper (Mallove, Attachment 8) omits all evaporative effects.)

Greater fluid loss takes place in light water, presumably by evaporation, as expected because of its higher vapor pressure. I have not been able to make a reasonably simple model to fit the presumed evaporative losses, but a crude estimate shows that evaporation of a water mass comparable to the electrolytic loss is not excluded either by cell power or by plausible gas-flow rates, estimated from saturation water vapor that may be carried both by gas bubbles and from the free cell surface. The data are not complete enough to allow a simple theory to include both the heat flow changes and any resistive effects of water loss and level change. The authors also did not offer any quantitative model for the empirically quite linear drift in power, though they outline the issues clearly. (The high -frequency heater power noise differs markedly between the two cells as well. There is a UROP study to be done here some day.)

V. Short-Time Confirmation

The week-long runs have attracted all the comment. But the published paper also presents full data for a short-time test that directly compared the measured heat production in light and heavy water cells over a time so short that the slow change in water level can be neglected. In Figure 3 the noise power is again about 40 milliwatts; the light- and heavy-water cell powers agree to well within that limit. The effect expected on scaling the Utah results would be double that, and should be visible if present.

This single test takes on a special interest because it was made at the end of the long run, after about 200 hours of electrolysis. If slow gas charging of the palladium electrode is a determining parameter, this was the likely optimum for the experiment reported. The hydrogen content of the Pd electrodes was measured after the experiment by degassing; the loading factor found was 75 to 80 %. If higher loading still is a necessary condition for excess heat, this early negative result could not in itself be final. That objection remains true for <u>any</u> negative test result until a necessary state of the electrode has been fully characterized!

VI. Recommendations

1. The full file I have seen (i.e, the papers of Mallove, Swartz, and the Luckhardt memo), including your queries to me and my own two responses, should be available to interested persons on request. True, that is a lot of paper; they could be given a listing and offered a choice.

2. Dr. Luckhardt should be encouraged to prepare an account of the drift correction based on his March 10 ,1992 memo to me, perhaps adding a brief introduction, and Dr. Parker asked if he would issue it as a brief amplifying note from the Plasma Fusion Center. That can be sent by you to anyone who has written for more information, including of course the people who have already done so.

3. I hope that everyone will cool his comments: enough of acrimony. There are plenty of data from this powerful early experiment, though puzzles about the complex system remain even after two more years of widespread reports.

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CONFIDENTIAL DRAFT INTERIM REPORT CONFIDENTIAL

ADDENDUM TO:

PATTERNS OF FAILURE IN COLD FUSION EXPERIMENTS

SEMIQUANTITATIVE ANALYSIS AND EXAMINATION OF MIT PFC PHASE-II COLD FUSION DATA

MITCHELL R. SWARTZ

January 14, 1992; update April 14, 1992

This Addendum supplements the above-entitled manuscript prepared in January, 1992. Additional data, recently provided by the MIT Plasma Fusion Center on 3/10/1992 has corroborated the initial observations, and has also given an opportunity to analyze the paradigm used by the PFC. The information discussed herein has not been incorporated into the most recent draft of the manuscript [dated 3/7/1992].

1: ERRATA

The date of Professor Morrison's first response to President Vest was previously incorrectly listed as August 10, 1991, instead of October 14, 1991.

Much current skepticism of the cold fusion phenomena was created by the PFC paper with its reported "failure-to-reproduce". The author previously had mistakenly believed that the PFC may have published a clarification or retraction regarding the 40 milliwatt sensitivity of the Phase-II experiment making it a "too-insensitive-to-confirm" experiment. This statement was made at MIT Technology Day 1991, but the author remains unable to find any other published statement.

2: POORLY DESCRIBED DATA POINTS

The March 10, 1992 PFC memo presents two "new" curves. The new curves for the heavy water experiment [from the 3/10/92 Luckhardt/Parker memo] are documented as Curves (or Types) 4 and 5 in Exhibit 1. With the generation of the two new curves, the total number of distinguishable curves, used to describe a single experiment, increases from three to five. A table helps sort them out [Table 1].

Table 1 also lists the several different curves characterizing that single MIT PFC heavy water run. Six groups are listed, and five groups are shown in Exhibit 1. The Type 1 curves are presumed to be the raw data with the best experimental graph being 1D (shown in Exhibit 1). Types

2 & 3 (as Dr. Mallove correctly noted) do differ. The other Types will be discussed in detail below.

The first "new" curve [Type 4 in Table 1] is labelled as "Fig. 5" in the 3/10/92 document. An independent analysis has now been conducted to compare this new curve [Type 4] to the published curve [Type 3B]. This was done using the same optical techniques as described in the original manuscript {prior to this Addendum}.

One of the results of this optical comparison can be seen in Exhibit 2. The published curve [Type 3B, very similar to Type 3 (PFC/JA-89-34)] is on the right hand side of Exhibit 2, and is colored gray. The data from the July 10, 1989 curve [Type 2] is also shown on the right side, slightly above it, shown as a black continuous curve. Finally, a portion of the "first new" curve [obtained from "Fig. 5"; Type 4 in Exhibit 1] is shown on the left hand side of Exhibit 2. The "new" curve appears as a clustered group of symbols. The times are in hours, and the power (vertical axis) is in watts.

On Exhibit 2, it can be seen that the more inferior of these data points (labelled "A" on the right hand side of Exhibit 2, and labelled as "A" in the original manuscript, and labelled "A'" on the left hand side of Exhibit 2) has been moved in position. The data point has now shifted in a direction back up towards the baseline. This data point shift is one of a number of changes which can be used to distinguish and identify this curve as a Type 4 PFC Phase-II heavy water curve. The magnitude of this shift amounts to approximately -24 milliwatts [see point "A'" on Exhibit 2].

Given this observation of a shifted data point on a "new" Type 4 PFC Phase-II curve, the scientific method was followed and a similar imaging analysis was then undertaken to compare the number of dots in this new Type 4 curve to the published curve [Type 3B]. Exhibit 3 shows the results. The two curves have been normalized, put in registration and overlaid for Exhibit 3. In the original 3/10/92 PFC memo at the symbols are "[ ]". For easier identification here in Exhibit 3, a black dot has been centered between each pair of brackets.

It clearly can be seen that at least three of the previously specified data points previous reported on, and identifying, the Type 3 (or 3B) curve [labelled "i", "ii", and "iii" in Exhibit 3] are missing from the "first new" Type 4 curve. One more notable absent data point ("iii") is the expected opposite superior half of the incidental experimenter-derived time-glitch pair (ie., the data point opposed to "A" and "A'" in the Types 3, 3B, and 4 curves respectively).

