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Some Personal Reflections on Scientific Ethics and the Cold Fusion "Episode"

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This note was prepared in response to Dr. Scott Chubb's invitation to discuss issues concerning ethics in scientific research that I may have observed during the hectic period following the public announcement of "Cold Fusion" (CF) by Drs. Pons and Fleischmann in 1989. I would like to preface this note with some reflections on select "events" I was personally involved in as editor of *Fusion Technology (FT)* and as one of the early researchers in CF (who has persistently kept going!). Then I will discuss several ethical "issues" relating to scientific conduct from my viewpoint as an editor and researcher in the field.

Reflections

In 1989, as I was preparing to leave on a trip to Japan, I received a call from Prof. Steve Jones of Brigham Young University (BYU) asking if *FT*, for which I am the editor, was an appropriate journal to publish a paper on CF. He also asked how quickly we could get it into print. My reply was that I didn't know what CF was, but if he felt it to be appropriate for *FT*, I felt confident it would be. Since I had to leave for the airport immediately, I asked him to mail the paper to me so that I could look it over upon my return in a week. As I stepped out of the plane in Tokyo, my Japanese host waved a copy of the *Asian Wall Street Journal* in my face and excitedly inquired whether I knew about CF! (Too bad that I hadn't had time to learn that from Steve!) Later I realized that this paper was the one which finally ended up in *Nature*, after the "famous" controversy about publication disclosures between Jones and Pons/Fleischmann.

My next encounter with CF was at the initial congressional hearing in Washington D.C. on the topic. I was selected to provide input from a fusion researcher known for innovative research who might comment on CF from a "neutral position". Thus, I was "squeezed" into the testimony order between the originators of the field, Pons and Fleischmann, and a strong opponent of CF, Harold Furth, the then director of the Princeton Plasma Physics Laboratory. The hearing was unbelievably intense and the hearing room was "hot," overflowing with an excited audience and the flashbulbs from the media. Despite the fact that I was not the center of attention, I too felt the pressure. As the frenzied day wore on, I felt tired—both physically and mentally. I have often wondered how the others more solidly in the "spotlight" felt. (As they freely admitted later, such pressure eventually took a toll on Pons and Fleischmann, forcing them to try to isolate themselves from the turmoil). The audience generally "took a break" as I spoke, but at one point my comments drew attention. In contradiction to statements by Pons and Fleischmann about how benign and safe CF was, I speculated that we did not yet know enough about the phenomena to predict future direction. Thus, I stated the possibility could not be ruled out that “a cell using D-T might be used as a neutron source or that a deuterium loaded cell might be a good source of tritium.” After the hearing, a CIA agent caught me in the hall and warned that someone like myself with a “Q clearance” should not publicly air such sensitive speculation. As it turns out, my speculation had some validity. For example, several researchers such as Dr. Tom Claytor of Los Alamos National Laboratory (LANL) have reported conditions where tritium is a favored product. I have not yet heard about any D-T experiments however. Under other conditions, e.g. in my own research on thin-film electrodes, cells appear to operate almost radiation free. We still do not understand well the connect between the various CF reaction channels whereby radiation or tritium is sometimes enhanced and in other cases subdued.

While I have attended a number of scientific meetings over the years, my experiences at the various early CF meetings also stands out in my mind. An almost “carnival atmosphere” was created by the combination of reporters, entrepreneurs, garage inventors, curious on-lookers, politicians, financial brokers, and scientists at the initial LANL sponsored meeting in Santa Fe and at the first “International Conference on CF” (ICCF) series” in Salt Lake City. Discussions ranged the full spectrum from theoretical quantum electrodynamics to new nuclear particles, on to the price of palladium on the commodity market!

Then there was the “famous” NSF-EPRI meeting in Washington DC where the NSF ended up withdrawing “official” sponsorship at the last moment due to the swing in opinion against CF. Despite this controversy, Edward Teller attended this meeting in a wheel chair (due to a recent operation) and provided a guiding example of an open scientific mind by freely entering the discussion. Instead of ruling CF out due to lack of theoretical explanation, he suggested that a new particle, dubbed “meshuganon,” would be needed (and might actually exist) to explain the observations reported by Pons and Fleischmann. Indeed, this still may be the case!

The executive board for the ICCF series, concerned about the “circus-like” atmosphere in these early meetings, decided to move the next meetings out of the US so that only the serious researchers with sufficient resources could attend. This was an attempt to reduce attendance by garage-type researchers, curious on-lookers, and reporters. However, it was only partly successful as the carnival-like influence slowly subsided.

