MIT and Cold Fusion: A Special Report

Introduction by Dr. Eugene F. Mallove

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It is March 2003 as we mount permanently on the web this Special Report about MIT and Cold Fusion—almost the 14th anniversary of the announcement by Drs. Fleischmann and Pons at the University of Utah on March 23, 1989. We published this report in Infinite Energy Issue #24 in March/April 1999, but now it is available as a free internet download for all the world to see. Every citizen who is concerned about the future of clean energy generation and the future of our environment should read this report. Every MIT student, every MIT graduate, and every financial contributor to MIT should read it. Judge for yourself where the facts lead.

When many people are asked today about cold fusion, if they recall the 1989 announcement at all, they may offer remarks such as, "The experiment couldn't be reproduced." Or, "Cold fusion was quickly dismissed by other laboratories as a mistake." One of the most significant players in establishing in the public mind that thoroughly erroneous view was a team of investigators at MIT at its lavishly funded hot fusion laboratory, then called the MIT Plasma Fusion Center. The MIT group rendered a highly negative assessment of the Fleischmann and Pons claims, in part by performing its own attempt to reproduce the heavy-water/palladium excess heat experiment. The announced "failure to confirm" by the MIT group became one of the three top negative reports weighing against cold fusion in those early days. The U.S. Department of Energy (DOE) cited the MIT PFC's negative conclusion in rendering its rushed, condemning report in the fall of 1989; alphabetically, the MIT group's report is the first technical reference cited in the DOE Cold Fusion Panel's report.

It is therefore of considerable interest to understand what really happened at MIT in 1989, and the several years following, on the matter of cold fusion. The story is most certainly not what is regurgitated in numerous journalistic accounts, which are most often unflattering to Drs. Fleischmann and Pons and those researchers who followed their pioneering path. In fact, the story of cold fusion's reception at MIT is a story of egregious scientific fraud and the cover-up of scientific fraud and other misconduct—not by Fleischmann and Pons, as is occasionally alleged—but by researchers who in 1989 aimed to dismiss cold fusion as quickly as possible and who have received hundreds of millions of DOE research dollars since then for their hot fusion research. The cover-up of fraud, sad to say, reaches the highest levels at MIT and includes the current MIT President, Charles M. Vest. Remarkably, President Vest has recently been named by U.S. Secretary of Energy Spencer Abraham to head the Task Force on the Future of Science Programs at the Department of Energy. The high level task force will "examine science and technology programs across the department and consider future priorities for scientific research." MIT President Vest also serves on the President's Committee of Advisors on Science and Technology (PCAST) and is vice chair of the Council on Competitiveness. It is hoped that fair minded readers of the MIT and Cold Fusion Report will conclude that MIT's Charles Vest, who represents what are now provably unethical vested academic interests, is not a person whose scientific advice should be sought about DOE's science and technology plans.

The top man who, for now, will be leading DOE's panel of the "future of science" will get to pass judgment on whether hot fusion "science" should be funded at all, and if so to what extent, and at what institutions. One of those places just happens to be MIT, which receives tens-of-millions of dollars each year for its tokamak hot fusion research. Does this not seem to pose a slight conflict of interest—even had no scientific fraud been carried out against cold fusion at MIT in 1989, and even had Vest not participated in its cover-up? Will Vest recuse himself on the matter of hot fusion funding? Will there be any consideration of New Hydrogen Physics Energy (which includes cold fusion) by a DOE panel led by President Vest? We believe that under the circumstances it is not possible for cold fusion/LENR to receive any re-assessment—let alone a fair one—for a role in DOE's future science programs. If after reading this report concerned citizens feel the same way, they should consider writing to the White House to express their displeasure. Those who are more directly concerned about the integrity of MIT's research and reputation should write to Charles Vest or to other academic officers at MIT. Perhaps this might prompt a long-overdue official investigation of events in 1989-1992, followed by an official withdrawal of the MIT PFC's fraudulent calorimetry paper from the scientific literature.

MENS ET MANUS [MIT Motto—"Mind and Hand"]

The Official MIT Logo

Why "MIT and Cold Fusion"?

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MIT has played an extraordinary role in the history of cold fusion. By acts of commission and omission it continues to do so. On the occasion of the tenth anniversary of the startling announcement by Drs. Fleischmann and Pons on March 23, 1989, it is imperative that *Infinite Energy* explore the major role of MIT in shaping the history of the investigation of cold fusion.

Excess power evolution and unexpected ("impossible") nuclear changes in hydrogen-metal systems come under the rubric "cold fusion." Whatever its complete microphysical explanation turns out to be, cold fusion is of surpassing importance from the perspective of both science and technology. MIT's role in this affair bears close scrutiny by all who value what they assume MIT stands for: the open-minded quest for the truth about Nature and the application of new discoveries in science toward the betterment of humanity.

This report will be of special interest to all who are concerned about the well-being of MIT—its alumni/ae, students, faculty, and administration. What this brief history says about the actions and inactions in the area of cold fusion by one of the world's great technical universities has far-reaching implications for *everyone* interested in the heated cold fusion controversy. The history of MIT's reaction to cold fusion will become a remarkable case study in how a major scientific revolution is affected by the strong news media influence of MIT, by government funding of MIT, and by the scientific involvement of MIT professors, administration, students, and staff.

Extraordinary circumstances demand extraordinary action. It is our obligation—our moral imperative—to publish the detailed report that follows. Unfortunately for the world, many people still believe that the claims of a new, clean, abundant energy source and nuclear reactions occurring near room temperature were quickly and definitively disposed of by the careful work of scientists at MIT in the spring of 1989. Nothing could be further from the truth. These investigators at MIT did *not* produce definitive work. In fact, quite the contrary. A great opportunity for pioneering by MIT was missed and the baby *was* thrown out with the bath water—at least temporarily.

The actions of certain MIT staff members in 1989 were a major influence on the news media, on other scientists, and on the funding support for cold fusion. This is a matter of record. Though a small group of open-minded, involved faculty, staff, and alumni pursued and continue to pursue cold fusion, MIT as a whole did, indeed, acquire the deserved reputation as a "Bastion of Skepticism" on cold fusion. Sad to say, it was initially only a handful of MIT staff and faculty who gave MIT this reputation. They inappropriately drove many others—on campus and off—to dismiss the claims from Utah in 1989 and the research that has followed. Thus, the role of MIT in cold fusion—apart from the stellar accomplishments of those who persevered in scientific investigations—must be regarded as a permanent blemish on MIT's otherwise undisputed role as a leader in science, technology, and education. Fortunately, it is a bad mark that could be expunged by future good deeds—and apologies for past *mis*deeds. Is the MIT of 1999 up to that? We shall see.

One hopes that this true characterization of MIT's institutional behavior in the early 1990s and beyond will be but a temporary aberration. Yet if the past is any guide, there is little cause for optimism that a sudden awakening to the truth will occur at MIT. Perhaps the greatest hope lies in the youth—the students and graduates of MIT who will examine the scientific literature objectively. Most MIT professors today are simply oblivious to the subject. If they were to examine the research record of the past decade, they would readily see the opportunities to enter what is clearly an area of enormous potential. MIT students and alumni/ae may need to become catalysts that move faculty members and administration in the right direction, away from the present untenable position of denying well-established experimental facts and the theoretical developments by Professors such as Peter L. Hagelstein (Electrical Engineering and Computer Science) and Keith H. Johnson, formerly of the Department of Materials Science and Engineering.

I defy any previously uninvolved MIT student or graduate to examine the thirty-four references that are cataloged on pages 29-34 of this issue ("Key Experiments that Substantiate Cold Fusion Phenomena") and conclude that the information is not strong enough to warrant further investigation and action. These are only a small sample of many other papers and developments that can be cited. In 1999, it is possible for MIT graduates to visit laboratories in the U.S. and abroad where cold fusion investigation and development are moving forward. And soon enough there will be a host of demonstration sets and kits that research laboratories can purchase to observe the effects themselves. Some of these will be distributed by companies in which MIT graduates are involved.

The events of 1989-1992 are past history, but one must learn from the past or be condemned to repeat it. I hope that MIT students will also study the wrongs that have been done by MIT faculty and staff, which perverted the process of science in this area. Ironically, those very faculty and staff who so loudly pontificated about the *alleged* unethical actions of cold fusion researchers Drs. Martin Fleischmann and Stanley Pons are themselves most culpable. They launched distortions about cold fusion that have gained such wide currency.

As the record shows, the first assault against the truth in 1989 was press manipulation by faculty members engaged in the lavishly funded hot fusion research at MIT's Plasma Fusion Center (PFC). They did not believe the Utah work at all. They suspected that Pons and Fleischmann were engaged in a "scam," and they were concerned that if the public were to have a too open-minded attitude toward the prospect of cold fusion as an energy solution, funding for their beleaguered thermonuclear program would be endangered—even more so than in its perennial brushes with budgetary extinction.

The truth about the calorimetry experiment performed at MIT in 1989 under DoE contract funding (DoE Contract DE-ACO2-78ET51013) is stark and unambiguous. Its purported "negative" result was used to influence the U.S. Department of Energy's rushed 1989 report against cold fusion. In alphabetical sequence, it is the very first report cited in the U.S. DoE's ERAB (Energy Research Advisory Board) Cold Fusion Panel report of 1989. Some would characterize the data manipulation in the sixteen-author MIT paper of 1989 as mere "data fudging." We do not mince words: the use of improperly handled scientific data to draw in the public mind and in the mind of the scientific community a completely false conclusion about an emerging discovery of overarching importance to humankind is high-level scientific misconduct, plain and simple.

We do not know for certain who unethically manipulated the data, and that is not important, but it was, indeed, inappropriately manipulated. "Inappropriately manipulated" is actually a very charitable way of describing what was done. We *do* know, however, that this erroneous study in the spring of 1989 at the MIT Plasma Fusion Center was *defended* by then Plasma Fusion Center Director Ronald R. Parker. Parker continues to play a leading role in hot fusion. For several years after leaving the MIT PFC, he was stationed at the ITER (International Thermonuclear Experimental Reactor) in Garching, Germany. Since 1989, the U.S. Government has funnelled billions of dollars into magnetically confined thermonuclear fusion development on projects, such as ITER. Though ITER funding was recently killed by the U.S. Congress, funding of tokamak hot fusion continues at MIT and elsewhere.


Boston Herald

25 Cents

Monday May 1, 1989

MIT bombshell knocks fusion 'breakthrough' cold

The record is clear: Had MIT researchers behaved responsibly and ethically as scientists in the spring of 1989, it is most probable that a position of open-mindedness by MIT on the difficult subject of verifying the Utah claims would have averted the highly negative U.S. Department of Energy Report drafted in the summer of 1989. History would have been far different. Most likely, expensive engineering programs aimed at hot fusion reactors would have been cancelled in the early 1990s; plasma physics studies would have continued at MIT, and MIT researchers, including those at the Plasma Fusion Center, might have become the most eminent cold fusion researchers in the world.

It was not to be. Cool heads could have reserved judgement. They could have followed the experimental facts where they would ultimately lead, but they chose not to. Heads were not cool, they were hot. MIT could have been in the vanguard of the new scientific field as befits its leadership role in science, but this did not happen. MIT chose—and is continuing to choose—defense of its existing professional support from the Federal government over meticulously documented evidence of a new scientific field and the pathway to revolutionary technologies. In fact, the current President of MIT, Dr. Charles M. Vest, who ignored my written concerns in 1991-1992, is on a Federal panel that now has major impact on U.S. DoE energy research funding. Dr. Vest played a key role in papering over the misdeeds of 1989, as the following report clearly shows.

To use press manipulation and data manipulation to misdirect billions of dollars in Federal scientific funding is scandal of the highest order. To coin a phrase in this era of various "-Gate" scandals and cover-ups—this is HeavyWatergate, one of the greatest (but still to be acknowledged) scandals in the history of science.

To use the phrase "scientific schlock" which then MIT Plasma Fusion Center Director, Prof. Ronald R. Parker used against Fleischmann and Pons' work in 1989 (and later falsely denied using it, perhaps fearing legal action), aptly characterizes the methods used by certain MIT researchers against the new science of low-energy nuclear reactions. It was not only data manipulation or "processing," as Parker later would contend on the 1994 BBC and Canadian Broadcasting Corporation cold fusion documentary, "Too Close to the Sun," but there was a whole range of dirty tricks, deceptions, and self-deceptions that MIT professors and senior officials employed against cold fusion. We have catalogued most of them; this report is lengthy, but it is not comprehensive. Still, we urge you to read this history and learn from it. Perhaps concerned MIT graduates or faculty will have something to say about this. I encourage your feedback.

Some may say, "Why drag up the negative past, why not just emphasize the positive in the pages of this magazine?" To some degree that argument has merit, and we would like to be as positive as we can be. But we cannot ignore the reality that MIT's reputation as a "bastion of skepticism" against cold fusion has had a devastating effect on the progress of scientific investigation. As an article in *Infinite Energy* Issue No. 11, revealed, the U.S. Patent and Trademark Office is using bogus conclusions from MIT investigators in 1989 to deny U.S. citizens their Constitutional rights to be granted patents on their intellectual property. Some of those—at least two to my knowledge—are MIT graduates! If generations of MIT students, alumni/ae and professors do not learn from the tragic errors of the past, how can the future be anything but dim?

How can the scientific and technological might and the vast resources of MIT be turned to this most important scientific problem of cold fusion and new energy, if the subject is relegated to nonsense and pseudoscience by past unacknowledged misdeeds—done by *individuals*, but with the implied imprint of MIT? How many scientists are not tuned into cold fusion precisely because in their minds MIT's "smart people" back in 1989 proved that there was nothing to cold fusion claims? Lots. We hear from them all the time. When they first learn about what is happening in the cold fusion field today they are shocked, and then amazed to hear of the farce at MIT in 1989 and beyond.

MIT's Plasma Fusion Center continues to receive tens of millions of dollars per year for its tokamak hot fusion program. This clearly makes no sense now that cold fusion has begun to demonstrate its commercial potential as both an energy source and in the low-energy remediation of radioactivity generated in the past from the commercial and defense industry fission nuclear enterprises. Who would wish to waste further billions of dollars on a technology—hot fusion—that has already come under serious question for its technological and economic viability as a twenty-first century energy source, *if* there were a clear alternative?

The alternative is here. Come home to your traditions, MIT. Arise ye sons and daughters of MIT—help bring MIT back to excellence and intellectual integrity on a new science frontier.

MIT and Cold Fusion: A Special Report

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MIT Class of 1969, S.B. Aero/Astro Eng., 1970 S.M. Aero/Astro Eng.

Photo: E. Mallove

Introduction

When on March 23, 1989 Drs. Martin Fleischmann and Stanley Pons announced that they had measured nuclear-scale excess energy from a palladium-heavy water electrochemical cell, and that they had also detected some preliminary evidence of nuclear signatures from their exotic energy-producing reactions, the world was in awe. Their famous afternoon press conference at the University of Utah, coming less than twelve hours before the Exxon Valdez ran aground off Alaska's pristine coast and spilled millions of gallons of oil, reminded us of the serious problems linked with fossil fuel dependency. The Chernobyl nuclear reactor accident of 1986 also hovered in the background. It was already clear that conventional fission nuclear power was in deep political trouble in many countries. The close coupling of energy and the environment was growing ever more apparent.

Following the Utah disclosure, the prospect loomed of a quantum leap in energy technology—a solution to the dilemma of fossil fuel domination and its threats to the environment and world peace. The Utah claims soon came to be known as "cold fusion," because the electrochemists were saying that they had solved the problem that the plasma physicists and engineers in the "hot" fusion program had been working on for four decades.

The hot fusioneers had been trying to mimic the stars—to "bring the power of the Sun down to Earth" in the form of controlled, thermonuclear fusion. This was the attempt to use the deuterium in ordinary water as an effectively infinite fuel supply. In only one cubic kilometer of ocean, the nuclear fusion energy that could be extracted from the approximately 1/6,500th fraction of water's hydrogen that is heavy hydrogen exceeds the combustion energy content of all the known oil reserves on Earth.

Tantalizing as the prospect of infinite energy from the oceans was, the hot fusion program had never generated even a single watt of excess power in its huge plasma reactors, which cost hundreds of millions of dollars per year to support. Success—"break-even" or "more energy out than in"—with magnetically-confined hot plasma fusion always seemed to be twenty years away. This led to the perennial joke that hot fusion is "the energy source of the future. . .and always will be." Moreover, even if the hot fusion program were to succeed in building a commercially viable central-station generator of electricity sometime in the year 2050 or beyond, the technology would have serious limitations. The energy from the hot fusion reaction of deuterium and radioactive tritium, which had to be supplied in bootstrap-fashion from the reaction, would emerge in the form of deadly neutron radiation (14 MeV neutrons). That would have to be transformed into more benign thermal energy in a hot jacket of molten lithium in order to heat water for steam-generated electricity. The practical engineering problems would be enormous, the technology would add more nuclear waste to the global inventory (though not as much as conventional fission power, or so claim the tokamak hot fusion advocates), and it was far from certain to be economically viable.

In fact, in October 1983 MIT Professor of Nuclear Engineering, Lawrence M. Lidsky, published an article ("The Trouble with Fusion") condemning the hot fusion program. It was a high-profile cover story for MIT's *Technology Review*. The stark black and white cover of the issue read, "Even if the fusion program produces a reactor, no one will want it." Other key remarks made by the outspoken Lidsky, who was then an Associate Director of the Plasma Fusion Center: "Long touted as an

Partial Chronology of Events Relating to MIT's Handling of Cold Fusion

March 23, 1989, afternoon
Fleischmann and Pons announcement at the University of Utah.

April 17, 1989
Richard Saltus of the *Boston Globe* writes to MIT President Paul Gray complaining about lack of access to the MIT Plasma Fusion Center (see *Exhibit D*—May 1 response by MIT President Gray (see *Exhibit E*).

April 26, 1989
MIT Professor Ronald Ballinger testifies before U.S. House of Representatives Committee on Science, Space and Technology (see *Exhibit A*).

April 28, 1989
Professors Ronald R. Parker and Ronald Ballinger give interview to Nick Tate of the *Boston Herald*, planting anti-cold fusion story (see *Exhibit B*).

April 30, 1989
A late-night call by Professor Parker to Eugene Mallove's home in Bow, New Hampshire triggers press release to wire services denying the substance of the *Herald's* banner page-one story the next day (see *Exhibit C*).

May 1, 1989
Press release from the MIT News Office issued, which denies *Boston Herald's* characterization of Professor Parker's remarks about Pons and Fleischmann's work as "scientific schlock" and "maybe fraud." (See *Exhibit C.*) • MIT President Paul Gray sends letter to *Boston Globe*.

June 26, 1989
MIT Plasma Fusion Center holds "Wake for Cold Fusion" party *weeks* before Phase-II calorimetry data are analyzed!

July 10, 1989
Section of PFC/JA-89-34 report exists which shows intermediate processed Phase-II calorimetry data. Data are not yet time-averaged. This was *not* published (see graphs, p. 11).

July 13, 1989
Section of PFC/JA-89-34 exists which shows intermediate processed Phase II calorimetry data. Data for both H_2O and D_2O have been time-averaged in one-hour intervals. Power curve for D_2O result retains roughly the same shape as unaveraged data but has been shifted down. This *was* published (see graphs, p. 11).

July, 1989
Publication of PFC/JA-89-34 cold fusion experiments report based on work funded by DoE contract No. DE-AC0278ET51013. • Mid-July initial draft of DoE ERAB Cold Fusion Panel report is negative.

July 18, 1989
MIT PFC Director Parker's Memo on "Cold Fusion Mug" and "stamp out scientific schlock" t-shirt (see *Exhibit F*).

November 1, 1989
Final DoE ERAB Cold Fusion Panel report is issued. It cites negative MIT PFC report—"Albagli *et al*." as the first reference. (By contrast, positive results from U.S. Naval Surface Weapons Center are omitted.)

March 26-28, 1990
"Energy and Environment in the 21st Century" conference at MIT. MIT President Paul E. Gray makes unflattering comparison of cold vs. hot fusion (see *Exhibit G*).

July 19, 1990
Chief Science Writer Dr. Eugene Mallove of the MIT News Office hears for the first time parts of the Parker/Ballinger/Tate interview tape played over telephone by Nick Tate of the *Boston Herald* (see *Exhibit B*).

August 15, 1990
Meeting with Dr. Stanley Luckhardt (MIT Plasma Fusion Center) and independent scientist, electrochemist Dr. Vesco Noninski, in Dr. Luckhardt's office. Within a week Dr. Noninski is challenging the analysis of the MIT PFC calorimetry on analytical grounds.

September 8, 1990
Letter from PFC team member rejecting Noninski's analysis of the MIT experiment—letter provides minimal technical details.

October 10, 1990
Letter to Dr. Noninski from Chemistry Dept. head Professor Mark Wrighton saying "no evidence whatsoever" has been obtained to verify Pons and Fleischmann claims. Wrighton provides no technical details in rebuttal (see *Exhibit H*).

January 16, 1991
Eugene Mallove meets with Prof. Ballinger in his office and Ballinger remarks about Pons and Fleischmann being "crooks" who could have been "locked up in jail." At Gordon Institute lecture Ballinger makes other negative remarks about Pons and Fleischmann (see *Exhibit A*).

January 19, 1991
Mallove discovers the July 1989 down-shifted MIT excess-heat curve (See graphs, p. 11), which later became the subject of controversy.

January 25, 1991
Mallove has lunch at "Networks" in MIT Student Center with Dr. Luckhardt. Luckhardt can't explain how "bias" was taken out. Luckhardt said there could be 20 milliwatts excess power in the MIT PFC results, but "not the 80 mW that Fleischmann was talking about."

April 12, 1991
Letter from Eugene Mallove to MIT President Charles M. Vest, copy to former President Paul E. Gray, suggesting organizing an MIT panel to re-examine cold fusion in light of accumulating knowledge. No response was ever received from either MIT President (see *Exhibit I*).

April 29, 1991
Eugene Mallove writes letter to Dr. Luckhardt requesting calorimetry information (see *Exhibit J*).

May 13, 1991
Mallove's first call to Dr. Luckhardt to try to get MIT PFC H_2O curve.

May 20, 1991
Dr. Luckhardt cancels previously scheduled get-together with Mallove and says he forgot to get raw data at his other office. He puts Mallove off until the following Friday.

May 24, 1991
Two calls to Dr. Luckhardt (10 am and 1:30 pm)—phone messages left about getting data on H_2O curve. No response to Mallove's messages. • Near final version of Eugene Mallove's resignation letter exists.

May 29, 1991
Taping of WGBH Boston Channel 2 clip on Cold Fusion—Mallove and MIT PFC's Dr. Richard Petrasso. • Final refusal by Stan Luckhardt to turn over PFC calorimetry data.

June 7, 1991
Professor Ronald Parker publicly disparages the PFC team's calorimetry work on cold fusion! (See *Exhibit K.*) • Eugene Mallove submits his resignation from the MIT News Office (see *Exhibit L*) following the one-hour talk on cold fusion by Frank Close at the PFC and a heated question and answer session (see *Exhibit K*).

June 14, 1991
Eugene Mallove's request faxed to Professor Parker for promised data relating to PFC cold fusion calorimetry experiments (see *Exhibit M*).

July 30, 1991
No response yet received from the PFC. Second request sent to Professor Parker (see *Exhibit N*) • Press release from MIT PFC "stands by" the 1989 PFC results and conclusions (see *Exhibit T*).

August 8, 1991
Fax letter from Parker to Mallove giving Stan Luckhardt's revised objectives of MIT PFC experiments and stonewalling again on data transfer (see *Exhibit O*).

August 9, 1991
WBUR program about Mallove's resignation and charges airs in Boston (see *Exhibit P*).

August 13, 1991
Fax received by Mallove from Parker with heavy water and light water curves (see *Exhibit Q*).

August 18, 1991
Formal request by Eugene Mallove to MIT President Vest for investigation of scientific misconduct at MIT PFC, concerning both data mishandling and deception of press and MIT News Office (see *Exhibit R*).

September 16, 1991
Eugene Mallove responds to August 30, 1991 MIT PFC Press Release (see *Exhibit T*).

October 9, 1991
President Vest writes to Prof. Philip Morrison requesting misconduct inquiry opinion (see *Exhibit U*).

October 14, 1991

Prof. Morrison's initial inquiry report to President Vest (see *Exhibit V*).

October 17, 1991
President Vest's response letter to Mallove (see *Exhibit W*).

October 24, 1991
Mallove's letter to President Vest rejecting Morrison's assessment and requesting a formal investigation (see *Exhibit X*).

November 11, 1991
Nobel Laureate Julian Schwinger speaks about cold fusion at MIT physics gathering celebrating birthday of his former student. Evidently this has no effect on Physics Dept. resistance (see pages 18-20).

December 31, 1991
Mallove's letter to President Vest asking for status (see *Exhibit Y*).

January 2, 1992
Electrochemist Dr. Andrew Riley dies in cold fusion explosion at SRI International. Dr. Brian Ahern (an MIT graduate) tried to warn SRI of danger, but telephone call did not go through.

January 6, 1992
President Vest sends brush-off letter to Eugene Mallove (see *Exhibit Z*).

February 9, 1991
Eugene Mallove sends new evidence of scientific misconduct to President Vest based on report of MIT graduate Dr. Mitchell Swartz's independent investigation. Mallove demands thorough investigation (see *Exhibit Z-1)*. Further prompt to Vest on February 21 (see *Exhibit Z-2*).

March 10, 1992
Dr. Luckhardt sends memo to Prof. Morrison giving further explanations of 1989 work. Redefines the objective of experiment as "turn on" of "anomalous heating event" rather than D_2O vs. H_2O comparison! (See *Exhibit Z-3*.)

March 19. 1992
NIH physicist Dr. Charles McCutchen's letter to President Vest complaining about ethical problems with MIT PFC experiment (see *Exhibit Z-4*).

March 20, 1992
Prof. Morrison's second report to President Vest. Suggests Dr. Luckhardt continue to have possession of data and should make further assessments! (See *Exhibit Z-5*.)

April 1, 1992
President Vest's final brush off letter to Eugene Mallove giving an unacceptable conclusion. This was no April Fool joke (see *Exhibit Z-6*).

April 2, 1992
MIT Associate Provost Sheila Widnall's letter to Dr. McCutchen—a further brush-off and statement that experimenters will continue to be processing contested data and will be writing a future memo with experiment "clarifications." (See *Exhibit Z-7*.)

May 1992
Publication of MIT PFC Technical Report (PFC/RR-92-7), a single-author (Luckhardt) "Technical Appendix to D. Albagli *et al. Journal of Fusion Energy* article" (originally 16 authors!) Error limits of MIT PFC calorimetry are further expanded and the nature of the experiment was further redefined to deflect data mishandling accusation.

July 26, 1992
Dr. McCutchen letter to Provost Widnall, asks MIT PFC to publish a correction that the experiment was not as advertised (see *Exhibit Z-8*).

August 3, 1992
Provost Widnall's letter to Dr. McCutchen giving final MIT brush-off (see Exhibit *Z-9*).

August 18, 1992
Dr. McCutchen letter to Eugene Mallove details his frustration with Provost Widnall's response (see *Exhibit Z-10*).

August 19, 1991
Dr. McCutchen's final letter to Provost Widnall saying, "I am sorry MIT continues to tough it out. Apparently the university feels it need not be fair to cold fusion people." (See *Exhibit Z-11*.)

August 1992
Dr. Mitchell R. Swartz publishes fourteen page analysis of MIT PFC Phase II Calorimetry in *Fusion Facts* newsletter. Also published, in part, in subsequent *Proceedings of Fourth International Conference on Cold Fusion* and elsewhere.

MIT *TECHNOLOGY REVIEW* AND COLD FUSION

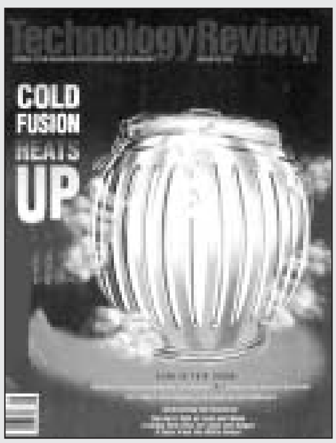
To its credit, MIT *Technology Review* published an excellent feature review article about cold fusion by Dr. Edmund Storms (Los Alamos National Laboratory, ret.) in the May/June 1994 issue. This might have been a turning point in media coverage of cold fusion, had this influential magazine continued to follow the subject. It did not.

A firestorm of protest against the Storms article had confronted then *TR* editor Dr. Steven J. Marcus, which led him to write an editorial in the August/September 1994 issue, "Don't Blame the Parent." He wrote, ". . .we'll occasionally make people angry for having allowed an author to present the 'wrong' point of view. But reaction to the cold fusion story marks the first time in my memory that dissenting readers criticized the magazine's editors not only for choosing to run this material—variously describing it as 'dreadful,' 'appalling,' 'pseudo-scientific,' 'irresponsible,' and 'an example of the goggle-eyed approach to science'—but for hurting the institutional parent in the process." Marcus heard from so-called scientists who said that the article "casts disgrace on MIT," one who said that it "trashes research at MIT," and one who wrote that it "embarrasses the Physics Department, MIT, and all graduates of MIT." (MIT students are advised to look up these articles to see for themselves what all the commotion was about.)

There were, of course, positive responses as well, which praised the editor for having found the courage to publish the Storms cold fusion article, but these did not apparently reflect the majority of the sentiments received. Marcus published six response letters in that August/September issue, including a positive one from cold fusion theorist and MIT Professor Keith Johnson and a negative letter from MIT Nuclear Engineering and Materials Science Professor Kenneth C. Russell.

Unfortunately, the protest of the Storms article in *Technology Review* was not the first time MIT faculty had become upset with *Technology Review* on the matter of cold fusion. The negative opinion of MIT Physics Professor Herman Feshbach caused the previous editor of *Technology Review*, Jonathan Schlefer, to back down in the spring of 1991 from his intent to publish *my* cold fusion review article. This 1991 article would have said essentially what Storms did in 1994, but by 1994, even more confirmatory evidence could be cited. Schlefer had *accepted* my article after much editorial revision! Both positive and negative viewpoints were presented in that approved article, plus my clearly identified opinion that the evidence was building strongly toward proof of the phenomenon. That was not negative enough for Feshbach—who called all evidence for cold fusion "junk." This sorry episode of censorship was one of the key reasons for my resignation from the MIT News Office in June 1991 (see *Exhibit K* for more on this event).

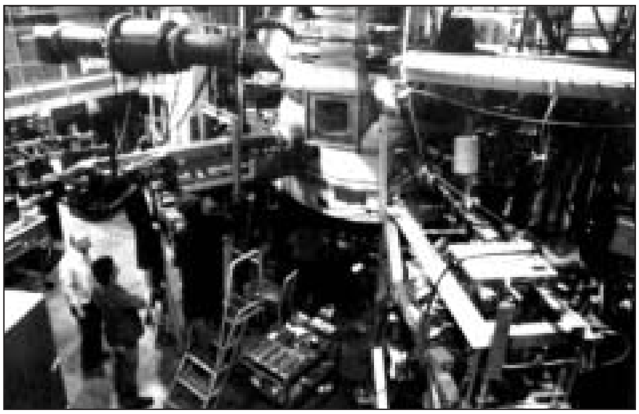
Prof. Feshbach had told me his other reason for not wanting the article to be published. He said that he had ". . .fifty years of experience in nuclear physics and I know what's possible and what's not." He later demonstrated the same sort of monumental arrogance and ignorance when he appeared on ABC Television's Nightline program, June 11, 1997. Even though Feshbach admitted that he knew absolutely nothing about the Patterson Power Cell™ cold fusion device which was the subject of the program, he told viewers that he could "categorically" state that there were no nuclear reactions occurring in it.—*EFM*



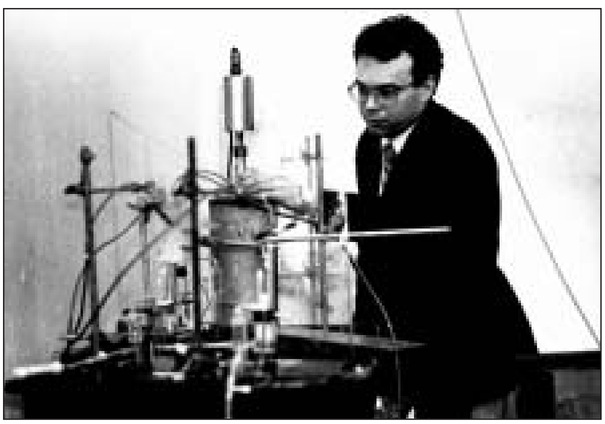
Professor Lawrence M. Lidsky

Technology Review October 1983

inexhaustible energy source for the next century, fusion as it is now being developed will almost certainly be too expensive and unreliable for commercial use."; "The scientific goal of the fusion program turns out to be an engineering nightmare."; "A fusion reactor might well produce only one-tenth as much power as a fission reactor of the same size."; "The drawbacks of the existing fusion program will weaken the prospects for other fusion programs, no matter how wisely redirected." Foreshadowing the benefit of cold fusion that would emerge over five years later, Lidsky also wrote of *aneutronic* hot fusion: "Neutrons induce radioactivity and damage reactors. Neutron-free fusion might provide inexhaustible, benign power." Prof. Lidsky later moved into work at MIT on advanced fission reactors, but kept an open mind about cold fusion after it emerged.



Alcator-C hot fusion tokamak reactor at MIT Plasma Fusion Center. Magnetic fields confine a hydrogen plasma while the temperature and density are increased. *(From MIT Plasma Fusion Center pamphlet, "Fusion Energy Research")*



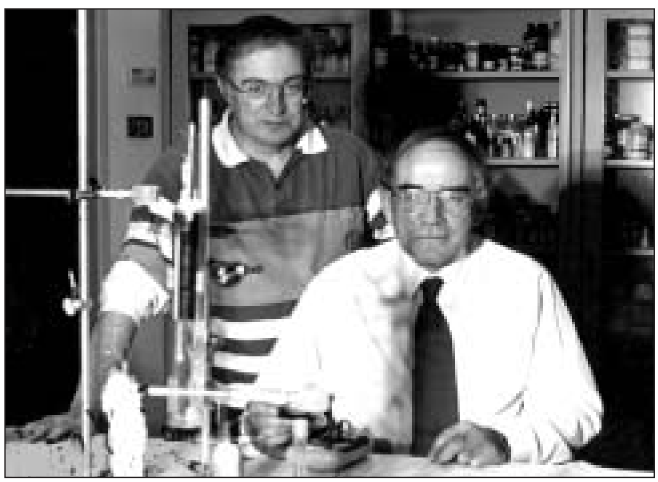
Dr. Stanley Luckhardt of the MIT Plasma Fusion Center poses in the spring of 1989 with nest of cold fusion cells on a lab cart. Ironically, this cold fusion equipment was removed from the cavernous neutron-shielded room to make way for the next generation Alcator-C hot fusion reactor. (*MIT News Office Photo*)

Enter Fleischmann and Pons

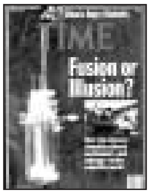
Onto the scene on March 23, 1989 came two world-class electrochemists, Professors Martin Fleischmann and Stanley Pons, who were boldly claiming on international television that they had already achieved break-even in some form of nuclear fusion, but in a humble jar of heavy water—without lethal attendant radiation! This was an instant prescription for controversy. By analogy, it was as shocking and insulting to the hot fusion people as if they had been told that their television set had not been able to turn on for decades because they had neglected to plug it in! The threat to the hot fusion enterprise was palpable and real. More to the point: even if the hot fusion people did not believe the Utah claims were sound, the threat that some hot fusion funding (perhaps $25 million) would be diverted by the U.S. Congress to study cold fusion was very real. The always financially embattled hot fusion program was running scared in the onslaught of cold fusion news.

MIT Professor Ronald Ballinger, who would play a key role in the scandalous attacks against cold fusion, testified before the U.S. House of Representatives' Committee on Science, Technology, and Space (see *Exhibit A*). His April 26, 1989 testimony had a seemingly appropriate "wait and see" message, but behind the scenes Ballinger, Parker, and other MIT hot fusioneers had among themselves already dismissed cold fusion. They were sharpening their knives against Fleischmann and Pons. (See recorded interview with *Boston Herald, Exhibit B.*)

The idea that deuterium in heavy water might be undergoing some kind of nuclear fusion reaction within the palladium cathodes of the Pons-Fleischmann cells was, of course, very difficult to accept. Where was the expected lethal radiation, for example, which standard nuclear physics would seem to predict? Why weren't Pons and Fleischmann dead if they had truly generated even a minor fraction of a watt of cold fusion-derived energy? This became known as "the dead graduate student" problem. Furthermore, how could the palladium cell have overcome the natural, very high electric repulsion force between the positively charged deuterium nuclei—the so-called Coulomb barrier that had been thought to put an absolute barrier between high energy nuclear physics and ordinary chemistry? Elements (except those that are radioactive or which spontaneously fission) should retain their identities. This is basic scientific "fact" doled out in high school science classes. Room-temperature fusion of even *light* elements such as hydrogen or lithium was considered to be prima facie impossible. (There was an old pre-



Professors Martin Fleischmann (R) and Stanley Pons (L).
Infinite Energy archives



cold fusion era joke among MIT students about the need for a Department of Alchemy, but MIT was apparently not quite ready for the real thing!)

Conventional understanding was that nothing at ordinary conditions could bring the nuclei of deuterium close enough together such that the nuclear forces, at very close approach, would take over and facilitate fusion to helium—or to anything else. That *two* "miracles" were implicit in cold fusion was just too much to bear for the mainstream physics community. Nonetheless, the establishment held its skepticism in check—at least publicly—for several weeks. Some scientists told the news media that the claims were "very interesting," but they thought they were unlikely to be true. By implication, they suggested there might be a mistake, which they would likely find after doing their experiments to check up on Fleischmann and Pons.

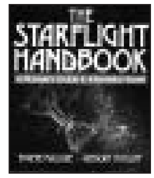
Immediately, the cold fusion story became very big news all around the world. Thousands of scientists and basement inventors tried to verify—or disprove—the claims from Utah. The May 8, 1989 editions of *Time*, *Newsweek*, and *Business Week* ran prominent cover stories on cold fusion—a first for science coverage apart from events in space exploration. The question of the hour was—as *Business Week* editorialized on its cover: Is cold fusion a "miracle or a mistake"? Of course that was the possibility that had to be excluded—a major mistake in either excess heat measurement or nuclear measurements.

When cold fusion was announced, I had the good fortune to be the chief science writer at the MIT News Office, the main public relations arm of MIT. My tenure was from September 1987 through June 1991. Previously, I had written major scientific articles for MIT *Technology Review*, the magazine of my alma mater's Alumni/ae Association. After leaving my job as an aerospace engineer at MIT Lincoln Laboratory in 1985, I shifted careers and had worked as a science writer and broadcaster for the Voice of America in Washington, DC. I would also eventually teach science journalism both at Boston University and at MIT in the Department of Humanities (both when I was in the MIT News Office, and for a time afterward).