These new changes observed in the "first new curve" presented in the PFC 3/10/92 memo corroborate the fact that the original published curves did contain two types of unusual data points. Such data points are now both volatile [points "i"-"iii" in Exhibit 3] and motile [by 24 milliwatts in Exhibit 2]. More importantly, such points, given this flueric and chimeric character, probably do not arise from a physical basis. Therefore, such points could have been better described in the text.

In the "second new", presumably penultimate, curve [Type 5 in Table 1] apparently <u>all</u> of the above-cited data points, previously commented upon in the above-entitled manuscript, have simply vanished.

In summary, Table 1 lists (and Exhibit 1 shows) the several different curves which have been purported to show the results of a single MIT Plasma Fusion Center heavy water experiment in 1989. The assignments are listed on the page behind Table 1, and are basically as follows.

Type 1 curves are the raw data. The best experimental graph is 1D. Type 1 transformations to Types 2, 3, 4, or 5 involve transformations of the baseline by circa -900+ nanowatts/second. Types 2 & 3 (as Dr. Mallove correctly noted) do differ by an amount of circa -94 nanowatts/second as noted in the manuscript. The newest curves [Types 4 and 5] confirm the initial observations. Types 4 and 5 curves are characterized by a shifted data point and progressive disappearance of other questionable "data" points. Disappearance and shift probably do not constitute an adequate response in the peer review system.

<u>3: CAN THE PFC Phase-II PARADIGM DETECT EXCESS POWER</u>

Although some aspects of these new curves (Types 4 and 5; cf. Exhibit 1) differ from both the previous July 10, 1989 (Type 2), July 13, 1989 (Type 3) and the published curves (Type 3B) curves, these newest two curves most importantly finally demonstrate the method used to analyze the Phase II cold fusion experiment at the Massachusetts Institute of Technology Plasma Fusion Center.

The March 10, 1992 memo by Prof. Luckhardt indicates that the PFC group did

> "first subtract the baseline drift, then [expected that] any onset of anomalous heating would appear as an excursion from zero."
> [Prof. Luckhardt memo to Prof. Morrison; 3/10/92]

Some of the new curves from the PFC in its 3/10/92 memo harmlessly list regression coefficients [used to shift the raw experimental data curve] to five (5) decimal places, yet leave off the coefficients of fit. These linear curves [derived by uncertain fit over apparently varying portions of the experimental data] were reportedly used to counteract a shifting baseline. As discussed below, the flawed paradigm can effectively mask the potential appearance of any constant [ie. DC signal] excess heat, especially if the first 15% of the curve is not used in a characterization of the baseline shift as may have occured with the Types 2, 3, 3B, 4, 5 curves, and their congeners.

Prof. Morrison notes that this technique of analyzing the data:

> "cannot disclose any <u>constant</u> power difference between the two cells, since the initial value of the baseline is set at zero within the visible noise. But any <u>change</u> in power yield between the two cells over time would appear."
>
> [Response of Prof. Morrison to Pres. Vest; 3/1992]

Given the widespread tardive reports heralding confirmation of the cold fusion phenomena <u>and</u> the recent release of the paradigm in the March 10, 1992 memo to Prof. Morrison, and given that Prof. Morrison may have assumed that asymmetric treatment of the two samples will not occur, it is now reasonable to determine if, and how, the MIT PFC Phase-II technique of "correcting" the entire baseline shift is sensitive to any putative <u>change</u> (or putative presence) of excess enthalpy. Because such an analysis may not have been done to date, the following independent gendanken analysis determined whether the PFC Phase-II method would be sensitive to various hypothetical fixed changes in excess enthalpy [eg. ramp, step function, in the presence of a changing baseline].

Thus, this mathematical model examined the effect of the PFC Phase-II paradigm upon three hypothetical successful cold fusion experiments. Exhibit 4 shows four sets of three curves (each a,b, and c) which are the result of a "transformation" or "operation" used to process the first set of curves (1a, 1b, 1c). The first set of curves show the the heater power curves (upper left; curves 1a, 1b, and 1c) from three hypothetical experiments wherein excess enthalpy successfully occured, with a turn-on at time T. These curves (lower case letters) should not be confused with the several curves used to describe the PFC-heavy water experiment [as shown in Exhibit 1, or listed in Table 1; eg. 3B published curve <-- note upper case].

Curve 1a is the first hypothetical heater power curve, as a function of time, with a hypothetical slow "turn-on" of excess heat at time T. In curve 1a of Exhibit 4 the heater power curve is initially stable (flat baseline) but then decreases as the excess heat slowly increases. That is, for the same temperature, the heater actually requires less input power, and thus assuming the corresponding increase of "another" heat source. Such an expected delay to a putative change in state may be reasonable, and was in fact expected by the PFC group. As was noted in the March 10, 1992 memo:

> " The level of excess heat .... was claimed to appear after an initial 'loading period' of some hours or days. Thus, to reproduce the claimed effect, we would expect the heater power to undergo a change of the claimed magnitude after some days of 'loading'".
> [Prof. Luckhardt memo to Prof. Morrison; 3/10/92]

Another complication is that the baseline of the initial power heater curve (before initiation of a "successful" experiment) may not be flat. Such baseline drift could be secondary to electrolysis loss (which would increase the thermal resistance barrier by increasing the thermal diffusion pathlength to the environment) or to evaporation or to other reasons. Thus, for the second hypothetical experiment, Curve 1b shows the same change in excess heat at time T, but with a slow initial (intrinsic) baseline drift prior to the onset of heating.

Curve 1c is similar to curve 1b, but in this third hypothetical experiment there occurs a sudden breakpoint at time T, with a step function rather than a ramp function.

This simple analysis demonstrates that it is critically important to note that regression fits can be made to fit <u>either</u> the entire curve, or just that portion of the heater power curve which occurs prior to the onset of excess power. Exhibit 4 continues by presenting both of these two methods in this "thought experiment". Curve sets 2 (and 4) and 3 (and 5) follow each way of doing this. The left-hand set of curves (2a, 2b, and 2c) show the derivations using regression fits made to the initial portions of the heater power curves <u>prior to the onset of excess enthalpy</u>.

The linear fits made using only the experimental data <u>prior</u> to the onset of excess heat are labeled Yht (for "true" linear regression fits) in order to distinguish them from the curves Yhf (for "false" linear regression fits) which are regression curves derived from the encompassment of data points from the entire experiment. That is, Yht curves are fit to the initial data prior to the onset of excess enthalpy, whereas the Yhf curves are fit to the entire data curve (including the excess enthalpic portion). The latter curves are shown as curves 3a, 3b, and 3c.

The "truth" and "falsity" of each method will be apparent now.

