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Introduction to the Special Series of Papers in Accountability in Research Dealing With "Cold Fusion"

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Background

During the 14th century, the noted theologian and philosopher William of Ockham identified and applied the "law of economy," as a fundamental postulate of logical thought. Subsequently, Galileo and others used this "law" as justification for the notion of "scientific parsimony." This idea, which is also often called "Ockham's Razor," states that simplicity should be the cornerstone of scientific logic: given a choice between competing theories of a particular phenomenon, the simpler explanation should be selected in preference to the more complicated ones.

Despite the fact that since the time of Galileo, Ockham's Razor has helped to advance science, it frequently fails. One important reason for this is that the simplest explanations of particular phenomena need not be correct. Also, Ockham's Razor cannot eliminate the problems that result from incorrect information. (It can't resolve the "Garbage in; Garbage out" problem.) Applying it may cause an additional problem: a loss of important information. (Applying Ockham's Razor, especially to new areas of science, may result in the problem of "throwing out the baby with the bathwater.")

However, as Ockham observed, applying the "law of economy" does provide a means of isolating irrationally-based (dogmatic) opinions that have no logical basis. Also, provided its limitations are recognized, Ockham's Razor can be used to provide a logical structure or context, defined by increasing levels of complexity, for a particular line of reasoning. For these reasons, as information about a particular subject evolves, by repetitively applying Ockham's Razor over a period of time, frequently, it is possible to identify particular flaws in a particular line of reasoning.

It is always useful, as a consequence, to consider the implications of repetitively applying this "law" (over time) to the arguments and process that are used to substantiate or refute all un-founded claims. Because considerable confusion has developed over the years, concerning the subject matter of the present issue of Accountability in Research, it is especially helpful, from the outset, to use Ockham's Razor (in its most general context as it applies to logical thought) to help identify dogmatic opinions that have evolved in the associated debate, and to examine questions associated with how the debate has proceeded. It is also useful to examine how Ockham's Razor has been applied to the relevant science.

In particular, the following collection of seven articles deals with an extremely controversial, emotionally-charged subject, "Cold Fusion." Despite the positive[1] and negative[2-8] views that were stated (often in violent terms) at early stages in the controversy, a more recent history[9], and presentations in scientific meetings and in the literature[10-12] suggest that a final verdict concerning the existence or non-existence of the associated phenomena is by no means at hand. From the outset, as a consequence, consistent with applying Ockham's Razor as means of distinguishing fact from dogma, we will assume that claims about Cold Fusion have not been either verified or discredited. Instead of dealing explicitly with this question, the goal of this particular issue of AIR is to examine the "method of science" that has been used during the adjudication of this controversy.

Although to date, it is possible to infer some of this information from the early histories (Taubes[2], Close[3], Huizenga[4], Peat[13]) of the subject, and several authors (Morrison[14], Nagel[9], Beaudette[15]) have speculated about the ramifications of the history from a sociology of science perspective, a detailed picture, based on the accounts of the individuals involved, with a specific focus on issues associated with the dissemination and adjudication of science, has not appeared. Given the peculiar history of Cold Fusion, it is also clear that particular subjective biases, based on language, perspective, and background are almost universally present in any single monograph, dealing with this subject. With these factors in mind, in the collection of the next seven articles, efforts were made to investigate the "method of science," as it has been carried out with regard to Cold Fusion, by chronicling the events and feelings of scientists, editors, and administrators of science who have been associated with both sides of the associated controversy.

In fact, a scientific consensus about Cold Fusion does not exist, as the collection of seven articles illustrates. At a basic level, an important reason that this has occurred is related to a breakdown of communication about this area of research that occurred at an early stage in the controversy. Because a basic requirement of "normal science" is that communication about scientific ideas take place, it follows that Cold Fusion is not "normal science." If it is not "normal science," one must ask how this came about.