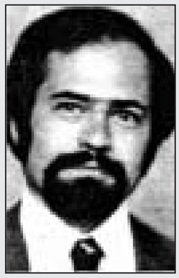
My position at the News Office required me to interact daily with members of the national and international press. Thus, when the Pons and Fleischmann announcement occurred, it was my job to report to the media what certain key scientists at MIT were thinking about the amazing claims out of Utah.

I had already been instrumental, some weeks before March 23, 1989, in exposing the entire science writing staff and senior editors of *The Wall Street Journal* to the hot fusion program at MIT, where the Alcator line of tokamaks were being developed. I did that proudly. In fact, I remember introducing Plasma Fusion Center (PFC) Director Ronald R. Parker to the *Wall Street Journal's* Jerry Bishop, the senior reporter who would later write an award-winning series of articles on cold fusion. As an engineer turned writer-engineer, I had been since age sixteen an advocate for hot fusion.

While a student in engineering at MIT in 1967, I remember being impressed by the Russian hot fusion exhibit at the world Expo in Montreal. I thought that hot fusion offered a real though difficult-to-develop solution to the world's energy needs. Because I had been trained as an aerospace engineer with a particular interest in interstellar propulsion methods, I was fond of hot fusion, because it might offer a very high performance propulsion system for limited travel to the "local" stars. I would write of this in my 1989 book, co-authored with colleague Dr. Gregory Matloff, *The Starflight Handbook: A Pioneer's Guide to Interstellar Travel* (John Wiley & Sons). In 1969 I had written a lengthy term paper for MIT course 16.53 on the Bussard Interstellar Ramjet concept, which used the hydrogen of the interstellar medium as fusion fuel. In the 1970s and 1980s, I collaborated with physicist Robert L. Forward of Hughes Research Laboratories on lengthy bibliographical studies of the related subjects of advanced interstellar propulsion concepts and the search for extra-terrestrial civilizations (SETI).



The cold fusion story quickly drew very heavy media attention, and I was rapidly drawn into the frenzy that resulted at the MIT News Office. There were many requests for interviews with



Massachusetts Institute of Technology
TECH TALK

September 16, 1987
Volume 32, Number 7

Dr. Eugene Mallove '69 named News Office science writer

Dr. Eugene F. Mallove '69, an engineer and scientist who has written widely on science for the Voice of America, *The Washington Post* and *Technology Review*, has been appointed chief science writer for the MIT News Office. The appointment, as assistant director, was announced by Kenneth D. Campbell, director of the News Office. "Gene Mallove brings three great strengths to the News Office: his background in science and engineering; his MIT experience; and, most importantly, his ability to communicate his fascination for science, both in the written word and on the air-waves. I am delighted to welcome Dr. Mallove back to MIT," said Mr. Campbell.

A science writer for the past five years, Dr. Mallove's most recent position was as international science writer and broadcaster at the Voice of America, which he joined in 1985. He was responsible for a weekly 15-minute "New Horizons" program on science, technology, and medicine, and for a daily five-minute program of science teaching to the world, "Science Notebook."

He has written free-lance articles for the *Washington Post* and other newspapers, and for *Technology Review* and a new magazine, "Computers in Science." He is the author of *The Quickening Universe*, to be published by St. Martin's Press this fall.

Dr. Mallove received his SB in aeronautical and astronautical engineering from MIT in 1969, and his SM in the same field in 1970. In 1975, he received from Harvard University his ScD degree in environmental health sciences, specializing in aerosol physics and air pollution control.

His career in science and engineering includes work as a consulting astronautical engineer on space propulsion systems with Hughes Research Laboratories, 1970-77; engineer with The Analytical Science Corporation, 1977-79, and with Northrop Co. (Precision Products Division), 1980-81; systems engineering manager with Jaycor, Systems Engineering Division, 1981-82; and engineer with MIT Lincoln Laboratory, 1983-85.

He founded a firm, Astronomy New England, Inc., which developed and marketed astronomy-related products for six years, ending in 1985. (Reprinted from *MIT Tech Talk*)

Ronald Parker and others at the PFC. A group of PFC and Chemistry Department scientists and students had immediately set out to check the Utah claims. There were regular calls to me at the News Office to provide status reports, photo opportunities, and interviews for members of the PFC team. Then in mid-April 1989, Professor Peter L. Hagelstein, a laser and quantum physics expert in the MIT Department of Electrical Engineering and Computer Science, went public with a theory of how cold fusion might be explained in terms of "coherent nuclear reactions." Professor Keith H. Johnson of the MIT Dept. of Materials Science, another MIT luminary, with deep knowledge of palladium hydrides and superconductivity in his background, also put forth a theory that allowed nuclear reactions to occur in the Pons-Fleischmann cells. Unlike Hagelstein, who proposed pure nuclear reactions operating in a coherent fashion with a metal lattice, Johnson tried to explain the excess heat as a result of peculiar effects of so-called Jahn-Teller chemical bonding. I thought this was a wonderful honor for MIT, to have two open-minded theorists approaching the Utah results with caution, but attempting to pose explanations for it if it could be confirmed. Others at MIT did not hold this view. The Hagelstein-Johnson work was almost immediately regarded with disdain—particularly by the plasma fusion people. So there were early-on two camps at MIT, one largely negative (but at that point generally restrained in its public comments), and another putting forth hope that the Utah discovery was no mistake and could be explained on theoretical grounds—much as Nobel Laureate Julian Schwinger began to try to do at that time. (See Julian Schwinger's talk on cold fusion, which he delivered at MIT in November 1991.)

The story was growing more fascinating every day, as reports of positive results in replication efforts came in from around the world, as well as news of negative results from other laboratories. I was able to write a series of articles* about MIT's response to cold fusion for *MIT Tech Talk*, the administration newspaper that circulates on campus and is used as a public relations tool to influence the general mass media. (*Tech Talk* is not to be confused with the MIT student newspaper, *The Tech*.) [*April 5, 1989, Vol. 33, No. 27, "Recent 'cold fusion' claims baffle experts"; April 26, 1989, Vol. 33, No. 29, "Cold fusion: theories, controversy abound": May 3, 1989, Vol. 33, No. 30, "Group finds flaw in cold fusion experiment"; May 31, 1989, Vol. 33, No. 34, "Cold fusion is still a hot topic."]

There were also a few immediate false-positive results from outside MIT, such as from Georgia Tech, that were reported prematurely to the press. These left several scientists embarrassed when they had to retract or sneak away with red faces—as Charles Martin did at Texas A&M University. Unfortunately,

MIT Prof. Peter L. Hagelstein in his laser laboratory at MIT, circa 1994.
(*Photo by Eugene Mallove*)

MIT Prof. Keith H. Johnson at home in his movie production studio.
MIT News Office

Office of the President

May 6, 1988

Mr. Eugene F. Mallove
Room 5-111

Dear Gene,

I write, a bit belatedly, to express my appreciation to you for your tremendous help this spring in preparing those remarks on scientific illiteracy. As you know, I have gotten considerable mileage from them already (the Washington campaign opening, the New York Academy, and the Time-Life editors) and will get even more mileage from them in the months ahead. Your collaboration in this task was a tremendous help, and I am much in your debt.

I suspect you were startled, as was I, by the revelations this week about the Reagans' reliance on astrology. On second thought, I guess we should not be surprised.

With warmest regards and best wishes,

Sincerely yours, Paul E. Gray PEG/mmd cc: Kathryn W. Lombardi

Eugene Mallove was held in high regard by many members of the MIT faculty and administration, as this 1988 letter from Dr. Gray attests.

Dr. Eugene F. Mallove's Input for MIT President Paul Gray's Remarks for MIT Technology Day 1989 (Prepared, May 11, 1989)

Two decades ago humanity was poised to begin an epic journey—the first manned landing on another celestial body, the Moon. We well recall the extraordinary contributions made by the thousands of scientists and engineers to the epic flight of Apollo 11 in July, 1969—in particular the work of MIT engineers who developed the guidance and navigation systems for our spectacularly successful lunar missions.

We remember the electric atmosphere and the spirit of global celebration—even amidst domestic conflict and war—as the world in rapt attention watched a tiny contingent of humanity make a giant leap onto a new world. And we remain very proud that a son of MIT who received his doctorate from the Department of Aeronautical and Astronautical Engineering, Col. Edwin E. Aldrin, Jr., was aboard Apollo 11 and became the second human being to step onto the lunar surface.

The launching of the first Sputnik in 1957 is often cited as the beginning of the space age. The era from 1957 through the watershed year of 1969—incidentally, the time of graduation of the 100th MIT class—contrasts remarkably with the present. Slide rules were still the indispensable companions of scientists and engineers; desk-top computers were unheard of and children did not play at video games; energy crises were only a theoretical possibility—as was the threat of global climate change; environmental issues were yet to rise to the fore of public attention; genetic engineering had not entered the public lexicon; and nuclear power seemed to have an endless, promising future.

Today we aim to go beyond the Moon, not only with our space program, but with our ambitions to make a better planet Earth through science and technology. But these ambitions are beclouded by a worrisome anti-technology backlash and pervasive scientific illiteracy in our society that threatens not only our venture into space and efforts to reach a reasonable accommodation with nature, but our very standard of living. As MIT's recently released "Made in America" study suggests". . .

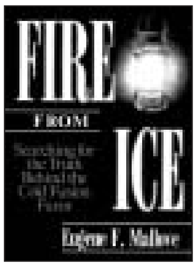
these and other political issues came to color the attitude of many observers of the cold fusion scene—especially because Pons and Fleischmann had been forced to make their announcement via a press conference, rather than through scientific publication. The reasons for the press conference are too involved to explore here, although Dr. Fleischmann himself sheds some additional light on the topic in an essay in this issue (not reprinted here, see Issue 24 of *Infinite Energy*). However, it is a matter of record that Fleischmann and Pons really did not want to make their disclosure for another eighteen months until they understood their discovery better. The parallel claims by physicist Steve Jones of nearby Brigham and Young University, patent issues, and other conflicts brought the issue into public view in March 1989. Further complicating the story and enraging other scientists, lawyers at the University of Utah prohibited or retarded the disclosure of experimental details by Fleischmann and Pons. As a historian of this subject, I feel confident in stating that if Fleischmann and Pons had been allowed to hand out at their press conference the pre-print of their paper which was later that spring published in the *Journal of Electroanalytical Chemistry*, the intensity of opposition to cold fusion would have been reduced by at least 50%.

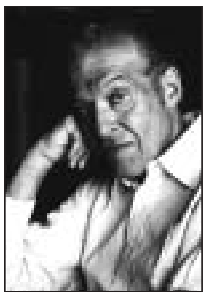
If one had any interest in the process of science, this was already a first-class captivating story. Naturally, I called my literary agent at the time, Richard Curtis, and alerted him that there might be a new book for me in this saga. Because I had already written two books (at the time I was still completing *The Starflight Handbook*), it was not difficult to convince John Wiley & Sons to offer me a contract for a book on cold fusion. I didn't know how the story would turn out, but it was certainly going to be a matter of some interest given the already huge media coverage. My first book was *The Quickening Universe: Cosmic Evolution and Human Destiny* (St. Martin's Press, 1987), which had been published soon after I arrived at the MIT News Office.

Nick Tate, then of *Boston Herald,* now with *Atlanta Journal Constitution*
(Photo courtesy of AJC)

One of the stipulations in the cold fusion book contract was that if *Nature* or *Science* magazine (or both) were to reach the general editorial conclusion that cold fusion was not real, the publisher could revoke the contract. As would transpire, that happened; the contract was revoked. In the spring of 1989 and beyond, the complex politics among the hot fusion program, the Department of Energy ERAB Cold Fusion Panel, the cold fusion camp, the media, and the mainstream science community led to widespread rejection of cold fusion as a Big Mistake—incompetence on the part of Pons and Fleischmann and others reporting positive results, or worse. "Possible fraud" and "scientific schlock" is how PFC Director Ronald Parker would characterize Pons and Fleischmann's work to *Boston Herald* environmental reporter Nick Tate in an interview in late April 1989, which surfaced on May 1. That May day in Baltimore, the absent electrochemists were viciously attacked at the meeting of the American Physical Society. The "F-word"—fraud—had been unleashed against cold fusion, thanks in no small way to the MIT PFC. *Boston Herald* Reporter Nick Tate would later write in a retrospective (June 8, 1991): "The MIT analysis debunked the Utah claims, and in an interview with the *Herald*, Parker—who wrote the report with Dr. Richard Petrasso—said the chemists misinterpreted their results. He also called it possibly fraudulent 'scientific schlock.' Some say those comments set the tone for the national criticism of the Utah work that followed."

But as we all know, the cold fusion story did not die. Positive results, as well as negative results in attempts to replicate the Pons and Fleischmann experiment, continued to be reported through 1989 and beyond. I was fascinated by the trend, not knowing how it would all come out. I was trying to be as objective as possible within the tumult. Certainly, I was encouraged by much of what I heard, but I was also discouraged by what my contacts at the MIT Plasma Fusion Center were saying. Some of them, such as Dr. Stan Luckhardt, told me that the tritium detection in cold fusion experiments at Los Alamos National Laboratory should be ignored because it had been done by "third-rate scientists." I assumed, provisionally, that these MIT experts knew what they were talking about. These were Dr. Edmund Storms and Dr. Carol Talcott—in retrospect definitely not "third rate." Despite *Nature* and *Science* magazines' negativity, eventually the sharp editor at John Wiley & Sons, David Sobel, perceived that it would be a good idea to reinstate the book contract, so I continued to follow the story. Even without the contract, I would have continued to be deeply immersed in the field. How could any serious person with a strong science background not be, so intriguing had become the physical evidence—and, in parallel, its public rejection. And several MIT professors remained very interested in it—not only Peter Hagelstein and Keith Johnson, but Prof. Louis Smullin, Prof. Lawrence Lidsky, Prof. Donald Sadoway (who filed a patent too!), and Prof. Philip Morrison.



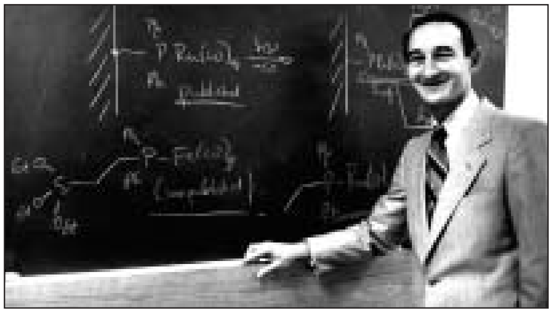


Julian Schwinger
Infinite Energy archives

In May 1991, *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor* came out. Its general conclusion was that the evidence for cold fusion was overwhelmingly compelling. In my view, for four or five years now, the basic evidence has been 100% confirmed; it is not merely compelling. Commercial opportunities abound for engineering power-generating reactors, even though the precise microphysical characterization of "cold fusion" remains contentious. In 1991, Julian Schwinger offered this promotional comment for the jacket of *Fire from Ice*: "Eugene Mallove has produced a sorely needed, accessible overview of the cold fusion muddle. By sweeping away stubbornly held preconceptions, he bares the truth implicit in a provocative variety of experiments." (See page 17 for further positive comments on *Fire from Ice* by Schwinger and other MIT-affiliated people.)

In 1991, I thought that both cold fusion and hot fusion could play a complementary role in the energy economy of the world—even though neither technology had been developed to the stage of commercial devices. I offered that opinion in *Fire from Ice*. But I was on dangerous ground. That was the last thing that the hot fusion people wanted to hear! They thought they had buried cold fusion about two years before. They had been fighting cold fusion in the press and in government from the outset.

Today, it is hot fusion that will be buried. Once the first commercial prototype reactors using cold fusion get widespread public acceptance—and they inevitably will—the white elephant of the tokamak hot fusion program is likely to be abruptly canceled by an outraged Congress. The U.S. DoE-Academia scandal against cold fusion demands a Congressional investigation if ever a matter of pressing scientific, technological, and legal importance did. Congress has already killed U.S. involvement in the $10-billion ITER (International Thermonuclear Experimental Reactor). The media, in general, are still largely ignoring scientific and commercial developments in cold fusion, but commercial-scale reactors will be impossible to deny—even for some heretofore obtuse science journalists who should have been continuing their coverage had they not been so strongly influenced by the likes of the negativists at MIT and elsewhere.



Prof. Mark S. Wrighton, then head of the MIT Chemistry Dept., signed the U.S. Dept. of Energy negative cold fusion report, which relied heavily on later contested results from the MIT PFC-Chemistry Department team.
Photo: MIT News Office

It Began at MIT

By the spring of 1991, most of the media and certainly the vast bulk of the scientific establishment had written off cold fusion. Fortunately for us all, they were—and are—all wrong. How did the scientific community and the media get the idea that cold fusion was bunk, "pathological science," and worse, when experiments continued worldwide? Substantial, increasingly refined experimental proofs were published—even in peer-reviewed journals, but the goal posts kept being moved by the opposition. Today, these goal posts are so distant, they are off the planet.

Prof. Mildred Dresselhaus
MIT News Office

In retrospect, I have concluded that much of the blame for the "cold fusion war"—and it certainly has been just that—stems from a vituperative campaign against the field with deep roots at MIT, specifically at the MIT Plasma Fusion Center. Not exclusively in that lab, however. Then chemistry Professor Mark S. Wrighton was also on the team that was investigating cold fusion. He later signed the infamous rush-to-judgement report against cold fusion by the U.S. Dept. of Energy (Prof. Mildred Dresselhaus of MIT also signed the negative DoE report but was much less involved, and as of 1999 is apparently "neutral" about cold fusion. One wonders about the propriety of her public silence). Wrighton became Provost of MIT in (1990) after Charles Vest (formerly of the University of Michigan) became President of MIT and picked him. Since 1995, Wrighton has been Chancellor at Washington University in St. Louis.)

In the spring of 1991, as I was finishing *Fire from Ice*, and feeling increasingly uncomfortable with what was happening at MIT with respect to cold fusion, I made a fateful discovery. Questions had already arisen about exactly how the MIT PFC-Chemistry Dept. team had analyzed their excess heat calorimetry study that compared a heavy water / palladium cell with an ordinary water / palladium cell. This was the so-called "Phase-II Calorimetry" study that had been published in the *Journal of Fusion Energy*. (Edited at the MIT Plasma Fusion Center—how's that for short-circuiting peer review!) From the pile of information that I had been collecting about the on-going work at MIT and elsewhere, I found two draft documents concerning this calorimetry that had been given to me by PFC team members during the rush toward publication. I could see immediately that there was a serious discrepancy between the unpublished, pre-processed raw data (the July 10, 1989 draft) and the final published data on the July 13, 1989 draft. (See page 11 graphs reproduced from these drafts). At first glance, it appeared that the data had been altered between July 10th and 13th to conform to what would be most welcome to the hot fusion people—a null result for excess heat in the heavy water data. I would later publicly challenge the creation and handling of these graphs by MIT PFC staff (see extensive *Exhibits J* through Z-11).

The Phase-II Calorimetry curves were later investigated in the outstanding analysis by my cold fusion colleague and fellow MIT graduate Dr. Mitchell R. Swartz. There can be no doubt now that these curves were the end result of a serious lapse in scientific standards in this affair that happened at MIT.

Our alma mater, which had played such a critical role in the development of radar in World War II, in the Apollo flights to the Moon, in deep space missions, in electronics, in biotechnology, in the chemical industry, in defense systems, and too many other fields to mention in one sentence, would acquire the reputation in the media as the "bastion of skepticism" against cold fusion. Tragically, MIT as an institution was not to fulfill the role it could have played in bringing cold fusion technology to the world. Quite the contrary, thanks to various false information coming from the hot fusion lab at MIT, the high-profile reputation of MIT was used to legitimize the view that cold fusion is bunk. It was said that the PFC calorimetry results disproved cold fusion—showed no excess heat. This is far from correct, as Dr. Swartz admirably showed. His analysis has been published in several venues.

From the vantage point of 1999, the role of the MIT Plasma Fusion Center / Chemistry Department team that investigated the cold fusion claims in 1989 grows clearer. It is a sad tale that cannot be fully addressed in a short space. Suffice it to say that early on, senior members of the PFC / Chemistry group, such as Dr. Richard Petrasso and Prof. Ronald Parker, took the view that the Utah claims were flawed, or worse, fraudulent. It went downhill from there. In

JOURNAL OF FUSION ENERGY

Journal of Fusion Energy. Vol. 9, No. 2, 1990

Measurement and Analysis of Neutron and Gamma-Ray Emission Rates, Other Fusion Products, and Power in Electrochemical Cells Having Pd Cathodes

David Albagli,[1] Ron Ballinger,[3,4] Vince Cammarata,[1] X. Chen,[2] Richard M. Crooks,[1] Catherine Fiore,[2] Marcel P. J. Gaudreau,[2] I. Hwang,[3,4] C. K. Li,[2] Paul Linsay,[2] Stanley C. Luckhardt,[2] Ronald R. Parker,[2,5] Richard D. Petrasso,[2] Martin O. Schloh,[1] Kevin W. Wenzel,[2] and Mark S. Wrighton[1,5]

Results of experiments intended to reproduce cold fusion phenomena originally reported by Fleischmann, Pons, and Hawkins are presented. These experiments were performed on a pair of matched electrochemical cells containing 0.1 x 9 cm Pd rods that were operated for 10 days. The cells were analyzed by the following means: (1) constant temperature calorimetry, (2) neutron counting and γ-ray spectroscopy, (3) mass spectral analysis of ^{4}He in effluent gases, and ^{4}He and ^{3}He within the Pd metal, (4) tritium analysis of the electrolyte solution, and (5) x-ray photoelectron spectroscopy of the Pd cathode surface. Within estimated levels of accuracy, no excess power output or any other evidence of fusion products was detected.

KEY WORDS: Fusion; cold fusion; palladium; excess heating.

The New York Times

NEW YORK, SUNDAY, MARCH 17, 1991

Cold-Fusion Claim Is Faulted On Ethics as Well as Science

By WILLIAM J. BROAD

THE WALL STREET JOURNAL WEDNESDAY, APRIL 15, 1992

TECHNOLOGY

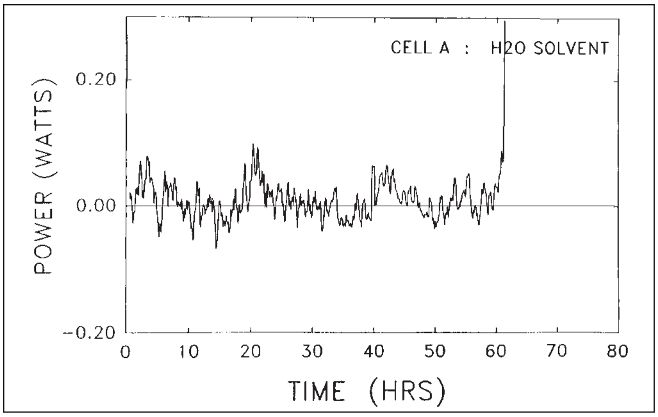
Physicist to Report Cold Fusion Findings From Japan at MIT's Bastion of Skeptics

By JACOB M. SCHLESINGER — soft-spoken, graying nuclear physicist says

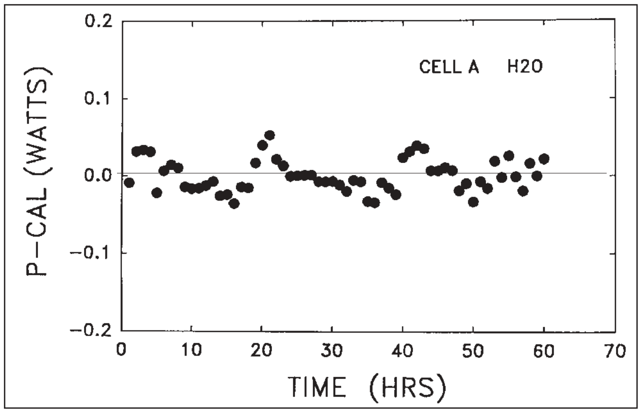
The two *pairs* of graphs (below), referring to the same experiment, are from *two* drafts (executed three days apart) of the MIT PFC Phase-II Calorimetry comparative study of a heavy water (D_2O) Fleischmann-Pons cold fusion cell and an ordinary water (H_2O) control cell. In the July 10, 1989 draft, there is clear evidence of excess heat (beyond electrical input power) in the D_2O cell, but no visually apparent excess in the H_2O cell. The data were averaged over-one-hour intervals to produce the July 13, 1989 draft, which shows no excess heat in the D_2O cell. There is now no doubt that to produce the July 13, 1989 draft, the D_2O data had to be treated differently than the H_2O data to give the final impression of a "null" result—no excess heat for D_2O. The results were published in this form in the *Journal of Fusion Energy* and a MIT PFC Tech Report, widely cited (especially by DoE) as evidence that the Fleischmann and Pons claim was false. In essence, the hour-averaged data were properly transformed from the intermediate processed form (July 10) for the H_2O control experiment, but the D_2O experiment curve in the July 13 draft appeared to be arbitrarily shifted down to make the apparent excess heat vanish. There is no justification for this curve shifting. The manipulation of the data between dates July 10 and 13 was more disturbing and unexplained, because the two sets were "asymmetrically" treated, as proved in the extensive analysis done by MIT graduate Dr. Mitchell R. Swartz.

Unpublished data

Published data



Excess Power Data. July 10, 1989 H_2O Unpublished.



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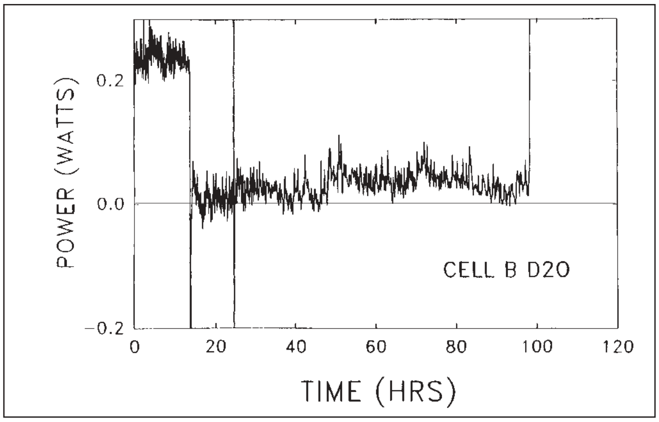
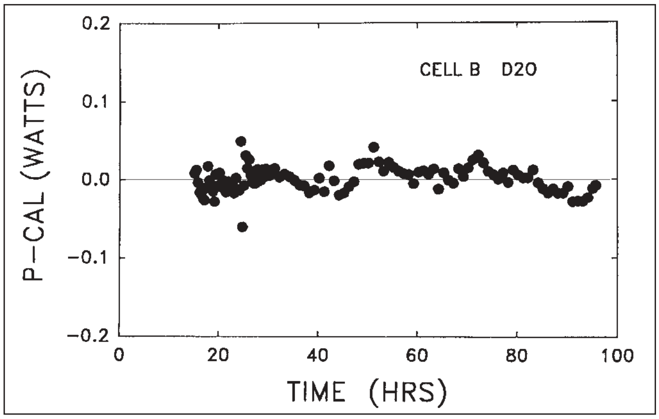


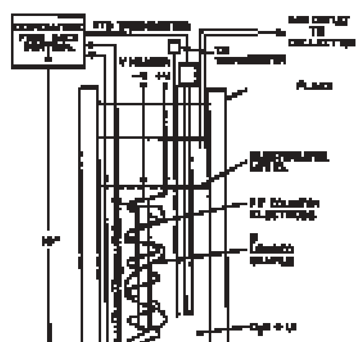
Excess Power Data. July 10, 1989 D_2O Unpublished.



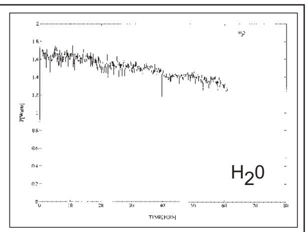
Excess Power Data. July 13, 1989 D_2O Published.

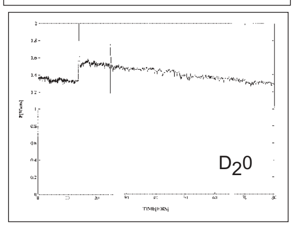
How the MIT PFC Experiment Worked—Or Didn't!

Accurate calorimetry of electrolytic cells is a difficult task, prone to many subtle errors, which crept into the 1989 MIT PFC Phase-II Calorimetry experiment. A schematic diagram of the experiment is at the right. A temperature sensor monitors the temperature of the water. Auxiliary heater power is automatically adjusted to maintain constant cell temperature, so the heater power is a measure of the energy released in the cell. Thus, if heat is generated within the cell, less heater power is required. However, water is lost from the cell during the experiment, reducing the ease with which heat is conducted to the environment, which also tends to reduce the heater power requirement. During the experiment, the input power shows a declining heater power trend from water loss. The graphs above have been compensated for this water-loss trend. "Compensation" is error prone, especially where the heat release (possible cold fusion power) may be steady. The MIT researchers *later* (after their report was challenged) said they expected a "sudden turn on" of excess heat. Dr. Swartz concludes that "The Phase -II methodology is flawed because it masks a constant [steady-state] excess heat." He also notes, ". . .the PFC data itself indicates that evaporation was a minor source of solvent loss...most solvent loss occurred by electrolysis. Such solvent loss would be greater for the H_2O solution...such electrolysis is used commercially to isolate heavy water...putative differential excess solvent loss for heavy water is not a reasonable explanation for the asymmetric algorithm used to shift the 7/10/1989 D_2O curve." On June 7, 1991, Prof. Ronald R. Parker publicly stated that data from the MIT PFC was "worthless," yet it had been published in a fusion journal edited at MIT. Later in 1991, he said that he stood by the negative con-



Schematic of PFC Experiment
From Draft MIT PFC Report

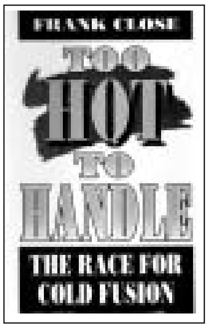




Heater power decline.
MIT PFC Raw Data

William Broad's 1991 front-page news story in the March 17, 1991 Sunday *New York Times*, senior PFC physicist Richard Petrasso revealed his original views about Pons and Fleischmann: "I was convinced for a while it was absolute fraud. Now I've softened. They probably believed in what they were doing. But how they represented it was a clear violation of how science should be done." This is final proof, as though more were needed, that the scientific experiments to investigate cold fusion were inappropriately biased from the outset.

Petrasso's comments came within Broad's article, bannered with "Cold-Fusion Claim is Faulted On Ethics as Well as Science." The article amounted to a virtual promotional book review of UK physicist Frank Close's book, *Too Hot to Handle*, which came out at about the time of *Fire from Ice*. *The New York Times* also reviewed Close's book in its Book Review section. Curiously, *Fire from Ice* was never reviewed by the *Times*. Frank Close, who worked closely with Petrasso *et al.* in assaulting Pons and Fleischmann, falsely accused them of having fudged gamma-ray spectroscopy data. The bizarre truth is that even had Pons and Fleischmann faked gamma ray data—they most certainly had not—their all-important nuclear-scale excess power results, the key signature of cold fusion, has withstood the test of time. Cold fusion is now being developed commercially. To their credit, Fleischmann and Pons were not comfortable with the preliminary nature of their neutron/gamma-ray data and have long since withdrawn those data. Others subsequently confirmed much lower levels of neutron emission. On the other hand, the use of the strawman of gamma-ray curves by Petrasso *et al.* at the MIT PFC is all the more reprehensible when the history of *real* data fudging in cold fusion is examined—the data "processing" (*i.e.* improper manipulation) of calorimetry curves from electrochemical experiments performed at the MIT PFC in the spring of 1989.



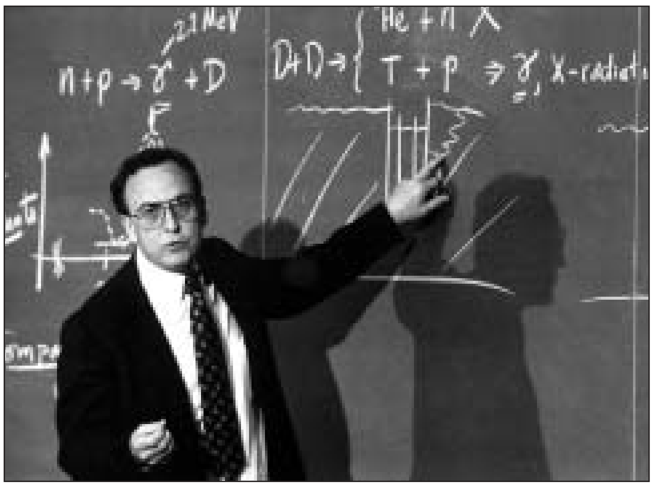
The energy and environmental future of the world hung in the balance—and the MIT PFC people failed us. They preferred to get rid of a scientific claim in which they did not believe, and which threatened their federally-funded program, by playing politics with the media, trivializing their experiments, and ultimately foisting on the world highly flawed data—some would say fraudulently represented data—from a calorimetry experiment ostensibly performed to determine scientific truth. . .

Let us not forget, these were serious experiments, funded by the U.S. Department of Energy under Federal contract. The authorization to investigate came from U.S. President George Bush through Energy Secretary Admiral Watkins. (As a general matter, people who file false reports to Federal agencies are subject to criminal sanctions if this work is brought to the attention of appropriate investigative authorities before the statutes of limitation expire.) This calorimetry issue was not a small matter. In the spring of 1989 it was absolutely critical to determine whether there was anything to the Pons and Fleischmann claims. The energy and environmental future of the world hung in the balance—and the MIT PFC people failed us. They preferred to get rid of a scientific claim in which they did not believe, and which threatened their federally-funded program, by playing politics with the media, trivializing their experiments, and ultimately foisting on the world highly flawed data—some would say fraudulently represented data—from a calorimetry experiment ostensibly performed to determine scientific truth.

To understand how the curves that I and later Dr. Swartz analyzed in his report came about, one should have some background. In late April 1989, Professor Parker and Professor Ronald Ballinger, both members of the PFC team then investigating the claims of Pons and Fleischmann, held a covert interview with *Herald* reporter Nick Tate to plant a very negative story against the Utah work. No one at the MIT News Office was told of this interview until late on the night before the story was to appear in banner headlines in the *Boston Herald*. As Parker told Tate (a tape released by the *Herald* confirms this—see *Exhibit B*), Parker and Ballinger *et al.* were opposed to the "cheer-leading" for cold fusion by the *Boston Globe*. They wanted to give Tate an exclusive story about some nuclear physics evidence that they said they had developed, which they claimed would prove the Fleischmann-Pons experiments to be highly flawed. This evidence concerned the gamma-ray spectra coming from attempts to measure neutrons impinging on a water bath near the Pons-Fleischmann cells.

Historically, it is evident that this *Herald* story helped unleash the tidal wave of negativity against Fleischmann and Pons and others who continue to work in the field. Ironically, Parker *et al.* accomplished what they really set out to do with that story, but at the time Parker attacked reporter Tate for *allegedly* mis-reporting what he had said during his interview. Tate came very close to being fired on the spot by his editor; *he would have been fired* had he not had an audio tape of the interview to confirm what he had been told by Parker. After all, it was an MIT professor's word against that of a young reporter.

A frantic Ronald Parker, perhaps fearing that he would be sued by Pons and Fleischmann for the harsh words that were quoted a bit too explicitly for his taste, called me late on the night of April 30, 1989. He had me dispatch a press release to the wire services denying the impending *Boston Herald* story, the exact nature of which he had learned from a call from CBS television. Of course, I had at that time no reason to doubt what he was telling



Prof. Ronald R. Parker, at hastily called MIT press conference on May 1, 1989, explains what the MIT PFC team believed to be the errors in the Fleischmann-Pons neutron measurements. He also denied using the harsh language against Fleischmann and Pons quoted in the *Boston Herald* story that day. *Photo: MIT News Office* me: that the story was a distortion. I would learn the stark truth about this deception only over a year later when Tate allowed me to listen to the actual tape. There can be no denying what Parker told Tate about Fleischmann and Pons. In one key passage in the interview, Parker says this: "So, what are you going to do with this, uh, Nick? You know this is. . .what you are hearing is that we think it's a scam, right?" Tate responded: "Why is it today that you think it's a scam?" Parker's reply: "We have been studying the evidence together very slowly and we want to have a paper out on this before we actually blast them. Monday (May 1, 1989) we're putting a paper out on it. . ." In addition to this, the actual word "fraud," was used by Parker no less than five times on the audio tape—as he discussed Pons and Fleischmann's work. This tape is a key "smoking gun" of the entire cold fusion controversy. The treachery and conniving by Parker and Ballinger are there for all to see—disgraceful!

MIT

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URGENT MEDIA ADVISORY

For Immediate Release May 1, 1989
MIT Contact: Eugene F. Mallove, Sc.D.
Chief Science Writer
617-253-2701

CAMBRIDGE, Mass., May 1-- Professor Ronald R. Parker, Director of the MIT Plasma Fusion Center responded today to an article published this morning in the Boston Herald, an article that he says has seriously misquoted him and given a largely incorrect view of his discussions with the Boston Herald's reporter, Nick Tate.

Professor Parker issued this statement:

"The article erroneously characterizes remarks that I made regarding the cold fusion experiments done at the University of Utah. Specifically, I did not: (1) Deride the University of Utah experiments as "scientific schlock" or (2) Accuse Drs. Fleischmann and Pons of 'misrepresentation and maybe fraud'."

Today, Professor Parker's colleagues will present a paper (co-authored with him) at the meeting of the American Physical Society in Baltimore, Maryland, in which they suggest that data that Drs. Pons and Fleischmann claim support the observation of neutron emission in their experiments were misinterpreted by Pons and Fleischmann.

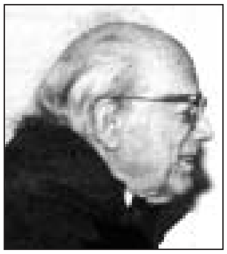
Based on their independent analysis, the MIT researchers say that if neutron emission occurred in the Pons and Fleischmann experiment that they reported in the *Journal of Electroanalytical Chemistry*, it would have been at a level far below that reported by the University of Utah group.

By June of 1989, the hot fusion community and the physics establishment were very satisfied that they had debunked cold fusion. Any of the growing numbers of positive reports could readily be dismissed by reporters and other, less involved scientists. After all, the plasma physicist authorities at MIT had spoken. In fact, so convinced were the PFC people that they had killed off cold fusion, they held a celebratory party—billed as a "Wake for Cold Fusion" on June 26, 1989. The humorous poster for the party notes: "Brought to you by the Center for Contrived Fantasies—Black Armbands Optional."

What is most interesting about this anything-but-funny mockery by the PFC is that at the time the party was held, the data for the Phase-II calorimetry experiments had not yet been analyzed! It was not until mid-July 1989 that the calorimetry data were put in anything like final published form. No formal conclusion had been set into print. How do we know this? Simple. In the course of my investigations into cold fusion, I would of course regularly ask PFC team members for their latest impressions, data, etc. So I was given many, many documents that piled up on my desk, not all being closely examined when received. But as I was completing *Fire from Ice* in the spring of 1991, questions about the PFC calorimetry had been brought up by my cold fusion colleague, electrochemist Dr. Vesco Noninski. Was the methodology and analysis of the PFC Phase-II calorimetry reported in the paper published by the PFC in the *Journal of Fusion Energy* sound? Noninski had many doubts and so did I. We approached a team member for clarification and got no satisfaction—just continued brush-off. I then looked through my stacks of papers from the PFC and found to my complete astonishment (and dismay) the two draft reports on the Phase-II calorimetry. One was dated July 10, 1989 and the other July 13, 1989, a clearly more complete version—the version that was actually published in both a formal PFC report and the *Journal of Fusion Energy*. Only a week after this MIT PFC analysis solidified, PFC Director Parker occupied himself dispensing "humorous" cold fusion mugs that were obtained "wholesale" in Utah (*see Exhibit F*)!


