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Reflections on the Sociology of Science and Social Responsibility in Science, in Relationship to Cold Fusion

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Section 1. Introduction

I have been asked on several occasions during the last ten years to provide a commentary on subjects such as the Sociology of Science, Social Responsibility in Science etc. with special reference to the topic of "Cold Fusion". I have always been reluctant to do so for two principal reasons. The first is that I believe that the consideration of these topics is premature. The reason is that I am convinced that there is much more information which bears on such topics which will be revealed in the fullness of time. Furthermore, there are aspects of the past history, developments and, indeed, of the science which I do not wish to discuss even at the present time. Inevitably, therefore, any such article will be incomplete, will lack focus and will be couched in rather general terms. This is the way the present article has turned out to be; in view of the evident deficiencies, it should only be taken as a first step in trying to develop a discussion of the important Social Issues.

The second reason for my reluctance to write on such topics is that I believe that one cannot develop such discussions without an understanding of the Science involved. While such an understanding will always be important, it is especially important for the case of "Cold Fusion" because this work was paradigm driven; i.e., it was undertaken because of a belief that the present day interpretation of condensed matter is incomplete. My view has always been that the revelation and discussion of this aspect would only lead to further adverse criticism (and it will be interesting to see whether this will be the case!). My preference was to restrict attention to the experimental results and to await the development of the necessary paradigm. This is the normal pattern of scientific research, and my attitude was determined by my wish to follow a "hidden agenda" as described in Section 4.

However, it is now 10 years since the initial announcement, 30-40 years since my realization that such effects must be present (and **might** be observable depending on whether one takes 1970 or 1960 as the starting time for such considerations) and more than 50 years since I first discovered some of the key background information. Perhaps, therefore, it is now time to start to discuss the Sociology of the subject?

This article is divided into the following Sections

Of these sections. Sections 2 - 4 cover the essential background material and Appendix A may be useful in reading Sections 2 and 3. Sections 5 - 7 deal with the start-up of the project, and the initial results; while Section 10 comments on some of the results obtained after March 1989. Parts of Section 7, as well as Sections 8, 9 and 11 deal with the Sociological issues.

Section 2: Background

In the 1960's I became convinced that the behavior of ions in solution can only be explained in terms of Quantum Electrodynamics, Q.E.D. rather than in terms of Classical Mechanics, C.M., or Quantum Mechanics, Q.M. (here, by Q.M., I am explicitly referring to the forms of this subject that apply in a "mean field" approximation in which many-body effects are unimportant; this is by no means a complete definition; additional details concerning the definitions of Q.M. and Q.E.D. and related concepts are included in Appendix A). The somewhat tortuous path which led to this conclusion is not relevant to the material to be discussed here; instead, I will give a useful illustration of the nature of the problem.

The generally accepted model of electrolyte solutions, the Debye-Huckel Theory[1], is based on the calculation of the self-energy of the systems due to the electrostatic interactions of the ions, see Fig. 1, i.e. on C.M. (while making some allowance for the fact that we are dealing with a many-body problem). However, the ions are not at rest and it has been known since the last century that ions move independently of each other at high dilutions (Kohlrausch[2]). Moreover, the dominant motion of the ions must be attributed to Brownian movements (Einstein[3]) so that the ions must accelerate and decelerate. Fig. 2. The concomitant radiation (Maxwell[4]) should therefore cool the electrolyte to absolute zero at which hypothetical limit the Debye-Huckel Theory would become valid. We therefore need to ask ourselves: why do the solutions not cool spontaneously? The answer is that the electrostatic model, when combined with Brownian motion, violates the Second Law of Thermodynamics (see also the next Section and Appendix A). In point of fact, the motion of the ions must be rigorously quantized and the correct theoretical framework is many-body physics (i.e., Quantum Field Theory, or Quantum Electrodynamics), which, henceforth, will be referred to simply as "Q.E.D".

Section 3: On Choosing the Correct Paradigm: the Influence of Paradigms on Scientific Research

The example which I have just outlined (the use of electrostatics in the Debye-Huckel Theory) is an useful illustration of the influence on scientific research of models based on particular paradigms[5]. We understand the world in terms of models which are based in turn on particular paradigms. I believe that all scientists would agree that, in constructing such models, we should use the simplest model/paradigm combination sufficient for the task in hand[6]. Thus the Debye-Huckel Theory leads to the interpretation of the variation of the self-energy of electrolyte solutions with the concentration (albeit at very low concentrations). We do not need to use a more complicated model/paradigm as long as our attention is limited to the thermodynamics of such systems.

At the same time, there are dangers in such a minimalist approach. The models/paradigms tend to develop a life of their own to the extent that they become regarded as a form of revealed truth to which nature is expected to conform. It becomes difficult (perhaps even impossible) to ask whether deviations from any predicted behavior may not be due to the use of an incorrect paradigm? We do not ask the question; what would be the consequences of using a different paradigm? In extreme situations we see attempts "to save the paradigm" (a well-known activity) with increasingly improbable special assumptions[7]. We also see the denial of the reality of experimental observations if these cannot be fitted into the generally accepted paradigm.

This embedding of particular paradigms/models in the methodology of science also leads to the further difficulty that these paradigms/models are frequently applied to the interpretation of properties (or properties of other systems) where their use is restrictive, possibly even incorrect. Thus, for the illustration used in this article, electrostatic models have been used in the interpretation of the dynamical properties of electrolyte solutions. Inevitably, such interpretations are kinematic. The consequences of using dynamic models remain hidden from view, and the further development of research in this particular field is frustrated.

Evidently, we need some methodologies for choosing appropriate paradigms, a problem which has been much discussed in the Philosophy, History and Sociology of Science. Such discussions usually center on the question: were there (or are there) an increasing range of phenomena which could not (or cannot) be explained by an existing paradigm or model? We should also discuss the question: can we arrive at "better interpretations" of given phenomena by changing the paradigm or model? However, this second question can open the way for much subjective musing of doubtful value. It has therefore always seemed to me that we should use more "hard-nosed" approaches and I have found three such approaches to be especially useful.

In the first of these approaches, we can ask the questions: can a given property A be interpreted in terms of paradigm/model X by using the argument a? Can a second property, B, be interpreted in terms of the same paradigm/model X by using the argument b? Such a list can be extended to include further properties. We then ask the farther question: are a and b self-consistent? If this is not the case, then the likely cause is the inapplicability of the paradigm/model X. It is of interest that the dissolution of hydrogen and deuterium in palladium affords several excellent examples of such inconsistencies if the paradigm X is Q.M.

In the second approach, we investigate whether the application of the paradigm/model leads to the violation of the Second Law of Thermodynamics. I believe that most scientists would agree that the inability to violate this law (or else of some related principle such as microscopic reversibility) is our first line of defense against the development of invalid interpretations. However, it is also clear that whereas Scientists are very adept at criticizing Engineers, members of the public, or even each other (!) for the invention of devices based on Perpetual Motion, they are not very adept at subjecting paradigms and their derived models to the same scrutiny.

In the third approach, we investigate whether the description of a paradigm/model is mathematically and physically complete. Needless to say, this is the most difficult area from a technical point of view as it leads to the heart of Theoretical Physics. Interested readers may wish to explore the development of Q.F.T. and Q.E.D. from Q.M. as an example of this type of investigation.

Fig. 3 is a flow diagram which illustrates the material outlined in this Section. The development of models frequently poses difficult problems so that there can be considerable delays between the establishment of a paradigm and its associated models. It is perhaps therefore inevitable that the distinction of paradigms and models becomes blurred so much so that "models" can be given the status of "paradigms". However, I believe that the distinction should be preserved, if only because a model may be incorrectly constructed even though the relevant paradigm is valid.

We should also take note of the fact that the data derived from relevant experiments are the raw material for interpretations based on various paradigms. One of the failures of present day research is that such "raw data" are not properly archived so that much of the information which could be derived is "lost". The processes of data evaluation are also undervalued. As far as this particular aspect is concerned, it would be interesting to determine whether present day research applications include realistic, separate estimates for such activities - even whether they include any such separate estimates at all and, if so, whether such work is funded. Research workers appear to have developed an attitude which can be summarized by the statements: "I will do this tomorrow, next week, next year", but then, next year may never come.