The breakdown in communication occurred early in the associated controversy as a result of an apparent consensus by mainstream scientists. Subsequently, although research in this area has continued, mainstream scientists are largely unaware of this fact. As a consequence, a large number of experiments have been carried out that are not widely known, in which positive Cold Fusion findings have been reported. Not only have these results failed to alter the predominant view that Cold Fusion is not possible, it appears that the establishment of this view, as a result of the consensus that was established early in the controversy, itself, has subverted the communication process. Thus, the breakdown in communication has persisted. Because it is possible that additional observations that potentially could prove or disprove the associated phenomena are not being made as a result of this breakdown in communication, it follows that the consensus that brought about this situation is responsible for a breakdown in the scientific process. For this reason, one concludes that Cold Fusion is not "normal science" and that at an early stage in the controversy a consensus by scientists was established that has resulted in a breakdown in the process of unbiased, objective reporting of scientific information about this area of research. This conclusion holds regardless of whether or not the associated claims are valid.

If we apply Ockham's Razor (as it applies in a general sense to the structure of logical arguments) as a criteria for testing if the scientific process has either functioned or not functioned as it has been applied to Cold Fusion, as a consequence, we find a rather startling conclusion: bias and pre-judgment that have resulted from the early consensus by mainstream scientists seem to have so unfairly altered the scientific process at such an early stage in the associated controversy that even after a decade, it is virtually impossible to rule-out the possibility that at least a number of the most important, initial, purported claims may very well be valid . Given the potential implications of these claims (which include the development of new, pollution-free, non-hydrocarbon forms of energy, that are as much as ten million times as efficient as the conventional fossil fuel forms that are in use today), this finding is not-only startling, it suggests that a very serious reconsideration of the associated adjudication process should be undertaken.

Ironically, in part, the origin of this remarkable controversy can be traced to a somewhat naïve application of Ockham's Razor. In particular, the "Cold Fusion" controversy resulted from claims that it is possible to create nuclear fusion (or other nuclear reactions) by passing a current through heavy water at room temperature. Because the associated phenomena are difficult to reproduce (assuming they exist, at all), initially, a naïve application of "Ockham's Razor" almost universally led to a simple conclusion: "Cold Fusion" did not exist, and the purported claims were a mistake.

With time, however, a large number of groups presented evidence that the phenomena could be reproduced, albeit in a somewhat unpredictable manner. More significantly, probably, is the fact that evidence of a potential, quantifiable causal link (associated with a nuclear by-product, ^{4}He, at levels consistent with the observed excess energy) began to be observed in a number of experiments in a manner that suggested that a new phenomenon (other than conventional fusion) could be involved (Arata/Zhang[11], Bush and Miles[10], McKubre[12]). An important point is that because this kind of evidence was radically different from what had been anticipated, it was not included in the initial analyses and critiques of the initial work. As a result, the simplest arguments against "Cold Fusion" subsequently have proven to be potentially flawed. For this reason, viewed as a basis of scientific argument, the initial application of Ockham's Razor, as it dealt with the purported claims, can no longer be accepted as being valid.

Despite this fact, however, much of the information about these developments has not become part of the mainstream scientific literature. In fact, if anything, communication about scientific results associated with this field has deteriorated. As a consequence, when one applies Ockham's Razor in a more general sense to the questions related to the scientific method and process as they have (or have not) been applied appropriately to Cold Fusion, one arrives at an obvious answer: empirical evidence, based not only on the volume[16] of information (but also on its content) that has accumulated during the associated scientific "debate," indicates that the process has not functioned in a manifestly self-correcting fashion.

There is an additional irony associated with the history surrounding applications of Ockham's Razor that has relevance in the analysis of the underlying dynamic of the debate. In particular, historically, William of Ockham used his "law of economy" as justification for eliminating dogmatic biases by the church towards particular ideas, rituals, and practices. Ironically, this has relevance to understanding the dynamic of the on-going controversy because a very real polarization has occurred between individuals who are actively involved in the field and those who are not. For this reason, the tenor of the present "debate" has become dogmatic in tone to the point that even some of the terminology that one associates with religion has crept into the "dialogue" surrounding the subject. (For example, beginning early in the controversy, adherents of conventional "scientific wisdom" began to refer to Cold Fusion as a "corpse,"[5] Cold Fusion scientists as "true believers"[7], their meetings as "séances"[7] and the podia from which

Cold Fusion talks were presented, as “pulpits”[6].) In other (more mundane) references to the subject, Cold Fusion findings were referred to as “An Embarrassment”[8] and “Pathological.”[14] Taubes[2] simply referred to the work as “Bad Science.”