Curves 4a, 4b, 4c, and the curves 5a, 5b, 5c show the result of then deriving the "measured" excess power. That is: Curves 4a, 4b, and 4c show the calibrated derivation of excess heat: Pxt = Yht - Ph. Curves 5a, 5b, and 5c show the "falsely derived" excess power using the "false" linear regression fits: Pxf = Yhf - Ph. The net value of each paradigm [transformation] can be easily seen by comparing the groups of Curves 4a with 5a, 4b with 5b, and 4c with 5c. On the left hand side it can be noted that the Pxt curves do qualitatively reveal the excess enthalpies which were postulated in this gendanken "successful" experiment. On the other hand, and on the right side of Exhibit 4 at curves 5a, 5b, and 5c, are the generated Pxf curves. The flawed technique does apparently mask the excess heat in each and every hypothetical "successful" case so derived.

Further observation reveals that although on the left side (curves 4a, 4b, 4c) the excess heats are correctly deconvolved as having begun at time T, on the right-hand side (curves 5a, 5b, 5c) there is a breakpoint at time T, but the presence of excess heat is not so revealed. Instead, what appears is surge of "negative" excess heat just prior to the initiation of the hypothetical excess heat. It is interesting that an examination of some of the PFC heavy water published curves (Types 3 and 3B) appear to reveal a similar pattern lending possible further corroboration of this interpretation.

4: CONCLUSIONS

Two "new" curves confirm a pattern of mutable data points, superimposed on a number of baseline shifts. One implication is that the observations made, and conclusions suggested, in the draft manuscript remain.

A baseline shift of circa 900 nanowatts/second appears to have been applied to the Type 1 curves in order to get the Type 2 curve.

Yet another 94 nanowatts/second shift was apparently applied to get version protoType 3. The superposition of questionable points complete the transformation to Types 3, and then 3B (published in J. Fusion Energy).

Types 4 and 5 attempt to present partial, and then total, removal of, and/or shift of, the questionable data points.

Recent release of the algorithm used has permitted a "thought experiment" which examined the method used by the MIT-PFC Phase-II group. Elementary analysis demonstrates that inclusion of significant portions of the post-turnon excess enthalpy curves in any regression analysis creates likely obfuscation of any constant [D.C.] output. Another obvious corollary is that reliance on such an insensitive paradigm to detect excess heat from cold fusion (or any other source) would be a priori flawed.

Another implication is that the most critical part of the data analysis requires measurement of the baseline drift <u>before</u> the turn-on of any putative excess heat. Therefore the PFC perhaps ought not to have added the 5 ml. of D2O to the solution after the experiment started, but should have tried to obtain the best possible initial baseline. It is similarly important to publish that same best-possible initial baseline in all curves [Types 3, 3b, 4 and 5 lack the initial portion (Table 1)], and to clearly state what portion of the experimental data was used in the regression-subtraction scheme.

A final consideration is that the PFC Phase-II D2O experiment ought to be reexamined using the initial baseline drift procedure for the regression correction, as discussed above [Pxt; Exhibit 4 (sets 2 and 4)]. Therefore, a corrected power output curve for the PFC Phase-II experiment has been derived (listed as Type 6 in Table 1), and is shown in Exhibit 5. Pxt is the "true" corrected excess power, and is shown as function of time. The uncertainties of the absolute value are significant. The calibrated excess heat derived by this method appears to be 62 milliwatts (+/- 34 milliwatts).

This calculated value of excess heat is the order of the measured difference between the Type 2 and Type 3 curves. It is also qualitatively close to the value expected for a "successful" experiment.

In addition, time of apparent turn-on of excess heat in the derived Type 6 curve appears close to the expected time cited above, but the problems with the data cited above limit any further discussion.

Complimentary support for excess heat in the MIT PFC Phase-II experiment was cited in the published Journal of Fusion Energy paper itself.

> "When enough solvent was added to the D2O cell to compensate for that lost to electrolysis at the end of the 100 h period shown in Figure 6, Ph returned to within 20% of its original value."
> [ALBAGLI et al., J. of Fusion Energy, 9, 133 (1990)]

This "20% discrepancy in heater power (over 200

milliwatts) to heat <u>the same volume of fluid</u> that was present ... might be the best evidence of all that the heavy water cell really did produce anomalous excess heat"
[Complaint from Dr. Mallove to Dr. Rowe; Aug 18, 1991]

The ratio of the D2O-only heater power shift between the Type 2 and Type 3 curves (circa 94 nanowatts/second) to the total baseline correction (circa >900 nanowatts/second) may be similarly related to the PFC-cited difference of the power heater requirement reported to occur at the end of the well publicized 1989 MIT PFC Phase-II heavy water experiment.

APPENDIX [accompanies Addendum of April 10, 1992]

1: THE ISSUE IS AN ASYMMETRIC LINEAR FUNCTION

The letter [March 10, 1992] from MIT President Vest to Professor Morrison appears to have contained an inaccuracy:

> "Dr. Swartz challenges whether the data reduction method used is as described, namely 'correcting the sloping baseline with a linear function'",
> [Pres. Vest's letter to Prof. Morrison; 3/10/1992]

In contrast, the issues and observations were:

> "An attempt was made to determine the semiquantitative linearized correction between the two recently described heavy water curves. It was found to be in the range of fixed baseline shift of [A=] -9.6 milliwatts and a time varying component in the range of [B=] -94 nanowatts/second".
> [M. Swartz, PATTERNS OF FAILURE . PFC. DATA; 1/1992]

The examination of two of the many PFC heavy water Phase-II curves [Types 2 and 3 in Exhibit 1] challenged nothing. The examination attempted to measure differences between several curves describing one experiment performed at MIT in 1989, published in the J. of Fusion Energy, 9, 133 (1990).

Prof. Parker had stated:

> "the data for the H2O curve ... was taken at the time of the 1989 experiments, and in the exact same way that the data was obtained for the D2O curve",
> [Prof. Parker's letter to Dr. Swartz; August 30, 1991]

However, a potential "curve shift" was reported in 1991 [Curves 2 and 3 in Exhibit 1]. Prof. Morrison recommended accounting for the difference and releasing the method used to derive the curves.

The manuscript reported the detection of an asymmetric pair of transforms between the D2O and the "control(H2O)" curves of July 10, 1989; the light water "control" was treated with the identity transform. Other comments were made regarding the curves.

Therefore, a better, more appropriate question for Professor Morrison might have been as follows:

> "Did the published data and descriptions thereof indicate that both curves (for the heavy and light water) were identically treated? And if not, was the asymmetric treatment of the original data fully discussed in the text?"

2: ISSUES OF DATA REDUCTION and PARADIGM

What constitutes "data reduction" or a "paradigm" should be open both to scientific debate and evaluation by peer, academic, and legislative review.