Electrochemist Dr. Vesco C. Noninski questioned MIT PFC's analysis of its calorimetry data. *Photo by E.Mallove*

On June 7, 1991 I resigned from the MIT News Office, to protest the outrageous behavior of the PFC and others at MIT against cold fusion. Among other disgraceful happenings, an article of mine on cold fusion that had been approved for publication by the then editor of MIT *Technology Review*, was canceled after being trashed by MIT Physics Department Professor Herman Feshbach. Feshbach told me over the phone when I inquired, "I have fifty years of experience in nuclear physics and I know what is possible and what is impossible." He also told me that he did not want to see any more evidence for cold fusion, which I offered to show him, because, "It's all junk!"


MIT Prof. Herman Feshbach *MIT News Office*

Hours before my formal resignation, the PFC was having another of its "celebrations" for the death of cold fusion. Dr. Frank Close was speaking at a seminar there, billed "An Exposé of Cold Fusion," in which he lashed at Pons and Fleischmann for their alleged fudging of gamma ray curves. He had nothing of significance to say about the P&F calorimetry, consistent with this appalling high-energy physicist mind-set that "knew everything that could and could not happen" among nuclei. After Close was finished, Dr. Petrasso as master of ceremonies, very reluctantly gave me some time to comment. ("Just *one* minute, Gene!") I showed the July 10-July 13 curve shifting with overhead transparencies and suggested sarcastically to Close that he should consider covering this important documentary finding in the next edition of his book (Heaven forfend that there should be another!). It was as though I were talking to a wall. This was


Dr. Frank Close
Princeton University Press


Dr. Richard Petrasso
Photo by E. Mallove

not deemed important. After all, hadn't he just "proved" that cold fusion was dead?

PFC Director Parker then stated that this was the first time he had seen the data I had flashed on the screen—it probably was. Then Parker made the astounding assertion that "you can put those curves anywhere you wish." He publicly stated that the data from the MIT PFC was "worthless." (See *Exhibit K*). Many weeks later, after I had revealed the PFC story to the world, Parker reverted to *defending* the conclusions of the calorimetry data—in an informal press release put out by the MIT News Office (see *Exhibit T*). It must take many years of training to maintain such mutually contradictory opinions with a straight face—on national television and in written documents.

Let me be clear: There was likely no grand "conspiracy" to suppress a positive finding for excess heat in the MIT PFC-Phase-II calorimetry, it's just that the mind-set of the MIT hot fusioneers and Chemistry Department people allowed lower echelon persons to monkey with the data. He or she could not possibly bring anything to his superiors—Ronald Parker and then MIT Chemistry Dept. Head Mark Wrighton—that looked remotely positive for excess heat. This would have opened up the cold fusion story again in the summer of 1989, this time with MIT coming in with some encouraging news. So, the data was "fudged." I can think of another F-word—beyond "fudging"—that applies. It is closer to the truth. Ronald Parker likes to bandy it about in interviews with newspaper reporters. This groundless, manipulated and fabricated data has subsequently been cited over and over again by the U.S. Patent Office to reject cold fusion patent applications. It was even used, in part, ultimately to kill the Pons and Fleischmann patent itself, which happened in the Fall of 1997. Other MIT-trained cold fusion inventors have also had their patent applications attacked with this unscientific travesty from MIT.

Perhaps the most remarkable aspect of the MIT PFC experiment was that after I publicly challenged it, the objective of the experiment was *redefined* by its defenders! Thus, it is quite literally true that the experiment published in the *Journal of Fusion Energy* and the MIT PFC technical report is *by definition* fraudulent—*if only because the ground rules for comparing the heavy water and ordinary water experimental outputs were subsequently changed and are not as stated in the article.* These ground rules went from the obvious implication that can be taken from the lack of difference between the published curves to the statement that the MIT PFC team were looking for "fast turn on" of 79 mW excess heat and didn't find it! See NIH physicist Dr. Charles McCutchen's letters to the MIT Administration about this key point—*Exhibits Z-4, Z-8,* and *Z-11*. Dr. Mitchell Swartz has produced a remarkable, clear analysis of the data produced by the MIT PFC—including all of the various inconsistent versions of the data and their interpretation). The work speaks for itself. Interested readers may request the original color-graphic paper which is included in a paperback book from JET Technology, P.O. Box 81135, Wellesley Hills, MA 02481.

Dr. Mitchell R. Swartz of JET Technology, Inc. lectured on cold fusion calorimetry, January 20, 1996 at Cambridge Marriott Hotel. Meeting sponsored by *Infinite Energy* Magazine.

Photo by E. Mallove

• Swartz, Dr. Mitchell R., "Re-Examination of a Key Cold Fusion Experiment: 'Phase-II' Calorimetry by the MIT Plasma Fusion Center," *Fusion Facts*, August 1992, pp. 27-40.

• Swartz, Dr. Mitchell R., "A Method to Improve Algorithms Used to Detect Steady State Excess Enthalpy,"*Proceedings: Fourth International Conference on Cold Fusion* (December 6-9, 1993, Lahaina, Maui, Hawaii), and in Transactions of *Fusion Technology*, Vol.26, December 1994, pp. 369-372.

• Swartz, Dr. Mitchell R., "Some Lessons from Optical Examination of the PFC Phase-II Calorimetric Curves, *Proceedings: Fourth International Conference on Cold Fusion* (December 6-9, 1993, Lahaina, Maui, Hawaii).

The Sham MIT "Inquiry"

I am very glad that Dr. Swartz undertook the task of this essential analysis, because certainly he was more capable than I in this kind of detailed examination of points that appeared and disappeared in various versions put out by the PFC. He did it himself after I turned over to him the materials that I had discovered. I was so revolted by the handling of this matter by the MIT Administration, that I really could not stand to wallow in the falsehoods coming out of the MIT PFC. My feeling was: "Let them stew in their own self-created problems. The world will eventually understand what they did." It will.

After my formal complaint to MIT President Charles Vest in August 1991 (see *Exhibit R*), in which I asked for an appropriate investigation of scientific misconduct in the data handling and in the planting of a false press story by Parker in 1989, the whole matter was, in effect, swept under the rug by Vest after an utterly insufficient examination of the technical issue by Professor Professor Philip Morrison, who was a friend of MIT PFC report co-author, Dr. Petrasso.

Morrison's down-playing of the issues involved was a great disappointment, but not surprising for someone who to this day does not comprehend the significance of the research results in the cold fusion field. A symptom of this: To my knowledge, Prof. Morrison—at least as of early 1999—has never reviewed in his wide ranging columns *any* cold fusion books—either positive or negative. In one of his notes to President Vest (*Exhibit V*), Morrison stated that cold fusion findings "would at most open some way to build a new battery, possibly a fuel cell." This kind of ill-informed remark should be beneath the author of *The Ring of Truth!*

Concerning the ethical issues of Parker's dealings with the press and the MIT News Office, President Vest stated that his legal counsel advised him no action was necessary. It was a shameful, sham "inquiry," not a thorough investigation, as the subsequent portion of this report and the various Exhibits show. I complained vigorously to President Vest that the inquiry was totally inadequate. In fact, the people who should should have been under investigation were allowed to continue

MIT President Charles M. Vest, who continues to ignore cold fusion research. He excused the unethical behavior of MIT PFC staff against cold fusion. He is on a Federal panel that has advised the Clinton Administration to increase funding for hot fusion—a benefit for the MIT PFC (see Exhibits R through Z-11).

MIT Photo by Edward McCluney

to handle the data and write a subsequent "Technical Appendix" that made further excuses for data mishandling. As Dr. Swartz has shown, the data was, indeed, altered yet again during the "investigation"! For now, I hope that Dr. Swartz's analysis, and my own assessments and exchanges with President Vest, will be examined carefully by all who still have an open mind about the historical development of the cold fusion controversy.

My conclusions about the inappropriate data manipulation at the MIT PFC are my own and my *opinions about the implications of this data mishandling are to be considered distinct from Dr. Swartz's.* My assessments of the MIT calorimetry and data handling appear in my Letter of Resignation (*Exhibit L*), my formal request for an investigation of scientific misconduct (*Exhibit R*), and other exchanges with President Vest that form the exhibits to this report. But let me quote Dr. Swartz's summary conclusions from his fourteen-page technical paper:

> From: Dr. Mitchell R. Swartz's, "Re-Examination of a Key Cold Fusion Experiment: 'Phase-II' Calorimetry by the MIT Plasma Fusion Center," *Fusion Facts*, August 1992, pp. 27-40.
>
> The light water curve was published by the PFC essentially intact after the first baseline shift, whereas the heavy water curve was shifted a second time. The cells were matched,[12] and solvent loss would be expected to be greater for H_2O.
>
> The Phase-II methodology is flawed because it masks a constant [steady-state] excess heat. Furthermore this paradigm fails to use either the true baseline drift, and may avoid the first 15% of the D_2O curve in Types 3, 3_B, 4, and 5 curves.
>
> What constitutes "data reduction" is sometimes but not always open to scientific debate. The application of a low pass filter to an electrical signal or the cutting in half of a hologram properly constitute "data reduction," but the asymmetric shifting of one curve of a paired set is probably not. The removal of the entire steady state signal is also not classical "data reduction."
>
> In the May 1992 Appendix, the PFC retroactively claims its "systematic errors now total 100 to 400 milliwatts, implying an insensitivity of >30 kilojoules.
>
> Much current skepticism of the cold fusion phenomenon was created by the PFC paper's reporting "failure-to-reproduce."[12] as opposed to its later claimed "to insensitive-to-confirm" experiments[17]]. Because it may be the single most widely quoted work used by critics of cold fusion to dismiss the phenomenon, the paper should have clarified all "data" points and the methodology used. Apparent curve proliferation, volatile points, asymmetric curve shifts, combined with an impaired methodology have needlessly degraded the sensitivity, and believability of the Phase II calorimetry experiment.
>
> **12.** D. Albagli, R. Ballinger, V. Cammarata, X. Chen, R.M. Crooks, C. Fiore, M.P.J. Gaudreau, I. Hwang, C.K. Li, P. Linsay, S.C. Luckhardt, R.R. Parker, R.D. Petrasso, M.O. Schloh, K.W. Wensel, M.S. Wrighton, "Measurement and Analysis of Neutron and Gamma-Ray Emission Rates, other Fusion Products, and the Power in Electrochemical Cells Having Pd Cathodes," *Journal of Fusion Energy*, **9**, 133, 1990.
> **17.** S.C. Luckhardt, "Technical Appendix to D. Albagli *et al., J. Fusion Energy*, 1990, Calorimetry Error Analysis," MIT Report PFC/RR-92-7, (May 1992).

Present MIT students as well as alumni should investigate this most unfortunate episode for themselves, and take action—for the well-being of MIT. There is no doubt in my mind that the MIT PFC calorimetry was mishandled and fraudulently misrepresented. Dr. Swartz's paper, using proper analysis that could have been performed by the MIT PFC, determined that "the average power by this method is 62 milliwatts (±34 milliwatts)." As Dr. Swartz states, this is "qualitatively similar to the value expected for a 'successful' experiment." Furthermore, Dr. Swartz credits in his references and conclusions my August 1991 complaint to President Vest (see *Exhibit R*) that a "20% discrepancy in heater power, used to heat *the same volume of fluid*, has been suggested as corroborating evidence that the heavy water cell produced excess heat."

At the very least it was scientifically and morally required that the MIT PFC group repeat its experiments, rather than having them cited year after year against cold fusion, when they should have been retracted or corrected, per the suggestion of physicist Dr. Charles McCutchen—see *Exhibit Z-11*. To cover up a sorry episode may have been comfortable for the MIT administration in an era in which cold fusion had not yet achieved general acceptance (thanks in no small way to some on the MIT staff), but that era will pass. An age of enlightenment is coming that will make the tokamak hot fusion program at MIT a footnote to history. The era of safe, clean, and abundant energy from water—non-chemical energy from hydrogen—will drown the deceivers from MIT to Princeton. (If anyone has any doubt about this emerging commercial reality, they should consult one of the energy-from-water corporations that was influenced by the announcement of Fleischmann and Pons—see BlackLight Power Corp. [www.blacklightpower.com]. No doubt many bright MIT graduates will be employed there.) No one can say that we did not warn MIT officials of the consequences if this important matter was allowed to be mishandled at MIT the way it was and continues to be.

Other Issues

The preceding is the basic story of what went on at MIT in 1989-1992. Much of this could have been avoided if President Vest had had an open-door policy toward appropriate scientific dissent. On April 12, 1991, I had sent a letter to President Vest (see *Exhibit I*), at a time when I was feeling optimistic about what could be accomplished. I had hoped that the new MIT President, who had replaced the outgoing Dr. Paul E. Gray, would take action on its important message. I recommended that a study group be formed to re-examine what had been learned about cold fusion since 1989. Should I have been surprised at not receiving a response? Not when President Vest had chosen Chemistry Department head Mark Wrighton, to be Provost. Examine Wrighton's brusk and totally inappropriate response to Dr. Noninski (*Exhibit H*). Wrighton's "let me make this perfectly clear I have no comment" letter is not a response that a scientist with integrity would have written.

After the events of 1991-1992, there followed many hard years of struggle, working with other engineers and scientists in cold fusion research, and trying to correct false impressions about cold fusion investigations that were being made by journalists and government officials. The launching of *Infinite Energy* magazine in 1995 (and its short-lived precursor, *Cold Fusion* magazine, 1994) was, in part, a response to the egregious distortions about cold fusion that were initiated by members of the MIT PFC.

Fire from Ice was well received by many reviewers, but its message was largely drowned out by an onslaught of scurrilous anti-cold fusion books, the first one by Frank Close, *Too Hot to Handle* (1991). Dr. Richard Petrasso of the MIT PFC had aided Close's work. He was in complete agreement with Close's opinions; witness his comment published on the front page of the Sunday *New York Times*, March 17, 1991, which was essentially a laudatory review of the book by Close. Recall Dr. Petrasso's words: "I was convinced for a while it was absolute fraud. Now I've softened. They [Pons and Fleischmann] probably believed in what they were doing. But how they represented it was a dear violation of how science should be done." A case of the pot calling the kettle black, I'd say, in light of the technical publication to which Petrasso (and fifteen others) has signed his name. Nothing much has changed for Dr. Petrasso. In 1997 he was quoted by writer Bennett Daviss: "The ongoing reports of excess heat and nuclear by-products catch people's attention about as much as the occasional UFO report. I have better things to do with my time." (In *TWA Ambassador* article, September 1997, see reprint in IE No. 17.) He and Professor Parker continue to spend *your* money on hot fusion.

There were other negative books, one by DoE's John Huizenga (*Cold Fusion: The Scientific Fiasco of the Century*, 1992), and another by science journalist Gary Taubes (*Bad Science: The Short Life and Weird Times of Cold Fusion*, 1993). Taubes became a Knight Science Journalism Fellow at MIT for a year, a nominal honor for him, if not a disgrace for MIT. The MIT News Office, to my knowledge, never published one word about the existence of *Fire from Ice*, nor the fact that *Fire from Ice* was nominated in 1991 for the Pulitzer Prize by John Wiley & Sons as one of only two of its books so nominated that year. Professors at MIT routinely bombard the News Office with requests that their every major or minor award be acknowledged in *Tech Talk*. Virtually all such requests are granted.

So goes PR at MIT—ever protective of the MIT Administration and its deficiencies—whether in flaps over an MIT student being killed by an alcohol overdose at an MIT fraternity after warnings were ignored by President Vest (See *Boston Globe*, October 1, 1997, p.1 "Students Warned MIT on Drinking—Complaints Began in 1992), or the very serious matter of data fudging and misrepresentation by MIT hot fusion scientists. The MIT Administration clearly was not happy by the spate of publicity that my resignation from the News Office generated. It acted accordingly.

The MIT PFC continues to receive Federal funding for its lucrative hot fusion projects—over $250 million since 1989. One of the ways that MIT helps to insure the continued flow of such funding is by having President Vest sit on the various Federal panels that make recommendations to the Administration and the the Department of Energy. Now that former Physics Dept. Head Professor Ernest Moniz is a Deputy U.S. Secretary of Energy, MIT's ability to bring influence to bear for hot fusion will be even stronger.

In a 1995 issue of the *Journal of Fusion Energy* we find "The U.S. Program of Fusion Energy Research and Development: Report of the Fusion Review Panel of the President's Council of Advisor's on Science and Technology (PCAST)," (Vol. 14, No. 2, 1995, pp. 213-250). One of the nine co-authors is none other than Charles M. Vest. The report's summary states, in part: "Funding for fusion energy R&D by the Federal government is an important investment in the development of an attractive and possibly essential new energy source for this country and the world in the middle of the next century and beyond. . .The private sector can not and will not bear much of the funding burden for fusion at this time because the development costs are too high and the potential economic returns too distant. But funding fusion is a bargain for society as a whole." That's their opinion, not ours. This is not even the opinion about hot fusion of many technologists who have nothing to do with cold fusion.

The report states, ". . .we believe there is a strong case for the funding levels for fusion currently proposed by the U.S. Department of Energy (DoE)—increasing from $366 million in FY1996 to about $860 million in FY2002 and averaging $645 million between FY1995 and FY2005." It goes on to acknowledge that "Although the program just described is reasonable and desirable, it does not appear to be realistic in the current climate of budgetary constraints. . ." So the report asks for less, in the tradition of grabbing for whatever the bureaucracy thinks it can get: ". . .to preserve what we believe to be the most indispensable elements of the U.S. fusion effort and associated international collaboration." The panel recommended about $320 million/year and continues further fantasy thinking about committing Federal $billions in continued support for ITER (International Thermonuclear Experimental Reactor). Fortunately, the U.S. Congress withdrew support from ITER in late 1998.

And, wonder of wonders, the 1995 report speaks directly about the need to continue to support MIT's Alcator C-Mod tokamak reactor. I suppose that in the general run of how Federal funding of science is promoted by numerous interest groups, this apparent conflict of interest—an MIT President recommending that MIT receive further funding for its hot fusion reactor—is not unusual. However, such advising by MIT's Vest is very unseemly, when seen in the context of the cut-off of all DoE funding for cold fusion, resulting from the 1989 negative report and from the MIT PFC experiment on which that report was based. Furthermore, as this history has made clear, President Vest played no small role in the whitewashing of this 1989 misconduct.

In another report, this one directly to President Clinton on November 4, 1997, "Report to the President on Federal Energy Research and Development for the Challenges of the Twenty-First Century," the Energy Research and Development Panel of PCAST, which includes Dr. Vest, we find the general recommendation spelled out in the cover letter to President Clinton: "The report recommends an increase, over a five-year period, of $1 billion in the Department of Energy's annual budget for applied energy technology R&D. The largest share of such an increase would go to R&D in energy efficiency and renewable energy technologies, but nuclear fusion and fission would also receive increases. The composition of the R&D supported on advanced fossil-fuel technologies would change in favor of long-term opportunities, including fuel cells and carbon sequestration technologies, but the overall spending level for fossil fuel technologies would stay roughly constant in real terms." In table "ES.1" we find the fusion wish list after 1998 in "millions of as-spent dollars": 1997—$232 (actual); 1998—$225 (request); 1999—$250; 2000—$270; 2001—$290; 2002—$320; 2003—$328.

The report states that the request for fusion is the "third largest increase" of the various energy items. It calls the funding ". . .easily justified as the sort of investment government should be making in a high-risk but potentially very-high yield energy option for society, in which the size and time horizon of the program essentially rule out private funding." Well, virtually all of the scientists working in cold fusion in 1999 think that cold fusion is, indeed, "a very-high yield energy option" for society. Private industry has invested in it in a limited way, and more will come. If it were not for the Federally paid scientists—in hot fusion and high energy physics—who assaulted cold fusion with lies and deceptions—there would likely be even more private money now flowing into cold fusion. One thing is certain: no private company in its right mind will spend any significant money on tokamak hot fusion, as practiced at MIT and elsewhere.

What it boils down to is this: By studying the history MIT and cold fusion, one learns that paradigm-paralyzed and unethical scientists have the motive and means to wreck massive damage against an emerging science and technology, especially when an aging and well-financed program is threatened. An MIT President who has access to the highest power levels of the Federal government should not be contributing to the distortion of government spending by feathering MIT's nest and ignoring facts. MIT alumni/ae, students, staff, and President Charles M. Vest need to consider this—E. Mallove

Letter by Julian Schwinger

Re: Eugene Mallove's Fire from Ice

Letter of February 5, 1991 from physics Nobel Laureate Julian Schwinger (Nobel Prize for physics in 1965, shared with Sin-Itiro Tomanaga and Richard P. Feynman "for their fundamental work in quantum electrodynamics, with deep-ploughing consequences for the physics of elementary particles"). This handwritten letter was sent to John Wiley & Sons, Inc., concerning the manuscript of Eugene Mallove's book, *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor*, which would soon be published in May 1991. [Note: Italics and square brackets have been added by E. Mallove.]

Dear Judith McCarthy [John Wiley & Sons]:

Thank you very much for sending me Mallove's typescript. For almost two years, I have been muttering: "Someone has to write a book about this!" "This"is the bizarre story of cold fusion—its bizarre science, and its bizarre human behavior. The author of that book would need some familiarity with the relevant physics (atomic and nuclear), chemistry (electrolysis, at least), and should have had first-hand experience of some of the events and their participants. But, most of all, he must have a balanced view that incorporates an understanding of what the "scientific method" really means.

I have just finished reading every word of 470 pages of typescript. (In modest proof thereof, I offer two 'Typos. . .etc.) *I enjoyed it very much. Eugene Mallove, in my book, is the right one to write about "the truth behind cold fusion."*

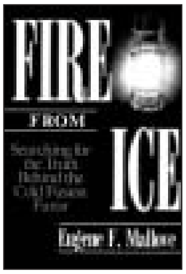
I have enclosed two recent articles of mine, one delivered the day before December 7 [1990], in Tokyo, the other a short supplement that has been submitted to a Japanese journal. Please send them on to E.M. (beyond MIT, I am unaware of his address) for his possible interest and, at least, amusement.

I should also like to add, vis-a-vis his recognition of the absurdity of the Editorial note on p. 435, that its promise—"duty to give him the opportunity to explain his ideas and present his case. . ." was a lie. Only the short introductory note, Part 1, was published. When Part 2 and the much more substantive Part B were submitted, they received the usual vituperative reviews and were rejected; they have never been published.

Incidentally, the other paper of mine cited on p. 551, *Cold Fusion: A Hypothesis*, which was published after more than a year's delay, went first to PRL [*Physical Review Letters*]. Although I anticipated rejection, I was staggered by the heights (depths?) to which the calumny reached. My only recourse was to resign from the American Physical Society, (APS).

Dr. Petrasso, Prof. Hagelstein, and Prof. Fleischmann at First International Conference on Cold Fusion, 1990, Salt Lake City.

Photo by E. Mallove



You ask for ". . .a few words. . ." Perhaps they can be found above. If not, how about: *Eugene Mallove has produced a sorely needed, accessible overview of the cold fusion muddle. By sweeping away stubbornly held preconceptions, he bares the truth implicit in a provocative variety of experiments.*

Yours, Julian Schwinger

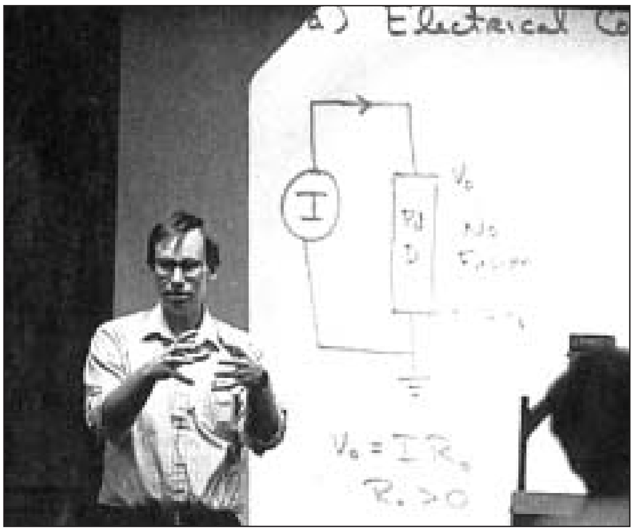
P.S. I am grateful for E.M. for quoting A.C.D. [Arthur Conan Doyle] on p. 216. I have long been conscious of that bit of sherlock Holmes wisdom, but could not recall the particular story in which it appears. J.S.

Other Comments on Fire from Ice

by MIT-Affiliated People (from the book jacket)

"Mallove brings dramatically to life the human side of this important scientific controversy, which has tapped the emotions of its scientific participants in a way usually typical only of major scientific revolutions. *Fire from Ice* is highly recommended reading for anyone who is interested in the nature of scientific controversy and scientific change. I frankly could not put the book down once I had started it."
—Dr. Frank Sulloway, former MacArthur Fellow, science historian, MIT Program in Science., Technology, and Society

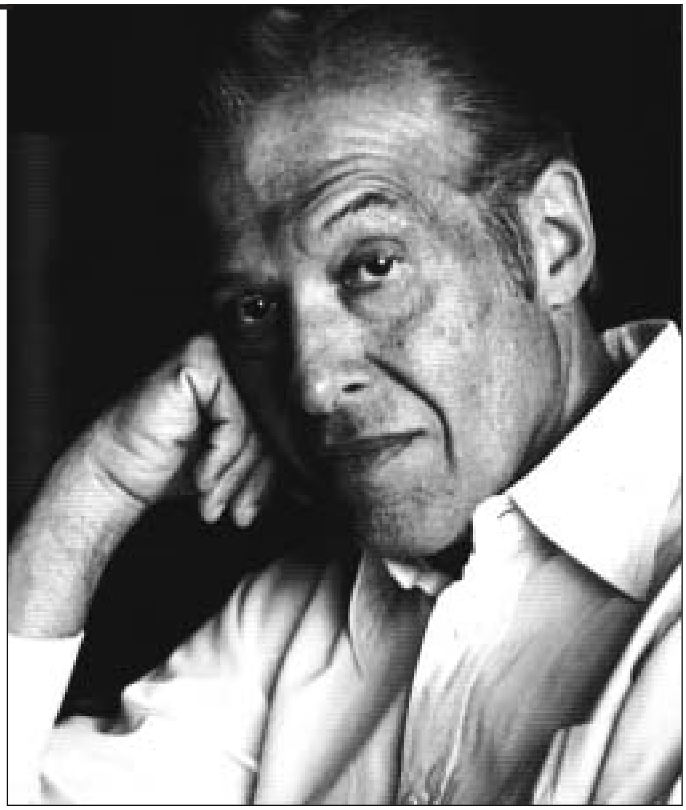
"*Fire from Ice* is a masterpiece of scientific documentation. Progress in deciphering the cold fusion effect is now stalemated by an establishment pressure for conformity. An authoritative book needed to be written, and it had to come from someone with roots in both the science and journalism communities; there are very few people in the world as qualified as Eugene Mallove is to write it and give the story the meticulous attention it required."
—Dr. Henry Kolm, co-founder of MIT's Francis Bitter National Magnet Laboratory



Prof. Peter Hagelstein lecturing on cold fusion theory at MIT April,1989.

MIT Photo

A lecture by cold fusion theorist Nobel Laureate Julian Schwinger, November 11, 1991, at MIT celebrating the 60th birthday of Professor Kenneth Johnson—a former student



In Nobel Laureate Julian Schwinger's eloquent talk at MIT, he compared the possible theoretical foundation of cold fusion with that of the much more accepted but equally mysterious phenomenon, sonoluminescence. Julian Schwinger had resigned from the American Physical Society (APS) to protest its censorship of his theoretical work on cold fusion from APS publications. It was an honor for me to have become a good friend of Schwinger's due to my involvement with cold fusion. His praise for my book, Fire from Ice, *was a very great honor (see prior page). Unfortunately, Schwinger's 1991 message at MIT was not absorbed by the assembled MIT physicists.—EFM*

A Progress Report: Energy Transfer in Cold Fusion and Sonoluminescence

by Julian Schwinger, University of California

Birthday celebrations are inevitably somewhat nostalgic. Appropriately, then, I found the cover title for this lecture in my own distant past. I first came to Berkeley on the day that World War II began. Not long after, Robert Oppenheimer gave a lecture—perhaps on cosmic ray physics—which he called "A Progress Report," in the sense, he explained, that time had elapsed. A similar expression of modesty is in order here. I have no great discoveries to announce; only feelings, hypotheses, and programs. As Mort Sahl once proclaimed:

The future lies ahead.

I am sure that my first topic, cold fusion, has caused many eyebrows to levitate. Cold fusion? Isn't all that nonsense dead and buried? How can anyone be so insane as to talk about this totally discredited subject?

Well, to the extent that sanity implies conformity with the mores of a society—didn't the Soviets clap their egregious dissidents into insane asylums?—sanity, I submit, is not a canon of science. Indeed, isn't it a goal of physics, specifically, to push at the frontiers of accepted theory through suitably designed experiments, not only to extend those frontiers, but, more importantly, to find fundamental flaws that demand the introduction of new and revolutionary physics?

—sanity, I submit, is not a canon of science. Indeed, isn't it a goal of physics, specifically, to push at the frontiers of accepted theory through suitably designed experiments, not only to extend those frontiers, but, more importantly, to find fundamental flaws that demand the introduction of new and revolutionary physics?

The seemingly bizarre behavior of some key players in the cold fusion melodrama has managed to obscure a fundamental challenge that this episode presents. Whether or not the reality of cold fusion has been demonstrated experimentally, one must ask if any conceivable mechanism now exists, or might be devised, whereby nuclear energy could be extracted by manipulations at the atomic level.

One is mindful of the high temperature superconductivity story. Despite the assurances of theorists that superconductivity could not exist much above absolute zero, that barrier was broken experimentally. Although it took time to get reproducible results, the reality of the phenomenon is completely established, despite the absence (to my knowledge) of any accepted theory.

High temperature superconductivity is an atomic process. Cold fusion is that too, but also involves the much shorter space and time scales of nuclear physics. It should therefore be much more difficult to control this phenomenon by manipulations at the atomic, perhaps better said: at the chemical, level. More difficult, but not necessarily impossible.

Despite my earlier qualification of the established reality of cold fusion, one cannot ignore the evidence accumulated in many laboratories—of excess heat production, of tritium production—all of which is characterized by irreproducibility and by uncontrollable emission in bursts. But, from what has just been said, that kind of behavior is expected; it is not a basis for rejecting the reality of the phenomena.

This brings me to study the validity of the case against cold fusion, as seen by a hot fusioneer—henceforth known as HF—who rejects the possibility that new physics is involved.

In the hot fusion of two deuterons—the D-D reaction—the formation of a triton (^{3}H) and a proton proceeds at about the same rate as that for the creation of ^{3}He and a neutron. But, given the claims of tritium production in cold fusion experiments, neutrons at the expected intensities are conspicuously absent, although low levels of neutrons, appearing in bursts, have been observed. To HF the conclusion is obvious: No neutrons—no tritium—no cold fusion. Moreover, the two cited reactions are the only important ones in hot fusion. So: No neutrons—no cold fusion—no excess heat.

Very soon after March 23, 1989—which one might well call D-day—-the idea was advanced that excess heat is produced by the formation of ^{4}He in the ground state. To this HF responds that the suggested reaction is weak, and no one has detected the γ-rays of roughly 20 MeV that should accompany the formation of ^{4}He.

Then came the suggestion that excess heat might result from the HD, rather than the DD, reaction. Heavy water (D_2O) always has some small contamination of light water (H_2O). The fusion of a proton and a deuteron produces ^{3}He. To which HF responds that no γ-ray of roughly 5 MeV, which should accompany this reaction, has been observed.

With heat production and tritium production allocated to the HD and DD reactions, respectively, how can one understand the suppression of neutron production? It may be that two fusing deuterons populate, not the quite remote ground state, but rather the first excited state of ^{4}He. That excited state decays into a triton and a proton. But, decay into ^{3}He and a neutron is energetically forbidden. Tritium—Yes. Neutrons—No. HF responds to this by pointing to the absence of the roughly 4 MeV γ-ray that should accompany the ^{4}He excited state.

Thus presented, the experimental aspects of HF's indictment of cold fusion come down to the non-existence of various γ-rays that the tenets of hot fusion require. What rebuttal can one give to these charges?

Well, consider the following bit of insanity:

The circumstances of cold fusion are not those of hot fusion.

In contrast with hot fusion, where energies are measured in substantial multiples of kilovolts, cold fusion deals with energies that are a fraction of a volt. The dominant electromagnetic mechanism for hot fusion is electric dipole radiation, in which the parity of the particle system reverses.

Now, at the very low energy of cold fusion, two deuterons, for example, which carry even intrinsic parity, have very little chance of fusing in other than the orbital state of zero relative angular momentum—of even orbital parity. Thus, an excited state of ^{4}He is formed that has even parity. Possibly it radiates down to the first excited state, or the ground state of ^{4}He. But both of the latter states also have even parity. With no parity change, electric dipole radiation is forbidden. There are, of course, other mechanisms that might intervene, albeit much more weakly—electric quadrupole radiation, magnetic dipole radiation, electron-positron pairs. But, much more important is the impetus this result gives to considering the following additional bit of insanity:

The excess energy liberated in cold fusion is not significantly transferred by radiation.

If not radiation, what? HF, with his focus on near-vacuum conditions, would have no answer. But cold fusion does not occur in vacuum—it appears in a palladium lattice within which deuterium has been packed to form a sub-lattice. Which leads to the next bit of insanity:

The excess energy of cold fusion is transferred to the lattice.

This is the moment to introduce HF's theoretical ace in the hole. In hot fusion work it is taken for granted that the fusion reaction rate is the product of two factors: the barrier penetration probability that stems from the Coulomb repulsion of like charges; and the intrinsic reaction rate that refers mainly to the nuclear forces. At the very low energy of cold fusion, the penetrability of the Coulomb barrier is so overwhelmingly small that nothing could possibly happen.

How does one respond to that? By sharpening the initial insight:

The circumstances of cold fusion are not those of hot fusion.

At the very low energy of cold fusion, one is dealing essentially with a single wave function, which does not permit the factorization that HF takes for granted. The effect of Coulomb repulsion cannot be completely separated from the effect of the strongly attractive nuclear forces. This is a new ball game.

All very well, but can one be a little more specific about the new mechanisms that might produce cold fusion?

If, as I hypothesized, the lattice is a basic part of that mechanism, some knowledge of the palladium lattice, loaded with deuterium, is needed. That knowledge exists for light loading, but, as far as I am aware, not for heavy loading. There is, however, a theoretical suggestion that, for sufficiently heavy loading, a pair of new equilibrium sites, for hydrogen or deuterium ions, comes into being within each lattice cell. The equilibrium separation of such a pair is significantly smaller than any other ionic spacing in a cell.

It would seem that, to take advantage of those special sites, a close approach to saturation loading is required. (Indeed, that is so if a steady output is to occur.) But, the loading of deuterium into the palladium lattice does not proceed with perfect spatial uniformity. There are fluctuations. It may happen that a microscopically large—if macroscopically small—region of the lattice attains a state of such uniformity that it can function collectively in absorbing the excess nuclear energy released in an act of fusion.

And that energy might initiate a chain reaction as the vibrations of the excited ions bring them into closer proximity. This burst of energy will continue until the increasing number of irregularities in the lattice produce a shut-down. The start-up of another burst is an independent affair. It is just such intermittency—of random turnings on and off—that characterize those experiments that lead one to claim the reality of cold fusion.

Now we come to barrier penetration, or rather, what replaces it. HF accepts a causal order in which the release of energy—at the nuclear level—into the ambient environment, follows the penetration of the Coulomb barrier. The response to that carefully crafted statement is surely: Of course! What else? Well, how about this major bit of insanity?

Other causal orders and mechanisms exist.

Unlike the near-vacuum of HF, the ambient environment of cold fusion is the lattice, which is a dynamical system capable of storing and exchanging energy.

The initial stage of one new mechanism can be described as an energy fluctuation, within the uniform lattice segment, that takes energy at the nuclear level from a dd or a pd pair and transfers it to the rest of the lattice, leaving the pair in a virtual state of negative energy. This description becomes more explicit in the language of phonons. The non-linearities associated with large displacements constitute a source of the phonons of the small amplitude, linear regime. Intense phonon emission can leave the particle pair in a virtual negative energy state.

To illustrate the final stage of this mechanism, consider the pd example where there is a stable bound state: ^{3}He. If the energy of the virtual state nearly coincides with that of ^{3}He a resonant situation exists, leading to amplification, rather than Coulomb barrier suppression. Between the two extremes of causal order there are, of course, a myriad of intermediate energy transfer mechanisms, so that the mechanism, as a whole is devoid of causal order.

I note here the interesting possibility that the ^{3}He produced in the pd fusion reaction may undergo a secondary reaction with another deuteron of the lattice, yielding ^{5}Li. The latter is unstable against disintegration into a proton and ^{4}He. Thus, protons are not consumed in the overall reaction, which generates ^{4}He.

The suggestion that nuclear energy could be transferred to an atomic lattice is usually dismissed (contemptuously, I might add) because of the great disparity between atomic and nuclear energy scales; of the order 10^{7}, say. It is, therefore, of great psychological importance that one can point to a phenomenon in which the transfer of energy between different scales involves--and here I quote—"a focusing or amplification of about eleven orders of magnitude."

It all began with the sea trials, in 1894, of the destroyer HMS Daring. The onset, at high speeds, of severe propeller vibrations led to the suggestion that bubbles were forming and collapsing—the phenomenon of cavitation. Some twenty-three years later, during World War I, Lord Rayleigh, no less, was brought in to study the problem. He agreed that cavitation, with its accompanying production of pressure, turbulence, and heat, was the culprit. And, of course, he devised a theory of cavitation. But, there, he seems to have fallen into the same error as did Isaac Newton who, in his theory of sound assumed isothermal conditions. As Laplace pointed out in 1816, under circumstances of rapid change, adiabatic conditions are more appropriate.

During World War I, the growing need to detect enemy submarines led to the development of what was then called (by the British, anyway) subaqueous sound-ranging. The consequent improvements in strong acoustic sources found no scientific applications until 1927. It was then discovered that, when a high intensity sound field produced cavitation in water, hydrogen peroxide was formed. Some five years later came a conjecture that, if cavitation could produce such large chemical energies, it might also generate visible light. This was confirmed in 1934, thereby initiating the subject of sonoluminescence (SL). I should, however, qualify the initial discovery as that of incoherent SL, for, as cavitation noise attests, bubbles are randomly and uncontrollably created and destroyed.

The first hint of coherent SL occurred in 1970 when SL was observed without accompanying cavitation noise. This indicates that circumstances exist in which bubbles are stable. But not until 1990 was it demonstrated that an SL stream of light could be produced by a single stable cavity.