Given the strange circumstances associated with the initial disclosure (as discussed below), it is perhaps understandable that a degree of derision in the “dialogue” concerning this subject was to be expected. However, even after a considerable volume of information about the subject had become available, the tenor of the “dialogue” did not change significantly. In response to this situation, beginning at the end 1993, the language of the “debate” became further polarized after Storms coined the expression, “Pathological Skepticism,” during the third International Conference on Cold Fusion (ICCF3), in reference to the manner in which a number of the individuals associated with the negative view of the phenomena had treated the available evidence. Again, a simplified model is plausible: At an early stage, meaningful scientific discussion about the subject came to an end; derisive comments about the subject became commonplace. Communication about the subject became increasingly difficult, which resulted in a kind of Catch-22 situation, in which information about positive findings was (and has) not been widely-believed or circulated, in many cases, because of potential fears of embarrassment. Cold Fusion has been treated derisively by most mainstream scientists, who essentially view the subject with scorn.

Implicitly, funding and the fears associated with the loss or control of funding have played an important role in defining the scientific and sociological dynamics of the controversy. The importance of this factor, especially now, 11 years after the controversy began, seems obvious. However, at the present time, it still is not obvious what role the funding process (through actions associated with research proposals, and the awarding and solicitation of grants) had in triggering the on-going debate, well before the disclosure of claims that occurred during the initial press conferences and subsequent uproar. In particular, at a very early stage, the funding process seems to have played a significant (even dominant) role in bringing about the dynamic that is responsible for the controversy. This began well before the initial News Conference on 23 March, 1989, by the two chemists, Stanley Pons and Martin Fleischmann from the University of Utah, and the subsequent (though nearly-simultaneous) announcement of findings by Steven Jones (and co-workers) from Brigham Young University (BYU).

Specifically, as Martin Fleischmann describes in his contribution to the present collection, during the summer of 1988, the need to procure funding led to a conscious decision by him and Stanley Pons to submit a grant proposal to Ryszard Gayewski, of the Department of Energy (DOE). This (as stated by Fleischmann) led to a “worst case scenario” in which their ideas “were forced into the public domain.”

In point of fact, considerable confusion resulted, not only through the manner in which funding was sought, but by the review process, itself, which resulted in an apparent obfuscation and over-simplification associated with the purported claims. In particular, because Steven Jones and his group were also involved with a project that was funded by Gayewski that seemed to be related to the work by Pons and Fleischmann, pressures associated with potentially patent-able ideas, disclosure of information, and property rights came into play. (For example, Ryszard Gayewski encouraged Steven Jones to provide notification of the disclosure of BYU findings to a public forum[17,18], prior to the University of Utah Press Conference.) Unfortunately, as a consequence, when the particular announcements were made, it was widely believed that the research efforts that were being carried out by the two groups were closely related. With hindsight, it has become apparent that this simply was not true. In particular, Pons and Fleischmann's most important claim involved the identification of anomalously large amounts of heat, during the prolonged electrolysis of heavy water (D_2O) by palladium (Pd), at levels that could not be accounted for by the normal laws of chemistry. They also said they had found evidence for the products (energetic neutrons, in particular) that one would observe in conventional nuclear fusion. However, subsequently, they withdrew this last set of claims. Steven Jones, and his co-workers from BYU, on the other hand, never claimed that they had observed significant levels of heat. Instead, they said they had observed the emission of neutrons, at low (but statistically significant) levels, during the electrolysis of D_2O by Pd. (They have never retracted the claim that they have observed some form of low-level nuclear reaction in these kinds of systems. Steven Jones, in particular, in his contribution to the present collection attests to this fact.)

Because of the timing of the two announcements, which were made almost simultaneously, most scientists believed that the two forms of experiments (and sets of results) were associated with the same phenomenon. However, as is immediately obvious from the first two articles in the collection, the underlying approaches that were being followed at the University of Utah (as discussed by Martin Fleischmann) and Brigham Young University (as discussed by Steven Jones) were entirely different. Despite the very different phenomena that were being examined and the nature of the claims that were being made, the idea that the claims from the two groups were related not only was widely accepted, subsequently, the fact that apparent differences in the purported claims existed was used as formal justification[19] for disregarding both sets of claims.