The letter from President Vest to Prof. Morrison states that the Massachusetts Institute of Technology should only consider if:

> "the data reduction method that was used to produce the final curves ... [was] satisfactorily described in the text?"

The application of a low pass filter to an electrical signal and/or the cutting of a hologram in half properly constitute "data reduction", but no basis, no data, and no theory has been advanced or offered to enable the asymmetric shifting of one data curve to be regarded as "data reduction".

In addition, the removal of the entire DC signal [thereby including baseline drift and baseline shift (ie. the signal)] is also not classical "data reduction" unless the paradigm is clearly stated that way in the text.

TABLE 1 : THE SIX HEAVY WATER [PHASE-II] CURVES

The PFC data [Phase-II heavy water] has been displayed several ways between July 10, 1989 and the present. This table lists all the known curves of the single experiment, and separates ten of them into six groups (Types). Five different types have been reported by the PFC since 1989. See Assignments below, and on the next page.

#	CODE	DUR'N [HRS]	CONT/ DOTS	INCID PTS	QUEST PT	?CURV SHIFT	? PT SHIFT	BLS nW/s	DOCUMENT [DATE]	COMMENT
1	1	0-98	CONT	n.a.	n.a.	n.a.	n.a.	n.a.	7/10/89	raw data
2	1B	0-98	CONT	n.a.	n.a.	n.a.	n.a.	n.a.	JFE 90	published
3	1C	0-80	CONT	n.a.	n.a.	n.a.	n.a.	n.a.	9/1991	memo 1991
4	1D	0-98	CONT	n.a.	n.a.	n.a.	n.a.	n.a.	3/10/92	
5	2	0-98	CONT	n.a.	n.a.	?	n.a.	900	7/10/89	memo 7/10/89
6	3	18-98	DOTS	+++	+++	yes	n.a.	990	7/13/89	memo 7/13/89
7	3B	18-98	DOTS	+++	++++	yes	n.a.	990	JFE 90	published
8	4	18-98	DOTS	+	gone	yes	yes	900	3/10/92	memo 3/10/92
9	5	18-98	DOTS	gone	gone	yes	gone	1000	3/10/92	memo 3/10/92
10	6	18-98	CONT	yes		no	n.a.		INDEP	

<u>CODE</u> - TYPE. Type 1 has four graphs, all identical with various portions clipped. Curve 1D is the best raw data curve released to date [from 3/10/1992].

<u>DUR'N</u> - Length of Time shown in graph. Clipping occur at 80 hours and/or 0-16 hours.

<u>CONT</u> - Continuity. Continuous (CONT) or binned (sampled, DOTS).

<u>INCID(ental) Points</u> - Points from experimenter-created glitch.

<u>QUEST(ionable) Points</u> - Associated with outside of apparent raw data.

TABLE 1 (cont.): THE SIX HEAVY WATER [PHASE-II] CURVES

?CURV(e) SHIFT - Could there be shift of the curve beyond the degree expected by the baseline prior to the onset of putative anomalous heat.

? P(oint) SHIFT - Is there movement of point "A" to "A'".

BLS(Baseline Shift) - Is there any baseline shift?.

INDEP - Baseline Correction - cf. Addendum (Exhibit 5).

ASSIGNMENTS

These are the tentative assignments.

The raw data has been released clipped in several forms, which appear to be identical (Types 1A, 1B, 1C, 1D).

1D - PROBABLE RAW EXPERIMENTAL DATA - continuous.
1C - PROBABLE RAW EXPERIMENTAL DATA - continuous, identical to 1D; except clipped at circa 80 hours.

Types 2 and 3 are the previously released 7/10 and 7/13/89 curves. Type 3B was published, and does appear slightly different from Type 3, but was not changed enough so as to warrant a whole new category.

2 - FIRST BASELINE SHIFTED - July 10, 1989 curve - continuous [shift circa -900 nanowatts/second.
3 - SECOND BASELINE SHIFTED - July 13, 1989 curve - sampled data [shift circa -990 nanowatts/second].
3B- PUBLISHED CURVE - Figure 5b in J. Fusion Energy (9, 199) very close to curve #3 - sampled data.

Types 4 and 5 were released in a March 10, 1992 PFC memo using a derivation based on two different regression fits, which although given to five significant figures, the correlation coefficients were not.

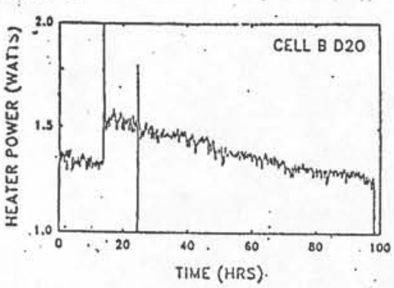
4 - NEW BASELINE SHIFTED - March 10, 1992 memo [FigPage 3a] sampled data : one incidental point missing; the other shifted 20+ milliwatts; other minor differences.
5 - NEWEST BASELINE SHIFTED - March 10, 1992 memo [FigPage 5] all incidental points missing; labelled "Constant sampling rate"; listed as "Fig. 9" on FigPage 5.

6 - SEMIQUANTITATIVE CORRECTION OF PFC-Phase II data
Best estimate of initial slope (see Addendum) shown in Exhibit 5,

EXHIBIT 1 - FIVE TYPES OF PHASE-II EXPERIMENTAL CURVES

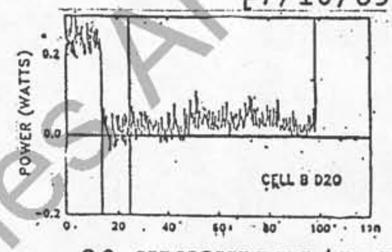
These are the nine different graphs available regarding a single MIT PFC Phase-II heavy water experiment in 1989. Five types are distinguishable. The four graphs on the left side (Types 1, 1B, 1C, and 1D) are identical with various portions clipped at 80+ hours and/or 0-16 hours. Curve 1D is the best raw data curve released to date [from 3/10/1992]. Types 2 and 3 are the 7/10/89 and 7/13/89 curves. Type 3B was published, and is slightly different from Type 3, but not enough to warrant a whole new category. Types 4 and 5 were released March 10, 1992; the previously cited "data" points vanish or drift. The Type 6 curve was independently derived from the PFC data and is not shown below, but is seen in Exhibit 5.