ICCF meetings followed in Como, Italy; Hawaii, U.S.; Nagoya, Japan; Monaco; Hokkaido, Japan; and Vancouver, Canada. Many CF researchers complained, as expected, that they did not have the resources to attend. Still, despite the reduced attendance, these meetings had the potential for strong external impact. For example, when Yamaguchi (NITT) announced his famous experiment correlating heat and He-4 at the Nagoya ICCF meeting, the price for NITT stock, one of the largest stock-held companies in the world, jumped several points, representing billions of dollars! This price gain slowly and quietly vanished in the following months as Yamaguchi’s experiment came into question, a phenomenon that has occurred all too often in this unpredictable field.

Looking back, the CF field has caused grief for many key persons who became “too” strongly involved. Pons and Fleischmann left the US for France (not a bad “exile” though, considering the impressive laboratory and surroundings there). The President of the University of Utah was forced to resign as a result of issues raised about CF funding procedures. A graduate student at Texas A&M was “side-tracked” in his thesis work due to (unfounded) accusations of “spiking” samples with tritium published (without substantiation) in *Science* magazine. John Bockris at Texas A&M, was bombarded with University-appointed investigating committees and, as a “crowning blow” was forced off-campus with the second International Meeting on Low Energy Nuclear Reactions that he hosted. Gene Mallove found it necessary to step down from his scientific information post at MIT following publication of his book, *Fire from Ice*. Peter Hagelstein faced a hostile promotion committee at MIT after his early theoretical work on CF; Terry Bishop, science reporter for the WSJ was sidelined because of his strong coverage of CF. The list could go on. Why should such intense controversy and drastic personal repercussions develop over a scientific field?

Certainly the unconventional manner in which Pons and Fleischmann introduced CF by announcing it to the press initiated the controversy which eventually polarized the field into camps of “believers” and “non-believers”. The fundamental reason behind this emotional approach to CF was, in my view, the tremendous impact that CF, if proven true, could have. Consequently, the vast amount of money and the prestige at stake brought out the “best” and the “worst” in people.

More could be written about personal “reflections,” but let me now turn to scientific ethics and publication issues. As editor of three scientific journals, *Fusion Technology (FT), Laser and Particle Beams,* and *Journal of Plasma Physics*, I feel especially well qualified to comment on publication issues. Due to my editorial decision to accept CF papers for review, in contrast to other prominent scientific journals in the U.S. (and elsewhere), a number of articles on CF research have been published in *FT*. This forced me into the forefront of the controversy about CF publication policy.

Publication Policies and Related Issues

The early view in 1989 was that CF represented a forefront of basic fusion science and technology. Thus I felt that CF was most appropriate subject matter for *FT*, a journal that till then exclusively covered ‘hot’ fusion. The early work was quite varied relative to scientific content so papers could vary tremendously in quality. However, since papers in *FT* undergo rigorous peer review, I had little concern about poor papers slipping through—I was confident the review process would filter out any “problem papers”. It seemed fundamental to me that the primary object of a scientific journal is to enable communication of fundamental science in an area that it covers. So, for these reasons, initially, I actively encouraged potential authors of CF papers to submit papers, distributing information about *FT* at the early CF meetings. Soon scientific sentiment turned against CF, and editors of *Nature* and the APS Physics journals quickly took the stance that CF did not have a “scientific” base. Thus, they would not even send papers on the topic out for review, shutting the door for any CF papers in these key journals. Despite that example, I stuck with the original decision that papers passing review should appear in *FT*. As a result, by default, *FT* virtually “cornered” the market for CF papers! A backlash quickly followed, with “hot fusion” members of the *FT* editorial advisory board and some readers vocally questioning my decision. Some declared these papers would “destroy” the journal. At that time, I strongly reiterated (and continue to do so) that the purpose of a journal is to communicate basic science and technology so that papers which can pass review should be published as long as the topic is consistent with journal coverage. I emphasized that I did not feel that I had the right as editor to arbitrarily turn papers away because they were from a "questionable" field. Critics countered that the field was so new and ill defined that reviewers had no basis for decisions and were letting unfounded speculative papers through. Others were more radical, claiming that the original Pons and Fleischmann paper had been shown wrong, hence any other articles on the subject should be rejected outright. Yet a few others claimed that I was using reviewers from the CF community who were obviously biased and let "garbage" through. I countered these comments saying reviewers were selected who had solid scientific credentials, and they were instructed to insure scientific accuracy in the papers. However, I supported the stance of some reviewers that due to the "newness" of the field, "conceptual" studies meeting this standard (i.e. were not flawed in a known or provable scientific way) could be accepted. I also explained that reviewers from the nuclear physics community were used to reviewing those papers concentrating on nuclear phenomena, from the chemistry community for papers where electrolytic phenomena were important and from the materials science community where solid-state aspects was stressed.