Scope of the Special Issue

As I have noted above, whether or not claims associated with Cold Fusion are justified is an open question. It is not the goal of this collection of articles to answer this question. It is clear that a number of complicating factors played a role in undermining the scientific process. Although the following collection of seven articles summarizes a number of these, because of the limited available space and the complexity that is involved, the collection must be viewed as a first attempt to understand some of the more important issues. Although in soliciting articles for the collection, I suggested that individual authors focus on the "scientific process" associated with the adjudication of the underlying phenomena (as opposed to questions relating to whether or not the phenomena have a sound scientific basis), not unexpectedly, a degree of personal bias seems to be prevalent in many of the contributions. Despite this fact, each author has contributed a number of important themes and "lessons-to-be-learned," associated with the controversy, that have value, regardless of whether or not Cold Fusion effects are a mistake or the result of real phenomena.

In an attempt to get a cross-section of differing views concerning this subject, I requested that a number of individuals who were involved in the initial debate participate in this Special Issue. In addition to Steven Jones and Martin Fleischmann, these included David Lindley and Steven Koonin. I also asked a number of individuals who became intimately involved in the associated controversy at an early stage, (but who were not directly involved in the most contentious areas of debate), to contribute articles. This second group of people consisted of

George Miley, John Bockris, Francesco Scaramuzzi, and Hideo Ikegami. In addition, I asked three additional individuals, Talbot Chubb, David Nagel and Carol White, who became involved in the controversy at a later stage, to participate in the project.

Unfortunately, Steven Koonin was unable to actively participate. However, through my contact with him, I corresponded with his colleague, David Goodstein, who has been actively involved in investigating issues related to the scientific process as it has related to Cold Fusion. He has graciously consented to allow us to include an article he prepared in 1994 (for **The American Scholar**) as part of the collection[20]. Each of the remaining participants of the project participated directly or indirectly in the formulation and preparation of the following collection of seven articles. (In the end, a number of them were unable explicitly to contribute articles.)

Content of the Special Issue

The result of this effort is a series of articles by individual authors, in which one finds an interesting cross-section of experiences and opinions that seem to reflect the diversity of backgrounds of the individuals who were involved. In particular, at one extreme is the testimonial by Steven Jones, which deals with the nature of performing research associated with anomalies and the need to persevere in the study of these kinds of effects in order to understand the relevant science. Because of the previous history associated with the controversy, he clearly delineates the BYU efforts from the anomalous heat experiments (which he refers to as "Cold Fusion"). In describing the BYU work, he examines in detail (a) a series of experiments that seem to provide tantalizing evidence of low level nuclear activity in electrolytic cells (and other room temperature environments) that is extremely difficult to reproduce, (b) the need to use adequate instrumentation to validate/invalidate these experiments, and (c) the importance of trying to understand even low-level effects (properly instrumented) in order to advance science.

At the other extreme, there are contributions from individuals who have been involved with the controversy, primarily, as outside observers of (as opposed to being directly involved with) the experiments, and whose role has been more as adjudicators of the relevant science. The contributions from these individuals tend to be more focused on general scientific issues, the "pros" and "cons" of the associated debate, and the methods of science that have gone on during the controversy.

The contributions from David Goodstein and David Nagel fall into this category. Specifically, these authors deal in general terms with the associated science and the manner in which the scientific process has "functioned" (or ceased to function) with respect to the controversy. However, although these two contributions are similar in focus, important differences between them are present because of the different perspectives that the authors bring to the associated analysis. In particular, David Goodstein discusses the controversy using ideas and terminology that are frequently applied in the history of science and science ethics. David Nagel deals with similar issues but with greater focus on the specific details associated with the purported claims and the manner in which they have been adjudicated, both inside and outside the active community of individuals who have been involved in investigating the associated phenomena. He also provides important background material, associated with conventional fusion and nuclear physics, and its relationship to the controversy. This is useful because it clarifies a number of the issues that are responsible for the skepticism that most mainstream scientists have with regard to the subject.

Intermediate between these two sets of extremes are the contributions by Martin Fleischmann, Francesco Scaramuzzi, John Bockris, and George Miley. Each of these authors discusses one or more experiments involving some form of "anomalous" result, in which he was personally involved in assessing its validity and in reporting the finding to the scientific community. In each case, the author describes how the particular experiment(s) was (were) conducted, provides some background material about the motivation that led his group to become involved in the work, the findings associated with the work, the reactions of outside adjudicators and colleagues, and the manner in which the methods of science have dealt with (or failed to deal with) the associated controversy. All of these contributions also provide interesting perspectives, based on the history of the subject, associated with lessons to be learned from how the field has progressed in the past and how it might (or might not) progress in the future. Again, however, although there are similarities in the topics that are covered in these contributions, important differences are also present.