TYPE 1: RAW DATA [7/10/89] AND CONGENERS



- 900 NANOWATTS SECOND

TYPE 2: FIRST SHIFTED CURVE [7/10/89]



-90 NANOWATTS/SECOND

TYPE 1B [J.F.Energy /90]

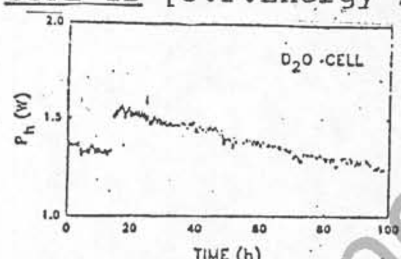
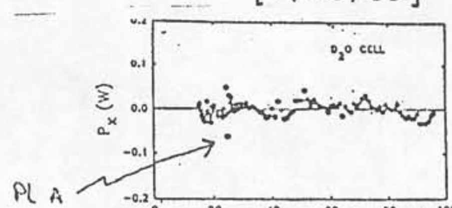
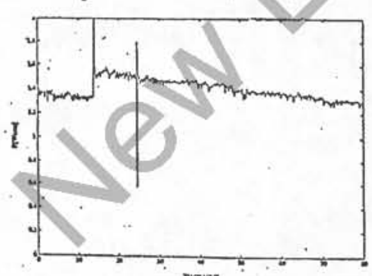


Fig. 6. Time history of the calorimeter heater power over a 100-h

TYPE 3: SECOND SHIFTED CURVE [7/13/89]



TYPE 1C [circa 9/91]



TYPE 3B [J. Fus. Energy '90]

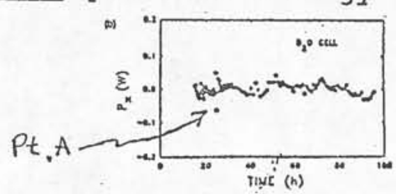
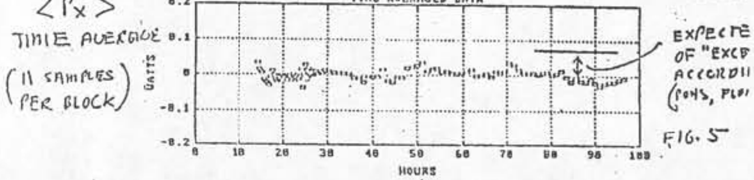


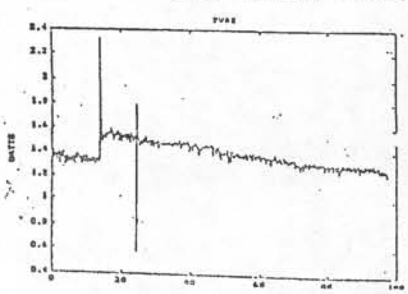
Fig. 5. (a) Time history of the cell current, I_c; voltage, V_c; and

TYPE 4 [3/10/92; 24 milliwatt shift]

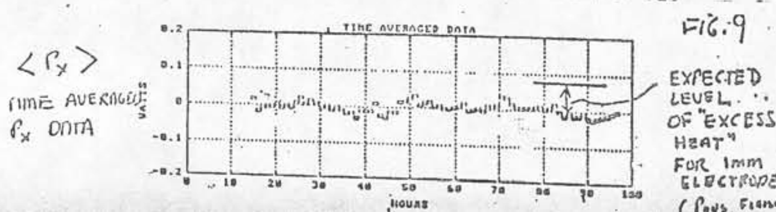


TYPE 1D [3/10/92]

RAW HEATER POWER DATA



TYPE 5 [3/10/92: "cleansed"]