Ordinarily, a cavity in a liquid is unstable. But it can be stabilized by the alternating cycles of compression and expansion that an acoustic field produces, provided the sonic amplitudes and frequencies are properly chosen. The study of coherent SL, now under way at UCLA under the direction of Professor Seth Putterman, has yielded some remarkable results.

What, to the naked eye, appears as a steady, dim blue light, a photomultiplier reveals to be a clock-like sequence of pulses in step with the sonic period, which is of the order of 10^{-4} seconds. Each pulse contains about 10^{5} photons, which are emitted in less than 50 pico seconds, that is, in about 10^{-11} seconds.

When I first heard about coherent SL, some months ago, my immediate reaction was: This is the dynamical Casimir effect. The static Casimir effect, as usually presented, is a short-range non-classical attractive force between parallel conducting plates situated in a vacuum. Related effects appear for other geometries, and for dielectric bodies instead of conductors.

A bubble in water is a hole in a dielectric medium. Under the influence of an oscillating acoustical field, the bubble expands and contracts, with an intrinsic time scale that may be considerably shorter than that of the acoustical field. The accelerated motions of the dielectrical material create a time-dependent—dynamical—electromagnetic field, which is a source of radiation. Owing to the large fractional change in bubble dimensions that may occur, the relation between field and source could be highly nonlinear, resulting in substantial frequency amplification.

The mechanisms that have been suggested for cold fusion and sonoluminescence are quite different. But they both depend significantly on nonlinear effects. Put in that light, the failures of naive intuition are understandable.

So ends my Progress Report.

Julian Schwinger's cold fusion work has been published in non-APS journals, including the *Proceedings of the National Academy of Sciences*. We proudly reprinted his "Cold Fusion: A Brief History of Mine," in Issue No.1 of *Infinite Energy*, 1995.

For a few years, the "cold fusion underground" at MIT held a well-attended cold fusion symposium during the IAP (Independent Activities Period). Since 1996, this activity has moved off campus.—EFM

COLD FUSION
A Massachusetts Institute of Technology
IAP Program—Video-Lecture-Demonstration Program

January 21,1995, Saturday 9AM-5PM
Room 6-120, Physics Lecture Hall
First floor, main building of MIT.

TENTATIVE PROGRAM - Subject to Change

Start at 9:00 am sharp

* Dr. Eugene F. Mallove, MIT'69, Organizer -—Introduction, outline, and overview of latest results (30-45 min)
* Dr. Peter Graneau (Video tape of water plasma explosions) "Anomalous Forces in Water Plasma Explosions" (45-60 min)
* J. Patterson's U.S. Patent and Technology—video tape and lecture by staff of Clean Energy Technology, Dallas, TX (30 min)
* James Griggs—The Hydrosonic Pump (video and lecture) (45 min)
* Coffee Break
* Ray Conley, MIT -- Results of Light Water Excess Heat Experiments (20min)
* Fred Jaeger, ENECO (Patents and Commercialization) (10 min)
* Recent results of experiments at E-Quest Sciences—Helium and Excess Heat (10 min)
* Lunch Break of 20-25 minutes, refreshments to be served outside 6-120
* Professor Peter L. Hagelstein, MIT "Neutron Transfer Reactions"—Progress in theory (45 min)
* Professor Keith Johnson, MIT, Progress in Theory of Excess Heat and Progress in Producing "Cold Fusion: The Movie" (45 min)
* Professor Vesco Noninski, Fitchburg State College "Nuclear measurements—new understandings" (20 min)
* Bertil Werjefelt, PolyTech(USA) (45 min) "'Magnetic Energy': Experiments, Commercial Prospects, and Theory"
* Video Tape from Japan, Fuji Television (8 minutes)—"Magnetic Energy"
* Time allotted for late-arriving additions in cold fusion and enhanced energy
* CBC Cold Fusion Program, "Too Close to the Sun" (50 min)
* Evening Break at 5:00 p.m. for dinner and possibly resume for 7:00-8:30

General Discussion of Business and Social Issues—Possible Panel Discussion. Refreshments and organizing costs contributed by ENECO, a company committed to commercialization of cold fusion and enhanced energy technologies.

The full tapes of the program and a written record summarizing the meeting will also be available through Dr. Gene Mallove, Bow, NH.

EXHIBIT A

While the MIT PFC-Chemistry Department team was going through the early stages of its motions to "debunk" the work of Drs. Fleischmann and Pons, one of the team members, Professor Ronald Ballinger, was sent to Washington to testify before Congress. The MIT hot fusion people wanted to minimize the chance that Congress would divert any hot fusion funding to the investigation of cold fusion. In his testimony, Ballinger audaciously claimed that the MIT calorimetry methods were more sophisticated than those of Fleischmann and Pons—a great irony in view of later serious questions about the MIT PFC work. While this Congressional blocking action was carried out, the plan to launch a PR assault against cold fusion was moving forward. Only two days later, Professors Ballinger and Ronald R. Parker would give a secret interview with Boston Herald *reporter Nick Tate (see Exhibit B), the story that would mark the beginning of accusations of fraud against the Utah electrochemists.—Eugene Mallove (EFM).*

Comments on "Cold Fusion"

Testimony presented to the Committee on
Science, Space, and Technology
U.S. House of Representatives
Washington, D. C.

by Professor Ronald G. Ballinger, Department of Nuclear Engineering,
Department of Materials Science and Engineering,
Massachusetts Institute of Technology, Cambridge, Massachusetts

April 26, 1989

Mr. Chairman, Members of the Committee:

I am Ronald Ballinger, a faculty member of the Departments of Nuclear Engineering and Materials Science and Engineering at the Massachusetts Institute of Technology. I am very grateful for your invitation to convey my views related to the recent reports of the achievement of "cold fusion."

Professor Ronald G. Ballinger
MIT Photo

I am a member of an interdisciplinary team at MIT that is involved in an attempt to reproduce the reported "Cold Fusion" results of Professors Pons and Fleischmann of the University of Utah. The team's principals include Dr. Ronald R. Parker, Director of MIT's Plasma Fusion Center; Professor Mark S. Wrighton, Head of the Chemistry Department; and myself. (A complete list of team members and areas of expertise is included). The team is composed of experts in the fields of physical metallurgy, electrochemistry, plasma physics, instrumentation, and radiation detection. The team has been involved in attempts to reproduce the results, reported by Professors Pons and Fleischmann since shortly after their results were released to the press and for publication in the *Journal of Electroanalytical Chemistry*.

As I am sure that you and the members of this committee are aware, any breakthrough in the area of energy production that has the potential to supply current and future energy needs in a non-polluting manner must be given serious attention. Quite apart from its impact on basic science, the results recently reported by Professors Pons and Fleischmann, should they prove to be correct, represent such a breakthrough. The basic nature of their results have been described and discussed by earlier testimony before this committee. Basically, the team at the University of Utah has reported the fusion of deuterium atoms in a palladium matrix at room temperature.

As evidence that "cold fusion" has taken place, the production of excess heat and neutron radiation has been reported. The reported magnitude of both of these is such that their presence could be verified by other investigators.

Much more modest results have been reported by a team of investigators at Brigham Young University. We feel that it is important to distinguish between the BYU results, which are of scientific interest but of limited or no practical significance and those of the University of Utah which, should they prove correct, have major implications for future energy production.

Since the reports of these results, a number of teams worldwide have been attempting to reproduce these results. To my knowledge, with the possible exception of the Stanford results and results from Europe and the USSR of which I have no personal knowledge, no team has been successful. As far as the results of attempts by the team at MIT are concerned, we have been thus far unable to scientifically verify any of these results. This is in spite of the fact that we are employing calorimetry and radiation detection methods of even greater sophistication and sensitivity than those of the University of Utah. Having said this, I can assure you that these negative results have not been the results of a lack of effort. The MIT team has been, as I am sure is the case with other teams, laboring around the clock. However, we and the other teams have been handicapped by a lack of enough scientific detail to guarantee that we are actually duplicating these experiments.

In the scientific community, the soundness of experimental or theoretical research results is evaluated through peer review and duplication. For results such as those reported, whose potential impact on the scientific community and the world are so great, this review process is absolutely essential. Unfortunately, for reasons that are not clear to me, this has not happened in this case—at least so far. The level of detail concerning the experimental procedures, conditions and results necessary for verification of the Pons and Fleischmann results have not been forthcoming. At the same time, almost daily articles in the press, often in conflict with the facts, have raised the public expectations, possibly for naught, that our energy problem has been "solved." We have heard the phrase "too cheap to meter" applied to other forms of electric energy production before. And so the scientific community has been left to attempt to reproduce and verify a potentially major scientific breakthrough while getting its experimental details from the *Wall Street Journal* and other news publications.

Experiments conducted in haste and based on insufficient detail coupled with premature release of results have often resulted in retractions and embarrassment on the part of the scientific community—caught in the heat of the moment. I guess we are all human.

The result of this unsatisfactory situation has been that a healthy skepticism and, in some cases, distrust of the reported results has developed. We at MIT share this skepticism.

At the risk of becoming too technical in my comments, I feel that I must be a bit more specific with regard to the source of this skepticism. As I mentioned earlier the major results, reported by the University of Utah group, are that there has been a generation of excess heat and the measurement of neutron radiation. By excess heat I mean that there has been a measurement of more energy produced than has been supplied to the system. From our standpoint, the key point of verification is the detection of neutron radiation. From an engineering point of view, however, the importance of excess heat production is critical. On these two critical points we have found that the results reported in the few available published documents from the University of Utah are inconclusive or unclear. For example, with respect to the detection of neutrons, critical products of the fusion reaction, the reported results are confusing. They either do not agree with or are not presented completely enough to show that they are consistent with what one would expect from the emission of neutrons from the deuterium fusion reaction. Specifically, the γ-ray spectrum shown in the Fleischmann/Pons paper and attributed to neutron emission does not exhibit a shape and intensity that demonstrates the increase reported in the number of detected neutrons above normal background. Further, the reported rate of neutron emission and level of tritium production are consistent with natural background. The results have nevertheless been reported as "significant." Those inconsistencies can only be resolved by a full disclosure of the details of the experimental measurements for examination by the scientific community. Until such time as this occurs we feel that the data is insufficient to demonstrate the presence of neutrons.

As far as the issue of excess energy is concerned we are also faced with a confusing situation. While the presence of excess energy is documented in the *Journal of Analytical Electrochemistry* paper, the method by which this excess energy was determined is not clear. With metals, such as palladium, which act as hydrogen storage media and at the same time as catalysts for many chemical reactions, both situations which can result in discontinuous chemical energy releases, it is critical that a total energy balance over time be done. To us it is not clear that this has been the case. Until this issue is clarified we are unable to make a judgement concerning the excess energy issue.

In conclusion, I feel that it is safe to say that the scientific community is (1) excited about the possibility of a significant advance in the area of fusion energy research, (2) but is, at the same time, skeptical of results that have not been verified to this point and (3) is very frustrated at the methods by which the discovery has been handled both in the scientific and non-scientific community. Thank you.

PROFESSOR RONALD GEORGE BALLINGER

Professor Ballinger is an Associate Professor at the Massachusetts Institute of Technology with a joint appointment in the Departments of Nuclear Engineering and Materials Science & Engineering. Professor Ballinger's areas of specialization are as follows: (1) Environmental effects on material behavior, (2) Physical metallurgical and electrochemical aspects of environmentally assisted cracking in aqueous systems, (3) Stress corrosion cracking and hydrogen embrittlement in Light Water Reactor systems, (4) The effect of radiation on aqueous chemistry and stress corrosion cracking, (5) Experimental fracture mechanics techniques and analytical methodology, and (6) Materials development for cryogenic applications. Professor Ballinger is the author of several papers in the above areas and is a member of several professional societies including the National Association of Corrosion Engineers, The American Society for Metals, The Electrochemical Society, The American Nuclear Society, and the American Society for Testing and Materials. Professor Ballinger is a member of the International Cyclic Crack Growth Review Group and the International Cooperative Working Group in Irradiation Assisted Stress Corrosion Cracking.

MIT Cold Fusion Group

Plasma Fusion Center

Professor Ronald R. Parker
Director, Plasma Fusion Center
Plasma Physics/Fusion Research

Dr. Xing Chen
Postdoctoral Associate
Radiation Detection

Dr. Catherine Fiore
Research Scientist
Radiation Detection

Dr. Marcel Gaudreau
Research Engineer
Fusion Engineering

Dr. David Gwinn
Research Engineer
Instrumentation/Design

Dr. Paul S. Linsay
Principal Research Scientist
Radiation Physics

Dr. Stanley Luckhardt
Principal Research Scientist
Plasma Physics

Dr. Richard Petrasso
Research Scientist
X-and γ-ray Spectroscopist

Mr. Kevin Wenzel
Graduate Student
Radiation Detection

Dept. of Nuclear Engineering and Dept. of Materials Science and Engineering

Professor Ronald G. Ballinger
Associate Professor
Physical Metallurgy and Electrochemistry

Dr. Il Soon Hwang
Research Scientist
Physical Metallurgy/Electrochemistry

Dr. Alan Turnbull
Visiting Scientist (National Physical Laboratory, UK)
Electrochemistry/Surface Science

Martin Morra
Graduate Student
Physical Metallurgy

Mr. Frank Wong
Graduate Student
Mechanics/Instrumentation

Department of Chemistry

Professor Mark Wrighton
Head, Department of Chemistry
Chemistry/Electrochemistry

Dr. Richard Crooks
Postdoctoral Associate
Electrochemistry

Mr. Vincenzo Cammarata
Graduate Student
Chemistry/Electrochemistry

Mr. Martin Schloh
Graduate Student
Chemistry/Electrochemistry

Mr. David Albagli
Graduate Student
Chemistry/Electrochemistry

"Words to Eat"

MIT Professor Ronald George Ballinger may hold the all-time record for making a foolish statement against cold fusion. He wrote in 1991: "It would not matter to me if a thousand other investigations were to subsequently perform experiments that see excess heat. These results may all be correct, but it would be an insult to these investigators to connect them with Pons and Fleischmann."

These words of "wisdom" appeared in the *Gordon Institute News*, March/April 1991. Apart from their unrepentant mean spirit, they are internally inconsistent. If in his *hypothetical* the remarkable discovery of Fleischmann and Pons were to be validated, why would the scientists not be due praise? Is Ballinger's sense of righteous indignation about Fleischmann and Pons so pronounced that he could not grant them credit—*ever*? One would think that scientific ethics alone would mandate that these "thousand other investigations" should be tied directly to those who inspired them!

Ballinger wrote in the same venue: "Putting the 'Cold Fusion' issue on the same page with Wien, Rayleigh-Jeans, Davison-Germer, Einstein, and Planck is analogous to comparing a Dick Tracy comic book story with the Bible." The facts about this moralizing hypocrite, Prof. Ballinger, are even more amazing when one learns that Ballinger subsequently personally sought funding support from Dr. Thomas O. Passell at the Electric Power Research Institute (EPRI) to carry out materials science projects related to cold fusion!

Exhibit B: Partial transcript of tape of interview (Friday, April 28, 1989) by Nick Tate of the *Boston Herald* with Professor Ronald R. Parker and Associate Professor Ronald G. Ballinger

This tape was released to the public, according to a *Boston Herald* story on May 2, 1989. The contents of this tape clearly contradict the MIT Press Release of May 1, 1989, which was issued by the MIT News Office on behalf of Professor Parker.

Prof. Ballinger

Prof. Parker

Nick Tate, then of the *Boston Herald*

From this "smoking gun" interview, it is clear that the story in the Boston Herald, *May 1, 1989, was an eminently fair reflection of the interview with Professors Parker and Ballinger. Virtually the full April 28, 1989 interview with Parker and Ballinger is transcribed from the audio tape released by the* Boston Herald. *It is fascinating to hear Parker telling a DoE Cold Fusion Panelist (Dr. Richard Garwin of IBM) that they think they have evidence of "fraud" by Fleischmann and Pons. Parker is seen coordinating with NBC reporter Bob Bazell the forthcoming "blast" against the Utah electrochemists.—EFM*

[***Editor's Note: "xxxx" means inaudible portion of audio tape.***]

Parker: . . .accuse them of fraud, one could.

Tate: Can you—can you tell me what the uh—what exactly the significance of the 2.5 is? I mean, understand I've attempted—I'm not a scientist. I've attempted to read as much as I can understand.

Parker: I can give you a short synopsis of that.

Ballinger: Can we, uh—can I make—say something here about —? You're going to publish this right?

Tate: Yes.

Ballinger: You're not a scientific person, right?

Tate: That's correct.

Ballinger: What's the procedure about this? Can we see what you're going to print, before you're going to print it? Not to change anything, but to make sure you don't screw something up here.

Tate: In technical terms?

Ballinger: In technical terms.

Tate: Uhhh—

Ballinger: You know we're talking about serious business here and I have seen crap in newspapers that comes out, you know, that quotes the generation of isotopes which aren't—don't exist and all kinds of stuff like that. Nobody's going to change anything—

Parker: That's a good point. The reason I stopped talking to the *Globe*, for example, is that I felt that they were reporting irresponsibly.

Ballinger: They interviewed me but didn't (xxxx). . .

Parker: Yeah, and you know they were out there just leading the cheers instead of being objective.

Tate: Let me say this to you, that in general the process is—the policy of the paper is to turn down a story before having it proof read outside of the paper. But I understand what you're saying. I think that given I am not a scientific person, we could work something out.

Ballinger: There has to be a way because there's sort of a moral obligation here on our part to make sure that . . .

Parker: Let me—Yeah let me put it another way, I mean we're beginning to get a very short fuse on this whole issue, as you can tell, because for example these guys were down in Congress when Ron was down there on Wednesday asking for twenty-five million bucks. [***Editor's Note: See Ballinger's Congressional testimony, page 84.***]

Ballinger: A hundred and twenty five.

Parker: Well, a hundred. . .

Tate: Was it $125 million?

Parker: Only a mere twenty-five from the government, right?

Ballinger: Twenty-five from the government, the rest from industry.

Parker: And, you know, it's one thing when they come out with something that's potentially interesting scientifically and so on and so forth. It's quite another thing when they're out there trying to fleece the public money to push something that, uh, has no credibility at this point. Now in a (xxxx) way what we're saying is we're ready to begin getting into the controversial issue.

Tate: I should explain to you. . .

Parker: We don't want to do that without trusting the source, Okay? In other words, you know I can't trust the *Globe*, I'd like to trust you. I can't trust you unless I know what you're going to turn out.

Tate: I guess it depends on what we talk about. What you're suggesting is that what I would like to do based on just a little information that I've heard is write a story that indicates you have serious questions and concerns about what Pons and Fleischmann are saying. . .

Parker: We can go beyond the concerns and questions to say that what they have reported is not true. That's a much stronger statement.

Tate: And potentially what you're suggesting is that—to bring some money into the university.

Parker: I shouldn't say that, I should say that that's your conjecture, not mine. Okay, the fact that they're down there asking for $125 million you can draw your own conclusions from that.

Ballinger: I would suggest that you probably have a tape of the entire hearing.

Parker: Do you have one?

Tate: I don't have one, no.

Ballinger: Well, you should get one and you should look at it and spend the time, because then you'll understand what was going on down there. In terms of your background, it's a very important thing for you to look at. Even though you may be on a deadline and it may be six hours long and all that stuff, you really need to see what was going on.

Tate: What was your impression of what was happening?

Ballinger: It was a fairly well-orchestrated attempt to, in my mind, short circuit in this case well-established and well-recognized review process for any kind of research much less this kind of research and get uh—diversion of funds to uh from the government from other projects presumably to the University of Utah. And, uh, they used uh you know. If you assume that what they are saying is correct, you may argue about the heavy-handedness of what their logic is of their methods, but for something that's not been proved to be correct, the fact that they used this (xxx) was going on plus the fact that (xxx). . .

Ira Magaziner
He later played a role in the Clinton Administration on health policy.
Photo: Courtesy White House

Ballinger: If you don't look at the tape, you should read Ira Magaziner's testimony. He is the consultant that they hired to uh . . .

Parker: Is he the one who went into this society in Rome?

Ballinger: He made a very, you know, a very pseudo-truth — that's the word I'd use. You start out with something which is fundamentally true, but everything is not so true after that. We are, in fact, getting killed by the Japanese. I mean we're great inventors, but we don't do a good job of bringing things to market. The Japanese are excellent at that and were getting beaten. We're getting our ass beaten, right? And that's the argument that he used, we should definitely—we should stuff all kinds of money in here, we should go on parallel paths, we should establish a center—an international center in Utah, naturally in Utah, because that's where the best scientists are. And we should get going on this right away. That makes sense, if you're trying to, if you have an established, verified product. You know, I mean I think I agree that we're getting killed by the Japanese and so therefore we should —there has to be a way to augment the way we do that kind of thing. But he started from a fundamental assumption which was not correct, and that is, we don't have anything that's proven, and moreover not only do we not have anything that is proven, but there's a lot of reason to believe that not only will it be disproven, but it will turn out that it's not correct.

Tate: Let me ask you, just back up a step. You're talking about—I presume you're talking about traditional scientific controls and traditional scientific methods that have not been observed in this particular situation.

Parker: This is sci— I'll give you a quote: This is scientific schlock, Okay.

Tate: Tell me specifically what they've done.

Parker: [Parker laughs].

Tate: That is that may. . .

Parker: I'll just tell you about the neutrons, Okay.. That's really important, Okay. They've taken some data. They didn't even take it themselves, they had people take it for them. They published it in their paper and they claimed that it showed the presence of neutrons from their experiment. The data is patently, has been patently falsely interpreted. Neutrons are not present at anywhere near the level their own data shows. They're not there. They've misinterpreted their results. They falsely interpreted their results. Whether they did this intentionally or not I don't know, but they did not present—interpret their results correctly. It's a key point in their paper.

Tate: Specifically what they're claiming, that it was neutrons they were creating. . .

Parker: That they were creating neutrons from their experiment. Their documentation unfortunately shows that not only was it falsely interpreted, but there were no neutrons at anywhere near the level they claimed. You can use the data in two ways, to show that they falsely interpreted it, but also that there weren't neutrons at the level they claimed.

Tate: So at best it's misinterpretation and at worst it's — as you were saying. . .

Parker: It's fraud.

Tate: Now do you know this from studying their research, from reviewing their information, or have you tried—and I presume you've, in addition, attempted to parallel what they've done?

Parker: We reproduce their results so we completely understand why they misinterpreted. Let me put it a different way, we don't see why they misinterpreted, we don't understand what they should have seen and didn't.

Tate: So you've reproduced their experiment?

Parker: We've simulated the neutrons. We've said, suppose there were no neutrons, what would it have looked like? And we find something quite different from what they claim.

Ballinger: We find what we should expect.

Tate: Would you care to speculate on their intent?

Parker: I think Ron made it perfectly clear that when you're asking for $125 million for the university, I mean I don't want to be tied into that quote, but I mean you have to draw the (xxxx). They were in Washington Wednesday asking for $125 million dollars.

[Editor's Note: At this point in the interview, Parker gets a phone call from Dr. Richard Garwin of IBM Corporation, one of the key people on the U.S. Department of Energy's cold fusion review panel. . .]

Dr. Richard Garwin
Photo: IBM Corporation

Parker (to Garwin): I just talked to Richard [Petrasso] who wrote the *Nature* piece. I don't know if you saw that? But he and I basically chuck it off, I mean you know I said his piece was the best thing written so far. And he told me he saw the original submission and it did have the line at 2.5 [MeV]. The original submission had the line at 2.5 so, you know that's, uh, the smoking gun with fingerprints, Okay, you don't even need (xxxx). Oh, gee, I don't want to quote him, but that's a good question, but the original submission to the journal had 2.5, just as the 2.5 in the equation, so you know now it transcends I think the question of whether they misinterpreted to the question of whether there was deliberate fraud. Okay, alright. . .(xxx). Well, all your detective work was correct, but now he has the smoking gun with the fingerprints on it, right? [Laughs] Okay, right, see ya! Bye. . .

Parker: So I thought he would be good to talk to and he just volunteered. He'd seen the original submission to the journal. The line was at 2.5.

Ballinger: That's what we suspected.

Parker: Right (xxxx). . .

Parker: This is where I'd say, uh, we just don't know, I mean —misinterpretation or was it malicious. . .

[Editor's Note: Now Parker remarks about Prof. Huggins' positive results in his Stanford University replication of P&F's experiment.]

Prof. Huggins
Photo, Stanford University

Parker: Unfortunately I've seen that paper. I'd give it a B as a senior thesis . . .

[Parker and Ballinger laugh intensely about Fleischmann's implication about Japanese work.]

Parker: So, what are you going to do with this, uh, Nick? You know this is. . .what you're hearing is that we think it's a scam, right?

Tate: Why is it today that you think it's a scam?

Parker: We have been studying the evidence together very slowly and we want to have a paper out on this before we actually blast them. Monday we're putting a paper out on it. . .

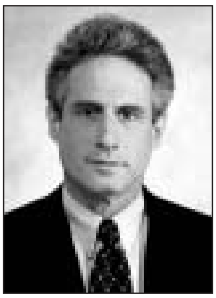
Parker: It depends on what magnitude you want to break it.

Tate: Well, it seems to me that it's a very significant story for you to be saying...

Parker: It's the first time I've actually been (xxx) this strong. Up until now I've been hoping...

Tate: I mean everybody thinks you have been very skeptical, as have other teams (xxx) can reproduce it. . .

Parker: Open to the possibility. I think after five weeks we are basically getting to the point where we can no longer suspend the disbelief.



Robert Bazell
Photo, NBC TV

[Parker gets a phone call from science reporter Bob Bazell of NBC-TV]

Parker (to Bazell): Hello, Bob. Thanks for calling me back. Okay, appreciate it because uh (xxxx) we don't want them to have a chance to uh come up with any sort of (xxxx) Now I promise on Monday we'll have it out. I'll fax it to you. Okay, alright? I've got one in my office! Ha, ha. It's a local paper. No, we have not done anything as far as a press release. . .Uh, well maybe we can work something out. It depends on how big a story he wants to do. Well, if they didn't see neutrons. You know I just talked by the way to Richard Garwin and he confirmed that the first paper that Pons and Fleischmann submitted had the line at 2.5 MeV. Did you know that? Well, that's important because they moved it. And now the question is, is it fraud, or is it (xxxx)?

Parker: Well, that was Bazell, Bob Bazell — you know who he is, of NBC? — he's a little concerned about how you're going to handle it. He's concerned and I am too, because he's been very good to me as far as being confidential and respecting my views. He's at the (xxxx) right now where he wants to run something on this. And I feel like I'd like to, you know, I don't mind if it hits the streets the same day, but I think it would be. . .

Tate: That's fine.

Parker: I think if you'd respect that we can probably give you more. . .

Tate: I would just ask that no other media outlets get this information beforehand. I think that's fair.

Parker: I was just thinking in my mind. I have a list of sort of good (xxxx). . .

Ballinger: *Technology Review*. . .

Eugene Mallove

Parker: Yeah, they'll come out months from now. We'll have to give it to MIT actually, I mean Mallove.

Tate: I'm not real hot to scoop anybody with the story. It's a big story. I'd like to do that and respect your wishes. But if it comes out in another publication, a competitor or a daily publication . . .

Parker: It's not coming out in the *Globe*.

Tate: Okay.

Ballinger: And I don't answer phone calls unless they're from inside MIT. . .

Tate: Obviously, we're going to need to get into more of the technical aspects of it. Can you tell me some of those for that story in Monday's paper or would you prefer to handle that?

Parker: I'm going to have to leave in ten minutes anyway, so it's not going to be great. . . Let's see, how to handle it. We're going to get into trouble with Mallove, if we don't apprise him on Monday. But you could break the story on Monday.

Parker: [Parker on the phone to Harold Furth of Princeton Plasma Physics Lab]. . . We're also working with a guy called Wrighton, who is an electrochemist. . . Next week we're definitely going to hit them. . . So meanwhile, we're pretty much going to blast these guys on Monday — on the neutrons. . . Well you know, you can take that one on. I'm not going to get into the calorimetry. I think, having done the calorimetry for several weeks now, I understand much better about the problems, and I think I could speculate on what they did or didn't do. I certainly know enough to discount completely the Stanford experiment, only because they published enough details so I could see where they went wrong. Now in the case of Utah, they didn't publish details, so I can't say. . . All I'm going to focus on— I know the following facts. They published a peak initially at 2.5, they then moved it to 2.2 for the same data, alright? Now that could be either fraud or it could be just misinterpretation. I'm not going to comment on that. However, the line that they finally show is xxx sodium iodide, 3-inch crystal. . .

Harold P. Furth
Princeton Plasma Physics Laboratory

Parker: How are we going to leave it? You're going to hold this for Monday, right?

Ballinger: I'd really like to see it for technical content. You know nobody's going to try to, and although we might like to sometime. *End of Tape*

"In one word, it's garbage."

MIT Professor of Physics Emeritus Martin Deutsch May 6, 1989, characterizing cold fusion.

Exhibit C

MIT News Office Deceptive Press Release

I had been up into the wee hours of the night of April 30-May 1, 1989, sending a press release dictated to me over the telephone at my home in Bow, New Hampshire by Professor Parker. I telephoned it to UPI, Reuters, and the Associated Press, and it denied what Parker had said in the interview with the Boston Herald's *Nick Tate. When I arrived at the MIT News Office early that morning after a sleepless night, we hastily put together a printed form of the press release to handle the approaching storm. This is the text of the Press Release that was issued from the MIT News Office on May 1, 1989. On the day of my resignation from my MIT News Office position, June 7, 1991, I publically disavowed this Press Release—an unintended falsification of the truth in which I was used as a dupe in part of an orchestrated campaign against cold fusion (an image of this Press Release appears on page 76)—EFM*

MIT News Office PRESS RELEASE May 1, 1989

URGENT MEDIA ADVISORY

For Immediate Release May 1, 1989
MIT Contact: Eugene F. Mallove, Sc.D. Chief Science Writer

CAMBRIDGE, Mass., May 1—Professor Ronald R. Parker, Director of the MIT Plasma Fusion Center responded today to an article published this morning in the *Boston Herald*, an article that he says has seriously misquoted him and given a largely incorrect view of his discussions with the *Boston Herald's* reporter, Nick Tate.

Professor Parker issued this statement:

"The article erroneously characterizes remarks that I made regarding the cold fusion experiments done at the University of Utah. Specifically, I did not: (1) Deride the University of Utah experiments as "scientific schlock" or (2) Accuse Drs. Fleischmann and Pons of 'misrepresentation and maybe fraud'."

Today, Professor Parker's colleagues will present a paper (co-authored with him) at the meeting of the American Physical Society in Baltimore, Maryland, in which they suggest that data that Drs. Pons and Fleischmann claim support the observation of neutron emission in their experiments were misinterpreted by Pons and Fleischmann.

Based on their independent analysis, the MIT researchers say that if neutron emission occurred in the Pons and Fleischmann experiment that they reported in the *Journal of Electroanalytical Chemistry*, it would have been at a level far below that reported by the University of Utah group.

Exhibit D — *Boston Globe* Letter to

MIT President Paul Gray —April 17, 1989

There is convincing evidence (see Exhibit B) that Prof. Parker had made a deliberate attempt to exclude the "cheer-leading" Boston Globe *from getting access to the MIT PFC in the hectic early days of the cold fusion uproar. Frustrated* Globe *science writer Richard Saltus wrote an extraordinary letter on April 17, 1989 to then MIT President Paul E. Gray. This was* before *the* Herald's *bombshell story of May 1, 1989 broke!—EFM*

The Boston Globe, Boston Massachusetts 02107
Telephone 617-929-2000

Paul E. Gray, President, Massachusetts Institute of Technology

Dear Dr. Gray:

We in the *Sci-Tech* section have always regarded MIT as a rich and crucial source of ideas and information. The News Office does an invaluable job in bringing stories to our attention, and the faculty and staff generally are very helpful when we call on their expertise. In addition, your visit to the *Globe* left a strong impression, and created renewed interest in subjects like science and math education and global environmental issues.

It has been disturbing, therefore, to have encountered such a lack of cooperation—a selective one, it appears—from the leadership of the Plasma Fusion program during our reporting of the claimed breakthrough at the University of Utah. We felt our readers would want to know how this scientific controversy has affected a premier fusion research center—and one in the *Globe's* city.

However, repeated attempts by myself and another reporter to talk to individuals at the Plasma Fusion Center have met with little success. In the first weeks of the story, Dr. Ronald Parker did make himself available on a few occasions, but for the past week or more has failed to return phone calls.

I also was utterly rebuffed in an attempt, which I had cleared with the News Office, to visit the fusion center briefly— entirely at Dr. Parker's convenience—so that I could convey something of the activity there during this highly unusual time. I called several times, dropped by once, and left telephone numbers where I could be reached, saying I'd be glad to talk with anyone who had a free moment. The secretary promised to let me know what could be arranged. However, no one ever responded. I appreciate the enormous demands on Dr. Parker's time. Yet, he has found time for other publications, including the *Washington Post* —whose reporter toured the facility, took photographs and interviewed several researchers and the *New York Times*, which quoted Dr. Parker as recently as last Sunday.

Whether this selective access reflects caprice or some bias against the *Globe* is hard to tell. In any case, it is regrettable that we have had to give up on MIT and turn to institutions like Princeton, where, although I am sure they are no less busy, researchers have been more helpful.

Sincerely,
Richard Saltus, Science writer

cc: Dr. Ronald Parker, Plasma Fusion Center

Exhibit E — MIT President Paul Gray's Letter to the *Boston Globe* May 1, 1989

MIT President Gray, apparently unaware of Parker's true dealings, was himself duped into writing what he honestly thought was a valid response to the Globe's *Saltus.—EFM*

OFFICE OF THE PRESIDENT

Richard Saltus, Science Writer
The Boston Globe

Dear Mr. Saltus:

I write in response to your letter of April 17, which laments the recent inaccessibility of Professor Ronald Parker. I have looked into this and find that Professor Parker has been deluged by requests for information about cold fusion and is unable to respond to all of them. He has tried to be as helpful as possible, consistent with his belief that judgment should be reserved until the scientific facts are clarified. That cautious stance has led him to discourage all media visits to the Plasma Fusion Center, although his efforts have not always been successful.

I have been assured that there was no discrimination against the *Boston Globe* and that, to the contrary, Professor Parker spoke five or six times with your colleague, Mr. David Chandler.

I regret that you feel ill used during these recent events, but I am satisfied that they are merely a consequence of the extraordinary circumstances attendant to the claim of cold fusion. We certainly hope that our good relations with the *Boston Globe*, and with the *Sci-Tech* section in particular will continue.

Sincerely yours,

Paul E. Gray

PEG/mmd Signed in his absence
cc: Kenneth D. Campbell, Ronald R. Parker

Photos, Infinite Energy archives

Exhibit F

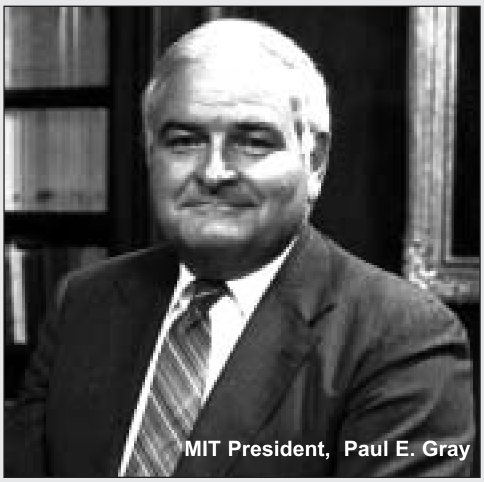
Plasma Fusion Center
Massachusetts Institute of Technology

To: Terri Priest
From: Ron Parker
Subj: Cold fusion Mug
Date: July 18, 1989

Thanks for your thoughtful procurement of the "cold fusion" mug. I really enjoyed it and will keep it with my "stamp out scientific schlock" tee-shirt and other cold fusion memorabilia. We have ordered two dozen (at quantity discount) for souvenirs to members of the MIT Cold Fusion Group. When they arrive, I'll send you one in case you know of someone else who would enjoy it.

Thanks again!

Exhibit G - MIT President Paul Gray's 1990 Remarks on Cold vs. Hot Fusion



MIT President, Paul E. Gray

This public statement clearly shows how badly the MIT PFC had duped the rest of the MIT community.—EFM

"If ever there was, in the media's eye, a silver bullet, 'cold fusion' certainly fit the bill. According to the first news release, it was 'simple, safe, and easy to implement.' Unfortunately, all the media attention surrounding the controversy over the veracity of the cold fusion experiments has overshadowed the quality work that has gone into 'hot' plasma fusion research over the last forty-five years. Here the potential energy payoff is so great and the scientific and political motivation so strong that a very large and productive research effort is already in place."

Energy and The Environment in the 21st Century (Proceedings of a Conference held at MIT March 26-28, 1990), MIT Press, 1991, p. 119-136, in "Energy Technology: Problems and Solutions," by Paul E. Gray, Jefferson W. Tester, and David O. Wood.

Exhibit H
Prof. Mark Wrighton's Letter to Dr. V. C. Noninski
October 10, 1990

This brusque letter from Prof. Wrighton, offering no scientific discussion, is an insult, yet so symptomatic of how the MIT Administration went about its anti-cold fusion work.—EFM

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF CHEMISTRY

MARK S. WRIGHTON
DEPARTMENT HEAD AND
CIBA-GEIGY PROFESSOR OF CHEMISTRY

Dr. V.C. Noninski
New York, NY

Dear Dr. Noninski:

Unfortunately, I have not had time to review your various pieces of correspondence with me concerning our work directed toward establishing the validity of claims concerning cold fusion. Let me be perfectly clear with you: we have obtained no evidence whatsoever to verify the original claims by Pons and Fleischmann concerning cold fusion. I believe that we have indicated the nature of the errors involved in the calorimetry that we have done and do not believe that there is experimentally significant evolution of "excess heat."

Sincerely yours,
Mark S. Wrighton

MSW:jvs
cc: Dr. S. Luckhardt

Exhibit I
Eugene Mallove's Letter to MIT President Charles Vest
April 12, 1991

My urgent letter to President Vest, copied to President Gray, went unanswered. Should I have been surprised? Not when President Vest had chosen Chemistry Department head Professor Mark Wrighton as Provost. Wrighton was a co-leader of the 1989 MIT PFC cold fusion experiments and a signer of the 1989 negative DoE cold fusion report. If President Vest had given him my letter to review, Wrighton would probably have dumped it in his circular file.—EFM

Eugene F. Mallove, Sc.D., Chief Science Writer
MIT News Office, Room 5-111
Lecturer in Science Journalism, Department of Humanities
Massachusetts Institute of Technology

President Charles M. Vest
Massachusetts Institute of Technology

Dear Dr. Vest:

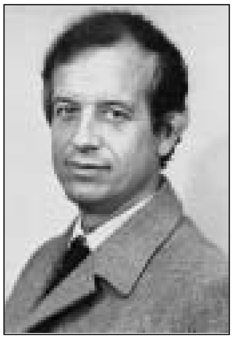
I am reminded of wonders wrought by science and technology on this day, the 30th anniversary of the first flight into space by a human being, Yuri Gagarin, and also the 10th anniversary of the flight of our space shuttle. I recall my feelings of awe—as a child and later as a young engineer, that human beings could accomplish these wondrous things. It seems that on the frontiers of science and technology, when dedicated men and women give their energies to a task, they can achieve wonders.