In particular, Martin Fleischmann's contribution touches on a wide-range of topics, including his personal history with Cold Fusion, his reasons for performing Cold Fusion experiments, and his view of a number of more philosophical topics associated with the nature of science that have relevance beyond the immediate issues associated with Cold Fusion. Francesco Scaramuzzi's contribution focuses on a considerably smaller set of topics, associated with work at his laboratory. Though less general in nature, his article provides an important historical overview of changes that occurred in his and other Cold Fusion research programs in response to important, new experimental information that was presented in a series of talks (by McKubre and Miles) during the second International Conference on Cold Fusion. In this overview, he also explains the scientific motivation behind these changes. (The changes came about in response to evidence that was presented in the talks of a plausible causal link between two of the potentially more important Cold Fusion claims, involving the appearance and triggering of excess heat and a possible new form of nuclear reaction.)

John Bockris focuses not only on developments in his laboratory but on developments in other laboratories at Texas A & M. He also discusses friction, associated with issues related to academic freedom, that developed within his department as a result of the controversial nature of his findings and the topic of his research. In particular, both he and one of his graduate students (Nigel Packham) were severely criticized for being involved with Cold Fusion and were accused of making fraudulent claims. Although none of these claims was ever substantiated, the impact of the accusations affected both of their reputations. In his contribution, Professor Bockris also provides important background associated with an additional (extremely contentious) area of investigation that has evolved as a result of Cold Fusion research, namely, the study of phenomena associated with the anomalous appearance and disappearance of small amounts of material in electrolytic cells (and other environments) and accompanying "evidence" of low-level nuclear reactions. He and others have referred to this line of research as Low Energy Nuclear Transmutations.

Besides discussing a number of the issues associated with the dynamic of the controversy and his own involvement with a particular line of research (which includes work in Low Energy Nuclear Transmutations), George Miley also provides a unique series of observations concerning the role of the media in the controversy. In particular, as editor of the American Nuclear Society journal, **Fusion Technology**, he has made a number of editorial decisions that have allowed articles about Cold Fusion to appear in a "peer-reviewed" publication. Because most journals bar Cold Fusion papers from being reviewed and published, this editorial decision has placed him and his journal in a high-profile position. However, interest in the field has persisted, despite the lack of information that has been provided by established lines of communication. Professor Miley points out that the information-gathering process has also continued, but that effectively this has occurred through new, non-standard forms of communication, including the Internet, and informal newsletters and emerging periodicals.

Both George Miley and Martin Fleischmann suggest that uncontrolled uses of Information Age Era technologies (FAX machines, initially, and subsequently the Internet) have significantly undermined the Scientific Review process associated with Cold Fusion. David Lindley also suggested[21] that the Review process (as it applied to the initial submission by Pons and Fleischmann to NATURE magazine) was adversely affected by the widespread dissemination of pre-prints of the paper by FAX machines during the initial stages of the controversy.

It is clear that the extraordinary nature of the claims, and the presence of these new avenues of communication helped to fuel the associated debate. However, taken in isolation, it seems questionable that these factors were responsible for the breakdown of the scientific process. A more plausible hypothesis is that the disruption of normal Communications occurred once an apparent consensus was established that the claims associated with Cold Fusion were the result of errors.

In fact, an important series of events associated with this breakdown can be directly related to a series of extemporaneous talks that were held during a special session of the American Physical Society (APS), on 1 May 1989. In his article, David Goodstein provides a nice discussion of the associated history. He also identifies these events as playing a pivotal role in Cold Fusion's ceasing to be a "normal" form of science. It is worthwhile noting that the "normal" rules of scientific protocol (publication of abstracts, records of presentations, announcement of speakers, etc) that are conventionally practiced during APS meetings were abrogated during this session. As a result of these digressions, a number of unsubstantiated claims were allowed to be made[22] about the scientific worthiness of practices that were followed by the two experimenters (Pons and Fleischmann) whose work inspired the session, but who were not in attendance. It is also clear that after this event occurred, a notable deterioration in language associated with the scientific dialogue about Cold Fusion began. This included the first series of comments about Cold Fusion from the director of Public Information of the American Physical Society in which the subject was openly mocked[5].