As this heated debate continued, I eventually, though reluctantly, agreed that a third referee from "hot fusion" should be added to all CF paper reviews to avoid any possibility of excess enthusiasm (bias) from the small CF community frequently involved in reviews. A further concession was that CF papers should deal with topics directly related to nuclear phenomena, not "extraneous" topics like electrochemistry or pure calorimetry (despite the fact that these represent the "technology" aspects of the field). This is *FT*'s current position (This could change in 2001, however, since just after this article was drafted, I announced my retirement as editor in 1/2001 after 20 years in that position). I would note there is a continual small but significant flow of CF papers being published in CF. The rejection rate remains noticeably higher than that for hot fusion, possibly due to the extra reviewers variable quality of research done in a field like CF that is out of the mainstream scientific community.

Another criticism of my editorial policy on CF has been that since I have done research on the topic, I must be biased in favor of it. It's true that I have had papers in most ICCF meetings, starting from the original LANL meeting in Santa Fe. This criticism, in my view, amounts to a double standard. My initial selection as *FT's* editor, and the other two journals, was based on my recognized research on fusion, lasers, and plasma physics. This track record was assumed to provide me with better insight into the technical content of the papers, and allow me to select top reviewers. In universities, teaching and research are well recognized as reinforcing each other. The same is certainly true for editing and research. Why wouldn't the same be true for CF? Again, this ethical issue is left to the reader to consider, namely, do we want general managers as journal editors or, do we want experts from the field, despite possible conflicts of interest?

In conclusion, the issue of whether my *FT* position, as opposed to *Nature*'s closed-door policy, is proper for a scientific journal must be left to the reader. The question to be answered, in my opinion, is which policy will advance science best in the long run? To rephrase the question, we might ask if the publications in *FT* have communicated new scientific information or have they mislead readers? Have the editors who refused to send CF papers out for review protected the scientific community from being confused by "garbage" papers or have they missed the opportunity to provide an outlet for important scientific data?

Bulletin Boards and New Journals

In some people's view, CF research went "underground," following the Pons-Fleischmann backlash, totally vanishing from well-known government and industrial laboratories. However, since experiments could be done cheaply in a basement laboratory, a number of individual inventors jumped into this fledgling science. Others with an interest in science, but with full time jobs in other fields, closely followed CF research and interacted via the growing interchanges on the World Wide Web. Due to the rapid growth and popularity of the Internet and inexpensive computer publishing techniques, this new "community" was drawn together by the development of bulletin boards such as *Vortex* and *Sci.Physics.Fusion*. This effort was supplemented by numerous individuals and company web sites like BlackLight Power, CETI, INES, etc., and through individual e-mail distributions using extensive address lists. Also, the rapid emergence of newsletters like *Fusion Facts* and new "unreviewed" (or "semi-reviewed") journals such as *Infinite Energy, Journal of New Energy*, and *The Cold Fusion Times* provided alternative communications channels that pulled this "underground" CF community closer together.

These avenues substituted fairly well for the "blocked" traditional journals in terms of communication among researchers in CF. However, such publications lacked the prestige offered by peer reviewed journals, hence failed to gain notice outside the small CF community. The wide mixture of quality of the papers in such publications further hampered these journals/web sites from gaining the attention of the mainstream scientific community. Still a major benefit of these new publications was that this open and fast interchange of ideas provided important information and a feeling of "community." At the same time, there were significant problems. Without any control, some participants went overboard, personally insulting other workers in open ("public") postings rather than discussing scientific issues. Such actions have caused a number of serious researches to turn away. Other web users unfairly monopolize the space by sheer volume of verbiage. Gossip mills abound since there are no checks and balances to help regulate postings. Still, due to the lively interchanges that developed, some have claimed that this type of communication negates the need for peer reviewed journals. I certainly question that. The aforementioned problems ultimately reduce many of the discussions and critiques to superficial interchanges representative of a "debating society". While traditional peer review procedures can (and do) sometimes error, this time honored and tested technique has been found to work in terms of furthering basic knowledge in science. While, like our system of democracy, there may be a better approach, we have yet to find it.