We are now facing, I believe, a new wonder in science. It is one, to be sure, that seems to be having an exceedingly difficult birth. I speak of what some people consider to be preposterous and "pathological" science, but others whom I believe have probed deeper into the matter, consider to be no longer deniable: that unusual nuclear reactions of incompletely understood character have been produced in metal lattice systems. Of course I am speaking of the controversial "cold fusion" phenomena. As

Nobel Laureate Julian Schwinger said in March 1990, "It is no longer possible lightly to dismiss the reality of cold fusion."

After long and careful study of this controversy in both its scientific, media, and political dimensions, I am personally convinced at greater than a 99% confidence level that cold fusion is real—both the nuclear emanations that have been reported and the excess enthalpy that seems to emerge from various experiments. The erratic nature of the phenomena —the lack of reproducibility "on demand"—has clearly been the central obstacle to acceptance in the scientific community, but extraneous "political" and programmatic factors have also played a role. It would seem, however, that reproducibility—no doubt a function of certain critical atomic structure and composition factors in the test systems—is getting to be less and less a problem.

Two unusual documents that have come to my attention are only the most recent in a cascade of information that is now emerging in the field. Physicist David Worledge of the Electric Power Research Institute (EPRI), who has just returned from a trip to the Soviet Union, supplied me with the astounding report of the "Workshop on Nuclear Fusion Reactions in Condensed Media," which was held at a world-class high-energy physics center under the sponsorship of the USSR Academy of Sciences, among other prestigious scientific organizations. This is extraordinary because this heretofore unknown but suspected level of effort on cold fusion in the Soviet Union gives an independent check on some of the nuclear effects work in the U.S. (I regret to say that in the present atmosphere of hostility to cold fusion in the U.S., such a conference would now be unthinkable at places like Brookhaven National Laboratory and Fermilab.) Noteworthy is the claimed increasing levels of reproducibility in the experiments, which incidentally, is also happening in the U..S—*e.g.* at Los Alamos National Laboratory and at SRI International in Palo Alto, which has carried out reproducible excess energy production in electrochemical cells.


Dr. Worledge of EPRI

The other paper comes from my colleague, Dr. M. Srinivasan, Head of the Neutron Physics Division at the Bhabha Atomic Research Center, BARC, in India. This is his recent excellent summary of the experimental evidence for cold fusion, which BARC has played a major role in supplying. Read it and perhaps be amazed, as I have been. I was already aware of most of the results that he cites, but he assembles it so nicely.


Dr. M. Srinivasan of BARC

As for experimental work here at MIT in this exciting new field, I regret to tell you that it does not exist. After the initial brief but intense period of experimental assessment in the spring of 1989 by an interdisciplinary team drawn from the Plasma Fusion Center and from the Chemistry Department, led by PFC Director Professor Ronald R. Parker and then Chemistry Department head, Professor Mark S. Wrighton, to my knowledge, nothing further has been done along experimental lines. It is notable, however, that researchers in several departments at MIT have continued a strong interest in the field.

An atmosphere of hostility, analogous to the editorial position on cold fusion of a certain well-known scientific journal [*Nature*], is prevalent. I do not feel that MIT's best interests are served any longer by unwarranted ignoring of the mounting experimental evidence for cold fusion. It seems to me essential that members of the MIT community reassess the experimental findings that have come and are coming from both domestic and foreign laboratories. To do any less would be, it seems to me, an abdication of scientific responsibility, not to mention a possible longer range injury to the reputation of MIT. It is even possible that the international competitiveness position of the U.S. might be at stake, something we here have given much attention to. There is strong evidence, for example the enclosed Matsushita Corporation patent application, that Japanese laboratories are devoting their considerable talents to this field. (I believe it probable that a major Japanese Corporation may be funding the work of Drs. Fleischmann and Pons now in France.) [***Editor's Note:** That corporation turned out to be IMRA, an affiliate of Toyota Corp.—EFM*]

Basically, I think the train is leaving the station, and MIT is not on it. This deeply troubles, saddens, even embarrasses me —as an alumnus who cares deeply about MIT and its image. May I suggest that you assemble very soon and publicly a panel of MIT scientists and engineers to consider and evaluate the status of research on "nuclear reactions in deuterium infused metals." (There is no need to call it the politically charged, "cold fusion," even though that may well be what it is.) I can imagine the composition of such a panel, who would hear from researchers both within and from outside MIT—including from foreign countries. Obviously, MIT's thoughtful skeptics (*e.g.* Dr. Richard Petrasso) as well as proponents of these phenomena (*e.g* Professor Peter Hagelstein) should be aboard. As a chairperson, I would offer the names of three outstanding scientists, who could guide deliberations in a fair manner: Professors Philip Morrison, Jerome Friedman, or Henry Kendall. Because of my knowledge of the field through being a conduit of information, I would be honored to assist any such panel in its deliberations.

I have sent a copy of this letter to your predecessor, Professor Paul Gray, with whom I have discussed cold fusion earlier, in the days when the controversy arose. My deep appreciation to you for carefully considering this suggestion. I look forward to discussing the idea further with you, if you feel that it has merit, and of course I hope you will.

Sincerely, Eugene F. Mallove

Exhibit J
Eugene Mallove's Letter to Dr. Stanley Luckhardt
April 29, 1991

My written request to Dr. Luckhardt for clarification and other data was rebuffed.—EFM

Eugene F. Mallove, Sc.D., Chief Science Writer, MIT News Office, Room 5-111, Lecturer in Science Journalism, Department of Humanities

Dr. Stanley C. Luckhardt
Room 36-293

Dear Stan:

Glad that you were able to come to Dr. Fred Mayer's [cold fusion] seminar last week and ask some good questions. It's nice to have an alternate theory to compare with Peter's [Hagelstein's] ideas.

I have been meaning to submit a short note to the *Journal of Fusion Energy*, a comment of sorts about the MIT experiments in the spring of 1989 and where they fit into the big picture. I would mainly be addressing the calorimetry issue and in that regard would want to refer to both your perspective and to that of Dr. Noninski. I realize that there are two pieces of information that it would be helpful, though not essential, for me to have: (A) The precision and assumed accuracy of each of the measuring devices (current, voltage, and temperature) and (B) The plot of the heater power versus time for the light water comparison cell run that corresponds to the D_2O heater power plot presented in the PFC report.

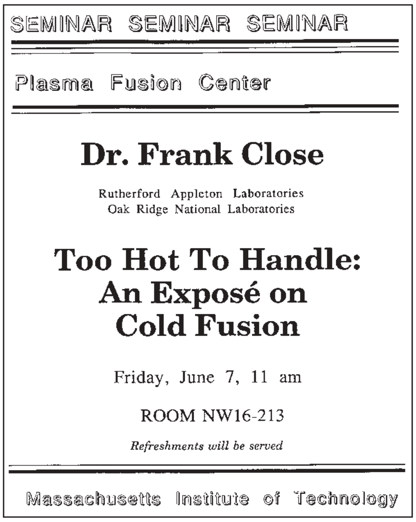
Thanks in advance for your help, and I look forward to sharing with you some of my ideas, once I get them on paper.

Sincerely, Eugene F. Mallove

Exhibit K

Question and Answer Session for Frank Close's talk at MIT Plasma Fusion Center (“Too Hot to Handle: An Exposé on Cold Fusion”), Friday June 7, 1991. (Final interchange, in which the PFC Director Ronald Parker was introduced by Richard Petrasso)

Transcription by Eugene F. Mallove. Bold type sections are of particular interest (bold added by E. Mallove).



Matters came to a head on June 7, 1991, when—unknown to me until the very few days before it occurred—a lecture by a strong critic of cold fusion, Dr. Frank Close of the UK, was scheduled for a Friday seminar at the Plasma Fusion Center. The posters for the talk proclaimed it to be “An Exposé on Cold Fusion”—and indeed, it was just that— a slanderous attack of Fleischmann and Pons! It turned out to be a climactic event in my career and in the history of cold fusion.—EFM

Parker: Looking at the Pons and Fleischmann experiment is a valuable object lesson, you know, regardless of whether there is anything to the field that sort of followed their work. And to me probably the most disturbing comment that was made and left out there was the almost inference that it’s O.K. to drop a data point if your name is Millikan [the physics Nobel laureate]. I don’t think it’s O.K. to drop a data point if your name is Millikan, Parker, Fleischmann, or Pons. That’s the lesson. That is what science is about. We don’t drop data points, we don’t become passionate, you know, about “this has to be right, we have to make that data look this way.” [***Ironically, that is precisely what the MIT PFC did with its data! —EFM***] Science is supposed to be objective, even if it sometimes goes against the grain, and that is what we try to teach here at MIT to our students. Let it come out the way it comes out and don’t mount a big PR campaign, you know, and if it doesn’t fit, then force the data to fit. We’re trying to be dispassionate. That’s what science is about and I hope that’s what students will take out of this whole thing. Regardless of whether or not any of these other experiments which you can mention are right or wrong, let’s look at them one at a time. Let’s try to reproduce them.

We at MIT looked very carefully at Fleischmann and Pons, and this is what we came up with. [If we] think we ought to look at another set of experiments and we think we have expertise, we will. But just let it fall where it lies. We’re not going to come out one way or another until we look at it.

MIT PFC Promotional Brochure

Mallove: Would you consider re-evaluating your own experiment, if I brought in experts to evaluate it? Would you consider that? Because I’ve asked Dr. Luckhardt for several weeks now—and I know he’s not here today. He told me at one point he would provide me with the heater power curve for the light water experiment so that I could ascertain what the heck was going on in that experiment. He then finally ended up saying to me he would not give it to me—or that it would take a week to do it.

Parker: I think, Gene, that what you showed up here earlier is completely a surprise to me. [The Phase II comparison power tests of light water versus heavy water, published and unpublished versions.] We will give you every piece of data we ever took.

Parker: My personal. . .

Mallove: Fine.

Parker: I’ll tell you what my opinion is of that work, because I was part of it. I don’t think it’s worth very much. Alright? And that’s why it’s just published in a tech report. I don’t think it’s worth very much. I think to do calorimetry is one of the hardest things I ever tried to do. I’d rather stick to plasma physics.

Mallove: But, Ron, with all due respect, I agree with you, I agree with you. [that the work was not conclusive]

Parker: When you have an open system is where you can make big errors, where you don’t know the overpotential, the electrode potential, and so on. These things are unknown. I mean it’s really tough **and that’s why I don’t put any stock at all -- you can redraw those curves anyway that you want. I don’t think that data is worth anything.** Now you may be able to find something in it. I did the experiment; I don’t think it’s physics.

Mallove: But what I’ve seen, because I certainly see it from Douglas Morrison [of CERN] and I see it from people like Frank Close and others, that your prestigious laboratory with its excellent resources is being used in some respect as a standard which everyone else is supposed to adhere to. My own personal feeling is that those who have continued beyond May of 1989 to do experiments, have gotten some very significant results that this laboratory and other laboratories at MIT ought to take a look at again, and that’s the only thing that will ultimately clear this up. I don’t agree that passion and PR and so forth should solve this: I think experiment should, but they are not being done here.

Frank Close: Can I say something? It’s one o’clock and we’ve got to go to a luncheon. [inaudible] I think that what Ron just said about moving data points and [inaudible]. Whether this turns out in the long run to be right or wrong is a completely separate issue as against what happened at the time. This really addresses the question of what you were saying to the students. One cannot do science and start just dropping data points because it was convenient for you, changing curves around because you wanted to prove something. If you do, and you’re caught out, that’s how it is and I could not rightly suppress information once it had come my way. If scientists try to hide the fact when they discover that things are being done in the name of science malevolently, then science is going to suffer for it. And if then people who come out and whistle blow get attacked for it, it’s even more disturbing. We saw what happened over many years with the David Baltimore case and how long it did take for that to come out. I don’t think that those sort of things will give science a very good name, if we didn’t address them when they came up.

Petrasso: Thank you very much for coming today.

Fall 1991 (NASW's)* Newsletter ("SW")

SCIENCE WRITER QUITS MIT NEWS OFFICE, CITES COLD-FUSION DISPUTE

by Lee Edson (Reprinted with permission from NASW).
(Freelance writer living in Stamford, CT)

Photo by E. Mallove

"I am convinced at greater than 99% confidence level that cold fusion is real—both the nuclear emanations that have been reported and the excess enthalpy that seems to emerge from various experiments." Thus did Eugene Mallove, chief science writer of the MIT News Office, write to MIT President Charles Vest in April 1991. After detailing new, and as he put it, astounding findings in Russia and India, he decried the lack of experimental work on cold fusion at MIT "after the initial but intense period of experimental assessment in the spring of 1989."

Mallove based his expertise in large part on his research for *Fire from Ice*, an optimistic book on cold fusion published in July by John Wiley & Sons. He also holds a B.S. and M.S. in astronautical engineering from MIT, a Sc.D. in environmental health sciences from Harvard, and has authored several other scientific books.

Urging the MIT president to set up a panel to investigate the status of research on cold fusion, Mallove went on to say, "I do not feel that MIT's interests are best served any longer by unwarranted ignoring of the mounting experimental evidence for cold fusion. It seems to me essential that members of the MIT community reassess experimental findings that are coming from both foreign and domestic laboratories. To do any less would be an abdication of scientific responsibility, not to mention a longer range injury to the reputation of MIT. . ."

By June, Mallove had not heard from MIT and was convinced that the scientific community had closed its mind to cold fusion as a real phenomenon, even if not promising as a source of endless cheap energy. Frustrated and feeling uncomfortable about continuing his role as a spokesman for MIT, NASWer Mallove quit his job in the News Office, announcing his resignation at a public meeting, and submitted a 17-page "J'Accuse" letter to his alma mater. The litany of charges expanded on his earlier note to the president. He accused the university of publishing fudged experimental findings to support MIT's early condemnation of the work of Pons and Fleischmann—a condemnation he charged that helped propel the nation's negative tone toward the Utah scientists.

Mallove went on to blast MIT Professor Ronald Parker, head of the Plasma Fusion Center, for "using" him and the press of– fice in publishing a false press release. In that release Parker had denied that he had ever called Pons and Fleischmann frauds as reported by Nick Tate in the *Boston Herald*. Tate later produced transcripts that showed Parker had indeed used the expression "fraud" on several occasions, and in a classic riposte Parker said that he didn't mean it in connection with the controversial cold fusion findings.

Mallove also charged that the university was seeking to censor his writings by killing a 9,000-word article that he had written for the MIT magazine, *Technology Review*, explaining his views on cold fusion. He claimed the article had been accepted after revision but was later turned down because of the negative comments of reviewers, especially by an MIT physicist who was violently anti-cold fusion. (In a subsequent telephone interview Jonathan Schlefer, the former managing editor of *Technology Review*, who told me he was responsible for rejecting the article, firmly denied Mallove's allegations, saying that his article was too one-sided and not up to snuff.)[Ed. Note: This reconstruction by Schlefer is utterly false—EFM]. Nevertheless, Mallove was paid the full price of $1,000 for the article.

The core of the scientific misconduct alleged by Mallove has to do with calorimetry experiments performed by Professor Parker and his group. In one calorimetry experiment a Pons-Fleischmann electrochemical cell was filled with heavy water and a control cell with ordinary water. The power curves generated were published in the *Journal of Fusion Energy* and came out looking essentially the same, apparently indicating that the heavy-water cell had not produced excess heat as might have been expected if a fusion process were going on. Mallove says that the heavy-water curve was shifted by the experimenters to make it look the same as the ordinary water curve but that actually the heavy-water cell experiment did show excess heat. Parker's explanation is that the shift was made in accordance with conventional data treatment.

All this came at time when MIT was still reeling from the Baltimore-Imanishi-Margot O'Toole furor. Perhaps that is why Mallove's resignation drew only mixed attention from the media. *The Boston Herald*, UPl, the *Chronicle of Higher Education*, and the *Christian Science Monitor* thought it newsworthy, but *The New York Times*, *Science* magazine, and *Nature*, which would normally have covered or at least noted such a dramatic form of professional self-immolation in academia, were notably silent. Interestingly the *Wall Street Journal*, which in July reported a series of new cold fusion findings (or "sightings of the dead" as the physicists regard them), failed to mention Mallove's whistle-blowing departure from MIT.

The MIT administration also did not respond, although Professor Parker said he thought the affair ridiculous and dismissed the alleged new evidence of cold fusion as no evidence at all.

Mallove then accepted an invitation from National Public Radio to air the controversy over WBUR in Boston. The broadcast of 9 August 1991 led off with: "A crisis of confidence in Boston's leading research institution . . .MIT scientists are now being charged with manipulating the media and altering data in an attempt to shoot down the work of the Utah scientists."

Then Mallove's voice cut in: "What went on behind closed doors at my alma mater is so upsetting that I will not rest until the whole matter is given thorough airing. We have a major big science program, hot fusion, which is literally trying to squash cold fusion."

In late August, Mallove pressed Parker and his group for their lab notebooks to allow for an independent check of the calorimetry work in 1989. So far, he says, only an item or two has turned up. Parker insists that it isn't worth the effort to have an assistant generate all the lab data involved. While Mallove interprets this as bad faith, the prevailing view at MIT is that rather than fraud the scientists involved may have conducted poor science that they would not like to expose.

Mallove has now escalated the furor once again by dispatching a registered letter to Mary Rowe, assistant to the president of MIT, requesting a formal inquiry into the misconduct charges. This time *Nature* did report the incident as a cold-fusion tempest at MIT. Much of the betting is that a formal inquiry is unlikely to take place, mainly because it would be hard to prove malicious intent, and even if the data had been improperly handled at a time of high tension at MIT, it would mark only a minor footnote to the now largely discredited work of Pons and Fleischmann.

Is there life beyond the MIT News Office for Mallove? Apparently yes. At 44 he intends to retain his post as a lecturer in science journalism in the humanities department at MIT and he enjoys the thought of no longer daily commuting 60 miles to Cambridge from his home in Bow, New Hampshire. He has a dozen book proposals making the rounds of publishing houses and if none of them works out, he could return, he says, to an early love—the entrepreneurial life. In the 1980s he produced astronomical materials such as sky maps for museums and consulted with aerospace manufacturers like Hughes on the potential of innovative space propulsion systems.

At the moment he has no special plans for cold fusion except to write a sequel to his book—assuming, of course, that there is also a sequel to cold fusion.

Exhibit L
Dr. Mallove's Resignation Letter from the MIT News Office June 7, 1991

My letter of resignation form the MIT News Office was submitted June 7, 1989, two days before my 42nd birthday. It details the constellation of concerns about unethical press manipulation and data manipulation that was the central fact of the MIT PFC's response to cold fusion.—EFM

Eugene F. Mallove, Sc.D., Engineering
Lecturer in Science Journalism, Department of Humanities
Massachusetts Institute of Technology

June 7, 1991

Kenneth Campbell, Director and
Robert DiIorio, Associate Director
MIT News Office, MIT Room 5-111

Dear Ken and Bob:

The time has come to formalize what I have been alluding to these past few weeks. Regrettably, I must tell you that I intend to leave the MIT News Office within this year as soon as I am able to obtain employment elsewhere. Circumstances surrounding the cold fusion controversy and the unfortunate way it has been dealt with at MIT leave me no choice. Furthermore, the appearance of *Fire from Ice*, has already prompted insulting attacks by those negativists—on and off campus—who think that they have a monopoly on scientific wisdom in this area.

I feel increasingly uncomfortable being the ex-officio representative of the tragic and indefensible abrogation of academic standards that has occurred at MIT in this matter. The latter characterization will prompt raised eyebrows, I'm sure, given that in the (erroneous) popular view it is cold fusion researchers who are the exclusive violators of such standards. But this amazement will merely be another manifestation of arrogance toward and misinformation about cold fusion research. Please excuse the length of this letter, which is of the nature of a report, albeit not a comprehensive one, on the treatment of cold fusion at the Institute.

This is a serious matter, not some esoteric quibbling about a peripheral exotic question. The sooner the MIT administration understands this and acts upon it, the better it will be for this cherished place of great dreams, visions, and deeds. I am proud to be an alumnus of MIT, but I am outraged, embarrassed, and amazed at what has happened here. Of course there may well be an open-minded attitude toward cold fusion among a large "silent majority" of students and faculty here. I hope that my book will be able to inform those at the Institute who still are curious about cold fusion. The most visible MIT response to cold fusion so far, however, has been an appalling arrogance and intolerance, combined with actions that have significantly hindered understanding of the phenomenon here and elsewhere. The consequences for MIT could well be devastating when the last "i" is dotted and the last "t" crossed toward proof that cold fusion phenomena exist. The shield that falsely protects the Institute now is the milieu of skepticism that surrounds cold fusion in certain prominent publications and societies, but that skepticism is doomed to collapse like a house of cards. It is only a matter of time, and it may be sooner than many believe. Ironically, this is a false shield of skepticism run amok that some researchers within MIT have labored mightily to help build.

Frankly, the direct evidence for nuclear effects in many cold fusion experiments is already overwhelming. If and when—more likely I would say, when—the measurement of real excess power production is resolved and proved to come from heretofore unknown nuclear processes, the MIT response to cold fusion will be judged most severely; and that negative assessment will be completely correct unless an immediate and dramatic change of course occurs. If cold fusion ultimately proves to be a utilitarian power source, it will be very difficult for MIT to recover its credibility.

Some of my intolerant critics will probably hasten to suggest that it is I who will suffer the consequences of a too credulous view of cold fusion. On the contrary, I will never be embarrassed by my views, first because they have been honestly reached; I started with deep skepticism, went back and forth from belief to disbelief many times, and arrived at what is to me an inescapable conclusion. Second, even were I to be proved wrong—an unlikely event—I have taken great pains to spell out precisely the required circumstances for the collapse of the multiple channels of experimental evidence that would have to occur to prove that cold fusion is an illusion. If that unbelievable circumstance should arise, so be it, but I wouldn't recommend waiting for it.

I know that there are many other dimensions of my job in the News Office that present no apparent conflict. By right, there should have been no conflict in the matter of cold fusion either —even though I have written a book on the subject that takes a contrary view to widely held skeptical opinions. After all, isn't diversity in scientific viewpoint supposed to be the driver of progress at a great research university? And I do have scientific and engineering training and experience, and am presently a Lecturer in Science Journalism in the Department of Humanities. These credentials certainly qualify me to discuss this subject as a peer of those who decry it. But cold fusion is no ordinary topic. Regrettably, it has not been possible to discuss it here as one would, for example, relativistic space travel or "child universes"—concepts that are hardly "accepted," but which apparently do not cause the visceral reaction to their mere mention that cold fusion does. As Dr. James McBreen of Brookhaven National Laboratory has said, "A lot of people undergo personality changes when discussing this topic."

Indifference, Intolerance, Ridicule, Censorship

On 12 April [1991] I wrote to President Vest about cold fusion, and sent a copy of the letter to former MIT president Gray (see attached). The letter was a summary of where I thought matters stood now in the field, including the reports of the recently announced Soviet work and the well-known Japanese involvement. I asked that Dr. Vest consider appointing a panel to assess the field in light of many new developments. I presume he has taken the matter under advisement, but I find it distressing that no hint of a response has come on this earnest appeal. I know that our chief executive is very busy, but this is an important matter. It would not surprise me at all, though, if that letter were being disparaged by high-level negativists here who are legion.

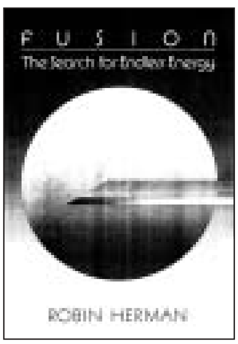
Much more disturbing is the stark reality that since the spring of 1989, no experimental work on cold fusion has occurred at MIT, an indisputable message of indifference. Thus we have the institutional response, in effect, "It's dead." One of the world's greatest scientific institutions has not actively participated in its splendid laboratories in getting to the bottom of a possible new scientific phenomenon. Incidentally, even if "cold fusion" were not to be a revolutionary nuclear process, there is broad agreement even among skeptics that some unusual thermal effects have been seen in palladium-platinum heavy water cells. So where is the scientific curiosity among our resident skeptics to put that final nail in the excess power issue by doing experiments to discover what is causing these effects—possibly interesting and useful in their own right even if not nuclear? Are our resident skeptics waiting for government funding? No, they don't really want to be bothered with this research. Even if they were inspired to do it, they wouldn't get the money, of course. The skeptics who influence and control DoE's purse strings have made sure of that. What follows is the untenable and unscientific position of DOE, as stated by Secretary of Energy Admiral James Watkins in a recent speech (May 6, 1991). This attitude came about not of course exclusively from, but in no small way through the efforts of influential members of the MIT community:

"Remember cold fusion? Front page news for weeks on end. Will it work or won't it? Is it the key to our energy freedom, or a hyped-up hoax? In the end, it was neither. Just bad science. But how was the public to form an opinion when the scientific community itself and the reporters who covered the story were unable to persuasively lay out the scientific merits of the issue."

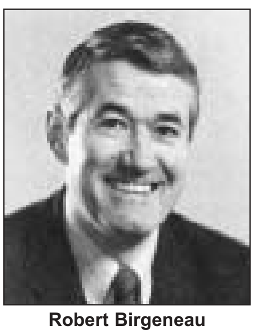
Sec. of Energy Admiral Watkins

"But there was damage done here too. Two members of the scientific community made everyone in white lab coats look fraudulent. Congress held hearings and railed against my agency and others for not pouring millions into cold fusion, in the process shedding no light on the real underlying issues of energy production and use. And at the same time, they cut my department's budget for real fusion energy by $50 million."

I am aware of some relatively quiet cold fusion work done by staff members of MIT, that was conducted discretely off campus. There were also a few efforts carried out at Lincoln Laboratory. Some anomalies were seen, and interest there thankfully has not died completely. [Ed. Note: Definite excess energy later *was* observed at Lincoln Labs, but the results have been withheld from the public.—EFM] But there has been no other significant experimental work, as far as I am aware. This is disappointing, but not surprising. Professor Ronald R. Parker, one of the two professors who led the limited MIT cold fusion experimental effort from late March to late May, was quoted in his letter to author Robin Herman (*Fusion: The Search for Endless Energy*, 1990). Her book has a concluding chapter mocking cold fusion in which Professor Parker is quoted. "Unfortunately, a lot of time and effort has been wasted due to this blunder." I was amazed to discover that when this statement was made (May 11, 1989), the experiments to explore cold fusion at MIT had not even been completed.



Though there has been no laboratory work since the spring of 1989, cold fusion has been the butt of jokes and the focus of merriment at MIT. At the Plasma Fusion Center in the summer of 1989, a "Wake for Cold Fusion" party was held. One of the MIT reviewers for a major publication [*Nature*] that has blocked numerous attempts by researchers with positive cold fusion results to publish them, once was known in this area by an editor of that publication as, "Rambo." [This was Dr. Richard Petrasso of the MIT PFC] The head of the Physics Department [Prof. Robert Birgeneau] remarked with humor and pride in the summer of 1989 department newsletter, "I should like to note, however, that none of our faculty contributed to the confusion surrounding 'cold fusion.'"



Robert Birgeneau

There have been other remarkable comments. "Garbage" was how one MIT physics professor [Prof. Martin Deutsch] bluntly characterized cold fusion work to a prominent science magazine [*Science News*] in 1989. One of the researchers who was on the Plasma Fusion Center / Chemistry Department team evaluating cold fusion [Prof. Ronald Ballinger] told me five months ago that he thought Pons and Fleischmann were "crooks who should be put in jail." Another team member, Dr. Richard Petrasso, was quoted recently on the front page of the *New York Times* (March 17): "I was convinced for a while it was absolute fraud. Now I've softened. They [Pons and Fleischmann] probably believed in what they were doing. But how they represented it was a clear violation of how science should be done." Apparently another skeptical physicist could not sanction that severe charge. In a letter to the *New York Times* printed April 9, 1991, Yale physicist Robert Kemp Adair wrote: "Last November, I served on a a committee that met with Dr. Pons in a review of the National Cold Fusion Institute at the University of Utah. Though I concluded that he and Dr. Fleischmann had seen no cold fusion, I am confident they reported no invented data and committed no egregious breach of scientific ethics." Unfortunately, the insinuation of fraud applied to these researchers was given early impetus here.

Recently, the attacks took an uglier turn both from within and from outside MIT. Physicists Dr. Frederick Mayer and Dr. John Reitz of Ann Arbor, Michigan—both with distinguished scientific careers—were invited to MIT by Professors Peter Hagelstein and Lawrence Lidsky—to conduct a scientific seminar about their theory of "cold fusion" that appeared in their "Nuclear Energy Release in Metals" paper, which has been published in *Fusion Technology*. The seminar was advertised in the usual channels on campus. To my knowledge, since the affair began it was the first technical seminar on cold fusion open to the general public at MIT that actually cast the phenomenon in a positive light. The presentation was informative and was conducted with dignity. I was proud to have helped facilitate this meeting—a worthy effort, I thought, to clear the air on the topic. In advance, Dr. Mayer had expressed a fear that he would be scurrilously attacked, as opposed to being challenged with reasoned arguments—as he hoped he would be. Since Dr. Mayer is an acquaintance of President Vest (their sons are friends too, and Dr. Mayer was at one time a soccer coach of President Vest's son's team), that would have been especially offensive.

Fortunately, Dr. Mayer was not attacked at the seminar because those most likely to offend didn't show up; the critics held their fire until afterwards. Apparently Dr. Robert L. Park of the American Physical Society's Washington office took offense not only at the theory presented by these scientists, but at their press conference in the Boston Sheraton Hotel the day after the seminar at MIT. (Only three members of the media attended.) Park, who has mocked cold fusion from the beginning, much as his weekly electronic mail column, "What's New," ridicules those who study the possible effects of low frequency electromagnetic fields on biological systems, was also upset that I had provided nominal assistance (on my own time) for the Reitz-Mayer press conference.

What was the nature of this aid? It was merely to fax a news release (which they had prepared) and their technical paper to a handful of media outlets; I also made a few phone calls, and was contacted by people who had heard about the press conference, including Park's secretary. That's how he was informed to write his diatribes. My news judgement then and now was that the seminar at MIT reflected well on the Institute; it showed that we are at least nominally in the business of openly discussing even controversial scientific matters. Also, by facilitating news stories that reporters apparently found interesting (such as William Broad of the *New York Times*), the News Office maintained its deserved reputation as a useful information outlet. I would make that same judgement again about any other topic. Occasionally some interesting figure from outside MIT arrives to make a controversial statement, *e.g.* scientist Dr. James Lovelock (the Gaia hypothesis), and our office doesn't hesitate to "promote" them to the press.

As a result of these highly appropriate scientific events, these are the "gifts" we received from Park. They came in two successive weeks of his widely circulated column, which is signed "Robert L. Park, The American Physical Society," giving the impression that his is an official Society view, even though it is not. Not once did Park mention the scientific seminar at MIT. He preferred what he evidently considered to be the pejorative "press conference." Here is the 1st message:

Dr. Robert L. Park

> "INCREDIBLE COINCIDENCE: SIMULTANEOUS BOSTON PRESS CONFERENCE! At the very instant that Mills was revealing his startling new findings in Lancaster, two-well known physicists, Fred Mayer and John Reitz, were in Boston announcing their new cold fusion theory, with the help of the MIT press office. Their paper, which will also be published by *Fusion Technology*, involves—are you ready?—tiny hydrogen atoms! Except they call them 'hydrons' and attribute them to 'continuum bound state resonances.' Mayer expects prototype power generating systems in about five years. Neither Mayer or Reitz is associated in any way with MIT. How then did the MIT press office get involved? Very good question?" (April 26, 1991)

Dr. Fred Mayer

Professor Ronald R. Parker of the PFC chose not to bring this piece of slander and omission directly to my attention. Evidently he agreed with its tenor and was stirred up about the Mayer-Reitz press conference. Instead, he faxed a copy of it to someone in the MIT News Office, who has little familiarity with the scientific issues of cold fusion and with whom you know I have had clashes.

In the *Washington Post* on Friday 26 April, Park made this statement about Mayer after describing his theory as "wacky": "There is no reason to doubt the sincerity of the two scientists involved, who are respected and well known as science managers [note the put-down "managers"—the two are practicing physicists!] But there are also sincere scientists who believe in psychokinesis, flying saucers, creationism, and the Chicago Cubs."

Continuing his coordinated attack, Park made this insulting statement to the Chronicle of Higher Education--his assessment of the Mayer-Reitz theory: "It is proof again that a degree in science is not an inoculation against foolishness and mendacity. It's just got to be wrong."

The following week, Park attacked me in his column again, this time directly:

> "MIT FUSION FLAKE FLACKS NEW BOOK! TINY LITTLE HYDROGEN ATOMS called 'hydrons,' explain cold fusion, according to two Ann Arbor physicists who held a press conference in Boston last week. Why was the press conference in Boston—and why was the MIT press office helping? The answer seems to be that an MIT science writer is promoting his new book, which contends that the evidence for cold fusion is persuasive. He predicts that in the history of science Pons and Fleischmann will be viewed as heroes."

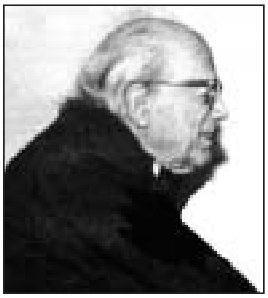
The major falsehood of this nearly libelous statement: at no time during the Mayer-Reitz visit to Cambridge-Boston was *Fire from Ice* mentioned publicly, in any context. What a stupid way for me to "flack" a book! Park comes closer than anyone I know to wearing well the term: "scientific bigot." It was he who in March 1990 described the First Annual Conference on Cold Fusion in Salt Lake City as a "seance of true believers"—without having attended the meeting or learned what went on.

Now, despite their outrageous intent to defame and ridicule, one could dismiss the mouthings and electronic missiles of Robert Park as the pathetic prejudice of an aging physicist, who may fear that his world view is crumbling—as the history of science has shown happens time and again. Perhaps all wisdom does not reside in the APS, Park may be thinking. However, it is unfortunately not so easy to dismiss the outright censorship of one's writings, particularly when that censorship is influenced by another physicist, this one at MIT.

Over many months I had prepared a lengthy (9,000 word) feature article on cold fusion that was to appear in *Technology Review* at the end of this summer. The piece recounted the essentials of the cold fusion story (see attached draft), by presenting the arguments on both sides, though it did come to the general conclusion that cold fusion might well be real, given the accumulating evidence. My article had passed through a major revision cycle, in which I had carefully adhered to the wishes of editor Jonathan Schlefer. In mid to late April the word came back from Jonathan that the revision seemed to be fine—not to worry. Someone else within *Technology Review* even told me it was being considered as a cover story. It was considered that good.

Imagine my shock on May 9, when I received a call from Schlefer telling me that the article was not going to be published. He offered no suggestion of any changes that could make it acceptable— the usual option when an editor has some new problem with a piece, particularly after he has professed to find it basically satisfactory. Schlefer told me that it had been sent out for review to three technical people, each of whom allegedly had some problems with it, though these problems were not clearly indicated nor were they discussed. There was a blanket statement that each reviewer had found the piece too positive. Further investigation on my part determined that, except in one case, this was far from true.

A senior and respected MIT physicist seems to have been mainly responsible for scuttling the article [Prof. Herman Feshbach]. I called to ask him what he had found objectionable. Despite my distress that the article would not appear in *Technology Review*, throughout the telephone conversation I was calm and polite. His evident anger increased through the call. He began by saying that fundamentally my article was "not a piece



Prof. Herman Feshbach
MIT News Office

of journalism." He described it as more of an "advocacy piece." This assessment struck me as very peculiar for three reasons: 1.) I had deliberately balanced both sides of the controversy as closely as possible, even as I interjected my own view by suggesting that it was difficult to explain away all the phenomena—that some unexplained cold fusion phenomena seemed truly spectacular; 2.) *Technology Review* is a forum for advocacy—sometimes not even advocacy with which many readers will identify. Many if not most pieces in *Technology Review* are advocacy pieces, some quite markedly so—like the cover story that suggested that the U.S. used atomic bombs against Japan to demonstrate a threat against the Soviet Union; like the recent cover that all but ratified the threat of global warming (even though the question is still being debated); or like the most recent cover story that suggests that the motivating force for the war in the Gulf was the Pentagon's need to check out a new military paradigm; 3.) Finally, it became completely clear in the remaining conversation that an advocacy piece against cold fusion would very much have pleased this professor.

I was astounded that this professor had not learned that arguments "from authority" don't hold water. He ridiculed me citing "that Bulgarian chemist." He did not use the chemist's name; it happens to be Dr. Vesco Noninski, a multi-talented electrochemist from Bulgaria who is fluent in many languages and who is visiting the United States. Noninski, apart from his having carried out in Bulgaria very novel heat measuring experiments on cold fusion—ones that demonstrated excess power—has published and prepared very interesting analyses of the MIT and Caltech cold fusion calorimetry experiments. These indicate the possibility that these teams may have measured excess power, but didn't realize it because of improper analysis of their own data. These analyses are very convincing. They don't prove, of course, that cold fusion is real, but they do indicate that not all negative result experiments may be truly null. Furthermore, Noninski is far from an "advocate" of cold fusion. He maintains that he does not know what the phenomenon is, but insists that careful calorimetric (heat measuring) analyses must be carried out even to begin to discuss "cold fusion." He has considered the error source aspects of cold fusion calorimetry more than anyone I have encountered in the field.

So what did our physicist non-calorimetry expert say about Noninski? He challenged me angrily, "You would trust this Bulgarian chemist over what Mark Wrighton said [about cold fusion]?" I didn't answer him on that point, but yes, I would, because Noninski has put far more time into the problem (see account below). The physicist was not saying "Dr. Noninski"; I kept hearing "this Bulgarian." And MIT is supposed to stand for cultural diversity? This is incredible! I could hardly believe what I was hearing from this man whom I had admired and respected. McBreen of Brookhaven was right!

The physicist [Feshbach] told me that he had "50 years of experience in nuclear physics and I know what's possible and what's not." I brought up several other names of physicists who had initially been skeptical of cold fusion, but who had done experiments of their own that had convinced them something was going on. One was at Los Alamos National Laboratory, Dr. Howard Menlove. Almost before I could get a researcher's name out of my mouth, I was cut off by an angry objection from the physicist, generally of the character, "I don't know who he is!" Finally, I tried to suggest that it might be a good idea if my critic examined a recent technical review of the entire cold fusion field that has been prepared by physicist Dr. M. Srinivasan of the Bhabha Atomic Research Center, one of India's premier nuclear facilities. Srinivasan is the head of BARC's Neutron Physics Division, but the physicist would hear no more about him. He was getting too angry to stay on the line much longer. He blurted out, "I don't want to see any more evidence! I think it's a bunch of junk and I don't want to have anything further to do with it." With that, the conversation ended.

This arrogance is so offensive that it can hardly be suffered, and it need not be. That is one of the reasons I'm leaving the News Office. I'm profoundly embarrassed that we have such close-mindedness here on scientific issues. I cannot represent such attitudes even in an ex-officio capacity. Furthermore, I intend to attack them, not only after I leave this office, but in my remaining time here.