It is clear that a number of factors, including the use of FAX machines, the Internet, and the abrogation of conventional procedures associated with science, seem to have had an important impact in shaping and defining the initial controversy during its early years. How seriously the events associated with the 1 May 1989 American Physical Society meeting, and the suspension of customary procedures associated with such meetings by the organizers, affected the associated debate is unclear.

As David Nagel notes in his contribution, in science (as well as elsewhere), a degree of accountability is always required. In particular, for communication to occur between individuals, an individual and a group of individuals, or between groups, mutual trust is necessarily required at some level. David Nagel also points out that the standard definition of “Accountability” includes the idea of liability; i.e., the notion that a group or a particular individual should be held responsible for a particular set of actions. An interesting point is that in “normal” circumstances, “liability” and “responsibility” can be identified frequently either by precedent or through the potential for pecuniary damages or rewards (as defined through the marketplace, for example). Thus, in a typical scenario, where science and the associated flows of information, technology, and money can be viewed almost in terms of a marketplace type of scenario, “liability” and “responsibility” can be defined in terms of how these processes are enhanced or impeded by a particular set of actions.

Within the context of “normal” or “abnormal” science, it is relatively easy to identify the terms of accountability, which David Nagel accomplishes through a matrix representation of the involved parties. An important point, however, that he does not emphasize directly, as it relates to the present set of circumstances as they apply to the scientific process, is that ultimately, the definition of “liability” hinges upon a subjective appraisal of a particular situation, based on precedent or bias. In scientific research, even more fundamental than the products of the effort is an important goal, the attainment of knowledge, based on verifiable hypotheses and measurements. Implicit in the way that scientists attempt to accomplish this goal are a number of important ethical practices that the majority of scientists presumably recognize: 1. Scientists seek the truth; 2. Because they recognize that trial and error is part of the scientific process, when scientists find flaws in what they have done, they freely admit their mistakes, attempt to correct them, and try a new approach. Thus, in an idealized situation, scientists are accountable only to themselves, and their community. If they are truthful in these endeavors, their accountability, as scientists, has been fulfilled.

The important point of contention in the Cold Fusion controversy (and, more generally, most of science) associated with this protocol for accountability involves the “identification of errors” and whether or not the scientists who are involved are required to admit their mistakes and attempt new approaches. In particular, in the present scientific climate, it is exceedingly difficult for a scientist to admit mistakes, unless he is permitted to do so in such a way that he does not embarrass himself or his sponsors. It is for precisely this reason that scientists give preliminary talks and presentations in particular settings where they can obtain valuable feed-back without jeopardizing their sources of funding. An unfortunate result of the way Cold Fusion evolved was that this kind of process was largely circumvented. Further aggravating the situation is the fact that the APS compounded the problem by allowing a confrontational environment (and subsequent climate) to evolve as a result of the extemporaneously called session on 1 May 1989.

Should the APS be held responsible for the consequences of what transpired as a result of these activities? Ultimately, this decision should and will be made by the members of the scientific community, not as a consequence of whether or not errors in judgment were made at the time, but whether or not, with time, mutual trust between the APS, its members, and the scientific community as a whole remains in tact. In fact, because the APS freely allows individuals to present and debate controversial topics at its meetings, it does provide a useful setting for airing opinions concerning contentious issues. During the last four years, a number of individuals (including myself) have used this venue in order to publicize ideas and findings associated with Cold Fusion-related phenomena[23-26]. With time, one hopes that this type of activity will prove useful in resolving the existing controversy.

In the final analysis, a number of factors significantly undermined the scientific process in the dissemination of information about Cold Fusion. Initially, these included not only the frequently identified problems associated with errors in judgment that resulted from the disclosure of incorrect and incomplete information by a number of the scientists, who were involved, but more subtle errors associated with the review process, the resulting confusion associated with competing claims, the abrogation of established rules and protocols by the American Physical Society, and the wide-spread circulation of incomplete, incorrect results, using Information Era Technologies. The latter set of (not frequently identified) factors seem to have had a longer-lasting effect because of an unanticipated effect: as interest intensified, Information Era Technology inspired a confrontational environment that appears to have taken on an identity of its own. In the resulting climate, a serious lack of trust and respect seems to have become prevalent between individuals who have participated in Cold Fusion research, and those who have not.