The emphasis on the role of paradigms on the "scientific process", as in Fig. 3, also illustrates an important aspect of research which is frequently neglected. Thus the normal procedure is to use paradigms and their associated models to interpret data sets generated by experiments which are carried out without any particular reference to the paradigms/models used in their evaluation. The measurement of thermodynamic quantities is a well-understood example of this type of activity aimed at the understanding of the "know-how" of matter. However, if our aim is the understanding of the "know-why", then the council of perfection is that the paradigm/model should itself determine the experiment design and data accumulation as is shown in the right-hand stream for paradigm 3, Q.E.D. We can use measurements on electrolyte solutions as a useful illustration. Suppose the lower half of Fig. 3 for paradigm I, C.M., refers to the measurement of conductances of such solutions and that we choose to interpret the data using a model based on electrostatic interactions. Then, as we have already noted, we will arrive at a kinematic description. Suppose, on the other hand, that we base our interpretation on paradigm 3, Q.E.D.   In that case we may arrive at a dynamic description.   However, if that is our objective, then we would be well advised to change the experiment design, data evaluation and modeling to be more directly relevant to the application of this paradigm (the right-hand stream for paradigm 3, Q.E.D.).   In the late 1960's, I decided that the measurement and interpretation of fluctuations in small systems was one possible route for probing the applicability of Q.E.D., especially the applicability to the behavior of condensed matter.

Fig. 3 also shows paradigm 2, Q.M., as being somewhat separated from C.M. and Q.E.D. My reason for showing it in this way is that I believe that the application of Q.M. to condensed matter does not satisfy the third criterion (mathematical and physical completeness), which I have referred to earlier in this Section. Applications of Q.M. to condensed matter always appears to demand special assumptions; i.e., Q.M. is an example of a paradigm which has been saved repeatedly (as is illustrated by the item "modification of paradigm" in Fig. 3).

The sub-divisions used in Fig. 3 also demonstrate that research projects may fail to progress (or fail completely) for a variety of reasons.   Quite apart from the use of an incorrect underlying paradigm, the models may have been incorrectly formulated; the experiment design may have been inadequate, and the accumulation of the raw data may be insufficient or lack the required accuracy; lastly, the data evaluation may be incomplete. These are matters which are rarely discussed in the scientific literature.

The application of these comments to the development (and lack of development!) of "Cold Fusion" will be apparent from the other Sections of this article.

Section 4. The First Interregnum: the Search for Demonstrations of the Need to invoke the Q.E.D. Paradigm.

My conclusion during the 1960's that the behavior of electrolytes had to be modeled in terms of the Q.E.D. paradigm did not come as any great surprise as it was apparent by that time that this was the correct paradigm for the consideration of condensed matter. At that time (and at various times since then) I discussed some of the problems involved with other scientists whom I placed in two broad categories: those familiar with Q.F.T./Q.E.D. and those, who as far as I could judge, were not familiar with this particular field. Such a classification was indeed useful: the response of those in the first group (by now 7 in total) to the problems raised by the modeling of electrolyte solutions was always immediate: "you must use Q.E.D." The response of those in the second group was uniformly negative - and, frequently, quite violently so. I came to believe that any research on this topic would have to follow "a hidden agenda". The topics had to be chosen so that they could be justified in terms of foreseeable interpretations in terms of C.M. or Q.M.; the specification of Q.E.D. at the outset would simply lead to sterile debate which would frustrate the research. The need to invoke this paradigm would therefore have to emerge from the interpretation of those parts of the problem which were not essential to those parts leading to interpretations based on C.M. or Q.M.; i.e., it was necessary to be able to specify:

"given that the systems behave in the way seen, we can then use C.M. or Q.M. to reach the following conclusions".

Q.E.D. would need to be restricted to the interpretation of the statement:

"given that the systems behave in the way seen".

The particular approach which I favored at that time was the study of the dynamics of small systems as revealed by the direct observation of the fluctuations of the properties (see also Section 3). In the fullness of time we could specify four systems which had the potential of satisfying the "hidden agenda". Two of these systems were investigated, but research on one of these had to be abandoned because of the pressure of work on "Cold Fusion"; evaluation of the data sets for the other system was not completed for the self-same reason (specifically, those aspects which required interpretation by Q.E.D.). In addition, there were nine further projects of which four were investigated. However, the interpretation of the results were (or were expected to be) dependent on deviations from the predictions based on C.M. or Q.M. The demonstration of the applicability of the Q.E.D. paradigm was therefore not clear-cut.

It also became apparent that this particular line of research suffered from a general, major drawback, in that the present day theory required for the interpretation of the experiments (the theory of stochastic processes) is quasi-classical. The effects of Q.M. are only included to the extent of the recognition of distinct quantum states (this is perhaps one of the clearest examples of the long-term persistence of the ideas of the "Copenhagen School of Q.M."). Progress with these topics was therefore dependent on the development of a theory of stochastic processes based on Q.E.D. - a task of monumental proportions (readers may note that this is a good illustration of the fact that the development of appropriate models does not follow immediately on the recognition of the need to invoke a particular paradigm, see Section 3).

My attention therefore returned to systems where the need to invoke the Q.E.D. paradigm would become apparent from macroscopic observations. The behavior of electrolyte solutions was evidently a suitable example.

Section 5. The Second Interregnum: towards the Start of Work on the Pd/D System

My realization that models of electrolyte solutions had to be based on the Q.E.D. paradigm inevitably focused my attention again on the Pd/H and Pd/D systems. I had realized since the end of 1947 that these were the most extraordinary examples of electrolytes. At that time I had found the early papers of Coehn[8] who had already shown in 1929 that H was present as H^+ in Pd host lattices (deuterium had not been discovered at that time). Moreover, the H species was highly mobile in the lattice and, as the mobility obeyed the Nemst-Einstein relation, the species had to be present as bare protons. In point of fact, the system behaves as an extremely dense plasma of protons (concentration ~ 100 M) present within even higher concentration of electron$(concentration ~ 1000 M).

The investigations of Coehn led to a number of very uncomfortable questions about the properties of the Pd/H and Pd/D systems. Evidently, the hydrogen ions have to be extremely strongly bound in the lattice so that the dissolution can be exothermic (i.e. H^+ in the lattice is more strongly bound than in H_2). At the same time, the hydrogen ions are highly mobile, a conundrum which defied resolution within the framework of Q.M. (this is an example of the first type of approach which can be used to judge the applicability of a given paradigm, see Section 3). There was also the further question: would it be possible to change the potential energy of D^+ in the host lattice sufficiently (by means of applied electric fields) to induce nuclear reactions? My answer at that time was "no" (based on the available Q.M.), except, possibly, under "heroic conditions" (the topic of Q.E.D. had not as yet been developed, although it had been foreshadowed by the work of Einstein[9]).

The matter rested there until the 1960's, at which time I came to realize that the Pd/H and Pd/D systems had to be modeled using Q.E.D. At that time we started a number of haphazard investigations of the Pd/H system. The question of whether one could induce nuclear reactions became more clearly-defined at the end of that decade.

Work on the isotopic separation of H and D showed that it was necessary to assume that the H and D present had to be modeled as many-body systems in order to explain the macroscopic behavior. I assembled equipment to start work on the putative nuclear processes on two occasions but each time decided that such research would be judged as being inconsistent with holding an Academic Appointment!

Section 6. The Start-up of the Project

In the early 1980's Stan Pons and I started a number of collaborative projects. During 1983 we decided that we could add one further major project to the topics being investigated by our groups. The topics we considered were:

(a) Relativistic effects in chemical reactions;

(b) extension of the investigation of the structure and spectroscopy of interfaces;

(c) the behavior of electrons in metals;

(d) nuclear reactions of D^+ in metal host lattices.

Of these projects, we decided that (a) was beyond our means; (b) was dependent on obtaining major funding which we could not secure. Of the remaining two projects, (c) was our first choice but it rapidly became apparent that this, too, was beyond our means.