An Unfortunate News Release

MIT always speaks the truth in press releases emanating from the News Office, right? Wrong—not always, sorry to say. On May 1, 1989, the News Office issued a press release, a copy of which is attached; it was prepared by me in telephone consultation with Professor Ronald Parker of the PFC at his request. The press release was crafted to deny statements that *Boston Herald* reporter Nick Tate had attributed to Professor Parker, which attacked Pons and Fleischmann in the manner denied in the press release. When I prepared that release, and for more than a year afterward, I believed that the statement that our office had issued was valid, though I had some doubts. I simply trusted what was being asked of me in preparing the release. I trusted too much—another reason for my leaving the News Office. I do not wish to be put in that compromised position again. My own integrity is too important.

After having listened to the actual tape of the interview that Nick Tate at long last provided me (in July 1990), I believe that his story's characterization of what Professor Parker and Professor Ballinger had said in their conversation with Tate the week before, is substantially correct. It was a well-orchestrated attempt to condemn the work of Pons and Fleischmann, not merely to criticize it technically. In my view, virtually any other competent reporter would have written essentially the same story that Tate wrote. In fact, the partial transcript published by the *Herald* on May 2, 1989, should have been enough to convince me of the soundness of Tate's story, had I not been told that these remarks were "out of context." Parker and Ballinger may deny it, but I know what I heard on that tape, which I was deeply disturbed to listen to. I have been wrestling with the knowledge of what it held, and this has been cutting me up inside ever since first hearing it.

I have a doctorate in engineering, and I came to my career in science writing at no small sacrifice in compensation, simply because I enjoy writing more and I believed that it would give me greater reach. It definitely has. I also have been proud to represent my alma mater and its fine research, including the many outstanding accomplishments in magnetic confinement fusion at the Plasma Fusion Center. Everyone, including Professor Ballinger and Professor Parker, knows that. I had, and at some level still do have affection for the people at the Plasma Fusion Center. They are personable and they have a good cause, which I am solidly behind—as I spell out clearly in *Fire from Ice*, even as I strongly believe in the prospect of cold fusion. Hot fusion research has many merits that go far beyond the distant and admirable goal of commercial power reactors, and these may still be necessary; we don't know enough to say no yet. But I am absolutely outraged to have played a role in this public deception that involved such objectionable language and accusations. I hope that an instance like this never occurs again for any member of the News Office. I officially withdraw my approval of the MIT news release in response to the *Herald* story.

More important than any feelings I have about being deceived was the effect that the intended disparagement of Pons and Fleischmann's work had on the future course of cold fusion research. Yes, others would have jumped in anyway to assail Pons and Fleischmann; the chorus might have been equally loud. But sending out the kind of calculated, negative message against Pons and Fleischmann was wrong. To add insult to injury, members of the PFC staff act as though Pons and Fleischmann were the only ones capable of what they claim to be irregularities in research. Unfortunately, their own house is not in such fine order (see below).

Further actions were taken by various members of the PFC to assist in discrediting Pons and Fleischmann. In particular, there was clearly significant cooperation with physicist Frank Close, who has written in my view a highly negative and extremely imbalanced account of the cold fusion story—his book, *Too Hot to Handle*. It focuses on numerous alleged departures in scientific ethics on the part of Fleischmann and Pons, while saying virtually nothing about or belittling all subsequent experimental work that provides supporting evidence for cold fusion, *e.g.* physicist Howard Menlove's neutron burst detection work at Los Alamos National Laboratory is not discussed. The dedication to Close's book reads precisely: "To the xxxxx from MIT and the friends and colleagues who shared the spring of 1989." [The "xxxxx" was Richard Petrasso] Incidentally, Close is uniformly disparaging of the role of the media in the cold fusion saga, paralleling the attitude of some on the PFC staff (and Park of the APS) who continue to get upset whenever cold fusion is given some credence in the media. When cold fusion is disparaged, it's fine with them. Close shamelessly brings up several times the now thoroughly discredited accusation by another journalist that fraudulent adulteration of experimental cells with radioactive tritium occurred at Texas A&M University. An interesting aside: Close is so reckless in his treatment that he was able to confuse the *Boston Globe* with the *Boston Herald*, which confusion is sure to please neither paper.

Questions About the MIT Cold Fusion Calorimetry Experiments

The MIT Plasma Fusion Center/Chemistry Department experimental contribution to cold fusion research was conducted from late March 1989 through late May 1989. The search for evidence of cold fusion in heavy water cells and comparisons with light water cells included attempts to find various nuclear products, as well as indications of excess power. The final report of the sixteen-member team appeared as Plasma Fusion Center Report, PFC/JA-89-34, dated July, 1989: "Measurement and Analysis of Neutron and Gamma Ray Emission Rates, Other Fusion Products, and Power in Electrochemical Cells Having Pd Cathodes." The authors are listed as: D. Albagli,[1] R. Ballinger,[2,3] V. Cammarata,[1] X. Chen, R. Crooks,[1] C. Fiore, M. Gaudreau, I. Hwang,[2,3] C.K. Li, P. Linsay, S. Luckhardt, R.R. Parker, R. Petrasso, M. Schloh,[1] K. Wenzel, and M. Wrighton[1] [1 = Dept. of Chemistry, 2 = Department of Nuclear Engineering, and 3 =Department of Materials Science and Engineering]. The abstract to the report concludes, "Within estimated levels of accuracy, no excess power output or any other evidence of fusion products was detected." Later this report was reprinted in essentially the same form in the *Journal of Fusion Energy*, June 1990, Vol.9, No.2, pp.133-148.

Here I wish to comment not on the nuclear product measurements discussed in this report and subsequent paper, but on the power measurements. In fact, one experiment in the series of experimental cells reported in this work is of particular interest, because it is the only case in which graphs of the raw data that form the power measurements are shown. This so-called "Phase-II" calorimeter experiment compared the power production of a light water control cell and a heavy water cell. The record of the controversy clearly shows that at that time skeptical scientists were placing great emphasis on the need to find differences in the power production between light water cells and heavy water cells. The presumption by many at the time and subsequently was that if a heavy water cell produced excess power and a light water cell of identical form did not, then there was more reason to investigate further the possibility that unknown nuclear reactions might be occurring.

A Possibly Incorrect Power Analysis

Regardless of how the data from this experiment are interpreted, I am firmly of the opinion, as are many others, that it is not possible to use the latter MIT experiment, or even the more crude series discussed earlier in the PFC paper, to conclusively prove anything one way or another about the reality of cold fusion. It is simply far too limited a check. Some other laboratories that have obtained sporadic positive results for excess power have generally spent much more time in their trials and tried a greater series of electrodes, a possible requirement sometimes to pick up the anomalous thermal effects.

However, there has been a technical analysis of this MIT experiment, which leads me to believe that at least in the heavy water case in the Phase-II experiment, there is evidence of excess power production. The evidence of excess power production based on this analysis is to appear in a forthcoming issue of *Fusion Technology*. In that paper, the power density produced in the palladium electrode rises from zero at 20 hours into the test to close to 2 watts per cubic centimeter at 100 hours.

The analysis on which this conclusion is based differs from the MIT analysis because that analysis has introduced an adjustment to the raw data whose validity remains uncertain. (This adjustment is a subtraction from the raw data of a linear fit to the noisy and declining heater power, with no quantitative assessment as to why this should be done.) In fact, if this adjustment is performed, for the reasons suggested in the MIT paper, it is possible to get a null result for the excess power measurement in every case. It is surprising that this was done, because in the methodology of the MIT calorimeter, declining heater power should have suggested the presence of an unknown heat source. The explanation in the paper does not seem satisfactory.

Without question, more work needs to be done to decide what analysis of the Phase-II experiment is appropriate. Other than that, the result cannot be used to draw any firm conclusions about excess power.

An Unwarranted Curve Shift

There is another aspect of the Phase-II excess power experiment that is troubling. Even if the MIT thermal analysis (which sanctions the forementioned subtracting out of the heater power) is presumed to be correct, data provided to me in the summer of 1989 show that there was likely to have been a difference between the heavy water cell and the light water cell; the heavy water water cell seems to be evolving excess power, while the light water cell does not—exactly what many wanted to see at the time as an indication of anomalous nuclear effects.

Attached are four figures. The first two are from the published MIT paper and show the excess power produced by the Phase-II heavy water and light water cells. The raw signal was noisy and was averaged in hour-long intervals to produce the data (black dots) seen in these figures. The results rise and fall above the zero excess power line, and there is nothing that leaps out from this data comparison to suggest that excess power is being produced in the heavy water cell and not in the light water cell. It looks as though both excess power plots are about equally noisy. This data that form these curves was prepared at least as early as July 13, 1989, because I was given a draft article by the PFC that bears that date. [Ed. Note: See page 74.]

On the other hand, I also was given the processed but unaveraged, and hence more noisy, data that went into forming these published curves. These data appear in the two other attached figures. The figures are copies of graphs that appear in another PFC draft report to me on calorimetry, dated July 10, 1989, three days before the draft with the averaged data. The light water graph oscillates above and below the zero excess power line (which I have introduced as a dotted line to make the comparison more clear), with no obvious bias above or below the zero line. There appear to be cyclic (24 hour?) variations in the presented excess power, but it is not clear what these are from. The heavy water curve by contrast is dominantly above the zero line, indicating the strong possibility of a residual excess power (even though the magnitude of the variation may be below the stated sensitivity, 40 milliwatts). The two curves are simply quite different. There could be something like a few tens of milliwatts excess power here, on average, as one PFC researcher agreed there could be. For this 0.1 centimeter diameter electrode, 9 cm long, 20 milliwatts would translate to excess power of 0.28 watts per cubic centimeter.

So why do we see no evidence of this possible excess power in the graphs that are in the final report and the published paper? The inescapable answer seems to be that the averaged data for the heavy water was moved down an arbitrary amount so that it now has more the appearance of the null result in the case of the light water averaged data. Interestingly, the light water averaged data seem to be consistent in level with the corresponding curve of raw processed data, that is, it has not been moved down.

I am planning to prepare an article for *Fusion Technology* that will address some of these data analysis issues.

Lessons

The recent turn of events in the David Baltimore-Imanishi Kari-Margot O'Toole affair offers some lessons for MIT on the matter of cold fusion. This is brought home most effectively by Dr. Baltimore's recent apology to Dr. O'Toole. As quoted in *The Tech* (May 5, 1991), Baltimore said, "I recognize that I may well have been blinded to the full implications of the mounting evidence by an excess of trust, and I have learned from this experience that one must temper trust with a healthy dose of skepticism. This entire episode has reminded me of the importance of humility in the face of scientific data." Clearly many MIT scientists who have recklessly attacked honest efforts to come to grips with a possible new phenomenon have lacked "humility in the face of scientific data."

Another pertinent comment was recently made by Professor William F. Schreiber of Electrical Engineering and Computer Science. Writing on the Baltimore affair in the MIT *Faculty Newsletter* (April 1991), he said: "A name on a paper implies responsibility for its contents. We certainly ought not to appear as authors of work we have not watched carefully enough to know whether or not it is correct."

Employment Prospects

[*Editor's Note: non-relevant sections of this part of resignation letter omitted for brevity*.]

. . .Working at the MIT News Office as Chief Science Writer has been a great privilege and an enlarging experience for me. The vistas that have opened up are immense and the talented people and friends I have come to know are many. I wish that the MIT community had been able to react with less acrimony and divisiveness in the matter of cold fusion. I will not reconsider my decision to leave the News Office unless that situation changes radically, something I do not foresee happening soon. But I am deeply grateful to both of you for having selected me in the summer of 1987 to fill the important role of science writer, and I appreciate that you have always respected my abilities and sought my perspectives. But circumstances dictate a moving on. Whatever may transpire, I hope to stay in touch with you and perhaps even work with you in some new capacity in the future.

Science Reporting Suggestions

[*Editor's Note: non-relevant sections of this part of resignation letter omitted for brevity*.]

. . .Difficult as some of these matters are to hear about, I hope this airing of views has been helpful to you and will lead to beneficial changes within MIT. You have been great people to work with. (Even though this letter is being given to you today, it was written in nearly its present form on May 24, 1991).

Sincerely, Eugene F. Mallove

Exhibit M
Eugene Mallove's Formal Request for MIT PFC Raw Data
June 14, 1991

Following my resignation from the New Office, I attempted, in vain, to get the data that Prof. Parker had promised me at the public forum on June:

To: Professor Ronald Parker, Director
Plasma Fusion Center, MIT

From: Dr. Eugene F. Mallove, Bow, New Hampshire
Date: June 14, 1991
Re: Data Required for Further Evaluation of the MIT Cold Fusion Calorimetry Experiment

In response to your offer to provide data that I might request concerning the cold fusion calorimetry experiments carried out in the spring of 1989 at the PFC, I would appreciate receiving the following items:

(1) The unpublished Phase-II experiment heater power curve for the H_2O case, corresponding to the D_2O heater power curve that was published.

(2) Copies of all laboratory notebook pages relating to the PFC calorimetry experiments on cold fusion, both Phase-I and Phase-II.

(3) An explanation of why the hour-interval-average excess power curve for the case of the D_2O Phase-II experiment is centered around the zero excess power level, when the processed data (before time- averaging) on Dr. Luckhardt's memo of July 10, 1989 are almost entirely above the zero excess power line. A memo dated July 13, 1989 is where this apparent change occurs, and that is the graph that was published.

(4) An exact data-processing and mathematical description of how the excess heater power curves were arrived at from raw experimental measurements.

(5) Calculations, if any, that provide a thermal analysis for heat

flow out of the top of the cell, including the glass tubes, etc., which touch the cell solution.

My sincere thanks in advance for any information that you might provide on your experiments. Some of my colleagues may be interested in the nuclear products data, but I am concerned only with the heat measurements.

Sincerely, Eugene F. Mallove

Exhibit N
Eugene Mallove's Letter to Professor Parker
July 30, 1991

The data and notebooks promised and requested were still not forthcoming from Prof. Parker as of July 30, so I sent him a reminder—not really expecting to get satisfaction from this stonewaller.

Professor Ronald Parker, Director
MIT Plasma Fusion Center, Room NW16-288

Dear Professor Parker:

It is surprising and distressing that six weeks after my written request to you and your colleagues for data and information that you publicly promised to provide about the PFC cold fusion experiments, not a single requested item has been sent to me. I hope that you will remedy this situation very soon or at least explain the reasons for the delay. I have attached a copy of the fax to you of 14 June 1991, which lists the needed information.

Sincerely, Eugene F. Mallove

cc: Kenneth Campbell, MIT News Office

Exhibit O
Prof. Ronald Parker's Letter to Eugene Mallove
August 8, 1991

By a "miraculous coincidence" a letter from Parker arrived by fax at my home on August 8, 1991, the very day before the Broadcast of the August 9, 1991 WBUR radio program by David Baron concerning the cold fusion furor at MIT and my resignation (see Exhibit P). Of course, since Parker had been interviewed for this program, he knew it would be broadcast, though he could not know how revealing it would be of his various perfidies against cold fusion. However, no data—on request since June 14, 1991—accompanied Parker's fax. This letter is an insult to the intelligence of any scientist who thinks about it. Here we have Parker post-experiment reconstructing the objective of the PFC Phase-II calorimetry so that it coincides with his hoped-for outcome, namely a null result!

PLASMA FUSION CENTER
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
Cambridge, Massachusetts 02139
Ronald R. Parker, Director

Dr. Eugene F. Mallove, Bow, NH

Dear Gene:

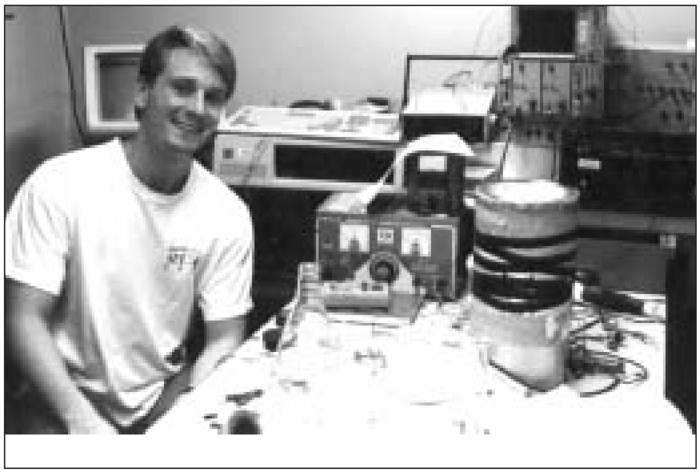
Regarding the specific information that you requested in your June 14, 1991 fax, I will attempt to respond to your point (3), the same point which you raised after the seminar by Frank Close, and, as I understand it, the same point you raised with Stan Luckhardt who gave you the same answer as I will give you here.

As the calorimetry experiments progressed, electrolyte evaporated causing a decrease in thermal conductivity of the system to the external world and a concomitant decrease in heater power required to sustain constant temperature. The difference in the two curves corresponds to two different ways of accounting for this systematic baseline drift. In one, the drift was fitted with a somewhat arbitrary linear function, and the data in Figs. 4b and 5b of the 7/10/89 draft was produced after subtraction. In the other, the drift was fitted with a different linear function, this time a least-squares fit, and the data appearing in the final version of the paper were produced. The difference in the two results is an indication of the error intrinsic in the measurement. The implicit assumption was that we were looking for a fast turn-on of the anomalous heat production and so it was legitimate to subtract out a slow baseline drift caused by depletion of the electrolyte. Whether this is a correct assumption is arguable, but in any event the main conclusions stand: We detected no significant difference between H_2O and D_2O, and in both cases any excess power would have been less than 79 milliwatts, the level claimed for a similar experiment by the Utah group. Our paper estimates the uncertainty of calorimetry measurement as 40 mW, and so you are free to posit an excess heat less than this level it you wish.

As for the other points raised in your fax, I believe that Stan has been extremely forthcoming in discussing them with you, and would be willing to assist you further. However, he has major responsibility for several PFC projects and I cannot allow him to take time away from, them for this purpose. I suggest that you negotiate directly with him to see what arrangements could be made.

Sincerely, Ronald R. Parker

cc: Ken Campbell, MIT News Office



MIT Aero/Astro student Ray Conley, works on his cold fusion project in the Dept. of Aeronautics and Astronautics. His patent application was attacked by the USPTO, in part by the citation of the "negative" MIT PFC cold fusion experiment of 1989 (See *Infinite Energy*, No.11).

Photo E. Mallove

Exhibit P Transcript of WBUR (90.9 FM)
Radio Broadcast,
Friday, August 9, 1991, 5:50 a.m. and 7:50 a.m., Boston, MA.
[WBUR is a National Public Radio affiliate station.]

In disgust with all that had happened against cold fusion, I resigned from MIT on June 7, 1991. Later that summer, August 9, after I battled with the Plasma Fusion Center to get data that Parker had promised me on June 7 (see Exhibits K, M, and N), science journalist David Baron broadcast over Boston's National Public Radio affiliate, WBUR, a program that encapsulated the controversy at MIT up until then.—EFM

A crisis of confidence in Boston's leading research institution, details just ahead here on 90.9 WBUR Boston at Boston University. I'm Jordan Weinstein. . . [weather report and lead-in music]

Announcer: One of the Massachusetts Institute of Technology's top spokesmen on scientific matters has resigned, charging researchers at the school with misconduct. The dispute centers around events of two years ago during the flurry of activity stemming from claims of cold fusion by researchers at the University of Utah. MIT scientists are now being charged with manipulating the media and altering data in an attempt to shoot down the work of the Utah scientists. In our weekly segment on science and health, WBUR's David Baron reports:

David Baron
WBUR Science Broadcaster
Boston University Photo Services

Baron: For almost four years, Gene Mallove represented MIT to the world. As chief science writer for the Institute, Mallove produced press releases and encouraged reporters to write stories about the school. With a bachelors and masters degree from MIT and several books to his credit, Mallove was considered well-qualified for the job. Two months ago, he quit.

Mallove: What went on behind closed doors at my alma mater is so upsetting that I will not rest until the whole matter is given thorough airing.

Baron: In a seventeen-page resignation letter, Mallove alleged misconduct by scientists at MIT's Plasma Fusion Center, misconduct he says he unearthed while researching a book on cold fusion. MIT's Plasma Fusion Center receives tens of millions of dollars in federal funds each year to develop an energy source based on hot fusion, the same process that powers the sun. Mallove says the scientists had a vested interest in seeing cold fusion die as quickly as possible.

Mallove: We have a major Big Science program, hot fusion, which is literally trying to squash cold fusion.

Baron: For most people, the cold fusion story began on March 23, 1989. On that day, two chemists at the University of Utah announced that they had accomplished in a simple table-top experiment what other scientists had failed to do after decades of work. Stanley Pons and Martin Fleischmann said they'd produced sustained power using nuclear fusion—potentially providing a cheap, safe, and nearly inexhaustible source of energy. Scientists worldwide raced to replicate the Utah work. Some laboratories initially reported success, but the excitement soon turned to skepticism when many of those early claims proved incorrect. Then on May 1, 1989, the *Boston Herald* ran a story on its front page with the banner headline: "MIT Bombshell Knocks Fusion Breakthrough Cold." The article said that MIT scientists had discovered serious flaws in Pons and Fleischmann's work. Ron Parker, head of MIT's Plasma Fusion Center, reportedly accused the Utah scientists of misrepresentation and maybe fraud and called the research "scientific schlock."The story immediately spread across the country. MIT issued a press release, denying that Parker had made the published comments. Parker spoke at a news conference that day:

Tape of Parker's remark at 1989 press conference: Let me just say quite clearly for everybody, that I am not, have not, and, uh, really seriously doubt whether I ever will accuse Professors Pons and Fleischmann of fraud.

Baron: Parker says he was misquoted. The *Herald* reporter who wrote the story, Nick Tate, says when he heard that, he was flabbergasted.

Tate: Not only were the concerns about possible misinterpretation and fraud and scientific schlock repeated to me more than one time during the course of our interview, but I also had them on tape.

Baron: At the time, Tate decided not to release his recording of the interview, but a year later he gave a copy to Gene Mallove at MIT. And this week he gave a copy to WBUR. On five separate occasions during the taped interview, Parker uses the word "fraud." At one point he says, quote, "Now it transcends the question of whether they misinterpreted to the question of whether there was deliberate fraud."

Tape of Parker talking in 1989 interview: . . .you know now it transcends I think the question of whether they misinterpreted, to the question of whether there was deliberate fraud.

Baron: At another point, Parker says to reporter, Tate, quote, "So what are you going to do with this, Nick? What you're hearing is we think it's a scam, right?"

Tape of Parker talking in 1989 interview: What you're hearing is we think it's a scam, right?

Baron: Elsewhere in the interview Parker indicates he purposely did not contact the *Boston Globe* with his story, because that newspaper was, he said, "leading the cheers for cold fusion." The way Gene Mallove sees it, Parker and colleague Ron Ballinger, who also took part in the *Herald* interview, planted the critical story in the *Herald* to generate widespread negative publicity regarding cold fusion. MIT's Ron Parker says he stands by his claim that he was misquoted. He points out that the *Herald* said he accused Pons and Fleischmann of misrepresentation. The word he really used was misinterpretation.

Parker: I have said that they have published data which was incorrectly interpreted and not real data. Uh, I, have, uh, no way of knowing if they knew at the time or even since that time whether the data was incorrect.

Baron: Parker says his comment about a scam was meant to refer not to Pons and Fleischmann's research, but to their request for $25 million from Congress. And Parker denies he was trying to manipulate news coverage. He says he was merely trying to communicate important findings about the Utah work.

Parker: When we had found out that they had made fundamental mistakes in interpreting their data, we felt certainly an obligation to bring this to the attention of the media at that time.

Baron: But when Gene Mallove resigned from his post at MIT, he said Ron Parker's unethical behavior went beyond the episode with the *Boston Herald*. In July of 1989, Parker and his colleagues reported the results of their attempt to replicate Pons and Fleischmann's work. They claimed they found no evidence of fusion, either in the form of radiation or energy produced. But when Mallove recently found a draft of that final report, he discovered what he considers a significant discrepancy. One of the graphs in the draft report suggests some low level of power was produced, but in the final version, the points on the graph have been moved.

Mallove: The data has been shifted down to make it look like there was no net power. Now in my estimation, there was absolutely no justification for this and I do not know why this was moved, nor have I had an answer from them as to why it has been moved.

Baron: Mallove suspects the data was moved because the scientists didn't want to provide any positive evidence for cold fusion supporters. But Ron Parker says it was unclear exactly where to put the data points on the graph and in the final report he used a more sophisticated method for determining their placement. Besides, he adds, the level of power hinted at in the earlier graph falls within the experiment's margin of error. That is, says Parker, it's effectively zero.

Parker: It's as simple as that and that's the end of the story on it.

Baron: But Gene Mallove hasn't let the story end there. He has continued to press Parker and his colleagues for their lab notebooks to check their work. Parker has declined to turn them over, saying to collect all the relevant papers would be time consuming and not worth the effort. One MIT faculty member familiar with but not involved in the dispute said he thinks Mallove's claims have a germ of truth to them. The professor, who asked not to be identified, said scientists at the Plasma Fusion Center didn't keep a very open mind to the claims of cold fusion. The faculty member said he doesn't think anyone was guilty of deliberate misconduct, though they may have conducted bad science. MIT's administration has so far stayed out of this cold fusion controversy. Gene Mallove hasn't pushed for any formal inquiry, and the Institute's Provost, Mark Wrighton, says he doesn't see anything worth investigating. Observers say even if MIT scientists were guilty of misconduct, it's unlikely their actions changed the course of history. By the late spring of 1989, scientists across the country were coming to the same conclusion—that the Utah work was seriously flawed. And while intriguing claims of cold fusion continue to be reported at laboratories around the world, the field has had a hard time regaining its credibility. For WBUR, I'm David Baron.

Exhibit Q
Prof. Ronald Parker's Letter to Eugene Mallove
August 13, 1991

After the WBUR radio program, another fax arrived from Prof. Parker, this one having some but not all the requested data appended to it. And, he was asking me not to disseminate data that had been generated during a Federally funded research project!

Dr. Eugene F. Mallove, Bow, NH

Dear Gene:

At my request, Stan Luckhardt dug out the data you requested concerning the heater power for the H_2O cell in the Phase-II calorimetry measurements. For convenience in comparing with D_2O, he plotted the corresponding D_2O result on the same scale. The two graphs that he produced accompany this letter.

Since these data are unpublished, they are provided only for your information and should not be reproduced or disseminated without our permission.

Sincerely, Ronald R. Parker

XC: S. Luckhardt, K. Campbell

Exhibit R
Eugene Mallove's Formal Request for an Investigation of Scientific Misconduct at MIT

The stonewalling and obstruction of fact-finding about the MIT PFC experiment had been unrelenting. After much deliberation, I made the decision to submit a request for a formal investigation of scientific misconduct.—EFM

Dr. Eugene F. Mallove
The Writing Program, 14N-316, Department of Humanities
Massachusetts Institute of Technology, Cambridge, MA 02139

August 18, 1989

Dr. Mary P. Rowe
Special Assistant to the President, Room 10-213
Massachusetts Institute of Technology
Cambridge, MA 02139

Dear Dr. Rowe:

I regret to have to bring to your attention a serious matter of possible scientific misconduct, which has to do with the mishandling, analysis, and representation of scientific data by a group of researchers at MIT. Much as I did not wish to involve your office in a formal inquiry into this matter, I believe that the circumstances and evolution of this controversy now demand this action.

Specifically, I refer to some key data presented in a technical report of July 1989 done by a team of researchers from the MIT Plasma Fusion Center and their associates in the Department of Chemistry. The report is designated PFC/JA-89-34, "Measurement and Analysis of Neutron and Gamma Ray Emission Rates, Other Fusion Products, and Power in Electrochemical Cells Having Pd Cathodes"(Attachment #1). This report was subsequently published as a paper in the *Journal of Fusion Energy*, Vol. 9, No. 2, 1990, pp. 133-148 (Attachment #2). The research was supported, the report says, "in part by the United States Department of Energy contract number DE-AC02-78ET51013." So I presume that not only MIT guidelines but Federal regulations as to the handling of scientific data govern here. The sixteen authors of the paper are: D. Albagli,[1] R. Ballinger,[2,3] V. Cammarata,[1] X. Chen, R. Crooks,[1] C. Fiore, M. Gaudreau, I. Hwang,[2,3] C.K. Li, P. Linsay, S. Luckhardt, R.R. Parker, R. Petrasso, M. Schloh,[1] K. Wenzel, and M. Wrighton[1] [1=Dept. of Chemistry, 2=Department of Nuclear Engineering, and 3=Department of Materials Science and Engineering].

As an engineer with a long-standing professional interest in energy systems and in my former role as chief science writer at the MIT News Office, my interest in this paper evolved as the controversy about the claims of cold fusion emerged beginning with the announcement on March 23, 1989 by Professors Fleischmann and Pons at the University of Utah. As you know, I am the author of a book, *Fire from Ice*, about the scientific, political, and media aspect of the cold fusion controversy. My recent resignation from the MIT News Office was prompted by my deep concerns about the way prominent members of the MIT faculty have dealt with the subject. However, the issue at hand is not the truth or falsity of cold fusion claims, nor the issue of the way in which cold fusion has been regarded at MIT generally, but the manner in which a specific piece of scientific research was conducted by one particular group at MIT.

Yet the effects on the scientific process to understand a baffling new phenomenon cannot be ignored. The MIT work has been widely cited as a keystone in dismissing the claims of Fleischmann and Pons. It also no doubt was one factor in the decision by former Chemistry Department head, Mark S. Wrighton, to sign the DoE Energy Research and Advisory Board report (November, 1989), "Cold Fusion Research," which most observers would agree eventually ended DoE funding of cold fusion research in the United States. Professor Wrighton was a key author of the MIT research paper.

The MIT work in question has to do with calorimetry, the measurement of heat in electrochemical cells that were set up by the MIT research group to test the claims by Professors Pons and Fleischmann to have measured significant excess power in some cells. The part of the paper that is of most concern is the section on calorimetry, not the results of nuclear measurements, although I do believe that the paper's statement in its final summary with regard to nuclear measurements is completely unfounded: "Importantly, the level of fusion products present is by far a more sensitive indicator of fusion reactions than are the relatively insensitive heat-based measurements which form the foundation of the claim of nuclear fusion as put forth by FPH." This statement presumed that all the possible nuclear reaction paths and products that might explain cold fusion were known *a priori* and were measured by the MIT group, clearly a statement that an objective scientist investigating a puzzling new phenomenon should not be making. In fact, theoretical and experimental developments have, I believe, totally invalidated those early beliefs by the MIT team. So the nuclear effects part of the paper, while useful and interesting, does not go to the heart of the cold fusion question as it was in 1989: Is there unexplained excess power in some electrochemical cells run in heavy water and palladium-platinum electrodes?

I respectfully ask you to initiate an immediate inquiry to determine whether a thorough investigation of the specific concerns that I have enumerated below is warranted. I believe that these concerns are certainly of sufficient magnitude to justify an investigation consistent with the Policies and Procedures guide for MIT faculty and staff. Those who are co-authors of the paper, including Professor Wrighton, should, of course, disqualify themselves from participating in the inquiry and subsequent investigation. I also request that Dean of Science Professor Robert Birgeneau not be involved in any inquiry and possible subsequent investigation, due to his well known negative view of cold fusion research.

The objectives of the investigation should be three-fold: (1) To determine whether there was misconduct in the handling and representation of the data to other scientists and to the public; (2) To determine whether there was misconduct and/or sufficiently egregious errors in the work to warrant the paper's retraction or significant amendment; and (3) To determine whether two of the paper's authors, Professor Ronald R. Parker and Associate Professor Ronald Ballinger, engaged in unethical behavior in orchestrating a public attack on the motives of researchers whose work they hoped to prove incorrect -- in part with the forementioned research paper. Further, to determine whether Professor Parker subsequently engaged in unethical behavior in deceiving the scientific community, the MIT News Office, and former MIT President Paul Gray about the nature of his actions.

Specific Concerns Enumerated

The MIT Plasma Fusion Center/Chemistry Department experimental contribution to cold fusion research was conducted from late March 1989, through late May 1989. The search for evidence of cold fusion in heavy water cells and comparisons with light water cells included attempts to find various nuclear products, as well as indications of excess power.

One experiment in the series of experimental cells reported in this work is of particular interest, because it is the only case in which some of the graphs of the raw data that form the power measurements are shown. This so-called "Phase II" calorimeter experiment compared the power production of a light water control cell and a heavy water cell. The record of the controversy clearly shows that at that time scientists were placing great emphasis on the need to find differences in the power production between light water cells and heavy water cells. Numerous statements by a variety of scientists at the time—on both sides of the controversy—attest to this, and these statements could readily be assembled if necessary. The presumption by many at the time and subsequently was that if a heavy water cell produced excess power and a light water cell of identical form did not, then there would be motivation to investigate further the possibility that unknown nuclear reactions might be occurring—particularly if the magnitude of the integrated release of energy were to exceed the energy from any conceivable chemical reactions in the cell.

1. Shift of Intermediate Processed Data, Perhaps to Suppress Evidence of Possible Excess Power

This is one of the most troubling aspects of the Phase-II excess power experiments. Even if the processing of the raw data for this experiment were presumed to be correct, which I doubt to be the case (see subsequent listed concerns), data provided to me in the summer of 1989 by members of the MIT research team show that there was likely to have been an apparent difference in the performance of the heavy water cell and the light water cell; at least on one extant graph the heavy water water cell seems to be evolving excess power, while the light water cell seems not to be—exactly what many hoped to find at the time as an anomalous effect.

Attached are four figures, which have been adjusted in size by me from their original form to better compare them (see Attachment #3). The first two are from the published MIT paper and show the excess power produced by the Phase-II heavy water and light water cells. The intermediate processed signal was noisy and so was averaged in hour-long intervals to produce the data (black dots) seen in these figures. The results rise and fall above the zero excess power line, and there is nothing that leaps out from this data comparison to suggest that excess power is being produced in the heavy water cell and not in the light water cell. It looks as though both excess power plots are about equally noisy. This data that form these curves was prepared at least as early as July 13, 1989, because I was given a draft article by the PFC that bears that date (see Attachment #4). [Ed. Note: See graphs, p. 74.]

On the other hand, I also was given the processed but unaveraged , and hence more noisy data that went into forming these published curves. These data appear in the two other attached figures. The figures are copies of graphs that appear in another PFC draft report to me on calorimetry, dated July 10, 1989, three days before the draft with the averaged data (see Attachment #5). The light water graph oscillates above and below the zero excess power power line (which I have introduced to make the comparison clearer), with no obvious bias above or below the zero line. There appear to be cyclic variations in the presented excess power, but it is not clear what these are from. The heavy water curve, by contrast, is dominantly above the zero line, indicating the possibility of a residual excess power (even though the magnitude of the excess power may be below the report's stated sensitivity of their calorimetry, 40 milliwatts). The two curves are simply quite different. There could be something like a few tens of milliwatts excess power here, on average, as several PFC researchers have subsequently agreed there could be. For this 0.1 centimeter diameter electrode, 9 cm long, 20 milliwatts would translate to excess power of 0.28 watts per cubic centimeter.

So why do we see no evidence of this possible excess power in the graphs that are in the final report and the published paper? The inescapable answer seems to be that the averaged data for the heavy water was moved down an arbitrary amount so that it now has more the appearance of the null result in the case of the light water averaged data. Interestingly, the light water averaged data seem to be consistent in level with the corresponding curve of raw processed data, that is, it has not been moved down.

In a recent letter to me (see Attachment #6), Professor Ronald R. Parker, who has been aware of my concerns since at least early June of this year, states: "Our paper estimates the uncertainty of calorimetry measurements as 40 mW, and so you are free to posit an excess heat less than this level if you wish." This blithe remark is of concern for a number of reasons. Foremost is because I reject the entire methodology of getting to these intermediate processed curves in the first place (see below). Second, there is no substantiation of how this 40mW sensitivity of the calorimeter was derived—certainly of key importance in knowing whether any significant shift of the data is warranted at all. I requested via a letter of April 29, 1991 to another team member, Dr. Stanley Luckhardt, information on how the sensitivity of the calorimeter was computed (see Attachment #7). No information on this point has been forthcoming from him, as you will see below.

There is yet another concern about the downward shift in the heavy water cell data. Note that on page 4 of the July 10 draft of the calorimetry section (Attachment #5), there is no final paragraph of conclusions. However, on the July 13 draft (Attachment 4), a conclusion paragraph has been inserted—after the curve was shifted down. The key sentence is: "The data show a slowly fluctuating power level in both the H_2O and D_2O cells, but neither show evidence of sustained power production at the levels claimed in Ref. [Pons and Fleischmann's report]." How is one to judge what level of power may have been present in the heavy water case if the heavy water curve is arbitrarily moved to make it look like the light water curve? The readers of this report are expected to believe that the calorimetry was sensitive enough to present evidence of a "slowly fluctuating power level" (clearly with less than 40 mW sensitivity required to observe them), but they are misled by the contrived down-shift of the heavy water power curve.

2. A Grossly Incorrect Analysis of Raw Data that was Guaranteed to Produce Zero or Minimal Difference Between Light Water and Heavy Water Power

There is a simple equation cited by the paper's authors that governs whether some unknown source of anomalous power, p_x, is acting in the cell. That equation is: $p_h + p_x$ = constant. Now by the methodology of this paper, if the heater power, p_h, is declining in time, that would be an immediate indication that there might be some excess power building up. Now if one fits any kind of curve to the ragged raw data that describes p_h as a function of time and then subtracts this from the raw data, one does not get p_x. Yet this is precisely what the group apparently did. The result is merely a reflection of the "noise" that is left over once the subtraction is done. There is no way that one can say that this processing gives p_x.

It is obvious that p_x is linearly related to p_h, but to justify a subtraction of any part of the raw p_h signal would require a careful thermal analysis of the cell's heat transfer characteristics to determine how much, if any, of the heater power drift should be removed. This analysis was not done by the MIT group.

There has been a more elaborate technical analysis of this experiment, which leads me to believe that at least in the heavy water case in the Phase II experiment, there could be evidence of excess power production. The evidence of excess power production based on this analysis appears in a technical letter to the editor in *Fusion Technology* , Vol. 19, May 1991, pp. 579-580 (see Attachment #8). In that letter, the power density produced in the palladium electrode is shown to rise from zero at 20 hours into the test to close to 2 watts per cubic centimeter at 100 hours. In view of the very belated release of the light water heater power curve (see concern 6. below), I am less confident of all the details of the analysis in *Fusion Technology,* but I am very confident that the MIT group's method is without justification.

The analysis on which Drs. Noninskis' conclusion in *Fusion Technology* is based differs from the MIT group analysis because the latter analysis has introduced the forementioned subtraction from the raw data that does not appear to be proper -- a subtraction from the raw data of a linear fit to the noisy and declining heater power. In fact, if this adjustment is performed, for the reasons suggested in the MIT paper, it is possible to get nearly a null result for the excess power measurement in every case. It is surprising that this was done, because in the methodology of the MIT calorimeter, declining heater power should have suggested the presence of an unknown heat source. In Professor Parker's letter to me of August 8, 1991 (Attachment #6), remarkably he acknowledges that the subtracting out of p_h due to the depletion of the electrolyte may not necessarily be justified : "Whether this is a correct assumption is arguable. . ." Then he states by pure fiat: "but in any event, the main conclusions stand: We detected no significant difference between H_2O and D_2O. . ." In other words, even though the PFC's assumptions about the manipulation of the data may be incorrect, we are expected to believe that there is no significant difference between H_2O and D_2O because the PFC group says without substantiation that its calorimeter's sensitivity was 40 milliwatts.