As noted by George Miley, irresponsible postings, abusive rhetoric, and other behaviors by outside followers of the controversy through the Internet have acted to intensify the difficulties associated with the existing, polarized environment. In many cases, this has increased the tenor of an already acrimonious debate, while adding little of scientific value. This suggests a hidden irony that may provide the most valuable lesson of all that has resulted from the controversy: effective scientific communication (and more generally communication between any group of individuals) requires a degree of trust and respect. When this does not exist, the most powerful Information Era Technologies can actually impede a meaningful discussion of the relevant issues. Alternatively, one might say that “unless we can trust the people we are trying to talk to and speak nicely to them, the fanciest communication machines won’t help the situation, and they very easily can make it worse!”

It has been said that unless we study and analyze history, it has a tendency to repeat itself. The Cold Fusion controversy is an extraordinary scientific and sociological event that has been going on for more than a decade. I believe that the lessons I have mentioned here, as well as the many additional lessons and comments that are included in the following collection of seven articles may help us to prevent this kind of thing from happening again.

Acknowledgement

I would like to thank all of the participants for their patience and perseverance. Besides those individuals who were directly involved in preparing articles for the collection (which include John Bockris, Martin Fleischmann, David Goodstein, Steven Jones, George Miley, David Nagel, and Francesco Scaramuzzi), a number of others participated indirectly in planning and developing the Special Issue. For their help in this context, I would especially like to thank Talbot Chubb, David Lindley, and Carol White. I am deeply grateful to my wife, Anne Pond, and my father, Charles Chubb, for the strong moral and emotional support that they have provided during the last decade. Without their help, it is doubtful that this project would have been completed. I would also like to thank Charles Beaudette for providing valuable background material. I also acknowledge valuable discussions with Robert Terry, which were essential in initiating the work. Finally, I would like to thank Adil Shamoo for his encouragement and help in making the Special Issue a reality.

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20. The major portion of this article is re-printed from *The American Scholar*, Volume 63, No. 4, Autumn 1994, Copyright c 1994 by David Goodstein, with the permission of David Goodstein. Reasons for including it in the present collection are the following:. 1. The periodical where it appeared previously has a limited distribution; 2. The article provides a central theme (associated with the definition of "normal science" and why Cold Fusion is not "normal science") that is used elsewhere in the collection; 3. The article includes additional information, not found in the original, that re-affirms the point that despite the fact that important ^{4}He findings have been observed, this fact is not widely known and that for this reason, the field should be viewed as not being "normal;" and 4. The article provides a degree of balance to the collection because its author has not been actively involved in the debate and is an expert on issues related to Ethics in Science.
21. David Lindley, private communication (1997).
22. As discussed in David Goodstein's article, on the basis of information inferred from television coverage, Nathan Lewis suggested that Pons and Fleischmann had made serious errors in their calorimetric measurements. Although Lewis never prepared a detailed publication, based on this particular talk, he did publish an analysis that suggested why one would not expect to obtain excess heat in a Pons/Fleischmann type of experiment. Subsequently, in papers by Noninski and Noninski (V. C. Noninski and C. I. Noninski, *Fusion Technology,* **23**, 474 (1993).) and Miles et al (M. H. Miles, R. A. Hollins, B. F. Bush, and J. J. Lagowski, *J. Electro Anal Chem*, 346, **99** (1993).), Lewis's analysis was criticized.
23. http://www.aps.org/meet/MAR00/baps/abs/S1110.html#SC32.001
24. http://www.aps.org/meet/CENT99/BAPS/abs/S9500.html#SZC07.004
25. http://www.aps.org/BAPSMAR98/abs/S4170.html#SU26.002.
26. T. A. Chubb and S. R. Chubb, "Ion Band States, Many-Body Effects, Implications for Cold Fusion," Bull. Amer. Phys. Soc. **41**, #1, 332 (1996). S. R. Chubb and T. A. Chubb, "Overlap Properties of D^+ Ion Band State Matter: Implications for Cold Fusion," Bull. Amer. Phys. Soc. **41**, #1, 341 (1996).