In summary, the network of web sites, newsletters, etc. set up for CF represents one of the early examples of how this new type of scientific communication affects a scientific community. Such communications are clearly destined to play an enhanced role in scientific fields in the future, but the pressing issue is whether they will have a positive or a negative impact on progress. The jury is still out on its effectiveness, however. In view of the lack of restraint by some participants, I personally worry about where this new media is headed for. Much will depend on the etiquette and restraints that are exhibited by present practitioners, since this will set the tone for the future.

Experiences with Web Interchanges

Shortly after presenting startling (and controversial) new results on possible nuclear transmutations in thin films undergoing electrolysis at the 1996 Texas A&M meeting on Low Energy Nuclear Reactions, I found myself the center of attention in the Web discussions. I was soon overwhelmed with the postings, which were not confined to questions, and criticisms, but also included a generous dose of "irrational" praise and insults. I tried to selectively sort out and analyze concrete issues posed in these various postings. However, this required time before I could fully respond. Consequently, in the meantime others sometimes took it on themselves to respond "on my behalf," sometimes brilliantly, sometimes in an off-base fashion. In the midst of these pointed interchanges, several lengthy critiques were posted that included a bizarre mixture of technique issues and personal attacks. Rather than enter a non-productive "name calling" contest, I deliberately ignored those postings. Later I came to realize that some others felt my lack of response meant I conceded. This situation poses a serious dilemma for researchers in a field where e-mail/web insults become a norm for some participants. Looking back over this, I still favor the approach of trying to avoid 'shouting matches' which frequently solve nothing scientifically, while generating bad feelings on all sides.

Several other self-appointed critics sent "critiques" of my work out by e-mail to long lists of people without raising the issues with me first. Such critiques have also come to me in a "spam" fashion. The validity is uncertain, and the temptation is to trash them. Still, even if one only looks at the titles, I suspect they can have a subconscious influence on attitudes towards a subject or research. In any case, this e-mail procedure has a definite negative effect. Some issues raised were simply misunderstandings that could easily be cleared up through prior discussion. While these points may be explained in follow-up e-mails, readers are already focused on the initial erroneous interpretation, causing misconceptions to linger on. With a "critique" posted in this way, without an adjoining response, the experimenter is in a sense judged "guilty" without a "trial". The response, even if provided quickly, faces a built-in bias formed by this "first impression." We are all well aware of this psychology from experience with the newspapers and magazines. Readers typically remember the first news release printed, even if it is retracted later. Indeed, my policy as a journal editor is to send copies of "letters to the editor" to persons cited in them, asking if they wish to provide a response that can be printed *along with* the original letter. Many on the e-mail circuit don't understand the importance of this courtesy.

Along these lines, I was shocked to find that one of the new CF newsletter-like journals reproduced, in full, my paper from the Texas A & M meeting which was published as part of the meeting proceedings in the *Journal of New Energy.* This was clearly a violation of copyright, but the issue was not pursued. Along with the article, a critique taken from a bulletin board, containing many misleading comments was also printed. The editor had never contacted me about the article or the critique!

Recently another of these types of "journals" ran a long article describing "results and conclusions" from a "second independent verification" I did for a small company. In fact, I had not finished my review and had not reached a conclusion about these experiments. Further, the company had agreed not to release any information about my study without prior approval. But, I was never contacted by the editor or by the writer who prepared the article to confirm its content. The possibility of a lawsuit passed through my mind, but I instead complained loudly to those involved. Interestingly, they didn't seem to be too concerned about my complaint, again illustrating the confused ethics that has developed in these new type journals.

Clearly, repeated irresponsible actions like these can offset the "value added" of this new avenue for fast, low cost communications. What can be done about this type of unprofessional journalism? The editors should be asked to consider attending workshops on professional ethics in journalism (if there are not such workshops, the news media and journals should develop a series). Alternately, researchers could simply stop using such web sites and journals. However, that is, no doubt, unlikely to happen, just as attempts to control the paparazzi by asking people not to purchase distorted tabloids have met with little success.