3. False Assertion About the Significance of Unpublished Test Results That Motivated The Incorrect Analysis

The authors of the MIT work attempt to justify their unwarranted subtracting out of the heater power. On page 20 of the PFC report (Attachment #1) we find the statements: "However, measurement of p_h

over a 100 h period, Figure 6, indicates a significant drift caused by a reduction of the solvent volume. We demonstrated that this drift was due to solvent loss rather than to an unknown power source, p_x, by calibrating ph as a function of the electrolyte solution volume. When enough solvent was added to the D_2O cell to compensate for that lost to the electrolysis at the end of the 100 h period shown in figure 6, p_h returned to within 20% of its original value." This statement is shocking because the authors are suggesting that a 20% discrepancy in heater power (over 200 milliwatts) to heat the same volume of fluid that was present initially can be ignored. In fact, if this experimental finding of a 20% discrepancy is true, it might be the best evidence of all that the heavy water cell really did produce anomalous excess heat.

The report immediately follows with yet another blatantly untenable statement: "If the total volume of solvent loss over the course of the experiment had been taken into account, including that lost to evaporation, p_h would have been even closer to its original value." If the cell has been refilled to compensate for the water lost to electrolysis, then what fraction of that refilled water disappeared through evaporation and what fraction disappeared through gas evolution is irrelevant. The net effect of these statements—just as in the unexplained downshift—is to obscure the possible evidence for excess power.

4. Subsequent Claims that the Basic Objective of the Research was Other than Claimed in the Report

Long before the concerns that are being enumerated in this letter had been brought to the attention of the MIT group, I met with Dr. Stanley Luckhardt to try to understand in detail how the MIT work was done. Frankly, I was politely looking for assurances that the data had not been arbitrarily down-shifted and that there might be some fundamental unpublished reason for the shift. I met with Dr. Luckhardt on January 25, 1991 and, without being accusatory at the time, received extremely unsatisfactory explanations. Dr. Luckhardt could not explain how the "bias," as he called it, was taken out to make the resulting power curves seem so similar. In fact, he agreed at the time that there might be 20 milliwatts of excess power in the MIT group's heavy water cell, "but not the 80 milliwatts that Fleischmann was talking about."

This litany continues of redefining the objectives of the Phase II experiment to argue that no unwarranted data moving has occurred. In the belated letter of explanation that I received from Professor Parker (see Attachment #6) he writes: "The implicit assumption is that we were looking for fast turn-on of the anomalous heat production and so it was legitimate to subtract out a slow baseline drift caused by depletion of the electrolyte." It seems to me that technical papers cannot have "implicit assumptions" as to their basic objectives. The basic objective was to find out whether there was a difference between heavy water and light water performance. There is no clearer evidence that such a comparison was what was originally intended by the MIT group than the paper's remark about the comparison of the light water and heavy water cell power output near the end of a 200 hour run (page 19 of Attachment #1). There is no discussion there of looking for "fast turn on" of power. I believe that this is a newly invented experimental objective to justify any past juggling of data that the group might have done.

5. Public Renunciation in June 1991 by Professor Parker of the 1989 Calorimetry Work, Whereas the Work Was and Continues to be Promoted as Sound and Definitive Regarding Excess Heat

On June 7, 1991, during the question and answer period following the talk by Dr. Frank Close at the PFC, Professor Parker had this exchange with me (transcribed from a tape of the meeting):

Parker: We at MIT looked very carefully at Fleischmann and Pons, and this is what we came up with. [If we] think we ought to look at another set of experiments and we think we have expertise, we will. But just let it fall where it lies. We're not going to come out one way or another until we look at it.

Mallove: Would you consider re-evaluating your own experiment, if I brought in experts to evaluate it? Would you consider that? Because I've asked Dr. Luckhardt for several weeks now—and I know he's not here today. He told me at one point he would provide me with the heater power curve for the light water experiment so that I could ascertain what the heck was going on in that experiment. He then finally ended up saying to me he would not give it to me—or that it would take a week to do it.

Parker: I think, Gene, that what you showed up here earlier is completely a surprise to me. [The Phase II comparison power tests of light water versus heavy water, published and unpublished versions.] We will give you every piece of data we ever took.

Parker: My personal. . .

Mallove: Fine.

Parker: I'll tell you what my opinion is of that work, because I was part of it. I don't think it's worth very much. Alright? And that's why it's just published in a tech report. I don't think it's worth very much. I think to do calorimetry is one of the hardest things I ever tried to do. I'd rather stick to plasma physics.

Mallove: But, Ron, with all due respect, I agree with you, I agree with you [that the work was not conclusive].

Parker: When you have an open system is where you can make big errors, where you don't know the overpotential, the electrode potential, and so on. These things are unknown. I mean it's really tough and that's why I don't put any stock at all -- you can redraw those curves anyway that you want. I don't think that data is worth anything. Now you may be able to find something in it. I did the experiment; I don't think it's physics.

Mallove: But what I've seen, because I certainly see it from Douglas Morrison [of CERN] and I see it from people like Frank Close and others, that your prestigious laboratory with its excellent resources is being used in some respect as a standard which everyone else is supposed to adhere to. . .

Professor Parker continues to insist in representing his data in two different ways. Both before and after this remarkable June 7, 1991 renunciation we have statements that the work shows no evidence of excess power. How is it that Professor Parker thinks he can disparage his group's own work in public and later claim that it has validity? This is not a small point. The MIT work has been cited on numerous occasions by people bent on attacking cold fusion researchers for their supposed "delusions" and "incompetence." We never before heard from any member of the PFC team that the work was "worthless." Quite the opposite. Professor Parker's colleague Professor Mark S. Wrighton in a letter on October 10, 1990 (to Dr. Vesco Noninski (see Attachment #9), was quite firm that the MIT work was definitive.

6. Obstruction of Other Scientists' Attempts to Access Data Critical to Understanding the Group's Experiments

It is clear that for about a year now the MIT group has been very reluctant to release data that would allow other researchers, including myself, to gain insight into their calorimetry work. I introduced an electrochemist visiting from Bulgaria, Dr. Vesco Noninski, to Dr. Stanley Luckhardt on August 15, 1990. Noninski had viewed the MIT work very positively, because it was one of the few experiments in calorimetry in which at least some raw data was published. He had begun to believe that the MIT heater power data for the heavy water cell could be analyzed to show evidence of excess power. There followed discussions between Drs. Noninski and Dr. Luckhardt in which it became clear that there was a difference of opinion between the two on how the experiment should be analyzed. But in spite of Dr. Noninski's completely valid request to Dr. Luckhardt to receive the raw data corresponding to the light water cell, he would not release this data. Both Dr. Luckhardt and Professor Wrighton wrote letters to Dr. Noninski in which they dismissed his analysis outright.

Much later, in early May 1991, I was attempting to write a paper for *Fusion Technology*, to assess the MIT Phase-II calorimetry and to remark about the relative points made by Drs. Noninski and Luckhardt. So I asked Dr. Luckhardt if he could give me the light water heater power curve. Initially he said he would do this at a pre-arranged meeting between us. There followed over the next several weeks a series meetings that were cancelled by him. Finally on May 29, 1991, in a lengthy phone conversation with Dr. Luckhardt, he said that he would not turn over the data to me. The reasons he offered were that it would take too much time for him to explain to me how to correctly interpret the data. He said it would take four or five days to interpret the data. He further put me off by suggesting that I talk to another group "perhaps SRI" to get advice on calorimetry. Even though he would not give me the data, he said of the two heater power curves that they "looked pretty much the same" but that "fluctuations" were different, and he could not recall which one was unusual in which respect.

After I showed the apparently down-shifted intermediate processed data at the seminar at the Plasma Fusion Center on June 7, 1991, Professor Parker publicly said, "We will give you every piece of data we ever took." So on June 14, 1991 I submitted a request to Professor Parker for various data items that would help me clarify the issue (Attachment #10). Another scientific colleague, Dr. Mitchell R. Swartz, independently had made several phone calls to Professor Parker, in which messages were left about requests for the data that Swartz had heard Parker on June 7 offer to make available.

Not having received any communication from Parker regarding my June 14 request, I submitted a reminder letter on July 30, 1991 (Attachment #11). There was no response to this letter either. Nor were any of Dr. Swartz's calls returned by Professor Parker. The first response from Professor Parker came in a faxed letter to me (Attachment #6) on the evening before a WBUR radio program about cold fusion, in which Parker and I were both pre-recorded participants. In this letter, he continued to put obstacles in the way of getting the light water heater power curve. None of the other data requests of June 14, 1991 were even alluded to. In this letter he claims that Dr. Luckhardt "has been extremely forthcoming in discussing them with you and would be willing to assist you further." He wrote, "However, he has major responsibility for several PFC projects and I cannot allow him to take time away from them for this purpose. I suggest that you negotiate directly with him to see what arrangements could be made."

Finally, to cap this absurd one-year history of evasion at every turn, I received yet another fax letter from Professor Parker on August 13 (Attachment #12), in which inexplicably there has been some time for Dr. Luckhardt to generate (or simply retrieve from his files) the light water heater power curve. But the letter suggests that the accompanying curves (both heavy water and light water) "should not be reproduced or disseminated without our permission." This statement is put forth even though the heavy water heater power curve was already published by the MIT group! For reasons unknown to me, Professor Parker wants the light water power curve to remain out of the public domain.

I think that it is absolutely clear that there has been an unwarranted withholding of data by the MIT group from people with a legitimate need to access it. In fact, this withholding of data was the turning point that prompted this request for an inquiry. I had considered making the request earlier, but was very reluctant to make it. However, the final evasions raised even greater suspicions in my mind about the nature of this data and how it was handled.

7. Attacks on the Motives of Scientists Who Were Making Positive Claims About Cold Fusion

The method of performance, representation, and access to the MIT team's research is the central issue of this request for an inquiry and investigation. However, the MIT group's work did not occur in an intellectual vacuum. It is impossible to dissociate from the MIT group's performance the unethical behavior of some of the key organizers of that research in view of their attacks on the motives of Professors Pons and Fleischmann, whose work they apparently wished to discredit with their own results. This raises questions about the integrity with which the MIT group's work was carried out.

On May 1, 1989, the MIT News Office issued a press release (see Attachment #13) that was prepared by me in telephone consultation with Professor Parker of the Plasma Fusion Center in the early morning hours of May 1, 1989. The press release was crafted to deny statements that *Boston Herald* reporter Nick Tate had attributed to Professor Parker, which attacked Professors Pons and Fleischmann in the manner denied in the press release. When I prepared that release, and for more than a year afterward, I believed that the statement that the MIT News Office had issued was valid, though I had some doubts. I simply trusted what was being asked of me in preparing the release.

After having listened to the actual tape of the interview that Nick Tate played for me in July 1990, I am certain that his story's characterization of what Professor Parker and Professor Ballinger had said in their conversation with Tate the week before is substantially correct. Professor Parker and Ballinger clearly were conducting a well-orchestrated attempt to condemn the work of Pons and Fleischmann, not merely to criticize it technically. In the taped interview, a copy of which I obtained from Nick Tate in February 1991 (a partial transcript of which appears in Attachment #14), Professor Parker used the word "fraud" no less than five times and he does refer to the work of Pons and Fleischmann as "scientific schlock," despite his subsequent and continuing denials. At one point in the interview Professor Parker says to Nick Tate, "..what you're hearing is that we think its a scam, right?" In my view, virtually any other competent reporter would have written essentially the same story that Tate wrote. In fact, the partial transcript published by the Herald on May 2, 1989, should have been enough to convince me of the soundness of Tate's story, had I not been told by Professor Parker that these remarks were "out of context," and that reporter Tate was a "viper."

Professor Parker deceived me and other members of the MIT News Office about what he had said, then in a press conference on May 1, 1989 he deceived the world about what he had said, and he continues to deceive the world again and again (see transcript of recent WBUR radio program, Attachment #15).

Remarkably, Professor Parker apparently deceived even former MIT President Paul Gray about the nature of his effort to manipulate the News Media. An April 17, 1989 letter from *Boston Globe* Reporter Richard Saltus to President Gray (see Attachment #16) had complained about the lack of access to Professor Parker. President Gray's letter of response to Richard Saltus on May 1, 1989 (Attachment #17) says, "He [Parker] has tried to be as helpful as possible, consistent with his belief that judgement should be reserved until the scientific facts are clarified. That cautious stance has led him to discourage all media visits to the Plasma Fusion Center, although his efforts have not always been successful. I have been assured that there was no discrimination against the *Boston Globe* and that, to the contrary, Professor Parker spoke five or six times with your colleague, Mr. David Chandler."

The truth about Professor Parker's media manipulation is clear from this interview with Nick Tate. At one point Parker says, "The reason I stopped talking to the *Globe* for example is that I felt that they were reporting irresponsibly...they were out there just leading the cheers instead of being objective." Then later, ". . .you know I can't trust the *Globe*, I'd like to trust you."

This deception and compounded deception about what was said and how it was arranged to be said to Nick Tate amounted to a direct attack on the integrity of an honest reporter as well as the contrived involvement of a number of MIT personnel, including the former President, in that deception. But as important or more so was the effect that the intended disparagement of Professor Pons and Fleischmann's work had on the future course of cold fusion research. The work was difficult enough to assess without imputing the motive of possible fraud, which allegation at that time and to the present day remains completely unfounded.

Professor Parker was not the only member of the MIT research team to bring up allegations of fraud about Professors Pons and Fleischmann. I am aware of at least two other authors of the MIT research paper who have made such allegations publicly and privately.

I hope you will determine that the concerns I have raised about the conduct of the MIT group's work merit a prompt investigation. If carried out objectively, such an investigation will, I am very confident, reveal various levels of scientific misconduct.

To conclude this formal request, I would like to make clear my belief that however the data from the Phase-II calorimetry experiment are interpreted, it is clear that it is impossible to use the raw data behind these flawed results or the data of the cruder series discussed earlier in the PFC paper to conclusively prove anything one way or another about the reality of cold fusion. I do not believe that any member of the MIT research group ever actually believed it had discovered evidence for cold fusion and then "covered it up." However, I do firmly believe that they mishandled their data in a manner calculated not to leave any room for doubt about the finality of their conclusions, when in fact there was considerable room for such doubt. In my view, the data in this work were not only improperly manipulated to emphasize an allegedly negative result, but the significance of conclusions about that data were and continue to be misrepresented and confused.

Even if this work had been done properly and offered a clear null result, it would still have been far too limited a check. Other laboratories that have obtained sporadic positive results for excess power—and by now even reproducible excess power (SRI, Inc. in Palo Alto under EPRI contract)—have generally spent much more time in their trials and employed a larger series of electrodes, a possible requirement sometimes to pick up the anomalous thermal effects. The body of evidence in support of some if not all cold fusion claims, in fact continues to grow, as can be seen in several reviews and compilations of many experiments:

* Edmund Storms (Los Alamos National Laboratory), "Review of Experimental Observations About the Cold Fusion Effect," accepted for publication in *Fusion Technology*, 1991.
* M. Srinivasan (Bhabha Atomic Research Centre), "Nuclear Fusion in an Atomic Lattice: Update on the International Status of Cold Fusion Research," *Current Science*, 25 April 1991.
* Steven E. Jones, Franco Scaramuzzi, and David Worledge (editors), Anomalous Nuclear Effects in Deuterium/Solid Systems, American Institute of Physics Conference Proceedings 228, 1991.
* *Investigation of Cold Fusion Phenomena in Deuterated Metals* (four volumes), by the National Cold Fusion Institute, June 1991, now available from NTIS.

What is most disturbing, however, about the way in which the MIT research was conducted was the arrogant dismissal of the work of other scientists with the group's insubstantial, manipulated, and flawed evidence. It smacks of a rush to judgement by a group that had made up its mind that it would never find anything positive nor would it report such. Even before they had completed their experiments, members of the MIT group made it quite clear to the world in so many ways that they didn't expect to find anything. The parallel effort to discredit the motives of other scientists and the denial that such an effort was ever made has had a lasting corrosive effect on the entire field. I hope that when the inquiry and expected investigation of this matter is completed, MIT will adopt guidelines to insure that this behavior is never repeated.

Sincerely, Eugene F. Mallove

List of Attachments (those with * can be found in this issue)

1. Plasma Fusion Center Report, PFC/JA-89-34, "Measurement and Analysis of Neutron and Gamma Ray Emission Rates, Other Fusion Products, and Power in Electrochemical Cells Having Pd Cathodes"
2. The corresponding paper in the *Journal of Fusion Energy*, Vol.9, No.2, 1990, pp.133-148.
***3.** Four figures, two of which were obtained from the MIT team's draft material and two of which were published. These have been adjusted in size by me from their original form for easier comparison.
4. Draft report of calorimetry section, July 13, 1989.
5. Draft report of calorimetry section, July 10, 1989.
***6.** Letter from Professor Ronald R. Parker to Eugene Mallove, August 8, 1991.
***7.** Letter to Dr. Stanley C. Luckhardt from Eugene Mallove, April 29, 1991.
8. A technical letter to the editor in *Fusion Technology*, Vol. 19, May 1991, pp. 579-580.
***9.** Letter to Dr. Vesco C. Noninski from Professor Mark S. Wrighton, October 10, 1990.
***10.** Letter (June 14, 1991) from Eugene Mallove to Professor Ronald R. Parker, requesting data that was promised at an open public meeting.
***11.** Letter to Professor Parker from Eugene Mallove, July 30, 1991.
***12.** Letter to Eugene Mallove from Professor Parker, August 13, 1991.
***13.** MIT News Office press release, May 1, 1989.
***14.** Partial transcript of April 28, 1989 interview by *Boston Herald* reporter Nick Tate with Professor Ronald R. Parker and Associate Professor Ronald Ballinger.
***15.** Transcript of David Baron's WBUR radio program (August 9, 1991) about the cold fusion controversy.
***16.** Letter from Richard Saltus of the *Boston Globe* (April 17, 1989) to President Paul E. Gray.
***17.** Letter from President Paul E. Gray to Richard Saltus (May 1, 1989).

Exhibit S
Permission Given to Transmit Request to Dr. Vest
September 9, 1991

It was necessary for Dr. Rowe to have my formal permission to submit my request for an investigation to President Vest.—EFM

Dr. Eugene F. Mallove
The Writing program, 14N-316, Department of Humanities
Massachusetts Institute of Technology

Dr. Mary P. Rowe
Special Assistant to the President, Room 10-213
Massachusetts Institute of Technology

Dear Dr. Rowe:

As per our telephone conversation of last week and earlier telephone message exchanges, I agree with you that my formal request for an inquiry and investigation of possible scientific misconduct at MIT (letter and attachments dated August 18, 1991) should be redirected by your office to President Vest. Though I am assuming that you have already made this transfer, I hereby formally give you permission to direct the request to Dr. Vest.

Sincerely, Eugene F. Mallove

Exhibit T
Eugene Mallove's Response to a Statement on Cold Fusion Issued by the MIT News Office 8/30/91
Received by Mallove only on 9/16/91

The MIT PFC had put out an outrageous "Press Release" on August 30, 1991, which I did not get wind of until mid-September. Here is my rebuttal of its various claims. —EFM

Dr. Eugene F. Mallove
Bow, New Hampshire, September 17, 1991

On August 30, 1991, the MIT News Office issued a disgraceful and misleading statement about the cold fusion controversy (see attached). What follows is my point-by-point response to this statement. Bold type are quotations from the August 30, 1991 MIT News Office statement.

• **"MIT scientists intensely investigated the phenomenon called 'cold fusion' for two months in 1989. Like other scientists around the world, they were unable to duplicate the Pons and Fleischmann experiment They have concluded that, while the reaction termed cold fusion is scientifically interesting, it is not one which is valuable for them to pursue at this time."**

Note the implication that all scientists at MIT who had anything to do with cold fusion investigated it for only two months and then dropped it after being unable to duplicate the Pons and Fleischmann experiment. This is not true. To my knowledge, at least five MIT professors continue to be actively interested in cold fusion experiments and theories. Numerous other professors, researchers, and students at MIT, not directly engaged in cold fusion, have expressed intense curiosity about the status of current cold fusion research. At least two MIT professors have applied for patents on cold fusion concepts. The cold fusion theory of one MIT professor is highly regarded in the field. Moreover, cold fusion research is right now being actively pursued at dozens of laboratories in the U.S. and abroad.

I challenge the term "intensely investigated for two months." The MIT professors who influenced this statement arrogantly suggest that it is possible to perform two months of experiments and come to definitive conclusions about so difficult a phenomenon as cold fusion. They are wrong about this and they know it, as the phrase "scientifically interesting" so clearly reveals. The preparers of this statement are well aware that they do not understand this "scientifically interesting reaction," nor is it likely that they have they kept current on what is really happening in this field, yet they claim to be able to determine that "it is not one which is valuable for them to pursue at this time." This is an indictment of the appalling lack of scientific curiosity manifested by these individuals. If this is supposed to be an example to MIT students about how deeply scientific curiosity should be followed, the Institute is in grave trouble.

The statement also incorrectly implies that other scientists have not been able to duplicate the Pons Fleischmann experiment. The phrase used was, "like other scientists around the world." Again, the statement preparers are engaging in smokescreen tactics. Numerous other investigators have to their own satisfaction and to the satisfaction of other objective evaluators replicated cold fusion phenomena, including excess energy evolution in excess of what can be explained by conventionally understood chemical or mechanical energy release, the production of tritium, the production of neutron bursts, low-level neutron emissions, and charged particle emissions.

• **"They note that the University of Utah, where 'cold fusion'**

began, has closed its cold fusion institute."

What a pathetic bit of innuendo—to imply that scientific, budgetary and programmatic difficulties at one research laboratory somehow validates the statement preparers' lack of interest in cold fusion research. For less than $5 million, the National Cold Fusion Institute generated a large body of impressive scientific data in a research program that was favorably critiqued by four outside scientists in the spring of 1991, some of whom were and continue to be skeptical of cold fusion. However, the reason that NCFI was mothballed had to due only with the inability to attract private funding for the Institute after state funding expired, as was the agreement from the outset. Since when do funding successes or failures reflect on the scientific and technological merits of a new area of research—as implied by the News Office statement?

• "He [Eugene Mallove] says that MIT scientists should be conducting further investigations into cold fusion. The question he raises is basically a matter of who should set MIT's research priorities. It is the role of the individual MIT professor to set those priorities. Research into this phenomenon is low on their priority list at this point."

Indeed, I have suggested to the President of MIT that research into cold fusion should be re-evaluated in view of mounting laboratory evidence. My letter of April 12, 1991, in which this opinion was brought to Dr. Vest's attention was never answered and remains unanswered to this day. I believe that it is the responsibility of the administration of MIT to be watchful for new scientific opportunities and to encourage, not enforce, various research directions. MIT presidents have advocated increased efforts in nuclear reactor safety, scientific literacy, molecular biological research, and the like.

Again we see an attempt to lump all MIT professors into the category of those who put a low priority on cold fusion research.

While drawing attention to the issue of research priorities, the preparers of the News Office statement have neglected to mention that in a letter dated August 18, 1991, I have requested a formal investigation of possible scientific misconduct on the part of some MIT researchers in their handling and representation of scientific data in their spring of 1989 cold fusion experiments. I have also asked for an investigation into the likelihood that certain researchers at MIT orchestrated an unethical attack on the work of Pons and Fleischmann. My concerns about cold fusion research at MIT are more comprehensive than matters of research priorities.

• "MIT scientists have reviewed their paper that contains the data about which Mallove raised questions. Following the review, Professor Ronald R. Parker said, the conclusions of the study stand as published."

The latter statement is completely untenable in view of the June 7, 1991 statement made by Professor Ronald R. Parker at an open seminar at the Plasma Fusion Center. On this occasion (when he no doubt thought no one outside the room would ever hear his words), Professor Parker severely deprecated the experiment that I have questioned, but now he wants the world to believe that it was a good experiment and that his group's negative conclusions stand. This is, in part, what Professor Parker said on June 7, 1991 about the experiment for which I have requested an investigation:

"I'll tell you what my opinion is of that work, because I was part of it. I don't think it's worth very much. Alright? And that's why it's just published in a tech report.*

" I don't think it's worth very much. I think to do calorimetry is one of the hardest things I ever tried to do. I'd rather stick to plasma physics. . . When you have an open system is where you can make big errors, where you don't know the overpotential, the electrode potential, and so on. These things are unknown. I mean it's really tough and that's why I don't put any stock at all—you can redraw those curves anyway that you want. I don't think that data is worth anything. Now you may be able to find something in it. I did the experiment; I don't think it's physics."

[*The research was published not only in a "tech report," but also in the *Journal of Fusion Energy*.]

Exhibit U
President Charles Vest's Letter to Prof. Morrison
October 9, 1991

CHARLES M. VEST, PRESIDENT, ROOM 3-208

In President Vest's request to Prof. Morrison to assess my request for a full investigation, there were already some disquieting signs. He was deferring consideration of the MIT PFC's "ulterior purposes." The press deception, as it turned out, was never addressed after an opinion of Vest's "legal counsel."—EFM.

Dr. Philip Morrison
Institute Professor Emeritus, Rm 6-205

Dear Philip:

Dr. Eugene F. Mallove has written to my office to bring to my attention "a serious matter of possible scientific misconduct, which has to do with the mishandling, analysis, and representation of scientific data by a group of researchers at MIT." Our normal procedure would be to turn to the Provost to make judgment whether and how to proceed to consider a matter of this type. In the present instance, however, the Provost is one of a number of authors of a paper referred to in Dr. Mallove's statement of concern. Therefore, I am turning to you, as a distinguished member of the community, to assist the Institute in formulating the initial judgments and/or actions that should be taken in response to Dr. Mallove's letter.

I am enclosing Dr. Mallove's letter, dated August 18, 1989 and a subsequent letter dated September 9, 1991. Both of these letters are addressed to Dr. Mary P. Rowe, and the second one asks that the initial letter be redirected by her to me. All background materials that were supplied by Dr. Mallove, together with his August 18 letter, are also enclosed.

I would ask that you review Dr. Mallove's letter and the attached materials, and give me your specific advice as to how the Institute should proceed. I would suggest that there are three options that you might consider:

1. Should an inquiry be conducted, and, if so, what mechanisms and individuals might serve this function well?

2. Should a formal investigation occur at this time, and, if so, what mechanisms and individuals might be appropriate?

3. Is there an alternative course of action to 1 or 2 above, which you believe is preferable?

It seems appropriate to me that consideration, at this stage, should focus on the criticisms of the science and methodology which are raised in Dr. Mallove's letter and not on the questions of motives or ulterior purposes attributed by Dr. Mallove to various MIT scientists. In my view, these questions are not appropriate to consider until after the scientific issues are addressed.

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

Sincerely yours, Charles M. Vest

CMV: cbb Enclosures
cc: Dr. Mary Rowe
bcc: Constantine Simonides

Mark Wrighton

Exhibit V
Prof. Morrison's Report to President Charles Vest
October 14, 1991

Professor Morrison's conclusion that the MIT PFC paper "does not mislead" is completely untenable. It does not square with the facts and it never will.—EFM

MIT Department of Physics, Room 6-205

To: Charles Vest, President
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 [people] 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. (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

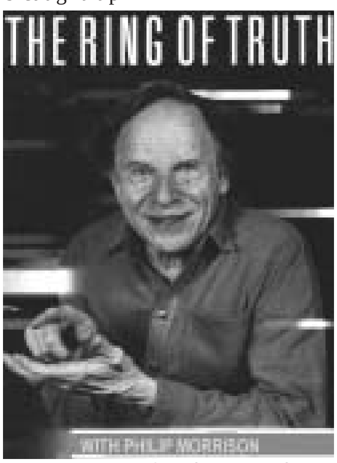
The focus of Mallove's criticism is on a difference between the two partial MIT draft manuscripts. In the earlier one (Attachment 5) the excess power is corrected by a simple linear fit for long-term drift and plotted, with many points, both for heavy water and the light water control cell. I measured 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



From Philip Morrison's PBS Television Series 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.

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

Exhibit W
President Charles Vest's Letter to Eugene Mallove
October 17, 1991

President Vest tries to settle the matter with the Morrison memo. Nice try, but no cigar.—EFM

Dr. Eugene F. Mallove, Bow, New Hampshire

Dear Dr. Mallove:

As you know from my letter of October 15, 1991, I have asked Institute Professor Emeritus Philip Morrison to review the materials you sent me, in light of your concerns about the scientific content of investigations by MIT scientists into possible cold fusion phenomena. Professor Morrison has now done so and has submitted to me a thoughtful memorandum stating the issues as he views them and presenting his recommendations regarding an appropriate response for the Institute. I am sure that you join me in appreciating Professor Morrison's careful work in this regard.

Enclosed is a copy of Professor Morrison's memorandum, dated October 14, 1991. Before any further actions are taken by the Institute, I would like to have your reactions to his recommendations. Professor Morrison has kindly agreed to make himself available to discuss the content of his memorandum with you, if you so desire.

I hope that this represents a constructive step toward resolving your concerns.

Sincerely yours, Charles M. Vest

Enclosure
cc: Philip Morrison, Mary P. Rowe

Exhibit X: Eugene Mallove's Letter to President Charles Vest, October 24, 1991

I opposed the conclusions of the Morrison memo in very strong terms, asking again for a full-scale investigation. —EFM

President Charles M. Vest
MIT Room 3-208
Dear Dr. Vest:

Thank you for sending me Professor Philip Morrison's memorandum of 14 October to you regarding my 18 August request for an inquiry into scientific misconduct at MIT.

First, let me say that I have the highest regard for Professor Morrison as a gifted scientist and educator, a man of impeccable character, and one of the finest human beings I know. I also count him as a friend and a person who made an honest attempt to be fair to all sides in his letter to you. I deeply appreciate the time and effort he made on your and my behalf. With that said, I regret to tell you that I am in substantial disagreement with Dr. Morrison's conclusions about the technical matters that he discussed. These disagreements I detail below.

At your request, Professor Morrison did not concern himself with the substantial issues of motivation and dealings with the press by Professors Parker and Ballinger. That was a significant part of my request for an inquiry and investigation. Not only is it absolutely clear to me that these individuals engaged in reprehensible conduct in the manner that I have described and documented, but their behavior significantly illuminates the handling and representation of the technical aspects of their experiments. The PFC cold fusion experiments of 1989 were by no means conducted in an intellectual vacuum. There was a clear rush to judgement by that group and a calculated attempt by at least two of its members to besmirch the scientific work of Drs. Fleischmann and Pons. I urge you to immediately turn over this aspect of my allegations of misconduct to a panel of individuals who will assess these concerns fairly and make recommendations.

Now let me return to Professor Morrison's memorandum to you. I do not agree with his conclusions that there is no need for an inquiry and no need for an investigation. I have read Professor Morrison's comments carefully. With due respect for his prodigious scientific talent and respected judgement, I come to very different conclusions, for reasons that will become clear.

First, let me begin by noting Professor Morrison's initially stated bias, a term that I hasten to characterize precisely in imputing such to Dr. Morrison. It is certainly not a bias that arises from any animus, but it is a bias nonetheless. He writes, "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." I suggest to you and to Dr. Morrison that this flies in the face of the work of hundreds of researchers around the world —many working right now and getting remarkable results—scientists who have done experiments that clearly reveal nuclear processes at work where none should be, by conventional reckoning.

If Professor Morrison relies mainly on what he has read in *Nature* about cold fusion or what the people at the Plasma Fusion Center have told him, then that may explain his missing a host of phenomena that many observers, who did not reject cold fusion in the spring of 1989, see as firmly established. There are two classes of phenomena that in my view have been experimentally confirmed in deuterated metal systems: (1) Calorimetrically measured excess energies that exceed megajoules per mole (tens of MJ/mole has been reliably found) and (2) Nuclear anomalies that may well be linked to the excess heat, albeit not in naive or one-to-one correspondence to nuclear products that have been measured (there may be others not yet measured). Among the byproducts that have been found are tritium, neutron emissions (both burst and continuous), helium-4, and charged particles with MeV energies. There is a steadily growing literature of such findings, both thermal and nuclear effects. This cannot be brushed away. It must be evaluated and studied carefully, without the preconceptions that have plagued this controversy. The phrase, "They would at most open some way to build a new battery . . .," I have heard before. It was made by Dr. Frank Close, whose assessment of cold fusion deliberately did not include the vast bulk of supporting data.

In evaluating the MIT experiment, Professor Morrison repeats the canard used by those who have tried to "wish away" many cases of experimentally determined excess heat: "Uncontrolled catalytic recombination of the oxygen and hydrogen produced gas is a source of possible excess power in the right range." A number of excellent experiments have thoroughly investigated this "explanation" and have rejected it. Recombination is not a significant factor in open cell work. (In closed-cell, deliberate recombination work, it is no factor at all, and there are excellent positive experiments of that kind too.) On the other hand and to his great credit, Professor Morrison correctly notes, "It is possible that helium is lost from the cells." Indeed, this is precisely the result that researchers at the Naval Weapons Center have recently reported in connection with their calorimetric work.

There is a significant omission in Professor Morrison's critique. Nowhere do I find an assessment of the validity of subtracting a "somewhat arbitrary linear function" from the raw heater power data to get net excess power. Dr. Morrison assumes *a priori* that the group employed a correct algorithm. But as my 18 August letter suggests, that methodology is completely incorrect.

Professor Morrison accepts what I believe to be a gratuitous statement in Professor Parker's hastily prepared letter to me of August 8, 1991—the one I received on the eve of the WBUR radio broadcast—in which he tries to explain the apparent curve shift between July 10 and July 13. Parker's statement is: "In one, the drift was fitted with a somewhat arbitrary linear function. . . in the other, the drift was fitted with a different linear function, this time a least squares fit, and the data appearing in the final version of the paper were produced." I do not think it is possible to accept this statement on its face. The light water curve has not been moved down by any new form of mathematical adjustment, but the heavy water curve most certainly has been moved. In fact, I believe that this statement about two methods of fitting the drift to be a complete fabrication on the part of Parker, much as his statements about what he did or did not say to Nick Tate are completely false.

Professor Morrison is certainly correct that the July 13 draft results in an excess heat that is "visibly close to zero for both cells." I do not contest that, for that is precisely what this curve shift was apparently intended to bring about. So, I must strenuously disagree with Professor Morrison's statement, "Objectively, one has to say that the published paper does not mislead." I believe to the contrary. The paper misleads most egregiously. Professor Morrison's polite recommendation #3 directly governs this matter: "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." In my view, there is very strong evidence that the alleged "two approaches" is really only one method of fitting the data and performing the foremen-tioned inappropriate subtraction of the resulting fitted line. The second "approach" is merely data-averaging into one-hour samples and subsequent shifting of the curve down to give the impression of a null result. More evidence for my contention: If there really had been two methods of analysis applied to the data that would instantly explain the curve shift, why would not Professor Parker *et al.* have immediately shown me the two computer source codes or the notebook-written algorithms. This would have immediately cleared up at least the matter of the origin of the shift. Such was not done, I am quite certain, because two distinct approaches to the data processing did not exist.

I do not believe that Professor Parker *et al.* can be trusted at this time to give forth a complete account of how that curve adjustment occurred. If asked to do so, they might contrive some kind of algorithm with a convenient free parameter or parameters that will show how they processed the data to get the two sets of results.

In conclusion, and in regretted disagreement with the recommendations of Professor Morrison, I must again ask you to convene an appropriate panel to thoroughly explore scientific misconduct on the part of the MIT research group. When this panel renders its opinion, I am confident that it will reach the very conclusion about this experiment that Professor Parker publicly announced on June 7, 1991, and later implicitly rejected, namely that it not "worth anything." This is what he said and he will be held to his word that he believes the curves can be redrawn "anyway that you want":

"I'll tell you what my opinion is of that work, because I was part of it. I don't think it's worth very much. Alright? And that's why it's just published in a tech report. I don't think it's worth very much. I think to do calorimetry is one of the hardest things I ever tried to do. I'd rather stick to plasma physics. . . When you have an open system is where you can make big errors, where you don't know the overpotential, the electrode potential, and so on. These things are unknown. I mean it's really tough and that's why I don't put any stock at all—you can redraw those curves anyway that you want. I don't think that data is worth anything."

Rather than allow the PFC team members further opportunities to fiddle with their data, I recommend that all notebooks, computer files, and printed records relating to their experiment be immediately impounded and turned over to an investigative team for thorough analysis. I requested this inquiry in late August. It is now late October. Your letter to me in early September said that my material would be "appropriately reviewed during the coming several days." Time enough has gone by in deliberations over what to do. It was a good, but not a sufficient course to ask Professor Morrison's opinion, which he rendered the day after he received the material from you. However, it is now time to appoint a panel that will probe to the core of what I steadfastly believe to be evident scientific misconduct.

Both you and others, especially Professor Morrison, should be under no illusion that my hope for cold fusion lies in extracting some evidence of excess heat from the PFC data. You should know that positive conclusions about cold fusion are based on a preponderance of evidence from elsewhere. After the results that will likely be discussed at the third annual conference on cold fusion in Nagoya, Japan, in the fall of 1992, no one will need the PFC's ancient and discredited data. It will be past history, and, indeed, a very sorry history for MIT.

The case of the PFC data concerns scientific ethics: Is it permissible to massage data to one's taste—to artificially present a strong negative impression rather than an ambiguous and possibly positive one? I think not. I do not deny the PFC group the right to discuss the sensitivity of their experimental measurement and then to suggest that because of that sensitivity their result should be read as null. But it was clearly inappropriate to arbitrarily shift processed data to contrive a "desired appearance" and not let the viewers of their report form their own judgements. And do not forget my contention that the very basis for arriving at the original processed data (before the shift) is, in any event, not correct. At the very least, the paper requires substantial revision on that account.

Until I am persuaded by strong technical arguments that deliberate curve-shifting has not occurred, and that an appropriate processing of the data has been applied, I will continue to believe in the need for a thorough investigation. I appreciate Professor Morrison's offer to discuss the content of his memorandum with me, however at this time I do not feel a need for that kind of discussion. The discussion I and several of my colleagues would respectfully like to have with him would be to discuss the overwhelming ancillary evidence for cold fusion, not the PFC paper. I understand his contentions about the PFC work very well and do not need clarification. I look forward to your reply about the next steps that you will take.

Sincerely, Eugene F. Mallove

Exhibit Y
Eugene Mallove's Letter to President Charles Vest
December 31, 1991

Over two months have gone by and still no action has been taken on my request for a full investigation.—EFM

President Charles M. Vest, MIT Room 3-208

Dear Dr. Vest:

My last letter to you regarding the progress of the MIT misconduct investigation was on 24 October. With the exception of a subsequent telephone message from Ms. Laura Mersky stating that you considered the investigation to be a matter of high priority, which you would attend to upon returning from a foreign trip, I have received no communication from you. Would you kindly let me know what actions have been taken?

I can assure you that in 1992 there will be many developments regarding cold fusion, both scientific and governmental. It will accordingly be imperative to resolve the MIT misconduct matter as soon as possible. I look forward to your early reply.