We therefore embarked on (d) and considered the implications of carrying out this project. As I have already noted, I had previously excluded research work on this topic; however, I had by that time resigned from my full-time Academic Position. At the same time, the situation facing my colleague would clearly become serious if the nature of this project ever became known. We decided that the project not only had to have an "hidden agenda", it had to be totally hidden. This was all the more necessary because the military applications of any positive outcome of the research were not at all clear.

This early "prehistory" of the research is instructive from two points of view. In the first place, if one engages in innovative research, then the direction of ones work is by no means certain. Thus, if we had obtained funding for (b), then we would never have started on (d): I describe Academic Freedom as the freedom to carry out the research for which one can obtain funding. Secondly, the ways in which any positive results would be received were abundantly clear to us - the events post March 1989 did not surprise us in the least!

As is well-known, we posed the following two questions at the outset:

(i) would the putative reactions of D^+ compressed into host lattices be different from the reactions in a dilute plasma (or the reactions of highly excited D in solids)?

(ii) could such changes in the reactions be observed?

We expected the answer to (i) to be "yes". Thus at the simplest possible level the rates of reaction would inevitably be enhanced as the D+ in Pd-host lattices is present in a quantum system of macroscopic dimensions. However, we expected this enhancement to be insufficient to allow the observation (ii) so the answer to thil question was likely to be "no". Nevertheless, we started a limited investigation anc considered experiments based on the options A, B, C and E:

A. Compression of D^+ in the lattices using applied electric fields (i.e., electro-diffusion);

B. Compression of D in the lattices using electrochemical charging;

C. Charging of lattices by means of highly reducing media;

D. Highly oxidizing media and the link to "Hot Fusion";

E. Composite systems; e.g., B or C linked to A.

Of these systems, A was our first choice, but we started with B (as a preliminary to A) because we believed that such systems are closest to the dictates of Q.E.D., a view which was mistaken. Furthermore, electrochemical charging appeared to offer the easiest way of raising the potential energy of an extended quantum system in an energy efficient way. Systems of type D were added to the list when it became clear that the nature of nuclear reactions of D in host lattices (not just the rates) was radically different to that observed in "Hot Fusion" (see further below).

As is also well-known, we opted for calorimetry as our primary "catch-all" methodology. Calorimetric methods can be made to be nearly as sensitive as the readily accessible methods for the detection of nuclear particles and, indeed, are used in nuclear physics when it is necessary to make absolute measurements (e.g. in the estimation of plutonium). Furthermore, the use of calorimetry was consistent with our wish to follow a "hidden agenda". The calorimetric method chosen had to meet a number of important criteria which included conformation to "ideal behavior" (implying predictability from the laws of Physics, a concept which is well-known in the field of Chemical Engineering); high stability of the thermal impedances; uniformity of the temperature throughout the volumes of the cells; possibility of non-isothermal operation; high precision and accuracy; last, but not least, low unit costs, as we were financing the projects personally. These criteria dictated the use of calorimeters based on modified Dewar vessels, using a methodology known as isoperibolic calorimetry.

Section 7. The Initial Results; Sauce for the Goose and Sauce for the Gander; Serendipity; the Path to March 1989

The overall structure of the problem had become reasonably clear by the summer of 1988. We were observing the generation of heat in excess of the enthalpy input to the cells, and far above that commensurate with the generation of tritium and neutrons predicted by measurements on "Hot Fusion". Moreover, the excess enthalpy was far beyond that which could be attributed to any parasitic chemical reactions. It appeared, therefore, that it was possible to establish nuclear reactions in quantum systems of macroscopic dimensions contained in metal host lattices (following the dictates of the Q.E.D. paradigm), which not only had much higher cross-sections than those predicted on the basis of two-body processes, but which also differed in kind from those observed in such two-body reactions. Evidently, it would be necessary to establish the nature of the reaction path(s). The detection of the most likely product, ^4He, (the "nuclear ash") would be a project of the utmost difficulty and quantitative correlations of the yield of ^4He with excess heat production even more so (subsequent work by other research groups[10] has shown this to be the case). We obtained indications of the formation of ^4He during the first phase of our work but considered these results to be un-publishable. Instead, this was a factor in persuading us to continue the research. Such a distinction is important. Measurements are often made during the execution of an innovative program which are aimed precisely at answering the question: "are we justified in continuing with this project?" At that stage, the question of publication of the results is quite secondary.

The supposed lack of evidence for "nuclear ash" (supposed but not actual!) has proved to be one of the main points of criticism of the many "skeptics". However, our initial consideration of this question led to a further series of problems best described by the epithet, "what is sauce for the goose is sauce for the gander". Thus, to the best of our knowledge, the very low yield of ^4He in "Hot Fusion" (about 10^{-6} that of the yields of neutrons and tritium) appears to be based on the low yields of the highly energetic y-rays which accompany the formation of ^{4}He in the two-body processes. While such a conclusion may be valid for nuclear reactions in dilute plasmas, it may be far from valid for "Hot Fusion" in solid phases (more especially for "Warm Fusion" in such systems). In fact, work on "Hot Fusion" must be judged to be incomplete in the absence of precise thermal balances and correlations of the yields of ^{4}He with that of the production of γ-rays. It was therefore evident that the project would need to be extended to include:

D. Highly oxidizing media and the link to "Hot Fusion".

It is relevant to this particular point that recent work on neutron and tritium generation in solid phases at low energies of incident deuterons has indicated that extrapolations of the results obtained at high incident energies to lower energies may not be justified[11]. (The rates at low incident energies are higher than expected). Furthermore, many-body processes are evidently involved in the fusion reactions at the lower energies. There is also an interesting historical perspective to this aspect of the work.

The discovery of deuterium fusion in 1934 by Oliphant, Harteck and Rutherford[12] was followed shortly afterwards by measurements by Dee[13] of the tracks due to the H+ + T+ fusion path using a Wilson Cloud Chamber. As was noted by Dee, a proportion of these tracks had to be attributed to fusion reactions of deuterons which had lost most of their impact energy by collisions in the target (Cold Fusion?). Unfortunately, most of this early work has disappeared from view, no doubt in part because "deuterium" was called "diplogen" in the 1930's. One needs to ask: "should research on new topics be accompanied by a proper measure of Scientific Archaeology?" Interested readers may wish to survey the work on "Hot Fusion" and ask themselves a series of questions which include: "exactly why did research on ‘Hot Fusion’ become confined to the investigation of Tokomaks?" "exactly why was work on all the other possible systems terminated?" "could the search for energy generation have been successfully implemented using devices other than Tokomaks?" "why has further work on Tokomaks now apparently been terminated?"

In the summer of 1988, it had become clear that much further work was required, that the work would have to be broadened and that, with an achievable acceleration of the program, we might be able to assess the overall results by the autumn of 1990. We estimated that the cost of such an accelerated program would be ~ $600K, which was above the limit which we could meet personally. At that time, we also believed that we had reached a stage at which it was necessary to inform the United States Department of Energy and the United Kingdom Atomic Energy Authority of the nature of our research. This would in any event have become necessary at some stage during the research but we decided that it was opportune to combine the information with an application for funds for the envisaged accelerated program.

It is important that our initial results (and the results obtained since 1988) appeared to indicate that there might be useful applications in the civilian sector, without any military uses, a conclusion which ran counter to our initial expectations. However, closer reflection also showed that such a prediction might be incorrect. The system we had developed was based on the premise of the Q.E.D. paradigm, and it was not especially difficult to *specify* the changes which would need to be made to investigate whether such systems could be made to converge onto the Q.M. paradigm applied to the collision of two hydrogen isotopic particles (which is an adequate basis for existing military applications). We have never been questioned about this aspect, and we certainly never had any intention of investigating the required changes. At that time we therefore made it clear that we were unable to judge whether the work should be classified, at least for a defined period of time (see also Section 8). We believed that we ourselves did not have the necessary information to reach a decision on this issue but that, in any event, it was necessary to continue a secret program until such time as it would be possible to carry out a complete evaluation of the project. However, it transpired that the restrictions which we wanted to impose ran counter to the funding policies of the Department of Energy.