Communication Speed vs. Peer Review

Even in the brief time since the original CF announcement, scientific communications have changed drastically due to explosive increase in use of the web. The original Pons and Fleischmann "draft" paper was leaked to the world by fax without their knowledge or consent. Soon after their TV announcement, I received a copy, which had been reproduced so many times it was virtually unintelligible. If this fax proliferation didn't make the Guinness Book of World Records, it should have. A similar circumstance released my transmutation paper prematurely in 1998. My talk on nuclear transmutations was "leaked" via bulletin boards and Web sites based on a few preprints I distributed at the 1998, Texas A & M meeting, despite their being marked "draft, not for distribution." However, the consequences of this leak were quite different from the unauthorized faxing of the Pons and Fleischmann paper. The subsequent rapid exchange of information afforded by the web lead quickly to critiques, and comments on my work as noted earlier. In the Pons and Fleischmann case, general evaluation only slowly followed, e.g. via analysis at MIT of a video taping of sections of the TV coverage and the eventual outflow of data from attempts at other laboratories to duplicate the experiment. Had more rapid electronic communications been available, some of the confusion surrounding the Pons and Fleischmann announcement might have been avoided. Persons trying to duplicate the experiment could have better access to information about procedures and could have interacted more easily with the experimentalists themselves.

Clearly the web bulletin boards fill a very important role for rapid scientific exchanges. However, this is best used in a concurrent flow of papers through peer-reviewed journals. The review process is time consuming, but it serves to sort out and distill the fundamental results, providing for a more calmed deliberate interchange that ultimately enhances scientific progress in the field. The unfortunate refusal of editors to receive CF papers disrupted the normal system, and left the bulletin board crowd in charge. Without a counter-balance for peer review (except for *FT* which could not handle this volume and variety of topics involved), CF was left in a confused state. It is difficult, if not impossible, to sort through the bulletin board materials to focus on real issues. Now that bulletin boards of this type have spread widely, we can expect an even more explosive and disastrous episode if a situation like the CF news announcement occurs again. The best defense against reoccurrence of a CF-type episode in the future is for the major journals to assume their rightful role of an "open door" for papers passing peer review.

Scientific Integrity and Openness

Integrity in science has been a "hot issue" throughout the saga of CF. This issue has sometimes been confused with the problem of "openness" of discussion. The latter, which has been seriously hampered by the dominance of various companies and entrepreneurs in CF who hope to gain advantage through patents and proprietary information. Integrity and openness are, in fact, entirely different issues. Integrity must be maintained at all costs; openness is highly desirable, but is not always possible. In order to succeed, companies must often protect their base intellectual property.

This point was emphatically brought home to me when Martin Fleischmann stated during a coffee break discussion at the Monaco ICCF meeting, that ICCF meetings were becoming "too academic." The "real" developments, he continued, were not being discussed in the meeting, but in company conference rooms. Why did this situation become so pronounced in the field of CF? The situation largely stems from the fact that government-funding agencies (especially in the US) refuse to receive CF research proposals on the basis that the phenomena "doesn't exist". Thus, I recall an Air Force Research Board Review I attended some years ago, where the directors for the Air Force and Navy offices of basic research were asked to cite their "major achievements for the year". "Stopping wasteful funding on CF" was at the top of the list for both! Thus, small companies and individuals end up, by default being the main funding source for CF. As a result, they in turn, rightly feel that their investment is entitled to protection via patents and secrecy. Normally, in other fields, government funded research provides a balance of funding that leads to a flow of open publications covering the basic science underlying the field, while the practical technology funded by companies remains proprietary. In CF, "open" science only comes from a very few academics or others who can undertake research without "strings" attached. A majority of the work is more guarded. This abnormal imbalance gives observers a very distorted impression of CF research and also stifles interchanges between researchers in the field.

Let us now turn to issues of scientific integrity. Integrity, in the sense of avoiding fraud, has been an all too frequent topic of discussion in the CF field. Some even accused Pons and Fleischmann of fraud, or of purposely misleading others trying to replicate their results. To my knowledge, there is absolutely no truth to these innuendoes. As Pons and Fleischmann stated early on, and history has verified, their experiments were not reproducible due to unidentified factors in the materials science of the electrodes. Generally, when an electrode "worked," all from that batch of Pd did so, and conversely if it did not work, none did. (This problem is now generally thought to be associated with microcracking that occurs in some electrode materials during the expansion and stresses caused by loading. Thus, in my own research I have tried to avoid this problem by the use of thin sputtered films for the electrodes. These films have more elasticity so that the tendency to crack during loading is reduced.) While Pons/Fleischmann explained this problem at various meetings, many refused to accept their explanation, claiming something was being withheld. As time passed, it became clear that Pons/Fleischmann had indeed provided all of the factual information known about the electrode problem. However, they were significantly hampered in "openness" in some aspects of the research by overzealous sponsors requesting tight reigns on intellectual property, a situation that remains all too common in the company-dominated field of CF.