THREE CORRECTED PHASE-II D_2O CURVES

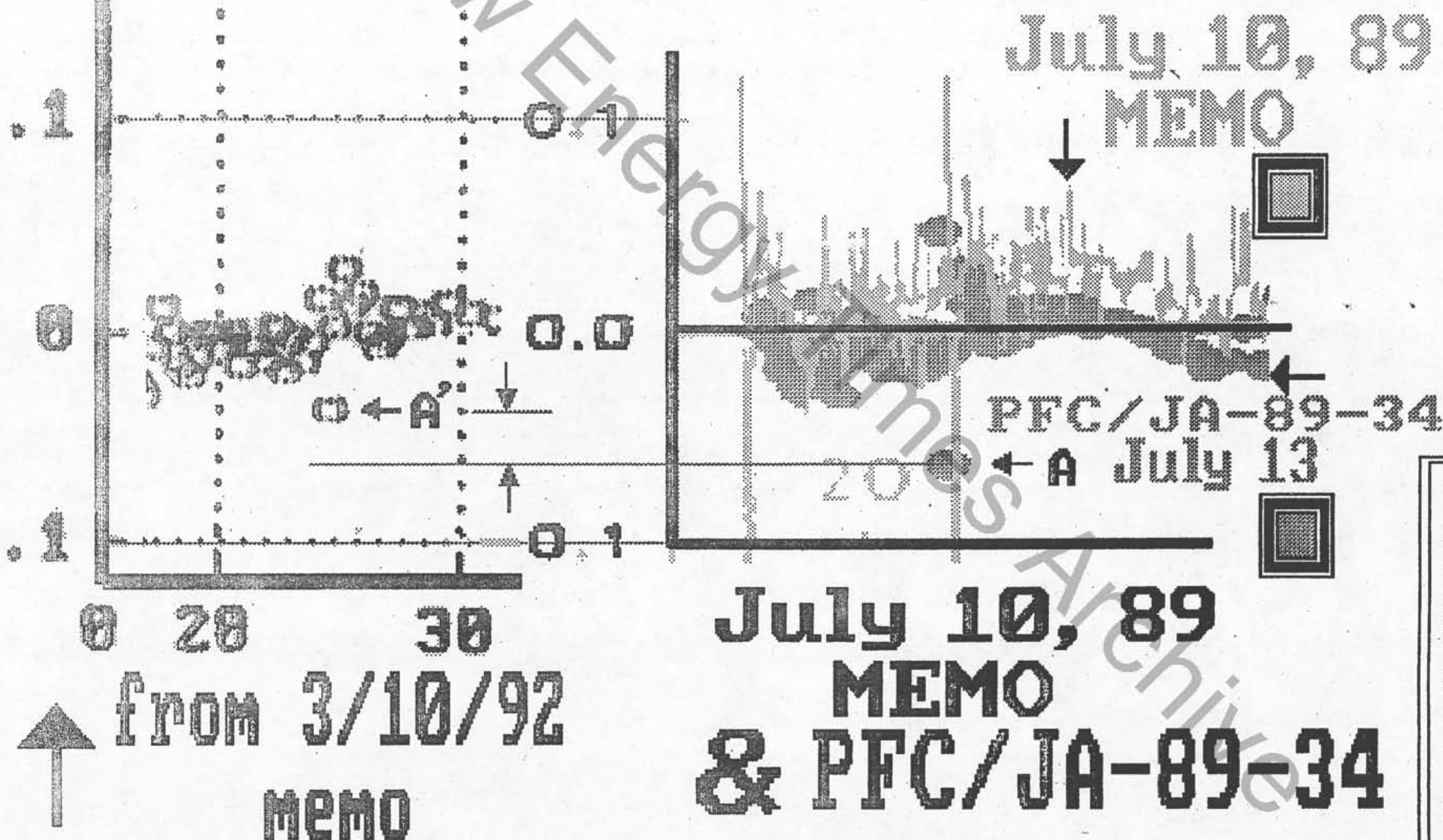


EXHIBIT 2

HEAVY WATER CURVES

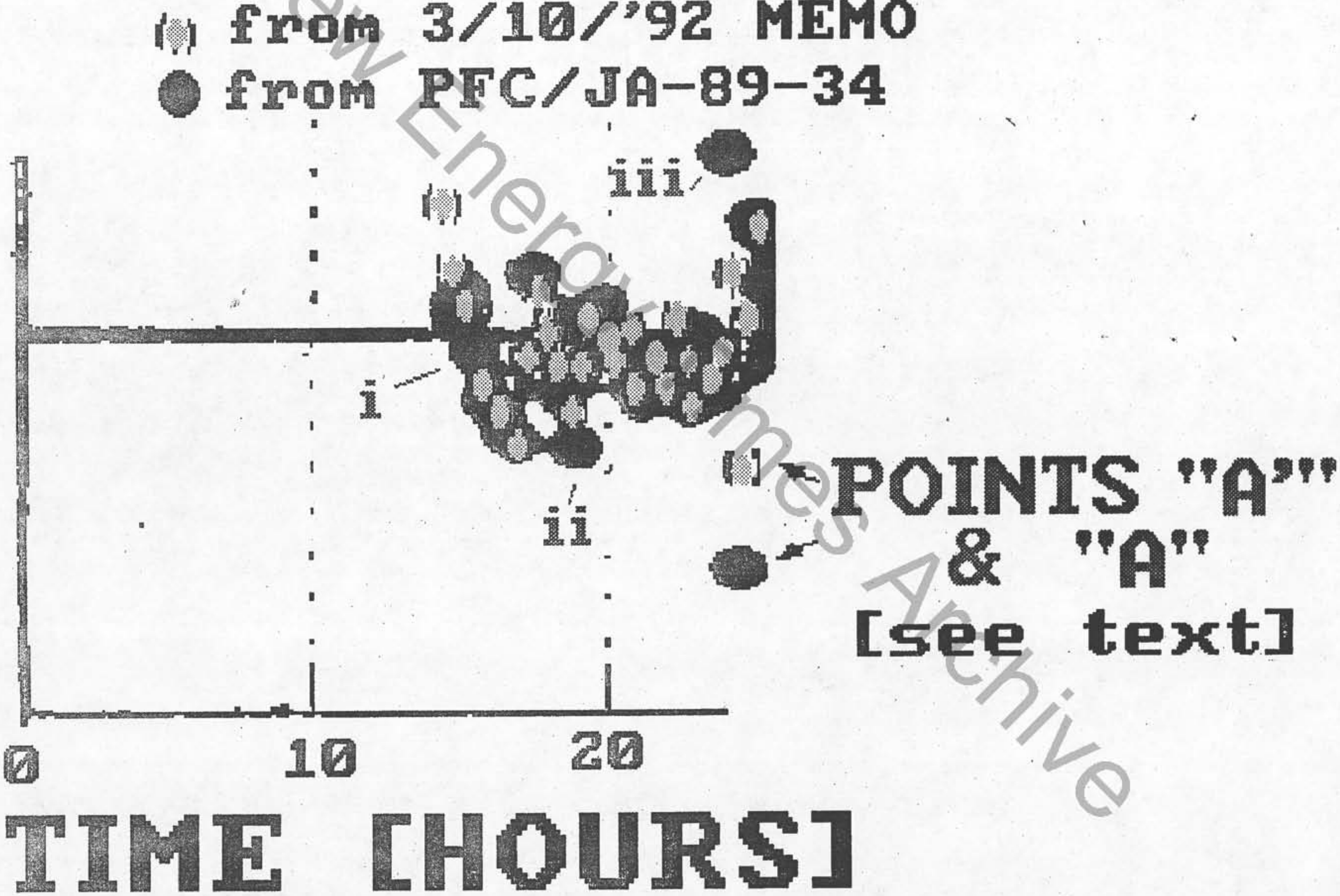
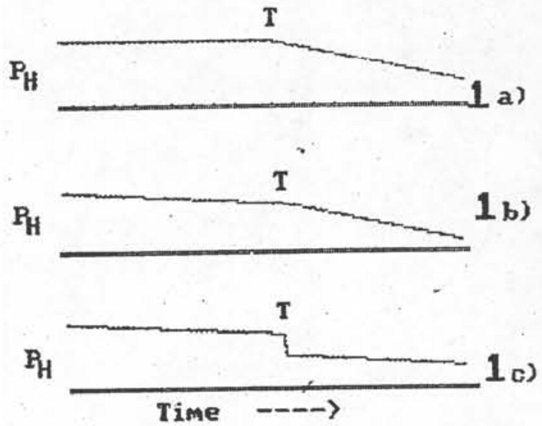


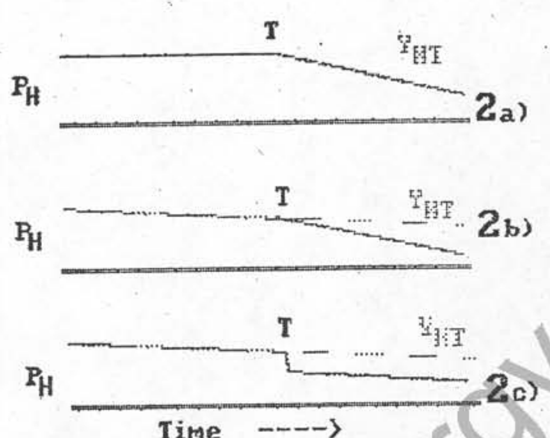
EXHIBIT 3

EXHIBIT 4 -

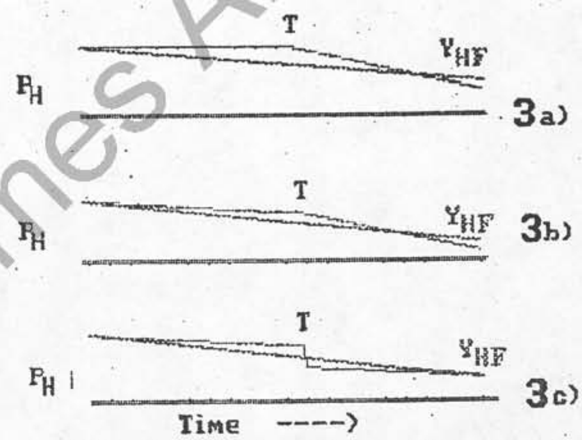
GENDANKEN EXPT TO INVESTIGATE BASELINE-SUBTRACTION PARADIGMS USED TO EVALUATE EXCESS ENTHALPY EXPERIMENTS