It is appropriate to consider here the role of serendipity in this research (indeed, in any research). It has frequently been asserted that these discoveries were made "by serendipity". This view is incorrect: the whole matter was a rather cerebral exercise.   However, serendipity certainly played a part in the progress of the work. In my view the true role of serendipity is the recognition of the significance of unusual results.   It is better to guide ones research by a series of logical steps, rather than to indulge in a process best described as "Gee Whiz". However, it is also important to accept and explain unusual results (provided these are at an adequately high level of statistical significance), rather than to ascribe them to unspecified errors (or to incorrect scenarios for imagined errors). One outcome of this research has been the demonstration that scientists have developed a blindness for accepting unusual results. No doubt this is due in part to an excessive faith in invalid paradigms.

Section 8. The Position in March 1989

The outcome of our application to the Department of Energy can best be described as leading to the "worst case scenario" for our future research work; the whole matter was evidently going to be forced into the public domain. We therefore had to disclose our results to the Administrative Authorities of the University of Utah, who, in turn, felt bound to apply for Patent Protection. It is important to recall that we had by that time reached specific rates of energy production roughly equal to those in gas cooled fission reactors.   In turn, the Patent Applications became the driving force for future actions.

The fact that this scenario followed this disastrous course has frequently been criticized.   However, one must ask: "can one imagine that the events could have followed any other scenario once the research was going to the driven into the Public Domain and given the nature of our results?"

I believe that it is important here to summarize our reasons for wishing to delay the publication of the results. In the first place, although we had indications for the formation of ^{4}He, these results were not publishable; secondly, we believed that most scientists would judge the work on the basis of the Q.M. paradigm, applied to the collision of two deuterons in a dilute plasma and would therefore conclude that our results had to be false; thirdly, we did not believe that industry would conclude that research in this field (let alone any products based on this research) would be in their short term or medium term interest although there might well be initial flashes of enthusiasm; fourthly, we believed that those concerned with National Security could hardly be expected to welcome such research in the University Sector; fifthly, we believed that we would lose our freedom of action because research on this particular topic would become constrained by targets and modes of operation ill-matched to achieving further progress; lastly, we really wished to return to the more general problem of searching for examples of the operation of the Q.E.D. paradigm (see Section 4).

I will comment here briefly on the third and fourth aspects; while I will consider the fifth aspect further in Section 8.

As far as the third aspect is concerned, it is frequently asserted that the attitude of industry towards innovative research can be summarized by Adam Smith's famous dictum:

> "it is in everybody's interest that a lighthouse be built";
> "It is in nobody's interest to build that lighthouse".

Judged from the perspectives of a single company, the "bottom-line" always appears be: "it is best to continue with our present technologies and methodologies".

However, Adam Smith's dictum was formulated for an age which was less driven by technology than is the case at the present time. The 20th Century version should really be revised to:

> "it is in everybody's interest that a lighthouse be built";
>
> "It is in nobody's interest to contribute more than one foundation stone to that lighthouse".

Such a revised stance dictates the execution of a limited research program and accumulation of appropriate data sets (no doubt suitably notarized), followed by the truncation of the program, preferably without drawing any definite conclusions. If necessary, it is possible to draw negative conclusions (to justify the truncation of the program), but the data sets which are used in the justification are not released. Such a strategy allows a later re-entry into the field (with appropriate claims for priority) if the topic is advanced by other investigations to the point where commercial applications are realizable. The earlier conclusions are not critically important: it is the data sets which matter.

It is also important that the most self-evident initial applications are as sources of heat. Any new technology would therefore need to be established in competition with existing sources of heat, notably combustors. It has often been maintained that the introduction of "Cold Fusion" into the energy field would produce dramatic changes in society. I believe that this view is mistaken because the "downstream technologies" would be unchanged. In fact the essential question is whether such new sources could compete with combustors? I believe that the answer to this question is "yes" but that in considering this question, one must bear in mind that the recurrent costs of such new energy sources are close to zero. Optimization therefore requires that the system be run at the maximum achievable specific power output (so as to

minimize the impact of the investment costs on the total costs). Such considerations had an
important influence on the design of our forward program.

As far as the fourth aspect is concerned, parts of this have already been considered briefly in Section 7. The military applications which we could envisage included: (i) neutron generation, (ii) the production of tritium, (iii) the use of compact sources of heat, (iv) a phenomenon which we described euphemistically as "the uncontrolled release of heat". Of these applications, (i) and (ii) are covered by the comments in Section 7; while (iii) is related to the phenomenon of "Heat-after-Death", described in Section 9. The question which gave us most concern was whether (iv) could be implemented. At that time, we suspected that the systems could eventually be driven into a regime of "positive feedback", in the sense that increases of temperature would lead to increases in the rates of excess enthalpy generation (see Section 9). However, we had no definitive evidence about this aspect until the summer of 1990. It is important that our knowledge of this topic is still far from complete because it is based on the response of the systems to small perturbations in the temperature. However, our concern about this matter was not so much the question of the magnitude of the effects of "positive feedback", as the realization that the existence of such an effect opened the way for the use of other perturbations. In fact, whereas we were unable to judge in 1988 (or 1989) whether the research should be classified, we would have opted for such a course of action in 1990 if the question of any publication had been delayed until September of that year (in accord with our wishes). I note also that classification would have allowed a much more orderly and comprehensive investigation of the topic than has been the case with the research in full public view.

It is pertinent here to consider the scale of the effort which would have been required to arrive at a reasonably comprehensive assessment of (iv), viewed in the context of "positive feedback". In addition to temperature perturbations, we could specify immediately four additional ways of perturbing the systems. The methods of implementing such perturbations as well as the effects of their magnitudes had to be investigated. It was necessary to explore at least ten variables determined by the form of the systems, several key variables in the production technologies for a given material and, last but not least, the effects of changes in composition of the systems. We described such a task as requiring the resources of a Manhattan II project.

Section 9 Reactions to our Preliminary Paper and the Press Conference in March 1989

I have often been asked: "did the reactions to your Preliminary Paper and the Press Conference in March 1989 surprise you?" I have replied: "not in the least". I fully expected that the majority of scientists would judge the results in terms of the application of the Q.M. paradigm to the fusion of two deuterons in a dilute plasma and would therefore conclude that our results had to be false (notable exceptions have been Schwinger, Preparata and Del Giudice, whose expertise, of course, was and is in Q.F.T. and Q.E.D.).

I have also been asked: "would the reactions have been different if you had delayed publication and/or published your initial results in an obscure Journal?" (it is well-known that I was in favor of both these stratagies; the Annals of Utah Science appeared to me a suitable Journal for our first publication). However, it does not appear to me that either of these courses of action would have changed the situation materially. At best it would have delayed the antagonistic reaction, a delay which would admittedly have been valuable.

What was surprising was the intensity of the negative reactions which bordered on hysteria. I believe that the root causes are not difficult to discern. Our publication coincided with the widespread use of a new publication medium, the FAX. It has always appeared to me that the advent of any new means of publication induces a period of hysteria (printing of pamphlets and political unrest, radio and the rise of Fascism, television and the unrest in the late 1960's?) In the present case, the use of the FAX abrogated the usual means of publication; thus it led to the dissemination of a paper which we had submitted but then withdrawn (who exactly was responsible for these actions?). It is important to understand that the funding for science is now inadequate whereas the number of scientists carrying out research has increased dramatically. Inevitably, this reduces the possibilities for innovative research and accentuates the tendency to "band wagon". The Preliminary Paper and Media Reports were therefore used as primers to start a series of investigations whose design, execution, extent and data analyses were inadequate for the task in hand. Of course, none of this would have mattered if the experiments had led to striking, positive conclusions. However, if the expectation is a negative result, then any near zero observation is taken as confirmation of a negative outcome. In the initial phase of research following our first publication, four of the most frequently cited publications came to such negative conclusions. However, episodes of excess enthalpy generation were, in fact, observed in the most comprehensive of these investigations. One method of data analysis in the second investigation was so strange that it obscured a positive result[14]; the third investigation almost certainly also gave a positive result although the data were presented in a way which makes it difficult to reach a definitive conclusion[15]; the fourth investigation was so limited (a single experiment) that it must surely be excluded from any further consideration. Of course, there were also investigations which came to positive conclusions, but these investigations were not cited (and are not cited) because a pattern of critique was immediately established, aimed at disproving our initial observations. Those engaged in this critique also established the terminology of "skeptics" and "true believers" which *de facto* prevented any rational discussion of the results.