Others have somehow tried to associate fraud with the initial introduction of CF via a public news announcement. That view is that the news announcement was purposely distorted for personal gain. To my knowledge, that is simply not true- The information provided was a factual presentation of the data as these researches saw it at the time. However, the news release approach is a most serious break from traditional behavior in any scientific field. In retrospect, it must be noted that the pressures on Pons/Fleischmann at that time were tremendous. Indeed, I would suspect that others who have been so vocally critical of them may have turned to this route if they were placed in a similar situation. Still, the disclosure of scientific results via new releases is certainly to be avoided if at all humanly possible. Such actions are certain to create a "backlash" in the community that interferes with (or may even stop) the scientific search for truth. Everything from the scientific community's evaluation of the basic science to funding for the field can become grossly distorted by the emotions set in force. Indeed, in the case of CF, the resulting "backlash" soon isolated the field from the mainstream scientific community.

Scientific Community Reactions

The "backlash" associated with premature press releases is one of the key "lessons learned" from the CF episode. Yet, human nature is such that things of this type have a way of repeating themselves, as memories grow dim. In fact, circumstances tempting scientists to follow a similar route will surely occur more frequently as science progresses in modern capitalist society. Thus, the best defense may be to ask the question: how should the scientific community react if such releases do occur?

In the CF case, the immediate response was, unfortunately, that many other scientists, both enthusiasts and critics, mirrored Pons and Fleischmann's actions, racing to premature news releases of their own results or views. In retrospect, it is amazing how many groups reported neutron measurements that were later found to be flawed due to noise pick-up from arcs in nearby electric circuits, changes in the temperature of neutron detectors, etc. Yet other groups did unrealistically short experiments, ignoring the need for a lengthy time period for loading the thick electrodes, and to pronounce that they had found nothing, so P/F were "wrong". In such a news-dominated atmosphere, persons in the field become severely tempted to go for a headline at all costs. In the case of CF, this tumult of confusion caused research managers, funding officers, journal editors and others to jump to premature decisions and ultimately cut CF researchers off from the mainstream scientific community.

Had the response by other scientists been more constrained—had others in and near the scientific community held to the norms expected of Pons and Fleischmann—the repercussions of the news announcement would not have been so disruptive of the scientific process. As to who is "guilty"—Pons and Fleischmann may have initiated the problem, but clearly many others surrounding the CF community share the guilt by premature reactions and unreasoned responses. The need for all involved to avoid "shooting from the hip" in future episodes of this type is perhaps the most important "lesson learned" from CF.

Conclusion

With the growing pressures on researchers in modern society, we must work hard to preserve an atmosphere where the primary objective is to "seek the truth". Clearly, the turmoil and divisions in the CF area created by persons both within and without the field confused and retarded this search for truth. With human nature being as it is, it is hard to believe that we can prevent a repeat of the CF episode in future areas where high stakes of money and prestige are involved. The education of upcoming scientists, journalists, research managers, etc. in scientific ethics is the best defense. Indeed, my only formal training in the area was a one-hour course on "professional ethics" required of all science/engineering students when I was a senior in college. The subject was not mentioned in my graduate studies and unfortunately, no one encouraged me to seek out a course on the topic. I suspect my experience is not unusual. Universities and professional societies need to view the situation with an eye towards expanded courses, workshops at scientific meetings, etc. dealing with such matters. Case studies should be an integral part of such courses. While other studies can be found, the "CF episode" is clearly a classic one for inclusion.

Other areas of science may appear to be immune to a "CF type fiasco," but that is not so. Who would have predicted this would occur in the field of fusion? The feeling that it "can't happen here" sets the community up to be taken by surprise and lead to hasty, ill conceived responses by others in the community. Thus, it behooves the scientific community as a whole to think some more about "lessons learned" from the CF episode. The present series of papers on ethics put together by Dr. Chubb should be a valuable source of information and insight into what happened and precautions to take to prevent a reoccurrence in the future.