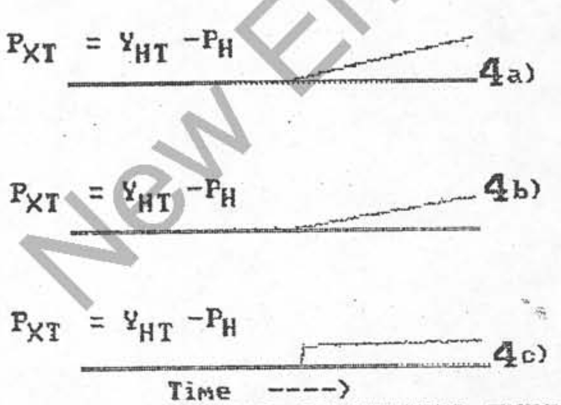
HYPOTHETICAL SUCCESSFUL EXPTS



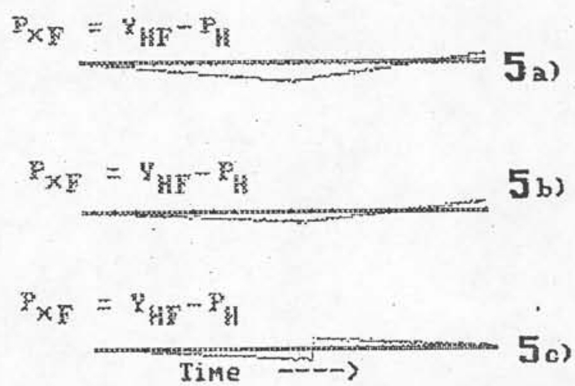
HYPOTHETICAL SUCCESSFUL EXPTS
'TRUE' REGRESSION FIT [Y_{HT}]



HYPOTHETICAL SUCCESSFUL EXPTS
'FALSE' REGRESSION FIT [Y_{HF}]



HYPOTHETICAL SUCCESSFUL EXPTS
'TRUE' REGRESSION FIT [Y_{HT}]



HYPOTHETICAL SUCCESSFUL EXPTS
'FALSE' REGRESSION FIT [Y_{HF}]

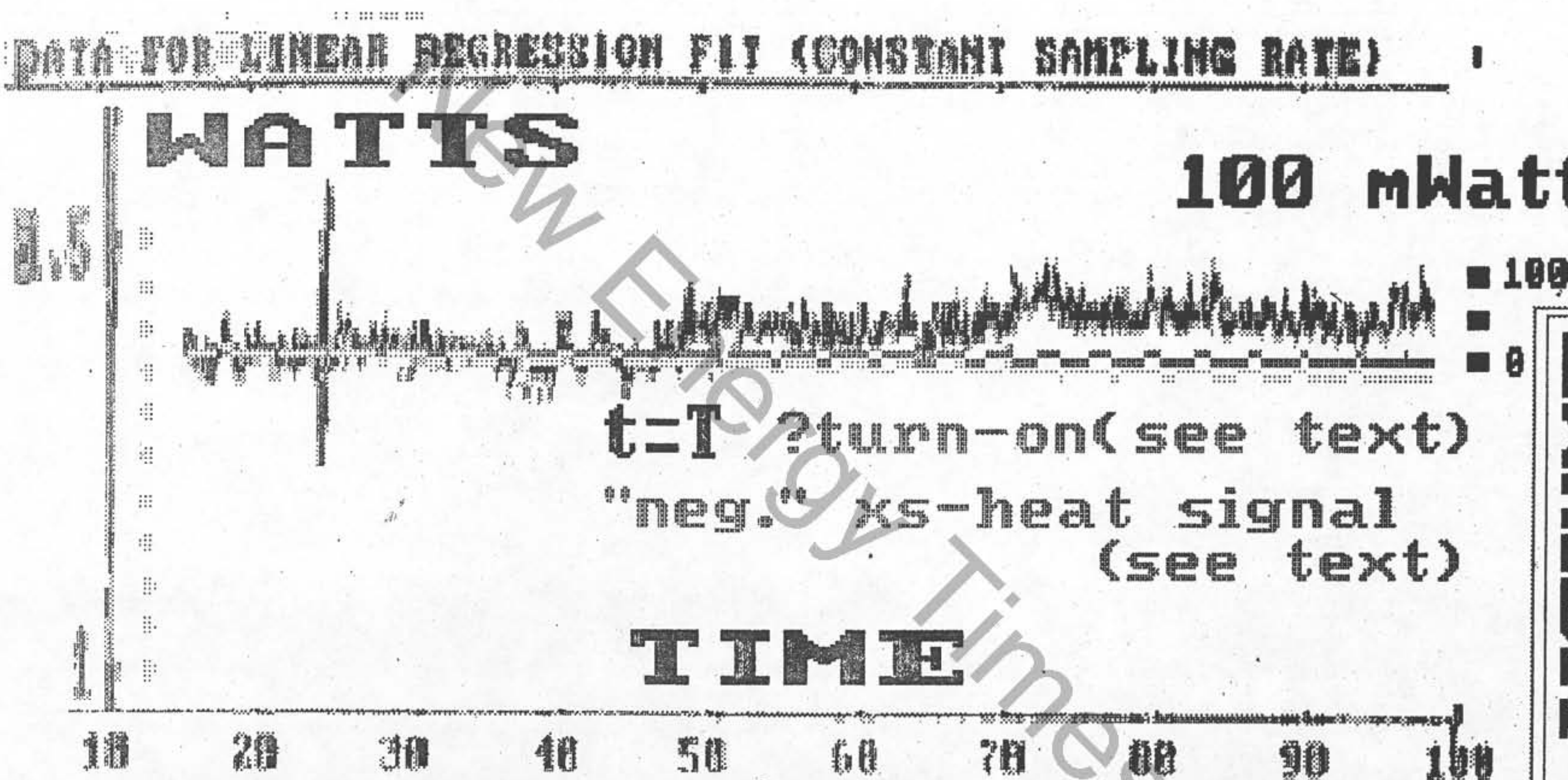


EXHIBIT 5

ANALYSIS OF PFC PHASE-2 DATA USING FIT TO PRELOADING BASELINE SHIFT

MITCHELL R. SWARTZ, M.D., Sc.D.