However, no matter, we can consider further the four key "negative papers" (as I have already noted we need to exclude one of these from further consideration). It is convenient to do this in terms of the methodology of The Experimenter's Regress as used in the Sociology of Science. As applied to the beginning of a new and controversial field of investigation, this principle tells us that we cannot tell which of the following two statements is correct:

A. "Positive" conclusions are correct; "negative" results are due to bad experimentation.

B. "Negative" conclusions are correct; "positive" results are due to bad experimentation.

However, this decision tree is too short because the only immutable outcome of experiments are the relevant data sets. The conclusion as to whether these show "positive" or "negative" results depends on the processes of interpretation, which should never be completed at a single point in time. As Scientists we therefore need to examine the possible applicability of the following statement (as well as its corollary):

C. Key "negative" conclusions have been due to incorrect evaluations/ interpretations; the results in fact point to "positive" conclusions.

If this statement applies, then the Experimenter's Regress should be seen to be broken. In fact. Statement C applies to two of the three key "negative" papers and, possibly, to all three. We must therefore ask: "why was there no revision of the "negative" view?"

It now appears to me that there are two principal reasons for such a lack of revision. The first is that whereas erroneous "positive", results are usually v/ithdrawn, erroneous "negative" results hardly ever meet such a fate. In practice, the "negative" papers are eventually simply forgotten. The second reason is that if a given result is judged to be incorrect on the basis of an entrenched paradigm, then the nature of the results becomes irrelevant to the critiques. Who now remembers the opposition which greeted Planck's initial formulation of Q.M.? Who now remembers the opposition which greeted Arrhenius' proposal that electrolytes dissolve as ions water?

In this connection, it is important here to draw a distinction between an understanding of "Cold Fusion" in terms of the Q.E.D. paradigm and, say, the understanding of "Black Body Radiation", and Planck's formulation of Q.M. Thus the fact that "Black Body Radiation" could not be explained in terms of Q.M. was accepted before the advent of the Quantum Theory. The opposition to Q.M. was therefore somewhat muted. On the other hand, research on "Cold Fusion" was started because of an understanding that Q.M. provides an inadequate basis for the understanding of condensed matter; i.e., the Q.E.D. paradigm preceded the experimental evidence. Inevitably, therefore, the assessment of the validity of the new results was going to be based on the understanding of paradigms and the understanding of Q.E.D. is sadly lacking. (Readers may wish to note that there is, in fact, now a large body of knowledge which has been integrated in terms of Q.E.D., rather than Q.M. [16]).

A number of further factors contributed to the rapid build up of the "skeptical" response. Thus 1989 was a singularly unfortunate year for the disclosure of the new results: in the first place, it was the 50th anniversary of the discovery of nuclear fission, a discovery which had also been made by investigating the Chemistry involved. Furthermore, work on "Hot Fusion" was reaching a decisive stage, and further work on this topic would evidently require new funding. I had repeatedly warned that the holding of a Press Conference would create a wave of negative publicity and disinformation, matters which we were ill-equipped to counter at that stage. In this connection it is appropriate to single out the Press Conference itself. The response can be summarized by the statement: "Physicists don't hold Press Conferences prior to the acceptance for publication of a Scientific Paper", followed by intense criticisms of our actions. In actual fact, such statements were incorrect on two counts. First of all Preliminary Paper had, in fact, been accepted for publication - and we would never have agreed to the holding of a Press Conference if that had not been the case. Secondly, Physicists are perhaps the worst offenders in the violation of the accepted norms for publication. For example, the results of the first investigation of "Hot Fusion", the Harwell Zeta project, were announced by H.M. Postmaster General to the House of Commons, prior to the submission of any paper. When it came to the matter of publication, it was found that the results were wrong. Clearly, such publicity seeking is ill-advised.

Naturally, this type of critique became a feature of comments in "the media", rather than the Scientific Literature. Here we must now extend the concept of "the media" to include the electronic mail and more recently the Internet. It appears to me that such disinformation could be controlled by insisting on a strict separation of fact from editorial comment, a step which would certainly lead to a great improvement of the body politic. Of course, I realize that many Social Scientists maintain that such a separation is not possible. While there is certainly a great deal of truth in such an attitude, it is a far cry from adopting an admixture of erroneous "facts" and "editorial comments" as the norm in publication. Unfortunately, this pattern of commentary on erroneous facts also became a feature of publications in the Scientific Literature, a matter which can readily be established by interested readers. We have frequently said: "if we had done this, or said that, then such a criticism would have been entirely justified". However, the points at issue were that we had not done this or said that and the critics felt free to make such assertions without following the accepted pattern of first clarifying the issues by consulting the authors. Readers may wish to assess whether the real purpose of such critiques was not the spreading of disinformation. In this context it is important to understand that incorrect statements acquire "a life of their own". Third parties do not need to establish whether the critiques were justified; it is sufficient to quote them.

A further favored technique has been to link "Cold Fusion" to other examples of scientific investigations which have proved to be erroneous. A favored methodology has been to describe such research as "Pathological Science", a concept first put forward in a lecture by Langmuir, as illustrated by the non-existent N-rays (was there in fact no basis for the original observations?). This lecture by Langmuir (great Scientist though he was) was manifestly silly, a fact which is well-illustrated by this reply to a question about the reality, or otherwise, of the theory of relativity, as illustrated by the measurements of the bending of light in a famous solar eclipse. In the hands of lesser men, such a topic becomes absurd. The point at issue is that each topic needs to be judged in its own rights: the linkage of different topics simply hinders the objective assessment of each subject. The consequence of such critiques was again the spreading of disinformation.

It is perhaps inevitable that the pattern of disinformation led to an antagonistic attitude among the referees of Scientific Papers and a virtual banning of the topic by many Journals. The Scientific Public (and the Public at large) were therefore deprived of the normal means of communication and debate. Indeed, the process of refereeing has long been due for a major overhaul. As is well-known, papers are judged by their Introductions and Conclusions, whereas they should be judged by the Experiment Design, Data Accumulation, Data Evaluation and Mathematical Analyses (where relevant). A revised attitude towards refereeing becomes ever more important as the process of publication moves towards monolithic institutions. Is it not time to institute a second review as a matter of routine and to restrict editorial decisions to a line of last resort?

The rapid polarization of opinion naturally also affected the funding of further work on the topic. This is considered further in the following Section 10.

It will be evident that I have excluded the Report commissioned by the U.S. Department of Energy from the topic of the reactions to our Preliminary Work although it has often been cited as giving useful evidence on this topic of research. The reason for this exclusion is that this Report requires a detailed analysis in its own rite (see also Section 12). Such a detailed analysis cannot be included in the present assessment. Moreover, one could not expect a report commissioned at such an early date to make any useful contribution to the research topic as, indeed, proved to be the case.

Section 10. Post March 1989, "Luck" in Research; Restrictions on the Research Program

As was to be expected, the topic of nuclear reactions of hydrogen isotopes in host lattices has turned out to be much more complicated and extensive than could have been predicted in March 1989. Parts of these topics will be described in other articles contributed to the present series. Furthermore it has become apparent that the systems are "pseudo-simple": while they are relatively simple to set up, their behavior is actually very complicated. It is necessary to establish extreme conditions in the bulk of the host lattice (the volume), as mediated by its surface. One would therefore expect the reproducibility to be low, as has turned out to be the case. It is therefore necessary to carry out a large number of experiments under extreme conditions for prolonged periods of time, a methodology which is not part of the usual approaches to the design and execution of experiments. Moreover, most of the key variables are "hidden" and are difficult to evaluate because the systems are only subject to very few controllable parameters (for Scenario B, the cell current or voltage, the temperature for any given material). Unfortunately, the methodology required for the evaluation of "hidden variables" is not generally understood so that progress has been restricted (this methodology is understood for example in some aspects of Control Engineering).