Sincerely, Eugene F. Mallove

Exhibit Z
President Charles Vest's Letter to Eugene Mallove
January 6, 1992

President Vest's penultimate brush-off letter, which suggests no need to do anything further. He checked with his legal counsel regarding the matter of unethical press deception, which he calls "bias in dealing with the media."—EFM

CHARLES M. VEST, PRESIDENT, Room 3-208

Dr. Eugene F. Mallove, Bow, New

Dear Dr. Mallove:

In your letter of October 24, 1991 commenting upon Professor Philip Morrison's report on his inquiry into your concern about possible scientific misconduct in the paper by Albagli *et al.*, you acknowledge Professor Morrison as:

> a gifted scientist and educator, a man of impeccable character and one of the finest human beings I know. I also count him as a friend and a person who made an honest attempt to be fair to all sides in his letter to you. I deeply appreciate the time and effort he made on your and my behalf. With that said, I regret to tell you that I am in substantial disagreement with Dr. Morrison's conclusions about the technical matters that he discussed.

I agree with all you have to say about Professor Morrison and am left with the question of whether I should ignore his conclusions and order the additional investigation which you requested in your October 24 letter. Since receiving your letter, I have sought an additional independent review by Professor J. David Litster, our interim Associate Provost and Vice President for Research, and he has confirmed Professor Morrison's conclusion that there is no basis for further investigation of the charges of scientific misconduct. I have also considered, and sought advice from legal counsel, on whether it was necessary or appropriate for me to investigate your charges of bias in dealing with the media, and I have concluded that is neither necessary nor appropriate for me to do so.

You will recall that Professor Morrison did recommend "that PFC should spend a personal 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." However, your letter of October 24, 1991 rejects this suggestion, so I am not requesting that it be done.

In closing, I do wish to express my regret for the length of time that it has taken to respond to your letter of October 24. Because the Provost had participated in the Albagli paper, I thought it best if I dealt with your concerns personally, and between a long trip in November and the holidays, this is the first opportunity I have had to respond.

Sincerely yours, Charles M. Vest

CMV:cbb

Exhibit Z-1
Eugene Mallove's Letter to President Charles Vest
February 9, 1992

New evidence in the form of Dr. Mitchell R. Swartz's analysis of the MIT PFC data manipulation had come to my attention. My request for a thorough investigation became more emphatic.—EFM

President Charles M. Vest, MIT Room 3-208

Dear Dr. Vest:

I had intended to reply promptly to your letter of 6 January 1992, but new information was unexpectedly brought to my attention, which delayed my response. I am referring to the substance of the draft report, "Semiquantitative Analysis and Examination of MIT PFC Phase-II Cold Fusion Data," which MIT graduate Dr. Mitchell R. Swartz submitted to you on 28 January after a lengthy period of very careful consideration. He has discussed his analysis with me and given me a copy of the report that he submitted to you and to Professor Ronald R. Parker. Let me assure you that Dr. Swartz initiated his investigation independently. I was surprised—shocked would be a better word—to see what he found. I was given a copy of his report to verify the accuracy of Dr. Swartz's use of quotations from correspondence that I had allowed him to see. Later, I requested and received a copy of his draft report.

In your one-page letter of 6 January, you mention consultations with your colleagues, Professor J. David Litster and legal counsel. Based on these consultations and the previous memorandum submitted to you by Professor Philip Morrison, you presumed that you had dealt appropriately with my request for a misconduct investigation. I am sure that it will not surprise you to hear now that I emphatically disagree. I am extremely disappointed that you chose to sweep this serious matter under the rug with what I consider to be a totally inadequate response. I trust, however, that what Dr. Swartz's analysis has revealed about the MIT PFC "Phase-II" data will have dismayed you and your colleagues and will lead you to quickly reverse your dismissal of my charges. It seems that Dr. Swartz has performed a major part of the investigation that I believe should have been your business to require, but which you did not.

To summarize what I understand Dr. Swartz has discovered about the PFC data from his computer processing of the electro-optically scanned published and unpublished PFC results:

(1) Extra data points have been arbitrarily and inexplicably added to the published heavy water curve. These may amount to between 10 and 20 percent of the data points.

(2) The addition of two more "data points" at the portion of the curve, which in the July 10 pre-publication data exists as a time-calibration mark. These points were clearly arbitrarily placed in their vertical positions. They should not have been published as data points if they were mere time-calibration points. (This is not a small matter, see comment below.)

(3) Conclusive evidence that the light water data and the heavy water data were "processed" differently. The light water data that was finalized on July 13, 1989 is a direct and appropriate hourly average of the July 10th pre-publication data. By contrast, the finalized heavy water data of July 13 cannot be obtained from the July 10 prepublication data in a similar manner. Furthermore, Dr. Swartz has shown that no linear transformation exists that successfully maps the July 10 heavy water data into the final published data. This means that whatever technique was used to create the final published data set was highly contrived, that is, was manipulated to give the curve its final appearance. This is in contrast to the impression that Professor Parker has given all along, that the two data sets were treated either identically or in some kind of equivalent manner.

Dr. Swartz uses "polite language" to characterize these findings. He writes, "There appear to be the possibility that some of the data points in the published heavy water curves are most likely artifact, rather than a result of the original experimental data." He states, ". .there does appear to have been an asymmetric algorithm used when the July manuscripts are examined. The light water curve was published essentially intact, whereas the heavy water does appear shifted without any clear explanation for the difference." He writes of the heavy water data, ". . .the possibility of additional superimposed components cannot be excluded. . ." and ". . .much would be clarified by the marking of incidental, questionable, or less clearly derived data points."

I will not be so polite. To be blunt: Dr. Swartz's findings mean that it is absolutely certain that the heavy water experiment data have been manipulated, adulterated, and presented in a way that is completely misleading. I no longer characterize as scientific misconduct what I formerly believed to be an unwarranted shifting down of a data curve, which was a serious enough charge itself. I now consider that an individual or individuals responsible for the preparation of that data are guilty of scientific fraud. I am using the term fraud, which connotes in my mind a deliberate attempt to mislead, to correspond with the definition of "misconduct" in "Section 50.102 Definitions of Sub-Part A, Section 493 of the Public Health Service Act." Furthermore, I would consider any significant delay in dealing appropriately and severely with these findings to be an attempt to cover up scientific fraud.

Some further conclusions that one can reasonably infer from Dr. Swartz's findings: The artifactual time-calibration mark is alluded to in the PFC *Journal of Fusion Energy* paper in Figure 6— in the context of the description of the declining heater power curve. There is no such description in the previous figure in which the data is presented, Figure 5, nor could there have been, since it would obviously have been ludicrous to insert points that look like data and then say, "by the way, they are not really data, they are calibration points." I suggest that these points seem to enhance the impression of a wider y-axis data spread—

a useful impression to give if one is trying to show that any possible excess power is within error bounds. It is inconceivable to me that these points were "inadvertently" put in. The only way that could have happened would have been if the time-calibration "data" were included erroneously in the purported algorithm that processed this data (which would then invalidate the entire purported "least squares" fit that Professor Parker has mentioned in one of his letters). These are dramatic outlying points that I believe may have been intentionally introduced.

I hope that in view of the seriousness of Dr. Swartz's findings, my assertions about them, and in the context of other major faults that I and others have alleged about the MIT PFC publication, you will now act immediately. You could put in place a formal investigation of the entire matter with a panel of experts from both inside and outside MIT, which should include people both favorably disposed to cold fusion, and those either not so disposed or neutral. But let me suggest that based on the facts that now appear so crystal clear, you should ask Professors Parker and Wrighton, who were the leaders of the research in question, to formally withdraw the entire "Phase-II" part of the Journal of Fusion Energy published paper, and the corresponding part of the PFC/JA-89-34 report, and all conclusions that derive therefrom.

I note that in the January 1992 issue of *Physics Today*, there is a news item (see attached) about the Council of the American Physical Society voting last November to adopt a set of guidelines "outlining professional conduct by physicists." The text of the guidelines is printed and contains several statements that I think are very pertinent to the PFC matter. One statement is: "Following publication the data should be retained for a reasonable period in order to be available promptly and completely to responsible scientists. Exceptions may be appropriate in certain circumstances in order to preserve privacy, to assure patent protection, or for similar reasons." The response of Professor Parker throughout this affair egregiously violates this guideline. For example, he has deliberately determined not to reveal the method of generating the heavy water curve of the "Phase-II" results.

In my 24 October 1991 letter to you I specifically suggested that obtaining the algorithms that processed the data would be important. So did Professor Morrison in his 14 October 1991 memo to you: "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." In your 6 January letter to me you did not mention this key suggestion by Dr. Morrison, which I most certainly did not reject. Nor, as you stated in that letter, did I reject Professor Morrison's suggestion "that [the] PFC should spend a personal day or two of work to compute the mean excess power for all four cases..." I must say, you have inadvertently twisted the interpretation of one brief phrase that I wrote, "..in regretted disagreement with the recommendations of Professor Morrison..", which was made in the context of a request for a much more complete investigation, to imply that I did not want to see those alleged PFC algorithms or have the means computed.

Another guideline that the APS Council put forth also has direct application to the matter at hand: "Fabrication of data or selective reporting of data with the intent to mislead or deceive is an egregious departure from the expected norms of scientific conduct, as is the theft of data or research results from others."

In the issue of *Tech Talk* dated 5 February 1992 is published the MIT procedures for dealing with "Academic Fraud in Research and Scholarship." The statement is made: "In addition, the Provost has the authority to mitigate the effects of the fraud by withdrawing MIT's name and sponsorship from pending abstracts and papers and by notifying persons known to have relied upon any work affected by fraud." I expect that this will be carried out for the matter at hand once the final determination has been made.

I note further in the MIT-published statement: "An inquiry must be initiated immediately after an allegation has been made and must be completed within 60 calendar days of its initiation unless circumstances clearly warrant a longer period. If the inquiry takes longer than 60 days, the record of the inquiry shall include documentation of the reasons for exceeding the 60 day period." My initial request for an inquiry and possible investigation was submitted on 18 August 1991. There have been a number of written exchanges between us, including my 31 December 1991 letter in which I asked about the status of your inquiry, following my earlier letter of 24 October. Your letter of 8 January 1992 seemed to terminate the matter as far as you were concerned. Thus, more than 140 days have gone by in which only one MIT faculty member, Professor Morrison, spent part of October 13 and October 14 reviewing (incompletely) my requests. After October 24, but perhaps as late as the first week of January 1992, Professor Litster, the Associate Provost to Provost Mark Wrighton (one of the authors of the questioned research) "independently" reviewed the matter and "confirmed Professor Morrison's conclusion that there is no basis for further investigation of the charges of scientific misconduct."

In your 8 January 1992 letter you wrote, "I have considered, and sought advice from legal counsel, on whether it was necessary or appropriate for me to investigate your charges of bias in dealing with the media, and I have concluded that it is neither necessary nor appropriate for me to do so." Please go back and read my 18 August request for an investigation, because you have again inadvertently twisted my position. I was not and am not concerned about "bias in dealing with the media." Bias in dealing with the news media (such as selecting which newspaper one wants to favor with an interview) is precisely the objective and accepted (if not altogether proper!) role of a news office; I have no objection to such "bias." What I was talking about in my request was the investigation of giving false information in matters that are not small—giving false information to me to prepare an erroneous new release, giving false information to others in the News Office, and giving false information to the greater world about what was said to a reporter. In other words, a calculated attempt to discredit that reporter and newspaper by giving false information. Moreover, a denial of giving this false information persists. Do you not think it is time to do something about that? Is this level of expected personal integrity no longer important at MIT?

In summary, I consider that the inordinately extended inquiry up to this point has not met MIT's own proclaimed standards. I hope that in light of the newly developed information, this time you will understand my points quite clearly. This is a very grave matter which has in my view, I regret to say, already damaged MIT. You cannot sweep research fraud under the rug -- even if it is in the context of a claimed phenomenon with which you, Provost Wrighton, and several others at MIT have little patience. If you do not do the necessary investigative job yourself, I am convinced that others inevitably will.

I am also sending a copy of this letter to my former associate, Kenneth Campbell of the MIT News Office, because I know he is familiar with the ramifications of this type of situation, having witnessed, as did I, another well-known controversy that did not help the image of MIT.

Sincerely, Eugene F. Mallove

cc: Kenneth Campbell

Exhibit Z-2
Eugene Mallove's Letter to President Charles Vest
February 21, 1992

President Charles M. Vest, MIT Room 3-208

Dear Dr. Vest:

On February 10 you received a letter from me that discussed a serious issue connected with research at MIT. Have you initiated any actions on this matter? When might I expect a reply from your office?

Sincerely, Eugene F. Mallove

cc: Kenneth Campbell [MIT News Office]

Exhibit Z-3
Dr. Stanley Luckhardt's Letter to Professor Morrison
March 10, 1992


Dr. Luckhardt
MIT News Photo

While President Vest continues to stonewall, a congenial memo is passed between Dr. Luckhardt (who still has control of the contested data) and Prof. Morrison—some "investigation"! Of course, this concerns the "discredited" cold fusion, so it is not important for MIT to observe the usual standards.—EFM

Plasma Fusion Center
Massachusetts Institute of Technology

MEMO

TO: Prof. Phil Morrison
FROM: Dr. Stan Luckhardt
MIT Plasma Fusion Center, NW16-266
DATE: 3/10/92
SUBJECT: ANALYSIS OF CALORIMETRY DATA IN THE PAPER: D. Albagii *et al.*, *Journal of Fusion Energy*, 9, 133, (1990).

In this memo, I will go through the analysis of the calorimeter data from our 1989 experiment, and show how we arrived at the reduced data presented in our paper. As explained in our paper, the calorimeter used a feedback controlled heating element to maintain a constant temperature in the electrolytic cells. Any production of "excess heat" would show up as a reduction in the heater power level. The heater power level could be accurately measured by monitoring the current and voltage applied to the resistive heating element. The signal of interest is then the heater power ($P_h = I_h \times V_h$). The level of excess heat claimed by Fleischmann & Pons for our conditions is ~79 mW, this excess 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."

In our experiments, and those of others using the open cell type calorimeters, the heater power undergoes a steady drift caused by the loss of solvent from the cell. This loss is caused mainly by electrolytic decomposition and evaporation. As solvent is lost, the level of solvent in the cell drops, this causes the thermal conduction path from the solvent to the top of the cell to increase, thus increasing the thermal "resistance" of the cell and reducing the rate of heat flow out of the cell. To maintain constant cell temperature, the heater power must also decrease slowly. This base line drift trend can be seen in the raw heater power plots attached.

To analyze our heater power data, we first subtract the baseline drift, then any onset of anomalous heating would appear as an excursion from zero. In particular, in the attached Figs. 2&3 I show the raw heater power data P_H for the D_20 cell and the linear regression fit y_H to the raw data. In Fig. 4 the difference $P_X = Y_H - P_H$ is shown. To remove the high frequency fluctuations, the data is time averaged, Fig. 5. We believe these rapid fluctuations are caused by the trapping and escape of gas bubbles from under the Teflon supports for the cell electrodes, (see drawing of the cell in our paper) and by condensation in the cell which causes water droplets to occasionally fall back into the cell. These effects cause fluctuations in the level of liquid and as explained above result in heater power fluctuations.

The time averaged data has one main feature, a slow variation having a 24 hour period. We believe this is caused by daily room temperature variations and/or some sunlight hitting the cells. Both the D_2O and H_2O cells exhibit this feature. Aside from this 24 hour period variation, the data is quite close to zero with some residual fluctuations of order 10-20mW. There does not appear to be any evidence of the claimed anomalous heating event of magnitude 79mW in Fig. 5. This conclusion was stated in our paper.

Sampling rate: As noted in the caption of Fig. 6 in our paper, the data sampling rate for the D_2O cell power was reduced at t=30hours. This was done to save disk space on the data acquisition computer. The regression analysis described above tends to weight the initial data more heavily because of its higher sampling rate. In the attached pages, I show a further analysis in which a uniform sampling rate is used throughout. In the attached Figs. 6-9 the data analysis is carried out again using the uniform sampling rate data and the final time averaged signal <Px> is plotted in Fig. 9. This signal is almost indistinguishable from the results of the original analysis shown in Fig. 5. So our conclusion from this analysis is the same as before.

The analysis of the H_2O cell data is shown in Figs. 10-14. Note the in Fig. 14 there is a residual, 24 hour period variation in the heater power. As discussed above we believe this is an error signal caused by the daily variation of the room temperature and/or variation in sunlight hitting the cells.

I hope this brief summary is of use to you, please feel free to contact me at 3-8606 if you need further information.

Dr. Charles McCutchen
MIT Photo

Exhibit Z-4
Dr. Charles McCutchen's Letter to MIT President Dr. Charles Vest
March 19, 1992

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
PUBLIC HEALTH SERVICE,
NATIONAL INSTITUTES OF HEALTH
BETHESDA, MARYLAND 20892 Bldg. 8, Room 403

I had asked NIH physicist Dr. Charles McCutchen to give me his opinion of the MIT PFC experiment and my interchanges with MIT. Dr. McCutchen, who with MIT Professor Robert Mann had had his own battles with unethical establishments, obliged and wrote to President Vest supporting my position.—EFM

Charles M. Vest, Massachusetts Institute of Technology

Dear Dr. Vest,

Eugene Mallove has sent me material about the MIT cold fusion experiment. Mallove contends that calorimeter data were manipulated to suppress experimental evidence that excess heat was generated when heavy water was electrolyzed. I can see why he is disturbed.

The experiment was not a gem. Of the 1.8 to 2.1 watts that went into each electrolysis cell, 1.25 to 1.55 watts were supplied by the heater. Over two thirds of the power entering the cell did not go into electrolysis. Any signal from excess power would have been diluted by more than a factor three.

Worse was the variability of the total power input. Over an interval of 80 hours, the heater power declined progressively by almost 20%. The experimenters ascribe this to the steady fall in level of the solution ln the cell. This reduced the working area of the heat-loss path through the insulation surrounding the cell, and lowered the power needed to keep the cell at constant temperature.

Power generation by cold fusion, if lt happened, would likewise be revealed by a reduction in heater power. The potential for confusion is obvious.

In his letter of August 8, 1991 to Eugene Mallove, Ronald Parker wrote, "The implicit assumption was that we were looking for a fast turnon of the anomalous heat production and so it was legitimate to subtract out a slow baseline drift caused by depletion of the electrolyte." This would have been a reasonable position, had the experimenters stated it in the paper. They did not do so. The ordinates of their figures are labeled P_x, elsewhere defined as unknown (*i.e.* excess) power, zero is marked on the ordinates, and nowhere is it stated that the height and slope of the curves mean nothing.

Had they wanted to be able to detect constant or slowly varying excess power the experimenters should have improved their calorimeters to get rid of the baseline slope. Their calorimeter depends on the constancy of the thermal conductance of its insulation, and steps should have been taken to make it constant. They could have put a thermally conductive sleeve between the cell and its insulation so as to keep the area of the heat path constant. They could have kept the solution at a constant level in the cell. Better, they could have done both.

They did neither. Instead, they say they subtracted a linear ramp from the data for apparent excess power in each cell. How they picked the height and slope of the ramp is not stated. Because I took their graphs of excess power to mean what they said, I assumed that they thought their procedure would not have concealed constant or slowly varying excess power, had such occurred.

For its own good, and to restore some civility to a contentious field, MIT should look into (1) how its scientists came to perform and publish such a poor experiment, (2) why they either misdescribed their results, making them seem more meaningful than they were or used a subtle correcting procedure without describing exactly what it was, (3) how it came about that data from calorimeters with a claimed sensitivity of 40 mw converged, between drafts, after completion of the experiments, to within perhaps 5 mw of the result that hot fusion people would prefer to see. It might have been chance, but it might not.

—Dr. Charles McCutchen

Like me, Philip Morrison took the paper's results at face value. He calculated for himself the mean excess power shown in the heavy and light water data in the draft of July 10, 1989. A little average excess power came from the light water cell and more from the heavy water cell, which suggests that the height of the curves was not intended to be meaningless. Had the procedure simply subtracted the best-fit ramp from each curve, both of these averages would have been close to zero.

The published paper shows negligible average excess power from either cell. The change between draft and published version is what would have happened had the ramps been adjusted to yield the result that hot fusion experimenters preferred.

I understand that the experimenters have been unwilling to explain their procedure when asked, and have refused to give others their data.

Another piece of apparent *a posteriori* adjustment in the paper concerns the calibrating procedure. The draft said that dry nitrogen was bubbled through the electrolyte to stir it. Nitrogen from from a gas cylinder or from evaporating liquid is dry. But dry nitrogen would cool the cell by evaporation. The nitrogen should have been bubbled through water at cell temperature on its way to the cell. In the published paper, "dry" was missing.

For its own good, and to restore some civility to a contentious field, MIT should look into (1) how its scientists came to perform and publish such a poor experiment, (2) why they either misdescrlbed their results, making them seem more meaningful than they were or used a subtle correcting procedure without describing exactly what it was, (3) how lt came about that data from calorimeters with a claimed sensitivity of 40 mw converged, between drafts, after completion of the experiments, to within perhaps 5 mw of the result that hot fusion people would prefer to see. It might have been chance, but it might not.

I think all parties would agree that if the experimenters thought their method of baseline correction would not conceal constant or slowly varying excess power they should have explained it in detail. If, on the other hand, both the height and the slope of their records were meaningless, they should have said so. I believe this information, whichever it is, should now be published.

Sincerely yours, Charles W. McCutchen

cc: Dr. Eugene F. Mallove, Professor Philip Morrison

Hand-written note attached in copy to Eugene Mallove:

Dear Gene,

Here it is. I hope MIT does something other than stonewall. I think my request is reasonable. If the height and slope of the curves mean nothing, the experimenters should say so in a corrigendum. If the slope subtraction scheme somehow left meaningful slope and height, they should explain why this is so in a corrigendum.

You have my permission to copy and distribute my letter if you think it would help to get the matter straightened out.

Sincerely yours, Charles McCutchen

Exhibit Z-5
Prof. Morrison's Report to President Charles Vest
March 20, 1992

Yet another letter from Prof. Morrison to President Vest, concerning the MIT PFC experiment controversy and the new analysis by Dr. Mitchell Swartz. Morrison's conclusion that "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," is patently not true. The MIT PFC paper on the Phase-II calorimetry is fraudulently deceptive.—EFM

Department of Physics,
MIT, Cambridge, MA 02139

From: Philip Morrison, Institute Professor
(emeritus)
To: Charles Vest, President
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 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 chance 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 rate, 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 any 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.

Exhibit Z-6
MIT President Dr. Charles Vest's Letter to Eugene Mallove
April 1, 1992

President Vest's final brush-off of my request for a full investigation appropriately fell on April 1. Note how he conveniently puts Dr. Swartz's analysis off limits for discussion.—EFM

CHARLES M. VEST, PRESIDENT, Room 3-208

Dr. Eugene F. Mallove, Bow, New Hampshire

Dear Dr. Mallove:

I write in response to your letter of February 9, 1992. Earlier, by letter dated January 6,1992. I had responded to your letters of August 18 and October 24, 1991. I believe the issues raised by your earlier letters have been addressed, and I will not repeat that response here.

As for your comments and requests related to the letter and manuscript of Dr. Mitchell R. Swartz, it is not appropriate for me to comment to you on Dr. Swartz's work.

Sincerely yours, Charles M. Vest

CMV/mmd

Exhibit Z-7
Prof. Widnall's Letter to Dr. Charles McCutchen
April 2, 1992

Now Charles Vest begins to act through subordinates, such as the new Associate Provost, Prof. Widnall, who would later become U.S. Secretary of the Air Force. She tells Dr. McCutchen, "the experimenters have been extremely forthcoming with Dr. Mallove," which clearly does not square with the facts as is evident by all previous

Courtesy U.S. Air Force

exhibits.—EFM

OFFICE OF THE PROVOST
SHEILA E. WIDNALL, ASSOCIATE PROVOST
ABBY ROCKEFELLER MAUZE PROFESSOR
OF AERONAUTICS AND ASTRONAUTICS
ROOM3-234

Mr. Charles W. McCutchen
Department of Health, Education, and Welfare
Public Health Service, National Institutes of Health
Bethesda, Maryland

Dear Dr. McCutchen:

President Vest has passed your letter regarding the experiment reported in Albagli *et al.* on to me as falling within my responsibilities as Associate Provost. I assume that you have written as an interested scientist and not as an official of NIH, which as far as I know, has no official interest in this topic nor any role in the sponsorship of the work.

I think MIT management should take a hand. In the end, this would waste less of its time than trying to stay out of the matter. It should ask the experimenters to publish a corrigendum saying that their original description obscured the fact that they were looking for an abrupt turn-on of power generation by the cell. I see no respectable reason for them not to comply. —Dr. Charles McCutchen

As you are no doubt aware, the paper in question has been the subject of some scientific debate and media attention. And that is entirely proper since the nature and importance of scientific contributions are quite properly dealt with through open debate in the scientific literature, through peer review, in open scientific meetings and through the media. Disputes are part of the scientific process and scientific results are always provisional, based on the data and theories developed to date. The paper you questioned was a contribution to this debate but clearly not the last word.

I believe that most of the issues raised in your letter are more appropriate for a letter to the editor of the journal in question or for a communication directly to the authors rather than a subject for action by MIT. MIT has separately considered the issues raised by Dr. Mallove. Contrary to the viewpoint expressed in your letter, the experimenters have been extremely forthcoming with Dr. Mallove. He has been given data from the calorimeter experiment and several opportunities to discuss the procedure used with the investigators.

In the near future, a memo will be prepared by the experimenters giving more details regarding the analysis of calorimetric data than were available in the manuscript.

I shall see that you receive a copy of this when it becomes available.

Sincerely, Sheila Widnall

Exhibit Z-8
Dr. Charles McCutchen's Letter to Prof. Sheila Widnall
July 26, 1992

Dr. McCutchen has not been impressed with Prof. Widnall's letter. He makes a simple request for an ethical clarification by the MIT PFC of its results. This suggestion was never carried out by MIT, thus maintaining the fiction, to this date, that the MIT PFC results of 1989 were definite and null for excess heat.—EFM

Sheila E. Widnall, Associate Provost
Massachusetts Institute of Technology, Room 3-234

Dear Dr. Widnall

Thank you for your letters and for talking with me on the telephone. I took your advice and telephoned Stanley Luckhardt.

As you probably know, because of the change in the thermal resistance between interior and exterior of its cell with time, the MIT cold fusion experiment could not have detected small, steady power production or small power production that varied linearly with time. However, Luckhardt says that the experiment specifically looked for a sudden onset of power production by the cell. Given this limited purpose, it was legitimate for the experimenters to subtract the the best-fit ramp from the data. So far, so good.

But the description of the experiment does not say that a sudden onset was all the experimenters were looking for. And it never says that ramp subtraction renders meaningless the height and slope of the resulting curve. Someone as sharp as Philip Morrison integrated the excess power signal numerically over the entire length of the experimental runs in the apparent expectation that the result meant something.

To the experimenters, a reduction in the integrated power between one draft and the next would mean only that their ramp subtraction had improved. To a reader who thought the absolute height of the curve was significant, this same reduction could look like data-cooking.

I therefore suggested to Luckhardt that science and civility would be served if the experimenters published a correction explaining that the height and slope of their final data meant nothing, and apologizing for not saying so in the original article. To my great surprise, he objected strongly to this proposal. For one thing, he said, in one part of their experiment, absolute signal height was significant. They had compared the excess power before and after fluid was electrolysed and then replaced. Somehow this was a reason for not explaining that, for most of the data, absolute height was not significant. More surprisingly, reference to the article shows that this absolute-height part of the experiment failed. The data were discrepant, though the experimenters thought that the discrepancy would be smaller if more care were taken to refill the cell to its original level.

I talked to Eugene Mallove, and found him unwilling to concede that the MIT experimenters' sin might be only misdescribing their experiment. He and Mitchell Swartz insist that comparison of the experimenters data before and after reduction shows that the reduction was not done properly, and that the effect was to suppress evidence of excess power that had a sudden onset. I think the experimenters had such a low opinion of both cold fusion and their own experiment that they would not have gone to the trouble of subtly cooking the results. Still, Luckhardt's adamant and puzzling refusal to clear up the confusion in their description make me wonder if they are keeping everything closed because there are things in their data reduction that will not stand examination.

So what should MIT do? Leaving the experimenters and the doubters to resolve the matter is creating a festering sore and a suspicion of coverup. The groups are not communicating effectively. Each accuses the other of non-cooperation.

I think MIT management should take a hand. In the end, this would waste less of its time than trying to stay out of the matter. It should ask the experimenters to publish a *corrigendum* saying that their original description obscured the fact that they were looking for an abrupt turn-on of power generation by the cell. I see no respectable reason for them not to comply. At the same time, they should be asked to give their original data, data reduction formulae and algorithms to the doubters, who should, in turn, be asked to give their objections, in writing, to the experimenters. Out of this head-knocking, truth should emerge.

These requests could not reasonably be refused. They are impolite, but both sides have broken social conventions (the doubters said the experimenters were crooks; the experimenters ran a party celebrating the death of cold fusion), so neither can expect the protections of politeness.

Sincerely yours, Charles W. McCutchen

Exhibit Z-9
Prof. Widnall's Letter to Dr. Charles McCutchen
August 3, 1992

Provost Windall's final stonewall letter to Dr. McCutchen.—EFM

Mr. Charles W. McCutchen
Princeton, New Jersey

Dear Dr. McCutchen:

I'm responding to your letter of July 26. I'm glad that you took the opportunity to speak with Dr. Luckhardt regarding his memo and the earlier paper. I recognize that this area remains controversial and the issue you raised is: Is there anything that MIT as an institution should do in response to the controversy?

MIT, along with all other universities that I know anything about, does not often intrude between its faculty members and their professional actions as scientists. We don't, for example, review the manuscripts of our faculty prior to publication, as do many corporations and government organizations. We are used to a high level of controversy, often between members of our own faculty. Disputes about scientific data, methods and results are common and play a positive role in advancing science. When MIT faculty take public positions as scientists or citizens, it is assumed that they are acting as individuals and not as official spokesman for the institution.

Criteria for institutional involvement in such matters derive from our contractual and institutional responsibilities. Alleged violations of institutional policies by members of our community will bring forth an institutional response. As you undoubtedly know, at our request Prof. Philip Morrison undertook a detailed examination of the issues raised by two individuals concerning the manuscript in question and determined that there was no tangible basis for further institutional action.

I hope that the various groups on our campus who are involved in this research will continue to have collegial, scientific dialogues but I see no basis to direct any of the groups to take specific actions.

Sincerely,
Sheila Widnall
Associate Provost and
Abby Rockefeller Mauze Professor of
Aeronautics and Astronautics

Exhibit Z-10
Dr. Charles McCutchen's Letter to Eugene Mallove
August 18, 1992

Eugene F. Mallove, Bow, NH

Dear Dr. Mallove,

As you can see from the enclosures, I did not get far with Sheila Widnall. I was surprised she did not respond to my point that Philip Morrison, their own expert, had been misled by the paper. I had previously been surprised when Stanley Luckhardt irritatedly refused to consider publishing a full description of the way the experimenters interpreted their calorimetry experiment. I thought this would be a good way to remove some confusion and lower the anger level.

How about taking the advice Dr. Widnall offered in her letter of April 2, 1992, and submitting a letter to *Journal of Fusion Energy*. You might get your points out ln the open for the experimenters to answer. Perhaps the hot fusioneers will stop the *Journal* from publishing your letter. This would be objective evidence that they are brass-knuckle types, evidence you could take to Sheila Widnall to show what happens when one tries to have an "open debate in the scientific literature, through peer review," with MIT scientists.

Sincerely yours, Charles W. McCutchen

Exhibit Z-11
Dr. Charles McCutchen's Letter to Prof. Sheila Widnall
August 18, 1992

Dr. McCutchen's final word to Provost Widnall fell on deaf ears.—EFM

Sheila E. Widnall, Associate Provost
Massachusetts Institute of Technology, Room 3-234

Dear Dr. Widnall,

Thank you for your letter of Aug. 3, 1992. I do not envy you in having to deal with matters that take time away from the constructive business of the university. But consider, it is the enforcement of decency among scientists that makes collegiality possible. Without sanctions for bad behavior, science becomes a jungle. Like it or not, you knuckle-rappers are keepers of the flame.

MIT is using formal procedures to evade responsibility. You and I agree, I think, that bad scientific ethics are a university's business. So far, so good. But MIT thinks that getting Philip Morrison to give the matter a once-over-lightly discharges its responsibility. (Substitute '"Eisen" for "Morrison" and you have the beginning of the Baltimore affair.) I explained that Morrison was misled by the inaccurate description of the experiment, the very thing that I object to. That he was thus misled shows that a correction should be published. You did not respond. This is stonewalling.

Collegial mechanisms will not resolve the issue. They work when all parties play fair. The MIT hot fusion people are not playing fair. They published a misleading description of an experiment. The errors ln the description were important. The collegial rules require that they publish a clarification. They refuse to do so.

What can the cold fusion people do now? If they submit a complaining letter to the journal that carried the original paper, the hot fusion people will probably try to prevent it from being published, and likely succeed. There will be more anger on both sides, and a lot less collegiality—all because MIT management cannot bring itself to make the hot fusion people, its own employees, behave like gentlemen. (Remember that what they refuse to publish is their own, verbal description of the way the major part of the experiment worked.)

I am sorry that MIT continues to tough it out. Apparently the university feels it need not be fair to cold fusion people. Perhaps it is afraid to be fair. Luckhardt's negative response to my proposal that a correction be published suggests that hot fusion patriotism *requires* one to be unfair to cold fusion people. Why else (unless there is real data faking that they are trying to hide) will the hot fusion people not publish a correction that would blunt some of the anger, and enhance their own reputation for honesty?

Sincerely yours, Charles W.

Press Release
MIT News Office
April 12, 1989

"MIT Professor Has a Theory to Explain 'Cold Fusion'"

MIT Provost John Deutch said, "MIT is a place where creative individuals are encouraged to address scientific subjects of the greatest significance. We are pleased to see Professor Hagelstein proposing an explanation for 'cold fusion' and we are encouraging investigators both here and at other research institutions to continue their work on this most surprising phenomenon, which may have enormous consequences."

[Later, Dr. Deutch became Director of the CIA in the Clinton

cc: Dr. Eugene F. Mallove

Ponder the unthinkable. Question the status quo. Live in the world as well as in your own nation. Dream of a better future, but contribute to the present. Share your talents. Commune with all people. Be steady friends and bold companions. Be honest in all that you do.

—MIT President Charles Vest's Commencement Address June, 1998.

Key Cold Fusion Publications of MIT Graduate, MIT Professor Peter L. Hagelstein

• "Coherent Fusion Theory," presented at the ASME Winter Meeting, San Francisco, Dec. 1989, paper TS-4.
• "Coherent Fusion Theory," *Journal of Fusion Energy*, Vol. 9, No. 4, 1990. pp. 451-463.
• "Status of Coherent Fusion," DoE Annual Report, January 1990.
• "Status of Coherent Fusion Theory," *Proceedings of The First Annual Conference on Cold Fusion*, March 28-31, 1990, Salt Lake City, pp. 99-118.
• "Coherent Fusion Mechanisms," AIP (American Institute of Physics) *Conference Proceedings #228, Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, 1990, Editors: Steven E. Jones, Franco Scaramuzzi, and David Worledge, pp. 734-781.
• "Coherent and Semi-Coherent Neutron Transfer Reactions," Conference Proceedings, Vol. 33, *The Science of Cold Fusion*, Ed: T. Bressani, E. DelGiudice, and G. Preparata, SIF Bologna, 1991, pp. 205-209.
• "Coherent and Semi-Coherent Neutron Transfer Reactions,"*Proceedings of the Third International Conference on Cold Fusion (October 21-25, 1992), Frontiers of Cold Fusion*, Ed., Hideo Ikegami, Universal Academy Press, Inc., Tokyo, pp. 297-306.
• "Coherent and Semi-Coherent Neutron Transfer Reactions I: The Interaction Hamiltonian," *Fusion Technology*, Vol. 22, 1992, pp. 172-180.
• "Coherent and Semi-Coherent Neutron Transfer Reactions III: Phonon Generation, "*Fusion Technology*, Vol. 23, 1993, p. 353-361.
• "Coherent and Semi-Coherent Neutron Transfer Reactions II: Dipole Operators," submitted to*Fusion Technology*, 1993.
• "Coherent and Semi-Coherent Neutron Transfer Reactions IV: Two-Step Reactions and Virtual Neutrons " submitted to *Fusion Technology*, 1993.
• "Lattice-Induced Atomic and Nuclear Reactions," *Transactions of Fusion Technology*, *Proceedings of the Fourth International Conference on Cold Fusion* (Lahaina, Maui, Hawaii, December 6-9, 1993, Vol. 26, No. 4T, December 1994, pp. xi-xii.
• "In Memory of Julian Schwinger," *Transactions of Fusion Technology*, *Proceedings of the Fourth International Conference on Cold Fusion* (Lahaina, Maui, Hawaii, December 6-9, 1993, Vol. 26, No. 4T, December 1994, pp. xi-xii.
• "A Possible Mössbauer Effect in Neutron Capture," *Hyperfine Interactions*, Vol. 92, 1994, p. 1059-.
• "Update on Neutron Transfer Reactions," *Proceedings of the Fifth International Conference on Cold Fusion* (9-13 April 1995, Monte Carlo, Monaco), pp. 327-337.
• "Proposed Novel Optical Phonon Laser Pumped by Exothermic Desorption," *Bull. APS*, Vol. 40, 1995, p. 808.
• "Anomalous Energy Transfer Between Nuclei and the Lattice," *Progress in New Hydrogen Energy: Proceedings of the Sixth International Conference on Cold Fusion*, October 13-18, 1996, Japan, pp. 382-386.
• "Models for Anomalous Energy Transfer,"*Proceedings of the Seventh*

Key Cold Fusion Publications of MIT Professor Keith H. Johnson

• "Hydrogen-Hydrogen/Deuterium-Deuterium Bonding in Palladium and the Superconducting/Electrochemical Properties of PdH/-PdD," K.H. Johnson and D.P. Clougherty, *Mod. Phys. Lett. B*, Vol. 3, 1989, p. 795-.

• "Jahn-Teller Symmetry Breaking and Hydrogen Energy in γ-PdD 'Cold Fusion' as Storage of the Latent Heat of Water," K.H. Johnson, *Transactions of Fusion Technology*, *Proceedings of the Fourth International Conference on Cold Fusion* (Lahaina, Maui, Hawaii, December 6-9, 1993, Vol. 26, No. 4T, December 1994, pp. 427-430.

• "Method of Maximizing Anharmonic Oscillations in Deuterated Alloys," U.S. Patent 5,411,654, Brian S. Ahern, Keith H. Johnson, and Harry R. Clark, Jr., Filed July 2, 1993, Date of Patent, May 2, 1995.

• "Water Clusters and Uses Therefore," K.H. Johnson, Bin Zhang, and Harry C. Clarke, US Patent 5,800,576, Filed, November 13, 1996, Date of Patent, September 1, 1998.

Cold Fusion Publications of MIT Graduate Dr. Mitchell R. Swartz

JET Energy Technology has contributed through our R&D, high standards, and quality control.

Publications on Research and Q/C

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