16 Pembroke Road
Weston, Massachusetts 02193
(617) 239-8383

CONFIDENTIAL

April 14, 1992

President Charles M. Vest
Room 3-208
77 Massachusetts Avenue
Massachusetts Institute of Technology
Cambridge, MA 02139

Dear President Vest:

Thank you for your letter of April 1, 1992, accompanied by Prof. Philip Morrison's March 20, 1992 response (to your March 10, 1992 request of him), and a memo prepared by Dr. Luckhardt (dated March 10, 1992) for Prof. Morrison.

Your March 10, 1992 letter to Professor Morrison appears to misstate the purpose of my report, which was not, as you suggested, to question whether the method of PFC Phase-II analysis was a linear function, nor even whether it was adequately described in the J. Fusion Energy paper. My report instead examined whether or not the same method of analysis was applied to the light and heavy water curves. The investigation centered on discrepancies between the final published data curves and earlier released data curves purportedly showing data from the same experiment. (Appendix of Addendum).

Far more disappointing is the fact that the March 10, 1992 Luckhardt/Parker memo now shows two new data curves for the same heavy water experiment which match neither the published curves nor the earlier released curves. It is now possible to optically distinguish five different curves, all purporting to show data from the same experiment. A table is now needed to sort them all out [Table 1 of Addendum lists the known curves, most shown in Exhibit 1].

The changes observed in the "first new" curve corroborate the fact that the published curve did contain "data" points that, at the very least, could have been better described in the text. One of the cited data points is motile by 24 milliwatts [Exhibit 2], and, the others are volatile [Exhibit 3]. Such flueric and chimeric character probably does not arise from a conventional physical basis.

In the "second new", presumably penultimate, curve in the Luckhardt/Parker memo [Type 5 in Table 1], <u>all</u> of the data points questioned and discussed in the original manuscript appear simply to have vanished, without explanation. Science by eraser could not be acceptable to the Office of the President because its tolerance indelibly insults the intelligence of the students, faculty, community, and alumnae/i at MIT.

Do disappearance and shift constitute an adequate response? Questions of possible asymmetric curve transformations and/or mislabeling of data, in a potential conflict-of-interest, have the appearance of potential impropriety appropriate for your office to consider. To limit concern as to whether curves are "satisfactorily described in the text" creates loopholes compatible with data manipulation, data fabrication and/or even plagiarism. In 1992, it would be wrong for MIT to even appear to condone any scientific misconduct, would it not?

Concern mounts because neither your letter, nor the documents with it, appears to respond to the critical issues. Old questions continue to remain unanswered. Table 1 and Exhibits 1, 2, and 3, and curves Types 4 and 5 suggest more questions.

The issues above must be distinguished from whether the PFC-Phase II paradigm had validity, a matter open to scientific debate and evaluation by peer review. Recognition is in order that it is through the Office of the MIT President that, for the first time the entire experimental data curve for D2O [Table 1, curve 1D] has been available along with the actual paradigm.

Because your response contains two new curves which confirm the incidental "data" points, but provides further Plasma Fusion Lab data only in the form of printed graphs, requiring laborious optical methods to analyze them, is it possible to please make promptly available one copy of the original experiment data [a copy of the original data in appropriate format such as the diskette used]. Your assistance would be very much appreciated in seeking release of this original data. It would also help bring the matter

Charles M. Vest, President

Room 3-208
77 Massachusetts Avenue
Cambridge, Massachusetts 02139-4307
617-253-0148

CONFIDENTIAL

April 29, 1992

Dr. Mitchell R. Swartz
16 Pembroke Road
Weston, MA 02193

Dear Dr. Swartz:

I have received and reviewed your letter of April 14, 1992, and subsequent corrections dated April 20, 1992.

The materials that you submitted on January 28, 1992, were reviewed by Professor Morrison who, while examining the specific issues of the linear transformation used to reduce the data in Albagli, et al, did, in fact, examine the adequacy of the data reduction more broadly, as well as other issues raised in your letter and manuscript.

In his memo to me, Professor Morrison recommended that Dr. Luckhardt prepare a supplementary data memo outlining the steps used to prepare the curves presented in Albagli, et al and to make this memo available to anyone who requested it. This seems to me to be a proper response to questions raised about the data presented in this publication. I will see that you receive a copy of this memo when it becomes available.

Disagreements about the interpretation of data that have been fully documented by publication is a proper subject for open scientific debate rather than continued internal examination by MIT. With the publication of the PFC memo, the data upon which such discussion can take place is now available to all. I intend to take no further actions with respect to this manuscript.

In this memo to me, Professor Morrison also recommended that your letter and manuscript, plus Professor Morrison's two reports and Dr. Luckhardt's data provided to Professor Morrison, be available to interested persons on request. In my letter to you, I asked for your permission to release your manuscript for that purpose. As I'm sure you understood, the only form of your manuscript which is acceptable for that purpose is that which was provided to Professor Morrison. If you do not wish us to release this manuscript and letter in that form, I will consider the matter closed and will not implement Professor Morrison's first recommendation.

Sincerely yours,

Charles M. Vest

Charles M. Vest

CMV:cbb

cc: Professor Morrison

CONFIDENTIAL

MITCHELL R. SWARTZ, M.D., Sc.D.
16 PEMBROKE ROAD
WESTON, MASSACHUSETTS 02193
(617) 237-3625

October 23, 1992

President Charles M. Vest
Massachusetts Institute of Technology
77 Massachusetts Avenue
Cambridge, Massachusetts 02139-4307

Dear President Vest:

Enclosed is an original copy of the article on the PFC Phase-II calorimetry data. The unexpected attacks on scientists discussing this issue have propelled me to reticently and tardively agree with you that this ought be available to the scientific community. Although the name of the document was changed to "Reexamination" rather than "Patterns of Failure", there appear to have been patterns of failure including lack of substantive response, failure to retract or clarify, and the decaying of awareness to graying and myopia [*].

For the record: Your administrators have told me that this matter has been "closed" to your office since January 1992. Since early February when you were given the first manuscript through today, I have had absolutely no direct substantive response from any other administration member or anyone at the PFC on this matter. This matter, generally unknown at MIT [no MIT publication appears to have covered Dr. McKubre's talk], has incurred other interest. Most ironic was a very early telephone call from Sri Lanka. Prof. Arthur C. Clarke seems to have more interest and diligence in corroborating what TIME [recent Millennium issue] labels as "fiction" than do either MIT or the Plasma Fusion Center regarding what they purport as "fact" in this matter.

Although any serious response would be appreciated, please spare me and do not send any new curves of, or derived from, the PFC Phase-II 1989 heavy water experiment.

My best wishes.

Sincerely yours,

Mitchell Swartz

[*] Attached is MIT's (vs PHS & NIH) definition of misconduct. Does misconduct have a 256-level [or more] gray scale?