The terminology "for any given material" in the previous paragraph is a matter of the utmost importance. We came to realize already in 1988 that there were some materials for which one could observe excess enthalpy generation with adequately high rates of success; while there were some which gave a zero result. It was apparent, therefore, that the development of the materials aspect was a matter of crucial concern. However, the resources for the investigation of these particular aspects have never been available (see further below). I have therefore recommended throughout that investigation of this topic should be delayed until such time as adequate resources might be available. Instead, I have recommended that in the interim, research should use a material known to give a reasonable rate of success (while realizing that such a material and the consequent data sets were unlikely to be optimal). Unfortunately, such a strategy lacks appeal, and my advice has never been accepted.

In this context, it is appropriate to consider the topic of "luck in research". If, in our early research, we had only used materials giving virtually zero rates of excess enthalpy generation, then we would certainly have abandoned the topic! In fact, I believe that it is quite generally true that innovative research is heavily biased towards those topics which "work first time".

Furthermore, if we had used a different method for assessing the presence or absence of changes in the rates of the nuclear reactions (e.g. the measurements of neutrons), then we would again have concluded that the effects were absent. This illustrates that a definitive answer to a particular question requires comprehensive investigations. Such investigations can never be mounted in the initial phases of a research project so that "luck" again becomes an important factor in assessing the answer to any question.

The influence of "luck" in the next stages of an investigation is equally important. Thus, in the present example, the system is subject to many variables (see also Section 8 and the end of this Section). The question of whether the ongoing research progresses is therefore dependent on whether one has chosen an achievable objective and an appropriate set of variables. Progress in research is therefore again subject to "luck", tempered by "judgment", a quality which is sadly lacking

As far as our own forward program was concerned, we took a firm decision to avoid all those aspects which could be construed to have implications for National Defense (see Section 8). Concentration on the civilian aspects inevitably meant that we should explore the ways in which the specific power outputs and energy efficiencies could be raised to the point at which one could envisage practical applications. Inevitably, in view of the financial and time-scale constraints, such work had to be carried out with a very narrow focus and, moreover, within an incomplete knowledge base. We opted for continuation of the work using Systems of Type B as a preliminary for work on Systems of Type A and on Composite Systems, Type E (see Section 6).

As far as the generation of excess enthalpy using Scenario B is concerned, it became apparent that the production of low levels of excess enthalpy is easy to demonstrate, provided the experiments are carried out with high precision and accuracy, and provided satisfactory electrode materials are used. These provisos have turned out to be critically important, problems which have not always been understood. With increasing time and/or temperature, the systems then pass through a region of "positive feedback" in the sense that increases in temperature lead to increases in the rates of excess enthalpy production. This "positive feedback" greatly complicates the investigation, which is another matter that has not been generally understood. "Positive feedback" leads to much higher levels in the rates of excess enthalpy generation, including the sustained production of heat at the boiling points, at rates roughly equivalent to those achieved in fast breeder reactors. Furthermore, it has become apparent that it is possible to construct a number of systems operating above/beyond the onset of "positive feedback" and which generate enthalpy at lower levels but at zero enthalpy input. These phenomena have been described with a number of epithets including "Heat-after-Death", "Heat-after-Life" and "After-Effects". The work of Mengoli et al[17] on this topic is especially noteworthy.

The use of devices based on "Heat-after-Death" etc. appears to open up the route to a range of "niche applications". The work on systems of Type B would open the way to a much wider range of applications, especially those which require the utilization of low-grade heat. However, it has also been shown that much higher specific rates of excess enthalpy production (in the range 10- 100 kW cm^{-3}) can be achieved using systems of Type A (note especially the work of Preparata and Del Giudice[18]). Systems of this type may well lead to a very wide range of applications.

The technical details given in this Section have necessarily been very restricted. Perhaps the most serious omission has been an account of the way in which the linkage between excess enthalpy production and ^{4}He production has been established. Readers interested in this and other aspects (generation of tritium and neutrons, indications of more complex transmutations, work using light water) should consult the Proceedings of the Series of International Conferences on Cold Fusion I - 7. These Conference Proceedings remain the best source of information in view of the restrictions which have been met by those wishing to publish papers in most Scientific Journals. It will be seen that research now covers a very wide field, notwithstanding the inadequate level of funding. The level required can perhaps be assessed when it is realized that, even if attention is confined to scenario B, and if materials variables and any military applications are excluded (see Section 8), then it is still necessary to explore between 10 and 15 variables in order to define the operating characteristics (the relevant field of work is known as the Factorial Design of Experiments).

Section II. The State of Science; the Relationship between Science and Society

In considering these topics, we should recognize at the outset that the true spirit of Scientific Enquiry is a somewhat delicate plant, unlikely to survive the impact of ill-considered external pressures. I believe that most people would place the starting date of this aspect of the Modern World in the 17th Century and consider it to be a rather late flowering of the Renaissance. The initial spirit was clearly Platonic, the attempt to understand the know-why of the Universe.. C.M. was the paradigm which underpinned this initial phase of enquiry. However, right at the outset, science was subjected to external pressures, such as the utilization of the knowledge gained for military and commercial purposes (to illustrate this one needs to look no further than the early history of the Royal Society). It is perhaps therefore inevitable that the paradigm became a form of intellectual *meccano,* used to underpin the Aristotelian enquiry into the know-how of matter.

A pessimistic assessment would be that the true spirit of enquiry contained the seeds of its own destruction?

Of course, since that time, the C.M. paradigm was replaced by Q.M., which should in turn have been replaced by Q.F.T. (with Q.E.D), applied to ordinary matter. This last step should have been taken in the second half of the present century thereby leading to the next phase of the investigation of the Natural World. However, this has not happened, a matter which should be of serious concern to Society. There are other serious matters, which need to be considered, and I will single out just two in the present article. The first is the question: what fraction of the total knowledge base is in the Public Domain? Indeed, is it acceptable that any knowledge should be removed from the Public Domain? Research should be carried out within the whole body of knowledge available at any given time. It is often said: "one cannot un-invent the results of research". By the same token one should say: "one must not remove knowledge from the Public Domain". Of course, it may well be that the consequences of particular research programs prove to be unacceptable by Society at large. In that case, we require further action on the consequences of the research, rather than the removal of the results of the research from the Public Domain.

The second serious matter is the development of monolithic research programs. It may well be that some of these programs are desirable, but the processes leading to their selection and abandonment are quite opaque to the Public-at-large. One might well ask: "if it was so clear that one should initiate a program on particle Physics, then what is the justification for truncating this program at a later stage?" If it was so clear that one should initiate a program on "Hot Fusion", then what is the justification for truncating this program at the present time?" "Exactly what benefits do we expect to derive from the Space Station, the Human Genome Project, or any other large-scale, concerted technological activity?"

What is at stake here is not so much a lack of communication between scientists and society at large, as the poor quality of that communication. This state of affairs is often described by the statement: "scientists are unwilling to discuss their work with the General Public". However, this statement is incorrect: in my experience, scientists are perfectly willing to engage in a general debate, but the attitude of "the media" appears to be that such material cannot be "dumbed down" to the point where it would make good entertainment. Inevitably, therefore, media attention focuses on issues which are quite peripheral to the subject matter. Such a choice of material is self-defeating because the presentation then lacks depth. It is unlikely that the viewing or reading public will analyze the presentations in these terms; in practice, they will simply switch off or abandon reading.

Two further factors militate against the communication between scientists and the public. The first applies to television alone (surely now the most important means of communication), and the second originates in television but has been almost universally adopted by the Press. The first factor is that much presentation on television now lacks visual appeal. The restrictions in funding preclude adequate preparation to ensure that viewers will be visually engaged: the material presented will therefore be rather boring. The second factor is that the same restrictions in funding lead to the construction of programs according to prepared scenarios. The factual material therefore becomes an illustration of the editorial opinion, and television programs inevitably become polarized. Such a *modus operandi* is perhaps inevitable for the medium of television; the more serious problem is that the same methodology has now been adopted by the Press. I believe that the written word must still be regarded as the principal means of information for matters which require some degree of engagement by recipients of the information. The admixture of fact and editorial opinion can never be an adequate means of conveying such information.

It is useful here to use the reportage of "Cold Fusion" as an illustrative example. For example, if the content of the key papers presented at the series of meetings devoted to the topic had been reported in the form: A said a, B said b…..

Z said z, followed by an editorial comment that I (the editor) believe this is all nonsense for reasons (x, 3...,. *0).,* then the reading public would have been able to assess whether it believes at least some of a, b ..... z, in preference to the relevant comments (x. P..... Q). The polarization of opinion would have been avoided and there would have been some chance of initiating a reasoned debate.

As is always the case for scientific investigations, such assertions should lead to interesting experiments (which should at the least be useful in falsifying the initial assumptions). Readers may care to take a small number of articles dealing with the subject of "Cold Fusion" and mark up the component parts according to whether they are "facts", "editorial opinions" or "extraneous material". This would lead to the straightforward exercise of separating the article into these three sections. Readers may then wish to ask themselves a further series of questions which include:

(i) Do the "facts" precede the "editorial opinions" or are these aspects confused (by accident or design)?

(ii) Are the "facts" correctly reported?

(iii) Do the "facts" sustain the "editorial opinions"?

(iv) Have the "facts" been selected to support particular "editorial opinions"?

(v) Are the "editorial opinions" quite independent of the "factual material"?

(vi) Is "extraneous material" introduced to support particular "editorial opinions"?

(vii) if (vi) does not apply, then what is the purpose of such "extraneous material"?

In such an exercise, it would also be useful if readers asked themselves the Question, whether the "facts" give an adequate summary of the state of knowledge. A convenient starting point would be:

(viii) what cross-section of the results reported at the International Conferences on "Cold Fusion" I - VII have been reported?

Needless to say, an exercise of this kind can be applied to other topics in Science (indeed also in Economics, Politics, etc.), covered in the Press. I believe that such an exercise will show very rapidly that the standard of reportage falls far short of that required to develop an informed readership. At the same time, the development of an informed readership is clearly a prerequisite for the proper extension, of the decision making process. In the absence of such an overdue extension the public-at-large will feel increasingly disenfranchised, leading to the extension of decision making through pressure groups. The end result will be an inability to seek rational solutions to problems subjected to rational analyses.

It appears to me that the improvement in the flow of information required to allow such an extension of the democratic process now needs some degree of regulation to ensure that earlier standards of Journalism are reintroduced. Such a view will surely be attacked as smacking of "censorship". It is appropriate, therefore, to emphasize that any such regulation would not deal with the content of articles but merely with their structure. Such a revision in structure would counteract the present-day deconstructive tendencies; it would also make it much more difficult to use the Press as a vehicle for propaganda.

Any analysis of the present-day State of Science also requires some consideration of the all-important question of the funding of research. Here, many of the comments made in the previous Sections will have "a familiar ring": the funding of research is now inadequate for the number of scientists engaged in this activity. Such a shortage of funding increases the tendency towards "band-wagoning", while at the same time increasing the pressure towards consensus science. Applications for funding will only succeed if the referees and program officers accept that a field of work is respectable - and then the referees and program officers are themselves susceptible to the pressures of the consensus. One outcome is inevitable: a move towards "safe science", the marginal extension of the knowledge base. Much of this is no doubt very laudable, but one can hardly expect such work to be epoch making or cost effective.

The pressures towards the trivialization of research have also been accentuated by changes in funding policies. This is a matter which I can only judge from an United Kingdom perspective. In the balmy far-off-days of the 1950's and 1960's, there was "dual funding" for research in the University Sector. Research was carried out in "well-found" laboratories i.e. laboratories which were equipped with most of the state-of-the-art instrumentation required for innovative research. Applications to funding bodies were therefore restricted to special items and manpower, and such research was carried out at the marginal cost. Such a costing applied equally to research carried out for Industry and Government Laboratories. Moreover, this attitude in the funding of research carried over to some extent into Industrial and Government Laboratories.

This funding policy made it relatively easy to carry out "unsafe" and "blue skies" research.

Starting in the 1970's there was a move towards making each project carry its full cost, and the policy of "well-found laboratories" was abandoned. It is hard to imagine how those concerned with the Strategies of Central Government could persuade themselves that such changes would improve the efficiency of the overall Science Establishment. Such a conclusion appears to be based on the belief that an optimization of the component parts will lead to a global optimization but, of course, such a view is incorrect. The component parts are inter-connected, and many of the inter-connections are non-linear, in the cost functions. If one wishes to optimize the global system, one must do just that. Optimization of the component parts can only increase the overall costs. Moreover, if one wishes the system to "learn", then one must certainly abandon linear programming.

The downward pressure on funding inevitably implies that laboratories are no longer "well-found", so that it becomes difficult (indeed, virtually impossible) to carry out "unsafe" research. Moreover, this downward pressure has been accompanied by an increase in the level of sophistication of instrumentation so much so that the accumulation of complex instrumentation becomes an end in itself. Inevitably, researchers need to invest considerable time and effort to acquire the equipment deemed essential for their research. There is then a need to explore to the fullest extent the potentialities of the instrumentation, and research becomes driven by the need to find problems suitable for the instrumentation, rather than the search for the solution of interesting problems. All these factors lead to an accentuation of the search for the know-how of matter, rather than the know-why.

The implications of the structure of Science Funding on the development of research into "Cold Fusion" (more correctly, the lack of development) will be self-evident. Such research could certainly not be considered as being "safe" science. Indeed, as I have already described, I did not embark on this topic until after I had resigned from my full-time Academic Appointment. Furthermore, we decided to cany out the research in secret and to fund it personally (if we could have afforded to invest $1M, we would not have made any application for funding). The hysterical reactions to our Preliminary Paper, the invalid conclusions drawn from some of the initial independent investigations, the disinformation in much of the Media coverage, and perhaps, most seriously of all, the strange phenomenon of the Report commissioned by the Department of Energy, ensured that the topic was firmly classified as being "unsafe". It therefore became impossible to obtain funding through the traditional means.

Section 12. In conclusion; Conspiracy Theories?

It will be clear to those familiar with some of the background of the topic of "Cold Fusion" that there are many aspects which I have not considered in this article - notwithstanding its length. The strange behavior of the Patent Office, the strange presentations in some of the books devoted to the subject, the strange behavior of a number of scientists concerned with the research, the strange circumstances surrounding the preparation and publication of the Report commissioned by the Department of Energy, the strange behavior of the Editors and Editorial Staff of some of the Scientific Journals and many other matters all deserve detailed scrutiny. Much of this behavior has an intimate bearing on the role of Science in our Society.

I have often been asked whether all of these strange happenings might not be explained by a Conspiracy Theory or else a series of conspiracies linked to some central Conspiracy Theory. My reply has always been that it is usually tempting to invoke Conspiracy Theories but that one should only do so only as a last resort. Instead, I subscribe to a different theory, put forward by a former colleague, best described as "The Cock-up Theory of History".

Nevertheless, one must ask oneself the question: suppose that one would wish to frustrate research within a given field of research, without wishing to admit that this is ones intent. Then would one not take the steps which have been illustrated by the example of "Cold Fusion"? At the present time readers will have to reach their own conclusions as to what may be the explanation of the strange events which have surrounded this field of research.

Appendix A. The Relationship of Quantum Electrodynamics, Q.E.D., to Classical Mechanics, C.M., and Quantum Mechanics, Q.M.

One *of the* major achievements of the 19th Century was the explanation of the behavior of gases in terms of C.M.; i.e., using Newtonian Mechanics and Maxwell-Boltzmann statistics. However, this freshly constructed topic of Classical Statistical Physics failed to account for the newly discovered Third Principle of Thermodynamics (the entropy approaches zero as the temperature approaches absolute zero). This failure was in large measure responsible for the abandonment of the concept that the behavior of matter can be explained by the collisions of localizable tiny spheres. Instead, a picture emerged in which matter is a quantum wave field described by two fundamental quantities: the "number of its quanta" which appear as atoms and give the intensity of the field and the "phase" which accounts for the interference effects which have been increasingly discovered by research carried out during the present Century. This matter field is coupled to other quantum fields such as the electromagnetic field which has an analogous internal structure and, thereby, gives rise to the observed natural phenomena which can be understood in the conceptual framework of Quantum Field Theory, Q.F.T. For the case of ordinary matter, made up of atoms interacting through electromagnetic fields, Q.F.T. specializes to Quantum Electrodynamics, Q.E.D.

This new quantum framework correctly describes the existence of macroscopic states of low entropy. The inability of C.P. to describe such states (as was shown dramatically by the "catastrophe" of the Third Principle of Thermodynamics) arises from the mutual independence of atoms (and molecules), coupled to the lack of a mechanism for suppressing their independent fluctuations. It is the experimentally observed low entropy of condensed matter at low temperatures which requires a microscopic dynamics that is able to correlate the motions of large numbers of particles (in contradistinction to the experimentally observed behavior of gases). Such a correlation can only be achieved in conventional Q.M. by making special assumptions.

Unfortunately, Q.F.T. has, until recently, only been used in the somewhat remote area of subatomic Physics. On the other hand, the everyday world has been interpreted in terms of C.P. although the inadequacies of this formalism were already recognized a long time ago (e.g. by Nernst). The general opinion appears to have been that C.P. gives a satisfactory description of the know-how of matter without any need to investigate the know-why.

[1] Debye, P. and E. -Huckel, *Phys. Z.* 24, 185 (1923).
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[6] **Editor's Note**: This idea is an example of the more general principle (commonly referred to as Ockham's Razor) that has been likened to the notion of "scientific parsimony": i.e., the simplest arguments should be used where possible; or, as stated by the noted English theologian William of Ockham, "It is futile to do with more what can be done with fewer. [Frustra fit per plura quod potest fieri per pauciora.] ." Hoffman et al (R. Hoffmann, V. Minkin, and B. Carpenter, on the Internet at http://rz70.rz.uni-karlsruhe.de/~ed01/Hyle/Hyle3/hoffman.htm) provide an interesting discussion of this principle as it applies to chemistry.
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private communication.

Legends for Figures

Fig I. Schematic diagram of the negative space charge around a central positive ion formed by the electrostatic interactions of the ions.

Fig 2. Schematic diagram of the Brownian Movements of ions. The arrows connect the points at which the ion is at rest.

Fig 3 The relationship of Experiment Design and Experimental Data to their Interpretations, using Models based on Paradigms.

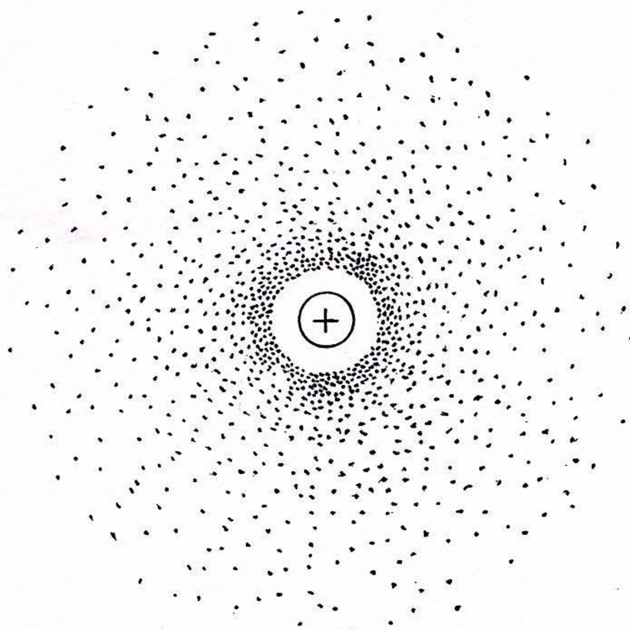


Fig 1

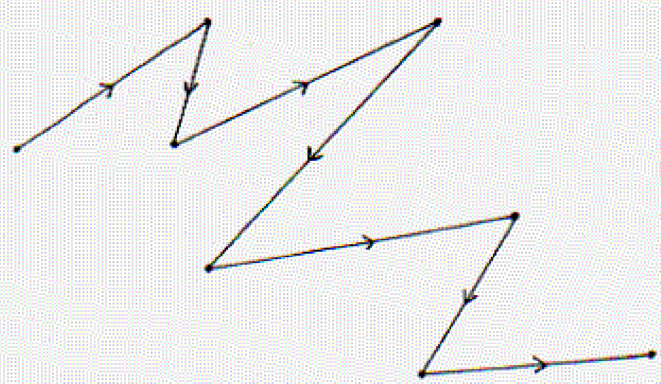
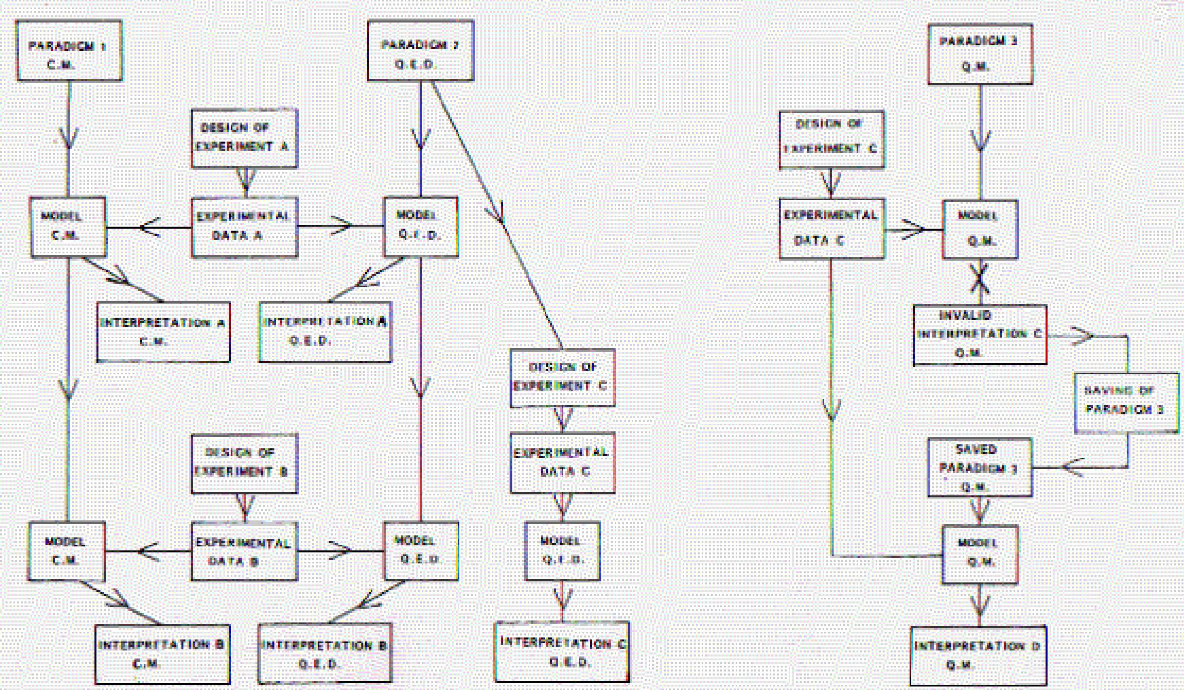


Fig. 2



PARADIGM 1
C.M.
DESIGN OF
EXPERIMENT A
PARADIGM 2
Q.E.D.
MODEL
C.M.
EXPERIMENTAL
DATA A
MODEL
Q.E.D.
INTERPRETATION A
C.M.
INTERPRETATION A
Q.E.D.
DESIGN OF
EXPERIMENT C
DESIGN OF
EXPERIMENT B
EXPERIMENTAL
DATA C
MODEL
C.M.
EXPERIMENTAL
DATA B
MODEL
Q.E.D.
MODEL
Q.E.D.
INTERPRETATION B
C.M.
INTERPRETATION B
Q.E.D.
INTERPRETATION C
Q.E.D.
PARADIGM 3
Q.M.
DESIGN OF
EXPERIMENT C
EXPERIMENTAL
DATA C
MODEL
Q.M.
INVALID
INTERPRETATION C
Q.M.
SAVING OF
PARADIGM 3
SAVED
PARADIGM 3
Q.M.
MODEL
Q.M.
INTERPRETATION D
Q.M.

Fig. 8